Heterogeneous Bank Lending Responses to Monetary Policy: New Evidence from a Real-time Identification

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Abstract: Heterogeneity in bank responses to monetary policy is consistent with an aggregate lending channel. However, estimates of bank responses are typically obtained using realized federal funds rate changes, which are endogenous to expected, macroeconomic fundamentals. As such, estimated heterogeneity can arise from expected fundamentals. Using an exogenous policy measure identified from narratives on FOMC intentions and real-time forecasts, we find greater heterogeneity in responses. There is a much stronger monetary policy transmission to smaller banks. The shielding of lending amongst holding companies is larger using the exogenous measure. Unlike previous research, we find that holdings of securities amplify exogenous policy transmission, while equity capital negates it. The results highlight the importance of controlling for policy endogeneity in future studies of bank lending behavior.

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1 Introduction

The role of the banking sector in the transmission of monetary policy has been studied in great detail in both the theoretical and applied literature. According to the theory of the lending channel, contractionary open market operations reduce banking sector reserves and deposits, forcing a reduction in lending volumes because banks are constrained in their ability to substitute lost deposits with alternative sources of finance. In practice, this mechanism may be reinforced by a broader credit channel, whereby tight policy reduces borrower collateral and induces banks to raise the premium on loans relative to the competitive cost of funds (the external finance premium). Propagation of monetary policy via these loan supply channels is different from textbook treatments of monetary transmission which emphasize the sensitivity of aggregate expenditure to interest rate movements.[1]

Following the work of Kashyap and Stein (2000), a number of empirical studies have explored the heterogeneity in bank lending responses to monetary policy. To the extent that financing constraints vary with banks' access to liquidity and collateral, the existence of a lending channel implies that loan responses to policy are a function of observable bank characteristics related to such access. Kashyap and Stein show that banks with relatively large and liquid asset bases are better able to shield their lending growth during periods of tight monetary policy. The same phenomenon has been documented for banks with relatively high equity capital-to-assets ratios (Kishan and Opiela, 2000), banks whose loan books are readily securitized (Loutskina, 2005), banks affiliated to a holding company (Ashcraft, 2006), and banks that can raise funds from international operations (Cetorelli and Goldberg, 2008).

A fundamental issue addressed in each of these papers is whether heterogeneity linked to a specific characteristic can be interpreted as a loan supply effect (as in the narrow and broad lending channels), rather than an amalgam of possible loan supply and loan demand effects. A large amount of evidence for homogeneous loan demand conditional upon one or more bank characteristics has been provided.[2] With homogenous loan demands, any heterogeneity in lending responses across the conditioning bank characteristics is consistent with the existence of a bank lending channel affecting loan supply.

By contrast, much less attention has been devoted to the question of what measure of

[1]See Bernanke and Gertler (1995) for a review of the different elements of the transmission mechanism.
[2]See Ashcraft (2006) for a discussion of this evidence.

monetary policy is appropriate for the assessment of bank lending behavior. Most papers use the change in the effective (realized) federal funds rate to capture monetary policy, reflecting the fact that the Federal Open Market Committee (FOMC) has targeted the federal funds rate for much of the last 30 years.[3] While federal funds rate changes initiated by the FOMC are surely exogenous to the circumstances facing any single bank, the factors to which policymakers respond (e.g., expected output growth and inflation) are potential determinants of individual bank lending, through *both* loan demand and loan supply effects. This raises the possibility that lending responses to federal funds rate changes confound the effects of monetary policy and other lending market drivers. Furthermore, if the strength of any effects from confounding variables is related to bank characteristics, the heterogeneity in lending responses to monetary policy will not be correctly estimated.

Motivated by these possibilities, we evaluate the heterogeneity in bank lending responses to federal funds rate changes that are plausibly exogenous to expected output growth and inflation. The identification of this policy component builds upon work by Romer and Romer (2004), who combined narrative evidence on Federal Reserve intentions and the Greenbook forecasts produced by Federal Reserve staff in order to control for endogenous policy changes. Bank lending responses to the identified policy measure are compared with lending responses estimated from realized federal funds rate changes that have been the focus of most previous research.

Our results indicate five important differences in bank lending responses to exogenous and endogenous components of monetary policy. First, one year after an exogenous monetary contraction, the reduction in lending growth at the average bank which is *not* part of a holding company is up to *twice* that from a rise in the realized federal funds rate. Second, the amount by which a bank can shield its lending growth from a monetary policy contraction, either through accessing a large asset base or drawing on funds from affiliates in a holding company, is *1.5 times larger* when estimated purely in response to identified, exogenous monetary policy. Third, the share of bank assets held as securities mitigates the lending response to a realized federal funds rate increase, but *amplifies* the lending contraction to an exogenous federal funds rate increase. In contrast, the ratio of equity capital-to-assets *shields* lending growth from an exogenous mon-

[3] Alternative monetary policy measures which have been used in the literature on bank lending include those due to Boschen and Mills (1991, 1995), Strongin (1995), and Bernanke and Mihov (1998). See section 2 for further discussion.

etary tightening, but is only weakly correlated with lending responses to realized federal funds rate increases.

We offer explanations for these findings in terms of the endogeneity of monetary policy. They provide a new perspective on the measurement of balance sheet liquidity and the consequences of shifts in balance sheet composition for the strength of monetary policy propagation. Our fourth finding qualifies the results on balance sheet composition. Following the introduction of the source of strength doctrine for bank holding companies in 1987, the effects of asset composition on lending responses to monetary policy occur only among banks that are *not* part of a holding company. Affiliated banks appear to be able to smooth lending in the face of monetary policy shocks using the internal capital markets of the holding company, such that balance sheet composition does not affect lending responses to monetary policy. Fifth and lastly, banks that do a larger proportion of business in the residential sector contract lending by a *smaller* amount following an exogenous policy tightening. This is consistent with better access to the finance needed to sustain such loans, as might arise with securitization (Loutskina, 2005). In contrast, following changes in the realized federal funds rate, this effect is much smaller and only marginally significant.

To place our paper in context, it is important to consider how potential biases from confounding monetary policy with other loan demand and loan supply determinants have been handled in previous research. Each of the papers discussed earlier directly controls for output growth, inflation, or both, in their empirical models of lending growth. To the extent that such variables effectively capture the sources of endogenous monetary policy, their inclusion in a lending growth regression accounts for extraneous loan demand and loan supply shifters, such that the structural effects of policy can be identified. Under the assumption that loan demand does not vary with the characteristic, heterogeneity in these structural effects is then measured via interactions with bank characteristics.

The starting point for our paper is that current output growth and inflation are not the only sources of endogenous policy – a forward-looking policymaker who desires to minimize cyclical fluctuations will also respond to forecasts for its objective variables. If these forecasts correlate with private sector expectations for growth and inflation, loan demand and loan supply may fluctuate in a manner that is systematically related to observable bank characteristics. Our results highlight instances in which this appears to be the case. In light of these findings, we argue that future studies of bank lending behavior should take into account the forward-looking component of endogenous monetary policy.

The remainder of the paper is structured as follows. In section 2, we explain how endogenous monetary policy movements may induce biased estimates of lending responses to monetary policy. Motivated by these possibilities, in section 3 we outline an identification strategy for exogenous monetary policy. We then discuss the bank-level econometric framework and data that we use to compare lending responses to identified policy changes with lending responses to realized changes in the federal funds rate. In section 4, we present our core results. We continue in section 5 with a consideration of their robustness to changes in estimation and data definitions. Finally, we conclude in section 6 with a summary and a discussion of the importance of monetary policy identification for future research concerning bank lending behavior.

2 Bank lending and monetary policy

How might endogenous monetary policy contaminate estimates of the lending channel? Here, we outline the potential biases affecting the estimates in the literature that rely upon the effective federal funds rate to measure monetary policy. In each of the cases discussed, the underlying idea is that expectations over output growth and inflation determine *both* policy and bank lending choices. These effects are not captured in the standard lending growth regression. Consequently, there is an omitted variable problem that affects the estimated response of bank lending to federal funds rate changes. Moreover, our discussion covers lending responses that are conditional upon bank characteristics, which have received considerable attention in recent research. We argue that estimates of these cross effects are also affected by the endogeneity of monetary policy. In light of the possible effects described, we compare bank lending responses to exogenous monetary policy changes (described in section 3.1) and to the realized monetary policy changes that have been used in previous research.

Studies of bank lending responses to monetary policy typically estimate regressions of the form:

$$\underset{(1\times 1)}{\Delta L_{i,t}} = \alpha + \beta \underset{(1\times 1)}{M_t} + \underset{(1\times J)}{X'_{i,t}} \underset{(J\times 1)}{\gamma} + \underset{(1\times J)}{X'_{i,t}M_t} \underset{(J\times 1)}{\delta} + \underset{(1\times K)}{Z'_{i,t}} \underset{(K\times 1)}{\phi} + \varepsilon_{i,t} \tag{1}$$

where i indexes banks, t indexes time, ΔL denotes the change (t minus $t-1$) in the natural logarithm of total loans measured at current prices, M is a monetary policy measure, X is a vector of J bank-specific characteristics, Z is a vector of K control variables, and ε is a mean-zero error term. All other Greek letters denote parameters. In practice, bank lending regressions are much richer than equation 1, typically including autoregressive terms and dynamics in M and X. In section 3, we describe a more complex version of model 1 that incorporates these features. It also will provide the basis for our empirical work. However, the present specification is sufficient to illustrate the arguments that we develop in this section.[4]

As noted in the introduction, the vector X comprises bank characteristics that capture access to finance. These might include total bank assets, bank holding company affiliation, an indicator for whether a bank operates internationally, measures of balance sheet composition such as equity capital-to-assets and securities-to-assets ratios, and information concerning the characteristics of the bank loan book (e.g., the ease with which it can be securitized). In the aftermath of contractionary monetary policy, banks that can access funds from these sources may shield lending growth from the effects of an erosion of reserves and deposits.

What interpretation can be given to the cross effects (interactions) between monetary policy and bank characteristics? It is commonly argued that while factors such as bank size and the bank equity ratio may capture access to funds that matters for loan supply, they also reflect features of loan demand. For example, large banks may cherry pick customers whose loan demand is relatively stable, while poorly capitalized banks may be overlooked by safe borrowers and forced to do business with risky customers whose loan demand is relatively volatile. On the other hand, Ashcraft (2006) presents evidence that bank holding company affiliation is less closely linked to the customer mix, and is to be preferred as an indicator for loan supply conditions. In this paper, we do not add to this debate. Instead, we consider the range of characteristics that have been studied in the literature. However, throughout our discussion we are mindful of the interpretations that can be given to cross effects between monetary policy and individual bank characteristics.

The monetary policy measure M most often employed is the change in the period average

[4]Alternatives to the single-step regression model have also been considered in the literature. Kashyap and Stein (2000) adopt a two-stage procedure, where the cross-sectional sensitivity of lending growth to balance sheet liquidity is estimated in a first stage, and a time series regression relating these cross-sectionally estimated liquidity constraints to monetary policy is estimated in a second stage. We do not adopt the two-stage approach in this paper.

effective federal funds rate, which has been the Federal Reserve's operating target since at least 1994, and arguably for many episodes since the post-war period.[5] Increases in the federal funds rate target induce *left*ward shifts of banks' loan supply schedules via the narrow and broad lending channels described in the introduction. These raise lending rates and reducing lending volumes. When the federal funds rate target is increased in response to forecasts of higher future economic growth and/or inflation, estimation of this relationship is no longer straightforward. In such circumstances, any loan supply contraction due to tight monetary policy may coincide with a *right*ward shift of loan demand, as consumers borrow against expected future income and firms invest in response to an improving outlook for profits. The loan demand shift will attenuate the reduction in lending from a monetary tightening and the β estimated from equation 1 will not capture the full effect of monetary policy. A similar result may arise via the effects of expected inflation. In particular, reductions in bank lending from a rise in the federal funds rate may be muted because the demand for loans in nominal units rises with expected inflation. As in the example based on expected economic growth, equilibrium lending is subject to countervailing effects from loan demand and loan supply, such that the β estimated from equation 1 is attenuated.

The drivers of endogenous monetary policy may also influence equilibrium lending via bank loan supply curves. The availability of non-deposit finance to banks is likely to vary positively with expected economic growth. At the start of cyclical upturns, institutional investors (e.g., pension funds, sovereign wealth funds) may invest more heavily in equities and loan-backed securities, at the expense of fixed income assets, given a greater appetite for higher yields and risk. To the extent that banks use equity issues and the securitization of loans to generate funding for new lending, loan supply will rise at each level of market interest rates.

Similarly, in models featuring information asymmetries and monitoring costs, loan supply incorporates an external finance premium that varies positively with lender risk aversion and

[5]See Meulendyke (1998) for historical evidence on the Federal Reserve's policy tool choices. Alternative policy measures due to Boschen and Mills (1991, 1995) and Bernanke and Mihov (1998) have also been employed in the literature (Kashyap and Stein, 2000). These measures of policy explicitly address possible changes to the instrument of policy through time, but remain measures of the endogenous stance of policy. As such, we believe that the arguments developed in this section are applicable to them. Loutskina (2005) considers the Strongin (1995) identification of exogenous movements in non-borrowed reserves. While this approach controls for reserve demand shocks, it does not directly control for endogenous policy moves by a forward-looking central bank. Jonas and King (2008) briefly consider the original Romer and Romer (2004) policy measure, which does control for policy endogeneity. However, this is used only as a robustness test in a study that focuses on the impact of bank efficiency on lending responses to general federal funds rate movements. The consequences of policy endogeneity for lending responses are not considered by Jonas and King.

negatively with borrower net worth (via a collateral effect) (Bernanke and Gertler, 1989). Expansion phases of the business cycle are typically associated with increases in lenders' risk appetite and agents' net worth, such that the external finance premium falls and loan supply expands. We do not emphasize any one of these channels ahead of the others. Instead, we highlight that when loan supply is affected by any one of them, the response of lending growth to the federal funds rate will be attenuated – the leftward shift of loan supply from tight policy is offset by a rightward shift of loan supply via one of the channels described. Furthermore, this will be the case *even* when controlling for current economic growth and inflation. The effect derives from the fact that *expected* economic conditions may influence monetary policy and loan supply simultaneously.

2.1 Policy endogeneity and bank characteristics

An important question is whether or not pro-cyclical loan demand and loan supply affect the cross effects in equation 1 that measure heterogeneity in bank lending responses. As discussed in the introduction, these are the terms that proxy the bank-level financial constraints that underpin the aggregate lending channel of monetary policy. Even if loan demands and supplies are affected identically across banks by expected macroeconomic conditions, the estimates of any heterogeneity in bank responses to the exogenous component of monetary policy will be attenuated. In this case, FF would implicitly introduce measurement error for policy through its inclusion of endogenous policy changes.

Alternatively, suppose that the attenuation of lending responses to monetary policy varies systematically with bank characteristics. Then, estimates of equation 1 which use the realized federal funds rate may either obscure or induce systematic heterogeneity in bank responses to monetary policy. In this sub-section, we describe three examples of potential biases: (i) expected macroeconomic conditions induce common loan demand shifts and bank responses depend upon their characteristics; (ii) expected macroeconomic conditions induce loan demand shifts that vary with bank characteristics; (iii) expected macroeconomic conditions induce loan supply shifts that depend on bank characteristics.

Asymmetric responses to common loan demand shifts may arise because bank characteristics capture access to liquidity. Banks that are able to access liquidity may be able to grow their loan portfolio more rapidly following a surge in loan demand. Given that the rise in loan demand facing each bank occurs against a backdrop of tighter central bank policy (which is an endogenous response to the underlying drivers of loan demand), the attenuation of bank lending will be more pronounced amongst the group of banks exhibiting the access-to-liquidity characteristic. When the realized federal funds rate is used to measure policy, estimates of the elements of δ from equation 1 will be positively biased and the evidence for heterogeneity in lending responses to monetary policy will be *over*stated.

Turning to the second of the three possibilities listed above, Kashyap and Stein (2000) advocate a rational buffer stocking theory to explain a possible correlation between loan demand curve shifts and bank characteristics. Under the assumption that some banks concentrate their lending in regions or industries that are especially sensitive to aggregate demand conditions, it is rational for such banks to select characteristics that help accommodate volatile loan demand (e.g., bank holding company affiliation or high balance sheet liquidity). When the federal funds rate rises during a cyclical expansion, shifts in individual loan demand curves will be largest amongst banks exhibiting the characteristic in question. The attenuation of lending growth reversals following rises in the federal funds rate would then be largest amongst that category of banks. As in the first case discussed, this effect would manifest as positive bias to the estimate of δ. Evidence that banks with access to liquidity can shield lending growth from Federal Reserve policy would be *over*stated.[6]

The final case described above is that of heterogeneous loan supply responses to expected macroeconomic conditions. Banks that face financing constraints, either due to a lack of affiliates, assets, or liquidity, may draw more heavily on the funds available during cyclical upturns, because of the fact that their lending was previously constrained. If this is the case, the rightward shifts of their loan supply curves (which counteract the leftward shifts from monetary tightening) will be larger, such that the net reduction in lending during periods of partially endogenous monetary tightening will be attenuated. This example is significant. It suggests that the evidence for financing constraints amongst banks will be *under*stated when a measure of the endogenous stance of monetary policy such as the realized federal funds rate is used.

We close this section by noting that these thought experiments raise the possibility that even

[6]There is a caveat. Banks trading with cyclically sensitive customers also likely face relatively interest rate elastic loan demand curves, such that cuts in lending from a rise in the federal funds rate will be larger. This potentially offsets the lending increase arising from a relatively large shift of the loan demand curve.

a purely exogenous monetary policy measure will elicit estimates of δ that measure something other than banks' ability to shield lending growth by virtue of their characteristics. For example, banks that can access liquidity may face a different loan demand elasticity and therefore adjust their lending differently for that reason. Some characteristics will be more prone to such effects than others. Ashcraft (2006) contends that the properties of loan demand are similar across banks, conditional upon bank holding company status (affiliation/non-affiliation). As such, a comparison of lending responses by bank holding company status is more likely to reflect genuine differences in banks' access to alternative finance. We return to this issue when discussing our empirical results in section 4. The point that we emphasize at this stage is that such effects impact *all* measures of monetary policy, both endogenous and exogenous. The main advantage of considering exogenous policy measures is that their effects on bank lending are less likely to be affected by the sources of bias discussed in this section.

3 Econometric methodology

In this section, we outline the methods that we use in comparing bank lending responses to exogenous monetary policy changes with realized federal funds rate changes. We first describe the identification procedure used to isolate exogenous variation in the monetary policy rate. Then, we outline the regression models that underlie our core results. Finally, we describe the data we use in the estimation.

3.1 Monetary policy identification

To identify exogenous variation in the U.S. monetary policy, we follow the two-step procedure outlined by Romer and Romer (2004), who consider U.S. monetary policy over the period 1969-1996. In the first step, narrative evidence is used to determine the size of the federal funds rate change targeted by the Federal Open Market Committee (FOMC) at their scheduled meetings. The advantage of this measure of monetary policy intentions is that during episodes of reserve targeting (e.g., under Volcker's chairmanship of the FOMC), it does *not* respond to supply and demand shocks in the reserve market that are unrelated to monetary policy. In contrast, the effective federal funds rate (the market clearing rate in the reserve market) will respond to such factors.[7]

We extend the original Romer and Romer (2004) target series by appending the FOMC's announced federal funds target rate changes for 1997-2001. Such announcements began in February 1994, overlapping with the original Romer and Romer series for 2 years. Although the announced target series does not capture all of the narrative evidence incorporated in the Romer and Romer (2004) series, we argue that the pooling of the two is defensible, since the transparency of policy intentions and the public announcement of policy changes are strongly related. During the overlapping period of 1994-1996, the two series have a correlation that is essentially 1.[8] The extension of the target rate series in this way ensures that we are able to recover exogenous variation in U.S. monetary policy for a longer sample period than that covered by Romer and Romer (2004).

In the second step, the targeted federal funds rate change is regressed upon the Federal Reserve's Greenbook (in-house) forecasts for real output growth, inflation, and unemployment over horizons of up to two quarters. These represent the central objective variables of the Federal Reserve.[9] Additionally, we supplement the Greenbook information with measures of capacity utilization and capacity utilization growth in the month of the FOMC meeting. The capacity utilization index is constructed by the Federal Reserve. However, it is not available to policymakers in real-time because the observations for a particular month are inferred by scaling production indicators with capacity measures interpolated from end-of-year observations – actual capacity is only benchmarked annually. The empirical relevance of capacity utilization is emphasized by Giordani (2004), who shows that controlling for such a proxy for production relative to potential is crucial for accurate policy identification. In the present application, we treat terms in capacity utilization as proxies for latent policymaker perceptions concerning the cyclical position of the economy, which may contribute to policy decisions even after controlling

[7] In previous research, the consequences of changes to the operating procedures have been investigated through comparing results from the effective federal funds rate with those from the Bernanke and Mihov measure of monetary policy, which accounts for changes to the operating procedure. See Kashyap and Stein (2000) for an application. We do not pursue that option in this paper.

[8] There is one instance in which the series differ. For the meeting on September 28, 1994, Romer and Romer (2004) argue that the language associated with the FOMC transcripts amounted to the intention to tighten by 12.5 basis points, even though there was no change in the announced, target federal funds rate.

[9] See Board of Governors of the Federal Reserve (2005) or the International Banking Act of 1978 (the Humphrey-Hawkins Act).

for the Greenbook forecasts. Formally, we estimate the following regression:

$$\begin{aligned}\Delta ff_m &= \alpha + \beta ff_{m-1} + \sum_{j=-1}^{2} \gamma_j \widehat{\Delta y}_{m,j} + \sum_{j=-1}^{2} \eta_j \left(\widehat{\Delta y}_{m,j} - \widehat{\Delta y}_{m-1,j} \right) \\ &+ \sum_{j=-1}^{2} \theta_j \hat{\pi}_{m,j} + \sum_{j=-1}^{2} \lambda_j \left(\hat{\pi}_{m,j} - \hat{\pi}_{m-1,j} \right) \\ &+ \sum_{j=-1}^{2} \mu_j \hat{n}_{m,j} + \sum_{j=-1}^{2} \rho_j \left(\hat{n}_{m,j} - \hat{n}_{m-1,j} \right) + \tau CU_m + \phi CUG_m + \varepsilon_m,\end{aligned} \tag{2}$$

where m indexes FOMC meetings, j indexes the forecast quarter relative to the current meeting's quarter, ff is the target federal funds rate level, Δy is real output growth, π is inflation, n is the unemployment rate, CU is the capacity utilization index, CUG is the current monthly growth rate of capacity utilization (both capacity terms are measured in percentage points), and ε is a mean-zero error term. A hat denotes the real-time forecast for a variable. All other lowercase Greek letters denote population parameters. Notice that the specification employs a larger set of unemployment forecasts than Romer and Romer (2004).

The results obtained from estimating equation 2 for a sample of 298 FOMC meetings from the period 1969-2001 are reported in table 1. The sums of the coefficients on forecast levels are generally of the same signs as those reported by Romer and Romer (2004), indicating tighter policy in response to stronger economic activity and higher prices. An exception occurs in the case of the sum of the coefficients on the growth forecasts, which is negative but insignificant. One explanation is that the capacity utilization terms capture information contained in the growth forecasts (the coefficient on capacity utilization growth is positive and significant). The inclusion of the capacity utilization and additional unemployment terms is also reflected in the regression R^2, which is higher than that for the original Romer and Romer (2004) specification (36% as compared to 28%).[10]

In order for the regression residuals from equation 2 to capture exogenous monetary policy that is useful in the estimation of bank lending responses, we require that: (i) the Greenbook forecasts and capacity utilization are not a function of the change in the federal funds rate target; and, (ii) the Greenbook forecasts and capacity utilization account for any changes to the target that are endogenous to factors that may influence bank lending via expected economic

[10] This may also reflect a reduction in the relative variability of the target federal funds rate over the years 1997-2001.

conditions. The first assumption rules out reverse causation in equation 2. As remarked upon by Romer and Romer (2004), the Greenbook forecasts are generally formulated under the assumption that there is no change in policy stance at least until the FOMC meeting after next, ruling out this possibility. One caveat is that Greenbook forecasts can draw upon forward-looking variables (e.g., asset prices, industry surveys) that embody market expectations over the policy change at the current meeting. In that case, our identification requires that output, inflation, unemployment and capacity utilization respond to policy with a sufficiently long lag such that the forecasts in equation 2 are not subject to reverse causation.

The second assumption is key in eliminating policy movements that may lead to biased estimates of lending responses to monetary policy. The Greenbook forecasts are a natural instrument in achieving this objective because they represent the real-time information available to policy-makers and are known to perform well relative to alternative forecasts (see Romer and Romer, 2000, 2008 and Bernanke and Boivin, 2003 for evidence).[11] Instances in which the controls in equation 2 may not eliminate policy movements that are endogenous to lending determinants occur when the Federal Reserve responds to banking sector conditions directly. If concerns over bank liquidity prompt the Federal Reserve to keep interest rates on hold even when Greenbook forecasts point to higher interest rates, a negative monetary policy change would be recorded. However, this may fail to stimulate lending growth if liquidity concerns prevent banks from doing new business. In terms of the present application, the banking crisis that followed the collapse of the sub-prime housing market in 2007 is excluded from the sample. However, two other relevant episodes are included in the sample: (i) the years surrounding the Basel I Accord (agreed in 1988 and implemented in 1992), which is often argued to have prompted bank balance sheet adjustment and a looser monetary policy than would otherwise have been the case (Ashcraft, 2006); and, (ii) the Federal Reserve Bank of New York's rescue of U.S. hedge fund Long-Term Capital Management (LTCM) in 1998, which may have induced similar effects. In section 5, we provide evidence that our core results are not affected by these episodes.

[11] It is of course possible that individual firms, consumers and banks have information concerning their future prospects (as opposed to general economic prospects) that is not reflected in the Greenbook. However, this will not lead to estimation bias provided that FOMC decisions regarding the target federal funds rate are not correlated with such information. In essence, it must be the case that any determinant of monetary policy decisions (e.g., the views of an influential FOMC member) does not contain information for loan supply and loan demand beyond that in the Greenbook.

For any identification scheme, a natural question is: what are the sources of the policy shocks estimated from equation 2? An important factor is likely to be that interest rate decisions depend on factors idiosyncratic to FOMC members. For example, even absent a future cyclical expansion, interest rates may be increased if FOMC members are concerned with their public reputation (Bluedorn and Bowdler, 2008 discuss a relevant example), possess a private forecast that points to an expansion that does not transpire (Romer and Romer, 2008), or hold a view of the economy that leads them to favor larger interest rate rises than are warranted given the available forecasts (Romer and Romer, 2004). Alternatively, FOMC membership may change such that policymaker preferences favor tighter or looser policy irrespective of the cyclical position. In other situations, policymakers may feel obliged to validate market beliefs over policy, even when such beliefs are incorrect (Christiano, Eichenbaum, and Evans, 1999). It is these federal funds rate adjustments, driven by errors and preference shifts, that we use to obtain estimates of bank lending responses to monetary policy.

The data on bank lending that we use in our empirical work are reported on a quarterly basis. Thus, monetary policy changes defined at the frequency of FOMC meetings, which currently take place eight times per annum, must be aggregated to the quarterly frequency. The appropriate method of aggregation depends critically on whether the data to be studied are measured on a quarter-average or quarter-end basis (see Bluedorn and Bowdler, 2008 for relevant discussion). In the present application, bank-level data are drawn from end-of-quarter reports filed with the Federal Deposit Insurance Corporation (FDIC). Balance sheet data are reported for the final day of a quarter and banks have up to 30 days in the following quarter to confirm the figures reported. To parallel this treatment, we construct a quarterly series for exogenous monetary policy by cumulating the post-meeting identified monetary policy changes at a daily frequency within a particular quarter, to give a variable that we denote UM.[12] This method is equivalent to defining a daily interest rate level from the cumulated value of all past identified policy changes and taking the change in the level from the final day of the previous quarter to the final day of the current quarter. Accordingly, we use precisely that method to

[12]To see the importance of consistent end-of-period measurement of balance sheet variables and monetary policy measures, suppose that lending responds in full to monetary policy within a month. It is then the case that a monetary policy shock in the third month in a quarter changes lending by the same amount as a shock observed in the first, even though a period average interest rate change would be smaller in the first scenario than in the second. The estimated effect of monetary policy on lending growth would then be distorted.

obtain analogous quarterly changes in the effective federal funds rate, denoted FF.[13] In figure 1, we present time series plots for UM and FF. During the sample 1976q2 to 1999q2 the variance of UM is 76 basis points and that of FF is 157 basis points, suggesting that roughly half the variation in the effective federal funds rate is eliminated from UM as part of the identification procedure. The correlation of the two series is 0.60.

3.2 Regression specification

To evaluate bank lending responses to monetary policy, we estimate regression models of the form:

$$\begin{aligned} \underset{(1\times1)}{\Delta L_{i,t}} &= \alpha + \sum_{j=1}^{4} \lambda_j \Delta L_{i,t-j} + \sum_{j=0}^{4} \underset{(1\times3)}{M'_{t-j}} \underset{(3\times1)}{\beta_j} + \underset{(1\times J)}{X'_{i,t-1}} \underset{(J\times1)}{\gamma} + \sum_{j=0}^{4} \underset{(1\times J)}{X'_{i,t-1}} M_{1,t-j} \underset{(J\times1)}{\delta_{1,j}} \\ &+ \sum_{j=0}^{4} \underset{(1\times J)}{X'_{i,t-1}} M_{2,t-j} \underset{(J\times1)}{\delta_{2,j}} + \sum_{j=0}^{4} \underset{(1\times J)}{X'_{i,t-1}} M_{3,t-j} \underset{(J\times1)}{\delta_{3,j}} + \mu t + \sum_{k=1}^{3} S_k \phi_k + \varepsilon_{i,t} \end{aligned} \tag{3}$$

where i indexes banks, t indexes time in quarters, ΔL denotes the change in the natural logarithm of total loans measured at current prices, M is a vector of three macroeconomic variables (described below), X is a vector of J bank characteristics (described below), S_k is a set of seasonal dummy variables equal to 1 in quarter k and zero otherwise, and ε is a mean-zero error term.

The components of vector M are:

1. a monetary policy measure, either UM or FF, as described in section 3.1;
2. the change in the natural log of GDP at current prices;
3. the change in the natural log of the consumer price index (CPI).

The vector of J bank characteristics comprises:

1. the natural log of bank assets in millions of dollars, at current prices;
2. an indicator variable set to unity post−1986 if a bank is part of a bank holding company and zero otherwise (following Ashcraft, 2006 this characteristic is dated t rather than

[13] The daily effective federal funds rate data come from the FRED database maintained by the Federal Reserve Bank of St.Louis.

$t-1)$[14]

3. the ratio of bank securities to assets;

4. the ratio of total equity capital to assets;

5. the ratio of internal cash generation to assets.

For the interaction terms, the components of M are broken out (denoted $M_{q,t}$ for $q \in \{1,2,3\}$). We give the exact variable definitions and data sources in section 3.3.

The regression specification in equation 3 is closely related to those employed by Ashcraft (2006) and Loutskina (2005). Once-lagged bank characteristics are included as controls, to allow for differences in lending growth conditional upon bank size, holding company affiliation, and balance sheet composition. The growth and inflation controls in the vector M account for variations in nominal lending growth arising from contemporaneous changes in prices and economic activity. Interactions between the macroeconomic variables and bank characteristics capture heterogeneity in bank lending responses to monetary policy, income growth, and inflation.

There are three points that we highlight in relation to equation 3. First, the interactions between macroeconomic variables and bank characteristics feature measures of characteristics dated $t-1$, except in the case of the bank holding company dummy which is dated t. As such, lending decisions in period t are conditional on characteristics that are pre-determined. They are thus less likely to be influenced by current lending behavior (the bank holding company indicator is not pre-determined, but it is not derived from the bank balance sheet). This structure mirrors that in Ashcraft (2006) and Loutskina (2005). A natural alternative would be to date interacted characteristics $t-j-1$ such that they are also pre-determined with respect to the monetary policy measure. We consider this case in our robustness tests in section 5. As we discuss there, the results change very little due to the fact that the variation in characteristics across quarters close in time is small relative to the cross-sectional variation in characteristics.[15]

[14]The indicator recognizes holding company status only in the post-1986 period, to reflect the inception of the Federal Reserve's source of strength doctrine, which underpins the interpretation of holding companies as credit networks through requiring that dominant holding company banks support their affiliates during periods of financial stress. Ashcraft (2008) shows that in practice, the functioning of internal capital markets improved significantly in 1989. However, we focus on the post$-$1986 period as in Ashcraft (2006).

[15]While we consider characteristics that are pre-determined for the current lending response to monetary policy, we make no claim to have identified exogenous variation in characteristics. In line with most of the literature, we do not model bank characteristics. The determinants of characteristics may include the properties of previous monetary policy regimes, raising the possibility that the effects of policy on bank lending are more complex

Second, each of the bank characteristics (excepting the binary variable for bank holding company status) are demeaned. Thus, the first component of the vector $\sum_{j=0}^{4} \beta_j$ measures the percentage change in lending a year after a 100 basis point (b.p.) monetary policy contraction for an unaffiliated bank at the sample mean of each characteristic (this overlooks contributions from autoregressive terms, a point to which we return in section 4). The q^{th} component ($q \leq J$) of the vector $\sum_{j=0}^{4} \delta_{1,j}$ measures the increment to the marginal lending response to a monetary contraction when the q^{th} characteristic is 1 unit above the sample mean (or a bank is affiliated with a holding company in the case of that characteristic).

Third, in addition to the levels of nominal income growth and inflation, the regression includes a full set of interactions between those variables and bank characteristics. This ensures that heterogeneity in bank lending responses to monetary policy is estimated after controlling for: (i) purely nominal effects on lending growth from inflation; and, (ii) heterogeneity in the response of real lending growth to macroeconomic factors like output growth and inflation.[16]

The final elements of the regression specification are a set of seasonal dummies and a time trend (although macro variables are seasonally adjusted, bank-level variables are not). The trend is included to deal with the fact that total assets (a bank characteristic) drifts through time whereas other variables are growth rates or ratios.[17]. We do not include separate trends for any drift in the interactions between macroeconomic variables and total assets. In section 5, we implement a different approach to dealing with the drift in total assets to handle any such effects.

The maximum lag order in the benchmark regression specification is 4, which is typical of micro bank lending regressions using quarterly data (see *inter alia* Kashyap and Stein, 2000, Ashcraft, 2006 and Loutskina, 2005). Lags in the dependent variable control for serial correlation in the data that is not eliminated by the control variables. Similar to Ashcraft (2006), we calculate all regression standard errors through clustering at the bank-level to deal with any residual heteroscedasticity and autocorrelation of unknown form.[18] One source of uncertainty

than our estimates indicate. It could even be the case that past values of a bank characteristic are endogenous to current monetary policy (e.g., via an expectations effect). Any resulting estimation biases are likely to be less important in the case of UM than in the case of FF, because the former is less easily predicted due to its orthogonality to economic forecasts.

[16]Inflation may affect real lending volumes if loan contracts are not fully inflation-indexed.

[17]Given the short time series for some panels, we do not undertake a full unit-root analysis.

[18]Wooldridge (2003) notes the importance of clustering in panels that explain micro responses to macro shocks, as in the present case.

that our standard errors do not take into account is the first stage regression used to identify UM. However, Pagan (1984) demonstrated that this uncertainty only affects inference based on non-zero null hypotheses – inference based on zero null hypotheses remains valid.

3.3 Data

3.3.1 Bank-level data

Our bank-level data are from the *Reports of Condition and Income* ("Call Reports") submitted to the FDIC at the end of each quarter by all insured banks in the United States.[19] The variable definitions that we outline follow those used in Ashcraft (2006). The Call Report line numbers used to generate individual series are provided in Kashyap and Stein (2000).

The dependent variable is derived from a series for total loans minus allowances for loan losses. This definition spans five major categories of loans (residential, consumer, commercial and industrial, agricultural, and municipal). It includes loans under commitment for some period (predominantly lines of credit to firms), as well as loans on flexible terms.[20] The correction for loan losses allows for the fact that a bank may reduce its loan book by writing off bad loans, as well as through varying the supply of new credit. However, as discussed by Ashcraft (2001) and Peek and Rosengren (1998), our measure of loans does not control for loans being moved off bank balance sheets via securitization. In our case, distortions to lending growth via securitization should be limited since our sample ends in 1999. This includes only a few years of the period of growth in the market for mortgage-backed securities which started in the mid-1990s. Consistent with this view, we show in section 4.2 that our results change little after controlling for the commercial and industrial loan share. Any data distortions from securitization should be minimized for this sub-sample, since relatively few loans from this category are securitized (Loutskina, 2005).

Total bank assets are reported net of loan loss reserves and form the basis for measuring balance sheet composition, across securities, equity capital and cash flow (each of these terms is measured relative to total assets). Bank securities are the sum of Total Investment Securities

[19]We are grateful to Adam Ashcraft for providing a dataset containing variables constructed from these sources using guidelines proposed by Kashyap and Stein (2000). Some series are dropped from the Call Reports during the period considered, while others are added. See Kashyap and Stein (2000) for notes on how such changes were handled.

[20]The data include international lending from 1978 onwards.

and Assets Held in Trading Accounts. Total Equity Capital is the book value of equity issued plus the cumulated value of retained earnings. Internal cash flow is the sum of net income before extraordinary items and loan loss provisions (this definition follows Ashcraft, 2006, who cites Houston, James, and Marcus, 1997).[21] The indicator for bank holding company status is taken from Ashcraft (2006), who identifies holding companies from sets of banks that have the same regulatory holder identification number.

In table 2, we report summary statistics for the bank-level variables and measures of loan composition by customer type. Summary statistics are calculated using data from three years corresponding to the beginning, middle and end of the sample (1977, 1988 and 1999), for all banks in the baseline estimation sample (see section 3.3.3 below). An inspection of these statistics supports our treatment of the series as stationary, with the exception of the total assets measure (see the discussion in section 3.2).

3.3.2 Macroeconomic data

The series for income growth is constructed from seasonally adjusted current price GDP, and that for the inflation rate is from the seasonally adjusted CPI. Both series are from the U.S. Bureau of Economic Analysis (BEA) and were extracted from the Federal Reserve Bank of St. Louis's FRED database. The output and price data are period average values. They refer to a flow of transactions within a particular quarter, whereas our bank-level data are end-of-quarter values from stock concepts on balance sheet statements. Unlike the interest rate series, for which we can examine data for the final day from a particular quarter, there are no end-of-period concepts for output and prices. This measurement mismatch could in theory limit the extent to which current output and inflation control for the endogeneity of the federal funds rate. However, in section 5, we show that our results are robust to measuring the realized federal funds rate on a period average basis that matches the output and inflation concepts.

[21]Each of the balance sheet characteristics are affected by the fact that prior to 1984, aggregates for certain asset and liability classes are not reported. They are therefore proxied through summing their relevant sub-components. For example, through 1983, Total Investment Securities is proxied by the sum of securities on the balance sheet from different issuers. See Kashyap and Stein (2000) for a full discussion.

3.3.3 Sample description

The dataset used for our baseline estimations is an unbalanced quarterly panel spanning 1976q2 to 1999q2. It features a maximum of 14,026 banks. The average number of observations per bank is 56.9 quarters. In line with other studies, this sample is obtained after excluding bank/quarter observations affected by mergers, since they may induce spurious movements in balance sheet variables (following a merger the merged banks are dropped and a new bank enters the dataset).[22] In order to deal with other exceptional movements in the data, we follow Ashcraft (2006) in fitting our benchmark regression by OLS for the largest possible sample and then eliminating outliers. These are defined as observations for which the absolute *DFITS* statistic (the scaled difference between the fitted values for the n^{th} observation when the regression is fitted with and without the n^{th} observation) exceeds the threshold $2\sqrt{\frac{K}{N}}$, where K is the total number of explanatory variables and N is the overall sample size (Welsch and Kuh, 1977). The number of observations excluded depends on whether the regression is fitted using UM or FF. Specifically, from a total sample of 1,079,960 observations we reduce the sample to 1,053,334 observations when UM is the policy measure and 1,052,453 observations when FF is the policy measure.[23] These differences are minor in the context of the sample size. We emphasize that our results across UM and FF do not depend on outlier exclusion. The comparisons presented in the next section are observed when using either the full or trimmed samples.

4 Empirical results

In table 3, we present $\sum_{i=0}^{I} \beta_i$ for $I = 0, 1, .., 4$ and the associated standard errors, for the two policy measures UM and FF. These statistics measure the percentage change in lending at various horizons following a 100 b.p. tightening at a bank that has the sample average balance sheet characteristics and is *not* affiliated with a holding company (we refer to such a bank as the representative bank). The full lending response also depends on the autoregressive parameters, but each of these is small (less than 0.1) and virtually identical across UM and FF versions of

[22]Due to consolidation of the banking sector, the number of banks falls to roughly 8,000 by the end of the sample – see table 2.

[23]The outlier exclusion procedure offers some robustness against certain changes to variable definitions that occur during the sample which are documented by Kashyap and Stein (2000).

the regression. As such, they do not affect our inferences. We follow Kishan and Opiela (2000), Loutskina (2005) and Ashcraft (2006) in reporting the direct effect of policy on lending.

At each of the horizons considered, the lending reduction estimated from an exogenous monetary policy contraction exceeds that from a policy contraction measured by the realized federal funds rate. Furthermore, the precision associated with our estimates is such that 95% confidence intervals for the two estimates are non-overlapping at all horizons beyond the current quarter. The difference between lending responses to UM and FF is most stark at the one and two quarter horizons. This comes from the fact that lending declines after an exogenous policy tightening are most rapid during this period, before decelerating at horizons closer to one year. In contrast, quarterly changes in loans following a rise in FF are smoother, reflecting a more gradual effect on bank lending behavior. The inertia in aggregate lending estimated from FF has been attributed to factors such as loans under commitment, which may thwart the withdrawal of bank credit to firms – see Bernanke and Blinder (1992), Morgan (1998) and Kishan and Opiela (2000). While such a possibility is plausible, our estimates suggest that at least part of the sluggishness in bank lending behavior is attributable to policy changes that are endogenous to other macroeconomic fundamentals. Controlling for extraneous loan demand and loan supply movements that may be linked to these fundamentals reveals a faster and quantitatively more important monetary transmission mechanism via credit markets.

The effects of bank size and holding company status

In table 4, we report the sums of cross effects between monetary policy and bank characteristics through horizon 4 (labeled interaction) when characteristics are set at 1 standard deviation of their sample distribution (except in the case of the holding company indicator which is set to unity). Sums of coefficients for other horizons are not reported given space constraints. However, they are consistent with the UM/FF comparisons developed below. To provide some context for our results, we also reproduce the horizon 4 lending response for the representative bank, as seen in table 3. We consider the marginal lending response to a 100 b.p. policy contraction for a bank that is one standard deviation above the sample mean for each of the characteristics considered. This is the sum of the response at the representative bank and the interaction effect for a particular characteristic.[24]

We first focus on the results for total assets and the bank holding company indicator. In both cases, the sums of the interaction terms are positive, indicating that the characteristics help banks shield their lending growth from policy contractions. These effects are much *larger* when monetary policy is measured using UM as opposed to FF. Controlling for the endogeneity of monetary policy implies not only more powerful lending responses at the representative bank, but also a *greater* dispersion in lending responses across the population of banks. This is consistent with our argument in section 2 that lending responses to the endogenous drivers of policy likely correlate with bank characteristics. In the present case, it appears that lending by small banks and banks not affiliated with holding companies is more responsive to factors like expected economic growth, such that lending responses to monetary policy are attenuated to a greater extent amongst banks exhibiting such characteristics. As discussed in section 2, a possible reason for this is that cyclical upturns provide access to finance that is used more intensively by banks that cannot access other sources of funds due to credit market imperfections.

The findings have important implications. Ashcraft (2006) argues that the composition of loan demand by borrower size and creditworthiness varies relatively little with holding company status, especially when compared with other characteristics such as total assets and leverage. Therefore, heterogeneity in lending responses associated with holding company status is more readily interpreted as evidence for differential loan supply responses of the sort predicted by the theory of the bank lending channel. The more powerful holding company effect estimated from the exogenous policy measure raises the possibility that the lending channel is quantitatively more important than previously believed.

As discussed by Ashcraft, an important caveat is that although unaffiliated banks may be subject to a lending channel, the borrowers turned away from such banks may be accommodated by bank holding company networks, whose funds fill the gap in the market. The aggregate lending channel of monetary policy could then be weak or non-existent. Our estimates indicate that after an exogenous policy contraction the representative unaffiliated bank reduces lending 0.94 percentage points in the first year, while the representative affiliated bank raises lending 0.95 percentage points over the same period. This evidence is consistent with a redistribution

[24] In the case of the bank holding company indicator, the marginal effect is calculated for a bank that belongs to a holding company.

of lending in the aftermath of shocks to bank funding.[25] To investigate whether the countervailing loan responses offset at the aggregate level, we re-estimated our baseline regression after excluding all terms from the vector of bank characteristics X, to obtain the marginal effect of a policy tightening for a bank at the sample average of all characteristics, including holding company status. The lending reduction from UM (standard error in parentheses) is 0.36 percentage points (0.03) and that from FF is 0.17 percentage points (0.01). Thus, despite the compensating effect from affiliated banks, it appears that an aggregate transmission mechanism exists. Furthermore, this mechanism is *twice* as strong when estimated from UM.

The much sharper heterogeneity in bank lending behavior from UM may help explain two important features of the aggregate transmission mechanism. These are: (i) the different effects of policy across regions and industries (Carlino and DeFina, 1998); and, (ii) a possible trend towards weaker propagation of monetary policy in recent decades (Boivin and Giannoni, 2002). Ashcraft (2006) presents weak evidence that state level lending responses to federal funds rate rises depend on the proportion of loans issued by affiliated banks. However, he finds that similar effects do not carry over to state income responses. The larger cross effects that we estimate from exogenous monetary policy suggest that much more of the heterogeneity in the aggregate effects of monetary policy may be attributable to banking sector structure than previous estimates suggest, particularly in light of the evidence that loan supply reductions by small banks exert larger effects on economic activity than do reductions by large banks (Hancock and Wilcox, 1998). Similarly, our results suggest that there is more scope for banking sector consolidation and the growth of bank holding companies to account for possible trends towards a weaker aggregate monetary transmission mechanism in recent decades.[26] The relevance of these conjectures depends on the precise configuration of banking sector characteristics. Specifically, a region or episode associated with a banking sector dominated by holding companies must *not*

[25] These estimates are from our baseline regression specification, which contrasts affiliated and non-affiliated banks, assuming all other characteristics remain unchanged. It is of course possible that the switch to bank holding company status is associated with changes to other bank characteristics that affect bank lending responses at the margin. However, if we exclude all bank characteristics other than holding company status, to estimate the unconditional effect of affiliation, the finding that holding company banks raise lending at the expense of stand-alone banks remains intact.

[26] A caveat should be noted in relation to the interaction effect based on bank assets. Our assertions rest on interpreting the differential effects by bank assets in terms of loan supply. Ashcraft (2006) argues convincingly that the slope of the loan demand curve varies with bank assets (larger banks trade with customers whose loan demand is less interest rate sensitive). Therefore, part of the interaction between monetary policy and assets that we estimate could reflect heterogeneity in loan demand. It is less clear that such a feature of lending markets could drive heterogeneity in the aggregate transmission mechanism. We implicitly assume that at least part of the asset-based interaction arises from loan supply effects.

be associated with other characteristics that reverse the impact of holding company affiliation on lending responses. We hope to address these questions in future research.

The effects of balance sheet composition

The most striking result that we present in table 4 relates to the securities-to-assets ratio. Following a 100 b.p. increase in the exogenous policy measure, a bank with securities one standard deviation above the mean *reduces* lending by a further 0.22 percentage points compared to the representative bank. In contrast, following a 100 b.p. increase in the realized federal funds rate, a bank with securities one standard deviation above the mean *shields* lending by 0.04 percentage points relative to the average bank. The UM interaction is significant at the 1% level and the FF interaction is significant at the 5% level. In previous work, the shielding effect from securities has been related to the idea that such holdings are a buffer stock of liquid assets which can be used to substitute lost reserves during policy contractions (Kashyap and Stein, 2000; Ashcraft, 2006). Our results suggest the empirical support for such an interpretation comes from a confounding of expected future growth and inflation with the monetary policy stance.

A possible explanation for the negative effect of monetary policy tightening upon lending for banks with large securities-to-assets ratios follows. An exogenous rise in interest rates is likely to raise the long end of the yield curve and depress securities prices, such that banks suffer a capital loss – see Bernanke and Gertler (1995) for a discussion of this effect. Banks with greater exposure to capital losses on securities will be forced to contract lending more aggressively, leading to an amplification effect. In such instances, seemingly liquid assets such as securities exhibit low 'market liquidity,' in the sense that their market value is driven below their fundamental value. As a result, banks may refrain from liquidating the assets and instead choose to contract their lending.

The final two rows in table 4 relate to the equity capital and net cash ratios. We consider these two characteristics together since cash flow contributes to equity capital. However, we focus upon the equity capital ratio since it is a more comprehensive indicator of the liquid funds available to banks. Both characteristics play a role in shielding bank lending from exogenous policy contractions. A bank that is one standard deviation above the mean equity capital ratio trims one quarter less from its lending following a rise in UM. In our results, it is equity capital that makes bank lending resilient to contractionary open market operations, *not* holdings of securities, which have typically been interpreted as a buffer against funding shocks.[27] As the difference between a bank's assets and explicit liabilities, equity capital is an indicator of the amount of liquidity on banks' balance sheets. Significantly, the value of liquid assets represented by equity capital does not appears to erode in response to tight monetary policy (unlike securities). Therefore, the full value of liquid assets is available to substitute lost reserves, and hence sustain deposits and loans, during periods of tight policy.[28] Estimates in the final row in table 4 indicate that current cash flow relative to assets (approximately the current period increment to the equity capital share) plays an especially important role in shielding lending growth from exogenous policy contractions. This may reflect greater resilience to policy contractions amongst banks that are relatively profitable and able to use operating surpluses to support lending activities.

Our corresponding results based on the realized federal funds rate provide an interesting contrast with the results from UM. The interaction between equity capital and FF is smaller and less significant than that from UM, while the interaction between the net cash ratio and FF is not significantly different from zero. In annual data regressions, Ashcraft (2006) estimates a negative and marginally significant equity capital interaction and a positive and significant cash ratio interaction. Using quarterly data and regressions for banks from different size and capitalization classes, Kishan and Opiela (2000) find that equity capital mitigates lending responses to FF amongst small banks (consistent with a positive interaction), but also some evidence that it can amplify lending responses amongst large banks (consistent with a negative interaction). The generally weaker evidence for a shielding effect from equity capital when using the realized federal funds rate may arise because loan responses are attenuated following endogenous policy changes. Moreover, they may be attenuated to a greater extent amongst poorly capitalized

[27]Ashcraft (2006) argues that the interest rate sensitivity of loan demand decreases with bank capitalization, which may account for some or all of the positive interaction between UM and the equity capital ratio. However, both Ashcraft and Kishan and Opiela (2000) present evidence that the shielding effect of policy is not weakened even if loan composition measures that are thought to proxy the elasticity of loan demand are controlled for.

[28]Kashyap and Stein (2000) question the interpretation of cash on the balance sheet as a liquidity buffer on the grounds that cash holdings may reflect required reserves that cannot be freely drawn down. It is important to emphasize that we use the equity capital share in assets relative to the sample mean. Therefore, our results show that equity capital helps to shield lending growth *not* when it is high in an absolute sense (which could be the case simply because a bank is large and holds lots of loans and hence reserves), but when it is high *relative* to assets amongst the population of banks, likely indicating an excess of liquidity relative to the required amount.

banks, since they likely expand loan supply more in response to the factors associated with endogenous policy tightening (see the discussion in section 2.1).

We draw attention to two implications of our finding that well-capitalized banks are able to shield their lending from exogenous monetary policy. First, banks that are unable to smooth their lending using funds from associate networks (e.g., holding companies) may be able to mitigate any lending contractions through the accumulation of equity capital buffers. At the end of our sample, the average equity capital ratio for stand-alone banks is 4 percentage points *higher* than for holding company banks, which is consistent with this idea (other interpretations of this observation are possible – e.g., the probability of joining a holding company may be linked to the equity capital ratio). Second, reductions in banking sector equity capital from loan write-offs during the recent housing market collapse and credit crunch may leave banks *less* able to shield their lending from contractionary monetary policy. This in turn may embed a more powerful monetary transmission mechanism until banks restore their capitalization ratios to levels seen prior to the current period of financial market turmoil.

4.1 Stability of the baseline results

An important issue in any study of monetary policy transmission to the banking sector is the temporal stability of the results – see Bernanke and Blinder (1992), Kashyap and Stein (2000) and Ashcraft (2006). In our sample, an important structural change may arise from the introduction of the source of strength doctrine (Ashcraft, 2006).[29] The Federal Reserve Board issued a formal statement in April 1987 indicating that failure by a parent bank to inject liquidity into a financially distressed subsidiary when funds are available would be considered an unsafe banking practice.[30]

In section 3.2, we argued that from 1987 onwards membership in a bank holding company should affect lending responses to monetary policy. Our baseline results are consistent with this

[29]Another source of structural change is the abolition of regulation Q, which restricted banks' ability to vary interest rates in order to attract deposits (a source of funding). The abolition of this restriction was largely implemented via the Monetary Control Act of 1980, and is therefore likely to induce heterogeneity in our results across a much shorter period than the source of strength doctrine. Due to the limitations in estimating heterogeneity in our results across a period of just three years or so, we do not address the effects of regulation Q. If observations from this period exerted undue influence on the results, the outlier detection procedure we employ ought to diagnose them.

[30]As noted in footnote 12, Ashcraft (2008) shows that the Financial Institutions Reform, Recovery, and Enforcement Act of 1989 unexpectedly strengthened the source of strength doctrine. Given that this change occurred just two years after 1987, we do not allow for a further structural change in 1989.

idea. In this sub-section, we take our analysis of the effects of the source of strength of doctrine one stage further. We interact each of the cross terms in $\sum_{j=0}^{4} X'_{i,t-1} M_{q,t-j} \delta_{q,j}, \forall q \in \{1,2,3\}$ with a binary variable that is set to unity post-1986 for banks that belong to a holding company (excluding the cross term that already features the holding company indicator). These extra terms are added to our baseline regression in 3. In table 5, we report interaction coefficients for policy measures and characteristics (similar to those in table 4), in addition to the changes to the interaction coefficients associated with the start of the source of strength doctrine.

The key feature of the results is that the post-1986 changes to the interaction coefficients (amongst holding company banks) are of approximately equal magnitude but opposite sign to the main interaction effects (the one exception is the interaction of FF with bank assets). As such, the total effect of balance sheet related characteristics on lending responses to monetary policy, both exogenous and endogenous, is close to zero during the second half of the sample for affiliated banks (and recall that affiliated banks represent over two thirds of all banks in this period). During the late 1980s and the 1990s, the principal source of heterogeneity in lending responses to monetary policy is affiliation with a holding company, not balance sheet composition. The roles of security holdings in amplifying and equity capital in mitigating the effects of exogenous policy on lending growth, are quantitatively smaller from the late 1980s onwards because they are observed *only* amongst banks that cannot access the financing networks provided by holding companies. In contrast, when affiliated banks face write-downs in securities prices or loan values following policy tightenings, they are able to tap loanable funds within the network, thus shielding their lending growth.

4.2 Loan composition effects

In this sub-section, we consider information on the composition of bank loan books. The Call Reports provide information on bank loans across five categories: residential loans, commercial and industrial loans, individual (consumer) loans, agricultural loans and municipal loans – see table 2 for some summary statistics relevant to our sample. This information is relevant to lending responses for a number of reasons. First, loans that are at least partially collateralized are more easily sold via the securitization process. In such instances, banks effectively serve as warehouses for loans and may liquidate them in response to funding shocks (Strahan, 2008).[31]

[31] This has not been the case for sub-prime loans during the financial crisis that started in 2007.

Collateral is a more common feature of loans to customers in the residential and consumer sectors. Banks concentrating in those areas may be more able to shield their lending growth from monetary policy via securitization.[32] Second, it is possible that the interest rate sensitivity of loan demand varies across sectors of the market. For instance, the shorter maturity of commercial and industrial loans may render them more responsive to monetary policy (Kishan and Opiela, 2000). Equally, the pro-cyclicality of loan demand and loan supply, which we discussed in section 2, may vary across sectors. For instance, consumer loan demand may respond more strongly to income expectations. If such heterogeneity is a feature of the banking system, it can be captured using information on loan composition.

We take the total loan shares for residential loans, consumer loans and commercial and industrial loans, and introduce them as additional bank characteristics in our baseline regression, equation 3. We consider just three elements of the loan decomposition from the Call Reports, since municipal lending is typically a small share of total lending. The four remaining loan categories account for almost 100% of bank lending and as such are nearly collinear. In order to handle this, we omit the agricultural loan share. In table 6, we report output analogous to that in table 4. The responsiveness of bank lending to a 100 b.p. tightening is reported for the representative bank. We also report the increments to the lending response associated with a loan share measure that is one standard deviation above the mean share for loans of that type. We do not report results for the bank characteristics discussed previously, but they confirm those presented in tables 3 and 4 (details available upon request).

Using changes in the realized federal funds rate, a bank whose participation in the residential loan market is one standard deviation above the mean contracts their lending by roughly *twice* as much as a bank that does an average amount of business in that market. A similar result holds for banks whose business is concentrated in the consumer loans market. In contrast, commercial lending is only slightly more responsive to federal funds rate rises than the average loan, and by implication agricultural loans must be *less* responsive to policy than loans in general. These results are consistent with those reported by Kishan and Opiela, who show that residential and consumer lending at small and poorly capitalized banks contracts more rapidly than commercial lending.

[32] Loutskina (2005) develops a more precise measure of the securitizability of bank loan books that incorporates information on the extent of securitization of different loan categories at the aggregate level, as well as the composition of bank loan portfolios.

The results estimated using UM indicate quite different effects of monetary policy by loan type. The reduction in the sensitivity of lending growth to monetary policy amongst banks whose participation in the residential loan market is one standard deviation above the mean is 0.13 percentage points when policy is measured using UM, compared to 0.04 when policy is measured using FF.[33] The changes to the responsiveness of bank lending growth to monetary policy when banks' operations are concentrated in the market for consumer loans are 0.05 and -0.24 percentage points for UM and FF respectively. Although the shielding of loan growth following a rise in UM is significant only at the 15% level, it provides a stark contrast with the FF case which suggests lending contracts by more at banks whose business is concentrated in the consumer sector. Kishan and Opiela report a similar result using the effective federal funds rate. On the other hand, the UM measure suggests slightly *stronger* shielding of commercial and industrial loans than consumer loans, even though the former are less readily securitized. We leave detailed examination of these results for future research.

5 Robustness

In this section, we report the results of robustness exercises performed for our baseline regression estimates presented in tables 3 and 4. First, in section 3.1 we noted that the policy measure UM may not eliminate endogenous policy movements during episodes in which the FOMC set interest rates in light of banking sector conditions. The episodes during which such a critique seems reasonable for our sample are: (i) the tightening of bank capital regulations due to the Basel I Accord, which may have induced less restrictive monetary policy than would have been implemented based on growth and inflation objectives alone; and, (ii) the Federal Reserve Bank of New York's rescue of the hedge fund LTCM in the late 1990s, which may have prompted a similar policy response. We define two separate dummy variables, one equal to unity for all quarters in the period 1990-1993 (Ashcraft, 2006 uses a similar dummy variable), and the second equal to unity for all quarters in 1998-1999 (the LTCM rescue occurred in 1998). We then interacted these dummy variables with each of the terms from equation 3 that feature a monetary policy measure, and estimated the extended specification using the procedure outlined in section 3. The results from this exercise, for both UM and FF, are presented in the first

[33] The result using FF contrasts with that reported by Kishan and Opiela (2000) who show that residential lending at small and poorly capitalized banks contracts *more* rapidly than commercial lending.

column of table 7. The effect of monetary policy on lending growth at the representative bank increases in absolute size only marginally, indicating little evidence that the estimated effects of monetary policy were attenuated during the two episodes considered. The interaction coefficients are in line with those presented in table 4, and the comparison of interaction effects across UM and FF supports each of the main results described in section 4.

In the second column in table 7, we report results obtained after augmenting equation 3 with bank-level fixed effects. Although substantial fixed effects are unlikely given that we model loan growth rather than total loans, we consider this robustness exercise given that it has been applied elsewhere in the literature. For example, Loutskina (2005) motivates a fixed effects lending growth specification based on differences in managerial preferences.[34] The results indicate that our main findings are generally robust to this model extension. A possible exception occurs in the case of the lending response at the representative bank, which narrows such that the intervals spanned after imposing two standard error bands around the point estimates are overlapping. However, this is mainly due to the selected reporting horizon. At other horizons in the first year after a monetary tightening the lending reduction from UM is more than twice that from FF and intervals formed from two standard error bands are non-overlapping. Another caveat to be noted is that the shielding effect from bank holding company membership is just one-sixth larger when estimated from UM as opposed to FF, whereas in our baseline results it was more than 1.5 times as large. Nevertheless, the precision with which the effects are estimated ensures that the intervals formed after imposing two standard error bands around the estimates are non-overlapping, matching our baseline results.

The third robustness test addresses the fact that in equation 3 each of the bank characteristics interacted with a monetary policy measure are dated $t-1$, even when the policy measure is dated somewhat earlier (e.g., $t-4$). The dating of characteristics in our baseline regressions is standard in the literature, but it leaves open the possibility that a characteristic value is a function of the earlier policy change with which it is interacted. In order to address this issue we date all characteristics in interaction terms $t-j-1$, such that they are pre-determined with respect to the policy variable with which they are interacted (the level characteristics,

[34]The inclusion of fixed effects and autoregressive terms raises the possibility of estimation bias of the form discussed by Nickell (1981). However, the size of this bias declines with the time dimension of the panel, and in our case an average number of time observations per bank of 57 likely means that this bias is minimal. Judson and Owen (1999) find quantitatively small bias provided $T \geq 30$. Interestingly, the autoregressive coefficients change very little across the baseline and fixed effects specifications (results not reported).

which enter the regression just once, continue to be dated $t-1$). The results from this exercise, performed for both UM and FF, are reported in the third column in table 7.

Our main findings are robust, except in two cases. First, the amplification of the contractionary effects of UM associated with large security holdings is absent in the third set of results, signaling the need for some caution in relation to this finding. Interestingly, the negative interaction between UM and the securities ratio is observed if we cumulate the interaction terms out to horizon 3 rather than horizon 4, and is present out to horizon 4 if we use the larger sample that does not exclude outliers. Second, the interactions between the policy measures and the net cash ratio change sign in column 3 relative to the baseline case. The effect of bank profitability/cash flow on lending responses to monetary policy is more dependent on the dating of the characteristic than are the effects of other characteristics. This is unsurprising since the net cash ratio, closely linked to profits, is less persistent than other components of assets. It therefore changes more through time (the relatively low persistence of this series is reflected in its large coefficient of variation, calculable from table 2). One interpretation of our previous result for the net cash ratio is that policy shocks exert a large and persistent effect on the lending of a certain group of banks, which then reduces their profitability and net cash ratio. The relatively large lending reversal for this set of banks then correlates with a low equity capital ratio, yielding the positively signed interaction from this characteristic in table 4. In table 7, this positively signed interaction is absent because the characteristics are pre-determined with respect to monetary policy and cannot respond to policy changes. Given this possible interpretation of the net cash ratio interaction, we do not emphasize our results from this particular characteristic, focusing instead on those related to total assets, holding company status, and the balance sheet shares of securities and equity capital.

In the final column in table 7, we present a version of our baseline results that uses an alternative definition of the total assets variable. This is the demeaned series for $\ln\left(\frac{assets_{i,t}}{\overline{assets_t}}\right)$ where $\overline{assets_t}$ is the cross-sectional mean of assets in period t (in the baseline case, we use the demeaned series for $\ln(assets_{i,t})$). Scaling each observation by the cross-sectional mean of bank assets at each point in time eliminates the drift in assets that occurs as a result of bank balance sheets growing in nominal terms through time. In the baseline results, this effect was handled via the inclusion of a time trend, an approach that is standard in the literature (e.g., Loutskina, 2005). Such a method will account for a linear trend in the assets of a representative bank, but it does not deal with changes to the average growth rate for bank assets. We use the alternative definition in all terms that feature assets (including the interaction terms), so that the new set of results does not reflect any common, time-varying drift in the data for total assets.

Our core results remain intact using the new measure of assets, except that the interaction between bank assets turns negative and marginally significant when UM is the policy measure, and negative and significant when FF is the policy measure. It appears that the evidence that large banks can shield their lending growth from monetary policy is partly a function of the upward trend in asset values, which correlates with the trend towards weaker monetary transmission through time. In the absence of this effect, notice how the impact of UM on lending growth at the representative bank moderates from -0.94 to -0.74. Crucially however, our other results are very robust in the final column in table 7. In particular, the lending contraction at the representative bank is one-third larger when policy is measured using UM rather than FF. Moreover, the shielding of lending growth amongst holding companies is 1.5 times larger using the exogenous policy measure. Finally, it is the equity capital ratio, not the securities ratio, that serves as a buffer to lending growth when monetary policy shocks are measured using UM.

6 Conclusions

The credit market turmoil of 2007 and 2008 has highlighted the critical role played by the banking system in the transmission of monetary policy to the real economy. Recently, policymakers have focused on the way in which banking sector conditions have blunted the stabilizing effects of the large interest rate reductions implemented by the FOMC during the first half of 2008 (Rosengren, 2008). During the last decade, considerable progress has been made in identifying the features of the banking industry that matter for monetary transmission, especially following the creation of the large database on the activities of FDIC-insured banks in the United States in work by Kashyap and Stein (2000). The bulk of this research has used the realized federal funds rate to measure monetary policy. The key point emphasized in our paper is that such a policy measure is endogenous to expected future macroeconomic conditions, which are likely to exert separate effects on both loan demand and loan supply. We have set out examples of such effects and have argued that they may induce bias in *both* the estimated direct impact of monetary policy on bank lending and in the estimated impact conditional upon bank characteristics. In the empirics, we provided a comparison of the heterogeneity in bank lending responses to an explicitly identified monetary policy measure and the realized interest rate which is more commonly used in the literature.

The results indicated both economically and statistically significant attenuation of lending responses to monetary contractions, accompanied by the shielding of lending associated with bank holding company affiliation. We also found sign reversals in the effects conditional upon some characteristics. Specifically, the share of securities in total assets was shown to amplify policy transmission from exogenous interest rate changes, while restricting the transmission of realized interest rate changes. One explanation for this result is that many types of securities are subject to an adverse valuation effect following exogenous monetary policy contractions, which limits the scope for lending at banks that hold them in large numbers. In contrast, endogenous rises in the federal funds rate may be associated with lending increases (due to the underlying macroeconomic conditions to which policy is endogenous) at banks which choose to invest heavily in securities. The bank characteristic found to correlate with shielding of lending growth after policy contractions was the equity capital ratio. This shielding effect was stronger using the exogenous policy measure.

An important research implication from our work is that future studies of the banking system and monetary transmission should consider exogenous policy measures, alongside other measures such as the realized federal funds rate. In particular, the identification of exogenous monetary policy should take into account the forward-looking drivers of monetary policy such as growth and inflation forecasts, because these forward-looking variables are likely to impact lending markets. Interesting avenues for future work include the examination of the impact of exogenous monetary policy upon regional, industrial, and firm-specific lending. It would then be possible to compare these impacts with those from the realized federal funds rate, and to consider their consistency with the banking sector structure relevant to the associated regions, industries, and firms. One possibility is that heterogeneity in the banking sector may account for a larger proportion of the differential effects of monetary policy across these units when an exogenous policy measure is employed (see section 4 discussion). Other interesting areas for future work include the application of the methodology used here to study heterogeneity in lending rates, as opposed to lending quantities. In addition, it would be useful to investigate the effects of bank characteristics on responses to policy at a more disaggregated level, such as the relationship to the particular mixture of securities held as bank assets.

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Table 1:

Determinants of changes in the intended federal funds rate		
regressor	Coefficient	Standard error
intercept	-0.910	1.168
target from last meeting	-0.024	0.011
forecasted output growth		
-1	0.006	0.010
0	-0.013	0.020
1	-0.025	0.028
2	0.016	0.031
total effect	-0.016	0.027
output growth revision		
-1	0.005	0.025
0	0.134	0.029
1	-0.022	0.041
2	-0.008	0.050
total effect	0.104	0.068
forecasted inflation		
-1	0.030	0.023
0	-0.017	0.028
1	0.020	0.043
2	0.014	0.044
total effect	0.047	0.020
inflation revision		
-1	0.007	0.030
0	-0.020	0.041
1	-0.016	0.066
2	-0.050	0.074
total effect	-0.079	0.082
forecasted unemployment		
-1	-0.137	0.162
0	0.599	0.352
1	-0.290	0.475
2	-0.218	0.319
total effect	-0.047	0.035
unemployment revision		
-1	-0.189	0.216
0	-0.515	0.319
1	0.684	0.441
2	-0.444	0.343
total effect	-0.464	0.204
capacity utilization	0.015	0.012
capacity utilization growth	0.136	0.035

R^2=0.36, N=298. The sample is all scheduled FOMC meetings from the period 1969-2001. See the main text for a description of the regressors. The total effects refer to the sum of the coefficients on sets of forecasts or forecast revisions for the previous, current and next two quarters.

Table 2:

Descriptive Statistics for Bank Level Variables						
	1977		1988		1999	
	Mean	Std dev	Mean	Std dev	Mean	Std dev
Loan growth	4.51	5.48	1.81	5.65	2.24	5.47
Assets	84.51	1105.97	209	2257.81	506.58	6761.38
Holding company status	26.29	44.02	68.86	46.31	79.67	40.25
Securities/assets	30.57	12.17	28.86	15.83	27.16	13.72
Equity capital/assets	8.55	2.48	8.78	2.84	10.23	3.51
Internal cash/assets	1.15	1.27	1.52	2.87	0.88	1.13
Residential loan share	33.15	17.25	44.11	17.53	56.3	18.74
Consumer loan share	28.54	14.34	20.54	13.11	14.6	12.09
Commercial loan share	19.41	12.75	21.17	13.45	17.06	11.35
No. of banks	14 026		12 516		8 030	

Summary statistics are calculated for all banks included in the baseline estimation sample for at least part of a year, and therefore reflect across bank and within bank variation. All variables are reported as percentages, except assets which is measured in millions of current price US$. The holding company status variable measures the fraction of banks in the sample affiliated with a holding company. See the text for further discussion concerning the measurement of the variables.

Table 3:
Lending Responses at the Representative Bank

Horizon	UM	FF
0	-0.04*	-0.03**
	(0.02)	(0.005)
1	-0.40**	-0.18**
	(0.02)	(0.009)
2	-0.75**	-0.38**
	(0.04)	(0.014)
3	-0.92**	-0.60**
	(0.05)	(0.02)
4	-0.94**	-0.73**
	(0.05)	(0.02)

Notes: Cumulative responses to a 100b.p. tightening at the specified horizon are reported for a bank at the sample average of continuously measured characteristics and not part of a holding company. Standard errors after clustering at the bank level are reported in parentheses. ** and * indicate significance at the 1% and 5% levels respectively.

Table 4:

Bank Lending Responses to Monetary Policy		
	UM	FF
representative bank		
marginal effect	-0.94**	-0.73**
	(0.05)	(0.02)
assets		
interaction	0.26**	0.08**
	(0.03)	(0.01)
marginal effect	-0.68**	-0.66**
	(0.06)	(0.03)
holding company status		
interaction	1.88**	1.21**
	(0.08)	(0.04)
marginal effect	0.95**	0.47**
	(0.07)	(0.03)
securities ratio		
interaction	-0.22**	0.04*
	(0.04)	(0.02)
marginal effect	-1.16**	-0.69**
	(0.06)	(0.03)
equity capital ratio		
interaction	0.24**	0.04**
	(0.04)	(0.016)
marginal effect	-0.70**	-0.69**
	(0.06)	(0.03)
net cash ratio		
interaction	0.97**	0.08
	(0.26)	(0.14)
marginal effect	0.03	-0.65**
	(0.30)	(0.13)
Observations	1053334	1052453
R^2	0.16	0.16

Notes: The reported lending responses are the sum of contemporaneous and four lagged responses to a 100 b.p. policy tightening. Interaction effects are calculated for a bank one standard deviation above the sample average for a characteristic and marginal effects sum the direct and interacted effects. Standard errors obtained after clustering at the bank level are reported in parentheses. ** and * indicate significance at the 1% and 5% levels respectively.

Table 5:

Holding Company Status and Lending Responses to Monetary Policy		
	UM	FF
representative bank		
marginal effect	-0.81**	-0.70**
	(0.06)	(0.02)
holding company status		
interaction with policy	1.60**	1.09**
	(0.10)	(0.04)
assets		
interaction with policy	0.48**	0.03
	(0.04)	(0.02)
interaction with policy	-0.65**	0.09**
and holding company	(0.07)	(0.03)
securities ratio		
interaction	-0.57**	0.04*
	(0.05)	(0.02)
interaction with policy	0.76**	-0.07
and holding company	(0.08)	(0.03)
equity capital ratio		
interaction	0.21**	0.07**
	(0.04)	(0.02)
interaction with policy	-0.14	-0.09**
and holding company	(0.08)	(0.03)
net cash ratio		
interaction	1.40**	0.14
	(0.27)	(0.09)
interaction with policy	-1.12**	-0.19
and holding company	(0.40)	(0.12)
Observations	1053673	1052665
R^2	0.16	0.16

Notes: The reported lending responses are the sum of contemporaneous and four lagged responses to a 100 b.p. policy tightening. Interaction effects are calculated for a bank one standard deviation above the sample average for a characteristic. Standard errors obtained after clustering at the bank level are reported in parentheses. ** and * indicate significance at the 1% and 5% levels respectively.

Table 6:

Monetary Policy and the Structure of Bank Lending		
	UM	FF
representative bank		
marginal effect	-0.80**	-0.70**
	(0.05)	(0.02)
residential		
interaction	0.13**	0.04*
	(0.04)	(0.02)
marginal effect	-0.66**	-0.66**
	(0.06)	(0.03)
consumer		
interaction	0.05	-0.24**
	(0.03)	(0.02)
marginal effect	-0.74**	-0.94**
	(0.05)	(0.03)
commercial		
interaction	0.08*	-0.003
	(0.04)	(0.02)
marginal effect	-0.71**	-0.70**
	(0.06)	(0.03)
Observations	1050736	1049694
R^2	0.17	0.17

Notes: The reported lending responses are the sum of contemporaneous and four laggeed responses to a 100 b.p. policy tightening. Interaction effects are calculated for a bank one standard deviation above the sample average for a given loan share and marginal effects sum the direct and interacted effects. Standard errors obtained after clustering at the bank level are reported in parentheses. ** and * indicate significance at the 1% and 5% levels respectively.

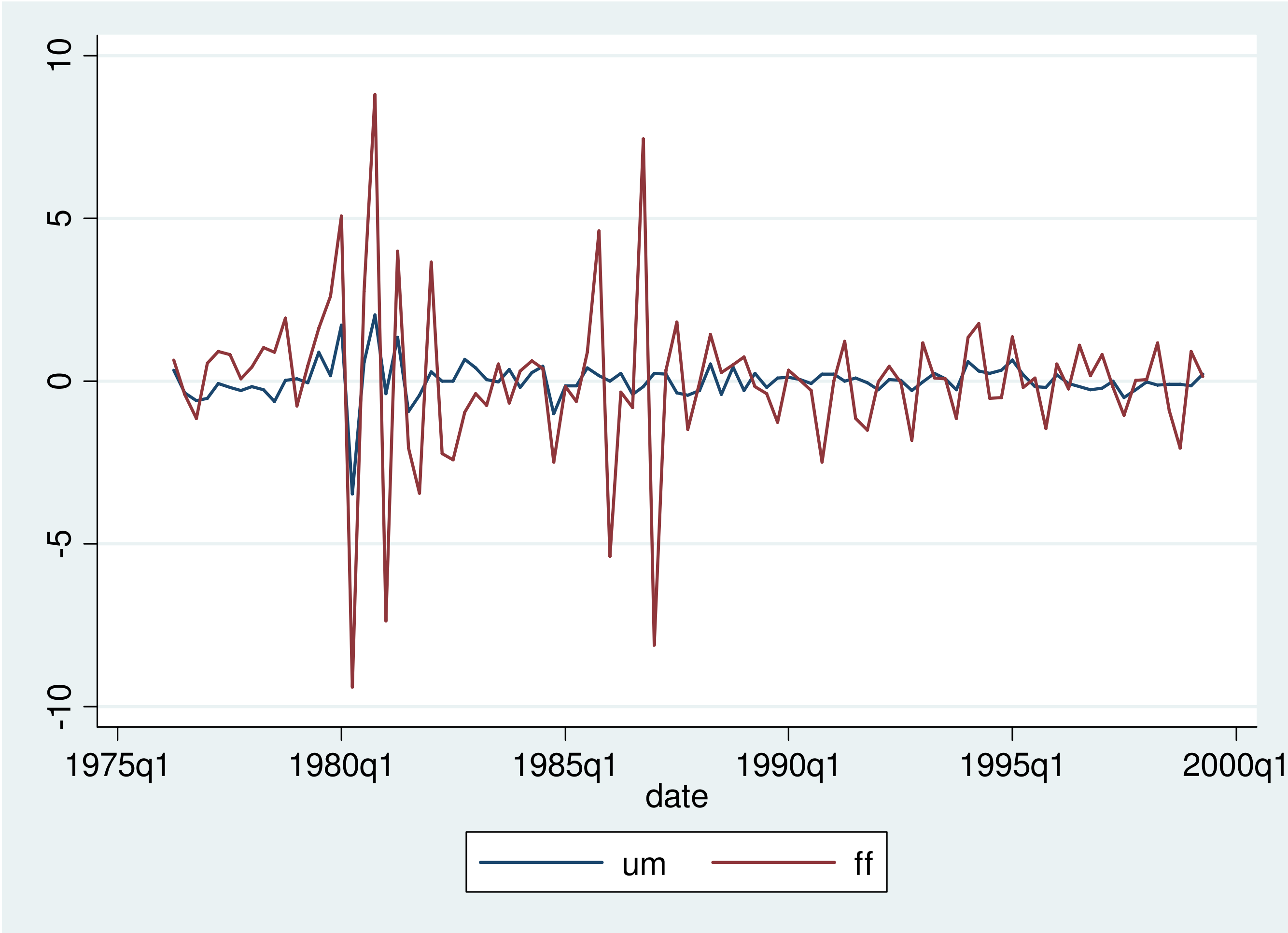
Table 7:

Robustness Tests for the Baseline Model

ROBUSTNESS TEST	BASEL & LTCM EPISODES		FIXED EFFECTS		PRE-DETERMINED CHARACTERISTICS		SCALE TOTAL ASSETS	
	UM	FF	UM	FF	UM	FF	UM	FF
representative bank								
marginal effect	-0.96**	-0.77**	-0.86**	-0.75**	-0.88**	-0.69**	-0.74**	-0.55**
	(0.06)	(0.02)	(0.05)	(0.02)	(0.04)	(0.02)	(0.06)	(0.02)
assets								
interaction	0.29**	-0.01	0.24**	0.04**	0.43**	0.09**	-0.05*	-0.09**
	(0.03)	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)
holding company status								
interaction	1.56**	0.96**	1.53**	1.24**	1.69**	1.08**	1.79**	1.19**
	(0.10)	(0.05)	(0.08)	(0.04)	(0.08)	(0.03)	(0.08)	(0.03)
securities ratio								
interaction	-0.36**	0.06**	-0.15**	0.1**	-0.01	0.06**	-0.23**	0.04*
	(0.04)	(0.02)	(0.04)	(0.02)	(0.03)	(0.01)	(0.04)	(0.02)
equity capital ratio								
interaction	0.09*	0.01	0.28**	0.04**	0.12**	0.01	0.08*	0.002
	(0.04)	(0.02)	(0.04)	(0.02)	(0.03)	(0.01)	(0.04)	(0.01)
net cash ratio								
interaction	1.60**	0.08	0.98**	0.01	-0.74**	-0.05*	1.31**	0.03
	(0.22)	(0.09)	(0.25)	(0.10)	(0.14)	(0.02)	(0.29)	(0.18)
Observations	1054328	1053721	1053334	1052453	1032780	1031899	1053403	1052489
R^2	0.16	0.16	0.13	0.13	0.16	0.16	0.16	0.16

Notes: The reported lending responses are the sum of contemporaneous and four laggeed responses to a 100 b.p. policy tightening. Interaction effects are calculated for a bank one standard deviation above the sample average for a given characteristic. Marginal effects sum the direct and interacted effects. Standard errors obtained after clustering at the bank level are reported in parentheses. ** and * indicate significance at the 1% and 5% levels respectively.

Figure 1: Time Series Plots for UM and FF



Notes: Variables are defined as in the main text. The correlation of UM and FF over the sample in the figure is 0.60. The sample variance of UM is 0.76 percentage points (76 basis points) and the sample variance of FF is 1.57 percentage points (157 basis points).