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WHAT DO A MILLION BANKS HAVE TO SAY ABOUT THE TRANSMISSION OF MONETARY POLICY?

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What Do a Million Banks Have to Say About
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ABSTRACT

In an effort to shed new light on the monetary transmission mechanism, we create a panel data set that includes quarterly observations of every insured commercial bank in the United States over the period 1976-1993. Our key cross-sectional finding is that the impact of monetary policy on lending behavior is significantly more pronounced for banks with less liquid balance sheets -- i.e., banks with lower ratios of cash and securities to assets. Moreover, this result is entirely attributable to the smaller banks in our sample, those in the bottom 95% of the size distribution. Among other things, our findings provide strong support for the existence of a "bank lending channel" of monetary policy transmission.

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Introduction

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 U.S.--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 (Bernanke and 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 test to see if the impact of monetary policy on lending behavior is stronger for banks with less liquid balance sheets, where liquidity is proxied for by the ratio of cash plus securities plus fed funds sold to assets. It turns out that the answer to this question is a resounding "yes". Moreover, the result is entirely driven by the smaller banks in our sample, those in the bottom 95% of the size distribution. As we discuss in detail below, this sort of cross-sectional pattern is exactly what one would expect based on "bank-centric" theories of the monetary mechanism, which are ultimately rooted in the premise that banks face frictions in raising non-deposit forms of external finance.

One aspect of such bank-centric theories is the so-called "lending view" of monetary transmission. At the heart of the lending view is the idea that the Federal Reserve can, simply by conducting open-market operations, shift banks' loan supply schedules.[1] 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 and bonds. Thus, if there is an operative lending channel, the Fed can move not only Treasury rates, but also the effective spread between bank loans and Treasuries.

This observation in turn has a number of implications for the conduct and consequences of monetary policy. To take just a couple of examples, the existence of a lending channel implies that a change in monetary policy may: 1) have a significant impact on investment and aggregate output even if it does not move long-term Treasury bond rates by much; and 2) have distributional consequences--in terms of its relative effects on bank-dependent and non-bank-dependent firms--that do not arise in models that use a single interest rate to summarize monetary policy.[2]

The remainder of the paper is organized as follows. We begin in Section I by using the bank lending view of monetary transmission to motivate the precise form of our tests. Section II describes our data sources. Section III lays out our baseline econometric specification, and discusses the potential biases and other pitfalls that we will have to deal with. Section IV

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[1]In addition, as emphasized by Bernanke and Blinder (1988) and many subsequent researchers, the lending channel also requires: 1) that some borrowers be unable to find perfect substitutes for bank loans; and 2) some form of imperfect price adjustment. However, these conditions have been viewed as less controversial.

[2]See Kashyap and Stein (1994) for more on why the debate over the lending channel is of practical and policy relevance.

presents our principal results. Section V summarizes auxiliary results from a wide range of alternative specifications and other robustness checks. Section VI concludes.

I. Identifying the Role of Bank Lending in the Transmission of Monetary Policy

The proposition that banks are central to the monetary transmission mechanism is both important and controversial. One benchmark--embodied in not only the textbook IS/LM analysis, but also in many theoretical models that generate monetary non-neutrality either through cash-in-advance constraints or by assuming that money enters agents' utility functions--is that banks are inessential. Indeed, one can think of monetary policy in these sorts of models as being implemented via helicopter drops of currency, with banks playing no role whatsoever.

Implicit in such banks-don't-matter formulations is the assumption that a version of the Modigliani-Miller (M-M) theorem holds for banks. In an M-M world, open-market operations have no meaningful effect on banks' activities. When the Fed drains reserves from the system, it may be able to compromise banks' ability to raise reservable forms of finance, such as insured transaction deposits. But Federal Reserve policy cannot constrain banks' use of non-reservable liabilities, such as large-denomination CD's. If M-M applies, banks are indifferent at the margin between financing themselves with transactions deposits and large CD's, so shocks to the former cannot have any impact on their "real" behavior, i.e., their lending decisions. All that monetary policy can do in this context is to alter the liability side of banks' balance sheets--i.e., change the relative proportions of deposits (aka "money") and CD's (aka "bonds") that they issue. In other words, banks are a passive conduit through which the Fed happens to implement a swap of bonds for money, and nothing else.[3]

The flip side of this reasoning is that if banks are to play a meaningful role in the transmission of monetary policy, there must be a significant departure from M-M for banking firms. In reality, many of the major classes of bank liabilities which escape reserve requirements are not covered by deposit insurance. Thus if a bank attempts to react to a contraction in reserves by substituting large CD's for transaction deposits, it has to deal with the fact that the large CD's are uninsured, and therefore expose investors to credit risk. If there are adverse-selection problems in the CD market, banks will typically be unwilling to use uninsured CD's fully offset a Fed-induced loss in insured deposits, so that there is indeed a net effect on lending behavior. The key point is that if there is a link between the reservability and insurability of bank liabilities, it is plausible that the M-M theorem will break down, and that open-market operations can have real consequences for bank lending behavior.

Stein (1995) develops an adverse-selection-based model of the banking sector which formalizes this intuition. Among other things, the model has the following three features. First, there is a bank lending channel of monetary transmission; open-market operations affect bank loan supply and hence the spread between bank loans and Treasuries. Second, because of financing frictions, banks have a well-defined buffer-stock demand for liquid assets. Those banks for whom adverse-selection problems are most pronounced and who therefore are apt to have the most trouble raising uninsured finance (i.e., small banks) hold on average the highest ratios of liquid securities to illiquid loans. Finally, in general equilibrium, imperfections at the

[3]For a recent articulation of this M-M based view of the monetary mechanism, see Romer and Romer (1990).

bank level can be a crucial factor in shaping not just loan-Treasury spreads, but also the response of Treasury rates to monetary shocks.[4]

This sort of theoretical exercise suggests that is certainly logically possible for banks to play a direct role in the transmission of monetary policy. At the same time, the M-M benchmark might lead one to be skeptical of the quantitative importance of banks, particularly in the current, deregulated environment where they would seem to have a wide range of non-reservable liability instruments at their disposal. Thus ultimately, we are left with a purely empirical set of questions.

With regard to the specific question over the existence of a lending channel, the good news is that a lot of relevant evidence has been produced in the last several years. Most of this evidence has come either from aggregate data, or from disaggregated data on non-financial firms--very little has been done with disaggregated data on banking firms.[5] The bad news is that, arguably, these 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 yet been viewed as airtight.

Although we will not provide an exhaustive survey of this literature here, a quick review helps to give an idea of the identification problems that arise.[6] An important starting point is the work of Bernanke and Blinder (1992). Among other things, they find that a contraction in

[4]This last effect is also ruled out in standard monetary models, where Treasury rates are determined solely by households' preferences across money and bonds.

[5]A partial exception is Kashyap and Stein (1995), which we discuss below.

[6]For detailed surveys, see Cecchetti (1994), Kashyap and Stein (1994), Hubbard (1994), and Bernanke and Gertler (1995).

monetary policy is followed by a decline in the volume of aggregate bank lending. This is consistent with the notion of a lending channel, but it also admits another interpretation: economic activity is being depressed via standard interest-rate effects, and as a consequence, it is a decline in loan demand, rather than loan supply that drives the results.

In an effort to resolve this ambiguity, Kashyap, Stein and Wilcox (1993) show that at the same time that a monetary contraction is reducing bank lending, it is increasing commercial paper volume. This makes it more likely that what has taken place is an inward shift in loan supply, as suggested by the lending view, rather than just an inward shift in loan demand. However, others have argued that this approach to identification is not decisive either. For example, it may be that during recessions, small firms are hurt badly, and hence have a sharply reduced demand for credit, while large firms fare better, and actually have an increased short-run demand for credit. Given that the majority of commercial paper volume comes from the largest firms, this could explain the Kashyap, Stein and Wilcox (1993) results.[7] So again, the effort to identify a loan supply effect bumps into an alternative hypothesis about the nature of loan demand, albeit in this case one that relies on compositional effects.

In light of the ambiguities inherent in working with the aggregate data, a natural next step is to use micro data to test some of the cross-sectional implications of the lending view. One prediction is that tight monetary policy should pose a particular problem for smaller (non-financial) firms, who are more likely to be bank-dependent. And indeed, several papers find

[7]This argument is made by Oliner and Rudebusch (1996). Kashyap, Stein and Wilcox (1996) rebut by noting that even within the class of the largest firms, commercial paper rises relative to bank lending after a monetary contraction. Hence the aggregate results for commercial paper vs. bank loans are not simply an artifact of compositional effects.

that contractions in policy intensify liquidity constraints in the inventory and investment decisions of small firms.[8] But again, while this is consistent with the existence of a lending channel, 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.

The bottom line is that cleanly identifying a bank lending channel is a tough problem. Our premise in this paper is that, to make real progress on this problem, one has to go down to the roots of the lending channel--i.e, to examine the imperfections at the individual bank level that ostensibly shape it. More precisely, one has to exploit the implications that models of the lending channel have for cross-sectional differences in the response of individual banks to monetary policy. As was discussed above, the lending view ultimately boils down to the proposition that banks cannot frictionlessly tap uninsured sources of external funds to make up for a Fed-induced shortfall in insured deposits. But if this is true, it must be that the effect of monetary policy on lending is more pronounced for some banks than for others.

Consider two small banks, both of whom face substantial limitations in raising uninsured external finance. The banks are alike in all respects, except that one has a more liquid balance sheet position than the other--specifically, one has a ratio of cash and securities to total assets of 65%, while the other has a ratio of 25%. 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

[8]See, e.g., Gertler and Hubbard (1988), Gertler and Gilchrist (1994), Kashyap, Lamont and Stein (1994), and Carpenter, Fazzarri and Petersen (1994).

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 cash and securities. In contrast, the less liquid bank is likely to have to cut loans significantly, if it does not want to see its ratio of cash and securities to assets sink to a level that is dangerously low.

This line of reasoning leads to our first principal hypothesis: for banks without perfect access to uninsured sources of finance, $d^2L_{it}/dB_{it}dM_t < 0$, where L_{it} is a bank-level measure of lending activity, B_{it} is a bank-level 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 involves both the cross-sectional and time-series dimensions of the data, and can be thought of in two ways, depending on the order in which one takes the derivatives. If one looks first at the cross-sectional derivative dL_{it}/dB_{it}--which captures the degree to which lending behavior is liquidity constrained at any point in time t--the hypothesis is that these liquidity constraints are intensified during periods of tight monetary policy. Alternatively, looking first at the time-series derivative dL_{it}/dM_t--the sensitivity of lending volume to monetary policy for a given bank i--the hypothesis is that this sensitivity is greater for banks with weaker balance sheets.

In testing this first hypothesis, we will focus on the smaller banks in our sample, with the idea being that these banks are least likely to be able to frictionlessly raise uninsured finance. This leads us to our second basic hypothesis, which is that $d^3L_{it}/dB_{it}dM_tdSIZE_{it} > 0$. Simply put, the effect that we are interested in should be most prominent for smaller banks. One would expect a priori that the largest banks would have a much easier time accessing a variety of markets for uninsured finance, which would make their lending less dependent on monetary policy shocks, irrespective of their internal liquidity positions.[9]

Before proceeding, it is useful to contrast the two hypotheses we have formulated here with a couple of closely related ones that can also be tested with bank-level data.[10] In Kashyap and Stein (1995), we test the proposition that $d^2L_{it}/dM_tdSIZE_{it} < 0$. In words, the lending behavior of large banks should be less sensitive to monetary shocks than the lending behavior of small banks. This hypothesis is motivated by logic similar to that above, and the evidence is strongly in its favor. Unfortunately, it is more subject to alternative interpretations than those we test below. For example, it may be that large banks lend to large customers, whose loan demand is less cyclical. The tests we conduct below effectively control for this concern, by holding bank size fixed and focusing on differences in balance sheet strength within a given size category.

Another hypothesis that has been suggested to us is that $d^2L_{it}/dB_{it}dSIZE_{it} < 0$--on average over time, small banks should face tighter liquidity constraints than large banks. Two things should be pointed out about this hypothesis. First, it says nothing particular about the role of monetary policy. Second, and more fundamentally, it is likely to be severely confounded in the data by the following endogeneity problem: if small banks really have a harder time raising external finance, we would expect them to hold larger buffer stocks of liquid assets. Indeed,

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[9]Both of these hypotheses can be formally derived in the context of the adverse-selection-based model of Stein (1995), if one thinks of size as proxying for the magnitude of the information asymmetry between bank management and outside investors.

[10]An interesting recent paper by Gibson (1996) uses aggregate bank data to test a hypothesis that is also similar in some ways to ours. Gibson finds that the effect of monetary policy on the economy is more pronounced when banks in the aggregate have a lower ratio of cash and securities to total assets. Thus Gibson is trying to exploit the time-series variation in bank balance sheets, while we are trying to exploit the cross-sectional variation.

as we will soon demonstrate, this tendency emerges very strongly in the data. Thus in equilibrium, little or no difference in the degree of effective liquidity constraint may be observed on average. Of course in extreme circumstances--e.g., when monetary policy is very tight--the buffer stock of the typical small bank will not be sufficient to completely insulate it, and it will become more liquidity constrained than the typical large bank. But one shouldn't necessarily expect small banks to be more liquidity constrained all the time.

II. 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 all insured banks must submit to the Federal Reserve each quarter. With the generous help of the Federal Reserve Bank of Chicago, we were able to compile a very large data set, with quarterly income statement and balance sheet data for all reporting banks over the period 1976Q1-1993Q2, representing a total of 961,530 bank-quarters. This data set presents us with a number of challenges, particularly in terms of creating consistent time series. This is because the definitions sometimes change for several of the variables of interest. The data appendix describes the construction of our key series in detail, and notes the various splices made in an effort to ensure consistency. 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 gives some basic information on what balance sheets look like for banks of different sizes. The table has two panels, corresponding to the starting and ending points of our sample period. In each panel, we report balance sheet data for six different 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 regularities emerge. On the asset side, larger banks hold significantly less in the way of cash and securities, and make more loans.[11] As noted above, this is precisely what one would expect in a world of financing frictions: smaller banks need bigger buffer stocks of cash and securities, because of their inability to raise external finance easily on short notice. 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 significantly less use of both deposits and equity, with the difference made up by a number of other forms of borrowing. For example, the largest two percent of banks make heavy use of the fed funds market to finance themselves; the smallest banks do virtually no borrowing in the funds market. Given that fed funds are a form of unsecured borrowing, this again fits with a financing-frictions story: small banks seem to find it harder to use instruments where credit risk is an issue.

The numbers in Table 1, as well as the baseline regression results that we report below, reflect balance sheet data at the individual bank level. An alternative approach would be to lump together the balance sheets of all banks that belong to a single bank holding company. This

[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.

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. Empirically, it is hard to gauge the importance of such reshuffling for our purposes. On the one hand, Houston, James and Marcus (1996) present evidence that shocks to one bank in a holding company are in fact partially transmitted to others in the same holding company. On the other hand, the vast majority of all banks are stand-alones, and even large holding companies are typically dominated by a single bank.[12] In the end, it is not obvious to us which is the conceptually more appropriate method, so as a precaution, we also reproduce all our results using the holding-company approach. As it turns out, nothing changes.

In terms of the specific variables required for our regressions, we make the following choices. First, for the lending volume variable L_{it}, we look at both total loans, as well as at the most commonly studied subcategory of loans, C&I loans. One motivation for examining both is the concern that any results for total loans might somehow be influenced by changing compositional effects over the business cycle. For example, it is conceivable that C&I loan demand and real estate loan demand move differently over the 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 $d^2L_{it}/dB_{it}dM_t$.[13]. A countervailing drawback of focusing on just C&I loans is that some banks

[12]Berger, Kashyap and Scalise (1995) report that in 1994, the top six holding companies accounted for about 18.8% of all banking assets, but within each organization, the largest bank had an average of 70% of assets.

[13]This is just a specific version of the more general proposition that B_{it} might be endogenously linked to the cyclical sensitivity of loan demand. We discuss this issue in detail in Section III.B.2 below.

do only a negligible amount of C&I business--i.e., have a ratio of C&I loans to total loans that is very small.[14] Thus in the regressions that use C&I lending, we throw out any banks for which this ratio is less than 5%. This screen leads us to drop approximately 7% of our sample.[15]

For the balance sheet variable B_{it}, we use the ratio of cash plus securities plus fed funds sold to total assets.[16] The intuition here is as described in Section I: 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 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 almost 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

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[14]This problem is even more pronounced when we look at other subcategories of loans, e.g., agricultural loans.

[15]Even after applying this filter, there are some very extreme values of loan growth in our sample, for both the C&I and total loan categories. To ensure that our results are not dominated by these outliers--which we worry could be data errors--we further eliminate any observations for which loan growth is more than 5 standard deviations from its period mean. However, it should be noted that our basic results do not appear to be sensitive to whether or not either of these screens is applied.

[16]We also experiment with some variations on this definition of B_{it}. See Section V.

\$400 million in 1993.)[17] 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[18].

B. Measures of Monetary Policy

A critical ingredient for our regressions is a good indicator of the stance of monetary policy M_t. Unfortunately, there is no consensus as to the single best way to measure monetary policy--indeed, a whole host of different indicators have been proposed in the recent literature on this topic.[19] Therefore, rather than take a stand on a single measure, we use three different ones throughout. While these three do not represent an exhaustive list of those that have been proposed, it is fair to say that they span the various broad types of methodologies that have been employed.

Our first measure, which we take to be representative of the "narrative approach" to measuring monetary policy, is the Boschen-Mills (1991) index. Based on their reading of FOMC documents, Boschen and Mills each month rate Fed policy as being in one of five

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[17]To get a feel for how small a \$400 million bank is, note that regulations restrict banks from tying up more than 15% of their equity in a single loan. This means that a bank with \$400 million in assets and a 6% equity ratio cannot make a loan of more than \$3.6 million to a single borrower.

[18]We experimented with further chopping up 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.

[19]See Bernanke and Mihov (1995) for a recent discussion of the literature on measuring monetary policy and for further references.

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 vs. unemployment.[20] Following Boschen-Mills, 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 Laurent (1988), Bernanke and Blinder (1992), and Goodfriend (1993) as a simple indicator of monetary policy, and has subsequently been used by a number of authors. However, it should be recognized at the outset 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 and more formal statistical analysis suggest that the funds rate may be on particularly shaky ground during the Volcker period, which roughly corresponds to the first half of our sample. As Bernanke and Mihov (1995) put it: "our results suggest that the federal funds rate, which was the best indicator of policy prior to 1979, has become so again during the tenure of Chairman Greenspan. However, the funds rate was not necessarily a good indicator of policy during the early to mid-1980's..." In light of this concern, we check below to see how our results using the funds rate hold up across sub-samples; if Bernake and Mihov are right, one might expect these results to be more clear-cut in the latter part of our sample.

[20] The other well-known indicator in this narrative vein is the so-called "Romer date" variable (Romer and Romer 1989.) Unfortunately, the Romer dates are not well-suited for our purposes. There are only three in our sample, and two of these--the August 1978 and October 1979 ones--are sufficiently close together that they probably cannot be considered completely independent observations. Moreover, the zero-one nature of the Romer variable means that there is no incremental information in the series beyond the identification of these three dates. The Boschen-Mills index, which embodies a finer measure of the stance of policy, is more appropriate for the relatively high-frequency sort of experiment we are conducting.

Our third and final measure, which we take to represent the current state of the art of the "structural VAR approach", follows Bernanke and Mihov (1995). Their work is motivated by the observation above, namely that Fed operating procedures have varied over time, and that as a result, any one simple indicator is likely to be problematic in some periods. They develop a model that is sufficiently flexible that it nests previous structural VAR's based on more specific assumptions about Fed operating procedures--i.e., their model accommodates as special cases either funds-rate targeting (Bernanke and Blinder 1992) or procedures based on non-borrowed reserves (Christiano and Eichenbaum 1992, Strongin 1992.) 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.[21] The appendix describes exactly how we have implemented the Bernanke-Mihov model for our sample period.[22]

Figure 1 plots our three measures in levels. (Throughout the paper, we invert the funds rate for comparability with the other two measures.) As can be seen, while they are not perfectly correlated, they all seem to contain broadly similar information. For example, all three indicate that monetary policy was extremely contractionary following the Fed's change in operating procedures in October 1979; all three suggest a relatively loose stance of policy in the period 1985-86; and all three capture the common wisdom that policy was tightened again in

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

[22]The main choice to be made involves how many sample breaks to allow for in estimating the Bernanke-Mihov VAR. The results we report below use one, in September 1979. However, we have experimented with several variants on this approach, and the results are essentially unchanged.

1988, before being eased once more beginning in late 1989.

Another noteworthy observation from Figure 1--which we found a bit surprising--is that during the Volcker period, the Bernanke-Mihov measure inherets much of the the extraordinary high-frequency volatility that characterizes the funds rate. This need not have been true as a matter of principle; since it puts some weight on movements in non-borrowed reserves, one might have expected the Bernanke-Mihov measure to have decoupled somewhat from the funds rate during this period. But as it turns out, there does not appear to be much decoupling. Thus to the extent that one has concerns about the adequacy of the funds rate as an indicator during the Volcker period, these concerns should carry over in part to the Bernanke-Mihov measure.

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 .61 and .95--over the full sample. By far the strongest of these correlations is that between the funds rate and the Bernanke-Mihov measure. This particular correlation also remains virtually intact when we look at annual and quarterly changes. In sharp contrast, however, the correlation of the Boschen-Mills index with the other two measures is much reduced when we look at higher-frequency changes. A glance at Figure 1 suggests that this is due to the discrete nature of the Boschen-Mills index, which at high frequencies effectively introduces a substantial degree of measurement error into this indicator of monetary policy. This measurement error should be borne in mind when we conduct our tests below, as it may make it harder to establish significant relationships between high-frequency changes in the Boschen-Mills index and other more continuous variables.

The table also looks at sub-sample correlations. The one point worth noting here is that the correlation of the Boschen-Mills index with the other two measures appears to be substantially lower in the first part of the sample, which we date as running from 1976Q1 to 1985Q4. For example, the correlation of annual changes in the Boschen-Mills index and the funds rate is .32 in the first part of the sample, and .65 in the second part. Similarly, the correlation of annual changes in the Boschen-Mills index and the Bernanke-Mihov measure is .34 in the first part of the sample, and .46 in the second part. This fits with the idea stressed above, namely that both the funds rate and the Bernanke-Mihov measure behave in a fundamentally different way in the early part of our sample, and are probably less accurate indicators of the stance of monetary policy during this period.

Overall, then, the conclusion from this brief analysis is that no single one of our measures of monetary policy is likely to be perfect. On the one hand, the funds rate--and to some extent the Bernanke-Mihov measure--can be criticized as being less appropriate during the early to mid-1980's. On the other hand, with the Boschen-Mills index, there are discreteness problems which are present throughout the entire sample period. In spite of these caveats, however, we are confident that each of the three measures captures at least some useful information about the stance of monetary policy. It now remains to see whether these measures also deliver the cross-sectional patterns in lending that we have hypothesized.

III. Econometric Specification

A. The Two-Step Regression Approach

Again, our basic goal is to measure the quantity $d^2L_{it}/dB_{it}dM_t$, for banks in different size classes. In doing so, one important choice is how tightly to parametrize our empirical model.

The tradeoffs involve not only how we characterize the link between lending and monetary policy, but also how we choose to allow a bank's balance sheet strength to affect this linkage.

We opt for a relatively flexible specification, which we implement with a two-step regression procedure. In the first step, we run the following cross-sectional regression separately for each size class and each time period t: We regress the log change in L_{it} against: i) four lags of itself; ii) B_{it-1}; and iii) a Federal-Reserve-district dummy variable (i.e., a geographic control).[23] We denote the estimated coefficient on B_{it-1} by β_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 β_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 ii) as a linear time trend.[24] In the second, "bivariate" specification, we also add the contemporaneous value and four lags of real GDP growth to the right-hand side.[25] In either case, our hypothesis implies that, for the smallest class of banks, an expansionary impulse to M_t

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[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 to our results.

[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 β_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.

should lead to a reduction in β_t.

In principle, we could also conduct our tests with a more tightly parametrized one-step, interactive regression specification, rather than with the two-step method outlined above. For example, for any size class, we could (roughly speaking) regress the log 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 key hypothesis test would center on the interaction coefficients. However, an advantage of our approach is that it imposes no a priori structure on the time-series properties of lending volume--the two-step method implicitly allows there to be a different macro shock in each time period for each Federal Reserve Bank district. In contrast, the one-step method forces all macro effects to work linearly through the monetary measure (or in an expanded version, through the monetary measure and real GDP growth.) Since we are ultimately interested in cross-sectional differences, it does not make sense to impose such time-series structure if--as is the case here--the data tell us it is not warranted.[26]

Panel A of Table 3 displays an example of the first-step regression, that for the total loans of the smallest category of banks in the 4th quarter of 1987. As can be seen, the lagged lending terms are--not surprisingly--extremely significant, and putting them in the regression generates a moderately high R^2--the value of 16.9% in this particular regression is typical of that obtained on average. Figure 2A plots the time series of the β_t's, (along with 2-standard-error confidence bands) again for the smallest banks' total loans.

[26]Of course, if the restrictions embodied in the one-step approach were not rejected by the data, there would be an efficiency loss in not imposing them. In this case, our two-step methodology would be too conservative, and might fail to reject the null in circumstances where a tighter specification would.

Panel B of Table 3 and Figure 2B present the corresponding results for C&I loans. Interestingly, the first-step regression tends to produce a much lower R^2 in this case, and β_t is estimated less precisely. Also, the sum of the coefficients on the lagged lending terms is now negative, rather than positive, which is at first glance a bit puzzling. Overall, these observations raise the concern that the simple linear lag structure in our first-step regression may be doing a less-than-adequate job of controlling for bank-specific loan demand shocks in the C&I case. In light of this concern, we experimented with a range of richer specifications of the first-step regression for C&I loans.[27] However, in none of these were our estimates of β_t--and hence our key conclusions--affected in any noticeable way. A final noteworthy observation from Figure 2B is that β_t is often negative for C&I loans, sometimes significantly so. We will return to this point momentarily.

B. <u>Potential Biases and Other Pitfalls</u>

Before turning to the results, we highlight a number of issues that could pose problems for our specification, and discuss how these problems might be dealt with. The single biggest source of concern for our entire approach 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 our purposes than others.

[27]For example, we considered longer lag lengths, allowed the lagged lending terms to enter in a variety of non-linear ways, etc.

1. Biases in the level of β_t

First and most obviously, the first-step regression delivers estimates of the level of β_t that are potentially biased. In principle, this bias could be either positive or negative, but in a banking context, a natural candidate story goes as follows. Because of demographic or other factors, some banks tend to have a strong comparative advantage at deposit-taking, but relatively few attractive lending opportunities. Rather than make bad loans, these banks tend to have portfolios that are heavily tilted towards securities. If the weak lending opportunities are only imperfectly controlled for by past loan growth, there may be a residual tendency for high values of B_{it-1} to be associated with slow growth of L_{it}--in other words, β_t will be biased downward.

We have already seen some evidence of such a bias in Figure 2B--the fact that β_t is frequently estimated to be negative for C&I loans, which is very hard to reconcile with just liquidity constraints in the absence of any econometric bias.[28] But the important point for our purposes is that biases in the level of β_t are in and of themselves not an issue, since our key hypothesis has to do with the correlation of β_t with M_t. Indeed, if the only source of variation in B_{it} had to do with the specific endogenous link sketched above--that some banks have fewer good lending opportunities and hence hold more in securities--then there would be no reason to expect a spurious correlation between β_t and M_t, and our tests would be on very solid ground.

2. Biases in the correlation of β_t and M_t

Unfortunately, there may also be other endogenous influences on B_{it} that are more

[28]Interestingly, negative β_t's are rarely observed in the case of total loans. As the discussion above suggests, this is likely because in our first-step regressions with total loans, the lagged loan growth terms do a much better job of controlling for variations in lending opportunities.

directly problematic for our approach, in that they lead to a bias in the estimated coefficients on M_t in the second-step regression. Generally speaking, this bias arises when there is an endogenous relationship between B_{it} and the cyclical sensitivity of the demand for the loans made by a given bank.

Depending on the direction of this relationship, the bias can go either way. First, consider what might be called the "heterogeneous risk aversion" story. According to this story, certain banks are inherently more conservative than others. Conservative banks will 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 β_t is too negative. In other words, we may be biased towards being too aggressive, rejecting the null hypothesis even when it is true. To see why, note that when monetary policy tightens, this will have the most negative effect on the loan demand of the riskiest borrowers. Since these risky borrowers tend to be customers of the less conservative banks with low values of B_{it}, we have a situation where tight policy has a more pronounced impact on the lending of banks with weak balance sheets. This effect goes in the same direction as the hypothesis we are trying to test.

Alternatively, consider the "rational buffer-stocking" story. According to this story, all banks have the same risk aversion, but some naturally tend to have more opportunities to lend to cyclically-sensitive customers than others. In this case, those banks with more cyclically-sensitive customers will rationally 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 very easily told within the context of a fully rational model.[29] Nonetheless, it is obviously important for us to ascertain which of the two stories carries more weight in the data. Fortunately, there are several distinct ways for us 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 coefficients on GDP growth in this regression should be negative. In contrast, if the rational buffer-stocking story is true, the coefficients on GDP growth should be positive. Again, 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 β_t on GDP.

As a second method of figuring out the direction of the bias, one can look at the results for the largest banks. In the limiting case where there are no capital market frictions facing these banks, any non-zero coefficients on M_t in the second-step regression must purely reflect the direction of the cyclicality bias. Thus if the coefficients for the largest banks are negative, this cuts in favor of the heterogeneous risk-aversion story, while if they are positive, this favors the rational buffer-stocking story.

Finally, we can compare the results for C&I loans to those for total loans. Whatever the direction of the bias, one would expect that it would be exacerbated by lumping various loan

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

categories together and looking at total loans. This is because the bias is driven by heterogeneity in the cyclical sensitivity of loan demand, and there is likely to be more of this heterogeneity when we go across categories, as opposed to looking only within a single category. Thus if our results are stronger for C&I loans than they are for total loans, this would be consistent with the rational buffer-stocking story, and would make us more comfortable that the C&I results are if anything too conservative. Conversely, if the results are stronger for total loans, we should be concerned that they might reflect a bias arising out of the heterogeneous risk aversion story.

To preview the results a bit, it turns out that all three pieces of evidence point in the direction of the rational buffer-stocking story. Thus if anything, our tests for the small banks are probably biased in the direction of 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 right requires creating an instrument for B_{it} that is uncorrelated with loan cyclicality--a difficult task. Still, we can at least take a step in the right direction, 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.[30] We take this approach as part of our robustness testing in Section V.

[30]Note that in so doing, we do not need to take a stand on whether we think C&I loans are more or less cyclical than other types of loans. We are just hoping that there is some difference, so that the regression absorbs some of the endogenous variation in B_{it}.

3. Disentangling the direct effects of monetary policy vs. bank capital shocks

Even if we can make a convincing case that our results are not driven by biases of the sort discussed above, there remains the issue of exactly what underlying economic phenomenon they are capturing. For example, one subtle objection might be that we are not measuring the direct effects of open market operations on bank lending behavior, but rather an indirect capital-shock effect. That is, it may be that contractionary monetary policy's first-round effect is through the usual interest rate channels. Of course, once this happens, and economic activity declines, banks may find some of their existing loans going bad, which in turn reduces their capital levels. Faced with lower capital, banks may then cut back on making new loans, in order not to run afoul of capital requirements. This story is consistent with banks facing a capital market imperfection (i.e., costs of raising outside equity) but it is a somewhat different channel than the one we are hypothesizing.

Fortunately, however, it is possible to disentangle the two alternatives. Specifically, if the capital-shock effect is the dominant one, then we have a couple of simple predictions about the bivariate version of the second-step regressions. First, adding GDP growth (or any other proxy for activity) to the regressions should eliminate the importance of the monetary measure M_t. Second, the coefficients on GDP growth should themselves be negative. As will become clear soon, neither of these predictions is borne out, suggesting that our results are not driven by capital-shock effects.

We should stress however, that we are not claiming that capital levels cannot have an important independent effect on bank lending behavior. This would fly in the face of much research which has shown that capital-crunch effects were present in the late 1980s and early

1990s, as banks (already hard-hit by loan losses in many cases) struggled to meet increasingly stringent capital requirements.[31] Indeed, our best guess is that the marginally positive time trend in β_t that we uncover in some of our regressions below--whereby bank liquidity constraints appear to become systematically more pronounced in the latter part of our sample--reflects exactly such bank capital problems. It is for precisely this reason that we include a time trend in our second-step regressions, in a crude attempt to control for this apparently secular phenomenon.

IV. Baseline Results

Tables 4 and 5 present the results from our baseline second-step regression specifications. Table 4 gives a compact overview of all the different specifications, and Table 5 contains the detail from the individual regressions.

A. Overview

Table 4 shows only one number (with the associated p-value) from each regression: the sum of the coefficients on the relevant monetary policy indicator--i.e., the total effect of monetary policy on β_t. The table is divided into two panels: Panel A for C&I loans, and Panel B for total loans. In each panel, there are effectively 12 key test statistics that we care about. First, we test six ways whether the sum of the coefficients on the monetary indicator is significantly negative for the "small" banks--those in the bottom 95% of the size distribution. The six tests correspond to our univariate and bivariate specifications for each of the three

[31]See Sharpe (1995) for a survey of this literature.

monetary indicators. Second, we test in the same six ways whether the sum of the coefficients on the monetary indicator is significantly lower for the small banks than for the "big" banks--those in the top 1% of the size distribution.

As can be seen from Panel A of Table 4, the results for C&I loans are extremely strong. Consider first the results for the small banks. In all six cases, the point estimates are negative, consistent with the theoretical prediction. Moreover, in five of the six cases, the estimates are significant at the 2.4% level or better; in four of six cases, the p-values are actually below 0.6%. [32] The only case in which the estimate is not statistically significant at conventional levels is the bivariate version of the Boschen-Mills specification. (Recall from above our caveat that the discreteness-induced measurement error in this monetary indicator might be expected to weaken the results obtained with it.)

Next, turn to the small-bank/big-bank differentials. In all six cases, the point estimate for the big banks is positive, so that the small-bank/big-bank differentials are always larger in absolute value than the corresponding figures obtained for the small banks in isolation. Moreover, in all six specifications, the small-bank/big-bank differentials are statistically significant at the 8.0% level or better; indeed, in five of six cases, the p-values are below 2.2%. The bottom line is that Panel A of Table 4 provides overwhelming support for both of our key hypotheses.

The results in Panel A of Table 4 also begin to allow us to discriminate between the two

[32] In calculating the p-values, we use robust standard errors that take account of both heteroskedasticity and serial correlation. Moreover, when comparing the small and big bank estimates, the p-values also account for the correlation of the residuals across these two equations.

types of endogeneity effects--the heterogeneous risk-aversion story and the rational buffer-stocking story--that might in principle be biasing our estimates for the small banks. As discussed above, the fact that the sum of the coefficients for the big banks is always positive cuts in favor of the rational buffer-stocking story. This suggests that if anything, the magnitude of the coefficients from the small-bank regressions might be understating the true effects of monetary policy on β_t. Indeed, these effects might be better measured by the magnitude of the small-bank/big-bank differentials, which are always larger.

The results in Panel B for total loans are qualitatively similar, but quite a bit weaker. Directionally, the point estimates go the same way--all six 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-fifth that of the corresponding estimates in Panel A. Consequently, none of the six test statistics for the small banks is now significant at conventional levels, though two have p-values below 12.1%. Similarly, just two of the six test statistics for the small-bank/big-bank differentials are now significant, with p-values of 4.4% or better.

Why are the results for C&I loans so much stronger than those for total loans?[33] There are at least two possible explanations. First, for reasons outlined earlier, this is what one would predict based on the bias associated with the rational buffer-stocking story. Again, under this interpretation, our estimates are generally too conservative, and this conservatism is especially pronounced for total loans, where aggregation across loan categories of different cyclicality

[33]See however Sections V.A and V.C below for alternative specifications in which our estimated coefficients for total loans are much more statistically significant (albeit still substantially smaller in magnitude than those for C&I loans).

exacerbates the bias. There is also a second, simpler explanation that has nothing to do with endogeneity biases. It may be that because of their relatively short maturity, banks can adjust the volume of C&I loans outstanding more readily than they can adjust their lending in other major categories, such as long-term mortgages. If this is so, the effects that we are interested in will come through more clearly when we look at C&I loans.

B. Details

Table 5 presents the details of the individual regressions that make up Table 4. There are 6 panels, A through F, one for each combination of loan type and monetary indicator. Most of the patterns are very similar across panels, so it is instructive to focus first on just one--panel C, for C&I loans and the Bernanke-Mihov indicator--for which the estimates are the most precise.

A couple of salient facts emerge. First, while we reported in Table 4 only the sums of the five coefficients on the monetary indicator (lags 0 through 4), we can now look at all the individual coefficients, and see that the sums are not hiding any erratic behavior. In fact, for the small-bank category, every single one of the individual monetary-policy coefficients is negative in the univariate specification, and all but one are negative in the bivariate specification. Moreover, in both cases, the implied response of β_t to a monetary shock has an economically 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 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.[34]

Comparing across the different panels in Table 5, one can get an idea of how well the second-step regressions fit with the different monetary indicators. For example, Panel A tells us that with C&I loans and the Boschen-Mills index, the univariate second-step regression for small banks achieves an R^2 of 13.1%. When we switch to the funds rate as an indicator (Panel B), the R^2 rises markedly, to 21.6%. This is consistent with our earlier concerns about the discreteness problems associated with the Boschen-Mills index. Finally, when we go to the Bernanke-Mihov measure (Panel C), the R^2 reaches 36.4% in the univariate specification. When one considers that both the left-hand-side and right-hand-side variables in this regression are themselves noisy proxies, this fact strikes us as quite remarkable.

C. Economic Significance of the Results

So far, we have focused on the statistical significance of our results. Now we ask whether the point estimates imply economically interesting magnitudes. Given the nature of our specifications, all we can do is try to quantify cross-sectional differences in how the lending volume of banks of varying sizes and liquidity positions responds to monetary shocks. As we discuss in the concluding section, the more ambitious goal of quantifying the output effects associated with these differences in lending volume requires a number of other pieces of

[34]There is another reason why the coefficients on GDP might be positive that has nothing to do with endogeneity biases of any kind. An increase in economic activity raises loan demand, and liquid banks are more able to accommodate their customers. In other words, there is a natural tendency for increased loan demand to make banks' liquidity constraints more binding.

information which are outside the scope of the paper.

For the sake of transparency, as well as ease of comparison with previous work, it is helpful to start with the estimates that come from using the funds rate as a monetary indicator. From Table 4, Panel A, the most conservative estimate of the total movement in β_t for small banks following a 1 percentage point (100 basis point) funds rate shock is about -0.013. (This comes from the bivariate specification for small banks.) This figure allows us to compute the differential response of small banks with varying balance sheets to a monetary shock. Think of a "liquid" bank as having a ratio of cash and securities to assets of 65%, and an "illiquid" bank as having a ratio of 25%; these values 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 increase in the funds rate, the level of C&I loans of the illiquid bank will be roughly 0.5 percentage points lower than that of the liquid bank.[35] In other words, if both banks started with a level of C&I loans equal to $1000, then purely on the basis of liquidity differences, we would predict a $5 gap between the two banks a year after the funds rate shock.

The estimates in Table 4 are also consistent with a much larger cross-sectional effect. For example, if we are more aggressive, and base our calculation on the small-bank/big-bank coefficient <u>differential</u> in Panel A of Table 4, the imputed effect is multiplied roughly fourfold, so that we would predict a 2.0% gap in the level of C&I loans across the liquid and illiquid

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[35]This comes from simply multiplying the total change in β that is traced out over the year by the liquidity differential (.013x.4 = .0052). To be more precise, one should also account for the dynamic effects that arise because our first stage regressions allow loan growth to be serially correlated. However, as Table 3 shows, there is very little persistence in either C&I or total loan growth, so these dynamic effects have no meaningful effect on our calculations.

small banks one year after a 100 basis point increase in the funds rate.[36] For benchmarking purposes, it should be noted that in Kashyap and Stein (1995), we estimate that one year after a 100 basis point funds rate shock, the aggregate C&I lending of all small banks (those in the 95th percentile and below) is reduced by roughly 3%.[37] Thus depending on which of our specifications one uses, it would seem that the cross-sectional effect we document is somewhere between moderate and extremely large in comparison to the aggregate effect.

Very similar inferences about economic magnitudes come from the other monetary measures. Roughly speaking, a one-unit movement in the Boschen-Mills index--e.g., from "neutral" to "mildly contractionary"--has about the same impact as a change in the funds rate of about 150 to 200 basis points, which seems quite plausible. In this spirit, one might think of a fairly major change in the stance of policy as being equivalent to either a 2-unit movement in the Boschen-Mills index or a 300 to 400 basis point change in the funds rate. With either definition, our most conservative estimates imply that such a change in policy will, after a year, create a roughly 1.5% gap in the level of C&I loans of a liquid vs. an illiquid bank, whereas our most aggressive estimates imply an 8% gap. These are certainly non-trivial magnitudes.

V. Robustness

We have already discussed a large number of robustness checks throughout the preceding

[36]Again, the motivation for using this difference in the coefficients between small and big banks is the possibility that our coefficient estimates for the small banks in isolation may be biased downward in absolute magnitude, as a result of the rational buffer-stocking effect.

[37]See Table 4 of Kashyap and Stein (1995). In a similar vein, the impulse responses in Bernanke and Blinder (1992) suggest that aggregate bank lending is about 2% reduced four quarters after a funds rate shock of 100 basis points.

text and footnotes. Just to remind the reader of some of the more significant ones, our results are unaffected by:

--how we screen for outliers;

--whether we base our analysis on banks vs. bank holding companies;

--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.

However, there still remain a few important items which merit a more detailed treatment.

A. An Alternative Measure of Balance Sheet Strength

Throughout, we have used the ratio of cash plus securities plus fed funds sold to assets as an empirical proxy for bank liquidity B_{it}. The idea is that this ratio captures the extent to which a bank has a buffer stock of liquid assets that it can draw down on when it is having difficulty raising external finance. One quibble with our definition is that cash is for the most part made up of required reserves, which a bank is not free to draw down on. Conceptually, it may be more appropriate to use something which captures liquid assets in excess of required reserves.

In this spirit, we modify our definition of B_{it} to be simply the sum of securities plus fed funds sold to assets--i.e., we drop cash from the numerator. Table 6 gives an overview of the revised results. Its structure is identical to that of Table 4. As can be seen, the results are qualitatively similar, though noticeably stronger. In fact, for both C&I and total loans, every single one of the coefficients of interest (those for small banks as well as the small-bank/big-bank differentials) increases in absolute magnitude relative to Table 4, and the p-values are correspondingly reduced. Most strikingly, with total loans, where our previous results were not so strong, six of the 12 coefficients of interest are now significant at 2.9% or better, and a seventh is significant at 5.8%. This outcome is consistent with the idea that, by deleting cash, we are substantially improving our measure of bank liquidity.

B. A "Quasi" Instrumental Variables Procedure

As discussed above, we would ideally like to have an instrument for B_{it} that is uncorrelated with loan cyclicality. Although there are no obvious truly exogenous instruments available, we can take a step in the right direction by regressing B_{it} against any observable measures of loan cyclicality, and using the residuals from this regression as our instruments.

To implement this approach, we begin by running for each size class a "step-zero" regression of B_{it} against: the ratio of C&I loans to total loans; the ratio of real estate loans to total loans; the ratio of individual loans to total loans; and a time trend. (In doing so, we return to our baseline definition of B_{it}, which includes cash in the numerator.) The idea behind this regression is the same as that which underlies risk-based capital standards, namely that some categories of loans are on average riskier or more cyclical than others.[38] Moreover, banks involved in riskier lines of business may hold more or less in the way of securities, depending on whether the rational buffer-stocking story or the heterogeneous risk-aversion story is at work.

As an example of this step-zero regression, we obtain for small banks the following

[38]Unlike with risk-based capital standards however, we do not need to specify which loan categories are riskier.

coefficients: for the C&I loan ratio, -.223 (t-stat = 182.2); for the real estate loan ratio, -.139 (t = 149.6); and for the individual loan ratio, -.008 (t = 6.6). In words, if a small bank were to shift 10% of its loans from the individual to the C&I category, we would expect its ratio of cash and securities to assets to go down by roughly .02. The R^2 of this regression is 5.2%, so that our proxies for loan cyclicality absorb only a small fraction of the variation in B_{it}.

Next, we take the residuals from these step-zero regressions, and use them in place of B_{it} in the first-step regressions. Everything else is done exactly as before. Table 7 gives an overview of the results. Again, the structure is the same as that of Table 4, and as it turns out, the key numbers are essentially unchanged. For example, in Panel A, we see that the results with C&I loans continue to be very strong: five of the six small-bank sums are significant at the 2.9% level or better, and five of the six small-bank/big-bank differentials are significant at the 4.0% level or better.

Of course, we fully recognize that Table 7 does not by itself represent a bulletproof argument against endogeneity concerns. Still, when one combines it with the several other lines of defense offered earlier, it becomes highly unlikely that our main conclusions are driven by endogeneity biases.

C. Results from Sub-Samples

Finally, we check to see how our baseline results hold up across sub-samples. There are two motivations for doing so. First, as emphasized earlier, there are reasons to believe that the funds rate may not be as good an indicator of monetary policy in the early to mid-1980's as at other times in our sample period. Thus we would like to see whether our results using both the funds rate and the Bernanke-Mihov measure (which is by construction very similar to the funds rate) are weaker in this part of the sample. Second, we would also like to know whether 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 of the sample, we can directly address this concern.

In Table 8, we split the sample into two parts, with the first one ending in 1985Q4. We then reproduce all the numbers in Table 4 for each of the two sub-samples. The key results are quite similar for the funds rate and the Bernanke-Mihov measure, so we lump them together for the purposes of discussion. First, consider the second sub-sample, which begins in 1986Q1. Here, in spite of the reduced statistical power associated with a shortened sample, the results for both the funds rate and the Bernanke-Mihov measure are extremely strong. This is true not only for C&I loans, but for total loans as well. Considering both types of loans and both monetary measures, there are a total of 16 test statistics that we care about. In every one of the 16 cases, the sum of the coefficients is of the right sign and significant at the 6.1% level or better; in 15 of the 16 cases, the p-values are below 3.7%. Thus our results for the second sub-sample are if anything more striking than those for the full sample, especially with respect to total loans. Since this sample period is well after the phase-out of Regulation Q, we conclude that our earlier full-sample results cannot be simply explained by, e.g., interest-rate ceilings on deposits or other regulatory restrictions on bank liability management.

The results for the first sub-sample are less impressive. Of the 16 key test statistics, only four are now of the correct sign and significant at the 5% threshold, and these are all for C&I loans; there is very little evidence of statistical significance for total loans in this sub-sample.

Indeed, in several cases, the sum of the coefficients is actually of the wrong sign in this sub-sample, something we have not encountered to this point. Our best guess is that this all reflects the inadequacy of the funds rate as a monetary indicator over much of the first sub-sample. And quite clearly, the fact that our results for total loans are not very strong in the full-sample regressions of Table 4 can be largely attributed to this early part of the sample period.

When we turn to the noisier Boschen-Mills index, the splitting of the sample causes us to run into serious problems with statistical power, and it is harder to make any clean inferences. Across all the specifications in both sub-samples, only three of the 16 coefficients of interest are statistically significant. Accordingly, it is harder to discern any clear-cut differences between the earlier and later periods.

VI. Conclusions

Previous work has documented that changes in the stance of monetary policy are associated with movements in aggregate bank lending volume (Bernanke and Blinder 1992). Moreover, monetary policy seems to have a markedly stronger effect on the lending of small banks than on the lending of large banks (Kashyap and Stein 1995). Unfortunately, while these (and many other) pieces of evidence are consistent with the existence of a lending channel of monetary transmission, they also admit 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, we find that changes in monetary policy matter more for the lending of those banks with the least liquid balance sheets. The results are strongly statistically significant, and seem to be impervious to a wide range of variations in specification and estimation technique. Moreover, the implied differences between banks are of a magnitude that, at a minimum, one would characterize as economically interesting.

Unlike with the earlier evidence, it is much harder to come up with alternative, non-lending-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 pretty hard to deny the existence of a lending channel of monetary policy transmission, at least for the U.S. in our sample period. The next logical question then becomes: quantitatively, how important is the lending channel for aggregate economic activity? In other words, even if we accept as airtight the case that open-market operations have a substantial causal impact on small banks' loan supply, how big is the ultimate effect on investment and GDP? For these sorts of calculations, a couple of other ingredients come into play. First, one must determine what fraction of the banking sector is made up of banks that are "small", in the sense of having meaningful difficulty raising uninsured forms of external finance. If we use the metric of being in the bottom 95% of the size distribution, we can see from Table 1 that roughly 25% of the banking system's assets reside in "small" banks--a modest, but certainly non-trivial fraction.

Second, and more subtly, one needs to know the elasticity with which borrowers can substitute between bank and non-bank 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 quantitative 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 the transmission of monetary policy, they are questions that must eventually be addressed.

Data Appendix

A. Call Report Data

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 worth of data. The actual number of observations in our regressions is somewhat 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% of their total lending. Together these hurdles knock out 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 use 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). The data on securities is added to data on cash holdings (rcfd0010) and Fed Funds Sold and Securities Purchased Under Agreements to Resell (rcfd 1350) to get an overall series for cash and securities.

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 re-estimated 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). 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.

B. Construction of Bernanke-Mihov (1995) Measure of Monetary Policy

The VAR-based measure of monetary policy we use corresponds to the just-identified case (Case V) in Bernanke and Mihov (1995), under a single sample split. A VAR is estimated on monthly data separately for two sample periods: 1967:1 to 1979:9; and 1979:10 to 1994:2. The data used was provided by Bernanke and Mihov and includes monthly data on three policy variables (total reserves, nonborrowed reserves, and the federal funds rate) and three non-policy variables (real GDP, the GDP deflator, and an index of commodity prices). The construction of the monthly figures for GDP and the GDP deflator are discussed in their paper. The VAR included 12 lags of these variables.

The identification method used is a "semi-structural" approach that does not require identification of the structural form of the non-policy variables. This is done by assuming that policy variables have no contemporaneous effect on non-policy variables, but respond to shocks in non-policy variables within the period. This leaves only the component of the shocks to the policy variables that are orthogonal to the non-policy variables to be explained.

The final key identifying assumption is that innovations to total reserves are demand shocks that the Fed fully accommodates in the short run. This is the least restrictive case considered by Bernanke and Mihov, in that it leaves the system just-identified, rather than overidentified. With this assumption, monetary policy shocks can be recovered from the innovations to the policy variables. The total policy measure is then constructed as that linear combination of the funds rate, nonborrowed reserves and total reserves for which the VAR innovations correspond to the estimated monetary policy shocks.

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Table 1

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)	32.8	119.1	247.7	556.6	1,341.5	10,763.4
Median Assets (1993 $ millions)	28.4	112.6	239.0	508.1	1,228.7	3,964.6
Fraction of Total System Assets	.128	.093	.064	.087	.070	.559
Fraction of Total Assets in Size Category						
Cash & Securities	.426	.418	.418	.408	.396	.371
Fed Funds Lent	.049	.040	.038	.045	.045	.025
Total Domestic Loans	.518	.531	.531	.531	.539	.413
Real Estate Loans	.172	.191	.196	.179	.174	.087
C & I Loans	.102	.131	.153	.160	.168	.171
Loans to Individuals	.147	.162	.148	.147	.138	.059
Total Deposits	.902	.897	.890	.868	.841	.810
Demand Deposits	.312	.301	.301	.313	.327	.248
Time & Savings Deposits	.590	.596	.589	.554	.508	.326
Time Deposits > $100K	.067	.095	.119	.139	.143	.156
Fed Funds Borrowed	.004	.010	.019	.039	.067	.076
Subordinated Debt	.002	.003	.004	.005	.006	.005
Other Liabilities	.008	.012	.013	.014	.017	.057
Equity	.085	.078	.074	.074	.070	.052

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)	44.4	165.8	380.1	1,072.6	3,366.0	17,413.4
Median Assets (1993 $ millions)	38.6	155.7	362.7	920.8	3,246.3	9,297.7
Fraction of Total System Assets	.105	.078	.060	.101	.106	.551
Fraction of Total Assets in Size Category						
Cash & Securities	.399	.371	.343	.333	.325	.311
Fed Funds Lent	.045	.040	.035	.041	.041	.040
Total Loans	.531	.562	.596	.594	.599	.587
Real Estate Loans	.296	.331	.337	.302	.252	.209
C & I Loans	.087	.101	.111	.117	.132	.183
Loans to Individuals	.086	.098	.120	.144	.166	.097
Total Deposits	.879	.868	.850	.794	.760	.690
Transaction Deposits	.258	.257	.254	.240	.258	.193
Large Deposits	.174	.207	.225	.248	.244	.212
Brokered Deposits	.002	.004	.008	.017	.016	.013
Fed Funds Borrowed	.010	.021	.039	.063	.097	.093
Subordinated Debt	.000	.000	.001	.002	.004	.017
Other Liabilities	.013	.021	.026	.054	.059	.129
Equity	.098	.090	.084	.086	.080	.072

Table 2

Correlations of Measures of Monetary Policy

	Correlation of:		
	Levels	Annual Changes	Quarterly Changes
A. Full Sample (76Q1 - 93Q2)			
1. Boschen-Mills/Fed Funds	.608 (.000)	.382 (.001)	.219 (.069)
2. Boschen-Mills/Bernanke-Mihov	.607 (.000)	.364 (.002)	.155 (.200)
3. Fed Funds/Bernanke-Mihov	.951 (.000)	.907 (.000)	.900 (.000)
B. 1st Half Sample (76Q1 - 85Q4)			
1. Boschen-Mills/Fed Funds	.514 (.001)	.318 (.046)	.233 (.148)
2. Boschen-Mills/Bernanke-Mihov	.591 (.000)	.338 (.033)	.148 (.361)
3. Fed Funds/Bernanke-Mihov	.956 (.000)	.915 (.000)	.911 (.000)
C. 2nd Half Sample (86Q1 - 93Q2)			
1. Boschen-Mills/Fed Funds	.733 (.000)	.647 (.000)	.414 (.023)
2. Boschen-Mills/Bernanke-Mihov	.594 (.001)	.457 (.011)	.341 (.065)
3. Fed Funds/Bernanke-Mihov	.855 (.000)	.879 (.000)	.686 (.000)

(p-values in parentheses)

Table 3

Example of First Step Regression
1987 Q4

A. Total Loan Growth, smallest category of banks

Coefficient on:

Loan Growth							
Lag 1	Lag 2	Lag 3	Lag 4	Security/Assets	R2	Adj. R2	N
0.12	-0.03	0.03	0.26	0.04	.1692	.1680	11,766
(12.23)	(-3.10)	(3.13)	(36.24)	(10.31)			

B. C&I Loan Growth, smallest category of banks

Coefficient on:

Loan Growth							
Lag 1	Lag 2	Lag 3	Lag 4	Security/Assets	R2	Adj. R2	N
-0.06	-0.02	-0.03	0.07	0.06	.0226	.0221	10,753
(-5.19)	(-1.80)	(-2.51)	(7.72)	(4.71)			

(t statistics in parentheses)

Table 4

The Impact of Monetary Policy on Beta: Overview

Panel A: C&I Loans

Sum of Coefficients on Monetary Policy Indicator

	Univariate	Bivariate
1. Boschen-Mills		
<95	-0.0374	-0.0149
	(0.0061)	(0.3509)
95-99	-0.0414	-0.0163
	(0.2174)	(0.6160)
>99	0.0286	0.0775
	(0.5006)	(0.0251)
Small-Big	-0.0660	-0.0924
	(0.0800)	(0.0222)
2. Funds Rate		
<95	-0.0210	-0.0128
	(0.0006)	(0.0241)
95-99	-0.0092	0.002
	(0.5515)	(0.8944)
>99	0.0274	0.0391
	(0.0453)	(0.0143)
Small-Big	-0.0484	-0.0518
	(0.0002)	(0.0068)
3. Bernanke - Mihov		
<95	-1.9903	-1.4416
	(0.0000)	(0.0000)
95-99	-0.7331	0.3281
	(0.4959)	(0.7975)
>99	1.2793	2.7467
	(0.2826)	(0.0231)
Small-Big	-3.2696	-4.1883
	(0.0025)	(0.0011)

(p-values in parentheses)

Table 4

The Impact of Monetary Policy on Beta: Overview

Panel B: Total Loans

Sum of Coefficients on Monetary Policy Indicator

	Univariate	Bivariate
1. Boschen-Mills		
<95	-0.0153	-0.0031
	(0.1021)	(0.7255)
95-99	0.0010	0.0175
	(0.9685)	(0.3850)
>99	0.0267	0.0607
	(0.4404)	(0.0052)
Small-Big	-0.0420	-0.0638
	(0.1611)	(0.0064)
2. Funds Rate		
<95	-0.0048	-0.0012
	(0.2320)	(0.6916)
95-99	-0.0035	-0.0053
	(0.5453)	(0.4308)
>99	0.0034	0.0102
	(0.8288)	(0.4790)
Small-Big	-0.0082	-0.0114
	(0.5539)	(0.4389)
3. Bernanke - Mihov		
<95	-0.3403	-0.0408
	(0.1210)	(0.8720)
95-99	-0.2974	-0.7665
	(0.4241)	(0.1401)
>99	0.8618	1.6622
	(0.3531)	(0.0437)
Small-Big	-1.2021	-1.703
	(0.1551)	(0.0444)

(p-values in parentheses)

Table 5

Panel A
Money Measure: Change in Boschen Mills
C&I Loans

		Monetary Policy Indicator					Change in GDP					Trend	R2	Adj. R2
		0	1	2	3	4	0	1	2	3	4			
Univariate														
	Small	-0.0053	-0.0070	-0.0117	-0.0128	-0.0005						0.0000	0.1308	0.0409
		(- 0.85)	(- 2.06)	(- 2.66)	(- 2.56)	(- 0.12)						(0.00)		
	Medium	0.0158	-0.0172	-0.0156	-0.0179	-0.0064						0.0001	0.0901	-0.004
		(1.31)	(- 1.33)	(- 0.95)	(- 1.79)	(- 0.46)						(0.25)		
	Large	-0.0210	-0.0063	0.0206	-0.0026	0.0379						-0.0014	0.1994	0.1166
		(- 1.69)	(- 0.53)	(2.15)	(- 0.12)	(3.06)						(- 2.80)		
Bivariate														
	Small	-0.0020	-0.0028	-0.0045	-0.0065	0.0008	0.1640	0.1258	0.3203	1.1380	0.2900	0.0000	0.3958	0.2704
		(- 0.34)	(- 0.64)	(- 0.94)	(- 1.10)	(0.19)	(0.49)	(0.66)	(1.29)	(4.71)	(0.94)	(0.00)		
	Medium	0.0193	-0.0160	-0.0078	-0.0077	-0.0042	0.7949	-0.5697	0.3243	0.9028	1.0624	0.0002	0.1722	0.0004
		(1.86)	(- 1.07)	(- 0.51)	(- 0.79)	(- 0.33)	(1.50)	(- 1.03)	(0.47)	(1.10)	(1.80)	(0.50)		
	Large	-0.0126	-0.0006	0.0308	0.0129	0.0470	1.4605	-0.6290	1.4226	-0.3067	2.5250	-0.0014	0.3896	0.2629
		(- 1.14)	(- 0.05)	(2.75)	(0.72)	(4.05)	(1.86)	(- 1.17)	(1.42)	(- 0.48)	(4.35)	(- 3.50)		

(t statistics in parentheses)

Table 5

Panel B
Money Measure: Change in Fed Funds Rate
C&I Loans

		Monetary Policy Indicator					Change in GDP					Trend	R2	Adj. R2
		0	1	2	3	4	0	1	2	3	4			
Univariate														
	Small	-0.0054	-0.0051	-0.0048	-0.0050	-0.0007						0.0002	0.2162	0.1351
		(- 3.00)	(- 2.55)	(- 3.00)	(- 4.55)	(- 0.64)						(0.67)		
	Medium	-0.0059	0.0022	-0.0022	-0.0046	0.0013						0.0001	0.0771	-0.0184
		(- 1.84)	(0.47)	(- 0.42)	(- 1.15)	(0.57)						(0.25)		
	Large	0.0033	0.0039	0.0134	0.0050	0.0018						-0.0016	0.1579	0.0708
		(0.85)	(0.83)	(3.12)	(1.61)	(0.69)						(- 4.00)		
Bivariate														
	Small	-0.0056	-0.0055	-0.0030	-0.0009	0.0022	0.2399	-0.2861	0.1443	1.2542	0.6488	0.0002	0.5196	0.4199
		(- 6.22)	(- 3.67)	(- 1.58)	(- 0.56)	(1.57)	(0.64)	(- 1.19)	(0.52)	(4.00)	(4.04)	(1.00)		
	Medium	-0.0072	0.0002	-0.0002	0.0019	0.0073	0.2479	-0.9337	0.2616	1.1362	1.7497	0.0001	0.1921	0.0245
		(- 2.40)	(0.04)	(- 0.04)	(0.39)	(1.92)	(0.30)	(- 1.32)	(0.38)	(1.32)	(3.46)	(0.20)		
	Large	0.0023	0.0052	0.0179	0.0101	0.0035	0.3743	-0.8447	2.1733	0.3968	1.5656	-0.0016	0.3198	0.1786
		(0.79)	(1.11)	(2.84)	(1.91)	(0.73)	(0.36)	(- 0.91)	(2.83)	(0.53)	(2.47)	(- 3.20)		

(t statistics in parentheses)

Table 5

Panel C
Money Measure: Change in Bernanke-Mihov
C&I Loans

		Monetary Policy Indicator					Change in GDP					Trend	R2	Adj. R2
		0	1	2	3	4	0	1	2	3	4			
Univariate														
	Small	-0.4587	-0.5718	-0.4704	-0.4246	-0.0648						0.0003	0.3641	0.2983
		(- 4.65)	(- 6.80)	(- 5.86)	(- 4.66)	(- 0.75)						(1.50)		
	Medium	-0.3432	0.0240	-0.1388	-0.4449	0.1697						0.0001	0.0929	-0.001
		(- 1.33)	(0.08)	(- 0.36)	(- 1.44)	(0.95)						(0.25)		
	Large	-0.0314	0.3227	0.7119	0.5133	-0.2372						-0.0014	0.1559	0.0686
		(- 0.13)	(0.88)	(1.80)	(1.90)	(- 0.93)						(- 2.80)		
Bivariate														
	Small	-0.4084	-0.5735	-0.3667	-0.2109	0.1178	0.5084	-0.2755	0.0348	1.0020	0.5832	0.0003	0.6027	0.5203
		(- 5.18)	(- 4.88)	(- 2.99)	(- 2.26)	(0.97)	(1.40)	(- 1.01)	(0.13)	(4.80)	(3.10)	(3.00)		
	Medium	-0.3201	-0.0880	0.0858	-0.0068	0.6572	0.5521	-1.0220	0.2263	0.8653	1.8852	0.0001	0.2059	0.0411
		(- 1.42)	(- 0.31)	(0.19)	(- 0.02)	(2.16)	(0.65)	(- 1.32)	(0.36)	(1.06)	(3.01)	(0.20)		
	Large	0.0583	0.4780	1.2462	0.9363	0.0278	0.2166	-0.9765	2.1532	0.6364	1.7275	-0.0016	0.3277	0.1882
		(0.26)	(1.02)	(2.49)	(2.76)	(0.08)	(0.16)	(- 1.10)	(2.69)	(0.70)	(2.82)	(- 3.20)		

(t statistics in parentheses)

Table 5

Panel D
Money Measure: Change in Boschen Mills
Total Loans

		Monetary Policy Indicator					Change in GDP					Trend	R2	Adj. R2
		0	1	2	3	4	0	1	2	3	4			
Univariate														
	Small	-0.0038	-0.0137	0.0034	0.0011	-0.0023						-0.0003	0.2597	0.1831
		(- 1.27)	(- 3.61)	(1.55)	(0.28)	(- 0.66)						(- 3.00)		
	Medium	0.0064	0.0001	0.0090	-0.0037	-0.0107						0.0001	0.0602	-0.0371
		(1.00)	(0.02)	(0.82)	(- 0.47)	(- 2.68)						(0.50)		
	Large	-0.0167	0.0043	0.0133	0.0165	0.0092						-0.0006	0.1215	0.0307
		(- 1.64)	(0.52)	(1.10)	(1.23)	(0.80)						(- 1.50)		
Bivariate														
	Small	-0.0011	-0.0114	0.0069	0.0042	-0.0017	0.3068	0.0987	0.2318	0.4952	0.0470	-0.0003	0.3899	0.2633
		(- 0.38)	(- 2.92)	(2.76)	(0.95)	(- 0.53)	(2.27)	(0.34)	(0.99)	(1.43)	(0.15)	(- 3.00)		
	Medium	0.0118	0.0055	0.0135	-0.0024	-0.0109	0.4416	0.6025	0.5638	0.6261	-0.6623	0.0001	0.198	0.0316
		(1.97)	(1.34)	(1.29)	(- 0.44)	(- 2.66)	(1.17)	(2.20)	(1.90)	(0.92)	(- 1.29)	(0.50)		
	Large	-0.0097	0.0056	0.0206	0.0316	0.0126	1.3087	-0.0326	-0.2986	1.2782	1.3344	-0.0006	0.3213	0.1805
		(- 1.62)	(0.80)	(1.86)	(2.87)	(1.01)	(2.02)	(- 0.05)	(- 0.34)	(1.28)	(1.74)	(- 3.00)		

(t statistics in parentheses)

Table 5

Panel E
Money Measure: Change in Fed Funds Rate
Total Loans

		Monetary Policy Indicator					Change in GDP					Trend	R2	Adj. R2
		0	1	2	3	4	0	1	2	3	4			
Univariate														
	Small	-0.0027	-0.0017	-0.0007	0.0005	-0.0002						-0.0003	0.1274	0.0371
		(- 2.25)	(- 1.55)	(- 0.58)	(0.42)	(- 0.17)						(- 3.00)		
	Medium	-0.0028	0.0018	-0.0006	-0.0012	-0.0007						0.0001	0.0379	-0.0617
		(- 2.80)	(1.06)	(- 0.27)	(- 0.75)	(- 0.33)						(0.50)		
	Large	-0.0051	0.0032	0.0039	0.0020	-0.0006						-0.0006	0.0846	-0.0101
		(- 1.46)	(1.03)	(0.75)	(0.59)	(- 0.17)						(- 1.50)		
Bivariate														
	Small	-0.0024	0.0002	0.0013	0.0013	-0.0015	-0.1144	0.3091	0.5892	0.3785	-0.1017	-0.0003	0.2819	0.1329
		(- 3.00)	(0.20)	(1.00)	(0.93)	(- 1.36)	(- 0.38)	(1.02)	(2.66)	(1.09)	(- 0.27)	(- 3.00)		
	Medium	-0.0001	0.0067	0.0003	-0.0058	-0.0064	0.1658	1.5649	0.7421	-0.2414	-0.8101	0.0001	0.2078	0.0433
		(- 0.07)	(3.35)	(0.10)	(- 3.05)	(- 2.67)	(0.32)	(4.12)	(1.85)	(- 0.63)	(- 1.46)	(0.50)		
	Large	-0.0040	0.0030	0.0044	0.0044	0.0025	0.6372	0.0881	-0.0186	1.0279	1.0057	-0.0006	0.1992	0.033
		(- 1.48)	(0.91)	(0.80)	(1.10)	(0.63)	(0.58)	(0.09)	(- 0.02)	(1.27)	(1.27)	(- 1.50)		

(t statistics in parentheses)

Table 5

Panel F
Money Measure: Change in Bernanke-Mihov
Total Loans

		Monetary Policy Indicator					Change in GDP					Trend	R2	Adj. R2
		0	1	2	3	4	0	1	2	3	4			
Univariate														
	Small	-0.2033	-0.2434	0.0127	0.0478	0.0460						-0.0003	0.1859	0.1017
		(- 2.40)	(- 3.93)	(0.17)	(0.55)	(0.61)						(- 3.00)		
	Medium	-0.0832	0.0500	-0.0433	-0.2278	0.0069						0.0001	0.0394	-0.0599
		(- 0.88)	(0.32)	(- 0.35)	(- 1.97)	(0.04)						(0.50)		
	Large	-0.1691	0.2184	0.5474	0.1328	0.1324						-0.0007	0.1009	0.0079
		(- 0.78)	(0.85)	(1.76)	(0.54)	(0.68)						(- 1.75)		
Bivariate														
	Small	-0.1714	-0.1283	0.1414	0.1144	0.0031	-0.0440	0.1389	0.4628	0.4371	0.0144	-0.0003	0.3097	0.1664
		(- 1.96)	(- 1.65)	(1.83)	(1.22)	(0.03)	(- 0.17)	(0.50)	(2.18)	(1.12)	(0.04)	(- 3.00)		
	Medium	0.1416	0.3556	-0.1533	-0.6896	-0.4208	0.4487	1.6498	0.5511	-0.2691	-0.8528	0.0001	0.2277	0.0674
		(1.20)	(2.21)	(- 0.80)	(- 3.53)	(- 2.36)	(0.96)	(4.32)	(1.65)	(- 0.51)	(- 1.50)	(0.50)		
	Large	-0.0759	0.2317	0.6858	0.3945	0.4262	0.6518	-0.1435	0.0191	1.0677	1.3032	-0.0007	0.2371	0.0787
		(- 0.44)	(0.72)	(1.94)	(1.67)	(1.70)	(0.58)	(- 0.14)	(0.03)	(1.19)	(1.67)	(- 1.75)		

(t statistics in parentheses)

Table 6

The Impact of Monetary Policy on Beta: Overview
Cash Excluded from Security Measure

Panel A: C&I Loans

Sum of Coefficients on Monetary Policy Indicator

	Univariate	Bivariate
1. Boschen-Mills		
<95	-0.0438	-0.0226
	(0.0199)	(0.2209)
95-99	-0.0339	0.0049
	(0.3974)	(0.8801)
>99	0.0960	0.1415
	(0.1463)	(0.0090)
Small-Big	-0.1398	-0.1641
	(0.0222)	(0.0035)
2. Funds Rate		
<95	-0.0267	-0.0193
	(0.0002)	(0.0035)
95-99	-0.0066	0.0055
	(0.6324)	(0.6639)
>99	0.0795	0.1034
	(0.0047)	(0.0058)
Small-Big	-0.1062	-0.1226
	(0.0003)	(0.0029)
3. Bernanke - Mihov		
<95	-2.3621	-1.8954
	(0.0000)	(0.0000)
95-99	-0.3062	0.7472
	(0.8115)	(0.6438)
>99	5.4974	8.1434
	(0.0058)	(0.0002)
Small-Big	-7.8595	-10.0388
	(0.0001)	(0.0000)

(p-values in parentheses)

Table 6

The Impact of Monetary Policy on Beta: Overview
Cash Excluded from Security Measure

Panel B: Total Loans

Sum of Coefficients on Monetary Policy Indicator

	Univariate	Bivariate
1. Boschen-Mills		
<95	-0.0179	-0.0080
	(0.1048)	(0.4841)
95-99	-0.0129	0.0094
	(0.5834)	(0.5379)
>99	0.0516	0.0840
	(0.3230)	(0.0457)
Small-Big	-0.0695	-0.0920
	(0.1340)	(0.0201)
2. Funds Rate		
<95	-0.0088	-0.0051
	(0.0181)	(0.1939)
95-99	-0.0126	-0.0097
	(0.1133)	(0.1160)
>99	0.0258	0.0421
	(0.1713)	(0.0256)
Small-Big	-0.0346	-0.0472
	(0.0576)	(0.0210)
3. Bernanke - Mihov		
<95	-0.7044	-0.4158
	(0.0047)	(0.2026)
95-99	-0.8098	-0.8522
	(0.1109)	(0.0600)
>99	2.3005	3.7978
	(0.0951)	(0.0018)
Small-Big	-3.0049	-4.2135
	(0.0292)	(0.0018)

(p-values in parentheses)

Table 7

The Impact of Monetary Policy on Beta: Overview
Quasi Instrumental Variable Approach

Panel A: C&I Loans

Sum of Coefficients on Monetary Policy Indicator

	Univariate	Bivariate
1. Boschen-Mills		
<95	-0.0284	-0.0058
	(0.0287)	(0.6959)
95-99	-0.0400	-0.0141
	(0.2362)	(0.6657)
>99	0.0365	0.0863
	(0.4231)	(0.0257)
Small-Big	-0.0649	-0.0921
	(0.1313)	(0.0395)
2. Funds Rate		
<95	-0.0212	-0.0121
	(0.0032)	(0.0203)
95-99	-0.0064	0.0043
	(0.6787)	(0.7708)
>99	0.0367	0.0486
	(0.0128)	(0.0111)
Small-Big	-0.0579	-0.0608
	(0.0001)	(0.0076)
3. Bernanke - Mihov		
<95	-1.8421	-1.1945
	(0.0000)	(0.0007)
95-99	-0.5808	0.4167
	(0.5804)	(0.7440)
>99	2.0938	3.5274
	(0.0817)	(0.0071)
Small-Big	-3.9359	-4.7219
	(0.0009)	(0.0017)

(p-values in parentheses)

Table 7

The Impact of Monetary Policy on Beta: Overview
Quasi Instrumental Variable Approach

Panel B: Total Loans

Sum of Coefficients on Monetary Policy Indicator

	Univariate	Bivariate
1. Boschen-Mills		
<95	-0.0115	0.0031
	(0.2074)	(0.6227)
95-99	-0.0036	0.0175
	(0.8895)	(0.3695)
>99	0.0324	0.0703
	(0.3548)	(0.0009)
Small-Big	-0.0439	-0.0672
	(0.1272)	(0.0023)
2. Funds Rate		
<95	-0.0028	0.0009
	(0.5187)	(0.6262)
95-99	-0.0033	-0.0041
	(0.5962)	(0.5357)
>99	0.0148	0.0235
	(0.3686)	(0.1319)
Small-Big	-0.0176	-0.0225
	(0.2002)	(0.1389)
3. Bernanke - Mihov		
<95	-0.1473	0.1660
	(0.5073)	(0.3729)
95-99	-0.3502	-0.7365
	(0.3763)	(0.1647)
>99	1.6596	2.6089
	(0.0725)	(0.0018)
Small-Big	-1.8069	-2.4429
	(0.0304)	(0.0066)

(p-values in parentheses)

Table 8

The Impact of Monetary Policy on Beta: Overview
Split Sample Results

Panel A: C&I Loans

Sum of Coefficients on Monetary Policy Indicator

	76Q1 - 85Q4		86Q1 - 93Q2	
	Univariate	Bivariate	Univariate	Bivariate
1. Boschen-Mills				
<95	-0.0631	0.0035	-0.0135	-0.0137
	(0.0000)	(0.6433)	(0.2605)	(0.4417)
95-99	-0.1023	-0.0259	0.0105	0.0125
	(0.0120)	(0.5450)	(0.7420)	(0.6388)
>99	-0.1081	-0.0310	0.1166	0.1202
	(0.0095)	(0.5280)	(0.0001)	(0.0026)
Small-Big	0.0450	0.0345	-0.1301	-0.1339
	(0.2415)	(0.4559)	(0.0007)	(0.0139)
2. Funds Rate				
<95	-0.0161	0.0043	-0.0324	-0.0296
	(0.0343)	(0.0014)	(0.0037)	(0.0606)
95-99	-0.0141	0.0076	-0.0011	0.0182
	(0.6080)	(0.6609)	(0.9530)	(0.2238)
>99	0.0105	0.0287	0.0624	0.0800
	(0.3596)	(0.0001)	(0.0166)	(0.0289)
Small-Big	-0.0266	-0.0244	-0.0948	-0.1096
	(0.0006)	(0.0018)	(0.0021)	(0.0155)
3. Bernanke - Mihov				
<95	-2.2357	0.1881	-1.7124	-1.4052
	(0.0000)	(0.2250)	(0.0000)	(0.0015)
95-99	-3.3104	-1.0797	1.8501	2.3277
	(0.0522)	(0.6189)	(0.0952)	(0.0163)
>99	-1.2218	1.9691	3.4730	4.0054
	(0.2700)	(0.0820)	(0.0099)	(0.0050)
Small-Big	-1.0139	-1.7810	-5.1854	-5.4106
	(0.2562)	(0.1141)	(0.0001)	(0.0002)

(p-values in parentheses)

Table 8

The Impact of Monetary Policy on Beta: Overview
Split Sample Results

Panel B: Total Loans

	Sum of Coefficients on Monetary Policy Indicator			
	76Q1 - 85Q4		86Q1 - 93Q2	
	Univariate	Bivariate	Univariate	Bivariate
1. Boschen-Mills				
<95	-0.0367	-0.0008	-0.0010	-0.0062
	(0.0025)	(0.9463)	(0.7950)	(0.2700)
95-99	-0.0605	-0.0105	0.0393	0.0454
	(0.0306)	(0.6884)	(0.0021)	(0.0000)
>99	-0.0577	0.0161	0.0758	0.0868
	(0.1954)	(0.6374)	(0.0000)	(0.0000)
Small-Big	0.0210	-0.0169	-0.0769	-0.0930
	(0.6533)	(0.7030)	(0.0000)	(0.0000)
2. Funds Rate				
<95	-0.0062	0.0050	-0.0106	-0.0151
	(0.2673)	(0.0514)	(0.0070)	(0.0020)
95-99	-0.0107	-0.0002	0.0063	-0.0169
	(0.0981)	(0.9596)	(0.7235)	(0.2069)
>99	-0.0130	0.0004	0.0355	0.0438
	(0.4077)	(0.9734)	(0.0631)	(0.0636)
Small-Big	0.0068	0.0046	-0.0462	-0.0589
	(0.5706)	(0.6459)	(0.0193)	(0.0125)
3. Bernanke - Mihov				
<95	-0.6599	0.7420	-0.3650	-0.4025
	(0.0852)	(0.0210)	(0.0371)	(0.0339)
95-99	-1.1360	-0.1102	0.0171	-0.5994
	(0.0653)	(0.8798)	(0.9875)	(0.3889)
>99	-0.4883	2.2249	2.3437	2.6064
	(0.6674)	(0.0369)	(0.0000)	(0.0000)
Small-Big	-0.1716	-1.4828	-2.7087	-3.0089
	(0.8544)	(0.2288)	(0.0000)	(0.0000)

(p-values in parentheses)

Figure 1

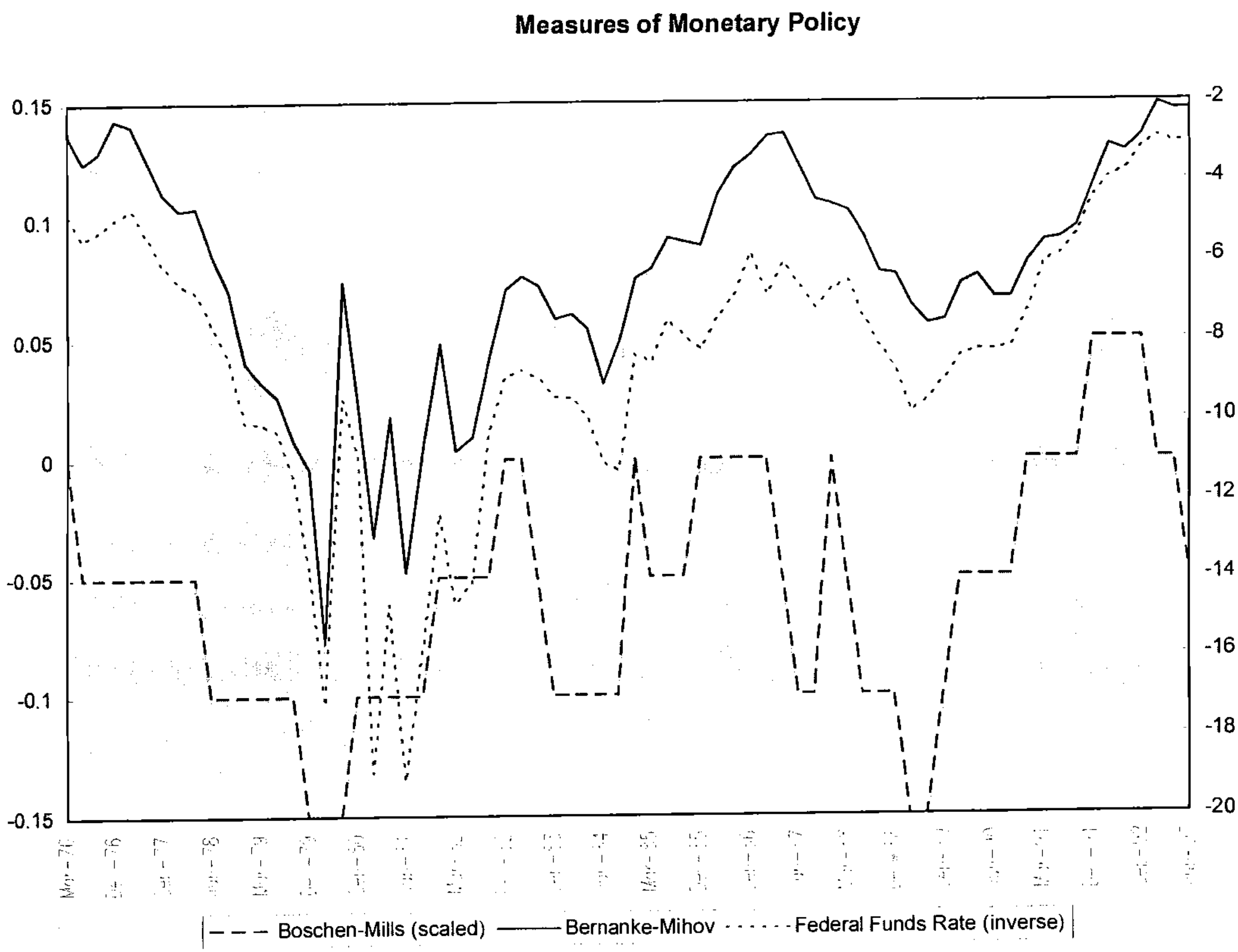
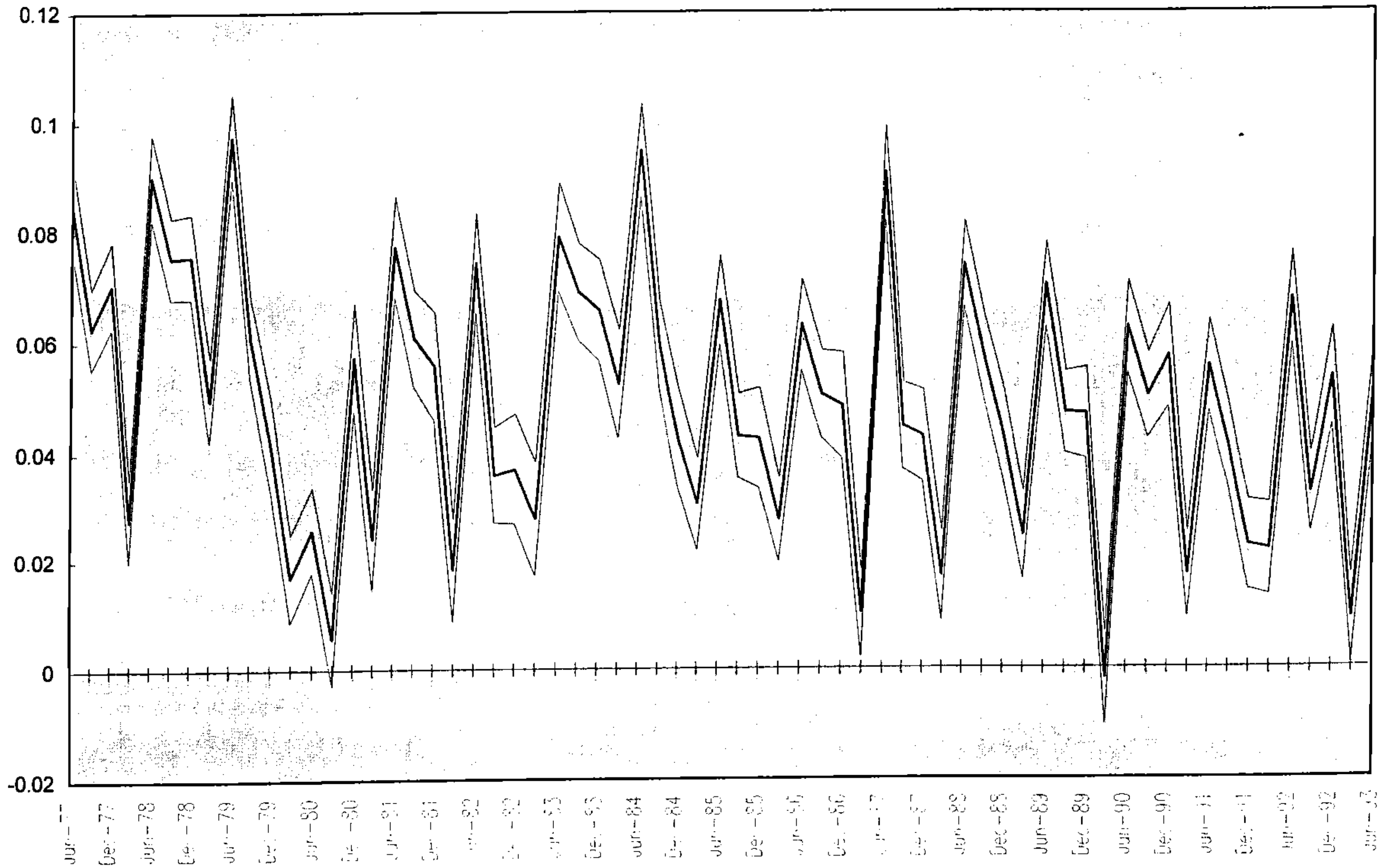

Measures of Monetary Policy
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-2
-4
-6
-8
-10
-12
-14
-16
-18
-20
Boschen-Mills (scaled)
Bernanke-Mihov
Federal Funds Rate (inverse)

Figure 2
Time Series of Betas for Small Banks

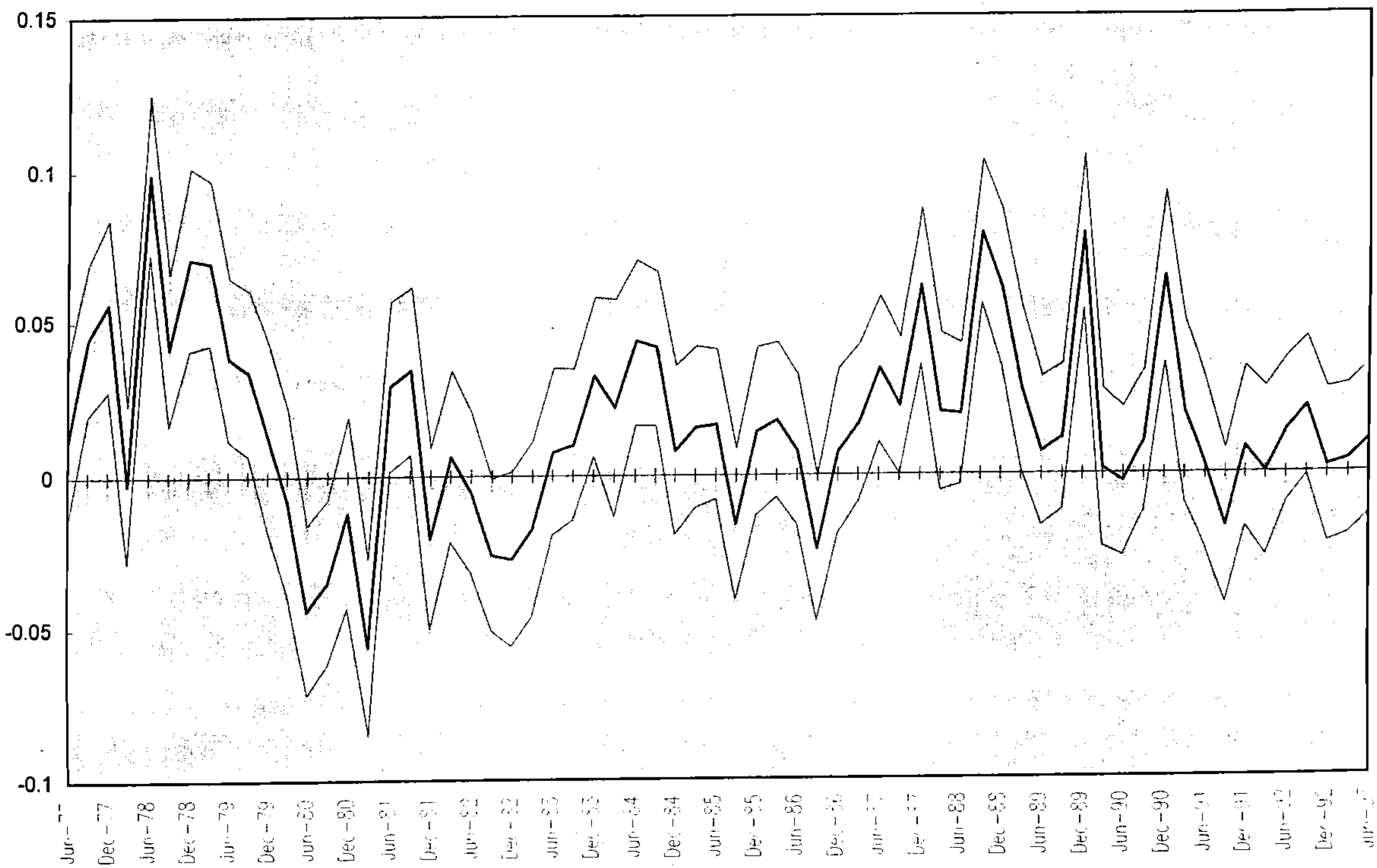
Panel A: Total Loans



Note: The betas are plotted along with 2-standard-error confidence bands.

Figure 2 (cont'd.)
Time Series of Betas for Small Banks

Panel B: C & I Loans



Note: The betas are plotted along with 2-standard-error confidence bands.