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Essays on Financial Economics

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A dissertation
submitted in partial fulfillment of the
requirements for the degree of

Doctor of Philosophy

University of Washington

2019

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Program Authorized to Offer Degree:
Department of Economics

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Abstract

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The thesis studies the topics of financial stability and their implication for financial economics. An exogenous shock to the financial system is critical because it can further result in an economic downturn. One of the reasons is that the financial system acts as a hub connecting all financial activities in the economy. In this thesis, I explain the mechanism of the shock transmission in the interconnected financial system and the real effect on the economy. In the first chapter, I discuss the impact of the oil price shock in 2014, which acted as a credit supply shock. Using the syndicated loan data, I find that the banks with high exposure to the oil price shock increased the loan interest rates for the new loans more than the banks with low exposure. In the second chapter, I investigate the real effects to the corporate borrowers after the credit supply shock. I find that the borrowing firms' investment spending decreased after the credit supply shock and the amount of cash holding mitigates the impact of the shock. Lastly, in the third chapter, I take a broader approach of studying financial stability. I investigate how the commonality of asset holdings of the banks can increase the systemic risk. In particular, I argue that the commonality of asset holdings make the financial system more vulnerable to a systemic event.

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ACKNOWLEDGMENTS

I would like to thank my dissertation advisors Yu-chin Chen and Eric Zivot for all the support throughout my Ph.D. education. Without their help, I would be nowhere near where I am now and their guidance greatly contributed to forming my research philosophy. I also thank Jonathan Brogaard for being approachable and willing to provide attention to details on my research. His help was critical in the early stage of research refining my research question more interesting. Special thanks go to Michael Blake, Peter Ward, and Benjamin Feintzeig for taking responsibility of the Graduate School Representative. I also thank Simon Reeve-Parker, who has helped me from the admission process to the graduation stage. Besides, I appreciate all participants in the MTI Brown Bag seminars and fellow graduate students at the University of Washington for their thoughtful comments.

DEDICATION

to my dear parents, Yang-Hoon Sonn & Jeongwha Ahn
and my dear brother, Hyo Bin Sonn

Chapter 1

BANK LENDING CHANNEL: EVIDENCE FROM THE OIL PRICE SHOCK

In this study, I find empirical evidence of the bank lending channel that transmits a shock from the banks to the corporate borrowers. In particular, when banks experience a shock that limits their credit availability, the banks change their lending standards by raising the loan interest rate or allowing less loan amount for the new loans. Consequently, the borrowers who are not affected by the shock initially then receive the burden indirectly from the banks, facing increased borrowing costs. To investigate the existence of the bank lending channel, I use the dramatic oil price drop in late 2014 as a shock, which stressed banks' credit availability due to a large number of troubled oil loans that were accumulated since the Shale revolution in 2010. As a result, I find that the treatment banks - banks that are more exposed to the oil loans - charged higher loan interest rate to all borrowers after the oil price shock, than the control banks - banks that have less exposure to the oil loans - did. Furthermore, the difference in loan pricing between the treatment and control banks gets more prevalent among the firms who are bank-dependent which have limited access to external funds. These findings indicate that the banks propagate shocks to the firms through the bank lending channel and may raise the loan interest rates selectively to the bank-dependent borrowers. Moreover, I find that the bank-dependent borrowers from the treatment banks performed relatively worse, which implies that the limited access to the external funds makes the firms' performances more prone to the bank lending channel effects.

1.1 Introduction

Historically, bank losses have generally coincided with shocks to the corporate borrowers. If there is a shock to the financial condition of corporate borrowers, it is inevitable for the banks to face the counterparty risk. In other words, in case of a borrowing firm's default, banks experience losses for the unfulfilled debt obligation. If the shock has a larger scale, for example, an industry-wide shock, banks' credit availability will be at risk, which can be a concern for the stability of the financial system and the broader economy. As Brunnermeier (2009) and Shleifer and Vishny (2010) explained, bank losses from troubled loan contracts can reduce the supply of loans to the borrowing firms. Hence, if a shock damages the financial condition of a group of borrowers, banks may change their lending standards to compensate for their losses by raising the loan interest rate or allowing less loan amount. If the change in bank lending affects other borrowers who are utterly unrelated to the original shock, then it is reasonable to claim that there may exist a bank lending channel that transmits shocks from banks to their borrowers. Moreover, the bank lending channel can generate further real effects on the economy, for example, the bank-dependent borrowers might experience difficulties of obtaining external funds that can directly influence their investment activities. Hence, the objective of this study is to find any evidence of the bank lending channel and then investigate further consequences to the borrowers.

This study is related to the literature that searches for the evidence of the bank lending channel. As mentioned in Khwaja and Mian (2008), the identification of the loan supply effect is critical on finding the evidence of the bank lending channel and investigating the further impacts to the borrowers. In other words, to identify the bank lending channel, a decrease in loan supply must be the only cause of the shock transmission to the borrowers, not a change in loan demand that is associated with the change in aggregate demand. In order to isolate the supply effect from the demand effect, many empirical studies have used major economic shocks to explain the transmission mechanism in the bank lending channel. However, the identification strategies in these papers are still vulnerable to the possibility of

the demand effect because the economic shocks that they used include, or are followed by, the recessions. Hence, to overcome the identification challenge, it is ideal to study a shock that stresses the banks' credit availability, but does not bring the recession.

In this study, I use a non-financial shock that fulfills the identification strategy, which is the negative oil price shock in 2014Q3. This is a shock to an industry (oil industry) that is highly levered, as many oil firms were heavily financed by the bank loans after the Shale revolution that started in 2010. The increase in the default risk of the oil industry gave enough pressure to the banks so that we can reveal the existence of the bank lending channel. To check if the shock to the oil firms is a valid example to identify the supply effect, I do thorough research about the state of the oil production and the details of the loan syndication. For the identification, I modify the model from Santos (2011) and investigate how oil loan exposures can affect the corporate loan pricing. As mentioned, loan contracts to oil industry became riskier as oil price plunged. Therefore, using the oil price shock as an exogenous shock, I find how banks that are associated with these oil loans change their loan pricing compared to the banks who have less exposure to the oil loans. I hypothesize that the banks with high exposure to oil loans charged higher loan price after the oil price shock, than the less exposed banks did. This increase in bank loan pricing can be an evidence of the bank lending channel, where banks distribute and amplify the initial shock that they received from oil drilling firms. Subsequently, firms that are unrelated to the oil price change then possibly face new loan contracts with a higher interest rate, less amount allowed, and shorter maturity. This paper is distinguished from the literature for the fact that I find evidence of shock propagation even outside of recession window using a non-financial shock, which is a better identification strategy of the supply effect of loans.

In addition to finding the evidence of the bank lending channel, it is also important to understand the real effects on the borrowers. In the second part of this paper, I investigate how the borrowers from the oil-loan exposed banks performed after the oil price shock based on the measures mentioned above. I hypothesize that the firms who are tied to exposed banks perform worse than the firms who borrowed from non-exposed banks. I also expect

that this gap gets widened when bank-dependent borrowers are compared.

To test my hypotheses, I do a firm-level analysis using loan spread data from Thomson Reuters Dealscan, which is the loan interest rate that has to be paid to the lender in the fixed payment schedule. More specifically, I use the all-in-drawn spread over LIBOR rate that can be interpreted as the overall cost of the loan including both one-time and recurring fees associated with the loan. Using this data, I do a comparative case study of the loan spreads by estimating the average treatment effect (oil-loan exposure) on the treatment group (high-exposure banks) relative to the control group (low-exposure banks). After controlling for a large number of factors known to affect loan interest rates, I find strong evidence of an overall increase in loan spread for the treatment group more than the control group. Furthermore, I compare the treatment effect by bank-dependency proxied by bond market access, credit rating, and firm size. I find the banks' tendency of raising the loan spread after the oil price shock gets more prevalent to the firms who are bank-dependent. This result implies that the banks may exert informational advantage by selectively raising the loan spreads to the borrowers who have limited sources of external funds. Additionally, using synthetic control methodology in Abadie, Diamond, and Hainmueller (2010), I support my main result with more objective assignment of the treatment and control groups. Lastly, I report the difference in the firm performance between the borrowers of the treatment and control group. The remainder of this paper is organized as follows. Section 1.2 reviews the related literature and Section 1.3 explains the methodology used in this paper. Section 1.4 describes the data details and sample. Section 1.5 presents the empirical results and Section 1.6 shows robustness checks of the results. Finally, Section 1.7 concludes.

1.2 Literature Review

Many studies have argued about the existence of bank lending channel that is a potential route of the shock propagation. An extensive literature on finding empirical evidence of bank lending channel has been evolved by studying critical economic events that are associated with bank losses. Bernanke (1983) argued that bank failures in the 1930s reduced bank

lending, which played important propagation role of the Great Depression. Bernanke (1983) found the correlations of bank lending and aggregate measures of economic activity, but this evidence was questioned because it was difficult to tell whether the incident was driven solely by the loan supply shock. In other words, it could have been the opposite direction where the recession reduced the demand for loans, which resulted in less bank lending and more bank failures. In order to argue that the banks propagate shocks to the economy, it must be the loan supply shock that initiated the change in bank lending, not the recession that changed the demand for loans. So the identification of the supply shock has been the most critical issue in this literature.

Another notable economic event was the recession accompanied by the introduction of the Basel Accord in 1988 for setting the new capital standards (risk-based capital) for financial institutions, and many attempts were made to identify the supply effect. Yet, opinions still diverged as some argued that the new capital standards encouraged banks to cut lending that helped to bring the recession (see Thakor (1996); Peek and Rosengren (1995); Berger (1995)), while others argued that the recession caused the fall in loan demand so that banks were forced to cut lending regardless of capital standards (see Bernanke and Lown (1991); Berger and Udell (1994); Gorton and Rosen (1995)). Besides, studying the transmission mechanism of monetary policies to financial systems were also popular for finding the evidence of bank lending channel as in Gertler and Gilchrist (1994), Kashyap, Stein, and Wilcox (1993), and Kashyap and Stein (2000), but still, different conclusions arose as Ashcraft (2006) found no significant evidence.¹

In order to support the existence of the bank lending channel, the subprime crisis seemed to be an adequate economic event of identifying the bank lending supply shock. Many studies highlighted that although the subprime crisis started in the housing market, the primary driving force of the recession was a significant amount in bank losses. Including

¹Apart from relying on economic events, some applied various methodologies that can identify the supply effect of loans such as using bankruptcy filings data (see Hertzler and Officer (2012)), instrumental variables (see Paravisini (2008)), natural experiments (see Peek and Rosengren (2000); Ashcraft (2005)), and cross-sectional variation in liquidity supply (see Gertler and Gilchrist (1994)).

the bankruptcy of Lehman Brothers, many large banks were under-capitalized, and loan supply declined significantly. Ivashina and Scharfstein (2010) argued that banks with more deposit financing before the crisis cut their lending by less during the peak period of the crisis than banks with less deposit financing, which can be evidence of reducing the loan supply depending on the credit availability of a bank.² Contessi and Francis (2013) also reported that there was a contraction in credit between the third and the fourth quarters of 2008 based on the banks' balance sheet data. In contrast to these empirical works that support the supply effect, some studies including Chari, Christiano, and Kehoe (2008) argued that the bank lending actually increased, during the crisis.³ The differing views were partly explained in Cohen-Cole, Duygan-Bump, Fillat, and Montoriol-Garriga (2008) that the decline in new lending did not appear in the aggregate data because the use of loan commitments may have increased or because securitization had decreased. Santos (2011) explained these inconsistent results by the differences in data type used in these studies and accused lack of identification strategy for the supply effects. By using loan pricing data, Santos (2011) found that the banks with larger losses during the subprime crisis charged higher loan spread to their corporate borrowers. Santos (2011) argued that although loan pricing does not directly reflect a bank's credit availability, banks price the loans based on their financial condition thus is an essential proxy for credit availability. In an attempt to identify the supply effect, Santos (2011) also performs an instrumental variable approach to check whether the increase in loan spread was actually driven by the bank losses.

Although the subprime crisis seems like an adequate event to identify the supply effect, I discuss two possibilities that can distract the identification. First, the subprime crisis started in the housing market when the housing price bubble burst. The banks had great contributions building this bubble by excessive securitization of mortgages. So the bank losses

²They also reported that banks with more credit lines outstanding at the end of 2007 reduced their number of loans during the crisis by more than banks with less exposure to credit lines

³A. R. Mian and Sufi (2012) and A. Mian, Rao, and Sufi (2013) explained how the accumulated debt before the crisis dropped the aggregate demand, which caused further consequences of a decrease in consumption and increase in unemployment.

during the subprime crisis were partly due to the banks themselves, and the identification of the supply effect must be incomplete. In other words, banks' securitization of mortgages could have increased the loan demand with rising housing prices, but when the bubble faded away, a rapid fall in loan demand could have been one of the reasons for bank losses. Second, the subprime crisis was directly followed by the recession. So there is always a room for the demand side of the story. The recession could have dropped the loan demand that lowered the loan price and hence worsened the bank losses. Because of the economic recessions, the sample selection needs to be done with extreme caution, to avoid endogeneity in bank losses. In sum, it is difficult to identify the supply effect of the bank loans that can be the evidence of banks lending channel because of the high association between the bank losses and the recession. If we can find a shock that creates bank losses but does not carry the recession, there is a chance of identifying the effect of bank losses on shock propagation mechanism. In an attempt to find such shock, Chava and Purnanandam (2011) adopted the Russian crisis in 1998 as a shock to the U.S. banking system. Their results suggested that the global integration of the financial system can contribute to the propagation of financial shocks through the bank lending channel. However, the evidence from Chava and Purnanandam (2011) can also be challenged for the same reason: although the default on Russian sovereign debt may have no direct impact to the U.S. banking system, there were continuous events that can influence the demand side, such as abrupt capital outflow in Brazil and the collapse of the hedge fund giant LTCM. As the global financial markets became more interconnected, these sequence of events could have been the result of overall risk accumulation, which weakens the idea of the Russian crisis being a completely exogenous shock. To overcome this identification problem, I use a non-financial shock that meets our interest. I consider the negative oil price shock in 2014Q3, as a shock to the corporate borrowers (oil drilling firms), who are heavily financed by the bank loans. To test if the shock to the oil firms is a valid example to identify the supply effect, I do thorough research about the state of the oil industry.

In addition to finding the evidence of the bank lending channel, it is also important to

understand the real effects on the borrowers. Khwaja and Mian (2008) stated the difficulty of estimating the bank lending channel and the real effects to the borrowers simultaneously stems from the identification problem. Assuming the identification of loan supply effect is correctly done, several efforts were made to scope out the consequences of shock propagation through the bank lending channel, primarily by understanding the relationship between the banks' health and the borrowers' performance. In order to evaluate the borrower performance, various indicators were used such as firms' stock return information (Kang and Stulz (2000); Ongena, Smith, and Michalsen (2003); Chava and Purnanandam (2011)), macroeconomic indices (Bernanke and Blinder (1992)), cash-flow sensitivities (Kaplan and Zingales (1997)), and capital expenditures (Whited (1992); Almeida, Campello, Laranjeira, and Weisbenner (2012); Duchin, Ozbas, and Sensoy (2010); Kahle and Stulz (2013)). Furthermore, it is possible to observe a heterogeneous effect on borrowers depending on a firm's ability to manage the financial constraint and the accessibility to external funds (see Chava and Purnanandam (2011); Campello, Graham, and Harvey (2010); Khwaja and Mian (2008)).⁴

1.3 Methodology

In this section, I first check the validity of oil price shock as a supply shock to the corporate loans by observing the state of the oil industry and its financing structure. Second, I confirm that the syndicated loans have relevant features to observe shock propagation by the banks. This is done by reviewing the syndication process and studying the advantages over other financing options. Lastly, I describe the comparative case methodologies used in this study.

1.3.1 Overview of the Oil Industry

Since 2010, the total U.S. crude oil production was doubled to nearly 10 million barrels per day in 2015. This exceptional increase was driven by the new technologies "horizontal drilling" and "hydraulic fracking", which are used to extract Shale oil (also widely known as

⁴See also papers about the substitution effect: Becker and Ivashina (2015); Iyer, Peydró, Da-Rocha-Lopes, and Schoar (2014); Adrian, Colla, and Song Shin (2013); Leary (2009)

Tight oil) that allows nearly identical usage as the crude oil does. As illustrated in Figure 1.1, production of Shale oil reached nearly half of the total U.S. crude oil production in 2015. This increase of Shale oil production was made possible by its distinctive characteristic: unlike the conventional drilling methods, the discovery of the Shale sites can be shared by many firms, which can be interpreted as a reduction in R&D costs for a firm with no prior experience of finding drilling sites. Hence, many firms entered the industry and started projects, and they were heavily financed by debts. As Figure 1.2 illustrates, the fitted line of industry-wide leverage ratio shifts upward after 2010 Q1, which matches the time of an abrupt increase in the total crude oil production. So in recent years, the oil industry became highly levered since a lot of the Shale projects are financed by bank loans or corporate bond issues.

This sharp increase in the industry-wide leverage was not a huge problem when the crude oil was traded at around \$100 per barrel, which was well above the break-even point for the Shale oil drilling firms. However, starting from late 2014, oil price plunged to the level where Shale oil drilling was no longer profitable. After the oil price shock, concerns about the debt contracts of these firms became apparent. As the default risk of the drilling firms rose, lenders of these debt contracts faced the counterparty risk that can potentially lead to bank losses. Depending on how exposed they are to the oil debt, the credit availability of the banks was stressed. In order to hedge or de-risk for these potential losses, banks are expected to adjust their loan pricing policy. Hence the oil price shock is a valid example to investigate how banks adjust their lending activities when an exogenous shock is present to the corporate borrowers. Also, the fact that the oil price shock was never followed by the recession emphasizes the validity of this example for identifying the supply effect.

1.3.2 Overview of Syndicated Loans

A syndicated loan is a loan provided by a group of lenders that consists of several commercial or investment banks and institutional investors. This type of loan provides benefits to both borrowers and lenders. For borrowers, syndicated loans are less expensive and more efficient to administer than traditional bilateral loans. Lenders can benefit by sharing risk exposure

with other lenders while maintaining a relationship with the borrower. With these desirable features to both parties, syndicated loan market has expanded and established efficient syndication process. First of all, an issuer (borrower) awards a mandate to an arranger (lender) bank that initiates the syndication process. Then the arranger launches a loan deal at a target spread or a range of spreads, which attracts the investors to make commitments. In the end, the arranger finalizes the price of the loan based on the investor commitments. In most of the cases, the loan price is displayed as "All-in-drawn" spread. This spread is calculated by the sum of interest margin charged to borrowers on the draw portion of the loan and annual facility fee, subtracted by the LIBOR rate. Since some loans are credit facilities that allow the borrowers to access the credit line at their convenience, it is important to use All-in-drawn spread in order to take only the used portion of loans into account. Although the All-in-drawn spread does not directly reflect the credit availability of a bank, it is a good proxy because the banks price their loans based on their financial conditions.

1.3.3 *Difference-in-Difference*

The main methodology used in this paper is the difference-in-difference panel regression with fixed effects. To investigate whether the banks with high exposure to oil loans charged higher loan spread than the banks with low exposure, I assign banks with high exposure as a treatment group and banks with low or no exposure as a control group. Unlike experimental studies, the definition of treatment is relatively unclear in this case, so I put much attention on how to measure the exposure to oil loans, which I will discuss more in detail in the later section. Also, in order to see how the oil price fall affected the bank lending activities, I consider late 2014 (2014Q4) as the start of the oil price shock. This setting allows us to compare the change in loan spreads between the two groups before and after the oil price shock. My main regression equation inspired by Santos (2011) is as follows:

$$LOANSPREAD_{l,b,t} = \alpha + \beta_1 TIME_t + \beta_2 TREATMENT_b + \beta_3 TIME_t \times TREATMENT_b$$

$$+ \sum_{k=1}^K LOAN_{k,l,t} + \sum_{i=1}^I BANK_{i,b,t-1} + \sum_{j=1}^J FIRM_{j,f,t-1} + \varepsilon_{f,t}$$

$LOANSPREAD_{b,f,l,t}$ is the All-in-drawn spread over LIBOR of loan l with bank b lending to the borrowing firm f at time t . $TIME$ is a dummy variable indicating whether the loan was made after the oil price shock and $TREATMENT$ indicates whether the loan was offered by the bank with high exposure to oil loans. To be consistent with my hypothesis, I expect β_3 to be a positive number. In order to take other factors that change loan spreads into account, I include specific control variables for each loan, bank, and firm characteristics named as $LOAN$, $BANK$, and $FIRM$ respectively. I follow the specification of control variables as in Santos (2011).

Loan controls are mostly the characteristics of each loan contract that can be obtained from Dealscan data. Since the loan spreads can be set very differently depending on the amount of lending, length of maturity, type of loan, and any restrictions that each deal possesses, I include the following control variables. Loan amount and the loan maturity would be the most critical determinants of a loan spread, but there are trade-offs in both categories. For example, a deal with a large amount with long maturity may represent more credit risk, but may also imply the creditworthiness of a borrower. So other characteristics must be included such as dividend restrictions, seniority of the loan, and whether the loan is secured. If a loan has a dividend restriction clause, is a senior, and secured, the loan is safer compared to the opposite, and the banks would usually require these conditions when they assess the borrower as a risky firm. Also, Dealscan provides the purpose of a loan including corporate purposes, debt repayment, working capital. Two major types of loans are also included whereas the line of credit allows a firm to access the fund when needed, and term loan grants the amount of loan deal to a firm for the fixed amount of time.

Bank controls are also determined by the relevance of a bank's characteristics to the loan spread. Total assets, capital-to-assets, return on assets, and volatility of return on assets are

included to capture the banks' financial condition. In addition, banks also face some costs when signing a loan deal. In many cases, banks use the deposits to lend the loan amount to corporate borrowers. So the cost of funds is captured by total deposits to assets ratio. Also, the liquidity of assets is included, since it also represents the cost of funds that banks have. As done in the previous studies, I also consider the relationship between the lender and the borrower. By doing this, we might be able to see whether banks charge lower loan spread for the firms that they have a relationship with previously, or charge higher loan spread for having an information advantage.

Lastly, firm controls are selected by the importance of a variable affecting a firm's overall risk when a bank assesses how much loan spread to charge to the firm. A firm's age and total sales can explain the reputation and the performance of the firm at which both can help to get lower loan spread charged from banks. While the age and sales can measure the general risk of the firm, other risk factors can affect the firm's debt financing. As noted in Denis and Mihov (2003), interest coverage is a good measure of a firm's ability to meet its debt obligation, which might be the most important variable for the banks determining the loan spread. Leverage can tell us how likely the firm's default is, so higher leverage can cause higher loan spread. Profit margin also reveals the firm's ability to secure the debt repayment and maintain the relationship. Furthermore, banks would also consider what would be the remaining value of a firm in case of the firm's default, so tangible assets, R&D, working capital, and market to book ratio are included. Lastly, apart from the risk factors mentioned above, some forward-looking measures like the firm's excess return and volatility fill the remaining factors that can influence the default risk.

To capture remaining unobserved characteristics after including all the control variables mentioned above, I use fixed effect at the firm level so that each firm has its own intercept term. Also, to avoid biases when estimating standard errors, I follow Petersen (2009) for using clustered standard errors at the firm level. This way, I can account for the residuals that may be correlated across firms in panel data, and hence do not underestimate standard errors.

1.3.4 *Definition of Treatment & Control Group*

Unlike experimental studies or policy intervention analyses that have a clear idea of who is being treated and who is being controlled, this study deals with a more ambiguous setting. Ideally, banks with exposure to oil loans can be assigned to the treatment group, and banks with no exposure to oil loans can be the control group. However, most of the banks in the sample have some exposure to oil loans, so there should be a reasonable measure to gauge how high is the high exposure and how low is the low exposure. I define the exposure to oil loans as how much a bank is involved in lending to oil producers relative to the other banks. Using the Dealscan data, I can calculate the total amount of loans lent to the oil industry using SIC code. Although it is an approximate measure, it is a reasonable proxy to the exposures. The left column of Table 1.1 lists banks ordered by oil loan amount in a million dollars. If I use, for example, top 10 banks from the list as a treatment group and the rest as a control group, the analysis would be comparing large banks to relatively smaller banks, not by the exposures possibly violating the parallel assumption of the (DID) estimator.

In order to overcome the problem of high correlation between oil loans and bank sizes, I compute a ratio by dividing the total oil loan amount by all loan amount. As Carey and Hrycray (1999) stated, Dealscan covers 50-75% of all commercial and industrial loans in the U.S., which is still an imperfect measure, but it provides a reasonable proxy for the oil loan exposure. The right column of Table 1.1 lists banks ordered by the proportion of oil loans to the entire loan portfolio. Still, where to draw a cut-line for the treatment group is challenging. First, I assign all banks with more than 4% exposure to the treatment group and the rest to the control group. Since many banks in the control group do not have oil loans at all, this can be a good division between high exposure and low exposure to oil loans. In order to check if this is a reasonable assignment, I do a thorough robustness check in the later section. Moreover, not only dividing the sample into two groups (high and low exposure groups), it might be a better strategy to exclude some banks in the middle to magnify the difference in exposures. For this strategy, I assign rank 1-15 banks to the treatment group

and rank 39-64 banks to the control group. Similarly, the robustness check is performed in the later section.

1.4 Data

1.4.1 Loan Pricing Data / Balance Sheet Information

The main data I use in this study is TR Dealscan data of syndicated corporate loans. The data set includes information about loan deals, borrower information, and lender information. Each loan deal is recorded as a loan facility, which is a unique relationship between a firm and a bank. In order to obtain balance sheet information for the firms and the banks, I use Compustat quarterly data. Since there is no common record between Dealscan and Compustat, merging two data involves a lot of manual matching process. Fortunately Chava and Roberts (2008) provides a linking table, so I use their linking table and add more matches by my own manual matching algorithm that is described in the appendix. Additionally, CRSP daily stock price data is used to compute forward-looking measures, excess return, and volatility.

1.4.2 Sample Characterization

The sample period is from 2010Q1 to 2017Q4, which is selected to exclude the influence from the financial crisis and to include major oil price shock. However, more importantly, this sample period includes all the Shale Revolution era. After merging all the data, I have 21,317 unique loan deals. Also, there are 2,207 public firms and 64 banks in the sample, and the unique number of bank-firm matches are 27,171 pairs. I include not only the U.S. banks but also non-U.S. banks as long as the country of syndication is in the U.S. and they have balance sheet information to control for bank-specific characteristics.

Table 1.2 is a descriptive statistics of the regressors that are used in the regression. As explained in the methodology section, each of the variables is selected by the specific

characteristics of the borrowing firm, the lending bank, and the loan contract that can affect the pricing of a loan.

1.5 Results

In this section, I present multiple results indicating the evidence of bank lending channel. I first present difference-in-difference regressions with different treatment groups. Then I perform robustness check by changing treatment group settings and the shock period for further analyses. Lastly, I report how the treatment effect appears to the borrowers with limited access to funds such as borrowers who are bank-dependent, have a low credit rating, and are small sized.

1.5.1 Difference-in-Difference (No exclusions)

The first result in Table 1.3 presents the main regression result with no exclusion in treatment group selection. Model (1) contains only dummy variables for difference-in-difference, Model (2) includes firm-specific control variables, Model (3) adds bank-specific control variables, and Model (4) has all control variables. All models have firm fixed effects and standard errors are clustered and the firm level.

As expected, all models have positive and statistically significant coefficients for the DID variable. This can be interpreted as, for example in Model (4), the loan spread of the treatment group pricing 5.690 basis points higher than that of the control group. The signs and the significance for other coefficients mostly matches our expectation that is described in the methodology section. Model (4) confirms that all else equal, the age and the sales of a firm reduce the loan spread since an old firm with high revenue would be considered as a credible borrower to the banks. If a firm is highly levered and has a high market to book ratio, it is likely that the firm will be charged higher loan spread. Another notable factor would be the stock volatility, as the loan spreads increase with the stock volatility of a firm. This is because the banks consider a firm with a volatile stock price movement to have some additional sources of risk, so they charge higher loan spread.

1.5.2 *Difference-in-Difference (With Exclusions)*

Table 1.4 shows same regressions as Table 1.3 but uses different treatment group. In order to maximize the difference between the treatment and control group, I exclude banks in the middle ranked from 16 to 38.

All four models report positive and significant coefficients, and more importantly, coefficients are larger than the ones from Table 1.3, which indicates that the exclusion strategy can help identifying the treatment effect. As a result in Model (4), the treatment group (firms who borrowed from highly exposed banks) experienced 7.119 basis points higher loan interest, and that is \$700K more on average and \$16 billion more in total, than the treatment group after the oil price shock.

So after controlling for multiple factors that affect the loan pricing, table 1.4 provides evidence of the loan contracts with highly exposed banks were charged higher spread than the ones with less exposed banks after the oil price shock. This coincides with the earlier hypothesis that after the oil price shock, highly exposed banks observe the increase in default risk of their borrowers and make adjustments to their loan policy.

1.5.3 *Robustness Tests*

As discussed earlier, the treatment effect (oil loan exposure) does not clearly divide the sample into exposure group and non-exposure group because many banks have some amount of oil loans. So for the result in Table 1.3 I set an arbitrary cut at 4% oil loan exposure to divide the banks into two groups. Moreover, in Table 1.4, I exclude some banks with medium exposures to magnify the difference between the treatment group and the control group. These treatment group assignments can be a concern in terms of its robustness. So to check my model's robustness, I run my main regression for all possible cut location.

Figure 1.3 plots the DID coefficients of the main regression equation scaled by the left vertical axis. The horizontal axis represents how many banks were included in the treatment group. For example, if a plotted dot is lies at 10 on the horizontal axis, it means that that

the cut was located at rank 10 and top 10 banks were assigned to the treatment group. Light green vertical lines describe the proportion of the treatment group in all banks. Similarly, the dark green lines present the proportion of the control group and ranks are indicated on the right vertical axis. From this figure, we can see that significance starts from rank 32 and persists until rank 46. This trend is continuous so it is possible that the distinction of exposures that allows a significant treatment effect occurs when we have top 32-46 banks in the treatment group. Moreover, this trend gets more robust if we take a look at cases with exclusions of medium exposure banks.

The dashed vertical lines in Figure 1.4 represents the portion of medium exposure banks that were excluded. For each treatment group cases, I move the bottom cut location and select results that require the least exclusion. We can quickly notice that significance for all treatment group cases increases and the coefficients form a flat trend that is continuously significant from rank 5 to rank 56. Also, the dark green vertical lines have similar lengths from rank 5 to rank 40 and they indicate that the control group contains rank 39 and below firms. Therefore it is reasonable to set the bottom cut to rank 39.

Additional robustness check is performed to see how the treatment effect changes with varying shock periods. The sharp fall in oil price started in 2014Q3. However, this low oil price continued until 2015Q4, so when exactly the oil firms started to default is unclear. I used 2014Q4 because I believe the shock transmission could have some lags in order to appear in balance sheet information. To confirm the robustness of this empirical exercise, I perform placebo estimation by using different dates to see the treatment effect as if the shock occurred on the date.

Supported by Table 1.5, setting 2014Q4 as the shock period is reasonable because it has the highest significance and highest coefficient. Moreover, we can observe the significance fading away as we move towards 2015Q4 and the coefficient getting smaller.

1.5.4 *Difference-in-Difference (More Borrower Characteristics)*

Next, I test whether banks have different loan pricing plans for the borrowers. As noted in Santos (2011), banks tend to have an informational advantage on borrowers. Throughout the repeated lending relationships, banks can accumulate information about the borrowers' financing structure and other characteristics that are related to the financial condition. First, I investigate whether the accessibility to the bond market is one of the information rent that banks have so that they charge higher loan spread to the firms who do not have corporate bond issues. I classify firms who do not have bond issues in three years prior to the loan as bank-dependent borrowers. Second, if a firm's credit rating is low, the firm will eventually rely more on alternative funding sources other than corporate bonds, which may lead to more bank loans and to an increase in bank-dependence. I filter firms with investment-grade and non-investment-grade credit rating to see if the treatment effect is different. Last, I look at whether firm size is another factor for the bank-dependency. Small firms tend to have less access to external funds so I compare the treatment effect of firm size below 25% quantile (small firms) and above 75% quantile (large firms).

Table 1.6 allows us to compare the difference of the treatment effects by dependency on bank loans. Model (1) is a sub-sample of firms who do not have access to the corporate bond market and Model (2) is a sub-sample of firms who have issued the corporate bonds during the previous year. Model (3) filters the firms with bad credit rating (high-yield bonds with S&P rating BB+ and below) and Model (4) includes firms with good credit rating (investment grade bonds with S&P rating BBB- and above). Lastly, Model (5) filters for the firms with the smaller size (measured by total assets) than 25% quantile and Model (6) is for the firms with the larger size than 75% quantile.

The results from Model (1) and Model (2) indicate that the DID coefficient is statistically significant for the borrowers without the access to the bond market while the coefficient for the borrowers with bond issues is not. So among bank-dependent borrowers who do not have bond market access, treatment group experienced 5.606 basis points higher loan spread

(\$520K on average). This can be a shred of evidence that oil loan exposed banks selectively increase the loans to bank-dependent borrowers. In Model (3) and Model (4), DID coefficient is only significant for the firms with bad credit rating. The treatment group experienced 19.38 basis points higher loan spread, that is \$1.9 million more on average. Although the causal relationship is difficult to tell whether the banks charged higher loan spread to firms with bad credit rating or bad credit rating firms happened to be charged higher spread since they are considered as riskier firms, my hypothesis is still valid because the treatment effect is more prevalent in the bad credit rating firms and the treatment effect is not significant for good credit rating firms. In other words, banks may have selectively raised the loan spread for bad credit rating firms, knowing that those firms will experience difficulties of raising additional funds from corporate bonds at a desirable interest rate. Lastly, according to Model (5) and (6), the disparity of the treatment effect on firm sizes implies that the selective increase in loan spread to the bank-dependent borrowers gets more severe for the small firms. Because small firms tend to have limited funding sources and rely more on bank loans, the banks may have informational rent on these borrowers. This result can be another evidence to support the selective loan pricing by the oil loan exposed banks on bank-dependent borrowers.

Furthermore, I apply the same strategy on loan amounts. Together with loan spreads, loan amounts is one of the critical components in the loan contracts. Using loan amounts as the dependent variable, I do the DID estimation as observed in Table 1.7. In line with loan spreads analysis, the treatment group was allowed less loan amount per deal. On average, the treatment group had 13.6% less loan amounts than the control group after the oil price shock. This indicates that not only the loan spreads were increased, but also loan amounts were limited. Both of the evidence can be interpreted as the increased borrowing costs for corporate borrowers. In Chapter 2, I investigate the real effects on the borrowers due to an increase in borrowing costs. Table 1.7 also presents different combination of control variables indicated in the bottom. Throughout different model specifications, DID coefficients are significant and have the same directions. Although I presented Model (4) in Table 1.7, this regression may suffer from endogeneity issues due to the inclusion of loan-specific control

variables. So for the following results, I use Model (3) as the baseline specification.

In Table 1.8, we see that the difference in loan amounts allowed to the treatment group is more prevalent in the group of bank-dependent borrowers. For example, the borrowers with no bond market access were allowed 29.5% lower amount from the treatment banks. On the other hand, the borrowers with access were allowed only 11% lower amount than the control group, which is even statistically insignificant. This pattern persists in other measures of bank-dependency, which clearly indicates the oil price shock was very similar to a credit supply shock that limits the banks' credit availability. This result is consistent with other papers in the literature that uses loan amounts as the key variable (see Khwaja and Mian (2008); Bidder, Krainer, and Shapiro (2018)).

1.6 Robustness Checks

1.6.1 Synthetic Control Method

The Difference-in-Difference model assumes the parallel trends between the treatment group and the control group. If the treatment is randomly assigned, then this assumption will always hold. However, this is not the case for the majority of intervention analyses, so we often select a control group for which we think the parallel assumption should hold. As seen in the left column of Table 1.1, since banks with high exposure to oil loans tend to be much larger than the banks with low exposure, typical Difference-in-Difference assumptions are unlikely to hold. That is why I calculated the ratios on the right column of Table 1.1 to avoid any violations of the assumptions. If we use the Synthetic Control (SC) model by Abadie et al. (2010), this problem is easily alleviated by reweighting the control group banks to match bank characteristics as well as the loan spread trends. In an attempt to remove subjective control group assignment, Abadie et al. (2010) introduced the SC method that chooses covariate weights to minimize the pre-treatment prediction error of the outcome variable and then rebalances control observations according to these weights. Suppose that we observe $J + 1$ banks. Without loss of generality, suppose also that only the first bank in

the panel is exposed to the oil price shock so that we have J remaining banks as potential controls. Following Abadie et al. (2010), let Y_{it}^{NS} be the loan spreads (or other outcome variables) that would be observed for bank i at time t in the absence of the oil price shock, for banks $i = 1, \dots, J + 1$, and time periods $t = 1, \dots, T$. Let T_0 be the number of periods before the shock, with $1 \leq T_0 < T$. Let Y_{it}^S be the loan spreads that would be observed for bank i at time t if bank i is exposed to the oil price shock during the periods from $T_0 + 1$ to T . Assuming that the oil price shock has no effect on the loan spreads during pre-shock period $t \in \{1, \dots, T_0\}$ and all $i \in \{1, \dots, J + 1\}$, we have that $Y_{it}^S = Y_{it}^{NS}$.

Let $\alpha_{it} = Y_{it}^S - Y_{it}^{NS}$ be the effect of the oil price shock for bank i at time t and let $D_{i,t}$ be an indicator that has a value one if bank i is exposed to the shock and value zero otherwise. So the observed outcome for bank i at time t is

$$Y_{it} = Y_{it}^{NS} + \alpha_{it}D_{it}$$

and since only the first bank ($i = 1$) is exposed to the shock after period T_0 , we have that

$$D_{it} = \begin{cases} 1, & \text{if } i = 1 \text{ and } t > T_0 \\ 0, & \text{otherwise} \end{cases}$$

The treatment effect we want to estimate is $(\alpha_{1,T_0+1}, \dots, \alpha_{1T})$, which is the time varying treatment effect of the oil price shock on the loan spreads. For $t > T_0$,

$$\alpha_{1t} = Y_{1t}^S - Y_{1t}^{NS} = Y_{1t} - Y_{1t}^{NS}$$

Since Y_{1t}^S is observed, we just need to estimate Y_{1t}^{NS} to estimate α_{1t} . Suppose that Y_{it}^{NS} is given by a factor model:

$$Y_{it}^{NS} = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \varepsilon_{it}$$

where δ_t is unknown common factor, Z_i is a $(r \times 1)$ vector of observed predictors for the loan spread that are not affected by the oil price shock, θ_t is a $(1 \times r)$ vector of unknown parameters, λ_t is a $(1 \times F)$ vector of unobserved common factors, μ_i is an $(F \times 1)$ vector of

unknown factor loadings, and ε_{it} is the unobserved transitory shocks at the bank level with zero mean.

Consider a $(J \times 1)$ vector of weights $W = (w_2, \dots, w_{J+1})'$ such that $w_j \geq 0$ for $j = 2, \dots, J+1$ and $w_2 + \dots + w_{J+1} = 1$. Each value of W is a weight on a control bank that is contributed to form a synthetic control when computing the weighted average. Suppose that there are $(w_2^*, \dots, w_{J+1}^*)$ such that

$$\begin{aligned} \sum_{j=2}^{J+1} w_j^* Y_{j1} &= Y_{11} \\ &\vdots \\ \sum_{j=2}^{J+1} w_j^* Y_{j,T_0} &= Y_{1,T_0}, \\ \sum_{j=2}^{J+1} w_j^* Z_j &= Z_1 \end{aligned}$$

The system of equations above, can hold exactly only if $(Y_{11}, \dots, Y_{1,T_0}; Z_1')$ belongs to the convex hull of

$$\left[\left(Y_{2,1}, \dots, Y_{2,T_0}; Z_2' \right), \dots, \left(Y_{J+1,1}, \dots, Y_{J+1,T_0}; Z_{J+1}' \right) \right]$$

Usually, no set of weights exists to let the system of equations hold exactly in practice, so the synthetic control bank is selected to make the equations hold approximately. As an estimator of α_{1t} for $t \in \{T_0 + 1, \dots, T\}$, Abadie et al. (2010) uses

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$$

The optimization process for finding the optimal weights $(w_2^*, \dots, w_{J+1}^*)$ can be described as follows. Let $T_1 = T - T_0$ be the number of post-shock periods. Suppose Y_1 be the $(T_1 \times 1)$ vector of post-shock loan spreads for the exposed bank and let Y_0 be the $(T_1 \times J)$ matrix of post-shock loan spreads for the potential control banks. The linear combination of pre-shock loan spreads, $\bar{Y}_i^K = \sum_{s=1}^{T_0} k_s Y_{is}$ is computed by a $(T_0 \times 1)$ vector $K = (k_1, \dots, k_{T_0})$. Consider M of such linear combinations defined by the $(T_0 \times 1)$ vectors K_1, \dots, K_M and let

$X_1 = (Z'_1; \bar{Y}_1^{K_1}, \dots, \bar{Y}_1^{K_M})'$ be a $(k \times 1)$ vector of pre-shock loan spread linear combinations and loan spread predictors, where $k = r + M$. Similarly, let X_0 be a $(k \times J)$ matrix that contains the same variables for the banks with no exposure to the shock (j -th column of X_0 is $(Z'_j; \bar{Y}_j^{K_1}, \dots, \bar{Y}_j^{K_M})'$).

To measure the discrepancy between X_1 and X_0W , Abadie et al. (2010) employed

$$\|X_1 - X_0W\|_V = \sqrt{(X_1 - X_0W)'V(X_1 - X_0W)}$$

where V is some $(k \times k)$ symmetric and positive semidefinite matrix. Finally, the optimal weights $(w_2^*, \dots, w_{J+1}^*)$, are chosen by minimizing the above distance subject to $w_j \geq 0$ for $j = 2, \dots, J + 1$ and $w_2 + \dots + w_{J+1} = 1$.

Statistical inference aspect of Synthetic Control (SC) is quite limited because large-sample asymptotic inference is not possible. So Abadie et al. (2010) suggested the use of Permutation Methods for inference. First, we estimate a placebo treatment effect $\hat{\alpha}_{1,l}^{Placebo}$, where $l \in \{T_0 + 1, \dots, T\}$, for each unit in the control banks. Then we can calculate an empirical p-value for the effect estimated on the treatment bank:

$$p\text{-value}_l = Pr(\hat{\alpha}_{1,l}^{Placebo} < \hat{\alpha}_{1,l}) = \frac{\sum_{j=2}^{J+1} I(\hat{\alpha}_{1,l}^{Placebo(j)} < \hat{\alpha}_{1,l})}{J}$$

where $\hat{\alpha}_{1,l}^{Placebo(j)}$ is the l -specific effect of a disaster when control bank j is assigned a placebo exposure to loans at the same level as bank 1.⁵

Meanwhile, there are two challenges in adopting the Synthetic Control (SC) method suggested by Abadie et al. (2010) in this paper. First, the Synthetic Control (SC) model requires the panel data to be perfectly balanced in order to find optimal weights to minimize the mean squared prediction error. The main data I used in this paper (more details in Section 1.4), is syndicated loan origination data from TR Dealscan. After merging with firm and bank characteristics (balance sheet information), each observation is a loan deal that contains the information about the borrower, the lender, and the loan specific details. Since

⁵See Abadie, Diamond, and Hainmueller (2015) for using Root Mean Squared Prediction Error to resolve erroneous inferences problems when the synthetic control fit is bad.

the loan deals hardly get originated each quarter or each year, the panel data I use in this paper is unbalanced. So the Synthetic Control (SC) may not be the perfect model to analyze the treatment effect on loan spreads but may be useful for other outcomes variables such as stock returns, balance sheet variables, and other variables that can construct a balanced panel. However, to support the results from Difference-in-Difference, I construct a balanced panel of approximated loan spreads by taking an average of all loan spreads outstanding before loan maturity at each point of time for each bank. The second challenge in adopting SC is that Abadie et al. (2010) suggested only one treated unit and multiple control units while many comparative case studies require multiple treated units including this paper.

To extend the model from Abadie et al. (2010), several recent methods have been developed which allow statistical inference with multiple treated units. Cavallo, Galiani, Noy, and Pantano (2013) made inference about the dynamics of the average treatment effect using multiple treated units. Following their method, under our setting, I calculate the estimated average effect for top G banks who are exposed to the oil price shock:

$$\bar{\alpha} = (\bar{\alpha}_{T_0+1}, \dots, \bar{\alpha}_T) = \frac{1}{G} \sum_{g=1}^G (\hat{\alpha}_{g,T_0+1}, \dots, \hat{\alpha}_g^T)$$

To conduct a valid inference for $\bar{\alpha}$, I follow the steps described in Cavallo et al. (2013). For each exposed bank g , I compute all the placebo effects using the available control banks. At each lead $l \in (T_0 + 1, \dots, T)$, I select one placebo estimate corresponding to each disaster g and take the average across the G placebos. I do this step for all possible combinations of placebo averages. I compute the number of all possible combination as: $N_{Placebo} = \prod_{g=1}^G \text{Number of Control Banks}$. Finally, I compute the l specific p -value for $\bar{\alpha}_l$:

$$p\text{-value} = \frac{\sum_{p=1}^{N_{Placebo}} I\left(\bar{\alpha}_l^{Placebo(p)} < \bar{\alpha}_l\right)}{N_{Placebo}}$$

In Section 1.5, I report results using the Synthetic Control (SC) method on loan spread data.

1.6.2 Synthetic Control Results

As mentioned in the previous section, the Synthetic Control (SC) method might not be the best technique to adopt in this study because of two challenges. First, the panel data in this paper is not balanced. Second, the number of treated units I hypothesize is larger than one, which does not fit the setting from Abadie et al. (2010). The second problem can be resolved if I use the technique developed in Cavallo et al. (2013), but the first problem cannot be resolved easily because the main panel is a loan origination data. So I construct a new panel for banks that is completely balanced. For each bank, I filter all the loan contracts originated by the bank and filter outstanding loan deals for each period. Then I calculate the average loan spreads for each period, as well as the averages of other balance sheet variables that are used in the Difference-in-Difference (DID) regression as independent variables.⁶ In order to check the robustness of my DID results, I first apply the SC method on rank 1-15 banks as treated units and rank 39-64 banks as control units. The right panel of Figure 1.5 plots the average loan spreads for the treatment banks (rank 1-15) and the average loan spreads of the Synthetic Control Bank formed by banks with rank 39-64. The left panel of Figure 1.5 presents the differences between the treatment and control group.

According to Figure 1.5, although the treatment group has higher loan spreads after the shock, the pre-shock SC fit is not good enough to claim that the treatment effect is identified by the Synthetic Control bank, so this specification does not provide evidence to test the robustness of DID results. This may be due to the discrepancy between the characteristics of the treatment banks and control banks. For example, a small bank that is ranked high in the right column of Table 1.1 should be matched with small banks with low ranks as a potential control group in order to have a good SC fit, but the treatment group and the control group assignment according to the right column of Table 1.1 are mixed groups of small and large banks. So I use the left column of Table 1.1 as a measure of exposure to oil price shock.

⁶Each observation of this new panel contains the averages of the loan-specific variables (loan spreads, amounts, maturity, etc.) for the loans that are alive at the time, the averages of the firm-specific variables for the borrowers at the time, and bank-specific variables at the time.

Although having larger banks in the treatment group can violate Difference-in-Difference assumption, Synthetic Control (SC) alleviates the problem by reweighting the control group banks to match bank characteristics as mentioned in Cetorelli and Traina (2018). Figure 1.6 illustrates the SC result using oil loan amount as a measure of exposures. Treatment group contains rank 1-13 banks from left column of Table 1.1 and the control groups have rank 41-64 banks.

As a result, changing the exposure criteria improved the pre-shock SC fit significantly and the average loan spread for the treatment group banks after the shock is much higher than that of the Synthetic Control bank. However, to make a statistical inference for this specification requires a high computing power to conduct placebo tests.⁷ So I choose rank 1-7 banks as the treatment banks and rank 41-64 banks as the control banks.⁸ Figure 1.7 illustrates how the placebo test is conducted and the corresponding p -values. For each treatment bank, for example rank 1 bank, the average treatment effect is estimated with the control banks with ranks 41-64. Then I select rank 41 as a placebo treatment bank with replacing rank 1 to the control banks and estimate the treatment effect. I do this until all control banks received the placebo treatment and moved on to the next actual treatment bank and repeat same procedure. So for each of 7 treatment banks, 14 placebo effects are estimated and in a total of 16^7 permutations. Each gray line in the left panel of Figure 1.7 represents one of 16^7 .⁹

At each post-shock period l , I locate the actual average treatment effect $\bar{\alpha}_l$ (red line in Figure 1.7 left panel) from all placebo average treatment effect $\bar{\alpha}_l^{Placebo(p)}$ (gray lines) and find the probability of a placebo effect exceeding the actual treatment effect. The p -value

⁷In total, 16^{13} iterations (Number of Control Banks^{Number of Treatment Banks})

⁸I also tried using rank 51-64 banks because banks with rank below 51 do not have oil loan amounts at all. This allows us to study the treatment effect with a more precise selection of control banks. However, having fewer control banks decreases the accuracy of inference, so I use rank 41-64 banks as control banks and this is a reasonable assignment since the difference in levels of exposure between the treatment (1-7) and the control (41-64) is notable.

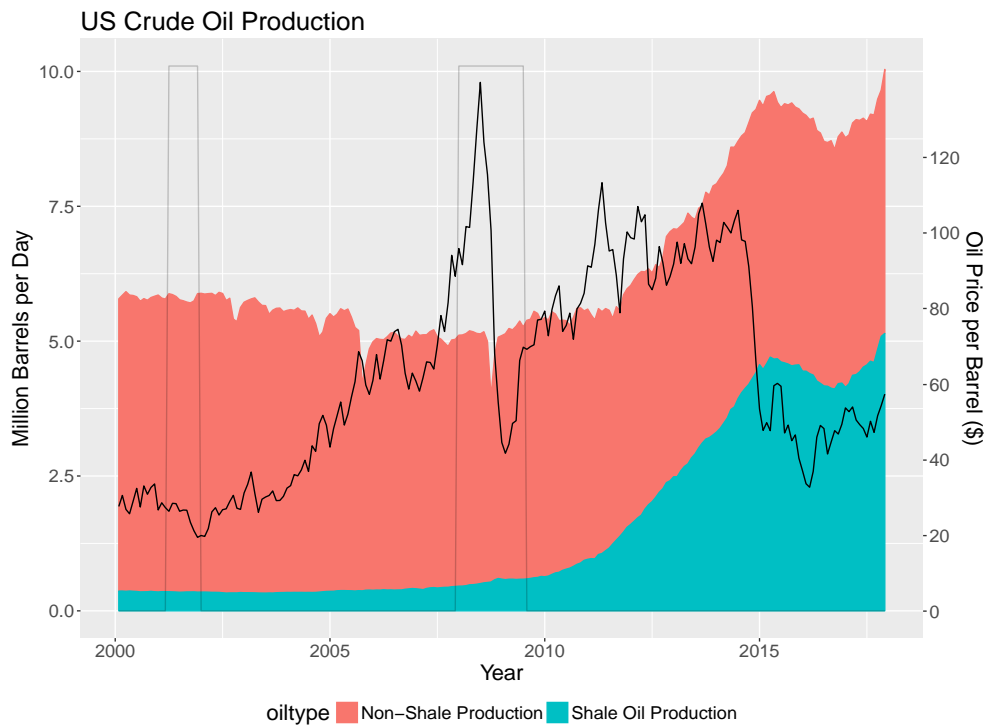
⁹I excluded all the noisy placebos to have a better visualization but the figure presents a brief description of the placebo distributions.

suggests that about 10% of placebo effects exceed the actual treatment effect from 2014Q4 to 2016Q1. For later periods, the p -values decrease along time to nearly 5%. As a result of the permutation test, banks who rank 1-7 on oil exposures (by oil loan amounts) raise the loan spread more than the control banks after the oil price shock with 90% confidence approximately. One of the limitations of the permutation test is that it requires a heavy computation so it is difficult to test the robustness by varying the settings. Although the loan spread data was artificially balanced, Figure 1.7 displays consistent result as in DID case.

1.7 Conclusion

Unlike other industries, the oil industry is very vulnerable to the price shocks that are associated with the global demand and global supply of crude oil. The likelihood of banks being affected by the oil price shock has been increased since the introduction of Shale oil because many firms at the Shale sites relied heavily on the bank loans. For banks, due to the lender-borrower relationship with the oil industry, the oil price shock appears as a direct exogenous shock to their credit availability and they immediately adjust their lending standards by charging higher loan spread to new loan deals, to hedge for potential losses. I argue that these adjustments cause a shock transmission via bank lending channel since the borrowing firms with no relation to crude oil are charged higher loan spreads after the oil price shock. Here, the banks with high exposure to oil loans play the role of amplifying the original shock and distributing the risk associated with troubled oil firms. Indeed, my empirical results suggest that not only the oil firms, but all firms which borrowed from the banks with high exposure to oil loans were charged higher loan spread after the oil price shock, than the firms funded by the banks with low or no exposure to oil loans. Also, I found that the banks tend to raise the loan spreads selectively to the borrowers that have limited access to external funds. By using unique financing structure of the oil industry, this study contributes to the literature finding the evidence of shock propagation in the bank lending channel with better identification of the supply effect of loans.

Figure 1.1: Total U.S. Crude Oil Production and WTI Oil Price (2000Q1-2017Q4)



Note: Green shaded area represents the total U.S. crude oil production by Shale oil, and red shaded area represents the total U.S. crude oil production by conventional drilling method. Both follow the left vertical axis, which is the total U.S. crude oil production in a million barrels per day. Solid black line is the WTI crude oil spot price, and it follows the right vertical axis in dollars per barrel. (Source: www.eia.gov)

Figure 1.2: Oil Industry Leverage (debt/equity) vs. WTI Oil Price (per barrel)

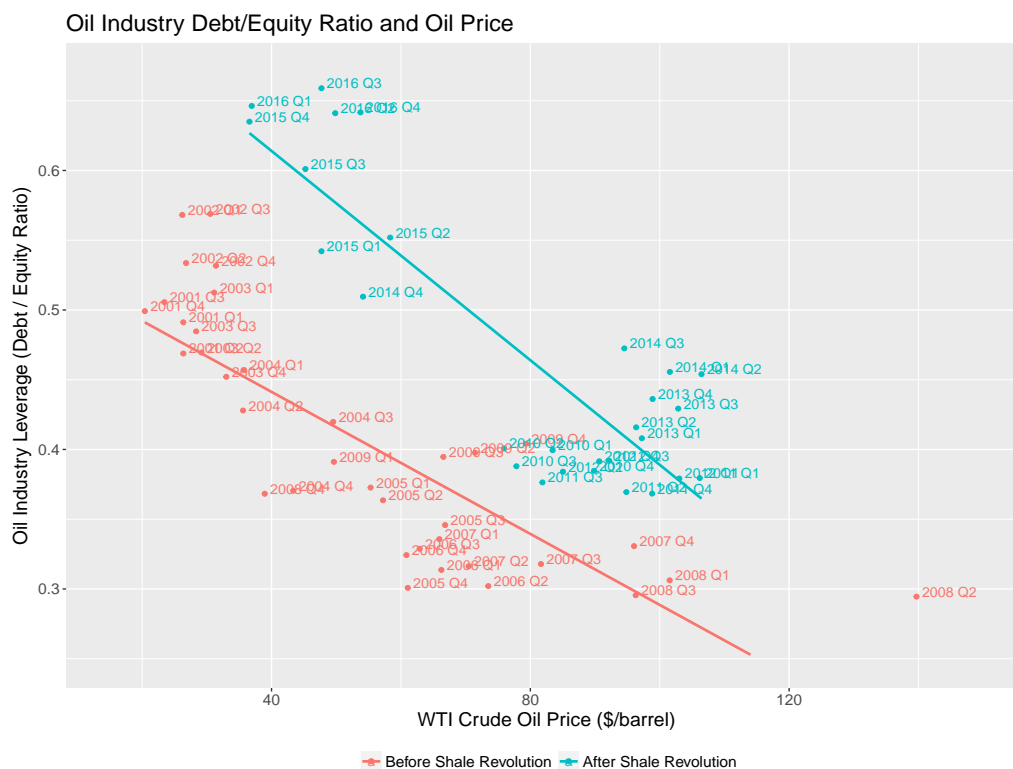
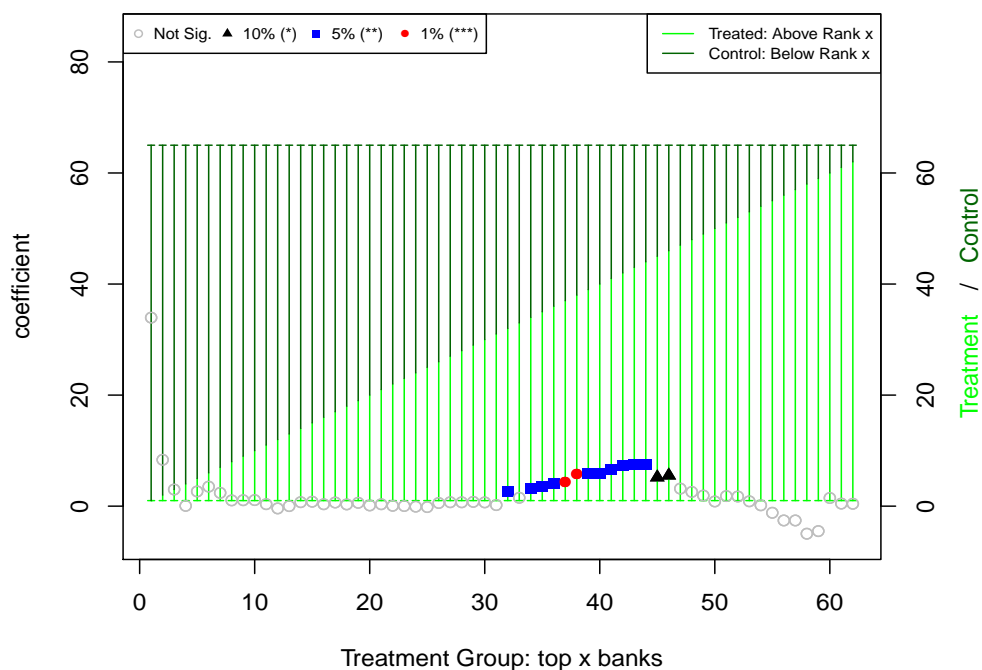
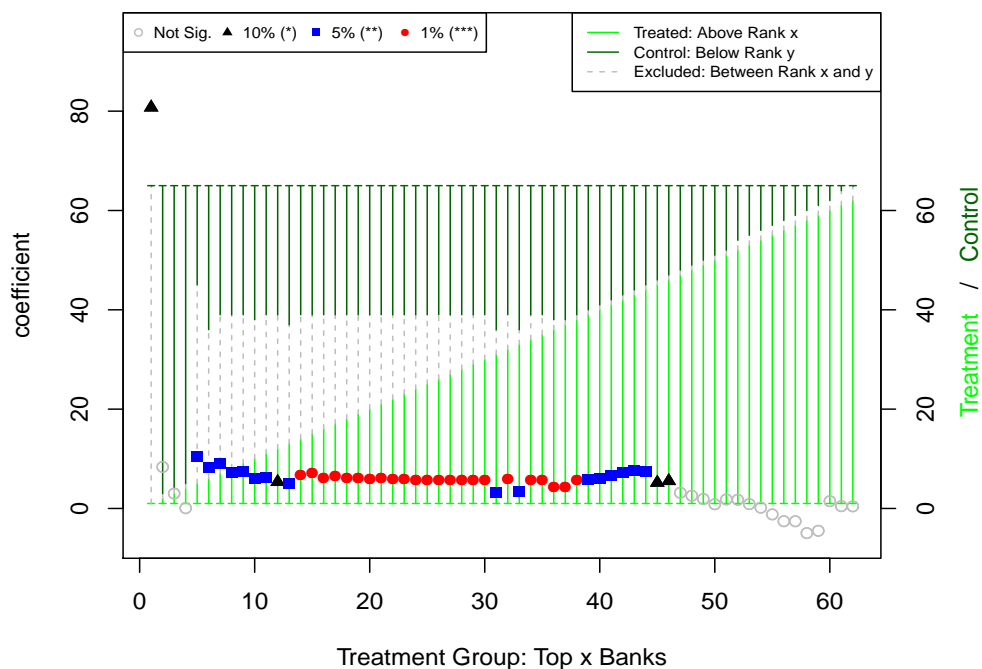


Figure 1.3: Coefficients for Possible Treatment Group Assignments



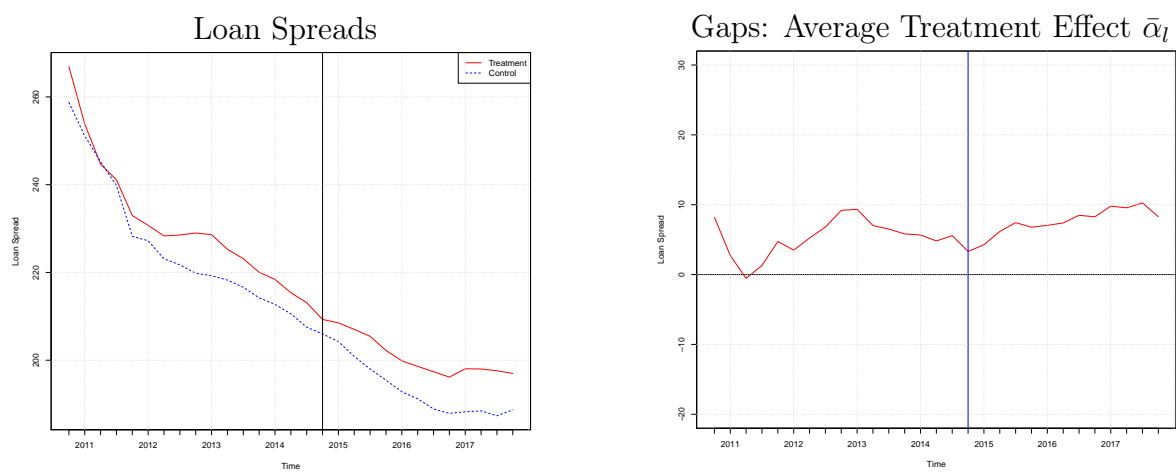
Note: Each point plots the DID coefficient indicated by the left vertical axis for each treatment group specification (indicated by the horizontal axis). For example, if the point has “10” tick mark on the horizontal axis, then its treatment group includes rank 1-10 banks. The shape and the color indicate the significance of the coefficient (top-left legend). The vertical lines visualize the proportion of the treatment and control groups for each specification indicated by the right vertical axis. The light green lines are the proportion of treatment banks and the dark green lines are the proportion of the control banks (top-right legend).

Figure 1.4: Coefficients for Possible Treatment Group Assignments (with exclusions)



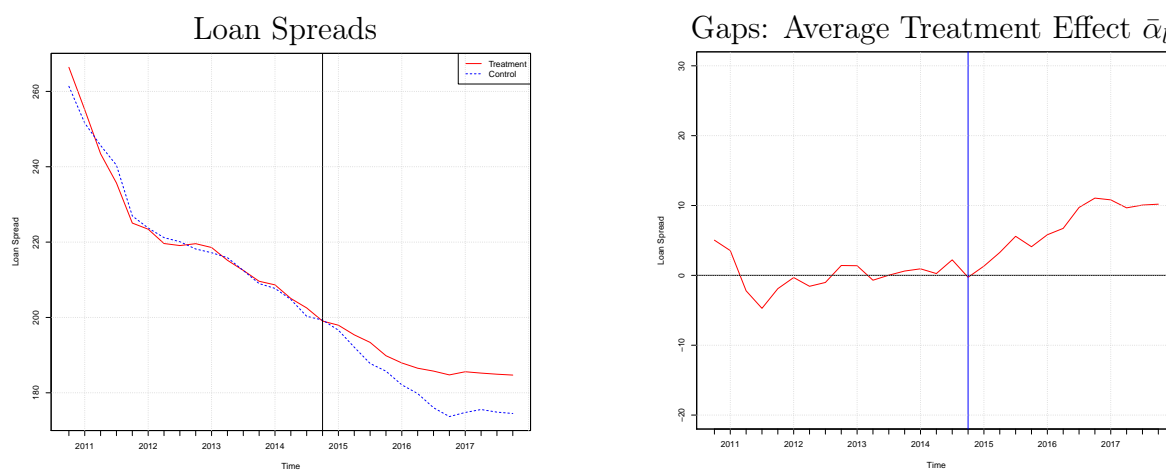
Note: Each point plots the DID coefficient indicated by the left vertical axis for each treatment group specification (indicated by the horizontal axis). For example, if the point has “10” tick mark on the horizontal axis, then its treatment group includes rank 1-10 banks. The shape and the color indicate the significance of the coefficient (top-left legend). The vertical lines visualize the proportion of the treatment and control groups for each specification indicated by the right vertical axis. The light green lines are the proportion of treatment banks and the dark green lines are the proportion of the control banks (top-right legend). The dashed lines represent the proportion of excluded banks from the sample.

Figure 1.5: SC w/ Exposure: "Oil Loan / All Loan ratio" (Treatment: 1-15 / Control: 39-64)



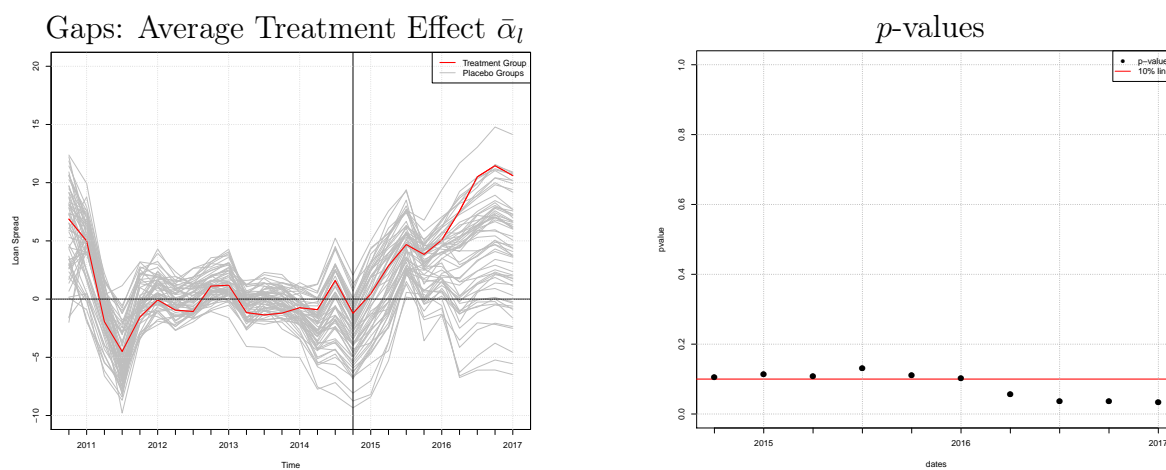
Note: For this exercise, I include rank 1-15 banks in the treatment and rank 39-64 banks in the control group as in the DID case. The left figure plots the loan spreads of the treatment group and the synthetic control group. The vertical line represents the time of the oil price shock. The right figure presents the loan spread gap between the treatment group and the synthetic control group. The gaps after the shock represent the average treatment effect at each lead.

Figure 1.6: SC w/ Exposure: "Oil Loan Amount" (Treatment: 1-13 / Control: 41-64)



Note: For this exercise, I include rank 1-13 banks in the treatment and rank 41-64 banks in the control group using the left criteria in Table I. The left figure plots the loan spreads of the treatment group and the synthetic control group. The vertical line represents the time of the oil price shock. The right figure presents the loan spread gap between the treatment group and the synthetic control group. The gaps after the shock represent the average treatment effect at each lead.

Figure 1.7: SC Placebo Tests (Treatment: 1-7 / Control: 41-64)



Note: For this exercise, I included rank 1-7 banks in the treatment and rank 41-64 banks in the control group to reduce the number of placebo tests. The left figure presents the average treatment effect at each lead and the right figure plots the p -values from the placebo distribution. The red horizontal line indicates the 10% probability.

Table 1.1: Identifying the Treatment Group

#	Banks ordered by oil loan amount (\$m)	Oil loan	#	Banks ordered by ratio (Oil loan / All loan)	ratio
1	WELLS FARGO & CO	230,169	1	ZIONS BANCORPORATION	45.7%
2	JPMORGAN CHASE & CO	217,161	2	COMERICA INC	21.6%
3	BANK OF AMERICA CORP	189,667	3	SOCIETE GENERALE GROUP	20%
4	CITIGROUP INC	174,630	4	CANADIAN IMPERIAL BANK	18.3%
5	MITSUBISHI UFJ FINANCIAL GRP	154,210	5	BBVA	17.4%
6	BNP PARIBAS	151,444	6	BANK OF MONTREAL	17.1%
7	ROYAL BANK OF SCOTLAND GROUP	137,696	7	ING GROEP NV	15.4%
8	BANK OF NOVA SCOTIA	124,700	8	BANK OF NOVA SCOTIA	14.8%
9	ROYAL BANK OF CANADA	120,124	9	BNP PARIBAS	13%
10	BBVA	116,232	10	LLOYDS BANKING GROUP PLC	12.6%
11	U S BANCORP	113,356	11	UBS GROUP AG	11.8%
12	BARCLAYS PLC	110,245	12	JEFFERIES GROUP LLC	11.2%
13	DEUTSCHE BANK AG	108,917	13	ROYAL BANK OF CANADA	10.6%
14	CREDIT SUISSE GROUP	107,852	14	MITSUBISHI UFJ FINANCIAL GRP	10.5%
15	UBS GROUP AG	102,344	15	SUMITOMO MITSUI FINANCIAL GR	9.9%
16	COMERICA INC	97,854	16	WELLS FARGO & CO	9.9%
17	GOLDMAN SACHS GROUP INC	88,184	17	REGIONS FINANCIAL CORP	8.7%
18	BANK OF MONTREAL	87,380	18	CREDIT SUISSE GROUP	8.7%
19	SUMITOMO MITSUI FINANCIAL GR	80,766	19	ROYAL BANK OF SCOTLAND GROUP	8.6%
20	MORGAN STANLEY	80,748	20	CITIGROUP INC	8.5%
21	SOCIETE GENERALE GROUP	69,311	21	KEYCORP	8.4%
22	MIZUHO FINANCIAL GROUP INC	67,165	22	MIZUHO FINANCIAL GROUP INC	8%
23	ING GROEP NV	55,726	23	ASSOCIATED BANC-CORP	7.8%
24	SUNTRUST BANKS INC	49,863	24	DEUTSCHE BANK AG	7.8%
25	HSBC HLDGS PLC	47,621	25	U S BANCORP	7.7%
26	PNC FINANCIAL SVCS GROUP INC	47,018	26	JPMORGAN CHASE & CO	7.5%
27	TORONTO DOMINION BANK	45,831	27	BB&T CORP	7.2%
28	CANADIAN IMPERIAL BANK	43,318	28	BARCLAYS PLC	6.9%
29	ZIONS BANCORPORATION	41,126	29	SUNTRUST BANKS INC	6.9%
30	KEYCORP	40,547	30	TORONTO DOMINION BANK	6.9%
31	FIFTH THIRD BANCORP	35,008	31	GOLDMAN SACHS GROUP INC	6.8%
32	REGIONS FINANCIAL CORP	34,380	32	BANK OF AMERICA CORP	6.3%
33	BANK OF NEW YORK MELLON CORP	27,300	33	MORGAN STANLEY	6.2%
34	BB&T CORP	21,077	34	HSBC HLDGS PLC	5.1%
35	NORTHERN TRUST CORP	21,000	35	FIFTH THIRD BANCORP	5%
36	BANCO SANTANDER SA	18,865	36	COMMUNITY BANCORP INC/VT	4.7%
37	LLOYDS BANKING GROUP PLC	16,800	37	BANCO SANTANDER SA	4.3%
38	JEFFERIES GROUP LLC	6,847	38	PNC FINANCIAL SVCS GROUP INC	4.3%
39	GENERAL ELECTRIC CAP CORP	4,491	39	BANK OF NEW YORK MELLON CORP	4%
40	CIT GROUP INC	3,128	40	NORTHERN TRUST CORP	3.4%
41	ASSOCIATED BANC-CORP	2,781	41	FIRST NIAGARA FINANCIAL GRP	2.8%
42	RAYMOND JAMES FINANCIAL CORP	2,579	42	CIT GROUP INC	2.8%
43	HUNTINGTON BANCSHARES	2,216	43	TRUSTMARK CORP	2.5%
44	FIRST NIAGARA FINANCIAL GRP	1,386	44	RAYMOND JAMES FINANCIAL CORP	1.7%
45	M & T BANK CORP	1,378	45	GENERAL ELECTRIC CAP CORP	1.6%
46	COMMUNITY BANCORP INC/VT	1,090	46	M & T BANK CORP	1.4%
47	TRUSTMARK CORP	175	47	HUNTINGTON BANCSHARES	1.3%
48	FIRSTMERIT CORP	175	48	FIRSTMERIT CORP	0.4%
49	STIFEL FINANCIAL CORP	74	49	STIFEL FINANCIAL CORP	0.3%
50	BANK OF HAWAII CORP	13	50	BANK OF HAWAII CORP	0.1%
51	ALLY FINANCIAL INC	0	51	ALLY FINANCIAL INC	0%
52	MARSHALL & ILSLEY CORP	0	52	MARSHALL & ILSLEY CORP	0%
53	STATE STREET CORP	0	53	STATE STREET CORP	0%
54	SYNOVUS FINANCIAL CORP	0	54	SYNOVUS FINANCIAL CORP	0%
55	WESTPAC BANKING	0	55	WESTPAC BANKING	0%
56	UNITED OVERSEAS BANK LTD	0	56	UNITED OVERSEAS BANK LTD	0%
57	ALLIANZ SE	0	57	ALLIANZ SE	0%
58	DBS GROUP HOLDINGS LTD	0	58	DBS GROUP HOLDINGS LTD	0%
59	PEOPLE'S UNITED FINL INC	0	59	PEOPLE'S UNITED FINL INC	0%
60	SVB FINANCIAL GROUP	0	60	SVB FINANCIAL GROUP	0%
61	WEBSTER FINANCIAL CORP	0	61	WEBSTER FINANCIAL CORP	0%
62	TRUSTCO BANK CORP/NY	0	62	TRUSTCO BANK CORP/NY	0%
63	CITIZENS FINANCIAL GROUP INC	0	63	CITIZENS FINANCIAL GROUP INC	0%
64	CAPITAL ONE FINANCIAL CORP	0	64	CAPITAL ONE FINANCIAL CORP	0%

Note: Left column is a list of banks ordered by the total amount of oil loans. Right column is a list of banks ordered by the total amount of oil loans divided by total amount of all loans. As a result, we see a mixture of small and large banks on top of the list.

Table 1.2: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Max
LOANSPREAD	63,324	193.350	98.074	1.750	1,225.000
LAGE	63,324	4.489	0.620	1.946	5.403
LSALES	63,324	6.653	1.520	-2.154	11.661
LEVERAGE	63,324	0.346	0.197	0.00002	1.825
MB	63,324	0.289	0.865	-9.594	1.469
PROFMARGIN	63,324	0.071	0.482	-27.431	18.453
INTERESTCOV	63,324	10.316	73.658	-1,073.913	3,467.800
NWC	63,324	11.942	475.752	-6.163	31,237.920
TANGIBLES	63,324	0.465	0.479	0.000	5.445
RANDD	63,324	-0.0002	0.004	-0.164	0.000
STOCKVOL	63,324	0.086	0.047	0.018	1.785
EXCESSRET	63,324	0.014	0.097	-0.485	2.099
LASSETS	63,324	13.714	1.204	6.315	14.937
ROAVOL	63,286	1.065	1.388	0.078	19.444
ROA	63,324	1.782	1.426	-30.947	27.057
SUBDEBT	63,324	0.011	0.057	0.000	1.419
CAPITAL	63,324	0.004	0.025	-0.006	0.877
LAMOUNT	63,324	20.019	1.158	13.791	24.615
LMATURITY	63,324	3.929	0.447	0.000	5.193
SECURED	63,324	0.398	0.489	0	1
SENIOR	63,324	1.000	0.008	0	1
DIVIDENDREST	63,324	0.318	0.466	0	1
GUARANTOR	63,324	0.155	0.362	0	1
TERMLOAN	63,324	0.225	0.418	0	1
CREDITLINE	63,324	0.665	0.472	0	1

Note: Data source is a merger of TR Dealscan and Compustat for time period 2010 Q1 to 2017 Q4. (LOAN): *LOANSPREAD* is the all-in-drawn spread over LIBOR; *LAMOUNT* is the log of the facility amount in millions of dollars; *LMATURITY* is the log of maturity; *SECURED* indicates whether the facility is secured; *SENIOR* indicates the seniority of the debt; *GUARANTOR* indicates whether a guarantor exists; *DIVIDENDREST* presents dividend restriction of the borrower; *CORPURPOSE* indicates whether the loan purpose was for corporate purpose; *DEBTREPAY* indicates whether the loan purpose was for debt repayment; *WORKCAPITAL* indicates whether the loan purpose was for working capital; *TERMLOAN* indicates whether the loan purpose was for term loan; *CREDITLINE* indicates whether the loan purpose was for opening a credit line; *relationship* indicates whether the firm borrowed from the bank more than once within a year. (FIRM): *LAGE* is the log of firm age; *LSALES* is the log of net sales; *PROFMARGIN* is the profit margin calculated by net income over sales; *INTCOVERAGE* is the interest coverage ratio calculated by EBITDA over interest expense; *STOCKVOL* is the stock volatility of the borrower's stock return across last 365 days prior to the loan date; *EXCESSRET* is the excess return on the borrower's stock return across last 365 days prior to the loan date; *LEVERAGE* is computed by the sum of current and long term debt divided by the total assets; *TANGIBLES* are the sum of inventory and the total of property, plant, and equipment; *NWC* is the net working capital computed by the difference between the assets and current liabilities divided by the total debt; *MB* is market to book value ratio. (BANK): *LASSETS* is the log of total assets of banks; *ROA* is the banks' net income over assets; *ROAVOL* is the volatility of the bank ROA computed over the last three years from the loan date; *CAPITAL* is the bank equity capital over assets.

Table 1.3: DID Estimation (No exclusions)

<i>Dependent variable: LOANSPREAD</i>				
	(1)	(2)	(3)	(4)
Treatment Group:	Rank: 1-38	Rank: 1-38	Rank: 1-38	Rank: 1-38
Control Group:	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64
DID	7.183*** (2.91)	6.233*** (2.60)	7.003*** (2.90)	5.690*** (2.67)
TREATMENT	-0.586 (-0.43)	0.0921 (0.07)	-1.992 (-1.41)	-1.841 (-1.52)
TIME	-21.90*** (-6.61)	-1.997 (-0.49)	-2.274 (-0.56)	-4.863 (-1.32)
LAGE		-73.54*** (-4.61)	-72.61*** (-4.55)	-85.98*** (-5.78)
LSALES		-32.25*** (-5.89)	-31.86*** (-5.82)	-22.51*** (-4.59)
LEVERAGE		42.71** (2.19)	38.31** (1.97)	64.45*** (3.89)
MB		9.947*** (2.82)	9.885*** (2.80)	14.23*** (4.28)
PROFMARGIN		-0.0694 (-0.03)	-0.185 (-0.07)	-0.771 (-0.32)
INTERESTCOV		-0.00189 (-0.16)	-0.00267 (-0.22)	-0.00983 (-0.85)
NWC		0.000250 (0.43)	0.000299 (0.51)	-0.000698 (-1.13)
TANGIBLES		28.67*** (2.71)	26.53*** (2.58)	23.44*** (2.69)
RANDD		150.2*** (2.83)	148.7*** (2.84)	62.52 (1.03)
STOCKVOL		389.9*** (6.52)	377.5*** (6.27)	342.1*** (6.42)
EXCESSRET		-22.09 (-1.49)	-22.92 (-1.55)	-18.50 (-1.38)
LASSETS			0.556** (2.32)	0.427** (2.01)
ROA			-1.110*** (-4.93)	-0.900*** (-4.62)
SUBDEBT			72.29* (1.77)	27.50 (0.74)
CAPITAL			196.3* (1.89)	244.2** (2.46)
LAMOUNT				-2.649* (-1.91)
LMATURITY				-7.126* (-1.86)
SECURED				18.67*** (3.57)
SENIOR				-657.3*** (-85.68)
DIVIDENDREST				-5.053 (-1.22)
GUARANTOR				-12.53*** (-2.77)
CORPURPOSES				-43.83*** (-12.21)
DEBTREPAY				-30.99*** (-3.36)
WORKCAPITAL				-40.54***

TERMLOAN				(-7.62)
				-10.21**
				(-2.08)
CREDITLINE				-29.45***
				(-6.58)
_cons	198.7***	672.6***	664.0***	1450.2***
	(128.77)	(9.50)	(9.41)	(22.20)
Fixed effects:	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank
<i>N</i>	63286	63286	63286	63286
adj. <i>R</i> ²	0.646	0.680	0.681	0.721

Note: Control variables are muted and indicated at the bottom to save space. t-statistics are reported in the parentheses and standard errors are clustered at the firm level. (*p<0.1; **p<0.05; ***p<0.01)

Table 1.4: DID Estimation (Exclusions)

<i>Dependent variable: LOANSPREAD</i>				
	(1)	(2)	(3)	(4)
Treatment Group:	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15
Control Group:	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64
DID	10.88*** (3.45)	8.633*** (2.86)	8.719*** (2.88)	7.119*** (2.89)
TREATMENT	-2.587 (-1.60)	-1.154 (-0.76)	0.348 (0.15)	0.285 (0.15)
TIME	-21.27*** (-5.61)	-1.556 (-0.31)	-1.546 (-0.31)	-3.851 (-0.87)
_cons	199.0*** (124.51)	670.5*** (8.18)	678.2*** (8.43)	1468.2*** (18.39)
LOAN CONTROLS	No	No	No	Yes
BANK CONTROLS	No	No	Yes	Yes
FIRM CONTROLS	No	Yes	Yes	Yes
Fixed effects:	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank
<i>N</i>	16466	16466	16466	16466
adj. <i>R</i> ²	0.666	0.700	0.701	0.742

Note: Control variables are muted and indicated at the bottom to save space. t-statistics are reported in the parentheses and standard errors are clustered at the firm level. (*p<0.1; **p<0.05; ***p<0.01)

Table 1.5: Robustness Tests: Changing Oil Price Shock Period

<i>Dependent variable: LOANSPREAD</i>						
Treatment:	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15
Control:	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64
Shock Period	(2014Q3)	(2014Q4)	(2015Q1)	(2015Q2)	(2015Q3)	(2015Q4)
DID	6.141** (2.55)	7.119*** (2.89)	6.406** (2.43)	5.255* (1.96)	5.440* (1.75)	5.390 (1.58)
TREATMENT	0.457 (0.24)	0.285 (0.15)	0.828 (0.45)	1.177 (0.65)	1.674 (0.95)	1.908 (1.10)
TIME	-2.775 (-0.62)	-3.851 (-0.87)	-2.782 (-0.61)	-5.580 (-1.33)	0.258 (0.06)	2.042 (0.41)
_cons	1472.1*** (18.06)	1468.2*** (18.39)	1470.5*** (18.86)	1436.8*** (20.28)	1482.5*** (21.86)	1490.7*** (21.59)
LOAN	Yes	Yes	Yes	Yes	Yes	Yes
BANK	Yes	Yes	Yes	Yes	Yes	Yes
FIRM	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects:	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank
N	16466	16466	16466	16466	16466	16466
adj. R^2	0.742	0.742	0.742	0.742	0.742	0.742

Note: Control variables are muted and indicated at the bottom to save space. t-statistics are reported in the parentheses and standard errors are clustered at the firm level. (*p<0.1; **p<0.05; ***p<0.01)

Table 1.6: Bank Dependency: Access to Corporate Bonds / Credit Rating / Firm Size

		<i>Dependent variable: LOANSPREAD</i>					
		(1)	(2)	(3)	(4)	(5)	(6)
Bond Access:	No	Yes					
Credit Rating:			Bad	Good			
Firm Size:					$\leq 25\%$	$\geq 75\%$	
Treatment:	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15	
Control:	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64
DID	5.606** (1.98)	3.585 (1.06)	19.38*** (4.25)	-0.627 (-0.35)	10.28* (1.76)	1.826 (0.51)	
TREATMENT	1.881 (0.79)	-0.555 (-0.21)	-2.635 (-0.82)	0.676 (0.54)	2.225 (0.45)	1.925 (0.67)	
TIME	-1.132 (-0.18)	-7.386 (-1.02)	-9.039 (-1.32)	-2.253 (-0.46)	-11.91 (-1.25)	-3.580 (-0.47)	
_cons	1480.8*** (13.87)	644.4*** (3.53)	814.7*** (5.69)	848.1*** (7.68)	1417.8*** (9.73)	948.6*** (4.94)	
LOAN	Yes	Yes	Yes	Yes	Yes	Yes	
BANK	Yes	Yes	Yes	Yes	Yes	Yes	
FIRM	Yes	Yes	Yes	Yes	Yes	Yes	
Fixed effects:	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank
N	11198	5268	6491	6202	3391	6772	
adj. R^2	0.759	0.772	0.656	0.766	0.720	0.715	

Note: Control variables are muted and indicated at the bottom to save space. t-statistics are reported in the parentheses and standard errors are clustered at the firm level. (* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$)

Table 1.7: DID Estimation (Loan Amount)

<i>Dependent variable: LOANAMOUNT</i>				
	(1)	(2)	(3)	(4)
Treatment Group:	Rank: 1-38	Rank: 1-38	Rank: 1-38	Rank: 1-38
Control Group:	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64
DID	-0.136* (-1.74)	-0.174** (-2.22)	-0.158** (-1.99)	-0.144* (-1.88)
TREATMENT	0.141*** (2.73)	0.146*** (2.62)	0.122** (2.19)	0.0989* (1.90)
TIME	0.362*** (4.51)	0.308*** (3.62)	0.292*** (3.37)	0.267*** (3.24)
_cons	19.74*** (384.49)	16.88*** (26.59)	16.46*** (24.95)	14.85*** (23.96)
LOAN CONTROLS	No	No	No	Yes
BANK CONTROLS	No	No	Yes	Yes
FIRM CONTROLS	No	Yes	Yes	Yes
Fixed effects:	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank
<i>N</i>	37853	27208	25820	25700
adj. <i>R</i> ²	0.650	0.644	0.644	0.652

Note: Control variables are muted and indicated at the bottom to save space. t-statistics are reported in the parentheses and standard errors are clustered at the firm level. (*p<0.1; **p<0.05; ***p<0.01)

Table 1.8: Bank Dependency: Access to Corporate Bonds / Credit Rating / Firm Size

<i>Dependent variable: LOANSPREAD</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Bond Access:	No	Yes				
Credit Rating:			Bad	Good		
Firm Size:					≤ 25%	≥ 75%
Treatment:	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15	Rank: 1-15
Control:	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64	Rank: 39-64
DID	-0.295*** (-2.73)	-0.110 (-1.08)	-0.0733 (-0.60)	-0.0450 (-0.69)	-0.232** (-2.57)	-0.0583 (-0.42)
TREATMENT	0.214*** (2.94)	0.0882 (1.20)	0.0816 (0.85)	0.0558 (1.30)	0.122** (2.04)	0.155 (1.27)
TIME	0.430*** (3.83)	0.220** (2.00)	0.139 (1.00)	0.112 (1.17)	0.366*** (3.44)	0.175 (1.20)
_cons	16.02*** (18.97)	15.92*** (14.66)	16.90*** (17.16)	13.44*** (6.49)	16.76*** (20.56)	15.68*** (15.36)
LOAN	No	No	No	No	No	No
BANK	Yes	Yes	Yes	Yes	Yes	Yes
FIRM	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects:	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank	Firm-Bank
<i>N</i>	6823	18997	8021	22986	10176	15644
adj. <i>R</i> ²	0.640	0.596	0.410	0.633	0.508	0.546

Note: Control variables are muted and indicated at the bottom to save space. t-statistics are reported in the parentheses and standard errors are clustered at the firm level. (*p<0.1; **p<0.05; ***p<0.01)

Chapter 2

REAL EFFECTS OF THE CREDIT SUPPLY SHOCK

This chapter investigates the real effects of a negative credit supply shock. As studied in Chapter 1, if a shock damages banks' credit availability, corporate borrowers receive the transmitted shock through new loan contracts. If the borrowing costs change due to the shock, corporate borrowers face external finance problems that can result in further real effects. The real effects can range from changes in corporate borrowers' performance to their investment decisions due to the limited external financing opportunities. In this chapter, I explain the consequences of a negative credit supply shock. In particular, I study how corporate borrowers' investment activities change after a negative credit supply shock. Furthermore, I see how a borrowing firm's cash reserves mitigate or worsen the impact of the shock.

2.1 Introduction

The importance of the financial system's role in an economy is inevitably significant because it acts as a hub that connects the households, firms, and governments. If the financial system collapses, eventually the entire economy is at risk, as observed in the 2008 financial crisis. Although the crisis began in the housing market, exponentially increasing loan defaults directly damaged banks' credit supply, and further led the economy into a recession. While Chapter 1 described evidence of the shock transmission in the credit supply channel, this chapter focuses on what happens after the shock transmission. In other words, I explore the real effects of a credit supply shock by studying corporate borrowers behavior when facing increased borrowing costs due to the transmitted credit supply shock.

As found in Chapter 1, banks tend to change their lending standards to all corporate

borrowers when they experience a credit supply shock. If this change involves increases in loan interest rates, it has a direct impact on corporate borrowers' financing decisions. If a borrowing firm is heavily relying on bank lending, then it faces difficulties in funding its investment projects. On the other hand, if a firm has sufficient internal funds or cash reserves, it may mitigate the impact of changes in bank lending standards. So this second stage of shock transmission investigates corporate borrowers' response in financing activities to the initial banks' response in the first stage to a credit supply shock. This chapter shifts the focus towards the investment at the firm level as my hypotheses are based on standard models of investment with financing frictions (see Jaffee and Russell (1976); Stiglitz and Weiss (1981); Holmstrom and Tirole (1997)). Theoretical models explain that negative shocks to the supply of external funds along with the presence of financial frictions might damage investment if firms lack sufficient funds to finance investment projects internally. Moreover, this damage can be more severe for firms that are more dependent on external financing or more financially constrained to maintain their investment activities. To support this idea with empirical evidence, a vast majority of papers exploit 2008 financial crisis, which was essentially a credit supply shock, and studied its real effect on the corporate sector and consumers (see Duchin et al. (2010); Tong and Wei (2008); Ivashina and Scharfstein (2010); Campello et al. (2010); Almeida, Campello, Laranjeira, and Weisbenner (2009)). Some papers focused on the effects of corporate cash holdings that is not only subjected to the crisis but also in a classic line of research in corporate finance (see Opler, Pinkowitz, Stulz, and Williamson (1999); Almeida, Campello, and Weisbach (2004); Modigliani and Miller (1958)). To test my hypotheses, I support my idea based on both strands of the literature.

In this chapter, I use oil price shock as a negative credit supply shock. In Chapter 1, I found that the negative oil price shock caused several loan defaults in the oil industry and gave enough pressure to the banks that resulted in increased loan spreads not only for the oil-industry borrowers but also to all corporate borrowers. I argue that this is the evidence of shock transmission in the credit supply channel. Coincided with this finding, in this chapter, I further investigate the consequences on the corporate borrowers after the shock

transmission. Specifically, I hypothesize that a credit supply shock creates external financing problems for corporate borrowers and discourages investment activities at the firm level. In other words, due to the limited external financing, firms are forced to reduce the amount spent on investments. As done in Duchin et al. (2010), I measure investment activities using capital expenditures data. Also, I test whether a firm's financial positions mitigate or worsen the impact of the credit supply shock on investment. I use cash reserves to see the ability of the firm to buffer the impact of the credit supply shock on investment.

The baseline empirical model is comparing the investment of firms before and after the oil price shock in 2014. To control for other factors that can affect corporate investments besides the indicator before and after the shock, I include Tobin's Q and cash flow measures to account for observable investment opportunities. I include cash holdings data to check whether a firm's financial position plays a role in determining investment spending. In order to analyze the credit supply effect on firms' investments, I use the data one year before and after the shock and change the sample window for the robustness check. As described in Duchin et al. (2010), to alleviate concerns of endogeneity of variation in cash reserves to unobserved variation in investment opportunities, I use one-year prior cash reserves data assuming that year-before cash reserves are not positively correlated with unobserved within-firm changes in investment opportunities (i.e. unobserved firm-specific demand shocks). To support the baseline model's identification assumption, I also see whether the change in investments is more severe for financially constrained firms than the less constrained firms. If the supply effect is present, then we expect more change among financially constrained firms.

The main contribution of this chapter to the literature is that unlike other studies, this work investigates the real effect of a credit supply shock that is not as massive as other economic events that are commonly used. The most frequently used event was the 2008 financial crisis that led the entire economy into a recession. Since aggregate demand is also affected immensely during this period, the identification of credit supply effect is quite challenging. Instead, I study a shock that is relatively smaller in terms of magnitude and

has less distraction from the demand side. This strategy allows us to understand the effect of credit supply shock outside of a recession period, and hence allows better identification of the effects of the credit supply channel.

This chapter proceeds as follows. Section 2.2 reviews the literature and Section 2.3 specifies the empirical strategy. Section 2.4 describes the data and Section 2.5 displays the results. Finally, Section 2.6 concludes.

2.2 Literature Review

This study is related to several strands of literature. In order to investigate whether a credit supply shock gets transmitted in the credit supply channel in the first place, many papers studied major economic events to design the shock transmission mechanism. These events range from the great depression (see Bernanke (1983)) to Basel I (see Thakor (1996); Peek and Rosengren (1995); Berger and Udell (1995)) and monetary policy shock (see Gertler and Gilchrist (1994); Kashyap et al. (1993); Kashyap and Stein (2000)). But most importantly, the 2008 financial crisis was followed by a surge of empirical evidence of the shock transmission in credit supply channel (see Ivashina and Scharfstein (2010); Contessi and Francis (2013); Chari et al. (2008); Santos (2011)). In addition to this evidence, many papers worked on identifying the real effects or the consequences of the credit supply shock, which this chapter focuses on.

There are a set of papers investigating the real effects on the corporate sector after the credit supply shock. Tong and Wei (2008) use stock price changes during the financial crisis and find that stock price declines were more severe for more financially constrained firms. Ivashina and Scharfstein (2010) find that banks with more deposit financing before the crisis cut their lending to corporate borrowers less than banks with lack of deposit financing. Similarly, Santos (2011) finds that the loan interest rate is charged higher after the crisis depending on the amount of loans charged off. This evidence supported how the 2008 financial crisis influenced external financing for borrowing firms. Campello et al. (2010) find that if firms are financially constrained, they give up pursuing profitable investment

opportunities. Duchin et al. (2010) finds how the financial crisis caused costly external financing and changed firms' investment activities. I adopt their identification strategy of using one-year prior cash holding to my data using different credit supply shock. As mentioned in Chapter 1, using the oil price shock as a credit supply shock improves the identification strategy, this paper contributes to the empirical methods in Duchin et al. (2010) for using better-identified credit supply effect.

This paper is also related to theoretical framework on standard models of investment with financing frictions (see Jaffee and Russell (1976); Stiglitz and Weiss (1981); Holmstrom and Tirole (1997)). Theoretical models explain that negative shocks to the supply of external funds along with the presence of financial frictions might damage investment if firms lack sufficient funds to finance investment projects internally. Moreover, this damage can be more severe for firms that are more dependent on external financing or more financially constrained to maintain their investment activities.

Another significantly related body of research is on corporate cash holdings as one of my hypotheses is that a firm's financial position can play a role in mitigating or worsening the impact of the credit supply shock. Opler et al. (1999) suggests that industry-level cash-flow volatility is a key determinant of corporate cash holdings. Almeida et al. (2004) show that firms maintain extra cash reserves from their cash flows if they are financially constrained and run the risk of underinvesting, which agrees to the idea of Modigliani and Miller (1958) that cash only matters to the company when financial markets are not frictionless. In line with these results and my findings, Faulkender and Wang (2006) find that the marginal value of cash holdings is higher for the firms that are financially constrained. Acharya, Almeida, and Campello (2007) find the hedging role of cash when cash flows are low, and investment opportunities are high.

2.3 Empirical Strategy

In this section, I describe the empirical methods. The identification strategy that this paper uses is inspired by Duchin et al. (2010). In order to test whether the investment activities

change after the oil price shock and see if the cash reserves play a mitigating role, I compare the capital expenditures to total assets ratio of firms before and after the shock in late 2014. To control for other factors that can affect corporate investments other than the shock indicator variable, I include measures of observable investment opportunities such as Tobin's Q and cash flow. I further use the cash amount to total assets ratio to see if firms' financial position plays a role in making investment decisions. In order to analyze the credit supply shock effect on firms' investments, I choose the sample window of two years prior and two years after the oil price shock. I also check the robustness of the results by changing the sample window. The baseline regression model can be described as follows:

$$\begin{aligned} INVESTMENT_{i,t} = & \alpha_i + \beta_1 TIME_t + \beta_2 TIME_t \times CASH_{i,t} + \beta_3 TOBIN Q_{i,t} \\ & + \beta_4 CASH FLOW_{i,t} + \gamma_i + \varepsilon_{i,t} \end{aligned}$$

where *INVESTMENT* is the capital expenditure divided by the total assets (capxy/atq) and *TIME* takes a value of 0 for before the shock and 1 for after the shock. *CASH* is the cash holdings to total assets ratio (chq/atq) and *TOBIN Q* is calculated by the market value of equity and liability divided by the book value of equity and liability of a firm. Lastly, *CASH FLOW* is operating income before depreciation divided by the total assets (oibdpq/atq). The firm fixed effect is included, and the standard errors are clustered at the firm level (see Petersen (2009)). So from this regression, I expect β_1 to be negative in line with my hypothesis that firm investment was discouraged after the credit supply shock. Also, if firms' financial position plays a role in mitigating the impact of the shock, then I expect β_2 to be positive so that firms' with enough cash reserves have more investments than that of the firms who have less cash reserves.

As described in Duchin et al. (2010), to alleviate concerns of endogeneity of variation in cash reserves to unobserved variation in investment opportunities, I use one-year prior cash reserves data assuming that year-before cash reserves are not positively correlated with unobserved within-firm changes in investment opportunities (i.e. unobserved firm-specific demand shocks). To check if one-year prior cash reserves is a sufficiently long period to

remove the correlation with unobserved firm-specific demand shocks, I try even more past cash reserves for robustness.

To support the baseline model’s identification assumption, I also see whether the change in investments is more severe for financially constrained firms than the less constrained firms. If the supply effect is present, then we expect more change among financially constrained firms.

In addition, to strengthen the identification strategy, I specifically look at oil firms that have clear investment plans during the oil price shock. Since oil prices are determined globally, oil firms are inevitable to adjust their production based on the oil price. Although there is not a clear direction of the influence, lagged oil prices must have a significant influence on the current period’s investment spending for the oil firms. So to capture the change in investment due to the oil price movements, I include 1-quarter lagged change in oil price in the baseline regression model:

$$INVESTMENT_{i,t} = \alpha_i + \beta_1 TIME_t + \beta_2 TIME_t \times CASH_{i,t} + \beta_3 \Delta OIL PRICE_{i,t-1} + \gamma_i + \varepsilon_{i,t}$$

where $\Delta OIL PRICE$ is the percent change in oil price from the previous quarter. I check the robustness of this model by changing the number of lags of the oil price.

2.4 Data

This chapter mainly uses the Compustat quarterly fundamentals data. In the main sample, I use data from 2012Q4 to 2016Q4 to have equal lengths before and after the oil price shock in 2014Q4 to average out any seasonal patterns in the data (e.g. Shin and Kim (2002)). I exclude utility firms (sic in 4900-4949) and financial firms (sic in 6000-6999) and winsorize data by 1% and 99% in firm size. Investment is measured by capital expenditures to total assets ratio. For the oil price, I use WTI Crude oil index. When measuring how much financially constrained a firm is, I use measures like Kaplan-Zingales (KZ) index, Whited-

Wu (WW) index, and payout ratio as below:

$$KZ = -1.002 * Cash\ Flow + 0.283 * Q + 3.319 * Debt - 39.368 * Dividends - 1.315 * Cash$$

$$WW = -0.091 * Cash\ Flow + 0.062 * Dividend\ Dummy + 0.021 * Long-term\ Debt \\ - 0.044 * Size + 0.102 * Industry\ Sales\ Growth - 0.035 * Sales\ Growth$$

$$Payout = (Cash\ Dividends + Repurchases) / Income\ Before\ Extraordinary\ Items$$

2.5 Results

Throughout this section, the dependent variable is *INVESTMENT*, which is calculated by the firm's capital spending. Table 2.1 displays different combination of regressors. In Model (1), there is a positive coefficient for the *TIME* dummy variable, but it is not statistically significant. In Model (2), we see that firms' investment spending relative to the assets decreased by 0.183% after the oil price shock. Although it is relatively a small magnitude, the overall impact can be quite large because this sample includes all firms. Moreover, the coefficient for the interaction term is positive and statistically significant. This can be interpreted as the investment spending relative to the assets increased by 1.49% for a 1% increase in cash holdings after the shock. So although the oil price shock discouraged investment activities because it acted as a credit supply shock causing many external finance problems, firms with enough cash holdings were able to mitigate the impact of the shock and invest more than the firms with less cash holdings. Model (3) can be interpreted similarly, and finally, Model (4) with all the observable investment opportunities controlled, suggests consistent result and interpretation.

To alleviate concerns of identification issues, I further distinguish the role of financial constraint as suggested in Duchin et al. (2010). More specifically, I divide the sample into financially unconstrained firms and financially constrained firms based on three different financial constraint measures. Table 2.2 display six different regressions. For each financial constraint measure, the left columns are the sub-sample of financially unconstrained firms, and the right columns are of financially constrained firms. In other words, low (KZ) index, low

(WW) index, and high payout ratio indicate financially unconstrained status and vice versa. Table 2.3 divides samples into industry characteristics. For example, if a firm is in an industry that promotes high external-finance dependence, then this firm is the sub-sample reported in the second column. As a result, firms that are in the high external-finance dependence industry, they end up investing less after the oil price shock while the other firms actually increase investment spending after the shock. Also, cash holding plays mitigation role of the shock more than the firms in low external-finance dependence industry. Also, for the firms that are in the high equity dependence industry, reduced investment spending after the oil price shock while their counterparts increased investments. Cash holding mitigates the shock in this case again for both sub-samples. Moreover, firms in high informational asymmetry industry reduced investments after the oil price shock and cash holding mitigated the shock for both sub-samples.

As suggested in Duchin et al. (2010), I also try other investment measures such as general administrative expense, R&D expense, net working capital, and inventory. According to Table 2.4, although some of the coefficients are not statistically significant, signs for the coefficient on *TIME* variable is negative except for R&D case. Most of the investment measure regressions indicate mitigation role of cash holdings.

In order to overcome identification issues for cash holding's mitigation role, I suggest another approach. Since oil firms are most heavily affected by the oil price shock, it is easier to control for their other investment opportunities besides relying on Tobin's Q and cash flow variable. By including lagged oil price change, we can capture oil firms' investment decision mainly due to the change in oil price. By doing this, *TIME* variable can capture other influences of making investment decisions for oil firms, such as a change in bank lending or difficulties in external financing. Table 2.5 displays four regressions with different specification. Model (1) suggests investments of oil firms decreased by 1.6% relative to their assets after the oil price shock. Model (2) suggests 2.1% drop in investments but having 1% more cash holdings result in 6.3% increase in investment spending relative to their assets. Finally, Model (3) includes lagged oil price change, and now the magnitude of

TIME coefficient is much reduced to about 1% drop in investment after the oil price shock. The coefficient for cash holding interaction term still hints mitigation role after the shock. Lastly, Model (4) produces consistent results as in previous cases. This result tells us that cash holding indeed mitigates the impact of the credit supply shock that is propagated to the borrowers causing external financing difficulties. Of course, lagged oil price change should not be affected by other factors, but the oil price is determined globally that can alleviate endogeneity concerns. To check its robustness, I try different lags of oil price change as in Table 2.6. We can observe that the increase in the oil price increases the investment spending for as long as three quarters of lags and the relationship between the two becomes the opposite after four quarters of lags. Lastly, I run the regression model of financial constraints in 2.7. Signs and statistical significance are as expected for *TIME* variable, but there are mixed results for cash holding interaction term. This may be due to small sample size, or because oil firms were heavily impacted by the oil price shock regardless of financial constraints.

2.6 Conclusion

This study explores real effects after a credit supply shock. If banks' credit availability is damaged, then their lending becomes more conservative, and that can change corporate borrowers' investment decisions. Due to the change in lending terms, external financing becomes more difficult, especially for financially constrained firms. If they cannot find alternative funding, firms are discouraged from making more investments. This transmission channel is easily observed in critical economic crises. However, I suggest that the oil price shock that is relatively less destructive in terms of magnitude can also hamper firms' investment decisions. Moreover, having enough cash holdings, which can be viewed as an internal financing source, helps firms mitigating the shock and make better investment decisions. Identifying the entire transmission mechanism from the beginning to the end is exceptionally challenging. Although this study does not estimate the effect simultaneously, it gives a promising idea of a small-magnitude credit supply shock can end up having serious real effects such as decrease in investments.

Table 2.1: Firm Investment Before and After the 2014 Oil Price Shock

	<i>Dependent variable: INVESTMENT</i>			
	(1)	(2)	(3)	(4)
TIME	-0.0000111 (-0.02)	-0.00247*** (-2.98)	-0.00230*** (-2.77)	-0.00248*** (-3.01)
TIMEXCASH		0.0173*** (3.50)	0.0180*** (3.66)	0.0182*** (3.71)
tobinq			0.00500** (2.03)	0.00424* (1.78)
cashflow				-0.0565** (-2.42)
_cons	0.0363*** (118.03)	0.0363*** (116.61)	-0.00517 (-0.25)	0.00141 (0.07)
<i>N</i>	35659	35659	35659	35659
adj. <i>R</i> ²	0.535	0.535	0.536	0.536
Fixed effects:	Firm	Firm	Firm	Firm

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 2.2: Investment by Financial Constraints

	<i>Dependent variable: Investment</i>					
	Kaplan-Zingales index		Whited-Wu index		Payout ratio	
	Low	High	Low	High	High	Low
TIME	-0.00106 (-1.01)	-0.00381*** (-3.39)	-0.000264 (-0.38)	-0.00520*** (-3.40)	0.00129 (1.51)	-0.00487*** (-3.73)
TIMEXCASH	0.00865* (1.75)	0.0315*** (3.81)	0.0193*** (4.18)	0.0212*** (3.12)	0.00595 (1.35)	0.0250*** (3.65)
tobinq	0.00638*** (2.60)	-0.000516 (-0.16)	0.00556*** (3.06)	0.00169 (0.45)	0.00174 (0.58)	0.00473 (1.36)
cashflow	0.0194 (0.78)	-0.124*** (-3.87)	0.00312 (0.10)	-0.0705** (-2.46)	0.0921** (2.28)	-0.0932*** (-2.74)
_cons	-0.0216 (-1.06)	0.0464* (1.80)	-0.0112 (-0.75)	0.0227 (0.72)	0.0157 (0.64)	-0.00206 (-0.07)
<i>N</i>	19405	18270	16354	19305	15075	20584
adj. <i>R</i> ²	0.581	0.579	0.631	0.533	0.648	0.509
Fixed effects:	Firm	Firm	Firm	Firm	Firm	Firm

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 2.3: Investment by Industry Characteristics

	<i>Dependent variable: Investment</i>					
	External-Finance Dependence		Equity Dependence		Information Asymmetry	
	Low	High	Low	High	Low	High
TIME	0.00188*** (5.58)	-0.00842*** (-3.99)	0.000153 (0.19)	-0.00273** (-2.36)	0.000711 (0.65)	-0.00445*** (-3.28)
TIMEXCASH	0.00415** (2.09)	0.0319*** (3.62)	0.0157*** (2.98)	0.0133*** (3.27)	0.0114** (2.09)	0.0258** (2.53)
tobinq	0.00855*** (10.26)	0.0102** (2.26)	0.00972*** (3.15)	0.00592*** (2.67)	0.00153 (0.74)	0.00815* (1.86)
cashflow	-0.0158 (-1.15)	-0.0575* (-1.72)	-0.0209 (-0.66)	-0.0568*** (-3.52)	-0.0398* (-1.71)	-0.0853** (-1.99)
_cons	-0.0461*** (-6.70)	-0.0302 (-0.81)	-0.0376 (-1.49)	-0.0224 (-1.21)	0.0166 (0.96)	-0.0235 (-0.66)
<i>N</i>	22499	13160	20024	15635	17852	17807
adj. <i>R</i> ²	0.562	0.595	0.788	0.523	0.540	0.536
Fixed effects:	Firm	Firm	Firm	Firm	Firm	Firm

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 2.4: Other Investment Measures

<i>Dependent variable: INVESTMENT</i>				
	(1)	(2)	(3)	(4)
	General Expense	R&D	NWC	Inventory
TIME	-0.0259 (-0.93)	-0.000121 (-0.84)	-0.115*** (-4.93)	-0.000812 (-0.98)
TIMEXCASH	0.0945 (0.96)	0.00230*** (2.68)	0.466*** (3.46)	0.0139*** (2.64)
tobinq	0.0144*** (3.71)	0.00360*** (8.21)	-0.340*** (-3.04)	0.0253*** (8.81)
cashflow	-1.704*** (-11.47)	-0.0413*** (-9.19)	1.301*** (2.63)	-0.0558*** (-2.94)
_cons	0.193*** (5.60)	-0.0162*** (-4.39)	3.381*** (3.66)	-0.0700*** (-2.93)
<i>N</i>	27552	14395	35031	18636
adj. R^2	0.369	0.910	0.970	0.940
Fixed effects:	Firm	Firm	Firm	Firm

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 2.5: Oil-Firm Investment Before and After the 2014 Oil Price Shock

	<i>Dependent variable: INVESTMENT</i>			
	(1)	(2)	(3)	(4)
TIME	-0.0163*** (-5.36)	-0.0211*** (-5.52)	-0.0154*** (-3.88)	-0.0189*** (-4.39)
TIME × CASH		0.0629** (2.09)	0.0635** (2.13)	0.0677** (2.29)
TIME × dOILPRICELAG			0.0423*** (9.90)	0.0413*** (9.47)
tobinq				0.00278 (0.45)
cashflow				-0.0559* (-1.66)
_cons	0.116*** (69.87)	0.115*** (67.65)	0.115*** (67.57)	0.0954** (2.09)
N	4336	4185	4185	4112
adj. R^2	0.390	0.389	0.392	0.401
Fixed effects:	Firm	Firm	Firm	Firm

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 2.6: Robustness Check: Oil-Firm Investment Controlled by Lagged Oil Price Change

	<i>Dependent variable: INVESTMENT</i>			
	(1)	(2)	(3)	(4)
TIME	-0.0189*** (-4.39)	-0.00976** (-2.36)	-0.0169*** (-4.48)	-0.0354*** (-9.28)
TIME × CASH	0.0677** (2.29)	0.0662** (2.29)	0.0640** (2.15)	0.0712** (2.36)
tobinq	0.00278 (0.45)	-0.000646 (-0.11)	0.00494 (0.80)	-0.00159 (-0.26)
cashflow	-0.0559* (-1.66)	-0.0405 (-1.33)	-0.0555 (-1.64)	-0.0476 (-1.49)
TIME × dOILPRICELAG	0.0413*** (9.47)			
TIME × dOILPRICELAG2		0.166*** (20.33)		
TIME × dOILPRICELAG3			0.0673*** (7.88)	
TIME × dOILPRICELAG4				-0.123*** (-11.29)
_cons	0.0954** (2.09)	0.121*** (2.71)	0.0795* (1.73)	0.128*** (2.83)
<i>N</i>	4112	4112	4112	4112
adj. <i>R</i> ²	0.401	0.451	0.402	0.414
Fixed effects:	Firm	Firm	Firm	Firm

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 2.7: Oil-Firm Investment by Financial Constraints

	<i>Dependent variable: Investment</i>							
	Kaplan-Zingales index		Whited-Wu index		Payout ratio		Assets	
	Low	High	Low	High	High	Low	High	Low
TIME	-0.0238*** (-4.93)	-0.00937 (-1.22)	-0.0179*** (-3.65)	-0.0233*** (-3.09)	-0.00146 (-0.20)	-0.0205*** (-3.16)	-0.0154*** (-3.84)	-0.0257*** (-3.09)
TIMEXCASH	0.133*** (3.75)	-0.0488 (-0.67)	0.116* (1.74)	0.0739** (2.02)	0.0241 (0.37)	0.0703* (1.77)	0.131*** (2.73)	0.0764** (2.09)
tobinq	-0.00792 (-0.98)	0.0186* (1.75)	0.0155 (1.58)	-0.0103 (-1.42)	-0.00702 (-0.41)	0.00190 (0.26)	0.0300*** (3.45)	-0.0106 (-1.15)
cashflow	-0.117*** (-3.04)	0.00565 (0.76)	-0.0494 (-1.11)	-0.0644** (-2.26)	0.382** (2.14)	-0.0576 (-1.58)	-0.0706*** (-2.71)	-0.0477** (-2.31)
TIME × dOILPRICELAG	0.0396*** (8.12)	0.0427*** (4.87)	0.0563*** (10.21)	0.0237** (2.50)	0.0424*** (2.70)	0.0392*** (6.52)	0.0468*** (9.70)	0.0399*** (4.91)
_cons	0.173*** (2.92)	-0.0244 (-0.30)	-0.0207 (-0.28)	0.210*** (4.00)	0.150 (1.16)	0.102* (1.92)	-0.110* (-1.66)	0.193*** (2.92)
<i>N</i>	2722	1390	2010	2102	1236	2876	2204	1908
adj. <i>R</i> ²	0.388	0.463	0.480	0.460	0.483	0.390	0.431	0.411
Fixed effects:	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Chapter 3

BANK-LEVEL SYSTEMIC RISK AND COMMONALITY OF ASSET HOLDINGS

Systemic risk is risk associated with the relationship between the financial system and individual banks. Some papers interpret systemic risk as each individual bank's contribution to the overall market risk while others view systemic risk as exposure to the overall market risk. Either interpretation is plausible and useful for understanding the concept of systemic risk. In this paper, I investigate what the major component that drives systemic risk is. An extensive literature on financial contagion explains the commonality of asset holdings can propagate shocks through fire sales. By suggesting the commonality of asset holdings measure, I argue that the commonality of asset holdings makes the financial system more interconnected, and more vulnerable to systemic events due to financial contagion channels. I find that commonality of asset holdings plays a significant role in measuring systemic risk. Based on my result, I detect limitations on some of the existing systemic risk measures, so I further suggest a new method of estimating systemic risk.

3.1 Introduction

Systemic risk is defined as risk from the failure of a firm (or a small number of firms) that triggers a collapse in the entire system due to the interconnectedness of financial markets. The extensive literature on systemic risk has become an intersection of finance, macroeconomics, econometrics, and network theory. The theoretical framework has been long developed to identify the main mechanisms behind the systemic risk.¹ However, the empirical literature surged after the Financial Crisis in 2008, which reflected many features of systemic risk that

¹Bernanke and Gertler (1989); Rochet and Tirole (1996); Allen and Gale (2000); Acharya (2009)

are described in the theoretical studies. Both academic researchers and financial regulators put much effort to construct systemic risk measures that capture the risk accumulation in the financial system.

As described in Benoit, Colliard, Hurlin, and Pérignon (2017), there are two distinctive approaches of measuring the systemic risk in the literature. First, a strand of the literature studies the sources of systemic risk, such as correlation risk², liquidity risk³, and tail risk⁴. Also, sources like balance sheet contagion⁵, informational contagion⁶, liquidity crises⁷, and bank runs⁸ describe the contagion and amplification feature of systemic risk. The other part of the literature aims to establish more general measures of systemic risk, encompassing various contagion mechanisms studied in the source-specific systemic risk literature. The general approach often uses market information to derive statistical measures of systemic risk or describe the interconnectedness within the financial system. Hence, this approach provides simplicity compared to macroprudential tools based on source-specific analysis. However, it also comes with costs because the regulation based on these statistical measures can be unreliable and lack intuition. Also, the statistical measures provide different selection of the Systemically Important Financial Institutions (SIFIs). So in order to develop statistical measures as a part of regulatory device, they need to be consistent and provide intuitive understanding of the mechanism. Most well-known statistical systemic risk measures that use market information are Marginal Expected Shortfall (*MES* / *SRISK*)⁹ and conditional value-

²see Farhi and Tirole (2012); Lehar (2005); Blei and Ergashev (2014)

³see Brunnermeier and Oehmke (2013); Brunnermeier, Gorton, and Krishnamurthy (2014); Jobst (2014)

⁴see Perotti, Ratnovski, and Vlahu (2011); Freixas and Rochet (2013)

⁵see Allen and Gale (2000); Freixas, Parigi, and Rochet (2000); Elsinger, Lehar, and Summer (2006); Drehmann and Tarashev (2011)

⁶see Y. Chen (1999); Calomiris and Mason (1997); Bae (2003)

⁷see Shleifer and Vishny (1992); Brunnermeier and Pedersen (2009); Greenwood, Landier, and Thesmar (2015)

⁸see Diamond and Dybvig (1983); Calomiris and Kahn (1991); Martin, Skeie, and Thadden (2014); Iyer and Peydró (2011); Iyer and Puri (2012)

⁹By Acharya, Pedersen, Philippon, and Richardson (2010) and Brownlees and Engle (2016) respectively.

at-risk ($CoVaR / \Delta CoVaR$)¹⁰, which estimate the expected loss for each financial institution given the market being in distress.

In this study, I cover both source-specific and general approach. For the source-specific approach, I extend financial contagion literature to exploit the role of commonality of asset holdings in measuring systemic risk. Three main channels of contagion in the literature are direct linkages between banks, information contagion, and commonality of asset holdings. Direct linkages between banks are quite obvious interconnectedness as once bank defaults, it can propagate stress to other creditor banks (see Allen and Gale (2000); Gorton and Metrick (2012); Giglio, Kelly, and Pruitt (2016)). As studied in Y. Chen (1999), information contagion channel describes the behavior of investors: if one bank is in distress, investors reassess the risk of other banks with similar exposures. Lastly, the commonality of asset holdings channel propagates shocks through fire sales, when banks need to sell assets to reduce their leverage. Inspired by Cai, Eidam, Saunders, and Steffen (2018) that develops an interconnectedness measure by using the syndicated loan data, I suggest another measure using both syndicated loan data and banks' balance sheet data. I find that commonality of asset holdings can increase systemic risk through financial contagion. Since the financial system is most vulnerable during recessions, I also test whether the impact of commonality on systemic risk gets more severe during recession periods. I find that commonality in asset holdings increase systemic risk measure $CoVaR$, but not so much for $LRMES$ and $SRISK$. This inconsistent result may arise from different conditioning information among the measures. $CoVaR$ not only rely on market return for the conditioning information but also it takes account other state variables that can affect each financial institutions' stock return. On the other hand, $LRMES$ only depend on market return as the conditioning information, and $SRISK$ uses the non-linear transformation of $LRMES$. I claim that this inconsistent result is due to the lack of conditioning information, so I suggest a way to improve both $LRMES$ and $SRISK$ measures by incorporating bank-specific information into the calculation of systemic

¹⁰Adrian and Brunnermeier (2016)

risk. Since each bank has different exposure to each source of systemic risk, I believe that this information can improve the statistical measures that consider the market return as the only source of systemic risk. Specifically, I look at each bank's loan portfolio to approximate exposures to each sector-specific shock grouped by 2-digit SIC codes. I find the weights of loans to each sector for each bank. Then I calculate systemic risk measure conditioning on the sector return instead of the market return and find the weighted average of systemic risk measures. Although there are loan types other than the commercial loans in the banks' portfolio, shock exposures in the commercial loans can improve the market-based statistical measures by adding the source of systemic risk to the conditioning information.

3.2 Literature Review

As mentioned in the previous section, the theoretical framework has been established long ago, but there was a surge in demands for empirical exercises recently after the financial crisis in 2007-2008. Both academics and regulators searched for a robust and consistent measure of systemic risk to prevent another financial disaster. In this section, I review papers that have significant contributions to systemic risk literature.

The literature of systemic risk can be divided into two families by the source-specific approach and general approach. Benoit et al. (2017) surveys the theoretical framework and empirical evidence by each approach.¹¹

The source of systemic risk is categorized into systemic risk-taking, contagion, and amplification. In systemic risk-taking category, correlation risk is a type of source as Acharya (2009) explained the mechanism of how banks have incentives to invest in the same assets and thus fail or survive together.¹² Liquidity risk is another form of systemic risk-taking that banks invest too much in illiquid assets that lead to an aggregate liquidity shortages.¹³ Tail risk is a source that presents the commonality of the banks' risk exposures being not

¹¹Y.-A. Chen (2015) also helped to form this section.

¹²see also Farhi and Tirole (2012); Lehar (2005); Blei and Ergashev (2014)

¹³see Brunnermeier and Oehmke (2013); Brunnermeier et al. (2014); Jobst (2014)

only correlated, but also being large.¹⁴

The contagion approach studies how losses in a financial institution spillover to other institutions that are linked. Balance sheet contagion describes the interbank links that can propagate bank defaults through domino effects due to the connectedness through the balance sheets. Allen and Gale (2000) show that a complete interbank network (all banks are connected to each other) allows banks to engage in risk-sharing, but an incomplete network (some links are missing in the interbank market) tends to generate contagion through the hub bank.¹⁵ Moreover, information contagion occurs if depositors and investors believe that the failure of a bank is a signal on the health of another bank, and there is an informational link that is a potential channel of contagion. Y. Chen (1999) explains the bank run by observing a run on one bank makes uninformed depositors run on other because the banks' returns are correlated.¹⁶

Lastly, amplification mechanisms explain why relatively small shocks can lead to large aggregate impacts. A leading example is liquidity-driven crises where financial institutions liquidate their assets due to the drop in market prices, and the new sales amplify the downturn leading to further sales and so on.¹⁷

Recently developed systemic risk measures are not targeted to a source of systemic risk or a channel of transmission. Instead, having markets being efficient, these measures attempt to assess the financial condition of the financial institutions using the current market prices of securities. These measures can be computed with market data but lack theoretical foundation and do not provide an identification of the source of the risk.

Two major market-based measures are *MES* by Acharya et al. (2010) and $\Delta CoVaR$ by Adrian and Brunnermeier (2016). The interpretation of these two measures can be slightly different as one views the systemic risk as a firm's exposure to the market risk and the

¹⁴see Perotti et al. (2011); Freixas and Rochet (2013)

¹⁵see also Freixas et al. (2000); Elsinger et al. (2006); Drehmann and Tarashev (2011)

¹⁶see also Calomiris and Mason (1997); Bae (2003)

¹⁷see Shleifer and Vishny (1992); Brunnermeier and Pedersen (2009); Greenwood et al. (2015)

other views as a firm's contribution to the market risk. Acharya et al. (2010) focus on high-frequency marginal expected shortfall (*MES*) as a systemic risk measure and find which institutions are most exposed to a financial crisis. Brownlees and Engle (2016) extends this marginal expected shortfall to calculate capital shortfall of individual institutions conditional on the market being in distress (*SRISK*). For a contribution point of view, Adrian and Brunnermeier (2016) introduces a concept of conditional Value-at-risk, also called as *CoVaR*, which calculates the component of systemic risk associated with a particular institution.

3.2.1 Exposure to Systemic Risk: *MES* / *SRISK*

Acharya et al. (2010) presents a simple model of systemic risk and show that each financial institution's exposure to systemic risk can be measured as its systemic expected shortfall (*SES*), which is its propensity to be undercapitalized when the system as a whole is undercapitalized. This measure is developed by the concept of the expected shortfall, which calculates the expected firm's equity loss w_1^i conditional on its equity being below zero:

$$ES^i \equiv -E [w_1^i | w_1^i < 0]$$

The systemic expected shortfall for a firm i , SES^i is the amount a bank's equity w_1^i drops below its target level:

$$SES^i \equiv E [za^i - w_1^i | W_1 < zA]$$

where z is a fraction, a^i is assets of firm i , W_1 is the aggregate banking capital, and A is the aggregate assets in the system. To focus on empirical analysis on the cross-sectional systemic risk, the authors control for each firm's size by initial equity,

$$\frac{SES^i}{w_0^i} = \frac{za^i}{w_0^i} - 1 - E \left[\frac{w_1^i}{w_0^i} - 1 | W_1 < zA \right]$$

where the first part $za^i/w_0^i - 1$ being a positive value hints that the firm is already undercapitalized, and the second part is the expected equity return conditional on the occurrence of a crisis. Hence, the sum of the two terms determines whether the firm will be undercapitalized in a crisis. If one defines the crisis as the worst 5% market outcomes, denoted by

$I_{5\%}$, the marginal expected shortfall (MES) can be defined by the second part of the above equation using net equity returns of firm i during these crises:

$$MES_{5\%}^i \equiv -E \left[\frac{w_1^i}{w_0^i} - 1 | I_{5\%} \right]$$

Then empirically, to estimate the systemic expected shortfall (SES), the authors use the marginal expected shortfall (MES) and leverage estimated as follow.

$$\begin{aligned} MES_{5\%} &= \frac{\sum_{system\ in\ its\ 5\%\ tail} R_t}{\#\ of\ days} \\ LVG &= \frac{book\ assets - book\ equity + market\ equity}{market\ value\ of\ equity} \end{aligned}$$

Brownlees and Engle (2016) suggests another measure of systemic risk SRISK by using marginal expected shortfall (MES) developed in Acharya et al. (2010). SRISK measures the capital shortfall of a firm conditional on a severe market decline and is a function of the firm's size, leverage, and risk. Intuitively, this measure provides early warning signals of distress. The capital shortfall of firm i on the day t is defined as:

$$CS_{i,t} = kA_{i,t} - W_{i,t} = k(D_{i,t} + W_{i,t}) - W_{i,t}$$

where $W_{i,t}$ is the market value of equity, $D_{i,t}$ is the book value of debt, $A_{i,t}$ is the value of quasi assets and k is the prudential ratio. Since the objective is to predict the capital shortfall of a financial firm in case of a systemic event, they define the systemic event as a market decline below a threshold C over a time horizon h . SRISK is then defined as:

$$\begin{aligned} SRISK_{i,t} &= E_t(CS_{i,t+h} | R_{m,t+1:t+h} < C) \\ &= kE_t(D_{i,t+h} | R_{m,t+1:t+h} < C) - (1 - k)E_t(W_{i,t+h} | R_{m,t+1:t+h} < C) \end{aligned}$$

In order to carry out this expectation, they assume that in the case of systemic event, debt cannot be renegotiated, implying that $E_t(D_{i,t+h} | R_{m,t+1:t+h} < C) = D_{i,t}$. So,

$$\begin{aligned} SRISK_{i,t} &= kD_{i,t} - (1 - k)W_{i,t}(1 - LRMES_{i,t}) \\ &= W_{i,t}[kLVG_{i,t} + (1 - k)LRMES_{i,t} - 1] \end{aligned}$$

where $LVG_{i,t}$ denotes the quasi-leverage ratio $(D_{i,t} - W_{i,t})/W_{i,t}$ and

$$LRMES_{i,t} = -E_t(R_{i,t+1:t+h} | R_{m,t+1:t+h} < C)$$

is the long run MES. Brownlees and Engle (2016) estimate $LRMES_{i,t}$ by making assumptions that the firm returns $r_{i,t}$ and the market returns $r_{m,t}$ follow an unspecified distribution conditional on the information set F_{t-1} with zero mean and time varying covariance. Having these assumptions, they use TGARCH-DCC to specify the time varying volatility and correlation, but since $LRMES$ is not available in a closed form in this setting, the Monte Carlo simulation is adopted to find average returns. $LRMES_{i,t}$ with sample S at horizon h is computed as:

$$LRMES_{i,t} = \frac{\sum_{s=1}^S R_{i,t+1:t+h}^s I\{R_{m,t+1:t+h}^s < C\}}{\sum_{s=1}^S I\{R_{m,t+1:t+h}^s < C\}}$$

Then finally the total amount of systemic risk in the financial system is calculated as,

$$SRISK_t = \sum_{i=1}^N \max(SRISK_{i,t}, 0)$$

Contribution to Systemic Risk: CoVaR

Adrian and Brunnermeier (2016) suggests conditional value-at-risk (CoVaR) approach to measure the systemic risk in the financial system. Although I categorize this paper as measuring systemic risk having direction from a firm to the entire financial system, the authors include the reverse conditioning called, *Exposure- Δ CoVaR*, which is a measure of an individual firm's exposure to system-wide distress, similar to Acharya et al. (2010) and Brownlees and Engle (2016). One clear distinction from other market based systemic risk measures, is that this paper addresses the stylized fact that risk builds up in the background during boom phases characterized by low volatility and materializes only in crisis times, naming it as "volatility paradox."

An institution's contribution to systemic risk is defined as the difference between *CoVaR* of a financial firm i being in distress and *CoVaR* of the firm being in the median state. By denoting $CoVaR_q^{j|C(X^i)}$ as the *VaR* of firm j (usually the financial system) conditional on

some event $C(X^i)$, where X^i is the return loss of firm i , $CoVaR_q^{j|C(X^i)}$ is implicitly defined by the $q\%$ -quantile of the conditional probability distribution:

$$Pr\left(X^j|C(X^i) \leq CoVaR_q^{j|C(X^i)}\right) = q\%$$

Then the systemic risk of firm i can be defined as:

$$\Delta CoVaR_q^{j|i} = CoVaR_q^{j|X^i=VaR_q^i} - CoVaR_q^{j|X^i=VaR_{50}^i}$$

Using weekly returns data and state variables including three-month yield change, term spread change, TED spread, credit spread change, market return, real estate excess return, and VIX, quantile regression is described as follows:

$$\begin{aligned} X_t^i &= \alpha_q^i + \gamma_q^i \mathbf{M}_{t-1} + \varepsilon_{q,t}^i \\ X_t^{system|i} &= \alpha_q^{system|i} + \gamma_q^{system|i} \mathbf{M}_{t-1} + \beta_q^{system|i} X_t^i + \varepsilon_{q,t}^{system|i} \end{aligned}$$

where \mathbf{M}_{t-1} is a vector of lagged state variables. Then the time-varying $CoVaR_{q,t}^i$ and $VaR_{q,t}^i$ is estimated as,

$$\begin{aligned} VaR_{q,t}^i &= \hat{\alpha}_q^i + \hat{\gamma}_q^i \mathbf{M}_{t-1} \\ CoVaR_{q,t}^i &= \hat{\alpha}_q^{system|i} + \hat{\gamma}_q^{system|i} \mathbf{M}_{t-1} + \hat{\beta}_q^{system|i} VaR_{q,t}^i \end{aligned}$$

Then finally $\Delta CoVaR_{q,t}^i$ for each institution is computed:

$$\begin{aligned} \Delta CoVaR_{q,t}^i &= CoVaR_{q,t}^i - CoVaR_{50,t}^i \\ &= \hat{\beta}_q^{system|i} (VaR_{q,t}^i - VaR_{50,t}^i) \end{aligned}$$

In summary, $\Delta CoVaR$ is a measure of systemic risk that captures the directed tail-dependency between an institution and the financial system as a whole. The appealing feature of this measure is that $\Delta CoVaR$ takes the stylized fact into account, which systemic risks typically build in times of low volatility and materialize during the crises.

There are also other studies that develop general measures of systemic risk. Billio, Getmansky, Lo, and Pelizzon (2012) use Granger causality tests and principal component analysis to measure the interconnectedness of financial institutions. Similarly, Diebold and Yilmaz

(2014) use VAR framework to compute the volatility connectedness by decomposing forecast error variance of stock returns. This approach also allows finding directional spillovers of return volatility. Huang, Zhou, and Zhu (2009) propose a hypothetical insurance premium against systemic financial distress where total losses exceed a given threshold of 15% of major banks' liability. They use CDS spread to calculate the probability of default as well as high-frequency equity data to calculate forecasted asset return correlation. They estimate the Distressed Insurance Premium (DIP) with these two variables using Monte Carlo simulation. Oh and Patton (2018) finds a joint probability of distress based on a dynamic copula model of CDS spreads.

3.3 Methodology

In this section, I describe how the commonality of asset holdings measure is constructed. As suggested in Cai et al. (2018), syndicated loan data allows detailed information about each loan deals. Using TR Dealscan syndicated loan data, we can track which firm borrowed from which bank along with information including loan spread, loan amount, maturity, and etc. This data is useful in approximating risk exposure for each bank. For example, if we filter loan deals by SIC industry code, we can calculate risk exposure in loan deals to each industry. Cai et al. (2018) only focuses on 2-digit SIC code to construct risk exposure to each industry. In order to fully utilize this data, I suggest to breakdown the exposure to the firm-level, not to industry level. This way, it is more appealing to explain the commonality of asset holdings concept because each syndicated loan deal consists of one borrower and multiple lenders. If the borrower defaults, all banks within the loan deal face the same risk exposure that can hurt their balance sheets. If two banks share similar syndicated loan portfolio, then their risk exposure must be similar, and hence their interconnectedness is very strong. In contrast, if two banks' portfolios are not very common, their risk exposure must be very different and hence have weak interconnectedness. Having each bank's loan portfolio weights, I calculate the Euclidean distance between bank i and bank j to find the

distance (or difference) between two banks' loan portfolio as follows:

$$Distance_{i,j,t} = \sqrt{\sum_{p=1}^P (w_{i,p,t} - w_{j,p,t})^2}$$

where p indicates the portfolio level such as firm-level, 4-digit SIC level, 3-digit SIC level, and 2-digit SIC level. In Section 3.5, I present all different levels of aggregation. After calculating distances for all possible pairs, I aggregate the paired distances for each bank and compute commonality of asset holdings measure.

$$Commonality_{i,t} = \left(1 - \sum_{j \neq i} x_{i,j,t} \cdot Distance_{i,j,t} \right) \times 100$$

where $x_{i,j,t}$ is either equal-weight or size-weight when aggregating the distances. Intuitively, large banks must have more interconnectedness with other banks, so their distances are weighted more than the small banks. In Section 3.5, I first present what are the main component that explains the variation in commonality measure. Then I test whether commonality increases systemic risk measures with the following regression model:

$$\begin{aligned} SYSTEMIC\ RISK_{i,t} = & \alpha_i + \beta_1 SYSTEMIC\ RISK_{i,t-1} + \beta_2 COMMONALITY_{i,t} + \beta_3 BETA_{i,t} \\ & + \beta_4 LEVERAGE_{i,t} + \beta_5 ASSETS_{i,t} + \gamma_i + \varepsilon_{i,t} \end{aligned}$$

where *SYSTEMIC RISK* can be one of *CoVaR*, *LRMES*, and *SRISK*. *BETA* is the market beta that captures the market return exposure, and *LEVERAGE* captures the financial position of each bank. *ASSETS* is included to control for the size of each bank. From this model, I expect β_2 to be positive and statistically significant as commonality of asset holdings increases systemic risk. Further, I check whether commonality plays a larger role during recessions. If the banking system is highly interconnected, a shock that can lead the economy into a recession must damage more severely. To test this hypothesis, using the

NBER recession indicators, I estimate the following regression model: ¹⁸

$$\begin{aligned} \text{SYSTEMIC RISK}_{i,t} = & \alpha_i + \beta_1(\text{COMMONALITY}_{i,t} \times \text{EXPANSION}_t) \\ & + \beta_2(\text{COMMONALITY}_{i,t} \times \text{EXPANSION}_t) \\ & + \beta_3\text{RECESSION}_t + \beta_4\text{SYSTEMIC RISK}_{i,t-1} + \gamma_i + \varepsilon_{i,t} \end{aligned}$$

and to check whether commonality increases systemic risk more severely during recessions, I test the following hypothesis:

$$H_0 : \beta_2 - \beta_1 = 0$$

If the null hypothesis is rejected, it implies that commonality of asset holdings does influence systemic risk more during recessions.

Since syndicated loans are only a portion of a bank's entire loan portfolio, I do the robustness check by using Bank Regulatory Data (FR Y-6), which is a quarterly report of bank-level balance sheet information. This data does not specify both borrower and lender, so it may not be appropriate to approximate risk exposure to specific borrowers or industries. However, it is possible to understand the structure of their loan activities. For example, the data specifies total loan amount to depository institutions, to commercial and industrials, to household, to farmers, etc. So this allows us to see another aspect of commonality in asset holdings, which is the structure of the balance sheet. This aspect is also related to information contagion channel where investors reassess the risk of a bank when a systemic event occurs. For example, if a bank with large loan amounts to farmers is in distress due to massive defaults following a natural disaster, investors reassess the risk level for other banks with similar loans. I present this alternative commonality measure also increases systemic risk in Section 3.5.

Furthermore, I suggest a way to improve existing systemic risk measures. Unlike *Co-VaR*, *LRMES* and *SRISK* rely only on market return as the conditioning information. If commonality in asset holdings does play an important role in systemic risk, there needs to

¹⁸This model is adopted from Cai et al. (2018)

be more information besides market return to estimate systemic risk accurately. Hence, I focus on improving two measures *LRMES* and *SRISK* by incorporating loan portfolio to approximate risk exposure to each sector. Since *LRMES* and *SRISK* are estimated differently from *CoVaR*, they often produce very different lists of systemically risky banks. Figure 3.1 displays no correlation between *LRMES* and $\Delta CoVaR$ in 2018Q1, which is the time little before the collapse of Lehman Brothers. Although both measures indicate a group of risky banks, they do not provide a consistent and credible rank at the bank level. This implies the need for a composition of statistical methodology and economic intuition to produce a reliable standard of systemic risk.

Using the syndicated loan data, I calculate the weights to each sector for each bank.¹⁹ Although the syndicated loan data only covers a portion of a bank's portfolio, it is a good proxy of exposures to the shocks from the other sectors. Once the weights are computed, I use the sector returns as the conditioning information of systemic risk calculation. For example, original calculations of *MES* and *CoVaR* are as below:

$$\begin{aligned}
 MES_{i,t} &= -E(R_{i,t} | R_{m,t} < c) \\
 LRMES_{i,t} &= -E_t(R_{i,t+1:t+h} | R_{m,t+1:t+h} < C) \\
 SRISK_{i,t} &= kD_{i,t} - (1 - k)W_{i,t}(1 - LRMES_{i,t}) \\
 &= W_{i,t}[kLVG_{i,t} + (1 - k)LRMES_{i,t} - 1]
 \end{aligned}$$

where i is a financial institution, and m represents the entire market. If for each bank, i has a bank portfolio weights w_j for each sector j , then we can modify the above equations to

¹⁹Calculating weights at the firm level has a computational burden and may contain too much noise, so I suggest estimating at a broader level.

find the weighted average of the systemic risk measures:

$$\begin{aligned}
 iMES_{i,t} &= -\sum_{j=1}^J w_{i,j,t} * E(R_{i,t} | R_{j,t} < c) \\
 iLRMES_{i,t} &= -\sum_{j=1}^J w_{i,j,t} * E_t(R_{i,t+1:t+h} | R_{j,t+1:t+h} < C) \\
 iSRISK_{i,t} &= kD_{i,t} - (1 - k)W_{i,t}(1 - iLRMES_{i,t})
 \end{aligned}$$

In Section 3.5, I compare the new measures to the existing ones and the impact of commonality in asset holdings on these measures.

3.4 Data

The main data that is used in this study is TR Dealscan Syndicated Loan Data. This data entails loan origination details that specify both borrower's and lender's information. For all balance sheet information, I use Compustat quarterly fundamentals data provided by WRDS. For analyzing banks' entire loan portfolio for the robustness check, I use bank regulatory data (FR Y-6) to compute alternative commonality measure. For systemic risk measures *CoVaR*, *LRMES*, and *SRISK*, I use data provided by the authors.^{20 21}

3.5 Results

Since commonality measures are essentially calculated to reflect the interconnectedness of banks, it is of interest to see what makes interconnectedness strong and weak. Table 3.1 presents pairwise correlation of commonality, assets, loans, diversification, and specialization. Total asset is included to reflect size influence on interconnectedness and loans to capture syndicated loan market activity. If a bank has a large size and actively lending in the syndicated loan market, it is more likely to be interconnected with many other banks. Diversification is calculated by the sum of squared weights, and specialization is a number

²⁰Adrian and Brunnermeier (2016); Brownlees and Engle (2016)

²¹These data have been explained thoroughly in previous chapters.

of loan deals a bank is engaged in. As displayed in the table, diversification has the largest correlation with commonality measure. This may not be too surprising because the way the commonality measure is calculated involves weights to each borrower or industry. However, intuitively, a bank engaging in lending to more borrowers, has more chance of connecting to other banks. Table 3.2 displays four different regressions. When computing the commonality measure, the weights can be calculated at the firm level or at the industry level. Also, when aggregating calculated distances, Table 3.2 uses equal weights. Model (1) uses the firm-level weights measure as the dependent variable and Model (2), (3), and (4) vary by the number of SIC digits used in calculating the weights. Again, diversification is significantly contributing to the commonality measure throughout different aggregation methods. Table 3.3 presents similar models but uses commonality measures calculated by size weights when aggregating the distances. Consistent to the previous table, diversification plays an important role in the commonality measure.

To see if commonality in asset holdings increases systemic risk, I use various existing firm-level systemic risk measures, as shown in Table 3.4. For the regressors other than the commonality measure, I include beta to capture the market exposure, leverage to control for each bank's financial position, and assets to account for size effect on systemic risk measures. Also, I include the 1-period lag term for each systemic risk measure. For both *CoVaR* measures, we see positive and statistically significant coefficients, which are consistent with my hypothesis that commonality in asset holdings increase systemic risk. However, the direction is opposite for *LRMES* coefficient, and there is no significance in *SRISK* coefficient. This is the motivation that is explained in Section 3.3 for improving *LRMES* and *SRISK* for their lack of conditioning information. Table 3.5 shows similar results for size-weighted commonality measures. Again the coefficients for the commonality measure are inconsistent in terms of signs and statistical significance. The lag terms contribute significantly to systemic risk measures.

The inconsistency that is observed in previous results may be due to the usage of the full sample. Therefore, it makes sense to distinguish the impact of the commonality in asset

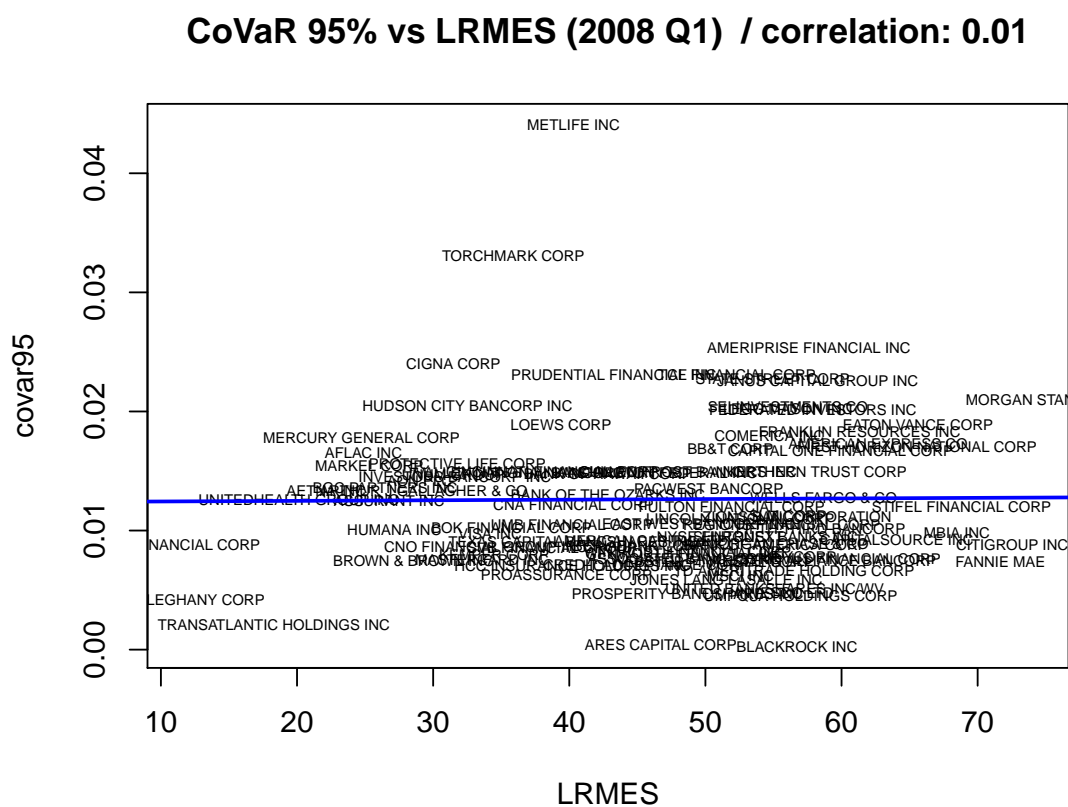
holdings during recession and expansion periods. Table 3.6 includes the interaction term between commonality and expansion (recession). If the commonality increases systemic risk more during recessions, our null hypothesis described in Section 3.3 must be rejected. In the bottom of Table 3.6, the hypothesis testing is displayed. Unlike previous results, now all the coefficients of *CoVaR* and *LRMES* indicate the null hypothesis is rejected, which means that the commonality increases systemic risk more during recessions. *SRISK* coefficient is not statistically significant, and this is partly due to the non-linear transformation during the calculation of *SRISK*.

To alleviate concerns of the syndicated loan being only a part of each bank's balance sheet, I use bank regulatory data to calculate weights to each loan category. Compared to syndicated loan data, this approach cannot specify the destination of loans in detail, but it covers a broader loan portfolio of each bank. Table 3.7 reports similar results as in Table 3.4. The coefficients for *CoVaR* are positive and significant while coefficients for the other two measures have opposite signs. Again, the explanation for this inconsistency is similar to previous cases.

To overcome inconsistent results, I propose new measures for *LRMES* and *SRISK* by improving conditioning information with loan portfolio. As a result, the sign and statistical significance are improved, as shown in Table 3.8. Model (1)-(4) are the same systemic risk measures as the previous exercise. Model (5) and (6) reports improved systemic risk measures. By comparing commonality coefficients of Model (3) and (4) to Model (5) and (6), it is clear that additional conditioning information improves the signs and significance. In addition, by comparing pairwise correlation matrices Table 3.10 and Table 3.11, we see that the correlation between systemic risk measure improves significantly. To argue that these new measures not only increase the correlation with other measures during the recession but also during non-recession periods, I produce pairwise correlation matrices only for recession periods. As reported in Table 3.12 and Table 3.13, correlation with other measures increase significantly when using new measures during non-recession periods.

3.6 Conclusion

Syndicated loans attract banks for their favorable risk-sharing feature. This results in banks having similar loan portfolio, which directly indicates an increase in the commonality of asset holdings. Banks become more interconnected since multiple banks are tied to the same loan deal. If a default occurs, banks that are lending to the firm share the same risk exposure. If the magnitude of defaults is massive during systemic events, these interconnected banks can collapse altogether. In this study, I highlight the importance of commonality in asset holdings in financial stability. Conceptually, I relate market-based systemic risk measures with balance sheet contagion channel. As a result, I find that commonality in asset holdings increases systemic risk, and the increase is more prevalent during recessions. Further, I suggest new systemic risk measures by improving existing ones with more conditioning information.

Figure 3.1: $LRMES$ vs. $\Delta CoVaR$ 

Note: Above figure plots $LRMES$ by Brownlees and Engle (2016) and $\Delta CoVaR$ by Adrian and Brunnermeier (2016). For both measures, I plotted 2008Q1 data, which is the time just before the collapse of Lehman Brothers. The figure displays no correlation between the two systemic risk measures.

Table 3.1: Pairwise Correlation

	commonality	assets	loans	diversification	specialization
commonality	1	0.329	0.312	0.939	0.365
assets	0.329	1	0.926	0.207	0.401
loans	0.312	0.926	1	0.199	0.382
diversification	0.939	0.207	0.199	1	0.240
specialization	0.365	0.401	0.382	0.240	1

Table 3.2: Commonality Determinants (Equal-weighted)

	<i>Dependent variable: Commonality</i>			
	(1)	(2)	(3)	(4)
Weights:	Firm-level	SIC (2 digits)	SIC (3 digits)	SIC (4 digits)
assets	-0.00000125 (-0.48)	-0.00000389** (-2.19)	-0.00000371*** (-2.80)	-0.00000390*** (-2.81)
loans	0.000000272 (0.04)	0.00000270 (0.66)	0.00000461 (1.54)	0.00000325 (1.09)
diversification	0.853*** (8.15)	0.388*** (3.56)	0.531*** (5.93)	0.651*** (8.63)
specialization	-0.00450*** (-3.55)	0.00271** (2.69)	0.00141* (1.81)	0.00134 (1.58)
_cons	-8.229 (-0.86)	48.48*** (4.97)	30.47*** (3.80)	17.82** (2.63)
<i>N</i>	2264	2264	2264	2264
adj. <i>R</i> ²	0.933	0.730	0.843	0.899
Fixed effects:	Bank	Bank	Bank	Bank

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 3.3: Commonality Determinants (Bank-size-weighted)

	<i>Dependent variable: Commonality</i>			
	(1) Firm-level	(2) SIC (2 digits)	(3) SIC (3 digits)	(4) SIC (4 digits)
assets	0.00000189 (1.00)	-0.00000320 (-1.67)	-0.00000257 (-1.50)	-0.00000248 (-1.60)
loans	0.00000385 (0.77)	0.00000537 (1.13)	0.00000760* (1.86)	0.00000637* (1.72)
diversification	0.904*** (7.72)	0.420*** (3.38)	0.568*** (5.47)	0.690*** (7.90)
specialization	-0.00317*** (-2.77)	0.00337*** (2.78)	0.00231** (2.33)	0.00238** (2.36)
_cons	-5.738 (-0.54)	49.89*** (4.47)	32.31*** (3.46)	19.88** (2.53)
<i>N</i>	2264	2264	2264	2264
adj. R^2	0.958	0.756	0.859	0.911
Fixed effects:	Bank	Bank	Bank	Bank

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 3.4: Systemic Risk and Commonality Measures (Equal-weighted)

	<i>Dependent variable: Systemic Risk Measures</i>			
	(1)	(2)	(3)	(4)
	covar95	covar99	lrmes	SRISK
commonality	0.00307*** (3.08)	0.00288** (2.46)	-0.0162** (-2.42)	47.25 (0.03)
beta	-0.00271** (-2.46)	-0.00316** (-2.61)	0.230*** (43.50)	-13780.7*** (-7.73)
leverage	0.0000749*** (3.51)	0.0000977** (2.62)	0.000961*** (13.26)	537.4*** (13.31)
assets	5.86e-10 (1.60)	3.77e-10 (0.91)	8.40e-10 (0.38)	0.00137 (1.48)
covar95_lag	0.545*** (44.86)			
covar99_lag		0.563*** (39.52)		
lrmes_lag			0.0643*** (15.96)	
SRISK_lag				0.840*** (22.52)
_cons	0.0105*** (10.21)	0.0107*** (8.69)	0.101*** (21.06)	6122.4*** (5.62)
<i>N</i>	1108	1108	2083	2083
adj. <i>R</i> ²	0.512	0.685	0.970	0.899
Fixed effects:	Bank	Bank	Bank	Bank

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 3.5: Systemic Risk and Commonality Measures (Bank-size-weighted)

	<i>Dependent variable: Systemic Risk Measures</i>			
	(1)	(2)	(3)	(4)
	covar95	covar99	lrmes	SRISK
commonality	0.00172** (2.38)	0.00150* (1.82)	-0.0143** (-2.10)	206.7 (0.14)
beta	-0.00271** (-2.48)	-0.00314** (-2.61)	0.230*** (43.46)	-13778.2*** (-7.71)
leverage	0.0000695*** (3.30)	0.0000923** (2.48)	0.000961*** (13.22)	537.7*** (13.29)
assets	4.97e-10 (1.37)	2.85e-10 (0.69)	9.38e-10 (0.42)	0.00138 (1.49)
covar95_lag	0.552*** (46.42)			
covar99_lag		0.568*** (40.85)		
lrmes_lag			0.0641*** (15.92)	
SRISK_lag				0.840*** (22.57)
_cons	0.00952*** (11.45)	0.00973*** (9.79)	0.102*** (20.11)	6241.1*** (5.60)
<i>N</i>	1108	1108	2083	2083
adj. <i>R</i> ²	0.511	0.685	0.970	0.899
Fixed effects:	Bank	Bank	Bank	Bank

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 3.6: The Effect of Commonality on Systemic Risk Measures During Recessions

	<i>Dependent variable: Systemic Risk Measures</i>			
	(1)	(2)	(3)	(4)
	covar95	covar99	lrmes	SRISK
commonality \times expansion	-0.000709 (-0.64)	-0.00125 (-1.01)	0.0174* (1.74)	-730.9 (-0.59)
commonality \times recession	0.00829*** (4.34)	0.00431** (2.05)	0.0869*** (5.45)	-150.3 (-0.08)
recession	-0.00464*** (-2.95)	-0.00318* (-1.80)	-0.0315** (-2.32)	-770.8 (-0.46)
covar95_lag	0.665*** (32.63)			
covar99_lag		0.784*** (46.73)		
lrmes_lag			0.740*** (62.10)	
SRISK_lag				0.931*** (138.30)
_cons	0.00531*** (6.05)	0.00426*** (4.35)	0.0771*** (9.15)	1449.1 (1.54)
$H_0 : \beta_2 - \beta_1 = 0$	0.0089945***	0.0055618**	0.0695845***	580.6679
F-stat	(16.65)	(5.16)	(13.80)	(0.06)
N	1480	1480	3023	3023
adj. R^2	0.524	0.638	0.594	0.865
Fixed effects:	Bank	Bank	Bank	Bank

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 3.7: Commonality Measured by Banks' Entire Loan Portfolio

	<i>Dependent variable: Systemic Risk Measures</i>			
	(1)	(2)	(3)	(4)
	covar95	covar99	lrmes	SRISK
commonality	0.00358*** (3.40)	0.00441*** (3.56)	-0.00778** (-2.51)	-7.431 (-0.66)
beta	-0.00415*** (-5.85)	-0.00565*** (-5.94)	0.307*** (57.92)	4256.2** (2.52)
leverage	0.000109*** (3.83)	0.000168*** (4.17)	-0.000376** (-2.18)	237.9 (1.62)
assets	2.54e-10 (0.52)	3.07e-11 (0.05)	5.87e-09** (2.56)	0.00670*** (3.00)
covar95_lag	0.572*** (48.42)			
covar99_lag		0.578*** (45.13)		
lrmes_lag			0.0129*** (3.06)	
SRISK_lag				0.795*** (34.46)
_cons	0.00621*** (8.97)	0.00758*** (9.63)	0.0868*** (21.47)	-5957.7*** (-2.82)
<i>N</i>	1813	1813	2393	2393
adj. <i>R</i> ²	0.582	0.687	0.991	0.898
Fixed effects:	Bank	Bank	Bank	Bank

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 3.8: Commonality and Improved Systemic Risk Measures (Equal-weighted)

	<i>Dependent variable: Systemic Risk Measures</i>					
	(1) covar95	(2) covar99	(3) LRMES	(4) SRISK	(5) iLRMES	(6) iSRISK
commonality	0.0287*** (5.22)	0.0106** (3.15)	-0.0450 (-0.38)	30656.5 (1.89)	0.507*** (4.02)	29992.9* (2.01)
beta	0.00132 (0.20)	0.000236 (0.09)	0.0631 (1.23)	1012.7 (0.57)	0.0394 (1.09)	1362.1 (0.83)
leverage	0.00000480 (0.06)	0.0000700 (1.49)	0.00352** (2.76)	394.3 (1.02)	0.00291** (3.05)	429.2 (1.04)
assets	1.55e-09** (2.56)	8.12e-10** (3.31)	2.62e-08* (2.40)	0.00822*** (4.60)	1.94e-08* (2.08)	0.00952*** (3.96)
covar_lag	0.768*** (17.62)					
dcovar_lag		0.772*** (23.64)				
lrmes_lag			0.426*** (5.10)			
srisk_lag				0.789*** (21.07)		
i_lrmes_lag					0.602*** (10.68)	
i_srisk_lag						0.793*** (19.43)
_cons	0.0266** (3.34)	0.00969* (2.33)	0.115 (1.02)	17846.1 (1.63)	0.495*** (4.52)	16233.9 (1.66)
<i>N</i>	364	364	364	364	364	364
adj. <i>R</i> ²	0.605	0.607	0.615	0.918	0.625	0.943
Fixed effects:	Bank	Bank	Bank	Bank	Bank	Bank

Note: t-statistics are reported in the parentheses and standard errors are clustered at the bank level.
(*p<0.1; **p<0.05; ***p<0.01)

Table 3.9: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Max
covar	67,317	0.025	0.014	0.007	0.151
dcovar	67,317	0.010	0.008	0.001	0.112
MES	67,317	0.030	0.028	-0.008	0.608
SRISK	67,317	24,950	45,832	0.000	260,511
LRMES	67,317	0.450	0.146	-0.564	0.995
i.covar	67,317	0.025	0.013	0.008	0.135
i.dcovar	67,317	0.009	0.008	0.0005	0.097
i.MES	67,317	0.026	0.024	-0.003	0.481
i.SRISK	67,317	23,235	44,851	0.000	260,393
i.LRMES	67,317	0.402	0.145	-0.282	0.986

Table 3.10: Pairwise Correlation of Existing Measures

	covar	dcovar	MES	SRISK	LRMES
covar	1	0.892	0.861	0.092	0.261
dcovar	0.892	1	0.892	0.052	0.468
MES	0.861	0.892	1	0.214	0.570
SRISK	0.092	0.052	0.214	1	0.305
LRMES	0.261	0.468	0.570	0.305	1

Table 3.11: Pairwise Correlation of New Measures

	covar	dcovar	MES	SRISK	LRMES
covar	1	0.892	0.897	0.119	0.508
dcovar	0.892	1	0.901	0.065	0.606
i.MES	0.897	0.901	1	0.185	0.696
i.SRISK	0.119	0.065	0.185	1	0.222
i.LRMES	0.508	0.606	0.696	0.222	1

Table 3.12: Pairwise Correlation of Existing Measures in Non-recession Periods

	covar	dcovar	MES	SRISK	LRMES
covar	1	0.771	0.741	0.037	0.090
dcovar	0.771	1	0.789	-0.071	0.418
MES	0.741	0.789	1	0.213	0.602
SRISK	0.037	-0.071	0.213	1	0.272
LRMES	0.090	0.418	0.602	0.272	1

Note:

Table 3.13: Pairwise Correlation of New Measures in Non-recession Periods

	covar	dcovar	MES	SRISK	LRMES
covar	1	0.771	0.824	0.070	0.473
dcovar	0.771	1	0.798	-0.060	0.623
i.MES	0.824	0.798	1	0.138	0.770
i.SRISK	0.070	-0.060	0.138	1	0.185
i.LRMES	0.473	0.623	0.770	0.185	1

Note:

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Appendix A

DATA DETAILS

First of all, loan specific data are obtained from Dealscan. "loanspread" is the all-in-drawn spread over LIBOR rate at origination. "lamount" is the log of the facility amount in millions of dollars. "lmaturity" is the log of maturity that are recorded by months. "secured" indicates whether the facility is secured and "senior" indicates the seniority of the debt. "guarantor" indicates whether the loan deal specifies a guarantor, and "dividendres" presents dividend restriction of the borrower. Primary purpose of the loan is also specified by dummy variables named "corpurpose", "debtrepay", "workcapital", "termloan", and "creditline". Lastly, "relationship" indicates whether the firm borrowed from the bank more than once within a year.

Firm control variables are obtained from Compustat fundamental quarterly data. "lage" is the log of firm age that is a number of years since the initial starting date of the firm's data. "lsales" is the log of net sales (saleq) and "profmargin" is the profit margin calculated by net income (niq) over sales (saleq). "intcoverage" is the interest coverage ratio calculated by EBITDA (ibq+xintq) over interest expense (xintq). "stockvol" is the stock volatility of the borrower's stock return across last 365 days prior to the loan date and "exreturn" is the excess return on the borrower's stock return across last 365 days. "leverage" is computed by the sum of current and long term debt (dlcq+dlttq) divided by the total assets (atq). "tangibles" are the sum of inventory and the total of property, plant, and equipment (invtq+ppegqtq) divided by the total assets (atq). "nwc" is the net working capital computed by the difference between the assets (actq) and current liabilities (lctq) divided by the total debt (dlcq+dlttq). MB is market (ceqq-cshoq*prccq+atq) to book value (atq) ratio.

Bank control variables are obtained from Compustat bank quarterly data. "lassets" is the log of total assets of banks (atq). "ROA" is the banks' net income (niq) over assets (atq) and "ROAVOL" is the volatility of the bank ROA computed over the last three years from the loan date. "chargeoff" is the net chargeoffs (ncoq) divided by total assets (atq). Lastly, "capital" is the bank equity (pstkq) capital over assets (atq).

VITA

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