[PRELIMINARY]

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

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Abstract: Heterogeneity in U.S. bank lending responses to monetary policy is often used as a lever to evaluate the importance of the aggregate lending channel for monetary transmission. Although progress has been made in identifying bank characteristics that isolate the loan supply effects that are central to the theory, most research examines the consequences of these characteristics on lending responses to realized federal funds rate changes. Since monetary policy changes are endogenous to expectations over macroeconomic fundamentals, heterogeneous bank lending responses (loan supply shifts) may reflect the evolution of these expectations rather than funding pressures due to monetary policy. We estimate the heterogeneity in lending responses to an exogenous policy measure identified from narrative evidence on FOMC intentions and real-time macroeconomic forecasts prepared by Federal Reserve staff. A comparison of the responses with those estimated from federal funds rate changes indicates larger lending reductions following an exogenous policy contraction and a different picture of bank lending response heterogeneity. The shielding of loan growth associated with holding company affiliation is larger using the exogenous policy measure. Substantial holdings of securities amplify the transmission of exogenous policy shocks, but mitigates the transmission of movements in the realized federal funds rate, challenging the view that securities provide a liquidity buffer. Instead, a high equity capital ratio is the bank balance sheet characteristic that makes lending resilient to exogenous monetary contractions.

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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 monetary policy reduces banking sector reserves and deposits. The fall in reserves and deposits then forces a reduction in bank 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 monetary policy reduces borrower collateral and induces banks to raise the premium on loans relative to the competitive cost of funds (the external finance premium). The propagation of monetary policy via these loan supply channels is different from that seen in textbook treatments of monetary transmission that emphasize the sensitivity of aggregate expenditure to interest rate movements. See Bernanke and Gertler (1995) and Kuttner and Mosser (2002) for reviews of different elements of the transmission mechanism.

Following the work of Kashyap and Stein (2000), a number of empirical studies have explored the heterogeneity in U.S. bank lending responses to monetary policy. To the extent that financial constraints vary with banks' access to liquidity and collateral, the existence of a lending channel implies loan responses to policy that are contingent on observable bank characteristics that are related to such access. Kashyap and Stein (2000) showed that banks with relatively large, and relatively liquid, asset bases are better able to shield their lending growth during periods of tight monetary policy. The same has been documented for banks with relatively high equity capital to assets (Kishan and Opiela, 2000), banks affiliated to a holding company (Ashcraft, 2006), banks whose loan books are readily securitized (Loutskina, 2005), 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 is a pure loan supply effect, as in the theory of the narrow lending channel and the broad lending channel, or is an amalgam of loan supply and loan demand effects. Consequently, an important element of research design in this area has been to provide evidence for homogeneous loan demand along the dimension of one or more characteristics, such that heterogeneity in bank lending responses associated with a characteristic can be interpreted as a loan supply effect relevant to the bank lending channel at the macro level. See Ashcraft

(2006) for a discussion along these lines.

In these studies, little attention has been devoted to the question of what measure of monetary policy is appropriate in assessing bank lending behavior. Most papers use the change in the effective 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.[1] While federal funds rate changes initiated by the FOMC are surely exogenous to the idiosyncratic circumstances facing any single bank, the factors to which policymakers do 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 the confounding variables depends on bank characteristics, estimated heterogeneity in lending responses to monetary policy will be biased. Motivated by these possibilities, we evaluate the heterogeneity in bank lending responses to federal funds rate changes arising from exogenous factors, such as errors in policymakers' information and beliefs, reputational concerns, and preference shifts. The identification of this policy component extends work by Romer and Romer (2004), which combines narrative evidence on Federal Reserve intentions and the real-time U.S. macroeconomic (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, which have been the focus of most previous research.

Our results indicate a radically different picture of the heterogeneity in bank lending responses to monetary policy. We summarize the comparison across policy measures in five main findings. First, following an exogenous monetary contraction, the reduction in lending growth during the first year at a bank associated with average values of each of the characteristics, is up to twice that following a contraction measured using the realized federal funds rate. This is consistent with the hypothesis that lending responses to monetary policy are attenuated when policy changes are partly endogenous to expected output growth and inflation.

Second, the amount by which a bank can shield its lending growth from a monetary policy

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

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 from exogenous monetary policy. One potential implication is that differences in the structure of local banking sectors explain a larger fraction of the heterogeneity in monetary policy effects, by region and industry, than previously believed.[2] Furthermore, based on the argument that differences in lending responses by bank holding company status provide the most convincing evidence for a lending channel (Ashcraft, 2006), our results suggest that the lending channel is an important constituent of the more powerful monetary policy propagation that we document.

Third, the share of bank assets held as securities mitigates the lending response to a realized federal funds rate increase, but amplifies the lending response to an exogenous federal funds rate increase. In contrast, the ratio of equity capital to assets shields lending growth from an exogenous monetary tightening, but is only very 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, arguing that they provide a new perspective on the measurement of balance sheet liquidity and the consequences of balance sheet composition for the strength of monetary policy propagation. An important qualification to our results on balance sheet composition is our fourth finding.

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 amongst banks that are not part of a holding company. Affiliated banks appear able to smooth lending in the face of monetary policy shocks using funds from the holding company network, such that balance sheet composition does not affect lending responses to monetary policy.

Fifth, banks that do a larger proportion of business in the residential sector reduce lending by a smaller amount following an exogenous policy tightening, consistent with better access to the finance needed to sustain such loans, via the securitization process (Loutskina, 2005). In contrast, following realized federal funds rate changes, this effect is much smaller and only marginally significant.

To put our paper in context, we consider how potential biases from the confounding of monetary policy with other loan demand and loan supply determinants have been handled

[2] Of course, local banking sector differences by region/industry are likely driven by differences in the local client base.

in the literature. Each of the papers discussed at the start of this introduction control for output growth, inflation, or both, in empirical models of lending growth. To the extent that such variables are 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. Heterogeneity in these structural effects is then measured via interactions with bank characteristics, under the assumption that loan demand does not vary with the characteristic.

The starting point for our paper is that current output growth and inflation are not the only sources of endogenous policy – a forward-looking policy maker aiming to minimize cyclical fluctuations also responds to forecasts for its objective variables. If these forecasts correlate with private sector expectations for growth, inflation, and policy, loan demand and loan supply may fluctuate, potentially in a way that is systematically related to observable bank characteristics. It is important to address these possibilities in any research design. To this end, we investigate lending behavior at the micro level using an exogenous policy measure and a range of bank characteristics. The heterogeneity associated with some of these characteristics is best interpreted in terms of the overall contribution of the lending market to monetary transmission, because they are likely to capture effects from both loan demand and loan supply. On the other hand, the heterogeneity associated with some characteristics is more plausibly a pure loan supply effect that may be interpreted as evidence for a bank lending channel. In both instances, however, the results estimated from an exogenous policy measure offer new insights into the strength of the monetary transmission mechanism, the heterogeneity in monetary transmission, and factors that may affect the strength of monetary transmission in future episodes. We therefore argue that an important research implication supported by our work is 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, based on a simple textbook model of loan supply and demand. Motivated by these possibilities, in section 3 we outline an identification of exogenous monetary policy shocks and discuss the origins of such shocks. We then describe the bank level data and the econometric framework that we use to compare lending responses to identified policy shocks with lending responses to effective federal funds rate changes. In section 4 we present our core results and discuss our principal findings, and in section 5 we consider the robustness of these results to various changes to our basic research design. Finally, in section 6 we conclude with a summary of the paper and a discussion of the importance of monetary policy identification for future research concerning bank lending behavior.

2 Bank lending and monetary policy

In this section, we motivate an analysis of bank lending responses to exogenous monetary policy shocks. We consider potential biases in the estimated effects of U.S. policy when the effective federal funds rate is used as a measure of monetary policy. In each of the cases discussed, the underlying idea is that expectations over output growth and inflation directly determine both policy decisions and bank lending, but that these effects are not captured in the lending growth regression. Consequently, there is an omitted variable problem that affects the estimated response of bank lending to movements in the realized federal funds rate. These distorted lending responses differ from the structural effects of monetary policy that are the focus of the theoretical literature. In addition, our discussion covers lending responses that are contingent on bank characteristics and 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 propose a comparison of bank lending responses to exogenous monetary policy shocks and the broader measure of monetary policy that has been used in previous research. This exercise is described in section 3 of the paper.

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

$$\Delta L_{i,t} = \alpha + \beta M_t + \zeta' \mathbf{X}_{i,t} + \gamma' \mathbf{X}_{i,t} M_t + other\ \ controls + \varepsilon_{i,t} \tag{1}$$

where i indexes banks, t indexes time, L denotes the natural log of total loans measured at current prices, M is a monetary policy measure, $\mathbf{X}$ is a vector of bank characteristics, and

ε is an error term. We stress that in practice bank lending regressions are much richer than equation (1), oftentimes including autoregressive terms, dynamics in M and X, and a wide range of controls. In section 3, we describe a more complex version of model (1) that incorporates these features and which provides the basis for our empirical work. The present specification is sufficient to illustrate the arguments that we develop in this section.[3]

As noted in the introduction, the vector $\mathbf{X}$ comprises bank characteristics that capture access to finance. These might include total bank assets, affiliation to a bank holding company, an indicator variable for whether a bank operates internationally, measures of balance sheet composition such as equity capital to assets and securities to assets, and information concerning the bank loan book (e.g., the ease with which loans can be securitized). In the aftermath of a contractionary monetary policy shift, banks that can access funds from these sources may shield their lending growth from the effects of an erosion of reserves and deposits. In the literature, an important issue is the interpretation that can be given to a cross effect between monetary policy and a bank characteristic. It is commonly argued that although 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. We do not add to this debate, but instead 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 effective federal funds rate, which has been the Federal Reserve's operating target since at least 1994, and arguably for many episodes from the post-war period. In some papers, alternative monetary policy measures due to Boschen and Mills (1995) and Bernanke and Mihov (1998)

[3]Alternative regression frameworks have been considered in the literature. Most notably, Kashyap and Stein adopt a two-stage procedure in which the cross-sectional sensitivity of lending growth to balance sheet liquidity is estimated at the first stage, and a time series regression relating these cross-sectionally estimated liquidity constraints to monetary policy is estimated at the second stage. We do not adopt the two-stage approach in this paper. They also consider a specification similar to equation (1) as a robustness check.

are used. These are intended to be robust to changes in the operating target (e.g., the switch to targeting non-borrowed reserves under Volcker during 1979-81). We do not consider these alternative policy measures in this paper, but as they are also measures of the endogenous stance of monetary policy we believe that the arguments developed in this section are applicable to them.[4]

The potential effects of monetary policy endogeneity can be illustrated in a textbook model where loan supply and loan demand (in nominal units) are functions of the interest rate on loanable funds. This interest rate will increase with the federal funds rate, which influences loan supply via the cost of overnight finance for banks. Now suppose that the Federal Reserve targets a rise in the effective federal funds rate via open market operations. All else equal, the theory of the lending channel predicts a leftward shift of loan supply and a reduction in equilibrium lending, because an erosion of reserves forces banks to substitute away from deposits towards more expensive types of funding.[5] In many instances, however, rises in the federal funds rate target are a response to forecasts indicating future higher inflation and strengthening economic activity. With higher expected output growth, the loan supply contraction may coincide with a rightward shift of loan demand, as consumers borrow against expected future income and firms invest in response to an improving outlook for profits. If banks respond to these improved lending opportunities, any reduction in lending from a monetary tightening will be attenuated, and the β estimated from (1) will not capture the full response of lending to monetary policy.[6] 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 output growth, equilibrium lending is subject to countervailing effects from loan demand and loan supply, such that β estimates from (1) are attenuated.

The drivers of endogenous monetary policy may also influence equilibrium lending via bank loan supply curves. First, the availability of non-deposit finance to banks may vary positively

[4]Loutskina (2005) considers results for the Strongin (1995) policy measure, which treats non-borrowed reserves in the interbank market as the policy instrument and uses data on total reserves to net out influences from reserve demand shocks. While this identification may control for some aspects of policy endogeneity, it is not clear that the approach deals with forward-looking policy moves. Given that policy measures linked to the federal funds rate are the focus of the literature, we concentrate on such measures in the rest of this paper.

[5]Alternative sources of funding such as equity and securities are imperfect substitutes for deposits because they are not insured and are therefore subject to risk premia from adverse selection and moral hazard effects.

[6]The logic that underpins this argument is similar to that employed by Romer and Romer (2004) in explaining biases in the effect of federal funds rate changes on output growth.

with expected economic growth. At the start of cyclical upturns, pension funds and sovereign wealth funds may invest more heavily in equities and loan backed securities, at the expense of fixed income assets, based on a greater appetite for high 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 increase at each level of market interest rates. Second, to the extent that lender risk aversion plays a role in explaining holdings of excess reserves (at the expense of loans), the prospect of a business cycle expansion may boost loan supply through reducing excess reserves relative to loans. This could occur either because banks anticipate smaller losses from bad loans during episodes of strong economic growth, or because banks' valuation of the risk from loan writedowns declines during cyclical upturns. Third, in models featuring information asymmetries and monitoring costs, loan supply incorporates an external finance premium that varies inversely with borrower net worth (Bernanke and Gertler, 1989). Expansion phases of the business cycle are typically associated with increases in agents' net worth, such that the external finance premium falls, loan supply increases and lending rises.[7] We do not emphasize any one of these three channels ahead of the others. Instead, we highlight that when loan supply responds to expected economic conditions via any one of them, the response of lending growth to a policy measure such as the federal funds rate will be attenuated. Furthermore, this will occur even when controlling for current economic growth and inflation, because the effect derives from the fact that expectations influence both monetary policy decisions and loan supply.

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 regression (1) that measure heterogeneity in bank lending responses. As discussed in the introduction, these are the terms that proxy financial constraints at the bank level, and it is such constraints that underpin the aggregate lending channel of monetary policy. If banks are homogeneous and equally affected by expected macroeconomic conditions, then the introduction of bank characteristics allow for some time-varying aggregate effects of monetary policy (conditional on characteristics), since there is no cross-sectional variation in banks. Con-

[7] An external finance premium may also characterize interbank lending rates, creating a further channel through which expected cyclical conditions may propagate to loan supply.

sequently, endogenous variation in monetary policy likely attenuates the γ coefficients, via the channels described in the previous discussion.

The alternative scenario is that in which the attenuation of lending responses to monetary policy varies systematically with bank characteristics. Then, estimates of equation (1) obtained using the realized federal funds rate may either obscure or induce systematic heterogeneity in bank responses to monetary policy. Given the complexity of individual bank lending decisions, it is not possible to pin down the precise direction of these effects. Instead, in the remainder of this section, we outline mechanisms behind potential biases, some of which build on scenarios that have been emphasized in previous research.

We consider four cases: i) expected macroeconomic conditions induce common loan demand shifts, but bank responses depend on characteristics; ii) expected macroeconomic conditions induce loan demand shifts that vary with bank characteristics; iii) expected macroeconomic conditions induce common loan supply shifts, but adjustment of equilibrium lending depends on bank characteristics; iv) expected macroeconomic conditions induce loan supply shifts dependent 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 (in response to the factors driving loan demand) the attenuation of a bank lending effect will be more pronounced amongst the group of banks exhibiting the access to liquidity characteristic – estimates of the elements of γ from (1) will be biased away from zero, and evidence for heterogeneity in lending responses to monetary policy will be overstated when estimated from the realized federal funds rate.

Turning to the second of the four possibilities that we have listed, Kashyap and Stein (2000) argue that rational buffer stocking by banks may lead to a 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 them to accommodate volatile loan demand (e.g., bank holding company/global network 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 will then be largest amongst that category of banks. As in the first case discussed, this effect would manifest as positive bias in the estimate of γ – evidence that banks with access to liquidity can shield lending growth from Federal Reserve policy would be overstated.[8]

The opposite result, where estimates of cross effects are subject to negative bias, can arise when common loan supply shifts elicit different lending responses. Consider markets in which some lending opportunities are left unexploited–loan demand exceeds loan supply at the market interest rate. Market clearing may fail because interest rate rises induce moral hazard or adverse selection effects (Jaffee and Russell, 1976 and Stiglitz and Weiss, 1981). [9] The value of unexploited projects will be lower for banks whose supply curves are further to the right because they can access funds more easily, that is, for banks exhibiting network affiliation or balance sheet liquidity. Consequently, when an upturn in the business cycle stimulates loan supply, lending growth will be smaller amongst that group of banks, because there is less unexploited loan demand to be tapped. This in turn means a smaller offset to any reduction in lending growth from a rise in the federal funds rate that is implemented in response to the cyclical expansion. The result is that the γ estimated from (1) using realized changes in the federal funds rate will be smaller than that estimated using purely exogenous variation in the federal funds rate.

The final case that we consider features heterogeneous loan supply responses to expected macroeconomic conditions. Specifically, banks that face financing constraints, either due to a lack of affiliates, assets or liquidity, and are unable to shield their lending from contractionary open market operations may make greater use of the funds available during cyclical upturns, such that their loan supply increases more when policy is tightened endogenously. Similarly, constrained banks may be more risk averse or deal with customers for whom the external finance premium fluctuates more in response to cyclical conditions. In these instances, expected macroeconomic conditions will boost loan supply by more, limiting the lending reduction esti-

[8]One caveat must be noted here. 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, offsetting the lending increase from a relatively large shift of the loan demand curve.

[9]Freixas and Jorge (2008) model rationing effects in the interbank market.

mated from an endogenously rising federal funds rate. Put slightly differently, lending growth is shielded from monetary policy by the economic forces to which policy is endogenous, and if such shielding effects are more widespread amongst banks that would otherwise be constrained by tight monetary policy, the estimated effects of the constraints (characteristics) on lending responses will be attenuated.

We close this section by noting that these thought experiments raise the possibility that even a purely exogenous monetary policy measure will elicit estimates of γ 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 to other banks. Alternatively, under rational buffer stocking, banks trading with more cyclically sensitive customers may implement a larger leftward shift of loan supply because loans to those customers are relatively more risky after a policy tightening. Certain characteristics are more likely to be prone to such effects than others. For example, Ashcraft (2006) contends that bank holding company status allows a comparison between banks that are similar in terms of other factors that may influence loan supply and loan demand. Such a comparison is less susceptible to confounding effects. 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 will affect 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 affected by the sources of bias discussed previously.

3 Econometric methodology

In this section we outline the methods that we use in comparing bank lending responses to exogenous monetary policy with those to the realized federal funds rate. The discussion comprises three main elements: (i) a description of the identification procedure used to isolate exogenous variation in the federal funds rate; (ii) an explanation of the regression models that we estimate in order to obtain our core results; and (iii) a description of the data used in the empirical work.

3.1 Monetary policy identification

In order to identify exogenous variation in the federal funds rate 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 percentage point 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, for example 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.[10]

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 commenced in 1994, overlapping with the original Romer and Romer (2004) series for 2 years. Even though 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 given that policy intentions have been much more transparent since the mid-1990s. During the overlapping period of 1994-1996, the two series have a correlation that is essentially 1.[11] The extension of the target rate series in this way ensures that we are able to recover exogenous variation in the federal funds rate 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.[12] 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

[10] 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-Mihov (1998) measure of monetary policy, which accounts for changes to the operating procedure, see for example Kashyap and Stein (2000). We do not pursue that option in this paper.

[11] 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 b.p., even though there was no change in the announced, target federal funds rate.

[12] See Federal Reserve (2005), or the International Banking Act of 1978 (the Humphrey-Hawkins Act).

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 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), ε is a mean-zero error term, and a hat denotes the real-time forecast for a variable. Other lowercase Greek letters denote population parameters. Notice that the specification employs a larger set of unemployment forecasts than do 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 (capacity utilization growth is positive and significant). The inclusion of the capacity utilization 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%).[13]

[13]This may also reflect a reduction in the variation in the target federal funds rate over the years 1997-2001.

In order that the regression residuals from equation (2) capture exogenous monetary policy that may be used to estimate 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 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 may be generated from forward-looking variables 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 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 policymakers and are known to perform well relative to alternative forecasts (Romer and Romer, 2000, 2008 and Bernanke and Boivin, 2003).[14] Instances in which the controls in (2) may not eliminate policy movements that are endogenous to lending determinants occur when the Federal Reserve responds to banking sector conditions directly. For instance, 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 shock would be recorded, but this may fail to stimulate lending growth if the crisis prevents 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

[14]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 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, for example the views of an influential FOMC member, does not contain information for loan supply and loan demand beyond that in the Greenbook.

monetary policy than would otherwise have been the case (Ashcraft, 2006); (ii) the Federal Reserve Bank of New York's rescue of U.S. hedge fund Long-Term Capital Management in 1998, which may have induced similar effects. In section 5, we provide evidence from robustness tests indicating that our core results are not affected by these episodes.

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, see 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, and therefore policy shocks 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. 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 through cumulating the post-meeting shocks in a particular quarter, to give a variable that we denote UM.[15] This method is equivalent to defining a daily interest rate level from the cumulated value

[15] 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, but 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.

of all past shocks 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 obtain quarterly changes in the effective federal funds rate, denoted FF.[16] 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 UM 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

In order to evaluate bank lending responses to monetary policy we first estimate regression models of the following form:

$$\begin{aligned} \Delta L_{i,t} &= \alpha + \sum_{k=1}^{4} \lambda_k \Delta L_{i,t-k} + \sum_{k=0}^{4} \beta_k \mathbf{M}_{t-k} + \sum_{j=1}^{J} \zeta_j X_{i,j,t-1} \\ &\quad + \sum_{j=0}^{J} \sum_{k=0}^{4} \gamma_{k,j} X_{i,j,t-1} \mathbf{M}_{t-k} + \mu \cdot t + \sum_{q=1}^{3} S_{q,t} + \varepsilon_{i,t} \end{aligned} \tag{3}$$

where i indexes banks, t indexes quarters, L denotes the natural log of total loans measured at current prices, $\mathbf{M}$ is a vector comprising three U.S. macroeconomic variables, $\mathbf{X}$ is a vector of J bank characteristics, $S_q \forall q = 1, 2, 3$, is a set of seasonal dummy variables equal to 1 in quarter q and zero otherwise, and ε is an error term. The vector of macroeconomic variables M consists of: (i) a monetary policy measure, either UM or FF, as described in section 3.1, (ii) the change in the natural log of GDP at current prices, and (iii) the change in the natural log of the consumer price index (CPI). The vector of J bank characteristics comprises: (i) the natural log of bank assets in millions of dollars, at current prices; (ii) 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 $t-1$)[17]; (iii) the ratio of bank securities to assets; (iv) the ratio of total equity capital to assets; and (v) the ratio of internal cash generation to assets.

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

[17] 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 by requiring that dominant holding company banks support their affiliates during periods of financial stress. Ashcraft (forthcoming) argues that in practice the functioning of internal capital markets improved significantly in 1989, but we focus on the post−1986 period as in Ashcraft (2006).

More detailed variable definitions and information on data sources are provided in section 3.3.

The regression specification in equation (3) is closely related to those employed by Ashcraft (2006) and Loutskina (2005). Bank characteristics lagged once are included as controls, to allow for differences in lending growth contingent on bank size, affiliation and balance sheet composition. The growth and inflation controls in the vector $\mathbf{M}$ account for variations in nominal lending growth arising from drift in prices and economic activity. Interactions between 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 and therefore less likely influenced by lending behavior (the bank holding company indicator is not pre-determined, but this variable 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 $t-1-k$ such that they are also pre-determined with respect to the monetary policy measure, and we consider this case in our robustness tests in section 5. As we discuss in section 5, the results change very little in this case due to the fact that the temporal variation in characteristics across near quarters is small relative to the cross-sectional variation in characteristics.[18]

Second, each of the bank characteristics are adjusted such that their sample mean is zero, except the binary variable for bank holding company status. One year after the quarter in which a 100 b.p. monetary tightening occurs, the term $\sum_{k=0}^{4} \beta_k$ then measures the percentage change in lending for a bank at the sample average of each continuous characteristic and not affiliated with a bank holding company (this overlooks contributions from autoregressive terms, a point to which we return in section 4). The term $\sum_{k=0}^{4} \gamma_{k,j}$ measures the increment to the marginal lending response when characteristic j is 1 unit above the sample average (or a bank is affiliated with a holding company in the case of that characteristic). In section 4 we explain how we account for the fact that 1 unit represents different contrasts depending on the sample

[18] It should be emphasized that while we consider characteristics that are pre-determined for the current lending response to monetary policy, there is 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 than our estimates indicate.

distribution of the characteristic.

Third, the regression includes not only the levels of nominal income growth and inflation, but also 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; (ii) any heterogeneity in the response of real lending growth to macroeconomic factors such as output growth and inflation.[19] As discussed in section 2, controlling for inflation and output growth may account for some endogenous movements in the federal funds rate, and represents the main way in which extraneous movements in loan demand and loan supply have been addressed in previous studies. However, as discussed in section 2, there is no guarantee that such a measure deals with loan demand and loan supply shifts originating from expected future macroeconomic conditions, and which are also correlated with FF. This is the reason we expect to observe differences in results estimated from the identified policy measure, UM, and the broader measure, FF.

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 one of the covariates, total assets, drifts through time whereas other variables are growth rates or ratios, which are likely stationary (we have not performed panel unit root tests due to the short time series available for some banks in the panel). We do not include separate trends to deal with drift in the interactions between macro variables and total assets. In section 5 we implement a different approach to dealing with the drift in total assets that handles 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 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. As a further precaution, we calculate all regression standard errors through clustering at the bank level, as in Ashcraft (2006), to deal with residual heteroscedasticity and autocorrelation of unknown form.[20] One source of uncertainty that our standard errors do not take into account is the first stage regression used to identify UM. Pagan (1984) shows that this uncertainty affects inference based on non-zero

[19] Inflation may affect real lending volumes if loan contracts are not fully indexed against the effects of inflation.

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

null hypotheses, but not inference based on zero null hypotheses.

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 Federal Deposit Insurance Corporation at the end each quarter by all insured banks in the United States. We are grateful to Adam Ashcraft for providing a dataset that constructs variables from these sources using guidelines proposed by Kashyap and Stein (2000). The variable definitions that we now outline follow those used in Ashcraft (2006). The Call Report line numbers used to generate individual series are provided in Kashyap and Stein (2000).[21]

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) and includes loans under commitment for some period (predominantly lines of credit to firms) as well as loans on flexible terms.[22] The correction for loan losses allows for the fact that a bank may reduce its loan book through 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 this effect may be limited because our sample ends in 1999, and includes just a few years from the period since the huge growth in the market for mortgage-backed securities in the mid-1990s. Consistent with this view, we show in section 4 that our results change very little after controlling for the commercial and industrial loan share, which should reflect any data distortions from securitization given that 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 and Assets Held in Trading Accounts. Total Equity Capital is the book value of equity issued

[21]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.

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

plus the cumulated value of retained earnings. Internal cash flow is the sum of net income before extraordinary items and loan loss provisions (the definition of internal cash flow follows Houston, James and Marcus, 1997).[23] 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, see Ashcraft (2006) for full details.

In table 2 we report summary statistics for the bank level variables described here, and also measures of loan composition by customer type (see section 4.2). Summary statistics are calculated using data from three years corresponding to the beginning, middle and end of sample (1977, 1988 and 1999), and for all banks in the baseline estimation sample (see section 3.3.3 below). An inspection of these statistics supports our assertion from section 3.2 that the series are stationary, with the exception of the total assets measure.

3.3.2 Macro data

The monetary policy measures that we compare are described in section 3.1. 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 Bureau of Economic Analysis and were extracted from the Federal Reserve Bank of St. Louis FRED database. The output and price data are period average values in that 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 in the case of 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 federal funds rate on a period average basis that matches the output and inflation concepts.

[23]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 and therefore are proxied through summing 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, with a maximum cross-section of $14,026$ banks. The average number of observations per bank is 56.9. In line with other studies, this sample is obtained after excluding bank/quarter observations affected by mergers, which may induce spurious movements in balance sheet variables (following a merger the merged banks are dropped and a new bank enters the dataset).[24] 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 $2\sqrt{\frac{\textit{slope coefficients}}{\textit{sample size}}}$ defined by 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 cut the sample to $1,053,334$ observations when *UM* is the policy measure and $1,052,453$ observations when *FF* is the policy measure.[25] These differences are minor in the context of the sample size, and we emphasize that our results across *UM* and *FF* do not depend on outlier exclusion–the comparisons in section 4 do not change when using the full sample instead of the trimmed sample.

4 Empirical results

In table 3 we present $\sum_{k=0}^{K}\beta_k$ for $k = 0, 1, .., 4$ and 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 basis point 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 very small (less than .1) and virtually identical across *UM* and *FF* versions of the regression. As such, they will not affect our inferences. We follow Kishan

[24] Due to consolidation of the banking sector, the number of banks falls to roughly 8000 by the end of the sample. See table 2.

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

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 general movements in the 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 – equality of the two effects can be ruled out with high probability. The more powerful lending effect estimated from *UM* is consistent with our basic argument that the effects of endogenous policy movements are obscured by pro-cyclical loan demand and loan supply. The contrast between lending responses to *UM* and *FF* is most stark at the one and two quarter horizons, which 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 thwarting the withdrawal of 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. Based on such findings, we argue that monetary policy identification is an important element of research design in empirical studies of bank lending.

In table 4 we report the sums of cross effects between monetary policy and bank characteristics through horizon 4 (labeled interaction), after setting characteristics to one 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 the space constraint, but are consistent with the UM/FF comparisons developed in what follows. In order to provide context for our results we also reproduce the horizon 4 lending response for the representative bank, as presented in table 3, and present the marginal lending response to a

100 b.p. policy contraction for a bank one standard deviation above each of the characteristics considered,[26] which is the sum of the response at the representative bank and the interaction effect for a particular characteristic.

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. The point that we emphasize is that these shielding effects are much larger when monetary policy is measured using UM as opposed to FF, and that differences in the sizes of the interactions across policy measures remain after imposing two standard error bands around the estimates. Controlling for the endogeneity of monetary policy implies not only more powerful lending responses at the representative bank, but also much 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 may 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 such as 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. The result is that cross effects between monetary policy and characteristics such as holding company status and total assets are smaller when policy is measured using FF as opposed to UM.

These findings have important research implications. Ashcraft (2006) argues that the composition of loan demand by borrower size and creditworthiness varies relatively little with holding company status, 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, a potentially important caveat is that although unaffiliated banks may be subject to a lending channel, the borrowers turned

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

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% in the first year, while the representative affiliated bank raises lending 0.95% over the same period. This evidence is consistent with a redistribution of lending in the aftermath of shocks to bank funding.[27] In order to investigate whether the conflicting loan responses cancel at the aggregate level we re-estimated our baseline regression after excluding all terms from the characteristics vector $\mathbf{X}$, 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% (0.03%) and that from *FF* is 0.17% (0.01%). Despite the compensating effect from affiliated banks it appears that an aggregate transmission mechanism exists. Furthermore, this mechanism is much stronger 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: (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 (see the discussion in 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, but 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 existing estimates indicate. Similarly, our results suggest much 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.[28] The relevance of these

[27] These estimates are from our baseline regression specification and contrast 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.

[28] 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, and it is less clear that such a feature of lending markets

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 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 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% compared to the average bank. In contrast, following a 100 b.p. increase in the overall federal funds rate a bank with securities one standard deviation above the mean *shields* lending by 0.04% relative to the average bank. Both effects are significant at the 1% level. In previous work the shielding effect from securities has been related to the idea that such holdings represent a buffer stock of liquid assets which can be used to substitute lost reserves during policy contractions (Kashyap and Stein, 2000 and Ashcraft, 2006). Our results suggest this interpretation is tenable only when using a measure of monetary policy that responds to expected future growth and inflation. Once these endogenous policy changes are eliminated from the monetary policy measure the shielding effect is eliminated.

One explanation is that banks facing loan demand that is especially sensitive to cyclical upturns (and which therefore decrease lending by less at the time of an endogenous federal funds rate rise) tend to invest more heavily in securities, to smooth their income stream in the face of uncertain lending opportunities. A high securities ratio would then correlate with weaker lending responses to policy measured by the realized federal funds rate. The same is not be true of the exogenous policy measure, which is orthogonal to future cyclical conditions. Indeed, we estimate a negative interaction between the exogenous policy measure and the securities ratio, indicating lending responds more to tight monetary policy amongst banks with a large share of securities in total assets. Why might this be? 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 (Bernanke and Gertler, 1995). Banks with greater exposure to capital losses on securities will be forced to contract lending more aggressively, yielding the amplification effect. In such instances, seemingly liquid assets such as securities exhibit low 'market liquidity', in

could drive heterogeneity in the aggregate transmission mechanism. We implicitly assume that at least part of the asset based interaction is from loan supply.

the sense that the market value of the assets is driven below their fundamental book value as a consequence of the exogenous policy tightening. For this reason banks refrain from liquidating the assets and instead choose to contract their lending base.

The final two rows in table 4 relate to the equity capital and net cash ratios. We consider these two characteristics together given that cash flow contributes to equity capital. However, we emphasize the equity capital ratio given that it is a more comprehensive measure of the liquid funds available to banks. Both characteristics play a role in shielding lending from exogenous policy contractions. A bank one standard deviation above the sample average equity capital ratio shaves one quarter less from its lending following a policy tightening. 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 to funding shocks.[29] Significantly, equity capital is liquid cash on banks' balance sheets, the value of which is not eroded by tight monetary policy (unlike the case of securities). Such funds are available to substitute lost reserves and sustain deposits and loans during periods of tight monetary policy.[30] 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, perhaps reflecting greater resilience to policy contractions amongst banks that are relatively profitable and able to use operating surplus to support lending activities.

Our corresponding results based on the effective 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 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

[29] 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 on controlling for loan composition measures thought to proxy the elasticity of loan demand.

[30] 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 estimate the effect of the equity capital share in assets relative to the sample mean on lending responses to monetary policy. As such, equity capital helps to shield lending growth not when it is high in an absolute sense (which may reflect high lending activity at a large bank), but when it is high relative to assets amongst the population of banks, and therefore likely to indicate excess liquidity relative to the required amount.

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 general movements in the federal funds rate may arise because loan responses are attenuated following endogenous policy changes, and are attenuated to a greater extent amongst poorly capitalized banks who respond more strongly to endogenous policy drivers, similar to our argument above in respect of small or unaffiliated banks.

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 networks such as holding companies may be able to compensate through accumulating 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.[31] 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 open market operations. This in turn may embed a more powerful monetary transmission mechanism during the period in which banks restore capitalization ratios to levels seen prior to the current period of credit market turmoil.[32]

4.1 Stability of the baseline results

An important issue in studies of the transmission of monetary policy to the banking sector is the stability of the results through time (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).[33] The Federal Reserve Board issued a

[31] Of course, other interpretations of this observation are possible. For example, the probability of joining a holding company may be linked to the equity capital ratio.

[32] It should be noted that both interpretations are subject to the caveat stated in the previous footnote.

[33] Another source of structural change is the abolition of regulation Q, which restricted banks' ability to vary interest rates in order to attract deposits, an important 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 explicitly. If observations from this period exert undue influence on the results, the outlier detection procedure we employ ought to diagnose them.

statement in 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. In section 3.2 we argued that following the inception of the source of strength doctrine, from 1987 onwards membership of a bank holding company should condition bank lending responses to monetary policy. Our baseline results are consistent with this idea. In particular, post-1986 the lending reductions at stand alone banks estimated from both exogenous and general movements in the federal funds rate are countered by significant lending increases at networked banks.

In this sub-section we take our analysis of this structural change one stage further through interacting each of the cross terms in $\sum_j \sum_{k=0}^{4} \gamma_{k,j} X_{i,j,t-1} \mathbf{M}_{t-k}$ with the binary variable that is set to unity post-1987 in the case of banks that are part of a holding company (excepting the cross term that already features the bank holding company indicator). These extra terms are added to our baseline regression in (3), allowing the effects of characteristics such as the asset base and balance sheet liquidity on lending responses to monetary policy to vary from 1987 onwards amongst banks affiliated with a holding company. In table 5 we report interaction coefficients for policy measures and characteristics similar to those in table 4, and also the changes to those interaction coefficients associated with the inception of the source of strength doctrine.

The key feature of the results is that post-1986 changes to the interaction coefficients amongst holding company banks are approximately of 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 holdings of securities in amplifying the effects of exogenous policy on lending growth, and equity capital in making lending growth resilient to exogenous tightening of policy, 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 experience funding shocks from write-downs in securities prices or loan values following a policy tightening, they are able to tap loanable funds within the network provided by the holding company, and as a result shield their lending growth.

4.2 Exploring loan composition effects

In this sub-section we incorporate information on the composition of bank loan books in our investigation of lending responses to monetary policy. 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 quite readily in response to funding shocks (Strahan, 2008). Collateral is a more common feature of loans to customers in the residential and consumer sectors, and banks concentrating in those areas may be more able to shield their lending growth from monetary policy via securitization.[34] 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 treat them as additional bank characteristics in our baseline regression in (3). We consider just three elements of the loan decomposition from the Call Reports. The reason is that municipal lending is typically a very small share of total lending. The four remaining loan categories account for almost 100% of bank lending and as such are almost 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

[34]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.

is reported for the representative bank. We also report are the increments to the lending response associated with a loan share measure one standard deviation above the sample average 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 (full details are available on request).

The results in table 6 support the hypothesis that residential lending is less sensitive to monetary policy. The reduction in the sensitivity of lending growth to monetary policy for a bank whose participation in the residential loan market is one standard deviation above the sample average is 0.13 when the policy measure is UM, compared to 0.04 when the policy measure is FF.[35] The evidence for resilience among residential lenders is stronger using the identified policy measure, UM, possibly because loan demand and loan supply shifts, which distort the estimated effects of FF, vary in intensity across different parts of the market for loans. Results based on the exogenous policy measure are less affected by these biases and yield stronger support for the proposition that more widespread securitization of residential loans leads to reduced sensitivity of such loans to monetary policy.

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 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 2000, report a similar result using the effective federal funds rate). On the other hand, the UM measure suggests slightly stronger shielding effects apply to commercial and industrial loans than to consumer loans, even though the former are less readily securitized. Overall, the evidence suggests a less important role for loan securitization in shaping the response of consumer lenders to monetary policy when compared to residential lenders. This may reflect less widespread securitization of consumer loans. One option for future research would be to control for differences in the extent of securitization using a method such as that introduced by Loutskina (2005).

[35]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.

5 Robustness

In this section we report the results of robustness exercises performed for our baseline regression estimates presented in tables 3 and 4. Each of the exercises addresses questions regarding our baseline econometric methodology that have been raised earlier in the paper. 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 are: (i) the tightening of bank capital regulations as part of 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 Long-Term Capital Management (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 $1990-93$ (this follows the dummy variable procedure used by Ashcraft, 2006 to handle the effects of the Basel Accord), and the second equal to unity for all quarters in $1998-99$ (the LTCM rescue occurred in 1998). We then interacted each of these dummy variables with each term from equation (3) that features 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 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 (Loutskina, 2005 motivates a fixed effects lending growth specification based on differences in managerial preferences).[36] The results indicate that our

[36]The inclusion of fixed effects and autoregressive terms raises the possibility of estimation bias of the form discussed by Nickell (1981). The size of this bias declines with the time dimension of the panel, and in our case an average number of time observation per bank of 57 is likely to mean this bias is minimal (Judson and Owen 1999 find quantitatively small bias provided $T \geqslant 30$). Interestingly, the autoregressive coefficients change very

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 (one standard error bands do not overlap). 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 (for example $t-4$). The dating of characteristics in our baseline regressions is standard in the literature, but leaves open the possibility that a characteristic value is a function of the earlier policy change with which it is interacted. The usual interpretation of interaction effects from regressions such as that in (3), namely that characteristics condition bank lending responses to monetary policy, would not be valid in such cases. In order to address this issue we date all characteristics in interaction terms $t-1-k$, such that they are pre-determined with respect to the policy variable with which they are interacted (the level characteristics, 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, save two exceptions. First, the amplification of the contractionary effects of *UM* associated with large holdings of securities is absent in the third set of results, signaling the need for some caution in relation to this finding. The statistically significant negative effect from *UM* in our baseline results is present in the version of the results that uses pre-determined characteristics if we cumulate the interaction terms out to horizon 3 rather than horizon 4, and is present out to horizon 4 in the version of the results that does

little across the baseline and fixed effects specifications (results not reported).

not exclude outliers. These conflicting pieces of evidence indicate some fragility in our baseline estimate of the securities ratio effect, but it seems unlikely that the entire effect is driven by variations in the securities ratio arising from policy changes because signs of the basic result are evident when using pre-determined characteristic measures.

Second, the interactions between the policy measures and the net cash ratio change sign in column 3 relative to the baseline case. Given that net cash flow is a component of the equity capital ratio, part of the negative effects from the net cash ratio in the column 3 specifications are offset. Nevertheless, it is clear that the effect of bank profitability/cash flow on lending responses to monetary policy is more dependent on the dating of the characteristic than is true for other characteristics. This is unsurprising given that the net cash ratio, which is closely linked to profits, is less persistent than total assets and asset composition and therefore changes more through time (the relatively low persistence of this series is reflected in its small standard deviation in 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 (for whatever reason), 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, and instead focus 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-bank mean of assets in period t (in the baseline case we use the demeaned series for $\ln assets_{i,t}$). Scaling each observation by average bank assets at a point in time eliminates the drift in the assets series that occurs as a result of all 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 (Loutskina, 2005). Such a method will account for a linear trend in the assets of a representative bank, but will not deal with changes to the average growth rate for bank assets. Furthermore, the time trend handles drift only in the log level of assets, not the asset terms that are interacted with monetary policy (capturing the latter would require triple interactions between monetary policy measures, bank assets and time trends). In contrast, since we use the alternative definition in all terms that feature assets, including interactions, the new set of results will not depend on the drift that characterizes data for total assets.

The results indicate that our core findings remain intact, 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 in part a function of the upward trend in asset values, which correlates with the trend towards weaker monetary transmission through time – notice how in the absence of this effect 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 banks is one third larger when policy is measured using UM rather than FF, the shielding of lending growth amongst holding companies is 1.5 times larger using the exogenous policy measure, and it is the equity capital ratio, not the securities ratio, that serves as a buffer to lending growth in the presence of monetary policy shocks measured by 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 a large database on the activities of insured banks in the United States in work by Kashyap and Stein (2000). The bulk of this research has used the federal funds rate to measure monetary policy. The key point emphasized in this paper is that such a policy measure is endogenous to expected future macroeconomic conditions, which are likely to exert quite 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 direct impact of monetary policy on bank lending, and the part of the transmission mechanism conditional on bank characteristics. In the empirical part of the paper we provided a comparison of the heterogeneity in bank lending responses to an explicitly identified monetary policy measure, and the measure which is more commonly used in the literature.

The results indicated both economically and statistically significant attenuation of lending responses to monetary contractions, and the shielding of lending associated with bank holding company affiliation. We also estimated sign reversals for some characteristics. Specifically, the share of securities in total assets was shown to amplify policy transmission from exogenous interest rate changes, but restrict 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 limit 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 the largest lending increases (from the factors to which policy is endogenous) at banks which choose to invest heavily in securities. If the latter effect dominates the former, the positive interaction with the securities share may occur using changes to the federal funds rate. The bank characteristic found to associate with shielding of lending growth after policy contractions was the equity capital ratio. Its effect was much stronger using the exogenous policy measure.

We argued that 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 the exogenous policy measure 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 examining the impact of exogenous policy measures on regions, industries and firms, and asking whether or not those impacts, and their comparison with similar results from the realized federal funds rate, are consistent with banking sector structures relevant to those 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, as discussed earlier. Other interesting areas for future work include applying the methodology used here to study heterogeneity in lending rates, as opposed to lending quantities, and examining the effects of bank characteristics on responses to policy at a more disaggregated level, such as holdings of different types of securities.

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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 10% 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 10% 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 10% 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 10% 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 10% 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).