Borrowers' Financial Constraints and the Transmission of Monetary Policy: Evidence from Financial Conglomerates[*]

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Abstract

Exploring the functioning of internal capital markets in financial conglomerates, this paper conducts a novel test of the credit channel of monetary policy. We look at differences in the response of lending to monetary policy shocks across small banks that are affiliated with the *same* bank holding company but that operate in *different* geographical areas. These banks tap into the same pool of funds but face different pools of borrowers. Because small subsidiary banks concentrate their lending with small local businesses (whose fortunes are tied to their local economies), we can exploit cross-sectional differences in local economic conditions at the time of a monetary policy shock to study whether the strength of borrowers' balance sheets influences the response of bank lending to policy. We find evidence that the negative response of bank loan growth to a monetary contraction is significantly more (less) pronounced when borrowers are more likely to have weak (strong) balance sheets. On the flip side, borrowers with weak balance sheets obtain more new bank credit than other borrowers in monetary expansions. Our results are consistent with the operation of a demand-driven transmission mechanism that works independently of the bank-supply ("lending") channel. In fact, our estimates suggest that borrowers' balance sheet strength accounts for a significant fraction of the "broad credit channel" of monetary policy.

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1 Introduction

Why do small, transitory changes in short-term interest rates often drive lagged responses of the aggregate economy? The excessive sensitivity of output to monetary policy has prompted researchers to look for endogenous mechanisms through which the effects of interest rate changes are amplified. In this vein, recent theories have emphasized the role of informational frictions in tightening financing constraints during monetary contractions.[1] There are two main views on this sort of "credit" transmission mechanism. The *lending channel* presumes that monetary policy affects the *supply* of loans by banks. Draining deposits from banks will reduce lending if banks face financial constraints when attempting to smooth deposit outflows by issuing uninsured liabilities. When lending relationships provide banks with an informational advantage about their borrowers, firms find the credit offered by other sources to be an imperfect substitute. A monetary contraction therefore bears much larger effects on the investment of bank-dependent firms than what is implied by the actual change in interest rates. The *balance sheet channel*, on the other hand, presumes that monetary policy affects loan *demand* through its effect on firms' net worth. Higher interest rates increase debt service, erode firm cash flow, and depress collateral values, exacerbating conflicts of interest between lenders and high information/agency cost borrowers. This deterioration in firm creditworthiness increases the external finance premium and squeezes firm demand for credit.

A number of empirical studies have tried to assess whether financial constraints play any significant role in the transmission of monetary policy. Assuming that organizational size should capture the types of frictions that constrain access to credit, most of those studies compare how firms and banks in different size categories alter their investment and lending behavior following policy changes.[2] Unfortunately, a significant caveat to this literature is that this common identification strategy cannot distinguish between the role of financial constraints in firms that would correspond to the balance sheet channel and those in banks that would correspond to the lending channel. Since small firms are typically bank-dependent, any observation that small firms are hurt the hardest by a monetary contraction cannot distinguish between this being driven by a deterioration in firm creditworthiness or by a general decline in the supply of credit by banks. Identifying the impact

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[1] See Hubbard (1994) or Bernanke and Gertler (1995) for a review of this literature.

[2] Bernanke, Gertler, and Gilchrist (1996) show that small and large firms have significantly different investment, borrowing, and inventory responses to monetary contractions. Findings of the same nature are reported by Kashyap, Lamont, and Stein (1994), Oliner and Rudebush (1996), and Gilchrist and Himmelberg (1998). Using data from banks, Kashyap and Stein (1995, 2000) show that the lending of large commercial banks is significantly less sensitive to monetary policy than that of small banks.

of monetary policy purely along the lines of the size of firms and banks is further compromised by the well-documented evidence that small (large) banks tend to concentrate their lending with small (large) firms. This association makes it hard to disentangle a differential response of loan demand across firm size from a differential response of loan supply across bank size following policy shocks.

The ideal strategy for identifying the lending channel is to look at cross-sectional variations in banks' ability to smooth policy-induced deposit outflows holding constant the characteristics of those banks' loan portfolios. In this vein, recent research shows that small banks that are affiliated with large multi-bank holding companies (BHCs) are effectively 'larger' than their size would suggest *a priori* with respect to the ease with which they smooth Fed-induced deposit outflows (see, e.g., Ashcraft (2001), Campello (2002), and Holod (2003)). Consistent with Kashyap and Stein's (2000) evidence on the behavior of large banks, this recent literature shows that the lending of small subsidiaries of large BHCs is less sensitive to monetary contractions than the lending of other comparable small, independent banks. Those studies argue that, differently from stand-alone banks, members of large BHCs can resort to funds available from conglomerates' internal capital markets to sustain their supply of loans during a contraction.[3]

On the flip side, the ideal strategy for identifying the balance sheet channel is to examine cross-sectional differences in firms' financing constraints holding constant the characteristics influencing the policy-sensitivity of the banks from which those firms borrow. This paper builds on the recent evidence that distributional policies promoted by internal capital markets in large BHCs minimize differences in financial constraints across subsidiary banks to conduct a novel test of the balance sheet channel. When lending is ultimately determined by the marginal cost of funds of the holding company and not of the subsidiary bank, any *differential response* of lending to monetary policy *across subsidiaries* of the same conglomerate must be driven by differences in the response of loan demand and not loan supply. If we isolate and shut down the supply channel by looking at conglomerates that are "immune" to Fed policies, we can then look for evidence that within-BHC shifts in lending activity are influenced by the creditworthiness of firms to which banks lend. Our study accomplishes this by comparing monetary policy responses of similar-size banks that are affiliated with the *same* large financial conglomerate but that face *different* pools of borrowers.

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[3]The most straightforward mechanism through which internal capital markets work is that the holding company could issue uninsured debt on cheaper terms than the subsidiary bank and then downstream funds to the bank. This could be done either via deposits or by purchasing loans from the bank; in either case the transaction would offset the impact of insured deposit outflows. See Mayne (1980) and Ashcraft (2003) for evidence on BHC fund channeling.

The borrowing clienteles are distinguished by looking at the lending of (same-BHC) small affiliates that reside in distinct geographical locations. Because these small subsidiary banks concentrate their lending with small businesses whose fortunes are intrinsically tied to their local economies, we can exploit cross-sectional variations in local economic indicators at the time of a monetary policy shock to gauge whether borrowers' financial strength drives significant changes in bank lending.[4]

In implementing our proposed strategy, we first examine whether there is evidence consistent with significant variations in borrowers' financial strength for banks contained in a comprehensive sample of small subsidiaries of multi-state bank holding companies. This is a necessary first step since we need to verify that depressed local economic activity indeed weakens local borrowers' creditworthiness in lending relationships (as we hypothesize). We do this by looking at the correlation between the business conditions in the localities where those small subsidiary banks operate and the proportion of non-performing loans that they report. Using Hodrick-Prescott-filtered quarterly series on local GDP gap for every U.S. state over a 21-year period, we find that contemporaneous cross-sectional differences in local economic conditions do indeed drive significant differences in the fraction of non-performing loans across subsidiaries of the same BHC.

We then design a test of monetary policy transmission that relates the sensitivity of bank lending to local economic conditions and the stance of monetary policy by combining cross-sectional and times series regressions (as in Kashyap and Stein (2000) and Campello (2002)). Our two-step procedure shows that the negative response of loan growth to a monetary contraction is much stronger for subsidiary banks operating during state-recessions than for subsidiaries of the *same* holding company that operate in state-booms. Put differently, our evidence suggests that *borrowers' strength* drives the allocation of loanable funds — consistent with a credit channel mechanism that is independent of the impact of monetary policy on loan supply. We also look at the implied aggregate magnitudes of our estimates employing a VAR framework. Using the results from our bank-level estimations together with sample moments on bank size and state GDP, we estimate that a temporary 100-basis-point increase in the federal funds rate causes a small subsidiary bank facing a local downturn (i.e., in a state witnessing a one-standard-deviation GDP gap "recession") to cut back on lending by almost 40 percent *more* within one year than a small subsidiary operating

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[4]Our empirical analysis revolves around data on bank loans as opposed to data on borrowers' balance sheets. To the extent that a relationship-lender can do a better job than the econometrician in assessing a borrower's financial strength, examining the joint decision of firms and banks to sign a loan contract in the aftermath of a policy shock might be more appropriate than looking at changes in firm accounting ratios following that same shock.

in a state witnessing no income gap. We conjecture that a significant fraction of the "broad credit channel" of monetary policy transmission can be ascribed to changes in borrowers' creditworthiness over the policy cycle. Our results hold for a number of different proxies for the stance of monetary policy and our conclusions are robust to various changes in the specification of our empirical tests.

Can our results simply mean that small businesses invest and borrow less (more) when their local economies are facing a recession (booming)? The answer is no. Our findings describe a sort of dynamics between borrowers' creditworthiness and monetary policy in which it is not always the case that there is less borrowing in bad (local) times. In fact, our results show that during monetary expansions bank loan growth will be more pronounced in localities witnessing the *worst* economic conditions. This is consistent with a theory that predicts that lower interest rates have the ability of relaxing financing constraints faced by borrowers with relatively poor balance sheets.

How do our results differ from those generated by a neoclassical "interest rate channel" in which higher interest rates mechanically hamper investment and external borrowing? Once again, the difference comes from the double layer of contrasts we use. The neoclassical argument does not allow for a role of financing frictions in influencing the demand for loans. Yet, we show that cross-sectional differences in borrowers' creditworthiness drive asymmetries in borrowing over the monetary cycle that are consistent with the endogenous mechanism behind the balance sheet channel. Some of our results, on the other hand, could be interpreted as just showing that investment is more sensitive to the cost of funds when the expected profitability of capital is low, where weak local business conditions are simply a proxy for low expected return. We develop this point more formally below, showing that this alternative interest rate story could only underlie our empirical results if lagged, short-term deviations from local GDP growth trend were strongly correlated with the expected profitability of investment. We then tackle this story by including in the first stage of our two-stage procedure a proxy that should expunge from our measure of balance sheet strength its predictive power over future business profitability. As it turns out, our results are virtually unaffected by the addition of any measure meant to capture the effects of a competing interest channel story. We find it difficult to argue that our results could simply reflect the consequences of concurrent changes in the hurdle rate of investment and expected local business profitability.

As we discuss in detail below, we design our basic tests so that usual concerns about the endogeneity of lending/borrowing decisions and financial constraints are minimized. This contrasts with comparable existing studies, which typically rely on *ad hoc* auxiliary strategies to help address

endogeneity issues. On the other hand, one potential source of concern for our tests is sample selection. We collect data from banks belonging to certain types of financial conglomerates in order to identify the balance sheet channel of monetary policy. To the extent that financial institutions may *choose* to organize their business in particular ways (e.g., operate in various geographical regions at the same time), one can argue that our inferences could be biased because of particular sample characteristics. In that vein, a selection bias story can be argued along the following lines. Expansionary monetary policies might prompt BHCs to enter new, fast growing markets (states). If a given BHC based in state A sees an opportunity to enter the fast growing loans market of state B when access to reserves is easy, it may change its status from a single-state BHC to a multi-state BHC and thus enter our sample, possibly contaminating our inferences.

Fortunately for our testing design, regulatory constraints have largely prevented banks from pursuing growth strategies of this sort through most of our sample period. Notwithstanding the exogenous, idiosyncratic nature of regulatory constraints on banking activity (e.g., out-of-state branching and M&A laws), we strive to address the concern that sampling could still be a source of biases for our testing strategy in a number of different ways. For example, in some of the experiments below we "intervene" in the sample formation by randomly re-assigning subsidiary banks to different conglomerates before performing our tests. Our principal findings remain unchanged. The same holds when we use a set of Heckman's (1976) procedures to correct for sample selection biases that could stem from regulatory changes and from the monetary policy cycle. We fail to find support for a plausible alternative selection story capable of delivering the same results we find in the data.

Overall, our study suggests the existence of an independent, demand-driven credit channel in the transmission mechanism of monetary policy. Our evidence implies that when engaging in monetary policy the central bank should consider the amplification effects of changes in interest rates on the economy that are triggered by borrowers' financial strength. In particular, our analysis agrees with the belief that monetary tightenings during recessionary periods may further depress activity (Gertler and Gilchrist (1994)). In using bank organizational form as a way to identify the transmission of monetary policy, our findings also add to the growing literature on the role internal capital markets play in the allocation of funds inside financial conglomerates.

The rest of the paper is organized as follows. Section 2 provides a description of our empirical strategy and sampling criteria. Our results are presented in Section 3. A number of robustness checks for our main findings are conducted in Section 4. Section 5 concludes the paper.

2 Empirical Strategy

In order to identify the response of a loan *demand* to monetary policy, it is necessary to eliminate any differences in financial constraints across banks that would drive a differential policy-response of loan *supply*. Such an analysis requires one to look at banks that face similar financial constraints, but that experience differential strength in their borrowers' balance sheets. Our study employs such a strategy to look for evidence on the balance sheet channel of monetary policy. In this section, we describe our identification approach in detail and discuss the characteristics of our sample.

2.1 Identification

The case supporting the balance sheet channel of monetary policy is typically based on the joint predictions that (a) financial factors affect investment demand and (b) innovations in monetary policy affect financial factors. While there is some evidence of the latter empirical fact, the literature has struggled to convincingly demonstrate that financial constraints indeed affect investment. An alternative approach to the problem is to evaluate a different prediction of the theory that might be easier to bring to the data. One such prediction is that monetary policy should have a *relatively* more pronounced effect on the behavior of firms which face more severe financing constraints. Along these lines, Gertler and Gilchrist (1994) document significant differences in the response of small and large firms to monetary contractions. Under the assumption that financial factors would have a disproportionately larger effect on small firm investment, the differential response of investment across firm size could then be attributed to the effects of financing constraints. The weakness of this strategy, though, is that small firms are also more likely to be bank dependent, so in the presence of a lending (supply-side) channel of monetary policy this strategy may not pin down the role of firm balance sheets in the transmission mechanism. The challenge is thus to find a way to shut down the lending channel of monetary policy and simultaneously isolate the demand effect in which one is interested. In this paper, we propose the use of microdata from lenders to achieve this goal.

We model the differential response of lending to monetary policy across banks by explicitly separating the demand- and supply-side effects of monetary policy. As we now explain, this task is made easier by our use of data from financial conglomerates; specifically, from small subsidiaries of large multi-bank BHCs. Let r_t denote the stance of monetary policy as of time t. Eq. (1) writes

the response of loan growth to policy for an individual bank i that is part of BHC j at time t:

$$\frac{\delta \Delta ln(Loans)_{ijt}}{\delta r_t} = \alpha_0 + \alpha_1 D_{ijt}^{bs} + \alpha_2 D_{ijt}^{nonbs} + \alpha_3 S_{ijt}^{bank} + \alpha_4 S_{ijt}^{BHC} + v_{ijt}. \qquad (1)$$

Differences in the response of loan demand across banks are captured by D_{ijt}^{bs} and D_{ijt}^{nonbs}, which correspond to balance sheet and non-balance sheet effects, respectively. The first of these demand components relates to the response of loan demand to monetary policy that is governed by the strength of borrower creditworthiness. The second captures differences in the response of loan demand to monetary policy that are driven by underlying characteristics of the borrowers in a market, such as the sensitivity of product demand to interest rates. It is safe to assume that such characteristics (given by industrial structure, demand elasticity, etc.) evolve quite slowly over time and are essentially fixed over short intervals. And accordingly, in implementing our tests we exploit high-frequency variations in borrowers' demand for loans that are induced by short-run changes local business conditions. Differences in the response of loan supply across banks are driven by differences in the severity of financial constraints at the bank level, S_{ijt}^{bank}, or at the holding company level, S_{ijt}^{BHC}.[5] These latter controls capture lending channel effects, where financial constraints affect the ability of banks to replace outflows of insured deposits with funds from other sources.

Given the appropriate data on each of the regressors, estimating Eq. (1) via OLS would recover the correlation between firm balance sheet strength and the response of bank lending to monetary policy through the estimate of α_1. The problem with this strategy, though, is lack of data on relevant dimensions of some of the regressors. In particular, there are likely to be unobserved components of S_{ijt}^{bank} and S_{ijt}^{BHC} that are correlated in the short run with the observed dimensions of borrowers' balance sheet strength D_{ijt}^{bs}, in which case the OLS estimation will be compromised by an omitted variables-type bias.

We attempt to minimize the problems involved in the estimation of α_1 from Eq. (1) using a series of devices. First, following the insight from recent evidence on the bank lending channel we restrict our sample to banks that are affiliated with large financial conglomerates. Kashyap and Stein (2000) show that large commercial banks are largely "immune" to monetary policy shocks, as their ability to tap into non-reservable sources of funds at low cost allows them to shield their lending from Fed-induced contractions. Ashcraft (2001) and Campello (2002) further show

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[5]Dependence on holding company-level financial strength is induced by regulation requiring that financial conglomerates must operate on consolidated basis (see Houston, James, and Marcus (1997) and Ashcraft (2003)).

that, just like large banks, subsidiaries of large BHCs are far less constrained than comparable independent banks during contractions. According to those authors, this happens because of the workings of active, efficient internal capital markets inside large conglomerates.[6] Based on these findings, our initial sample restriction alone should all but eliminate the importance of bank (supply-side) financial constraints in explaining the response of lending to policy, allowing us to disregard S_{ijt}^{bank} and S_{ijt}^{BHC}. We, however, weaken such an assumption and estimate Eq. (1) including a set of controls which, according to the lending channel literature, should exhaust the sources of variation in bank-level financial constraints: capitalization, size, and liquidity. In the end, α_3 and α_4 should be very small (if not zero) so that even if there are unobserved dimensions of bank/BHC financial constraints, any correlation of these unobservables with firm balance sheet strength will be mitigated.[7]

The second device we employ to mitigate the omitted variables problem is to focus the analysis on the *difference* between a subsidiary's response to monetary policy and that of the other banks affiliated with the *same* holding company. Focusing on within-conglomerate comparisons is useful because it eliminates financial constraints at the BHC-level from the equation, purging a potential source of biases, and also minimizes any residual differences in financial constraints across the banks in our sample. Define Ω_{ijt}^x as the difference between a subsidiary's x_{ijt} and its holding company mean in a given quarter. We can re-write Eq. (1) in differences from the holding company mean:

$$\frac{\delta\Omega_{ijt}^{Loans}}{\delta r_t} = \beta_1\Omega_{ijt}^{D^{bs}} + \beta_2\Omega_{ijt}^{D^{nonbs}} + \beta_3\Omega_{ijt}^{S^{bank}} + v_{ijt}. \tag{2}$$

Once we have minimized supply-driven differences in loan-policy responses, the next device we use is to isolate independent sources of variations in loan demand. Arguably, depressed economic activity within a state will lead to a deterioration in local borrowers' creditworthiness, as small local businesses' fortunes (cash flows, collateral values, etc.) are intrinsically tied to their local economies. Our identification scheme is complete if we can assume that small local businesses concentrate their borrowing with small banks. Fortunately, such an assumption is well-supported

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[6]While limited liability and private information about loan quality are a source of financing frictions for small stand-alone banks, one can argue under the Federal Reserve's Source of Strength policy that the BHC owners of small banks face full liability for their subsidiary's debts. This is an important check for asset substitution incentives by the subsidiary as large BHCs have massive amounts of resources relative to their small subsidiaries. In our sample, the 90th percentile of the subsidiary-to-BHC size ratio is less than five percent, suggesting that it would not be much of a burden for a parent to provide assistance to a troubled subsidiary.

[7]Note that omitted variables bias depends *both* on the correlation of the omitted variable with the variable of interest and on the coefficient on the omitted variable in the original model. As this coefficient goes to zero, the bias created by any correlation with the variable of interest also goes to zero.

by research on business lending practices of small and large banks.[8] Our tests essentially isolate differences in borrowers' strength across members of a given banking conglomerate $(\Omega_{ijt}^{D^{bs}})$ by looking at data from small subsidiaries of large multi-state BHCs — i.e., we compare policy responses of similar-size banks that tap into funds of the same conglomerate but that face different pools of borrowers. By design, our proposed strategy revolves around short-run observable cross-sectional variations in demand for business loans that are largely unrelated with non-balance sheet effects, and thus our basic estimations largely ignore those effects. In the robustness exercises, nonetheless, we deal with the most natural threats to this assumption, finding little change in results.

2.2   Data

The microdata used in this paper come from banks. We collect quarterly accounting information on the population of insured commercial banks from the Federal Reserve's *Call Report of Income and Condition* over the 1976:I-1998:II period, using a version of the data compiled by the Banking Studies Function of the Federal Reserve Bank of New York. After a initial screening, we retain only bank-quarters with positive values for total assets, loans, and deposits. Details about the construction of the panel data set and formation of consistent time series are given in Appendix A.

The single most important bank-level variable used in our analysis is loan growth. This variable is defined as the quarterly time series difference in the log of total loans. We use the bank merger file published online by the Federal Reserve Bank of Chicago to remove any quarter in which a bank makes an acquisition. This reduces measurement problems with the differenced data. In addition, we eliminate bank-quarters with loan growth exceeding five standard deviations from the mean. Since the regressions below include four lags of loan growth as explanatory variables, the sample is limited to banks having at least five consecutive quarters of data. The first five quarters of our data set are lost in order to construct lagged dependent variables and appropriate differences.

Our analysis focuses on the lending of small banks. This sample restriction is made in order to best match the market (i.e., the state) in which the bank is chartered with regional business conditions.[9] Similar to previous studies, we define as "small banks" those banks in the bottom

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[8]See, among others, Nakamura (1994), Strahan and Weston (1998), and di Patti and Gobbi (2001).

[9]Extant research suggests that large banks' loan opportunities are poorly measured by the economic conditions of the states in which they are chartered. The state-level economy, on the other hand, provides for a good proxy for the business environment that small banks face when making lending decisions. We provide evidence consistent with this in Section 3.1.

95th percentile of the assets size distribution of all observations in a given quarter.[10] The second restriction we impose on the data is to retain only small banks that are part of multi-bank holding companies which control at least one large bank (i.e., a bank in the top 5th percentile of the asset distribution). Next, we require that small banks must be affiliated with holding companies that have subsidiaries residing in at least two different U.S. states during the same quarter. These restrictions leave 39,892 observations from banks in our data set. The distribution of the number of bank-quarters in our sample of multi-state BHC subsidiaries is reported in Table 1. The table shows a steady increase in the number of observations in each quarter until the advent of problems in the banking industry in the late 1980s. During the last decade, consolidation within the industry (and inside BHCs) has reduced the number of small banks affiliated with large BHCs.[11]

– insert Table 1 here –

The first column of Table 2 reports the mean and standard deviation of the bank-level variables used in our tests. The statistics in the first column of the table are for the small banks that are included in the sample. The figures for basic balance sheet information such as size, loan growth, leverage, etc., are similar to those reported in other studies on small subsidiary banks (e.g., Campello (2002)). Banks in our final sample display a quarterly loan growth average of 1.57 percent with a standard deviation of 7.6 percent. Note that the standard deviation of long-run loan growth and non-performing loans are similar in magnitude to the long run means, implying that there are significant variations across banks in long-run average loan growth and non-performing loans.

As we discuss below, one could be concerned with the fact that our data selection criteria may create sample biases that affect our inferences. To check whether the observations in our sample are "unique" in some obvious way, we also compute descriptive statistics for the variables of interest using the population of small banks that are left out of our sample. These are displayed in the second column of Table 2. Comparisons based on those statistics suggest that one would find it difficult to argue that small subsidiaries of multi-state BHCs are much different from other banks

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[10]This particular small bank cutoff is used by Kashyap and Stein (2000). Results are qualitatively similar when we employ other size cutoff criteria used in previous empirical work on the lending channel, such as the 90th and 75th asset size percentiles (e.g., Jayaratne and Morgan (2000)).

[11]Without weighting these trends, statistics constructed on this sample would place an unusual amount of weight on the first decade of data. As the analysis below is done quarter by quarter via a two-step procedure, this will not be a concern. The potential impact of deregulation on our sample (and on our results) is explicitly considered below.

in the same size category. We, however, will revisit the issue of sample selection in Section 4.

$$- \text{ insert Table 2 here } -$$

Finally, our analysis also necessitates data on the stance of monetary policy and on the business environment in which the small affiliate banks in our sample operate. The measures of monetary policy we use are fairly standard and are described in detail in Appendix B. Most of these policy measures are constructed with series available online from the Federal Reserve Bank of St. Louis. In order to measure local business conditions we use nominal state income series available online from the Bureau of Economic Analysis. Deviations from the long-run economic growth trend in each state are used to characterize state-recessions and state-booms. Specifically, a state income gap ($YGap$) is constructed by applying a Hodrick-Prescott filter (bandwidth of 1600) to the time series difference of the log of total state income. The filtering is performed for each state and the District of Columbia. In our tests, a positive (negative) $YGap$ — i.e., a positive (negative) short-term deviation from a state's secular growth trend — will indicate a "state-boom" ("state-recession").

3 Results

3.1 Local Business Conditions and Bad Loans

In order to substantiate our testing strategy we need to find evidence that depressed economic activity indeed depresses borrowers' creditworthiness in lending relationships. To our knowledge, there are no publicly available data on small firms' (or individuals') borrowings that serve our purposes. On the other hand, we have data on the loan portfolio of their banks. In establishing a link between the local economic environment and borrowers' balance sheets, we argue that an unexpected deterioration in borrowers' circumstances should show up in the quality their banks' loan portfolios. We examine this working hypothesis in turn.

For each bank i affiliated with the BHC j at time t, let $\Omega_{ijt}^{BadLoans}$ denote the difference between a subsidiary's bad loans (i.e., the ratio of non-performing to total loans) and the average bad loans of all other small banks in the same BHC. Similarly, define Ω_{ijt}^{YGap} as the difference between a subsidiary's state income gap and the average income gap of all other small banks in its BHC:

$$\Omega_{ijt}^{BadLoans} = BadLoans_{ijt} - \overline{BadLoans_{jt}}, \tag{3}$$

$$\Omega_{ijt}^{YGap} = YGap_{ijt} - \overline{YGap_{jt}}. \tag{4}$$

The issue of interest is whether subsidiaries operating in states with relatively poorer economic conditions report a greater fraction of problem loans. We use the following empirical model to tackle this question on quarterly data:

$$\Omega_{ijt}^{BadLoans} = \eta + \sum_{k=1}^{4} \lambda_k \ Local \ Shock_{ijt-k} + \Omega_{ijt-k}^{\mathbf{X}} + \sum_t \mu_t 1_t + \varepsilon_{ijt}. \qquad (5)$$

The four lags of shocks to local economic conditions (*Local Shock*) are meant to capture the relative strength of the balance sheets of the subsidiary banks' borrowers. For robustness, we measure these income shocks in two ways: (a) simply as the state income gap (*YGap*), and (b) as the correspondent "relative-to-BHC" income gap measure (Ω_{ijt}^{YGap}, from Eq. (4)). The set of controls included in $\mathbf{X}$ is composed of lagged log bank assets, the lagged bank equity ratio, and the lag of bank liquid-to-total assets ratio (see Appendix A). The μ coefficients absorb time-fixed effects. We are, of course, interested in the relationship between a small subsidiary's ratio of bad loans and the financial status of the borrowers in its market, which is captured by $\sum \lambda_k$.

We report the estimates returned for $\sum \lambda_k$ from Eq. (5) in the first column of Table 3. Panel A uses the state income gap $YGap_{ijt}$ as the local income shock proxy, while Panel B uses Ω_{ijt}^{YGap}. The results from both panels agree with our intuition. The most conservative estimate in the table (-0.024) implies that an *increase* in the state income gap by one standard deviation (about 2.5 percentage points) for one quarter *reduces* the fraction of bad loans in a small bank's loan portfolio by about 6 basis points after four quarters. This figure represents nearly 5 percent of the sample mean estimate for problem loans.

Although the results from Eq. (5) seem to confirm our expectations, we note one potential limitation with that specification is that it exploits both permanent and transitory differences in the fraction of bad loans across subsidiaries. In principle, we are interested in bad loans created by what are temporary changes in local economic conditions, so it makes sense to purge long-run individual subsidiary bank effects. This can be accomplished by separating out bank-level long-run differences relative to the BHC, defining $\Omega_{ijt}^{\widetilde{BadLoans}}$ as follows:

$$\Omega_{ijt}^{\widetilde{BadLoans}} = \Omega_{ijt}^{BadLoans} - \overline{\Omega_{ij}^{BadLoans}}. \qquad (6)$$

We re-examine the question of relative loan performance, now only exploiting transitory differences in bad loans across subsidiaries, by estimating the following "double-difference" equation:

$$\Omega_{ijt}^{\widetilde{BadLoans}} = \eta + \sum_{k=1}^{4} \lambda_k \ Local \ Shock_{ijt-k} + \beta \Omega_{ijt}^{\mathbf{X}} + \sum_t \mu_t 1_t + \varepsilon_{ijt}. \qquad (7)$$

The results from this last estimation are reported in the second column of Table 3. There continues to exist strong evidence that differences in the state income gap are correlated with differences in non-performing loans across bank subsidiaries of multi-sate BHCs. We interpret these results as supporting evidence for using the state income gap as a proxy for borrower creditworthiness in lending relationships with small subsidiary banks.

$$- \text{ insert Table 3 here } -$$

3.2 Local Business Conditions and Monetary Policy Effects

We have verified that cross-sectional differences in economic conditions amongst the various markets in which a conglomerate operates correlate with differences in the loan quality (indicative of borrowers' financial strength) amongst the various subsidiaries of the conglomerate. We now turn to the main question of the paper: Whether there's a balance sheet channel of monetary policy.

To investigate this transmission mechanism we use a two-step approach which resembles that of Kashyap and Stein (2000) and Campello (2002). The idea is to relate the sensitivity of bank lending to local economic conditions and the stance of monetary policy by combining cross-sectional and times series regressions. The approach sacrifices estimation efficiency, but reduces the likelihood of Type I inference errors; that is, it reduces the odds of concluding that borrowers' finances matter when they really don't.[12]

Define Ω_{ijt}^{Loans} as the difference between a small subsidiary lending and the average loan growth of all other small banks in the same conglomerate. The first step of our procedure consists of running the following cross-sectional regression at each quarter t in our sample period:

$$\Omega_{ij}^{Loans} = \eta + \sum_{k=1}^{4} \pi_k \Omega_{ijt-k}^{Loans} + \sum_{k=1}^{4} \gamma_k \ Local \ Shock_{ijt-k} + \beta \Omega_{ijt-1}^{\mathbf{X}} + \varepsilon_{ij}. \tag{8}$$

As in Eq.(5), $Local \ Shock$ is alternatively measured as either (a) $YGap$ or (b) Ω_{ijt}^{YGap}. The set of controls in $\mathbf{X}$ includes lagged log bank assets, the lagged bank equity ratio, and the lag of bank liquid-to-total assets ratio. To explicitly account for the individual idiosyncratic effects discussed in Section 3.1, we also estimate the following double-differenced equation:

$$\widetilde{\Omega_{ij}^{Loans}} = \eta + \sum_{k=1}^{4} \pi_k \widetilde{\Omega_{ijt-k}^{Loans}} + \sum_{k=1}^{4} \gamma_k \ Local \ \widetilde{Shock}_{ijt-k} + \beta \widetilde{\Omega_{ijt-1}^{\mathbf{X}}} + \varepsilon_{ij}, \tag{9}$$

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[12] An alternative one-step specification — with Eq. (10) below nested in Eq. (8) — would impose a more constrained parameterization and have more power to reject the null hypothesis of borrowers' finances irrelevance. However, tests of coefficient stability indicate that the data strongly rejects those parameter restrictions.

where $\widetilde{\Omega_{ijt}^{Loans}} = \Omega_{ijt}^{Loans} - \overline{\Omega_{ij}^{Loans}}$, and similarly for the remaining variables.

From each sequence of cross-sectional regressions, we collect the coefficients returned for $\sum \gamma_k$ and 'stack' them into the vector Ψ_t, which is then used as the dependent variable in the following (second-stage) time series regression:[13]

$$\Psi_t = \alpha + \sum_{k=1}^{8} \phi_k MP_{t-k} + \sum_{k=1}^{8} \mu_k \Delta ln(GDP)_{t-k} + \sum_{k=1}^{3} \sigma_k Q_k + \rho Trend_t + u_t. \qquad (10)$$

We are interested in the impact of monetary policy, MP, on the sensitivity of loan growth to borrower balance sheet strength. The economic and the statistical significance of the impact of monetary policy in Eq. (10) can be gauged from the sum of the coefficients for the eight lags of the policy measure ($\sum \phi_k$) and from the p-value of this sum. Since there is no consensus about the most appropriate measure of the stance of monetary policy, we use five alternative proxies in all estimations we perform: (a) the Fed funds rate (Fed Funds); (b) the spread between the rates paid on six-month prime rated commercial paper and 180-day Treasury bills (CP-Bill); (c) the spread between the Fed funds rate and the rate paid on 10-year Treasury bills (Funds-Bill); (d) the log change in non-borrowed reserves (NonBorrowed); and (e) Strongin's (1995) measure of unanticipated shocks to reserves (Strongin). All monetary policy measures are transformed so that increases in their levels represent Fed tightenings. Because policy changes and other macroeconomic movements often overlap, we also include eight lags of the log change in real GDP in the specification. This allows us to check whether policy retains significant predictive power after conditioning on aggregate demand.[14] The variable Q corresponds to quarter dummies, and $Trend$ represents a time trend.

Figure 1 plots the empirical distribution of the coefficient of interest from the first-stage regressions, $\sum \gamma_k$. We perform the first-stage estimations of our two-step procedure in four different ways (see below), which yields a total of 364 coefficient realizations. As could be expected, those regressions return positive estimates in most runs. The mean (median) $\sum \gamma_k$ equals 0.078 (0.027) and is statistically different from zero at the 0.1 (0.1) percent level. A positive coefficient indicates

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[13]To see how this procedure accounts for the error contained in the first-step, assume that the true Ψ_t^* equals what is estimated from the first-step run (Ψ_t) plus some residual (ν_t): $\Psi_t^* = \Psi_t + \nu_t$. One would like to estimate Eq. (10) as $\Psi_t^* = \alpha + \mathbf{X}\theta + \omega_t$, where the error term would only reflect the errors associated with model misspecification. However, the empirical version of Eq. (10) uses Ψ_t (rather than Ψ_t^*) on the right hand-side. Consequently, so long as $E[\mathbf{X}'\nu] = 0$, α will absorb the mean of ν_t, while u_t will be a mixture of ν_t and ω_t. Thus, the measurement errors of the first-step will increase the total error variance in the second-step, but will not bias the coefficient estimates in θ.

[14]Our results remain virtually unchanged when we include changes in the rate of inflation (lagged changes in CPI) in the specification of Eq. (10).

that there is more demand for credit in states where business conditions are more favorable, which agrees with intuition. But note that $\sum \gamma_k$ also take on negative values. This suggests that under certain circumstances we might find banks giving out relatively more loans to borrowers with weaker balance sheets. This could happen when favorable interest rate movements have a particularly stronger impact on weaker borrowers' financing constraints — the ease of the monetary policy significantly relaxes those borrowers' (binding) constraints. Although noteworthy, the distribution of $\sum \gamma_k$'s alone don't say much about the dynamics of the transmission of monetary policy.

$$- \text{ insert Figure 1 here } -$$

The main results of the paper are shown in Table 4. The table reports the sum of the coefficients for the eight lags of the monetary policy measure ($\sum \phi_k$) from Eq. (10), along with the p-values for the sum. Heteroskedasticity- and autocorrelation-consistent errors are computed with Newey-West lag window of size eight in all regressions. The table summarizes the results of twenty two-step estimations (four different first-stage regressions × five monetary policy measures). The estimations in Panel A use the state income gap $YGap_{ijt}$ as the proxy for local borrowers' financial status (*Local Shock*), while those in Panel B use Ω_{ijt}^{YGap} (the difference between the income gap facing a subsidiary and the average gap of all other subsidiaries of the same BHC) as the relevant borrower proxy. The first row of each panel reports results from regressions that use Ω_{ijt}^{Loans} (the relative-to-BHC subsidiary loan growth) as the dependent variable (see Eq. (8)), while those in the second row use $\widetilde{\Omega_{ijt}^{Loans}}$ (the double-differenced Ω_{ijt}^{Loans}) as the dependent variable (Eq. (9)).

All of the estimates reported in Table 4 suggest that borrowers' financial status influence the response of bank lending to monetary policy along the lines of the balance sheet channel. This is remarkable given the relatively limited time dimensionality of our sample — our times series regressions have only 84 observations — and the well-documented differences in the time series properties of policy measures that are based on rates of interest and those that are based on monetary aggregates. Of those estimates, ten (five) are significant at the 9.6 (3.9) percent level or better. The coefficients for the most conventional measure of policy, the Fed funds rate, are all significant at better than the 6.5 percent level.

$$- \text{ insert Table 4 here } -$$

In order to interpret the economic significance of the estimates in Table 4 at the individual bank level, it is necessary to design a baseline policy experiment. Consider the scenario in which

the central bank increases the funds rate by 50 basis in four consecutive quarters, implying a 200 basis point change over the entire horizon. Using the most conservative of our Fed funds rate estimates (0.031), a one standard deviation deterioration in the state income gap (0.025) would amplify the impact of the contraction on bank loan growth by some 15 basis points in the current quarter alone. To see what this result would imply in dollar terms, consider two subsidiaries of the same BHC, both with a loan portfolio equal to $100 million (about the average figure for banks in our sample as of 1998:II). Suppose one of the subsidiaries operates in a state where the income gap is one standard deviation above its average while the other operates in a state where the income gap is one standard deviation below average. Then, a 50 basis point increase in the Fed funds rate sustained over one year would lead the bank facing a local downturn to cut back on lending by $300,000 *more* in the current quarter than the bank facing a local boom.

Table 5 describes the 'impulse-response' of the amplification mechanism reported in Table 4 using the federal funds rate as the measure of monetary policy. The rows of Table 5 are similar to those in the previous table, while the columns correspond to the point estimate and p-value for sum of coefficients of different lags of the funds rate. Those estimates indicate that the bulk (nearly half) of the amplification effect implied by the balance sheet channel takes place immediately after a policy change. They also show that the effects of monetary policy on bank lending that are induced by borrowers' weakening is very persistent through time. The timing and duration of the credit channel effects reported in the table are comparable to those in Gertler and Gilchrist (1994).

<div align="center">– insert Table 5 here –</div>

3.3 Assessing the Economic Impact of the Balance Sheet Channel

The preceding calculations only provide an assessment of the differential effect of monetary policy on the loan portfolios of two hypothetical banks rather than an estimate of the aggregate impact of the channel we have isolated. Measuring the economic magnitude of *any* form of transmission mechanism is not an easy task (see, e.g., Gertler and Gilchrist (1994) and Kashyap and Stein (2000)). However, it is important to further characterize the economic significance of the transmission mechanism we uncovered. We do this via a structural VAR approach that draws on Bernanke and Blinder (1992). Our VAR system contains three variables (loans, real GDP, and the federal funds rate) and eight lags of each variable are included in each equation. We identify the effect of monetary policy using the standard ordering assumption that the funds rate has no immediate

impact on loans or real GDP. In order to ensure a stationary system, we use the percentage change in loans ($\Delta ln(L_t)$) and real GDP ($\Delta ln(Y_t)$) as well as the change in the federal funds rate (Δi_t) during estimation. Using quarterly data from 1947:I through 2002:III, the reduced-form model can be written as follows:

$$\begin{pmatrix} \Delta ln(L_t) \\ \Delta ln(Y_t) \\ \Delta i_t \end{pmatrix} = \sum_{j=1}^{8} \begin{bmatrix} \pi_j^{LL} & \pi_j^{LY} & \pi_j^{Li} \\ \pi_j^{YL} & \pi_j^{YY} & \pi_j^{Yi} \\ \pi_j^{iL} & \pi_j^{iY} & \pi_j^{ii} \end{bmatrix} \begin{pmatrix} \Delta ln(L_{t-j}) \\ \Delta ln(Y_{t-j}) \\ \Delta i_{t-j} \end{pmatrix} \tag{11}$$

The estimated VAR coefficients from Eq. (11) imply that a 1 percentage point increase in the federal funds rate leads to a decrease in real output of 0.48 percent and aggregate bank loans of 0.28 percentage points after one year. After four quarters, the federal funds rate has been cut by about 33 basis points. Since the above VAR estimation measures the average response of the economy to an innovation in the funds rate, and on average the income gap is equal to zero, one can think of the estimated impulse-response measures as the response of bank lending to monetary policy when the income gap is equal to zero. Armed with an estimate of how much an innovation to monetary policy affects bank lending, it is now possible to evaluate the importance of firm balance sheets in the transmission mechanism.

We use the VAR-implied path of the federal funds rate (from (11)), the estimated coefficients on the federal funds rate from Table 4, and a one-standard-deviation decrease in the state income gap (2.5 percent) to compute the response of lending to policy when local borrowers are particularly more and less financially constrained. Figure 2 illustrates the estimated response of commercial bank loans to a 100-basis-point innovation in the Fed funds rate in states with null income gap ($YGap = 0$) — see the dashed line — and in states where borrowers' balance sheets are weak ($YGap = -0.025$); see solid line. After four quarters, loan growth falls by an *extra* 16 basis points in response to the policy tightening when banks face constrained borrowers with weaker balance sheet. This result is remarkable: the VAR estimates suggest that banks facing weaker borrowers during a Fed-induced tightening contract their loans some by 40 percent *more* than those banks facing comparable borrowers with "average" balance sheet strength. And those effects are persistent: eight quarters after the initial monetary contraction, *aggregate lending* falls by some 0.9 percentage points while lending to firms with weak balance sheets drops by over 1.3 percent.

− insert Figure 2 here −

In interpreting these estimated magnitudes, one must recognize that the our results seem to

17

imply a relatively small shock to bank credit, but crucially, that the overall response of the real economy to monetary policy, too, is generally small (see Bernanke and Gertler (1995) for a survey). In fact, what our study suggests is that a significant fraction of the "broad credit channel" of monetary policy transmission can be attributed to changes in borrowers' creditworthiness (or balance sheet strength) over the monetary policy cycle. We believe that the effects we report should be considered by future researchers and policymakers.

4 Robustness

Although our approach resembles Kashyap and Stein's (2000) two-step procedure, our analysis is far less subject to the types of simultaneity biases discussed in their paper. Specifically, while our second-stage times series regressions are similar to those used by Kashyap and Stein, their paper's first-stage regressions involve estimating the sensitivity of a bank's choice variable (lending) to another *endogenous* variable (liquidity). Our first-stage regressions, in contrast, involves estimating the sensitivity of lending to local economic conditions, which are *exogenous* to the bank's choice set. This relieves us from having to consider whether our results could be explained away under various scenarios in which banks may choose to behave in a particular way (say, they may hold more liquid assets) when they know their borrowers to be especially sensitive to monetary policy or business cycles. Our approach, on the other hand, is subject to other types of criticisms. We address them in this section.

4.1 The Interest Rate Channel

Our results suggest that as basic interest rates go up the sensitivity of (within-BHC) subsidiary lending to local economic conditions increase, because monetary contractions bring about a more pronounced deterioration of the creditworthiness of borrowers with weaker balance sheets. However, our estimates could also be interpreted as just saying that investment is more sensitive to the cost of capital when the expected profitability of capital is low, where weak local business conditions are simply a proxy for low expected return. To formalize this point, we build upon the neoclassical interest channel to write an alternative model that could deliver empirical results that are similar to ours, but that do not hinge on financial frictions.

Consider a simple setting in which a firm uses capital k in the production of output y according the production function $y = \theta_t f(k)$; where we assume that capital is productive $f'(k) \geq 0$, subject

to diminishing returns $f''(k) \leq 0$, and has time-varying profitability 'scaler' $\theta_t > 0$. If the cost of capital is equal to r_t, then the firm chooses the first-best capital k_t^* according to

$$\theta_t f'(k_t^*) = r_t. \tag{12}$$

Investment I_t is simply the difference between the desired k_t^* and current k_{t-1} capital stock.

If there is a one-to-one correspondence between monetary policy and the cost of capital, then the response of investment to monetary policy is given by

$$\frac{\delta I_t}{\delta r_t} = \frac{1}{\theta_t f''(k_t^*)}. \tag{13}$$

Note that the response of investment to monetary policy depends on the scaling factor for the profitability of investment θ_t. In particular, θ_t has a larger effect on the marginal product of capital at low levels of capital (i.e., when the marginal product of capital $f'(k)$ is high) than at high levels of capital (when $f'(k)$ is low). It follows that economic conditions may not only affect the position of the marginal product of capital schedule, but also its slope, and it does so in a way that monetary policy has a larger effect on investment when expected profitability is low. Assuming that past local economic performance correlates strongly with future expected profitability of investment, one could argue that our main empirical findings mechanically reflect the outcomes from concurrent movements in businesses' cost of funds and expected profitability that shift the local demand for loans in ways that are unrelated to financing frictions.

One way to address this alternative story is to include a proxy for the profits associated with local business financing in the first stage of our two-stage procedure. The idea is that the profitability of loans made out to local businesses shall capture some of the rents from the underlying investment opportunities.[15] It is not immediately obvious where to find this proxy, but one such measure is used by Houston, James, and Marcus (1997) in their study of subsidiary lending and internal capital markets in BHCs: subsidiary income (or cash flows from operations) plus loan loss provisions divided by loans. We re-estimate our two-step regressions using that proposed proxy for cash flows in the first step and collect the results from the second step in Table 6 (exactly as we do in Table 4). The results in Table 6 suggest that controlling for the profitability of business financing has little effect on our estimates — the newly reported coefficients are very similar to

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[15]This identifying assumption finds support in an extensive theoretical and empirical banking literature on the allocation of rents between lenders and opaque — in particular, small firms — borrowers (see, among others, Rajan (1992) and Petersen and Rajan (1995)).

those from our baseline tests.

<center>– insert Table 6 here –</center>

Although adding Houston et al.'s measure to our first step cross-sectional regressions might help tackle the issue of differences in expected business profitability across bank localities, one problem with those authors' measure is the potential for endogenous biases arising from the joint decision to lend (the denominator of their measure) and to add to loss provisions (in the numerator). As a robustness check, we perform the same tests of Table 6 using ROA in the first stage regression as opposed to Houston et al.'s measure. The new results are reported in Table 7. It is again apparent that our results cannot be easily explained away by an interest rate channel story with asymmetric cyclical effects.

<center>– insert Table 7 here –</center>

4.2   Sample Selection: Heckman Correction

Arguably, one potential source of concern for our tests is sample selection. In particular, we sample from the population of commercial banks only those banks belonging to certain types of financial conglomerates. To the extent that those financial institutions may *choose* to organize their business as multi-bank firms and decide whether or not to operate in various geographical regions at a given point in time, one can argue that our data do not come from a random sample of banks. If our sample of banks was constant over the entire sample period, this would not be an important issue as inferences could simply be done conditional on the sample. The potential problem is that the sample changes in non-random ways over time as bank holding companies acquire other institutions and consolidate their subsidiaries into larger banks.

A selection bias story can be argued along the following lines. Expansionary monetary policies might prompt BHCs to expand into new markets (states). If a given BHC with operations in Massachusetts sees an opportunity to enter the fast growing loans market of New York when access to reserves is easy it may change its status from a single-state BHC to a multi-state BHC, thus entering our sample. Changing the number of banks in the holding company via mergers and acquisitions might change the average sensitivity of lending to economic conditions unless the holding adds a bank with a sensitivity exactly at the other subsidiaries' mean. Thus the main threat to identification from sample selection is that during a monetary expansion BHCs are acquiring small banks with a low sensitivity of loan growth to income growth. Of course, such

<center>20</center>

a story would require a bank's acquisition strategy to quickly reverse itself with reversals in the stance of monetary policy, which seems unlikely. But a more general argument linking geographic diversification to local economic conditions and the monetary policy cycle could pose a challenge to our main conclusions.

Our first line of defense against this argument comes from the fact that the secular movements towards deregulation of conglomerate activities and bank mergers are already captured in our second-stage regression through the included trend. As it turns out, this regressor never shows any statistical significance. Our second (more formal) strategy in addressing that argument consists of a couple of Heckman-type corrections for sample selection. We explain the details in turn.[16]

Let y_i correspond to the sensitivity of loan growth to interest rates. We are interested in how this sensitivity changes in response to the state income gap, which is in the subset of regressors x_i:

$$y_i = \beta x_i + \varepsilon_i. \tag{14}$$

Our problem in estimating Eq. (14) is that we do not have a random sample of banks and it is possible that bank holding companies tend to acquire banks operating under specific circumstances (e.g., during local economic booms).

Define z_i^* an indicator function for being part of the sample, and w_i the set of variables which affect this probability

$$z_i^* = 1(\gamma w_i + u_i > 0). \tag{15}$$

It is standard to assume u_i and ε_i as bivariate normal random variables with zero mean, variances σ_u and σ_ε, respectively, and correlation ρ. The conditional expectation of y_i for the observations in our sample can be written as

$$E[y_i|z_i^* > 0] = \beta x_i + E[\varepsilon_i|z_i^* > 0] = \beta x_i + \beta \lambda_i(-\gamma w_i), \tag{16}$$

where $\lambda_i(z) = \frac{\phi(z)}{1-\Phi(z)}$, with $\Phi(\cdot)$ ($\phi(\cdot)$) defining the normal cumulative (density) function. We are interested in the average marginal effect of x_{ik} on y_i, but do not observe the variable $\lambda_i(-\gamma w_i)$. The equation below shows that an OLS estimation suffers from omitted variables bias if there is any correlation between regressors in Eqs. (14) and (15):

$$\frac{\delta E[y_i|z_i^* > 0]}{\delta x_{ik}} = \beta_k - \gamma_k \rho \sigma_\varepsilon [\lambda_i^2(-\gamma w_i) - \gamma w_i \lambda_i(-\gamma w_i)]. \tag{17}$$

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[16]The next subsection explores yet another strategy to address any potential concerns with sample selection.

The Heckman (1979) correction for this problem consists of a first-stage probit of a dummy indicating selection into the sample on variables driving selection. From this selection equation it is possible estimate the omitted variable in Eq. (16) using $\lambda_i(\hat{\gamma}w_i)$. One can then estimate the original Eq. (14) including this predicted value as an extra regressor for consistent estimates of β.

We employ two Heckman-correction strategies to deal with concerns about sample selection in our analysis. First, we try to capture the impact of deregulation on geographic diversification and sample inclusion. Several states did not permit the operation of multi-bank holding companies until the mid 1980s, and until the late 1980s there were several restrictions on BHC's ability to acquire out-of-state banks. As we noted above, the inverted U-shaped pattern in the number of banks in our sample is plausibly explained by deregulation trends affecting banking consolidation. We correct for these trends using a selection equation that includes a full set of state effects, a full set of time effects, and dummy variables indicating that a state has deregulated its banking activities.[17] Our second approach speaks directly to the influence of the monetary policy on sample inclusion. We estimate a Heckman-corrected procedure that includes eight lags of the federal funds rate in the selection equation. In both the deregulation and the federal funds Heckman procedures, we use the selection equation to predict inclusion in the sample, and then use this predicted inclusion variable as a control in the first-stage of our two-step estimations.

The results for the Heckman-corrected estimations are displayed in Table 8. In both cases, they consistently indicate that potential sample selection biases associated with deregulation trends and/or with the monetary policy cycle are unlikely to exert any significant influence on our conclusions.

– insert Table 8 here –

4.3 BHC Assignments

The second selection bias we consider as potentially affecting our inferences comes from the non-randomness in the process through which bank affiliates are assigned to their particular BHCs. Mergers, acquisitions, and reorganizations are not random events, and are thought to occur whenever it 'makes economic sense' to combine certain businesses in specific ways. Although it is still a matter of debate what is economically sensible in the conglomeration trend in the U.S. banking industry, conceivably, banking conglomerates may structure the operations in ways that could

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[17] Our branching deregulation proxies are taken from Jayaratne and Strahan (1996).

explain why their subsidiaries display different responses to monetary policy shocks. Of course, the only circumstance in which this may be concerning to our conclusions is under a scenario in which those underlying reasons why particular subsidiary structures display different responses to monetary policy correlate with short-term fluctuations in their borrowers' financial strength. While it is difficult to pin down a mechanism that could systematically bias our results along those lines, we try to address this possibility in a very general way.

Again, the claim is that our inferences are based on the specific sample we have and that the way the data are endogenously presented to us — rather than the workings of internal capital markets — might explain our results. To see whether the patterns in affiliate loan growth we observe are robust to changes in the structure of the data, we "intervene" in the formation of the BHCs by way of a randomization procedure. This consists of randomly re-assigning affiliates in the data to different conglomerates and estimating our two-step procedure on the randomized parent–affiliate matching. In this exercise we look at the same sample of banks used in the baseline specification, however, instead of relying on how banks have selected to a particular holding company, on a quarterly basis, we randomly assign each of these banks to one of 100 fictional holding companies. The first stage of our two-step procedure then estimates the sensitivity of subsidiary loan growth to state economic conditions with each variable measured relative to the fictional holding company mean. The second-stage regressions are unchanged. While we are breaking the link between a bank and its own conglomerate, according to our story it should not matter to which large holding company subsidiary banks are assigned to when they face borrowers with weak balance sheets.

Results from our in-sample randomization are presented in Table 9, which has the same structure of Table 4 above. Most of the $\sum \phi_k$ estimates have the same sign and level of statistical significance of those displayed in Table 4, pointing to similar conclusions about a dimension of the balance sheet channel of monetary policy that is identified through data from large financial conglomerates.

<div align="center">– insert Table 9 here –</div>

5   Conclusions

This paper improves upon the existing knowledge about the transmission of monetary policy by devising an empirical strategy that more effectively isolates the amplification effect of policy on bank credit through borrower creditworthiness. We interpret our empirical results as evidence that

the balance sheet channel is a part of how monetary policy works. In fact, our results suggest that borrowers' balance sheet presumed status can explain a significant fraction of the overall response of bank lending to monetary policy. Our findings also add to the growing literature on the role internal capital markets play in the allocation of funds inside financial conglomerates. One advantage of active capital markets inside the banking firm is that they may offset perverse effects of monetary policy on the supply of loans. This happens because market internalization ameliorates frictions that prevent banks from making sound loans due to the inability of accessing fairly-priced funds when money is tight. The associated cost of this arrangement is that it may imply transfers of resources across different subsidiaries of the same conglomerate that could lead to shortage of loanable funds to worthy borrowers when internal capital markets are inefficient. This phenomenon points at need to understand in more detail the influence that bank conglomeration may exert on the impact of Federal Reserve policies on lending activity and aggregate investment. Finally, while we are able to neutralize the confounding effects of the lending channel using bank microdata, we are unable to pin down precisely the marginal contribution of financing constraints (asymmetric information and agency problems) to the amplification of monetary policy on output. While this question is empirically challenging, the answer has potentially important welfare implications and should therefore be pursued by future researchers.

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Appendix A: Construction of Panel Bank Microdata

All of the bank-level data used in the analysis is derived from the Federal Reserve's *Report of Condition and Income (Call Reports)*. We employ a version of the *Call Reports* cleaned by the Banking Studies Function of the Federal Reserve Bank of New York, and thus may differ from the data made publicly available online at the Federal Reserve Bank of Chicago. We collect quarterly data on insured commercial banks over 1976:I-1998:II. This requires the bank type (RSSD9331) be identified as a "commercial bank" by having a value equal to one and the reporting level code (CALL8786) identified as "Not Applicable" by having a value equal to zero. FDIC-insured banks are identified by the deposit insurance status (RSSD9424) reflecting the FDIC as the bank's insurer by having a value of 1.

There are many well-known reporting discontinuities in the data and rely on notes by Anil Kashyap and Jeremy Stein published online by the Federal Reserve Bank of Chicago to construct consistent times series. Each of the variables used in our analysis are constructed as follows:

Loans. The aggregate gross book value of total loans and leases before deduction of valuation reserves (RCFD1400) includes: a) acceptances of other banks and commercial paper purchased in open market; b) acceptances executed by or for account of reporting bank and subsequently acquired by it through purchase or discount; c) customers' liability to reporting bank on drafts paid under letter of credit for which bank has not been reimbursed; and d) "cotton overdrafts" or "advances", and commodity or bill of lading drafts payable upon arrival of goods against which drawn for which reporting bank has given deposit credit to customers. Also includes: a) paper rediscounted with Federal Reserve or other banks; and b) paper pledged as collateral to secure bills payable, as marginal collateral to secure bills rediscounted, or for any other purpose. Before 1984:I, this item does not include lease-financing receivables, so in order to ensure continuity, total loans must be computed as the sum of total loans (RCFD1400) and lease-financing receivables (RCFD2165) for the period prior to 1984:I.

Bad Loans. The measure of loan performance employed avoids managerial discretion in reporting losses. Bad loans are defined as the ratio of the sum of loans not accruing (RCFD1403) and loans over 90 days late (RCFD1407), divided by total loans. Loans not accruing (RCFD1403) measures the outstanding balances of loans and lease financing receivables that the bank has placed in nonaccrual status. Also includes all restructured loans and lease financing receivables that are in nonaccrual status. Loans and lease financing receivables are to be reported in nonaccrual status if: a) they are maintained on a cash basis because of deterioration in the financial position of the borrower, or b) principal or interest has been in default for a period of 90 days or more unless the obligation is both "well secured" and "in the process of collection". Loans over 90 days late (RCFD1407) measures loans and lease financing receivables on which payment is due and unpaid for 90 days or more. The measure includes all restructured loans and leases after 1986:II, which

27

was reported separated as Renegotiated "Troubled" Debt (RCFD1404).

Capitalization. The capital-to-asset ratio is computed as equity (RCFD3210) divided by total assets (RCFD2170). Equity capital (RCFD3210) is the sum of "Perpetual Preferred Stock and Related Surplus", "Common Stock", "Surplus", "Undivided Profits and Capital Reserves", "Cumulative Foreign Currency Translation Adjustments" less "Net Unrealized Loss on Marketable Equity Securities".

Liquidity. Up until 1983:IV, bank liquidity is computed as the sum of items RCFD0400 (U.S. Treasury securities), RCFD0600 (U.S. government agency and corporate obligations), RCFD0900 (obligations of states and political subdivisions), RCFD0380 (all other bonds, stocks, and securities), and RCFD1350 (Fed funds sold and securities purchased under agreements to resell). For the 1984:I to 1993:II period, liquidity is the sum of RCFD0390 (total investment securities), RCFD1350, and RCFD2146 (assets held in trading account). For the remainder of the sample period, it equals the sum of RCFD1350, RCFD1754 (securities held to maturity), and RCFD3545 (trading assets).

Deposits. Total deposits are measured using item RCFD2200.

Bank Size. At each quarter, all banks in the data are ranked according to their total assets (RCFD2170). Small and large banks are identified using the 95th percentile of the asset distribution as a size cut-off.

Multi-Bank Holding Company Affiliation. Affiliation with a multi-bank holding company is identified the number of insured commercial banks that have a common regulatory direct holder (RSSD9348) or high holder (RSSD9379) being larger than one.

Large Multi-Bank Holding Company Affiliation. Affiliation with a large multi-bank holding company is determined by the holding company owning more than one bank and either the regulatory direct holder or regulatory high holder owning at least one subsidiary considered to be a large commercial bank.

Large Multi-State Bank Holding Company Affiliation. Affiliation with a large multi-state bank holding company is determined by the holding company being a large multi-bank holding company that has two small subsidiaries operating in separate states (RSSD9210).

Appendix B: Measures of Monetary Policy

The monetary policy measures we use are standard in the literature. All of our policy measures are constructed with series available from the Federal Reserve system's data bank.

Fed Funds. We use the monthly series of effective annualized Fed funds rates from the Board of Governors' Release H.15. Bernanke and Blinder (1992) argue that this rate captures the stance of monetary policy well because it is sensitive to shocks to the supply of bank reserves. The Fed funds rate is the prevalent measure of monetary policy in related empirical work. However, the adequacy of this proxy has been questioned for periods when the Fed's operating procedures were modified (e.g., the Volker period).

Funds-Bill. Motivated by Bernanke and Blinder (1992), this is computed as the difference between the effective annual Fed funds rate and the rate on 10-year Treasury bills. These series are gathered from Board of Governors' Release H.15.

CP-Bill. This is computed as the difference between the rates paid on six-month prime rated commercial papers and 180-day Treasury bills. These series are also available from Board of Governors' Release H.15, but the paper series is discontinued in 1997:I. The paper rates are given as discount rates and the Treasury bill as coupon equivalent rates. We transform both series into effective yield rates before computing the difference. Bernanke (1990) argues that CP-Bill increases capture Fed tightenings since banks will cut loans and corporations are forced to substitute commercial paper for bank loans.

NonBorrowed. Measured as the log change in non-borrowed reserves. We perform this computation using data from the Federal Reserve's FRED data bank.

Strongin. Strongin (1995) argues that previous studies attempting to identify the stance of monetary policy fail to properly address the Fed's strategy of accommodating reserve demand shocks. Strongin measures the portion of non-borrowed reserves growth that is orthogonal to total reserve growth. It equals the residual of a linear regression of total reserves on non-borrowed reserves, where both series are normalized by a 24-month moving average of total reserves prior to the estimation. We perform this computation using data from the FRED data bank.

Table 1: Banks Affiliated with Large Multi-State Holding Companies

Year	Quarter				Total
	I	II	III	IV	
1977	-	195	194	195	584
1978	195	195	196	196	782
1979	196	196	198	197	787
1980	198	199	199	200	796
1981	202	202	204	195	803
1982	197	197	198	198	790
1983	208	213	206	195	822
1984	206	189	200	189	784
1985	237	234	293	286	1,050
1986	320	357	411	576	1,664
1987	665	778	739	801	2,983
1988	892	883	854	823	3,452
1989	862	859	871	849	3,441
1990	835	800	794	751	3,180
1991	733	762	745	722	2,962
1992	693	713	724	717	2,847
1993	684	737	717	713	2,851
1994	695	706	631	656	2,688
1995	596	597	599	575	2,367
1996	578	520	490	480	2,068
1997	468	430	344	329	1,571
1998	301	319	-	-	620
Total					39,892

Table Notes: The table reports the number of small banks that are affiliated with a large multi-state bank holding company in each quarter and contain enough consecutive quarters of data (five) to be used in the analysis.

Table 2: Descriptive Statistics on Small Banks

	In the Sample	Not in the Sample
$\Delta ln(Loans)_{ijt}$	0.0157	0.0211
	(0.0764)	(0.0680)
$BadLoans_{ijt}$	0.0144	0.0156
	(0.0259)	(0.0267)
$ln(Assets)_{ijt-1}$	11.4992	10.3804
	(0.8750)	(0.9803)
$(Equity/Assets)_{ijt-1}$	0.0811	0.0912
	(0.0406)	(0.0340)
$(Securities/Assets)_{ijt-1}$	0.2378	0.2987
	(0.1406)	(0.1447)
$\overline{\Delta ln(Loans)_{ij}}$	0.0240	0.0245
	(0.0222)	(0.0189)
$\overline{BadLoans_{ij}}$	0.0118	0.0148
	(0.0143)	(0.0119)
$\Delta ln(Loans)_{ijt} - \overline{\Delta ln(Loans)_{ij}}$	−0.0084	−0.0034
	(0.0748)	(0.0668)
$BadLoans_{ijt} - \overline{BadLoans_{ij}}$	0.0026	0.0008
	(0.0222)	(0.0235)
N	39,892	926,845

Table Notes: The table refers to the sample mean and standard deviation for a number of variables in the population of small insured commercial banks. The first column refers to small banks that are part of large multi-state bank holding companies while the second column refers to all other small banks. Reading down, the measures include quarterly loan growth, bad loans as a fraction of total loans, one lag of log bank assets, one lag of bank leverage, one lag of bank liquidity, average quarterly loan growth and bad loans for the bank, and the difference in quarterly loan growth and bad loans from its long-run average.

Table 3: Local Economic Conditions and Bank Loan Quality

Dependent Variable	$\Omega_{ijt}^{BadLoans}$	$\widetilde{\Omega_{ijt}^{BadLoans}}$

A. Borrower's Balance Sheet proxyed by $YGap_{ijt}$

	−0.0244	−0.0289
	(1.93)	(2.92)

B. Borrower's Balance Sheet proxyed by Ω_{ijt}^{YGap}

	−0.0420	−0.0286
	(2.50)	(2.33)

N	36,090	36,090

Table Notes: The table refers a regression of a function of bank-level bad loans on on state economic activity and other covariates (Eqs. (5) and (7) in the text). This measure of economic activity includes the state income gap in the first row and the difference in state income gap from the average gap faced by banks in the subsidiary in the second row. In the first column the dependent variable is the difference in bad loans from the holding company mean while in the second column it is this variable differenced again against its long-run mean. The coefficient on state economic activity is reported as well as t-statistics, which have been corrected for error heteroskedasticity and individual bank clustering.

Table 4: Monetary Policy and the Balance Sheet Channel

First-Stage	Measure of Monetary Policy				
Dep. Variable	Fed Funds	CP-Bill	Funds-Bill	NonBorrow	Strongin

A. Borrower's Balance Sheet proxied by $YGap_{ijt}$

Ω_{ijt}^{Loans}	0.041	0.200	0.036	0.941	0.793
	(0.020)	(0.021)	(0.246)	(0.604)	(0.185)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.042	0.143	0.054	2.125	0.931
	(0.009)	(0.127)	(0.039)	(0.194)	(0.181)

B. Borrower's Balance Sheet proxied by Ω_{ijt}^{YGap}

Ω_{ijt}^{Loans}	0.032	0.215	0.039	2.184	1.070
	(0.063)	(0.024)	(0.323)	(0.410)	(0.009)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.031	0.100	0.055	5.166	0.942
	(0.065)	(0.266)	(0.124)	(0.013)	(0.096)

Table Notes: The table refers to the second-stage regression described in the text (Eq. (10)). The dependent variable is the average sensitivity of bank loan growth to the state economic activity, while explanatory variables include 8 lags of monetary policy measures, 8 lags of aggregate output growth, a time trend, and quarter effects. The estimation period is 1977:II through 1998:II. The table reports the sum of coefficients on the 8 lags of each measure of monetary policy and the p-value for the hypothesis test that this sum is no different from zero. Each of the last five columns refers to specifications characterized by the employed measure of monetary policy. In Panel A, borrowers' balance sheet strength is proxied by $YGap_{ijt}$, while in Panel B the relevant proxy is Ω_{ijt}^{YGap}.

Table 5: Cumulative Balance Sheet Effect of the Funds Rate on Lending

First-Stage	Cumulative Lags of the Fed Funds Rate							
Dependent Variable	1	2	3	4	5	6	7	8

A. Borrower's Balance Sheet proxied by $YGap_{ijt}$

	1	2	3	4	5	6	7	8
Ω_{ijt}^{Loans}	0.017	0.024	0.021	0.024	0.026	0.029	0.039	0.041
	(0.175)	(0.144)	(0.239)	(0.173)	(0.142)	(0.102)	(0.034)	(0.020)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.016	0.023	0.022	0.026	0.026	0.030	0.039	0.042
	(0.149)	(0.107)	(0.162)	(0.124)	(0.137)	(0.112)	(0.028)	(0.009)

B. Borrower's Balance Sheet proxied by Ω_{ijt}^{YGap}

	1	2	3	4	5	6	7	8
Ω_{ijt}^{Loans}	0.023	0.024	0.019	0.025	0.027	0.029	0.036	0.032
	(0.133)	(0.158)	(0.299)	(0.157)	(0.087)	(0.049)	(0.049)	(0.063)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.019	0.020	0.017	0.022	0.022	0.025	0.033	0.031
	(0.118)	(0.147)	(0.327)	(0.223)	(0.210)	(0.160)	(0.083)	(0.065)

Table Notes: The table refers to the second-stage regression described in the text (Eq. (10)). The dependent variable is the average sensitivity of bank loan growth to the state economic activity, while explanatory variables include 8 lags of monetary policy measures, 8 lags of aggregate output growth, a time trend, and quarter effects. The estimation period is 1977:II through 1998:II. The table reports the sum of coefficients on lags of the funds rate and the *p*-value for the hypothesis test that this sum is no different from zero. Each of the last eight columns refers to statistics characterized by the number of lags over which to sum. In Panel A, borrowers' balance sheet strengh is proxied by $YGap_{ijt}$, while in Panel B the relevant proxy is Ω_{ijt}^{YGap}.

Table 6: The Balance Sheet and Interest Rate Channels — Controlling for cash flows

First-Stage	Measure of Monetary Policy				
Dep. Variable	Fed Funds	CP-Bill	Funds-Bill	NonBorrow	Strongin

A. Borrower's Balance Sheet proxied by $YGap_{ijt}$

Ω_{ijt}^{Loans}	0.032	0.116	0.050	1.941	0.679
	(0.013)	(0.089)	(0.103)	(0.293)	(0.164)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.037	0.149	0.051	1.111	0.621
	(0.006)	(0.023)	(0.167)	(0.505)	(0.194)

B. Borrower's Balance Sheet proxied by Ω_{ijt}^{YGap}

Ω_{ijt}^{Loans}	0.024	0.086	0.049	5.363	0.839
	(0.096)	(0.384)	(0.375)	(0.040)	(0.081)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.033	0.185	0.058	2.732	1.012
	(0.108)	(0.098)	(0.353)	(0.453)	(0.010)

Table Notes: The table refers to the second-stage regression described in the text (Eq. (10)). The dependent variable is the average sensitivity of bank loan growth to the state economic activity, while explanatory variables include 8 lags of monetary policy measures, 8 lags of aggregate output growth, a time trend, and quarter effects. The estimation period is 1977:II through 1998:II. The table reports the sum of coefficients on the 8 lags of each measure of monetary policy and the p-value for the hypothesis test that this sum is no different from zero. Each of the last five columns refers to specifications characterized by the employed measure of monetary policy. In Panel A, borrowers' balance sheet strengh is proxied by $YGap_{ijt}$, while in Panel B the relevant proxy is Ω_{ijt}^{YGap}.

Table 7: The Balance Sheet and Interest Rate Channels — Controlling for ROA

First-Stage	Measure of Monetary Policy				
Dep. Variable	Fed Funds	CP-Bill	Funds-Bill	NonBorrow	Strongin

A. Borrower's Balance Sheet proxied by $YGap_{ijt}$

Ω_{ijt}^{Loans}	0.037	0.141	0.048	1.599	0.687
	(0.026)	(0.072)	(0.109)	(0.425)	(0.160)
$\Omega_{ijt}^{\widetilde{Loans}}$	0.044	0.177	0.052	1.174	0.656
	(0.006)	(0.048)	(0.102)	(0.511)	(0.195)

B. Borrower's Balance Sheet proxied by Ω_{ijt}^{YGap}

Ω_{ijt}^{Loans}	0.029	0.108	0.033	4.214	0.710
	(0.037)	(0.157)	(0.388)	(0.072)	(0.138)
$\Omega_{ijt}^{\widetilde{Loans}}$	0.039	0.204	0.046	1.727	0.872
	(0.012)	(0.010)	(0.314)	(0.575)	(0.026)

Table Notes: The table refers to the second-stage regression described in the text (Eq. (10)). The dependent variable is the average sensitivity of bank loan growth to the state economic activity, while explanatory variables include 8 lags of monetary policy measures, 8 lags of aggregate output growth, a time trend, and quarter effects. The estimation period is 1977:II through 1998:II. The table reports the sum of coefficients on the 8 lags of each measure of monetary policy and the p-value for the hypothesis test that this sum is no different from zero. Each of the last five columns refers to specifications characterized by the employed measure of monetary policy. In Panel A, borrowers' balance sheet strengh is proxied by $YGap_{ijt}$, while in Panel B the relevant proxy is Ω_{ijt}^{YGap}.

Table 8: Heckman Sample Selection Correction

First-Stage	Measure of Monetary Policy				
Dep. Variable	Fed Funds	CP-Bill	Funds-Bill	NonBorrow	Strongin

A. Branching Variables in Selection Equation

Ω_{ijt}^{Loans}	0.039	0.212	0.072	1.602	0.886
	(0.047)	(0.026)	(0.088)	(0.465)	(0.077)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.032	0.157	0.065	1.972	0.872
	(0.094)	(0.093)	(0.089)	(0.280)	(0.084)

B. Lagged Funds Rate in Selection Equation

Ω_{ijt}^{Loans}	0.034	0.145	0.045	0.874	0.769
	(0.024)	(0.061)	(0.259)	(0.624)	(0.091)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.029	0.111	0.044	1.375	0.836
	(0.047)	(0.155)	(0.204)	(0.393)	(0.072)

Table Notes: The table refers to the second-stage regression described in the text (Eq. (10)). The dependent variable is the average sensitivity of bank loan growth to the state economic activity, while explanatory variables include 8 lags of monetary policy measures, 8 lags of aggregate output growth, a time trend, and quarter effects. The estimation period is 1977:II through 1998:II. The table reports the sum of coefficients on the 8 lags of each measure of monetary policy and the p-value for the hypothesis test that this sum is no different from zero. Each of the last five columns refers to specifications characterized by the employed measure of monetary policy. Each specification uses the state income gap in the first-stage regression ($YGap_{ijt}$). In Panel A we use dummies for state branching deregulation in the selection equation while in Panel B the we use eight lags of the federal funds rate.

Table 9: Random Assignment of Subsidiaries to Bank Holding Companies

First-Stage	Measure of Monetary Policy				
Dep. Variable	Fed Funds	CP-Bill	Funds-Bill	NonBorrow	Strongin

A. Borrower's Balance Sheet proxied by $YGap_{ijt}$

Ω_{ijt}^{Loans}	0.038	0.214	0.038	−6.564	1.890
	(0.023)	(0.061)	(0.499)	(0.249)	(0.001)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.041	0.241	0.077	−4.260	2.577
	(0.022)	(0.076)	(0.130)	(0.394)	(0.000)

B. Borrower's Balance Sheet proxied by Ω_{ijt}^{YGap}

Ω_{ijt}^{Loans}	0.043	0.240	0.028	−7.355	1.869
	(0.014)	(0.041)	(0.636)	(0.229)	(0.002)
$\widetilde{\Omega_{ijt}^{Loans}}$	0.045	0.265	0.073	−4.362	2.526
	(0.015)	(0.056)	(0.146)	(0.406)	(0.000)

Table Notes: The table refers to the second-stage regression described in the text (Eq. (10)). The dependent variable is the average sensitivity of bank loan growth to the state economic activity, while explanatory variables include 8 lags of monetary policy measures, 8 lags of aggregate output growth, a time trend, and quarter effects. The estimation period is 1977:II through 1998:II. The table reports the sum of coefficients on the 8 lags of each measure of monetary policy and the p-value for the hypothesis test that this sum is no different from zero. Each of the last five columns refers to specifications characterized by the employed measure of monetary policy. In Panel A, borrowers' balance sheet strengh is proxied by $YGap_{ijt}$, while in Panel B the relevant proxy is Ω_{ijt}^{Y}.