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

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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. We use a new policy measure identified from narratives on FOMC intentions and real-time economic forecasts. This policy measure eliminates some of the movements in the actual federal funds rate that are endogenous to expected economic conditions. We find much stronger dynamic effects, and greater heterogeneity, in U.S. bank lending responses to the new monetary policy measure compared with the standard measure 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 macro

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fundamentals. 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 Codes: E44, E50, G21.

1. 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 (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 with 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.[1] 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.

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. Most papers examining the lending channel in the United States 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 thirty years.[2] 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

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

[2]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.

that are independent of the Federal Reserve staff's expectations for output growth, inflation, unemployment, and capacity utilization, and therefore remove a significant amount of the endogenous variation in monetary policy. The new monetary policy change measure elaborates upon and extends earlier work by Romer and Romer (2004). We compare bank and state-level lending responses to the new policy measure with lending responses estimated from realized federal funds rate changes that have been the focus of most previous research. As we discuss in section 3, it is unlikely that the new policy measure is independent of all macroeconomic conditions that may affect either loan demand or loan supply. However, we argue that the identification of the new policy measure eliminates some major sources of endogenous variation in standard policy measures such as the change in the actual federal funds rate. We therefore interpret the differences between estimates based on the new policy measure and those based on actual changes to the federal funds rate as a significant finding that points to the importance of some of the endogeneity concerns highlighted in the paper. We argue that future research concerning the lending channel of monetary policy should take into account the possible effects of endogenous monetary policy changes.

Our results highlight four important differences between bank lending responses to the new policy measure and actual changes to the federal funds rate. First, one year after a contraction measured using the new monetary policy measure, the reduction in lending growth at the average bank which is *not* part of a multibank 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 sheets. The share of bank assets held as securities mitigates the lending response to a realized federal funds rate increase, but *amplifies* the lending contraction in response to an increase in the new monetary policy measure. On the other hand, it is the ratio of cash to assets that *shields* lending growth from a tightening of the new monetary policy measure that is independent of some of the endogenous drivers of policy.

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 from the new monetary policy measure. Bank size appears to shield lending only in response to the new monetary policy measure, but not in response to potentially endogenous, realized 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 state-level representative banks, defined as the average bank of median size. The estimated lending responses at state-level representative banks to the new, 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 representative bank to policy changes switches, sometimes dramatically. For example, California's representative bank is estimated to be the first most sensitive to monetary policy when realized federal funds rate changes are used, while it becomes only the forty-fourth most sensitive when the new monetary policy measure is used. These findings suggest that the measurement of monetary policy has important implications when estimating the lending channel at the bank level and how the strength of such a channel varies with bank types and bank location.

To place our paper in context, we 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 findings using the new monetary policy measure are consistent with such possibilities. Although the construction of the new monetary policy measure does not eliminate all of the biases that may arise with endogenous monetary policy interventions, the contrasts between our results and those based upon actual changes in the federal funds rate suggest that some of the effects from expected macroeconomic conditions that we describe are relevant in practice. In light of these findings, we argue that future studies of bank lending behavior should take into account the forward-looking, endogenous component of monetary policy.

Moreover, 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' U.S. 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 monetary policy. We then discuss the bank-level econometric framework and data that we use to compare lending responses to identified policy changes with lending responses to realized changes in the federal funds rate. In section 4, we present our core results. We continue in section 5 with a consideration of their robustness to changes in estimation and data definitions. Finally, we conclude in section 6 with a summary and a discussion of the importance of monetary policy identification for future research concerning bank lending behavior.

2. Bank Lending and Monetary Policy

2.1 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 new monetary policy measure (described in section 3) and to the realized federal funds rate changes that have been used in previous research.

An intuitive alternative to attempting to identify 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 control for the endogenous variation in monetary policy at the decision frequency for monetary policy, namely each FOMC meeting. These changes can then be mapped to the quarterly frequency, as described in detail in section 3.1.

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.[3]

The vector B comprises bank characteristics that proxy access to non-reservable finance (that is, liabilities that do not require reserves or assets on hand). These might include total bank assets, a multibank 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 better access non-reservable financing sources may be able to better 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 bank characteristics proxy for access to funds that affect loan supply, then the cross-effects (δ) represent how a bank's characteristics help to shield loan supply from monetary policy changes (or amplify its effects). However, many bank characteristics are also correlated with drivers of a bank's loan demand. For example, large banks (proxied

[3]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.

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 independent of demand. 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.[4] Increases in the federal funds rate target induce *left*ward shifts of banks' loan supply schedules via the narrow and broad lending channels (see Bernanke and Gertler 1995). 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

[4]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 (2011) and Cetorelli and Goldberg (2012) consider Strongin's (1995) identification of exogenous movements in nonborrowed 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.

such circumstances, any loan supply contraction due to tight monetary policy may coincide with a *right*ward shift of loan demand, as consumers borrow against expected future income and firms invest in response to an improving outlook for profits. The loan demand shift will attenuate the reduction in lending from a monetary tightening, and the β estimated from equation (1) will not capture the full effect of monetary policy. A similar result may arise via the effects of expected inflation. In particular, reductions in bank lending from a rise in the federal funds rate may be muted because the demand for loans in nominal units rises with expected inflation. As in the example based on expected economic growth, equilibrium lending is subject to countervailing effects from loan demand and loan supply, such that the β estimated from equation (1) is attenuated.

The drivers of endogenous monetary policy may also influence equilibrium lending via bank loan supply. The availability of nonreservable 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.

2.2 Policy Endogeneity and Bank Characteristics

An important question is whether or not procyclical 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 subsection, 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 incorporating 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-stock 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 (for example, multibank 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.[5]

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 multibank holding company status (affiliation/non-affiliation). In this case, a comparison of lending responses by multibank holding company status is more likely to reflect genuine differences in banks' access to alternative finance. We return to this issue when discussing our empirical results in section 4. The point that we emphasize at this stage is that such effects impact *all* measures of monetary policy, both endogenous and exogenous. The main advantage of considering exogenous policy measures is that their effects on bank lending are less likely to be affected by the sources of bias discussed in this section.

3. Econometric Methodology

In this section, we outline the methods that we use in comparing bank lending responses to identified monetary policy changes with

[5]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.

realized federal funds rate changes. We first describe the identification procedure used to remove endogenous variation in the monetary policy rate. Then, we outline the regression models that underlie our core results. Finally, we describe the data we use in the estimation.

3.1 Monetary Policy Identification

To control for endogenous movements in 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 (such as some periods 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 target federal funds rate changes for 1997 to 2007, the last year for which Greenbook forecasts were publicly available. Such announcements began in February 1994, overlapping with the original Romer and Romer series for two 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.[6] 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). Their subsample analysis and the results in Orphanides (2003) suggest that pooling over time is appropriate (or at least approximately so).

[6]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.

In the second step, the target federal funds rate change is regressed upon the Federal Reserve's Greenbook (in-house) forecasts for real output growth, inflation, and unemployment over horizons of up to two quarters. These represent the central objective variables of the Federal Reserve.[7] 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:

$$\begin{aligned}
\Delta \mathrm{ff}_m = \alpha &+ \beta \cdot \mathrm{ff}_{m-1} \\
&+ \sum_{\ell=-1}^{2} \varphi_\ell^y \cdot \widetilde{\Delta y}_{m,\ell} + \sum_{\ell=-1}^{2} \varphi_\ell^y \cdot \left(\widetilde{\Delta y}_{m,\ell} - \widetilde{\Delta y}_{m-1,\ell}\right) \\
&+ \sum_{\ell=-1}^{2} \varphi_\ell^\pi \cdot \widetilde{\pi}_{m,\ell} + \sum_{\ell=-1}^{2} \varphi_\ell^\pi \cdot \left(\widetilde{\pi}_{m,\ell} - \widetilde{\pi}_{m-1,\ell}\right) \\
&+ \sum_{\ell=-1}^{2} \varphi_\ell^n \cdot \widetilde{n}_{m,\ell} + \sum_{\ell=-1}^{2} \varphi_\ell^n \cdot \left(\widetilde{n}_{m,\ell} - \widetilde{n}_{m-1,\ell}\right) \\
&+ \sum_{\ell=-1}^{2} \varphi_\ell^u \cdot \widetilde{u}_{m,\ell}^{mfg} + \sum_{\ell=-1}^{2} \varphi_\ell^u \cdot \left(\widetilde{u}_{m,\ell}^{mfg} - \widetilde{u}_{m-1,\ell}^{mfg}\right) + \varepsilon_m, \qquad (2)
\end{aligned}$$

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

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

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 backcasts, nowcasts, 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 percent compared with 28 percent).[8]

In order for the regression residuals from equation (2) to reflect exogenous monetary policy changes appropriate to our application, we require that (i) the Greenbook forecasts of output, inflation, unemployment, and capacity utilization are not a function of the contemporaneous change in the target federal funds rate; (ii) the Greenbook forecasts account for expected economic and banking-sector conditions that directly influence bank lending; and (iii) the regression parameters linking the target federal funds rate to the Greenbook forecasts are constant over the sample period.

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 (for example, asset prices, industry surveys) that may incorporate 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

[8]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 Output Growth	−1	0.0012	0.0093	0.1331
	0	−0.0122	0.0262	−0.4652
	1	−0.0537	0.0402	−1.3350
	2	0.0567	0.0443	1.3007
	Total Effect	−0.0070	0.0306	−0.2280
Output Growth Revision	−1	−0.0117	0.0227	−0.5153
	0	0.1056	0.0332	3.1829
	1	0.0116	0.0450	0.2573
	2	−0.0483	0.0566	−0.8541
	Total Effect	0.0572	0.0712	0.8028
Forecast Inflation	−1	0.0202	0.0206	0.9786
	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 Revision	−1	0.0171	0.0355	0.4815
	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 Unemployment	−1	−0.0440	0.1687	−0.2609
	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 Revision	−1	−0.4282	0.3463	−1.2364
	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	−2.5044
Forecast Manufacturing Capacity Utilization	−1	−0.0218	0.0496	−0.4386
	0	−0.1091	0.1146	−0.9521
	1	0.2020	0.1850	1.0919
	2	−0.0629	0.1093	−0.5757
	Total Effect	0.0082	0.0077	1.0561

(*continued*)

Table 1. (Continued)

		Coeff.	**S.E.**	**t-stat.**
Manufacturing Capacity Utilization Revision	−1	−0.1124	0.0629	−1.7867
	0	0.1688	0.1226	1.3769
	1	−0.2145	0.1819	−1.1792
	2	0.1399	0.1187	1.1793
	Total Effect	−0.0182	0.0342	−0.5335
		Obs.	R^2	DW
		357	0.3196	1.8407

Notes: 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. "Total Effect" refers to the sum of the coefficients on sets of forecasts or forecast revisions for the previous, current, and next two quarters.

such that the forecasts in equation (2) are not subject to reverse causation.

The second assumption is crucial to exclude endogenous 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 macroeconomic information available to policymakers and are known to perform well relative to alternative forecasts (see Romer and Romer 2000, 2008, and Bernanke and Boivin 2003 for evidence). Of course, the Greenbook is one of many inputs to FOMC decisions alongside individual member forecasts, the assessments formed by members based on discussions with industry leaders and anecdotal evidence, and alternative data sources, among others. That said, our aim is not to construct a detailed model of FOMC decision making. Instead, it is to construct a measure of monetary policy changes that controls for some of the most important sources of endogenous variation in policy.

Alternative FOMC member beliefs may not be encompassed by the Greenbook forecasts leading to FOMC decisions that are unrelated to the Greenbooks but still potentially endogenous. For instance, in the 1990s, Greenspan argued for relatively loose monetary policy based on a more optimistic assessment of productivity than that in the Greenbook assumptions (which was proved correct when productivity outturns were better than expected by the Greenbook). Another example is that some FOMC decisions in the 1970s are likely to have been responses to macroeconomic volatility induced by price and wage controls that are not fully captured in the Greenbook. Such episodes will lead to endogenous monetary policy being retained in the residuals from equation (2) if these alternative FOMC member beliefs regarding future economic conditions are more highly correlated with bank lending than are the Greenbook forecasts.

The second assumption may also be violated if the Federal Reserve directly responds to expected banking-sector conditions that are not correlated with the Greenbook forecasts. For example, the Beige Book or Senior Loan Officer Opinion Survey could point to assessments of lending standards and credit conditions that are different from that implied by the Greenbook alone. This could happen if concerns over bank liquidity prompt the Federal Reserve to keep interest rates on hold even when Greenbook forecasts point to higher interest rates. The new monetary policy measure might then show a negative monetary policy change which would be unassociated with higher 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 subprime housing market in 2007 is excluded from the sample, mitigating the above concern. However, three other relevant episodes where banking-sector conditions may have motivated policy changes are included in the estimation sample: (i) the federal funds rate change in 1980 to offset the impact of credit controls introduced by the Carter administration (see Council of Economic Advisors 1980); (ii) 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 (iii) the Federal Reserve Bank of New York's rescue of U.S. hedge fund Long-Term Capital Management (LTCM) in 1998, which may have induced similar effects.

The third assumption required for the validity of the identification set out in equation (2) is that the regression parameters are constant over the sample period. Unmodeled parameter changes could lead to some intended but endogenous monetary policy changes being captured by the new monetary policy measure. Romer and Romer (2004) provide some subsample estimates of a regression similar to that in equation (2) and show that shocks from regressions that allow the parameters to vary across subsamples are highly correlated with those obtained from a full sample under the hypothesis of constant parameters. However, we recognize that such exercises need not detect parameter change that is non-monotonic, as may have occurred during the Volcker disinflation of the early 1980s, and it is therefore possible that parameter change leads to some endogenous variation being retained in the new monetary policy measure.

Despite such threats to the identifying assumptions for the new monetary policy measure, we contend that any such remaining sources of bias will be less severe than in the case in which monetary policy is measured from actual federal funds rate changes. Consistent with this view, in section 4 we report results indicating significant differences between bank lending responses estimated based on the new monetary policy measure versus actual federal funds rate changes. Furthermore, in section 5, we provide evidence that such differences remain after controlling for a series of dummy variables set to unity for the banking-sector episodes discussed here as sources of endogeneity in the new policy measure.

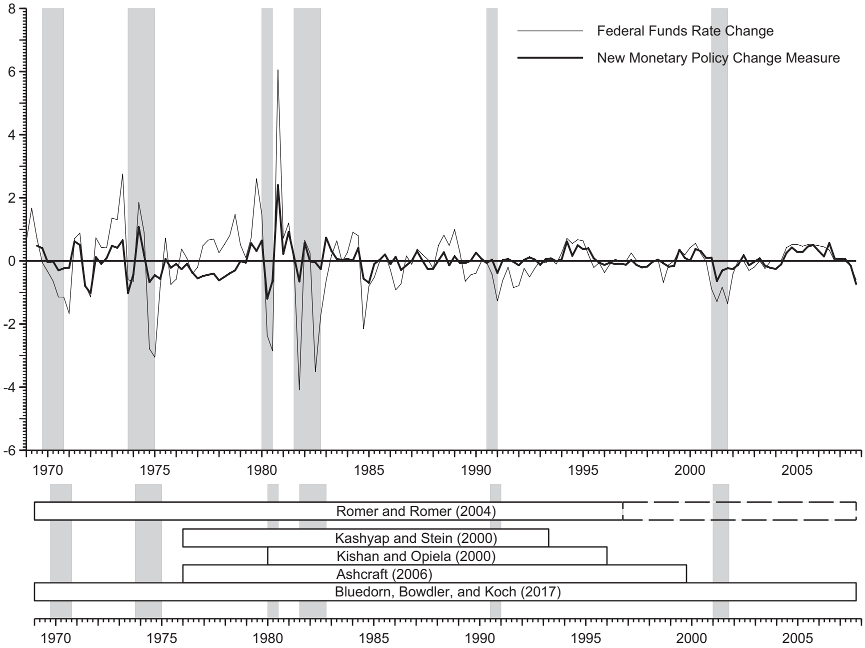
For any identification scheme, a natural question is, what are the sources of exogenous policy variation (for our application, in the residuals estimated from equation (2))? A key element is likely to be the idiosyncratic component of FOMC member interest rate choices, unrelated to future expected economic or banking-sector conditions. 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).[9] It is these federal funds rate adjustments, driven by errors and preference shifts, that constitute sources of exogenous policy variation for estimating bank lending responses.

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 thirty days in the following quarter to confirm the figures reported. We first sum the meetings-based monetary policy shocks on a 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 averages, denoting it 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.[10] 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

[9]An important caveat here is that Federal Reserve decisions to validate incorrect market beliefs must occur randomly in order for the new monetary policy change measure to give unbiased estimates. If, for instance, the FOMC felt obliged to validate beliefs only in turbulent periods in which the outlook for lending growth was weak, then biases will still occur and this should be kept in mind when contrasting the estimates from the new policy measure with those from actual federal funds rate changes.

[10]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. Changes in the Federal Funds Rate versus the New Monetary Policy Measure



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. Note that the aggregation from meeting frequency to quarterly frequency described here will result in the quarterly UM series exhibiting positive serial correlation, even if the underlying meeting-based shocks are serially uncorrelated.

To this point we have discussed at length the extent to which the new measure of monetary policy eliminates endogenous fluctuations in the actual federal funds rate, which may be a source of bias in studies of the bank lending channel. Another property of monetary policy interventions that may bias lending channel estimates is their ex ante predictability: a least-squares regression will not estimate the full effect of a policy change that is entirely exogenous but at least party predictable, since banks will vary their lending at the point in time at which the policy becomes predictable rather than the point at which the policy is implemented. Figure 1 shows several episodes in which the quarterly version of the new monetary policy measure is serially correlated, with runs of either positive or negative changes. This implies that our new measure of monetary policy (UM) is ex post predictable. Some of the predictability is likely due to the mapping from the somewhat irregular FOMC meeting frequency to quarterly frequency, but some of these serial correlation episodes are likely to have been ex ante predictable (in real time). For instance, the sustained policy loosening under Arthur Burns during the 1970s, shown here to be in part independent of the Greenbook forecasts, is commonly linked to pre-election attempts to stimulate the economy and therefore may be predictable from the perspective of the political cycle. Likewise, in figure 1, the gradual tightening of policy over the period 2004–07 is classified as at least partly exogenous by the UM measure, but the regularity of 25-basis-point rate hikes at this time likely rendered the tightening largely predictable.

In principle, the real-time predictability of policy measures can be addressed, for instance, by using expectations revealed in futures markets as in Kuttner (2001). However, owing to the relatively recent inception of such markets, an approach along these lines would more than halve the sample period that we consider. Consequently, UM must be viewed as a policy measure that is exogenous with respect to the Greenbook forecasts but potentially in part ex ante predictable from the perspective of banks making lending decisions. This means that the estimates we present using UM may suffer from some attenuation bias if banks act in anticipation of UM movements. However, such effects will apply equally to estimates from actual federal funds rate changes, which have hitherto been the focus of the literature in this area. The dimension on which UM does bring to bear new methodology is that it controls for some endogenous movements in the federal funds rate, and to the extent that estimates of the bank lending channel differ using UM and actual federal funds rate changes, there will be evidence of the importance of controlling for endogeneity, even if the full extent of this evidence is understated due to the effects of policy predictability.

3.2 Regression Specification

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

$$\Delta L_{i,t} = \alpha_i + \sum_{\ell=1}^{4} \rho_\ell \cdot \Delta L_{i,t-\ell} + \sum_{m=1}^{3} \sum_{\ell=0}^{4} \beta_{m,\ell} \cdot M_{m,t-\ell} + \sum_{k=1}^{5} \gamma_k \cdot B_{k,i,t-1} + \sum_{m=1}^{3} \sum_{k=1}^{5} \sum_{\ell=0}^{4} \delta_{m,k,\ell} \cdot B_{k,i,t-1} \cdot M_{m,t-\ell} + \mu \cdot t + \sum_{i=1}^{N} \sum_{q=1}^{3} \phi_{i,q} \cdot S_{i,q} + \varepsilon_{i,t}, \quad (3)$$

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), $S_{i,q}$ is a set of seasonal bank-specific dummy variables equal to one in quarter q and zero otherwise, and ε is a mean-zero error term.

The components of vector M are

- a monetary policy measure, either UM or FF, as described in section 2;
- real GDP growth in percentage points; and
- growth in the personal consumption expenditure (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

- the bank size percentile within a given quarter;
- an indicator variable set to unity post-1986 if a bank is part of a multibank holding company and zero otherwise

(following Ashcraft 2006, this characteristic is dated t rather than $t-1$);[11]
- the ratio of bank securities to assets;
- the ratio of total equity capital to assets; and
- 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 3.3.

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 multibank holding company dummy, which is dated t. As such, lending decisions in period t are conditional on characteristics that are predetermined. They are thus less likely to be influenced by current lending behavior. The multibank holding company indicator is not predetermined, 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 predetermined with respect to the monetary policy measure. We consider this case

[11]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).

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.[12]

Second, each of the bank characteristics ratios, except the binary variable for multibank 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 fifty, 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 (bp) 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-sized bank or a bank is affiliated with a holding company in the case of multibank holding company (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

[12] While we consider characteristics that are predetermined 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.

inflation; and (ii) heterogeneity in the response of real lending growth to macroeconomic factors like current output growth and inflation.[13]

Similar to Ashcraft (2006), we calculate all regression standard errors through clustering at the bank level to deal with any residual heteroskedasticity and autocorrelation of unknown form.[14] 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.

3.3 Data

3.3.1 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.[15] 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 vary slightly by some business days around the end of the quarter, and for some instances reporting is semi-annual rather than quarterly. Thus, two additional assumptions are necessary in order to make good use of that earlier data. First, we assume that these earlier timing differences do not matter

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

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

[15]We are grateful to Adam Ashcraft for providing a data set 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.

Figure 2. Macroeconomic Controls during the Sample

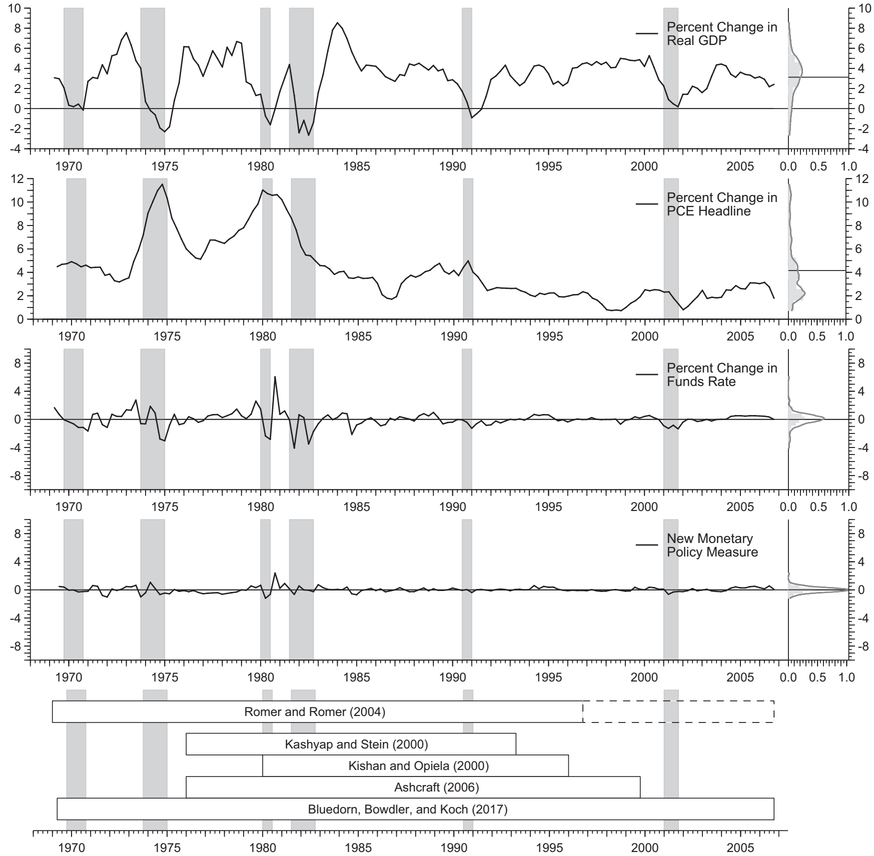
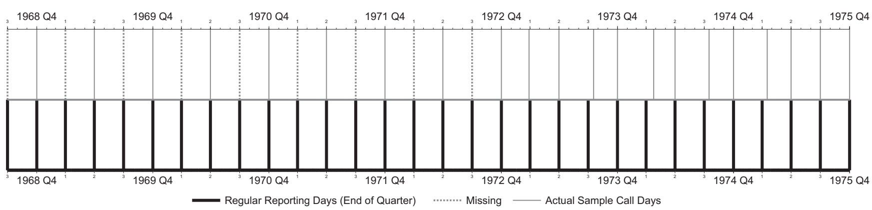


Figure 3. Imperfectly Measured Variables Prior to 1975



substantively. Second, we linearly interpolate bank-level variables for the few quarters that data were missing.

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.[16] 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 Peek and Rosengren (1998) and Ashcraft (2001), our measure of loans does not control for loans being moved off bank balance sheets via securitization.

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.[17] 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 data set 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.[18] The average number of observations per bank is 112 quarters, or about twenty-eight years. In line with other studies, this sample is obtained after excluding bank/quarter observations affected by mergers, since they

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

[17] 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 subcomponents. 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.

[18] For an empirical analysis of the increasing concentration of banking assets in the U.S. banking system, see Fernholz and Koch (2016).

may induce spurious movements in balance sheet variables (following a merger, the merged banks are dropped and a new bank enters the data set).[19]

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 $2 \cdot \sqrt{\frac{K}{N}}$, where K is the total number of explanatory variables and N is the overall sample size (Welsch and Kuh 1977). The number of observations excluded depends on whether the regression is fitted using UM or FF. Specifically, from a total sample of 1,435,713 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.[20] 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 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 3.2).

3.3.2 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 PCE price index. Both series are from the U.S. Bureau of Economic Analysis (BEA) and were extracted from the

[19] 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.

[20] 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).

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 Thousand U.S. Dollars)	39,110	482,340	113,460	1,697,992	181,063	2,097,823	263,195	2,815,227
Multibank 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	

Table 3. Comparison of Dynamic Lending Responses 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)
R^2	0.3548	0.3493	0.1040	0.1123
Observations	1,435,422	1,435,435	1,467,813	1,467,762

Federal Reserve Bank of St. Louis's Federal Reserve Economic Data (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 3.2. Figure 2 displays time-series and data summaries of all macroeconomic controls.

4. Empirical Results

In table 3, we present estimates of $\sum_{\ell=0}^{L} \beta_\ell$ and the associated standard errors for the two policy measures UM and FF. These statistics measure the percentage change in lending at various horizons following a 100 bp 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), Ashcraft (2006), and Loutskina (2011) in reporting the direct effect of policy on lending.

At each of the horizons considered, the lending reduction estimated from an increase in the new monetary policy measure UM exceeds that from a policy contraction measured by the realized federal funds rate. Furthermore, the precision associated with our estimates is such that 95 percent 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. Partly 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.

4.1 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 one standard deviation of their sample distribution (except in the case of the multibank 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

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***	−1.4521***	−1.6727***	−1.3450**
		(0.1512)	(0.3027)	(0.2188)	(0.5318)
	R^2	0.3548	0.3493	0.1040	0.1123
	Observations	1,435,422	1,435,435	1,467,813	1,467,762

3. We consider the marginal lending response to a 100 bp 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.[21]

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, a bank that is ten percentiles above the median lending would shrink by 0.26 bp ($= 10 \times 0.0267$) less in response to a UM contraction of 100 bp. For the multibank holding company (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

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

by the theory of the bank lending channel. The more powerful multibank holding company effect estimated from the identified policy measure raises the possibility that the lending channel is quantitatively more important than previously believed.

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 a contraction of the new monetary policy measure, 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.[22]

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 the new monetary policy measure 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

[22]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 multibank 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 in order to estimate the unconditional effect of affiliation, the finding that holding company banks raise lending at the expense of stand-alone banks remains intact.

possible trends towards a weaker aggregate monetary transmission mechanism in recent decades.[23]

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 4.4.

4.2 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 bp increase in the new policy measure, a bank with securities one standard deviation above the mean *reduces* lending by a further 0.96 percentage points compared with 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 bp 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 that the empirical support for such an interpretation may come from a confounding of expected future growth and inflation with the monetary policy stance.

[23]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.

A possible explanation for the negative effect of monetary policy tightening upon lending for banks with large securities-to-assets ratios follows. A 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.

4.3 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).[24] The Federal Reserve Board issued a formal statement in April 1987 indicating that failure by a parent bank to

[24]Another source of structural change is the abolition of Regulation Q (Koch 2015), which restricted banks' ability to vary interest rates in order to attract deposits (a source of funding). The abolition of this restriction was largely implemented via the Monetary Control Act of 1980, and is therefore likely to induce heterogeneity in our results across a much shorter period than the source-of-strength doctrine. Due to the limitations in estimating heterogeneity in our results across a period of just three years or so, we do not address the effects of Regulation Q. If observations from this period exerted undue influence on the results, the outlier detection procedure we employ ought to diagnose them.

inject liquidity into a financially distressed subsidiary when funds are available would be considered an unsafe banking practice.[25]

In section 3.2, we argued that from 1987 onwards membership in a bank holding company should affect lending responses to monetary policy. Our baseline results are consistent with this idea. In this subsection, we take our analysis of the effects of the source-of-strength doctrine one stage further. We interact each of the cross-terms in $\sum_{m=1}^{3}\sum_{k=1}^{5}\sum_{\ell=0}^{4}\delta_{m,k,\ell}\cdot B_{k,t,t-1}\cdot M_{m,t-\ell}$ with the binary variable that is set to unity post-1986 for banks that belong to a multibank holding company (excluding the cross-term that already features the holding company indicator). These extra terms are added to our baseline regression in (2). 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 multibank holding company, not balance sheet composition. The roles of security holdings in amplifying and cash in mitigating the effects of the new policy measure 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.

4.4 Differences in the Median Lending Response across States

The distribution of bank characteristics across states is far from homogenous, as shown in table 6, which summarizes the representative median bank during the sample period. Our finding that the

[25] 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)
	R^2	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

(continued)

Table 6. (Continued)

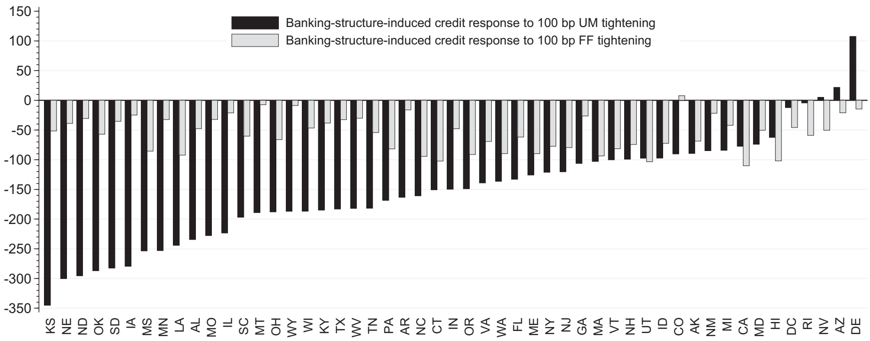
State	Assets		MBHC		Securities		Equity		Cash	
	Value	**Rank**	**Value**	**Rank**	**Value**	**Rank**	**Value**	**Rank**	**Value**	**Rank**
Kentucky	5.56	33	0.22	14	−0.03	16	−0.07	10	−0.42	49
Louisiana	8.78	28	0.05	51	−0.01	15	−0.18	20	−0.12	17
Maine	26.62	11	0.16	32	−0.51	38	−0.39	43	−0.29	33
Maryland	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

(continued)

Table 6. (Continued)

State	Assets		MBHC		Securities		Equity		Cash	
	Value	**Rank**	**Value**	**Rank**	**Value**	**Rank**	**Value**	**Rank**	**Value**	**Rank**
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

Figure 4. U.S. States' Lending Sensitivity Based on States' Banking-Sector Structures

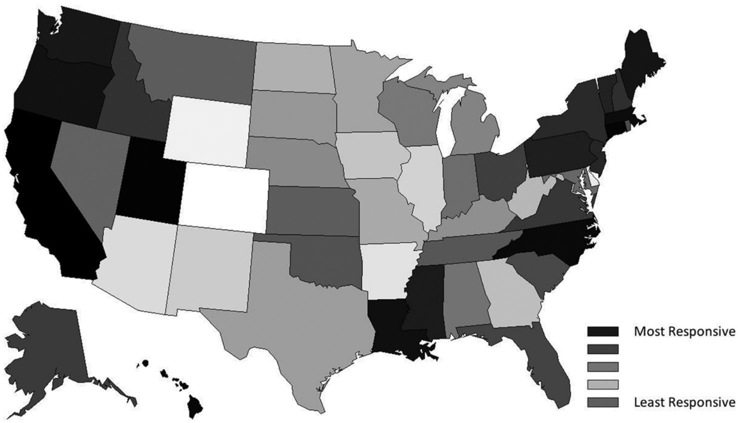


new measure of monetary policy leads to evidence of greater heterogeneity in bank-level lending responses suggests that differences in state median bank lending responses may be larger when measured using the new monetary policy measure as opposed to the change in the actual federal funds rate. To assess 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 multibank 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.

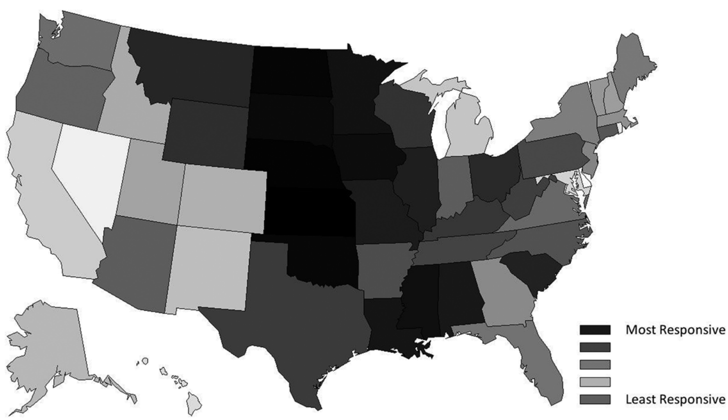
As illustrated in figures 4 and 5 and summarized in table 7, the estimated heterogeneity of the representative bank lending response across states is much larger when using the new monetary policy measure than when using realized policy changes. Moreover, the ranking of U.S. states according to the sensitivity of their representative bank to policy changes switches. For example, California's representative bank is estimated to be the first most sensitive to monetary policy when realized federal funds rate changes are used, while it becomes only the forty-fourth most sensitive when the identified monetary policy measure is used. We do not make any strong claims based on these results, nor do we link them to recommendations for the conduct of monetary policy. While the representative

Figure 5. Regional Dispersion of Credit Channel Impacts (by state rank)

A. Ranking of Banking-Structure-Induced Credit Response to 100 bp FF Tightening



B. Ranking of Banking-Structure-Induced Credit Response to 100 bp UM Tightening



bank in each state is a convenient point of comparison, it may well be that such a bank has different importance across states and over time due to changes to the distribution of banks across each characteristic. Furthermore, our comparison of states does not control for interstate differences in the structure of production (e.g., agriculture

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

State	Year over Year				Quarter over Quarter			
	FF		UM		FF		UM	
Alabama	−47.68	30	−234.50	10	−81.91	25	−212.37	9
Alaska	−68.63	19	−89.56	41	−109.96	12	−71.62	34
Arizona	−21.02	46	21.82	50	−27.39	49	98.55	50
Arkansas	−16.15	47	−163.40	23	−34.57	46	−126.56	24
California	−110.24	1	−77.46	44	−149.53	1	−41.40	41
Colorado	7.81	51	−90.33	40	−9.86	50	−29.42	43
Connecticut	−102.29	3	−150.79	25	−133.10	5	−113.93	25
Delaware	−14.63	48	107.64	51	71.24	51	178.45	51
District of Columbia	−45.85	32	−12.35	47	−68.22	33	37.47	47
Florida	−61.85	21	−133.00	30	−90.15	20	−92.68	31
Georgia	−26.36	42	−106.35	34	−32.96	47	−59.24	37
Hawaii	−102.01	4	−62.55	46	−143.67	3	−27.64	44
Idaho	−72.41	17	−97.36	39	−105.13	14	−50.86	38
Illinois	−21.17	45	−223.47	12	−48.96	40	−183.17	11
Indiana	−47.84	29	−149.76	26	−54.40	37	−105.64	27
Iowa	−24.83	43	−279.59	6	−52.21	38	−240.82	7
Kansas	−51.56	26	−345.18	1	−106.60	13	−319.60	1
Kentucky	−38.29	35	−184.97	18	−44.64	41	−144.38	16
Louisiana	−92.26	7	−244.27	9	−149.29	2	−229.36	8
Maine	−89.82	9	−125.85	31	−103.71	15	−80.29	32
Maryland	−50.32	28	−74.08	45	−32.19	48	−23.03	46
Massachusetts	−93.64	6	−103.01	35	−125.45	8	−60.84	36
Michigan	−42.11	33	−84.23	43	−42.03	44	−27.57	45
Minnesota	−32.44	38	−253.10	8	−71.06	32	−207.44	10
Mississippi	−85.56	11	−253.63	7	−137.85	4	−243.26	6
Missouri	−32.23	39	−227.82	11	−59.14	36	−182.71	12
Montana	−7.44	50	−189.14	14	−42.68	43	−139.49	21
Nebraska	−38.90	34	−300.32	2	−84.98	23	−268.59	2
Nevada	−50.33	27	5.11	49	−71.80	30	50.84	49
New Hampshire	−74.40	16	−99.20	37	−90.58	19	−47.19	39
New Jersey	−79.58	14	−120.41	33	−101.04	16	−78.77	33
New Mexico	−21.73	44	−84.88	42	−49.33	39	−37.87	42
New York	−77.34	15	−121.24	32	−98.91	17	−94.16	30
North Carolina	−94.47	5	−160.90	24	−115.25	11	−141.91	19

(*continued*)

Table 7. (Continued)

State	Year over Year				Quarter over Quarter			
	FF		UM		FF		UM	
North Dakota	−30.56	40	−259.54	3	−76.81	28	−265.56	3
Ohio	−66.24	20	−187.95	15	−83.64	24	−153.28	15
Oklahoma	−57.00	24	−287.05	4	−118.26	10	−264.56	4
Oregon	−91.32	8	−149.00	27	−125.31	9	−112.14	26
Pennsylvania	−81.79	12	−168.59	22	−90.10	21	−138.57	22
Rhode Island	−58.98	23	−4.43	48	−65.80	34	38.34	48
South Carolina	−60.32	22	−196.96	13	−85.97	22	−181.03	13
South Dakota	−35.37	36	−282.53	5	−71.42	31	−253.30	5
Tennessee	−54.30	25	−181.77	21	−78.08	27	−141.32	20
Texas	−32.54	37	−183.13	19	−94.32	18	−157.27	14
Utah	−103.45	2	−97.50	38	−125.47	7	−67.33	35
Vermont	−81.55	13	−100.23	36	−79.03	26	−43.36	40
Virginia	−69.28	18	−139.16	28	−75.37	29	−96.99	29
Washington	−89.74	10	−136.46	29	−129.08	6	−101.11	28
West Virginia	−30.08	41	−182.07	20	−36.08	45	−142.24	18
Wisconsin	−46.65	31	−186.80	17	−60.06	35	−137.72	23
Wyoming	−8.77	49	−186.91	16	−43.24	42	−143.56	17

versus manufacturing) that may influence the behavior of loan demand and loan supply. Nevertheless, we do contend that our findings suggest that controlling for the endogeneity of monetary policy is an important concern for any future research seeking to evaluate state-level differences in banks' responses to Federal Reserve decisions.

5. Robustness

In this section, we report the results of robustness exercises performed for our baseline regression estimates presented in tables 3 and 4. First, in section 3 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) changes to the intended federal funds rate to offset the dramatic effects of credit controls introduced by the Carter administration in 1980; (ii) 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 (iii) the Federal Reserve Bank of New York's rescue of the hedge fund LTCM in the late 1990s, which may have prompted similar policy responses. We define three separate dummy variables, one equal to unity for all quarters of 1980, another for all quarters in the period 1990–93 (Ashcraft 2006 uses a similar dummy variable), and a third for all quarters in 1998–99 (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 estimate 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 a 100 bp increase in UM on lending growth at the representative bank increases in absolute size in the quarter-over-quarter results, but decreases in absolute size in the year-over-year results, but in both cases the changes are very small. As such, there is little evidence that the estimates of UM were attenuated in the three episodes considered, though of course the dummy variable exercise we have implemented is only a first step in understanding the impact of endogenous monetary policy changes that are not eliminated from UM via our identification procedure. The interaction coefficients are in line with those presented in table 4.

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.[26] The results

[26]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 fifty-seven 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).

Table 8. Robustness Checks

		Basel & LTCM & Credit Controls				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	−0.7066***	−2.4303***	−1.8086***	−3.0877***	−1.0997***	−3.1595***	−1.3416***	−3.4472***
		(0.1247)	(0.2820)	(0.1210)	(0.2372)	(0.1492)	(0.3039)	(0.1415)	(0.2426)
MBHC	Interaction with Policy	3.5389***	4.8229***	5.8358***	5.7362***	3.2124***	5.0807***	4.9932***	5.6292***
		(0.4251)	(0.7192)	(0.4967)	(0.8833)	(0.5006)	(0.8286)	(0.0672)	(1.1310)
Assets	Interaction with Policy	−0.0103***	0.0236***	0.0052	0.0351***	0.0006	0.0349***	0.0016	0.0166**
		(0.0030)	(0.0056)	(0.0053)	(0.0097)	(0.0031)	(0.0057)	(0.0054)	(0.0082)
Securities	Interaction with Policy	0.4771***	−0.9628***	0.2648	−1.2057***	0.0836	−1.6721***	−0.3485*	−1.9420***
		(0.1198)	(0.1969)	(0.1838)	(0.3781)	(0.1226)	(0.1967)	(0.1919)	(0.2807)
Capitalization	Interaction with Policy	0.1574	0.6414**	0.7815**	0.2111	−0.1596	0.4625*	0.4027	0.6590
		(0.1834)	(0.2826)	(0.3926)	(0.5744)	(0.1853)	(0.2506)	(0.3669)	(0.4302)
Cash	Interaction with Policy	0.4766***	1.2330***	0.0615	1.3950***	0.5224***	1.1576***	0.0432	1.6773***
		(0.1111)	(0.1898)	(0.1880)	(0.4300)	(0.1159)	(0.1774)	(0.1662)	(0.3255)
	R^2	0.3547	0.3501	0.1076	0.1162	0.2545	0.2533	0.0145	0.0156
	Observations	1,435,390	1,435,431	1,467,768	1,467,776	1,434,635	1,434,677	1,459,846	1,459,337

(*continued*)

Table 8. (Continued)

		Predetermined 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)	(0.0083)	(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)
	R^2	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

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 is 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 predetermined 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.[27] 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.

6. 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

[27]The data are 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/.

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 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. In evaluating the new measure of monetary policy, we highlighted episodes in which some endogenous policy changes continue to occur, and also drew attention to examples of the predictability of the new policy measure. Both occurrences may contribute to some bias in our estimates of the lending channel, but we argued that any such biases will be smaller than when using actual federal funds rate changes that do not control for any forms of possible endogeneity.

The results indicate both economically and statistically significant attenuation of estimated lending responses to monetary contractions, accompanied by the shielding of lending associated with multibank 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 changes to the new policy measure, while restricting the transmission of realized federal funds rate changes. One explanation for this result is that many types of securities are subject to an adverse valuation effect following 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.

Throughout the paper we have remained mindful of the limitations of our identification framework and the fact that our estimates from the new measure of monetary policy cannot be said to be free of all forms of endogeneity bias. Instead, our results highlight how controlling for some of the endogenous variation in monetary policy affects empirical estimates of the lending channel. On this basis, we argue that future studies of the banking system and monetary transmission should consider measures to control for endogenous movements in monetary policy. In particular, monetary policy identification in bank-level lending analyses 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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