# What Do a Million Observations on Banks Say About the Transmission of Monetary Policy?

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*We study the monetary-transmission mechanism with a data set that includes quarterly observations of every insured U.S. commercial bank from 1976 to 1993. We find that the impact of monetary policy on lending is stronger for banks with less liquid balance sheets—i.e., banks with lower ratios of securities to assets. Moreover, this pattern is largely attributable to the smaller banks, those in the bottom 95 percent of the size distribution. Our results support the existence of a "bank lending channel" of monetary transmission, though they do not allow us to make precise statements about its quantitative importance.* (*JEL* E44, E52, G32)

In this paper, we use a new and very big data set to address an old and very basic question, namely: how does monetary policy work? With an almost 20-year panel that includes quarterly data on every insured commercial bank in the United States—approximately 1 million bank-quarters in all—we are able to trace out the effects of monetary policy on the lending behavior of individual banks. It is already well known that changes in the stance of monetary policy are followed by significant movements in *aggregate* bank lending volume (Ben S. Bernanke and Alan S. Blinder, 1992); what we seek to learn here is whether there are also important *cross-sectional differences* in the way that banks with varying characteristics respond to policy shocks.

In particular, we ask whether the impact of monetary policy on lending behavior is stronger for banks with less liquid balance sheets, where liquidity is measured by the ratio of securities to assets. It turns out that the answer is a resounding "yes." Moreover, the result is largely driven by the smaller banks, those in the bottom 95 percent of the size distribution.

This empirical exercise is best motivated as a test of the so-called "bank lending view" of monetary transmission. At the heart of the lending view is the proposition that the Federal Reserve can, simply by conducting open-market operations, shift banks' loan supply schedules. For example, according to the lending view, a contraction in reserves leads banks to reduce loan supply, thereby raising the cost of capital to bank-dependent borrowers. Importantly, this effect is on top of any increase in the interest rate on open-market securities such as Treasury bills.[1]

The lending view hinges on a failure of the Modigliani-Miller (M-M) proposition for banks.[2]

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[1] See Kashyap and Stein (1994) for a detailed discussion as to why the debate over the lending channel is of practical and policy relevance.

[2] The lending channel also requires: (1) some borrowers who cannot find perfect substitutes for bank loans; and (2) imperfect price adjustment. See Bernanke and Blinder (1988).

When the Fed drains reserves from the system, it compromises banks' ability to raise *reservable* forms of finance, such as insured transaction deposits. But it cannot constrain banks' use of *nonreservable* liabilities, such as large-denomination CD's. In an M-M world, banks are indifferent at the margin between issuing transactions deposits and large CDs, so shocks to the former do not affect their lending decisions. All that monetary policy can do in this textbook setting is alter the amounts of deposits (aka "money") and CDs (aka "bonds") outstanding.[3]

So if there is to be an active lending channel, it must be that banks cannot frictionlessly tap uninsured sources of funds to make up for a Fed-induced shortfall in insured deposits. Stein (1998) develops this argument, observing that many classes of bank liabilities which escape reserve requirements are not covered by deposit insurance, and hence are potentially subject to adverse-selection problems and the attendant credit rationing. For example, if there is adverse selection in the market for large uninsured CDs, a bank that loses a dollar of insured deposits will not raise a full dollar of new CD financing to offset this loss. As a result, its lending is likely to decline. Thus if there is a link between the *reservability* and *insurability* of bank liabilities, the M-M theorem can break down, and open-market operations can matter for bank lending.

These theoretical arguments notwithstanding, the M-M benchmark might lead one to be skeptical of the empirical importance of the lending channel, particularly in the current, deregulated environment where banks have a wide range of nonreservable liability instruments at their disposal. And while a great deal of relevant evidence has been produced in the last several years, it can be argued that previous studies have not completely overcome the fundamental but very difficult problem of disentangling loan-supply effects from loan-demand effects. Consequently, the empirical case in support of a lending channel has not been viewed as airtight.

A quick literature review highlights the identification problems that arise.[4] Bernanke and Blinder (1992) find that a monetary contraction is followed by a decline in aggregate bank lending. This is consistent with the lending view, but also admits another interpretation: activity is being depressed via standard interest-rate effects, and it is a decline in loan *demand,* rather than loan *supply,* that drives the results. In an effort to resolve this ambiguity, Kashyap et al. (1993) show that while a monetary contraction reduces bank lending, it *increases* commercial paper volume. This fact would seem to suggest an inward shift in loan supply, rather than an inward shift in loan demand. However, others have argued that it is not decisive either: perhaps in recessions there is a compositional shift, with large firms faring better than small ones, and actually demanding more credit. Since most commercial paper is issued by large firms, this could explain the Kashyap et al. (1993) results.[5]

Moving away from the aggregate data, a number of researchers have used micro data to test the cross-sectional implications of the lending view. One prediction is that tight money should pose a special problem for small firms, which are more likely to be bank-dependent. And indeed, several papers find that contractions in policy intensify liquidity constraints in the inventory and investment decisions of small firms.[6] But again, while this is consistent with the lending view, there is another interpretation: what Bernanke and Gertler (1995) call a "balance sheet channel," whereby tight monetary policy weakens the creditworthiness of small firms, and hence reduces their ability to raise funds from *any* external provider, not just banks.

Our premise here is that, to make further progress on this difficult identification problem, one has to examine lending behavior at the individual bank level. As discussed above, the theory ultimately rests on the idea that banks cannot frictionlessly tap uninsured sources of funds to make up for a Fed-induced shortfall in

[3] For an articulation of this M-M view, see Christina D. Romer and David H. Romer (1990).

[4] For detailed surveys, see Kashyap and Stein (1994), Bernanke and Mark Gertler (1995), Stephen G. Cecchetti (1995), and R. Glenn Hubbard (1995).

[5] See Stephen Oliner and Glenn D. Rudebusch (1996). Kashyap et al. (1996) rebut by noting that even *within* the class of the largest firms, commercial paper rises relative to bank lending after a monetary contraction. Sydney Ludvigson (1998) provides further evidence that financing "mix" results like those of Kashyap et al. (1993) are not an artifact of compositional effects.

[6] See, e.g., Gertler and Hubbard (1988), Robert E. Carpenter et al. (1994), Gertler and Simon Gilchrist (1994), and Kashyap et al. (1994).

insured deposits. But if this is true, then the effect of monetary policy on lending should be more pronounced for some banks than for others.

Consider two small banks, both of which face limitations in raising uninsured external finance. The banks are alike, except that one has a much more liquid balance sheet position than the other. Now imagine that these banks are hit by a contractionary monetary shock, which causes them both to lose insured deposits. In the extreme case where they cannot substitute at all towards other forms of finance, the asset sides of their balance sheets must shrink. But the more liquid bank can relatively easily protect its loan portfolio, simply by drawing down on its large buffer stock of securities. In contrast, the less liquid bank is likely to have to cut loans significantly, if it does not want to see its securities holdings sink to a dangerously low level.

This logic leads to our first hypothesis: for banks without perfect access to uninsured sources of finance, $\partial^2 L_{it}/\partial B_{it}\partial M_t < 0$, where $L_{it}$ is a bank-level measure of lending activity, $B_{it}$ is a measure of balance sheet strength, and $M_t$ is a monetary-policy indicator (with higher values of $M_t$ corresponding to easier policy). This hypothesis exploits both cross-sectional and time-series aspects of the data, and can be thought of in two ways, depending on the order in which one takes the derivatives. Looking first at the cross-sectional derivative $\partial L_{it}/\partial B_{it}$—which captures the degree to which lending is liquidity constrained at any time *t*—the hypothesis is that these constraints are intensified during periods of tight money. Alternatively, looking first at the time-series derivative $\partial L_{it}/\partial M_t$—the sensitivity of lending volume to monetary policy for bank *i*—the hypothesis is that this sensitivity is greater for banks with weaker balance sheets.[7]

In testing this first hypothesis, we focus on the smaller banks in our sample, based on the idea that these banks are least likely to be able to frictionlessly raise uninsured finance. This leads us to our second hypothesis, which is that $\partial^3 L_{it}/\partial B_{it}\partial M_t\partial SIZE_{it} > 0$. Simply put, the effect that we are interested in should be strongest for small banks. One would expect the largest banks to have an easier time raising uninsured finance, which would make their lending less dependent on monetary-policy shocks, irrespective of their internal liquidity positions.[8]

The rest of the paper proceeds as follows. Section I describes our data set. Section II lays out the baseline econometric specification, and discusses potential biases and other pitfalls. Section III presents our main results, and Section IV follows with a range of robustness checks. Section V assesses the quantitative importance of the results, and Section VI concludes.

## I. Data Sources and Choice of Variables

### A. *Bank-Level Data*

Our sources for all bank-level variables are the Consolidated Report of Condition and Income (known as the Call Reports) that insured banks submit to the Federal Reserve each quarter. With the help of the staff of the Federal Reserve Bank of Chicago, we were able to compile a large data set, with quarterly income statement and balance sheet data for all reporting banks over the period 1976Q1–1993Q2, a total of 961,530 bank-quarters.[9] This data set presents a number of challenges, particularly in terms of creating consistent time series, as the definitions often change for variables of interest. The Appendix describes the construction of our key series in detail, and notes the various splices made in an effort to ensure consistency.[10]

[7] Michael Gibson (1996) finds that the impact of monetary policy is stronger when banks *in the aggregate* have lower securities holdings. His approach exploits the time-series variation in bank balance sheets, while we use the cross-sectional variation. Also somewhat related is John C. Driscoll (1997), who shows that state-level shocks to banks' deposits affect their loan supply.

[8] In Kashyap and Stein (1995), we test the related hypothesis that $\partial^2 L_{it}/\partial M_t\partial SIZE_{it} < 0$: the lending of large banks should be less sensitive to monetary shocks than that of small banks. Although the evidence strongly supports this hypothesis, there are alternative interpretations—e.g., large banks lend to large customers, whose loan demand is less cyclical. The tests we conduct below control for this, by focusing on differences in balance sheets *within* size classes.

[9] These data are available on the internet at: www.frbchi.org/rcri/rcri_database.html.

[10] In addition to these splices, we also further cleaned the data set by eliminating any banks involved in a merger, for that quarter in which the merger occurs.

Table 1 examines balance sheets for banks of different sizes. There are two panels, corresponding to the starting and ending points of our sample. In each, we report data on six size categories: banks below the 75th percentile by asset size; banks between the 75th and 90th percentiles; banks between the 90th and 95th percentiles; banks between the 95th and 98th percentiles; banks between the 98th and 99th percentiles; and banks above the 99th percentile.

Whether one looks at the data from 1976 or 1993, several patterns emerge. On the asset side, small banks hold more in the way of securities, and make fewer loans.[11] This is what one would expect to the extent that small banks have more trouble raising external finance: they need bigger buffer stocks. On the liability side, the smallest banks have a very simple capital structure—they are financed almost exclusively with deposits and common equity. In contrast, the larger banks make less use of both deposits and equity, with the difference made up by a number of other forms of borrowing. For example, the largest 2 percent of banks make heavy use of the federal funds market to finance themselves; the smallest banks do virtually no borrowing in the funds market. Given that federal funds are unsecured borrowing, this again fits with the existence of financing frictions: small banks are less able to use instruments where credit risk is an issue.

The numbers in Table 1, as well as the baseline regression results below, reflect balance sheet data at the *individual bank level.* An alternative approach would be to aggregate the balance sheets of all banks that belong to a single bank holding company. This latter approach makes more sense to the extent that bank holding companies freely shift resources among the banks they control as if there were no boundaries.[12] A priori, it is not obvious which is the conceptually more appropriate method, so as a precaution, we have also reproduced our main results using the holding-company approach. As it turns out, nothing changes.[13]

In terms of the specific variables required for our regressions, we make the following choices. First, for the lending volume variable $L_{it}$, we use both total loans, as well as at the most commonly studied subcategory, commercial and industrial (C&I) loans. One reason for examining both is the concern that any results for total loans might be influenced by compositional effects. For example, it may be that C&I loan demand and real estate loan demand move differently over the business cycle. If, in addition, banks that tend to engage primarily in C&I lending have systematically different levels of liquidity $B_{it}$ than banks that tend to specialize in real estate lending, this could bias our estimates of $\partial^2 L_{it}/\partial B_{it}\partial M_t$.[14] A countervailing drawback of focusing on just C&I loans is that some banks do only a negligible amount of C&I business.[15] Thus in the regressions that use C&I lending, we omit any banks for which the ratio of C&I to total loans is less than 5 percent. This screen leads us to drop approximately 7 percent of our sample.[16]

For the balance sheet variable $B_{it}$, we use the ratio of securities plus federal funds sold to total assets.[17] The intuition is as described above: banks with large values of this ratio should be better able to buffer their lending activity against shocks in the availability of external finance, by drawing on

[11] In Panel A, for 1976Q1, we report data for *domestic* loans only. This is because prior to 1978, figures for international loans are not available, although such loans implicitly show up in total bank assets. Consequently, for the very largest category of banks—the only ones with significant international activities—we are understating the true ratio of loans to assets in 1976.

[12] Joel Houston et al. (1997) present evidence that shocks to one bank in a holding company are in fact partially transmitted to others in the same holding company.

[13] It should not be too surprising that the results are robust in this way, since the vast majority of all banks are stand-alones, and even large holding companies are typically dominated by a single bank. See Allen N. Berger et al. (1995).

[14] This is just a specific version of the general proposition that $B_{it}$ might be endogenously linked to the cyclical sensitivity of loan demand. We discuss this issue in Section II, subsection B2, below, and argue that the bias is likely to make our tests with total loans too conservative.

[15] This problem is even more pronounced with other subcategories, e.g., agricultural loans.

[16] Even after this filter, there are some extreme values of loan growth in our sample. To ensure that our results are not driven by these outliers—which could be data errors—we drop any further observations for which loan growth is more than five standard deviations from its period mean. However, our results are not sensitive to either of these screens.

[17] We do not include cash in the numerator, because we suspect that cash holdings largely reflect required reserves, which cannot be freely drawn down. However, our results are very similar if cash is added to our measure of $B_{it}$. See the NBER working paper version (1997) for details.

TABLE 1—BALANCE SHEETS FOR BANKS OF DIFFERENT SIZES

Panel A: Composition of Bank Balance Sheets as of 1976 Q1

| | Below 75th percentile | Between 75th and 90th percentile | Between 90th and 95th percentile | Between 95th and 98th percentile | Between 98th and 99th percentile | Above 99th percentile |
|---|---|---|---|---|---|---|
| Number of banks | 10,784 | 2,157 | 719 | 431 | 144 | 144 |
| Mean assets (1993 $ millions) | 32.82 | 119.14 | 247.73 | 556.61 | 1,341.45 | 10,763.44 |
| Median assets (1993 $ millions) | 28.43 | 112.63 | 239.00 | 508.06 | 1,228.66 | 3,964.55 |
| Fraction of total system assets | 0.13 | 0.09 | 0.06 | 0.09 | 0.07 | 0.56 |
| *Fraction of total assets in size category* | | | | | | |
| Cash | 0.09 | 0.09 | 0.10 | 0.12 | 0.13 | 0.22 |
| Securities | 0.34 | 0.33 | 0.32 | 0.29 | 0.27 | 0.15 |
| Federal funds lent | 0.05 | 0.04 | 0.04 | 0.05 | 0.04 | 0.03 |
| Total domestic loans | 0.52 | 0.53 | 0.53 | 0.53 | 0.54 | 0.41 |
| Real estate loans | 0.17 | 0.19 | 0.20 | 0.18 | 0.17 | 0.09 |
| C & I loans | 0.10 | 0.13 | 0.15 | 0.16 | 0.17 | 0.17 |
| Loans to individuals | 0.15 | 0.16 | 0.15 | 0.15 | 0.14 | 0.06 |
| Total deposits | 0.90 | 0.90 | 0.89 | 0.87 | 0.84 | 0.81 |
| Demand deposits | 0.31 | 0.30 | 0.30 | 0.31 | 0.33 | 0.25 |
| Time and savings deposits | 0.59 | 0.60 | 0.59 | 0.55 | 0.51 | 0.33 |
| Time deposits > $100K | 0.07 | 0.10 | 0.12 | 0.14 | 0.14 | 0.16 |
| Federal funds borrowed | 0.00 | 0.01 | 0.02 | 0.04 | 0.07 | 0.08 |
| Subordinated debt | 0.00 | 0.00 | 0.00 | 0.00 | 0.01 | 0.01 |
| Other liabilities | 0.01 | 0.01 | 0.01 | 0.01 | 0.02 | 0.06 |
| Equity | 0.08 | 0.08 | 0.07 | 0.07 | 0.07 | 0.05 |

Panel B: Composition of Bank Balance Sheets as of 1993 Q2

| | Below 75th percentile | Between 75th and 90th percentile | Between 90th and 95th percentile | Between 95th and 98th percentile | Between 98th and 99th percentile | Above 99th percentile |
|---|---|---|---|---|---|---|
| Number of banks | 8,404 | 1,681 | 560 | 336 | 112 | 113 |
| Mean assets (1993 $ millions) | 44.42 | 165.81 | 380.14 | 1,072.57 | 3,366.01 | 17,413.41 |
| Median assets (1993 $ millions) | 38.59 | 155.73 | 362.75 | 920.78 | 3,246.33 | 9,297.70 |
| Fraction of total system assets | 0.10 | 0.08 | 0.06 | 0.10 | 0.11 | 0.55 |
| *Fraction of total assets in size category* | | | | | | |
| Cash | 0.05 | 0.05 | 0.05 | 0.07 | 0.07 | 0.09 |
| Securities | 0.34 | 0.32 | 0.29 | 0.27 | 0.25 | 0.22 |
| Federal funds lent | 0.04 | 0.04 | 0.03 | 0.04 | 0.04 | 0.04 |
| Total loans | 0.53 | 0.56 | 0.60 | 0.59 | 0.60 | 0.59 |
| Real estate loans | 0.30 | 0.33 | 0.34 | 0.30 | 0.25 | 0.21 |
| C & I loans | 0.09 | 0.10 | 0.11 | 0.12 | 0.13 | 0.18 |
| Loans to individuals | 0.09 | 0.10 | 0.12 | 0.14 | 0.17 | 0.10 |
| Total deposits | 0.88 | 0.87 | 0.85 | 0.79 | 0.76 | 0.69 |
| Transaction deposits | 0.26 | 0.26 | 0.25 | 0.24 | 0.26 | 0.19 |
| Large deposits | 0.17 | 0.21 | 0.22 | 0.25 | 0.24 | 0.21 |
| Brokered deposits | 0.00 | 0.00 | 0.01 | 0.02 | 0.02 | 0.01 |
| Federal funds borrowed | 0.01 | 0.02 | 0.04 | 0.06 | 0.10 | 0.09 |
| Subordinated debt | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.02 |
| Other liabilities | 0.01 | 0.02 | 0.03 | 0.05 | 0.06 | 0.13 |
| Equity | 0.10 | 0.09 | 0.08 | 0.09 | 0.08 | 0.07 |

their stock of liquid assets. Of course, as in all of the liquidity-constraints literature, we must be aware that $B_{it}$ is an endogenous variable. We discuss the potential biases this might cause, as well as our approach to controlling for these biases, below.

Finally, we need to decide on cutoffs in order to assign banks to size categories. Because of the extremely skewed nature of the size distribution, an overwhelming majority of the banks in our sample are what anyone would term "small," by any standard. (Recall from Table 1 above that even banks between the 90th and 95th percentiles have average assets of below \$400 million in 1993.)[18] In the end, we choose to use three categories: the smallest one encompasses all banks with total assets below the 95th percentile; the middle one includes banks from the 95th to 99th percentiles, and the largest one has those banks above the 99th percentile.[19]

### B. *Measures of Monetary Policy*

A prerequisite for all our tests is a good indicator of the stance of monetary policy $M_t$. Unfortunately, there is no consensus on this topic—indeed, a whole host of different indicators have been proposed in the recent literature.[20] Therefore, rather than trying to argue for a single best measure, we use three different ones throughout. While not an exhaustive list, these three do span the various broad types of methodologies that have been employed.

Our first measure, which represents the "narrative approach" to measuring monetary policy, is the Boschen-Mills (1995) index. Based on their reading of FOMC documents, John Boschen and Leonard Mills each month rate Fed policy as being in one of five categories: "strongly expansionary," "mildly expansionary," "neutral," "mildly contractionary," and "strongly contractionary," depending on the relative weights that they perceive the Fed is putting on inflation versus unemployment.[21] We code these policy stances as 2, 1, 0, −1, and −2 respectively.

Our second measure is the federal funds rate, which has been advocated by Robert Laurent (1988), Bernanke and Blinder (1992), and Marvin Goodfriend (1993). However, it should be noted that as the Fed's operating procedures have varied over time, so too has the adequacy of the funds rate as an indicator. Both conventional wisdom as well as the formal statistical analysis of Bernanke and Mihov (1998) suggests that the funds rate may be particularly inappropriate during the high-volatility Volcker period, which fits within the first half of our sample period.

Motivated by this observation, we also work with a third measure of monetary policy, that developed by Bernanke and Mihov (1998). They construct a flexible VAR model that nests previous VARs based on more specific assumptions about Fed operating procedures—i.e., their model contains as special cases either funds-rate targeting (Bernanke and Blinder, 1992) or procedures based on nonborrowed reserves (Christiano and Martin Eichenbaum, 1992; Steven Strongin, 1995). The Bernanke-Mihov methodology can be used to calculate either high-frequency monetary-policy *shocks*, or an indicator of the overall stance of policy. We focus on the latter construct, as it is more appropriate for the hypotheses we are testing.[22] The series we use is exactly that shown in Figure III of their paper (p. 899).

[18] To get an idea of how small a \$400-million bank is, note that regulations restrict banks from having more than 15 percent of their equity in a single loan. Thus a bank with \$400 million in assets and a 6-percent equity ratio cannot make a loan of more than \$3.6 million to a single borrower.

[19] We experimented with further subdividing the smallest category—e.g., looking only at those banks below the 75th percentile—but did not discern any differences amongst the subcategories. We also tried using an expanded definition of the largest category—all banks above the 98th percentile—but this also made no significant difference to our results.

[20] See Bernanke and Ilian Mihov (1998) and Lawrence Christiano et al. (2000) for a recent discussion of the literature on measuring monetary policy and for further references.

[21] The other well-known indicator in this vein is the so-called "Romer date" variable (Romer and Romer, 1989). However, there are only three Romer dates in our sample, and two—August 1978 and October 1979—are so close that they are not completely independent observations. Moreover, their zero-one nature further limits the information in the series. The Boschen-Mills index, which embodies a finer measure of the stance of policy, is more appropriate for the high-frequency experiment we are conducting.

[22] Even if a contraction in policy is partially anticipated by banks, it should still have the cross-sectional effects that we hypothesize.

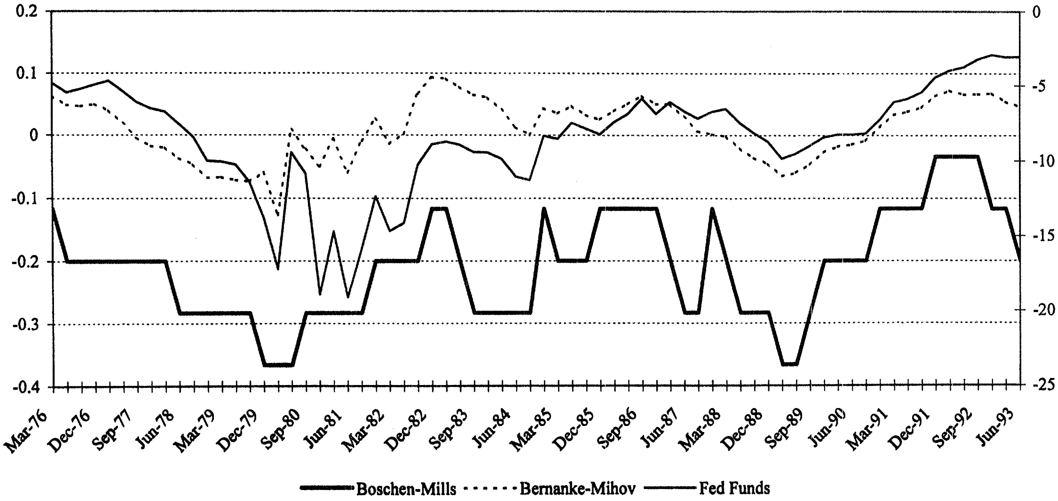


FIGURE 1. MEASURES OF MONETARY POLICY

Figure 1 plots the three measures in levels. (Throughout the paper, we invert the funds rate for comparability with the other two measures.) As can be seen, they all seem to contain broadly similar information. All three indicate that monetary policy was very tight following the Fed's change in operating procedures in October 1979; all three suggest a relatively loose stance of policy in the period 1985–1986; and all three capture the common wisdom that policy was tightened again in 1988, before being eased once more beginning in late 1989.

Table 2 documents the statistical correlations among the three measures. Overall, the numbers confirm the visual impressions from Figure 1, with some qualifications. In levels, the pairwise correlations are all moderately high—between 0.58 and 0.71—over the full sample. The lowest of these correlations is that between the funds rate and the Bernanke-Mihov measure. However, this correlation remains relatively stable when we look at annual and quarterly changes. In contrast, the correlation of the Boschen-Mills index with the other two measures is much reduced when we look at higher-frequency changes. This is due to the discrete nature of the Boschen-Mills index, which at higher frequencies effectively introduces measurement error into this indicator of monetary policy.

The table also looks at subsample correlations. In general, all of the correlations appear to be lower—in many cases substantially so—in the first part of the sample, which we date as running from 1976Q1 to 1985Q4. For example, the correlation of quarterly changes in the Boschen-Mills and Bernanke-Mihov indicators is only 0.02 in the first part of the sample, but rises to 0.36 in the second part. Apparently, given the enormous volatility during the Volcker period, it is harder to get an unambiguous reading of the stance of monetary policy, even if one uses measures other than the funds rate. In light of this concern, we check below to see how our results hold up across subperiods; one might expect a priori that they would be more clear-cut and consistent in the more recent data.

## II. Econometric Specification

### A. *The Two-Step Regression Approach*

Again, our basic goal is to measure the quantity $\partial^2 L_{it}/\partial B_{it}\partial M_t$, for banks in different size classes. In doing so, one important choice is how tightly to parametrize our model. As a baseline, we opt for a flexible specification, which we implement with a two-step procedure. In the first step, we run the following cross-sectional regression *separately* for each size class and each time period $t$: the log change in $L_{it}$ against (i) four lags of itself; (ii) $B_{it-1}$; and

TABLE 2—CORRELATIONS OF MEASURES OF MONETARY POLICY

| | Correlation of: | | |
|---|---|---|---|
| | Levels | Annual changes | Quarterly changes |
| A. Full sample (76Q1–93Q2) | | | |
| 1. Boschen-Mills/Federal funds | 0.608 | 0.382 | 0.219 |
| 2. Boschen-Mills/ Bernanke-Mihov | 0.710 | 0.416 | 0.099 |
| 3. Federal funds/ Bernanke-Mihov | 0.580 | 0.486 | 0.483 |
| B. 1st-half sample (76Q1–85Q4) | | | |
| 1. Boschen-Mills/Federal funds | 0.514 | 0.318 | 0.233 |
| 2. Boschen-Mills/ Bernanke-Mihov | 0.665 | 0.293 | 0.018 |
| 3. Federal funds/ Bernanke-Mihov | 0.526 | 0.476 | 0.471 |
| C. 2nd-half sample (86Q1–93Q2) | | | |
| 1. Boschen-Mills/Federal funds | 0.733 | 0.647 | 0.414 |
| 2. Boschen-Mills/ Bernanke-Mihov | 0.844 | 0.734 | 0.361 |
| 3. Federal funds/ Bernanke-Mihov | 0.871 | 0.567 | 0.730 |

*Notes:* Annual changes are defined as the change between the level of a variable in a certain quarter and the level four quarters before that. The sign of the federal funds rate has been inverted to preserve the convention in the paper that a higher level of the monetary-policy measure reflects a looser policy.

(iii) a Federal Reserve-district dummy variable (i.e., a geographic control).[23] That is, we estimate:

$$(1)\quad \Delta\log(L_{it}) = \sum_{j=1}^{4} \alpha_{tj}\Delta\log(L_{it-j}) + \beta_t B_{it-1} + \sum_{k=1}^{12} \Psi_{kt} FRB_{ik} + \varepsilon_{it}.$$

The key item of interest from this regression is the estimated coefficient on $B_{it-1}$, which we denote by $\beta_t$. As discussed earlier, this coefficient can be thought of as a measure of the intensity of liquidity constraints in a given size class at time $t$.

In the second step of our procedure, we take for each size class the $\beta_t$'s, and use them as the dependent variable in a purely time-series regression. We consider two variants of this time-series regression. In the first, "univariate" specification, the right-hand-side variables include: (i) the contemporaneous value and four lags of the change in the monetary measure $M_t$, as well as (ii) a linear time trend:[24]

$$(2)\quad \beta_t = \eta + \sum_{j=0}^{4} \phi_j \Delta M_{t-j} + \delta TIME_t + u_t.$$

In the second, "bivariate" specification, we also add the contemporaneous value and four lags of real GDP growth to the right-hand side:[25]

$$(3)\quad \beta_t = \eta + \sum_{j=0}^{4} \phi_j \Delta M_{t-j} + \sum_{j=0}^{4} \gamma_j \Delta GDP_{t-j} + \delta TIME_t + u_t.$$

In either case, our hypothesis is that, for the smallest class of banks, an expansionary impulse to $M_t$ should lead to a reduction in $\beta_t$—i.e., the sum of the $\phi$'s should be negative.

As an alternative to this method, we also try in Section IV, subsection A, below a more

[23] For the smallest size class, we also tried replacing the Federal Reserve-district dummies with state-level dummies, to get a tighter geographic control. This made no difference. Nor did using more complex lag specifications, including, e.g., quadratic lagged-lending terms.

[24] The time trend turns out to be borderline significant in some cases, and insignificant in others. If it is deleted from the specification, nothing changes significantly. We discuss one potential economic interpretation of the time trend below.

[25] We also experimented with including four lags of the dependent variable $\beta_t$ to the right-hand side. However, conditional on the real GDP lags being already in the regression, this adds nothing further—the lagged dependent variables are always insignificant, and have no substantive impact on any of the other coefficient estimates.

tightly parametrized one-step, interactive specification, where we run the change in $L_{it}$ against: (i) $B_{it-1}$; (ii) the change in $M_t$; and (iii) $B_{it-1}$ interacted with the change in $M_t$. In this case, the tests center on the interaction coefficients. What distinguishes the two-step approach is that it *allows for a different macro shock in each period for each Federal Reserve district.* This makes it harder to explain away our results based on unobserved loan-demand variability. For example, the two-step specification prevents us from taking credit for any decline in lending that is common to all banks in the Chicago district in a given quarter, even if all these banks have similarly weak balance sheets. As will become clear, the trade-off relative to the one-step method is that this potentially sacrifices a great deal of statistical power.

One control that we do not adopt is a bank-level fixed effect. There are two reasons for this. First, we would lose much of the variation in our explanatory variable—67 percent of the total variation in $B_{it}$ is eliminated by bank fixed effects.[26] Second, we worry that the remaining within-bank variation in $B_{it}$ is contaminated by the kind of endogeneity that is most difficult to address.[27] This is not to say that there are no endogeneity issues with respect to across-bank variation in $B_{it}$, but as we argue momentarily, these can be dealt with to some degree.

## B. *Potential Biases and Other Pitfalls*

Before turning to the results, we highlight a number of issues that could pose problems. The single biggest source of concern is that in our first-step regression—like in all of the liquidity-constraints literature—we use an endogenous right-hand-side variable in $B_{it}$. This endogeneity can take a number of different forms, some of which are more troubling for us than others.

1. *Biases in the Level of* $\beta_t$.—First and most obviously, the first-step regression delivers estimates of the *level* of $\beta_t$ that are potentially biased. In principle, this bias could be either positive or negative, but in a banking context, a natural story goes as follows. Because of demographic factors, some banks have an advantage at deposit taking, but few good lending opportunities. Rather than make bad loans, these banks have portfolios that are skewed towards securities. If the weak lending opportunities are only imperfectly controlled for by past loan growth, there may be a tendency for high values of $B_{it-1}$ to be associated with slow growth of $L_{it}$—i.e., $\beta_t$ will be biased downward.

However, the key point to note is that biases in the *level* of $\beta_t$ are in and of themselves not an issue, since our hypothesis centers on the *correlation* of $\beta_t$ with $M_t$. Indeed, if the *only* variation in $B_{it}$ across banks arose from the specific link sketched above—that some banks have fewer lending opportunities and hence hold more in securities—there would be no reason to expect a spurious correlation between $\beta_t$ and $M_t$, and our tests would be wholly uncontaminated.

2. *Biases in the Correlation of* $\beta_t$ *and* $M_t$.—Unfortunately, there may be other endogenous influences on $B_{it}$ that are more problematic, in that they lead to a bias in the estimated $\phi$ coefficients on $M_t$ in the second-step regression. Generally speaking, this happens when there is an endogenous link between $B_{it}$ and the *cyclical sensitivity* of loan demand. In principle, the bias can go either way. First, consider what might be called the "heterogeneous risk aversion" story, wherein certain banks are inherently more conservative than others. Conservative banks tend to protect themselves both by having larger values of $B_{it}$, as well as by shunning cyclically sensitive customers—i.e., there is a negative correlation between $B_{it}$ and the cyclical sensitivity of loan demand. This can lead to a bias in which the estimated effect of $M_t$ on $\beta_t$ is too negative. Thus we may be biased towards being too aggressive, rejecting the null hypothesis even when it is true.

Alternatively, consider the "rational buffer-stocking" story, in which all banks have the same risk aversion, but some have more opportunities to lend to cyclically sensitive customers than others. In this case, those banks with more cyclically sensitive customers will rationally

[26] This is after accounting for the time/geographic dummies.

[27] For example, consider a bank with a $B_{it}$ that is only 20 percent at time $t - 1$, but that spikes up to 25 percent at time $t$. A fixed-effects model would deem the bank unusually liquid at time $t$ (although its value of $B_{it}$ is still lower than most banks'). But the shock may just reflect a surge in bank profits due to improved borrower performance. So if we now see the bank lending more, it would be wrong to credit a strong balance sheet—rather it may just be an increase in loan demand.

choose to insulate themselves against the greater risk by having higher values of $B_{it}$. Now the direction of the bias is reversed—there will be a positive influence on our key coefficients—and we will tend to be too conservative, failing to reject the null hypothesis even when it is false.

A priori, the latter story strikes us as more plausible, in that it can be easily told within the context of a fully rational model.[28] Nonetheless, it is obviously important for us to ascertain which of the stories is of more relevance in the data. Fortunately, there are a couple of distinct ways to do so. The first emerges out of the bivariate version of the second-step regression. If the heterogeneous risk aversion story is true, the $\gamma$ coefficients on GDP growth should be negative. In contrast, if the rational buffer-stocking story is true, the $\gamma$'s should be positive. The intuition is straightforward. Under the heterogeneous risk-aversion story, an increase in GDP favors riskier borrowers, who are affiliated with less conservative banks, who in turn have lower values of $B_{it}$. Thus an increase in GDP has a more positive impact on the lending of low-$B_{it}$ banks, which implies a negative coefficient in a regression of $\beta_t$ on GDP.

As a second method of deducing the direction of the bias, one can look to the results for the largest banks. In the limiting case where there are no capital-market frictions facing these banks, any nonzero $\phi$ coefficients on $M_t$ in the second-step regression must reflect the direction of the bias. If the $\phi$'s for the largest banks are negative, this supports the heterogeneous risk-aversion story, while if they are positive, this favors the rational buffer-stocking story. Thus, while it may seem counterintuitive, the evidence will be more strongly in favor of our hypothesis if we get the *opposite signs* on the $\phi$'s for large and small banks. As will be seen shortly, both pieces of evidence point to the rational buffer-stocking story. So if anything, our tests for the small banks are probably biased towards being too conservative.

Ideally, in addition to just figuring out the direction of the bias, we would also devise an instrumental-variables procedure to purge it from our estimates. Unfortunately, to do this properly requires creating an instrument for $B_{it}$ that is uncorrelated with loan cyclicality—a difficult task. Still, we can at least make a partial effort, by regressing $B_{it}$ against any plausible *observable* measures of loan cyclicality, and using the residuals from this regression as our instruments. For example, it seems reasonable to posit that some categories of loans are on average more cyclically sensitive than others. In this spirit, we can regress a bank's $B_{it}$ against its ratio of C&I to total loans, its ratio of mortgages to total loans, etc., and use the residuals as instruments. The results from these "quasi-IV" tests are virtually identical to those from the baseline specifications that we report below.[29]

3. *Disentangling the Direct Effects of Monetary Policy vs. Bank Capital Shocks.*—While our focus is on the narrow question of how open-market operations work, there are other mechanisms that can generate similar effects on bank lending. In particular, a growing literature argues that lending will be constrained by banks' equity capital, which in turn can be impacted by a wide variety of shocks—changes in interest rates, real estate values, etc.[30] From the perspective of this literature, one caveat is that our results may not be capturing the workings of the lending channel, but rather an indirect capital-shock effect. According to this story, tight money simply raises rates and suppresses economic activity, causing banks to experience loan losses and reductions in capital. This in turn leads weaker banks to cut back on new lending.

Fortunately, it is possible to disentangle the two alternatives. The capital-shock story implies two

[28] Although the heterogeneous risk-aversion story might be justified by appealing to agency effects that vary in strength across banks.

[29] For the sake of brevity, we do not tabulate the results of the quasi-IV regressions here, but they can be found in the NBER working paper version (Kashyap and Stein, 1997).

[30] This literature shares with our work the broad theme that banks face costs of external finance, but the emphasis is on frictions in the equity market, as opposed to the market for uninsured bank debt. See Bengt Holmström and Jean Tirole (1997) for a model; Katherine Samolyk (1994), Joe Peek and Eric Rosengren (1995, 1997), Houston et al. (1997), and Ruby P. Kishan and Timothy P. Opelia (2000) for examples of recent empirical work; Steven A. Sharpe (1995) for a survey.

predictions about the bivariate version of the second-step regressions. First, adding GDP growth (or any proxy for activity) should diminish the importance of the monetary measure $M_t$. Second, the $\gamma$ coefficients on GDP growth should be negative. As will be seen, neither prediction is borne out, suggesting that our results are not driven by capital-shock effects.[31]

## III. Baseline Results

Tables 3 and 4 present the results of our second-step regressions. Table 3 gives a compact overview of all the specifications, showing only one number (with the associated standard error) from each regression: the sum of the $\phi$ coefficients on the relevant monetary indicator. The table is divided into two panels: Panel A for C&I loans, and Panel B for total loans. In each panel, there are 12 test statistics. First, we test six ways whether the sum of the $\phi$'s is negative for the "small" banks—those in the bottom 95 percent of the size distribution. The six tests correspond to our univariate and bivariate specifications for each of the three monetary indicators. Second, we test in the same six ways whether the sum of the $\phi$'s is *lower* for the small banks than for the "big" banks—those in the top 1 percent of the size distribution.

As can be seen from Panel A of Table 3, the overall results for C&I loans are strong. Consider first the results for the small banks. In all six cases, the point estimates are negative, consistent with the theory. Moreover, in two of six cases, the estimates are significant at the 2.0-percent level or better; in two others, the standard errors imply $p$-values that are around 9.0 percent.[32]

Next, turn to the small-bank/big-bank differentials. In every case, the estimate for the big banks is *positive,* so that these differentials are *larger* in absolute value than the

TABLE 3—TWO-STEP ESTIMATION OF EQUATIONS (1), (2), AND (3): SUM OF COEFFICIENTS ON MONETARY-POLICY INDICATOR

Panel A: C&I Loans

| | Univariate | Bivariate |
|---|---|---|
| 1. Boschen-Mills | | |
| <95 | −0.0438 | −0.0131 |
| | (0.0188) | (0.0187) |
| 95–99 | −0.0339 | 0.0094 |
| | (0.0401) | (0.0303) |
| >99 | 0.0960 | 0.1411 |
| | (0.0661) | (0.0428) |
| Small-Big | −0.1398 | −0.1542 |
| | (0.0611) | (0.0449) |
| 2. Funds rate | | |
| <95 | −0.0267 | −0.0151 |
| | (0.0071) | (0.0089) |
| 95–99 | −0.0066 | 0.0097 |
| | (0.0137) | (0.0112) |
| >99 | 0.0795 | 0.1175 |
| | (0.0281) | (0.0314) |
| Small–Big | −0.1062 | −0.1327 |
| | (0.0296) | (0.0376) |
| 3. Bernanke-Mihov | | |
| <95 | −1.8633 | −0.5269 |
| | (1.0933) | (1.2463) |
| 95–99 | 0.7345 | 3.3461 |
| | (2.1853) | (2.1119) |
| >99 | 4.7862 | 7.5911 |
| | (3.5220) | (2.3927) |
| Small–Big | −6.6495 | −8.1181 |
| | (3.3966) | (3.0215) |

Panel B: Total Loans

| | Univariate | Bivariate |
|---|---|---|
| 1. Boschen-Mills | | |
| <95 | −0.0179 | −0.0044 |
| | (0.0110) | (0.0120) |
| 95–99 | −0.0129 | 0.0167 |
| | (0.0236) | (0.0118) |
| >99 | 0.0516 | 0.0921 |
| | (0.0522) | (0.0373) |
| Small–Big | −0.0695 | −0.0965 |
| | (0.0464) | (0.0348) |
| 2. Funds rate | | |
| <95 | −0.0088 | −0.0046 |
| | (0.0037) | (0.0049) |
| 95–99 | −0.0126 | −0.0040 |
| | (0.0079) | (0.0060) |
| >99 | 0.0258 | 0.0460 |
| | (0.0188) | (0.0152) |
| Small–Big | −0.0346 | −0.0506 |
| | (0.0182) | (0.0174) |
| 3. Bernanke-Mihov | | |
| <95 | −0.1926 | 0.7827 |
| | (0.5344) | (0.5780) |
| 95–99 | −0.2849 | 1.1191 |
| | (1.1178) | (0.7766) |
| >99 | 3.6558 | 6.7373 |
| | (2.5209) | (1.4636) |
| Small–Big | −3.8484 | −5.9545 |
| | (2.2180) | (1.5971) |

*Note:* Standard errors are in parentheses.

[31] We are not claiming that bank capital does not affect lending—only that it does not explain away our results. Indeed, the positive time trend in $\beta_t$ that shows up in some regressions may reflect the well-documented bank-capital problems of the late 1980's and early 1990's.

[32] In all cases, the tables display robust standard errors that account for heteroskedasticity and serial correlation. Moreover, when comparing the small and big bank estimates, the standard errors also account for the correlation of the residuals across these two equations.

TABLE 4—TWO-STEP ESTIMATION OF EQUATIONS (1), (2), AND (3): FULL DETAILS

Panel A: Money Measure: Change in Boschen-Mills

| | Monetary-policy indicator | | | | | Change in GDP | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| C&I loans | 0 | 1 | 2 | 3 | 4 | 0 | 1 | 2 | 3 | 4 |
| Univariate | | | | | | | | | | |
| <95 | −0.0074 | −0.0066 | −0.0138 | −0.0137 | −0.0023 | | | | | |
| ($R^2$ = 0.1357) | (0.0074) | (0.0041) | (0.0044) | (0.0056) | (0.0048) | | | | | |
| 95–99 | 0.0088 | −0.0172 | −0.0226 | −0.0139 | 0.0111 | | | | | |
| ($R^2$ = 0.1259) | (0.0139) | (0.0132) | (0.0211) | (0.0132) | (0.0140) | | | | | |
| >99 | −0.0193 | 0.0105 | 0.0329 | −0.0002 | 0.0722 | | | | | |
| ($R^2$ = 0.0988) | (0.0114) | (0.0189) | (0.0315) | (0.0220) | (0.0205) | | | | | |
| Bivariate | | | | | | | | | | |
| <95 | −0.0037 | −0.0008 | −0.0064 | −0.0054 | 0.0031 | 0.6259 | 0.2955 | 0.7707 | 0.9165 | 0.2920 |
| ($R^2$ = 0.3404) | (0.0057) | (0.0046) | (0.0033) | (0.0076) | (0.0047) | (0.5292) | (0.2489) | (0.3127) | (0.2665) | (0.3831) |
| 95–99 | 0.0151 | −0.0088 | −0.0149 | −0.0033 | 0.0213 | 0.9987 | 0.7424 | 0.6632 | 0.5668 | 1.2774 |
| ($R^2$ = 0.2086) | (0.0133) | (0.0148) | (0.0177) | (0.0107) | (0.0133) | (0.5464) | (0.8997) | (1.2009) | (1.0185) | (0.6125) |
| >99 | −0.0141 | 0.0100 | 0.0317 | 0.0225 | 0.0910 | 1.2170 | 0.8454 | −3.8951 | 4.2036 | 3.1755 |
| ($R^2$ = 0.2589) | (0.0088) | (0.0175) | (0.0201) | (0.0146) | (0.0185) | (1.4284) | (1.1834) | (2.8855) | (2.4012) | (1.0001) |

Panel B: Money Measure: Change in Federal Funds Rate

| | Monetary-policy indicator | | | | | Change in GDP | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| C&I loans | 0 | 1 | 2 | 3 | 4 | 0 | 1 | 2 | 3 | 4 |
| Univariate | | | | | | | | | | |
| <95 | −0.0069 | −0.0077 | −0.0062 | −0.0054 | −0.0005 | | | | | |
| ($R^2$ = 0.2868) | (0.0019) | (0.0022) | (0.0020) | (0.0012) | (0.0012) | | | | | |
| 95–99 | −0.0018 | −0.0030 | −0.0027 | −0.0056 | 0.0065 | | | | | |
| ($R^2$ = 0.0834) | (0.0021) | (0.0038) | (0.0058) | (0.0054) | (0.0013) | | | | | |
| >99 | 0.0118 | 0.0140 | 0.0235 | 0.0142 | 0.0161 | | | | | |
| ($R^2$ = 0.0958) | (0.0075) | (0.0063) | (0.0082) | (0.0042) | (0.0075) | | | | | |
| Bivariate | | | | | | | | | | |
| <95 | −0.0053 | −0.0059 | −0.0040 | −0.0023 | 0.0024 | 0.5664 | −0.0587 | 0.4089 | 1.0498 | 0.5039 |
| ($R^2$ = 0.4526) | (0.0014) | (0.0021) | (0.0024) | (0.0026) | (0.0021) | (0.5115) | (0.3923) | (0.3866) | (0.4193) | (0.2334) |
| 95–99 | 0.0015 | 0.0006 | 0.0002 | −0.0036 | 0.0110 | 1.0847 | 0.7481 | −0.0294 | 1.3017 | 1.2965 |
| ($R^2$ = 0.1870) | (0.0034) | (0.0045) | (0.0052) | (0.0063) | (0.0028) | (0.7850) | (1.1979) | (0.9570) | (0.8977) | (0.6423) |
| >99 | 0.0146 | 0.0113 | 0.0236 | 0.0250 | 0.0431 | 1.2667 | −0.7602 | −5.1621 | 7.5245 | 3.3015 |
| ($R^2$ = 0.3442) | (0.0068) | (0.0051) | (0.0103) | (0.0093) | (0.0133) | (1.8068) | (1.6353) | (3.4558) | (2.1000) | (1.6629) |

Panel C: Money Measure: Change in Bernanke-Mihov

| | Monetary-policy indicator | | | | | Change in GDP | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| C&I loans | 0 | 1 | 2 | 3 | 4 | 0 | 1 | 2 | 3 | 4 |
| Univariate | | | | | | | | | | |
| <95 | −0.0237 | 0.2379 | −0.2093 | −1.2548 | −0.6135 | | | | | |
| ($R^2$ = 0.1440) | (0.3317) | (0.4173) | (0.3736) | (0.3094) | (0.2980) | | | | | |
| 95–99 | 1.1588 | 0.9354 | −0.3920 | −1.5933 | 0.6255 | | | | | |
| ($R^2$ = 0.0887) | (0.5546) | (0.6414) | (0.9313) | (1.0750) | (0.5074) | | | | | |
| >99 | 0.0938 | 0.5484 | 0.6991 | 1.7404 | 1.7045 | | | | | |
| ($R^2$ = 0.0309) | (1.7450) | (1.3596) | (1.5339) | (1.0032) | (1.0018) | | | | | |
| Bivariate | | | | | | | | | | |
| <95 | 0.1069 | 0.1053 | −0.1351 | −0.6614 | 0.0574 | 0.6555 | 0.1692 | 0.8708 | 0.8861 | 0.3600 |
| ($R^2$ = 0.3535) | (0.2957) | (0.3275) | (0.2422) | (0.3316) | (0.4119) | (0.6661) | (0.2782) | (0.3824) | (0.2394) | (0.2609) |
| 95–99 | 1.4562 | 0.8915 | −0.1876 | −0.6774 | 1.8634 | 1.5949 | 0.5113 | 1.3196 | 0.6149 | 1.089 |
| ($R^2$ = 0.2043) | (0.4627) | (0.5309) | (0.8566) | (0.8073) | (0.4651) | (0.6306) | (0.8733) | (1.3575) | (0.8721) | (0.5117) |
| >99 | 1.1890 | 0.7249 | −0.3289 | 2.6111 | 3.3949 | 2.5876 | 0.8679 | −3.8538 | 4.2408 | 1.8398 |
| ($R^2$ = 0.1781) | (1.2061) | (0.9098) | (1.8175) | (1.0684) | (0.9044) | (1.8625) | (1.2244) | (3.9985) | (2.7145) | (1.0878) |

corresponding figures for the small banks in isolation. Moreover, each of the six small-bank/big-bank differentials is significant at the 5.0-percent level or better; indeed, four of the $p$-values are well below 1.0 percent. These results help us begin to discriminate

TABLE 4—*Continued.*

| Panel D: Money Measure: Change in Boschen-Mills | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Monetary-policy indicator | | | | | Change in GDP | | | | |
| Total loans | 0 | 1 | 2 | 3 | 4 | 0 | 1 | 2 | 3 | 4 |
| Univariate | | | | | | | | | | |
| <95 | −0.0045 | −0.0124 | 0.0023 | 0.0012 | −0.0045 | | | | | |
| ($R^2$ = 0.2346) | (0.0040) | (0.0033) | (0.0021) | (0.0035) | (0.0044) | | | | | |
| 95–99 | 0.0028 | −0.0066 | 0.0024 | 0.0005 | −0.0121 | | | | | |
| ($R^2$ = 0.0692) | (0.0064) | (0.0046) | (0.0090) | (0.0073) | (0.0049) | | | | | |
| >99 | −0.0238 | 0.0121 | 0.0213 | 0.0202 | 0.0218 | | | | | |
| ($R^2$ = 0.1208) | (0.0118) | (0.0077) | (0.0210) | (0.0155) | (0.0140) | | | | | |
| Bivariate | | | | | | | | | | |
| <95 | −0.0024 | −0.0099 | 0.0049 | 0.0051 | −0.0020 | 0.3639 | 0.2310 | 0.0841 | 0.4924 | 0.1157 |
| ($R^2$ = 0.3216) | (0.0036) | (0.0040) | (0.0022) | (0.0044) | (0.0042) | (0.2315) | (0.2037) | (0.1818) | (0.3089) | (0.3651) |
| 95–99 | 0.0068 | 0.0019 | 0.0087 | 0.0070 | −0.0077 | 0.2406 | 1.4108 | 0.2038 | 0.9510 | −0.2656 |
| ($R^2$ = 0.2847) | (0.0055) | (0.0033) | (0.0064) | (0.0047) | (0.0037) | (0.2614) | (0.2796) | (0.3785) | (0.4722) | (0.5489) |
| >99 | −0.0182 | 0.0153 | 0.0244 | 0.0364 | 0.0341 | 1.1544 | 0.6884 | −1.7162 | 2.6482 | 1.6745 |
| ($R^2$ = 0.2880) | (0.0115) | (0.0095) | (0.0153) | (0.0132) | (0.0110) | (0.9052) | (0.5291) | (1.6450) | (1.0135) | (0.8612) |
| **Panel E: Money Measure: Change in Federal Funds Rate** | | | | | | | | | | |
| Total loans | Monetary-policy indicator | | | | | Change in GDP | | | | |
| 0 | 1 | 2 | 3 | 4 | 0 | 1 | 2 | 3 | 4 | |
| Univariate | | | | | | | | | | |
| <95 | −0.0033 | −0.0032 | −0.0015 | −0.0001 | −0.0006 | | | | | |
| ($R^2$ = 0.1607) | (0.0011) | (0.0013) | (0.0011) | (0.0009) | (0.0010) | | | | | |
| 95–99 | −0.0042 | −0.0038 | −0.0023 | −0.0018 | −0.0005 | | | | | |
| ($R^2$ = 0.0769) | (0.0018) | (0.0016) | (0.0025) | (0.0016) | (0.0021) | | | | | |
| >99 | −0.0041 | 0.0044 | 0.0102 | 0.0068 | 0.0085 | | | | | |
| ($R^2$ = 0.1084) | (0.0037) | (0.0045) | (0.0056) | (0.0036) | (0.0037) | | | | | |
| Bivariate | | | | | | | | | | |
| <95 | −0.0025 | −0.0020 | −0.0004 | 0.0003 | 0.0000 | 0.1830 | 0.3422 | 0.0758 | 0.3096 | 0.1688 |
| ($R^2$ = 0.2202) | (0.0012) | (0.0017) | (0.0015) | (0.0015) | (0.0018) | (0.3502) | (0.3213) | (0.2251) | (0.4556) | (0.3703) |
| 95–99 | −0.0021 | 0.0002 | 0.0014 | −0.0024 | −0.0011 | −0.0999 | 1.7631 | 0.0265 | 0.5797 | −0.1162 |
| ($R^2$ = 0.2665) | (0.0011) | (0.0011) | (0.0023) | (0.0018) | (0.0021) | (0.4222) | (0.4076) | (0.3354) | (0.5349) | (0.5732) |
| >99 | −0.0021 | 0.0034 | 0.0101 | 0.0115 | 0.0231 | 0.8456 | −0.0946 | −2.9525 | 3.7976 | 2.2407 |
| ($R^2$ = 0.3450) | (0.0030) | (0.0038) | (0.0054) | (0.0056) | (0.0057) | (1.0891) | (0.6442) | (1.9395) | (0.0745) | (0.9924) |
| **Panel F: Money Measure: Change in Bernanke-Mihov** | | | | | | | | | | |
| | Monetary-policy indicator | | | | | Change in GDP | | | | |
| Total loans | 0 | 1 | 2 | 3 | 4 | 0 | 1 | 2 | 3 | 4 |
| Univariate | | | | | | | | | | |
| <95 | 0.2491 | 0.0389 | 0.0671 | −0.4285 | −0.1192 | | | | | |
| ($R^2$ = 0.1254) | (0.2014) | (0.3583) | (0.1698) | (0.2176) | (0.1805) | | | | | |
| 95–99 | 0.4467 | 0.4244 | 0.1109 | −1.1053 | −0.1616 | | | | | |
| ($R^2$ = 0.1280) | (0.2725) | (0.2800) | (0.3769) | (0.3610) | (0.4164) | | | | | |
| >99 | −0.7211 | 1.0204 | 2.3224 | 0.2335 | 0.8005 | | | | | |
| ($R^2$ = 0.1406) | (1.3088) | (0.6189) | (0.8896) | (0.8930) | (0.7678) | | | | | |
| Bivariate | | | | | | | | | | |
| <95 | 0.3874 | 0.0854 | 0.1358 | −0.1057 | 0.2799 | 0.6243 | 0.3872 | 0.138 | 0.2682 | 0.1502 |
| ($R^2$ = 0.2420) | (0.1781) | (0.2625) | (0.1331) | (0.1916) | (0.2215) | (0.2974) | (0.1333) | (0.2239) | (0.3109) | (0.3757) |
| 95–99 | 0.5088 | 0.5294 | 0.3072 | −0.5354 | 0.309 | 0.4486 | 1.3427 | 0.0998 | 0.6261 | −0.0366 |
| ($R^2$ = 0.3040) | (0.2571) | (0.2064) | (0.2425) | (0.3127) | (0.3746) | (0.2477) | (0.2580) | (0.3455) | (0.4909) | (0.5315) |
| >99 | 0.0652 | 1.2694 | 2.0188 | 1.079 | 2.3049 | 2.585 | 0.9495 | −1.4611 | 1.6821 | 1.439 |
| ($R^2$ = 0.3068) | (1.0846) | (0.4983) | (0.9654) | (1.0308) | (0.3927) | (0.6407) | (0.5772) | (2.0710) | (1.3014) | (0.7247) |

*Notes:* Standard errors are in parentheses. All regressions also contain a time trend, which is not shown.

between the two types of endogeneity effects that might be biasing our estimates for the small banks. As discussed above, the fact that the sum of the $\phi$'s for the big banks is always positive is supportive of the rational buffer-stocking story.

This suggests that the magnitude of the $\phi$'s from the small-bank regressions might be *understating* the effects of monetary policy on $\beta_t$. Taking the logic further, one might be tempted to argue that the effects of monetary policy would be more accurately measured by the small-bank/big-bank differentials. However, some caution is probably warranted on this latter point. Not only is the sum of the $\phi$'s for the big banks positive in all our specifications, in most cases the estimates are surprisingly large, often several times (in absolute value) the size of the corresponding negative estimates for small banks. It may well be reasonable to ascribe these large positive values to a strong bias induced by rational buffer-stocking, and to posit that the bias has the same *sign* for big and small banks. It is more of a leap to claim that the bias is of the same *size* for big and small banks, which is what one must believe if one is to use the small-bank/big-bank differentials to explicitly quantify the effects of monetary policy on $\beta_t$. Given that the implied bias is so large for the big banks, and given that we do not have a precise understanding of why this might be so, care should be taken not to overinterpret the small-bank/big-bank differentials in this regard.

In Panel B, with total loans, the point estimates generally go in the same direction as in Panel A—five of six estimates for the small-bank category are negative, and all six for the big-bank category are positive. But the magnitude of the small-bank estimates is typically only about one-third to one-quarter that of the corresponding values in Panel A. Consequently, only 4 of the total of 12 test statistics are significant at 2.0 percent; three other $p$-values are below 10.5 percent.

Why are the results for C&I loans stronger than those for total loans? There are at least two possible explanations. First, this outcome is to be expected based on the rational buffer-stocking story. If this story is correct, our estimates are generally too conservative, and the conservatism will be more pronounced for total loans, since aggregation across loan categories of different cyclicality exacerbates any bias. Second, and more simply, it may be that because of their short maturity, banks can adjust C&I volume more readily than volume in other categories, such as long-term mortgages. If this is so, the effects that we are looking for will emerge more clearly with C&I loans.

Table 4 presents the details of the individual regressions that make up Table 3. There are six panels, A through F, one for each combination of loan type and monetary indicator. Most of the patterns are similar across panels, so it is instructive to focus first on just one—Panel B, for C&I loans and the federal funds rate—for which the estimates are the most precise. A couple of salient facts emerge. First, while we reported in Table 3 only the sums of the five $\phi$ coefficients (lags 0 through 4), we can now look at all the individual $\phi$'s, and see that the sums are not hiding any erratic behavior. In fact, for the small-bank category, every single one of the individual $\phi$'s is negative in the univariate specification, and all but one are negative in the bivariate specification. Moreover, in both cases, the implied response of $\beta_t$ to a monetary shock has a plausible hump shape for the small banks, with the coefficients increasing over the first couple of lags and then gradually dying down. Second, in the bivariate versions of the specifications, the $\gamma$ coefficients on GDP are for the most part positive. Again, this is consistent with the rational buffer-stocking story, and thus gives us yet another reason to think that our estimates for the small-bank category err on the side of conservatism.[33]

Comparing across the different panels in Table 4, one can get an idea of how well the second-step regressions fit with the different indicators. The federal funds rate clearly has the most explanatory power of our three measures. For example, Panel B tells us that with C&I loans, the univariate second-step regression for small banks that uses the funds rate achieves an $R^2$ of 29 percent. In the bivariate specification that adds GDP, the $R^2$ rises to 45 percent. Considering that the left-hand-side variable in this regression is just a noisy proxy for the degree of banks' liquidity constraints, these numbers strike us as quite remarkable.

[33] There is another reason why the coefficients on GDP might be positive: an increase in activity raises loan demand, and liquid banks are more able to accommodate their customers—i.e., increased demand makes banks' liquidity constraints more binding.

## IV. Robustness

We have already mentioned a number of robustness checks throughout the text and footnotes. Just to remind the reader of some of the more significant ones, our results are generally unaffected by: how we screen for outliers; whether we base our analysis on banks versus bank holding companies; whether we include cash in our measure of liquidity; whether we use a more complex lag specification or tighter geographic controls in our first-step regressions; and whether or not a time trend is included in the second-step regressions. We also obtain essentially identical results with a "quasi-IV" approach that purges our liquidity measure of any correlation with observable measures of loan riskiness. However, there remain a couple of items which merit a more detailed treatment.

### A. *An Interactive, One-Step Regression Approach*

As argued above, our two-step method probably errs on the side of being overparameterized. Thus we now consider a more tightly structured approach, compressing our "univariate" and "bivariate" two-step models into the following one-step models respectively:

$$(4)\quad \Delta \log(L_{it}) = \sum_{j=1}^{4} \alpha_j \Delta \log(L_{it-j}) + \sum_{j=0}^{4} \mu_j \Delta M_{t-j} + \Theta TIME_t + \sum_{k=1}^{3} \rho_k QUARTER_{kt} + \sum_{k=1}^{12} \Psi_k FRB_{ik} + B_{it-1}\left(\eta + \delta TIME_t + \sum_{j=0}^{4} \phi_j \Delta M_{t-j}\right) + \varepsilon_{it}$$

$$(5)\quad \Delta \log(L_{it}) = \sum_{j=1}^{4} \alpha_j \Delta \log(L_{it-j}) + \sum_{j=0}^{4} \mu_j \Delta M_{t-j} \Delta \log(L_{it}) = \sum_{j=1}^{4} \alpha_j \Delta \log(L_{it-j}) + \sum_{j=0}^{4} \mu_j \Delta M_{t-j} + \sum_{j=0}^{4} \pi_j \Delta GDP_{t-j} + \Theta TIME_t + \sum_{k=1}^{3} \rho_k QUARTER_{kt} + \sum_{k=1}^{12} \Psi_k FRB_{ik} + B_{it-1}\left(\eta + \delta TIME_t + \sum_{j=0}^{4} \phi_j \Delta M_{t-j} + \sum_{j=0}^{4} \gamma_j \Delta GDP_{t-j}\right) + \varepsilon_{it}.$$

By comparing equations (4) and (5) with equations (1)–(3), one can see the main differences between the two methods. In the two-step method, macro variation in loan growth is absorbed with a separate dummy term for each of $k$ Federal Reserve districts in each of $t$ periods (i.e., a total of $kt$ dummies). In the one-step method, there are only $k$ time-invariant Federal Reserve-district dummies, and macro effects are modeled much more parsimoniously as a linear function of changes in monetary policy and GDP.[34]

Table 5 presents an overview of the estimates of $\phi$ generated by the one-step approach. As can be seen, the point estimates are generally quite close to those in Table 3. However, the standard errors are much reduced, leading to more strongly significant $p$-values. This outcome is

[34] The two-step method also allows the lag coefficients on past loan growth—the $\alpha$'s—to vary period by period, while the one-step method makes them time-invariant.

TABLE 5—ONE-STEP ESTIMATION OF EQUATIONS (4) AND (5): SUM OF COEFFICIENTS ON MONETARY-POLICY INDICATOR

| Panel A: C&I Loans | Univariate | Bivariate |
|---|---|---|
| 1. Boschen-Mills | | |
| <95 | −0.0614 | −0.0430 |
| | (0.0069) | (0.0077) |
| 95–99 | −0.0171 | 0.0242 |
| | (0.0276) | (0.0288) |
| >99 | 0.0862 | 0.1337 |
| | (0.0530) | (0.0581) |
| Small–Big | −0.1475 | −0.1767 |
| | (0.0535) | (0.0586) |
| 2. Funds rate | | |
| <95 | −0.0339 | −0.0238 |
| | (0.0022) | (0.0041) |
| 95–99 | −0.0013 | 0.0102 |
| | (0.0133) | (0.0147) |
| >99 | 0.0602 | 0.0903 |
| | (0.0239) | (0.0266) |
| Small–Big | −0.0941 | −0.1141 |
| | (0.0240) | (0.0269) |
| 3. Bernanke-Mihov | | |
| <95 | −2.7518 | −1.7802 |
| | (0.3920) | (0.4567) |
| 95–99 | 1.3557 | 3.7417 |
| | (1.5929) | (1.7226) |
| >99 | 3.8509 | 6.3203 |
| | (2.7288) | (3.0582) |
| Small–Big | −6.6027 | −8.1005 |
| | (2.7568) | (3.0921) |

| Panel B: Total Loans | Univariate | Bivariate |
|---|---|---|
| 1. Boschen-Mills | | |
| <95 | −0.0309 | −0.0268 |
| | (0.0022) | (0.0022) |
| 95–99 | −0.0029 | 0.0392 |
| | (0.0155) | (0.0164) |
| >99 | −0.0117 | 0.0379 |
| | (0.0336) | (0.0380) |
| Small–Big | −0.0191 | −0.0647 |
| | (0.0337) | (0.0380) |
| 2. Funds rate | | |
| <95 | −0.0144 | −0.0119 |
| | (0.0007) | (0.0009) |
| 95–99 | −0.0013 | 0.0056 |
| | (0.0077) | (0.0084) |
| >99 | 0.0297 | 0.0509 |
| | (0.0130) | (0.0141) |
| Small–Big | −0.0440 | −0.0628 |
| | (0.0130) | (0.0141) |
| 3. Bernanke-Mihov | | |
| <95 | −0.6628 | 0.0803 |
| | (0.1410) | (0.1626) |
| 95–99 | 0.4789 | 2.5306 |
| | (0.8378) | (0.8774) |
| >99 | 1.6325 | 5.3095 |
| | (1.5571) | (1.8224) |
| Small–Big | −2.2953 | −5.2292 |
| | (1.5634) | (1.8296) |

*Note:* Standard errors are in parentheses.

what one would expect—to the extent that we are willing to impose more structure, and not throw away much of the variation in the data, our tests should become more powerful.

### B. *Results from Subsamples*

Finally, we check to see how our results hold up across subsamples. There are two motivations for doing so. First, as noted earlier, there are reasons to think that our monetary indicators may not be as reliable during the first part of our sample period, which contains the Volcker regime. Second, we would like to know if our conclusions are colored by Regulation-Q type restrictions, which were still in place in the early part of our sample period. By looking only at the latter part, we can directly address this concern.

In Table 6, we reproduce all the numbers in Table 3 for each of two subsamples. A clear pattern emerges: the results are almost uniformly stronger and more statistically significant in the second subsample, which begins in 1986Q1. For example, in spite of the reduced number of observations, we find that for this later period 10 of the 12 test statistics for C&I loans in Panel A have $p$-values of 5.5 percent or lower; eight have $p$-values below 1.0 percent. In contrast, while all but one of the C&I point estimates for the earlier period go the right way, only four are significant at the 5.2-percent level or better. This fits with the idea that it is harder to get an accurate handle on monetary policy during the first half of our sample period. It also makes it clear that our earlier results are not in any way driven by Regulation-Q-related factors.

## V. Economic Significance of the Results

So far, we have focused on the statistical significance of our estimates. Now we ask whether they imply economically interesting magnitudes. For the sake of transparency, we focus on the estimates from the funds-rate regressions. A first step is to quantify how two equal-sized banks with different values of $B_{it}$ respond to a shock. From Table 3, Panel A, the most conservative estimate of the sum of the $\phi$'s for small banks' C&I loans is −0.0151. (This comes from the bivariate spec-

TABLE 6—TWO-STEP ESTIMATION OF EQUATIONS (1), (2), AND (3): SPLIT SAMPLE RESULTS: SUM OF COEFFICIENTS ON MONETARY-POLICY INDICATOR

Panel A: C&I Loans

| | 76Q1–85Q4 | | 86Q1–93Q2 | |
|---|---|---|---|---|
| | Univariate | Bivariate | Univariate | Bivariate |
| 1. Boschen-Mills | | | | |
| <95 | −0.0756 | −0.0049 | −0.0074 | −0.0074 |
| | (0.0201) | (0.0156) | (0.0106) | (0.0175) |
| 95–99 | −0.1271 | −0.0537 | 0.0397 | 0.0317 |
| | (0.0445) | (0.0502) | (0.0269) | (0.0221) |
| >99 | −0.0561 | 0.0082 | 0.1981 | 0.1620 |
| | (0.0857) | (0.1092) | (0.0388) | (0.0361) |
| Small–Big | −0.0195 | −0.0131 | −0.2056 | −0.1694 |
| | (0.0897) | (0.1206) | (0.0407) | (0.0448) |
| 2. Funds rate | | | | |
| <95 | −0.0193 | −0.0004 | −0.0260 | −0.0256 |
| | (0.0099) | (0.0070) | (0.0101) | (0.0133) |
| 95–99 | −0.0203 | −0.0030 | 0.0367 | 0.0519 |
| | (0.0232) | (0.0167) | (0.0216) | (0.0225) |
| >99 | 0.0473 | 0.0899 | 0.1936 | 0.2084 |
| | (0.0258) | (0.0392) | (0.0318) | (0.0270) |
| Small–Big | −0.0665 | −0.0903 | −0.2196 | −0.2339 |
| | (0.0256) | (0.0456) | (0.0292) | (0.0294) |
| 3. Bernanke-Mihov | | | | |
| <95 | −0.9554 | 1.6565 | −1.8269 | −2.9728 |
| | (1.6555) | (0.4844) | (0.9415) | (0.8072) |
| 95–99 | −1.5482 | 1.2918 | 5.8490 | 8.2468 |
| | (3.5492) | (2.2360) | (2.4472) | (2.9796) |
| >99 | −0.1705 | 2.4275 | 15.2314 | 15.1635 |
| | (3.4808) | (2.6893) | (2.3654) | (2.6942) |
| Small–Big | −0.7849 | −0.7710 | −17.0583 | −18.1363 |
| | (2.2147) | (2.7472) | (2.2301) | (2.4477) |

Panel B: Total Loans

| | 76Q1–85Q4 | | 86Q1–93Q2 | |
|---|---|---|---|---|
| | Univariate | Bivariate | Univariate | Bivariate |
| 1. Boschen-Mills | | | | |
| <95 | −0.0417 | −0.0095 | 0.0018 | 0.0008 |
| | (0.0114) | (0.0160) | (0.0043) | (0.0055) |
| 95–99 | −0.0798 | −0.0204 | 0.0334 | 0.0309 |
| | (0.0189) | (0.0154) | (0.0106) | (0.0189) |
| >99 | −0.0800 | −0.0265 | 0.1449 | 0.1455 |
| | (0.0469) | (0.0601) | (0.0249) | (0.0289) |
| Small–Big | 0.0384 | 0.0170 | −0.1430 | −0.1447 |
| | (0.0473) | (0.0609) | (0.0268) | (0.0312) |
| 2. Funds rate | | | | |
| <95 | −0.0062 | 0.0015 | −0.0079 | −0.0135 |
| | (0.0056) | (0.0048) | (0.0041) | (0.0062) |
| 95–99 | −0.0217 | −0.0082 | 0.0175 | 0.0076 |
| | (0.0089) | (0.0051) | (0.0158) | (0.0151) |
| >99 | 0.0050 | 0.0290 | 0.1004 | 0.1121 |
| | (0.0162) | (0.0129) | (0.0180) | (0.0265) |
| Small–Big | −0.0113 | −0.0275 | −0.1083 | −0.1256 |
| | (0.0141) | (0.0167) | (0.0194) | (0.0282) |
| 3. Bernanke-Mihov | | | | |
| <95 | −0.0246 | 1.6395 | 0.0169 | −0.2564 |
| | (0.9106) | (0.3712) | (0.3865) | (0.5633) |
| 95–99 | −1.4103 | 0.1350 | 1.9791 | 2.3618 |
| | (1.7311) | (0.7047) | (1.2238) | (1.5434) |
| >99 | 1.1932 | 4.6248 | 10.0419 | 12.2748 |
| | (3.1775) | (1.6549) | (1.1024) | (1.3265) |
| Small–Big | −1.2178 | −2.9853 | −10.025 | −12.5311 |
| | (2.3791) | (1.7011) | (1.0415) | (1.1759) |

*Note:* Standard errors are in parentheses.

ification.) Now think of a "liquid" bank as having $B_{it}$ = 60.2 percent, and an "illiquid" bank as having $B_{it}$ = 20.6 percent; these numbers correspond to the 90th and 10th percentiles of the distribution for small banks in 1993Q2. In this case, four quarters after a 100-basis-point hike in the funds rate, the level of C&I loans of the illiquid bank will be roughly 0.6 percent lower than that of the liquid bank.[35] That is, if both banks started with a level of C&I loans equal to \$1,000, then purely on the basis of liquidity differences, we would predict a \$6 gap between the two banks a year after the funds-rate shock.

The estimates in Table 3 are also consistent with a much larger cross-sectional effect. If we base our calculation on the bivariate small-bank/big-bank coefficient *differential* of $-0.1327$ in Panel A of Table 3, we get a 5.3-percent gap in the level of C&I loans across the liquid and illiquid small banks one year after the rise in the funds rate. However, it is important to recall the caveat that applies to this second type of calculation: it implicitly assumes that the size of the rational buffer-stocking bias is the same for small and big banks. Given that we are attributing a large bias to the big banks, and given that we do not have a detailed understanding of the roots of this bias, such an assumption may well lead us to overstate the quantitative effects of monetary policy.

The preceding calculations only compare banks at extremes of the liquidity spectrum. To get an idea of the total impact of liquidity constraints across all small banks, we integrate over the distribution of $B_{it}$. To do this we use the actual $B_{it}$'s from 1993Q2, and assume that liquidity constraints are binding everywhere below the 90th percentile value of $B_{it}$. For example, if we maintain the conservative estimate of $-0.0151$ for the sum of the $\phi$'s, we conclude that one year after the shock to the funds rate, the *total* C&I lending of all small banks is 0.41 percent lower than it would be if all these small banks were unconstrained. Using the more aggressive estimate based on the small-bank/big-bank differentials, the corresponding number is 3.62 percent.

Table 7—Movement in Aggregate Small-Bank Lending Accounted for by Constrained Banks Four Quarters After a Federal Funds-Rate Shock of 100 Basis Points

| | Percentage change in lending due to constraints | Aggregate percentage change in lending |
|---|---|---|
| A. C&I loans | | |
| 1. Using univariate, small-bank sum of phi's | 0.73 | 1.01[a] |
| 2. Using bivariate, small-bank sum of phi's | 0.41 | 3.33[b] |
| 3. Using univariate, small–big bank differentials | 2.90 | 1.01[a] |
| 4. Using bivariate, small–big bank differentials | 3.62 | 3.33[b] |
| B. Total loans | | |
| 1. Using univariate, small-bank sum of phi's | 0.24 | 2.39[c] |
| 2. Using bivariate, small-bank sum of phi's | 0.13 | 3.15[d] |
| 3. Using univariate, small–big bank differentials | 0.95 | 2.39[c] |
| 4. Using bivariate, small–big bank differentials | 1.39 | 3.15[d] |

*Notes:* The numbers in the first column are based on the two-step estimates reported in Table 3.

The numbers in the second column are drawn from Kashyap and Stein's (1995) estimates for the "small95" category as follows:
[a] Table 4, Panel 1;
[b] Table 4, Panel 2;
[c] Table 3, Panel 1;
[d] Table 3, Panel 2.

Once we have the total effect due to liquidity constraints among small banks, it can be compared with aggregate movements in small-bank lending. Here, we draw on Kashyap and Stein (1995), who, using the same sample period and methodology, find that in a bivariate specification, the *aggregate* C&I lending of all small banks is reduced by 3.33 percent a year after a 100-basis-point funds-rate shock.[36] Thus based on our conservative estimates of the $\phi$'s, one might argue that liquidity constraints "explain"

[35] This comes from multiplying the total change in $\beta$ that is traced out over the year by the liquidity differential (0.0151 × (0.602 − 0.206) = 0.006). To be more precise, one should account for the dynamic effects that arise from serial correlation in loan growth. However, there is very little persistence in either C&I or total loan growth, so these effects are trivial.

[36] See Table 4, Panel 1, of Kashyap and Stein (1995), the line labeled "small95". The advantage of using these estimates (rather than the one-step results reported here) is that the unit of observation is aggregate small-bank lending—i.e., the numbers are value-weighted.

12 percent of the total decline in small-bank C&I lending subsequent to a monetary shock. Using the more aggressive estimates, this ratio is increased to 109 percent. Table 7 presents a more complete set of numbers, covering both C&I and total loans, and drawing on the parameter estimates from both our univariate and bivariate specifications. Although it is hard to be precise, this crude analysis would seem to imply economically noteworthy magnitudes.

From a macro perspective, we are arguably not quite finished with this exercise, because small-bank lending, as we have defined it, is only a fraction (about one-quarter) of total lending. In other words, the next question one would like to answer is: "what portion of the total economywide drop in lending is due to liquidity constraints?" Unfortunately, here our evidence is of little direct use; we have been interpreting the surprisingly large positive $\phi$'s for the big banks as indicative of a bias, which leaves us with no scope to measure the extent of their liquidity constraints. Rather than basing a further set of calculations on totally arbitrary assumptions about big-bank constraints, we simply make the following observation. If one wants a very loose lower bound, one can assume that *all* medium and big banks are completely unconstrained. In this case, the relative importance of liquidity constraints for total bank lending would be roughly one-quarter of what it is for small-bank lending.[37]

## VI. Conclusions

Previous work has uncovered a variety of evidence that is consistent with the existence of a lending channel of monetary transmission. Unfortunately, much of this evidence also admits other interpretations. Our premise in this paper has been that to provide a sharper test of the lending channel, one has to examine in more detail how monetary policy impacts the lending behavior of *individual* banks, as opposed to broadly aggregated measures of lending.

Our principal conclusions can be simply stated. Within the class of small banks, changes in monetary policy matter more for the lending of those banks with the least liquid balance sheets. The results are for the most part strongly statistically significant, and are robust to a wide range of variations in estimation technique. Moreover, the implied differences across banks are of a magnitude that, at a minimum, one would call economically interesting.

Unlike with the earlier evidence, it is much harder to come up with alternative, nonlending-channel stories to rationalize our results. In particular, if one wants to explain our results using a standard interest-rate channel, one has to argue that those banks whose customers' loan demand is most sensitive to monetary policy systematically opt to hold less in the way of liquid assets—i.e., one has to invoke the heterogeneous risk-aversion story. Not only is this story somewhat implausible from a theoretical perspective, we have been able to marshall several distinct pieces of evidence which all imply that it is not borne out in the data.

The bottom line is that it now seems hard to deny the *existence* of a lending channel of monetary transmission, at least for the United States in our sample period. The next logical question then becomes: quantitatively, *how important* is the lending channel for aggregate economic activity? As we have begun to see in Section V above, this question is harder to answer definitively with our data set. First, while our results leave open the possibility that the aggregate loan-supply consequences of monetary policy could be very substantial, our attempts to precisely measure this aggregate effect are hampered by the large estimation biases in the big-bank regressions.

Second, even if one can make a stronger case that monetary shocks have a large impact on total bank-lending volume, there is a further missing piece to the puzzle. In particular, one still needs to know the elasticity with which borrowers can substitute between bank and nonbank forms of credit on short notice. For example, if a small company is cut off from bank lending, how much higher is the implicit cost of capital if it has to instead stretch its accounts payable? And what are the implications for its inventory and investment behavior? These are questions that will not be easy to answer satisfactorily. Nonetheless, if the goal is to achieve a full and accurate picture of the role of banks in

[37] This is also overly conservative for another reason: Kashyap and Stein (1995) show that small-bank lending falls by substantially more than large-bank lending after a funds-rate shock.

the transmission of monetary policy, they will eventually have to be addressed.

## Data Appendix

Our sample is drawn from the set of all insured commercial banks whose regulatory filings show that they have positive assets. Between the first quarter of 1976 and the second quarter of 1993, this yields 961,530 bank-quarters of data. The actual number of observations in our regressions is less, for several reasons. First, because our regressions involve growth rates, we lose an initial observation for each bank. Second, because mergers typically create discontinuities in the surviving bank's balance sheet, we also omit banks in any quarters in which they are involved in a merger. These first two cuts leave us with a sample of 930,788 observations which could potentially be analyzed. Next, in order to make sure that outliers are not driving our results, we eliminate any observations in which the dependent variable is more than five standard deviations from its mean. In the regressions involving C&I loans we further eliminate any banks for which C&I lending constitutes less than 5 percent of their total lending. Together these filters remove about another 67,000 bank-quarters. Finally, we require that all the banks in our sample have four consecutive quarters of loan growth. The cumulative effect of all these screens is that our basic C&I regressions use 746,179 observations. For the total loan regressions we follow the same procedures except that we skip the check on the ratio of C&I loans to total loans, so that our total sample size is 836,885.

Our main results depend on accurately measuring a bank's size and its lending and securities holdings. Our size categories are formed by sorting the banks on the basis of their total assets—call report item rcfd2170. Although the total asset data are measured on a consistent basis throughout our sample, much more detail concerning bank assets and liabilities has been collected starting in March 1984, so that most of the other asset data is measured differently before and after that point.

For our securities variable after March 1984 we begin with the sum of the book value of total investment securities (item rcfd0390) and assets held in trading accounts (rcfd 2146). Prior to 1984 it is not possible to separately add up all of the items that are now counted as investment securities. As an approximation we take the sum of items rcfd0400 (U.S. Treasury Securities), rcfd0600 (U.S. Government Agency and Corporate Obligations), rcfd0900 (Obligations of States and Political Subdivisions), and rcfd0380 (All Other, Bonds, Stocks and Securities). In either case, we then add on Fed Funds Sold and Securities Purchased Under Agreements to Resell (rcfd 1350) to get an overall series for securities holdings.

The data for total loans after March 1984 come from item rcfd1400, Gross Total Loans and Leases. Prior to March 1984 "Lease Financing Receivables" (rcfd 2165) are not included as part of total loans so the two series need to summed to insure comparability. More importantly, in December of 1978 banks began reporting their lending on a consolidated basis so that foreign and domestic loans were no longer separately identified. Prior to that period the foreign data were unavailable. Since most banks had only limited foreign operations at that time, this shift is relatively unimportant for the typical bank. However, for many of the biggest banks the change generates a noticeable discontinuity in reported lending. One of the advantages of our two-step regression approach is that it helps limit the influence of this one-time jump in lending—the jump is absorbed in the constant term of the first-step regression. Nevertheless, to confirm that the shift was not responsible for any of our key findings, we also reestimated our main regression omitting this period and found no important changes.

The data for commercial and industrial loans are taken from rcfd1600. Starting in March 1984 the series begins to include holdings of those bankers' acceptances which are accepted by other banks. Prior to that time only each bank's own acceptances are included, but there is no way to create a series which is consistent in the treatment of acceptances because a bank's own acceptances are never separately reported. As in the total loan data, the reported level of C&I lending for large banks also shows a jump in the fourth quarter of 1978.

The snapshots of the data given in Table 1 involve a number of other items from the call reports. The details concerning these variables are given in the appendix to Kashyap and Stein

(1995) and are available at: www.frbchi.org/rcri/rcri_database.html. The only noteworthy aspect of these items is that data on deposits was reported differently before and after March of 1984. These different reporting conventions explain why we break out deposits into slightly different subcategories in 1976 and 1993.

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