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CREDIT CONDITIONS AND THE CYCLICAL BEHAVIOR OF INVENTORIES: A CASE STUDY OF THE 1981-82 RECESSION

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ABSTRACT

This paper examines micro data on U.S. firms' inventories during different macroeconomic episodes. Much of the analysis focuses on the 1981-82 recession, a recession that was apparently precipitated by tight monetary policy. We find important cross-sectional effects in this period: firms that were "bank-dependent" were much more prone to shed inventories than their non-bank-dependent counterparts. In contrast, such cross-sectional differences are largely absent during a period of "loose" monetary policy later in the 1980s. Our findings are consistent with the view that 1) there is a bank lending channel of monetary policy transmission; 2) the lending channel is likely to be particularly important in explaining inventory fluctuations during downturns.

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1. Introduction

This paper is motivated by three stylized facts. The first fact is that inventory movements play a dominant role in business cycle fluctuations. For example, Blinder and Maccini (1991) document that, in postwar U.S. recessions, declines in inventory investment account for an average of 87% of the total peak-to-trough movement in GNP. The second fact is that recessions usually follow a period of tight credit. Eckstein and Sinai (1986) argue that each of the six recessions between 1957 and 1982 was preceded by a "credit crunch"--a time of restrictive monetary policy and rising interest rates.

At first glance, it would appear that these two facts can be tied together with a simple and obvious story. The story goes as follows: firms' desired stock of inventories depends importantly on the cost of carry; as financing becomes more expensive, firms cut back on their inventory holdings. According to this story, one of the most significant effects of restrictive monetary policy is thus its impact on inventory behavior, an impact which is transmitted through a cost-of-financing channel.

There is only one problem with this story. Its basic premise--that inventories are sensitive to financing conditions--finds scant support in most empirical work. This is our third stylized fact. As Blinder and Maccini put it in their survey paper, "little influence of real interest rates on inventory investment can be found empirically."

So where does this leave the simple "financial" account of the cyclical behavior of inventories? In our view, it would be premature to dismiss the theory. The failure of empirical models of inventories to find a significant role for financial variables may say more about the inadequacies of the specifications used in these models than about anything else.

There are at least two reasons why standard specifications--which typically use security market interest rates such as the commercial paper rate as explanatory variables--might do a poor job of capturing changes in "financial conditions", broadly defined. First, some borrowers may face quantity rationing constraints of the sort described by Stiglitz and Weiss (1981), Jaffee and Russell (1976) and others, and thus may be unable to obtain funds at the observed commercial paper rate. Second, some borrowers may be "bank-dependent", in the sense that they do not have easy access to public debt markets. To the extent that there are important variations in the relative cost of bank loans versus commercial paper, the commercial paper rate may again be a poor measure of financing costs for these firms.

Kashyap, Stein and Wilcox (1992) present some aggregate time-series evidence on the relative costs of bank loans and commercial paper. KSW begin by constructing a quantity financing variable--the "mix"--which they argue captures movements in bank loan supply. The mix is defined (roughly) as the ratio of corporate bank borrowing to commercial paper borrowing, and the basic intuition is that a decline in the mix is indicative of a contraction in bank loan supply. Next, KSW demonstrate that tight monetary policy typically leads to a fall in the mix--i.e., tight money causes an inward shift in bank loan supply.

Finally, KSW show that when standard inventory models are augmented to include the mix variable, the mix enters in a fashion that is both economically and statistically significant. In other words, information on the state of bank loan supply does a better job of explaining inventory movements than do open-market interest rates. KSW interpret their results as evidence that: 1) monetary policy has an important effect on bank lending conditions; and 2) a significant number of firms are bank-dependent, and therefore are more sensitive to bank lending conditions than to security market rates.

Thus the KSW results at least partially resuscitate the simple financial account of inventory fluctuations given above, while at the same time rationalizing the failure of conventional inventory models to find a significant role for interest rates. However, their focus on aggregate data leaves an important gap still remaining. The "lending" theories of monetary policy transmission stressed by KSW and many previous authors[1] make strong cross-sectional predictions that have not yet been tested. In particular, if the lending view is correct, one should expect the inventories of bank-dependent firms to fall more sharply in response to a monetary contraction than the inventories of those firms who do not need to rely on bank financing.

The goal of this paper is to provide an empirical test of this cross-sectional hypothesis. In doing so, we devote most of our attention to firm-level inventory movements during the calendar year 1982. We focus on this year because it roughly encompasses the recession of 1981Q3-1982Q4. This recession was preceded by a clear tightening of monetary policy, beginning with the Fed's change in operating procedures in October of 1979. In other words, the aggregate data point to this episode as a natural "case study" of a monetary policy-induced contraction, and hence as an obvious place to begin looking for the sorts of cross-sectional effects we are interested in.

[1] Early work on the distinction between the "money" and "lending" channels of monetary policy transmission includes Modigliani (1963), Tobin and Brainard (1963), and Brainard (1964). More recent contributions have come from Bernanke and Blinder (1988, 1990), King (1986) and Romer and Romer (1990), among others.

In order to test the hypothesis, we need an operational definition of the notion of bank-dependence. We argue below that a firm is most likely to be bank-dependent if it meets two criteria: 1) it has a "low" level of cash and other liquid assets on hand; and 2) it does not have access to public debt markets. Thus all our specifications involve regressing some measure of firm inventory investment against variables intended to capture liquidity and market access (as well as against a number of control variables). As will become clear, our basic strategy is closely related to that used by Fazzari, Hubbard and Petersen (1988) and others to study fixed investment.

Our results from the 1982 data confirm the predictions of the lending account of inventory movements--we find that bank-dependent firms do indeed cut their inventories by significantly more during this period than do their non-bank-dependent counterparts. This conclusion appears to be robust to a wide range of variations in specification and estimation technique.

After completing our analysis of the 1982 episode, we re-estimate the same sort of cross-sectional regressions for a couple of other time periods. First, we study inventory behavior during the two-year interval 1985-1986, a time of apparently unrestrictive monetary policy. This "easy money" period provides an important control in evaluating our results from 1982. For if the pronounced differences in inventory investment between bank-dependent and non-bank-dependent firms during the recession are really due to a monetary-policy induced constriction of bank lending, then we should not observe such differences at a time of looser monetary policy. Again, our results for 1985-86 support this prediction--we find little evidence of the significant differences in inventory behavior across firms that characterized the 1982 episode.

Finally, we also compare our results for 1982 to those obtained over the entire rest of the decade -- i.e., from 1983-1989. Although it is much less clear that this entire seven-year period can be interpreted as a time of easy money, we make the comparison as a form of robustness check on our results -- to see whether 1982 really does look different than the "average" year. Once again, the answer here is yes.

Overall, our results strongly suggest that--contrary to the conclusions drawn from prior research--financial factors do indeed matter for inventory movements. Moreover, the impact of financial factors on inventories is primarily felt during tight money/recessionary episodes.

Our results are consistent with the cross-sectional implications of the lending view of monetary policy transmission. A harder question is whether we can sharply differentiate between the lending view and other financial accounts of inventory movements also based on capital market imperfections. In other words, do our results "prove" that the Fed was able to engineer an inward shift in the bank loan supply schedule in 1982, or are there other possible interpretations?

Perhaps the most natural alternative hypothesis involves shocks to the value of firms' collateral. For example, recessions might be times when collateral values fall. In a world of information and/or moral hazard problems, this could increase the cost of bank financing, even if banks' willingness to supply loans (for a fixed amount of collateral) was unchanged.[2]

[2] Bernanke and Gertler (1989) develop a model in which shocks to collateral amplify business cycle fluctuations in this fashion.

Clearly, the lending story and the collateral story are closely related. Both attribute inventory movements in 1982 to a "cutoff" in the flow of bank credit, although in the former case the cutoff represents an inward shift in the loan supply schedule, while in the latter it does not. Can the two hypotheses be differentiated? We make an attempt in this direction in the last section of the paper.

The remainder of the paper is organized as follows. Section 2 provides some background macroeconomic facts, in an effort to better motivate both 1) the selection of 1982 as our primary focus of study, as well as 2) our use of the period 1985-86 as an "easy money" control. Section 3 describes our sample and data. In Section 4, we develop and estimate our baseline cross-sectional specifications, and then perform a number of robustness tests. In Section 5, we re-estimate our baseline model over the 1985-86 and 1983-89 periods. Section 6 concludes by addressing the distinction between the lending and collateral stories.

2. Monetary Policy in the 1980s

As noted above, our basic empirical strategy is to concentrate on an episode for which all the aggregate evidence suggests that tight monetary policy had a real effect on the economy, and on inventories in particular. *A priori,* such an episode will be the best place to look for the cross-sectional effects that we are interested in.

One concern that arises with this approach is that we might somehow "rig" the experiment if we examine a particular pre-selected episode. However, our pre-selection criteria refer solely to the characteristics of the aggregate data. If financial factors are not important, we see no reason to expect the cross-sectional data to conform to our predictions.[3]

The 1981-82 recession would appear ideal for our purposes. In recent history, it stands as the best American example of Milton Friedman's (1983) well-known observation that "... no country has cured substantial inflation without going through a transitional period of slow growth and high unemployment." (page 202) Indeed, in their textbook, Dornbusch and Fischer (1990) argue that "the decision [by the Fed to disinflate] was dramatic because there was little disagreement among economists of widely different macroeconomic persuasions that the move toward tight money would cause a recession along with a reduction in the inflation rate." (page 511)

Figures 1 through 3 provide some indicators of the stance of monetary policy starting around the time the Fed began tightening in October 1979, through the end of the next decade. Figure 1 plots the Federal funds rate, which Bernanke and Blinder (1990), Goodfriend (1992) and many others believe is a good indicator of the stance of monetary policy. At the time Paul Volcker became Fed chairman in August 1979, the funds rate stood in the neighborhood of 11 percent. Following the October shift in Fed operating procedures, the funds rate began to rise quickly, reaching almost 18 percent by the spring of 1980. With the imposition of the Carter credit controls, short-term interest rates, including the funds rate, dropped precipitously. By late-1980, with the controls lifted, the funds rate again began to climb, reaching 19 percent by year end. During the first eight months of

[3] To object to our approach, one must argue that the patterns we uncover would be expected to prevail during recessions for reasons unrelated to financial factors. We are unaware of any theories that would support such an argument.

1981, the funds rate was volatile but on the whole remained high -- averaging around 17.5 percent. In the last quarter of the year, the funds rate began to decline noticeably.

Thus on the basis of the funds rate, we are led to conclude that the tightening that started in 1979 was in place at least through the third quarter of 1981. Alternatively, because the funds rate drifted back up during the first part of 1982, before finally retreating to pre-Volcker levels, one might argue that policy was actually tight through the middle of 1982. Either way, it seems clear that monetary policy was restrictive at least until the onset of the recession, which began in the third quarter of 1981.

Figure 2 tells a similar story using another interest rate-based measure of monetary policy, the spread between the prime rate and the commercial paper rate. KSW document that this spread typically rises in the wake of a monetary contraction, and interpret this pattern as reflecting relative shifts in loan supply.[4] The prime-CP spread also began to rise shortly after the shift in operating procedures, dipped sharply around the credit control period, and then was high through late 1981. As with the funds rate, there was a final local peak in summer of 1982 before the spread dropped back to its 1979 level. Although the prime-CP spread slightly lags the funds rate, this measure too suggests that tight policy persisted at least through most of 1981.

Figure 3 examines a quantity-based indicator, the level of real M2. Here too it appears that monetary policy was tight through most of 1981. The real money supply contracted sharply through 1979 and early 1980, and then was roughly flat until the end of

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[4] The natural interpretation is that when the Fed tightens, the cost of bank financing rises relative to the cost of open market financing.

1981. Indeed, real M2 did not regain its early 1979 levels until the end of 1982.

Figures 4 and 5 examine the associated movements in output and inventories. As Figure 4 shows, real GNP growth was negative in the fourth quarter of 1981, and in two of the next three quarters. Figure 5 demonstrates that manufacturers cut real inventories for six consecutive quarters, also beginning in the fourth quarter of 1981. In relative terms, inventory behavior in this episode was typical of that seen in recessions: Blinder and Maccini (1991) note that from the third quarter of 1981 through the fourth quarter of 1982, the change in inventory investment represented 90% of the output decline. This percentage almost exactly matches their 87% average for post-war US recessions.

As noted in the Introduction, we also want to contrast the 1982 episode with an "easy money" control period. Here the choice of dates is less obvious. We take two different approaches that hopefully span the range of reasonable alternatives. Our preferred approach is to isolate the two years 1985-1986 as the easy money episode. For a couple of reasons, we view this sub-period as the decade's cleanest example of loose policy. First, roughly two years had elapsed since the Fed changed course and started to ease. Thus, even allowing for the famous long and variable lags, one would expect that by 1985 this change would begin to show its real effects (if any). Second, during the 1985-86 period, all the indicators from Figures 1 through 3 tell a similar story: the funds rate was low and declining, real money growth was healthy and the prime-CP spread was low.

Indeed, some observers consider the Fed's policy during this period to have been one of its worst mistakes in recent memory, because on the heels of the favorable oil shock in 1986, the Fed continued with its relatively loose stance, rather than using the opportunity to further cut back inflation. For instance, the Shadow Open Market Committee in its September 1986 Policy Statement wrote (p. 6), "Current Federal Reserve policy is irresponsible. After paying a high price to reduce inflation, the Federal Reserve, urged on by the administration, has returned to the short-sighted policies that produced the inflation of the 1970s."[5]

Although 1985-86 appears in many ways to be an excellent example of easy policy, we realize that focusing exclusively on these two years as a control period may invite the criticism of data snooping. Thus as an alternative, we take the entire remainder of the decade--1983 to 1989--as the easy money contrast to 1982. We believe that this sample period is less defensible on economic grounds. For example, taking 1983 to be representative of easy money is probably dubious. Yet this strategy has the appeal of being very conservative: if 1982 stands out relative to all other years, then the results will seem much more robust.

3. Data

The sample that we consider is taken from the Compustat data base which tracks publicly traded firms. We restrict our attention to manufacturing companies for which Compustat provides information. Ideally, we would prefer to also examine non-traded firms, since we suspect that these companies are most dependent on bank financing and hence most likely to be susceptible to a credit crunch. Unfortunately, we are unaware of any consistent

[5] Note that the Shadow Open Market Committee is not always critical of Fed policy. For instance, in September of 1982 they wrote (p. 3), "We applaud the Federal Reserve's commitment and the success of its policy to reduce inflation."

firm-level data for non-traded companies. Because of this undersampling and because even the smallest Compustat firms (by virtue of being publicly listed) are at least marginally integrated with capital markets, we conjecture that our analysis if anything understates the magnitude of any loan supply effects.

In our first set of tests covering the 1982 episode, we start with the 2,328 U.S. manufacturing companies that had complete (i.e. non-zero and non-missing) data on assets, sales, inventories and cash holdings for the fiscal years ending in 1980, 1981 and 1982. We then eliminate the roughly 30% of companies that reported mergers or acquisitions during this period, because these events can induce discontinuities in the balance sheet items that we are studying.

Finally, we restrict our attention to the majority of companies (approximately 60% of the remaining sample) whose fiscal years end in the fourth quarter, i.e. in either October, November or December.[6] We do this for two reasons. First, this time period roughly lines up with the recession. Secondly, by using companies that have similar fiscal years, we ensure that all the firms in our sample are operating under the same basic macroeconomic conditions. After all these screens, we are left with 933 companies.

In our later tests that examine other time periods, we reapply the same criteria each year to draw the sample. For instance, the sample for 1985 was selected by including all U.S. manufacturing companies whose fiscal years ended in the fourth quarter, who had

[6] We also examined a sample of companies whose fiscal years ended in third rather than the fourth quarter, i.e. in either July, August and September. These results were very similar to the results reported below. We also used a slightly different definition of the fourth quarter -- taking firms whose fiscal years ended in either November, December or January. Again the results were essentially identical.

complete data on assets, sales, inventories and cash holdings for 1983, 1984 and 1985, and who were not involved in any merger activity. In applying this set of screens independently for each year we are left with an unbalanced panel.[7]

Table 1 presents some basic summary statistics for two of the years that we examine, 1982 and 1985. In each year, the table divides our Compustat sample into two sub-samples, representing 1) the firms that have a bond rating from Standard and Poors at the beginning of the year in question, and 2) those that do not have such a rating. In addition to data from these two Compustat sub-samples, the table also includes some analogous information from the *Quarterly Financial Report for Manufacturing Corporations*, which covers the entire universe of manufacturing firms. The QFR numbers help give some idea of the extent to which our Compustat numbers are representative of those for all manufacturing firms.

A couple of observations stand out. First, the firms with bond ratings are, not surprisingly, much larger than average. These larger firms also tend to hold somewhat less cash (as a fraction of assets) than the typical Compustat company.

Perhaps a little more surprising is the relative behavior of sales and inventories for larger and smaller firms. Both in 1982 and again in 1985, the larger Compustat firms (i.e., those with bond ratings) have markedly lower sales and inventory growth than either the typical Compustat or QFR company. The fact that this phenomenon occurs in both years suggests that it may be due in part to secular forces, rather than just to differing sensitivities of large firms to recessions.

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[7] Because we were worried about survivorship bias we did not use a balanced panel that contained only continuously listed companies. Indeed, roughly 42 percent of the companies in the 1982 sample no longer exist and had to be retrieved from the Compustat research tape.

In terms of the lending view, one suggestive (albeit very crude) comparison can be made using the numbers in Table 1. Firms without bond ratings had nominal inventory growth that was 6.1% greater in 1985 than in 1982. For firms with bond ratings, the comparable 1985-1982 inventory growth differential is only 2.4%. Thus, the inventory investment of firms without bond ratings seems to benefit more from the shift from tight money in 1982 to easy money in 1985.

4. Cross-Sectional Determinants of Inventories During 1982

In this section, we present our empirical results for 1982. We start with a "baseline" set of specifications, which we estimate both by OLS and by IV. We then discuss the economic significance of our parameter estimates. Finally, we question some of the modeling choices embodied in our baseline specifications, and examine the robustness of our results to a number of alternative specifications.

4.1 Baseline Specifications

Table 2 summarizes our baseline regression results for the period 1981:4-1982:4. (i.e., the calendar year 1982) In each of the eight regressions, the dependent variable is the change in the log of firm inventories over the period. The right hand side variables include a constant term; the change in the log of firm sales over both the current and preceding years; as well as 19 dummy variables corresponding to two-digit SIC codes.

Equation 1 in the table represents the simplest possible specification. We add to the sales and industry controls the variable "LIQ", which is defined as a firm's ratio of cash and marketable securities to total assets at the beginning of the period (i.e., as of the end of 1981). The equation is then estimated by OLS. As can be seen, the LIQ variable is strongly significant--it enters with a coefficient of .39, and a t-statistic of 3.55.[8]

This result is consistent with the notion that bank-dependent firms cut their inventories by more--all else equal, firms with a higher value of LIQ should have less need for bank financing--but it is also subject to other interpretations. The ambiguity arises because the LIQ variable may be endogenous, and may be proxying for other factors that should affect inventory behavior. For example, it might be that LIQ is a proxy for innovations in firm profitability--firms that have a high value of LIQ might be firms that have recently become more profitable. If this is the case, it would not be surprising to see these firms devoting more resources to inventory investment, regardless of whether or not they are bank-dependent.[9]

There are two basic ways that one can address this ambiguity. The first approach involves putting a more precise structure on the concept of bank-dependence, so that we can sharpen our predictions relative to the "endogenous LIQ" hypothesis. The second approach is to estimate the coefficient on LIQ using an instrumental variables procedure that should mitigate any endogeneity bias. We present the results of both approaches in the table.

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[8] All standard errors are calculated using White's procedure to correct for heteroskedasticity.

[9] Alternatively, an endogeneity problem with regard to LIQ could arise if firms planning to increase inventories set aside the cash to do so several months in advance. It should be noted that not all possible endogeneity problems lead to an upward bias in the LIQ coefficient. For instance, if those firms that anticipate having the most severe liquidity constraints attempt to offset them by stockpiling more cash, the estimated LIQ coefficient will be pushed toward zero.

In equation 2, we add another variable to the OLS specification of equation 1. This variable is given by LIQ*B, where B is a "bond market access dummy" that takes on the value one if the firm in question has a Standard and Poors bond rating as of the beginning of the period (i.e., as of 1981:4). As noted in Table 1, roughly 14% of our 1982 sample firms have such a rating. The idea behind this interactive term is that a firm should only be bank-dependent if two conditions are satisfied: 1) it has a small amount of cash on hand; and 2) it is unable to raise money in public markets. Thus our bank-dependence hypothesis predicts a positive coefficient on the LIQ term, and a negative coefficient on the LIQ*B term. In contrast, the "endogenous LIQ" hypothesis makes no such prediction about the coefficient on the LIQ*B term. Continuing with the above example, if LIQ is simply proxying for firm profitability, one might expect that this effect would be similar for all firms, and hence that LIQ*B would have a coefficient of roughly zero.[10]

As can be seen from the table, the coefficient on LIQ*B in equation 2 is negative, and, at -.30, three quarters the magnitude of the positive coefficient on LIQ. Thus it appears that LIQ is much less important for firms with access to public debt markets, which is consistent with the bank-dependence hypothesis.

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[10] A similar logic is invoked by Fazzari, Hubbard and Petersen (1988) and Hoshi, Kashyap and Scharfstein (1991) in their analyses of fixed investment. For example, our notion that LIQ should matter more for the inventories of firms without access to public bond markets is analogous to the HKS insight that liquidity should be more important for explaining the investment behavior of Japanese firms without close ties to an industrial group. However, there is one key difference between our work and these others: we are looking for time variation in the cross-sectional coefficients on LIQ and LIQ*B. That is, we expect the coefficients to be larger in absolute magnitude during a period of tight money than at other times. In contrast, the above studies assume time-invariant coefficients. We discuss these differences in further detail below.

Equations 3 and 4 make a similar point with a slightly less constrained specification. Rather than using the LIQ*B interaction term, the equations (with just the LIQ variable) are run separately for firms with B=1 and B=0. This allows the two types of firms to have different intercepts and different sensitivities to current and lagged sales. For firms without access to public bond markets, LIQ is again positive, at .41, and strongly significant. For those with access, LIQ is actually negative at -.14, and statistically insignificant. The difference between the LIQ coefficients in the two regressions is strongly significant.

In Panel B of Table 2, we re-run equations 1-4, using (optimal) IV, rather than OLS. In each case, we use a firm' lagged value of LIQ (i.e., LIQ as of 1980:4) as an instrument for beginning-of-period LIQ. The results are very similar to those obtained with OLS. For B=0 firms, the LIQ coefficient remains at .41, while for B=1 firms it rises to -.09. The difference between these two coefficients is still statistically significant at the 5% level. Again, this is consistent with our formulation of the bank-dependence hypothesis.[11]

4.2 Economic Significance of the Results

While the LIQ coefficient of .41 for B=0 firms may be statistically significant, it is not immediately obvious whether its magnitude is economically important. For the purposes of a "back of the envelope" calculation, we return to the summary statistics in Table 1. First, the median B=0 firm in our sample cut its inventories by about 6.4% in 1982. Second, the median value of the LIQ variable for these firms is 5.4%, with a standard

[11] To be conservative, we also tried instrumenting with twice-lagged LIQ (i.e., LIQ as of 1979:4) for the 760 B=0 firms for which this was available. The estimated coefficient in this case was .36, with a t-stat of 2.02.

deviation of 13.6%. This means that for a B=0 firm, a one standard deviation change in LIQ results in an increase in inventories of 13.6% x .41 = 5.7%. Loosely speaking, if we start with a "typical" B=0 firm that is cutting its inventories by 6.4%, and then increase its cash holdings from, say 5% of assets to 19% of assets, we eliminate almost all of the inventory reduction.[12]

Although we fully appreciate the potential pitfalls inherent in drawing a precise structural interpretation from our reduced-form regressions, we nonetheless think these calculations are suggestive. Even if the coefficient used is only half the size (i.e., .2 instead of .41) the effect would still appear to be economically meaningful. Thus although the exact quantitative importance of the effect is somewhat uncertain, it is likely to be non-trivial.

4.3 Robustness to Alternative Specifications

Our specifications in Table 2 embody a number of modeling choices that could conceivably influence our results. In general, we would like to be able to control for all the fundamental firm-level determinants of inventories. (Note that any industry-wide or economy-wide factors--e.g. interest rates--will be subsumed in the constant term and the

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[12] Some care should be taken in interpreting these numbers. Even if inventories wouldn't have declined at all without a loan supply effect, this does not imply that loan supply completely "explains" the behavior of inventories in 1982. In a normal year, inventories do not stay flat--rather, they tend to grow. For example, in 1981, economy-wide inventories rose by $24.6 billion (in 1987 dollars). In 1982 there was a fall of $17.5 billion. Thus even if loan supply effects can account for a large part of the decline in 1982, they would still explain less than half of the abnormal movement relative to the previous year. A similar logic implies that the stylized fact introduced above--that reductions in inventory investment account for 87% of the GNP drop in recessions--should also be interpreted with care. Fluctuations in inventory investment are a smaller part of the abnormal movement of GNP relative to its normal (increasing) growth path.

industry dummies.) At the firm level, sales stand out as the principal driving force behind inventory behavior. Thus, one of the most important open questions is our treatment of the inventory-sales relationship.

Although we have controlled for contemporaneous and lagged changes in log sales, we have done so in a fairly unstructured manner. One could imagine appealing to a particular structural model of inventory behavior in an effort to come up with a more precise specification of the relationship between inventories and sales. For example, many models would predict that inventories would respond differentially to expected versus unexpected shocks to sales. Thus one might attempt to decompose the change in sales variable into expected and unexpected components, and allow these components to enter with different coefficients.

We take a somewhat different tack. We begin by acknowledging that not only is our current treatment of the inventory-sales relationship open to criticism, but, given the large number of alternative models, so is almost anything else we might try. Instead, we focus on a less ambitious objective. Rather than trying to come up with the "right" specification of this relationship, we try to argue that our conclusions about the coefficients on LIQ (for both B=0 and B=1 firms) are relatively insensitive to how we model the impact of sales on inventories.

We do so by trying two polar alternative specifications of the inventory-sales relationship. These are intentionally simple specifications, much cruder than those used in Table 2. The basic idea is that if these alternative specifications do not produce coefficients on LIQ markedly different than those reported in Table 2, then our conclusions are probably not very sensitive to how we control for sales.

The results are presented in Table 3. First, in Panel A, we use exactly the same IV specification seen in Panel B of Table 2, with one exception--we completely delete both the current and lagged sales terms. Two observations stand out. First, the R^2's of the regressions fall dramatically, from about .36 to .09.[13] Changes in sales are obviously extremely important for understanding changes in inventories, and our new specification is thus a much poorer one than we had before. Second, as can be seen in equations 2 and 3, the coefficient on LIQ for B=0 firms increases to .75, thereby almost doubling in absolute magnitude. (Equation 4 shows that for B=1 firms, the coefficient on LIQ is still very close to zero.) Thus omitting the sales terms leads to something of an increase in the LIQ coefficient.

In Panel B, we err in the opposite direction. Rather than excluding sales from the specification, we take as our null model the case where inventories and sales move in proportional lockstep. That is, we use the change in the log of the inventory-to-sales ratio as our dependent variable, and again exclude any sales terms from the right hand side of the equation. And not surprisingly, the coefficients on LIQ in equations 5-7 are now smaller in absolute magnitude, on the order of .30.

But even in this case, the basic conclusion seems to be the same--the coefficient on LIQ is still significantly positive for firms with B=0, and (as can be seen in equation 8) close to zero for firms with B=1. Thus even extreme misspecifications of the inventory-

[13] Note that, with IV, the R^2 is not bounded between 0 and 1. Nevertheless, it remains a useful measure of the fit of the regression.

sales relationship (in either direction) do not undo the positive correlation between inventories and LIQ for B=0 firms. This suggests that any modest improvements we might make to the specifications seen in Table 2 would probably not make much of a difference either.[14]

In addition to experimenting with alternative specifications of the inventory-sales relationship, we tried a number of other modifications, none of which had a substantive impact on the results. First, we tried adding the lagged change in the log of inventories to the right hand side of the specifications in Table 2. One could motivate this either: 1) by appealing to gradual adjustment in inventories; or 2) in a more atheoretical VAR-type framework, simply as an additional control variable. This modification has almost no effect whatsoever. The lagged inventory term always has a near-zero coefficient, and the coefficients on LIQ in our various specifications are essentially unchanged -- see Panel B of Table A-1.

Second, we tried adding a size control--the log of assets--to all the regressions in Table 2. The summary statistics in Table 1 raise the possibility that even within, say, the B=0 category, the larger firms might on average have somewhat different inventory and sales characteristics than the smaller firms. As it turns out, however, the size control makes

[14] One such modification that we did try was to instrument for the contemporaneous change in sales. This might be expected to matter if anticipated changes in sales influenced inventories differently than unanticipated changes. However, this modification had no impact at all on the LIQ coefficients -- for the B=0 sample, the coefficient drops from .41 to .40. See Panel A of Table A-1.

virtually no difference for the LIQ coefficients - see panel C of Table A-1.[15]

We also tested whether our results were dependent on the log-changes formulation employed for inventories and sales. We used logs in Table 2 because the raw percentage changes in these variables are highly skewed--several firms have extremely low values at the beginning of the period, and thus show enormous percentage changes over the period. Using logs eliminates much of this skewness, and apparently, produces a better specification. When we re-run the regressions using percentage changes instead, the R^2's are much lower, on the order of .12 rather than .36. Again, however, we reach a qualitatively similar conclusion: the LIQ coefficient is positive and statistically significant for B=0 firms, and close to zero for B=1 firms.

Finally, although our results for the entire sample of manufacturing firms look to be robust, there remains the question of whether any particular industries are driving these results. One (potentially disturbing) possibility is that the correlation between inventories and LIQ is actually not very broad-based, but rather is due to large effects in just one or two

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[15] A distinct question is whether size might be a better measure of bank dependence than whether or not a firm has a bond rating. Obviously, as shown in Table 1, large firms are much more likely to have bond ratings, so there is a good deal of overlap between the two measures. As a practical matter, both probably capture bank dependence in a noisy way, and it may be difficult to disentangle the two. Nevertheless, there are a couple of reasons why we view a bond rating as a slightly better indicator. First, it more directly addresses the notion of access to non-bank financing. Second, in the limited checking that we have done, larger firms without bond ratings behave more like smaller non-rated companies than like rated companies. For instance, even for the larger half of the B=0 sample (i.e., firms with assets above the median value of assets), the coefficient on LIQ in the basic specification (line 7, Table 2) is .27, with a t-statistic of 2.05.

industries.[16]

Table 4 investigates this question. The table documents the results of running separate equations (for B=0 firms only) for each 2-digit SIC industry for which we had more than 30 observations. Each specification is exactly identical to that in equation 7 of Table 2. Although the individual industry estimates are substantially noisier, the overall results suggest that the correlation between inventories and LIQ for B=0 firms is indeed quite broad-based. First, 11 of the 14 point estimates are positive. To put this in perspective, note that under the null hypothesis where a positive and negative estimate are equally likely, the probability of obtaining 11 or more positive values out of 14 is only 2.9%.

Furthermore, in spite of the noise, four of the positive estimates are significant at the 10% level. In contrast, none of the negative point estimates are close to being statistically significant. The median estimate is .50, and the unweighted mean is .38. The fact that these values are close to the coefficient of .41 obtained for the entire B=0 sample suggests that the results in Table 2 do indeed reflect the behavior of a "typical" industry, rather than being the consequence of one or two outlier industries.

In sum, we are left with two main conclusions from our analysis of the 1982 data. First, the IV model used in Panel B of Table 2 is probably the single most sensible one, and the parameter estimates drawn from it are likely the most reliable. Second, the finding that LIQ is a significant determinant of inventories for firms without access to public debt markets appears to be both robust and broad-based.

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[16] We also checked to see whether our results were drive by a handful of outlier firms. Omitting extreme values of the left hand side variable changes the coefficients somewhat, but does not alter any of the important conclusions.

5. Comparison of 1982 Versus Other Periods

We now turn to our analysis of other periods in the 1980's. As explained above, our aim in looking at these other periods is to have yet another set of controls against which to assess our results for B=0 firms in 1982. If the large LIQ coefficient in 1982 stems from contractionary monetary policy and an attendant reduction in bank loan supply, then we should expect to see smaller coefficients during times of easier money.

However, it should be emphasized that we also do not necessarily expect a coefficient of zero during these other periods. Even during a time of easy money, it is possible that there will still be some liquidity effects in inventory investment. These effects might be due to the usual (time-invariant) information and incentive problems that create a wedge between the costs of internal and external sources of finance. Put differently, even when bank lending is relatively "unconstrained" by monetary policy, it may still not be a perfect substitute for internal finance, because an information problem still remains between the bank and the firm. Indeed, it is exactly these sorts of time-invariant liquidity effects that have been the subject of study in the literature on fixed investment.

Thus our central hypothesis is that tight monetary policy is likely to intensify the correlation between liquidity and inventory investment for B=0 firms. Table 5 investigates this hypothesis. All the specifications in the table are exactly analogous to equation 7 of Table 2, with the one exception being that they allow for a separate intercept term for each year in the regression (i.e., the regressions contain year dummies). Equation 1 simply restates our earlier finding from 1982, namely a coefficient of .41 on LIQ.

Equation 2 focuses on our preferred easy money control period, the two years 1985-

86. In these two years, the coefficient on LIQ is actually slightly negative, although statistically insignificant. The difference between the LIQ estimates in Equations 1 and 2 is large and strongly significant. Equation 3 makes the same point with a slightly more constrained specification. In this equation, the loose and tight money periods are pooled together, and we use an interactive term, LIQ*1982, to capture intertemporal differences in the LIQ coefficient. The interactive term has a point estimate of .45, and a t-statistic of 2.44. Thus there appears to be strong support for the notion that the LIQ coefficient is higher during the tight money year 1982 than in the easy money years 1985-86.

Equations 4 and 5 repeat the same basic exercise, but use the entire seven-year period 1983-89 as our contrast to 1982. The results are qualitatively similar, though the differences between 1982 and the control period are now slightly less pronounced. The LIQ coefficient for 1983-89 is positive, at .09, but not significant. The LIQ*1982 interaction term has a point estimate of .31, with a t-statistic of 1.86. Thus 1982 stands out not only relative to 1985-86, but also relative to the entire remainder of the decade.[17,18]

These results -- that there is time-variation in the importance of liquidity constraints -- have parallels in the literature on fixed investment. Gertler and Hubbard (1988) essentially re-run the investment-cashflow equations estimated by Fazzari, Hubbard and Petersen (1988),

---

[17] We obtain similar results when we look at how the differential between the LIQ coefficients for B=0 and B=1 firms varies over time. This differential is significantly higher in 1982 than in either 1985-86 or 1983-89. This result reflects the fact that for B=1 firms, the LIQ coefficient is actually slightly lower in 1982 than in the other periods.

[18] We also tried re-estimating equation 5 of Table 5 over 1982-89 using fixed effects and Newey-West standard errors, with a balanced panel consisting of the 196 firms which passed our screens for each year. We reached a qualitatively similar conclusion: the LIQ*1982 coefficient is .68 with a t-statistic of 2.01.

but allow liquidity effects to be different in the recession years 1974, 1975, 1981 and 1982.[19] Like we do, they find significant positive coefficients on their "recession dummy". (Unlike us, however, they also find that liquidity constraints are significant for fixed investment even in non-recession years.) Similarly, using Japanese data, Hoshi, Singleton and Scharfstein (1992) re-run the equations estimated by Hoshi, Kashyap and Scharfstein (1991), and find that a large part of the HKS results are attributable to periods of restrictive policy on the part of the Japanese Central Bank.

Our results also help to explain why previous research has failed to uncover significant liquidity effects in inventory investment. Given the success of the (time-invariant) FHP and HKS specifications for fixed investment, one might naturally have expected that analogous results for inventories would soon follow. But to our knowledge, none have. The results in Table 5 make it clear why: In a typical year (e.g., a year in the period 1983-89) liquidity has a slightly positive, but not statistically significant, effect on inventories.[20] Evidently, important effects can only be found by narrowing the search to a period of tight monetary policy.

6. Conclusions: Can We Distinguish a Loan Supply Shift from a Collateral Shock?

Contrary to much previous research, our results suggest that financial factors do

[19]Note that the 1974-75 recession also appears to have involved tight monetary policy. For example, April 1974 is selected by the Romer and Romer (1990) criterion as a time when the Fed moved to significantly tighter policy.

[20] Even if we lump 1982 in with 1983-89, and estimate a single, "averaged" LIQ coefficient for the eight-year period, this coefficient is only .12.

indeed influence inventory movements. Moreover, financial constraints appear to be much more binding during a tight money/recessionary episode.

We have motivated our empirical work primarily as a cross-sectional test of the "lending view" of monetary policy transmission. Although all our results support the predictions of the lending view, it is less clear whether we can sharply differentiate between the lending view and other financial accounts of inventory movements also based on capital market imperfections. As noted in the Introduction, one plausible alternative hypothesis is that, as a consequence of the recession, there was an economy-wide decline in collateral values. This could--in the presence of information and/or incentive problems--increase the cost of bank finance, even if banks' willingness to supply loans (for a fixed amount of collateral) was unchanged.

How can one differentiate between the "loan supply shift" hypothesis and the "collateral shock" hypothesis? First, before turning to our micro data set, it should be noted that there is other macro evidence that directly supports the former. For example, as was shown in Figure 2, the tightening in monetary policy was accompanied by a widening of the spread between the prime rate and the commercial paper rate. Also, KSW find that the volume of CP issuance was rising relative to bank lending volume during this period. Both of these facts are consistent with the notion that the Fed was able to engineer an inward shift in bank loan supply.

Turning to the cross-sectional data, the collateral shock hypothesis would seem to make a clear prediction: if impaired collateral is responsible for the liquidity constraints we observe in 1982, then the LIQ coefficient should be higher in 1982 for firms with lower levels of collateralizable assets. The predictions of the loan supply shift hypothesis on this point are murkier. On the one hand, an inward shift in loan supply could take the form of banks cutting off credit most sharply for their lowest-collateral customers. On the other hand, this need not necessarily be the case.

Thus, if one were to find a cross-sectional relationship in 1982 between the level of collateral and the LIQ coefficient, this probably would not differentiate very clearly between the two hypotheses. However, if no such relationship exists, this would cast doubt on the collateral hypothesis.

Table 6 investigates the relationship (for B=0 firms in 1982) between the LIQ coefficient and a measure of collateral--the book debt-to-assets ratio, which we label DRATIO. First, in rows 1-3 of the table, we divide our sample into thirds, reflecting low, medium and high DRATIO firms. We then re-run exactly the same regressions seen in row 7 of Table 2 for each of these sub-samples. If the collateral hypothesis is correct, we should see a higher LIQ coefficient for the high DRATIO firms, since these are the most likely to be collateral-impaired.

However, the regressions reveal no support for this proposition. The high DRATIO firms have a LIQ coefficient of .25, which is almost identical to that of the low DRATIO firms. The highest coefficient is actually seen in the middle sample, although none of the differences are close to being statistically significant.

The regressions in rows 4 and 5 address the same basic question with a slightly different specification. We now estimate a single equation over the entire sample, but add an interaction term, LIQ*DRATIO. This specification effectively allows the LIQ coefficient to be a continuous linear function of a firm's value of DRATIO. In row 4, we also add DRATIO by itself as an additional right-hand-side variable, thereby allowing for the possibility that collateral might affect both the level of inventories and its sensitivity to LIQ. (In row 5, we delete the DRATIO term.)

The results in both cases echo those discussed just above. In row 4, the coefficient on LIQ*DRATIO is of the wrong sign, but small and statistically insignificant. The point estimates in this equation suggest that a firm with no debt has a sensitivity to LIQ of .52, while a firm with a debt ratio of 40% has a sensitivity to LIQ of .52 - (.37 x .40) = .37. In row 5, the coefficient on LIQ*DRATIO changes sign, but again is small and statistically insignificant. Here too, the point estimates suggest that even firms with no debt whatsoever have a sensitivity to LIQ that is very close to the full-sample value.

In sum, the evidence in Table 6 offers no positive support for the collateral hypothesis. The one caveat is that the standard errors involved (e.g., the standard error on the LIQ*DRATIO coefficient) appear to be fairly large. Thus the tests may not be very powerful in their ability to uncover such support

Our overall reading of the data--both from the cross-sectional and the aggregate perspectives--leads us to favor the loan supply shift interpretation, although we concede that there is probably not very strong evidence against the collateral story.

Although it is certainly interesting for some purposes to contrast these two hypotheses, it is also worth emphasizing their similarities. Under either interpretation, the inventory declines seen in 1982 were partially due to a cutoff in bank lending--the two hypotheses only disagree about the source of this cutoff. Thus under either interpretation, we have a financial account of the cyclical behavior of inventories, an account that differs sharply from that given in most previous work on the subject.

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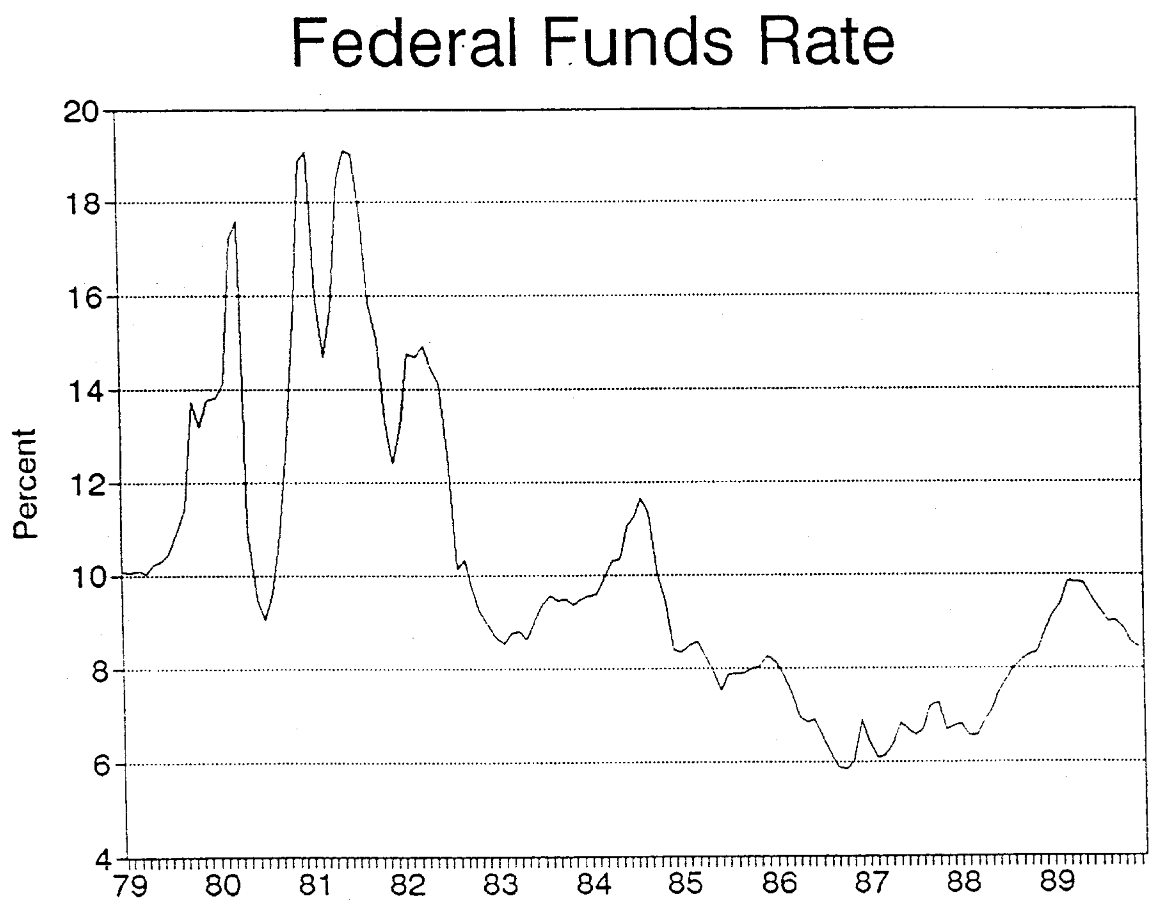
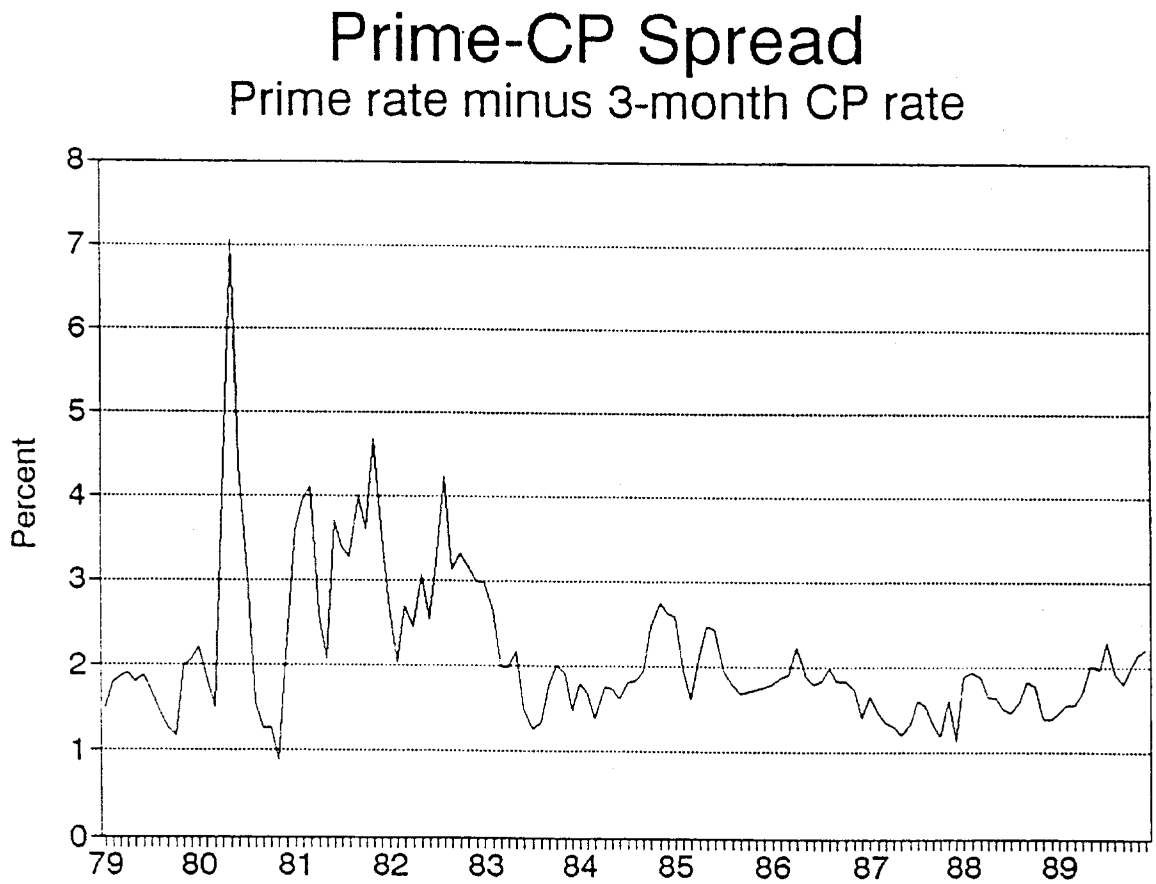
FIGURE 1



Federal Funds Rate
Percent
20
18
16
14
12
10
8
6
4
79
80
81
82
83
84
85
86
87
88
89

FIGURE 2



Prime-CP Spread
Prime rate minus 3-month CP rate
Percent
0
1
2
3
4
5
6
7
8
79
80
81
82
83
84
85
86
87
88
89

FIGURE 3

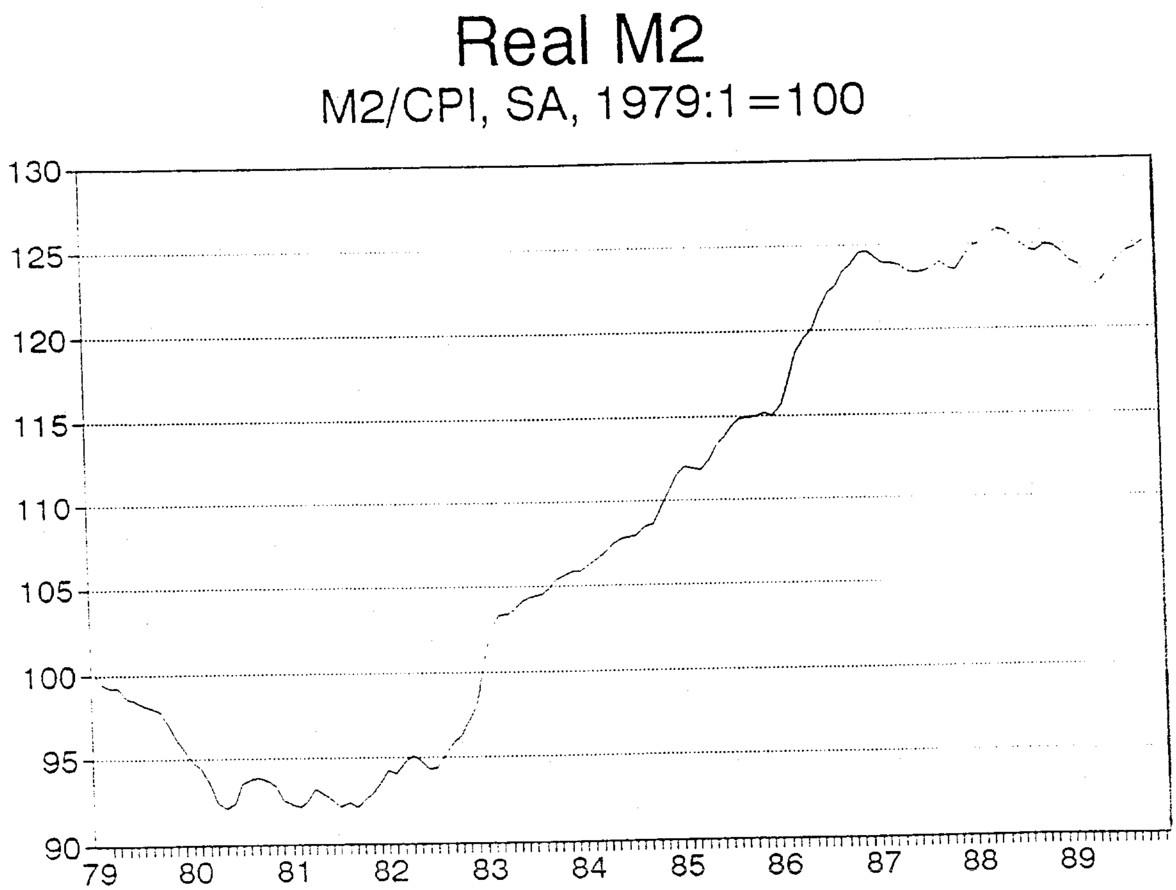
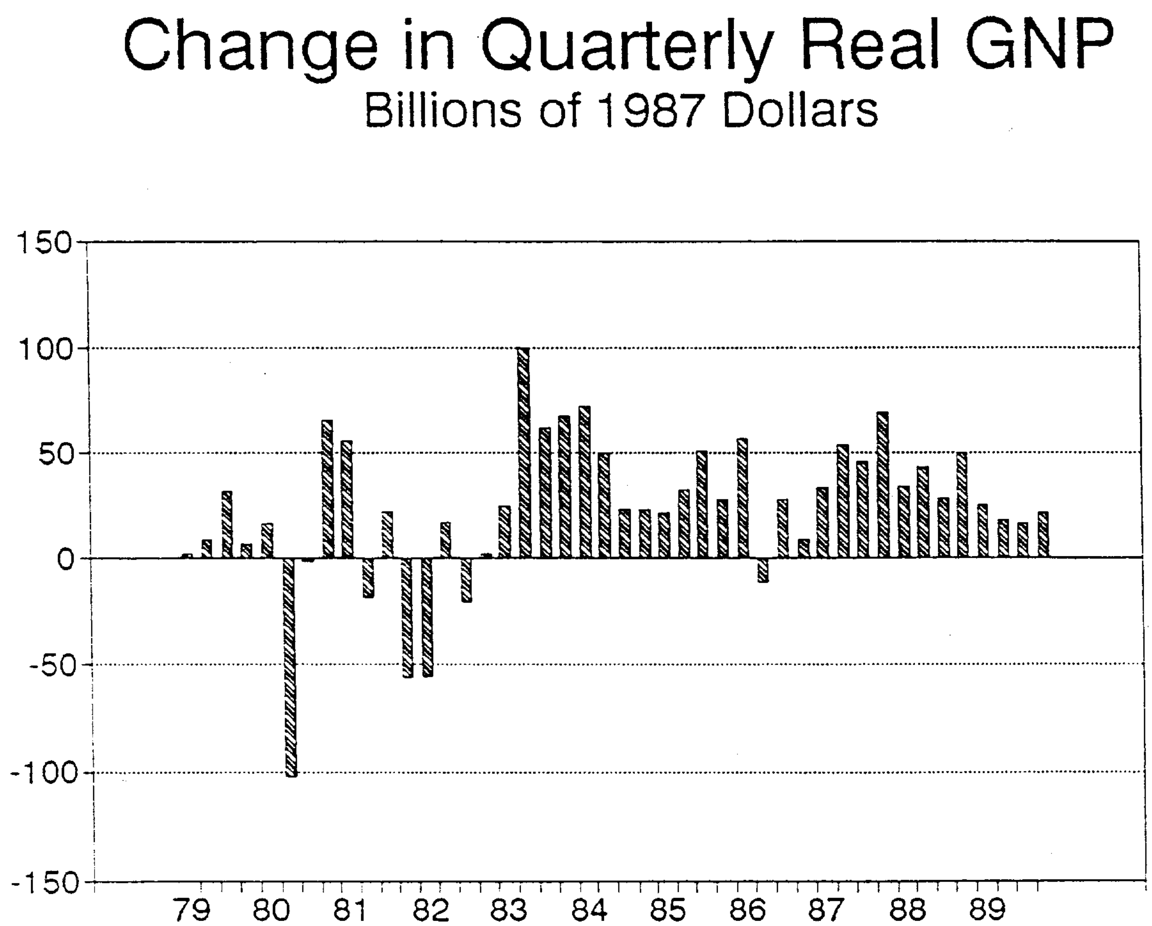

Real M2
M2/CPI, SA, 1979:1=100
130
125
120
115
110
105
100
95
90
79
80
81
82
83
84
85
86
87
88
89

FIGURE 4



Change in Quarterly Real GNP
Billions of 1987 Dollars
150
100
50
0
-50
-100
-150
79
80
81
82
83
84
85
86
87
88
89

FIGURE 5

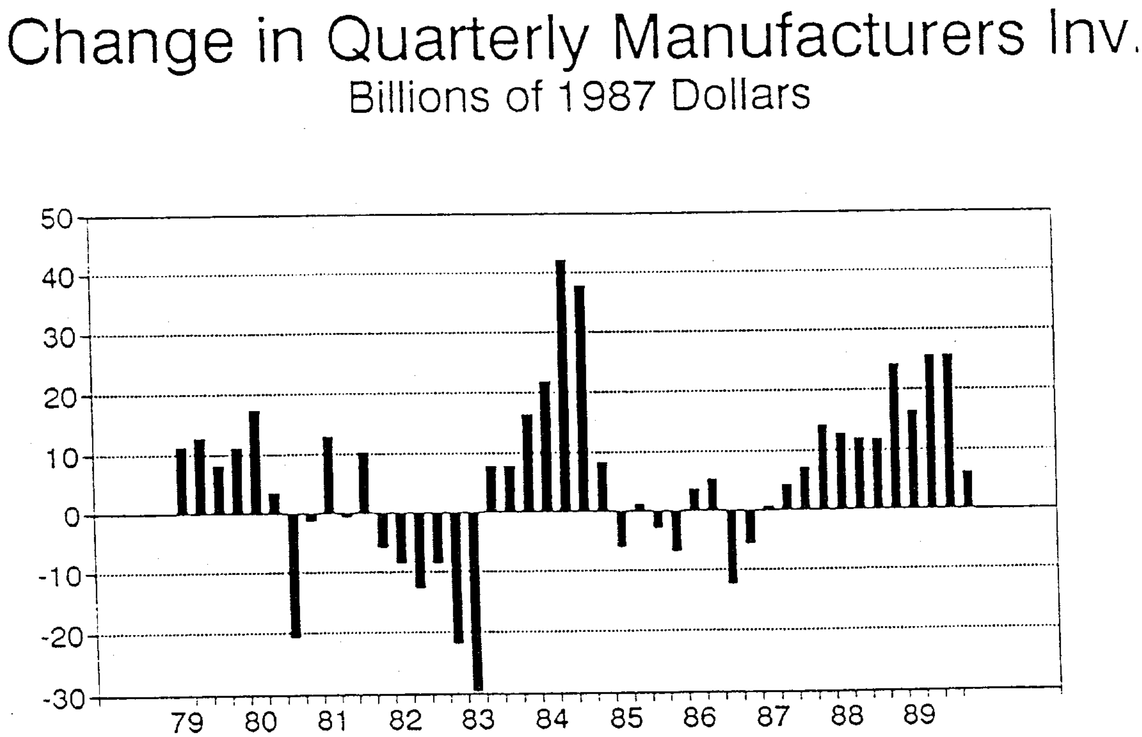

Change in Quarterly Manufacturers Inv.
Billions of 1987 Dollars
50
40
30
20
10
0
-10
-20
-30
79
80
81
82
83
84
85
86
87
88
89

Table 1

Summary Statistics for Selected Periods

	1982 Sample	1985 Sample
Number of Firms		
Compustat sample	933	841
without bond rating	802	698
with bond rating	131	143
Median Assets beginning of period		
Compustat sample	$44.2 M	$44.2 M
without bond rating	$31.8 M	$29.6 M
with bond rating	$1573.2 M	$1325.2 M
Median Liquid Assets to Total Assets		
Compustat sample	5.00%	6.84%
without bond rating	5.37%	7.65%
with bond rating	3.51%	4.65%
QFR sample average liquid assets to total assets	5.05%	3.08%

Table 1 cont.

Summary Statistics for Selected Periods

	1982 Sample	1985 Sample
Median % Change in Sales		
Compustat sample	-4.26%	0.77%
without bond rating	-3.08%	2.13%
with bond rating	-8.04%	-2.53%
QFR sample % change in sales	-5.41%	0.38%
Median % Change in Inventories		
Compustat sample	-7.11%	-1.18%
without bond rating	-6.40%	-0.31%
with bond rating	-10.52%	-8.08%
QFR sample % change in inventories	-4.12%	-1.18%

Notes:

The Compustat sample was selected following the procedure described in the text.

The "QFR" sample comes from the Quarterly Financial Report for Manufacturing Corporations.

All data are in nominal terms.

Table 2

Baseline Specifications, 1981:4 - 1982:4:

ΔLog(Inv) vs. ΔLog(Sales); ΔLog(Sales)$_{-1}$; LIQ; LIQ*B; Industry Controls*
(t-statistics in parentheses)

	Sample	ΔLog(Sales)	ΔLog(Sales)$_{-1}$	LIQ	LIQ*B	R^2	N
	A.	OLS Regressions					
1.	All Firms	.58 (7.50)	.14 (2.87)	.39 (3.55)	-	.36	933
2.	All Firms	.58 (7.42)	.14 (2.87)	.40 (3.58)	-.30 (-1.68)	.36	933
3.	B = 0	.57 (7.18)	.14 (2.88)	.41 (3.64)	-	.36	802
4.	B = 1	.76 (4.50)	-.14 (-.67)	-.14 (-.77)	-	.42	131
	B.	IV Regressions(with LIQ$_{-1}$ as instrument for LIQ)					
5.	All Firms	.58 (7.52)	.14 (2.87)	.39 (2.74)	-	.36	933
6.	All Firms	.58 (7.45)	.14 (2.87)	.40 (2.77)	-.27 (-1.45)	.36	933
7.	B = 0	.57 (7.22)	.14 (2.88)	.41 (2.75)	-	.36	802
8.	B = 1	.76 (4.49)	-.14 (-.66)	-.09 (-.43)	-	.42	131

*All regressions use White's robust errors. Industry controls are dummy variables corresponding to 2-digit SIC codes.

Table 3

Alternative Specifications of Inventory/Sales Relationship, 1981:4-1982:4
(t-statistics in parentheses; all equations estimated using IV)

	Sample	LIQ	LIQ*B	R^2	N
A.	Dependent Variable is ΔLog(Inv), but no Sales Terms on Right-Hand Side				
1.	All Firms	.71 (2.97)	-	.09	933
2.	All Firms	.72 (3.01)	-.62 (-2.56)	.09	933
3.	B = 0	.75 (2.94)	-	.09	802
4.	B = 1	-.12 (-.42)	-	.13	131
B.	Dependent Variable is ΔLog(Inv/Sales)				
5.	All Firms	.31 (2.32)	-	.04	933
6.	All Firms	.31 (2.33)	-.07 (-.41)	.04	933
7.	B = 0	.33 (2.36)	-	.04	802
8.	B = 1	-.11 (-.50)	-	.17	131

Table 4
Disaggregated Results, B=0 firms, 1981:4-1982:4
(t-statistics in parentheses; all equations estimated using I.V.)*

SIC # and Classification		LIQ	R^2	N
35	machinery, ex. electrical	0.35 (1.27)	.44	126
36	electrical and electronic equip.	0.48 (1.39)	.57	117
38	instruments and related equip.	1.07 (2.89)	.32	77
28	chemicals and allied prod.	-0.96 (-1.23)	.41	73
34	fabricated metal prod.	-0.39 (-0.53)	.19	62
37	transportation equip.	1.16 (0.91)	.15	50
33	primary metal industries	0.22 (0.71)	.21	50
30	rubber and misc. plastic prod.	0.85 (1.70)	.20	48
20	food and kindred prod.	-0.11 (-0.30)	.21	43
26	paper and allied prod.	0.06 (0.25)	.31	40
27	printing and publishing	0.79 (2.03)	.53	40
23	apparel and related prod.	0.56 (1.74)	.39	35
32	stone, clay and glass	0.69 (0.60)	.25	35
22	textile mill prod.	0.51 (0.34)	.39	34

*Specification is identical to that used in Table 2, equation 7.

Table 5
Comparison of Results for 1982 vs. Other Years
(t-statistics in parentheses, all equations estimated using I.V.)*

Sample	ΔLog(Sales)	$\Delta Log(Sales)_{-1}$	LIQ	LIQ*1982	R^2	N
1. 1982 (B=0 firms only)	.57 (7.22)	.14 (2.88)	.41 (2.75)	-	.36	802
2. 1985-86 (B=0 firms only)	.53 (5.76)	.17 (3.47)	-.02 (-.22)	-	.24	1364
3. 1982 plus 1985-86 (B=0 firms only)	.55 (7.90)	.16 (4.07)	-.04 (-.32)	.45 (2.44)	.27	2166
4. 1983-89 (B=0 firms only)	.52 (10.54)	.15 (5.19)	.09 (1.33)	-	.24	5019
5. 1982 plus 1983-89 (B=0 firms only)	.52 (11.81)	.15 (5.65)	.09 (1.34)	.31 (1.86)	.26	5821

*Specifications are analogous to those in Table 2, equation 7, except that they allow for different intercept terms for each year.

Table 6
Relationship Between LIQ Coefficient and
Debt-to-Assets Ratio
for B=0 Firms in 1982
(t-statistics in parentheses, all equations estimated using I.V.)*

Sample	ΔLog(Sales)	ΔLog(Sales)$_{t}$	LIQ	LIQ*DRATIO	DRATIO	R^2	N	Sample Median D/TC
A. Separate Regressions for Low, Medium and High DRATIO Firms								
1. Firms with DRATIO in Bottom Third of Sample	.75 (6.11)	.19 (1.35)	.26 (1.26)	-	-	.47	253	.05
2. Firms with DRATIO in Middle Third of Sample	.50 (5.15)	.17 (1.57)	.63 (1.72)	-	-	.33	254	.22
3. Firms with DRATIO in Upper Third of Sample	.55 (4.33)	.12 (2.66)	.25 (0.64)	-	-	.39	252	.42
B. Continuous Interaction between LIQ and DRATIO								
4. Full Sample	.59 (7.39)	.15 (3.05)	.52 (2.34)	-.37 (-0.31)	.12 (0.95)	.36	759	.22
5. Full Sample	.58 (7.13)	.14 (2.85)	.40 (2.50)	.26 (0.31)	-	.37	759	.22

Table A-1

Further Alternative Specifications of Inventory Equations, 1981:4-1982:4
(t-statistics in parentheses, all equations estimated using I.V.)

Sample	ΔLog(Sales)	ΔLog(Sales)$_{-1}$	LIQ	Δ Log(Inv)$_{-1}$	Log(Assets)	R^2	N
A. Instrumenting for ΔLog(Sales) with Δ Log(Inv)$_{-1}$							
1. B = 0	.62	.14	.40			.36	802
	(3.13)	(3.05)	(2.68)				
2. B = 1	.94	-.20	-.08			.40	131
	(1.71)	(0.67)	(0.38)				
B. Log(Assets) on Right Hand Side							
3. B = 0	.57	.14	.41		-.00	.36	802
	(7.24)	(2.90)	(2.67)		(.25)		
4. B = 1	.77	-.14	-.11		-.01	.42	131
	(4.65)	(0.71)	(0.55)		(.60)		
C. ΔLog(Inv)$_{-1}$ on Right-Hand Side							
5. B = 0	.57	.13	.41	.02		.36	802
	(7.47)	(2.46)	(2.76)	(0.28)			
6. B = 1	.75	-.16	-.07	.05		.42	131
	(4.72)	(0.69)	(0.31)	(0.37)			