Can Monetary Policy Support Financial Stability? Evidence From Bank Leverage

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Abstract

Should monetary policy be used to support financial stability? While this question has been hotly debated for decades, it remains unresolved. Clearly, it is not possible to resolve this debate without first knowing whether monetary policy *can* support financial stability, in particular, by reducing bank leverage. A vast theoretical literature claims that increasing interest rates reduces bank leverage. However, I show empirically that raising interest rates actually *increases* bank leverage. My result is robust to varying the specification and using different measures of monetary policy shocks. I propose and empirically validate a mechanism that can explain this result which I term the *loan-loss mechanism*: contractionary shocks increase loan losses which reduce bank profits and bank equity, which ultimately increases bank leverage. I show that this mechanism explains the overall increase in bank leverage in response to such shocks. Finally, I highlight why much of the theoretical literature is unable to correctly explain the leverage response and identify the elements that are necessary for an empirically consistent model.

Keywords: Leverage, Monetary Policy, Financial Stability, Banking

JEL Codes: E52, G18, G21, G23

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1 Introduction

Should central banks raise interest rates in order to reduce bank leverage and subsequently lower the risk of financial crises? The question of whether monetary policy should target financial stability has been widely debated in academia and central banks for decades. Indeed, Former Federal Reserve Chair Ben Bernanke, as recently as 2020, in his American Economic Association Presidential Address, emphasised that this lively debate remains unresolved.[1] However, before one can resolve this debate, it is critical to first know whether monetary policy *can* support financial stability. In this paper, I explicitly address a core component of this first-order question: do contractionary monetary policy shocks actually reduce 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) takes 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 cycles, also supports leaning against a boom.[2] 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,

[1] https://www.brookings.edu/wp-content/uploads/2019/12/Bernanke_ASSA_lecture.pdf

[2] 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).

leverage."[3]

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.[4]

The first contribution of this paper is to provide compelling 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 (e.g., Romer and Romer (2004) and Gertler and Karadi (2015)). 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 completely contrary to much of the theoretical literature. I find that increasing interest rates leads to an increase in bank leverage. I show that this finding is robust to using 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 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

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

[4] 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 estimation of the domestic bank leverage response to domestic monetary policy shocks. Indeed, 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.

losses decrease bank profits which subsequently reduce bank equity.[5] 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.

My final contribution is to dissect the theoretical literature in order to show where and why so many, and such different, models make an empirically inaccurate claim as well as highlight the elements of an empirically consistent model. 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 failure of the literature appears to derive from three broad, though not necessarily mutually exclusive, modelling decisions. First are models such as Gertler and Kiyotaki (2010) and Gertler and Karadi (2011) that, while able to generate an increase in leverage in response to contractionary monetary policy shocks, attribute this rise to an increase in profitability. However, this proposed mechanism is inconsistent with the observed evidence that profitability falls rather than rises in response to a monetary contraction. Second are models such as Woodford (2012) and Rannenberg (2016) that incorrectly rely on the observed procyclical behaviour of leverage and incorrectly conclude from this that leverage declines in response to monetary policy tightening. Third are models such as Angeloni and Faia (2013) and Drechsler et al. (2018b) that rely on a typical substitution effect as the dominant mechanism through which higher interest rates reduce bank leverage. As such, these models also inaccurately claim contractionary monetary policy reduces bank

[5] 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.

leverage.

A common missing ingredient from all these models is the loan-loss mechanism. I argue that a core element of an empirically consistent model is that bank profits both today and in the future are negatively affected by a monetary policy contraction today which can be interpreted empirically as loan losses. I use the framework of Corbae and Levine (2022) as an example to show how such dynamics can be modelled.

The overall contributions of this paper lend support to the conclusions of Svensson (2017) and Svensson (2018) that monetary policy should not target financial stability. Svensson argues that an important cost of monetary policy that targets financial stability is that it weakens the economy by having 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. 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. Monetary policy should focus solely 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 wrong while Section 5 highlights the important elements of an empirically consistent model. Section 6 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, and Appendix C presents robustness checks in relation to the time-series evidence.

2 Data

An important component of this paper is to bring together both aggregate and individual bank-level data, ensuring that the individual bank-level data aggregates up to match the aggregate data, with both 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 begins from the first quarter of 1984 to the last quarter of 2007. Given that the GFC from 2007-08 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 Banking Sector-Level 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.[6]

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. The second measure is regulatory equity which is also known as Tier 1 capital. This measure 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 has agreed that the Tier 1 Leverage Ratio (i.e., regulatory equity divided by average assets over the quarter) represents a more accurate measure of the losses a bank can withstand in response to a shock. Therefore, when referring to leverage, I will be referencing total assets divided by regulatory equity, unless otherwise specified.[7]

From aggregate income statements, I have four main variables. First, I collect data on

[6] 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.

[7] Note that this differs in two minor ways from the regulatory measure. 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.

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.[8] 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.[9] 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 10 in Appendix C).

2.2 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 forecasts (i.e., the Greenbook 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

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

[9] 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)).

the economy from the desired exogenous shock by regressing the intended funds rate change on the current rate and on the Greenbook 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 2007. 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 also repeat my analysis with two additional monetary policy shock series. I choose shock series that are estimated using completely different identification strategies and that have a sufficient time-series. 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-section 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.

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 1984-2007 (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.[10]

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, and the R^2 is barely above 0.15.

3.1 Key result

My baseline specification is to estimate (1) using data from the first quarter of 1984 until the last quarter of 2007 with 16 lags and the RR shock series. Figure (1) below depicts the impulse responses of the FFR and leverage.

[10] 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.

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 around 5 percent within a year. 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 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 regulatory definition of leverage described in Section 2.1 (i.e., regulatory equity divided by total assets). In Figure 11, I show the same analysis when using the simple measure of leverage (i.e., total equity divided by total assets). The results do not change in any meaningful way. Next, in Figure 13, 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 1987-2007, 1990-2007, and 1993-2007. 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, 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.

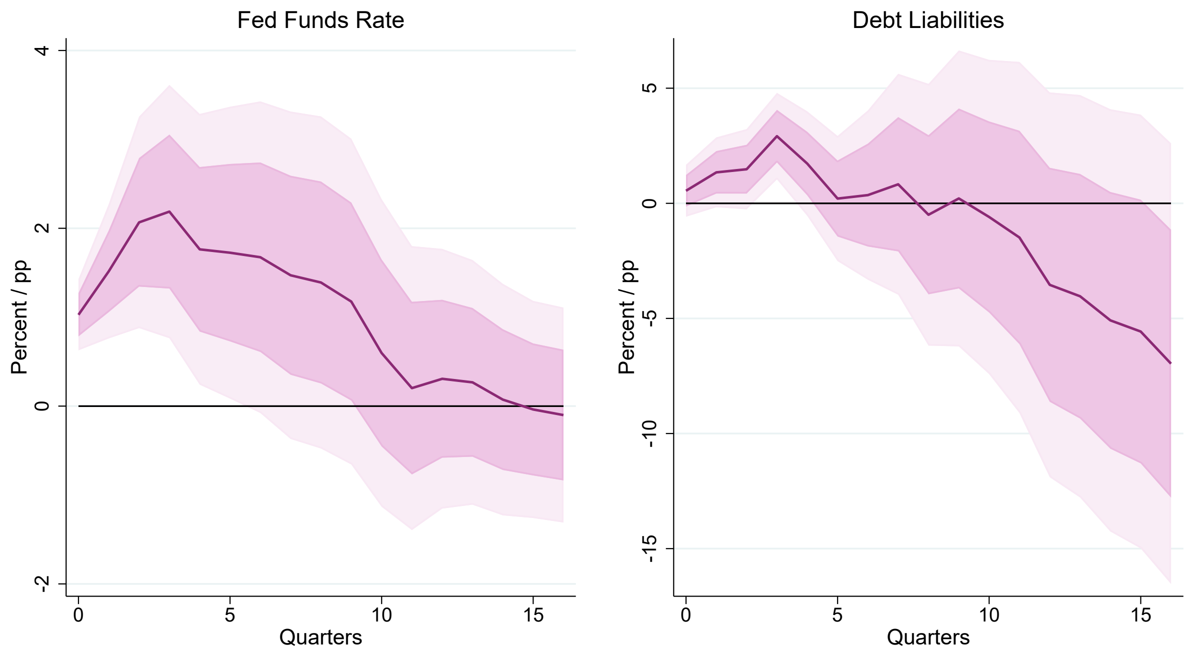
My final, and perhaps strictest, robustness test is to use completely different shock series. To ensure comparability, I estimate all of them using data from 1994 until 2007 as this is the largest overlapping period. Given the shorter time-horizon, I use 4 lags, otherwise the specification is as in (1). Figure 15 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. The robustness of the result warrants further exploration into the possible mechanisms to understand what is driving this potentially counterintuitive result.

3.2 The Loan-Loss Mechanism

The literature highlights several different mechanisms that might cause an increase in interest rates to decrease leverage (see Section 4). One of the more intuitive reasons is that higher interest rates make debt-financing (i.e., borrowing) more expensive relative to equity-financing for banks. Given banks decrease the size of their balance sheets (i.e., total assets or total liabilities) in response to a 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.

My main finding that higher interest rates increase bank leverage does not necessarily contradict the prediction that, *all else equal*, an increase in debt financing costs would reduce leverage. Indeed, when estimating (1) with debt-liabilities as the outcome variable, I find that debt-liabilities decrease in response to a contractionary shock (Figure 12).

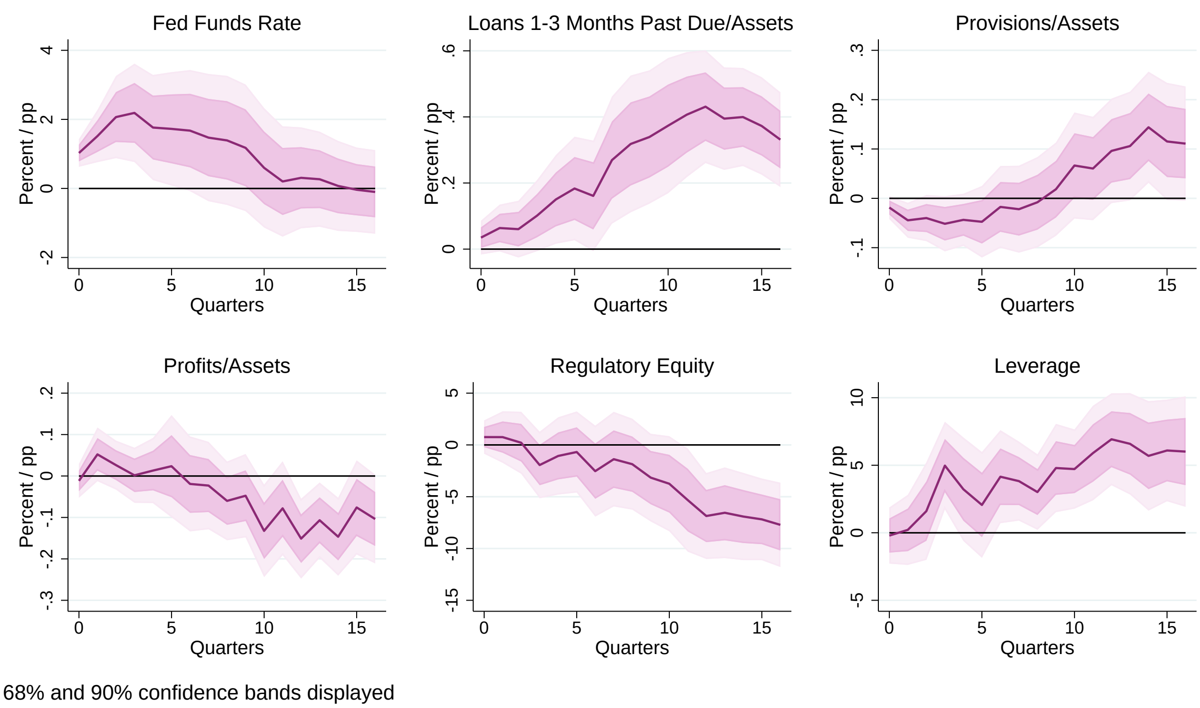
Figure 2: Mechanism Underlying Leverage Response



Therefore, 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 1984-2007) separately for each of the five variables emphasised in the paragraph before. Specifically, each of the variables will be z in (1). Figure 3 show 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 1 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 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 fourth panel (bottom-left) shows that profits as a proportion of total assets decrease by around 0.15 percentage points at around the same time 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 fall 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 3: Mechanism Underlying Leverage Response



3.3 Importance of the Loan-Loss Mechanism

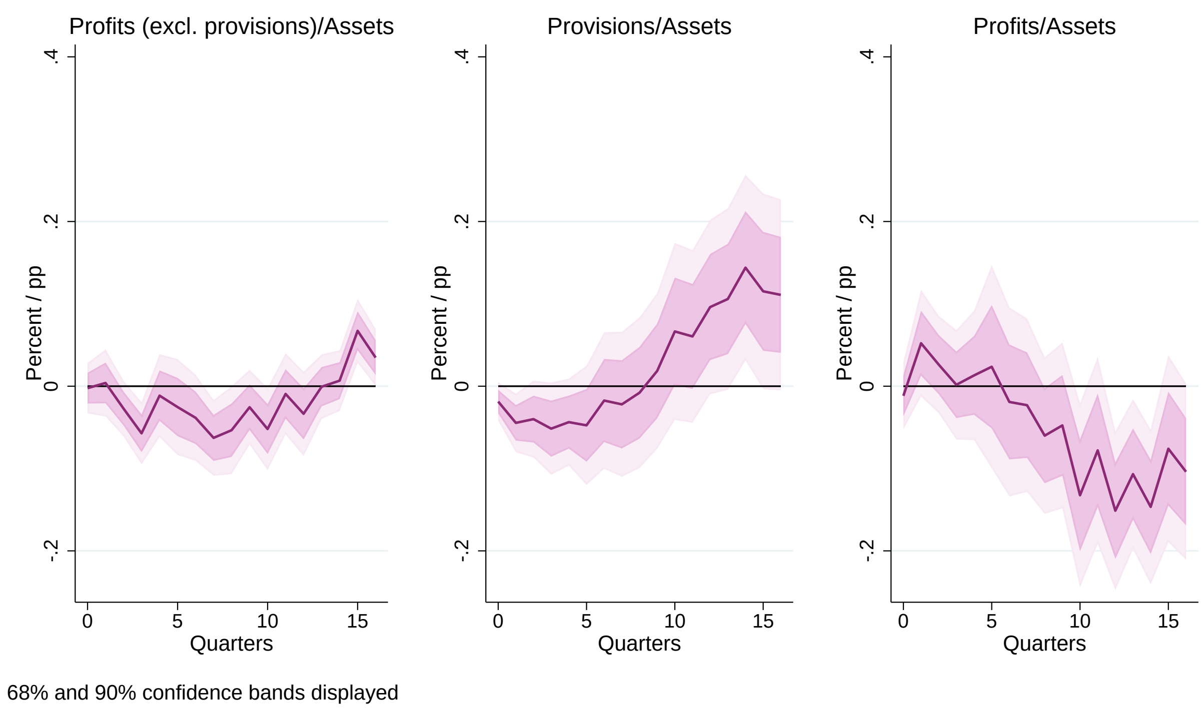
Despite the evidence supporting my proposed mechanism, it is still possible that there are other mechanisms that might be more important in terms of driving the overall increase in leverage. One general approach to deal with this kind of concern is to rule out alternative mechanisms. However, such an 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. However, in this section, I take advantage of accounting identities to show that my proposed mechanism directly explains 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 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:

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

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. Figure 4 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.

Figure 4: Decomposing the Profit Decline

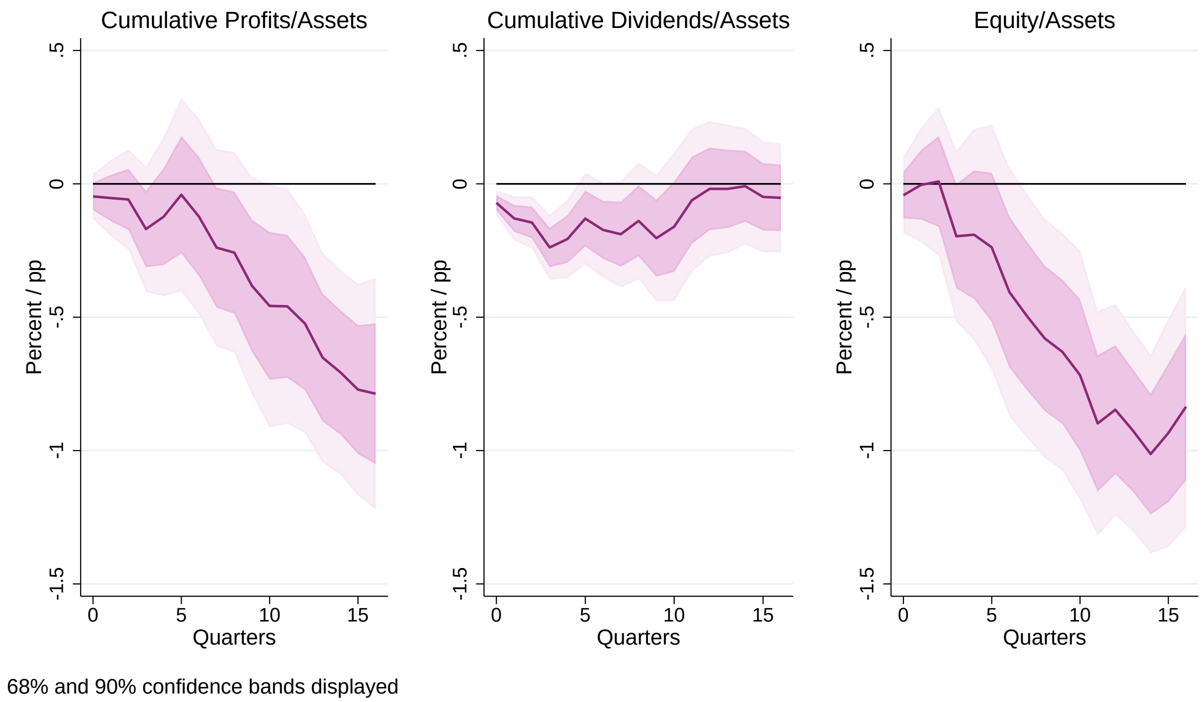


As can be seen, the variation in overall profits is almost entirely driven by the variation in provisions with the remaining variation (captured by profits excluding provisions) being relatively immaterial. Therefore, my mechanism appears to be the core driving force behind the fall in profits. The second 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 will take is to utilise the following identity for a balance sheet item at time T:

$$\frac{\sum_{t=1}^{T} \text{Profits}_t}{\text{Assets}_T} - \frac{\sum_{t=1}^{T} \text{Dividends}_t}{\text{Assets}_T} + \frac{\text{Accounting Adjustments}_T}{\text{Assets}_T} = \frac{\text{Equity}_T}{\text{Assets}_T} \tag{3}$$

Note that (3) shows we need a measure of cumulative profits to transform an income statement measure to a balance sheet measure. Equity at time T is constructed by adding all profits earned before T then subtracting all dividends paid before T and finally making some accounting adjustments (e.g., revaluations) at time 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. 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 5 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.

Figure 5: Decomposing the Leverage Increase



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. Indeed, it is interesting that the response of cumulative dividends is so 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 whole section is that the macroeconomic/banking models used to understand monetary policy and its interaction with financial stability should allow for, at least, the potential for contractionary interest rates to raise bank leverage.

4 Why do so many models get this wrong?

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, i.e., that 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. However, this mechanism 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 failure of the literature appears to derive from three broad, though not necessarily mutually exclusive, modelling choices: relying on an empirically inconsistent profitability channel; relying on procyclical leverage; and, relying on a substitution effect. In this section, I will explain how each of these modelling choices leads to the model being empirically inconsistent as well as highlighting the types of papers in each category.

4.1 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 increase alongside an increase in profitability (which I term the profitability channel). This is completely 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.[11]

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

[11] This modelling approach is widely used in the literature e.g., Gertler et al. (2012), Gertler and Kiyotaki (2015), Maggiori (2017), Gertler et al. (2020), Ghote (2021), and Sims and Wu (2021).

highlight the impact of monetary policy on bank leverage.[12]

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 empirically inconsistent. 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.[13] Each claim has price Q. Therefore, the net worth (equity), N, of the bank is given by the following balance sheet constraint:

$$N = QS - B \tag{4}$$

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

[12] 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.

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

liabilities. The value of the bank is therefore given by the following:

$$V = R_k QS - RB \tag{5}$$

We can plug in the balance sheet constraint, equation (4), into the bank objective function above to yield the following:

$$\begin{aligned} V &= R_k QS - R(QS - N) \\ &= \underbrace{(R_k - R)}_{\text{profitability}} QS + RN \end{aligned} \tag{6}$$

Equation (6) 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 (6) 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:

$$V \geq \lambda QS \tag{7}$$

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 (7) will hold with equality and so we can equate equations (6) and (7) together. This yields the following:

$$\lambda QS = (R_k - R)QS + RN$$
$$\implies \text{leverage} \equiv \frac{QS}{N} = \frac{R}{\lambda - (R_k - R)} \tag{8}$$

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. 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 subquently 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 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.

On the other hand, 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 ignore how monetary policy is being transmitted to banks (i.e., through loan losses), it actively suggests a mechanism that is contradicted by the data (profits rise in Gertler and Karadi (2011) but fall in my empirical analysis). 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.

Figure 6: Impulse Response of Profits to Contractionary Monetary Shock

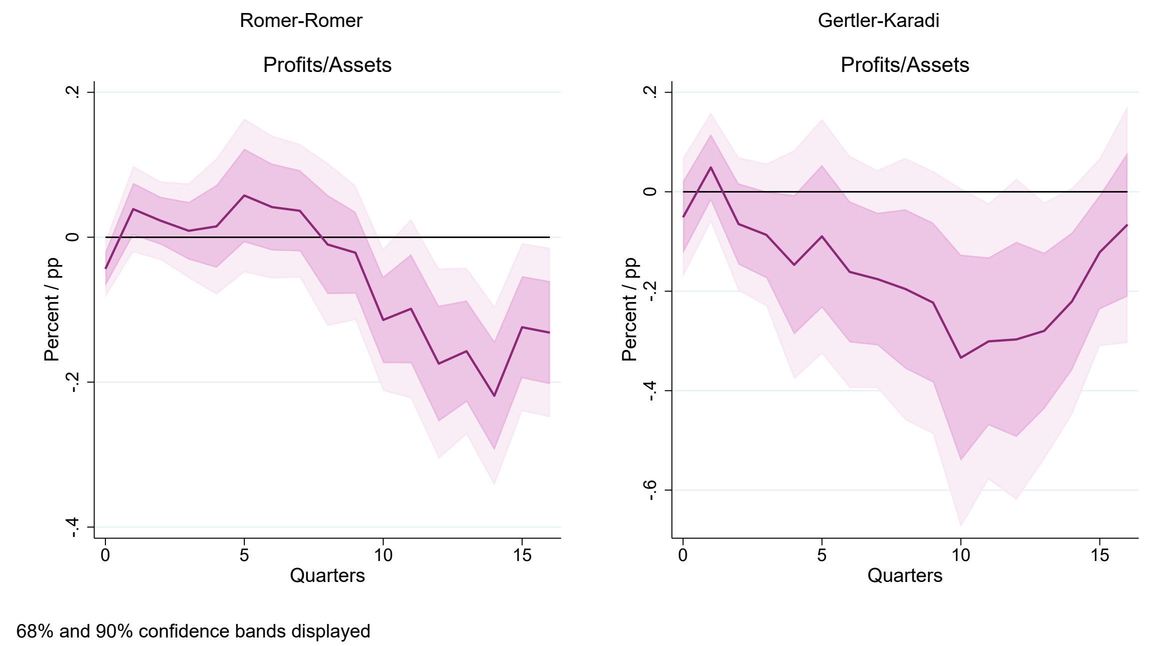


Figure 6 uses the specification in equation (1). It shows the response of profits with an RR shock and a GK shock.[14] Both shock series generate broadly the same result, lending further credibility to the empirical finding that a contractionary shock decreases profits while the Gertler and Karadi (2011) model predicts a rise.[15]

Secondly, even if the primary objective of this class of models is prediction rather than understanding an economic mechanism, the results remain unconvincing. 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 (7) to replace QS). This yields the following:

$$\text{leverage} = \frac{V}{\lambda N} \tag{9}$$

[14] Given that the GK shock starts later than the RR shock, the data underlying the figure is from 1994 onwards and twelve lags are used instead of sixteen due the shorter time horizon.

[15] Note that 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.

In these types of models, bank value (V) is linear in bank assets and wealth (net worth)[16]

$$V = vQS + \eta N \tag{10}$$

where v is the marginal value of expanding assets and η is the marginal value of expanding net worth. Plugging equation (10) into equation (9) yields the following:

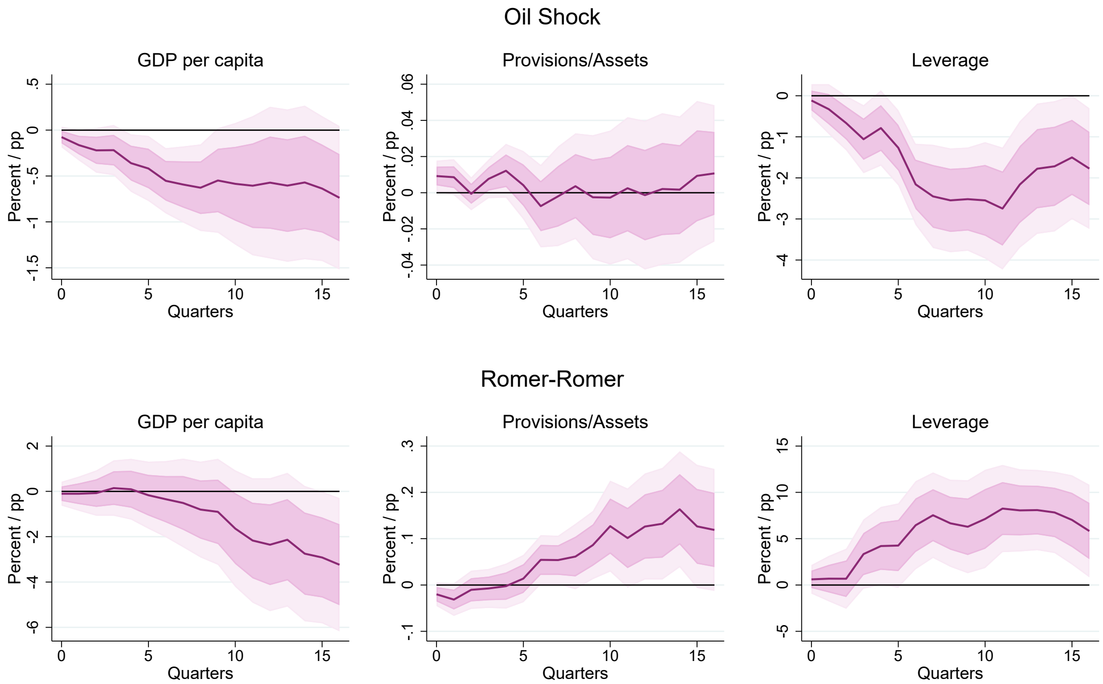
$$\begin{aligned}
\text{leverage} &= \frac{vQS + \eta N}{\lambda N} \\
\implies \lambda \cdot \text{leverage} &= v\frac{QS}{N} + \eta \\
\implies \lambda \cdot \text{leverage} - v \cdot \text{leverage} &= \eta \\
\therefore \text{leverage} &= \frac{\eta}{\lambda - v}
\end{aligned} \tag{11}$$

where the penultimate step makes use of the fact that leverage $\equiv \frac{QS}{N}$. Equation (11) 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 *any* negative shock increases the marginal value of net worth (η) and assets (ν), then *any* negative shock will also increase leverage.[17] 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, one immediate counterexample can be obtained by using oil shocks. I use the recent oil shock series of Känzig (2021) which exploits institutional features of the Organization of the Petroleum Exporting Countries and high-frequency data to identify an oil supply news shock.

[16] 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.

[17] 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.

Figure 7: Impulse Responses to Oil Shock vs Monetary Policy Shock



68% and 90% confidence bands displayed

Figure 7 above shows the impulse responses in relation to negative oil shocks (top row) and monetary policy shocks (bottom row) using the specification in equation (1).[18] It clearly provides a counterexample to the prediction in Gertler and Karadi (2011) as we have a negative shock that leads to a decrease in leverage (top right panel). So why is Gertler and Karadi (2011) able to accurately predict leverage in one scenario but not the other? One reason is because there is no distinction by type of shock in the model. However, as is evident in the figure, the type of shock matters empirically not just in terms of magnitude but also direction. While both shocks cause a decline in GDP (column one), only the monetary policy shock increases loan losses (column two).[19] This highlights the importance of the underlying loan-loss mechanism; a component entirely 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 appears somewhat spurious.

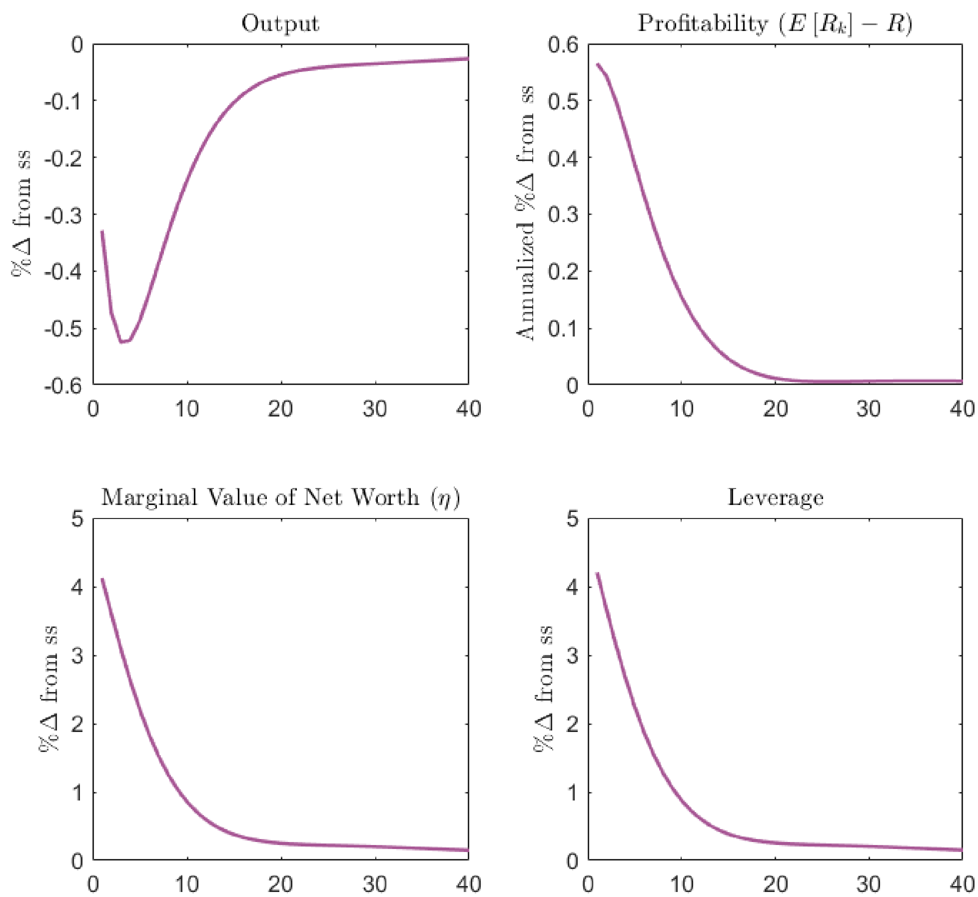
Thus far, we have only used a partial equilibrium analysis to understand how and why models that use a Gertler-Karadi-Kiyotaki type constraint to model banks generate implications that are inconsistent with empirical evidence. While such analysis more easily highlights the

[18] Specifically, I use data from 1984 until 2007 and use sixteen lags.

[19] One reason that higher interest rates increase loan losses is that many loans such as Commercial and Industrial loans are variable-rate loans and so move with the central bank rate. See https://fred.stlouisfed.org/series/EEANQ

underlying intuition and dynamics, one might argue that the full GE model could generate different results. Therefore, in Figure 8 below, I show the results obtained from the full GE model in response to a contractionary monetary policy shock.[20]

Figure 8: Gertler and Karadi (2011) Model Response to Monetary Policy Contraction



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 (11), Gertler and Karadi (2011) also show that a negative total factor productivity shock leads to an increase in leverage.

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

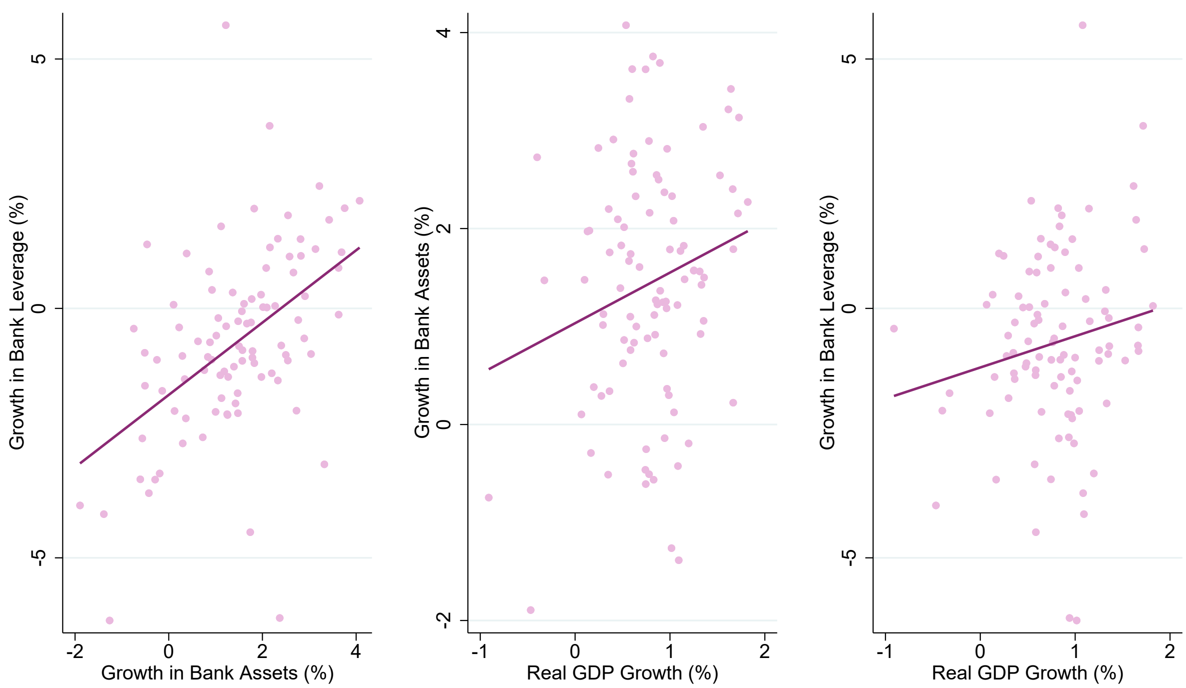
[20] The model code is obtained from the Macroeconomic Model Data Base (see `https://www.macromodelbase.com/`).

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.

4.2 Models that rely on leverage procyclicality

This class of models are more eclectic in their underlying structures than those in Section 4.1, 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 9 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.[21]

Figure 9: Procyclicality of Bank Leverage



The core issue in this class of models is that they *incorrectly* rely on leverage procyclicality. Specifically, in these models, a contractionary monetary shock will reduce output and

[21] 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.

because leverage is procyclical, it will also reduce leverage. Clearly relying on leverage procyclicality in this way yields an empirically inconsistent result.

There are several ways in which models rely on procyclicality. 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 simply match conditional moments in a model (i.e., how leverage responds to different shocks) to unconditional moments in the data (i.e., how leverage is correlated with GDP) as these are two entirely different measures. Consider the evidence in Figure 7. 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 nonsensical 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 and the response of leverage to monetary policy. This leads to the erroneous 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 incomes of the two types of households. Woodford (2012) then derives a modified intertemporal IS equation:

$$y_t - g_t + \chi\Omega_t = \mathbb{E}_t[y_{t+1} - g_{t+1} + \chi\Omega_{t+1}] - \sigma[i_t - \mathbb{E}_t\pi_{t+1}] \tag{12}$$

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 (12) 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.

$$\pi_t = \kappa_y y_t + \kappa_\Omega \Omega_t + \beta\mathbb{E}_t\pi_{t+1} + u_t \tag{13}$$

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 new 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). Moreover, 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. This latter probability is crucial as it both time-varying and is a function of endogenous macroeconomics conditions. Specifically, Woodford (2012) assumes that γ_t is an increasing function of bank leverage (L_t). Intuitively, as leverage is higher, the probability of going into a crisis is higher. However, to complete the model, Woodford (2012) connects leverage with the remaining endogenous variables by postulating a simple law of motion:

$$L_t = \rho L_{t-1} + \xi y_t + v_t \tag{14}$$

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:

$$\frac{1}{2}E_0\sum_{t=0}^{\infty}\beta^t\left[\pi_t^2+\lambda_y y_t^2+\lambda_\Omega\Omega_t^2\right] \tag{15}$$

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 (14), 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, unlike Gertler and Karadi (2011), 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 be that not only would both inflation and the output gap fall 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.

One important aside here is that this class of models typically derives a monetary policy *rule* that suggests leaning against the wind (e.g. Woodford (2012) and Ghote (2021)). On the other hand, other models (e.g., Gertler and Karadi (2011) and Drechsler et al. (2018b)) use exogenous monetary policy shocks. Therefore, one may argue that my analysis is only pertinent to the papers considering exogenous shocks. However, Wolf and McKay (2022) show that analyses using exogenous shocks and analyses considering alternate policy rules are actually equivalent under certain conditions. Specifically, if policy affects private-sector behaviour only through the current and future expected path of the policy instrument (which is 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.

The Woodford (2012) model, while highly stylised, is very influential as it builds on a simple New Keynesian structure. Like Gertler and Karadi (2011), however, it completely ignores the empirical loan-loss mechanism and relies instead on a postulated law of motion that embeds procyclicality. However, 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 substitution effect

The mechanisms in Sections 4.1 and 4.2 were more involved than a substitution effect. As such, in those sections I used specific models to shed light on the underlying intuition. However, the substitution effect is a far more intuitive and simple mechanism and so does not require as much analysis. Specifically, the substitution effect would require that an increase in interest rates would raise the relative cost of debt-financing for banks and so banks would 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 as much detail 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 on 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 (i.e., the banks 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 deposits. Again, this is essentially a substitution effect but in this model it is induced by the dynamics of liquidity insurance.

Clearly, models that predominantly rely on a substitution effect as the dominant mechanism will inevitably make the empirically inaccurate conclusion that contractionary monetary policy reduces bank leverage. It is worth noting here that my empirical analysis does not disregard whether this mechanism can exist. Rather, my analysis actually supports the existence of such an effect as non-equity liabilities (including deposits) fall in response to a contractionary monetary policy shock (See Figure 12 in Appendix C). However, the more important contribution of my empirical analysis is that the empirically dominant mechanism is the loan-loss mechanism which not only offsets the effect on leverage but actually leads to a reversal in sign such that contractionary monetary policy shocks increase leverage.

5 Elements of an empirically consistent 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 the models 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)). Therefore, in this section, rather than adding another model to an already rapidly growing set of models, I will highlight what are the core ingredients required within a model of the banking sector such that the model is better able to match the empirical dynamics. I will show this using the example of the Corbae and Levine (2022) model as it performs especially well empirically.

Corbae and Levine (2022) provide a tractable dynamic model of banking which features financial frictions (agency conflicts and limited liability), endogenous market structure, and optimal regulatory and monetary policy. The model features N banks which Cournot compete for insured deposits. To start a bank i, the manager must pay a fixed entry cost, κ, which is financed through an initial equity injection E_i from investors who have linear preferences and discount factor δ. Loans, L_i, are financed solely through deposits, D_i, as secondary equity issuance is prohibitively expensive. This implies that for each bank i, $L_i = D_i$. The total supply of deposits, Z, is simply the sum of the deposits at each bank (i.e. $Z = \sum_{i=1}^{N} D_i$) with an inverse deposit supply function $r_D(Z) = \gamma Z$. γ can be thought of as capturing the intensity of competition as a higher γ would raise the cost of attracting deposits. The loan works as follows: the bank manager chooses the size of the loan, D_i, and the riskiness, S_i where $S_i \in [0, 1]$ yields $A \cdot S_i$ with probability $p(S_i)$ and 0 otherwise. Lending exhibits a typical risk-return trade-off as $\frac{dp(S_i)}{dS_i} < 0$. Corbae and Levine (2022) specifically assume the parametric form of $p(S_i)$ as $p(S_i) = 1 - S_i^\eta$ where $\eta \geq 1$. Crucially, they also introduce a parameter α that controls the marginal cost of obtaining funds which reflects the central bank interest rate. The incumbent bank's static profit function, where N is taken as given, is therefore

$$\pi_i(S_i, D_i; N) = p(S_i)[\underbrace{A \cdot S_i - (r_D(Z) + \alpha)}_{R_i}]D_i \tag{16}$$

where R_i is the measure of profitability. Given the above, the incumbent manager maximises the present value of the solvent bank i at discount rate β which yields the following dynamic problem:

$$V_i(N) = \max_{S_i, D_i} \pi_i(S_i, D_i; N) + \beta p(S_i) V_i(N') \tag{17}$$

subject to the following leverage constraint

$$\frac{D_i}{E_i} \leq \lambda \tag{18}$$

where N' represents the number of banks next period and λ represents the leverage limit set by regulators. Note that Corbae and Levine (2022) assume $\delta \geq \beta$ which generates the agency conflict between managers and investors. However, neither the agency conflict nor the leverage constraint will be consequential for the remaining analysis.

Given this basic and fairly intuitive set-up, why and how does this model succeed? The intuition is best ascertained by deriving the first order condition for choice of risk, S_i, with respect to the bank manager problem in two equations above. This yields the following equation:

$$p(S_i)AD_i + \frac{dp(S_i)}{dS_i}R_iD_i + \frac{dp(S_i)}{dS_i}\beta V_i(N') = 0 \tag{19}$$

The first term is the marginal benefit from taking more risk (i.e., the expected revenue from that risk in the successful state). The second and third terms are marginal cost terms (they are negative as $\frac{dp(S_i)}{dS_i} < 0$). The second term shows the cost of the lower likelihood of success on current profits due to the higher risk. Finally, and perhaps most relevant in relation to my empirics, is the third term. This shows that higher risk has a cost in terms of *future* value. While Corbae and Levine (2022) do not link this explicitly to loan losses, one can certainly consider this as one interpretation of the third term. Specifically, monetary policy raises the risk of existing loans (borrowers have to pay more to finance their debts). Therefore, using the first order condition (i.e., equation (19)), we can see that this higher risk will imply lower future profits, exactly akin to my empirics.

A natural next question is to ask how leverage is affected. One can show analytically (see appendix), using the parameters set by Corbae and Levine (2022), that a contractionary monetary policy shock (i.e., an increase in α) increases bank leverage. The model works as follows. A contractionary shock raises the marginal cost of obtaining funds which lowers profits and leads banks to take more risk. Together, these effects reduce equity. The reason equity falls is because equity is just the present discounted value of profits (i.e., $\frac{\pi_i(N)}{1-\delta p(S_i)}$) and we know the numerator falls (due to declining profits) and the denominator rises (higher risk means $p(S_i)$ falls). While lending also falls, the fall in equity is greater which leads to an overall increase in leverage. Table A3 in the Appendix of Corbae and Levine (2022) documents the numerical estimates: profits fall by 27%, equity falls by 42%, and leverage rises by 57%. This set of results map remarkably well onto my empirical mechanism whereby a contractionary monetary policy shock raises loan losses which reduce profits and equity and subsequently raise leverage. Again, while Corbae and Levine (2022) do not explicitly mention loan losses, their model is general enough to accommodate that interpretation.

This section has therefore highlighted that a core element for an empirically successful model is that future bank value is affected by a monetary policy contraction today either directly through loan losses (as per my empirical findings) or in a way that is more general but accommodates the interpretation of loan losses (as per Corbae and Levine (2022)). Moreover, this channel needs to be strong enough in the model so that a contractionary shock will, on net, reduce future value by causing profits to decline which will subsequently erode equity and therefore increase leverage.

6 Conclusion

In this paper, I explore the following question: can contractionary monetary policy in the form of 'leaning against the wind' support financial stability by reducing bank 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, using accounting identities, I show that the loan-loss mechanism can explain nearly all the variation in bank leverage in response to monetary policy shocks. This highlights the importance of understanding bank balance sheets in order to understand the transmission of monetary policy.

Moving on to the theoretical literature, I show that the failure to make empirically consistent claims derives from 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 the loan-loss mechanism. I show, using the example of Corbae and Levine (2022), that when monetary policy tightening has a negative effect on bank profits today and in the future through loan losses, the model is able to successfully match the empirical dynamics.

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 Svensson (2017) and Svensson (2017) that monetary policy should focus on its mandate of price stability, leaving issues of financial stability to macroprudential policy.

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Appendix A 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.

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 evaluate aggregate macroeconomic effects. Market leverage is only available for listed banks and so would significantly narrow the scope of the analysis.

Appendix C Robustness Checks

Figure 10: Impulse Response of Provisions and Write-Offs

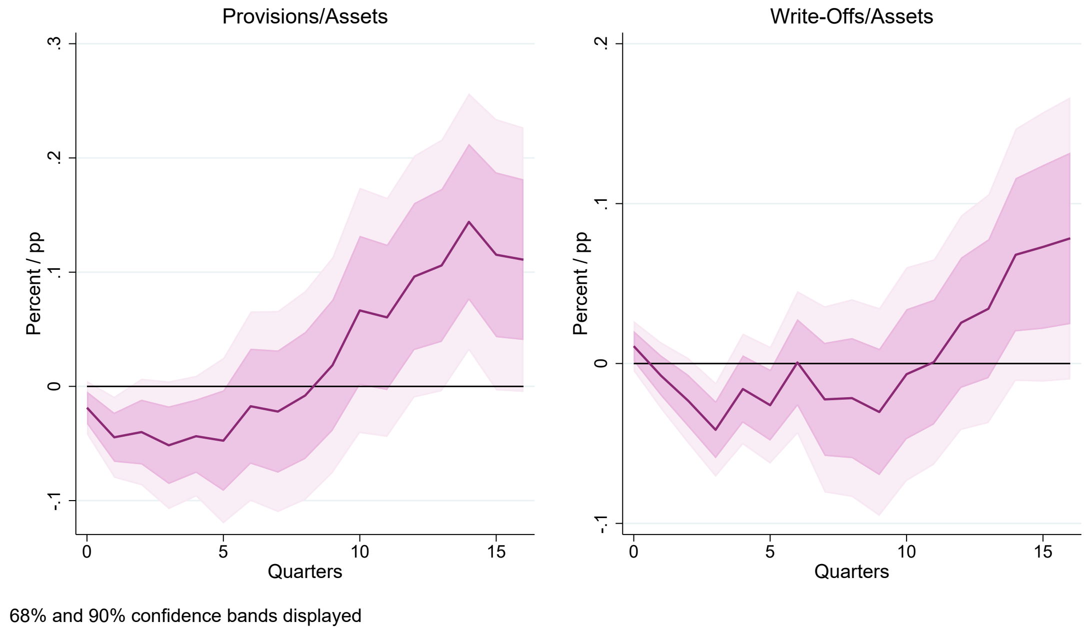

Figure 11: Impulse Response of Simple Leverage to Contractionary Monetary Shock

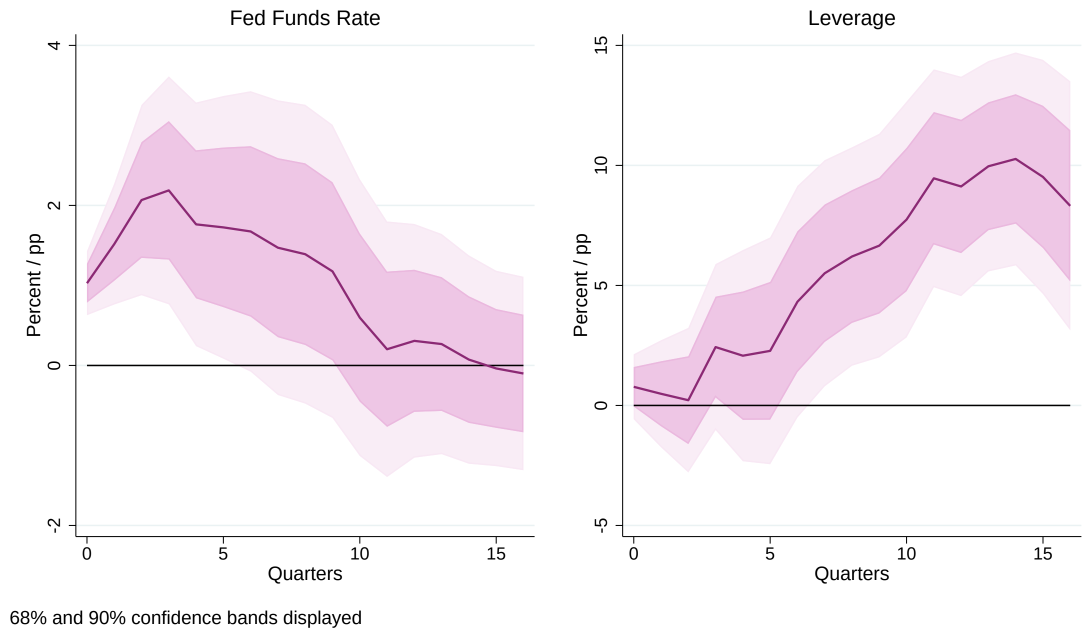

Figure 12: Impulse Response of Debt Liabilities to Contractionary Monetary Shock

Figure 13: Different Time Periods

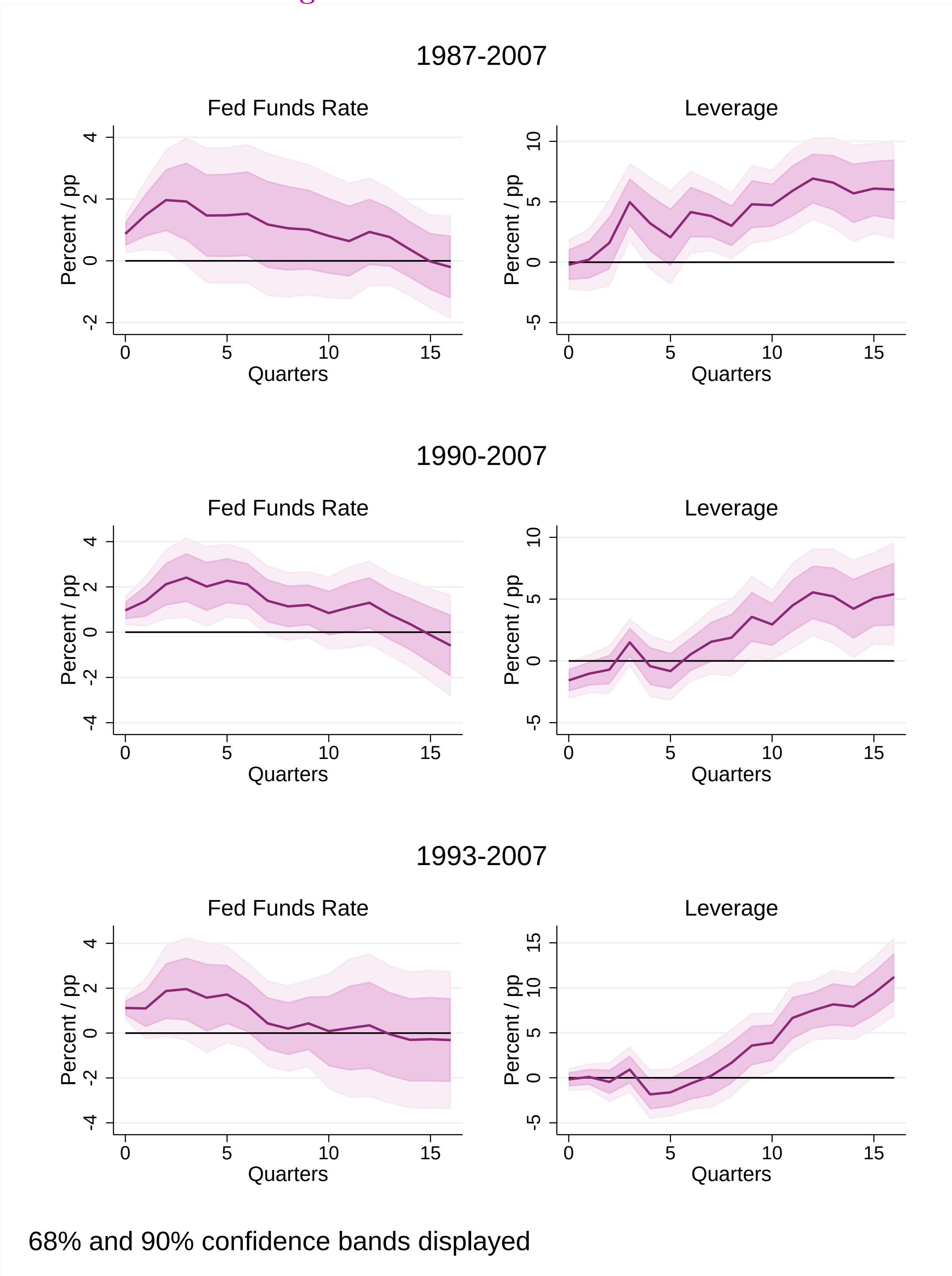

Figure 14: Different Number of Lags

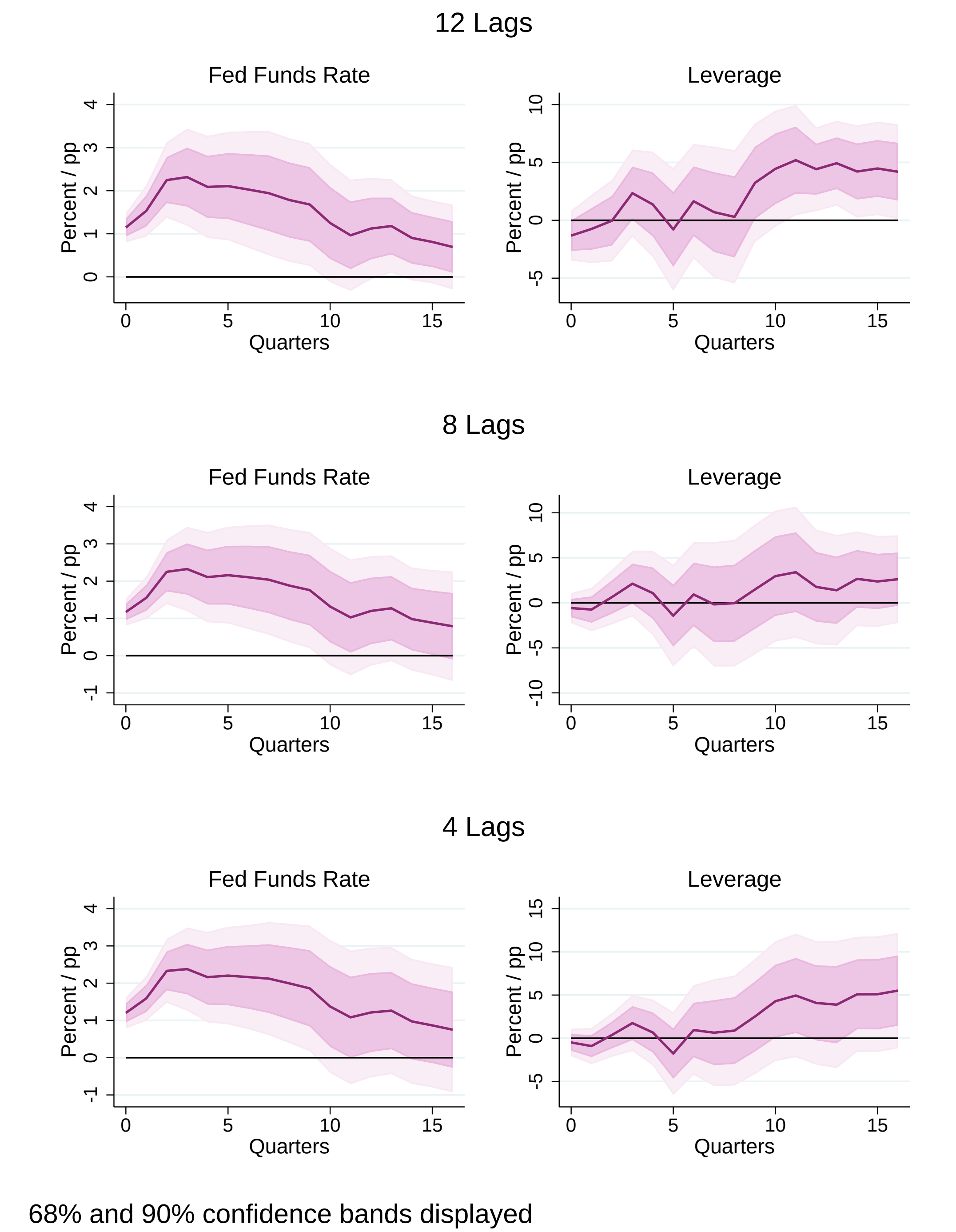

Figure 15: Different Monetary Policy Shock Series