Essays on Banking

by

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Business Administration
Duke University

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Dissertation submitted in partial fulfillment of the requirements for
the degree of Doctor of Philosophy in Business Administration
in the Graduate School of Duke University

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ABSTRACT

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Abstract

I explore the determinants of banks' deposit funding conditions and their implications for banks' lending decisions. In the first chapter, I uncover a new distortionary mechanism through which unemployment insurance policies affect bank deposits. Large number of countries provide unemployment insurance (UI) to reduce individuals' income risk and to moderate fluctuations in the economy. However, to the extent that these policies are successful, they would be expected to reduce precautionary savings and hence bank deposits– households' main saving instrument. I study this reduced incentive to save and uncover a novel distortionary mechanism through which UI policies affect the economy. In particular, I show that, when UI benefits become more generous, bank deposits fall. Since deposits are the main source of funding for banks, this fall in deposits squeezes bank commercial lending, which in turn reduces corporate investment.

In the second chapter, I document a new mechanism through which banks gain flexibility to manage the size of their loan portfolio across monetary policy cycles. It is well-documented that an increase in the Fed funds rate reduces the lending capacity of banks, which in turn induces banks to cut corporate lending. However, it is not clear how banks achieve this reduction towards their existing borrowers because outstanding long-term loans may limit their ability to do so. I study how banks set loan contract terms to prepare for future contractionary monetary policy shocks. I find that banks with higher monetary policy exposure – banks whose lending capacity shrinks more during Fed hiking cycles – write loan contracts with stricter covenants. This ex-ante bank behavior makes borrowers more likely to violate covenants, giving banks the right to reduce lending ex-post when their lending capacity is hit by a monetary policy shock. These findings highlight the role of covenants in the transmission of monetary policy and imply that even firms with long-term loans remain exposed to monetary policy shocks.

I dedicate this dissertation to Sinem and Sinan, who have always been with me during my PhD journey. I also dedicate it to my deeply missed mom, Vahide, who passed away in the final year of my PhD.

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Chapter 1

Introduction

Deposits are the main and most stable source of funding for most banks. In this dissertation, I explore the determinants of deposits and their implications for banks' lending decisions. In particular, in the first chapter, I study the impact of unemployment insurance (UI) policies on bank deposits and lending.[1] By using disaggregated U.S. data, I show that more generous UI benefits lower bank deposits– households' major saving instrument. This decrease leads banks to squeeze their commercial lending to firms. The contraction in lending then lowers firm investment.

In the second chapter, I analyze how the monetary policy sensitivity of bank deposits affects the loan contracting behavior of banks. It is well-documented that an increase in the Fed funds rate reduces the amount of deposits in the banking industry, which in turn reduces the lending capacity of banks. I investigate how banks set loan contract terms in a way that would help them reduce lending to existing borrowers who have outstanding loan balances during times of monetary policy contractions. I find that banks prepare for future monetary policy shocks by writing stricter covenant. To document this, I use DealScan data on commercial loan contract agreements and Summary of Deposits data on annual branch level bank deposit holdings.

[1]This chapter is based off of a paper of the same name co-authored with Yavuz Arslan and Gazi Kabas (Arslan, Degerli, and Kabas, 2018).

Chapter 2

Unintended Consequences of Unemployment Insurance Benefits: The Role of Banks[1]

2.1 Introduction

A rise in unemployment is an important concern for policymakers due to its large economic and social costs for households. To alleviate the associated large costs, governments provide unemployment insurance (UI) to their citizens by providing a certain level of income for a limited time during the unemployment spell. At the macro level, when the economy worsens, UI policies act as a countercyclical fiscal policy and transfer funds to those with the highest need and highest marginal propensity to consume. In that respect, it is an efficient fiscal policy tool. However, to the extent that UI lowers individual and macroeconomic risks, it should be reducing household savings as it weakens the precautionary motive.

Motivated by this households' lower incentive to save, we uncover a novel distortionary mechanism through which UI policies affect the economy. In particular, by using disaggregated US data, we show that more generous UI benefits lower bank deposits, households' major saving instrument. Since deposits are the major and stable source of funding for most banks, this decrease leads banks to squeeze their commercial lending to firms. The contraction in lending then lowers firm investment.

Two points are noteworthy. First, the results highlight a new and previously unnoticed mechanism that is relevant for the policy discussions surrounding UI policies. On the benefits side of UI policies, most discussions have concentrated on consumer welfare through consumption smoothing. On the costs side, UI policies' distortionary effects on job search

[1]This chapter is based off of a paper of the same name co-authored with Yavuz Arslan and Gazi Kabas (Arslan, Degerli, and Kabas, 2018).

and job creation have been the main focus.[2] Our findings suggest that UI policies may have large negative macroeconomic implications via their effects on bank funding. Specifically, we show that firms may suffer from lower bank credit induced by generous UI policies and lower their investment in response.

Second, the mechanism uncovered in this paper suggests an externality that may create further inefficiencies akin to the well-known "the paradox of thrift." The externality can be described as follows. In the US, where our data comes from, each state can choose its own UI benefit generosity. Therefore, a state would prefer to have more generous UI benefits if it considers the benefits outweigh the costs in that state. As states are small compared to the whole U.S. economy, they will not take into account that such a policy will lower total savings, deposits and credit. However, on aggregate, if all states increase their UI benefits, total savings in the country will be lower, leading to lower deposits and credit on average. Still, states with more generous UI benefits might look relatively better, as is found in the literature that uses within-location identification strategies, yet aggregate welfare could be lower. Our results in this paper suggest that this externality may have quantitatively large welfare costs, and should be taken into account by policymakers.

We start our deposit analysis by using annual county-level deposit data from Summary of Deposits (SOD), and test the prediction that more generous UI benefits reduce bank deposits. However, the main identification challenge is contemporaneous changes in economic conditions. For instance, if economic conditions deteriorate contemporaneously as state UI benefits increase, then we would see that county deposits decline even if UI benefits have no impact on households' savings and hence on deposits.

We address this identification challenge by using the discontinuous change in the level of UI benefits at state borders. Instead of simply comparing the deposits of counties with different levels of UI benefits, we compare the deposits of two contiguous counties at state

[2]See Section 2.2 for more discussion on the literature's findings.

borders, one of them in one state and the other in the neighboring state (*à la* Dube et al. 2010; Hagedorn et al. 2018). Since the level of UI benefits is determined at the state level, these neighboring counties in different states have different levels of UI benefits. However, being neighbors to each other, they share similar characteristics (e.g., geography, climate, access to transportation routes) that may affect economic conditions. The key identifying assumption in this contiguous county comparison is that state-level economic shocks that may be correlated with state-level UI benefit changes do not stop at the state border and affect the two contiguous counties at the border symmetrically. We provide a validation test to support this identifying assumption by empirically showing that relevant state-level variables in one state have symmetric effects on the deposits of the same-state border county and the neighbor-state border-county.

The empirical results confirm our prediction. In response to a one standard deviation increase in state UI benefits, county total deposits decline by 2.2 percent. In dollar terms, per capita deposit holdings in a median U.S. county decrease by 82 USD following a 1,000 USD expansion of state UI benefits. The results are robust to including additional county-level variables (county income, unemployment rate) to control for county economic conditions; and to including county fixed effects to control for time-invariant county-level characteristics. Furthermore, the results are robust to making our within county-pair comparison for a subset of counties that are more similar to each other along several dimensions, such as the distance between the centers of two counties in a pair, local banking competition, industrial composition, and core-based statistical area.

In addition to the county-level deposit analysis, we do a branch-level deposit analysis to rule out the alternative explanation that bank deposit demand, instead of household deposit supply, may drive our results. For instance, if the banks operating in the two contiguous counties at the state border have different characteristics and respond differently to changes in economic conditions, then bank deposit demand may differ across the two contiguous counties. In the branch-level analysis, instead of comparing the deposits of two contiguous counties, we compare the deposits of two branches of the same bank, one branch in one of the contiguous counties and the other branch in the other county. Since banks can allocate deposits that they collect in one branch to another branch for lending to exploit lending opportunities as much as possible, there is no reason for a bank to decrease its deposit demand in one branch, but increase it in another branch. Therefore, bank demand for deposits remains constant across its branches, which allows us to measure the impact of UI benefits on deposits supply. To make this within-bank estimation, we use only the sample of banks that have branches in both contiguous counties at state borders, and exclude all others since the coefficient is not identified for other banks. The results verify our county-level findings.

To further establish the causal link between UI generosity and bank deposits, we exploit the design of UI policies in the U.S. and the different types of deposits that banks hold in their balance sheets. First, as the UI system in the U.S. imposes percentage caps on the maximum benefit payment an unemployed person can obtain, the change in the dollar amount of UI benefits is not binding for some people. For instance, the average percentage cap in our sample is 50 percent; that is, an unemployed worker is able to obtain UI benefits of up to 50 percent of his previous wage income. If the previous wage of this worker is too low, then the percentage cap would not allow him to benefit from increases in the dollar amount of state UI benefits. For this worker, the change in UI generosity should not have any impact on his saving behavior and hence his deposit holdings. To test this prediction, we use county-level realized UI payments from the Bureau of Economic Analysis (BEA) to identify UI-sensitive counties, and show that the effect of UI benefits on deposits is stronger in counties for which the change in state UI benefits is more binding. Second, since UI benefits target mainly low-income households exposed to unemployment risk, and hence the benefits are expected to influence the saving decisions of those households, we should find the impact of UI generosity on small deposits stronger than its impact on large deposits. We use bank-level Call Reports data and confirm this prediction for small and large deposits by implementing a propensity score matching on banks characteristics.

Several additional analyses and robustness checks lend support to our interpretation of the results. First, by using Google Trends data, we document that internet search trends for terms related to unemployment insurance and changes in UI benefits are positively correlated, suggesting that the public is aware of the changes in the UI benefits. Second, we show that the results are independent of excluding large bank branches from our sample. This mitigates the concern that the changes in firm deposit holdings rather than household deposits holdings drive the results. Third, the results do not change when we control for other state level social welfare policies that might be correlated with state UI policies. Fourth, we do not observe that households switch from holding deposits to holding riskier assets, such as bonds and stocks.

Next, we examine the impact of UI generosity on bank commercial lending. As the banking literature documents, deposits are unique for banks in the sense that they are the largest and most stable funding source that banks rely on (Hanson et al., 2015; Stein, 1998). We therefore predict that the contraction in deposits due to higher UI generosity should reduce bank loan supply to firms. To test this prediction, we first calculate bank-level UI exposure as banks can reallocate deposits that they collect from one branch to another branch for lending. In particular, we take the weighted average of the UI benefits of states where a bank raises deposits by using the bank's deposit levels in those states as weights. This measure reflects the bank's overall exposure to changes in the level of UI benefits, and is referred as *bank-UI exposure* throughout the paper.

The common identification challenge in uncovering the effect on loan supply is to keep loan demand constant. For instance, if firm loan demand decreases as bank UI exposure increases, then the decline in loan demand would drive the decrease in the equilibrium amount of loans even if banks have no incentive to decrease loan supply. Following Khwaja and Mian (2008), we implement within-firm estimation using annual firm-bank level Dealscan data on commercial loans by banks. In particular, we use firm-year fixed effects, and compare the loan amounts to the same firm in the same year by banks with different UI exposure. This within-firm estimation holds firm loan demand fixed, and hence enables us to uncover the effect of bank UI exposure on their loan supply. We find that banks that collect deposits in states with generous UI benefits originate less commercial lending compared to other banks. The effect is economically significant, with a 2.5 percent decrease in commercial lending in response to a one standard deviation increase in bank UI exposure. Furthermore, we show that the link between bank UI exposure and loan supply is especially strong for two sets of banks: i) banks that have a higher small deposit share in their balance sheets and hence experience more reduction in their deposits in response to an increase in UI benefits, and ii) financially constrained banks that have more difficulty in replacing the lost deposits with other sources of funding. These findings further support our causal interpretation.

Lastly, using annual Compustat data, we analyze the impact of UI generosity on firm investment. We document that firms served by banks with higher UI exposure have lower investment. More specifically, when a firm's UI exposure through its lenders increases by one standard deviation, its investment declines by 3 percent. The impact is stronger for financially constrained firms, consistent with the idea that these firms have more difficulty in replacing bank credit with other sources of external funding. Furthermore, in all investment regressions we include firm location-year fixed effects, which means that we compare the firms that face the same level of state UI benefits but have different UI exposure through their lenders. This is important in the sense that we control for the direct effects of state UI benefits on firm decisions documented in the literature (Agrawal and Matsa, 2013; Hagedorn et al., 2018), and hence measure only the bank channel of UI on firm outcomes.

The rest of the paper is organized as follows: Section 2.2 discusses the related literature, Section 3.2 describes the data and variables constructed, Section 2.4 presents results on deposits, Section 2.5 reports results on commercial lending, Section 2.6 presents results on firm investment, and Section 3.6 concludes.

2.2 Related literature

Our paper is related to the literature that studies the effect of income risk on household precautionary savings. Most directly related to the UI generosity, Engen and Gruber (2001) find that reducing UI benefits by $1124.35 would increase households' financial asset holdings by $85.80. Although they rely on a different empirical setting with a different dataset, their findings are very close to our estimates. Using SIPP data, Bird and Hagstrom (1999a) reach larger estimates. Hansen and İmrohoroğlu (1992) use a calibrated model and show large and nonlinear effects of UI benefits on household savings. In a more general context, Carroll and Samwick (1998) estimate that approximately 45 percent of wealth accumulation is attributable to precautionary motives; Zeldes (1989), Hubbard, Skinner, and Zeldes (1995), Caballero (1990), and Weil (1990) establish that precautionary savings increase significantly in response to higher income risk. More recently, Carroll and Kimball (2008), Fuchs-Schündeln and Schündeln (2005), Mody, Ohnsorge, and Sandri (2012), Gourinchas and Parker (2002), and Cagetti (2003) confirm the significance of the precautionary motive on household savings. Our paper uses changes in UI benefits as a source of variation in household precautionary saving motives; and complements this literature by linking precautionary savings to bank deposits (households' main saving instrument), and by analyzing its effect on bank lending and firm investment.[3]

[3]While we consider the precautionary motive to be the main driver, it may not be the only one. For instance, with reduced income risk due to more generous benefits, households may have an easier access to

Our paper contributes to the recent literature on the impact of UI policies on the economy. Using household-level data, Hsu, Matsa, and Melzer (2018) and Di Maggio and Kermani (2017) emphasize the stabilizing role of UI benefits. Hsu, Matsa, and Melzer (2018) show that UI benefits prevent the mortgage defaults of unemployed people, and hence insulate the housing market from labor market shocks. Di Maggio and Kermani (2017) find that household consumption and delinquencies become less responsive to local shocks when UI benefits are more generous.[4] They argue that generous UI benefits decrease the incentive of banks to tighten credit conditions in response to negative economic shocks. Our findings, however, suggest that while UI may stabilize the economy through its effect on the household sector, this is done at the expense of banks and firms. The reason is that deposits are the largest and most reliable source of funding for banks; hence, deprived of deposits, banks are less able to support firms through commercial lending.

We contribute also to the literature that studies the distortionary effects of UI benefits. Motivated by the slow recovery of the U.S. labor market in the aftermath of the financial crisis, several papers examine the role of higher UI generosity in increasing the reservation wages of employees, and therefore decreasing the job creation incentives of firms (Chodorow-Reich, Coglianese, and Karabarbounis, 2018; Hagedorn, Manovskii, and Mitman, 2015; Hagedorn, Karahan, Manovskii, and Mitman, 2018).[5] Our paper provides an alternative mechanism that may explain the slow recovery of the U.S. labor market. Our results imply that higher UI benefits during the crisis may have reduced firms' access to

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mortgage credit. Hence, they may shift their savings from bank deposits to housing. Or, households may shift their saving from low risk deposits to riskier equities as they have a higher risk bearing capacity. We stick with the precautionary motive throughout the paper as it is the best established mechanism in the literature and we show that households do not shift to equity or bond markets.

[4]See also Gruber (1997); Browning and Crossley (2001); Bloemen and Stancanelli (2005); Chetty and Szeidl (2007) for further findings about the benefits of UI policies.

[5]See also Mulligan (2012); Barro (2010); Card and Levine (2000); Ham and Rea Jr (1987); Johnston and Mas (2018); Lalive, Landais, and Zweimüller (2015); Inderbitzin, Staubli, and Zweimüller (2016); Zweimüller (2018) for further discussion about the distortionary effects of UI policies.

bank credit, which in turn lowered firm investment and, hence, employment.

Our paper is also related to the role of the deposits in the banking industry and internal capital markets within the banks.[6] The literature offers evidence that both bank fundamentals and panics may lead to deposit outflows (Iyer and Puri, 2012; Iyer, Puri, and Ryan, 2016; Calomiris and Mason, 1997, 2003). In this paper, the driving force behind the decline in deposits is not the deterioration of bank fundamentals or panics, but instead the change in the pre-cautionary saving motivation of households. However, consistent with the findings of the literature on the importance of deposits for bank funding,[7] the decline in deposits still leads to a reduction in bank loan supply to non-financial firms. The literature also documents that economic shocks can be transmitted through banks' internal capital markets (Gilje, Loutskin, and Strahan (2016); Cortés and Strahan (2017); Doerr and Kabas (2019)). Our findings show that the impact of UI in one state is channeled to other states via the banking system.

Finally, our paper provides additional support to the literature that emphasizes the role of bank credit in firm-level outcomes.[8] This literature shows that a contraction in bank loan supply negatively affects firm capital expenditures (Almeida, Campello, Laranjeira, and Weisbenner, 2009; Lemmon, Roberts, and Zender, 2008), R&D investment, and productivity (Braggion and Ongena, 2017; Banerjee and Duflo, 2014; Krishnan, Nandy, and Puri, 2014). This incentivizes firms, especially financially constrained and informationally opaque ones, to build relationships with banks in order to maintain their access to external funding (Diamond, 1991; Petersen and Rajan, 1994; Drucker and Puri, 2005). However,

[6]For theoretical foundations, see for example Diamond and Dybvig (1983); Calomiris and Kahn (1991); Rochet and Vives (2004); Chari and Jagannathan (1988); Jacklin and Bhattacharya (1988).

[7]See for example Hanson, Shleifer, Stein, and Vishny (2015); Kashyap, Rajan, and Stein (2002); Gorton and Pennacchi (1990); Diamond and Rajan (2000)

[8]See for example Kaplan and Zingales (1997); Rajan and Zingales (1996); Campello, Graham, and Harvey (2010); Beck, Demirgüç-Kunt, Laeven, and Maksimovic (2006); Coluzzi, Ferrando, and Martinez-Carrascal (2015); Jiménez, Ongena, Peydró, and Saurina (2012a, 2017); Faulkender and Petersen (2005); Garcia-Posada (2018).

having relationships with banks does not completely eliminate the risk of losing access to external funding as these firms are still exposed to changes in the lending capacity of their lenders. For instance, using the same data set that we use, Chodorow-Reich (2013) finds that the firms that had borrowing-lending relationships with unhealthy banks prior to the crisis lowered their employment more. In our case, it is bank-UI exposure that drives the decline in bank deposits, which in turn reduces the lending capacity of banks and hence firm investment due to less access to bank credit.

2.3 Data

2.3.1 Data sources and variables

The analysis in this paper relies on numerous data sources that cover the period from 1994 to 2010. In this section, we detail only the main data sources and the variables that play the central role in the analysis, and postpone describing the others until when they are used.

State-level unemployment insurance data: The U.S. Department of Labor issues "Significant Provisions of State UI Laws" that provides information on state UI benefit schedules for the period after 1938.[9] There are mainly two types of unemployment insurance (UI) payments in the U.S.: regular benefit payments and extended benefit payments. The regular UI system in the U.S. provides payments to eligible workers when they involuntarily become unemployed. These are weekly payments, the duration and the level of which are determined by state governments. According to state UI schedules, an unemployed individual is paid a predetermined percentage of his previous wage income, which is capped at the state's maximum weekly benefit level. To be more precise, there are two caps that the state UI schedule imposes on the weekly UI payment that an individual can obtain: a percentage cap and a dollar cap. The unemployed individual obtains the minimum of the

[9] We use the data obtained and provided by Hsu et al. (2018).

two. In our analysis throughout the paper, we follow the literature and use the product of the state's maximum weekly payments (dollar cap) and the duration of the payments as the main independent variable, and refer to it as *state UI benefit*. This variable shows the maximum total UI payment an unemployed individual can obtain during his unemployment spell, and reflects the generosity of a state's UI system.

UI schedule explained above has one important feature. When a state decides to increase its dollar cap, individuals that are already bound by percentage cap are not affected by the increase at all. Given that, by definition of percentage cap, the ones who are bound by percentage cap are the ones who have the lowest wages. Thus, an increase *UI benefit* does not have any impact on individuals with lowest wages. To put it differently, an increase in *UI benefit* affects middle and high income individuals who are expected to hold significant amount of deposits. We expect the middle income individuals to react more to changes in*UI benefit* since the relative importance of *UI benefit*s is higher for such individuals. Therefore, we anticipate that the decline in deposits that we document is coming from middle income individuals.

A rise in the UI generosity would influence the savings of the employed people only if they are aware of the changes in the policies. In the Appendix, we provide a supporting evidence to show that this is indeed the case. By using Google Trends data, we show that households increase their "Unemployment Benefits" searches as *UI benefit*s change (Table A1). Moreover, the relationship between the internet search activity and the changes in UI benefits stays significant even when we control for the state level income, unemployment and GDP. Overall, these findings suggest that people are aware of the changes in the UI benefits.

During times of high unemployment, the federal government might extend the duration of the benefit payments. When the maximum number of weeks under the regular payments is reached during such times, the unemployed receive additional payments for an extended period of time. In our analysis, we exclude extended benefit payments periods, and focus only on regular UI payments. We do so mainly due to two considerations. First, the benefit extensions are triggered by the economic conditions (i.e., unemployment rate) of a state. Therefore, by the very nature of the UI system, the endogeneity concern that state economic conditions and state UI benefits are highly correlated is more severe for the periods in which extended benefit payments are triggered. Second, the novel mechanism that we propose needs the changes in UI benefits to be persistent to have an impact on household saving behavior. The generosity of the state UI system in non-crisis periods serves this purpose better since the extended benefit payments are in effect only during periods of high unemployment, and hence are of a more temporary nature.

County-level deposit data: The Federal Deposit Insurance Corporation (FDIC) issues the Summary of Deposits (SOD) survey, which provides information on the annual deposit holdings of U.S. bank branches. The data set has information on branch location, allowing us to do county-level and bank branch-level[10] analyses. As our dependent variable, we use county total deposits and branch total deposits in county-level deposit analysis and branch-level deposit analysis, respectively.

Loan-level data: The data on loans are from Dealscan and contain loan-level information on syndicated loans in the U.S. market. The information is collected by the Loan Pricing Corporation (LPC) from SEC filings and lead lenders, and available on Wharton Research Data Services (WRDS). It provides detailed information on syndicated loans, i.e., amount, purpose, type, origination date, maturity, and the types of financial covenants included in the loan contract. The dataset also provides the name and location information for the borrowers and the lenders of syndicated loans, which are used to merge the loan data with other datasets.

[10]If a bank has more than one branch in a county, then we aggregate those branch deposits into bank-county level. For ease of reference, however, we refer to them as branch level instead of bank-county level.

In our commercial lending analysis, we use the annual total outstanding loan amount between a firm and its lender as the dependent variable. Unlike credit registry data, Dealscan is flow data, and provides information on loans only at their origination; hence, we do not directly observe the outstanding loan amount between a firm and its lender in each year. We follow the literature (Lin and Paravisini, 2013; Di Maggio et al., 2017; Chakraborty et al., 2018), and construct the annual outstanding loan amount by using the information on loan origination date, termination date, and loan amount.

Bank-level balance sheet data: The bank balance sheet data is from U.S. Consolidated Reports of Condition and Income filings (Call Report), submitted by banks regulated by the Federal Reserve System, the FDIC, and the OCC, and available from WRDS.

We aggregate bank-level data at the bank holding company level by using all subsidiary banks of a bank holding company. Both the banking industry practices and the data matching process necessitate our using bank holding company level data. First, the internal capital markets within a bank holding company imply that making a bank holding company level analysis is more consistent with the findings of the banking literature. Second, the information provided on lenders of loans in Dealscan is more complete on the ultimate owner of the lender. More specifically, although some loan observations provide information on the immediate lender, which allows us to know both the subsidiary bank and its parent bank holding company, some loan observations report only the ultimate lender, in which case we do not know the immediate lender, i.e., the subsidiary bank of the bank holding company.

To support our county- and branch-level deposit analysis, we also do bank-level deposit analysis. Namely, we exploit the granularity of the Call Reports regarding the deposit size (i.e., small versus large deposits) and average interest payments on deposits. We also use several balance sheet items to construct a set of additional control variables. Bank equity ratio (bank equity normalized by bank assets) and bank size (log of bank assets) are the two widely used variables in the literature on bank lending behavior. To further control for the structure of bank balance sheets that may impact bank lending practices, and hence mitigate the omitted variable bias, the share of securities in total assets, and the share of core deposits in liabilities are also controlled for.

Firm-level data: Firm characteristics and annual accounting information comes from Compustat, available on WRDS. The main firm-level variable is firm investment rate, which is defined by the total amount of capital expenditures divided by lagged total firm assets. Several firm-level variables are used to control for the firm's investment opportunities such as firm size (log of total assets), marginal Q, leverage, and Altman's Z-score.

Merged Datasets: We have three main analyses. First, we analyze how the changes in state unemployment insurance affect households' deposit holdings. This deposit analysis is based on comparing two border counties located at state borders. Therefore, we aggregate SOD's branch-level deposits into county deposits, and supplement the data with annual county-level income, unemployment rate, and annual state UI benefit payments.[11] We do the same analysis at the branch level without aggregating the deposit data at county level, in which case we are comparing the two branches of the same bank located in the two counties across a state border.

Second, we study the effect of changes in unemployment insurance on bank commercial lending. The analysis is based on Dealscan's firm-bank level commercial loan data. Following Khwaja and Mian (2008) methodology, we use only the Dealscan firms that have outstanding loans from multiple banks in a given year, and make a within-firm comparison by including firm-year fixed effects in the regressions. To do the lending analysis, we supplement the loan data with bank-level UI exposure, which is calculated by taking the weighted average of the UI level of the states where a bank operates using the de-

[11]We obtain the county-level income and unemployment rate data from Bureau of Economic Analysis (BEA), and Bureau of Labor Statistics (BLS), respectively.

posits of the bank in those states as weights. We refer to this variable as bank UI exposure throughout the paper. Furthermore, we merge this data with the bank balance sheet information from Call Reports to control lender characteristics that may affect loan outcomes. We manually match lead lenders in Dealscan with commercial banks in Call Reports data based on name and location.[12] In the Call Reports data, commercial banks report their top-holder bank holding company, enabling us to aggregate bank-level variables into bank holding company level.

Third, we examine the effect of unemployment insurance on firm investment decisions. The analysis is at firm-year level and based on annual Compustat files that provide both the firm investment information and other firm-level controls. By using the link file provided by Chava and Roberts (2008), we match Compustat firms with their Dealscan borrowers, which allows us to calculate the unemployment insurance exposure of a Compustat firm through its lenders. To calculate the exposure, first we determine the banks the firm works with by using Dealscan loan origination date and loan maturity. Second, by using the outstanding loan amount between the firm and its lenders, we take the weighted average of the UI exposures of its lenders. We refer to this new constructed firm-year level variable as firm UI exposure throughout the paper.

2.3.2 Summary statistics

Table 2.1 reports the summary statistics in three panels. Panel A presents the summary statistics at the county level for the sample of border counties that we use in our deposit analysis. The weekly UI benefit payment in an average county is 330 USD for a period of 26 weeks. The product of weekly payments and the duration of the payments is the maximum total UI benefit payment an unemployed person can obtain during his unemployment spell. This variable is our main independent variable, which we refer to as *UI benefit*, in

[12] See Degerli (2019) for details.

the deposit analysis, and its average is 8,540 USD. The variable shows significant variation that mainly comes from weekly payments as the duration of payments is almost uniform across time and states. The median county in the sample has 313 million USD deposits and 624 million USD total income.

Table 2.1: Summary Statistics

	Mean	SD	25^{th} perc.	Median	75^{th} perc.
Panel A: County Characteristics					
UI Benefit, weekly (tho. $)	0.33	0.10	0.26	0.31	0.39
UI Benefit, duration (weeks)	26.08	0.54	26.00	26.00	26.00
UI Benefit (tho. $)	8.54	2.69	6.66	8.11	10.06
UI Benefit, growth (%)	3.40	3.65	0.00	3.20	4.51
Deposit (mil. $)	1921	12338	130	313	768
Deposits, growth (%)	3.59	5.94	0.45	3.30	6.34
Income (mil. $)	2933	9252	254	624	1729
Income, growth (%)	4.31	4.93	1.92	4.42	6.74
HHI, county	0.31	0.19	0.18	0.25	0.39
Obs. (county × year)	36,874				
Panel B: Bank Characteristics					
Bank UI exposure (tho. $)	10.00	2.21	8.46	10.08	10.95
Loan amount (mill. $)	566	1243	56	208	578
Loan amount, growth (%)	4.72	66.43	0.00	0.00	0.00
Size (bill. $)	475	532	80	212	655
Size, growth (%)	10.02	17.59	0.50	5.91	13.14
Equity (%)	9.03	1.66	8.07	9.06	10.12
Securities (%)	15.11	7.00	10.01	14.17	18.72
Core deposits (%)	44.54	15.39	33.05	48.39	55.23
Profitability (%)	1.23	0.80	0.94	1.24	1.64
HHI, Bank	0.18	0.04	0.16	0.18	0.20
Obs. (firm × bank × year)	174,151				
Panel C: Firm Characteristics					
Firm UI exposure (tho. $)	9.67	1.74	8.35	9.99	10.82
Investment rate (%)	7.11	7.93	2.44	4.64	8.47
Size (bill. $)	5.82	18.05	0.29	0.99	3.67
Tobin's q	1.69	2.28	1.05	1.34	1.86
Current ratio (%)	2.00	14.97	1.11	1.60	2.29
Leverage (%)	25.87	25.78	10.17	22.86	35.93
Fixed coverage	21.09	394.69	1.17	2.61	6.03
Altman's z-score	3.05	12.99	1.43	2.57	4.04
Obs. (firm × year)	29,685				

Panel B reports summary statistics at the firm-bank level for our commercial lending analysis. Since we implement a within-firm estimation, we keep only the sample of firms that have lending relationships with multiple lenders. The relevant variable for this analysis is bank-level UI exposure, which is obtained by taking the weighted average of UI benefits of states where a bank operates. It reflects the average level of UI benefits the bank faces. Since most of the banks in our sample operate in more than one state, averaging state UI benefits to obtain bank UI exposure decreases the variation in the variable. Furthermore, note that the average bank UI exposure is higher than the average state UI benefits, implying that our sample is biased toward banks that operate in states with high UI benefits. The typical bank in the sample is large, with an asset size of 475 billion USD and a Dealscan loan size of 567 million USD. The asset share of core deposits for an average bank is 45 percent, significantly lower than that for an average Call Report bank. This implies that we underestimate the effect of bank UI exposure on loan supply because the banks in our sample are less dependent on deposits and hence less affected by the decline in deposits.

Panel C presents firm-level summary statistics for the investment analysis. The typical firm in the sample is large. This is mainly because Dealscan data is biased toward reporting loan contract agreements of large banks and firms. Furthermore, the probability of matching large Dealscan borrowers with Compustat firms is higher since the information on those firms is more complete.

2.4 Deposit analysis

In this section, using county-level total deposits and state-level unemployment insurance (UI) benefits, we test the main prediction of our paper: an increase in UI benefits reduces household deposit holdings by lowering their incentive to save. The results of a model in which we simply regress county deposits on state UI benefits are contaminated by endogeneity. State UI generosity depends on state political factors (election concerns, party preferences), state economic factors (labor market conditions, state budget surplus/deficit), and the interaction of the two. Important to our empirical framework is that the economic conditions of a state potentially affect the economic activity of its counties, and hence the total county deposits. Therefore, to the extent that we omit relevant state economic conditions in our regression, the coefficient of state UI benefits would be biased. For instance, when an economic shock hits a state, the shock can trigger a change in state UI benefits, along with a change in the deposit levels of the counties that are located in that state. The estimated coefficient would erroneously attribute the effect of this economic shock on county deposits to the state UI benefits. To establish the causality from state UI to county deposits, therefore, we need to control for state economic conditions.

2.4.1 Identification strategy

We address this identification challenge by using the discontinuous change in UI benefits at state borders. Instead of simply comparing the deposits of any counties with different levels of UI benefits, we compare the deposits of two contiguous counties that neighbor each other at state borders, one of them in one state and the other in the neighboring state (Dube et al., 2010; Hagedorn et al., 2018; Heider and Ljungqvist, 2015). For instance, Figure 2.1 shows county-level maps of the state of North Carolina (NC) in red, and the state of Virginia (VA) in blue. The light-red county at the NC border is Stokes County. Since the only county located in VA that shares the same border with Stokes County is Patrick County (in light blue), we compare the deposits of these two counties. Throughout the paper, we refer to two such counties as a *county-pair* (or simply as a *pair*), and the approach of comparing the deposits of these two counties as *within county-pair estimation* (or simply as *within pair estimation*). Figure 2.2 provides a slightly different case of county-pair formation. Light red-painted Northampton County of NC shares the state border with three counties in VA: Southampton, Greensville, and Brunswick. This generates three different county-pairs in our empirical analysis: Northampton-Southampton, Northampton-Greensville, and Northampton-Brunswick. Figure 2.3 displays the location of all border counties used in our county-pair comparison analysis.

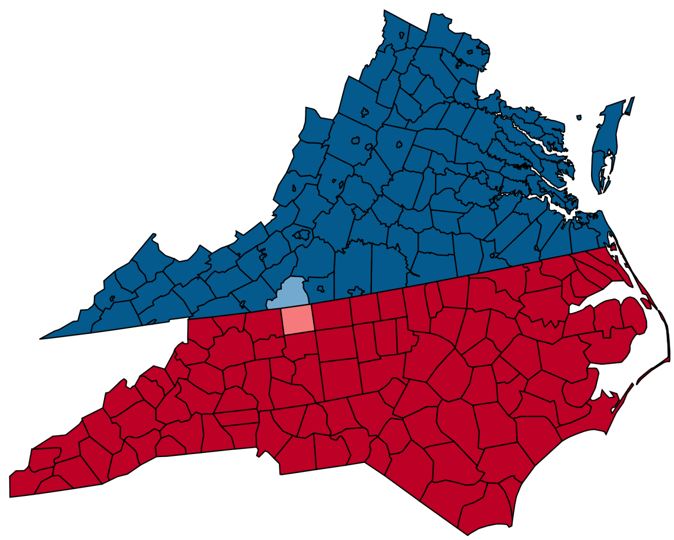

Figure 2.1: **NC and VA County-Level Map: County-Pair Formation (1)**

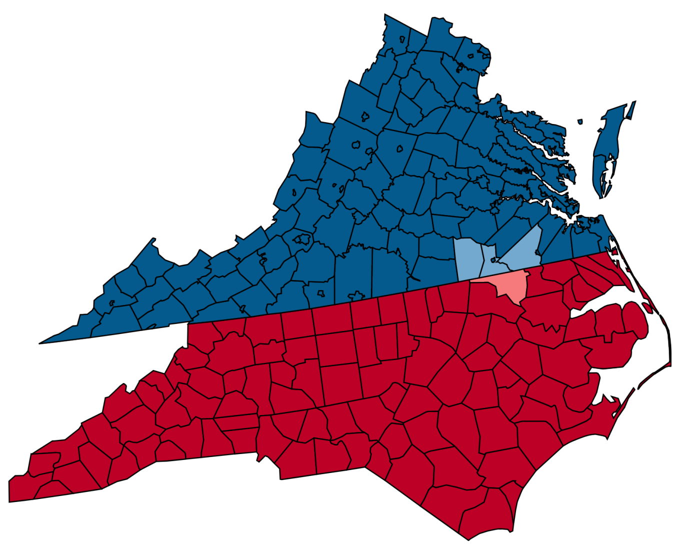

Figure 2.2: **NC and VA County-Level Map: County-Pair Formation (2)**

Why is this within county-pair estimation useful for our purpose? The two counties within a county-pair share the same geography and climate; have access to the same transportation routes; and more importantly are open to similar spillover effects of economic changes. These characteristics imply that a state-level economic shock is expected to affect the two counties within a county-pair symmetrically, since the economic conditions are continuous in the sense that state borders do not affect the movement of the economic shocks. Therefore, comparing the two counties within a county-pair controls for economic shocks that are expected to affect both state UI benefits and county deposit levels. The two counties in a county-pair, on the other hand, are subject to different levels of UI benefits since the generosity of UI policies is determined by state governments. This discontinuous variation in UI policies allows us to measure the effect of UI benefits on deposits.

One point is worth noting. The necessary identifying assumption for the validity of within county-pair estimation is not that the two counties in a county-pair are similar, but that state-level economic shocks that may be correlated with state-level UI benefit changes do not stop at the state border, and affect the two counties within a county-pair symmetrically. In Section 2.4.3, we provide robustness checks and tests to support this

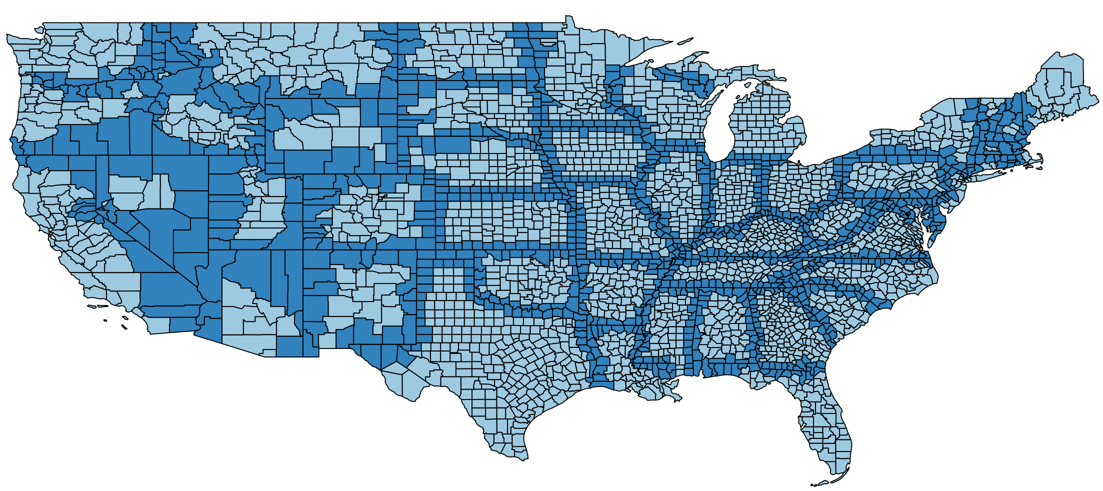

Figure 2.3: **Border Counties**

identifying assumption.

We estimate the following regression model for our within county-pair estimation:

$$\Delta log(deposit_{c,y}) = \beta \Delta log(UI_{s(c),y}) + \gamma_1 \Delta log(income_{c,y}) + \theta f(unemp.rate_{c,y}) + \delta_{p(c),y} + \eta_c + \varepsilon_{c,y} \tag{2.1}$$

where the dependent variable is the log change in the total deposits of county c from year $y-1$ to y, $\Delta log(UI_{s(c),y})$ is the contemporaneous log change in the UI benefits of the state where county c is located, $\delta_{p(c),y}$ are pair-year fixed effects for county-pair p where county c is located, and η_c are fixed effects for county c. Across different specifications, we also control for county income, and county unemployment rate up to its third-degree polynomial. The coefficient of interest is β, which is expected to take a negative sign.

The pair-year fixed effects, $\delta_{p(c),y}$, are key to the within county-pair estimation, and allow different county-pairs to have time-varying differences from each other. Under our identifying assumption that state-level economic shocks affect the two counties in a pair symmetrically, using these fixed effects cancels out the effect of state shocks on the deposits of the two counties within the pair. This allows us to identify the effect of state UI benefits on deposits. County fixed effects further control for the unobserved time-invariant differences, while county income and unemployment rates control for observed time-varying differences (e.g., county-level economic activity, and labor market conditions) across counties within a county-pair.

Clustering standard errors needs special consideration. First, since the level of UI benefits is determined at the state level, the variable of interest is constant across counties within a state. This creates downward bias in standard errors. Second, since a border county in one state may have a common border with more than one county on the other side of the border, the same county may be in more than one county-pair in our empirical setting, which generates a mechanical correlation across county-pairs. To address this correlation, we follow Dube et al. (2010), and double-cluster standard errors at the state and border segment level.[13]

2.4.2 Within county-pair estimation

Table 2.2 presents the main results for the deposit analysis. The analysis in columns (1) to (5) is at county level, and uses only the counties located at state borders. Each specification in these columns includes pair-year fixed effects, which means we are comparing the total deposits of the two border counties within a county-pair. Column (1) is our baseline specification with no control variables other than the pair-year fixed effects, and reports a negative and significant coefficient for state UI benefits. The economic meaning of this coefficient is that total county deposits decrease by 2.2 percent in response to a one standard deviation increase in the level of state UI benefits. In dollar terms, an individual in a median U.S. county decreases his deposit holdings by 82 USD when the state pays an additional 1,000 USD of unemployment insurance benefits.

Table 2.2: **Deposits and UI Benefits: Within-Pair & Within-Bank Estimation**

	$\Delta log(CountyDeposit)$					$\Delta log(BranchDeposit)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta log(UIBenefit)$, State	-0.054***	-0.059***	-0.061***	-0.061***	-0.061***	-0.106***	-0.085**
	(0.015)	(0.016)	(0.016)	(0.016)	(0.015)	(0.038)	(0.039)
$\Delta log(Income)$, County			0.043***	0.043***	0.045***	0.116***	0.089**
			(0.015)	(0.015)	(0.015)	(0.041)	(0.039)
Controls & Fixed Eff:							
Unemp.	N	N	N	Y	Y	Y	Y
cubic(Unemp.)	N	N	N	N	Y	Y	Y
Pair–Year FE	Y	Y	Y	Y	Y	N	N
County FE	N	Y	Y	Y	Y	Y	N
Pair–Year–Bank FE	N	N	N	N	N	Y	Y
County–Bank FE	N	N	N	N	N	N	Y
Obs.	36,148	36,148	36,148	36,148	36,148	37,012	37,012
R^2	0.553	0.596	0.597	0.597	0.597	0.608	0.678

Our estimate suggests an economically significant role for UI policies in household

[13] "A border segment is defined as the set of all counties on both sides of a border between two states" (footnote 17, Dube et al. (2010)).

savings, which qualitatively confirms the findings of the earlier literature. In particular, our estimates are very close to the findings of Engen and Gruber (2001), but smaller than the findings of Bird and Hagstrom (1999b). One concern may be that our results are not perfectly comparable to the earlier papers as the saving measures are different: while we explore the effects of UI benefits on deposits, earlier papers analyzed their effects on a broader measure of savings. That said, bank deposits is the most common saving instrument for most of the households, and for most of them it is the only one. According to the Survey of Consumer Finances (SCF), while more than 90 percent of families have transaction accounts with a median value of 4000 USD, only 20 percent of families directly hold stocks and/or bonds.[14] Furthermore, stocks and bonds holdings are mainly concentrated among the highest income people.[15]

We add additional controls to the regression in columns (2) through (5). To control time-invariant differences between the two counties in the pair, column (2) uses county fixed effects. Arguably, the total amount of county deposits is a function of county economic conditions; hence, we control for the county-level income in column (3) as a proxy for county economic conditions.[16] We further control for county labor market conditions that may be correlated with state-level economic conditions and hence with state UI by using county unemployment rate and its third-degree polynomial in columns (4) and (5), respectively. The coefficients across these columns are similar to that in column (1), and still highly significant.

A potential concern in county-level results of columns (1) through (5), and an alternative explanation to our findings, is the heterogeneity of banks across state borders. The

[14]These values are for 2004.

[15]Moreover, the findings that we report in Table 2.9 and Table 2.10 suggest that UI has no significant effect on stocks and bond holdings. As a result, we believe that our results capture a big part of the changes in precautionary savings in response to the changes in UI benefits.

[16]We also use county level wage income as a control instead of total income, and obtain similar results.

branch-opening decision of banks is not random. Banks endogenously choose which counties to locate their branches in based on the interplay of their own bank characteristics and the economic prospects of counties. This raises the concern that the characteristics of the banks that operate in one county may be different from those of the banks that operate in the other county within the pair; hence the banks in these two counties may differ in their incentives to raise deposits. For instance, these banks may differ in their country-wide lending opportunities, which creates heterogeneity among them in their incentives to raise deposits since deposits are the main funding source for bank lending. If these incentives are time-varying and correlated with the changes in economic conditions that drive changes in UI benefits, then our coefficient is biased. This discussion, in fact, relates to the discussion of whether the effect that we measure is demand-driven or supply-driven. The mechanism that we propose in this paper is supply-driven, meaning households save less and hence hold smaller deposits at banks when state UI becomes more generous. However, if the banks are heterogeneous across the state border, then their demand for deposits may respond heterogeneously to economic shocks, which may also explain our results.

To control bank-specific deposit demand across the two counties within a pair, in columns (6)-(7) of Table 2.2, we do branch-level analysis and use total branch-level deposits as our dependent variable. In these specifications, instead of using pair-year fixed effects, we use pair-bank-year fixed effects, which means we are comparing the deposits of the two branches of the same bank, one of them located in one county and the other one in the other county in the pair. In this within-bank estimation, the identifying assumption is that the deposit demand of a bank is determined at the bank level, not at the branch level. The economic rationale behind this assumption is that banks can allocate deposits that they collect in one branch to another branch for lending to exploit the lending opportunities as much as possible. This implies that there is no reason for a bank to decrease its deposit demand in one branch, but increase it in another branch. Empirical evidence in the banking literature also supports this intuition (Gilje et al., 2016; Drechsler et al., 2017). Therefore, the bank demand for deposits stays constant across its branches, which allows us to measure the impact of UI benefits on household deposit supply. To make this within-bank estimation, in columns (6) and (7) we use only the sample of banks that have branches in both counties in a pair, and exclude all others since the coefficient is not identified for banks that have a branch in only one of the two counties in the pair. Column (6) confirms our previous county-level deposit results. In column (7), we further refine the specification by including county-bank fixed effects. Absorbing time-invariant branch characteristics, we document a similar result. Therefore, both the within county-pair and the within-bank analysis of Table 2.2 support our interpretation of the results as a supply-driven mechanism.[17]

To mitigate the concern that a possible pre-trend in county deposit growth may drive the deposit outcomes, we include the two lags and lead of the state UI growth into our model along with concurrent growth rate. Figure 2.4 presents the point estimates and 90 percent confidence intervals around the coefficients, and indicates that the lags and the lead of unemployment insurance are not statistically different from zero, and the coefficient of interest is still negative and significant. Having a significant effect for the coefficient of the contemporaneous UI benefits growth without any lag, however, raises the question of why we see an immediate effect of UI policies on deposit holdings of households. There are three points to note. First, we use annual SOD deposits data, meaning that we measure the cumulative effect of the change in UI policies on household deposit holdings throughout the whole year. Second, there are potentially two different mechanisms through which the changes in UI benefit may affect household saving behavior. When the level of UI

[17] Showing that the branch-level and county-level analyses provide consistent results, we continue our analysis by using county-level data, as the branch-level analysis excludes more than three-quarters of the observations. The robustness checks that we provide in the next two sections at the county level also hold at the branch level.

benefits increases, households may start to save less out of the income that they earn after the policy change, or they may start to spend their existing savings after the increase in UI benefits. Third, in the regressions, we use the variables in growth, not level. Therefore, an insignificant coefficient for the lead variable means that the bank deposits level does not come back to its previous level, implying a persistent effect on deposits.

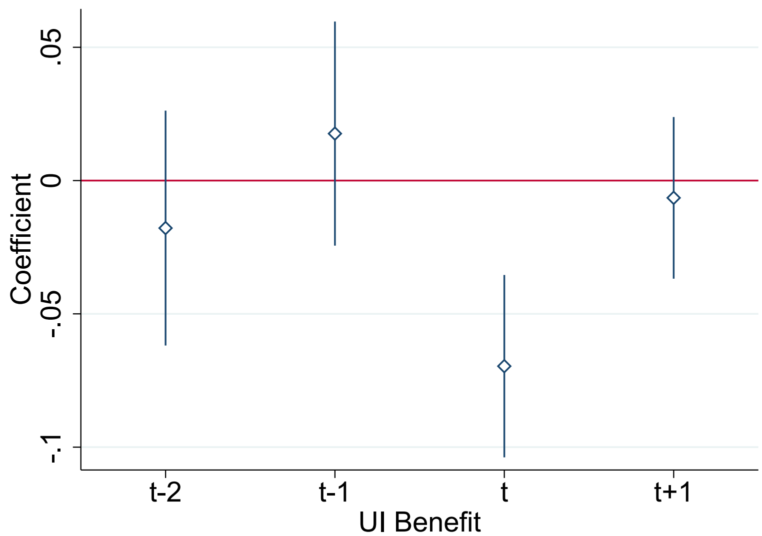

Figure 2.4: **Dynamic Effects**

2.4.3 Endogeneity concerns and robustness checks

In this section, we discuss possible endogeneity concerns. The first endogeneity concern is related to the similarity of two counties within a county-pair. Although the two counties are neighbors to each other and share the same geography, climate, and transportation routes, there is potentially a high degree of heterogeneity in terms of their features. For instance, the county-level U.S. map (Figure 2.3) shows that the distance between the centers of two counties within a pair is greater in the western part of the country than it is in the eastern part, which implies that the border counties in the west are expected to be less similar to each other. If the counties within a pair are different along a dimension and if this dimension makes these counties react asymmetrically to a state level economic shock, then this would create bias in our coefficient to the extent that we do not control for this dimension in our regression. To mitigate this concern, we make our within county-pair comparison for a subset of counties that are more similar to each other along several dimensions.

Each column in Table 2.3 uses a different criterion for county comparison, and excludes county-pairs from the sample if the counties in the pair are less similar to each other along that criterion. In column (1), for instance, the distance between the centers of two counties in the pair is used as a criterion for county similarity. We only use the county-pairs if the distance is less than or equal to 20 miles (i.e., in the first tercile of the distance distribution). The intuition is that we expect the economic conditions of these two counties to be more similar to each other as the distance between the counties decreases. The coefficient is similar to the one in the full sample.

Table 2.3: **Within-Pair Estimation: County Characteristics**

	$\Delta log(CountyDeposit)$			
	(1) Distance	(2) Industry	(3) Banking	(4) CBSA
$\Delta log(UIBenefit)$, State	-0.063**	-0.080***	-0.073***	-0.122***
	(0.026)	(0.020)	(0.018)	(0.038)
$\Delta log(Income)$, County	0.059**	0.046	0.044	0.007
	(0.024)	(0.030)	(0.027)	(0.067)
Controls & Fixed Eff:				
Unemp.	Y	Y	Y	Y
cubic(Unemp.)	Y	Y	Y	Y
Pair–Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y
Obs.	12,086	12,122	11,488	4,576
R^2	0.583	0.607	0.629	0.592

Column (2) classifies the two counties in the pair as more similar based on their industrial composition. To make this classification, first we calculate the employment share of each industry in the counties by using the Regional Economic Information System of the

Bureau of Economic Analysis (BEA). Next, we construct the Euclidian distance between the two counties in the pair, and include in our sample only the most similar counties (i.e., county-pairs with industry distance of less than the first tercile value). The idea is that the counties with similar industrial compositions are more likely to react symmetrically to an economic shock. We obtain a similar coefficient with the one in our full sample.

In column (3), we analyze the banking sector of the two counties in the pair. Drechsler et al. (2017) show that when the Fed funds rate rises, banks in more concentrated deposit markets experience larger deposit outflows. This would bias our coefficient if one of the counties in the pair has a more concentrated deposit market and experiences a higher increase in UI benefits relative to the other county in the pair during Fed funds rate hikes.[18] In this case, the decrease in deposit levels in the concentrated county may result from either the change in state UI benefits (household deposit supply) or the change in the Fed funds rate (bank deposit demand). Therefore, different banking sector competition (i.e., concentration) in the two counties in the pair may drive our deposit results. To mitigate this concern, we calculate the deposit market HHI of the counties, and restrict our sample to county-pairs where the two counties have similar HHIs. Our main results do not change.

Finally, in column (4), we use the core based statistical area (CBSA) definition of Office of Management and Budget, according to which the counties are in the same statistical area if they are similar and integrated to each other socioeconomically. Column (4) includes only the county-pairs if the counties in the pair are also in the same statistical area. Therefore, the economic conditions in these two counties are arguably similar to each other by construction. The coefficient is still negative and significant despite the dramatic decrease in the sample size.

The second endogeneity concern relates to the core of our identifying assumption. As

[18]In the data, the level of regular UI benefits is positively correlated with the economic activity indicators. This may be because the improved state budget balances during economic expansions give state governments more room to increase UI benefits.

discussed in the identification strategy section (Section 2.4.1), our main identifying assumption is that state-level economic shocks that are correlated with UI changes must affect the two counties in the county-pair symmetrically.[19] Since the level of UI benefits is determined at the state level, state-level economic conditions have the potential to affect the level of UI benefits and at the same time the level of county deposits. This is not an endogeneity concern only if these state-level economic conditions affect the other county in the county-pair symmetrically, in which case making a within county-pair comparison cancels out the impact of the state shock on county deposits. To empirically test whether state-level economic conditions affect the two counties in the pair symmetrically, we include relevant proxies for state-level economic conditions into our main regression. If the counties in the pair are affected symmetrically, then we should have a zero coefficient for the proxies for the state economic conditions (Hagedorn et al., 2018).

In columns (1) through (3) of Table 2.4, we use our main border county sample, and include state income, state GDP, and state unemployment rate into the regressions as proxies for state economic conditions, respectively. First, adding these state-level proxies has no significant effect on the coefficient of state UI benefits, mitigating the concern that state-level economic conditions may drive our results. Second, in each specification, the coefficients of the state-level proxy variables are insignificant. This means that state-level economic conditions affect each county in the pair symmetrically, and thus the net effect in the county-pair comparison is zero. Although these results are consistent with our identifying assumption, the remaining question is whether the state-level economic proxies that we use in columns (1) through (3) are relevant variables for the county deposits. If we use irrelevant state-level variables in the regressions, then the test has no power. To justify the use of these state-level proxies, therefore, we construct a random scrambled sample.

[19] The state level economic shocks may be driven either by the changes in economic conditions of the state itself, or by the heterogeneous responses of states to changes in nationwide aggregate macroeconomic conditions. Our identification strategy and robustness check appeal to both types of shocks.

Instead of matching two neighboring border counties located in different states, we match two non-neighboring counties located in different states. For instance, instead of pairing a North Carolina border county and a Virginia border county that share a common border, we match NC border county with a border county in California (CA). In this constructed border county sample, there should be discontinuity of economic conditions across the two counties in the pair by construction. Therefore, with the constructed sample, comparing the counties in the same pair does not cancel out the effect of state-level economic shocks on the deposits. This means that the proxies of state-level economic conditions should have statistically significant coefficients with the expected signs. The results in columns (4) through (6) confirm our identifying assumption. Namely, state income and state GDP, which are expected to affect deposits positively, have positive and significant coefficients, and state unemployment, which is expected to affect deposits negatively, has a negative and significant coefficient. These results ensure that the test we have in the first three columns has power. Another observation in columns (4) through (6) is that the coefficient of UI benefits is insignificant. This implies that when the economic conditions are not properly controlled for, our coefficient of interest is biased upward. Thus, the remaining correlation, if any, between UI benefit and the error term due to economic conditions in the main specification creates bias against our results. The findings of the robustness exercises in Table 2.3 are also are in line with this implication. As we constrain the sample to county-pairs with more similar counties, our estimates become more negative.

A similar but a slightly different form of the previous endogeneity concern is that the correlation between the characteristics of the two counties in the same pair might be lower than the correlation among the counties in the same state. This is a legitimate concern since the counties in the same state are subject to the same set of rules and regulations. If this concern is true, then the state-level economic conditions in the state are applicable to the same-state border county, but not to the across-state border county, which violates our identifying assumption. To mitigate this concern, we provide two exercises. First, we show that the counties in the pair are more similar to each other than they are to the rest of the counties in their own states. Second, we estimate our main specification with a sample in which the counties that are highly correlated with their own states are excluded, and find that the results do not change.

Table 2.4: **Within-Pair Estimation: Continuous Economic Conditions**

	Dependent Variable: $\Delta log(CountyDeposit)$					
	Main Sample			Scrambled Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta log(UIBenefit)$,	-0.061***	-0.061***	-0.062***	-0.008	-0.006	-0.013
State	(0.016)	(0.015)	(0.016)	(0.017)	(0.017)	(0.017)
$\Delta log(Income)$,	0.045***	0.044***	0.045***	0.075***	0.086***	0.099***
County	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.015)
$\Delta log(Income)$,	-0.001			0.225***		
State	(0.048)			(0.043)		
$\Delta log(GDP)$		0.019			0.144***	
State		(0.036)			(0.027)	
$Unemp.rate$			-0.191			-0.636***
State			(0.144)			(0.108)
Controls & Fixed Eff:						
Unemp. controls	Y	Y	Y	Y	Y	Y
Pair–Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Obs.	36,148	36,148	36,148	35,974	35,974	35,974
R^2	0.597	0.597	0.597	0.569	0.569	0.570

For the first exercise, Table 2.5 displays the results of two comparisons. The first one compares the characteristics of the neighbor border counties in the pair. The first three columns show the descriptive statistics of this comparison. In a similar way, we compare the characteristics of the border counties with the rest of the counties in their own states. The second three columns show the descriptive statistics of this comparison. In the last column, we calculate the difference between the two comparisons. A negative value in this column indicates that the border counties are more similar to each other than they are to the rest of the counties in the state. Almost all variables have a negative value, mitigating the concern that the counties are similar to each other within a state.

Table 2.5: **County Comparisons: Pair County vs. State Counties**

	|Pair-County|			|Rest-County|			Diff.
	Mean	Med	SD	Mean	Med	SD	– Diff.
log(deposit)	0.99	0.81	0.80	1.41	1.25	1.01	-0.43***
deposit, growth(%)	1.33	1.07	1.08	2.20	1.68	1.87	-0.87***
log(income)	0.94	0.76	0.75	1.35	1.23	0.89	-0.41***
income, growth(%)	0.89	0.64	0.88	0.96	0.74	0.81	-0.07*
log(ave. wage)	0.16	0.13	0.13	0.28	0.27	0.16	-0.12***
ave. wage, growth(%)	0.55	0.44	0.45	0.48	0.37	0.42	0.07***
log(labor force)	0.90	0.74	0.71	1.25	1.12	0.85	-0.34***
labor force, growth(%)	0.79	0.62	0.70	0.87	0.73	0.69	-0.08**
unemployment rate (%)	1.12	0.83	1.04	1.31	0.91	1.40	-0.20***
manufacturing share (%)	0.09	0.07	0.08	0.08	0.07	0.07	0.01**
HHI, county	0.15	0.11	0.15	0.13	0.10	0.13	0.02***
Observations	997			997			1994

Table 2.6 presents the results of the second exercise, in which we exclude from the sample the counties that have a high correlation with their own states. For this exercise, we follow two different methodologies. First, we estimate the county income beta with respect to state income by regressing county income on state income, and exclude the border counties with high betas from the sample. Column (1) presents the result for this exercise, and confirms that the coefficient is still negative and significant. Second, we exclude counties from the sample if they are large relative to their states. If a county is large, then the change in county economic conditions is more influential on the changes in overall state-level economic conditions, which implies a high correlation between county and state economic conditions by definition. To exclude these counties, in column (2), we restrict our sample to the counties that have two percent or less of the state employment. The result confirms the negative and significant effect.

One concern in our empirical strategy is picking up the effect of other state-level policies. For instance, the generosity of state-level social welfare programs might be correlated with that of unemployment insurance policies. To alleviate such concerns, in Table 2.7, we control for other state policies. Namely, we include changes in minimum wage, health insurance payments, union coverage, and total non-UI transfers as additional controls. Including these controls either individually or altogether does not change the magnitude and significance of the cofficient of UI generosity.

Table 2.6: **Excluding Correlated Counties**

	$\Delta log(CountyDeposit)$	
	(1)	(2)
	Inc. Beta, Low	Emp. share, Low
$\Delta log(UIBenefit), State$	-0.066**	-0.046**
	(0.027)	(0.018)
$\Delta log(Income), County$	0.041*	0.042***
	(0.022)	(0.014)
Controls & Fixed Eff:		
Unemp.	Y	Y
cubic(Unemp.)	Y	Y
Pair–Year FE	Y	Y
County FE	Y	Y
Obs.	9,312	24,076
R^2	0.569	0.582

It is important to separate the impact of UI policies on the deposit holdings of households from its impact on the firms' deposit holdings. To understand whether our results hinge on firms' deposit holdings, we exclude the branches that firms are more likely to work with. Firms are expected to hold their deposits in large branches (Homanen, 2018). Large branches have more officers, hence are able to provide services with less delays. This may make firms choose to work with these branches. Alternatively, firms may make some branches large by depositing their money into those branches. Without taking a stance on the exact mechanism, we exclude the largest one percent of the branches from the sample and calculate county deposits by using the deposits of the remaining branches. The results do not change (Table 2.8), suggesting that the impact of UI on deposits is as a result of the changes in households' precautionary savings.

Table 2.7: **Deposits and UI Benefits, Other Policies**

	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit),\ State$	-0.078***	-0.061***	-0.062***	-0.061***	-0.079***
	(0.019)	(0.015)	(0.015)	(0.016)	(0.019)
$\Delta log(Min.\,Wage),\ State$	1.047				1.079
	(0.774)				(0.766)
$\Delta log(Health\,Ins.),\ State$		0.007			0.006
		(0.007)			(0.008)
$\Delta Union\,Cov.,\ State$			-0.059		-0.055
			(0.058)		(0.071)
$\Delta log(non-UI\,Transfers),\ State$				-0.012	-0.015
				(0.014)	(0.014)
Controls & Fixed Eff:					
County Controls	Y	Y	Y	Y	Y
County Pair x Year FE	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y
Obs.	27,408	36,144	36,144	36,144	27,408
R^2	0.601	0.597	0.597	0.597	0.601

Table 2.8: **Deposits and UI Benefits, Excluding Largest Branches**

	$\Delta log(CountyDeposit)$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit), State$	-0.045***	-0.055***	-0.057***	-0.057***	-0.058***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
$\Delta log(Income), County$			0.055***	0.054***	0.055***
			(0.016)	(0.016)	(0.015)
Controls & Fixed Eff:					
Unemp.	N	N	N	Y	Y
cubic(Unemp.)	N	N	N	N	Y
Pair–Year FE	Y	Y	Y	Y	Y
County FE	N	Y	Y	Y	Y
Obs.	36,148	36,148	36,148	36,148	36,148
R^2	0.560	0.595	0.596	0.596	0.596

The SCF data show that majority of the households hold bank deposits as their main financial assets. However, UI may also have an impact on other types of financial assets (i.e., bonds, stocks). On the one hand, an increase in UI benefits may weaken the households' precautionary motive, hence lower their bonds and stocks holdings. On the other hand, households may want to increase their holdings of these assets as their income risk becomes lower. This portfolio adjustment may have important implications for the financing policy of firms. For instance, if UI increases bond holdings of households, then even though firms lose access to bank loans, they can replace this decrease with bond issuance. We perform two exercises to understand if these mechanisms are at play by using the Internal Revenue Service (IRS) data. IRS's Statistics of Income (SOI) database provides county level interest and dividend income statistics. Under the assumption that counties in the same pair have similar bond and stocks portfolios, differences in incomes generated by these assets imply different level of these asset holdings.[20] We replicate our main specification by replacing county deposits with interest earnings on bonds and dividend income on stocks. We find no effect of UI on bonds (Table 2.9) and on stock holdings (Table 2.10). These findings may be explained either by the two opposing effects discussed above or by low unemployment risk of stock and bond holders.

Table 2.9: **Interest Income and UI Benefits, Other Financial Assets: Bonds**

	$\Delta log(County Interest Income)$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit)$,	-0.080	-0.062	-0.089	-0.089	-0.093
State	(0.132)	(0.151)	(0.152)	(0.154)	(0.154)
$\Delta log(Income)$,			0.723***	0.723***	0.759***
County			(0.202)	(0.203)	(0.209)
Controls & Fixed Eff:					
Unemp.	N	N	N	Y	Y
cubic(Unemp.)	N	N	N	N	Y
Pair–Year FE	Y	Y	Y	Y	Y
County FE	N	Y	Y	Y	Y
Obs.	15,260	15,228	15,228	15,228	15,228
R^2	0.618	0.631	0.633	0.633	0.633

[20]We calculate interest income on bonds by subtracting the interest income on deposits from total interest income.

2.4.4 Heterogeneity

In this section, we exploit the heterogeneity at the county level and aggregate bank level to provide additional evidence that supports our conclusion: the change in the level of UI benefits is the driving force behind the decrease in deposits.

At the county level, we exploit the heterogeneity of counties in their sensitivities to the changes in UI benefits. If our economic mechanism is true then we should see stronger results for the subset of counties in which the changes in UI benefits are more relevant/binding. One possible way to test this is to classify counties based on their characteristics. For instance, since unemployment risk is higher for workers in the manufacturing industry,[21] the change in the level of UI benefits is expected to have a stronger impact on the saving behavior of workers in this industry, suggesting that our results should be stronger for counties where the employment share of the manufacturing industry is high. However, the U.S. UI system poses a challenge to using the share of manufacturing in the heterogeneity test. The challenge emerges in the following way: according to the UI system, changes in the

Table 2.10: **Dividend Income and UI Benefits, Other Financial Assets: Stocks**

	$\Delta log(CountyDividends)$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit), State$	0.041	0.050	0.042	0.042	0.042
	(0.050)	(0.054)	(0.053)	(0.053)	(0.054)
$\Delta log(Income), County$			0.220^{***}	0.221^{***}	0.221^{***}
			(0.063)	(0.063)	(0.063)
Controls & Fixed Eff:					
Unemp.	N	N	N	Y	Y
cubic(Unemp.)	N	N	N	N	Y
Pair–Year FE	Y	Y	Y	Y	Y
County FE	N	Y	Y	Y	Y
Obs.	36,128	36,128	36,128	36,128	36,128
R^2	0.754	0.760	0.760	0.760	0.761

[21]See Table A1 in Agrawal and Matsa (2013) for average layoff separation rates of U.S. industries based on BLS "Mass Layoff Statistics"

level of UI benefits are not binding for low-income employees due to percentage caps the UI benefit schedules impose. For instance, the average percentage cap in our sample is 50 percent, and indicates that an unemployed worker is able to obtain UI benefits of up to 50 percent of his previous wage income. If the previous wage of this worker is too low, then the percentage cap will be binding for him, and he would not be able to take advantage of the increases in the level of UI benefits (i.e. the level of UI benefits are not binding for him). This implies that, according to our proposed economic mechanism, this worker must not change his saving behavior, and by extension his deposit holdings at banks. This means that the percentage cap the UI benefit schedules impose is binding for low-income workers in the manufacturing industry. Therefore, the changes in dollar cap (i.e. UI benefits–our variable of interest) are not binding for them. As a result, the design of the UI system in the U.S. creates non-linearity in the effect of industrial composition on the strength of the link between UI benefits and household saving. This makes it harder to conjecture on which industry workers the changes in the level of UI benefits have a stronger effect.

To overcome this challenge, instead of exploiting the heterogeneity of county characteristics, we focus on the realized UI payments of counties. More specifically, we obtain the UI beta for each county in the sample by regressing the county-level realized UI payments on state UI benefits. High UI beta for the county implies that the changes in state UI benefits are more binding for the workers in the county, while low UI beta implies they are less binding. Table 2.11 shows that the effect of UI benefits is stronger for high beta counties, while it is not significant for low beta ones, consistent with our prediction.

2.4.5 Bank Level Evidence

To understand the implications of changes in UI on bank balance sheets, we now turn to Call Reports data and exploit heterogeneity of deposit accounts. We employ a propensity score matching procedure to compare banks that are similar to each other along variables that are commonly used in the literature. In our analysis, we consider a bank as treated in a given year if the bank's UI exposure is above the median value of UI exposure distribution in that year. Thus, the banks that are below the median value constitute our control group. After obtaining treatment and control groups, we run a logit model and estimate the propensity score for each year separately.[22] Then, we match each treated bank with a control bank that has the nearest propensity score.

We follow Imbens (2015), and restrict our matching sample in two ways. First, we exclude observations if the estimated propensity score is below 0.2 and above 0.8. This mitigates the concern about the validity of the overlap assumption in the tails of propensity score distribution. Second, we exclude the matches if the difference between the propensity scores of the treated and control bank is above 0.034, which corresponds to one fourth of the standard deviation of propensity score in our sample.[23] This criteria helps to increase the matching quality. Table 2.12 shows the summary statistics of treated and control groups

Table 2.11: **Heterogeneity: County UI Sensitivity**

	$\Delta log(CountyDeposit)$	
	(1)	(2)
	County UI Beta, High	County UI Beta, Low
$\Delta log(UIBenefit), State$	-0.097***	-0.049
	(0.034)	(0.030)
$\Delta log(Income), County$	0.020	0.013
	(0.024)	(0.043)
Controls & Fixed Eff:		
Unemp.	Y	Y
cubic(Unemp.)	Y	Y
Pair–Year FE	Y	Y
County FE	Y	Y
Obs.	5,472	5,646
R^2	0.601	0.623

[22]Control variables in this logit model are log(assets), equity ratio, cash ratio, liquidity ratio, bank level deposit market HHI, bank's personal income and unemployment rate exposures, squares of these variables, and interactions of balance sheet variables with each other.

[23]Scharfstein and Sunderam (2014) use a similar approach.

as well as the full sample. We provide the normalized difference for the treated and full sample in column 5 and for the treated and control sample in column 8. The normalized difference is the difference of mean values of two groups divided by square root of average of variances of the two groups. Imbens (2015) stresses the importance of this parameter as it "provides a scale and sample size free way of assessing overlap". The last column of the table shows how matching procedure improves the similarity. Except for equity ratio, the normalized differences of all variables get smaller significantly.

Table 2.12: **Deposits and UI Benefits: Matching Exercise-Balance Table**

	Treated		Full		Norm.	Control		Norm.	% Change in
	Mean	SD	Mean	SD	Diff.	Mean	SD	Diff.	Norm. Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
log(Assets)	11.59	1.34	11.56	1.35	0.02	11.61	1.30	-0.01	-49.08
Equity(%)	10.71	4.77	10.75	5.54	-0.01	10.76	4.95	-0.01	19.65
Liquidity(%)	30.64	14.65	31.41	15.58	-0.05	29.99	14.75	0.04	-15.95
HHI, Bank	0.20	0.12	0.23	0.14	-0.27	0.22	0.13	-0.16	-64.63
Cash(%)	5.44	4.98	5.85	5.59	-0.08	5.58	5.04	-0.03	-176.35
log(Income), county	14.70	1.91	14.49	1.97	0.11	14.59	1.91	0.06	-79.65
Unemp. Rate, county	5.37	2.19	5.59	2.51	-0.09	5.47	2.33	-0.04	-123.66
log(wage), county	20.51	2.21	20.21	2.31	0.13	20.30	2.25	0.09	-43.38
Obs	52,949		57,255			25,920			

For the sake of comparability to the county-level analysis, we keep the pair structure in the matched sample. This means that each treated and its matched bank constitute a bank pair and we compare the change in deposits of these two banks. In particular, we run the following model

$$\begin{aligned} \Delta log(deposit_{b,y}) = \beta \Delta log(UI\ Exp._{s(b),y}) + \gamma_1 \Delta log(Inc.\ Exp._{c(b),y}) \\ + \theta f(Unemp.\ Exp._{c(b),y}) + \delta_{p(b),y} + \varepsilon_{b,y} \end{aligned} \tag{2.2}$$

where the dependent variable is the change in the log of the total deposits of bank b from $y-1$ to y, $log(UI\ Exp._{s(b),y})$ is the contemporaneous log change in the UI exposure of the bank, and $\delta_{p(b),y}$ are the bank pair-year fixed effects. Similar to county-level analysis, we include income and unemployment rate exposures in some specifications. Again, we expect to find $\beta < 0$.[24]

Table 2.13 presents the main results of the matching exercise. Each specification in this table includes pair-year fixed effects, which means that every treated bank is compared to its matched untreated bank in a given year. Similar to Table 2.2, we don't include any control variable in column (1), and we include additional control variables in columns (2) through (5), such as banks' exposure to county-level income and unemployment rate. In all of these specifications, the coefficient of bank UI exposure is negative and statistically significant. The economic meaning of this coefficient is that one standard deviation increase in bank UI exposure decreases bank deposits by 1.82 percent at its mean value. Note that this effect is quite similar to what we find in our county-level analysis.

Table 2.13: **Deposits and UI Benefits: Matching Exercise-Bank Level**

	$\Delta log(Deposits, Bank)$			
	(1)	(2)	(3)	(4)
$\Delta log(UI\ Exposure),\ Bank$	-0.102*	-0.105*	-0.105*	-0.104*
	(0.050)	(0.051)	(0.050)	(0.051)
$\Delta log(Inc.\ Exposure),\ Bank$		0.108***	0.108***	0.108***
		(0.009)	(0.009)	(0.009)
Controls & Fixed Eff:				
Unemp. Exp.	N	N	Y	N
Cubic in unemp. exp.	N	N	N	Y
Bank Pair x Year FE	Y	Y	Y	Y
Obs.	96,618	96,618	96,602	96,602
R^2	0.504	0.511	0.511	0.511

Annual Call Reports data allows us to exploit heterogeneity of deposit accounts. The driving force behind the mechanism that we propose for the deposit outcome is the change

[24] Note that the independent variable in this analysis is slightly different from the one we use in the county-level analysis. Instead of using state UI benefit, we use a bank level UI exposure variable that shows the average UI level a bank faces, based on the states where the bank raises deposits. See Section 3.2.1 for the constructions of this variable.

in the precautionary saving motives of households who face unemployment risk. Since these households have relatively low incomes, they are not expected to have large deposits in their bank accounts. Therefore, we expect to find that the effect of state UI benefits on small deposits is strong and significant, but insignificant on large deposits. From 2006 onwards, banks have started to report the amount of small deposits and large deposits in their Call Reports. To test our hypothesis, we run the model in Equation (2.2) but instead of using all deposits as dependent variable we use small and large deposits separately. Table 2.14 shows the results for small deposits and Table 2.15 shows the results for large deposits. Conforming our intuition, the change in bank UI exposure has an impact only on small deposits.

Table 2.14: **Deposits and UI Benefits: Matching Exercise-Bank Level Small Deposits**

	$\Delta log(Deposits < 250k, Bank)$			
	(1)	(2)	(3)	(4)
$\Delta log(UI\ Exposure),\ Bank$	-0.542*	-0.574*	-0.474*	-0.562*
	(0.229)	(0.222)	(0.178)	(0.216)
$\Delta log(Inc.\ Exposure),\ Bank$		0.090***	0.092***	0.091***
		(0.006)	(0.005)	(0.006)
Controls & Fixed Eff:				
Unemp. Exp.	N	N	Y	N
Cubic in unemp. exp.	N	N	N	Y
Bank Pair x Year FE	Y	Y	Y	Y
Obs.	22,052	22,052	22,042	22,042
R^2	0.625	0.627	0.629	0.627

Further bank-level evidence comes from deposit rates that banks pay to their depositors and the heterogeneity of banks in terms of the share of small deposits in their balance sheets. As we discuss in Section 2.4.2, the mechanism in this paper is supply-driven, which is supported by our branch-level analysis. To further support our interpretation of the results, we analyze the effect of UI changes on deposit rates. If the results are supply driven, then the price (deposit rate) and quantity (deposit amount) should move in opposite directions; on the other hand, if the results are demand-driven they should move in the same direction. We obtain the deposit rate from Call Reports by dividing the end of year total deposit interest expenses to lagged total deposits. Column (1) of Table 2.16 reports that banks pay more interest on their deposits when their UI exposure increases, supporting the supply mechanism. Furthermore, in Table 2.17 and Table 2.18, we split the banks into two subsamples based on the share of small deposits in their balance sheets, and show that deposit rate increase is stronger for banks with a higher small deposit ratio.[25] The small deposits are more critical in the funding structure for these banks; hence, they are more eager to pay higher interest rates to their depositors in order to prevent the decrease in deposits.

Table 2.15: **Deposits and UI Benefits: Matching Exercise-Bank Level Large Deposits**

	$\Delta log(Deposits > 250k, Bank)$			
	(1)	(2)	(3)	(4)
$\Delta log(UI\ Exposure),\ Bank$	-0.170	-0.231	-0.219	-0.229
	(0.217)	(0.202)	(0.166)	(0.201)
$\Delta log(Inc.\ Exposure),\ Bank$		0.170**	0.170**	0.170**
		(0.035)	(0.034)	(0.035)
Controls & Fixed Eff:				
Unemp. Exp.	N	N	Y	N
Cubic in unemp. exp.	N	N	N	Y
Bank Pair x Year FE	Y	Y	Y	Y
Obs.	21,900	21,900	21,890	21,890
R^2	0.703	0.705	0.705	0.705

[25] The sample size decreases considerably as the information on small and large deposits is available only for the period after 2006.

2.5 Lending analysis

After establishing the negative effects of UI generosity on bank deposits, in this section, we study the impact of UI generosity on bank commercial lending. Since banks cannot perfectly replace deposits with other sources of funding, they are expected to squeeze their loan supply in response to an increase in their unemployment insurance exposure. The main identification challenge to test this prediction on loan supply is to control firm loan demand. If a firm's loan demand decreases as the UI exposure of its lenders increases,

Table 2.16: **Deposits and UI Benefits: Matching Exercise-Bank Level Deposit Rate**

	$\Delta(Interest\ Expense/Deposits)$			
	(1)	(2)	(3)	(4)
$\Delta log(UI\ Exposure),\ Bank$	0.003*	0.003*	0.003*	0.003*
	(0.002)	(0.002)	(0.002)	(0.002)
$\Delta log(Inc.\ Exposure),\ Bank$		-0.001*	-0.001*	-0.001*
		(0.001)	(0.001)	(0.001)
Unemp. Exp.	N	N	Y	N
Cubic in unemp. exp.	N	N	N	Y
Bank Pair x Year FE	Y	Y	Y	Y
Obs.	81,086	81,086	81,070	81,070
R^2	0.719	0.720	0.720	0.720

Table 2.17: **Deposits and UI Benefits: Matching Exercise-Bank Level Deposit Rate for Banks with High Small Deposit Ratio**

	$\Delta(Interest\ Expense/Deposits), < 250k,\ Bank$			
	(1)	(2)	(3)	(4)
$\Delta log(UI\ Exposure),\ Bank$	0.010*	0.010*	0.009*	0.010*
	(0.004)	(0.004)	(0.004)	(0.004)
$\Delta log(Inc.\ Exposure),\ Bank$		-0.001	-0.001	-0.001
		(0.001)	(0.001)	(0.001)
Unemp. Exp.	N	N	Y	N
Cubic in unemp. exp.	N	N	N	Y
Bank Pair x Year FE	Y	Y	Y	Y
Obs.	13,206	13,206	13,200	13,200
R^2	0.793	0.793	0.794	0.794

then the decline in the equilibrium amount of loans would be erroneously attributed to the increase in bank UI exposure.

2.5.1 Identification strategy

To address this identification challenge and to establish the causality from bank UI exposure to commercial lending, we follow Khwaja and Mian (2008), and implement within-firm estimation using annual firm-bank level Dealscan data on commercial loans by banks. In particular, we use firm-year fixed effects, and compare the loan amounts to the same firm in the same year by banks with different UI exposure. To make this within-firm estimation, we use only the sample of firms that work with at least two banks in a given year, and exclude all others since the coefficient is not identified for single-bank firms. Assuming that firm loan demand is symmetric across different banks, our empirical strategy holds loan demand fixed, hence enables us to uncover the effect of bank UI exposure on their loan supply.

For our within-firm estimation, we estimate the following regression model (a là Khwaja and Mian 2008):[26]

Table 2.18: **Deposits and UI Benefits: Matching Exercise-Bank Level Deposit Rate for Banks with Low Small Deposit Ratio**

	$\Delta(Interest\ Expense/Deposits), > 250k,\ Bank$			
	(1)	(2)	(3)	(4)
$\Delta log(UI\ Exposure),\ Bank$	0.005	0.004	0.004	0.004
	(0.003)	(0.003)	(0.004)	(0.003)
$\Delta log(Inc.\ Exposure),\ Bank$		-0.001	-0.001	-0.001
		(0.001)	(0.001)	(0.001)
Unemp. Exp.	N	N	Y	N
Cubic in unemp. exp.	N	N	N	Y
Bank Pair x Year FE	Y	Y	Y	Y
Obs.	12,076	12,076	12,066	12,066
R^2	0.808	0.809	0.809	0.809

[26]Examples of the Khwaja and Mian (2008) strategy include Jiménez et al. 2014 and, Amiti and Weinstein 2018.

$$\Delta log(loan_{f,b,y}) = \beta \Delta log(UI^{bank}_{b,y-1}) + \gamma \Delta BankControls_{b,y-1} + \delta_{f,y} + \alpha_b + \varepsilon_{f,b,y} \tag{2.3}$$

where the dependent variable is the change in log of the outstanding loan amount granted by bank b to firm f in year y, $\Delta log(UI^{bank}_{b,y-1})$ is the lagged change in log of the bank UI exposure of bank b, $\delta_{f,y}$ are firm-year fixed effects for firm f, and α_b are bank fixed effects for bank b. Across different specifications, we also control for bank size, equity ratio, average deposit rate, net interest income ratio, and Herfindahl-Hirschman index (HHI) of the deposit markets where the banks operate. The coefficient of interest is β, with an expectation of negative sign. We double-cluster standard errors at the bank and firm level.

The key control in this within-firm comparison is firm-year fixed effects, which allows time-varying differences among firms. Under the assumption that a firm's loan demand across its lenders is symmetric, using firm-year fixed effects controls for the firm's loan demand and enables us to measure the effect of bank UI exposure on loan supply. Including bank fixed effects further control unobserved time-invariant differences among banks (e.g., bank management).

Note that, instead of using the estimated change in deposits caused by the change in UI benefits, we use bank UI exposure as our variable of interest. This allows us to measure the effect on loan supply of the UI policy itself, instead of the changes in deposits. To better understand this, consider a bank whose UI exposure increases takes an action to slow down the decrease in its deposits. Then, although the UI exposure of the bank increases, the bank does not experience a decrease in its deposits; hence, there should be no effect on its loan supply. If other banks in our sample also take similar actions, then the coefficient of bank UI exposure in our model would be insignificant. A model that uses the estimated decrease in deposits, on the other hand, can mask this ineffectiveness of the UI policies on bank lending, since it recovers the treatment effect on the treated banks. In other words, that model is designed to measure the decrease in loan supply only for the banks whose deposits decline due to an increase in UI benefits. However, in our model, we allow banks to take actions to prevent the decline in their deposits, and hence measure the effect of bank UI exposure on bank loan supply. Therefore, our model provides more insights about the true policy effects on bank loan supply.[27]

2.5.2 Within-firm estimation

Table 2.19 presents the results for commercial lending analysis. Each specification in the table includes firm-year and bank fixed effects, meaning we compare the lending of different banks to the same firm in the same year, and control for time-invariant bank characteristics. Column (1) is our baseline specification with no control variables other than the firm-year and bank fixed effects, and shows a negative and significant coefficient for bank UI exposure. The economic meaning of this coefficient is that a one standard deviation increase in bank UI exposure decreases the loan supply by 2.6 percent at the mean value. In column (2), we saturate the model with bank control variables that are used in the bank lending literature: bank size, equity ratio, liquidity ratio, profitability, deposit rate, and average HHI of deposit markets where the bank operates. The magnitude of the coefficient is similar to column (1), but its statistical precision increases.

In columns (3) through (6), we provide two additional pieces of evidence that support our interpretation of the results. First, we exploit the heterogeneity of banks in their ability to replace the decrease in deposits. Banks with lower equity ratios are expected to suffer more from agency problems (Holmstrom and Tirole, 1997), and thus have more difficulty in substituting the decrease in deposits with external wholesale funding. Therefore, we

[27] Our model is comparable to the intention-to-treat effect estimator. For more discussion about the intent-to-treat and treatment-on-treated see *Mostly Harmless Econometrics* (Angrist and Pischke, 2008), and Dupas et al. (2018).

expect that these banks squeeze their lending supply more. In columns (3) and (4), we split the banks into two subsamples based on their equity ratio. In line with our expectation, the banks with low equity ratios decrease their lending more, whereas the effect is insignificant for the banks with high equity ratios.

Second, in columns (5) and (6), we report the results by dividing the sample into two based on the share of small deposits in bank balance sheets, and find that the effect is stronger for banks that have high small deposit ratios. This exercise serves two purposes. First, the exercise is parallel to the one that we do on deposit analysis, in which we show that the decrease in deposits is especially strong for banks with high small deposit ratios, suggesting that the lending effect should also be stronger for these banks. Second, this exercise helps us to mitigate concerns about omitted variable bias. Namely, if there were an unobserved bank-level variable correlated with bank UI exposure, our results could be driven by this variable. Yet, the findings in columns (5) and (6) depict that the concerns about the omitted variable bias are valid only if the unobserved bank-level variable is correlated with the bank small deposit ratio as well as bank UI exposure, which is highly unlikely. Therefore, finding stronger results for commercial lending for the banks with a high small deposit ratio further supports our mechanism.

Table 2.19: **Commercial Loan and Bank UI Exposure: Within-Firm Estimation**

	Dependent Variable: $\Delta log(loan)$					
	All		Equity Ratio		Small Deposit Ratio	
	(1) All	(2) All	(3) High	(4) Low	(5) High	(6) Low
$\Delta log(UIExposure)$, Bank	-0.125*	-0.129**	-0.054	-0.316**	-0.332***	-0.098
	(0.071)	(0.052)	(0.088)	(0.137)	(0.094)	(0.091)
Controls & Fixed Eff:						
Bank controls	N	Y	Y	Y	Y	Y
Firm–Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Obs.	174,179	174,179	45,377	46,498	34,222	37,140
R^2	0.647	0.648	0.708	0.674	0.723	0.729

2.6 Investment analysis

Lastly, in this section, we test whether firms that borrow from banks with high unemployment insurance exposure experience a reduction in their investment. Consistent with the literature, we find that the decrease in firms' access to bank lending adversely affects their investment. The effect is especially strong for financially constrained firms, which implies that these firms cannot substitute bank lending with other sources of external funding (e.g., bond issuance).

2.6.1 Identification strategy

For our investment analysis, we estimate the following regression model:

$$\begin{aligned} investment_{f,y} = \beta UI^{firm}_{f,y-1} + \gamma BankControls_{b,y-1} + \kappa FirmControls_{f,y-1} \\ + \delta_f + \alpha_b + \eta_{ind,y} + \lambda_{loc,y} + \varepsilon_{f,y} \end{aligned} \tag{2.4}$$

where the dependent variable is firm f's investment ratio (capital expenditure divided by lagged assets) in year y; $UI^{firm}_{f,y-1}$ is firm f's lagged UI benefit exposure; δ_f, α_b, $\eta_{ind,y}$, $\lambda_{loc,y}$ are firm, bank, firm's industry-year, and firm's location-year fixed effects, respectively. We saturate our model with bank controls, and firm controls. The coefficient of interest is β, with an expectation of a negative sign. We cluster standard errors at the firm and year level.

A major concern about this exercise is that a negative coefficient on β might be a consequence of the effects of UI benefits on labor markets rather than the bank lending channel that we aim to identify. For instance, if higher UI benefit decreases job search intensity and increases the equilibrium wage, then a decline in firm investment might be due to lower firm employment creation induced by the higher UI benefit in the firm's location.[28] It is also possible that an economic shock could lead to higher UI benefits

[28]The labor search literature discusses two types of effects: micro and macro (Diamond, 1982; Mortensen and Pissarides, 1994). The negative effect of UI benefits on the job search intensity of individuals is called the micro effect, and the negative effect of UI benefits on the job creation of firms due to higher equilibrium

and lower firm investment demand, which would create a spurious correlation between UI benefits and firm investment.

To tackle these concerns, in our specification we include the firm's location-year fixed effects, which means we compare the firms that face the same level of state UI benefits but have different UI exposure through their lenders. Moreover, we include industry-year fixed effects to control for industry-specific shocks. These controls warrant that we measure only the bank channel of UI on firm outcomes.

2.6.2 Investment results

Table 2.20 presents the results for our investment analysis. Each column includes firm, state-year, and industry-year fixed effects, meaning we are controlling time-invariant firm-level covariates, state-level economic shocks, and industry-level economic shocks. Column (1) of Table 2.20 is our baseline specification with no firm and bank controls, and shows that as firm UI exposure increases by one standard deviation, firm investment ratio decreases by 23 basis points. This magnitude implies that the firm investment level decreases by 3 percent at its mean value. In column (2), we include firm controls (Tobin's Q, leverage ratio, size, Z-score) and bank controls (size, equity ratio, and liquidity ratio).[29] Adding firm and bank control variables increases the magnitude of the coefficient slightly. These results show that the decrease in bank lending induced by the decline in deposits in response to more generous UI benefits has real consequences on firm investment.

In columns (3) and (4), we split the sample of firms into two groups based on their financial constraints. We follow the literature and use firm size as a proxy for firm financial

wage is called the macro effect. More recently, these effects are also discussed in (Agrawal and Matsa, 2013), and Hagedorn et al. (2018). On the one hand, the results in Hagedorn et al. (2018) imply that firm investment decreases if UI benefits increase due to a higher equilibrium wage. On the other hand, since the compensating wage premium that employees ask for decreases as UI benefits increase, Agrawal and Matsa (2013) suggest that the firm can pass the freed cash flow on the investment leading to a higher firm investment ratio.

[29]If a firm is served by more than one bank, the bank variables are the weighted average of individual bank variables by using the outstanding loan amount between the firm and its lenders as weights.

constraints. Small firms suffer more from agency problems and hence have more limited access to external funding sources other than bank lending. Therefore, for our investment analysis, we should find stronger results for small firms. The results confirm our intuition. The effect is larger for small firms, whereas the coefficient for the sample of large firms is insignificant.

Table 2.20: **Firm Investment**

	All		Firm Size		Small Deposit Ratio	
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Large	Small	High	Low
UI Exposure, Firm	-0.234**	-0.249**	-0.137	-0.448***	-0.456**	-0.039
	(0.109)	(0.093)	(0.216)	(0.098)	(0.178)	(0.184)
Controls & Fixed Eff:						
Firm Controls	N	Y	Y	Y	Y	Y
Bank Controls	N	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
State–Year FE	Y	Y	Y	Y	Y	Y
Industry–Year FE	Y	Y	Y	Y	Y	Y
Obs.	25,255	25,255	11,042	12,533	11,400	10,126
R^2(Adj.)	0.669	0.709	0.755	0.702	0.717	0.735

Columns (5) and (6) divide the sample of firms into two based on the share of small deposits in their lenders' balance sheets, and produce consistent results with the exercises that we previously did for deposits and commercial lending analysis. In those previous exercises, we find that the decrease in total deposits is mainly due to decreases in small deposits, and that the banks that have a higher reliance on small deposits squeeze their loan supply more. This implies that the decline in firm investment should be stronger if the firm's lender heavily relies on small deposits for its funding. The results are in line with our hypothesis, and support our interpretation of the results: the observed decline in firm investment is due to a decrease in loan supply.

In Table 2.21, we focus only on the sample of small firms. As in the previous table, each specification includes firm and bank control variables as well as firm and industry-year fixed effects. In column (1), we refine the spatial control by including county-year fixed effects instead of state-year fixed effects. With industry-year fixed effects, we compare the investment levels of different firms located in the same county while controlling for industry-specific investment demand. Even in this tight specification, the coefficient is still statistically significant and the magnitude does not change.[30] In column (2), we introduce bank fixed effects to control for bank characteristics that are time-invariant. The coefficient is still negative and statistically significant. Note that if a firm is working with more than one bank, we use the largest bank as the lender of the firm to define bank fixed effects. Since this assumption is not perfect, we introduce an attenuation bias, which explains the drop in the magnitude of the coefficient. Furthermore, including the bank fixed effects reduces the variation in bank UI exposure, and hence the variation in firm UI exposure, which may also partly explain the drop in the size of coefficient. In columns (3) and (4), to further investigate the bank-level external funding frictions, we divide our sample into two based on the equity ratio of the banks. Consistent with the loan outcome results, the effect

Table 2.21: **Firm Investment, Small Firms**

	All		Equity ratio	
	(1)	(2)	(3)	(4)
	All	All	High	Low
UI Exposure, Firm	-0.421***	-0.260***	-0.305	-0.378**
	(0.113)	(0.076)	(0.196)	(0.158)
Controls & Fixed Eff:				
Firm Controls	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Bank FE	N	Y	N	N
State–Year FE	N	Y	Y	Y
County–Year FE	Y	N	N	N
Industry–Year FE	Y	Y	Y	Y
Obs.	11,016	13,388	5,516	6,549
R^2(Adj.)	0.706	0.702	0.748	0.691

[30]Note that the comparable specification is reported in column (4) of Table 2.20.

is stronger for firms that borrow from the low equity banks. The effect is still negative for the other half of the sample, but the coefficient is not significant.

2.7 Discussion and Conclusion

It has been well documented that, both theoretically and empirically, lower income risk reduces precautionary savings for households. As UI benefits reduce the left tail of income risk, a rare but disastrous state for an individual, it also has similar, likely larger, effects. What has been missing is an analysis of the natural link between unemployment insurance, household savings and bank deposits, and bank lending. We aimed to fill this gap with this paper.

We have three sets of results. First, using both county- and bank-level data we show that more generous UI benefits reduce bank deposits. Second, we use matched bank-firm data from Dealscan and show that banks that collect deposits from counties that have more generous UI benefits originate less credit to firms. Third, we use Compustat data and find that firms that work primarily with banks that raise deposits in regions with higher UI benefits have lower investment. All of our results indicate both statistically and economically significant effects. Taken all together, our findings provide a strong set of evidence that UI benefits distort bank funding and commercial lending, and hence have an adverse impact on firm investment.

Our findings rely on U.S. data. In the U.S., social welfare programs are relatively less generous and firms finance themselves primarily from financial markets rather than from banks. Therefore, we suspect that the mechanisms highlighted in our paper may be even stronger in countries where both UI coverage ratios are larger and the duration of UI payments is longer, such as in European countries. Besides, since non-US firms are much more bank-dependent than their US counterparts, the real effects of bank UI exposure on firm outcomes may be stronger for these firms.

UI benefits certainly affect employed and unemployed people differently. For example, recent evidence by Hsu et al. (2018) suggests that UI benefits reduce default probability of the unemployed. Similarly, UI benefits are found to lower job search intensity and increase reservation wages for the unemployed. Different from this literature, our results are unconditional, i.e., UI benefits lower precautionary motive of every individual in the economy irrespective of the employment state. Therefore, the macroeconomic effects are likely to be stronger compared to the studies that base their analysis only to the unemployed, which forms on average about 5-6 percent of population.

Similar to many papers, we use cross-sectional data to identify the causal mechanism. As a result, our findings compare how different counties, banks and firms behave from their counterparts as the UI benefits that they face changes. By construction, this kind of methodology cannot say anything about the effects of the mean UI benefits on the macro economy. For that, one needs to have a general equilibrium model with an explicit treatment of income and unemployment risk, precautionary savings and bank lending. This is a topic of an ongoing research.

Chapter 3

Monetary Policy Exposure of Banks and Loan Contracting

3.1 Introduction

Monetary policy is transmitted to the real economy through several channels. A well-documented one is the "bank lending" channel: an increase in the Federal (Fed) funds rate reduces banks' lending capacity, which, in turn, induces banks to cut corporate lending.[1] For instance, during a typical 400 basis points (bps) Fed hiking cycle, banks reduce lending by as much as 10 percent (Drechsler et al., 2017).[2] Banks can achieve this reduction by simply rejecting new loan applications (Jiménez et al., 2012b). Alternatively, they may reduce the size of outstanding loans in their balance sheets if they have the flexibility to do so.

In this paper, I study how banks set loan contract terms in a way that would help them reduce lending to existing borrowers who have outstanding loan balances during times of monetary policy contractions. Theoretically, banks could prepare for future contractionary monetary policy shocks in two ways. One way is to write loan contracts with short maturities. This would allow banks to reduce lending by not renewing maturing loans to existing borrowers when their lending capacity is hit by a monetary policy shock. Another way to prepare is to write loan contracts with stricter covenants. This would make existing borrowers more likely to violate covenants, leading to more frequent contract renegotiations during which banks have the right to reduce lending. Which of these two ways prevails is

[1] See Blinder and Stiglitz (1983); Bernanke and Blinder (1988); Kashyap and Stein (1994); Van den Heuvel (2002).

[2] See Bernanke and Blinder (1992) for a similar magnitude of estimate.

an empirical question.

I find that the predominant way that banks prepare for future monetary policy shocks is by adjusting covenant strictness rather than loan maturity. In particular, banks that anticipate a larger decrease in their lending capacity during monetary policy contractions write loan contracts with stricter covenants. They do not shorten loan maturity, which is consistent with survey evidence indicating that firms may have a strong preference for long-term loans because of their operational needs and balance sheet structure (Graham and Harvey, 2001). Banks may also prefer issuing long-term loans with strict covenants over short-term loans because frequently evaluating all short-term borrowers could be costly. It is more efficient to only evaluate covenant violators, which are precisely the types of borrowers to which banks may want to reduce their exposure when their lending capacity is hit by a shock (Gertler and Gilchrist, 1994; Chodorow-Reich and Falato, 2017).

To investigate how banks set loan contract terms when facing monetary policy shocks, I use DealScan data on commercial loan contract agreements and supplement it with two measures. The first measure is loan-level covenant strictness and reflects how likely a borrower is to violate the covenants of the loan contract during the life of the loan (Murfin, 2012; Demerjian and Owens, 2016). A stricter loan contract results in more renegotiations and, hence, provides lenders with greater flexibility to change the contract terms.[3] The second measure is bank-specific monetary policy exposure and captures how sensitive a bank's lending capacity is to changes in the Fed funds rate. Following Drechsler, Savov, and Schnabl (2017), I use the average deposit market power of a bank as a proxy for its monetary policy exposure. In particular, I calculate the monetary policy exposure of a

[3]In this paper, I focus on financial covenants, which set the minimum thresholds for financial ratios that a borrower must satisfy. For instance, an interest coverage ratio covenant defines the minimum threshold for the ratio of EBITDA to interest expenses. This covenant requires the borrower to have an interest coverage ratio above this minimum threshold during the life of the loan contract. If the ratio falls below the threshold, a so called covenant violation occurs, and the control rights are transferred to the lender, indicating that the lender will have the right to change the contract terms, such as the right to reduce the loan amount. If the lender sets the minimum threshold at a higher value, the covenant is stricter.

bank by taking the weighted average of the deposit market concentrations of the counties in which the bank raises deposits.[4] The higher the deposit market power of a bank is, the larger the deposit outflow and the larger the decrease in its lending capacity will be during monetary policy contractions. Therefore, banks with a higher deposit market power have higher exposure to monetary policy.[5]

I find that banks at the 75$^{\text{th}}$ percentile of monetary policy exposure write 12 percent stricter covenants relative to banks at the 25$^{\text{th}}$ percentile. Stricter covenants lead to more covenant violations and, hence, contract renegotiations, thereby increasing the banks' ability to reduce lending when monetary policy shocks induce a contraction in their lending capacity. I find empirical evidence suggesting that banks indeed use covenant violations as an opportunity to reduce lending when needed. In particular, during times of monetary policy contractions, banks reduce lending to covenant violators but not non-violators. In contrast, during times of loose monetary policy, banks do not enforce violated covenants. These findings highlight the role of covenants in the bank lending channel of monetary policy transmission and lend support to the hypothesis that banks use covenants as an ex-ante tool to gain flexibility for future monetary policy shocks.

However, to establish causality, two alternative explanations must be excluded. The first one is the endogenous matching of firms and banks along the dimension of unobserved firm characteristics. For instance, if risky firms are matched with banks that raise deposits in concentrated deposit markets, stricter financial covenants could reflect the firms' higher credit risks. I address this concern by employing two identification strategies. First, fol-

[4]The county share of the total bank deposits is used as weights

[5]Larger deposit outflows from banks with a higher deposit market power are not inconsistent with profit-maximizing behavior by banks. According to the deposits channel proposed by Drechsler et al. 2017, banks exercise their deposit market power and pass only a fraction of the increase in the Fed funds rate onto their depositors. This results in an outflow of deposits from these banks, leading to a reduction in their lending capacity. However, the increase in the deposit spread (i.e., Fed funds rate minus deposit rate) easily offsets the deposit outflow. In other words, banks with a higher deposit market power cut their deposit supply more to maximize their deposit rent (deposit spread times deposit amount), similar to any monopolist.

lowing Khwaja and Mian (2008), I perform a within-firm estimation using firm-year fixed effects. This estimation compares the covenant strictness of two loan contracts, each originated to a given firm within a year by two different banks. This within-firm estimation holds the firm credit risk fixed. Second, I use merger-induced variation in banks' monetary policy exposure in an instrumental variable (IV) setting. Following Garmaise and Moskowitz (2006)[6], I use the sample of counties in which the deposit market concentration increases following bank mergers. By using the deposit market shares of merging banks, I obtain a simulated bank monetary policy exposure variable to instrument actual bank monetary policy exposure. Both identification strategies produce results consistent with my main findings.

The second alternative explanation that needs to be ruled out is banks' loan market power. The deposit and loan market power of a bank may be correlated. Since I use banks' deposit market power as a proxy for their monetary policy exposure, the monetary policy exposure of a bank could also reflect its loan market power. Following the literature, I consider the following three potential sources of banks' loan market power in the syndicated loan market: bank industrial specialization (Paravisini et al., 2015), informational advantage (Berger et al., 2018; Schenone, 2009), and geographical specialization. My findings remain virtually unchanged when I control for these three potential sources in separate specifications. I do so by either using more granular fixed effects and additional control variables or constructing a revised monetary policy exposure variable that excludes the variation coming from the borrower's lending market conditions.

To provide additional evidence consistent with a causal interpretation of my findings, I test several cross-sectional implications of my hypothesis. The relationship between covenant strictness and banks' monetary policy exposure is more pronounced for long-term loans originated during times of high monetary policy uncertainty. This finding is

[6]See also Scharfstein and Sunderam (2014), Favara and Giannetti (2017)

consistent with banks' incentive to use covenants as a tool to gain flexibility against future monetary policy shocks. The longer the maturity of a loan, the higher the likelihood that banks experience a contraction in their lending capacity due to a monetary policy shock before the loan contract expires. Similarly, banks' need for flexibility to adjust their loan portfolio size should be more intense when uncertainty regarding the future path of the monetary policy rate is high.

The results across loan types provide further insight into the channel driving my results. The effect for lines of credit is stronger than that for term loans. It is critical for banks to have control over the size of their outstanding lines of credit loans because these loans are mostly held on their balance sheets. In contrast, term loans are often sold to institutional investors (Drucker and Puri, 2008; Gatev and Strahan, 2009; Irani and Meisenzahl, 2017). Furthermore, from the perspective of banks, lines of credit loans generate more liquidity risk since banks do not know the exact timing of loan withdrawals by their borrowers (Gatev and Strahan, 2009; Balasubramanyan et al., 2019). Therefore, banks have a greater incentive to gain flexibility to decrease the availability of these loans to their borrowers during monetary policy contractions.

Finally, why do firms accept stricter loan contracts even if their risks remain the same? One might expect firms to switch to another bank with lower monetary policy exposure to secure a loan with less strict covenants. However, switching to a new bank is costly for financially constrained firms that have limited outside options for external funding. This gives banks monopoly power over their customers and allows them to write stricter loan contracts without losing the customers to rival banks (Fama, 1985; Diamond, 1991; Petersen and Rajan, 1994; Drucker and Puri, 2008). Consistent with this argument, I find that the effect of bank monetary policy exposure on contract strictness is stronger among small firms with no credit rating. Furthermore, among firms with multiple lending relationships in a given year, banks with higher monetary policy exposure still write stricter covenants but charge lower loan spreads. This finding suggests that high-exposure banks are willing to leave money on the table in exchange for the flexibility provided by stricter covenants.[7]

This paper contributes to four strands of literature. The first contribution is to the extensive literature on the bank lending channel. The existing papers in this literature focus on banks' ex-post lending behavior, i.e., their behavior after a change in the Fed funds rate (Blinder and Stiglitz, 1983; Bernanke and Blinder, 1988, 1992; Kashyap and Stein, 1995, 2000; Kishan and Opiela, 2000; Campello, 2002; Peek et al., 2003; Peek and Rosengren, 2010; Drechsler et al., 2017; English et al., 2018; Temesvary et al., 2018; Bräuning and Ivashina, 2019). Instead, in this paper, I study the ex-ante behavior of banks that are exposed to future monetary policy shocks and show how this ex-ante behavior helps them respond to these shocks in the future.

The findings of this paper also link the literature on the bank lending channel to the literature on the simultaneous choice of loan contract terms (Billett et al., 2007; Bradley and Roberts, 2015; Green, 2018). Specifically, my findings emphasize the importance of considering all contract terms to better understand the transmission of monetary policy. Even if firms obtain long-term loans to avoid refinancing risk (Harford et al., 2014; Auh and Landoni, 2016), banks can use stricter covenants to shorten the "effective" maturity of the loans they originate. This gives banks the flexibility to reduce their lending before the maturity date initially set in the loan contracts, suggesting that firms with long-term loans are still exposed to credit supply shocks induced by monetary policy.

This paper is also related to Chodorow-Reich and Falato (2017) and Ippolito et al. (2019). They study the importance of covenant violations in the transmission of bank health to non-financial firms during the recent financial crisis. They document that financially unhealthy banks use covenant violations to decrease their lending to firms. Two

[7] This result on loan spreads further rules out banks' loan market power as an explanation for my findings. If the driving force behind stricter loan contracts was banks' loan market power, we should not expect to find a negative effect on loan spreads.

points are noteworthy. First, their findings show that loan covenants are useful tools for banks to adjust their lending amount when necessary. My results complement theirs in that, given the usefulness of covenants, banks not only respond to covenant violations but also endogenously choose the probability of such violations by setting the strictness of covenants. Second, in Chodorow-Reich and Falato (2017) and Ippolito et al. (2019), the source of variation in banks' lending behavior after a covenant violation is their financial health. However, in this paper, the driving force is banks' exposure to monetary policy. Banks that collect deposits in concentrated markets behave as a monopolist and optimally choose to reduce their deposit supply when the Fed funds rate rises. These banks have good financial health but experience a contraction in their lending capacity due to their optimal behavior in the deposit market. I show that these banks write stricter covenants and gain flexibility to optimally adjust the size of their lending portfolio during monetary policy contractions.

Finally, I contribute to the literature studying the determinants of loan covenant strictness. The existing papers mainly focus on how borrower characteristics determine covenant strictness (Smith and Warner, 1979; Billett et al., 2007; Rauh and Sufi, 2010; Demiroglu and James, 2010). Murfin (2012) differs in that he studies the determinants of financial covenant strictness from a lender perspective. However, the mechanism underlying his results is still the performance of borrowers in the lender's loan portfolio. That is, banks begin writing stricter loan contracts as a result of a change in their perception of borrower risk.[8] My paper contributes to this borrower-centric literature by proposing a supply-side determinant of contract strictness. Independent of the riskiness of borrowers, banks set the strictness of covenants based on how sensitive their lending capacity is to changes in monetary policy.

[8]In particular, he documents that banks write stricter loan contracts when they update their perception of their own screening ability based on higher defaults of the borrowers in their loan portfolio.

In summary, I show that banks use loan covenants as an ex-ante tool to prepare for future monetary policy shocks. This ex-ante behavior helps banks reduce lending during monetary policy contractions. The findings highlight the role of covenants in the bank lending channel of monetary policy transmission and indicate that firms are exposed to policy shocks, even when they have long-term loans outstanding. Given how important access to bank lending is for firms, especially for small and opaque ones, the findings have potential implications for firms' liquidity management and investment policies.

The remainder of this paper is organized as follows. Section 3.2 describes the data sources, variables constructed, and relevant institutional details, along with the summary statistics. Section 3.3 provides the main results on the ex ante loan contracting behavior of banks based on their monetary policy exposure and supporting evidence from the ex post lending response of banks to monetary policy changes. Section 3.4 discusses two alternative explanations for my findings: banks' loan market power and endogenous bank-firm matching. Section 3.5 tests a number of cross-sectional implications to support the causal interpretation of the results. Section 3.6 concludes the paper.

3.2 Data and Institutional Background

The analysis in this paper relies on several data sources that cover the period from 1995 to 2013. In this section, I first provide information on the main data sources and the variables constructed with the necessary institutional details. Second, I present the summary statistics for the variables.

3.2.1 Data Sources and Variables

Loan-level data: The information on loan contracts is from DealScan database provided by Loan Pricing Corporation (LPC). LPC collects the information from SEC filings and lead lenders and makes the data available on Wharton Research Data Services (WRDS). The dataset provides the name and location of the borrowing firm and the lenders for each loan, which I use to merge the DealScan loan data with other datasets. The dataset also provides detailed information on loan contract terms: loan amount, purpose, type, origination date, maturity, and – most important to my analysis – the types of financial covenants and their thresholds.

There are mainly two types of covenants in loan contract agreements: non-financial and financial covenants. Non-financial covenants determine the actions that a borrower cannot take, such as the sale of its assets. Compliance with these covenants is under borrower control, and violations of them are rare. In this paper, I focus on financial covenants, which set the minimum or maximum thresholds for financial ratios that the borrower must satisfy. For instance, an interest coverage ratio covenant defines the minimum threshold for the ratio of EBITDA to interest expenses. The violation of this financial covenant by the borrower (i.e., the borrower's actual financial ratio falling below the threshold set in the contract) transfers the control rights to the lender, which gives the lender the right to change the contract terms, for instance, the right to reduce the loan amount. Note that compliance with financial covenants is not directly under borrower control. Changes in economic conditions may trigger a financial covenant violation.

The literature mainly uses two types of measures to capture the strictness of a loan contract based on the financial covenants included in the contract. The first is based on the number of financial covenants. The measure can be either a count measure (i.e., the total number of covenants in a loan contract) or an index ranging on a given interval (e.g., the covenant intensity index proposed by Bradley and Roberts (2015)). Although the number of covenants is an important aspect of evaluating the strictness of a loan contract, these measures are not capable of reflecting the covenant slackness and the covariance between different financial ratios on which the covenants are based. To mitigate these concerns, Murfin (2012) develops a new covenant strictness measure that accounts for the number of covenants in a loan contract, the slackness of these covenants, and the covariation of the borrower financial ratios on which these covenants are written. By using the information on the thresholds of financial covenants from DealScan and the information on the actual level of borrower financial ratios at loan origination from Compustat, he calculates a loan-level measure of contract strictness that ranges from 0 to 1.[9] This measure captures "the ex ante probability of covenant violation" at loan origination. The closer the measure is to 1, the stricter the loan contract and the higher the probability that the borrowing firm will violate a financial covenant throughout the life of the loan contract. Stricter loan contracts therefore provide banks with greater flexibility to change the contract terms once the loan is originated. I use this contract strictness measure as my main dependent variable throughout the paper.

Branch-level deposit data: The Federal Deposit Insurance Corporation (FDIC) issues the Summary of Deposits (SOD) survey, which provides information on the annual deposit holdings of U.S. bank branches. I use the location and the parent bank of these branches to merge deposit data with other datasets.

Using SOD data, I follow Drechsler et al. (2017) and calculate measures of deposit market concentration at two different levels: the county-year level and bank-year level. To compute the first, I sum the squared deposit market share of each bank that raises deposits in a county in a year. I refer to this county-year level variable as the county Herfindahl-Hirschman Index (county HHI). This variable is the county deposit market concentration and reflects how competitive the county deposit market is. Drechsler et al. (2017) show that county HHI captures the sensitivity of a bank's total deposits in that county to the changes in the Fed funds rate.[10] However, this variable does not reflect the overall deposit

[9]With the same intuition, but with a slightly different non-parametric methodology, Demerjian and Owens (2016) calculate loan-level contract strictness and make it available on their website.

[10]See Section 3.3 for the discussion of why this is the case under the deposits channel framework of Drechsler et al. (2017).

funding conditions of a given bank that raises deposits in the county because most banks in the U.S. collect deposits in more than one county and can reallocate deposits internally from one location to another to exploit lending opportunities. Therefore, to capture the deposit funding conditions of a given bank, I take the weighted average of the deposit market concentration of the counties in which the bank raises deposits, using the deposits of the bank in those counties as weights. Drechsler et al. (2017) show that this variable captures how sensitive a bank's total deposits, and hence its loan supply, is to the changes in the Fed funds rate. This bank-year level variable is the main variable of interest and is referred to as bank monetary policy exposure (bank MPE) throughout the paper.

Bank-level data: The bank-level data are from U.S. Consolidated Reports of Condition and Income filings (Call Report), submitted by all banks regulated by the Federal Reserve System, the FDIC, and the OCC. The data are available on WRDS.

I use bank balance sheets and income statements to construct a set of control variables. The bank equity ratio (bank equity normalized by bank assets) and bank size (log of bank assets) are two widely used variables in the literature on bank lending behavior. To further capture the differences among banks that may impact banks' lending practices, I also control for the following variables: the share of liquid assets (cash + securities) in total assets, the share of loans in total assets, the share of deposits in liabilities, the share of wholesale funding in liabilities, the average bank deposit rate, and bank profitability.

I aggregate bank-level data at the bank holding company level by using all subsidiary banks of a bank holding company. Both the banking industry practices and the data matching process necessitate the use of bank holding company-level data. First, the internal capital markets within a bank holding company imply that performing a bank holding company-level analysis is more consistent with the findings of the banking literature (Houston et al., 1997). Second, the information provided for lenders of loans in DealScan is more complete for the ultimate owner of the lender. Specifically, although some loan observations provide information on the immediate lender, which allows me to know both the subsidiary bank and its parent bank holding company, some loan observations report only the ultimate lender, in which case, I do not know the immediate lender (i.e., the subsidiary bank of the bank holding company).

Borrower-level data: Borrower characteristics and annual accounting information come from Compustat, which is available from the WRDS. Several borrower-level variables are used to control for the demand side of the loan market and for borrower quality. Among the variables used as borrower controls are borrower size (log of total assets), Tobin's q, the share of current assets in total assets, the share of tangible assets in total assets, leverage, fixed coverage ratio, and Altman's z-score. These variables are used widely in the literature and by banks to analyze a borrower before writing a loan contract. Furthermore, two credit rating dummies are also controlled for: one for whether a borrower has an S&P credit rating and one for whether the borrower has an investment-grade rating.

Macro data: The Fed funds target rate from Federal Reserve Economic Data (FRED) is used to measure the Fed's monetary policy stance. To measure monetary policy uncertainty in the U.S. economy, I use three different proxies. The first measure is the implied interest rate volatility from Cremers et al. (2017). Using the Treasury derivatives market, these authors calculate a five-year forward-looking implied interest rate volatility. Compared with historical interest rate volatility, this measure has the advantage of being forward-looking. Because I test whether a bank sets the strictness of a loan contract to affect the probability of future renegotiation of the contract depending on its exposure to monetary policy, the forward-looking measure of interest rate volatility better serves this purpose. The second measure is a newspaper-based index, constructed as a scaled frequency count of newspaper articles that discuss monetary policy uncertainty (Baker et al., 2016; Husted et al., 2017). As a final measure of monetary policy uncertainty, I use monetary policy shocks after FOMC meetings, defined in Gorodnichenko and Weber (2016), and take the average of the sizes of these shocks during the last twelve-month period. The larger the average size of the shocks is, the greater the monetary policy uncertainty in the market.

Other: Covenant violation data come from two different sources. First, based on a text-search algorithm, Nini et al. (2012) provide borrower-quarter-level information on whether a borrower is in violation of a loan contract covenant in a given quarter. Second, following Chava and Roberts (2008) and Chava et al. (2017), I determine whether a loan contract covenant is violated by comparing the actual borrower financial ratio to the financial ratio threshold set in the loan contract.

The list of bank mergers is given by the Federal Reserve Bank of Chicago. Following the literature, I use only the mergers that do not involve FDIC assistance and that have a significant impact on county deposit market concentration, i.e., the banks involved in a merger constitute 10% or more of the total county deposits.

3.2.2 Summary Statistics

The main sample that I use in my analysis consists of 7,765 loans with non-missing loan, borrower, and bank characteristics. I include only U.S. dollar-denominated loans to non-financial borrowers with a single lead lender in the loan syndicate. Figure 3.1 presents the time-series and cross-sectional variation of contract strictness. The average contract strictness measure reaches its highest value circa 2000 and then decreases until the financial crisis. Its mean value in the sample is 0.34. The measure also shows high cross-sectional variation throughout the sample period, with a standard deviation of 0.40.

The summary statistics for the remaining loan contract terms and borrower and bank characteristics are reported in three separate panels in Table 3.1: loan characteristics at the loan level in Panel A, borrower characteristics at the borrower-year level in Panel B, and bank characteristics at the bank-year level in Panel C. Because the main sample is obtained by matching several datasets, it is important to understand how the matching process affects the sample construction and what the resulting implications for my analysis. For this purpose, in each panel, I present the summary statistics for two different samples: one for a broader sample from the relevant dataset before the matching process (columns 1-2) and one for the main sample after matching (columns 3-7).

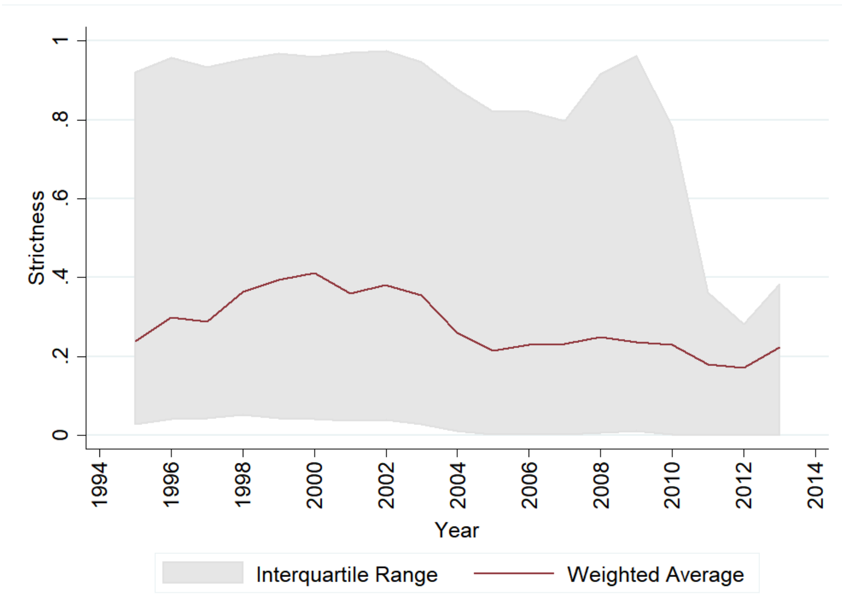

Figure 3.1: **Average Loan Strictness**

Panel A presents the summary statistics for loan characteristics. The average loan in the broader DealScan universe has a size of $291 million. Relative to this broader sample, the sample loans are larger in size and are originated to less risky borrowers (loans feature lower loan spreads and are less secured). Of the loans in the sample, 86 percent are lines of credit, also referred to as *revolving credit facilities*. Borrowers are able to draw funds against these credit facilities as long as they satisfy the conditions set in loan contracts, i.e., as long as they do not violate loan covenants. The average loan spread over LIBOR charged to borrowers is 167 basis points.

Panel B reports borrower characteristics for the sample at the borrower-year level. Consistent with Panel A, the matching process creates a sample containing borrowers that are

Table 3.1: **Summary Statistics**

Panel A: Loan Characteristics	DealScan Loans		Sample Loans				
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) 25^{th} perc.	(6) Median	(7) 75^{th} perc.
Line of credit (%)	79.90	40.07	86.49	34.18	100	100	100
Amount (mill. $)	291.33	323.90	385.78	389.46	80.00	230.00	575.00
Maturity (years)	4.22	1.92	3.84	1.55	3.00	4.84	5.00
Spread (bps)	249.52	121.36	167.29	91.58	88.00	150.00	250.00
Secured (%)	78.65	40.97	60.95	48.79	0.00	100.00	100.00
# of synd. members	5.44	4.72	8.69	6.59	3.00	7.00	13.00
Strictness	-	-	0.34	0.40	0.01	0.10	0.83
Obs. (Loan)	44,244		7,765				

Panel B: Firm Characteristics	Compustat Firms		Sample Firms				
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) 25^{th} perc.	(6) Median	(7) 75^{th} perc.
Size (bill. $)	0.94	1.54	1.87	2.47	0.19	0.70	2.36
Tobin's q	1.97	1.26	1.55	0.59	1.07	1.36	1.90
Leverage (%)	17.00	16.42	22.40	16.31	7.71	21.68	34.75
Tangibility (%)	30.09	23.90	31.70	21.92	12.81	25.55	48.15
Cash/Assets (%)	14.73	15.78	7.91	8.16	1.40	4.39	12.42
Altman's z	2.30	3.53	3.24	1.92	1.67	2.90	4.53
Fixed coverage	2.84	7.10	5.51	6.05	1.31	2.95	7.09
Have S&P Rating	0.22	0.41	0.41	0.49	0.00	0.00	1.00
Obs. (Firm x Year)	113,185		32,454				

Panel C: Bank Characteristics	Call Report Banks		Sample Banks				
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) 25^{th} perc.	(6) Median	(7) 75^{th} perc.
MPE, Bank	0.22	0.13	0.18	0.07	0.14	0.18	0.21
Size (bill. $)	0.17	0.17	55.08	67.00	6.94	21.54	78.95
Liquidity/Assets (%)	30.82	12.44	26.53	9.13	19.44	24.62	32.16
Loans/Assets (%)	61.55	12.46	62.55	10.97	56.66	65.25	70.84
Deposits/Liab. (%)	95.08	4.86	74.67	14.19	65.91	76.37	86.53
Wholesale/Liab. (%)	3.96	4.70	16.03	7.57	9.53	16.14	22.25
Equity/Assets (%)	10.26	2.36	8.87	1.65	7.48	8.49	10.04
Obs. (Bank x Year)	126,841		1,163				

larger than those in the universe of Compustat firms. Therefore, not surprisingly, the sample borrowers have higher leverage and hold less cash, and more of these borrowers have S&P credit ratings. However, the sample still features variation among borrowers. For instance, only 41 percent of borrowers in the sample have S&P ratings, implying that borrowers have different levels of bank-dependence/financial constraints.

Panel C presents the bank-year level summary statistics for banks. The average bank in the broader Call Report dataset has a size of $170 million and relies heavily on deposits for funding; the percentage of deposits in total liabilities is 95 percent. The main variable of interest, bank MPE, has an average value of 0.22 and shows variation comparable to other bank characteristics, with a standard deviation of 0.13. Consistent with the previous two panels, the banks in the main sample are larger ($55 billion vs. $170 million), use less deposits as a funding source (75% vs. 95%), and raise deposits in less concentrated deposit markets (bank MPE of 0.18 vs. 0.22) than those in the Call Report sample. This is mainly because the probability of matching large DealScan lenders with Call Report banks is higher as the information on those banks is more complete, which is consistent with the samples used in other studies that apply similar matching procedures (Schwert, 2018). Although the sample banks rely less on deposits and more on wholesale funding relative to a typical bank in the U.S., the fraction of deposits in their total liabilities is still sizable and constitutes the largest portion, and the loss of deposits in response to an increase in the Fed funds rate results in a strong loan contraction for these banks (Drechsler et al., 2017).

3.3 Baseline Specification and Main Results

In this section, I first provide a brief description of the underlying mechanism behind the deposits channel proposed by Drechsler, Savov, and Schnabl (2017) and define the monetary policy exposure of a bank in this framework. Then, by using my baseline specification, I test the main prediction of this paper: banks with higher monetary policy exposure write stricter loan covenants. Finally, I provide evidence on the ex post lending behavior of banks to support this prediction, namely, how stricter loan covenants help banks decrease their lending following an increase in the Fed funds rate.

Drechsler et al. (2017) present a model in which the deposit market power of banks drives changes in deposits and lending in response to monetary policy changes. These authors refer to this mechanism as the *deposits channel* and empirically show that "the deposits channel can account for the entire transmission of monetary policy through bank balance sheets". By using a structural estimation, Wang et al. (2018) and Xiao (2019) also document findings consistent with the deposits channel of monetary policy. Specifically, Drechsler et al. (2017) provide a model in which households hold cash or deposits to obtain liquidity. The opportunity cost of holding cash is the Fed funds rate, while that of holding deposits is the *deposit spread*, which is defined by the Fed funds rate minus the bank deposits rate. They show that banks use their market power to increase the deposit spreads that they charge their depositors in response to monetary policy tightening (i.e., by keeping their deposit rates low following Fed funds rate increases). This increase is possible because the opportunity cost of holding cash, the other alternative instrument for liquidity, increases; hence, banks do not lose deposits to cash. However, households respond by switching from holding liquid assets to holding illiquid assets, i.e., bonds, which leads to deposit outflows from banks. Because deposits are a large and stable source of funding, the banks that experience deposit outflows reduce their loan supply. They show that banks in more concentrated deposit markets (i.e., banks with more deposit market power) increase their deposit spread more and experience larger deposit outflows. Therefore, these banks have greater exposure to monetary policy changes and reduce their commercial lending by more in response to monetary policy tightening.

3.3.1 Ex Ante Loan Contracting

Relying on the findings of Drechsler et al. (2017), the main question of this paper is whether banks take ex ante actions based on their exposure to monetary policy. In particular, do banks with higher monetary policy exposure include stricter financial covenants in their loan contracts? They may have an incentive to do so because stricter financial covenants may increase the probability of covenant violation and hence give these banks the necessary contractual rights following a covenant violation to reduce the loan amount if monetary policy tightens ex post. In other words, stricter financial covenants may provide banks with more bargaining power during the life of the loan contracts that they originate and thus the flexibility to reduce their lending amount in response to an increase in the Fed funds rate.

To test this prediction, I use the information on DealScan loan contracts and show that banks with greater exposure to monetary policy write stricter loan contracts relative to banks with less exposure. Specifically, I begin the sample construction with loan-level strictness data (Demerjian and Owens, 2016). Then, by using unique loan ID, I complement the strictness data with other loan contract terms and the identity of the lead lender and borrower provided in the DealScan data. I manually match lead lenders in DealScan with commercial banks in the SOD and Call Reports based on their names and locations, which enables me to obtain my main variable of interest – bank-year-level monetary policy exposure – and to use bank balance sheet information to control for lender characteristics that may affect loan outcomes. Finally, to better control for borrower characteristics, I obtain Compustat data by using the borrower link file constructed by Chava and Roberts (2008).

My baseline specification controls for observable borrower, bank, and loan characteristics, unobservable time-invariant borrower and bank characteristics and, finally, economywide macroeconomic shocks. Formally, I estimate the following loan-level regression model:

$$Strict_{\ell,f,b,t} = \beta MPE_{b,t-1} + \alpha_f + \eta_b + \delta_t + \Gamma \mathbf{B}_{b,t-1} + \Lambda \mathbf{F}_{f,t-1} + \Theta \mathbf{L}_{\ell,f,b,t} + \varepsilon_{\ell,f,b,t} \qquad (3.1)$$

where $Strict_{\ell,f,b,t}$ is the covenant strictness of loan contract l originated by bank b to borrower f during year t; $MPE_{b,t-1}$ is the bank-level lagged monetary policy exposure of bank b; α_f, η_b, and δ_t are borrower, bank, and year fixed effects, respectively; and $\mathbf{B}_{b,t-1}$, $\mathbf{F}_{f,t-1}$, and $\mathbf{L}_{\ell,f,b,t}$ are bank, borrower, and loan control variables, respectively. The coefficient of interest is β, which is expected to take a positive sign. For ease of interpretation, all variables are standardized, i.e., β shows the impact of a one-standard-deviation increase in bank MPE on covenant strictness. Because our variable of interest, $MPE_{b,t-1}$, is constant across loans originated by a bank in a given year and because the loan contracting practices of different banks may be correlated across the loans they originate to a given borrower, the standard errors are two-way clustered at the bank and borrower levels for each regression.

Table 3.2 presents the results. Columns (1) through (5) use my main sample with 7,765 loan observations. The specification in column (1) has only year fixed effects, with no bank or firm controls other than the S&P credit rating dummies for firms, and hence, it uses cross-sectional variation in bank MPE. Other loan contract terms are also excluded due to the 'bad control' problem, as they may be determined simultaneously with the covenant strictness. The coefficient on bank MPE is positive and significant. The economic meaning of this coefficient is that a one-standard-deviation increase in the monetary policy exposure of a bank increases loan contract strictness by 12 percent at the mean value of the strictness measure.[11]

In column (2), I include firm and bank fixed effects to control for time-invariant differences among firms and banks. Both the size and statistical significance of the coefficient

[11] $0.04/0.34 = 12\%$

Table 3.2: **Monetary Policy Exposure of Banks and Covenant Strictness: Baseline Specification**

	Main Sample					Additional Controls	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MPE, Bank	0.04**	0.06***	0.06***	0.07***	0.07***	0.07***	0.07***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Size			-0.05**	-0.04**	-0.06***	-0.06***	-0.05**
			(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Tobin's q			-0.06***	-0.06***	-0.06***	-0.06***	-0.01
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Leverage			0.07***	0.07***	0.07***	0.06***	0.04***
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Current ratio							-0.03***
							(0.01)
Tangibility							-0.03
							(0.02)
Altman's z							-0.07***
							(0.02)
Fixed coverage ratio							-0.01***
							(0.00)
Controls & FEs:							
Rating Dummies	✓	✓	✓	✓	✓	✓	✓
Bank Controls				✓	✓	✓	✓
Loan Controls					✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE		✓	✓	✓	✓	✓	✓
Bank FE		✓	✓	✓	✓	✓	✓
Obs.	7,765	7,765	7,765	7,765	7,765	7,186	7,186
R^2(Adj.)	0.108	0.412	0.432	0.432	0.434	0.439	0.449

increase. In column (3), I saturate the model with firm controls: firm size, Tobin's q, and leverage. In column (4), I also include bank control variables that are used in the bank lending literature: bank size, equity ratio, liquidity ratio, profitability, bank average deposit rate, the share of loans in total assets, the share of deposits in liabilities, and the share of wholesale funding in liabilities. The specification in column (5) further controls for loan characteristics: loan purpose, loan type, loan size, and loan maturity. The magnitude of the coefficients in columns (3) through (5) is similar to that in column (2). In the last two columns of the table, I restrict the sample by excluding loan observations with missing information on additional firm controls: the current ratio, tangible asset ratio, Altman's z-score, and fixed coverage ratio. For this restricted sample, I first present the result of the regression without including these additional controls. Then, I add these controls. The coefficient on the main variable of interest does not change in either case.

There are two points to emphasize regarding the results in Table 3.2. First, the coefficients of the borrower-level variables all have the predicted signs (Demiroglu and James, 2010) and are statistically significant except for tangibility. Large borrowers that have low leverage with better investment opportunities obtain loan contracts with less strict covenants. Furthermore, as the share of a borrower's liquid assets in its balance sheet increases, as the borrower's income increases relative to its interest expenses, and as the borrower becomes less likely to go bankrupt, it obtain loans with less strict covenants. Second, while including firm and bank fixed effects increases the size of the main coefficient, including time-varying firm and bank controls does not have a meaningful impact on its size, which suggests that the nature of any potential omitted firm characteristic that may be correlated with contract strictness and bank MPE is time invariant, which is consistent with the finding in the literature that bank-firm relationships are sticky due to the cost of switching across banks. Thus, once the time-invariant firm and bank characteristics are controlled for, the monetary policy exposure of the bank with which a borrower works is orthogonal to borrower characteristics and hence mitigates the concern about endogenous matching of borrowers and banks along the dimensions of borrower credit risk. Section 3.4.2 more formally addresses the concerns on omitted time-varying firm characteristics.

The results in the table are robust to including loans with multiple lead lenders in the syndicate (Table B1), restricting the sample to the period before the financial crisis (Table B2), and using an alternative measure for banks' monetary policy exposure (Table B3).

The other alternative to gain flexibility for a potential monetary policy change in the future would be to originate short-term loans instead of long-term loans. Instead of writing loan contracts with strict covenants to increase the probability of contract renegotiation, banks may gain the same flexibility for future contingencies by writing short-term loan contracts. Using the same regression specification as in Equation (3.1) but with loan maturity as the dependent variable, I find no evidence for this (Table 3.3), which is is partly and possibly a result of the specific nature of the loan market that I study. The majority of loans in DealScan data have long maturities and show little variation[12] (Almeida et al., 2011; Chodorow-Reich and Falato, 2017). Within this class of loan contracts, financial covenants grant banks the necessary contractual right to gain flexibility for future contingencies. Furthermore, there are two additional points worth noting. First, the maturity choices of firms are to a large extent dictated by their operational needs and balance sheet structure (Graham and Harvey, 2001). Therefore, they may have a strong preference against having short-term loans and may not show flexibility on maturity dimension. Second, loan underwriting is a costly process for both borrowers and banks, especially in the syndicated loan market. Banks may, therefore, prefer long-maturity loans combined with strict covenants to short-maturity loans due to these cost concerns. Instead of evaluating borrowers with short-maturity loans on a frequent basis to make a lending decision, it is

[12] In my main sample, loan maturity has a median value of 4.8 years and an interquartile range of 3-5 years.

more cost-efficient to evaluate only the covenant violators. This is because violators are exactly the types of borrowers that banks may want to reduce their lending when their lending capacity is hit by a shock (Gertler and Gilchrist, 1994; Chodorow-Reich and Falato, 2017). Overall, the cost of writing loan contracts with 'short maturity/strict covenants' and renewing the loans each time 'at expiration/upon covenant violation' should be weighed against the benefit of having flexibility for future monetary policy contingencies.

Table 3.3: **Monetary Policy Exposure of Banks and Loan Maturity**

	Main Sample					Additional Controls	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MPE, Bank	0.04	0.01	0.01	0.01	0.03	0.04	0.03
	(0.06)	(0.06)	(0.06)	(0.06)	(0.04)	(0.04)	(0.04)
Size			0.00	0.01	0.18***	0.18***	0.17***
			(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Tobin's q			-0.02	-0.01	-0.03**	-0.02	-0.06***
			(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Leverage			0.00	0.00	0.00	-0.00	0.02
			(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Current ratio							-0.01
							(0.01)
Tangibility							0.01
							(0.03)
Altman's z							0.08***
							(0.03)
Fixed coverage ratio							-0.01
							(0.01)
Controls & FEs:							
Rating Dummies	✓	✓	✓	✓	✓	✓	✓
Bank Controls				✓	✓	✓	✓
Loan Controls					✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE		✓	✓	✓	✓	✓	✓
Bank FE		✓	✓	✓	✓	✓	✓
Obs.	7,765	7,765	7,765	7,765	7,765	7,186	7,186
R^2(Adj.)	0.141	0.306	0.306	0.307	0.470	0.474	0.476

3.3.2 Ex Post Lending Response

In Section 3.3.1, I document that banks with higher monetary policy exposure include stricter financial covenants in their loan contracts. To interpret this as banks gaining flexibility to reduce their lending amount following an increase in the Fed funds rate, the relevant question in this subsection is whether banks with higher monetary policy exposure indeed use stricter financial covenants as a tool to decrease their loan supply following an increase in the Fed funds rate. In other words, do ex ante loan contracting practices (i.e., writing stricter financial covenants) result in a consistent ex post bank lending response (i.e., decreasing loan supply following monetary policy tightening)? To show that this is indeed the case, I test the following two questions. First, do stricter loan covenants result in more covenant violations? Second, following an increase in the Fed funds rate, do banks with higher monetary policy exposure decrease their lending by more following a covenant violation? Each question is answered in the affirmative.

The first test is in fact a validation of the predictive power of loan contract strictness for actual covenant violations. Because covenant violations transfer control rights to lenders, this test answers the question of whether writing stricter loan contracts grants lenders more contractual control power over their borrowers to change loan contract terms. This finding is important in the sense that it reveals the relevance of contract strictness as a decision variable for banks and mitigates the concern that borrowers may accept stricter covenants if they anticipate that they will not violate the covenants and believe that stricter covenants are not binding. I estimate the following loan-level regression model for the test:

$$Violation_{\ell,f,b,t} = \beta Strictness_{\ell,f,b,t} + \delta_t + \psi_{ind} + \Lambda \mathbf{F}_{f,t-1} + \Theta \mathbf{L}_{\ell,f,b,t} + \varepsilon_{\ell,f,b,t} \tag{3.2}$$

where $Violation_{\ell,f,b,t}$ is a dummy variable that takes value 1 if a covenant of loan contract l originated by bank b to borrower f during year t is violated at any time during the life of the loan contract; $Strictness_{\ell,f,b,t}$ is the strictness of the same loan contract at origination; δ_t and ψ_{ind} are year and industry fixed effects, respectively; and $\mathbf{F}_{f,t-1}$ and $\mathbf{L}_{\ell,f,b,t}$ are borrower and loan control variables, respectively. The coefficient of interest is β, which is expected to take a positive sign.

Because DealScan provides information on loan contracts only at origination, I follow two different strategies to determine whether a loan covenant is violated during the life of the loan contract and show that the results are robust. First, I use the covenant violation data from Nini et al. (2012), who employ a text-search algorithm to SEC filings to determine whether a borrower is in violation of a loan covenant in a quarter. This algorithm does not specify which specific loan contract is violated. Therefore, as a proxy, I assume that any loan contract that is effective during the violation quarter is violated. For instance, if a borrower is in violation of a covenant in the last quarter of 2001 based on SEC filings, I examine the outstanding loan contracts in that quarter and assume that their covenants are violated. Second, following Chava and Roberts (2008) and Chava et al. (2017), I use the accounting information from Compustat and covenant threshold information from DealScan to determine whether a borrower's actual financial ratio is above or below the covenant threshold set in the loan contract.[13] For instance, if the actual current ratio of a borrower falls below the current ratio threshold set in the loan contract, this represents a covenant violation. Table 3.4 presents the results. OLS and probit regressions with different controls across two different datasets provide consistent results and imply that stricter loan contracts result in more violations[14]. The economic meaning of the coefficient is that a one-standard-deviation increase in covenant strictness increases covenant violation

[13] I am grateful to Sudheer Chava, Vikram Nanda, and Steven Chong Xiao for sharing the code to implement this strategy.

[14] To check if the predictive power of contract strictness for actual covenant violation does not differ across high-exposure and low-exposure banks, I split the sample into two based on banks' monetary policy expsoure. The results are similar across the two sub-samples (Table B4).

by 7%, which represents a 22% increase in violation probability at its mean value[15].

Table 3.4: **Covenant Strictness and Covenant Violation**

	Nini et al. (2012)			Chava et al. (2017)		
	(1) OLS	(2) OLS	(3) Probit	(4) OLS	(5) OLS	(6) Probit
Strictness	0.10***	0.07***	0.07***	0.07***	0.06***	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Size		-0.09***	-0.11***		-0.09***	-0.10***
		(0.01)	(0.01)		(0.02)	(0.02)
Tobin's q		-0.02**	-0.02***		0.02	0.02
		(0.01)	(0.01)		(0.01)	(0.01)
Leverage		0.01	0.01		0.01	0.01
		(0.01)	(0.01)		(0.01)	(0.01)
Current ratio		-0.00	0.00		0.00	-0.00
		(0.01)	(0.01)		(0.01)	(0.01)
Tangibility		-0.01	-0.01		0.01	0.01
		(0.01)	(0.01)		(0.01)	(0.01)
Altman's z		-0.02**	-0.02**		-0.06***	-0.06***
		(0.01)	(0.01)		(0.02)	(0.02)
Fixed coverage ratio		-0.00	-0.00		0.01	0.01
		(0.01)	(0.01)		(0.01)	(0.01)
Loan maturity		0.02***	0.02***		0.02***	0.02***
		(0.00)	(0.00)		(0.00)	(0.00)
Loan amount		-0.02*	-0.01		0.05***	0.06***
		(0.01)	(0.01)		(0.02)	(0.02)
No. of Lead Lenders		-0.01	-0.01		-0.01	-0.01
		(0.01)	(0.01)		(0.01)	(0.01)
Controls & FEs:						
Year FE	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓
Obs.	8,928	8,926	8,926	2,426	2,426	2,426
R^2(Adj.)	0.099	0.146	0.146	0.048	0.073	0.073

I provide the results of the second test in two steps. First, without any reference to covenant violations, I validate whether the bank-level monetary policy exposure variable measures what it is supposed to measure: do banks that operate in concentrated deposit markets decrease their lending more following an increase in the Fed funds rate? This is in fact the verification of the deposits channel defined by Drechsler et al. (2017). To

[15] 0.07/0.32=22%

conduct this analysis, I use the annual total outstanding loan amount between a borrower and its lender as the dependent variable. Unlike credit registry data, however, DealScan reports flow data and provides information on loans only at their origination; hence, I do not directly observe the outstanding loan amount between a borrower and its lender over time. I follow the literature (Lin and Paravisini, 2013; Gomez et al., 2016; Chakraborty et al., 2018) and construct the annual outstanding loan amount between a borrower and its lender from DealScan by using the information on loan origination date, termination date, and loan amount. Furthermore, I update the termination date of a loan contract if there is a new refinancing loan contract of the same type before the expiration date of the former (Chava and Roberts, 2008; Chava et al., 2017). I supplement this annual borrower-bank-level outstanding loan data with the annual bank-level MPE variable. I add to these data the bank balance sheet information to control for lender characteristics that may affect loan outcomes. Finally, I merge the data with the annual Fed funds rate. With these data, I follow Khwaja and Mian (2008) and implement a within-borrower estimation. In particular, I use borrower-year fixed effects and compare the outstanding loan amounts in a given year between a borrower and its lenders with different levels of monetary policy exposure.

To conduct this within-borrower estimation, I use only the sample of borrowers that work with at least two banks in a given year and exclude all others because the coefficient is not identified for single-bank borrowers. By assuming that borrower loan demand is symmetric across different banks, this empirical strategy allows for time-varying differences among borrowers and holds loan demand fixed, hence allowing me to uncover the effect of bank MPE on loan supply.[16] Formally, I estimate the following regression

[16]To test the prediction of the deposits channel on commercial lending, Drechsler et al. (2017) use annual bank-county-level data from the Community Reinvestment Act (CRA). To keep loan demand constant, they include county-year fixed effects in their regressions and hence compare the commercial lending of different banks within a given county. To the extent that borrower clientele is homogeneous across banks, this strategy controls for loan demand. However, if changes in monetary policy asymmetrically affect the loan demand

model[17]:

$$log(loan)_{f,b,t} = \beta(MPE_{b,t-1} \times \Delta ff_t) + \mu BankControls_{b,t-1} + \theta_{f,t} + \eta_b + \varepsilon_{f,b,t} \quad (3.3)$$

where the dependent variable is the log of the outstanding loan amount between bank b and borrower f in year t, $MPE_{b,t-1}$ is the bank-level monetary policy exposure of bank b in year $t-1$, Δff_t is the change in the Fed funds rate from year $t-1$ to t, $\theta_{f,t}$ are borrower-year fixed effects for borrower f, and η_b are bank fixed effects for bank b. Across different specifications, I also control for bank size, equity ratio, liquidity ratio, profitability, average deposit rate, share of loans in total assets, and share of deposits and wholesale funding in total liabilities. The coefficient of interest is β, which is expected to take a negative sign. I double cluster standard errors at the bank and borrower levels.

Table 3.5 presents the results. Each specification in the table uses the within-borrower sample: the sample of borrowers that have outstanding loans from at least two banks in a given year. For comparison, in columns (1) and (2), I first run the specification without borrower-year fixed effects. Column (1) shows a negative and significant coefficient for the interaction term, meaning that, in response to an increase in the Fed funds rate, banks in more concentrated deposit markets reduce their commercial lending relative to banks in less concentrated deposit markets and therefore are more exposed to monetary policy. In column (2), I saturate the model with the interactions of the change in the Fed funds rate and other bank characteristics that are widely used in the bank lending literature to control for the variation in bank-level financial constraints: bank size, equity ratio, and liquidity ratio. Two observations are noteworthy. First, the magnitude and statistical significance of

of borrowers across different banks, this strategy fails to measure the loan supply effect. Since the deposits channel is at the center of my empirical setting, I verify the commercial lending part of the deposits channel by analyzing ex post bank lending behavior in response to monetary policy changes by using more granular data and empirical strategy than those in Drechsler et al. (2017).

[17] Examples of Khwaja and Mian (2008) strategy include Jiménez et al. (2014) and Amiti and Weinstein (2018).

the coefficient increase relative to column (1), thereby again verifying the deposits channel of monetary policy. Second, the signs of interaction terms with bank size and equity are consistent with the findings in the literature, with only the interaction for the size being statistically significant and that for the equity ratio being slightly insignificant (Kashyap and Stein, 1995, 2000; Kishan and Opiela, 2000). In columns (3) and (4), I replicate the specifications in the first two columns but drop borrower and year fixed effects and instead use borrower-year fixed effects, which allows me to perform the within-borrower estimation specified in Equation (3.3). The coefficient that measures the deposits channel remains significant. The economic meaning of this coefficient is that, for each 100 bps increase in the Fed funds rate, a one-standard-deviation increase in bank MPE squeezes commercial lending by approximately 2 percent, which is consistent with the findings of Drechsler et al. (2017). As a result, the findings in Table 3.5 verify that the deposits channel proposed by Drechsler et al. (2017) is robust to using more granular data and a more endogeneity-proof empirical strategy.

However, the decrease in the loan supply following a Fed funds rate increase may not follow a covenant violation. Banks may also decrease their total lending by not renewing loans at the expiration date of the existing loan contract agreements. To test whether banks with higher monetary policy exposure decrease their lending by more in response to a covenant violation during monetary policy tightening, I take the findings in Table 3.5 a step further. Specifically, in column (1) of Table 3.6, I further interact the main interaction variable with a dummy variable that takes a value of 1 if a refinancing loan contract replaces an existing loan contract agreement during the year. Thus, while the main interaction term shows how banks with higher bank MPE react to an increase in the Fed funds rate in the absence of a refinancing loan, the double interaction term shows how these banks react to an increase in the Fed funds rate when there is a refinancing loan in that year relative to the base case. Because a refinancing loan may proxy for renegotiation between a borrower

Table 3.5: **Lending Response to Changes in the Fed Funds Rate**

	Panel Estimation		Within-firm Estimation	
	(1)	(2)	(3)	(4)
MPE, Bank x $\Delta FedFunds$	-0.013**	-0.021***	-0.014**	-0.020***
	(0.006)	(0.008)	(0.007)	(0.006)
MPE, Bank	-0.057*	-0.060*	-0.037	-0.040
	(0.032)	(0.031)	(0.029)	(0.028)
Size, Bank x $\Delta FedFunds$		0.009**		0.005*
		(0.004)		(0.003)
Equity, Bank x $\Delta FedFunds$		0.010		0.009
		(0.006)		(0.006)
Liquidity, Bank x $\Delta FedFunds$		-0.006		-0.007**
		(0.005)		(0.003)
Controls & FEs:				
Bank Controls	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Firm FE	✓	✓		
Year FE	✓	✓		
Firm x Year FE			✓	✓
Obs.	196,501	196,501	196,501	196,501
R^2(Adj.)	0.497	0.497	0.775	0.775

and its lender, a negative coefficient on the double interaction term would suggest that the reduction in bank lending occurs after a covenant violation, which entails a contract renegotiation. The first column does not confirm this. However, in column (2), I create separate interaction terms for term loans and lines of credit and report several coefficients with their significance levels at the bottom of the table. An interesting pattern emerges. Following a Fed funds rate increase, while the banks with higher monetary policy exposure decrease their supply of term loans in the absence of refinancing activity but not in the case of refinancing, the opposite is true for lines of credit. For lines of credit, the reduction in the loan supply by banks with higher monetary policy exposure occurs after a refinancing activity during periods of monetary policy tightening.

An important caveat regarding the previous analysis is that the refinancing dummy may not be a good proxy for contract renegotiation after a covenant violation. To mitigate this

Table 3.6: **Lending Response to Changes in the Fed Funds Rate: Loan Refinancing and Covenant Violation**

	Refinancing		Violation	
	(1)	(2)	(3)	(4)
MPE, Bank x $\Delta FedFunds$	-0.029***	-0.037***	0.001	0.006
	(0.011)	(0.013)	(0.009)	(0.015)
MPE, Bank x $\Delta FedFunds$ x Refinancing	0.014	0.028*		
	(0.013)	(0.015)		
MPE, Bank x $\Delta FedFunds$ x LoC		0.029**		
		(0.014)		
MPE, Bank x $\Delta FedFunds$ x LoC x Refinancing		-0.037**		
		(0.017)		
MPE, Bank x $\Delta FedFunds$ x Violation			-0.031*	-0.036
			(0.018)	(0.031)
MPE, Bank x $\Delta FedFunds$ x LoC				-0.001
				(0.015)
MPE, Bank x $\Delta FedFunds$ x LoC x Violation				-0.024
				(0.042)
Controls & FEs:				
Bank Controls	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Firm x Year FE	✓	✓	✓	✓
Obs.	196,501	185,355	79,208	75,279
R^2(Adj.)	0.776	0.813	0.774	0.808
Coefficient - Term loans		-0.037***		0.006
Coefficient - Term loans after Refinancing/Violation		-0.008		-0.030
Coefficient - LoC loans		-0.008		0.005
Coefficient - LoC loans after Refinancing/Violation		-0.017**		-0.056**

concern, in columns (3) and (4), I use the covenant violation data from Nini et al. (2012).[18] Column (3) shows that, following a Fed funds rate increase, there is no difference between the lending behavior of banks with low and high monetary policy exposure unless the borrower violates a covenant throughout the year. In the case of a violation, however, banks with greater exposure to monetary policy decrease their loan supply by more. Column (4) shows the effect separately for term loans and lines of credit. The effect is negative and significant only for lines of credit.

[18]This violation dataset is also not perfect for my purposes because it does not specify which specific loan contract is violated. It indicates only whether a firm is in violation of a loan contract during a quarter. However, given cross-violation clauses in loan contract agreements, this may not be an important concern.

As a result, the two pieces of ex post evidence in this section support the main finding of this paper: banks with higher monetary policy exposure write stricter financial covenants when they originate loans. These stricter covenants result in more covenant violations, which allow banks to decrease their loan supply to their borrowers following an increase in the Fed funds rate.

3.4 Alternative Explanations

In this section, I discuss two alternative explanations of my findings. One alternative explanation is that the bank MPE measure may capture the loan market power of a bank instead of its exposure to monetary policy changes. The other explanation is the sorting between borrowers and banks based on unobserved borrower characteristics, such as credit risk. I provide several tests to refute each of these potential explanations.

3.4.1 Loan Market Power

Relying on Drechsler et al. (2017), the monetary policy exposure of a bank in this paper is defined as the weighted average of the deposit market concentration of the counties in which a bank raises deposits. Because the market power of a bank in deposit and lending markets may be correlated, bank MPE may also reflect the overall loan market power of the bank, hence capturing the variation in lending conditions in addition to variation in deposit market conditions. Therefore, it is important to control for the loan market power of a bank when measuring the impact of its MPE on the strictness of a loan contract it originates.

A bank's loan market power can come from many sources. Banks may exercise market power in their lending behavior as a result of their industrial specialization, geographical specialization, or informational advantage. The relevant source of market power for a specific bank may vary depending on the specific loan market in question. For instance, while the geographical specialization of a bank may be an important determinant of its loan market power in originating small business loans in a local economy, its industrial specialization and information advantage due to a long-term lending relationship may be more important in originating large loans in the syndicated loan market. Throughout the analysis in this section, I remain agnostic on the source of banks' loan market power and test whether my main findings survive after controlling for the loan market power of banks arising from these three potential sources.

A bank may have loan market power in a specific industry because of its past experience or informational advantage in that industry (Paravisini et al., 2015). The specifications in Table 3.7 control for banks' loan market power due to their specialization in specific industries. For comparison purposes, column (1) replicates my baseline specification with year, bank, and firm fixed effects and control variables (i.e., column (5) of Table 3.2).[19] In columns (2) through (4), I control for loan market concentration in a given industry by including loan market HHI in that industry. I do so by using the syndicated loan market shares of banks in a given industry, where the industries are defined based on different industry classifications: 1-digit SIC codes, Fama-French 10 industries, and Fama-French 48 industries. The coefficient on my main variable of interest does not change. The coefficient on loan market concentration is positive and significant only for Fama-French 48 industry classifications. In columns (5) through (7), I drop year fixed effects and instead use industry-year fixed effects, again using several industry classifications for robustness. Thus I compare borrowers that operate in the same industry. This specification controls for the time-varying characteristics of a given industry, including changes in the industry's loan market concentration, which means that the coefficients for industry loan market concentration are not identified. As an additional control for industry-level loan market power, I add the loan market share of the lead lenders in the borrower's industry. The co-

[19] In fact, if the relevant loan market for syndicated loans is the entire U.S., i.e., there is no specialization of banks in a specific industry or geographic location, then the year fixed effects in column (1) control for the variation in loan market concentration over time.

efficient of interest remains the same, and the coefficient on the loan market share variable is significant and takes the predicted positive sign (for the FF-10 industry classification).

Table 3.7: **Monetary Policy Exposure of Banks and Covenant Strictness: Controlling for Industry-specific Loan Market Power**

	Baseline	Control Loan Market Concentration			Within-industry Estimation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MPE, Bank	0.07***	0.07***	0.07***	0.07***	0.07***	0.07***	0.07***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
HHI (loan), Industry (SIC-1)		0.01					
		(0.00)					
HHI (loan), Industry (FF-10)			-0.00				
			(0.01)				
HHI (loan), Industry (FF-48)				0.02***			
				(0.01)			
Loan market share, Bank (SIC-1)					0.00		
					(0.02)		
Loan market share, Bank (FF-10)						0.03**	
						(0.01)	
Loan market share, Bank (FF-48)							0.01
							(0.01)
Controls & FEs:							
Firm, Bank, Loan Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓			
Industry (SIC-1) x Year FE					✓		
Industry (FF-10) x Year FE						✓	
Industry (FF-48) x Year FE							✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓	✓	✓	✓
Obs.	7,765	7,765	7,765	7,765	7,652	7,654	7,317
R^2(Adj.)	0.434	0.434	0.434	0.435	0.435	0.438	0.444

A large body of literature documents the informational advantage of banks due to their long-term relationships with their borrowers (Berger et al., 2018; Schenone, 2009). This advantage may give banks monopoly power in their lending practices, which may allow banks to include stricter financial covenants in their loan contracts. In column (1) of Table 3.8, instead of borrower and bank fixed effects, I use borrower-bank fixed effects. This specification allows me to control for the time-invariant lending relationship for a given borrower-bank pair and measure the effect of a change in bank MPE over time on loan contract strictness. The coefficient of interest does not change. In the remaining columns, I add various time-varying relationship lending proxies that are used in the literature. I measure the relationship intensity between a borrower and its current lead lender in a loan contract by the total amount of loans that the borrower has drawn from this lender as a percentage of the total amount of loans it has drawn so far (Schenone, 2009). This measure reflects how dependent a borrower is on its current lead lender. Column (2) reports a positive and significant coefficient for this variable, suggesting that the more locked in the borrower is with its current lender, the more bargaining power the lender has in its loan contracting practices. The addition of this variable, however, does not change the coefficient on bank MPE. In column (3), as an additional control, I include relationship duration, which is defined as the number of years the firm has had a lending relationship with its current lender (Ongena and Smith, 2001). The results do not change. Finally, in the last column, I include several additional dummies: "switched lender," a dummy that indicates whether the firm starts a new lending relationship with a lender that it has never worked with; "immediate prior lender," a dummy that indicates whether the firm maintains the immediate previous lead lender in the current loan contract; and "first loan," a dummy that indicates whether a loan is the first loan observed for the borrower in the DealScan data. The coefficient on the bank MPE variable is robust to the inclusion of these dummies.

Finally, to the extent that local lending conditions are important for a borrower and correlated with local deposit market conditions, the variation in bank MPE may reflect local lending conditions rather than the monetary policy exposure of the lender. Note that the relevant monetary policy exposure measure in my analysis is at the bank-year level, i.e., I calculate the overall monetary policy exposure of a bank by taking the weighted average of the deposit market concentration of the counties in which a bank raises deposits. Thus, the relevant variable is not the monetary policy exposure of the bank's branch in the county where the borrower is located. This is because banks can reallocate deposits they collect

Table 3.8: **Monetary Policy Exposure of Banks and Covenant Strictness: Controlling for Relationship-specific Loan Market Power**

	(1)	(2)	(3)	(4)
MPE, Bank	0.07***	0.08***	0.07***	0.08***
	(0.02)	(0.02)	(0.02)	(0.02)
Relationship intensity		0.03**	0.04	0.08***
		(0.02)	(0.03)	(0.03)
Relationship duration			-0.00	-0.01
			(0.01)	(0.01)
Switched lender				-0.03
				(0.02)
Immediate prior lender				-0.02
				(0.02)
First loan				0.07***
				(0.02)
Controls & FEs:				
Firm, Bank, Loan Controls	✓	✓	✓	✓
Industry x Year FE	✓	✓	✓	✓
Firm x Bank FE	✓	✓	✓	✓
Obs.	6,288	6,288	6,288	6,288
R^2(Adj.)	0.485	0.485	0.485	0.486

from one branch to another branch in order to exploit lending opportunities. Therefore, the bank-year level monetary policy exposure measure reflects the bank's overall exposure to changes in monetary policy. This indicates that the changes in the lending conditions of the county where a borrower is located may have a limited impact on the changes in the bank's overall monetary policy exposure because the bank's deposits in the borrower's county constitute only a small portion of the bank's total deposits. However, to further mitigate this concern, I follow two different empirical strategies in Table 3.9. First, in columns (1) and (2), in addition to industry-year fixed effects, I include state-year and county-year fixed effects, respectively, which means that I compare the loan contract strictness of borrowers that are located in the same county[20] but work with banks with different levels of monetary policy exposure. This within-county comparison controls for changes in local loan market conditions. The coefficient of bank MPE remains positive and significant. Second,

[20]In the same state in column (1).

in columns (3) through (5), I exploit the source of variation in bank MPE. For instance, in column (3), I use the variation in bank MPE that comes from counties other than the borrower's own county. Specifically, I calculate a new borrower's county-specific measure for bank monetary policy exposure by excluding the deposit market of the borrower's own county. Note that unlike the bank-year level bank MPE, the new measure is at the bank-county-year level, which means that the specification in this column uses only the variation in a bank's overall monetary policy exposure from counties other than the borrower's own county. The coefficient does not change. In column (4), I restrict the sample to banks with limited presence in their borrowers' counties. Specifically, I exclude observations from the sample for which the deposits of the bank in the borrower's county constitute more than 1 percent of the bank's total deposits. In column (5), I rely on a stricter criterion and use only those observations for which the bank has no presence in the borrower's county deposit market. Although these two criteria considerably reduce the sample size, the size of the main coefficient does not change significantly.

3.4.2 Sorting between Firms and Banks: Credit Risk

While the main results are robust to various controls, fixed effects, and controlling for bank loan market power, the remaining identification challenge is the endogenous sorting between borrowers and banks based on an unobserved borrower characteristic. To obtain a causal interpretation of the coefficients, the sorting between borrowers and banks should be as good as random, which means that borrowers of high-exposure banks should not differ systematically from borrowers of low-exposure banks along dimensions related to borrower risk. If risky borrowers work with banks that have high exposure to monetary policy, stricter financial covenants would reflect the riskiness of these borrowers - not the monetary policy exposure of banks. For instance, an omitted variable may trigger an increase in both borrower credit risk and bank MPE, which induces banks to write stricter

Table 3.9: **Monetary Policy Exposure of Banks and Covenant Strictness: Controlling for Local Loan Market Power**

	Within-Location Estimation		Source of Variation		
	(1) Within State	(2) Within County	(3) Exclude Own County	(4) Low Share	(5) No Share
MPE, Bank	0.09***	0.10***		0.10***	0.09**
	(0.03)	(0.04)		(0.03)	(0.04)
MPE, Bank (others)			0.07***		
			(0.03)		
Size	-0.10***	-0.08***	-0.10***	-0.12***	-0.06
	(0.02)	(0.03)	(0.02)	(0.04)	(0.05)
Tobin's q	-0.06***	-0.07***	-0.06***	-0.05***	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Leverage	0.06***	0.07***	0.06***	0.07***	0.08***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Controls & FEs:					
Rating Dummies	✓	✓	✓	✓	✓
Bank Controls	✓	✓	✓	✓	✓
Loan Controls	✓	✓	✓	✓	✓
Industry x Year FE	✓	✓	✓	✓	✓
State x Year FE	✓		✓	✓	✓
County x Year FE		✓			
Firm FE	✓	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓	✓
Obs.	7,609	5,795	7,609	4,678	2,549
R^2(Adj.)	0.431	0.426	0.431	0.457	0.436

loan contracts to control borrower credit risk. To address this concern and establish the causal relation from bank MPE to contract strictness, I implement two additional empirical approaches in this section.

3.4.2.1 Within-firm Estimation

First, I perform a within-borrower estimation (a la Khwaja and Mian 2008). Within-borrower estimation uses borrower-year fixed effects and compares the strictness of loan contracts originated to the same borrower in the same year by banks with different levels of monetary policy exposure. Under the assumption that borrower riskiness remains constant throughout the year and hence that the terms of loans originated in the same year reflect the same level of borrower riskiness, this estimation controls for endogenous borrower-bank matching based on borrower riskiness. The regression model used in this estimation strategy is as follows:

$$Strictness_{\ell,f,b,t} = \beta MPE_{b,t-1} + \xi_{f,t} + \eta_b + \Gamma \mathbf{B}_{b,t-1} + \Theta \mathbf{L}_{\ell,f,b,t} + \varepsilon_{\ell,f,b,t} \tag{3.4}$$

where $\xi_{f,t}$ are borrower-year fixed effects for borrower f. Conducting this within-borrower estimation requires me to use only the sample of borrowers that have new loan originations from at least two banks in a given year and exclude all others because the coefficient is not identified for single-bank borrowers. However, the number of such borrowers in DealScan is limited, reducing the sample size to 250, which makes inference more difficult. To overcome this problem, I implement two separate modified within-borrower estimation models in Equation (3.4) and present results consistent in both cases with the results of my baseline specification – the estimation in Equation (3.1).

The first modified model changes the assumption that the terms of a loan contract reflect the riskiness of a borrower only in the loan origination year. I assume that loan terms reflect the riskiness of the borrower for all years from the loan origination year until the loan expiration year. In practice, this means that instead of only comparing two loans originated to a borrower by two banks in the same year, I can compare two loans that are outstanding in the same year even if their origination years differ. For instance, if there is a three-year loan that is originated in 2001, then I use three data points for this loan: one each for 2001, 2002, and 2003, which allows me to compare this loan with a loan originated in 2002. This modification increases the sample size from 250 to 3,065.

The second modified model does not require changes to the assumptions but regards only the definition of *year*. Note that the time frame used in Equation (3.4) is a calendar year. Thus, the estimation compares only the loans originated within the same calendar year regardless of the time distance between the origination date of the loans. For instance, consider the following two cases. In the first case, assume that a borrower obtains a loan from a bank in December of year t and a new loan from another bank in January of year $t+1$. The within-borrower estimation in Equation (3.4) does not use these two loan observations in the estimation. In the second case, assume that the borrower secures a loan that is originated in February of year t and another loan that is originated in November of the same year. These two loan observations are used in the within-borrower estimation. Note that although the time distance between the origination dates of loans is shorter in the first case, these two observations are not included in the sample simply because they are in different calendar years. However, as the time distance between two loans decreases, we should expect the two loans to reflect a more similar level of borrower riskiness. To avoid losing these observations from my within-borrower sample, I use a moving four-quarter window as a year instead of using a fixed calendar year. I refer to this new four-quarter moving window as the period throughout the paper. For instance, 1995:Q1–1995:Q4 is the first four-quarter period in my sample, 1995:Q2–1996:Q1 is the second period, 1995:Q3–1996:Q2 is the third period, and so forth. With this new "period" definition, I use the following modified within-borrower estimation model:

$$Strictness_{\ell,f,b,t} = \beta MPE_{b,t-1} + \xi_{f,p} + \delta_t + \eta_b + \Gamma \mathbf{B}_{b,t-1} + \Theta \mathbf{L}_{\ell,f,b,t} + \varepsilon_{\ell,f,b,t} \tag{3.5}$$

where $\xi_{f,p}$ are borrower-period fixed effects. Note that in this modified within-borrower estimation, $\xi_{f,p}$ replaces $\xi_{f,t}$. I also include calendar-year fixed effects, δ_t, to control for any calendar-year-specific shocks or regulations. This modification increases the sample size from 250 to 557. With the borrower-period fixed effects, I compare the strictness of loan contracts originated to the same borrower in the same period (i.e., four-quarter period) by banks with different levels of monetary policy exposure. Because the borrower of these loans is constant in this comparison, this strategy holds borrower riskiness constant and enables me to reveal the effect of bank MPE on contract strictness.

Table 3.10 reports the results of the first modified within-borrower estimation using the within-borrower sample: the sample of borrowers that have outstanding loans from at least two banks in a given year. To provide a comparison for the results in Table 3.2, I run the specification without borrower-year fixed effects in columns (1) through (3). Specifically, column (1) uses borrower, bank, and year fixed effects and borrower control variables. Columns (2) and (3) add bank and loan controls, respectively. The coefficients are consistent with those I obtain for the full sample. In columns (4) through (6), I drop borrower and year fixed effects and instead use borrower-year fixed effects. Note that I also drop borrower controls because they are not identified in a regression with borrower-year fixed effects. The coefficient that measures the effect of bank MPE on contract strictness remains significant.

Table 3.10: **Monetary Policy Exposure of Banks and Covenant Strictness: Within-firm Estimation (1)**

	Panel Estimation			Within-firm Estimation		
	(1)	(2)	(3)	(4)	(5)	(6)
MPE, Bank	0.04^{*}	0.06^{**}	0.06^{**}	0.05^{*}	0.08^{**}	0.08^{**}
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Controls & FEs:						
Firm Controls	✓	✓	✓			
Bank Controls		✓	✓		✓	✓
Loan Controls			✓			✓
Firm FE	✓	✓	✓			
Year FE	✓	✓	✓			
Firm x Year FE				✓	✓	✓
Bank FE	✓	✓	✓	✓	✓	✓
Obs.	3,065	3,065	3,065	3,065	3,065	3,065
R^2	0.677	0.678	0.685	0.734	0.735	0.740

Table 3.11 reports the results of the second modified within-borrower estimation using the within-borrower sample: the sample of borrowers that have new loan originations from at least two banks in a given period. The panel estimations in columns (1) through (3)

produce results consistent with the previous estimations. In columns (4) through (6), I drop borrower fixed effects and instead use borrower-period fixed effects. The effect of bank MPE on contract strictness remains positive and significant.

Table 3.11: **Monetary Policy Exposure of Banks and Covenant Strictness: Within-firm Estimation (2)**

	Panel Estimation			Within-firm Estimation		
	(1)	(2)	(3)	(4)	(5)	(6)
MPE, Bank	0.12**	0.12**	0.12**	0.12	0.12*	0.13*
	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)
Controls & FEs:						
Firm Controls	✓	✓	✓			
Loan Controls		✓	✓		✓	✓
Bank Controls			✓			✓
Firm FE	✓	✓	✓			
Calendar Year FE	✓	✓	✓	✓	✓	✓
Firm x Period FE				✓	✓	✓
Bank FE	✓	✓	✓	✓	✓	✓
Obs.	557	557	557	557	557	557
R^2(Adj.)	0.689	0.698	0.697	0.667	0.679	0.679

Note that for within-borrower estimations, by construction, I use only the borrowers that have more than one lending relationship in a given year, which raises the question of why a borrower accepts a stricter loan contract even if it has more than one relationship. A potential explanation for these types of borrowers is that the bank may offer a lower loan spread in return for stricter covenants. To test this possibility, I conduct the same within-borrower estimations by replacing loan strictness with loan spread as the dependent variable. Table 3.12 shows that banks with higher monetary policy exposure charge lower loan spreads. This means that banks gain flexibility by writing stricter loan contracts for future contingencies, but in return, they offer more favorable interest rates to their borrowers. Furthermore, the results on loan spread also mitigate the concern that loan market power of banks, instead of their exposure to monetary policy, may drive the results. If the driver of stricter loan contracts was banks' loan market power, we should expect to also find a positive effect on the loan spread.

Table 3.12: **Monetary Policy Exposure of Banks and Loan Spread: Within-firm Estimations**

	Within-firm Estimation (1)			Within-firm Estimation (2)		
	(1)	(2)	(3)	(4)	(5)	(6)
MPE, Bank	-0.11*	-0.14**	-0.14**	-0.12*	-0.16**	-0.15**
	(0.07)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)
Controls & FEs:						
Bank Controls		✓	✓		✓	✓
Loan Controls			✓			✓
Firm x Year FE	✓	✓	✓			
Firm x Period FE				✓	✓	✓
Calendar Year FE				✓	✓	✓
Bank FE	✓	✓	✓	✓	✓	✓
Obs.	1,190	1,190	1,190	2,555	2,555	2,555
R^2(Adj.)	0.592	0.599	0.659	0.596	0.602	0.666

3.4.2.2 Bank Mergers

The second way to address concerns about endogenous borrower-bank matching is to use merger-induced variation in banks' monetary policy exposure. Following Garmaise and Moskowitz (2006), Favara and Giannetti (2017), and Scharfstein and Sunderam (2014), I use the sample of counties in which deposit market concentration is increased by bank mergers. Specifically, first, I use the deposit market shares of merging banks in those counties and calculate simulated HHIs of the counties. The deposit market shares of non-merging banks in those counties are considered to be zero in the calculation of simulated county HHI. Second, following the same procedure used previously for bank MPE, I calculate a simulated bank MPE by taking the weighted average of the simulated HHI of counties in which a bank raises deposits. I use this simulated bank MPE in an IV framework as an instrument for bank MPE.

Table 3.13 presents the results of this IV estimation. The first column shows the first stage, in which I regress bank MPE on simulated bank MPE. The coefficient is significant and has the predicted sign, and the F statistics of the first stage is above 10. Note that I use bank and year fixed effects, which means that the variation in monetary policy exposure is within a bank after the merger relative to other banks that do not experience a merger. The second column shows the IV estimation results. Consistent with my previous results, the coefficient of bank MPE remains positive and significant. The size of the coefficient is larger than that of my baseline specification and is close to the estimate that I obtain by using a within-borrower estimation.

Table 3.13: **Monetary Policy Exposure of Banks and Covenant Strictness: Bank Mergers as an IV**

	First Stage	IV
	(1) Dependent Variable: MPE, Bank	(2) Dependent Variable: Strictness
Simulated MPE, Bank	0.215**	
	(0.095)	
MPE, Bank		0.171**
		(0.082)
Controls & FEs:		
Firm, Bank, Loan Controls	✓	✓
Firm FE	✓	✓
Bank FE	✓	✓
Year FE	✓	✓
Obs.	7,649	7,649
F-stat	11.22	55.97
R^2(Adj.)	0.70	0.45

Taken together, various controls and different empirical strategies produce results consistent with my baseline specification and ensure that the impact of monetary policy exposure on the lending behavior of banks does not reflect banks' loan market power or sorting of borrowers and lenders based on an omitted variable.

3.5 Heterogeneity

After ruling out alternative explanations, in this section, I test a number of cross-sectional implications of my hypothesis to support a causal interpretation of the results: it is the increase in the monetary policy exposure of banks that drives the increase in loan contract strictness.

The first heterogeneity test relates to banks' incentives to gain flexibility to adjust the size of their loan portfolio. The intuition behind my interpretation of the results is banks' concern about the future path of the Fed funds rate. Therefore, banks are in greater need of flexibility to adjust their loan portfolio size when there is greater uncertainty about the future path of monetary policy, which implies that I should find a stronger link between the monetary policy exposure of banks and loan contract strictness during periods of high monetary policy uncertainty. I use three different measures of this uncertainty. The first measure is implied interest rate volatility, which is obtained from future contracts on interest rates (Cremers et al., 2017). The second measure is the monetary policy uncertainty index. This index is a newspaper-based index and is widely used in the literature (Baker et al., 2016; Husted et al., 2017). The third measure captures recent monetary policy shocks. Specifically, I obtain the size of the monetary policy shocks in the last one-year period from Gorodnichenko and Weber (2016) and and take their average to obtain a proxy for monetary policy uncertainty. Table 3.14 shows the effect of bank MPE on loan contract strictness by splitting the sample into periods of low and high monetary policy uncertainty based on the three different measures. For each measure, the effect is strong and significant for periods of high uncertainty but not significant for periods of low uncertainty.

Heterogeneity in loan characteristics provides additional evidence for the causal interpretation of the link between bank MPE and contract strictness. First, I exploit the heterogeneity of loans with respect to maturity. In response to monetary policy tightening,

Table 3.14: **Heterogeneity: Monetary Policy Uncertainty**

	Implied IR Vol		MPU Index		MP Shocks	
	(1) Low	(2) High	(3) Low	(4) High	(5) Low	(6) High
MPE, Bank	0.04	0.17***	-0.02	0.23***	0.06	0.19**
	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.08)
Size	-0.03	-0.12**	-0.10**	-0.17***	-0.09*	-0.14***
	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)
Tobin's q	-0.04***	-0.06**	-0.04***	-0.07***	-0.06***	-0.09***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Leverage	0.09***	0.05***	0.08***	0.07**	0.07***	0.09***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
Controls & FEs:						
Rating Dummies	✓	✓	✓	✓	✓	✓
Bank, Loan Controls	✓	✓	✓	✓	✓	✓
Industry x Year FE	✓	✓	✓	✓	✓	✓
State x Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓	✓	✓
Obs.	2,516	2,505	2,381	2,393	2,416	2,416
R^2(Adj.)	0.432	0.487	0.395	0.451	0.461	0.412

banks have two options to decrease lending amounts to their existing customers. Either they must wait until the loan expiration date and then not renew the loan or they may use their contractual rights to decrease the loan amount if financial covenants are violated. This implies that the cost of not having stricter loan contracts is higher for long-term loans because there is more time until the loan expiration date. On the other hand, for short-term loans, waiting until the maturity date of the loan to terminate the loan contract is less costly. Furthermore, the longer the horizon is, the higher the probability that banks will see an increase in the Fed funds rates and the higher the probability that they will want to cut their lending amount. Hence, the incentive to impose stricter loan covenants to gain flexibility for future contingencies is also higher. Therefore, if the driver of the strict loan contracts of banks with high monetary policy exposure is their concerns about the future path of monetary policy, the incentive to write strict loan contracts should be higher when originating long-term loans. In column (1) of Table 3.15, I interact loan maturity with the monetary policy exposure of banks. The coefficient of the interaction term is positive and significant. In column (2), I estimate separate coefficients for loans with different maturities. In particular, I estimate separate coefficients for loans with a maturity of 0 to 2 years, loans with a maturity of 2 to 4 years, and loans with a maturity of more than 4 years. The results support my prediction. I find that the coefficients for longer-term loans are significantly higher than those for short-term loans.

Table 3.15: **Heterogeneity: Loan Characteristics**

	Maturity		Type
	(1)	(2)	(3)
MPE, Bank	0.03	0.06^{**}	0.01
	(0.04)	(0.03)	(0.07)
MPE, Bank x log(Maturity)	0.01^{**}		
	(0.01)		
MPE, Bank x Maturity(2 4]		0.03^{**}	
		(0.02)	
MPE, Bank x Maturity(4 .]		0.02^{**}	
		(0.01)	
MPE, Bank x LoC			0.16^{*}
			(0.09)
Controls & FEs:			
Firm, Bank, and Loan Controls	✓	✓	✓
Industry x Year FE	✓	✓	✓
State x Year FE	✓	✓	✓
Firm FE	✓	✓	✓
Bank FE	✓	✓	✓
Obs.	6,852	6,852	3,960
R^2(Adj.)	0.421	0.421	0.425

Another loan characteristic that provides more insight into the mechanism is the type of loan contract. There are two main types of loan contracts in the DealScan data: lines of credit and term loans. Lines of credit loans are commitment loans in which banks commit themselves to provide a contractually determined loan amount to their borrowers on demand as long as the borrowers satisfy certain conditions (i.e., financial ratio covenants), while term loans include an upfront advance of the loan amount to borrowers. Column

(3) of Table 3.15 explores the heterogeneity in the effect of monetary policy exposure on contract strictness with respect to loan type. I estimate separate coefficients for the two types of loans and show that the effect is significant only for lines of credit. This finding is noteworthy for several reasons. First, if banks wish to decrease the loan amount following a covenant violation, then this means that term loan borrowers must repay the loan amount to the lender, whereas line-of-credit borrowers simply lose their access to bank loans. The impact of decreasing the loan amount, therefore, is more detrimental for term loan borrowers than for line-of-credit borrowers. Considering the reputation concerns of banks and their desire to maintain relationships with their existing borrowers, the effect should be expected to be stronger for line-of-credit loans. This finding is also consistent with the findings of Chodorow-Reich and Falato (2017), who show that during the financial crisis, in response to covenant violations, banks tend to change the contract terms of lines of credit more than those of term loans, and with the findings of Section 3.3.2 that following an increase in the Fed funds rate, banks with higher monetary policy exposure decrease their loan supply following a covenant violation only for lines of credit. Second, this finding is also consistent with the literature on loan sales. In the syndicated loan market, the percentage of term loans that are sold to institutional investors is higher than that of lines of credit (Drucker and Puri, 2008; Gatev and Strahan, 2009; Irani and Meisenzahl, 2017). This indicates that banks have less exposure to term loans because they do not keep them on their balance sheets and therefore should have less incentive to write stricter loan contracts in the first place. Finally, from a bank perspective, lines of credit are more exposed to liquidity risk in the sense that the bank does not know the exact timing of loan withdrawals by its borrowers (Gatev and Strahan, 2009; Balasubramanyan et al., 2019), and hence has more incentive to gain flexibility for future contingencies.

A relevant question is why borrowers accept stricter loan contracts even if their own risk remains unchanged. Why do they not simply go to another bank with lower monetary policy exposure and obtain a loan with less strict covenants? This concern is related to the fact that borrowers, ceteris paribus, may desire less restrictive loan contracts because giving contingent control rights to their lenders restricts the optimal action space of borrowers in future contingencies and is therefore costly for them. As previously documented, offering lower loan spreads for multiple-bank firms in return for stricter covenants provides an explanation for this. Another potential explanation comes from the findings of two strands of literature. Switching across banks is not straightforward for borrowers. First, a wedge between internal and external costs of finance arises due to agency problems between lenders and borrowers. The greater the information asymmetry between the parties is, the higher the external finance premium. Second, banks are specialized at reducing this information asymmetry by generating soft information on their borrowers and using this information to serve them (Fama, 1985; Diamond, 1991; Petersen and Rajan, 1994; Drucker and Puri, 2008). The duration of the bank-borrower relationship is one of the most important determinants of the extent to which banks reduce the information asymmetry problem. These two strands of literature imply that if a borrower switches to a new bank, then the information asymmetry with its lender increases, which in turn raises its cost of financing. This switching cost gives banks monopoly power over their customers and hence allows them to write stricter loan contracts without losing customers. Therefore, I should find a stronger link between bank MPE and contract strictness for financially constrained borrowers that have less access to alternative external funding sources because these borrowers suffer more from information asymmetries.

I use two measures as proxies for borrower financial constraints: borrower asset size and credit rating. Small borrowers are usually more opaque and therefore more exposed to information asymmetries with their lenders, which implies that switching to a different bank is more difficult. Furthermore, they have limited access to sources of external funding other than bank lending (Hadlock and Pierce, 2010). Similarly, borrowers with no credit rating have limited access to the bond market as an external funding source (Sufi, 2007; Chava and Roberts, 2008). Therefore, they are expected to be subject to more monopoly power from their lenders. While columns (1) and (2) of Table 3.16 split the sample in two based on borrower size, columns (3) and (4) split the sample based on whether a borrower has a credit rating. The results support my hypothesis and show that the effect of monetary policy exposure on contract strictness is stronger for smaller borrowers and for borrowers with no credit rating.

Table 3.16: **Heterogeneity: Firm Financial Constraints**

	Firm Size		S&P Rating	
	(1)	(2)	(3)	(4)
	Small	Large	No	Yes
MPE, Bank	0.13^{**}	0.04^{**}	0.18^{***}	0.07^{**}
	(0.05)	(0.02)	(0.05)	(0.03)
Size	-0.07	0.02	-0.11^{**}	-0.04
	(0.05)	(0.09)	(0.05)	(0.07)
Tobin's q	-0.05^{**}	-0.04^{**}	-0.06^{***}	-0.07^{***}
	(0.02)	(0.01)	(0.02)	(0.01)
Leverage	0.04^{***}	0.08^{***}	0.05^{***}	0.07^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Controls & FEs:				
Rating Dummies	✓	✓	✓	✓
Bank, and Loan Controls	✓	✓	✓	✓
Industry x Year FE	✓	✓	✓	✓
State x Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Obs.	3,378	3,359	3,333	3,371
R^2(Adj.)	0.311	0.511	0.352	0.492

Building on contract theory (Aghion and Bolton, 1992), Christensen and Nikolaev (2012) categorize financial covenants into capital covenants and performance covenants. Capital covenants (e.g, leverage and current ratio covenants) serve as an ex ante tool to protect the value of debtholders' claims in a firm by aligning the interests of debtholders and shareholders and are calculated by using balance sheet items. On the other hand, performance covenants (e.g., interest coverage ratio and EBITDA level) detect any decline in firm performance and allow lenders to intervene to make changes in loan contracts and are calculated by using income statement items. Therefore, they distribute the control rights ex post. Table 3.17 tests whether monetary policy exposures of banks have heterogeneous effects on different covenant types. While contract strictness in column (1) is calculated by using all types of financial covenants in a loan contract, in columns (2) and (3), contract strictness is calculated based only on performance covenants and capital covenants, respectively. The effect of bank MPE is significant only on the strictness measure based on performance covenants. This finding is consistent with the idea that banks with high monetary policy exposure write stricter loan contracts to increase their control power over their customers ex post. If there were an omitted variable correlated with banks' loan market power or with borrower riskiness, one would expect the effect to also be significant for capital covenants.

Table 3.17: **Heterogeneity: Covenant Types**

	(1) All Covenants	(2) Performance Covenants	(3) Capital Covenants
MPE, Bank	0.09***	0.08***	0.02
	(0.03)	(0.03)	(0.01)
Size	-0.10***	-0.10***	0.00
	(0.02)	(0.02)	(0.01)
Tobin's q	-0.06***	-0.06***	0.00
	(0.01)	(0.01)	(0.01)
Leverage	0.06***	0.06***	0.02***
	(0.01)	(0.02)	(0.01)
Controls & FEs:			
Rating Dummies	✓	✓	✓
Bank, and Loan Controls	✓	✓	✓
Industry x Year FE	✓	✓	✓
State x Year FE	✓	✓	✓
Firm, and Bank FE	✓	✓	✓
Obs.	7,609	7,609	7,609
R^2(Adj.)	0.431	0.434	0.339

Finally, I exploit the syndicate structure of loan contracts. Most loans in DealScan are syndicated loans in which there are lead and participant lenders. Each of these lenders is responsible for providing a certain share of the total loan amount, while lead lenders are responsible for executing the loan contract and monitoring the borrower. Because changing loan contract terms during renegotiations requires majority voting, the higher stakes of lead lenders imply a greater ability to change the loan contract terms. Therefore, the main hypothesis of the paper predicts that the relationship between bank MPE and contract strictness should be stronger for loans with a higher lead lender share. I use two aspects of syndicated loan structures: the share of lead lenders in the total loan amount and the concentration of lender shares. I use a subset of DealScan loan contracts that have lender share information to calculate these two measures. Table 3.18 shows that the effect is stronger for more concentrated syndicates with a larger lead lender share, consistent with the prediction of the hypothesis of the paper.

Table 3.18: **Heterogeneity: Loan Syndicate Structure**

	Lead Lender Share		Loan concentration	
	(1) High	(2) Low	(3) High	(4) Low
MPE, Bank	0.199**	0.069	0.212**	0.074
	(0.088)	(0.041)	(0.103)	(0.045)
Controls & FEs:				
Firm, Bank, and Loan Controls	✓	✓	✓	✓
Industry x Year FE	✓	✓	✓	✓
State x Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Obs.	1,527	1,537	1,487	1,625
R^2(Adj.)	0.263	0.533	0.249	0.511

3.6 Discussion and Conclusion

There is a large literature on the bank lending channel of monetary policy transmission. This literature has documented that tight monetary policy lowers real economic activity by reducing the loan supply of banks to non-financial firms. However, how banks achieve reducing the loan supply to their existing borrowers is unclear because outstanding long-term loan contracts may prevent them from doing so. In this paper, I examine loan contract terms, and find that loan covenants help banks respond to a contractionary monetary policy shock.

I start by showing that banks use loan covenants as an ex-ante tool to prepare for future monetary policy shocks. In particular, banks with higher monetary policy exposure (i.e., banks that experience larger deposit outflows during monetary policy contractions) write tight loan contracts by using stricter covenants. This ex-ante behavior of banks makes borrowers more likely to violate covenants, giving banks the right to reduce lending ex-post when monetary policy shocks induce a contraction in their lending capacity. Then, I provide supporting evidence by showing that banks indeed use covenant violations as an opportunity to reduce lending during times of monetary policy contractions.

These findings indicate that loan covenants facilitate the transmission of monetary policy to the economy by giving banks more flexibility to respond to contractionary monetary policy shocks. This increased flexibility has implications for banks' lending behavior. Banks may adopt different strategies to respond to a decrease in their lending capacity during monetary policy contractions. One strategy could be to reject new loan applications and maintain the outstanding loans granted to existing borrowers. Another strategy could be to reduce the size of outstanding loans and take advantage of new, potentially more profitable lending opportunities as they arise. Stricter covenants increase banks' ability to follow the second strategy. In particular, stricter covenants lead to more frequent renegotiations and, hence, help banks reduce lending to covenant violators, which are precisely the types of borrowers to which banks may want to reduce lending when their lending capacity is hit by a shock. Therefore, covenants may help banks allocate their resources more efficiently during monetary policy contractions. However, the findings also indicate that firms are exposed to monetary policy shocks, even when they have long-term loans outstanding.

In this paper, I exploit the heterogeneity of banks' exposure to monetary policy and analyze its impact on their loan contracting behavior. The results may also extend to any bank characteristics that might influence the future willingness and/or capacity of banks to lend following a shock. For instance, some banks may hold a high share of a particular security, which may render the value and lending capacity of these banks more sensitive to changes in economic conditions. To the extent of their awareness of this sensitivity, banks may write stricter contracts to gain flexibility to reduce their lending as necessary in some future contingencies.

Chapter 4

Conclusion

The deposit market conditions of banks have a substantial impact on their lending behavior. In this dissertation, I study the interaction between banks' lending decisions and the deposit market conditions triggered by unemployment insurance policies and contractionary monetary policies. Understanding the impact of these policies on bank deposits will help us better understand how banks' lending decisions are affected by their liability structure.

Appendices

Appendix A

Table A1: Household Awareness, Google Trends

	$\Delta Web\ Interest$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit)$, State	5.353*	9.354**	10.308***	5.609**	5.323**
	(2.747)	(3.560)	(2.524)	(2.171)	(2.066)
$\Delta log\ (Income)$, State			-26.563***	4.673	8.359*
			(1.967)	(3.781)	(4.159)
$\Delta(Unemp.Rate)$, State				106.463***	96.508***
				(11.562)	(10.655)
$\Delta log(GDP, Real)$, State					-12.714***
					(3.382)
State FE	N	Y	Y	Y	Y
Obs.	294	294	294	294	294
R^2	0.009	0.042	0.337	0.566	0.582

Appendix B

Table B1: **Monetary Policy Exposure of Banks and Covenant Strictness: Including Loans with Multiple Lead Lenders**

	(1)	(2)	(3)	(4)	(5)
MPE, Bank	0.06***	0.06**	0.06***	0.09***	0.10***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
Size		-0.05**	-0.07***	-0.10***	-0.08***
		(0.02)	(0.02)	(0.02)	(0.02)
Tobin's q		-0.07***	-0.06***	-0.06***	-0.07***
		(0.01)	(0.01)	(0.01)	(0.01)
Leverage		0.07***	0.07***	0.06***	0.07***
		(0.01)	(0.01)	(0.01)	(0.01)
Controls & FEs:					
Rating Dummies	✓	✓	✓	✓	✓
Bank, and Loan Controls			✓	✓	✓
Year FE	✓	✓	✓		
Industry x Year FE				✓	✓
State x Year FE				✓	
County x Year FE					✓
Firm FE	✓	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓	✓
Obs.	7,934	7,934	7,934	7,780	5,952
R^2(Adj.)	0.413	0.433	0.435	0.432	0.428

Table B2: **Monetary Policy Exposure of Banks and Covenant Strictness: Pre-crisis Period**

	(1)	(2)	(3)	(4)	(5)
MPE, Bank	0.07*	0.08*	0.09**	0.10**	0.16***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
Size		-0.05*	-0.07***	-0.11***	-0.07*
		(0.03)	(0.03)	(0.03)	(0.04)
Tobin's q		-0.06***	-0.06***	-0.05***	-0.06***
		(0.01)	(0.01)	(0.01)	(0.01)
Leverage		0.07***	0.07***	0.07***	0.08***
		(0.01)	(0.01)	(0.01)	(0.01)
Controls & FEs:					
Rating Dummies	✓	✓	✓	✓	✓
Bank, and Loan Controls			✓	✓	✓
Year FE	✓	✓	✓		
Industry x Year FE				✓	✓
State x Year FE				✓	
County x Year FE					✓
Firm FE	✓	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓	✓
Obs.	5,817	5,817	5,817	5,711	4,433
R^2(Adj.)	0.386	0.406	0.409	0.412	0.395

Table B3: Monetary Policy Exposure of Banks and Covenant Strictness: An Alternative Measure

	Main Sample				Additional Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
MPE, Bank	0.06**	0.06*	0.06*	0.06*	0.05*	0.06*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Size		-0.05**	-0.05**	-0.07***	-0.06**	-0.04
		(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Tobin's q		-0.07***	-0.07***	-0.06***	-0.06***	-0.02
		(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Leverage		0.06***	0.06***	0.06***	0.05***	0.03**
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Current ratio						-0.05***
						(0.01)
Tangibility						-0.03
						(0.02)
Altman's z						-0.06***
						(0.02)
Fixed coverage ratio						-0.02***
						(0.00)
Controls & FEs:						
Rating Dummies	✓	✓	✓	✓	✓	✓
Bank Controls			✓	✓	✓	✓
Loan Controls				✓	✓	✓
Industry x Year FE	✓	✓	✓	✓	✓	✓
State x Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Obs.	7,494	7,494	7,494	7,494	6,893	6,893
R^2(Adj.)	0.411	0.429	0.429	0.431	0.433	0.446

Table B4: **Covenant Strictness and Covenant Vioaltion: Measurement Error**

	Dep Var: Covenant Violation		
	(1)	(2)	(3)
	OLS	High-MPE	Low-MPE
Strictness	0.10***	0.10***	0.10***
	(0.01)	(0.01)	(0.01)
Controls & FEs:			
Year FE	✓	✓	✓
Industry FE	✓	✓	✓
Obs.	6,613	3,313	3,300
R^2(Adj.)	0.097	0.121	0.066

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Biography

Ahmet Degerli was born in Turkey. He holds a B.A. degreee in Business Administration and a B.A. degreee in Economics from Bogazici University (2008, Istanbul). He earned his M.S. degree in Economics from Middle East Technical University (2012, Ankara) and his Ph.D. degree in Finance from the Fuqua School of Business at Duke University (2020, Durham NC). Prior to his Ph.D., he worked as a Researcher at the Central Bank of Turkey. In July 2020, he will join the Board of Governors of the Federal Reserve System as an Economist.