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Federal Reserve Bank of Dallas
Research Department
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Heterogeneous Bank Lending Responses to Monetary Policy: New Evidence from a Real-time Identification[1]

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Abstract

We present new evidence on how heterogeneity in banks interacts with monetary policy changes to impact bank lending, at both the bank and U.S. state levels. Using an exogenous policy measure identified from narratives on FOMC intentions and real-time economic forecasts, we find much stronger dynamic effects and greater heterogeneity in U.S. bank lending responses than that found in previous research based on realized federal funds rate changes. Our findings suggest that studies using realized monetary policy changes confound monetary policy's effects with those of changes in expected macrofundamentals. In fact, estimates from identified monetary policy changes lead to a reversal of U.S. states' ranking by credit's sensitivity to policy. We also extend Romer and Romer (2004)'s identification scheme, and expand the time and balance sheet coverage of the U.S. banking sample.

JEL Classification Numbers: E44; E50; G21

Keywords: Monetary Transmission; Lending Channel; Monetary Policy Identification; Banking

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I. INTRODUCTION

The role of the banking sector in the transmission of monetary policy to the economy has been studied in great detail in both the theoretical and empirical literature (for a review of the various channels of monetary transmission, see Bernanke and Gertler, 1995). If monetary policy is able to influence the supply of bank credit and borrowers have no perfect substitutes for bank-intermediated consumption and investment financing, a bank lending channel for monetary policy can operate (Bernanke and Blinder, 1988).

Following the pioneering work of Kashyap and Stein (2000), a number of empirical studies have explored the heterogeneity of bank-level lending responses to monetary policy. If a bank's characteristics are related to its ability to access non-deposit financing sources, then lending responses to monetary policy are related to bank characteristics. Kashyap and Stein show that banks with relatively large and liquid asset bases are better able to shield their lending growth during periods of tight monetary policy. The same phenomenon has been documented for banks with relatively high equity capital-to-assets ratios (Kishan and Opiela, 2000), banks whose loan books are readily securitized (Loutskina, 2011), banks affiliated to a holding company (Ashcraft, 2006), and banks that can raise funds from international operations (Cetorelli and Goldberg, 2012).

Heterogeneity in bank-level lending responses implies that bank credit at the state level may differ in its sensitivity to monetary policy, since the distribution of bank characteristics differs across states. To the extent that there is some geographic segmentation of capital and credit markets, differences in sensitivities may then lead to differences in the sensitivity of state economies to national monetary policy. However, using aggregate data, Driscoll (2004) and Ashcraft (2006) found little relationship between bank lending and economic growth at the state level.

A fundamental question confronted by any paper looking at the bank lending channel is whether or not any estimated differences in bank-level lending responses linked to a specific bank characteristic are the result of differences in loan supply (as in the lending and broad credit channels), or are a mixture of differences in loan supply and loan demand. There is now an extensive literature that argues that loan demand conditional upon one or more bank characteristics is homogenous.[2] With homogenous loan demands, any heterogeneity in lending responses given bank characteristics is consistent with the existence of a bank lending channel working through loan supply.

[2] See Ashcraft (2006) for a discussion of this evidence.

Much less attention has been devoted in the literature to the question of what measure of monetary policy is appropriate for the assessment of bank lending behavior.[3] Most papers examining the lending channel in the U.S. use the change in the effective (realized) federal funds rate to capture monetary policy, reflecting the fact that the Federal Open Market Committee (FOMC) has targeted the federal funds rate for much of the last 30 years.[4] While federal funds rate changes initiated by the FOMC are surely exogenous to the circumstances facing any single bank, the factors to which policymakers respond (e.g., expected output growth and inflation) are also potential determinants of individual bank lending, operating through *both* loan demand and loan supply changes. 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 other lending drivers is related to bank characteristics, the estimated heterogeneity in bank-level lending responses to monetary policy will be biased. These distortions would also show up in any estimates of state-level differences in the lending channel based on the distribution of bank characteristics.

Motivated by these possibilities, we evaluate the heterogeneity in bank lending responses to target federal funds rate changes that are plausibly exogenous to expected output growth, inflation, unemployment, and capacity utilization. We compare bank and state-level lending responses to the identified policy measure with lending responses estimated from realized federal funds rate changes that have been the focus of most previous research. The identified policy measure elaborates upon and extends earlier work by Romer and Romer (2004). They combined narrative evidence on Federal Reserve target rate intentions with in-house macroeconomic forecasts (the Greenbook) in order to control for endogenous policy changes (policy changes in response to the economy).[5]

Our results highlight four important differences between bank lending responses to exogenous and endogenous components of monetary policy. We offer explanations for these findings in terms of the endogeneity of monetary policy. They provide a new perspective on the measurement of balance sheet liquidity and the consequences of shifts in balance sheet composition for the strength of monetary policy propagation.

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[3] For instance, Jiménez and others (2012, 2013) consider the lending responses of Spanish banks to changes in the short-term policy rate set by the European Central Bank (ECB), which reacts to current and prospective euro area business cycle movements.

[4] See Meulendyke (1998) for an in-depth description of the Federal Reserve's choices of policy instrument over time. Alternative monetary policy measures which have been used in the literature on bank lending include those due to Boschen and Mills (1991, 1995), Strongin (1995), and Bernanke and Mihov (1998). See section 2 for further discussion.

[5] Since June 2010, the Tealbook contains all the components that were previously presented in the Greenbook and Bluebooks.

First, one year after an exogenous monetary contraction, the reduction in lending growth at the average bank which is *not* part of a multi-bank holding company is up to *twice* that from a rise in the realized federal funds rate.

Second, our findings provide a new perspective on measuring liquidity on commercial banks' balance sheet. The share of bank assets held as securities mitigates the lending response to a realized federal funds rate increase, but *amplifies* the lending contraction to an exogenous federal funds rate increase. On the other hand, it is the ratio of cash-to-assets that *shields* lending growth from monetary tightening.

Third, the amount by which a bank can shield its lending growth from a monetary policy contraction, through drawing on funds from affiliates in a holding company, is *up to two times larger* when estimated purely in response to identified, exogenous monetary policy. Bank size appears to shield lending only in response to exogenous monetary policy shocks, but not in response to endogenous changes in the federal funds rate.

Finally, these differences in estimated bank lending responses lead to large differences in the implied sensitivity of lending at the state-level representative banks. The estimated lending responses at state-level median banks to the identified monetary policy measure are up to 400 percent larger than those found when using realized federal funds rate changes. Moreover, the ranking of U.S. states according to the sensitivity of their median bank to policy changes switches, sometimes dramatically. For example, California's median bank is estimated to be the 1^{st} most sensitive to monetary policy when realized federal funds rate changes are used, while it becomes only the 44^{th} most sensitive when the identified monetary policy measure is used. These findings may have important implications for understanding how changes in a national monetary policy stance may affect states or regions differently.

To place our paper in context, it is important to consider how potential biases from confounding monetary policy with other loan demand and loan supply determinants have been handled in previous research. Each of the papers mentioned earlier directly controls for output growth, inflation, or both, in their empirical models of bank lending growth. To the extent that such variables account for the underlying drivers of endogenous monetary policy changes that also affect loan demand and supply, their inclusion in a lending growth regression enables the effects of exogenous monetary policy to be identified. Under the assumption that loan demand is homogenous across banks with similar characteristics, monetary policy's effect on lending through loan supply can be isolated through interactions of monetary policy changes with the relevant bank characteristics.

The starting point for our paper is that current output growth and inflation are not the only sources of endogenous policy—a forward-looking policymaker who desires to minimize cyclical fluctuations will also respond to their perceived prospects for the economy. If the policymaker's economic forecasts correlate with private sector expectations for growth and inflation, then the monetary policy stance can move with loan demand and supply in a manner that is systematically related to observable bank characteristics. For instance, a well-capitalized bank might hold a more cyclically sensitive loan portfolio, so that its lending response to realized federal funds rate changes could partly reflect its direct response to the cycle, rather than its response to monetary policy changes. Our results highlight cases in which this appears to be true. In light of these findings, we argue that future studies of bank lending behavior should take into account the forward-looking component of endogenous monetary policy.

Finally, since our identification of policy shocks relies on real-time FOMC meeting based data that is generally not synchronous with the reporting of bank balance sheets, the direct inclusion of the real-time Greenbook forecasts into any lending regression will not be sufficient to address the endogeneity problem that we highlight. The Federal Reserve's monetary policy meetings typically do not fall at the end-of-the-quarter and are more frequent than commercial banks' regulatory filings (the source of the bank-level data).

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

II. Bank Lending and Monetary Policy

A. Lending Responses to Endogenous Monetary Policy Changes

How might endogenous monetary policy contaminate estimates of the lending channel? Here, we outline the potential biases affecting the estimates in the literature that rely upon the effective federal funds rate to measure monetary policy. In each of the cases discussed, the key idea is that expectations over output growth and inflation affect *both* policy and bank lending choices. Standard lending growth regressions fail to account for this, leading to an omitted variable problem. This biases the estimated response of bank lending to monetary policy changes, even when lending responses are conditional upon bank characteristics, a focus of much recent research. To assess the relevance of these potential biases, we compare bank lending responses to our identified, exogenous monetary policy changes (described in section 3) and to the realized federal funds rate changes that have been used in previous research.

An intuitive alternative to identifying exogenous monetary policy changes would be to directly include the omitted expectations over output growth and inflation in the lending regressions. The key difficulty with this approach is mapping the expectations measure (which is a snapshot of views on future prospects at a particular moment in time) to the quarterly frequency. If expectations are measured late in the quarter, then we would be implicitly using some future information to explain lending earlier in the quarter. If expectations are taken from sometime in the prior quarter, then we would be using stale information, failing to eliminate much of the endogeneity problem. To avoid these problems, we opt to use policymakers' expectations about the economy to form identified, exogenous monetary policy changes, since we can match measures of the policymakers' expectations to specific policy decisions (viz., FOMC meetings). These changes can then be mapped to the quarterly frequency, as described in detail in section 3.A.

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

$$\Delta L_{i,t} = \alpha + M_t'\beta + B_{i,t}'\ \gamma + B_{i,t}'M_t\ \delta + Z\ '_{i,t}\phi + \ \varepsilon_{i,t} \qquad (1)$$

where i indexes banks, t indexes time, ΔL denotes the percentage growth of total loans measured at current prices, M is a monetary policy measure, B is a vector of J bank-specific characteristics, Z is a vector of K control variables, and ε is a mean-zero error term. All other Greek letters denote parameters. In practice, bank lending regressions are much richer than equation (1), typically including autoregressive terms and dynamics in M and B. In section 3, we describe a more complex version of model (1) that incorporates these features. It also will provide the basis for our empirical work. However, the present specification is sufficient to illustrate our argument.[6]

As noted in the introduction, the vector B comprises bank characteristics that proxy access to non-reservable finance (viz., liabilities that do not require reserves or assets on hand). These might include total bank assets, a multi-bank holding company affiliation, an indicator for whether a bank operates internationally, and measures of balance sheet composition, such as equity capital-to-assets, securities-to-assets, or cash-to-assets ratios. In the aftermath of contractionary monetary policy, banks that can access funds via these sources may shield lending growth from the effects of an erosion of reserves and deposits.

What interpretation can be given to the cross effects (interactions) between monetary policy and bank characteristics? If the bank characteristics proxy access to funds that matter for loan supply, then the cross effects (δ) represent how much the bank characteristics help to shield

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

loan supply from monetary policy changes (or amplify its effects). However, many bank characteristics are also correlated with drivers of a bank's load demand. For example, large banks (proxied by equity capital or total assets) 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 and sensitive to the business cycle. In other words, loan supply and demand effects of monetary policy changes conditional on bank characteristics may be confounded.

On the other hand, Ashcraft (2006) presents evidence that bank holding company affiliation is less closely linked to the customer mix and hence loan demand, and thus is preferable as an indicator for loan supply conditions. In this paper, we do not add to this debate. Instead, we consider the wide 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 over much of the post-war period.[7] Increases in the federal funds rate target induce *left*ward shifts of banks' loan supply schedules via the narrow and broad lending channels described in the introduction. These raise lending rates and reduce lending volumes. However, when the federal funds rate target is increased in response to forecasts of higher future economic growth and/or inflation, estimation of this relationship is no longer straightforward. In such circumstances, any loan supply contraction due to tight monetary policy may coincide with a *right*ward shift of loan demand, as consumers borrow against expected future income and firms invest in response to an improving outlook for profits. The loan demand shift will attenuate the reduction in lending from a monetary tightening and the β estimated from equation (1) will not capture the full effect of monetary policy. A similar result may arise via the effects of expected inflation. In particular, reductions in bank lending from a rise in the federal funds rate may be muted because the demand for loans in nominal units rises with expected inflation. As in the example based on expected economic growth, equilibrium lending is subject to countervailing effects from loan demand and loan supply, such that the β estimated from equation (2) is attenuated.

[7] See Meulendyke (1998) for historical evidence on the Federal Reserve's policy tool choices. Alternative policy measures due to Boschen and Mills (1991, 1995) and Bernanke and Mihov (1998) have also been employed in the literature (Kashyap and Stein, 2000). These measures of policy explicitly address possible changes to the instrument of policy through time, but still capture the endogenous stance of policy. As such, we believe that the arguments developed in this section are applicable to them. Loutskina (2005) and Cetorelli and Goldberg (2012) consider Strongin's (1995) identification of exogenous movements in non-borrowed reserves. While this approach controls for reserve demand shocks, it does not control for endogenous policy moves by a forward-looking central bank. Jonas and King (2008) briefly consider the original Romer and Romer (2004) policy measure, which does control for policy endogeneity. However, this is used only as a robustness test in a study that focuses on the impact of bank efficiency on lending responses to general federal funds rate movements. Jonas and King do not consider the consequences of policy endogeneity for lending responses.

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

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

B. Policy Endogeneity and Bank Characteristics

An important question is whether or not pro-cyclical loan demand and loan supply affect the cross effects captured by δ in equation (1) that measure heterogeneity in bank lending responses. As discussed in the introduction, these are the terms that proxy the bank-level financial constraints that underpin the aggregate lending channel of monetary policy. Even if banks are homogeneous and equally affected by expected macroeconomic conditions, the presence of endogenous variation in monetary policy would still attenuate the δ coefficients, via the mechanisms described above.

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

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

Turning to the second possibility listed above, Kashyap and Stein (2000) advocate a rational buffer stocking theory to explain a possible correlation between loan demand curve shifts and bank characteristics. Under the assumption that some banks concentrate their lending in regions or industries that are especially sensitive to aggregate demand conditions, it is rational for such banks to select characteristics that help accommodate volatile loan demand (e.g., multi-bank holding company affiliation or high balance sheet liquidity). When the federal funds rate rises during a cyclical expansion, shifts in individual loan demand curves will be largest amongst banks exhibiting the characteristic in question. The attenuation of lending growth reversals following rises in the federal funds rate would then be largest amongst that category of banks. As in the first case discussed, this effect would manifest as positive bias to the estimate of δ. Evidence that banks with access to liquidity can shield lending growth from Federal Reserve policy would be *over*stated.[8]

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

[8] There is a caveat. Banks trading with cyclically sensitive customers may also face relatively more interest rate elastic loan demand curves, such that drops in loan demand from a rise in the federal funds rate will be larger. This potentially offsets the lending increase arising from a relatively large rightward shift of the loan demand curve due to stronger macroeconomic expectations.

III. ECONOMETRIC METHODOLOGY

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

A. Monetary Policy Identification

To identify exogenous variation in the U.S. monetary policy, we follow and extend the two-step procedure outlined by Romer and Romer (2004), who consider U.S. monetary policy over the period 1969–96. In the first step, narrative evidence is used to determine the size of the federal funds rate change targeted by the Federal Open Market Committee (FOMC) at their scheduled meetings. The advantage of this measure of monetary policy intentions is that during episodes of reserve targeting (e.g., under Volcker's chairmanship of the FOMC), it does *not* respond to supply and demand shocks in the reserve market that are unrelated to monetary policy. In contrast, the effective federal funds rate (the market clearing rate in the reserve market) will respond to such factors.

We extend the original Romer and Romer (2004) target series by appending the FOMC's announced federal funds target rate changes for 1997 to 2007, the last year for which Greenbook forecasts are currently publicly available. Such announcements began in February 1994, overlapping with the original Romer and Romer series for 2 years. Although the announced target series does not capture all of the narrative evidence incorporated in the Romer and Romer (2004) series, we argue that the pooling of the two is defensible, since the transparency of policy intentions and the public announcement of policy changes are strongly related. During the overlapping period of 1994–96, the two series have a correlation that is essentially 1.[9] The extension of the target rate series in this way ensures that we are able to recover exogenous variation in U.S. monetary policy for a longer sample period than that covered by Romer and Romer (2004).[10]

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

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

[10] Romer and Romer (2004) conduct some sub-sample analysis on their estimates, finding that the implied reaction function pre versus post Volcker is not markedly different. Their findings and the results in Orphanides (2003) suggest that pooling over time is appropriate (or at least approximately so).

Reserve.[11] Additionally, we supplement the specification with real-time Greenbook information on manufacturing capacity utilization. The empirical relevance of capacity utilization is emphasized by Giordani (2004), who shows that controlling for such a proxy for actual output relative to potential is crucial for accurate policy identification. In the present application, we treat forecasts of manufacturing 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 real output growth, inflation, and unemployment. Formally, we estimate the following regression:

$$\begin{aligned}\Delta ff_m = \alpha + \beta ff_{m-1} &+ \sum_{l=-1}^{2} \varphi_l^{y}\, \widetilde{\Delta y}_{m,l} + \sum_{l=-1}^{2} \varphi_l^{\Delta y} \left(\widetilde{\Delta y}_{m,l} - \widetilde{\Delta y}_{m-1,l}\right) \qquad (2)\\ &+ \sum_{l=-1}^{2} \varphi_l^{\pi}\, \tilde{\pi}_{m,l} + \sum_{l=-1}^{2} \varphi_l^{\Delta\pi} \left(\tilde{\pi}_{m,l} - \tilde{\pi}_{m-1,l}\right)\\ &+ \sum_{l=-1}^{2} \varphi_l^{n}\, \tilde{n}_{m,l} + \sum_{l=-1}^{2} \varphi_l^{\Delta n} \left(\tilde{n}_{m,l} - \tilde{n}_{m-1,l}\right)\\ &+ \sum_{l=-1}^{2} \varphi_l^{u}\, \tilde{u}_{m,l}^{mfg} + \sum_{l=-1}^{2} \varphi_l^{\Delta u} \left(\tilde{u}_{m,l}^{mfg} - \tilde{u}_{m-1,l}^{mfg}\right) + \varepsilon_m\end{aligned}$$

where m indexes FOMC meetings, ℓ 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, u^{mfg} is the manufacturing capacity utilization index measured in percentage points, and ε is a mean-zero error term. A hat denotes the real-time forecast for a variable. All other lowercase Greek letters denote population parameters. Notice that the specification employs a larger set of unemployment forecasts than Romer and Romer (2004) and additionally includes real-time back-, now- and forecasts of manufacturing capacity utilization.

The results obtained from estimating equation (2) for a sample of 357 FOMC meetings from the period 1969-2007 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. The inclusion of the capacity utilization and additional unemployment terms is also reflected in the regression R^2, which is higher than that for the original Romer and Romer (2004) specification (32% as compared to 28%).[12]

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

[12] This may also reflect a reduction in the relative variability of the target federal funds rate over the years 1997–2007.

Table 1: Policy Identification Regression

		coeff.	s.e.	t-stat.
Intercept		-0.2780	0.7417	-0.3748
Target from last meeting		-0.0270	0.0104	-2.6034
Forecast	-1	0.0012	0.0093	0.1331
Output	0	-0.0122	0.0262	-0.4652
Growth	1	-0.0537	0.0402	-1.3350
	2	0.0576	0.0443	1.3007
	Total Effect	-0.0070	0.0306	-0.2280
Output	-1	-0.0117	0.0227	-0.5153
Growth	0	0.1056	0.0332	3.1829
Revision	1	0.0116	0.0450	0.2573
	2	-0.0483	0.0566	-0.8541
	Total Effect	0.0572	0.0712	0.8028
Forecast	-1	0.0202	0.0206	0.9786
Inflation	0	-0.0274	0.0252	-1.0859
	1	0.0516	0.0393	1.3152
	2	0.0114	0.0416	0.2754
	Total Effect	0.0559	0.0164	3.3970
Inflation	-1	0.0171	0.0355	0.4815
Revision	0	0.0011	0.0381	0.0278
	1	0.0043	0.0621	0.0697
	2	-0.0474	0.0565	-0.8395
	Total Effect	-0.0249	0.0741	-0.3367
Forecast	-1	-0.0440	0.1687	-0.2609
Unemployment	0	0.4077	0.3365	1.2116
	1	-0.3511	0.4227	-0.8307
	2	-0.0753	0.2803	-0.2686
	Total Effect	-0.0627	0.0272	-2.3046
Unemployment	-1	-0.4282	0.3463	-1.2364
Revision	0	-0.3105	0.3080	-1.0080
	1	0.4817	0.4046	1.1905
	2	-0.3235	0.2909	-1.1121
	Total Effect	-0.5805	0.2826	-2.0544
Forecast	-1	-0.0218	0.0496	-0.4386
Manufacturing	0	-0.1091	0.1146	-0.9521
Capacity	1	0.2020	0.1850	1.0919
Utilization	2	-0.0629	0.1093	-0.5757
	Total Effect	0.0082	0.0077	1.0561
Manufacturing	-1	-0.1124	0.0629	-1.7867
Capacity	0	0.1688	0.1226	1.3769
Utilization	1	-0.2145	0.1819	-1.1792
Revision	2	0.1399	0.1187	1.1793
	Total Effect	-0.0182	0.0342	-0.5335
		Obs.	R2	DW
		357	0.3196	1.8407

Note: The sample is all scheduled FOMC meetings from the period March 1969 to December 2007. 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.

In order for the regression residuals from equation (2) to capture exogenous monetary policy that is useful in the estimation of bank lending responses, we require that: (i) the Greenbook forecasts of output, inflation, unemployment, and capacity utilization are not a function of the change in the federal funds rate target; and, (ii) the Greenbook forecasts 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 the next, ruling out this possibility. The future path of policy underlying the Greenbook forecasts is assumed to be appropriate with the achievement of the FOMC's objectives (see Faust and Wright, 2008, for further detail about the Greenbook's policy rate conditioning assumptions). One caveat is that Greenbook forecasts can draw upon forward-looking variables (e.g., asset prices, industry surveys) that embody market expectations over the policy change at the current meeting. In that case, our identification requires that output, inflation, unemployment and manufacturing capacity utilization respond to policy with a sufficiently long lag such that the forecasts in equation (2) are not subject to reverse causation.

The second assumption is crucial to exclude policy movements that may lead to biased estimates of bank level lending responses to monetary policy. The Greenbook forecasts are a natural means to achieving this objective because they represent the real-time information available to policy-makers and are known to perform well relative to alternative forecasts (see Romer and Romer, 2000, Romer and Romer, 2008, and Bernanke and Boivin, 2003, for evidence).[13] Instances in which the controls in equation (2) may not eliminate policy movements that are endogenous to lending determinants occur when the Federal Reserve responds to banking sector conditions directly. If concerns over bank liquidity prompt the Federal Reserve to keep interest rates on hold even when Greenbook forecasts point to higher interest rates, a negative monetary policy change would be recorded. However, this may fail to stimulate lending growth if liquidity concerns prevent banks from doing new business. In terms of the present application, the banking crisis that followed the collapse of the sub-prime housing market in 2007 is excluded from the sample. However, two other relevant episodes are included in the sample: (i) the years surrounding the Basel I Accord (agreed in 1988 and implemented in 1992), which is often argued to have prompted bank balance sheet adjustment and a looser monetary policy than would otherwise have been the case (Ashcraft, 2006); and, (ii) the Federal Reserve

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

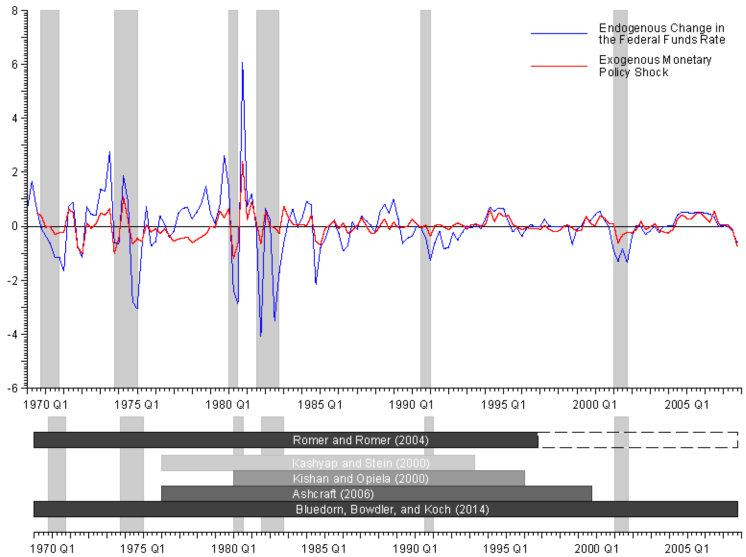
Bank of New York's rescue of U.S. hedge fund Long-Term Capital Management (LTCM) in 1998, which may have induced similar effects. In section 5, we provide evidence that our core results are not affected by these episodes.

For any identification scheme, a natural question is: what are the sources of the policy shocks estimated from equation (2)? A key element is likely to be the idiosyncratic component of FOMC member interest rate choices. 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, 2011, 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 might be warranted given the available forecasts (Romer and Romer, 2004). Alternatively, FOMC membership may change such that policymaker preferences favor tighter or looser policy irrespective of the cyclical position. In other situations, policymakers may feel obliged to validate market beliefs over policy, even when such beliefs are incorrect (Christiano, Eichenbaum and Evans, 1999). It is these federal funds rate adjustments, driven by errors and preference shifts, that we use to obtain estimates of bank lending responses to monetary policy.

The data on bank lending that we use in our empirical work are reported on a quarterly basis. Thus, monetary policy changes defined at the frequency of FOMC meetings, which currently take place eight times per annum, must be aggregated to the quarterly frequency. The appropriate method of aggregation depends critically on whether the data to be studied are measured on a quarter-average or quarter-end basis (see Bluedorn and Bowdler, 2011, for relevant discussion). In the present application, bank-level data are drawn from end-of-quarter reports filed with the Federal Deposit Insurance Corporation (FDIC). Balance sheet data are reported for the final day of a quarter and banks have up to 30 days in the following quarter to confirm the figures reported. We first sum the meetings-based monetary policy shocks on daily basis to obtain a "pseudo" level of the monetary policy shocks. We then average this level for each quarter to account for the timing of the FOMC meetings within the quarter and take the between quarter difference in these average and denote difference as UM. Similarly, for the effective federal funds rate we obtain quarterly averages of the daily rate and denote the difference between these quarterly averages FF.[14] In Figure 1, we present time series plots for UM and FF. During the sample 1969 Q2 to 2007 Q4 the standard deviation of UM is 41 basis points and that of FF is 198 basis points, suggesting that roughly four-fifths of 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 0.44.

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

Figure 1: Endogenous and Exogenous Monetary Policy Shocks



B. Regression Specification

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

$$\Delta L_{i,t} = \alpha + \sum_{l=1}^{4} \rho_l \Delta L_{i,t-l} + \sum_{m=1}^{3}\sum_{l=0}^{4} \beta_{m,l} M_{m,t-l} + \sum_{k=1}^{5} \gamma_k B_{k,i,t-1}$$
$$+ \sum_{m=1}^{3}\sum_{k=1}^{5}\sum_{l=0}^{4} \delta_{m,k,l} B_{k,i,t-1} M_{m,t-l} + \mu t + \sum_{q=1}^{3} \phi_q S_q + \varepsilon_{i,t} \quad (3)$$

where i indexes banks, t indexes time in quarters, $\Delta L_{i,t}$ denotes the percentage growth of total loans measured at current prices, M is a vector of $m = 3$ macroeconomic variables (described below), B is a vector of $k = 5$ bank characteristics (described below), S_q is a set of seasonal dummy variables equal to 1 in quarter q and zero otherwise, and ε is a mean-zero error term.

The components of vector M are:

1. a monetary policy measure, either UM or FF, as described in section 2;
2. real GDP growth in percentage points;
3. growth in the PCE core price index in percentage points.

We present two versions of the regressions: (i) a less noisy version based on year-over-year percentage growth in lending ($\Delta L_{i,t}$) and the non-policy macroeconomic controls and (ii) one version based on annualized quarter-over-quarter percentage growth in lending ($\Delta L_{i,t}$) and the non-policy macroeconomic controls. We also include bank specific quarterly dummies to account for any seasonality.

The vector of B bank characteristics comprises:

1. the bank size percentile within a given quarter;
2. an indicator variable set to unity post-1986 if a bank is part of a bank holding company and zero otherwise (following Ashcraft (2006) this characteristic is dated t rather than $t-1$);[15]
3. the ratio of bank securities to assets;
4. the ratio of total equity capital to assets;
5. the ratio of cash to assets.

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

The regression specification in equation (3) is closely related to those employed by Ashcraft (2006) and Loutskina (2011). Once-lagged bank characteristics are included as controls, to allow for differences in lending growth conditional upon bank size, holding company affiliation, and balance sheet composition. The growth and inflation controls in the vector M account for variations in nominal lending growth arising from contemporaneous changes in prices and economic activity. Interactions between the macroeconomic variables and bank characteristics capture heterogeneity in bank lending responses to monetary policy, income growth, and inflation.

There are three points that we highlight in relation to equation (3). First, the interactions between macroeconomic variables and bank characteristics feature measures of characteristics dated $t-1$, except in the case of the multi-bank holding company dummy which is dated t. As such, lending decisions in period t are conditional on characteristics that are pre-determined. They are thus less likely to be influenced by current lending behavior. The multi-bank holding company indicator is not pre-determined, but it is not derived from the bank balance sheet. This structure mirrors that in Ashcraft (2006) and Loutskina (2011). A natural alternative would be to date interacted characteristics $t-\ell-1$ such that they are also pre-determined with respect to the monetary policy measure. We consider this case in our robustness tests in section 5. As we discuss there, the results change very little due to the fact that the variation in characteristics across quarters close in time is small relative to the cross-sectional variation in characteristics.[16]

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

[16] While we consider characteristics that are pre-determined for the current lending response to monetary policy, we make no claim to have identified exogenous variation in characteristics. In line with most of the literature, we do not model bank characteristics. The determinants of characteristics may include the properties of previous monetary policy regimes, raising the possibility that the effects of policy on bank lending are more complex than our estimates indicate. It could even be the case that past values of a bank characteristic are endogenous to current monetary policy (e.g., via an expectations effect). Any resulting estimation biases are likely to be less important in the case of UM than in the case of FF, because the former is less easily predicted due to its orthogonality to economic forecasts.

Second, each of the bank characteristics ratios, except the binary variable for multi-bank holding company status, are demeaned by sample quarter and normalized by the standard deviation. The bank size controls as measured by total assets are normalized by computing the within-quarter bank size percentile and subtracting 50, such that a bank that is large relative to its peers is "large" even after accounting for the fact that bank assets grew faster (and potentially in a non-linear fashion) than GDP. Thus, the first component of the vector $\sum_{\ell=0}^{4} \beta_{\ell}$ measures the percentage change in lending a year after a 100 basis point (b.p.) monetary policy contraction for a median-sized, unaffiliated bank at the sample mean of each ratio characteristic (this overlooks contributions from autoregressive terms, a point to which we return in section 4). The k^{th} component ($k \leq 5$) of the vector $\sum_{\ell=0}^{4} \delta_{1,k,\ell}$ measures the increment to the marginal lending response to a monetary contraction ($m = 1$) when the k^{th} characteristic is one standard deviation above the sample mean for the ratios, one percentile larger than the median-size bank or a bank is affiliated with a holding company in the case of MBHC affiliation.

Third, in addition to the levels of real income growth and inflation, the regression includes a full set of interactions between those variables and bank characteristics. This ensures that heterogeneity in bank lending responses to monetary policy is estimated after controlling for: (i) purely nominal effects on lending growth from inflation; and, (ii) heterogeneity in the response of real lending growth to macroeconomic factors like current output growth and inflation.[17]

The final elements of the regression specification are a set of bank-specific seasonal dummies.[18]

The maximum lag order in the benchmark regression specification is 4, which is typical of micro bank lending regressions using quarterly data (see *inter alia* Kashyap and Stein, 2000, Ashcraft, 2006, and Loutskina, 2011). Lags in the dependent variable control for serial correlation in the data that is not eliminated by the control variables. Similar to Ashcraft (2006), we calculate all regression standard errors through clustering at the bank-level to deal with any residual heteroscedasticity and autocorrelation of unknown form.[19] One source of uncertainty that our standard errors do not take into account is the first stage regression used to identify UM. However, Pagan (1984) demonstrated that this uncertainty only affects inference based on non-zero null hypotheses – inference based on zero null hypotheses remains valid.

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

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

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

C. Data

Bank-Level Data

Our bank-level data are from the *Reports of Condition and Income* ("Call Reports") usually submitted to the FDIC at the end of each quarter by all insured banks in the United States.[20] One major contribution of this paper is an extension of the banking level sample back to 1969, and up to 2007 (Figure 2). In order to prevent window-dressing, historically U.S. commercial banks were "called" at surprise dates. Banks have reported consistently exactly at the end of the quarter only from 1975. Thus, for the beginning of the sample prior to 1975 there are some irregularities. Figure 3 shows the regular benchmark timing in the bottom and the actual call dates in the top of the panel. As you can see, there are some minor irregularities, for instance the dates varies slightly by some business days around the end of the quarter and for some instances reporting is semi-annual rather than quarterly. Two additional assumptions are necessary in order to make good use of that earlier data. First, we assume that the timing does not matter and merely induces measurement error in the variables that is not a function of any of the other controls (Figure 4). Second, we interpolate bank level variables for the few quarters that data were missing, inducing measurement error in both the dependent and independent variables (Figure 5). Again, as long as these are not systematically related to other controls this merely increases the variance of the estimators.

Otherwise, the variable definitions that we outline follow those used in Ashcraft (2006). The Call Report line numbers used to generate individual series are provided in Kashyap and Stein (2000).

The dependent variable is derived from a series for total loans minus allowances for loan losses. It includes loans under commitment for some period (predominantly lines of credit to firms), as well as loans on flexible terms.[21] The correction for loan losses allows for the fact that a bank may reduce its loan book by writing off bad loans, as well as through varying the supply of new credit. However, as discussed by Ashcraft (2001) and Peek and Rosengren (1998), our measure of loans does not control for loans being moved off bank balance sheets via securitization.

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

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

Figure 2: Macroeconomic Controls During the Sample

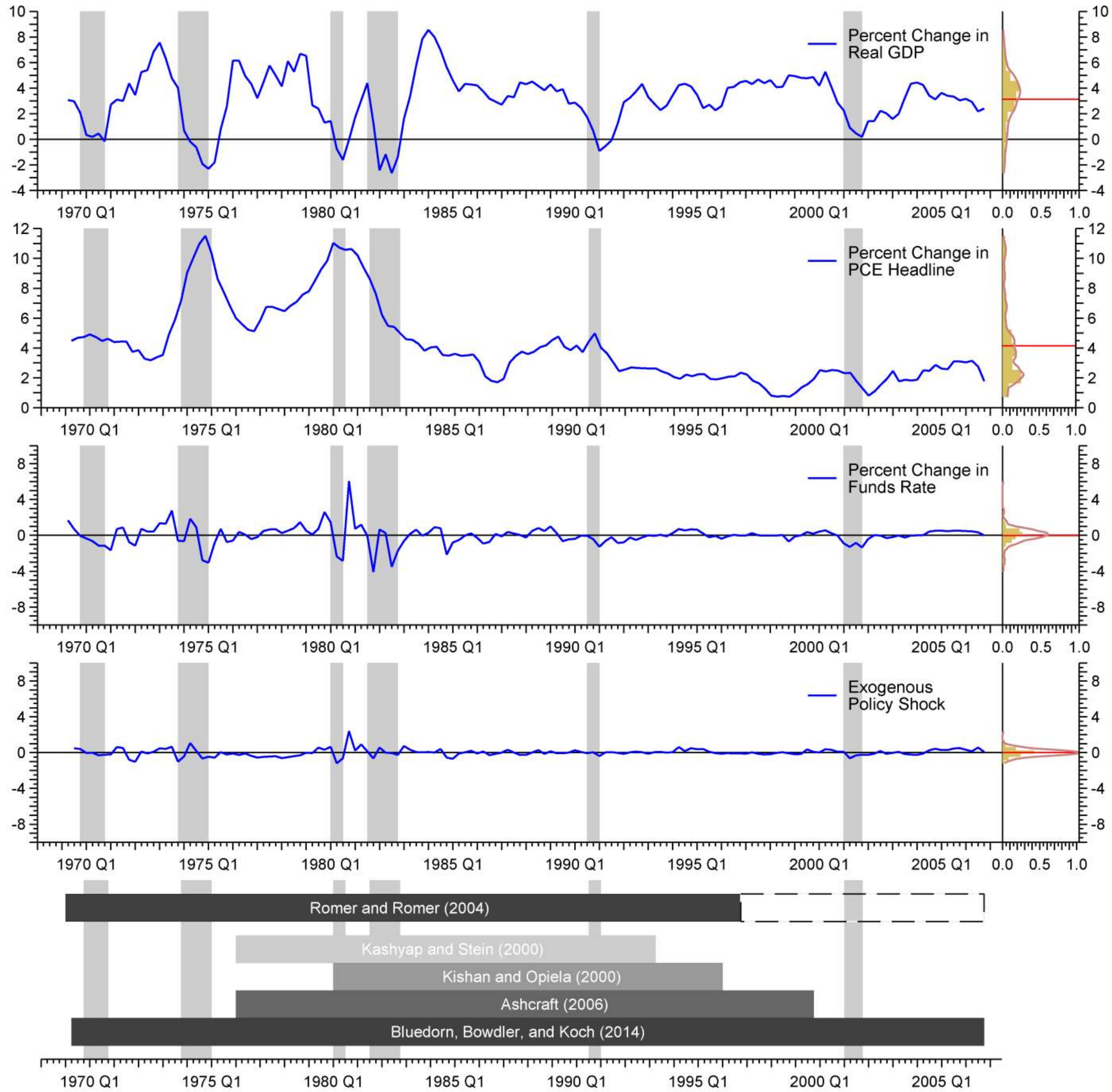


Figure 3: Imperfectly Measured Variables Prior to 1975

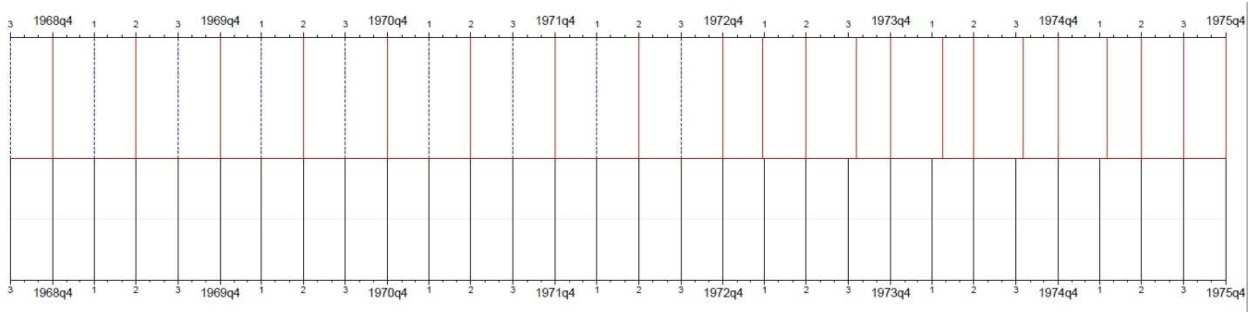


Figure 4: Assumptions Regarding Timing

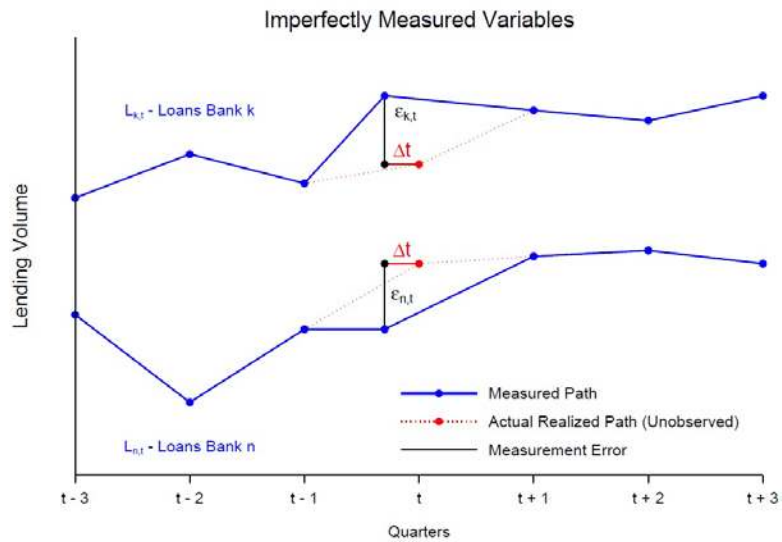
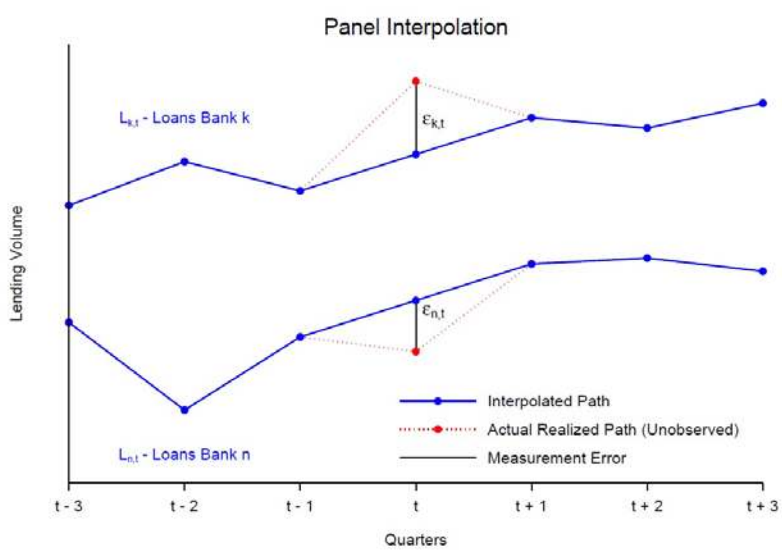


Figure 5: Assumptions Regarding Interpolation



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 (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 plus the cumulated value of retained earnings. Cash is cash on the asset side of the balance sheet. [22] 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.

The dataset used for our baseline estimations is an unbalanced quarterly panel spanning 1969 Q3 to 2007 Q4. It features a maximum of 15,306 banks and a minimum of 7,922 banks. The average number of observations per bank is 112 quarters or about 28 years. In line with other studies, this sample is obtained after excluding bank/quarter observations affected by mergers, since they may induce spurious movements in balance sheet variables (following a merger the merged banks are dropped and a new bank enters the dataset.[23]

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 $2 \cdot \sqrt{K/N}$, where *K* is the total number of explanatory variables and *N* is the overall sample size (Welsch and Kuh, 1977). The number of observations excluded depends on whether the regression is fitted using UM or FF. Specifically, from a total sample of 1,435,713 observations the outlier exclusion reduces the sample to 1,435,420 observations when UM is the policy measure and 1,435,435 observations when FF is the policy measure.[24] These differences are minor in the context of the sample size. The comparisons presented in the next section are observed when using either the full or trimmed samples.

In Table 2, we report summary statistics for the bank-level variables. Summary statistics are calculated using data from four years corresponding to the end of each decade (1970, 1980, 1990, and 2000), for all banks in the baseline estimation sample. An inspection of these statistics

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

[23] Due to consolidation of the banking sector, the number of banks falls to roughly 8,000 by the end of the sample - see Table 2.

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

supports our treatment of the series as stationary, with the exception of the total assets measure (see the description of the bank size normalization in section 3.B).

Macroeconomic Data

The series for income growth is constructed from seasonally adjusted real GDP, and that for the inflation rate is from the seasonally adjusted headline personal consumption expenditure price index (PCE). Both series are from the U.S. Bureau of Economic Analysis (BEA) and were extracted from the Federal Reserve Bank of St. Louis's FRED database. The output and price data are period average values. They refer to a flow of transactions within a particular quarter, whereas our bank-level data are end-of-quarter values from stock concepts on balance sheet statements, although our dependent variable lending growth refers to a (net) flow of transactions. 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. Apart from these two non-policy variables, two different policy controls FF and UM were described in section 3.B. Figure 2 displays time-series and data summaries of all macroeconomic controls.

Table 2: Bank Level Summary Statistics

	1970		1980		1990		2000	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Loan Growth (in %)	9.94	21.45	3.53	13.92	7.75	36.48	12.01	28.95
Assets (in US$ thds)	39,110	482,340	113,460	1,697,992	181,063	2,097,823	263,195	2,815,227
Multi-Bank Holding Company Status (in %)	6.91	25.37	15.43	36.13	28.82	45.29	24.45	42.98
Loans/Assets (in %)	47.86	11.00	53.70	11.17	53.35	15.21	61.70	14.38
Deposits/Assets (in %)	89.02	2.96	88.49	4.05	88.58	6.57	84.01	8.52
Securities/Assets (in %)	34.70	11.69	29.24	11.07	30.28	15.27	26.24	13.66
Equity Capital/Assets (in %)	8.20	2.41	8.58	2.36	8.81	3.81	10.44	4.39
Cash/Assets (in %)	12.31	5.70	9.18	5.22	7.25	5.28	4.94	3.86
Number of Banks	12,484		13,116		9,831		6,434	

IV. EMPIRICAL RESULTS

In Table 3, we present $\sum_{l=0}^{L} \beta_l$ for $L \in \{0,1,2,3,4\}$ and their associated standard errors for the two policy measures UM and FF. These statistics measure the percentage change in lending at various horizons following a 100 b.p. tightening at a bank that has the sample average balance sheet characteristics and is *not* affiliated with a holding company (we refer to such a bank as the representative bank). The full lending response also depends on the autoregressive parameters, but each of these is small (less than 0.1) and virtually identical across UM and FF versions of the regression. As such, they do not affect our inferences. We follow Kishan and Opiela (2000), Loutskina (2011) and Ashcraft (2006) in reporting the direct effect of policy on lending.

At each of the horizons considered, the lending reduction estimated from an exogenous monetary policy contraction exceeds that from a policy contraction measured by the realized federal funds rate. Furthermore, the precision associated with our estimates is such that 95% confidence intervals for the two estimates are non-overlapping at all horizons beyond the current quarter. The inertia in aggregate lending estimated from FF has been attributed to factors such as loans under commitment, which may thwart the withdrawal of bank credit to firms—see Bernanke and Blinder (1992), Morgan (1998) and Kishan and Opiela (2000). While such a possibility is plausible, our estimates suggest that at least part of the sluggishness in bank lending behavior is attributable to policy changes that are endogenous to other macroeconomic fundamentals. Controlling for extraneous loan demand and loan supply movements that may be linked to these fundamentals reveals quantitatively more important monetary transmission mechanism via credit markets.

Table 3: Comparison of Dynamic Lending Response of a Representative Bank

	Year-over-Year		Quarter-over-Quarter	
	FF	UM	FF	UM
t=0	-0.1056***	-0.6336***	0.0656	-0.0963
	(0.0384)	(0.0922)	(0.0408)	(0.0877)
t=1	-0.4730***	-1.7115***	-0.6675***	-1.8729***
	(0.0739)	(0.1647)	(0.0495)	(0.1114)
t=2	-0.5734***	-2.0869***	-0.8532***	-2.0735***
	(0.1061)	(0.2204)	(0.0749)	(0.1511)
t=3	-0.8793***	-2.4877***	-1.4203***	-2.2280***
	(0.1300)	(0.2662)	(0.0942)	(0.1763)
t=4	-0.9862***	-2.7069***	-1.6890***	-2.4433***
	(0.1448)	(0.2924)	(0.1154)	(0.2195)
R2	0.3548	0.3493	0.1040	0.1123
Observations	1,435,422	1,435,435	1,467,813	1,467,762

A. Effects of Bank Size and Holding Company Status

In Table 4, we report the sums of cross effects between monetary policy and bank characteristics through horizon 4 (labeled interaction) when characteristics are set at 1 standard deviation of their sample distribution (except in the case of the multi-bank holding company indicator which is set to unity). Sums of coefficients for other horizons are not reported given space constraints. However, they are consistent with the UM/FF comparisons developed below. To provide some context for our results, we also reproduce the horizon 4 lending response for the representative bank, as seen in Table 3. We consider the marginal lending response to a 100 b.p. policy contraction for a bank that is one standard deviation above the sample mean for each of the characteristics considered. This is the sum of the response at the representative bank and the interaction effect for a particular characteristic.[25]

We first focus on the results for total assets and the bank holding company indicator. In the case of UM bank size as measured by the normalized asset size percentile of the bank within the quarter, the effect of size is unambiguously a shielding of bank lending responses. For instance for a bank that is ten percentiles above the median lending would shrink by 0.26 b.p. (=10x0.0267) less in response to a UM contraction by 100 b.p.. For the MBHC indicator, in both cases, the sums of the interaction terms are positive, indicating that this characteristic helps banks shield their lending growth from policy contractions. However, these effects are much *larger* when monetary policy is measured using UM as opposed to FF. Controlling for the endogeneity of monetary policy implies not only more powerful lending responses at the representative bank, but also a *greater* dispersion in lending responses across the population of banks. This is consistent with our argument in section 2 that lending responses to the endogenous drivers of policy likely correlate with bank characteristics. In the present case, it appears that lending by small banks and banks not affiliated with holding companies is more responsive to factors like expected economic growth, such that lending responses to monetary policy are attenuated to a greater extent amongst banks exhibiting such characteristics. As discussed in section 2, a possible reason for this is that cyclical upturns provide access to finance that is used more intensively by banks that cannot access other sources of funds.

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

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

estimated from the exogenous policy measure raises the possibility that the lending channel is quantitatively more important than previously believed.

Table 4: Heterogeneity in Lending Responses due to Bank Characteristics

		Year-over-Year		Quarter-over-Quarter	
		FF	UM	FF	UM
Policy	Marginal Effect	-0.9862***	-2.7069***	-1.6890***	-2.4433***
		(0.1448)	(0.2924)	(0.1154)	(0.2195)
Assets	Interaction	-0.0061**	0.0267***	0.0007	0.0254***
		(0.0029)	(0.0052)	(0.0048)	(0.0088)
	Marginal Effect	-0.9923***	-2.6802***	-1.6883***	-2.4179***
		(0.1447)	(0.2901)	(0.1154)	(0.2218)
MBHC	Interaction	4.0538***	5.8789***	5.9909***	7.3098***
		(0.4465)	(0.7361)	(0.4580)	(0.8958)
	Marginal Effect	3.0676***	3.1719***	4.3019***	4.8664***
		(0.3600)	(0.5711)	(0.4521)	(0.8719)
Securities	Interaction	0.3038***	-0.9569***	0.0451	-1.1323***
		(0.1131)	(0.1840)	(0.1575)	(0.3327)
	Marginal Effect	-0.6824***	-3.6639***	-1.6440***	-3.5757***
		(0.1383)	(0.3843)	(0.1858)	(0.4445)
Capitalization	Interaction	0.1973	0.7647***	0.7084*	0.4406
		(0.1903)	(0.2581)	(0.3832)	(0.5868)
	Marginal Effect	-0.7889***	-1.9422***	-0.9806**	-2.0028***
		(0.2633)	(0.3985)	(0.4271)	(0.6476)
Cash	Interaction	0.5214***	1.2819***	0.0163	1.0983***
		(0.1052)	(0.1790)	(0.1645)	(0.3981)
	Marginal Effect	-0.4648***	-1.4251***	-1.6727***	-1.3450**
		(0.1512)	(0.3027)	(0.2188)	(0.5318)
	R2	0.3548	0.3493	0.1040	0.1123
	Observations	1,435,422	1,435,435	1,467,813	1,467,762

As discussed by Ashcraft (2006), an important caveat is that although unaffiliated banks may be subject to a lending channel, the borrowers turned away from such banks may be accommodated by bank holding company networks, whose funds fill the gap in the market. The aggregate lending channel of monetary policy could then be weak or non-existent. Our estimates based on the year-over-year changes indicate that after an exogenous policy contraction the representative unaffiliated bank reduces lending 2.71 percentage points in the first year, while the representative affiliated bank raises lending 3.17 percentage points over the same period. This evidence is consistent with a redistribution of lending in the aftermath of shocks to bank funding.[26]

The much sharper heterogeneity in bank lending behavior from UM may help explain two important features of the aggregate transmission mechanism. These are: (i) the different effects of policy across regions and industries (Carlino and Defina, 1998); and, (ii) a possible trend towards weaker propagation of monetary policy in recent decades (Boivin and Giannoni, 2002). Ashcraft (2006) presents weak evidence that state level lending responses to federal funds rate rises depend on the proportion of loans issued by affiliated banks. However, he finds that similar effects do not carry over to state income responses. The larger cross effects that we estimate from exogenous monetary policy suggest that much more of the heterogeneity in the aggregate effects of monetary policy may be attributable to banking sector structure than previous estimates suggest. Similarly, our results suggest that there is more scope for banking sector consolidation and the growth of bank holding companies to account for possible trends towards a weaker aggregate monetary transmission mechanism in recent decades.[27]

The relevance of these conjectures depends on the precise configuration of banking sector characteristics. Specifically, a region or episode associated with a banking sector dominated by holding companies must *not* be associated with other characteristics that reverse the impact of holding company affiliation on lending responses. We address this question in more detail in section 4.D.

[26] These estimates are from our baseline regression specification, which contrasts affiliated and non-affiliated banks, assuming all other characteristics remain unchanged. It is of course possible that the switch to multi-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.

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

B. Effects of Balance Sheet Composition

The most striking result that we present in Table 4 relates to the securities-to-assets ratio. Following a 100 b.p. increase in the exogenous policy measure, a bank with securities one standard deviation above the mean *reduces* lending by a further 0.96 percentage points compared to the representative bank in the year-over-year model and by a further 1.13 percentage points in the quarter-over-quarter model. In contrast, following a 100 b.p. increase in the realized federal funds rate, a bank with securities one standard deviation above *shields* lending by 0.28 percentage points relative to the average bank in the year-over-year model. In the case of the quarter-on-quarter model, no meaningful shielding effects model can be found. In previous work, the shielding effect from securities has been related to the idea that such holdings are a buffer stock of liquid assets which can be used to substitute lost reserves during policy contractions (Kashyap and Stein, 2000, and Ashcraft, 2006). Our results suggest the empirical support for such an interpretation comes from a confounding of expected future growth and inflation with the monetary policy stance.

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

In marked contrast cash holdings of a bank do shield banks from monetary contraction with estimates of the shielding effect for a bank that is one standard deviation above the mean in terms of its cash holding ranging from 1.28 in the year-over-year model to 1.10 percentage points in the quarter-over-quarter model. Unsurprisingly, equity capital shields bank lending responses to monetary shocks, but much stronger evidence of that is found based on the UM measure.

C. Stability of the Baseline Results

An important issue in any study of monetary policy transmission to the banking sector is the temporal stability of the results—see Bernanke and Blinder (1992), Kashyap and Stein (2000) and Ashcraft (2006). In our sample, an important structural change may arise from the introduction of the source of strength doctrine (Ashcraft, 2006).[28] The Federal Reserve Board

[28] Another source of structural change is the abolition of regulation Q, which restricted banks' ability to vary interest rates in order to attract deposits (a source of funding). The abolition of this restriction was largely implemented via the Monetary Control Act of 1980, and is therefore likely to induce heterogeneity in our results across a much

(continued…)

issued a formal statement in April 1987 indicating that failure by a parent bank to inject liquidity into a financially distressed subsidiary when funds are available would be considered an unsafe banking practice.[29]

In section 3.B, we argued that from 1987 onwards membership in a bank holding company should affect lending responses to monetary policy. Our baseline results are consistent with this idea. In this sub-section, we take our analysis of the effects of the source of strength of doctrine one stage further. We interact each of the cross terms in $\sum_{m=1}^{3}\sum_{k=1}^{5}\sum_{l=0}^{4}\delta_{m,k,l}B_{k,i,t-1}M_{m,t-l}$ with the binary variable that is set to unity post-1986 for banks that belong to a multi-bank holding company (excluding the cross term that already features the holding company indicator). These extra terms are added to our baseline regression in (3). In Table 5, we report interaction coefficients for policy measures and characteristics (similar to those in Table 4), in addition to the changes to the interaction coefficients associated with the start of the source of strength doctrine.

The key feature of the results is that the post-1986 changes to the interaction coefficients (amongst holding company banks) are of mostly opposite sign to the main interaction effects. During the late 1980s and the 1990s, the principal source of heterogeneity in lending responses to monetary policy is affiliation with a multi-bank holding company, not balance sheet composition. The roles of security holdings in amplifying and cash in mitigating the effects of exogenous policy on lending growth, are quantitatively smaller from the late 1980s onwards because they are observed *only* amongst banks that cannot access the financing networks provided by holding companies. In contrast, when affiliated banks face write-downs in securities prices or loan values following policy tightening, they are able to tap loanable funds within the network, thus shielding their lending growth in response to negative UM shocks.

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shorter period than the source of strength doctrine. Due to the limitations in estimating heterogeneity in our results across a period of just three years or so, we do not address the effects of Regulation Q. If observations from this period exerted undue influence on the results, the outlier detection procedure we employ ought to diagnose them.

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

Table 5: Bank Holding Company Lending Responses

		Year-over-Year		Quarter-over-Quarter	
		FF	UM	FF	UM
Policy	Marginal Effect	-1.0498***	-2.8627***	-1.6444***	-2.3081***
		(0.1558)	(0.3063)	(0.1234)	(0.2161)
MBHC Status	Interaction with policy	3.7764***	6.0905***	6.1698***	6.3914***
		(0.5036)	(0.8439)	(0.5175)	(0.9573)
Assets	Interaction with policy	0.0013	0.0351***	0.0121**	0.0391***
		(0.0033)	(0.0059)	(0.0050)	(0.0086)
	with policy and MBHC	-0.0353***	-0.0904***	-0.0279*	-0.1201***
		(0.0134)	(0.0224)	(0.0147)	(0.0298)
Securities	Interaction with policy	0.1638	-1.2245***	-0.3144*	-1.3552***
		(0.1294)	(0.1990)	(0.1907)	(0.2825)
	with policy and MBHC	-0.4166	1.9778***	-0.3872	3.5101***
		(0.4029)	(0.7283)	(0.5105)	(0.9726)
Capitalization	Interaction with policy	0.2293	0.8563***	0.8647**	1.0356**
		(0.2128)	(0.2670)	(0.3668)	(0.4488)
	with policy and MBHC	-2.4889**	-2.5520	-2.4705	-9.5964***
		(1.2689)	(2.5924)	(1.7674)	(2.9240)
Cash	Interaction with policy	0.5354***	1.2148***	0.0477	1.7158***
		(0.1210)	(0.1843)	(0.1698)	(0.3428)
	with policy and MBHC	-0.1744	0.3324	1.4900**	-2.0616
		(0.4486)	(0.8372)	(0.6559)	(1.8701)
	R2	0.3209	0.3187	0.0974	0.0997
	Observations	1,434,644	1,434,694	1,459,627	1,459,084

Table 6: U.S. States Banking Structures

State	Assets		MBHC		Securities		Equity		Cash	
	Value	Rank	Value	Rank	Value	Rank	Value	Rank	Value	Rank
Alabama	0.55	37	0.14	36	0.19	5	-0.00	5	-0.25	30
Alaska	39.87	1	0.12	39	0.00	14	-0.26	34	0.17	2
Arizona	19.25	18	0.31	2	-0.97	49	-0.59	51	0.08	8
Arkansas	3.92	35	0.23	10	0.09	9	-0.07	9	-0.20	26
California	24.50	14	0.08	48	-0.97	48	-0.39	44	0.13	6
Colorado	-5.83	42	0.30	3	-0.44	32	-0.28	35	-0.02	13
Connecticut	23.32	16	0.11	43	-0.50	37	-0.38	42	-0.19	25
Delaware	33.36	6	0.38	1	-1.08	50	0.20	1	-0.42	50
District of Columbia	33.38	5	0.22	13	-0.55	39	-0.47	48	0.16	4
Florida	16.81	22	0.17	26	-0.33	30	-0.32	38	-0.11	16
Georgia	0.77	36	0.24	8	-0.48	35	-0.06	8	-0.15	21
Hawaii	36.69	2	0.10	45	-0.70	44	-0.50	49	0.17	3
Idaho	17.43	21	0.16	29	-0.67	42	-0.44	46	0.01	12
Illinois	0.50	38	0.22	12	0.28	1	-0.22	27	-0.33	37
Indiana	16.08	23	0.22	15	-0.13	22	-0.20	22	-0.36	41
Iowa	-13.67	45	0.21	19	0.27	2	-0.10	15	-0.48	51
Kansas	-23.23	50	0.12	41	0.25	3	-0.09	14	-0.39	46
Kentucky	5.56	33	0.22	14	-0.03	16	-0.07	10	-0.42	49
Louisiana	8.78	28	0.05	51	-0.01	15	-0.18	20	-0.12	17
Maine	26.62	11	0.16	32	-0.51	38	-0.39	43	-0.29	33
Maryland	25.88	12	0.25	5	-0.44	33	-0.14	17	-0.38	43
Massachusetts	33.45	4	0.13	38	-0.50	36	-0.53	50	-0.07	14
Michigan	19.17	19	0.25	4	-0.47	34	-0.32	37	-0.25	31
Minnesota	-19.61	47	0.19	22	-0.13	21	-0.23	31	-0.31	34
Mississippi	10.15	27	0.06	50	0.18	6	-0.09	13	-0.17	23
Missouri	-8.18	43	0.21	16	-0.03	18	-0.21	25	-0.36	42
Montana	-17.97	46	0.24	6	-0.21	27	-0.21	26	-0.12	18
Nebraska	-28.85	51	0.15	34	-0.03	17	-0.02	6	-0.31	36
Nevada	32.10	8	0.21	18	-0.70	43	-0.38	41	0.24	1
New Hampshire	21.25	17	0.18	23	-0.65	40	-0.43	45	-0.18	24
New Jersey	35.79	3	0.17	27	-0.21	26	-0.46	47	-0.21	28
New Mexico	12.79	24	0.24	7	-0.29	28	-0.33	39	0.06	9
New York	32.96	7	0.14	37	-0.13	20	-0.21	24	-0.13	19
North Carolina	25.11	13	0.09	47	-0.20	25	-0.04	7	-0.22	29
North Dakota	-20.34	48	0.16	28	0.23	4	-0.07	11	-0.31	35
Ohio	10.69	25	0.16	31	-0.16	23	-0.13	16	-0.35	40
Oklahoma	-12.72	44	0.10	44	0.11	7	-0.15	19	-0.16	22
Oregon	10.54	26	0.11	42	-0.66	41	-0.28	36	-0.10	15
Pennsylvania	28.56	10	0.15	35	-0.03	19	-0.14	18	-0.41	47
Rhode Island	31.61	9	0.20	20	-0.73	46	-0.22	28	0.09	7
South Carolina	7.49	31	0.12	40	0.03	12	0.12	2	-0.20	27
South Dakota	-22.47	49	0.16	30	0.06	11	0.05	3	-0.34	39
Tennessee	7.61	30	0.18	24	-0.17	24	-0.23	32	-0.28	32
Texas	-1.43	40	0.15	33	0.01	13	-0.22	29	0.16	5
Utah	5.35	34	0.08	49	-1.14	51	0.01	4	0.02	10
Vermont	23.49	15	0.20	21	-0.72	45	-0.37	40	-0.38	44
Virginia	18.15	20	0.18	25	-0.37	31	-0.19	21	-0.34	38
Washington	5.61	32	0.10	46	-0.80	47	-0.24	33	0.01	11
West Virginia	8.41	29	0.23	11	0.11	8	-0.08	12	-0.41	48
Wisconsin	-1.01	39	0.21	17	-0.30	29	-0.20	23	-0.39	45
Wyoming	-5.75	41	0.24	9	0.06	10	-0.22	30	-0.14	20

Table 7: U.S. States' Lending Sensitivities Based on Banking Sector Structures

	Year-over-Year				Quarter-over-Quarter					Year-over-Year				Quarter-over-Quarter			
	FF		UM		FF		UM			FF		UM		FF		UM	
Alabama	-47.68	30	-234.50	10	-81.91	25	-212.37	9	Montana	-7.44	50	-189.14	14	-42.68	43	-139.49	21
Alaska	-68.63	19	-89.56	41	-109.96	12	-71.62	34	Nebraska	-38.90	34	-300.32	2	-84.98	23	-268.59	2
Arizona	-21.02	46	21.82	50	-27.39	49	98.55	50	Nevada	-50.33	27	5.11	49	-71.80	30	50.84	49
Arkansas	-16.15	47	-163.40	23	-34.57	46	-126.56	24	New Hampshire	-74.40	16	-99.20	37	-90.58	19	-47.19	39
California	-110.24	1	-77.46	44	-149.53	1	-41.40	41	New Jersey	-79.58	14	-120.41	33	-101.04	16	-78.77	33
Colorado	7.81	51	-90.33	40	-9.86	50	-29.42	43	New Mexico	-21.73	44	-84.88	42	-49.33	39	-37.87	42
Connecticut	-102.29	3	-150.79	25	-133.10	5	-113.93	25	New York	-77.34	15	-121.24	32	-98.91	17	-94.16	30
Delaware	-14.63	48	107.64	51	71.24	51	178.45	51	North Carolina	-94.47	5	-160.90	24	-115.25	11	-141.91	19
District of Columbia	-45.85	32	-12.35	47	-68.22	33	37.47	47	North Dakota	-30.56	40	-295.54	3	-76.81	28	-265.56	3
Florida	-61.85	21	-133.00	30	-90.15	20	-92.68	31	Ohio	-66.24	20	-187.95	15	-83.64	24	-153.28	15
Georgia	-26.36	42	-106.35	34	-32.96	47	-59.24	37	Oklahoma	-57.00	24	-287.05	4	-118.26	10	-264.56	4
Hawaii	-102.01	4	-62.55	46	-143.67	3	-27.64	44	Oregon	-91.32	8	-149.00	27	-125.31	9	-112.14	26
Idaho	-72.41	17	-97.36	39	-105.13	14	-50.86	38	Pennsylvania	-81.79	12	-168.59	22	-90.10	21	-138.57	22
Illinois	-21.17	45	-223.47	12	-48.96	40	-183.17	11	Rhode Island	-58.98	23	-4.43	48	-65.80	34	38.34	48
Indiana	-47.84	29	-149.76	26	-54.40	37	-105.64	27	South Carolina	-60.32	22	-196.96	13	-85.97	22	-181.03	13
Iowa	-24.83	43	-279.59	6	-52.21	38	-240.82	7	South Dakota	-35.37	36	-282.53	5	-71.42	31	-253.30	5
Kansas	-51.56	26	-345.18	1	-106.60	13	-319.60	1	Tennessee	-54.30	25	-181.77	21	-78.08	27	-141.32	20
Kentucky	-38.29	35	-184.97	18	-44.64	41	-144.38	16	Texas	-32.54	37	-183.13	19	-94.32	18	-157.27	14
Louisiana	-92.26	7	-244.27	9	-149.29	2	-229.36	8	Utah	-103.45	2	-97.50	38	-125.47	7	-67.33	35
Maine	-89.82	9	-125.85	31	-103.71	15	-80.29	32	Vermont	-81.55	13	-100.23	36	-79.03	26	-43.36	40
Maryland	-50.32	28	-74.08	45	-32.19	48	-23.03	46	Virginia	-69.28	18	-139.16	28	-75.37	29	-96.99	29
Massachusetts	-93.64	6	-103.01	35	-125.45	8	-60.84	36	Washington	-89.74	10	-136.46	29	-129.08	6	-101.11	28
Michigan	-42.11	33	-84.23	43	-42.03	44	-27.57	45	West Virginia	-30.08	41	-182.07	20	-36.08	45	-142.24	18
Minnesota	-32.44	38	-253.10	8	-71.06	32	-207.44	10	Wisconsin	-46.65	31	-186.80	17	-60.06	35	-137.72	23
Mississippi	-85.56	11	-253.63	7	-137.85	4	-243.26	6	Wyoming	-8.77	49	-186.91	16	-43.24	42	-143.56	17
Missouri	-32.23	39	-227.82	11	-59.14	36	-182.71	12									

Figure 6: U.S. States' Lending Sensitivity Based on States' Banking Sector Structures

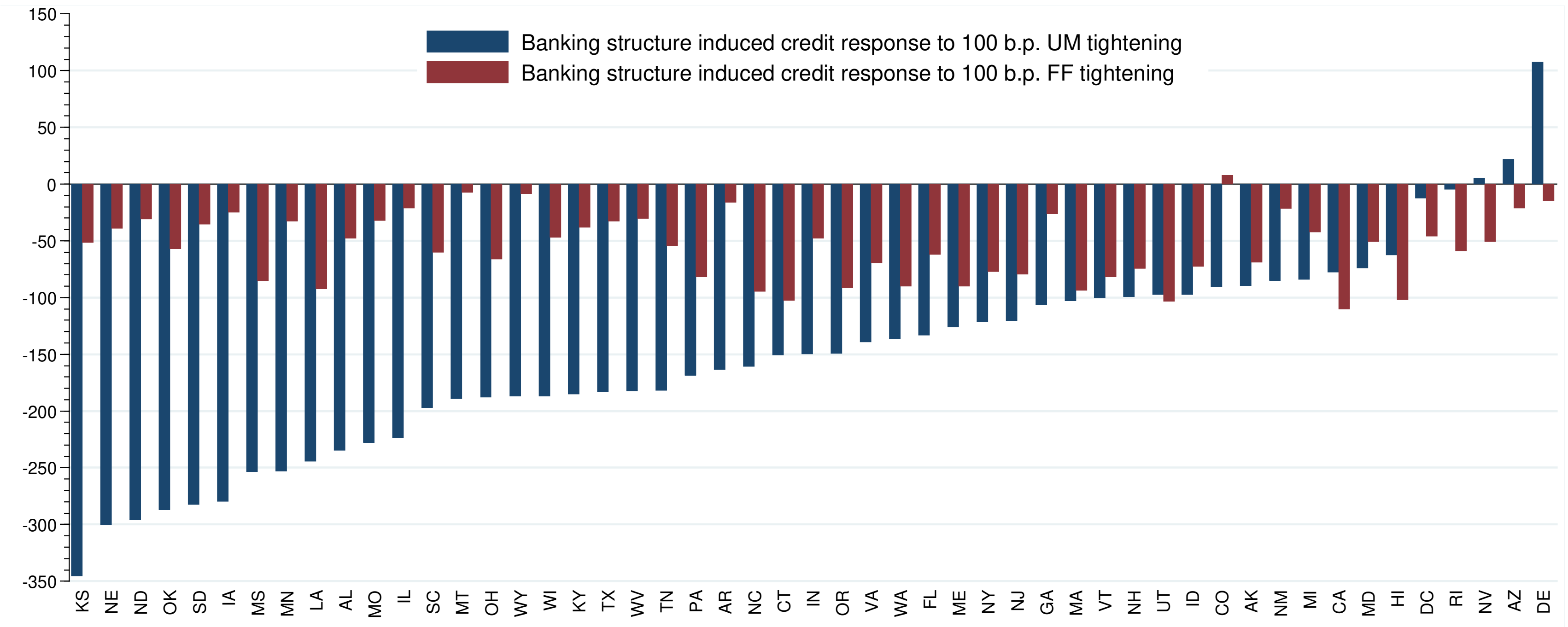
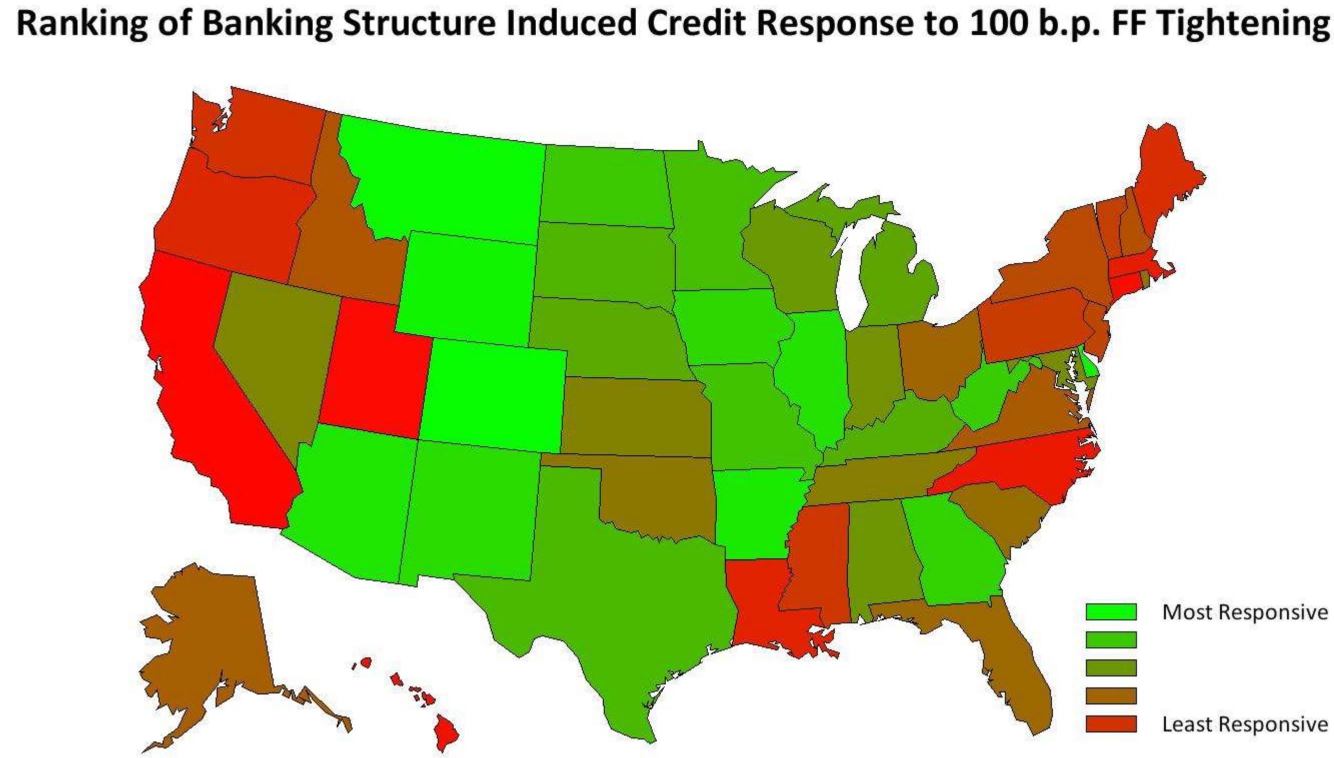
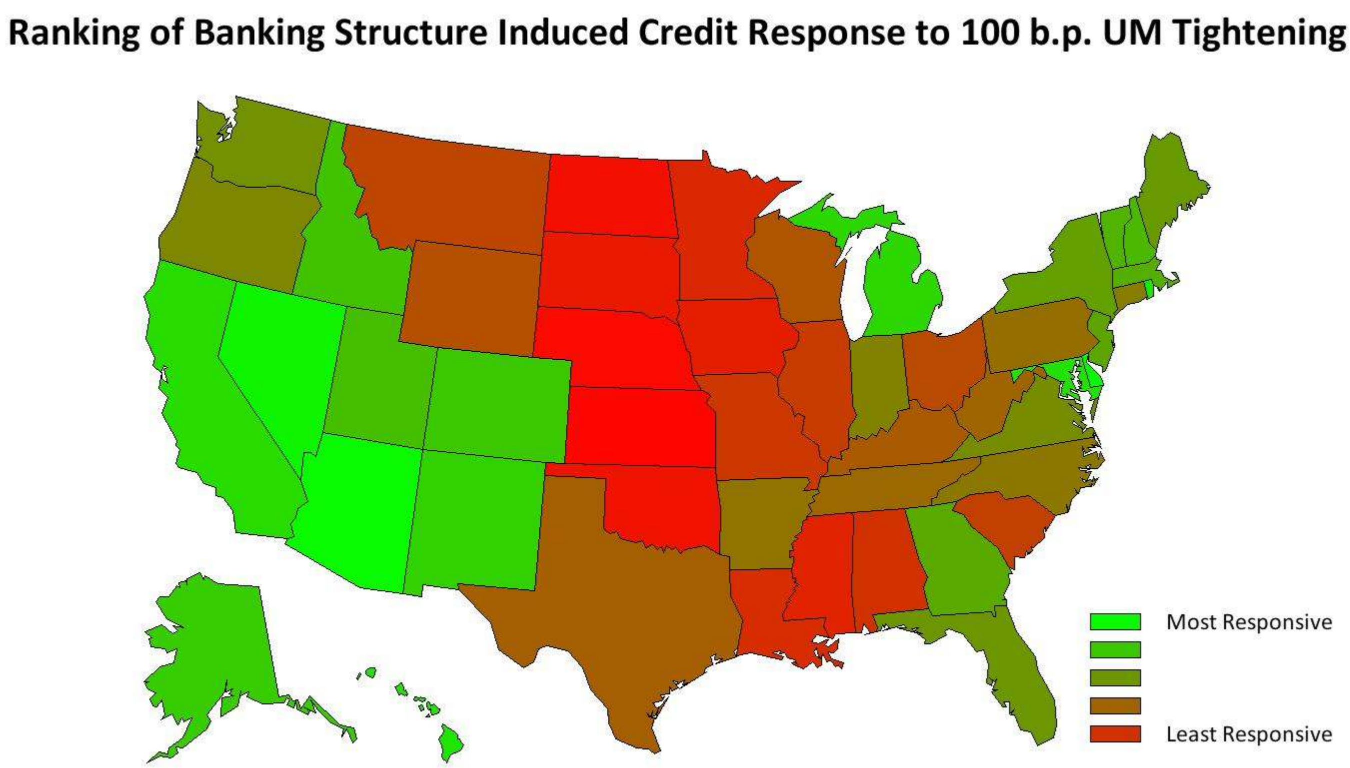


Figure 7: Regional Dispersion of Credit Channel Impacts (by State Rank)





D. Differences in the Median Bank Lending Response Across States

The distribution of bank characteristics across states is far from homogenous, as shown in Table 7, which summarizes the representative median bank during the sample period. Taken together with our findings of greater heterogeneity in bank-level lending responses, the differences in bank populations across states imply that the median bank lending response may be more different across states than previously thought. To assess this, we evaluate the estimated lending response for the notional bank at each state's median characteristics, apart from the MBHC indicator, which is taken to be the mean (thus, the proportion of the banks in each state that are part of a multi-bank holding company). Banks' locations are determined using the FDIC's Summary of Deposits which gives details as to the regional dispersion of commercial banks across states based on the location of their deposits.

As illustrated in Figure 6 and 7 and summarized in Table 7, the estimated heterogeneity of the median bank lending response across states is much larger using the identified monetary policy measure than using realized policy changes. Moreover, the ranking of U.S. states according to the sensitivity of their median bank to policy changes switches, sometimes dramatically. For example, California's median bank is estimated to be the 1st most sensitive to monetary policy when realized federal funds rate changes are used, while it becomes only the 44th most sensitive when the identified monetary policy measure is used. These findings may have important implications for understanding how changes in a national monetary policy stance may affect states or regions differently. In particular, states in the middle of the country, rather than on the East or West coast appear to be more affected by banking structure induced credit responses to monetary policy (figure 7).

V. ROBUSTNESS

In this section, we report the results of robustness exercises performed for our baseline regression estimates presented in tables 3 and 4. First, in section 3 we noted that the policy measure UM may not eliminate endogenous policy movements during episodes in which the FOMC set interest rates in light of banking sector conditions. The episodes during which such a critique seems reasonable for our sample are: (i) the tightening of bank capital regulations due to the Basel I Accord, which may have induced less restrictive monetary policy than would have been implemented based on growth and inflation objectives alone; and, (ii) the Federal Reserve Bank of New York's rescue of the hedge fund LTCM in the late 1990s, which may have prompted a similar policy response. We define two separate dummy variables, one equal to unity for all quarters in the period 1990-1993 (Ashcraft, 2006, uses a similar dummy variable), and the second equal to unity for all quarters in 1998-1999 (the LTCM rescue occurred in 1998). We then interact these dummy variables with each of the terms from equation (3) that feature a monetary policy measure, and estimated the extended specification using the procedure outlined in section 3. The results from this exercise, for both UM and FF, are presented in the first column of Table 8. 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 5.

In the second column in Table 8, we report results obtained after augmenting equation (3) with bank-level fixed effects. Although substantial fixed effects are unlikely given that we model loan growth rather than total loans, we consider this robustness exercise given that it has been applied elsewhere in the literature. For example, Loutskina (2011) motivates a fixed effects lending growth specification based on differences in managerial preferences.[30] The results indicate that our main findings are generally robust to this model extension.

The third robustness test addresses the fact that in equation (3) each of the bank characteristics interacted with a monetary policy measure are dated $t-1$, even when the policy measure is dated somewhat earlier (e.g., $t-4$). The dating of characteristics in our baseline regressions is standard in the literature, but it leaves open the possibility that a characteristic value is a function of the earlier policy change with which it is interacted. In order to address this issue we date all characteristics in interaction terms $t-\ell-1$, 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 8. Our findings on the direct effect of policy and the underlying heterogeneity are remarkably robust to the timing of characteristics.

In the final column in Table 8, we present a version of our baseline results that uses a set of forecasts instead of the actual, realized non-policy macroeconomic controls in M of specification (3). We use the historical data files for the Survey of Professional Forecasters' quarterly time series on nominal gross domestic product, the price index for gross domestic product, and the civilian unemployment rate for the current quarter, the quarter one period ahead and the quarter two periods ahead.[31]

Interestingly, the magnitude of the coefficients on the direct policy response shrinks somewhat. Note, however, that the sign of the direct policy response to FF alters its sign due to the direct inclusion of forward looking private sector variables, whereas UM is qualitatively consistent with earlier estimates.

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

[31] The data is publically available from the Federal Reserve Bank of Philadelphia at: http://www.philadelphiafed.org/research-and-data/real-time-center/survey-of-professional-forecasters/data-files/

Table 8: Robustness Checks

		Basel & LTCM				Fixed Effects			
		Year-over-Year		Quarter-over-Quarter		Year-over-Year		Quarter-over-Quarter	
		FF	UM	FF	UM	FF	UM	FF	UM
Policy	Marginal Effect	-1.1098***	-2.7578***	-1.8783***	-2.4377***	-1.0997***	-3.1595***	-1.3416***	-3.4472***
		(0.1520)	(0.3138)	(0.1166)	(0.2283)	(0.1492)	(0.3039)	(0.1415)	(0.2426)
MBHC	Interaction with policy	4.2459***	5.3738***	6.0322***	6.1606***	3.2124***	5.0807***	4.9932***	5.6292***
		(0.4597)	(0.7460)	(0.5209)	(0.8930)	(0.5006)	(0.8286)	(0.6072)	(1.1310)
Assets	Interaction with policy	-0.0052*	0.0242***	0.0008	0.0245***	0.0006	0.0349***	0.0016	0.0166**
		(0.0029)	(0.0051)	(0.0051)	(0.0092)	(0.0031)	(0.0057)	(0.0054)	(0.0082)
Securities	Interaction with policy	0.3633***	-0.9399***	0.1485	-1.2131***	0.0836	-1.6721***	-0.3485*	-1.9420***
		(0.1118)	(0.1825)	(0.1700)	(0.3494)	(0.1226)	(0.1967)	(0.1919)	(0.2807)
Capitalization	Interaction with policy	0.1939	0.6449***	0.7613**	0.3740	-0.1596	0.4625*	0.4027	0.6590
		(0.1781)	(0.2498)	(0.3826)	(0.5614)	(0.1853)	(0.2506)	(0.3669)	(0.4302)
Cash	Interaction with policy	0.5414***	1.2208***	0.1485	1.1283***	0.5224***	1.1576***	0.0432	1.6773***
		(0.1107)	(0.1807)	(0.1840)	(0.4050)	(0.1159)	(0.1774)	(0.1662)	(0.3255)
	R2	0.3502	0.3516	0.1079	0.1074	0.2545	0.2533	0.0145	0.0156
	Observations	1,435,371	1,435,364	1,467,757	1,467,655	1,434,635	1,434,677	1,459,846	1,459,337

		Pre-Determined Characteristics				Survey of Professional Forecasters			
		Year-over-Year		Quarter-over-Quarter		Year-over-Year		Quarter-over-Quarter	
		FF	UM	FF	UM	FF	UM	FF	UM
Policy	Marginal Effect	-0.9862***	-2.7069***	-1.6890***	-2.4433***	1.1858***	-1.5633***	1.0427***	-1.4126***
		(0.1448)	(0.2924)	(0.1154)	(0.2195)	(0.0861)	(0.2592)	(0.1370)	(0.2593)
MBHC	Interaction with policy	4.0538***	5.8789***	5.9909***	7.3098***	1.2965***	3.7731***	2.2789***	3.4618***
		(0.4465)	(0.7361)	(0.4580)	(0.8958)	(0.2824)	(0.7678)	(0.4206)	(0.8305)
Assets	Interaction with policy	-0.0061**	0.0267***	0.0007	0.0254***	0.0303***	0.0195***	0.0435***	0.0442***
		(0.0029)	(0.0052)	(0.0048)	(0.0088)	(0.0027)	(0.0048)	(0.0036)	(0.0092)
Securities	Interaction with policy	0.3038***	-0.9569***	0.0451	-1.1323***	0.3073***	-1.2711***	0.5282***	-1.2289***
		(0.1131)	(0.1840)	(0.1575)	(0.3327)	(0.0659)	(0.1819)	(0.1294)	(0.2944)
Capitalization	Interaction with policy	0.1973	0.7647***	0.7084*	0.4406	0.0986	0.3736	0.1357	0.0871
		(0.1903)	(0.2581)	(0.3832)	(0.5868)	(0.0883)	(0.2375)	(0.1627)	(0.5058)
Cash	Interaction with policy	0.5214***	1.2819***	0.0163	1.0983***	0.4203***	0.3980**	0.4204***	0.4944
		(0.1052)	(0.1790)	(0.1645)	(0.3981)	(0.0622)	(0.1589)	(0.1364)	(0.3759)
	R2	0.3548	0.3493	0.1040	0.1123	0.3464	0.3390	0.1046	0.1106
	Observations	1,435,422	1,435,435	1,467,813	1,467,762	1,435,386	1,435,371	1,466,090	1,465,772

VI. CONCLUSION

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

The results indicate both economically and statistically significant attenuation of estimated lending responses to monetary contractions, accompanied by the shielding of lending associated with multi-bank holding company affiliation as well as a very different U.S. state ranking in terms of the banking sector heterogeneity induced policy transmission to credit. We also found sign reversals in the effects conditional upon some characteristics. Specifically, the share of securities in total assets was shown to amplify policy transmission from exogenous interest rate changes, while restricting the transmission of realized interest rate changes. One explanation for this result is that many types of securities are subject to an adverse valuation effect following exogenous monetary policy contractions, which limits the scope for lending at banks that hold them in large numbers. In contrast, endogenous rises in the federal funds rate may be associated with lending increases (due to the underlying macroeconomic conditions to which policy is endogenous) at banks which choose to invest heavily in securities.

An important research implication from our work is that future studies of the banking system and monetary transmission should consider exogenous policy measures, alongside other measures such as the realized federal funds rate. In particular, the identification of exogenous monetary policy should take into account the forward-looking drivers of monetary policy such as growth and inflation forecasts, because these forward-looking variables are likely to impact lending markets.

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