

Ippolito, Filippo; Ozdagli, Ali K.; Perez, Ander

Working Paper

Is bank debt special for the transmission of monetary policy? Evidence from the stock market

Working Papers, No. 13-17

Provided in Cooperation with:

Federal Reserve Bank of Boston

Suggested Citation: Ippolito, Filippo; Ozdagli, Ali K.; Perez, Ander (2013) : Is bank debt special for the transmission of monetary policy? Evidence from the stock market, Working Papers, No. 13-17, Federal Reserve Bank of Boston, Boston, MA

This Version is available at:

<https://hdl.handle.net/10419/107224>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Is Bank Debt Special for the Transmission of Monetary Policy? Evidence from the Stock Market

Filippo Ippolito, Ali K. Ozdagli, and Ander Perez

Abstract:

We combine existing balance sheet and stock market data with two new datasets to study whether, how much, and why bank lending to firms matters for the transmission of monetary policy. The first new dataset enables us to quantify the bank dependence of firms precisely, as the ratio of bank debt to total assets. We show that a two standard deviation increase in the bank dependence of a firm makes its stock price about 25 percent more responsive to monetary policy shocks. We explore the channels through which this effect occurs, and find that the stock prices of bank-dependent firms that borrow from financially weaker banks display a stronger sensitivity to monetary policy shocks. This finding is consistent with the bank lending channel, a theory that posits that the strength of bank balance sheets matters for monetary policy transmission. We construct a new database of hedging activities and show that the stock prices of bank-dependent firms that hedge against interest rate risk display a lower sensitivity to monetary policy shocks. This finding is consistent with an interest rate pass-through channel that operates via the direct transmission of policy rates to lending rates associated with the widespread use of floating rates in bank loans and credit line agreements.

Keywords: bank lending channel, monetary policy transmission, firm financial constraints, bank financial health

JEL Classifications: G21, G32, E52

Filippo Ippolito is an assistant professor of finance at the Universitat Pompeu Fabra. Ali K. Ozdagli is an economist in the research department of the Federal Reserve Bank of Boston. Ander Perez is an assistant professor at the Universitat Pompeu Fabra. Their e-mail addresses are filippo.ippolito@upf.edu, ali.ozdagli@bos.frb.org, and ander.perez@upf.edu, respectively.

This paper, which may be revised, is available on the web site of the Federal Reserve Bank of Boston at <http://www.bos.frb.org/economic/wp/index.htm>.

We thank Stefan Pitschner, Miguel Karlo De Jesus, and Yifan Yu for excellent research assistance. We are grateful to Jeff Fuhrer, Refet Gurkaynak, Satadru Hore, Anil Kashyap, Alex Levkov, Juan Pablo Nicolini, Dino Palazzo, Daniel Paravisini, Joe Peek, Jose Luis Peydró, Matt Pritsker, Christina Romer, David Romer, Nancy Stokey, Geoff Tootell, Christina Wang, Paul Willen, and audiences in the Boston Fed, UPF, the 2013 NASM of the Econometric Society, the 2013 Meeting of Society of Economic Dynamics, the 2013 NBER Summer Institute in Corporate Finance, the 2013 NBER Summer Institute in Monetary Economics, the 2013 Gerzensee ESSFM, and the Barcelona GSE "II Asset Prices and the Business Cycle Workshop" for helpful comments. All remaining errors are our own.

The views and opinions expressed in this paper are those of the authors and do not necessarily represent the views of the Federal Reserve Bank of Boston or the Federal Reserve System.

This version: September 2013

1 Introduction

While ample evidence exists that monetary policy has important effects on real investment, consumption, the valuation of financial assets, and more generally aggregate economic performance, there is no consensus on the precise channels through which the interest rate decisions of central banks bear implications for the real economy.¹ An important strand of the finance and macroeconomics literature has been devoted to the exploration of the role of financial intermediaries in these channels, given their pre-eminent role in the implementation of monetary policy and in the intermediation of aggregate savings.² Many of the empirical studies in this literature use variation across firms as a source of evidence, and their main challenge lies in the identification of the degree of bank dependence of individual firms.

This paper contributes to our understanding of the importance of financial intermediaries for the transmission of monetary policy, by studying whether the stock prices of bank-dependent firms are relatively more responsive to monetary policy shocks than the stock prices of firms that do not depend heavily on banks for financing. We pursue this goal by employing a new, direct measure of bank dependence, computed as the ratio of a firm's bank debt to its total assets, for publicly listed U.S. firms between 2003 and 2008. Our measure of bank dependence, combined with data on bank financial health, bank-firm linkages, firm financial constraints, and a novel database on interest-rate hedging activities, enables us to provide evidence on the channels through which a bank-centered transmission mechanism might operate.

We find that stock prices of more bank-dependent firms react more strongly than stock prices of other firms to monetary policy surprises. In particular, the stock price of a firm with a two standard deviation higher value of our bank dependence measure decreases by about 1

¹Examples are Gertler and Gilchrist (1994), Angeloni, Kashyap, and Mojon (2003), Bernanke and Kuttner (2005), and Boivin and Giannoni (2006).

²Examples are Bernanke and Blinder (1988, 1992), Stein (1998), Kashyap and Stein (2000), Kishan and Opiela (2000), Van den Heuvel (2002), and Bolton and Freixas (2006).

percent more in response to a 1 percentage point surprise increase in the federal funds rate. This effect is economically very significant, as we find that a firm's stock price decreases by about 4 percent on average in response to a 1 percentage point surprise increase in the federal funds rate, meaning that a two standard deviation greater bank dependence strengthens the effect of monetary policy on stock prices by around 25 percent. This result is robust to controlling for firms' leverage, financial constraints, debt maturity, interest sensitivity of operating profits, and asset pricing risk factors. We also confirm the robustness of our results using an endogeneity test guided by asset pricing theory and an analysis of the firm characteristics that drive use of bank debt.

We explore two channels through which bank dependence might influence the sensitivity of stock prices to monetary policy. The first channel, which is typically referred to as the *bank lending channel*, is based on theories arguing that financial frictions faced by banks amplify the impact of monetary policy shocks.³ These theories rely on some failure of the Modigliani-Miller theorem for banks, in the sense that the supply of credit by banks is affected by variations in the strength of their balance sheet, which itself might depend on the stance of monetary policy.⁴

An implicit assumption of the bank lending channel is that some firms are not able to substitute alternative sources of financing at a low cost for their banking relationships.⁵ Therefore, we first provide evidence that bank dependence affects the sensitivity of stock returns to monetary policy surprises more strongly for unrated and young firms, which are more likely to have limited access to alternative sources of finance.

To test for the existence of a bank lending channel, we follow Kashyap and Stein (2000),

³For alternative theories of why banks' financial frictions matter, see Bernanke and Blinder (1988), Bernanke and Gertler (1995), Stein (1998), and Bolton and Freixas (2006), among others.

⁴Kashyap and Stein (2000), Kishan and Opiela (2000), and Jimenez et al. (2012) provide empirical evidence that financially fragile financial intermediaries reduce their supply of loans relatively more following a tightening of monetary policy.

⁵For evidence on this point see Kang and Stulz (2000), Khwaja and Mian (2008), Paravisini (2008), Chava and Purnanandam (2011), and Chodorow-Reich (2013).

Kishan and Opiela (2000), and Jimenez et al. (2012) and use bank size and the bank capital ratio as measures of bank financial health. We find that bank-dependent firms that borrow from banks with weaker balance sheets display a stronger sensitivity to monetary policy shocks, which is consistent with the prediction of the bank lending channel. Moreover, this relation is stronger for unrated and young firms. We address the potential endogeneity in firm-bank relationships by investigating which firm characteristics are relevant for the match between firms and banks. Additionally, we instrument for bank financial health using the instrumental variables approach proposed by Ashcraft (2008), Berger et. al. (2005), and Berger and Bouwman (2013). Both approaches confirm the robustness of our results.

We examine a second channel by which bank lending to firms can be special for the transmission of monetary policy. This second channel is based on the widespread use of floating-rate agreements in bank loans and the prevalence of fixed-rate agreements in nonbank liabilities. As monetary policy changes have a direct impact on the reference rates used in the floating-rate agreements, we expect these changes to be reflected mechanically in the cost of existing bank loans for firms. This suggests that bank debt might be special for the transmission of monetary policy because for bank-dependent firms a variation in federal funds rates is more likely to affect interest rate expenses and therefore profits. We call this channel the *interest rate pass-through channel*. In the presence of financing frictions, the impact of this channel could be amplified by the effect that higher interest expenses have on the firm’s liquidity position and net worth.

We test the interest rate pass-through channel by exploiting the fact that many firms hedge against interest rate risk by converting their floating-rate liabilities to fixed-rate liabilities through the use of interest-rate swaps and other derivative contracts. In particular, we collect data from annual 10-K Securities and Exchange Commission (SEC) filings using a text search (parsing) algorithm to identify which firms engage in interest rate hedging. Our results show that bank-dependent firms that hedge against interest rate risk display a

lower sensitivity to monetary policy shocks. We provide further evidence on the interest rate pass-through channel by showing that for bank-dependent firms the sensitivity of interest rate expenses to changes in monetary policy is lower if firms hedge. Taken together, these results are consistent with a channel that operates through a direct pass-through of policy rates to loan rates for a majority of bank debt users.

Overall, our results suggest that bank lending to firms plays an important role in the transmission of monetary policy, and that there is significant heterogeneity across bank-dependent firms in their reaction to monetary policy shocks. In particular, the sensitivity of stock prices to monetary policy changes is affected by the extent to which a firm uses interest rate hedging, borrows from a financially healthy bank, or has access to other sources of debt financing besides bank borrowing. Our study is important not only for academic economists and policymakers who are interested in the real effects of monetary policy, but also for investors who are interested in the exposure of their stock market portfolios to monetary policy shocks.

The results of our paper are relevant for several strands of the macroeconomics and finance literatures. They contribute to the literature that has studied cross-sectional heterogeneity in the reaction of firms to monetary policy shocks. Gertler and Gilchrist (1994) show that following a monetary policy contraction, small firms' short-term borrowing declines, while large firms' borrowing expands. Furthermore, the performance of small firms, measured by sales and inventories, deteriorates much more than that of large firms. While this evidence is consistent with a bank lending channel because small firms are more likely to be bank dependent, it is also consistent with a broader credit channel in which monetary policy contractions affect the general availability of external finance more for small firms. Ehrmann and Fratzscher (2004) find that the stock prices of small, poorly rated, low cash flow, and high Tobin's q firms in the S&P500 are relatively more responsive to monetary policy. Our paper contributes to this literature by employing a direct measure of bank dependence,

computed as the ratio of a firm’s bank debt to its total assets, that enables us to measure bank dependence precisely and to make statements about the quantitative importance of monetary policy transmission mechanisms that operate through bank lending. Furthermore, we also analyze the heterogeneous effects of monetary policy within the sample of bank-dependent firms and identify the factors that drive this heterogeneity.

Several theories have been put forward to explain why financing frictions that affect banks may influence the transmission of monetary policy to the real economy. In each theory, monetary policy has an effect on banks’ cost of funds beyond the effect of the change in the risk-free rate, leading to an additional response in the supply of intermediated credit (Bernanke and Blinder [1988], Bernanke and Gertler [1995], Stein [1998], Van den Heuvel [2002], and Bolton and Freixas [2006]). In parallel, empirical work has tested the main prediction of these theories, in particular, whether financially fragile financial intermediaries contract their supply of loans relatively more following a tightening of monetary policy. Bernanke and Blinder (1992) find that a tightening of monetary policy leads to a decline in aggregate bank lending activity, and later studies have found that this impact is stronger for small, less liquid, and more leveraged banks (Kashyap and Stein [2000], Kishan and Opiela [2000], and Jimenez, et al. [2012]), and for banks that are not affiliated with multibank holding companies (Ashcraft [2006]). Nevertheless, finding a relationship between monetary policy shocks and bank loan supply related to financing frictions at the bank level is a necessary but not sufficient condition for an operative bank lending channel. It must be also, as our evidence suggests, that firms are unable to costlessly replace bank loans with other nonbank sources of finance. Our paper thus contributes to this literature by evaluating how the financial health of a bank ultimately affects the response of its borrowers to monetary policy shocks.

Addressing the importance of bank dependence, several papers have explored exogenous shocks to bank capital, such as foreign sector shocks, political events, or government policy

changes, and have found that these shocks affect investment spending, capital structure, or the performance of their borrowers significantly (Peek and Rosengren [2000], Kang and Stulz [2000], Khwaja and Mian [2008], Paravisini [2008], and Chava and Purnanandam [2011]). Our results suggest that monetary policy shocks are also able to produce a sufficiently large impact on banks’ balance sheet strength so that bank financial health plays an important role in the transmission of monetary policy.

Finally, most of the previous papers measure firms’ reaction to shocks in bank loan supply using balance sheet variables such as capital expenditures, investment, or variations in bank debt. While informative, these studies might suffer from the problem that these variables are measured a long time after the bank capital shock or monetary policy event occurred, a time during which other shocks and indirect mechanisms might come into play. Most closely related to our paper in this aspect are those studies focusing on the stock price reaction to bank lending supply shocks (Ongena, Smith, and Michalsen [2003], Giannetti and Simonov [2011], and Carvalho, Ferreira, and Matos [2011]), none of which study bank lending supply shocks driven by monetary policy actions.

2 Data Description and Summary Statistics

2.1 Firm-Level Data

Our sample consists of U.S. firms covered by Capital IQ (CIQ), CRSP, and Compustat from 2003 to 2008, excluding utilities (SIC codes 4900–4949) and financials (SIC codes 6000–6999). We focus on this period because we do not have wide coverage of bank debt data before 2003 and there is no widely accepted measure of monetary policy shocks after the federal funds target rate hit the zero lower bound in June 2008. We remove firm-year observations with negative revenues, missing information on total assets, or a value of total assets under \$10 million. We also discard penny stocks, defined as those with a price of

less than \$5, as in Amihud (2002). After the above filters, the sample contains 21,745 firm-year observations comprising 4,408 unique firms. Exact variable definitions are given in the appendix. Following the common practice in corporate finance literature, all variables are winsorized at the 1 percent level in both tails of the distribution to prevent extreme values from overinfluencing our regressions.⁶ Total assets are expressed in terms of year-2000 dollars. Throughout the analysis, we use demeaned firm-level variables for the purpose of facilitating the interpretation of the coefficient estimates in the regressions with interaction terms.

CIQ compiles detailed information on capital and debt structure from the footnotes of 10-K SEC filings. In particular, from CIQ we obtain data on the amount of bank debt that firms have in their liabilities. Our main measure of bank dependence, $BankDebt/At$, is defined as total bank debt, defined as credit lines (CL) plus term loans (TL), divided by the total value of book assets (Compustat item 6). CIQ also reports the amount of undrawn credit lines. Therefore, we also employ a second measure of bank dependence computed as CL plus TL plus undrawn credit lines, divided by the total value of book assets.

Table 1 provides key statistics for the balance sheet variables we employ in our study. Across the entire sample (column 1), bank debt represents on average 5.11 percent of the book value of assets and 36.34 percent of total debt. For the subset of firms with some bank debt (column 2), the above ratios rise to 12.77 percent and 57.01 percent. In both samples, approximately half of bank borrowings are in the form of drawn credit lines and the other half are term loans.⁷

A comparison between columns 2 and 4 reveals that firms with bank debt are similar in size to leveraged firms without bank debt, but are more profitable, more likely to be rated, and more leveraged. Bank debt users are more likely to use floating-rate debt, which represents 10.80 percent of the value of their assets, compared with 2.04 percent for nonbank

⁶See, for example, Fama and French (1992) and Sufi (2009).

⁷The large dependence on bank debt reported in column 2 confirms previous findings on specialization in debt instruments previously examined in Colla, Ippolito, and Li (2013).

debt users. According to Chernenko and Faulkender (2011), the percentage of floating-rate debt represents 41.57 percent of total long-term debt reported in 10-K SEC filings over the period 1993–2003. In our sample, which covers a different set of years, the percentage of floating-rate debt in total debt is 32.71 percent for the entire sample, and 44.82 percent for the sample of bank debt users.

Figure 1 explores in more detail the relation between bank debt and floating-rate debt. Firm-year observations are grouped into deciles of bank debt as a percentage of total debt. On the vertical axis, we report floating-rate debt as a percentage of total debt. The figure shows a striking correlation between bank debt and floating-rate debt. For those firms for which the entire stock of debt consists of bank debt, more than 70 percent of it is floating-rate. For those firms whose debt is entirely from nonbank sources, however, only around 10 percent of debt is floating rate. These figures confirm the findings of Faulkender (2005) according to which 89.9 percent of bank loans are issued with a floating rate, compared to only 7 percent of floating-rate bonds.

2.2 Bank-Firm Matching and Bank Level Data

We calculate the exposure of each firm to the financial health of its lenders as the weighted average of current financial health of the banks a firm has borrowed from in the previous five years. We use syndicated loans and measure the weights as the amount lent by each bank as a share of total lending to the firm during those five years. The construction of this variable proceeds in three steps. First, we calculate a firm-bank-year variable for the volume of new lending each year from each bank to each firm, using the Loan Pricing Corporation’s Dealscan database. Dealscan reports the total amounts for all loans, but only reports the allocations to each bank in the syndicate for around one third of the observations. For this reason, we estimate a model of bank allocations as a function of the status of the bank as lead lender or participant, the number of lead banks, and the number of participant banks. For

consistency, once we have estimated the model, we apply the estimates to all observations, including the ones for which we have data.⁸ Second, we calculate for each firm, year, and bank the share that the bank has in all of the lending done in the previous five years (not including the current year), and we calculate this share using the estimated allocations and the total loan amounts.⁹

Third, we multiply the exposure variable by the financial health of the bank to construct a firm-year variable that captures the weighted average health of the banks that a firm has borrowed from in recent years. To obtain these measures of bank financial health for U.S. banks, we use Call Report information contained in the quarterly Consolidated Financial Statements (form FR Y-9C) for Bank Holding Companies (BHC). These reports have to be filed with the Federal Reserve by all U.S. bank holding companies with total consolidated assets of \$500 million or more. For banks not domiciled in the United States, we use the Bankscope database, in which we identify bank holding companies. We match the Dealscan database to the Call Report and Bankscope data by tracing owner-subsidiary relationships manually.

Our bank financial health proxies are bank size and capital ratio. Bank size is measured as total assets (Call Report item bhck2170, Bankscope item data 2025), expressed in terms of year-2000 dollars. Capital ratio is measured as Tier 1 capital divided by total risk-weighted assets (Call Report item bhck8274 divided by item bhcka223, Bankscope item data 18150).

2.3 Interest Rate Hedging Data

We collect data on interest rate hedging activities of U.S. firms using a text-search algorithm that scans 10-K corporate filings with the SEC. Disclosure of derivative hedging is

⁸The model provides a good fit for the sample for which we have lending allocations. The R^2 of the regression explaining the allocation of lead lenders using the number of leads and the number of participant banks as explanatory variables is 0.34.

⁹Our strategy to match banks to firms is supported by the evidence in Chodorow-Reich (2013), who shows that lending relationships are persistent.

mandatory under the 1998 Financial Reporting Release (FRR) No. 48 of the SEC and the 2001 SFAS No. 133. We do a detailed search of multiple phrases consistent with the use of interest rate derivatives (such as “hedge against interest rate,” “hedge interest rate,” “interest rate swap”), and then, for those filings for which we have a preliminary reading consistent with interest rate hedging, we check for false positives by controlling for negations, such as “not use any interest-rate swaps,” “not use interest-rate swaps,” “not currently use any interest-rate swaps,” “not hedge interest rate,” “not use derivative financial instruments as a hedge against interest rate,” “termination of interest rate swap,” “fixed to floating interest rate swap,” or “do not currently use interest rate swap.”

The following two paragraphs are examples of the type of discussion on hedging activities that we find in the 10-K files. In fiscal year 2008, BioFuel Energy Corp reports as follows:

We are subject to interest rate risk in connection with our bank facility. Under the facility, our bank borrowings bear interest at a floating-rate based, at our option, on LIBOR or an alternate base rate. (...). In September 2007, the Operating Company, through its subsidiaries, entered into an interest rate swap for a two-year period. The contract is for \$60.0 million principal with a fixed interest rate of 4.65%, payable by the Operating Company and the variable interest rate, the one-month LIBOR, payable by the third party.

Similarly, in fiscal year 2006 Netsmart Technologies reports:

In October 2005, we entered into a revolving credit and term loan agreement with the Bank of America (...). This financing provides us with a five-year term loan of \$2.5 million. The term loan bears interest at LIBOR plus 2.25%. We have entered into an interest rate swap agreement with the Bank for the amount outstanding under the term loan whereby we converted our variable rate on the term loan to a fixed rate of 7.1% in order to reduce the interest rate risk associated with these borrowings.

2.4 Monetary Policy Data

Because the equity market will already have responded to anticipated policy actions, we follow the approach of Kuttner (2001) and Bernanke and Kuttner (2005) to dissect

the monetary policy actions into the unexpected (*surprise*) component and the anticipated (*expected*) component. The identification of the surprise element in the target rate change relies on the price of the current month 30-day federal funds futures contracts, a price that encompasses market expectations of the effective federal funds rate. We follow this method because federal funds futures outperform target rate forecasts based on other financial market instruments or based on alternative methods, such as sophisticated time series specifications and monetary policy rules.¹⁰ Another advantage of looking at one-day changes in near-dated federal funds futures is that federal funds futures do not exhibit predictable time-varying risk premia (and forecast errors) over daily frequencies.¹¹

Following Bernanke and Kuttner’s analysis, we define an event as either an FOMC meeting or an announced change in the funds target rate. Kuttner (2001) and Bernanke and Kuttner (2005) obtain the corresponding surprise change in the target rate by first calculating the change in the rate implied by the corresponding futures contract, given by 100 minus the futures contract price, and then scaling this result by a factor associated with the number of days of the month in which the event occurred, because the payoff of the contract is determined by the average realized federal funds effective rate during the month. Accordingly, the unexpected target rate change, for an event taking place on day d of month m , is given by

$$\Delta i^u = \frac{D}{D-d}(f_{m,d}^0 - f_{m,d-1}^0),$$

where $f_{m,d}^0 - f_{m,d-1}^0$ is the change in the current-month implied futures rate, and D is the number of days in the month. To suppress the end-of-month noise in the federal funds rate, the unscaled change in the implied futures rate is used as the measure of target rate surprise when the event occurs on the last three days of a month. If the event happens on the first day of a month, $f_{m-1,D}^1$ is used instead of $f_{m,d-1}^0$. The expected federal funds rate change is

¹⁰See Evans (1998) and Gürkaynak, Sack and Swanson (2005) for details.

¹¹See, for example, Piazzesi and Swanson (2008).

defined as the difference between the actual change minus the surprise:

$$\Delta i^e = \Delta i - \Delta i^u,$$

where Δi is the actual federal funds rate change. The data for the decomposition of the federal funds target rate changes can be obtained from Kenneth Kuttner’s webpage.¹²

2.5 The Reaction of Equity Prices to Changes in the Target Federal Funds Rate

In this section, we compare the reaction of stock prices to monetary policy shocks in our sample and the CRSP universe. We start with the reaction of the aggregate CRSP value-weighted index between February 1994 and June 2008. Following Rigobon and Sack (2005), we focus on this period for two reasons. First, starting in February 1994, the FOMC’s policy of announcing target rate changes at pre-scheduled dates virtually eliminated the timing ambiguity associated with rate changes prior to this date. Second, after June 2008, the Federal Reserve switched from announcing a specific target rate to announcing a range for the target rate.

Following Bernanke and Kuttner (2005), we use the CRSP value-weighted index in order to identify outliers, defined as those event dates at which Cook’s D-statistic exceeds 0.1. We drop these outliers from our analysis. Our list of outliers for the 1994–2002 period matches those in Bernanke and Kuttner (2005), that is, October 15, 1998, January 3, 2001, March 20, 2001, April 18, 2001, and September 17, 2001, which are discussed in their paper in detail. We also identify two additional outliers for the post-2002 period, January 22, 2008, and March 18, 2008, both of which are characterized by very large rate cuts. On January 21, 2008, in response to deteriorating market conditions, the Federal Open Market Committee (FOMC)

¹²<http://econ.williams.edu/people/knk1/research>

held an unscheduled meeting (conference call) despite the national holiday (Martin Luther King day). They decided on a rate cut of 75 basis points, which they announced shortly before the opening of U.S. markets. Although the rate cut was almost entirely unexpected, with an unprecedented *surprise* of -74 basis points, stock prices declined by almost 100 basis points compared to their closing price before the holidays. Shortly after, on March 18, 2008, the FOMC announced another unusually large cut in the federal funds rate (-75 basis points) in response to turmoil in the markets and the collapse of Bear Stearns. Stocks rallied in response, although the federal funds futures data suggested that some market participants had expected an even larger rate cut (about 100 basis points).

Table 2 offers a comparison between the responses of equity prices to federal funds rate changes in different samples. Columns 1 and 2 show that on the day of an FOMC announcement, a 100 basis points surprise increase in the federal funds rate decreases stock prices by around 300 basis points when we look either at the value-weighted returns or the individual returns of the entire CRSP universe between 1994 and 2008. This result is comparable to the numbers reported in Bernanke and Kuttner (2005). Columns 3 and 4 show that the reaction of equity prices to surprise changes in monetary policy is stronger in the sample of 2003–2008 than in previous years. However, the sign and significance of the coefficient of surprise is the same for both samples. A comparison of columns 4 and 5 reveals that firms in the sample for which we have bank dependence data have a reaction to monetary policy shocks very similar to that of the overall CRSP universe during the 2003–2008 period.

3 Is Bank Debt Special for the Transmission of Monetary Policy?

3.1 Motivation of Empirical Predictions and Empirical Strategy

In this section, we explore whether bank lending to firms is special for the transmission of monetary policy. We start by analyzing whether a firm's stock price change $Ret_{i,t}$ on the day t in which a monetary policy shock $Surprise_t$ occurs depends on the importance of bank debt as a source of financing, $(BankDebt/At)_{i,t-1}$. Formally, we test whether

$$\frac{\partial^2 Ret_{i,t}}{\partial Surprise_t \partial (BankDebt/At)_{i,t-1}} = 0.$$

This is equivalent to testing whether $\beta_3 = 0$ in the following empirical specification:

$$\begin{aligned} Ret_{i,t} = & \beta_0 + \beta_1 Surprise_t + \beta_2 (BankDebt/At)_{i,t-1} \\ & + \beta_3 Surprise_t * (BankDebt/At)_{i,t-1} \\ & + \gamma Controls_{i,t-1} + \lambda Surprise_t * Controls_{i,t-1} + \varepsilon_{i,t}, \end{aligned} \quad (1)$$

where $Controls_{i,t-1}$ is a vector of firm characteristics.¹³ We use the last fiscal year-end data available before the date of the monetary policy event in order to capture the information available to investors at the time of the monetary policy announcement, in line with most of the cross-sectional asset pricing literature dating back at least to Fama and French (1992).¹⁴

¹³Our objective at this stage is to test whether β_3 is significantly different from zero, which would indicate that bank debt is special for the transmission of monetary policy. Even though most theories, as discussed later, predict that bank-dependent firms are more reactive to monetary policy ($\beta_3 < 0$), one could also suggest plausible and empirically motivated theories that would predict the opposite, based on the idea that bank-firm relationships enable firms to benefit from some degree of insurance provided by their lenders against changes in credit availability.

¹⁴With a slight abuse of notation, for firm characteristics, $t - 1$ refers to the most recent fiscal-year end prior to the federal funds rate target announcement date. For *Surprise* and *Ret*, t refers to the monetary policy announcement date, of which there are many in any given year.

Our firm-level controls include book leverage, firm size, market-to-book ratios, profitability, and interest rate sensitivity, which are described in Table A1 in detail. We control for book leverage because bank-dependent firms are more likely to be highly leveraged, and as such might be more sensitive to monetary policy. We control for firm size and market-to-book ratios because these variables are well-known risk factors for asset prices since the seminal paper of Fama and French (1992), and they can also affect the reaction of stock prices to policy surprises because they are related to financial constraints and investment opportunities.¹⁵ Profitability is included because, as shown in Fama and French (1995), the market-to-book ratio is associated with persistent differences in profitability and firms with bank debt tend to be more profitable, as shown in Table 1. Also, Ehrmann and Fratzscher (2004) report strong evidence that firms with low profitability are more responsive to monetary policy when profitability is measured as cash flow divided by income. Finally, we control for the interest rate sensitivity of operating profits because it might influence the propensity to borrow from banks. This would generate a correlation between bank dependence and the reaction of stock prices to monetary policy even if there were no causal relationship between these variables.¹⁶

3.2 Empirical Results

Table 3 presents the results of regression (1) using alternative specifications that become more restrictive across columns. The first column of Table 3 contains the result from a

¹⁵We also add CAPM betas, calculated as in Fama and French (1992), as an additional control in some specifications. Alternatively, we could use Fama-French factors based on these risk characteristics (size, market-to-book, beta) to calculate returns not explained by these factors. We prefer our approach because firm characteristics subsume the effect of the Fama-French risk factors. See, for example, Daniel and Titman (1997) and Ferson and Harvey (1999).

¹⁶Our particular concern is that bank-dependent firms are special in that they are on average riskier and more interest rate sensitive, which would suggest that we may overestimate the direct impact of bank debt. While interest rate sensitivity of operating profits should address these concerns, we look deeper into the relationship between a firm's riskiness and its bank financing behavior in Table A3. Contrary to our concerns, columns 1–3 show that bank debt use is negatively associated with alternative measures of risk, such as interest rate sensitivity of operating profits (insignificant), cash flow volatility, and CAPM beta.

basic random-effects panel regression with no controls and suggests that a two standard deviation (0.21) increase in our bank dependence measure causes the stock price to increase 1.04 ($= -4.955 \times 0.21$) percentage points more in response to a 1 percentage point surprise decrease in the federal funds rate. To put this effect in perspective, the same surprise decrease in the federal funds rate causes the stock price of the firm with the average amount of bank debt over assets (5.11 percent) to increase about 4.23 percent on average.

In order to address potential identification issues, such as nonspherical disturbances and omitted variables, we progressively add controls, industry fixed effects, both interacted and uninteracted, standard errors clustered at the event-industry level, and finally firm fixed effects both interacted and uninteracted. Nonsphericity would primarily affect the standard errors of our estimates rather than their consistency, which is the main reason why we use clustered errors. However, omitted variables can influence our inference by affecting both the standard errors and the consistency of our estimates. Therefore, controls and firm-level fixed effects specifications aim at distinguishing between bank debt being special or bank-dependent firms being special, for reasons that are not captured in our basic regression in column 1 of Table 3.

Column 2 introduces firm controls and year fixed effects, which leads to a significant increase in the estimate of β_3 . This increase seems to be robust to alternative specifications, as shown in columns 3 to 8.¹⁷ In column 3, industry fixed effects enter the regression both interacted with surprise and uninteracted, with industries classified according to Fama-French 48 sectors available from Kenneth French’s website. Column 4 adds error clustering at the event-industry level to address possible time-and-cross-section heteroskedasticity in the errors. Column 5 extends the definition of bank debt to include undrawn credit lines. Column 6 includes interest rate sensitivity, measured as in Faulkender (2005) as the correlation

¹⁷Table A2 in the appendix examines the coefficients of the interaction between *surprise* and firm characteristics across different sample periods. Some coefficients, such as the market-to-book ratio or size appear very stable over time, while the sign of coefficients on profitability or leverage change over time.

between quarterly firm EBITDA and three-month average LIBOR rates, cash flow volatility, and CAPM beta as additional controls. Column 7 replaces industry fixed effects with firm fixed effects. Finally, for completeness, column 8 includes fixed effects interacted with surprise.¹⁸ Overall, we find significant evidence that bank-dependent firms are more responsive to monetary policy shocks.

Although our specifications include controls that might potentially explain the reaction of stock prices to monetary policy, it is possible that we have omitted some other variables that might affect the reaction of stock prices to monetary policy shocks and that are also correlated with our bank dependence measure.¹⁹ Asset pricing theory offers us a solution to this apparent problem and allows us to use the standard Hausman test to compare the random-effects and fixed-effects models to test whether omitted variables are a cause for concern. If there is an omitted variable problem as suggested above, then asset pricing theory tells us that we should expect this variable to affect both a stock's responsiveness to monetary policy shocks and its expected returns. In particular, if a variable makes a stock more responsive to any macroeconomic shock priced by investors, this variable should directly affect the expected return the investors are willing to accept in order to take on the risk associated with that shock. In our context, because monetary policy affects the consumption-savings decisions of investors, it is reasonable to assume that the exposure of a stock to monetary policy shocks is priced by investors. Hence, any variable that makes the stock more responsive to monetary policy should directly affect firms' expected returns.

As a result, asset pricing theory tells us that the omitted variable issue should create a bias not only in the coefficient of bank dependence interacted with the policy surprise, but also in the coefficient of bank dependence. Therefore, we can safely argue that the

¹⁸To perform the interacted fixed-effects estimation of column 8 we resort to standard OLS and the Frisch-Waugh-Lovell theorem, which allows us to reduce the computation time significantly by reducing the number of control variables

¹⁹Because monetary policy surprises are exogenous, our estimator is still consistent if there are omitted variables that are correlated with our bank-dependence measure but do not affect the responsiveness of stock prices to monetary policy shocks. See Nizalova et al. (2012).

former coefficient, the one we are interested in, is free from the omitted variable bias if we can show that the latter coefficient does not suffer from omitted variable bias. We can test this second hypothesis using the standard Hausman test to compare the random-effects (column 2) and fixed-effects models (column 7). We find that the coefficient of our uninteracted bank dependence measure is 0.36 in the fixed-effects model versus 0.33 in the random-effects model, and the difference is statistically insignificant with a t -statistic of 0.3. Therefore, we find evidence that bank-dependent firms are more responsive to monetary policy shocks not because bank-dependent firms are special, but because bank debt is special in the transmission of monetary policy shocks. The Hausman test provides qualitatively similar results when we compare the random-effects model in column 2 with the setting where firm fixed effects are interacted with surprise (column 8), although the estimated coefficient of interest is somewhat larger, -9.71 vs. -6.76 , with a t -statistic of 0.77.

3.3 Robustness: Short-Term Debt

A possible concern in Table 3 is that bank debt may be proxying for the use of short-term debt. This concern finds support in the descriptive statistics reported in Table 1, which shows that bank debt users have a higher percentage of short-term debt than nonbank-debt users (4.14 percent versus 0.87 percent, calculated as a share of total assets). For example, to the extent that changes in monetary policy affect primarily the short end of the yield curve, one can expect firms with a shorter average maturity of debt to be more sensitive to increases in interest rates.

To test this hypothesis, we rewrite the specification provided in equation (1) in terms of short-term debt divided by the book value of assets, $STDebt/At$. Formally, the complete

regression specification is:

$$\begin{aligned}
Ret_{i,t} = & \beta_0 + \beta_1 Surprise_t + \beta_2 (BankDebt/At)_{i,t-1} + \beta_3 (STDebt/At)_{i,t-1} \\
& + \beta_4 Surprise_t * (BankDebt/At)_{i,t-1} + \beta_5 Surprise_t * (STDebt/At)_{i,t-1} \\
& + \gamma Controls_{i,t-1} + \lambda Surprise_t * Controls_{i,t-1} + \varepsilon_{i,t}.
\end{aligned} \tag{2}$$

Table 4 provides the empirical results of this test. Columns 1 and 2 show the results of a version of regression (2) in which the terms containing $(BankDebt/At)_{i,t-1}$ are removed. We observe that the amount of short-term debt in a firm's balance sheet is not significantly associated with the strength of the sensitivity to surprises in the federal funds rate. Columns 3 and 4 provide a complete specification of (2), including bank debt, both interacted and not interacted with *surprise*.²⁰ The coefficient β_5 becomes significant and positive in most specifications, while the coefficient β_4 retains the sign, size, and significance of the specifications reported in Table 3. In columns 5 and 6, we add a triple interaction term $Surprise_t * (BankDebt/At)_{i,t-1} * (STDebt/At)_{i,t-1}$ to measure whether bank-debt is relatively more special when it is short-term, and find no significant evidence consistent with this conjecture.²¹ We conclude that the higher sensitivity of bank debt users to federal funds rate surprises is not due to their higher exposure to short-term debt.

4 Why is Bank Debt Special? (I) The Bank Lending Channel

The results obtained in Section 3 suggest that bank debt is special for the transmission of monetary policy, and our next task is to understand why this may be so. Our results show

²⁰The Pearson pair-wise correlation between these two variables is 0.267.

²¹In this regression specification we also add the associated second order term $(BankDebt/At)_{i,t-1} * (STDebt/Assets)_{i,t-1}$.

that bank debt is special in that firms that use bank debt are more sensitive to monetary policy shifts. We focus on two mechanisms that predict a stronger sensitivity to monetary policy: the bank lending channel, which we analyze in this section, and the interest rate pass-through channel, which is discussed in Section 5.

4.1 Motivation of Empirical Predictions and Empirical Strategy

The bank lending channel is a mechanism whereby monetary policy shocks are amplified by the financial frictions that affect banks (Bernanke and Blinder [1988], Bernanke and Gertler [1995], Stein [1998], and Bolton and Freixas [2006]). This channel relies on the failure of the Modigliani-Miller theorem for the capital structure of banks, in the sense that the supply of credit by banks is affected by the strength of their balance sheet, which itself varies with monetary policy. Bernanke and Blinder (1992), Kashyap and Stein (1995), Kashyap and Stein (2000), Kishan and Opiela (2000), and Jimenez, et al. (2012) provide empirical evidence that financially fragile intermediaries reduce their supply of loans relatively more than peers with stronger balance sheets following a tightening of monetary policy.

We start our analysis by studying whether bank-dependent firms that suffer from financing constraints are relatively more reactive to monetary policy shifts; in other words, whether

$$\frac{\partial^3 Ret_{i,t}}{\partial Surprise_t \partial (Bankdebt/At)_{i,t-1} \partial Frictions_{i,t-1}} < 0,$$

where $Frictions_{i,t-1}$ is a measure of a firm's financing constraints, such that $\partial Frictions_{i,t-1} > 0$ captures a worsening of financing frictions. This analysis is motivated by the intuition that the bank lending channel should be stronger if bank borrowers find it relatively more difficult to switch lenders.²² For a given level of bank dependence (as measured by bank use),

²²The evidence from earlier literature supports this part of the mechanism by showing that some borrowers are affected by their banks' financial health. See, for example, Paravisini (2008), Khwaja and Mian (2008), Chava and Purnanandam (2011), and Jimenez, et al. (2012).

financing frictions can capture the potential heterogeneity across firms in their inability to substitute bank debt with alternative sources of financing. We use firm size, age, and credit rating as proxies for financing frictions, following the existing literature (Almeida, Campello, and Weisbach [2004], and Hadlock and Pierce [2010]). The specification we use for this test is

$$\begin{aligned}
Ret_{i,t} = & \beta_0 + \beta_1 Surprise_t + \beta_2 (Bankdebt/At)_{i,t-1} + \beta_3 Frictions_{i,t-1} \\
& + \beta_4 Surprise_t * Frictions_{i,t-1} * (Bankdebt/At)_{i,t-1} \\
& + (2nd\ order\ interactions) \\
& + \gamma Controls_{i,t-1} + \lambda Surprise_t * Controls_{i,t-1} + \varepsilon_{i,t},
\end{aligned} \tag{3}$$

and our coefficient of interest is β_4 , which we predict is negative.

The bank lending channel also predicts that the sensitivity of bank-dependent firms to monetary policy changes should be stronger if a firm's lender is financially weak. Denoting $BankHealth_{i,t-1}$ as the average financial health of the banks from which firm i is borrowing, this mechanism would predict that

$$\frac{\partial^3 Ret_{i,t}}{\partial Surprise_t \partial (BankDebt/At)_{i,t-1} \partial BankHealth_{i,t-1}} > 0.$$

We test this prediction using the following regression specification:

$$\begin{aligned}
Ret_{i,t} = & \beta_0 + \beta_1 Surprise_t + \beta_2 (BankDebt/At)_{i,t-1} + \beta_3 BankHealth_{i,t-1} \\
& + \beta_4 Surprise_t * (BankDebt/At)_{i,t-1} * BankHealth_{i,t-1} \\
& + (2nd\ order\ interactions) \\
& + \gamma Controls_{i,t-1} + \lambda Surprise_t * Controls_{i,t-1} + \varepsilon_{i,t}
\end{aligned} \tag{4}$$

Our choice of proxies for bank financial health draws on the existing literature. Previous work has shown that monetary policy generates a stronger loan supply contraction by small banks (Kashyap and Stein [1995] and Kashyap and Stein [2000]) and by banks with lower capital ratios (Kishan and Opiela [2000] and Jimenez, et al. [2012]). Accordingly, we take bank size and capital ratios as proxies for bank health. In specification (4) the coefficient of interest is β_4 , which we predict to be positive.

For both specifications (3) and (4), we run a series of different regressions including fixed effects at the firm, industry, and year level, and clustering of errors at the firm, industry, and date level. Among the controls we include leverage, firm size, profitability, and the market-to-book ratio, all entering the regression independently as well as interacted with *surprise*.

4.2 Empirical Results

We start by providing evidence on whether bank-dependent firms that are financially constrained are relatively more reactive to changes in monetary policy than bank-dependent firms that are not financially constrained. The results are displayed in Table 5 and are consistent with the predictions of the bank lending channel. In particular, we find that bank dependence is not associated with significantly different stock price responses for rated firms or for firms in the top quartile of age. However, for the subsample of unrated firms, a two standard deviation (0.21) increase in bank dependence causes the stock price to decrease 1.54 ($= 7.321 \times 0.21$) percentage points more in response to a 1 percentage point increase in policy surprise. Similarly, a 1 percentage point increase in policy surprise leads to a stock price decrease that is 2.60 ($= 12.371 \times 0.21$) percentage points higher for firms in the bottom quartile of age. A similar, but statistically insignificant, result holds for small firms relative

to large ones.²³

In Table 6, we test whether the reaction of the stock price of bank-dependent firms to changes in monetary policy is affected by the financial health of their lenders. In Panel A, we proxy for a bank’s financial health using its size. Consistent with the predictions of the bank lending channel, we find that bank-dependent firms that borrow from larger banks are less responsive to monetary policy than bank-dependent firms that borrow from smaller banks. For the set of firms that borrow from banks in the bottom quartile of the size distribution, a two standard deviation (0.22) increase in bank dependence causes the stock price to decrease 1.76 ($= 7.812 \times 0.22$) percentage points more in response to a 1 percentage point increase in policy surprise.²⁴ For the set of firms borrowing from banks in the top quartile of the size distribution, the same increase in bank dependence causes the stock the price to *increase* by 1.12 ($= (12.790 - 7.812) \times 0.22$) percentage points, following a 1 percentage point increase in policy surprise.

Panel B displays the results of the same regressions when we use bank capital ratios as the proxy for bank financial health. These results are also in line with the predictions of the bank lending channel, but statistically weaker. While an increase in bank dependence results in a stronger reaction to monetary policy shocks for firms borrowing from banks with a capital ratio in the bottom quartile, an increase in bank dependence results in a weaker reaction for those firms borrowing from banks with a capital ratio in the top quartile.²⁵

As a deeper analysis of our results, we also study whether bank-dependent firms that

²³There are variations in the sample size across the columns of Table 5. In columns 3–6 we compare the first and fourth quartiles of each financial constraint measure, and this is why the number of observations is lower than in columns 1 and 2. In columns 3 and 4, the number of observations is even lower because the age variable is missing in Compustat for a large number of firms.

²⁴Note that the standard deviation of bank dependence in the sample with bank health measures is slightly larger than the one obtained in the entire sample.

²⁵There is also evidence that less liquid banks react more to monetary policy shocks, most notably in Kashyap and Stein (2000) and Jimenez, et al. (2012), although in the latter they find that the effect is weak. In unreported tests, we do not find the above effect of liquidity in our sample. This may be due to the decreased importance of liquidity reserve requirements for banks in developed economies in favor of capital requirements. The sample period used in Kashyap and Stein (2000) is 1976–1993, a period during which reserve requirements still played a role, particularly in the early part of the sample.

are less able to substitute alternative sources of debt financing for bank debt are relatively more affected by the health of their lenders. In particular, we run the same regressions in Table 7 after sorting firms into two groups, depending on whether they have credit ratings and whether they are young or old. In Panel A, we observe that bank health, proxied by bank size, affects bank-dependent firms' responsiveness to monetary policy only if these firms are either unrated or young. Rated and old firms do not display such an effect. In fact, these firms seem to be more responsive to monetary policy shocks if they borrow from large banks, when compared to firms not dependent on banks, although the difference is statistically insignificant. In Panel B, in which bank health is proxied by the bank capital ratio, we observe similar results when comparing across rated and unrated firms, although the results are statistically insignificant. The same pattern does not appear, however, when we sort firms according to age, in part possibly because of the limited availability of data about firm age, which causes a large loss in sample size.

4.3 Robustness

The estimates of our coefficients of interest could suffer from endogeneity biases, mainly because of omitted variables bias due to endogenous matching of banks to firms. In particular, bank financial health might influence the matching of firms to banks along the dimension of sensitivity to monetary policy. It could be that large and well-capitalized banks specialize in riskier borrowers, as they are more capable of coping with such risks. Or, in an opposite fashion, it could be that certain banks adopt conservative risk management practices that include high capitalization and the choice of safe borrowers. Either way, this endogenous matching would directly bias the coefficient estimate on the double interaction term $Surprise_t * BankHealth_{i,t-1}$.

More importantly, to the extent that the size of the bias depends on the degree of bank dependence of firms, the coefficient estimate on the triple interaction term $Surprise_t *$

$(Bankdebt/At)_{i,t-1} * BankHealth_{i,t-1}$, which is our coefficient of interest, could also be biased. One plausible and problematic story that could explain our results for both the double and triple interaction terms could go as follows: large and well-capitalized banks choose riskier borrowers on average (negative and significant coefficient on $Surprise_t * BankHealth_{i,t-1}$), but when facing borrowers that demand large loans or are expected to draw down heavily on credit lines, they become relatively more conservative (positive and significant coefficient on $Surprise_t * (Bankdebt/At)_{i,t-1} * BankHealth_{i,t-1}$). Even in the absence of this, the endogeneity of the double interaction term $Surprise_t * BankHealth_{i,t-1}$ could render other coefficient estimates inconsistent.

To deal with these endogeneity concerns, we first evaluate the existence of endogenous matching of banks to firms on the basis of observable firm characteristics, and we later provide an instrumental variable estimation analysis.

4.3.1 Endogenous Bank-Firm Matching Analysis

In this subsection, we want to evaluate the impact of bank financial health on bank-firm matching, and how the association of bank financial health with risk-related firm characteristics changes with bank dependence. More precisely, we want to study whether financially healthier (larger or better capitalized) banks are relatively more likely to be associated with less risky and less interest rate sensitive firms when these firms become more bank dependent. Such a pattern would introduce a bias that could account for the findings reported in Section 4.2.

Accordingly, we study how bank financial health, proxied by bank size and by the bank capital ratio, is associated with risk-related firm characteristics in Table 8. In columns 1–3, we regress bank size on firm characteristics and include bank dependence interacted with all firm characteristics. We do not observe that larger banks are relatively more likely to become associated with less risky firms when these firms become more bank dependent. If anything,

in column 1 we observe that when firms increase their ratio of bank debt to total assets, larger banks are more closely associated with firms with higher cash flow volatility. Thus, in our findings there is no evidence supporting a type of endogenous matching of banks to firms that might introduce a problematic bias in our estimates in Tables 6 and 7. The case is not as clear when we study the capital ratio as a measure of bank health in the regressions in columns 4–6. We find significant evidence that better capitalized banks are more likely to be matched to more interest rate sensitive firms as they become more bank dependent, although the results reverse when we measure risk using CAPM beta.

4.3.2 Instrumental Variables Estimation

As described above, we suspect that there are two variables that may suffer from an endogeneity bias due to a problem of omitted variables, a double and a triple interaction term. Our instrumental variables strategy is based on the idea that if we find a valid instrument for bank health, then the double and triple interaction terms using such an instrument are valid instruments for the potentially endogenous double and triple interaction terms included in the regression. We proceed below to describe the IV analysis for bank size and Tier 1 capital ratios separately.

Bank Size IV Estimation We follow Berger et al. (2005) by introducing the log median size of banks in the Metropolitan Statistical Area (MSA) or rural county where a firm is located ($\ln MSAP50$) as an instrument for the size of the bank(s) lending to a firm. The rationale for this measure is that a firm is more likely to form lending relationships with banks located closer to it (relevance condition), but that the median size of banks in a region does not directly affect daily stock returns of firms (exclusion restriction). This reasoning for the relevance and exclusion conditions of this instrument choice is assumed to extend to the double and triple interaction terms. More formally, to check for concerns about the

potential weakness of our instruments, we calculate the first-stage Cragg-Donald F-statistics for the first-stage regressions for the double and triple interaction terms, which are always above 100 in both, and we also calculate the t -statistics for the excluded instruments in the respective first-stage regressions, both of which have associated p-values always lower than 0.001.

Given that OLS is preferred to instrumental variable regressions if the endogeneity problem is absent, we perform Durbin-Wu-Hausman tests for endogeneity and find that we cannot reject the null of exogeneity of the potentially endogenous double and triple interaction terms. In column 1 of Table 9, we report the results of the IV estimation for bank size and find that the coefficient on the triple interaction term is positive and highly significant, suggesting that there might be a strong negative bias in the OLS regression estimates for the triple interaction term.²⁶ These results provide robustness to the evidence in favor of the presence of a bank lending channel of monetary policy in which bank-dependent firms that borrow from financially sound banks are less responsive to monetary policy than those borrowing from less healthy banks.

Bank Capital Ratio IV Estimation We follow Ashcraft (2008) and Berger and Bouwman (2013) and introduce the corporate income tax rate in the state in which a firm is located (tax) as an instrument for the capital ratio of the bank(s) lending to a firm. The rationale for this measure is that a firm is more likely to form lending relationships with banks located closer to it, and that because interest on debt is tax deductible, but dividends are not, banks located in states with higher income tax rates are more likely to have lower capital ratios (relevance condition). On the other hand, a corporate income tax rate in a

²⁶The large difference in the size of the estimated coefficients could also be due to other reasons. It could be that the OLS estimates are biased towards zero because of measurement error, and that the IV estimates suffer less from this problem. It could also be that there are heterogeneous effects in our main variable of interest, whose average changes substantially because of the use of an instrument. The impact of heterogeneous effects could also be different in the IV regressions because of the loss in sample size due to limited data availability for the instrument.

state is unlikely to directly affect the stock returns of firms in that state (exclusion restriction). This reasoning for the relevance and exclusion conditions of this instrument choice is assumed also to extend to the double and triple interaction terms. More formally, to check for concerns about potential weakness of our instruments, we calculate the first-stage Cragg-Donald F-statistics for the first-stage regressions for the double and triple interaction terms, which are 4.9 and 13.9, respectively, and we also calculate the t -statistics for the excluded instruments in the respective first-stage regressions, both of which have associated p -values always lower than 0.001. We perform Durbin-Wu-Hausman tests for endogeneity and find that we cannot reject the null of exogeneity of the potentially endogenous double and triple interaction terms.

In column 2 of Table 9, we report the results of the IV estimation and find that the coefficient on the triple interaction term is positive and highly significant, in line with the OLS estimates, although the size of the coefficient estimate is much larger, as in the case of the bank size IV estimates.²⁷ These results lend further support to the role of bank health in influencing the transmission of monetary policy to firms.

5 Why is Bank Debt Special? (II) The Interest Rate Pass-Through Channel

5.1 Motivation of Empirical Predictions and Empirical Strategy

An important empirical regularity is the widespread use of floating-rate agreements in bank loans and the prevalence of fixed-rate agreements in nonbank liabilities. Floating

²⁷The reasons driving the large size of the IV estimate are probably similar to the ones we discussed for the case of bank size. It is worth noting that the explanation for the difference in size based on a larger problem of measurement error in the OLS estimates is supported by the observation that the increase in size in the coefficients is larger when we instrument for capital ratios, which is more likely to be biased by measurement error given the relatively large accounting management possibilities bank managers enjoy, impacting this variable's precision.

interest rates, also known as variable or adjustable rates, are typically calculated as a spread over a reference rate, such as LIBOR or the prime rate. In our sample (see Figure 1), 72 percent of debt is floating rate for those firms whose entire stock of debt consists of bank debt. For those firms whose debt is entirely from nonbank sources, however, only around 10 percent of debt is floating rate. In a sample of syndicated bank loans for the same period of our analysis, practically all of the loans are floating rate. Using data for U.S. firms in the chemical industry between 1994 and 1999, Faulkender (2005) finds that 89.9 percent of bank loans are issued with a floating rate, compared with only 7 percent of bonds being floating rate.

The floating-rate nature of most bank debt suggests that monetary policy actions, to the extent that they induce changes in the reference rates used in the floating-rate agreements, should be reflected mechanically in the cost of existing bank loans for firms. This means that bank debt might be special for the transmission of monetary policy because variations in policy rates are more likely to result in variation in the firm's interest expense and, as a result, in its profits. We call this channel the *interest rate pass-through channel*. In the absence of financing frictions, the impact on the firm's value would be the present value of the variation in interest expense. In the presence of financing frictions, the impact could be amplified through the effect of variations in the interest expense on the firm's liquidity position and net worth and, ultimately, on its ability to finance profitable investment projects.

Our empirical strategy provides evidence for the pass-through channel by exploiting the variation across bank-dependent firms in their use of floating-to-fixed interest rate hedging. Table 1 shows that this type of hedging is very common. Across the entire sample, 29.90 percent of firms engage in floating-to-fixed hedging activity. This percentage increases significantly to 44.23 percent for the subsample of bank debt users, and is much lower (15.64 percent) for nonbank debt users. Faulkender (2005) finds that 19.1 percent of the bank loans in his sample are swapped from floating rate to fixed rate. The prediction of the pass-through

channel is that bank-dependent firms that have hedged against interest rate risk should be, all else being equal, less responsive to monetary policy shocks.

The firm’s decision whether to hedge or not is clearly endogenous, however, and our choice of controls addresses possible biases arising from this endogeneity. Several reasons have been put forward for the widespread use of hedging instruments by firms, and most have to do with the presence of financing constraints. If firms face costs of raising external financing, they may find it optimal to hedge against low cash flow realizations to avoid having to forgo positive net-present-value (NPV) projects (Froot, Scharfstein, and Stein [1993]) or to avoid nonlinear costs of financial distress (Stulz [1984]).²⁸ These theories predict that hedging activities are positively related to the severity of financing constraints. The empirical evidence, however, does not provide support for this prediction and has documented that firms that are more likely to face financial constraints, such as small firms, are *less* likely to manage risk (Stulz [1996]).²⁹ Motivated by these findings, Rampini, Sufi, and Viswanathan (2013) introduce and test a theory that suggests there is a trade-off between hedging and financing, because both activities compete for the same collateral. In equilibrium, firms that are more financially constrained hedge less. The important role of financing constraints in firms’ willingness and ability to hedge suggests that one should control for financial constraints and how these constraints interact with both the ability to raise debt and to hedge. Accordingly, we control for several measures of financial constraints, both not interacted and interacted with monetary policy surprises and bank dependence.

We test the prediction that bank-dependent firms that hedge against interest rate risk should be, all else being equal, less responsive to monetary policy shocks, by using the following empirical specification:

²⁸Other motivations for the use of hedging have to do with corporate governance and managerial incentives (Chava and Purnanandam [2007]), and with market timing (Faulkender [2005]). More generally, the value creation of hedging has been examined by Nance, Smith, and Smithson (1993), Mian (1996), and Graham and Rogers (2002).

²⁹Column 6 of Table A3 shows that this is the case also in our sample. Larger, rated, and more profitable firms are more likely to hedge than their smaller, unrated, or less profitable counterparts.

$$\begin{aligned}
Ret_{i,t} = & \beta_0 + \beta_1 Surprise_t + \beta_2 Surprise_t * (BankDebt/At)_{i,t-1} + \beta_3 Surprise_t * Hedge_{i,t-1} \\
& + \beta_4 Surprise_t * (BankDebt/At)_{i,t-1} * Hedge_{i,t-1} \\
& + \beta_5 Surprise_t * (BankDebt/At)_{i,t-1} * FinConstraint_{i,t-1} \\
& + \beta_6 Surprise_t * FinConstraint_{i,t-1} \\
& + \beta_7 (BankDebt/At)_{i,t-1} + \beta_8 Hedge_{i,t-1} + \beta_9 FinConstraint_{i,t-1} \\
& + \beta_{10} Hedge_{i,t-1} * (BankDebt/At)_{i,t-1} \\
& + \beta_{11} FinConstraint_{i,t-1} * (BankDebt/At)_{i,t-1} \\
& + \gamma Controls_{i,t-1} + \lambda Surprise_t * Controls_{i,t-1} + \varepsilon_{i,t}..
\end{aligned} \tag{5}$$

The coefficient of interest is β_4 , and our prediction is that it is positive and significant, while we still expect to observe a negative and significant value for β_2 . The simultaneous significance of these two coefficients with opposite signs would indicate that while being exposed to bank debt increases the sensitivity of a firm to monetary policy changes, by engaging in hedging activities firms can reduce the effects of such exposure. Quantitatively it is interesting to assess whether the effect of hedging is strong enough to outweigh that of bank debt. If, conditional on hedging, there is no effect of bank debt on the relationship between equity prices and interest rate changes, this would suggest that most of the higher sensitivity to interest rates associated with the use of bank debt is effectively operating via the floating-rate nature of this type of debt.³⁰

³⁰One important question is why many firms finance themselves with floating-rate debt and then subsequently hedge the interest rate risk associated with floating interest rates, instead of issuing fixed-rate debt. One possible argument is based on the well-documented fact that financially constrained firms are more likely to rely on bank finance, which is mostly floating rate, rather than bond finance, which is mostly fixed rate (see columns 1–3 of Table A3 in the appendix, according to which smaller and unrated firms are more likely to borrow from banks, and also Houston and James (1996), Cantillo and Wright (2000), Hadlock and James (2002), and Denis and Mihov (2003)). One can then argue that insofar as some firms prefer fixed-rate debt but cannot access the bond market, we should expect these firms to swap their floating rate for a

5.2 Empirical Results

The results of our tests are provided in Table 10 and are consistent with our predictions. In Panel A, we begin with a simplified version of the specification provided in (5), where we exclude the triple interaction terms and the financial constraints. In columns 1 and 2, we examine the sample of hedgers, and in columns 3 and 4 the sample of nonhedgers. We find that a *surprise* increase of 1 percentage point in the federal funds rate causes the value of a firm with average bank debt (0.104 in column 1 and 0.038 in column 3) to drop by 5.88 percent and 4.85 percent, respectively. In column 1, the nonsignificance of the interaction term for $Surprise * (BankDebt/At)$ indicates that the relation between bank debt and the sensitivity to federal funds rate changes disappears when a firm swaps its interest rates from floating to fixed. On the contrary, for nonhedgers, the interaction term of bank debt with policy surprise is negative and significant, suggesting that, within this subsample, firms that rely more on bank finance are more sensitive to interest rate changes. More precisely, for the set of nonhedgers the interaction term shows that the effect of a two standard deviation increase in bank dependence (0.264), on the response to a 1 percentage point increase in *surprise* is a further drop in the value of equity of 1.56 percent.³¹

Panel B contains a set of estimates for the full model provided in equation (5) using

fixed rate. Yet another question is why bank credit is mostly floating rate, while nonbank debt is mostly fixed rate. Banks might have a preference for floating-rate loans due to regulatory reasons associated with required capital ratios. Relatedly, given that a large portion of bank liabilities is short term (deposits and wholesale financing for example), and hence is particularly sensitive to short-term interest rates (Landier, Sraer and Thesmar (2013)), banks achieve a better hedging of interest rate risk on their liabilities by issuing floating-rate loans on their asset side.

³¹In columns 2 and 4 we replace bank dependence with $FloatingRateDebt/At$, which is the ratio of floating rate debt to total assets. In line with our previous results, we also find that the impact of floating rate debt use on the responsiveness to monetary policy is greater for firms that do not hedge interest rate risk, although the difference is not statistically significant. Given that the floating-rate nature of bank debt is the driver of the pass-through channel, we would have expected that a direct measure of floating-rate debt use, irrespective of whether it is bank or nonbank debt, would have a significant impact on the sensitivity to monetary policy. Because most bank debt is floating rate, the additional information contained in our floating-rate variable is the presence of floating-rate bonds. The low statistical significance could then be due to the possibility that floating-rate nonbank debt is special in some way, for example, because it captures very short-term instruments such as commercial paper, or because most floating-rate bond users swap their floating rates for fixed rates, so that most bonds end up paying a fixed rate.

different measures of financial constraints based on rating, size, and age (Almeida, Campello, and Weisbach [2004], Hadlock and Pierce [2013]). The coefficient β_2 for $Surprise*(BankDebt/At)$ is negative and significant, indicating that the equity prices of bank debt users are more sensitive to federal funds rate changes. At the same time, β_4 , the coefficient for the triple interaction term $Surprise * (BankDebt/At) * Hedging$, is positive and significant (in two out of three cases) after controlling for the way financial constraints interact with the exposure to bank debt, which is consistent with our prediction that bank-dependent firms that hedge against interest rate risk should be, all else being equal, less responsive to monetary policy shocks.

In Table 11, we examine how monetary policy affects the cost of firms' debt, and how this effect varies across firms depending on their bank dependence and their hedging activity. We compute the average cost of debt as the ratio of interest expenses (Compustat item 15) to total debt (Compustat item 9 plus item 34) at the end of the previous year. The prediction that we want to test is that the cost of debt should be more sensitive to changes in the federal funds rate for firms that are financed with bank debt, but less so if they hedge their interest rate risk. For this purpose, we set up a regression specification that is analogous to that provided in equation (5) (excluding financial constraints), where policy *surprise* is replaced by the actual *change* in the federal funds rate and the dependent variable is the average cost of debt in the year following the one in which the monetary policy event takes place. Column 1 of Table 11 shows that the average cost of debt is positively and significantly related to *change*. Columns 2 and 3 show that bank debt use is weakly positively associated with the sensitivity of the cost of debt to *change*. Once we control for hedging in column 4, the coefficient for bank debt users becomes positive and significant, suggesting that users of bank debt that do not hedge have a cost of debt that is significantly more sensitive to changes in the federal funds rate. At the same time, the triple interaction term of $Change * (BankDebt/At) * Hedging$ is negative and significant, indicating that hedgers

have a cost of debt that is relatively less sensitive. These findings confirm and complement the results provided in Table 10 by providing further evidence consistent with the proposed mechanism of the interest rate pass-through channel.

6 Conclusion

Our analysis concludes that bank lending to firms plays an important role in the transmission of monetary policy. We show that stock prices of bank-dependent firms are significantly more responsive than stock prices of other firms to monetary policy shocks. We also find evidence that supports two channels consistent with this pattern. The first channel is the traditional bank lending channel, which argues that financial constraints of banks amplify the impact of monetary policy because monetary policy affects the strength of banks' balance sheets. The second and new channel is the interest rate pass-through channel, based on the direct pass-through of policy rates to lending rates. Our results also suggest that there is significant heterogeneity across bank-dependent firms in their reaction to monetary policy shocks.

Our paper is the first that uses bank debt as a measure of bank dependence to analyze the impact of monetary policy on firms. This precise and quantifiable measure of bank dependence allows us to make quantitative assessments about the transmission mechanisms we analyze. Moreover, we combine this measure with the financial health of firms' lenders and with firms' hedging activities to create a rich database that allows us to explore the different transmission mechanisms of monetary policy that operate through bank lending in detail.

We contribute to the macroeconomics literature by providing new evidence on the mechanisms through which monetary policy has implications for the real economy. We also contribute to the corporate finance literature by increasing our understanding of the determinants

and implications of different capital structure choices, and of the consequences of risk management decisions. Moreover, by showing that bank financial health can have an important influence on its borrowers' sensitivity to monetary policy, we contribute to the understanding of the banking literature about banks' sensitivity to monetary policy. Finally, our results are relevant to researchers and investors interested in asset pricing, and in particular to those interested in understanding how monetary policy shocks affect stock prices.

While we address several questions simultaneously, significant room remains for additional research that extends our approach. A direct extension can focus on the period of unconventional monetary policy that began in 2008 and the effect of unconventional policy on the bank lending and interest rate pass-through channels. We believe this to be a very important question, among other reasons because it would address one of the main criticisms of the current monetary policy, which is that banks have been hoarding cash in their reserve accounts rather than originating new loans. We leave this extension for future research, pending a reliable measure of monetary policy surprises in the period of unconventional monetary policy.

References

- [1] Almeida, Heitor, Murillo Campello, and Michael S. Weisbach. 2004. "The Cash Flow Sensitivity of Cash." *Journal of Finance* 59: 1777–1804.
- [2] Amihud, Y. 2002. "Illiquidity and Stock Returns: Cross-Section and Time-Series Effects." *Journal of Financial Markets* 5: 31–56.
- [3] Angeloni, Ignazio, Anil K. Kashyap, and Benoit Mojon, ed. 2003. Cambridge University Press *Monetary Policy Transmission in the Euro Area*.
- [4] Ashcraft, Adam. 2006. "New Evidence on the Lending Channel." *Journal of Money, Credit, and Banking* 32: 751–776.
- [5] Ashcraft, Adam B. 2008. "Does the Market Discipline Banks? New Evidence from Regulatory Capital Mix." *Journal of Financial Intermediation* 17: 543–561.
- [6] Berger, Allen N., and Christa HS, Bouwman. 2013. "How Does Capital Affect Bank Performance during Financial Crises?" *Journal of Financial Economics* 109: 146–176.
- [7] Berger, Allen N., Nathan H. Miller, Mitchell A. Peterson, Raghuram G. Rajan, Jeremy C. Stein. 2005. "Does Function Follow Organizational Form? Evidence from the Lending Practices of Large and Small Banks." *Journal of Financial Economics* 76: 237–269.
- [8] Bernanke, Ben S., and Alan S. Blinder. 1988. "Credit, Money, and Aggregate Demand." *American Economic Review*, 78: 435–439.
- [9] Bernanke, Ben S., and Alan S. Blinder. 1992. "The Federal Funds Rate and the Channels of Monetary Transmission." *American Economic Review*, 82: 901–921.
- [10] Bernanke, Ben S., and Mark Gertler. 1995. "Inside the Black Box: The Credit Channel of Monetary Policy Transmission." *Journal of Economic Perspectives* 9: 27–48.
- [11] Bernanke, Ben S., and Kenneth N. Kuttner. 2005. "What Explains the Stock Market's Reaction to Federal Reserve Policy?" *Journal of Finance* 60: 1221–1257.
- [12] Boivin, Jean, and Marc P. Giannoni. 2006. "Has Monetary Policy Become More Effective?" *Review of Economics and Statistics* 88: 445–462.
- [13] Bolton, Patrick, and Xavier Freixas. 2006. "Corporate Finance and the Monetary Transmission Mechanism" *Review of Financial Studies* 19: 829–870.
- [14] Cantillo, Miguel, and Julian Wright. 2000. "How Do Firms Choose Their Lenders? An Empirical Investigation." *Review of Financial Studies* 13: 155–189.
- [15] Carvalho, Daniel, Miguel Ferreira, and Pedro Matos. 2011. "Lending Relationships and the Effect of Bank Distress: Evidence from the 2007–2008 Financial Crisis." mimeo.

- [16] Chava, Sudheer, and Amiyatosh Purnanandam. 2007. "Determinants of the Floating-to-Fixed Rate Debt Structure of Firms." *Journal of Financial Economics* 85(3): 755–786.
- [17] Chava, Sudheer, and Amiyatosh Purnanandam. 2011. "The Effect of Banking Crisis on Bank-Dependent Borrowers." *Journal of Financial Economics* 99: 116–135.
- [18] Chernenko, Sergey, and Michael Faulkender. 2011. "The Two Sides of Derivatives Usage: Hedging and Speculating with Interest Rate Swaps." *Journal of Financial and Quantitative Analysis* 46: 1727–1754.
- [19] Chodorow-Reich, Gabriel. 2013. "The Employment Effects of Credit Market Disruptions: Firm-Level Evidence from the 2008–09 Financial Crisis." mimeo.
- [20] Colla, Paolo, Filippo Ippolito, and Kai Li. 2013. "Debt Specialization." *Journal of Finance* forthcoming.
- [21] Daniel, Kent D., and Sheridan Titman. 1997. "Evidence on the Characteristics of Cross Sectional Variation in Stock Returns." *Journal of Finance* 52: 1–33.
- [22] Denis, David J., and Vassil T. Mihov. 2003. "The Choice among Bank Debt, Non-Bank Private Debt, and Public Debt: Evidence from New Corporate Borrowings." *Journal of Financial Economics* 70: 3–28.
- [23] Ehrmann, Michael, and Marcel Fratzscher. 2004. "Taking Stock: Monetary Policy Transmission to Equity Markets." *Journal of Money, Credit and Banking* 36: 719–737.
- [24] Evans, Charles L., 1998. "Real-Time Taylor Rules and the Federal Funds Futures Market." *Economic Perspectives* 22: 44–55.
- [25] Fama, Eugene F., and Kenneth R. French. 1992. "The Cross-Section of Expected Stock Returns." *Journal of Finance* 47: 427–465.
- [26] Fama, Eugene F., and Kenneth R. French. 1995. "Size and Book-to-Market Factors in Earnings and Returns." *Journal of Finance* 50: 131–155.
- [27] Faulkender, Michael. 2005. "Hedging or Market Timing? Selecting the Interest Rate Exposure of Corporate Debt." *Journal of Finance* 60: 931–962.
- [28] Ferson, Wayne E., and Campbell R. Harvey. 1999. "Conditioning Variables and the Cross Section of Stock Returns." *Journal of Finance* 54: 1325–1360.
- [29] Froot, Kenneth A., David S. Scharfstein, and Jeremy C. Stein. 1993. "Risk Management: Coordinating Corporate Investments and Financing Policies." *Journal of Finance* 5: 1629–1658.
- [30] Gertler, Mark, and Simon Gilchrist. 1994. "Monetary Policy, Business Cycles, and the Behavior of Small Manufacturing Firms." *Quarterly Journal of Economics* 109: 309–340.
- [31] Giannetti, Mariassunta, and Andrei Simonov. 2009. "On the Real Effects of Bank Bailouts: Micro-Evidence from Japan." CEPR Discussion Papers 7441.

- [32] Graham, John R., and Daniel A. Rogers. 2002. "Do Firms Hedge in Response to Tax Incentives?" *Journal of Finance* 57: 815–839.
- [33] Gurkaynak, Refet S., Brian Sack, and Eric T. Swanson. 2005. "Do Actions Speak Louder than Words? The Response of Asset Prices to Monetary Policy Actions and Statements." *International Journal of Central Banking* 1: 55–93.
- [34] Hadlock, Charles J., and Christopher M. James. 2002. "Do Banks Provide Financial Slack?" *Journal of Finance*, 57: 1383–1419.
- [35] Hadlock, Charles J., and Joshua R. Pierce. 2010. "New Evidence on Measuring Financial Constraints: Moving Beyond the KZ Index." *Review of Financial Studies* 23: 1909–1940.
- [36] Houston, Joel, and Christopher James. 1996. "Bank Information Monopolies and the Mix of Private and Public Debt Claims." *Journal of Finance* 51: 1863–1889.
- [37] Jimenez, Gabriel, Steven Ongena, Jose-Luis Peydró, and Jesus Saurina. 2012. "Credit Supply and Monetary Policy: Identifying the Bank Balance-Sheet Channel with Loan Applications." *American Economic Review* 102: 2301–2326.
- [38] Kang, Jun-Koo, and Rene M. Stulz. 2000. "Do Banking Shocks Affect Firm Performance? An Analysis of the Japanese Experience." *Journal of Business* 73: 1–23.
- [39] Kashyap, Anil K., and Jeremy C. Stein. 1995. "The Impact of Monetary Policy on Bank Balance Sheets." *Carnegie-Rochester Conference Series on Public Policy*, 42: 151–195.
- [40] Kashyap, Anil K., and Jeremy C. Stein. 2000. "What Do a Million Observations on Banks Say About the Transmission of Monetary Policy?" *American Economic Review* 90: 407–428.
- [41] Khwaja, Asim, and Atif Mian 2008. "Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market." *American Economic Review* 98: 1413–1442.
- [42] Kishan, Ruby P., and Timothy P. Opiela. 2000. "Bank Size, Bank Capital, and the Bank Lending Channel." *Journal of Money, Credit and Banking* 32: 121–141.
- [43] Kuttner, Kenneth N. 2001. "Monetary Policy Surprises and Interest Rates: Evidence from the Fed Funds Futures Market." *Journal of Monetary Economics* 47: 523–544.
- [44] Landier, Augustin, David A. Sraer, and David Thesmar. 2013. "Banks' Exposure to Interest Rate Risk and the Transmission of Monetary Policy," NBER Working Paper No. 18857.
- [45] Mian, Shehzad L., "Evidence on Corporate Hedging Policy." 1996. *Journal of Financial and Quantitative Analysis*, 31: 419–439.
- [46] Nance, Deana R., Clifford W. Smith, and Charles W. Smithson. 1993. "On the Determinants of Corporate Hedging." *Journal of Finance* 48: 267–284.

- [47] Nizalova, Olena, and Irina Murtazashvili. 2012. "Exogenous Treatment and Endogenous Factors: Vanishing of Omitted Variable Bias on the Interaction Term." IZA Discussion Paper No. 6282.
- [48] Ongena, Steven, and David C. Smith, and Dag Michalsen, 2003. "Firms and Their Distressed Banks: Lessons from the Norwegian Banking Crisis." *Journal of Financial Economics* 67: 81–112.
- [49] Paravisini, Daniel. 2008. "Local Bank Financial Constraints and Firm Access to External Finance." *Journal of Finance* 63: 2161–2193.
- [50] Peek, Joe, and Eric S. Rosengren. 2000. "Collateral Damage: Effects of the Japanese Bank Crisis on Real Activity in the United States." *American Economic Review* 90: 30–45.
- [51] Piazzesi, Monika, and Eric T. Swanson. 2008. "Futures Prices as Risk-Adjusted Forecasts of Monetary Policy." *Journal of Monetary Economics* 55: 677–691.
- [52] Rampini, Adriano A., Amir Sufi, and S. Viswanathan. 2013. "Dynamic Risk Management." *Journal of Financial Economics* forthcoming.
- [53] Rigobon, Roberto, and Brian Sack. 2013. "The Effects of War Risk on US Financial Markets." *Journal of Banking and Finance* 29: 1769–1789.
- [54] Smith, Clifford W., and Rene M. Stulz. 1985. "The Determinants of Firms' Hedging Policies." *Journal of Financial and Quantitative Analysis* 28: 391–405.
- [55] Stein, Jeremy. 1998. "An Adverse-Selection Model of Bank Asset and Liability Management with Implications for the Transmission of Monetary Policy." *RAND Journal of Economics* 29: 466–486.
- [56] Stulz, Rene M. 1984. "Optimal Hedging Policies." *Journal of Financial and Quantitative Analysis* 19: 127–140.
- [57] Stulz, Rene M. 1996. "Rethinking Risk Management." *Journal of Applied Corporate Finance* 9: 8–25.
- [58] Sufi, Amir. 2009. "Bank Lines of Credit in Corporate Finance: An Empirical Analysis." *Review of Financial Studies* 22: 1057–1088.
- [59] Van den Heuvel, Skander J. 2002. "Does Bank Capital Matter for Monetary Transmission?" *Federal Reserve Bank of New York. Economic Policy Review* May: 259–265.

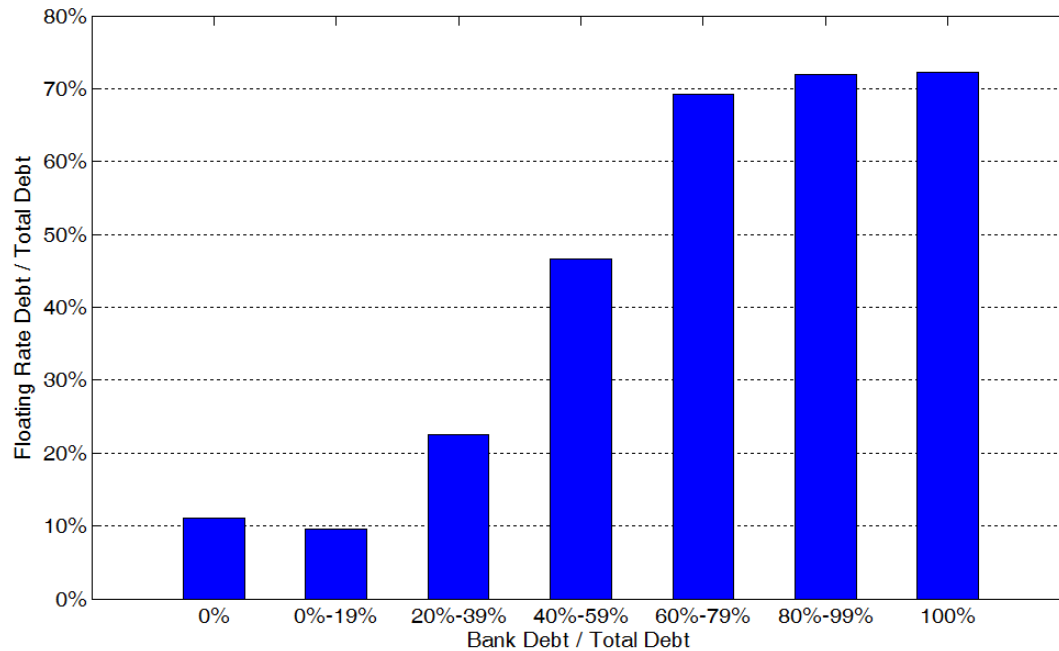


Figure 1

The relation between Bank Debt and Floating-Rate Debt

This figure displays the relationship between bank debt and floating-rate debt as a percentage of a firm's total debt. Firms are grouped on the horizontal axis according to bank debt as a percentage of total debt. The vertical axis shows the corresponding percentages of floating-rate debt as a percentage of total debt.

Table 1
Descriptive Statistics

This table provides summary statistics for the entire sample and for different subsamples. The entire sample consists of U.S. firms covered by Capital IQ, CRSP, and Compustat from 2003 to 2008, excluding utilities (SIC codes 4900--4949) and financials (SIC codes 6000--6999). We remove firm-year observations with negative revenues, missing information on total assets, or a value of total assets under \$10 million. We also discard penny stocks, defined as those with a price of less than \$5. After the above filters, the sample contains 21,745 firm-year observations comprising 4,408 unique firms. Complete variable definitions are given in the appendix. All variables are winsorized at the 1% level in both tails of the distribution, and total assets are expressed in terms of year-2000 dollars.

	(1)	(2)	(3)	(4)	(5)
	Entire Sample	Firms with Bank Debt	Firms without Bank Debt	Leveraged w/out Bank	Unleveraged
	Mean	Mean	Mean	Mean	Mean
Term Loans/At	2.69%	6.73%	0.00%	0.00%	0.00%
Drawn Credit Lines/At	2.30%	5.73%	0.00%	0.00%	0.00%
Bank Debt /At	5.11%	12.77%	0.00%	0.00%	0.00%
Bank Debt / Total Debt	36.34%	57.01%	0.00%	0.00%	0.00%
Undrawn Credit Line/At	9.22%	11.95%	6.32%	6.92%	5.42%
(Bank Debt + Und CL)/At	15.81%	24.72%	6.32%	6.92%	5.42%
Profitability	4.05%	5.54%	3.05%	3.07%	3.04%
Size (Total Assets)	2626.9	3891.7	1782.7	3984.4	453.6
Book Leverage	20.76%	36.97%	9.93%	26.53%	0.00%
Interest Rate Sensitivity	-11.35%	-13.30%	-10.01%	-11.66%	-8.96%
Hedging Dummy	29.90%	44.23%	15.64%	24.55%	0.00%
Rated Dummy	22.02%	36.57%	12.42%	30.93%	1.39%
Floating-Rate Debt/At	6.19%	10.80%	1.29%	2.04%	0.00%
Float-Rate Debt / Tot.Debt	32.71%	44.82%	11.39%	11.43%	0.00%
Short-Term Debt /At	2.17%	4.14%	0.87%	2.25%	0.00%
Observations	21745	8704	13041	4909	8132

Table 2**Response of Equity Prices to Federal Funds Rate Changes: Comparison Across Samples**

The table reports the results from regressions of 1-day equity returns on the surprise and expected components of the change in the federal funds rate, all expressed in percentage terms. Outliers are excluded. As in Bernanke and Kuttner (2005), for the period 1994--2002 outliers include October 15, 1998, January 3, 2001, March 20, 2001, April 18, 2001, and September 17, 2001. For the period 2003--2008, outlier dates are January 22, 2008, and March 18, 2008, based on a Cook's D statistic greater than 0.1. Column 1 contains returns for a value-weighted equity index. Columns 2--5 report returns for individual firm-date observations over different sample periods. Column 5 includes only observations for which data on bank debt is available. The firm level regressions contain random effects. Parentheses contain *t*-statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1) Daily Value- weighted Index 1994--2008	(2) Daily Panel All Firms 1994--2008	(3) Daily Panel All Firms 1994--2002	(4) Daily Panel All Firms 2003--2008	(5) Daily Panel Our Sample 2003--2008
Expected	0.421 (1.00)	0.209*** (8.40)	0.193*** (5.73)	0.133*** (3.90)	0.234*** (5.49)
Surprise	-3.359** (-2.05)	-2.704*** (-32.46)	-2.424*** (-25.67)	-4.665*** (-25.64)	-4.401*** (-21.22)
# Observations	115	536,357	363,290	173,067	99,047

Table 3

Is Bank Debt Special for the Transmission of Monetary Policy?

This table examines how the reaction of firm equity prices to changes in the federal funds rate varies with their level of bank dependence. The sample consists of U.S. firms covered by Capital IQ, CRSP, and Compustat from 2003 to 2008, excluding utilities (SIC codes 4900--4949) and financials (SIC codes 6000--6999). We remove firm-year observations with negative revenues, missing information on total assets, or a value of total assets under \$10 million. We also discard penny stocks, defined as those with a price of less than \$5. The sample comprises 43 monetary policy events from 2003 to 2008. Firm characteristics are demeaned and are lagged by one year and winsorized at the 1% level. The regression specification is as in equation (1). Unreported terms include a constant and non-interacted coefficients. In specification (5) undrawn credit lines are computed as part of bank debt, in addition to drawn credit lines and term loans. Standard errors are clustered at the date level in specifications (1)--(2) and two-way clustered at the date and industry levels in specifications (4)--(8). Industries are defined according to the Fama-French 48 sectors. Parentheses contain *t*-statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1) No Controls	(2) With Controls	(3) Controls and Ind. FE	(4) Event-indust. Clustering	(5) Including Credit Lines	(6) Int. Rate Sensitivity	(7) Firm Fixed Effects	(8) Interacted Firm Fixed Effects
Surprise	-4.221*** (-20.57)	-7.949*** (-32.95)	-7.170*** (-6.152)	-7.170 (-1.417)	-7.301 (-1.446)	-7.730 (-1.538)	-7.950*** (-6.464)	
Surprise*(BankDebt/At)	-4.955*** (-2.608)	-6.763*** (-3.032)	-6.962*** (-3.007)	-6.962*** (-2.602)	-6.843*** (-3.349)	-6.610** (-2.280)	-7.048** (-2.172)	-9.710** (-2.035)
Surprise*LnAssets		-0.956*** (-7.093)	-0.990*** (-6.837)	-0.990*** (-5.249)	-1.012*** (-5.242)	-0.992*** (-5.346)	-1.007*** (-5.026)	-4.947** (-2.502)
Surprise*Book Leverage		2.669*** (2.865)	3.001*** (3.056)	3.001*** (2.803)	3.091*** (3.007)	3.262*** (3.220)	2.883** (2.321)	9.561*** (3.693)
Surprise*Profitability		-9.818*** (-6.462)	-8.121*** (-4.660)	-8.121** (-2.462)	-7.606** (-2.319)	-8.961** (-2.469)	-9.121** (-2.094)	-16.55** (-2.046)
Surprise*M/B		0.172 (1.111)	-0.0326 (-0.190)	-0.0326 (-0.105)	-0.0563 (-0.179)	0.0219 (0.0702)	0.267 (0.696)	-0.390 (-0.604)
Surprise*Int Rate Sensitivity						-4.088** (-2.307)		
Surprise*Cash-Flow Volatility						41.99 (0.506)		
Surprise*Beta						0.737* (1.867)		
Firm FE	NO	NO	NO	NO	NO	NO	YES	YES
FF48 Industry FE	NO	NO	NO	YES	YES	YES	NO	NO
Year FE	NO	YES	YES	YES	YES	YES	YES	YES
Interacted FF48 Industry FE	NO	NO	NO	YES	YES	YES	NO	NO
Cluster (Fed event*IndustryFF48)	NO	NO	YES	YES	YES	YES	YES	YES
Observations	99,047	98,629	95,846	95,846	95,690	95,846	98,629	98,629

Table 4**Short-Term Debt and the Response of Equity Prices to Federal Funds Rate Changes**

This table examines how the reaction of firm equity prices to changes in the target federal funds rate varies with their use of short-term debt. Short-Term Debt/At is defined as debt in current liabilities (item 34) over the book value of assets. Columns 2, 4, and 6 include (unreported) interacted and non-interacted $\ln(\text{assets})$, profitability, and the market-to-book ratio. All firm characteristics are lagged by one year, demeaned, and winsorized at 1%. Parentheses contain t -statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)
Surprise	-4.148*** (-20.40)	-7.459 (-1.490)	-4.221*** (-20.56)	-6.988 (-1.388)	-4.189*** (-20.16)	-6.993 (-1.388)
Surprise*(ShortTermDebt/At)	6.281 (1.526)	6.362 (1.165)	7.508* (1.754)	7.841 (1.488)	9.498** (2.017)	11.36** (2.197)
Surprise*(BankDebt/At)			-5.707*** (-2.930)	-7.362*** (-2.733)	-5.419*** (-2.752)	-6.622** (-2.401)
Surprise*(ShortTermDebt/At)*(BankDebt/At)					-27.329 (-1.01)	-41.41 (-1.215)
Firm Characteristics	NO	YES	NO	YES	NO	YES
FF48 Industry FE	NO	YES	NO	YES	NO	YES
Year FE	NO	YES	NO	YES	NO	YES
Interacted FF48 Industry FE	NO	YES	NO	YES	NO	YES
Cluster (Fed event*IndustryFF48)	NO	YES	NO	YES	NO	YES
Observations	101,094	96,028	99,023	95,846	99,023	95,846

Table 5
Bank Debt Specialness and Firm Financing Constraints

This table examines how the effect of monetary policy on firm stock prices varies with firms' exposure to bank debt and their level of financial constraint. Financial constraints are proxied by the firm's credit rating, size, and age. The financial constraint measure takes value 1 if the firm has no rating (0 if it is rated) in columns 1 and 2, takes value 1 if the firm is in the bottom quartile of age (0 if the firm is in the top quartile) in columns 3 and 4, and takes value 1 if the firm is in the bottom quartile of size (0 if the firm is in the top quartile) in columns 5 and 6. A constant, noninteracted terms, and the interest rate surprise interacted with book leverage, profitability, and the market-to-book ratio are included but not reported. All firm characteristics are lagged by one year and winsorized at the 1% level. Industries are defined according to the Fama French 48 sector grouping. Parentheses contain *t*-statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)
	Unrated		Young		Small	
Surprise	-5.759*** (-15.89)	-4.437*** (-3.993)	-5.617*** (-9.119)	-5.428*** (-3.009)	-6.052*** (-15.74)	-7.054*** (-3.850)
Surprise*(BankDebt/At)	1.304 (0.428)	-3.881 (-0.697)	-1.121 (-0.191)	-3.556 (-0.429)	3.385 (0.897)	-0.987 (-0.176)
Surprise*Fin. Constraint Measure	2.250*** (5.088)	0.355 (0.349)	4.541*** (5.204)	3.534** (2.573)	5.369*** (9.102)	6.908** (2.526)
Surprise*(BankDebt/At)*Fin. Constraint Measure	-7.321* (-1.858)	-4.282 (-0.628)	-12.371* (-1.657)	-9.389 (-0.904)	-3.809 (-0.626)	-2.884 (-0.370)
Firm Controls	NO	YES	NO	YES	NO	YES
Industry FE	NO	YES	NO	YES	NO	YES
Year FE	NO	YES	NO	YES	NO	YES
Industry*Event Clustering	NO	YES	NO	YES	NO	YES
Observations	97,660	97,250	23,335	23,222	48,220	48,020

Table 6
The Financial Health of Lenders

This table examines the role of a lender's financial health on the relation between bank dependence and the sensitivity of prices to monetary policy surprises. Lender financial health is measured as: lender size (logarithm of lender assets), and the Tier 1 capital ratio. For each health measure we introduce a dummy that takes value 1 for values in the top quartile, and zero for values in the bottom quartile. BankDebt/At is defined as bank debt (term loans plus drawn revolving credit) over the book value of assets. We only report "Surprise" and the terms interacted with "Surprise." All regressions also include (unreported) a constant term, as well as $\ln(\text{assets})$, book leverage, profitability, the market-to-book ratio, and interest rate sensitivity. All firm and lender characteristics are lagged by one year and winsorized at 1%. Parentheses contain t -statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1)	(2)	(3)
	No Controls	Controls	Clustering, Interacted Ind. FE
<i>Panel A: Lender Size</i>			
Surprise	-3.268*** (-6.116)	-2.720*** (-4.691)	-4.994 (-0.664)
Surprise*(BankDebt/At)	-7.812* (-1.792)	-15.01*** (-3.059)	-13.07** (-2.469)
Surprise*Lender Size	-7.274*** (-9.826)	-6.315*** (-6.590)	-6.450*** (-3.710)
Surprise*(BankDebt/At)*Lender Size	12.79** (2.092)	18.30** (2.565)	15.04** (1.999)
Observations	29,722	29,678	28,697
Number of gvkey	1,551	1,550	1,524
<i>Panel B: Lender Tier 1 Capital Ratio</i>			
Surprise	1.635*** (3.306)	1.806*** (3.254)	-1.862 (-0.293)
Surprise*(BankDebt/At)	-7.760* (-1.865)	-11.37** (-2.280)	-9.149* (-1.691)
Surprise*Tier 1 Cap	-12.84*** (-17.88)	-11.54*** (-13.29)	-12.05*** (-8.248)
Surprise*(BankDebt/At)*Tier 1 Cap	12.40** (2.075)	8.402 (1.181)	8.244 (0.926)
Observations	29,980	29,903	28,999
Number of gvkey	1,883	1,880	1,842
Firm Controls	NO	YES	YES
Interacted Firm Controls	NO	YES	YES
Industry FE	NO	NO	YES
Interacted Industry FE	NO	NO	YES
Clustering Fed Date/Industry Level	NO	NO	YES

Table 7**Lender Financial Health, Bank Dependence and Financial Constraints**

This table examines the role of the interaction between the financial health of lenders and the financial constraints of firms, and how this interaction affects the sensitivity of bank-dependent firms to monetary policy surprises. Lender financial health is measured as: lender size (logarithm of lender assets), and the Tier 1 capital ratio. For each health measure we introduce a dummy that takes the value 1 for values in the top quartile, and zero for values in the bottom quartile. BankDebt/At is defined as bank debt (term loans plus drawn revolving credit) over the book value of assets. We only report “Surprise” and the terms interacted with “Surprise.” All regressions also include (unreported) a constant term, as well as $\ln(\text{assets})$, book leverage, profitability, the market-to-book ratio, and interest rate sensitivity. Small (Large) are those in the bottom (top) size interval. All firm and lender characteristics are lagged by one year and winsorized at 1%. Parentheses contain t -statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1)	(2)	(3)	(4)
	Unrated	Rated	Young	Old
<i>Panel A: Lender Size</i>				
Surprise	-2.695 (-0.327)	-8.005 (-1.105)	-17.90* (-1.877)	3.513 (0.516)
Surprise*(BankDebt/At)	-20.69** (-2.553)	0.445 (0.0640)	-22.21 (-1.164)	18.19 (0.729)
Surprise*Lender Size	-6.580*** (-3.827)	-7.076** (-2.501)	-7.753* (-1.717)	0.0723 (0.0254)
Surprise*(BankDebt/At)*Lender Size	42.01*** (3.365)	-4.312 (-0.428)	43.14* (1.689)	-27.69 (-0.923)
Observations	15,531	12,680	2,135	4,207
Number of gvkey	886	683	181	277
<i>Panel B: Lender Tier 1 Capital Ratio</i>				
Surprise	7.258 (1.586)	-8.575 (-1.090)	-4.463 (-0.816)	0.280 (0.0521)
Surprise*(BankDebt/At)	-8.028 (-0.963)	-4.507 (-0.562)	-6.604 (-0.297)	16.32 (0.956)
Surprise*Tier 1 Cap	-14.40*** (-8.549)	-9.058** (-2.555)	-6.253 (-1.494)	-17.32*** (-6.369)
Surprise*(BankDebt/At)*Tier 1 Cap	17.96 (1.473)	-6.761 (-0.592)	-33.64 (-1.358)	-12.72 (-0.515)
Observations	16,442	12,108	2,468	4,118
Number of gvkey	1,119	790	251	334
Firm Controls	YES	YES	YES	YES
Interacted Firm Controls	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Interacted Industry FE	YES	YES	YES	YES
Clustering Fed Date/Industry Level	YES	YES	YES	YES

Table 8
Bank Firm Matching Multivariate Analysis

This table examines bank-firm matching in a multivariate setup by displaying the association between the lender financial health, proxied by bank size and the Tier 1 capital ratio, and firm risk characteristics, proxied by cash-flow volatility, interest rate sensitivity, and CAPM beta. It also analyzes how the association between risk characteristics and bank health changes with the degree of bank dependence of firms. Bank characteristics, including the deposit ratio and liquidity ratio, and all firm characteristics interacted with BankDebt/At are included but not reported. Parentheses contain *t*-statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	Dep. Var. = Bank Size			Dep. Var. = Capital Ratio		
	(1)	(2)	(3)	(4)	(5)	(6)
Size	-0.001*** (-3.143)	-0.001*** (-3.201)	-0.001*** (-2.753)	-0.004*** (1.709)	-0.004*** (1.826)	-0.003*** (1.681)
Book Leverage	-20.23*** (-5.593)	-19.67*** (-5.441)	-11.52*** (-2.819)	-0.107 (-0.366)	-0.0224 (-0.0767)	-0.637* (-1.922)
BankDebt/At	-18.47 (-0.749)	26.45 (1.348)	-14.06 (-0.623)	-2.055 (-1.034)	-4.988*** (-3.151)	-5.644*** (-3.073)
Credit Line (dummy)	-2.643* (-1.854)	-2.568* (-1.801)	-3.037** (-2.006)	0.257** (2.200)	0.240** (2.053)	0.196 (1.566)
Age (since IPO)	11.68*** (41.91)	11.73*** (42.05)	12.29*** (40.94)	-0.0322 (-1.512)	-0.0403* (-1.890)	-0.0983*** (-4.249)
Rated (dummy)	17.37*** (6.644)	17.73*** (6.797)	13.78*** (4.774)	-0.0724 (-0.349)	-0.138 (-0.667)	0.138 (0.598)
Profitability	1.808 (0.226)	0.743 (0.0931)	-5.834 (-0.667)	-2.591*** (-4.043)	-2.532*** (-3.956)	-1.831*** (-2.589)
M/B	1.692** (2.520)	1.580** (2.353)	2.170*** (2.955)	0.0337 (0.615)	0.0564 (1.028)	0.147** (2.433)
Cash/At	-25.49*** (-4.147)	-26.73*** (-4.372)	-21.93*** (-3.300)	0.239 (0.485)	0.280 (0.573)	-1.723*** (-3.222)
Tangibility	-29.36*** (-3.511)	-29.93*** (-3.584)	-30.79*** (-3.417)	0.00574 (0.00978)	0.169 (0.288)	0.0586 (0.0922)
<u>Risk Measures:</u>						
Cash-Flow Volatility	633.0 (0.811)			-38.16 (-0.918)		
Interest Rate Sensitivity		-32.71** (-2.125)			2.523*** (3.110)	
Beta			-0.708 (-1.522)			-0.101*** (-2.606)
<u>Interaction terms:</u>						
Cash-Flow Volatility* BankDebt/At	5,022*** (3.308)			-118.1 (-0.969)		
Interest-Rate Sensitivity* BankDebt/At		-34.37 (-1.444)			-9.888*** (-5.155)	
Beta* BankDebt/At			4.257 (1.211)			-1.233*** (-4.193)
Observations	29,224	29,224	25,902	29,224	29,224	25,902

Table 9**Lender Financial Health and Bank Dependence: Instrumental Variables Estimation**

This table examines the role of a lender's financial health on the effect of bank dependence on the response of firms to monetary policy surprises using instrumental variables (IV) estimation. Lender financial health is measured in two different ways: lender size (logarithm of lender assets), and the Tier 1 capital ratio. Lender size is instrumented using the median bank size in the Metropolitan Statistical Area (MSA) or rural county where the firm is located (*lnmsa_p50*), and the Tier 1 capital ratio is instrumented using the corporate income tax rate in the state where the firm is located (*tax*). Column 1 displays the results of an IV regression in which the double interaction term *Surprise*Lender Size* is instrumented using *Surprise*lnmsa_p50*, and the triple interaction term *Surprise*(BankDebt/At)*(Lender Size)* is instrumented using *Surprise*(BankDebt/At)*lnmsa_p50*. Column 2 refers to an IV regression in which the double interaction term *Surprise*(Capital Ratio)* is instrumented for using *Surprise*tax*, and the triple interaction term *Surprise*(BankDebt/At)*(Capital Ratio)* is instrumented using *Surprise*(BankDebt/At)*tax*. BankDebt/At is defined as bank debt (term loans plus drawn revolving credit) over the book value of assets. All regressions also include (unreported) a constant term, as well as $\ln(\text{assets})$, book leverage, profitability, and the market-to-book ratio, also interacted with *Surprise*. All firm and lender characteristics are lagged by one year and winsorized at 1%. Parentheses contain *t*-statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1)	(2)
	Bank Health = Bank Size	Bank Health = Capital Ratio
Surprise	28.16*** (3.213)	27.08* (1.823)
Surprise*(BankDebt/At)	-78.84 (-1.544)	-268.8** (-2.231)
Surprise*Bank Health	-68.85*** (-4.645)	-52.59* (-1.722)
Surprise*(BankDebt/At)*Bank Health	164.3* (1.672)	522.7** (2.173)
Observations	15,269	28,125
Number of gvkey	852	1,785
Firm Controls	YES	YES
Surprise*Firm Controls	YES	YES
Industry FE	YES	YES
Interacted Industry FE	YES	YES

Table 10
The Role of Hedging Floating Interest Rate Risk

This table examines how the effect of monetary policy on firm stock prices varies with firms' hedging activity. Hedgers are defined as those firms that report having hedged their interest rate risk from floating to fixed in their 10-K annual reports. Bank Debt/At is defined as bank debt (term loans plus drawn revolving credit) over the book value of assets (At). All regressions include (unreported) a constant term. In Panel A the first two columns contain only firms that hedge, while columns 3--4 contain firms that do not hedge. Unrated is based on S&P ratings. Small is a dummy for the smallest size tercile. Young is a dummy for the smallest tercile of age, computed as number of years since IPO. All firm and lender characteristics are lagged by one year and winsorized at 1%. Parentheses contain *t*-statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

Panel A: Hedgers versus non Hedgers

	(1)	(2)	(3)	(4)
	Hedgers		Non-Hedgers	
Surprise	-5.888*** (-15.91)	-1.308 (-1.633)	-4.850*** (-17.00)	-0.159 (-0.222)
Surprise *(BankDebt/At)	0.053 (0.021)		-5.938* (-1.681)	
Surprise *(FloatingRateDebt /At)		-3.944 (-0.691)		-10.270 (-1.313)
Firm Controls	NO	NO	NO	NO
Surprise*Firm Controls	NO	NO	NO	NO
Firm FE	NO	NO	NO	NO
Observations	31,217		64,538	

Panel B: Financial Constraints

	(1)	(2)	(3)	(4)
		Measure of Financial Constraints		
		Unrated	Small	Young
Surprise	-5.093*** (-17.71)	-5.256*** (-8.980)	-6.470*** (-17.60)	-5.243*** (-11.71)
Surprise *(BankDebt/At)	-12.54*** (-3.475)	-10.76** (-2.158)	-17.70*** (-4.274)	-14.46*** (-2.682)
Surprise * Hedging	-0.0815 (-0.154)	0.128 (0.238)	-0.0715 (-0.135)	-0.828 (-1.026)
Surprise *(BankD/At)*Hedging	7.314* (1.674)	7.439 (1.602)	12.36*** (2.677)	10.56* (1.725)
Surprise *(BankD/At)*Financial Constraint		-0.809 (-0.179)	12.85** (2.467)	-4.187 (-0.718)
Surprise *Financial Constraint		0.125 (0.178)	4.231*** (5.969)	2.697*** (3.628)
Firm Controls	YES	YES	YES	YES
Surprise*Firm Controls	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES
Observations	92,984	91,660	92,984	51,849

Table 11**The Effect of Monetary Policy on the Cost of Debt**

This table examines how monetary policy affects firm cost of debt and how this effect varies with bank dependence and hedging activity. The average cost of debt is computed as the ratio of interest expenses over total debt of the previous year. The change in the average cost of debt is computed as the difference year on year. Change_FF is the change in the federal funds rate. Hedgers are defined as those firms that report having hedged their interest rate risk from floating to fixed in their 10K annual reports. Bank Debt/At is defined as bank debt (term loans plus drawn revolving credit) over the book value of assets (At). All regressions also include (unreported) a constant term. Unreported controls include uninteracted $\ln(\text{assets})$, book leverage, the market-to-book ratio, and profitability, as well as the lower interaction term between Bank Debt/At and hedging. All firm characteristics are lagged by one year and winsorized at 1%. Parentheses contain t -statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1)	(2)	(3)	(4)
	Next Year Change in the Average Cost of Debt			
Change_FF	0.030*** (5.873)	0.032*** (6.076)	0.035*** (6.708)	0.029*** (4.074)
Change_FF *(BankDebt/At)		0.066 (1.453)	0.073 (1.584)	0.231*** (3.094)
Change_FF * Hedging				0.014 (1.272)
Change_FF *(BankDebt/At)*Hedging				-0.227** (-2.336)
Change_FF *LnAssets			-0.015 (-0.989)	-0.022 (-1.376)
Change_FF *Book Leverage			-0.282*** (-2.905)	-0.324*** (-3.256)
Change_FF *Profitability			0.790*** (3.613)	0.804*** (3.614)
Change_FF *M/B			-0.015 (-0.680)	-0.034 (-1.465)
Firm Controls	YES	YES	YES	YES
Observations	66,383	64,718	64,698	61,427

APPENDIX

Table A1
Description of Firm-Level Variables
Item numbers are from Compustat. CIQ items come from Capital IQ.

Variable	Construction
Average Cost of Debt	Interest Expenses (15)/Previous Year Total Debt
Bank Debt 1	[Drawn Credit Lines (CIQ) + Term Loans (CIQ)] / Assets (6)
Bank Debt 2	[Drawn Credit Lines (CIQ) + Term Loans (CIQ) + Undrawn Credit Lines (CIQ)]/ Assets (6)
Book Leverage	(Total Debt) / (Total Debt + Book Value of Equity)
Book Value of Equity	Common/Ordinary Equity – Total (60)
Drawn Credit Lines/Assets	Drawn Credit Lines (CIQ) / Total Assets (6)
Floating Interest Rate Debt	Debt with floating interest rate (CIQ)
Hedging Dummy	A dummy variable that takes the value 1 if a firms reports floating-to-fixed interest-rate hedging activities in its 10-K
Interest Rate Sensitivity	Correlation between quarterly firm EBITDA and three-month average LIBOR rates
M/B	[Market Value of Equity + Total Debt + Preferred Stock Liquidating Value (10)] / Total Assets (6)
Market Value of Equity	Stock Price (199) × Common Shares Used to Calculate EPS (54)
Profitability	Operating Income before Depreciation (13) / Total Assets (6)
Rated	A dummy variable that takes the value of one if the firm is rated by the S&P, and zero otherwise
Short-Term Debt	Debt in current liabilities (34), equal to the total amount of short-term notes and the current portion of long-term debt due in one year. More precisely, short-term debt includes: 1. Bank acceptances and overdrafts; 2. Brokerage companies' drafts payable commercial paper; 3. Construction loans; 4. Current portion of long-term debt; 5. Debt in default; 6. Debt due on demand; 7. Due to factor of "interest bearing"; 8. Installments on a loan; 9. Interest payable when combined with notes payable; 10. Line of credit; 11. Loans payable to officers of the company; 12. Loans payable to parents, and consolidated or unconsolidated subsidiaries; 13. Loans payable to stockholders 14. Notes payable to banks and others; 15. Notes payable that are included in accounts payable, unless specifically trade notes payable; 16. Sinking fund payments.
Size (At)	Logarithm of Book Value of Total Assets (6) , deflated to base year 2000
Term Loans /Assets	Term Loans (CIQ)/ Total Assets (6)
Total Debt	Debt in Current Liabilities (34) + Long-Term Debt (9)
Undrawn Credit Lines / Assets	Undrawn Credit Lines (CIQ) / Total Assets (6)

Table A2
The Sign of Controls Across Different Sample Periods

This table examines the subsample stability of the interaction effects between firm-level controls and monetary policy surprises. A constant and noninteracted coefficients are included but not reported. All firm characteristics are lagged by one year and winsorized at the 1% level. Standard errors are two-way clustered at the date and industry levels. Industries are defined according to the Fama French 48 sector grouping. Parentheses contain *t*-statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1) 1989-2008	(2) 1989-1994	(3) 1994-2002	(4) 1989-2002	(5) 2003-2008
Surprise	-5.125** (-2.309)	-2.432 (-0.978)	-7.438** (-2.553)	-4.925** (-2.071)	-8.250*** (-3.710)
Surprise*LnAssets	-0.561* (-1.884)	-0.304 (-0.885)	-0.0930 (-0.162)	-0.410 (-1.210)	-1.116*** (-2.859)
Surprise*Book Leverage	0.730 (0.661)	-1.812* (-1.935)	1.044 (0.469)	0.361 (0.301)	1.557 (0.627)
Surprise*Profitability	4.457 (1.057)	1.564 (0.586)	11.37 (1.407)	7.042 (1.455)	-16.26** (-2.460)
Surprise*M/B	-2.030*** (-2.680)	-0.557 (-0.981)	-3.339*** (-2.872)	-2.098** (-2.562)	-0.422 (-0.654)
Year FE	YES	YES	YES	YES	YES
Industry FF48 FE	YES	YES	YES	YES	YES
Cluster (Fed event*Industry)	YES	YES	YES	YES	YES
Observations	367,883	106,450	181,797	266,582	101,301
Number of gvkey	6,063	3,033	4,960	5,263	3,551

Table A3**Which Firms Use Bank, Floating-Rate and Short-Term Debt, and Interest Rate Hedging**

This table examines the use of bank debt, floating-rate debt, short-term debt, and interest rate hedging, using firm-year data. A constant is included but not reported. All firm characteristics are winsorized at the 1% level. Standard errors are clustered at the firm level. Columns 1--3 use a Tobit specification with lower censoring at 0 and upper censoring at 1, while column 4 is a Probit. Parentheses contain *t*-statistics. The asterisks denote *** for $p < 0.01$, ** for $p < 0.05$, * for $p < 0.1$.

	(1)	(2)	(3)	(4)	(5)	(6)
	Bank/At	Bank/At	Bank/At	Floating-Rate Debt/At	Short-Term Debt/At	Hedging
LnAssets	-0.0147*** (-8.219)	-0.0151*** (-8.003)	-0.0149*** (-8.333)	-0.002** (-2.017)	0.001** (2.240)	0.177*** (8.854)
Profitability	0.144*** (7.385)	0.140*** (6.787)	0.133*** (6.889)	0.098*** (7.104)	-0.002 (-0.437)	1.771*** (7.867)
Market to Book	-0.0242*** (-10.91)	-0.0241*** (-9.994)	-0.0221*** (-10.36)	-0.010*** (-8.610)	-0.002*** (-4.705)	-0.179*** (-7.554)
Bank/At				0.892*** (52.10)	0.0930*** (6.139)	2.775*** (12.84)
Book Leverage	0.340*** (26.17)	0.342*** (24.98)	0.333*** (25.63)	0.0962*** (11.12)	0.0702*** (12.65)	0.626*** (6.435)
Unrated	0.0179** (2.353)	0.0206*** (2.626)	0.0196*** (2.602)	-0.00218 (-0.452)	0.00355 (1.204)	-0.381*** (-5.720)
Interest Rate Sensitivity	-0.0168 (-1.439)					
CAPM beta		-0.00453*** (-3.329)				
Cash Flow Volatility			-3.165*** (-5.078)			
Observations	99,621	89,424	99,621	98,769	99,621	93,653