Do Higher Interest Rates Make The Banking System Safer? Evidence From Bank Leverage

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Abstract

A vast theoretical literature claims that increasing interest rates reduces bank leverage, therefore making banks safer. The empirical validity of this claim is critical to improving our understanding of the transmission of monetary policy through banks in addition to informing the ongoing debate on whether monetary policy should be used to support financial stability. I show empirically that raising interest rates actually *increases* bank leverage. I propose and empirically validate a mechanism that explains the overall increase in bank leverage in response to monetary policy shocks which I term the *loan-loss mechanism*: contractionary shocks increase loan losses, reduce bank profits and equity, and ultimately increase bank leverage. I document why much of the theoretical literature is unable to explain the leverage response and develop a banking model where floating-rate loans entail a trade-off between interest rate risk and credit risk, which generates the loan-loss mechanism. Using microdata, I provide empirical evidence consistent with floating-rate loans hedging interest rate risk at the expense of generating loan losses.

Keywords: Leverage, Monetary Policy, Financial Stability, Banking, Floating Rates

JEL Codes: E52, G18, G21, G23

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1 Introduction

As early as 1945, one of the most influential economists of the twentieth century, Paul Samuelson, proclaimed that in response to an increase in interest rates, the banking system is "tremendously better off" (Samuelson (1945)).[1] Close to seventy years later, former Federal Reserve Chair Janet Yellen echoed a similar sentiment in an influential speech on monetary policy and financial stability, explaining that higher interest rates reduce financial sector vulnerabilities through reducing their leverage (i.e., ratio of assets to equity).[2] The empirical validity of these claims is critical to improving our understanding of the transmission of monetary policy through banks in addition to informing the ongoing debate on whether monetary policy should support financial stability. In this paper, I explicitly address this first-order question: do contractionary monetary policy shocks actually improve bank vulnerability by reducing bank leverage?

The answer to this question, from much of the theoretical literature, is yes. Woodford (2012) argues, using a typical New Keynesian model with credit frictions, that "it is appropriate to use monetary policy to 'lean against' a credit boom" which in his model implies tightening monetary policy to reduce leverage. Angeloni and Faia (2013) build a dynamic macroeconomic model featuring banks to similarly conclude that "the increase in interest rate activates the risk taking channel: bank leverage and risk decline." Dell'Ariccia et al. (2014) develop a model of financial intermediation where banks engage in costly monitoring to reduce the credit risk in their loan portfolios. Despite their different modelling approach, they reach the same conclusion: "a reduction in risk-free interest rates leads banks to increase their leverage" where the risk-free rate refers to the policy rate. Drechsler et al. (2018b) take yet another approach by developing a dynamic asset pricing model in which monetary policy affects the risk premium component of the cost of capital. Nonetheless, their analysis leads to the same claim: "Lower nominal rates make liquidity cheaper and raise leverage." Martinez-Miera and Repullo (2019) extend the banking model of Martinez-Miera and Repullo (2017) to include monetary policy and similarly show that "[monetary] tightening reduces aggregate investment. . . and reduces bank leverage." Finally, Martin et al. (2021) highlight that the framework of Ghote (2021), which consists of monetary and macroprudential policy intervention in a general equilibrium economy with recurrent boom-bust

[1] Specifically, Samuelson (1945) argues that an interest rate hike would significantly improve the profitability and stability of the banking system.

[2] https://www.federalreserve.gov/newsevents/speech/yellen20140702a.htm

cycles, also supports leaning against a boom.[3] In particular, Martin et al. (2021) summarise the theoretical literature by concluding the following: "This is true in most models...By tightening ex ante, monetary policy contributes to reducing credit and, more specifically, leverage."[4]

Given such strong and consistent claims across much of the theoretical literature and a plethora of modelling approaches, one might expect considerable empirical support. However, as Boyarchenko et al. (2022) highlight in their review paper, there is very limited and inconclusive empirical evidence on the causal impact of monetary policy on leverage and no empirical evidence of the underlying mechanism.[5,6]

The first contribution of this paper is to provide robust empirical evidence of the impact of contractionary monetary policy shocks on bank leverage. My empirical strategy relies on existing measures of exogenous monetary policy shocks which capture unexpected changes in the Fed Funds Rate (FFR).[7] Using quarterly data from the Federal Deposit Insurance Corporation between 1984 and 2007, I estimate lag-augmented local projections of aggregate bank leverage with these exogenous shocks. My first finding is contrary to much of the theoretical literature. I find that a contractionary monetary shock that induces a one percentage point rise in the FFR leads to a five to ten percent *increase* in bank leverage. I show that this finding is robust to different definitions of leverage, different time-periods, different lag lengths, and different monetary policy shock series.

My second contribution is to document empirically a mechanism that can explain why leverage increases in response to contractionary monetary policy shocks. This is important as it sheds light on how banks respond to and are affected by monetary policy. The literature has traditionally focused on the bank lending channel as the main way in which banks interact with monetary policy whereby contractionary monetary policy reduces bank lending (see for

[3] Macroprudential policy uses primarily regulatory measures (e.g., bank leverage requirements) to limit financial crisis risk. See for example Galati and Moessner (2018) and Gourio et al. (2018).

[4] See Appendix A for a brief summary of these models and the underlying mechanisms.

[5] The main empirical papers that are related to this question include Miranda-Agrippino and Rey (2020), Wieland and Yang (2020), and Li (2022). However, the primary focus of these papers is not the estimation of a domestic bank leverage response to domestic monetary policy shocks. The inclusion of such estimates in these papers are part of ancillary analyses and, as such, the analyses are not supported with sufficient robustness checks nor detailed discussion of potential mechanisms.

[6] While not directly examining the impact of monetary policy on leverage, Grimm et al. (2023) examine the impact of loose monetary policy on financial instability. Specifically, they find that extended periods of accomodative policy, when followed by a tightening, can increase the likelihood of financial distress.

[7] I use several different measures of exogenous monetary policy shocks including Romer and Romer (2004), Gertler and Karadi (2015), and Bu et al. (2021).

example Kashyap and Stein (1994) and Bernanke and Gertler (1995)). However, the last decade has seen a resurgence of research on the transmission of monetary policy through the financial system, largely driven by empirical evidence that monetary policy has meaningful consequences on financial institutions in ways that are not captured by the workhorse New Keynesian models (Drechsler et al. (2018a)). I show that while raising interest rates does indeed reduce bank borrowing (as per the bank lending channel), it also increases the proportion of loans that are delinquent and so increases loan losses. Unexpectedly higher loan losses decrease bank profits which subsequently reduce bank equity.[8] I find that the drop in equity increases leverage more than the drop in borrowing decreases leverage and so bank leverage increases overall. I term this the *loan-loss mechanism*. Moreover, instead of relying on ruling out alternative mechanisms to evaluate the importance of the loan-loss mechanism, I take advantage of accounting identities which allow me to show precisely that the loan-loss mechanism explains almost all the variation in bank leverage in response to contractionary monetary policy shocks. Finally, I show that while loan losses and leverage increase in response to monetary policy shocks (where the FFR rises), loan losses do not rise and leverage actually falls in response to contractionary oil shocks (where the FFR does not rise). This analysis provides suggestive evidence, at the aggregate level, of the importance of the rise in the FFR and hence the potential role of floating-rate loans in generating loan losses.

My next contribution is to dissect the theoretical literature in order to show where and why so many, and such different, models generate empirically inconsistent leverage responses. Investigating the literature in this way is important not only to provide an empirically-grounded theoretical answer to whether contractionary monetary policy reduces bank leverage, but also because bank leverage, per se, plays a vital role in macroeconomic models with financial sectors. For example, as highlighted in Adrian et al. (2014), in many models, such as Fostel and Geanakoplos (2008) and Geanakoplos (2010), when the bank's own funds are fixed, leverage is the key state variable and lending is determined solely by leverage. This directly connects leverage to the bank lending channel of monetary policy. Furthermore, as commented in Ajello et al. (2022), leverage is core to the financial accelerator models (e.g., Bernanke et al. (1999)), and typically both amplifies and propagates the response of the economy to shocks, thus generating aggregate fluctuations.

I show that the aforementioned empirical inconsistency of the literature appears to derive

[8] English et al. (2018) also find that contractionary monetary policy reduces bank profits while Altavilla et al. (2018) find that a prolonged period of low interest rates reduces loan losses which boosts bank profits.

from three broad, though not necessarily mutually exclusive, modelling decisions. First are models such as Angeloni and Faia (2013) and Drechsler et al. (2018b) that rely on some form of a substitution effect as the dominant mechanism through which higher interest rates reduce bank leverage. Second are models such as Woodford (2012) and Rannenberg (2016) that incorrectly rely on the observed procyclical behaviour of leverage in order to conclude that leverage declines in response to monetary policy tightening. Finally, models such as Gertler and Kiyotaki (2010) and Gertler and Karadi (2011), while able to generate an increase in leverage in response to contractionary monetary policy shocks, attribute this rise to an increase in expected profitability. However, despite an empirically consistent leverage response, the proposed mechanism is inconsistent with the observed evidence that profitability falls rather than rises in response to a monetary contraction. Moreover, this class of models typically generates an increase in leverage in response to any contractionary shock which is inconsistent with the empirical response of leverage to other (non-monetary) contractionary shocks, such as oil shocks. This underscores the importance of the underlying loan-loss mechanism which is specific to contractionary monetary policy shocks.

The empirical inconsistencies across models typically arise in the the banking block rather than the general equilibrium structure of the model. Indeed, some banking-specific models generate more empirically consistent dynamics as they feature both a fall in profits and a rise in leverage in response to contractionary monetary policy shocks.[9] However, such models still do not capture a loan-loss mechanism which features a role for floating-rate loans. Therefore, I develop a banking model, building on Kirti (2020), that emphasises the role of floating-rate loans and credit risk. In my model, banks optimise by choosing the floating share of their loan portfolio. While floating-rate loans hedge against interest rate risk, they do so by passing this risk onto borrowers which generates credit risk for the bank. As such, a key insight of the model is that banks are doing risk transformation, and that this implies a trade-off between managing interest rate risk and credit risk. The model generates implications for the data that depend on the share of a bank's loan portfolio that is floating rate. Specifically, the model predicts that banks with a higher share of floating-rate loans will see greater loan losses in response to a contractionary monetary policy shock.

Finally, I use microdata, in particular bank-level variation in the share of floating-rate loans, in a panel local projection framework to test the implications of the model. Consistent with

[9] For example, Van den Heuvel (2009) develops a bank capital channel of monetary policy which sees profits fall and leverage rise following contractionary monetary policy due to maturity transformation while Corbae and Levine (2023) also see profits fall and leverage rise in response to contractionary monetary policy as higher rates induce greater risk-taking.

the model, I find that banks with higher shares experience higher net interest income but also higher loan losses in response to contractionary monetary policy shocks. The effect on profits is ultimately negative for those with higher shares which results in a larger increase in leverage. This provides further evidence for the role of floating-rate loans in generating the loan-loss mechanism and has important implications for regions where loans are predominantly floating-rate (e.g., Europe).

The overall contributions of this paper lend support to the conclusions of Svensson (2017), Svensson (2018), and former Federal Reserve Chair Ben Bernanke that monetary policy should not target financial stability while also documenting an important channel through which tighter monetary policy adversely affects the stability of the banking system.[10] Svensson argues that an important cost of monetary policy that targets financial stability is that it weakens the economy by allowing lower inflation and/or higher unemployment than would otherwise be the case, hence reducing the economy's resilience to shocks. Importantly, these papers argue that this cost is larger than the potential benefit of reducing the probability of a crisis (e.g., by reducing bank leverage). However, even in these papers, there is an implicit acceptance that tight monetary policy reduces bank leverage. Without that key benefit, the argument in favour of monetary policy targeting financial stability appears substantially weaker, and the conclusions of Svensson (2017) and Svensson (2018) significantly stronger. Bernanke, Svensson, and I end up concurring with the Tinbergen (1952) rule which asserts that we need at least n policy instruments for n policy goals. Therefore, given the significant adverse impact contractionary monetary policy has on leverage, especially when the share of floating-rate loans is high, monetary policy should focus on its traditional mandate of price stability, leaving issues of financial stability to macroprudential policy.

The remainder of this paper is as follows. Section 2 describes the data used. Section 3 documents the time-series evidence. Section 4 explains where and why the theoretical literature is empirically inconsistent while Section 5 develops a model to highlight the role of floating-rate loans. Section 6 explores the implications of the model using microdata. Section 7 provides a conclusion. Appendix A summarises theoretical papers that predict contractionary monetary shocks reduce bank leverage, Appendix B provides further discussion about the differences between book leverage and market leverage, Appendix C presents robustness checks in relation to the time-series evidence, and Appendix D provides details of the theoretical model.

[10] https://www.brookings.edu/articles/should-monetary-policy-take-into-account-risks-to-financial-stability/

2 Data

This paper brings together both aggregate and individual bank-level data, ensuring that the individual bank-level data matches the aggregate data, and combines it with the Fed Funds Rates and different measures of monetary policy shocks. All data is either already at quarterly frequency or has been transformed to be at quarterly frequency. Finally, the data coverage is from the first quarter of 1984 up until the last quarter of 2006. Given that the 2007-08 global financial crisis (GFC) resulted in such substantive changes to the regulatory architecture, my analysis will focus on the period prior to the crisis. This section describes the different data and their sources.

2.1 Aggregate Banking Sector Time-Series Data

The aggregate banking sector time-series data is from the Federal Deposit Insurance Corporation (FDIC). Specifically, I obtain aggregated balance sheet and income statement data for all FDIC-insured institutions for each quarter starting in 1984 using the FDIC's Quarterly Banking Profile data. This provides me with accounting-based measures of different variables.[11]

From the aggregate balance sheet, I collect time-series data on four key variables. The first variable is total banking sector assets. While useful in its own right, the measure of total assets will mostly be used to normalise all remaining variables so that they are interpreted as a share of total assets. Second, I collect data on loans that are 30-89 days past due. This simply measures loans where the borrower is up to three months behind on a payment. The final two variables capture different measures of bank equity. The first is total equity which is also sometimes referred to as the net worth of the bank. Using this measure of equity, we can define the simple leverage of the bank as total assets divided by total equity. This is easily comparable across time, space, and banks. As such, when referring to leverage, I will be referencing simple leverage, unless otherwise specified. The second variable is regulatory equity which is also known as Tier 1 capital. This variable is a stricter definition of equity as it excludes several components from total equity such as revaluation reserves and hybrid capital instruments. The regulatory community argues that the Tier 1 Leverage Ratio (i.e., regulatory equity divided by average assets over the quarter) represents a more

[11] The accounting-based data is book leverage rather than market leverage. A number of papers (e.g., Adrian et al. (2019)) argue that this is the relevant measure for bank balance sheet decisions. See Appendix B for further discussion as well as a Figure C.6 for a robustness check based on a measure of market leverage from He et al. (2017). The robustness check shows the results are qualitatively similar across market and book leverage measures.

accurate measure of the losses a bank can withstand in response to a shock. Therefore, I will also use this measure of regulatory leverage (i.e., total assets divided by regulatory equity) for robustness.[12]

From aggregate income statements, I have four main variables. First, I collect data on aggregate profits as measured by net income from the income statements. Second, I collect data on dividends. The last two variables represent loan losses. Specifically, these variables are loan-loss provisions and net charge-offs. The former captures a bank's expectation of future loan losses, while the latter are recorded when a bank decides to finally write off a loan. If loan-loss provisions were perfectly estimated by banks, they would be exactly equal to net charge-offs over the long run. In the 10 years prior to the financial crisis, loan-loss provisions averaged around 110% of net charge-offs, which is consistent with regulatory examiners pushing for conservative estimates of expected losses.[13] Therefore, while loan-loss provisions might be slightly conservatively estimated, my analysis utilises provisions instead of net charge-offs for two reasons. First, provisions are recognised in a timelier fashion than charge-offs. Indeed, as soon as a shock occurs, banks will update their estimate of expected loss in accordance with accounting standards. Second, provisions directly impact bank profits and subsequently bank equity so there is a direct accounting-identity link between provisions and bank leverage, which will be important for my empirical work.[14] Nonetheless, the underlying mechanism in my empirical analysis remains the same whether one uses provisions or net charge-offs as both follow a very similar pattern in response to a contractionary monetary policy shock (see Figure C.1 in Appendix C).

2.2 Bank-Level Panel Data

Bank-level data requires a consistent time series that is merger adjusted. Therefore, I use the bank-level series of Drechsler et al. (2017). My bank-level analysis uses all the variables in the aggregate data and also includes the share of the loan portfolio that is effectively floating-rate. This floating-rate data starts from the second quarter of 1997. As such, the

[12] Note that this differs in two minor ways from the regulatory measure used in practice. First, the regulatory measure uses average assets over the quarter (see footnote 5 of https://www.kansascityfed.org/documents/8087/BankCapitalAnalysisTable_December31_2020.pdf) while, for data availability reasons, I use total assets at the end of the quarter. Second, I focus on leverage instead of the leverage ratio (one is just the reciprocal of the other) as it is far more intuitive.

[13] https://fraser.stlouisfed.org/title/economic-trends-federal-reserve-bank-cleveland-3952/economic-trends-november-5-2015-529746/loan-loss-provisioning-517772

[14] Note that there are some concerns that banks may manipulate the timing of loan-loss provisions for tax advantages. However, the 1969 and 1986 Tax Reform Acts largely removed these incentives (see Walter (1991)).

panel data analysis will all be after 1999 (to allow for sufficient lags). In Section 6, I show that the underlying bank-level data very closely matches the aggregate data for my sample of 1997 to 2006.

2.3 Monetary Policy Data

The monetary policy data has two components. The first is simply the Fed Funds Rate (FFR) which is directly from FRED. The second set of monetary policy data is more substantive. Specifically, I collect a number of different estimates of exogenous changes in monetary policy (i.e., monetary policy shocks). There is a large literature on constructing monetary policy shocks and a number of papers that compare and contrast the different shocks (see for example Ramey (2016)). This paper does not seek to evaluate the effectiveness of a given monetary policy shock measure. Instead, it focuses on how bank leverage responds to a given exogenous monetary policy shock. A benchmark monetary policy shock used in the literature is the shock series by Romer and Romer (2004) (hereafter the RR shock). Their identification strategy combined narrative methods with the Federal Reserve's (the Fed) own internal forecasts. Specifically, they used narrative methods to deduce a series of intended changes to the interest rate during the Fed's monetary policy meetings. Moreover, they separated the endogenous response of policy to information about the economy from the desired exogenous shock by regressing the intended funds rate change on the current rate and on the internal forecasts. The residuals of this regression are essentially the monetary policy shock. I use the updated RR shock series from Wieland and Yang (2020) which allows me to have a quarterly shock from 1984 to 2006. Given its prominence in the literature, the RR shock will be the monetary policy shock used in my baseline specification.

However, there have been specific concerns with the RR shock series. For example, Coibion (2012) finds that the results of Romer and Romer (2004), based on the RR shock series, are particularly sensitive to including the time period 1979-1982 as well as the number of lags. The former will not be an issue in my empirical work as my data start in 1984. I show that the latter is not an issue as my results are relatively robust to varying the number of lags.

To ensure my empirical results are not dependent on one specific measure of monetary policy shocks, I repeat my analysis with two additional monetary policy shock series that have a sufficient time series. I also choose shock series that are estimated using different identification strategies and as such have different features. While the RR shock relies on narrative identification, Gertler and Karadi (2015) (GK shock series) rely on high frequency identification, and Bu et al. (2021) (BRW shock series) utilise a heteroskedasticity-based partial least squares approach, combined with Fama-MacBeth style cross-sectional regressions. However, unlike the RR shock series which covers my entire sample, the GK shock series starts in 1990 and the BRW shock series does not start until 1994. One important feature of the BRW shock series is that Bu et al. (2021) show it contains no significant information effect.

2.4 Non-Monetary Shocks Data

To better understand the loan-loss mechanism, I utilise two additional series. While a contractionary monetary policy shock features a rise in the FFR and a decline in GDP, the two additional series have different economic implications which allows me to disentangle the drivers of loan losses.

The first series is an oil shock series. Oil shocks behave as a 'cost-push' shock and so typically do not feature a meaningful rise in the FFR but still result in a decline in GDP. The oil shock series I use is from Känzig (2021). He exploits the institutional features of the Organization of the Petroleum Exporting Countries (OPEC) and high-frequency variation in oil futures prices around OPEC announcements to identify an oil supply news shock. The time period of the shock series is sufficient to span my empirical exercise (i.e., 1984-2007).

The second series is not a shock per se but rather a measure of risk perceptions. Therefore, while analysis utilising this measure is more predictive in nature, rather than necessarily causal, it still provides important insights. Specifically, the measure of perceived risk I use is from Pflueger et al. (2020) who define it as the price of volatile stocks (PVS_t). It is calculated as the average book-to-market ratio of low-volatility stocks minus the average book-to-market ratio of high-volatility stocks. Given this definition, when PVS_t is high, agents are optimistic about the economy (e.g., banks report that they are loosening lending standards). Intuitively, one can think of an increase in PVS_t as acting like a positive demand shock and so should result in a rise in the FFR and GDP. I choose this particular measure of risk perceptions for several reasons. First, and perhaps most importantly, Pflueger et al. (2020) introduce this measure to explicitly evaluate risk-centric theories of business cycles (e.g., Caballero and Simsek (2020b)). Such theories present one important avenue for understanding the interactions between monetary policy and financial stability. Indeed, such models have been extended to show how monetary policy that leans against the wind might have financial stability benefits (see Caballero and Simsek (2020a)).[15] Second, changes in this financial market measure forecast well changes in the real economy. Finally, the time period of the series spans my empirical exercise.

3 Time-Series Evidence

My overall empirical approach uses existing measures of exogenous monetary policy shocks in the Jordà (2005) local projection method to estimate impulse responses using data from the start of 1984 until the end of 2006 (unless otherwise specified). This is sometimes referred to as the LP-IV approach (see for example Stock and Watson (2018)). Specifically, I estimate the following for each variable z at each horizon h:

$$z_{t+h} = \alpha_h + \sum_{l=0}^{L} \beta_{h,l} Shock_{t-l} + \sum_{m=1}^{M} \gamma_{h,m} z_{t-m} + \sum_{q=2}^{4} \delta_q Quarter_{qt} + \epsilon_{t+h}, \quad h = 0, \ldots, 16 \quad (1)$$

where z refers to the outcome variable of interest, $Shock$ refers to the exogenous monetary policy shock measure, and $Quarter$ represents quarterly dummies. The impulse response function is the sequence $\{\beta_{h,0}\}_{h=0}^{H}$ which captures the response of z at time $t+h$ to the shock at time t. In my baseline specification, the lag length is $L = M = 16$ quarters. In line with recent work by Montiel Olea and Plagborg-Møller (2021) on lag-augmented local projections, I use heteroskedasticity-robust standard errors.[16]

The lead-lag exogeneity condition is an important requirement for my specification, and indeed LP-IV approaches more broadly. Stock and Watson (2018) highlight that the main concern is that the shock at time t is correlated with past values of the outcome variable. As such, they suggest a simple test: the shock (i.e., the instrument) should be unforecastable in a regression of the shock at time t on the lags of the outcome variable (z_t in my case). Therefore, I regress the RR shock on 16 lags of leverage and find little evidence of predictability. Specifically, I find that each lag is individually statistically insignificant, the F-statistic when jointly testing all 16 lags also shows statistical insignificance.

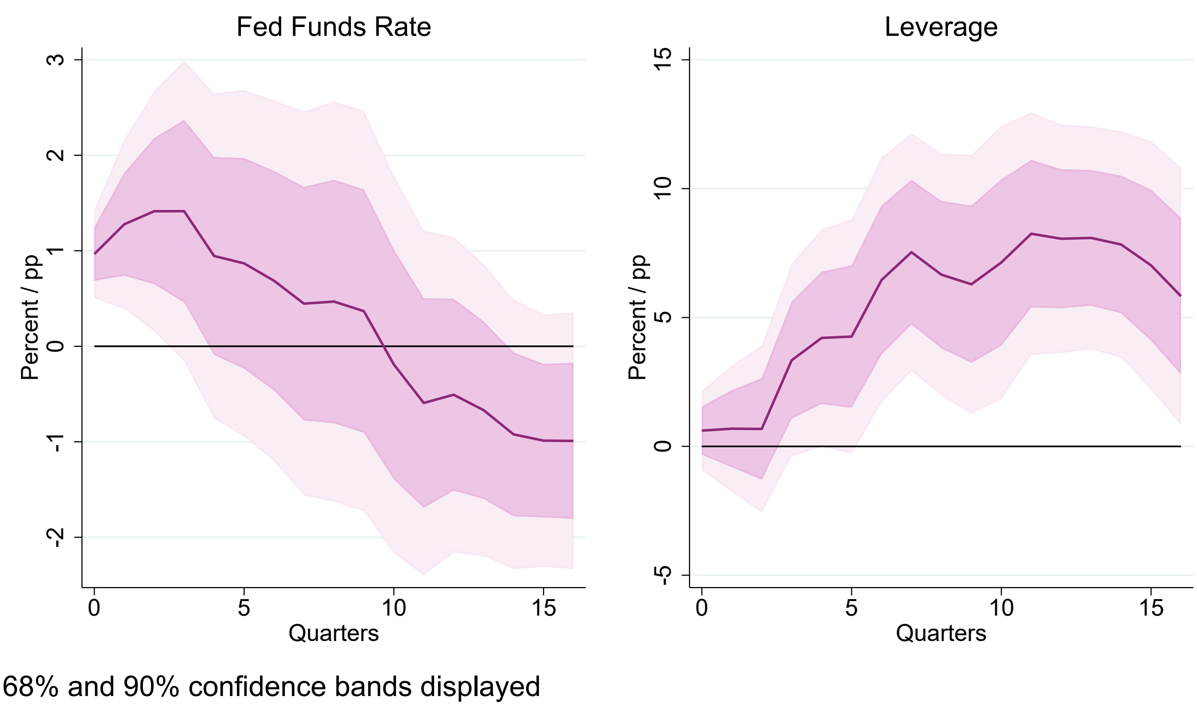
[15] Note that in a recent paper, Goldberg and López-Salido (2023) extend the framework of Caballero and Simsek (2020b) and show that leaning against the wind may worsen financial stability.

[16] Montiel Olea and Plagborg-Møller (2021) highlight that with lag-augmented local projections (i.e., where lags of the outcome variable are included as regressors) it is preferable to use heteroskedasticity-robust standard errors instead of Newey-West. They also explain that in the autoregressive literature, "lag augmentation" refers to the practice of using more lags than suggested by the true autoregressive model.

3.1 The Response of Leverage to a Monetary Policy Shock

My baseline specification is to estimate (1) using data from the first quarter of 1984 until the last quarter of 2006 with 16 lags and the RR shock series. Figure 1 below depicts the impulse responses of the FFR and leverage.

Figure 1: Impulse Response of Leverage to Contractionary Monetary Shock



The result above shows that a contractionary monetary policy shock that induces an increase in the FFR of about 1 percentage point significantly increases bank leverage by about 5 percent within a year, which then hovers around 8 percent higher for the remaining three years. This is meaningful response, both in size and persistence. As highlighted earlier, this is in strong contrast to the claims from much of the theoretical literature.

Given the result goes against much of the predictions in the literature and that a core objective of this paper is to provide a robust leverage moment to inform macroeconomic models, it is important to test the robustness of this finding. First, the main result in Figure 1 uses the simple definition of leverage described in Section 2.1 (i.e., total assets divided by total equity). In Figure C.2, I show the same analysis when using the simple measure of leverage (i.e., total assets divided by regulatory equity). The results do not change in any meaningful way. Next, in Figure C.3, I re-estimate (1) using different time periods. Specifically, I reduce the time horizon by three years each time so that I estimate over the period 1987-2006, 1990-2006, and 1993-2006. While the period 1989-92 contained a number of regulatory changes relating to bank leverage, the result is remarkably consistent before and after this period. Given the concerns highlighted by Coibion (2012) about the sensitivity to different lag lengths being used, in Figure C.4, I re-estimate (1) with 12 lags, 8 lags, and 4 lags (i.e., 3, 2, and 1 year, respectively). While the precision of the estimates varies across specifications, all of them have leverage rising eventually, though the 8-lag specification to a lesser extent.

My final, and perhaps strictest, robustness test is to use completely different shock series, in particular, ones that use distinct identification strategies. To ensure comparability, I estimate all of them using data from 1994 until 2007 as this is the largest overlapping period. Given the shorter time-horizon, I use 4 lags, otherwise the specification is as in (1). Figure C.5 shows the results from using the three different shocks series from Romer and Romer (2004), Gertler and Karadi (2015), and Bu et al. (2021), respectively. Remarkably, the result remains reasonably consistent despite using different shocks. I also repeat this exercise with the measure of market leverage from He et al. (2017). Specifically, in Figure C.6, I show how market leverage responds to each different shock and is qualitatively similar to the results for book leverage and quantitatively larger. The robustness of the result warrants further exploration into the possible mechanisms to understand what is driving the increase in leverage in response to contractionary monetary policy shocks.

3.2 The Loan-Loss Mechanism

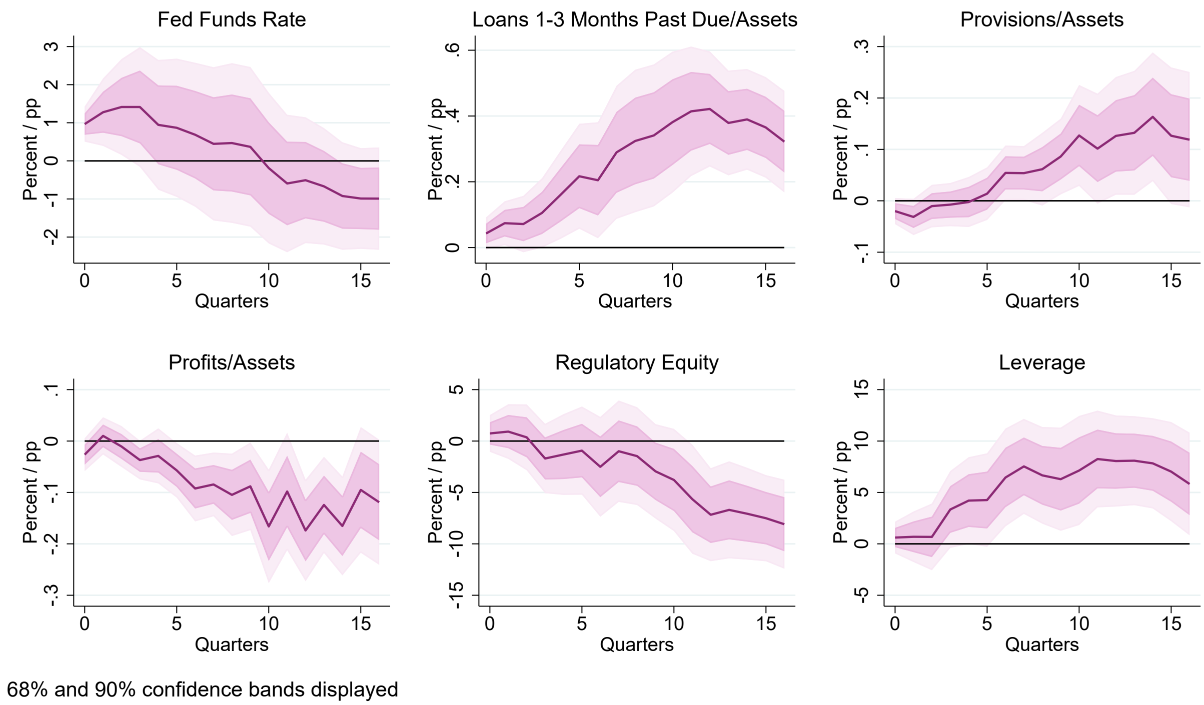
The literature highlights several different mechanisms that might cause an increase in interest rates to decrease leverage. One of the more intuitive reasons is that higher interest rates make debt financing more expensive relative to equity financing for banks. Given banks decrease the size of their balance sheets in response to contractionary shocks, the decrease in total liabilities will be driven more by a fall in debt liabilities than equity. This substitution effect therefore predicts that higher interest rates reduce bank leverage.

For leverage to rise overall, it must be that the fall in equity is more consequential. As such, I posit an additional mechanism, which I term the *loan-loss mechanism*, that might be driving the overall response in leverage (and offsetting the substitution effect). The mechanism is simple and intuitive and is best described in three key steps. First, a rise in interest rates leads to greater difficulty for borrowers to repay loans which leads to an increase in the proportion of loan repayments that are missed. This should result in *(i) an increasing* *proportion of loans past due* and *(ii) a delayed but increasing proportion in loan-loss provisions*. The latter rises as banks raise their estimates of expected losses due to the unexpected growth in missed loan repayments. Second, (i) and (ii) imply greater loan losses overall and therefore should result in *(iii) decreasing profits*. Finally, given changes in bank equity are largely driven by changes in profits, decreasing profits should lead to *(iv) decreasing bank equity* and if the overall fall in equity is more important than the fall in assets, then we would expect *(v) increasing leverage* as it is just the ratio of assets to equity.

To test the aforementioned mechanism, I estimate my benchmark specification (i.e., (1) with the RR shock, 16 lags, and data from 1984-2006) separately for each of the five variables emphasised in the paragraph above. Specifically, each of the variables will be z in (1). Figure 2 shows the results of this exercise by showing how these variables respond to a contractionary monetary policy shock.

The first panel (top-left) simply reproduces the impulse response function of the FFR and so the remaining analysis can be interpreted as responding to a monetary policy shock that induces the FFR to increase by around one percentage point on impact. The second panel (top-middle) shows that loans that are up to three months past due increase by nearly 0.5 percentage points as a proportion of total assets at their peak. This is a significant rise as the average share of loans past due during 1984-2006 is around 0.8%. This confirms (i). Similarly, the third panel (top-right) shows that provisions as a proportion of total assets also increase, albeit at a slower pace, which confirms (ii). The greater than 0.1 percentage point rise in provisions as a share of assets is also significant as it is roughly double its average by the end of the projection horizon. The fourth panel (bottom-left) shows that profits as a proportion of total assets decrease by around 0.15 percentage points from an average of around 0.22% at around the same time as when provisions rise which confirms (iii). The fifth panel (bottom-middle) shows regulatory equity falls by nearly five percent within two years and continues to fall to nearly a ten percent decline by the end of the horizon which confirms (iv). Finally, the sixth panel (bottom-right) simply reproduces the main finding in Figure (1) (i.e., that leverage rises) and thus confirms (v).

Figure 2: Mechanism Underlying Leverage Response



3.3 Importance of the Loan-Loss Mechanism

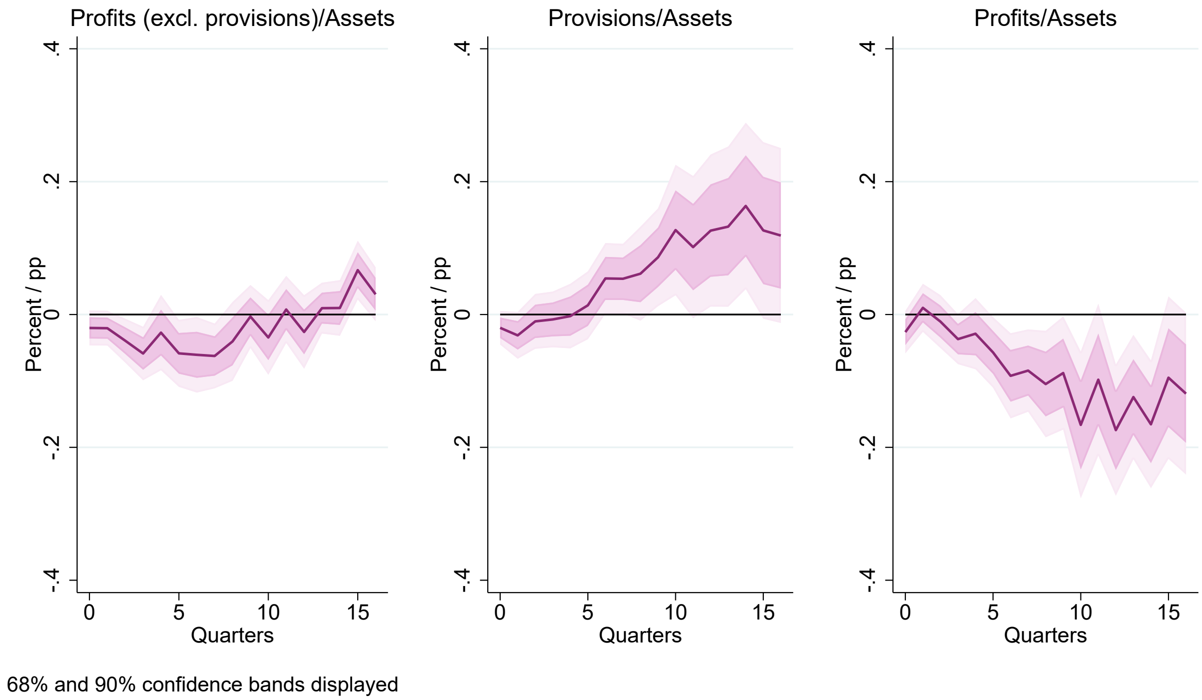
Despite the evidence supporting my proposed mechanism, it is still possible that there are other mechanisms that might be more important in terms of driving the overall increase in leverage. One general approach to deal with this kind of concern is to rule out alternative mechanisms. However, such an approach is not exhaustive as it is difficult to know all possible alternative mechanisms and therefore the best we can usually do is to rule out the most likely contenders. As such, in this section, I take advantage of accounting identities to show that my proposed mechanism explains most of the variation in leverage. Therefore, instead of relying on ruling out possible alternative mechanisms, I show empirically the importance of my mechanism directly.

For my mechanism to be driving the overall response, I need to document two steps. First, that the increase in loan losses, as measured by provisions, in response to the contractionary monetary policy shock (top-right panel of Figure 2) is *causing* most, if not all, of the decrease in profits (bottom-left panel of Figure 2). Profits can be decomposed into several components on a bank income statement. Specifically, one can utilise the following accounting identity:

$$\frac{\text{Profits (excluding provisions)}_t}{\text{Assets}_t} - \frac{\text{Provisions}_t}{\text{Assets}_t} = \frac{\text{Profits}_t}{\text{Assets}_t} \tag{2}$$

where the first term is constructed by adding together net interest income, net noninterest income, net gains on securities, and subtracting taxes. Therefore, if my proposed mechanism is important, it should be the case the variation in profits is driven by the variation in provisions rather than the other income terms. Figure 3 below shows the impulse responses of each term in (2) which are obtained by estimating (1) with each of those terms as the outcome variable z_t.

Figure 3: Decomposing the Profit Decline

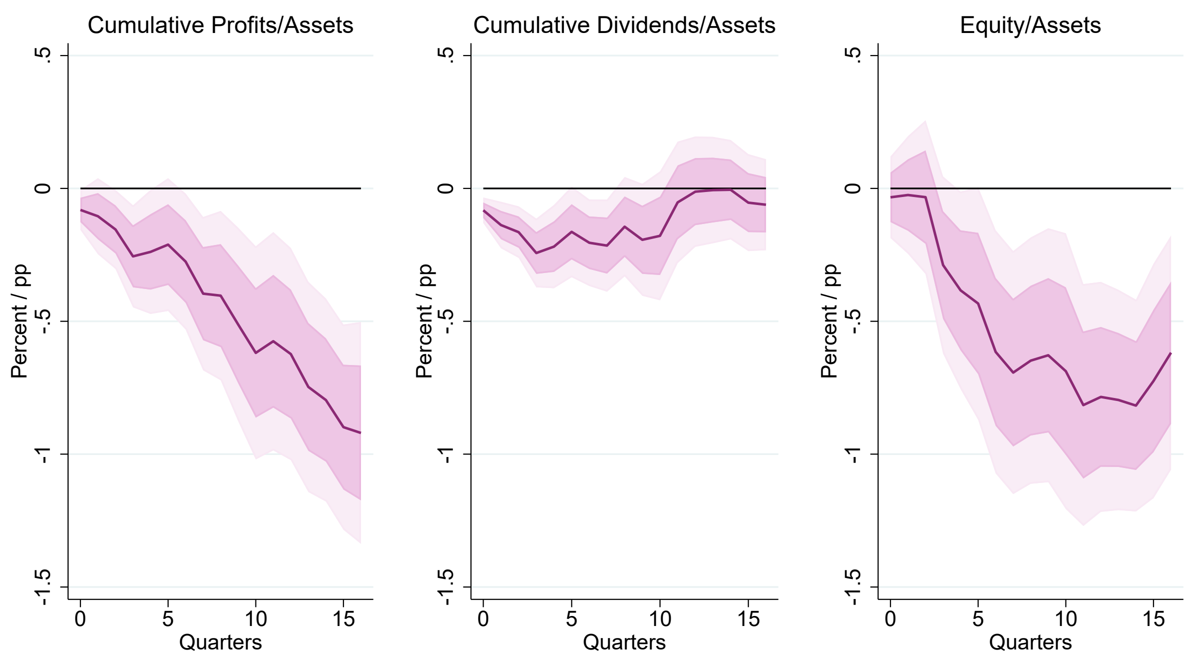


The variation in overall profits is almost entirely driven by the variation in provisions with the remaining variation (captured by profits excluding provisions) being relatively immaterial. This is consistent with findings in the literature that document the stability of bank net interest income (e.g., Drechsler et al. (2021)). Therefore, my mechanism appears to be the key driving force behind the fall in profits. The second step I would need to document is that the fall in profits (bottom-left panel of Figure 2) is *causing* most, if not all, of the increase in leverage (bottom-right panel of Figure 2). The accounting identity is less straightforward in this case as we are utilising information from both the income statement and balance sheet. The approach I take is to utilise the following identity for a balance sheet item at time t:

$$\frac{\text{Cumulative Profits}_t}{\text{Assets}_t} - \frac{\text{Cumulative Dividends}_t}{\text{Assets}_t} \approx \frac{\text{Equity}_t}{\text{Assets}_t} = \frac{1}{\text{Leverage}_t} \tag{3}$$

Note that (3) shows we need a measure of cumulative profits to transform an income statement measure (a flow) to a balance sheet measure (a stock). Equity at time t is constructed by adding all profits earned before t to the starting equity then subtracting all dividends paid before t and finally making some accounting adjustments (e.g., revaluations) at horizon t. While I do not have a direct measure of the accounting adjustments, I can construct the two cumulative measures: cumulative profits and cumulative dividends (accumulated from 1984 to $1984 + t$). Note that the final term, equity divided by assets, is simply the inverse of leverage. Therefore, if my proposed mechanism is important, it should be the case that the variation in leverage (or the inverse of leverage) is driven by the variation in profits. Figure 4 below shows the impulse response of the first, second, and final term of (3) which are obtained by estimating (1) with each of those terms as the outcome variable z_t.

Figure 4: Decomposing the Leverage Increase



68% and 90% confidence bands displayed

As can be seen, the variation in overall leverage (or more precisely the inverse of leverage) is largely driven by the variation in cumulative profits. As one might expect due to the potential penalties associated with reducing dividend payments (Guttman et al. (2010)), the response of cumulative dividends is muted. Moreover, while not shown, there is little unexplained variation after accounting for cumulative profits and dividends which implies that accounting adjustments would not be driving the overall response. Therefore, I have shown that my mechanism is driving the overall response in leverage as the decrease in profits is largely driven by the increase in loan losses and the increase in leverage is largely driven by the decrease in profits.

One take-away from this section thus far is that the macro-banking models used to understand monetary policy and its interaction with financial stability should allow for, at the very least, the potential for contractionary interest rates to raise bank leverage, given the robustness of the empirical moment. Furthermore, understanding what specifically drives loan losses will be key to determining the features that might be important when developing such models.

3.4 Drivers of Loan Losses

In Section 3.3, I showed that the rise in loan losses drives the variation in bank leverage in response to a contractionary monetary policy shock. However, it is not clear precisely why such a shock causes loan losses to rise in the first place. In this section, I attempt to shed light on this question using aggregate data.

Intuitively, one can think of contractionary monetary policy as leading to unexpected loan losses for two broad reasons. First, a higher FFR may directly raise the loan-servicing cost on floating-rate loans of any maturity or fixed-rate loans with a short maturity. This would reduce a borrower's ability to repay and hence raise loan losses. Second, a higher FFR may subsequently reduce incomes due to its recessionary impact as measured by a fall in GDP. This would also reduce a borrower's ability to repay and hence raise loan losses. However, a contractionary monetary policy shock both increases loan-servicing costs by directly raising the FFR and reduces borrower income by reducing GDP. Therefore, it is unclear by looking at such shocks whether a higher FFR or lower GDP is driving loan losses.

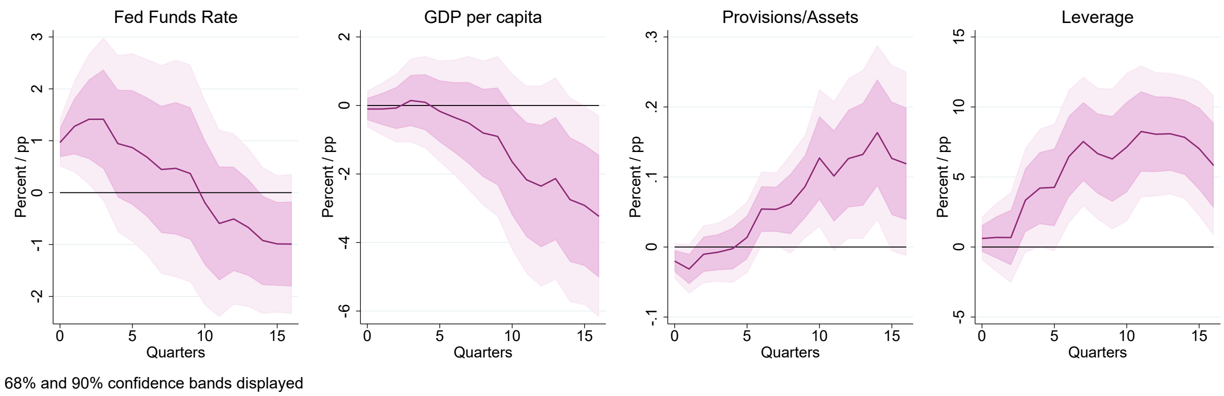
One approach to determine whether a higher FFR or lower GDP is driving loan losses is to consider variation that only affects one of the two factors. Cost-push shocks provide such variation as central banks are less likely to react to such shocks by raising interest rates. Therefore, cost-push shocks often feature little to no change in the FFR and hence minimal direct impact on loan-servicing costs but still have a decline in GDP and hence a reduction in borrower income. As highlighted in Section 2.4, oil shocks are a clear example of cost-push variation. Indeed, in response to an oil shock, we would expect a fall in GDP with little reaction of the FFR. This leads to the following empirical test: if loan losses are driven by the direct impact of the FFR on loan-servicing costs, then we expect loan-loss provisions to rise in response to a contractionary monetary shock but not in response to an oil shock.

Figure 5 presents the results of the empirical test. Specifically, Figure 5a shows the effect of a contractionary monetary policy shock on the FFR, GDP, loan-loss provisions and bank leverage while Figure 5b shows the effect of an oil shock on these same variables. As expected, both result in a decline in GDP, while only the monetary policy shock features a meaningful rise in the FFR. Interestingly, loan-loss provisions rise in response to the contractionary monetary policy shock but not in response to the oil shock which suggests that loan losses are more likely driven by the direct impact of the FFR on loan-servicing costs. Moreover, the fact that leverage only rises in the case of the monetary shock provides further validation of the importance of the loan-loss mechanism in driving variation in leverage.

Figure 5 also extends the empirical test with the risk perception series mentioned in Section 2.4. While the risk perception series is not a shock series, one can nonetheless use it as an additional robustness check. A rise in PSV (the measure of risk perception) can be thought of as a decline in risk and an increase in optimism by agents in the economy. Therefore, it behaves in a way similar to a positive aggregate demand shock. Figure 5c shows both the FFR and GDP rise, as is typically the case in response to a positive demand shock. Strikingly, loan-loss provisions rise despite the improvement in economic activity which suggests that the direct impact on loan-servicing costs due to the rise in the FFR is especially important.

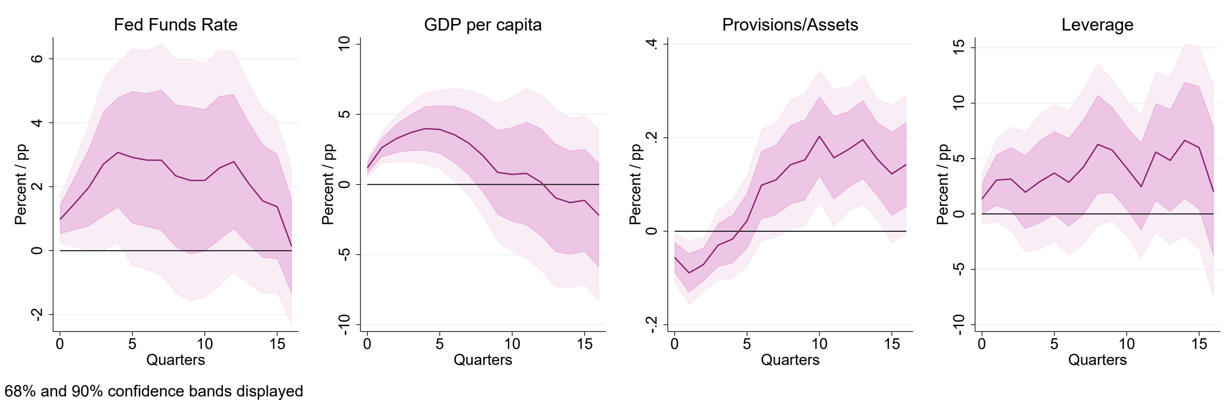
Figure 5: Disentangling the Drivers of Loan Losses

(a) Monetary Policy Shock



(b) Oil Shock

(c) Risk Perception (PVS)



Overall, Figure 5 lends support to the idea that loan losses are driven by higher interest rates directly increasing loan-servicing costs. Therefore, the overarching loan-loss mechanism is as follows: higher interest rates result in higher loan-servicing costs on loans more directly exposed to interest rates such as floating-rate loans. Loan losses appear to be driven by these higher loan-servicing costs. The increase in loan losses drives a decline in bank profits which reduces bank equity, and ultimately increases bank leverage. While the analysis highlights the potential role of floating-rate loans, it is still suggestive evidence. Therefore, in Section 5, I formalise the role of floating-rate loans in a simple banking model and in Section 6, I test it using microdata. Nonetheless, given I have now documented a clear mechanism, it is natural at this point to ask where the theory deviates from the empirical evidence whether this mechanism is captured in existing models.

4 Why do so many models generate a counterfactual leverage response?

In Section 3, I documented a robust finding: contractionary monetary policy shocks increase bank leverage. This result is almost entirely driven by the loan-loss mechanism whereby an unexpected increase in interest rates drives up loan losses at banks which reduces bank profits, subsequently eroding their equity, and ultimately increasing their leverage. I also provided suggestive evidence that floating-rate loans may play an important role in this mechanism. However, this mechanism, and the role of floating-rate loans in generating credit risk, is largely missing from the theoretical literature. In addition to missing the empirically dominant mechanism, much of the theoretical literature makes the opposite claim that leverage falls in response to a contractionary shock. While some do make an empirically consistent claim, they entirely ignore the loan-loss mechanism, and as a result have other predictions that are inconsistent with the observed data.

The divergence of the literature from the empirical evidence appears to derive from three broad, though not necessarily mutually exclusive, modelling choices: relying on a substitution effect; relying on procyclical leverage; and, relying on an a profitability channel. In this section, I will explain how each of these modelling choices leads the model to generate empirically inconsistent predictions as well as highlighting the types of papers in each category.

4.1 Models that rely on a substitution effect

The substitution effect is perhaps the most intuitive and simple mechanism that generates a counterfactual response of leverage to contractionary monetary policy shocks. Specifically, the substitution effect implies that an increase in the interest rate raises the relative cost of debt financing for banks and so banks substitute away from debt financing. A reduction in the reliance on debt financing is equivalent to a reduction in leverage. All else equal, this implies that higher rates reduce bank leverage, a claim I have shown to be empirically inaccurate.

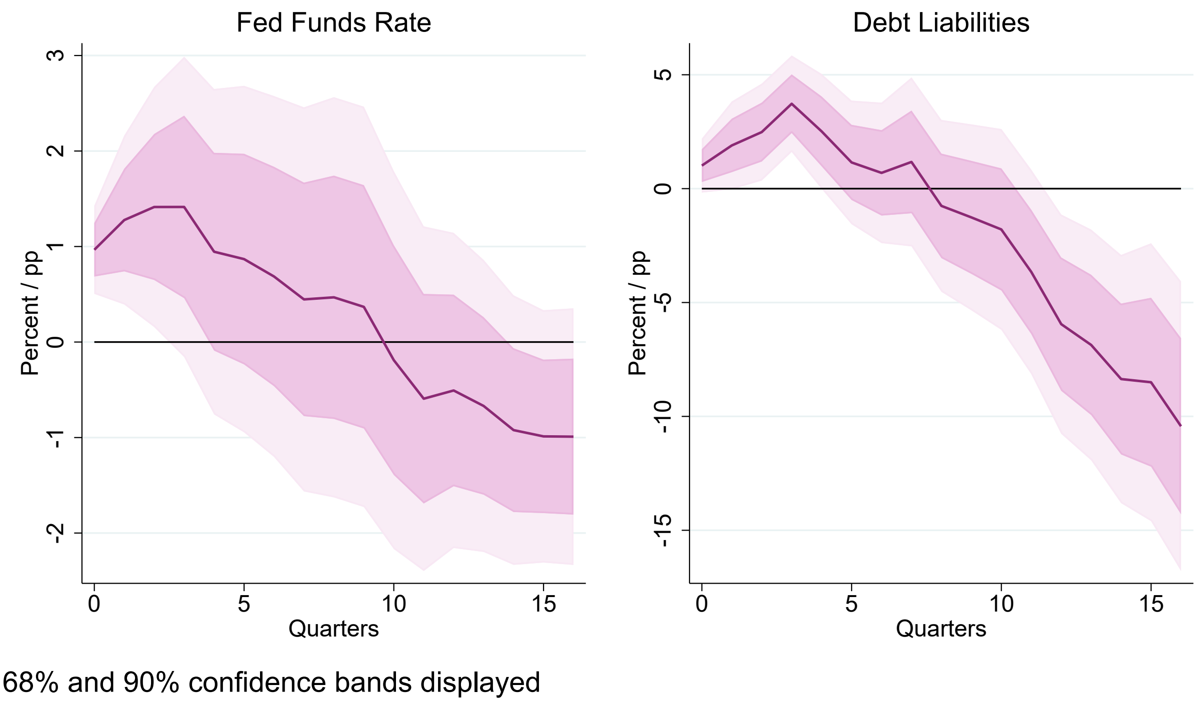
Given the relative simplicity of the substitution effect, I will not go into the details of any particular model; rather I will briefly highlight some examples. The overarching message of these models is summarised in the review paper by Ajello et al. (2022): "Accommodative monetary policy reduces the cost of funding for banks, and thus may increase reliance on debt by banks."

This type of mechanism is common across the literature. For example, Angeloni and Faia (2013) introduce banks to a conventional DSGE model with nominal rigidities. Banks exist in the model because they can extract more liquidation value from projects. Banks are financed with deposits and equity and they are also subject to the risk of a run. The return on a project is equal to the expected value plus a random shock. Moreover, a run occurs if the outcome of a project is too low to repay depositors. If there is a contractionary monetary policy shock, the deposit rate increases which reduces the bank's ability to repay its depositors. This increases the probability of a run and so the bank reduces its deposits which decreases its leverage. Indeed, this mechanism is essentially a substitution effect that is induced by an endogenous run probability.

Another, albeit very different, example is the model by Drechsler et al. (2018b). They develop a dynamic asset pricing model in which monetary policy affects the risk premium component of the cost of capital. Risk-tolerant agents (banks) borrow from risk-averse agents by taking deposits to fund levered investments. Leverage exposes banks to funding shocks. As such, banks hold liquidity buffers composed of safe assets (e.g., US Treasuries) to insure against such funding shocks. If the central bank raises interest rates, it raises the liquidity premium because the cost of holding liquid securities increase. This increase in the price of funding shock insurance means banks will reduce their liquidity buffers. Therefore, with lower insurance, banks reduce their exposure to funding shocks by reducing deposits. Again, this is essentially a substitution effect but in this model it is induced by the dynamics of liquidity insurance.

For the substitution effect to increase leverage in response to a contractionary monetary policy shock, the following must be true: (i) debt liabilities fall; and (ii) the fall in debt liabilities is greater than the fall in equity. In Figure 6 below, I show that debt liabilities do fall in my data, consistent with (i). However, the more important contribution of my empirical analysis is that (ii) does not hold in the data. As I show in Section 3, the empirically dominant mechanism is the loan-loss mechanism which not only offsets the effect on leverage from falling debt liabilities, but actually leads to a reversal in sign such that contractionary monetary policy shocks increase leverage.

Figure 6: Impulse Response of Debt Liabilities to Contractionary Monetary Shock

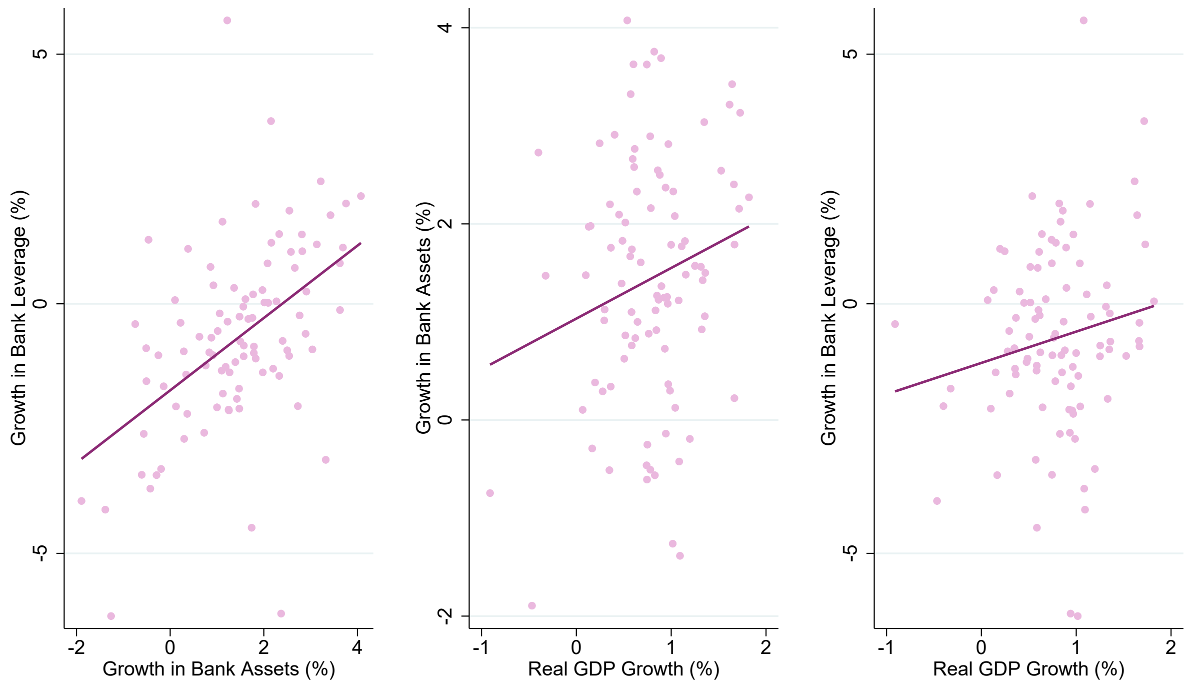


4.2 Models that rely on leverage procyclicality

Like models based on the substitution effect, this class of models are similarly eclectic in their underlying structures, but have the common feature that the results are driven by the procyclicality of bank leverage. This procyclicality has been widely documented in the literature (e.g., Adrian and Shin (2010), Laux and Rauter (2017), and Adrian et al. (2019)). Such studies typically document this procyclicality by showing a positive relation between the growth of bank leverage and the growth of bank assets. The latter is considered procyclical as bank lending grows during a boom and shrinks during a bust. Figure 7 shows that leverage is indeed procyclical in my data. Specifically, it shows the positive correlation between the growth in bank leverage and the growth in bank assets as well as growth in bank leverage and GDP growth directly.[17]

[17] One can also do a simple regression of the growth in leverage on GDP growth which would yield a positive coefficient with a t-statistic of 1.87.

Figure 7: Procyclicality of Bank Leverage



In these models, a contractionary monetary shock will reduce output and because leverage is procyclical, it will also reduce leverage. Such models rely on leverage procyclicality in different ways. Some use procyclicality of leverage as a target or a measure of success of the model. For example, Rannenberg (2016) points out that by introducing a firm sector in the spirit of Bernanke et al. (1999) to the model of Gertler and Karadi (2011) (he terms this combined model the "full model"), he is able to generate procyclical leverage. Specifically, he concludes that "in the full model, bank leverage declines in response to contractionary monetary policy and productivity shocks, which allows the full model to match the procyclicality of bank leverage in U.S. data. By contrast, bank leverage in the Gertler–Karadi-type model is strongly countercyclical."

However, one cannot match conditional moments in a model to unconditional moments in the data as these are two entirely different measures. Consider the evidence in Figure 5. Both negative oil shocks and contractionary monetary policy shocks reduce GDP. While the former decreases leverage, the latter increases it. One cannot conclude whether leverage is unconditionally procyclical or countercyclical from this information alone. Indeed, looking at the correlations of leverage with GDP from these impulse responses alone would result in the conclusion that leverage is both procyclical and countercyclical. Moreover, as Galí (1999) points out, evaluating models based on their ability to match unconditional moments in the data can be highly misleading as the model may perform well according to that criterion despite providing a very distorted image of the economy's response to different types of shocks. Therefore, a conditional leverage moment, as I have documented, serves as a much sharper test of the model, and one that directly provides insight on the role of monetary policy, though one must ensure that they make like-for-like comparisons.

Like Rannenberg (2016), many papers do not distinguish between the procyclicality of leverage in the data (an unconditional moment) and the response of leverage to monetary policy (a conditional moment). This leads to the conclusion that monetary policy should 'lean against the wind' by tightening in response to increasing leverage. One particularly prominent, albeit highly stylised, paper that makes this type of argument is Woodford (2012). He provides a simple and reduced-form model of the way in which endogenous state variables affect the probability of a crisis and what this means for optimal monetary policy. To highlight in more detail how issues arise when using this type of procyclicality, I will focus on the set-up in Woodford (2012). The advantage of this model is in its simplicity which allows one to easily see the intuition.

The model is a fairly typical three-equation New Keynesian (NK) model except with two types of households: those that are credit constrained and those that are not. This is represented by the existence of a credit friction Ω_t which essentially measures the gap at any point in time between the marginal utilities of the two types of households. Woodford (2012) then derives a modified intertemporal IS equation:

$$y_t - g_t + \chi\Omega_t = \mathbb{E}_t[y_{t+1} - g_{t+1} + \chi\Omega_{t+1}] - \sigma[i_t - \mathbb{E}_t\pi_{t+1}] \tag{4}$$

where y_t is the output gap, g_t is government purchases, i_t is the nominal interest rate set by the central bank, π_{t+1} measures inflation between period t and $t+1$, and the coefficients satisfy $\chi, \sigma > 0$. All variables represent deviations from the steady state. The only difference between equation (4) and the standard IS equation is the credit friction. Indeed, as one would expect, a higher credit friction would behave similarly to the effects of a reduction in government purchases. Therefore, real aggregate demand now also depends on the severity of credit frictions in the economy. A similar approach yields a modified NK Phillips curve.

$$\pi_t = \kappa_y y_t + \kappa_\Omega \Omega_t + \beta\mathbb{E}_t\pi_{t+1} + u_t \tag{5}$$

where u_t is a composite term denoting the different exogenous cost-push factors. Again, the Phillips curve is exactly the same as that in the standard model except for the additional credit friction. A key component of the model is to incorporate some endogeneity in how the credit fiction evolves. Ω_t is assumed to always be in one of two states: a normal state (low value of Ω_t) and a crisis state (high value of Ω_t). Each period, the probability of entering the normal state when in a crisis state is δ, while the probability of entering the crisis state when in a normal state is γ_t which is an increasing function of bank leverage (L_t). Intuitively, as leverage is higher, the probability of going into a crisis is higher. Therefore, to complete the model, Woodford (2012) connects leverage with the remaining endogenous variables by postulating a simple law of motion:

$$L_t = \rho L_{t-1} + \xi y_t + v_t \tag{6}$$

where v_t represents an exogenous disturbance term and importantly ξ is assumed to be positive. Therefore, this law of motion embeds the procyclicality of leverage as leverage is an increasing function of the output gap. Indeed, this type of assumption is the core reason models in this class are unable to generate empirically consistent dynamics.

To complete the framework, Woodford (2012) assumes that the goal of policy is to minimise the following loss function:

$$\frac{1}{2} E_0 \sum_{t=0}^{\infty} \beta^t \left[\pi_t^2 + \lambda_y y_t^2 + \lambda_\Omega \Omega_t^2\right] \tag{7}$$

This is an intuitive form of the loss function as the central bank is simply minimising losses from inflation, output, and financial instability. However, the problem arises because of the way in which monetary policy and leverage now intertwine. A contractionary monetary shock will reduce the output gap as is typically the case. However, because of equation (6), the same shock will also reduce leverage. Indeed, Woodford (2012) concludes that the model implies one should use monetary policy to 'lean against' a credit boom (which in this model would be to reduce leverage) even if it requires missing target values for inflation and the output gap. In this model, the primary prediction is inconsistent with my empirical findings and as such the consequences of following such a rule are more severe. For example, consider a central bank that is following such a rule when inflation and the output gap are on target, but credit frictions are far too high. As Woodford (2012) mentions, it would be appropriate for the central bank to use contractionary monetary policy. The consequences would not only see both inflation and the output gap falling below target, but leverage would actually rise due to increasing loan losses which are missing from the model. This would unambiguously worsen losses according to the central bank loss function.

The Woodford (2012) model, while highly stylised, is very influential as it builds on the workhorse NK structure. However, it entirely misses the empirical loan-loss mechanism and instead relies on a postulated law of motion that embeds procyclicality. This procyclicality between leverage and output is correlational, not structural. Indeed, when models rely on procyclicality in this way, they typically argue that monetary policy should lean against the wind because contractionary shocks, contrary to the evidence in this paper, reduce bank leverage.

4.3 Models that rely on a profitability channel

In this class of models, profitability and leverage move together and are connected by an incentive compatibility leverage constraint. Furthermore, these models have the feature that *any* negative shock will increase bank profitability as well as bank leverage. As such, while these models correctly show that leverage increases in response to a contractionary monetary policy shock, they have leverage increasing alongside an increase in profitability (which I term the profitability channel). This is at odds with the empirical evidence that the increase in leverage following a contractionary monetary policy shock *is caused by* a decrease in profits.

This class of models build on the canonical models of Gertler and Kiyotaki (2010) and Gertler and Karadi (2011) which are some of the most influential macroeconomic models featuring a banking sector. The defining feature of this class is that they use a Gertler-Karadi-Kiyotaki-type constraint (i.e., an incentive-compatibility leverage constraint) to model banks which generates the profitability channel.[18]

Given the empirical inconsistencies arising from using a Gertler-Karadi-Kiyotaki-type constraint are intricate, I will focus on the specific set-up in Gertler and Karadi (2011) to highlight how the issues arise. I choose Gertler and Karadi (2011) for two reasons. First, given it is one of the foundational models, most models in this class typically have the same underlying structure. Second, while Gertler and Kiyotaki (2010) is also a foundational model, Gertler and Karadi (2011) incorporates nominal rigidities and so is better able to highlight the impact of monetary policy on bank leverage.[19]

[18] This modelling approach is widely used in the literature, e.g., Gertler and Kiyotaki (2015), Maggiori (2017), Gertler et al. (2020), Ghote (2021), and Sims and Wu (2021).

[19] While Gertler and Kiyotaki (2010) is a purely real model, both models have the same underlying structure; one can think of Gertler and Karadi (2011) as extending the Gertler and Kiyotaki (2010) model to allow for nominal rigidities.

Gertler and Karadi (2011) builds on the seminal monetary dynamic stochastic general equilibrium (DSGE) models of Christiano et al. (2005) and Smets and Wouters (2007) by incorporating banks that transfer funds between households and non-financial firms. Banks exist as they have expertise in evaluating and monitoring borrowers and a simple agency problem between banks and households constrains the ability of banks to raise deposits. The model features five different agents: households, goods producers, capital producers, monopolistically competitive retailers, and banks. Monetary policy is characterised with a simple Taylor rule. Without banks, the model is isomorphic to Christiano et al. (2005) and Smets and Wouters (2007). While the model is a sophisticated general equilibrium (GE) model, one need only analyse the banking block of the model to understand how Gertler and Karadi (2011) generate the correct leverage prediction as well as where the model is diverges from the empirical evidence. Therefore, I will focus on the partial equilibrium of the banking block to highlight the intuition and precisely depict the underlying mechanisms. For completeness, I will also show the results from simulating the full GE model to show that the core insights obtained from examining the banking block do not change once we account for GE dynamics.

I will follow a stylised version of the Gertler and Karadi (2011) model. Banks obtain deposits, B, from households. These funds are then 'lent' to non-financial firms which gives banks a claim on those firms where S depicts the quantity of those claims.[20] Each claim has price Q. Therefore, the net worth (equity), N, of the bank is given by the following balance sheet constraint:

$$N = QS - B \tag{8}$$

The stochastic return on a single unit of lending is R_k while a single unit of deposits pay a non-contingent return R. Both returns are determined endogenously. Given this structure, the bank's objective is to maximise the expected value of the bank, V, which is simply maximising the difference between the expected earnings on assets and interest payments on liabilities. The value of the bank is therefore given by the following:

$$V = R_k QS - RB \tag{9}$$

[20] Technically, these loans by banks to non-financial firms are perfectly state-contingent debt and so are better thought of as equity.

We can plug in the balance sheet constraint, equation (8), into the bank objective function above to yield the following:

$$\begin{aligned} V &= R_k QS - R(QS - N) \\ &= \underbrace{(R_k - R)}_{\text{profitability}} QS + RN \end{aligned} \tag{10}$$

Equation (10) shows that a bank's value is a function of the premium the bank earns on its assets, which I have termed profitability. We can already see that in this model there is no measure of loan losses that were key to the empirical mechanism documented in Section 3. While one could argue that loan losses might already be included in the endogenously determined R_k, I will explain how this is not the case.

Thus far, the model is fairly standard. However, an important feature of equation (10) is that so long as the bank has positive profitability (i.e., $R_k - R > 0$), it will want to infinitely expand its assets. Put differently, bank value V is increasing in assets when banks have positive profitability. Therefore, a core component of this class of models is the introduction of a moral hazard/costly enforcement problem which generates an endogenous leverage constraint (i.e., the Gertler-Karadi-Kiyotaki constraint) and thus prevents banks from infinite expansion. The costly enforcement problem is modelled as follows. After households place their deposits in a bank, the bank can divert a fraction λ of the deposits for itself. However, if the bank diverts those deposits, the depositors will force the bank into bankruptcy and recover the remaining $1 - \lambda$ share of assets. Therefore, rational depositors will only deposit at a bank if the bank has no incentive to divert assets. This yields the following incentive constraint which must be satisfied:

$$V \geq \lambda QS \tag{11}$$

Intuitively, the incentive constraint above is saying that a depositor would only deposit at a bank if the bank value (i.e., the value the bank obtains from being honest) is greater than the value the bank receives if it diverts assets (i.e., the value the bank obtains from not being honest). One can already see that banks with high value will be able to attract more deposits and subsequently grow their assets. Therefore, the incentive constraint prevents banks from expanding their assets infinitely as they need their value, V, to be larger than the share of divertible assets. As such, banks will expand up to that point (so long as profitability is positive). This implies that equation (11) will hold with equality and so we can equate equations (10) and (11) together.[21] This yields the following:

$$\lambda QS = (R_k - R)QS + RN$$
$$\implies \text{leverage} \equiv \frac{QS}{N} = \frac{R}{\lambda - (R_k - R)} \tag{12}$$

Now we have an equation for leverage (i.e., total assets divided by net worth). This equation, which is derived from the bank problem and incentive constraint alone, has a very important implication: leverage is increasing in profitability (where profitability is $R_k - R$). The intuition behind this implication is that if a bank is able to make more profits, then it has less incentive to divert assets and cheat depositors. As such, depositors are more willing to lend to the bank which enables the bank to increase its leverage. Note that because of equity issuance frictions, the adjustments come from leverage. We are now in a position to contrast this simple intuition to the empirical findings.

Recall, I show that a contractionary monetary policy shock reduces profits which depletes net worth and subsequently increases leverage. Note specifically that leverage is rising *because* of falling profits. On the other hand, the model has leverage rise together with profits rising (i.e., the profitability channel). This is an important inconsistency. The reason profits fall in the data is driven primarily from a rise in loan losses as borrowers with floating-rate loans are less able to repay. The model has no measure of loan losses and as such does not capture that profits fall following a contractionary monetary policy shock. Therefore, even though the interest rate on lending, R_k, is endogenously determined, it is unable to capture the loan-loss dynamic. Hence, modelling banks through this type of Gertler-Karadi-Kiyotaki incentive constraint generates an empirically inconsistent profitability channel. While my empirical results were based on the monetary policy shocks identified in Romer and Romer (2004), one can actually use the monetary policy shock series in Gertler and Karadi (2015) to see whether the Gertler and Karadi (2011) model would be consistent with the Gertler and Karadi (2015) shock series.

[21] Gertler and Karadi (2011) explicitly state that the constraint always binds within a local region of the steady state.

Figure 8: Impulse Response of Profits to Contractionary Monetary Shock

(a) Model: Gertler and Karadi (2011) **(b)** Empirics: Gertler and Karadi (2015)

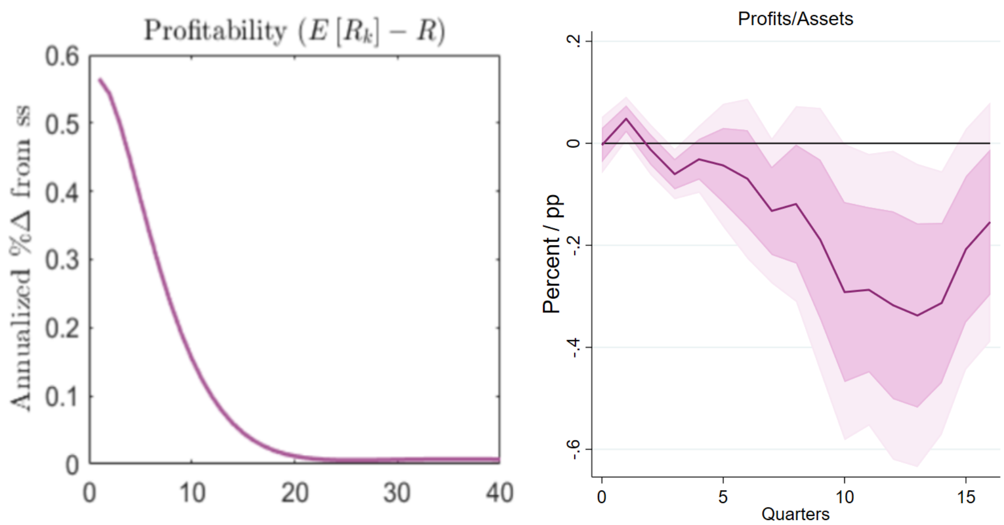


Figure 8a shows that in the Gertler and Karadi (2011) model, profits increase following a contractionary monetary policy shock, before returning to steady state. However, Figure 8b, which uses the same specification as throughout this paper, shows the shock series in Gertler and Karadi (2015) predicts a decrease in profits following a contractionary shock.[22] One possible explanation for the inconsistency with respect to profits is that $R_k - R$ can represent several different measures of profitability, where the book profits of banks is one possible measure. Other measures could include the risk premium in the economy or the expected return on bank stocks.

Moreover, one could argue that Gertler and Karadi (2011) and similar models are still able to predict that leverage rises following a contractionary monetary policy shock and perhaps that is sufficient despite the mechanism being empirically inconsistent. There are two important problems with this line of reasoning. First and foremost, one important rationale for developing macroeconomic models with a microfounded banking sector such as Gertler and Karadi (2011) is to help us understand the underlying economic mechanism. However, not only does the model miss an important channel through which monetary policy is being transmitted to banks (i.e., loan losses), it suggests a mechanism that is contradicted by the data as profits rise in the theoretical model of Gertler and Karadi (2011) but fall according to the Gertler and Karadi (2015) empirical evidence.

[22] Given that the GK shock starts later than the RR shock, the data underlying the figure is from 1994 onwards and eight lags are used instead of sixteen due the shorter time horizon. The empirical measure of profits is profit divided by assets and so is measuring profitability in a way that closely resembles the measure of profitability in the model.

Secondly, even if the primary objective of this class of models is prediction rather than capturing the underlying economic mechanism, the results may still be misleading. To highlight why, let us return to the Gertler and Karadi (2011) model and rewrite the definition of leverage using the incentive constraint (i.e., using equation (11) to replace QS). This yields the following:

$$\text{leverage} = \frac{V}{\lambda N} \tag{13}$$

In these types of models, bank value (V) is linear in bank assets and net worth[23]

$$V = vQS + \eta N \tag{14}$$

where v is the marginal value of expanding assets and η is the marginal value of expanding net worth. Plugging equation (14) into equation (13) yields the following:

$$\begin{aligned} \text{leverage} &= \frac{vQS + \eta N}{\lambda N} \\ \implies \lambda \cdot \text{leverage} &= v\frac{QS}{N} + \eta \\ \therefore \text{leverage} &= \frac{\eta}{\lambda - v} \end{aligned} \tag{15}$$

where the last step makes use of the fact that leverage $\equiv \frac{QS}{N}$. Equation (15) highlights a very important implication of this type model structure: leverage is increasing in the marginal value of net worth (as well as the marginal value of assets). Given the equity frictions, *any* negative shock that increases the marginal value of net worth (η) and assets (ν) will also increase leverage.[24] Therefore, while this feature allows such models to correctly predict that leverage rises in response to a contractionary monetary policy shock, they also predict that all other negative shocks would yield a rise in leverage which is a much stronger claim. If this were true, then perhaps one could put less emphasis on the underlying economic mechanism. However, I have already provided one counterexample to the claim that any negative shock will increase bank leverage. Figure 5b shows a negative oil shock that leads to a decrease

[23] Indeed, as highlighted in Ghote (2021), having V proportional to net worth implies that the bank problem is scale invariant. As such, optimality implies that leverage is the same for all banks regardless of their net worth which allows for a representative bank.

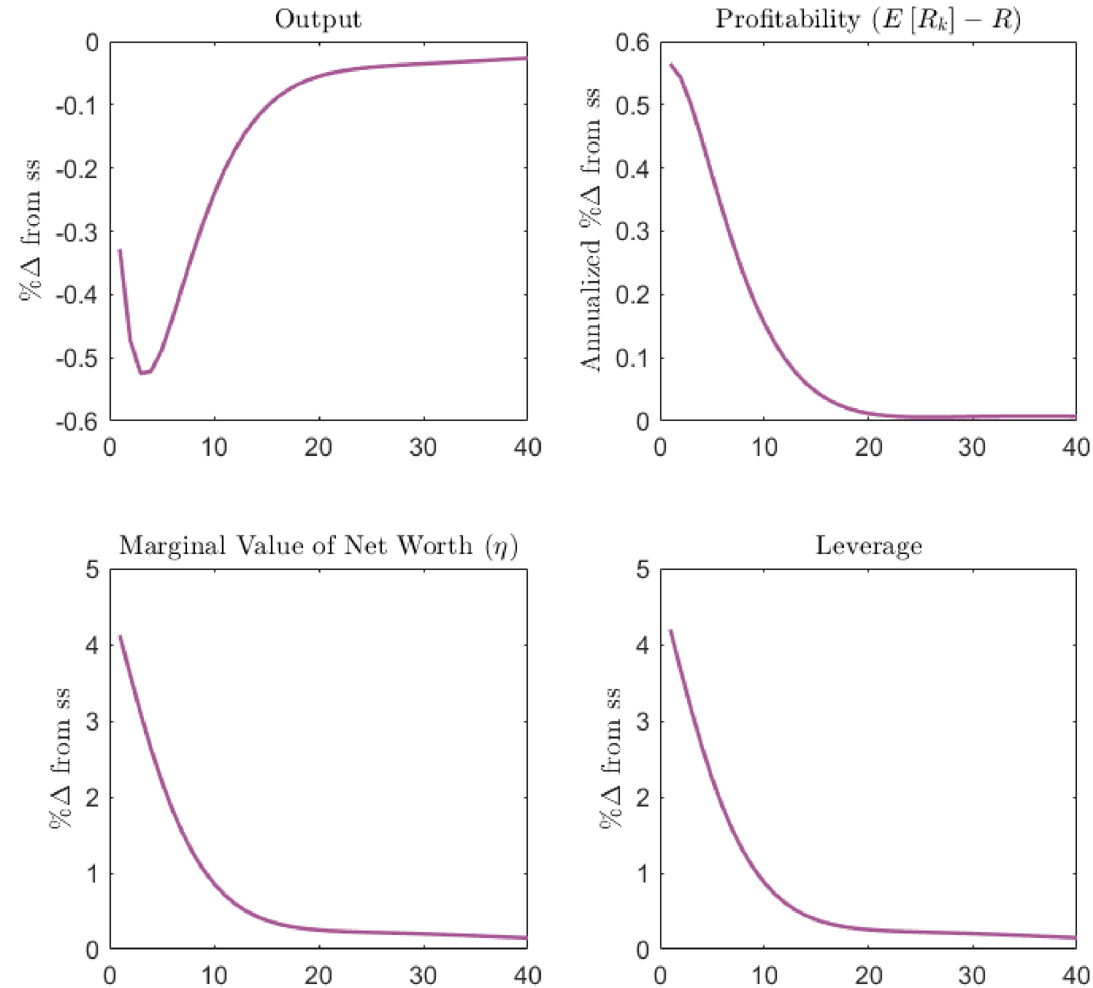
[24] A negative shock increases the marginal value of net worth because it causes an on-impact decrease in the price of capital, Q. This reduces bank net worth as bank assets are now worth less. However, a decline in net worth means banks are less able to lend which decreases total loans. A decline in total lending raises the expected profitability of lending which raises the marginal value of net worth.

in leverage. So why are Gertler and Karadi (2011) able to accurately predict leverage in the case of the contractionary monetary policy shock scenario but not negative oil shock? One reason is because there is no distinction by type of shock in the model. However, as is evident in Figure 5, the type of shock matters empirically not just in terms of magnitude but also direction. While both an oil shock and monetary policy shock cause a decline in GDP, only the latter increases loan losses, which also highlights the role of floating-rate loans. This again underscores the importance of the underlying loan-loss mechanism; a component missing from Gertler and Karadi (2011). Ensuring the mechanism is modelled appropriately ensures that the predictions are made in the right context. In this sense, the prediction in this class of models may be misleading.

Thus far, we have only used a partial equilibrium analysis to understand how and why models that use a Gertler-Karadi-Kiyotaki type constraint to model banks generate implications that are inconsistent with empirical evidence. While such analysis more easily highlights the underlying intuition and dynamics, one might argue that the full GE model could generate different results. Therefore, in Figure 9 below, I show the results obtained from the full GE model in response to a contractionary monetary policy shock.[25] As can be seen, in the full GE model, a contractionary shock decreases output, increases profitability, increases the marginal value of net worth, and increases leverage, all claims that were evident from analysing the banking block alone. Moreover, consistent with equation (15), Gertler and Karadi (2011) also show that a negative total factor productivity shock leads to an increase in leverage.

[25] The model code is obtained from the Macroeconomic Model Data Base (see `https://www.macromodelbase.com/`).

Figure 9: Gertler and Karadi (2011) Model Response to Monetary Policy Contraction



Therefore, for understanding bank leverage, the set of results in this section highlights that the insights obtained from examining the structure of the banking sector alone survives GE dynamics. Indeed, given the implications of my analysis of the banking block, and that the remaining components of these model (i.e., households and firms) are fairly standard, it appears that the inconsistencies in such models arise primarily due to the profitability channel, which ignores the loan-loss mechanism.

One important aside here is that there are typically two types of analyses. One that considers exogenous monetary policy shocks (e.g., Gertler and Karadi (2011) and Drechsler et al. (2018b)) and another that derives a monetary policy *rule* that suggests leaning against the wind (e.g. Woodford (2012) and Ghote (2021)). Therefore, one may argue that the analysis in this paper is only pertinent to the papers considering exogenous shocks. However, Wolf and McKay (2023) show that analyses using exogenous shocks and analyses considering alternate policy rules can be equivalent under certain conditions. Specifically, if policy affects private-sector behaviour only through the current and future expected path of the policy instrument (the case in most models), then in the eyes of the private sector, a prevailing non-leaning-against-the-wind monetary policy rule subject to a particular sequence of contractionary interest rate shocks is identical to some counterfactual leaning-against-the-wind policy rule. Put simply, the private sector is not able to distinguish between a contractionary shock and a change in the monetary policy rule that would generate the same contractionary shock. As such, my empirical findings are relevant for analyses involving both exogenous shocks and modified policy rules, albeit more directly for the former.[26]

5 An Empirically Consistent Theoretical Model

The models presented thus far capture a wide variety of the results in the literature as they are some of the most foundational models. However, they all fail to capture the empirical dynamics that I have documented. A crucial missing ingredient is the loan-loss mechanism. Interestingly, while many of the models explored in the previous section are GE models (as is typically the case when modelling monetary policy), we did not need to examine the whole GE structure to see where problems arise. Indeed, the problems shown in Section 4 arise primarily from how the banking system is modelled (e.g., from the bank problem in Gertler and Karadi (2011) or the bank leverage law of motion in Woodford (2012)).

It is instructive to consider whether partial equilibrium banking models, in particular, those that do not rely primarily on a substitution effect or leverage procyclicality, can be more empirically consistent. A few examples that are able to generate mostly consistent empirical dynamics are Van den Heuvel (2009) and Corbae and Levine (2023). Van den Heuvel (2009) develops the bank capital channel of monetary policy which sees profits fall and leverage rise following contractionary monetary policy. The underlying mechanism of the model is through maturity transformation rather than loan losses. However, he also shows how a default shock works in the model and it generates dynamics similar to the loan-loss mechanism. While the default shocks are exogenous, if they were a function of a contractionary monetary policy shock, the dynamics would appear to match those in the loan-loss mechanism. Nonetheless, there is still no explicit role for floating-rate loans which appear to be an important feature in the data (see Section 6). Corbae and Levine (2023) take a different

[26] Wolf and McKay (2023) note that their result is less suited to study policies that alter the steady state (e.g., changes in the inflation target). However, many analyses of optimal rules compare different cyclical stabilization policies such as augmented Taylor rules, where the results of Wolf and McKay (2023) apply.

modelling approach but also see profits fall and leverage rise in response to contractionary monetary policy. The mechanism is also different to the loan-loss mechanism as higher rates raise the marginal cost of financing for banks which induces greater risk-taking and a fall in profits. While such models get fairly close to matching the empirical dynamics, the mechanism underpinning the fall in profits in these models is different to loan losses and does not feature a role for floating-rate loans, both of which appear important for the empirical mechanism.

Therefore, from examining theories that are unable to match the empirical dynamics as well as those that match them better, one can surmise the following. First, the general equilibrium structure does not appear to be especially important in generating the dynamics of leverage in response to contractionary shocks. This means one can focus on a partial equilibrium banking model in order to illuminate the mechanism more clearly. Second, a common missing ingredient across most models is the loan-loss mechanism. As such, the model needs to capture loan losses that are increasing in contractionary monetary policy shocks. Third, there needs to be a potential role for floating-rate loans in generating the loan-loss mechanism. For these reasons, I develop a model with these three components. The purpose of the model is twofold. First, it formalises the role of loan losses in determining the response of bank profitability to contractionary monetary policy shocks and sheds light on the role of floating-rate loans. Second, it generates implications which I can then explore using microdata.

My model provides a novel way for thinking about banks by emphasising what I call *risk transformation* which works as follows. Banks are exposed to interest rate risk because their deposits are floating-rate liabilities (i.e., when interest rates rise, deposits become more expensive). To hedge the interest rate risk and alleviate the cash flow mismatch on their balance sheets, banks issue floating-rate loans. So when interest rates unexpectedly rise, while banks have to pay more to depositors, they also receive more income from floating-rate borrowers. However, this hedging strategy works by transferring the risk from banks to borrowers. Unlike banks, borrowers cannot hedge against unexpected interest rate changes.[27] As such, in response to contractionary monetary policy shocks, borrowers are less able to repay their loans which leads to loan losses for banks. Such losses represent a credit risk for the bank. Therefore, through issuing floating-rate loans, banks are conducting risk transformation as

[27] There is an important distinction between unexpected and expected changes in interest rates. Because expected interest rate changes are procyclical, borrowers are naturally hedged as while their loan-servicing costs rise, they also receive greater cash flows. See Figure 15 and the associated discussion for further detail.

they are hedging interest rate risk at the expense of greater credit risk.

My model builds on Kirti (2020) but incorporates credit risk via loan losses. Consider a one-period model with the following timeline. First, banks make loans funded by deposits and internal net worth. Second, the realization of the monetary shock takes place. Finally, repayment occurs. Banks are exogenously endowed with deposits D and a loan portfolio of size L, as such, internal net worth is $N = L - D$. The key choice for banks is the share of floating-rate loans f_L in their loan portfolio.[28] The deposits are floating-rate liabilities. However, as shown by Drechsler et al. (2017), there is not perfect pass-through of the central bank interest rate to deposit rates. In the model, the pass-through coefficient, known as the deposit-beta (β^{dep}), is exogenous but one can microfound this by using the approach in Drechsler et al. (2017). The interest rate is a random variable $r = \bar{r} + \varepsilon$ where $\varepsilon \sim \mathcal{N}(0, \sigma^2)$. Therefore, $E[r] = \bar{r}, Var[r] = \sigma^2$. Note that ε is the monetary policy shock.

Note that the one-period nature of the model implies leverage will move in lockstep with profits. Specifically, if profits fall, leverage will rise. I choose this approach because, as in most models, banks have limited scope to adjust dividends or raise equity. Therefore, the interesting variation comes from the response of profitability which subsequently determines leverage. In my model, all of the variation in leverage in response to contractionary monetary policy shocks will be explained by profitability which is broadly consistent with my empirical findings.

I model banks as having risk-averse preferences in order for them to dislike risk.[29] As such, banks maximise value V by choosing the share of its loan portfolio that is floating rate:

$$\max_{f_L} V = g(\pi)$$

where π is bank profits and $g(\cdot)$ represents a risk-averse functional form. For analytical simplicity, I will use mean-variance preferences. So the bank objective function is

$$\max_{f_L} V = E[\pi] - \frac{\gamma}{2} Var[\pi]$$

[28] One can think of this as a two-stage optimisation problem for banks. In the first stage, they choose the optimal balance sheet size (loans and deposits). In the second stage, they choose the share of their loans to be fixed or floating. Given I am only interested in the second stage, the loan size and deposits are exogenous.

[29] The assumption that banks have risk-averse preferences is not uncommon in the literature. See for example Di Tella and Kurlat (2021).

where γ captures risk-aversion. Solving this yields an expression for f_L^* in terms of μ which is solved in equilibrium with the firm problem (see Appendix D for the full model and an analytical expression for f_L^*). However, the core insight from the model comes from the following thought experiment: given the optimal choice f_L^*, what is a bank's profits and expected profits?

$$\pi = \overbrace{\underbrace{L(1-f_L^*)(\bar{r}+\mu^*(f_L^*))}_{\text{fixed-rate income}} + \underbrace{Lf_L^*(\bar{r}+\varepsilon+\mu^*(f_L^*))}_{\text{floating-rate income}} - \underbrace{D(\bar{r}+\beta^{dep}\varepsilon)}_{\text{cost of deposits}}}^{\text{Net Interest Income}} - \overbrace{Lf_L^*\theta(\varepsilon)}^{\text{Loan Losses}} \tag{16}$$

$$E[\pi] = L(1-f_L^*)\bar{r} + Lf_L^*\bar{r} + L\mu^*(f_L^*) - D\bar{r} - Lf_L^*E[\theta(\varepsilon)] \tag{17}$$

μ^* is the equilibrium loan spread between the lending rate charged to firms and the central bank interest rate. Note that μ^* will be decreasing in the floating share of loans as banks have to accept a lower spread because firms are risk-averse and will also want to avoid bearing interest rate risk. One key point in (16) is that net interest income is not the same as profits. Indeed, as seen in the aggregate data, loan losses drive the vast majority of the variation in bank profits in response to a monetary policy shock. Any expected losses would already be priced in and therefore, to highlight the mechanism, I focus on the unexpected losses. The model only features unexpected loan losses on floating-rate loans. $\theta(\varepsilon)$ is the loan-loss rate where $\theta'(\varepsilon) > 0$ and $\theta'(\varepsilon)$ is linear in ε. This is intended to capture in a reduced-form way that loan losses are increasing in the size of the monetary policy shock. There are no unexpected loan losses on fixed-rate loans as a change in the central bank interest rate does not impact the loan-servicing cost of the fixed-rate borrower. This argument also rules out a recessionary channel of defaults as the purpose is to specifically highlight the role of floating-rate loans in order to explain that in the aggregate data we see loan losses rise with contractionary monetary policy shocks but not for other contractionary shocks. Note that this does not imply that recessions cannot cause loan losses but it simply highlights the loan-loss mechanism that appears to be induced by floating-rate loans.

Using (16) and (17), I define deviations from expected profitability (as measured by return on assets) as the following:

$$\begin{aligned} \Delta &= \frac{\pi}{L} - \frac{E[\pi]}{L} \\ \implies \Delta &= \underbrace{f_L^*\varepsilon - \frac{D}{L}\beta^{dep}\varepsilon}_{\text{interest rate risk}} - \underbrace{f_L^*\left(\theta(\varepsilon) - E[\theta(\varepsilon)]\right)}_{\text{credit risk}} \end{aligned} \tag{18}$$

Equation (18) represents a key insight of the model. The bank is exposed to interest rate risk because a contractionary monetary policy shock makes deposits more expensive. Floating-rate loans generate more revenue for the bank when interest rates increase and therefore banks issue floating-rate loans as a way to hedge interest rate risk. This is consistent with Kirti (2020) who shows empirically that banks that have a higher deposit pass-through (higher β^{dep}) issue more floating-rate loans. However, the core insight of (18) is that this interest rate risk hedge comes at the expense of credit risk. Specifically, the bank hedges the interest rate risk by passing that risk onto the borrower. If a borrower cannot hedge this risk, this generates loan losses for the bank. In the model, this is captured by $\theta(\varepsilon)$. A simple example can illustrate this more clearly. Consider a bank that issues a floating-rate loan that exactly tracks the central bank rate. If the central bank raises the interest rate, the borrower now has to pay more on the loan which raises the probability of default of the borrower. The bank has merely traded interest rate risk for credit risk. While in many models, banks do maturity transformation, my model highlights a different function that banks carry out: *risk transformation*. Moreover, the way the model is written is such that the risks are not separable. The bank has a single choice variable to manage two opposing risks. Therefore, it specifically highlights the potential for floating-rate loans to generate loan losses in response to contractionary monetary policy shocks.[30]

By differentiating equation (18) with respect to the monetary shock (ε), we can construct the model counterparts to the empirical impulse response functions:

$$\underbrace{\frac{\partial \Delta}{\partial \varepsilon}}_{\text{Profits IRF}} = \underbrace{f_L^* - \frac{D}{L}\beta^{dep}}_{\text{Net Interest Income IRF}} - \underbrace{f_L^* \theta'(\varepsilon)}_{\text{Provisions IRF}} \tag{19}$$

Equation (19) has a simple form. It states that impulse response function of profitability with respect to an interest rate shock is equal to the difference between the impulse response functions of net interest income and loan-loss provisions. Note that I have abstracted away from other components of bank income such as net noninterest income which include items such as fee income or salary expenses as these components are not core to understanding the loan-loss mechanism.

Importantly, equation (19) yields specific implications for the role of floating-rate loans. First, let us look at the impulse response function of loan-loss provisions (the final term in

[30] See Hellwig (1994) for a similar argument about the trade-off between interest rate risk and credit risk in relation to the Basel I regulatory framework.

(19)). We can see that the term is increasing in the share of floating-rate loans which tells us that loan losses will increase by more in response to a contractionary shock for banks with a higher floating share. Second, the impulse response function for net interest income also appears to be increasing in the floating share which tells us that net interest income will respond more positively in response to a contractionary shock for banks with a higher floating share.[31] This captures the trade-off between interest rate risk and credit risk described earlier. The model suggests that in response to a contractionary shock, banks with a higher floating share should experience a larger increase in net interest income but also a larger increase in loan-loss provisions.

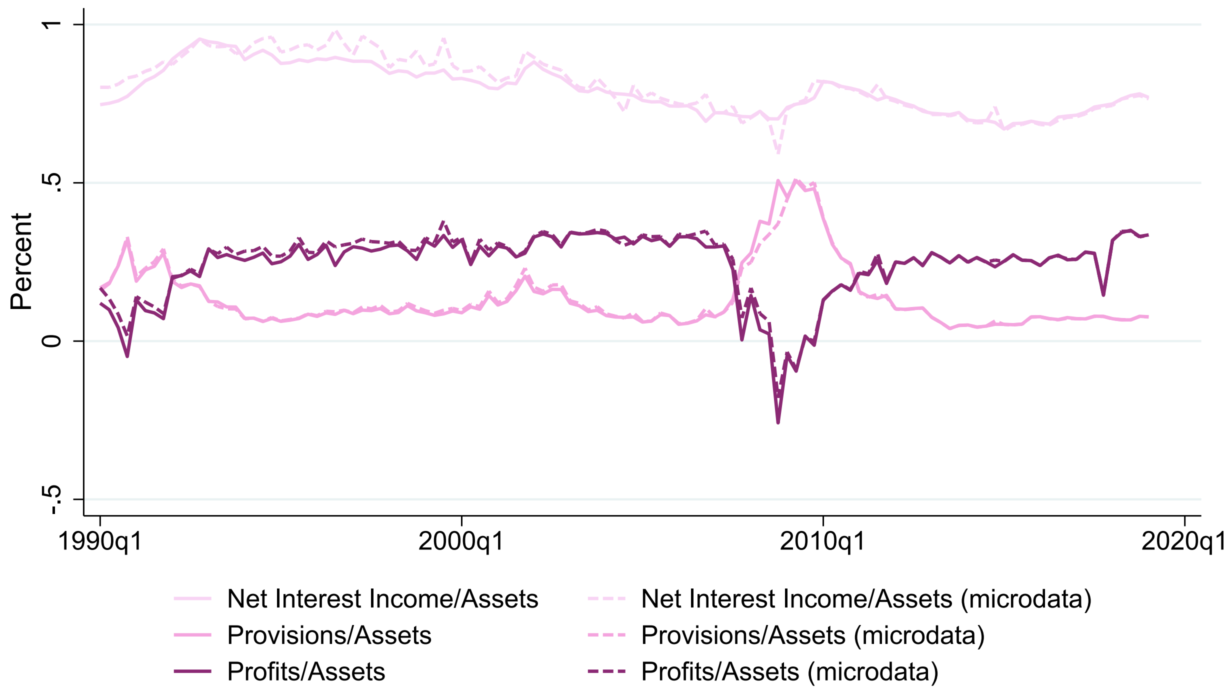
The overall impact on bank profits will depend on the impact on net interest income relative to the impact on loan losses. However, we know from the aggregate data that profits fall, so one would expect that the impact of loan losses will dominate. In the next section, I test these implications using microdata.

6 Microdata Evidence

First, I aggregate the bank-level data to ensure it is reasonably close to the aggregate data series from the FDIC. The main variables that I am interested in exploring in this section are net interest income, provisions, and profits (all normalised by assets) as these are the core components of the model. In Figure 10 below, I show both the aggregate data from the FDIC and aggregated microdata from Drechsler et al. (2017) for each of these variables.

[31] Strictly speaking, it will also depend on the correlation between the share of floating-rate loans and the product of the deposit-loan ratio and deposit beta.

Figure 10: Comparing Aggregate Data to Aggregated Microdata



As can be seen from Figure 10, the microdata matches the macro data very well, albeit not perfectly. While there are some deviations in the mid-1990s and during the global financial crisis, both of these will not be in my estimation sample. The former will be excluded as data on the share of floating-rate loans begins in the late-1990s, while the latter is excluded, as in my earlier empirical analysis, due to the myriad changes to the regulatory architecture at the time. I define the floating share as follows:

$$f_L = \frac{\text{loans with repricing maturity of less than three months}}{\text{total loans}} \tag{20}$$

The numerator consists of two types of loans: floating-rate loans where the interest rate resets every three months (or more frequently) and fixed-rate loans with a remaining maturity of three months or less. While the latter group is not technically floating-rate loans, it will be considered as such for the purposes of my analysis. This is because loans that are fixed rate but with short maturity effectively act as floating-rate loans given they require frequent repricing.[32]

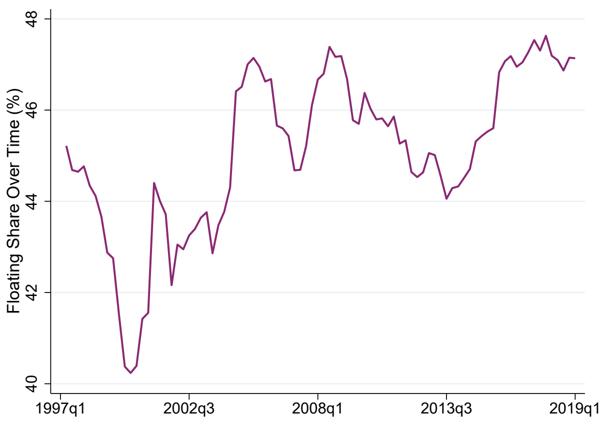
In Figure 11 below, I show the share of floating-rate loans in the time series and cross section. Figure 11a shows the time-series variation in the floating share for the aggregated

[32] The main difference is in cases where a borrower is unable to refinance a fixed-rate loan of short maturity due to a high likelihood of default, but would have been forced to default on a floating-rate loan with a longer maturity. As such, my measure is likely to understate, rather than overstate, potential defaults.

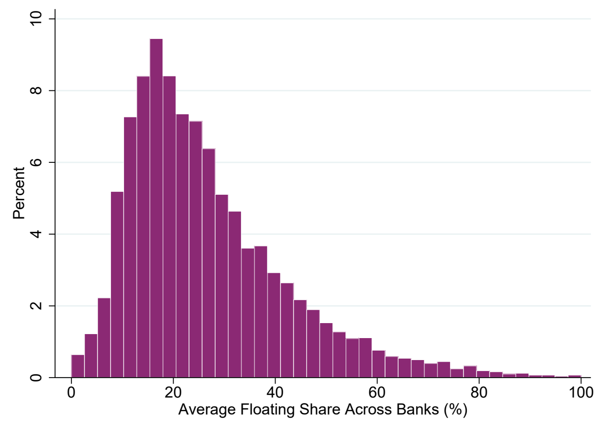
banking sector. The aggregate floating share varies between 40% and 48%. Figure 11b takes the average floating share per bank over time and plots a histogram. As can be seen, there is considerable cross-sectional variation. While close to ten percent of banks have just under 20% of their loan portfolio composed of floating-rate loans, the distribution is clearly right-skewed.

Figure 11: The Share of Floating Rate Loans

(a) Time-Series Variation in Floating Share



(b) Cross-Sectional Variation in Floating Share



While determining the specific causes of the floating share empirically is beyond the scope of this paper, it is worth documenting some of the average characteristics of banks with a lower average floating share relative to those with a higher average floating share within my estimation window (1999-2006). Specifically, I find that the average bank above the median floating share, relative to below the median, is substantially larger (over five times larger), has a higher share of commercial and industrial loans (20% versus 13%), has a slightly lower share of real estate-backed loans (62% versus 67%), has a lower share of personal loans (9% versus 14%), and is similarly profitable as measured by return assets (0.247% versus 0.250%).

Now that we have explored the floating-rate data, it is worth revisiting the model in the previous section. Recall that the model had a set of implications that depended on a bank's share of floating-rate loans. Specifically, the model suggests that in response to a contractionary monetary policy shock, banks with a higher floating share should experience higher net interest income but also higher loan-loss provisions, and that the impact on profits depends on the relative changes of the two components. I will test these implications using bank-level variation in the floating share. More precisely, I will estimate a panel local projection (a panel version of (1)) using data from 1999 to 2006 where the shock is interacted with the bank-specific floating share.[33] I also include horizon-specific bank fixed effects and consistent

[33] I allow the floating share to be time-varying as from a macroeconomic perspective, it is useful to capture

with (1), lags of the dependent variable and a quarter dummy. Therefore, I estimate the following specification for $h = 0 \ldots 16$:

$$z_{i,t+h} = \alpha_{i,h} + \sum_{l=0}^{L} \beta_{h,l}^{(1)} Shock_{t-l} + \beta_h^{(2)} FloatShare_{i,t} + \sum_{l=0}^{L} \beta_{h,l}^{(3)} Shock_{t-l} \cdot FloatShare_{i,t} + \sum_{m=1}^{M} \gamma_{h,m} z_{t-m} + \sum_{q=2}^{4} \delta_q Quarter_{qt} + \epsilon_{i,t+h} \quad (21)$$

Given the relatively short time series, I use four lags (i.e., $L = M = 4$). The main object of interest is the *interaction effect* $\{\beta_{h,0}^{(3)}\}_{h=0}^{H}$ for $h = 0 \ldots 16$. A positive value of $\beta_{h,0}^{(3)}$ at horizon h implies that a higher floating share increases the response of $z_{i,t+h}$ to a monetary policy shock at time t. To ease interpretation and to document the magnitude, I will also show the *total effect* which is given by $\{\beta_{h,0}^{(1)} + \beta_{h,0}^{(3)} \cdot FloatShare_{i,t}\}_{h=0}^{H}$ for $h = 0 \ldots 16$. The total effect measures the response of $z_{i,t+h}$ to a monetary policy shock at time t for specific values of the floating share. For illustrative purposes, I will show the 10th percentile and 90th percentile. However, these are only for illustrative purposes as the interaction effect directly captures the significance of the floating share for the responsive of z to the shock.

Figure 12 below shows the response of net interest income to a contractionary monetary policy shock. Consistent with the model, the interaction effect in Figure 12a is mostly increasing in the floating share, albeit turning negative towards the end of the projection horizon. To better interpret the interaction effect, it is worth comparing 12b and 12c which capture banks with a low and high floating share, respectively. As expected, banks with a low floating share are more negatively impacted by a contractionary monetary policy shock.

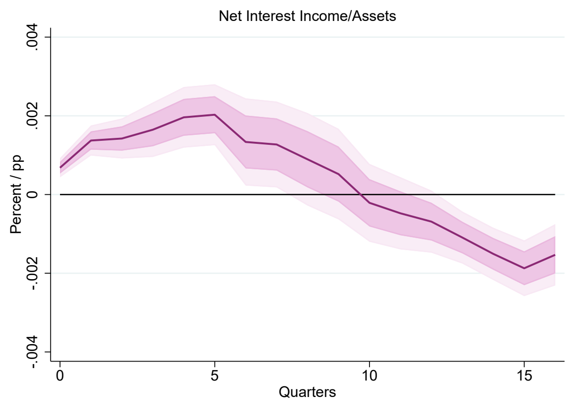
Specifically, banks with a low floating share see a persistent fall in their net interest income. Intuitively, one can think of such banks as issuing largely fixed-rate loans and seeing the cost of their funding rise with interest rates. As such, their net interest income will fall. On the other hand, banks with a high floating share see their net interest income rise on impact and remain elevated for over two years as they generate more revenues on their loans, despite the cost of funding increasing. However, because these banks have passed on the interest rate risk to borrowers, borrowers eventually default which reduces loan repayments over time such that net interest income becomes negative, even for these high floating share banks. Overall, banks with a low floating share see a cumulative fall of around 0.4 percentage

potential behavioural changes that result from the shock which might dampen its impact. However, I also estimate an alternative specification where I use the average floating share per bank which gives the same results.

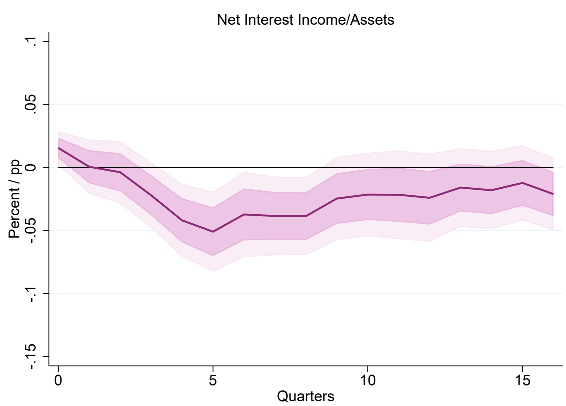
points in their net interest income while banks with a high floating share only experience a fall by about 0.1 percentage points. As the model suggests, banks with a high floating share are better hedged against interest rate risk.

Figure 12: Net Interest Income Response To Contractionary MP Shock By Floating Share

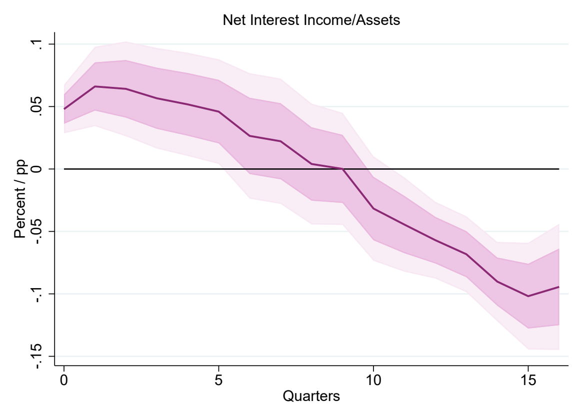
(a) Interaction Effect



(b) Low Floating Share



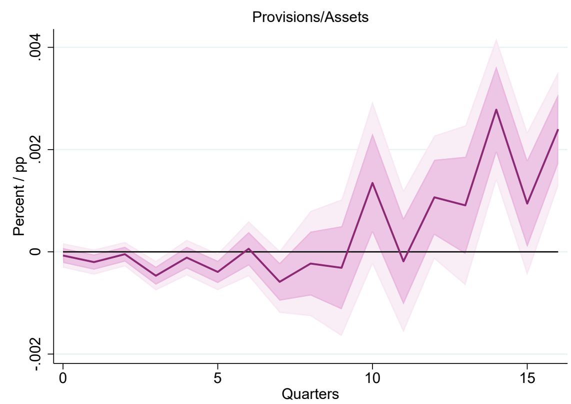
(c) High Floating Share



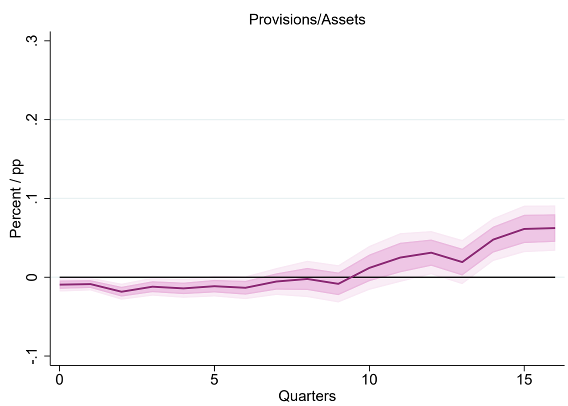
In Figure 13, I repeat the analysis with loan-loss provisions instead of net interest income. Recall that the model predicts that loan losses should rise more for banks with a higher floating share than those with a lower floating share. Figure 13a plots the interaction effect which confirms this prediction of the model. Moreover, given the low floating share banks see a negative impact on their net interest income, one would expect minimal loan losses for this group as these banks do not appear to pass on their interest rate risk to their borrowers and so should not experience much loss from credit risk. Figure 13b shows precisely that low floating share banks see minimal loan losses. On the other hand, Figure 13c shows that loan losses rise significantly for banks with a high floating share, around three times as much as those with a low floating share. Indeed, this is specifically the trade-off emphasised by the model: banks are transforming (near-term) interest rate risk into (longer-term) credit risk.

Figure 13: Loan-Loss Provision Response To Contractionary MP Shock By Floating Share

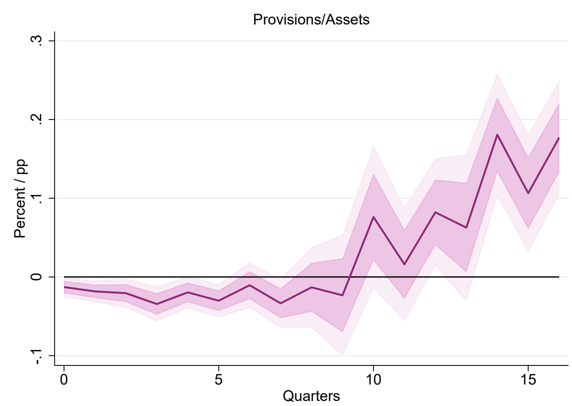
(a) Interaction Effect



(b) Low Floating Share



(c) High Floating Share



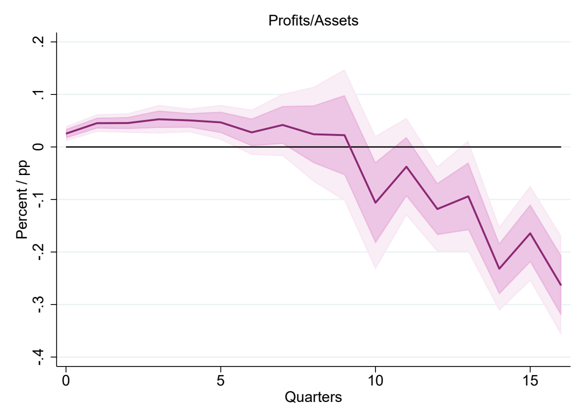
Finally, Figure 14 shows the same analysis but with overall bank profits. While the theoretical model in the previous section does not generate a directional prediction on overall profits, it does tell us that the impact on profits will be the difference between impulse response of net interest income and the impulse response of loan-loss provisions. One can see this immediately from Figures 14a, 14b, and 14c. For the low floating share banks (Figure 14b), profits are broadly flat, with a very small decline driven by the small increase in loan losses at the the end of the projection horizon. The high floating share banks (Figure 14c) initially see profits rise, driven by higher income on floating rate loans, but this hedged interest rate risk eventually becomes a crystallised credit risk. This results in a substantial rise in loan losses which leads to a significant overall decline in profits. Moreover, more of the variation in profits is due to loan losses than net interest income which is consistent with findings in the literature on the relative stability of bank net interest income (Drechsler et al. (2021)).

Figure 14: Profit Response To Contractionary MP Shock By Floating Share

(a) Interaction Effect

(b) Low Floating Share

(c) High Floating Share



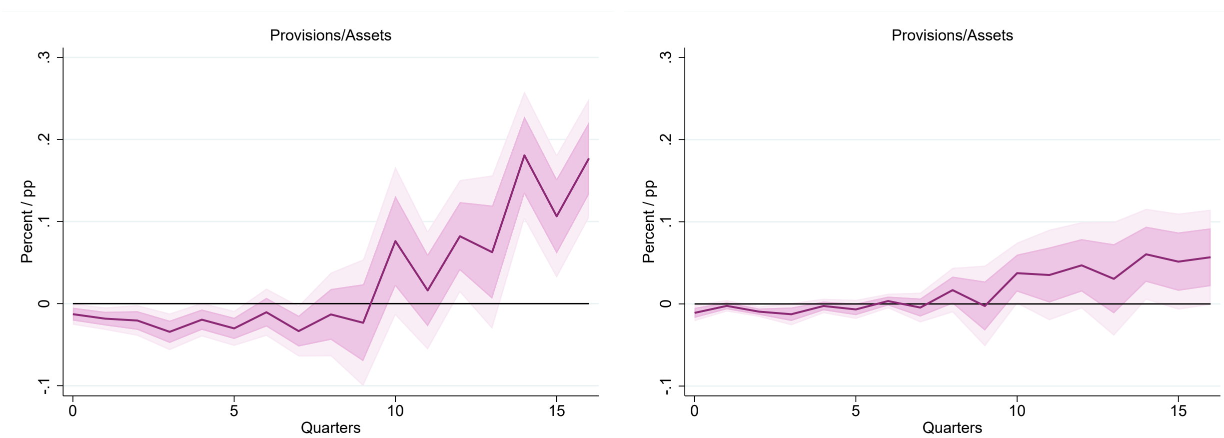
Taken together, Figures 12, 13, and 14 present evidence consistent with an important role for floating-rate loans in generating defaults, as in the theoretical model developed in Section 5. Put simply, the following story emerges. Banks are exposed to interest rate risk and as such issue floating-rate loans to hedge this risk. Given differential exposure to interest rate risk (e.g., through different deposit betas), banks issue different proportions of floating-rate loans. These floating-rate loans hedge the interest rate risk. However, because the interest rate risk is now with the borrower, it is now a credit risk for the bank. Ultimately, the credit risk component eventually becomes more important as loan losses offset the gain in net interest income, leading to a larger decline in profits for banks with a high floating share. The aggregate impact on profits is, as expected, in between the response of profits of low and high floating share banks.

One might ask why banks issue floating-rate loans if it leads to a decline in profits in response to a contractionary shock. First, it is worth noting that banks are not maximising value in order to just reduce the impact of interest rate shocks on profits. Second, and more intuitively, floating-rate loans are likely to see more benefit from expected interest rate changes rather than unexpected interest rate changes. While not explicitly modelled, as my focus is on the causal impact of monetary policy, a simple way to understand this point is the following. Higher interest rates lead to more income from floating-rate loans for banks. However, as I show, higher unexpected rates also result in less income for banks due to defaults. These defaults occur as borrowers are not well hedged against unexpected interest rate rises. The difference with higher expected interest rates is that expected interest rates are clearly not exogenous, they typically coincide with economic booms. As such, when interest rates rise, floating-rate borrowers experience higher income due to the economic boom, but also higher loan-servicing costs. This type of natural hedge is more pronounced with expected interest rate changes which are more procyclical than interest rate shocks. Therefore, while floating-rate loans may result in a decline in profits in response to contractionary monetary policy shocks, they are a far more effective hedge against typical interest changes.

Figure 15 below compares the response of loan-loss provisions to a contractionary monetary policy shock (15a) and to changes in the FFR (15b) where both result in a one percentage point rise in FFR.[34] I focus on comparing banks with high floating shares as they experience the largest increase in loan losses. The figures confirms that loan losses are substantially lower, and barely statistically significant, in response to changes in the FFR.

Figure 15: Loan Loss Response to Higher Interest Rates for High Floating Share Banks

(a) Contractionary Monetary Policy Shock **(b)** Higher Fed Funds Rate



[34] Changes in the FFR are mostly expected changes in the interest rate but will include both expected and unexpected changes. As such, it should be considered an upper bound on the impact on loan losses.

The fact that floating-rate loans, due to the pass-through of interest rates, generates an additional channel of defaults has been documented in different forms in the literature. For example, Campbell and Cocco (2015) find that when interest rates are high, defaults are higher for adjustable-rate mortgages relative to fixed-rate mortgages. In Europe, where floating-rate mortgages are more common, there is also evidence that floating-rate borrowers are more likely to default (Gaudêncio et al. (2019)).

In terms of external validity, my data stops before the GFC in 2007. Therefore, a natural question is whether the mechanism is likely to continue to apply. An important case study will be evaluating the consequences of the Fed's 2022 tightening cycle. While 2023Q3 banking data is yet to be released, there are early indications of rising defaults, in particular on floating-rate loans, which is precisely what my analysis would predict.[35] Moreover, one would potentially expect more significant loan losses in Europe where the share of floating-rate loans is higher. For example, in the UK, the Bank of England has projected that its rate hikes will lead to rising interest payments which will make it difficult for many companies to repay their debt.[36] More broadly, the implication of my analysis in this section is that the unintended consequences of higher interest rates on the stability of the banking sector are potentially more severe where the share of floating-rate loans is higher.

7 Conclusion

In this paper, I explore the following question: do contractionary monetary policy shocks make banks safer through reducing their leverage? While a vast theoretical literature claims the answer is yes, I show empirically that the answer is actually no. Not only is raising interest rates ineffective in reducing bank leverage, it is actively counterproductive as it increases leverage instead. I show this result is robust to varying specifications and using different measures of monetary policy shocks.

Next, I show empirically why leverage rises in response to contractionary monetary policy shocks. Higher interest rates increase loan losses for banks. This reduces bank profits overall which subsequently reduces bank equity. The fall in equity drives an increase in bank leverage. I term this mechanism the loan-loss mechanism. Moreover, I show empirically that the loan-loss mechanism can explain nearly all the variation in bank leverage in response to

[35] https://www.ft.com/content/9a7e9746-516b-4d37-a966-97259ec8aca6

[36] https://www.bankofengland.co.uk/bank-overground/2023/how-vulnerable-are-uk-companies-to-higher-interest-rates

monetary policy shocks. Finally, I show that while loan losses and leverage increase in response to monetary policy shocks (where the FFR rises), loan losses do not rise and leverage falls in response to contractionary oil shocks (where the FFR does not rise). This analysis provides suggestive evidence, at the aggregate level, of the importance of the rise in the FFR specifically and hence floating-rate loans. This highlights the importance of understanding bank balance sheets, and in particular the structure of the loan portfolio, in order to understand the transmission of monetary policy.

Moving on to the theoretical literature, I show that the divergence between the theoretical claims and empirical evidence is largely a result of three broad modelling choices and that there is one important factor that can help rectify this. The first modelling choice relates to models that rely on profitability rising in response to a contractionary monetary policy shock which is inconsistent with the empirical evidence. The second relates to models that incorrectly rely on the procyclicality of bank leverage and so erroneously conclude that leverage declines in response to rising interest rates. The third relates to models that rely on the substitution effect through which higher rates reduce bank leverage which is inconsistent with the observed evidence. The crucial missing factor in this eclectic mix of models is a loan-loss mechanism that is connected to the share of floating-rate loans issued by a bank.

I develop a banking model that emphasises the role of floating-rate loans and credit risk. Banks optimise by choosing the floating share of their loan portfolio which acts as a hedge against interest rate risk, but generates credit risk for the bank as the interest rate risk is now held by borrowers. A key insight of the model is that banks are doing risk transformation, and that this implies a trade-off between managing interest rate risk and credit risk. The model predicts that banks with a higher share of floating-rate loans will see greater loan losses in response to a contractionary monetary policy shock. I confirm this prediction using microdata, specifically, bank-level variation in the floating share.

My results have important implications for using monetary policy for financial stability purposes. First, an important reason to support a monetary policy strategy that targets financial stability (i.e., 'leaning against the wind') is the claim that higher rates reduce bank leverage. In this paper, I have shown this claim to be empirically false. Therefore, this paper lends support to the conclusions of Bernanke (2015) and Svensson (2017) that monetary policy should focus on its mandate of price stability, leaving issues of financial stability to macroprudential policy. However, my results also suggest that floating-rate loans are one specific way through which monetary policy creates unintended vulnerabilities in the banking sector. This is particularly pertinent in economies with a greater share of floating-rate loans (e.g., Europe). Indeed, future research could consider this novel trade-off for monetary policy: a higher share of floating-rate loans can increase the potency of monetary policy (e.g., Calza et al. (2013) and Auclert (2019)) but comes at the cost of a more vulnerable financial sector, as documented in this paper.

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Appendix A Existing Theoretical Models

This appendix briefly summarises how some of the different theoretical models predict that a contractionary monetary policy shock decreases bank leverage.

1. Woodford (2012), building on Curdia and Woodford (2010), uses a New Keynesian model to document that accomodative monetary policy increases the financial institution leverage. In his model, this increases the probability of a crisis by assumption. The mechanism relies on a postulated law of motion whereby leverage depends positively on the output gap. Therefore, a contractionary shock would contract output and subsequently leverage.

2. Dell'Ariccia et al. (2014) develop a model of financial intermediation where banks can engage in costly monitoring to reduce the credit risk in their loan portfolios. Monitoring effort and the pricing of bank assets and liabilities are endogenously determined. In equilibrium, they depend on the risk-free real interest rate (i.e., policy rate). Banks have limited liability and so take excessive risk which induces investors to enforce a leverage requirement. If the policy rate increases, this raises the rate the bank pays on debt liabilities and so exacerbates the agency problem. Investors therefore require banks to have more 'skin-in-the-game' to reduce this moral hazard and enforce tighter leverage requirements. Thus, the model features higher interest rates inducing lower bank leverage.

3. Drechsler et al. (2018b) develop a dynamic asset pricing model in which monetary policy affects the risk premium component of the cost of capital. Risk-tolerant agents (banks) borrow from risk-averse agents (i.e., take deposits) to fund levered investments. Leverage exposes banks to funding shocks. As such, banks hold liquidity buffers (e.g., US Treasuries) to insure against such funding shocks. If the central bank raises interest rates, the cost of holding liquid securities increase (i.e., there is a higher liquidity premium). This increase in the price of funding shock insurance means banks will reduce their liquidity buffers. Therefore, with lower insurance, banks reduce their exposure to funding shocks by reducing leverage. Hence an increase in the central bank rate reduces bank leverage.

4. Martinez-Miera and Repullo (2019), building on Martinez-Miera and Repullo (2017), in which competitive financial institutions that are funded with uninsured debt can engage in costly monitoring of entrepreneurial firms. However, monitoring is unobservable, so there is a moral hazard problem. They also include the possibility of costly equity financing for banks where greater equity can ameliorate the moral hazard problem. They find that tightening monetary policy reduces the wealth that investors allocate to funding entrepreneurs and banks. This decreases aggregate investment and lowers the return on debt and equity and ultimately increases leverage.

Appendix B Book Leverage vs Market Leverage

In this paper, I use accounting-based measures of leverage (i.e., book leverage). An alternative approach would be to use market-based measures of leverage. Each measure has its own advantages and disadvantages. The definition of book leverage is the ratio of total assets to book equity while the definition of market leverage is the ratio of enterprise value (i.e., the sum of total liabilities and market equity) to market equity where market equity captures the market value of equity. I use book leverage for several reasons.

The first reason is consistency with the overall policy framework. When considering financial stability, macroprudential regulations focus on book leverage rather than market leverage. As such, from a policy consistency perspective, one would expect that monetary policy that targets financial stability would also do so through book leverage.

The second reason relates to bank decision-making. Banks themselves present their targets for return on equity at book value and report the evolution of leverage at book value. Indeed, Adrian et al. (2019) documents empirically that banks base their balance sheet management around book equity and book leverage and as such actively manage book leverage. While they mention market leverage also plays a role, they conclude that it is secondary to book leverage determined primarily by market forces. Similarly, Li (2022) highlight that it is book leverage that matters for bank lending decisions. Nuño and Thomas (2017) also highlight that book equity is the appropriate notion of equity when interested in the bank lending while market equity would be more appropriate if interested in new share issuance or mergers and acquisitions decisions. Given the role of book leverage in lending decisions, it clearly interacts more directly with the bank lending channel of monetary policy and would therefore constitute the appropriate measure of leverage for my analysis.

The third reason relates to explicit modelling choices. While many papers do not explicitly model book leverage or market leverage, they often implicitly consider book leverage. For example, models that rely on procyclicality of leverage are considering book leverage as market leverage is countercyclical. Ottonello and Song (2022) show analytically that in their model there is a tight link between book leverage and market leverage. More recently, Begenau and Landvoigt (2021) construct a rich model where delayed loss recognition can explain why book values differ from fundamental values.

The final reason is a question of data. Book leverage captures the entirety of the banking system as this data is available for all banks. Including the entire system is important in order to most accurately evaluate aggregate macroeconomic effects. Market leverage is only available for listed banks and so would significantly narrow the scope of the analysis.

Despite its limited scope, I repeat my analysis using market leverage. Figure C.6 shows that the results are qualitatively similar, albeit noisier and larger in magnitude for market leverage. The measure of market leverage I use is from He et al. (2017). They construct it as follows:

$$\text{Market Leverage}_t = \frac{\sum_i (\text{Market Equity}_{i,t} + \text{Book Debt}_{i,t})}{\sum_i \text{Market Equity}_{i,t}} \tag{22}$$

A few reasons for the different response in terms of magnitude are that the measure is only for the bank-holding companies of primary dealers. My data for book leverage is at the commercial bank level. Therefore, the samples are not strictly comparable. However, it is not especially surprising that they yield similar qualitative results as He et al. (2017) highlight that book and market leverage exhibit a strong positive correlation for the primary dealers in their sample.

Appendix C Robustness Checks

Figure C.1: Impulse Response of Provisions and Write-Offs

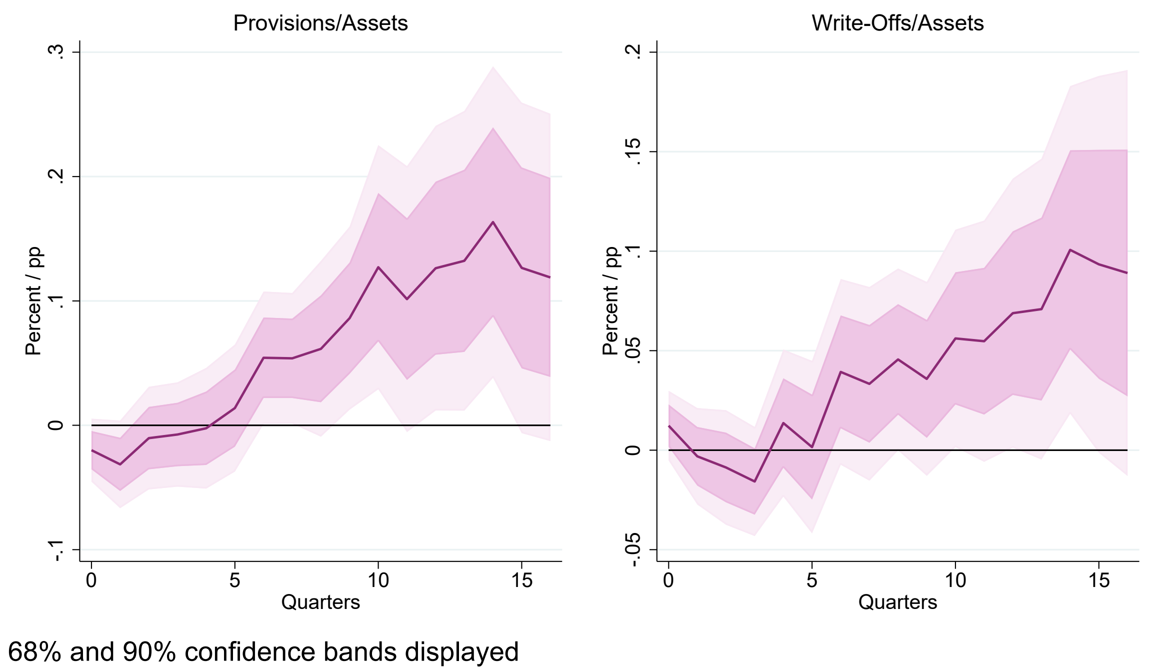


Figure C.2: Impulse Response of Regulatory Leverage to Contractionary Monetary Shock

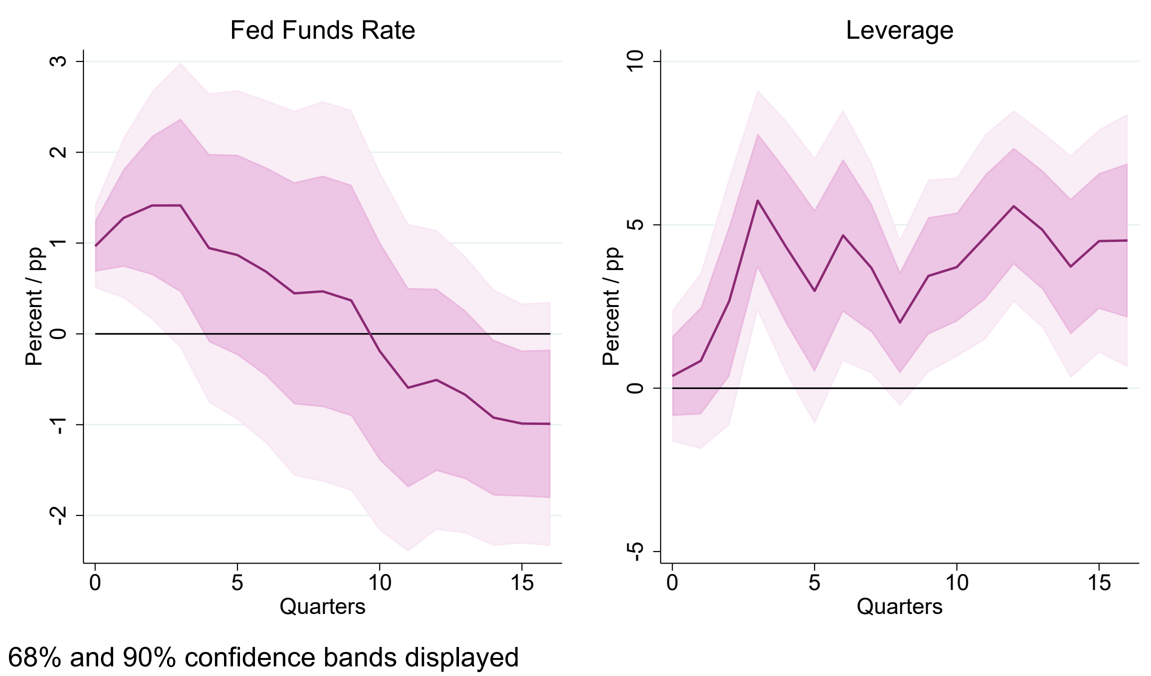


Figure C.3: Different Time Periods

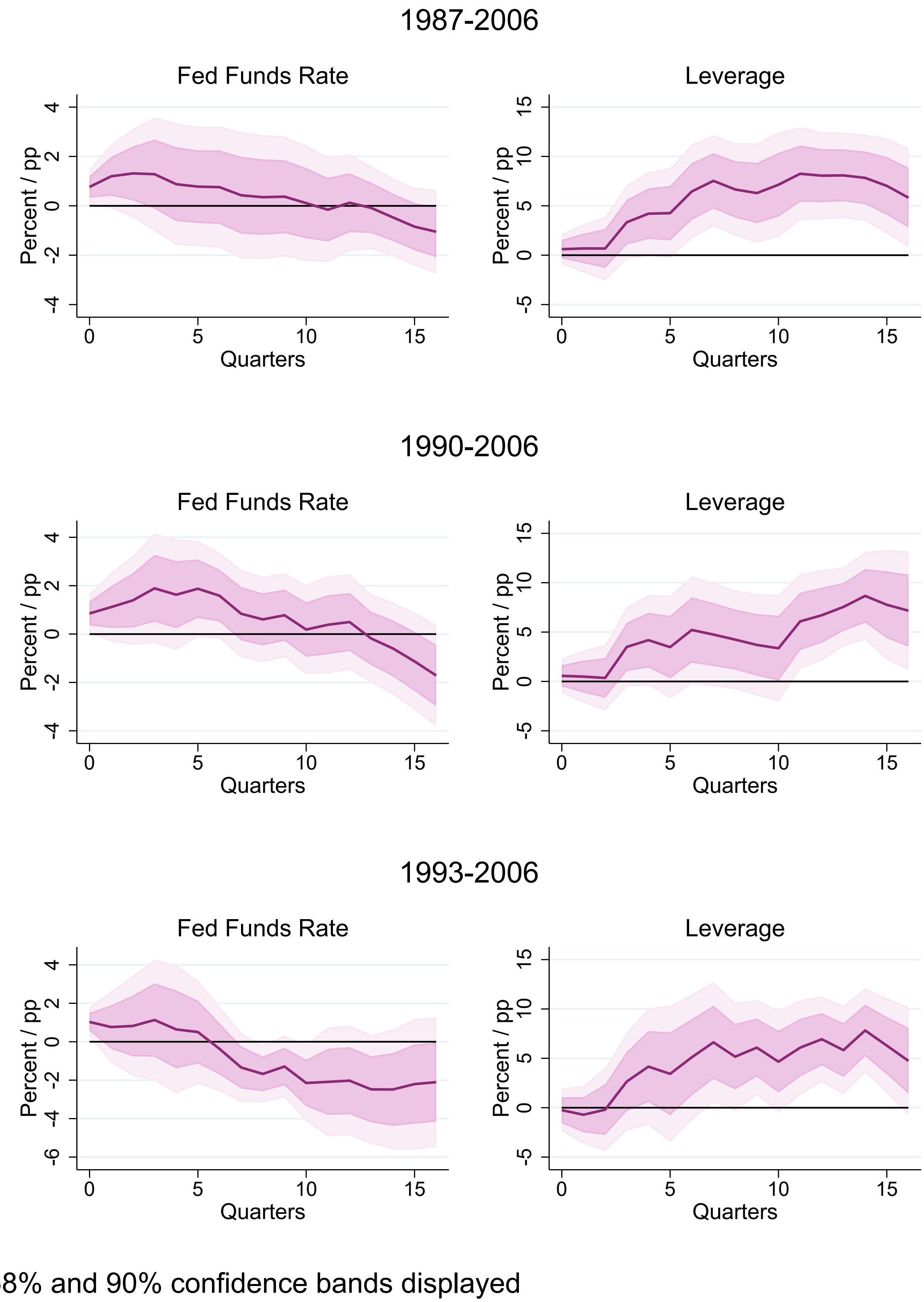
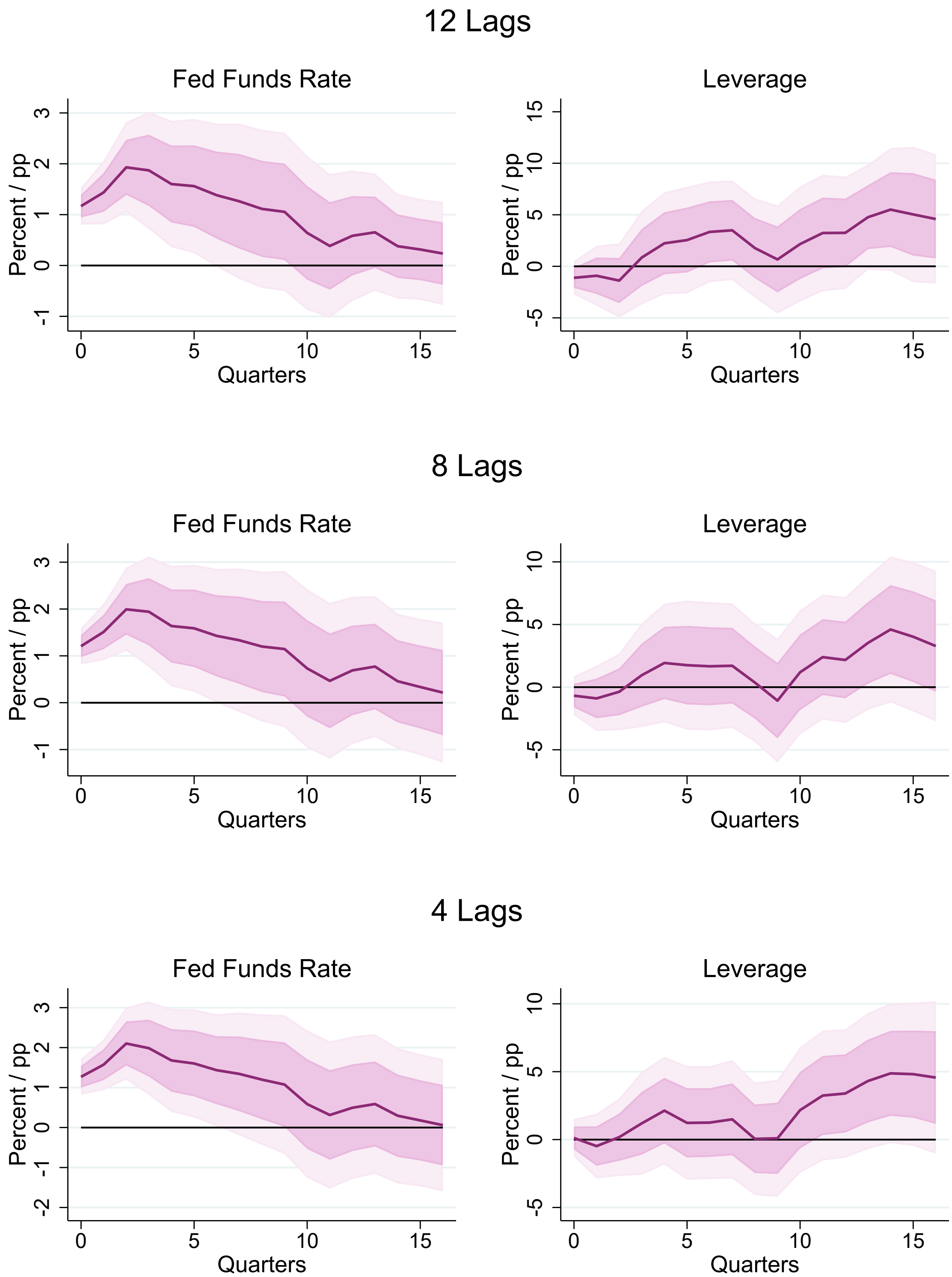
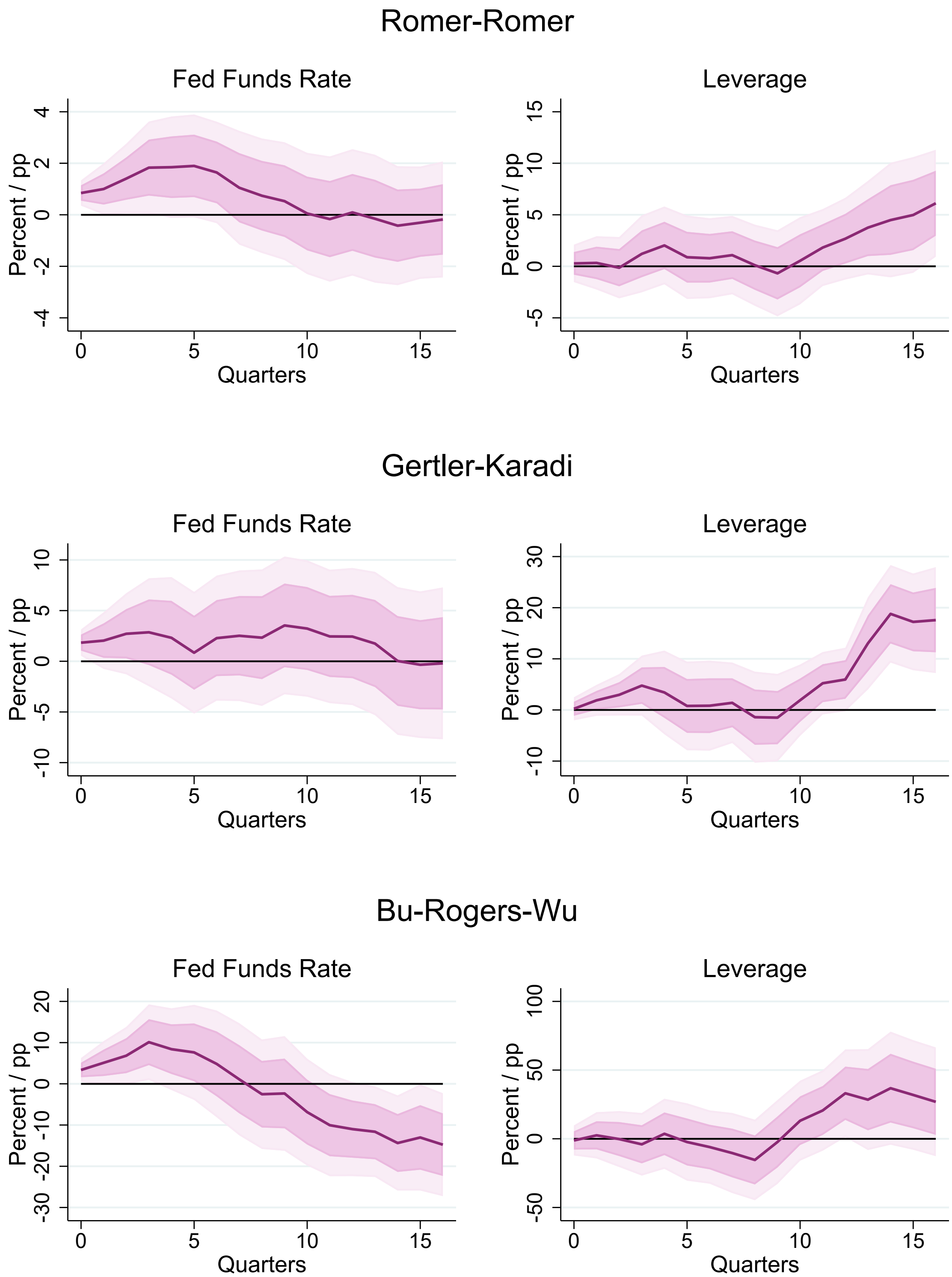


Figure C.4: Different Number of Lags



68% and 90% confidence bands displayed

Figure C.5: Book Leverage Response to different Monetary Policy Shock Series



68% and 90% confidence bands displayed

Figure C.6: Market Leverage Response to different Monetary Policy Shock Series

Appendix D Theoretical Model

The model builds on Kirti (2020) but incorporates credit risk through loan losses on floating-rate loans.

D.1 Bank Problem

The bank has the following objective

$$\max_{f_L} V_b = E[\pi_b] - \frac{\gamma}{2} Var[\pi_b] \tag{23}$$

where profits are given by the following

$$\pi_b = L(1 - f_L)(\bar{r} + \mu(f_L)) + Lf_L(\bar{r} + \varepsilon + \mu(f_L)) - D(\bar{r} + \beta\varepsilon) - Lf_L\theta(\varepsilon) \tag{24}$$

Therefore, we can rewrite V_b as

$$\begin{aligned} V_b = {} & L(1 - f_L)\bar{r} + Lf_L\bar{r} + L\mu(f_L) - D\bar{r} - Lf_L\overline{\theta(\varepsilon)} - \frac{\gamma L^2 f_L^2 \sigma_\varepsilon^2}{2} - \frac{\gamma D^2 \beta^2 \sigma_\varepsilon^2}{2} - \frac{\gamma L^2 f_L^2 \sigma_\theta^2}{2} \\ & + \gamma L f_L D\beta\sigma_\varepsilon^2 + \gamma L^2 f_L^2 \rho_{\varepsilon\theta} - \gamma D\beta L f_L \rho_{\varepsilon\theta} \end{aligned} \tag{25}$$

where $\sigma_\varepsilon^2 = Var[\varepsilon]$, $\sigma_\theta^2 = Var[\theta(\varepsilon)]$, $E[\theta(\varepsilon)] = \overline{\theta(\varepsilon)}$, and $Cov(\varepsilon, \theta(\varepsilon)) = \rho_{\varepsilon\theta}$. Note that I assume the following: $\sigma_\varepsilon^2 > \sigma_\theta^2$ and $\rho_{\varepsilon\theta} > 0$ where the latter captures that the loan-loss rate increases in the size of the monetary policy shock.

Taking the first-order condition with respect to f_L and simplifying yields the following expression for f_L^*

$$f_L^* = \frac{\frac{\partial\mu(f_L)}{\partial f_L} - \overline{\theta(\varepsilon)}}{\gamma L\left(\sigma_\varepsilon^2 + \sigma_\theta^2 - 2\rho_{\varepsilon\theta}\right)} + \frac{D\beta\left(\sigma_\varepsilon^2 - \rho_{\varepsilon\theta}\right)}{L\left(\sigma_\varepsilon^2 + \sigma_\theta^2 - 2\rho_{\varepsilon\theta}\right)} \tag{26}$$

Note that the term in parentheses in the denominator is positive as it is simply the variance of the difference between the monetary policy shock and the loan-loss rate. Therefore, the denominator is also positive. Moreover, the numerator in the second term is positive as $\sigma_\varepsilon^2 + \sigma_\theta^2 - 2\rho_{\varepsilon\theta} > 0$ and $\sigma_\varepsilon^2 > \sigma_\theta^2$, so $\sigma_\varepsilon^2 > \rho_{\varepsilon\theta}$.

All else equal, a bank would choose a higher floating share if it is more exposed to interest expense on its deposits (e.g., through a higher deposit-loan ratio or a higher deposit beta). This is because the floating share would act as a hedge. However, the bank will choose a lower floating share if it more exposed to credit risk from monetary policy shocks (e.g., through a higher $\overline{\theta(\varepsilon)}$). This is because the hedge comes at the cost of credit risk. The specific functional form of $\theta(\varepsilon)$ and its covariance with the shock will determine the sensitivity of these effects.

D.2 Firm Problem

The firm has a similar objective function (with the same risk-aversion coefficient), except that it is choosing how much invest, I, which it can only do through borrowing. So the firm objective function is

$$\max_I V_f = E[\pi_f] - \frac{\gamma}{2} Var[\pi_f] \tag{27}$$

where firm profits are given by the following

$$\pi_f = AI - I - I(1 - f_L)(\bar{r} + \mu(f_L)) - I f_L(\bar{r} + \varepsilon + \mu(f_L)) - I f_L \theta(\varepsilon) \tag{28}$$

Note that $I f_L \theta(\varepsilon)$ captures in, a reduced form way, that the firm cannot repay some of its floating-rate debt if there is a contractionary monetary policy shock.

We can now rewrite V_f as the following

$$V_f = AI - I - I\bar{r} - I\mu(f_L) - I f_L \overline{\theta(\varepsilon)} - \frac{\gamma}{2}\left(I^2 f_L^2 \sigma_\varepsilon^2 + I^2 f_L^2 \sigma_\theta^2 + 2 I f_L \rho_{\varepsilon\theta}\right) \tag{29}$$

Taking the first-order condition with respect to I and simplifying yields the following expression for $\mu(f_L)$

$$\mu(f_L) = A - 1 - \bar{r} - f_L \overline{\theta(\varepsilon)} - \gamma I f_L^2 \sigma_\varepsilon^2 - \gamma I f_L^2 \sigma_\theta^2 - \gamma f_L \rho_{\varepsilon\theta} \tag{30}$$

D.3 Equilibrium

In equilibrium, we will have a loan spread, μ^* that will equate firm credit demand, I, with bank loan size, L. So, using $I = L$ and plugging the derivative of (30) with respect to f_L into (26) yields the equilibrium f_L^*

$$f_L^* = \frac{D\beta\gamma\left(\sigma_\varepsilon^2 - \rho_{\varepsilon\theta}\right) - \gamma\rho_{\varepsilon\theta} - 2\overline{\theta(\varepsilon)}}{\gamma L\left(3\sigma_\varepsilon^2 + 3\sigma_\theta^2 - 2\rho_{\varepsilon\theta}\right)} \tag{31}$$

Note that the denominator of (31) is positive because γ, L, and $3\sigma_\varepsilon^2 + 3\sigma_\theta^2 - 2\rho_{\varepsilon\theta}$ are all positive.[37] Moreover, given that D and β are positive, and that $\sigma_\varepsilon^2 > \rho_{\varepsilon\theta}$, then we have $\frac{\partial f_L}{\partial \beta} > 0$, which is consistent with banks using floating-rate loans as a hedge against interest rate risk. One can also show that f_L^* is positive as $\frac{\rho_{\varepsilon\theta}}{D\beta} \approx 0$, $\frac{2\overline{\theta(\varepsilon)}}{D\beta\gamma} \approx 0$, and $\sigma_\varepsilon^2 > \rho_{\varepsilon\theta}$.[38]

[37] $3\sigma_\varepsilon^2 + 3\sigma_\theta^2 - 2\rho_{\varepsilon\theta}$ is positive because $3\sigma_\varepsilon^2 + 3\sigma_\theta^2 - 2\rho_{\varepsilon\theta} > \sigma_\varepsilon^2 + \sigma_\theta^2 - 2\rho_{\varepsilon\theta} \equiv Var(\varepsilon - \theta) > 0$

[38] Note this requires that σ_ε^2 has to be sufficiently large relative to $\rho_{\varepsilon\theta}$.