CREDIT CONDITIONS AND THE CYCLICAL BEHAVIOR OF INVENTORIES*

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This paper examines micro data on U.S. manufacturing firms' inventory behavior during different macroeconomic episodes. Much of the analysis focuses on the 1981–1982 recession, which was apparently caused in large part by tight monetary policy. We find that the inventory investment of firms without access to public bond markets is significantly liquidity-constrained during this period. A similar pattern emerges during the 1974–1975 recession, in which tight money also appears to have played a role. In contrast, such liquidity constraints are largely absent during periods of looser monetary policy in the 1970s and 1980s.

I. INTRODUCTION

This paper is motivated by three stylized facts. The first fact is that inventory movements play a major 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 percent 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 that is transmitted through a cost-of-financing channel.

There is only one problem with this story. Its basic premise—

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that inventories are sensitive to financing conditions—finds scant support in most empirical work. This is our third stylized fact. As Blinder and Maccini [1991, p. 82] 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 require external financing but 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 [1993] (hereinafter KSW) 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 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 have inventory behavior that is 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 "bank 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 have either plenty of internal funds or access to public debt markets and therefore do not need to rely on bank financing.

The goal of this paper is to provide an empirical test of this cross-sectional hypothesis. We begin by examining firm-level inventory movements during the calendar year 1982. We focus on this year because it roughly encompasses the five-quarter-long recession of 1981:3–1982:4. This recession was preceded by a clear tightening of monetary policy, beginning with the Fed's change in operating procedures in October 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.

Our results from the 1982 data confirm the predictions of the monetary policy/bank lending account of inventory movements. We find that the inventory investment of firms without access to public debt markets is significantly liquidity-constrained. Or said somewhat differently, firms that are bank-dependent—in the sense of having *neither* bond market access nor large internal cash reserves—do indeed cut their inventories by significantly more during this period than do their nonbank-dependent counterparts. This conclusion appears to be robust to a wide range of variation in econometric specification and estimation technique.

While the 1982 results are consistent with the lending view,

1 Early work on the distinction between the "money" and "lending" channels of monetary policy transmission includes Modigliani [1963], Tobin and Brainard [1963], Brainard [1964], and Brunner and Meltzer [1964]. More recent contributions have come from Bernanke and Blinder [1988, 1992], King [1986], and Romer and Romer [1990], among others. See Kashyap and Stein [1994] for a survey of much of this work.

they leave other possibilities open as well First, it may be that the liquidity constraints that we document in 1982 are always present, so that there is nothing unique happening in this year Second, even if the liquidity constraints observed in 1982 are out of the ordinary, they might be attributable to other consequences of the recession, rather than specifically to tight monetary policy A leading alternative hypothesis is that a recession impairs the value of firms' collateral In a world of information or moral hazard problems, such a "collateral shock" could increase the costs of external finance, even if banks' willingness to supply loans (for a *fixed* amount of collateral) was unchanged [2] Thus, the 1982 results do not by themselves prove that the Fed was able to engineer an inward shift in loan supply

Clearly, the lending story and the collateral story are closely related Both involve capital market imperfections, and both attribute inventory movements in 1982 to a "cutoff" in the flow of bank credit The difference is that 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? Ideally, we would like to have data for at least two more recessions one that was clearly related to tight monetary policy, and one that had nothing to do with tight monetary policy (i e , was caused instead by a supply shock) If we found evidence of liquidity constraints in inventory behavior during the former but not the latter, the lending story would be on firmer ground relative to the collateral story

Unfortunately, aside from the 1982 episode, there are no other such clear-cut natural experiments As Eckstein and Sinai [1986] suggest, it is going to be especially difficult to come up with an example of a purely nonmonetary recession Overall, the best we can do is to examine data from both 1974 (a close approximation to the span of the five-quarter-long recession of 1973 4–1975 1, and the 1985–1986 "slowdown " As we shall discuss in detail below, the former is a somewhat ambiguous example of another monetary-related recession, while the latter appears to be a period of economic weakness (though not an official recession) that was accompanied by *easy* monetary policy And interestingly, liquidity constraints are once again significant in 1974, but completely absent in 1985–1986 Moreover, liquidity constraints are also

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

generally very small in the other years between 1975–1989. Thus, the monetary-related recessions of 1974 and 1982 stand out not only relative to 1985–1986, but relative to the rest of the 1970s and 1980s as well. We take this to be good news for the lending view, although as we emphasize below, it is hard to argue that our evidence decisively rejects the collateral hypothesis.

The remainder of the paper is organized as follows. Section II provides some background macroeconomic facts in an effort to better motivate both the selection of 1982 as our primary focus of study, and our use of the periods 1974 and 1985–1986 as (admittedly imperfect) comparison episodes. Section III describes our sample and data. In Section IV we develop and estimate our baseline inventory specifications for 1982 and then perform a number of robustness tests. In Section V we perform the intertemporal comparisons, reestimating our baseline model over the 1974 and 1985–1986 periods, as well as over the entire 1974–1989 interval. Section VI concludes.

II. Background Macroeconomic Facts

A. The 1981–1982 Recession

As noted above, our basic empirical strategy is to start by concentrating 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.

The 1981–1982 recession would appear ideal for our purposes. In recent history it stands as the best American example of Friedman's [1983, p. 202] well-known observation that ". . . no country has cured substantial inflation without going through a transitional period of slow growth and high unemployment." Indeed, Dornbusch and Fischer [1990, p. 511] 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."

Figure I provides some indicators of the stance of monetary policy over the period 1972–1989. Panel A of the figure plots the Federal funds rate, which Bernanke and Blinder [1992], Goodfriend [1993], and many others believe is a good indicator of the stance of monetary policy. At the time Paul Volcker became Fed

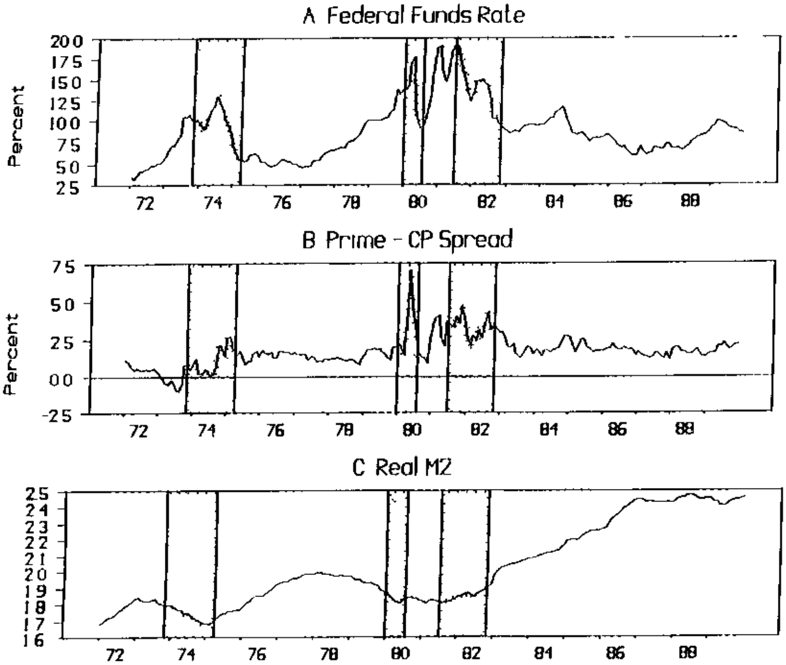

FIGURE I
Monetary Policy Indicators, 1972–1989 *Note* Shaded areas denote NBER recessions

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 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

Panel B of Figure I tells a similar story using another interest-rate-based measure of monetary policy, the spread between the prime rate and the commercial paper (CP) rate KSW document that this spread typically rises in the wake of a monetary contraction, and interpret this pattern as evidence that when the Fed tightens, the cost of bank financing increases relative to the cost of open market financing 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 the 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

Panel C of Figure I examines a quantity-based indicator, the level of real M2 [3] 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 1981 Indeed, real M2 did not regain its early 1979 levels until the end of 1982

Figure II displays the associated movements in output and inventories As Panel A shows, real GDP growth was negative in the fourth quarter of 1981 and in three of the next five quarters Panel B 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 percent of the output decline This percentage almost exactly matches their 87 percent average for postwar U S recessions

B The 1974–1975 Recession

As noted in the Introduction, it would be desirable if we could replicate our 1982 analysis for another recession in which tight monetary policy also played a clear-cut role Unfortunately, there does not appear to be another natural experiment quite as clean as

3 Given that it is likely to contain a large endogenous component, one must of course be careful in interpreting M2 as an indicator of the stance of monetary policy

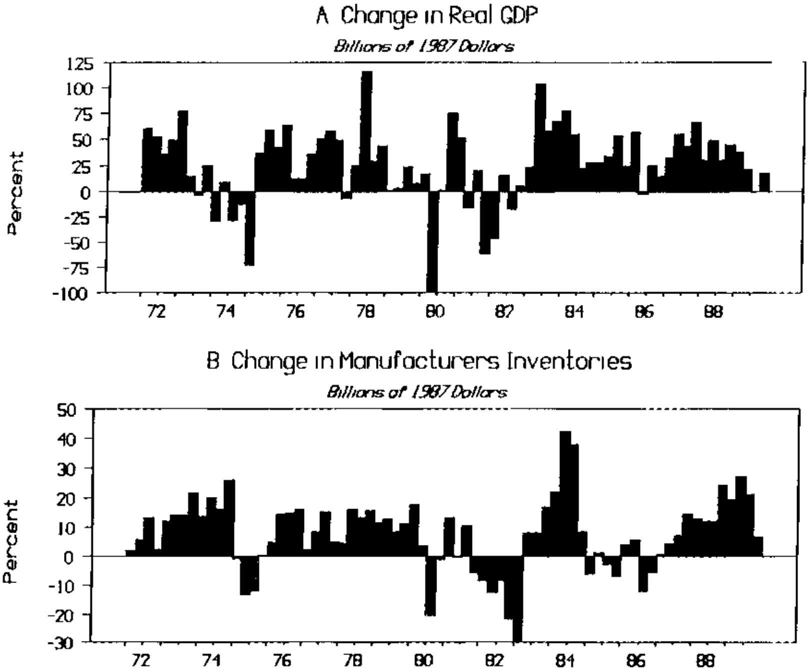

FIGURE II
Movements in GDP and Inventories, 1972–1989

1982 Our next best candidate is the recession of 1974–1975, which according to the NBER, lasted through all four quarters of 1974 and the first quarter of 1975 On the one hand, there seems to be considerable evidence of tight monetary policy leading up to this recession On the other hand, textbook descriptions of the period (e g , Dornbusch and Fischer [1990, pp 495–96]) also attribute the downturn in part to the 1973–1974 oil shock

One piece of evidence for the role of tight monetary policy comes from Romer and Romer [1990] Their reading of FOMC minutes selects April 1974 as a date when the Fed moved to significantly tighter policy in an effort to combat inflation Our other indicators confirm this view The funds rate began the calendar year 1973 at just over 5 percent, and then started a dramatic climb, peaking at almost 13 percent in July 1974 Real M2 declined during the latter part of 1973 and throughout 1974 In addition, there was a pronounced increase in the prime-CP spread, although this spread did not reach the very high levels that were seen in the 1981–1982 episode

As in the 1981–1982 recession, inventory cuts were again substantial, with three consecutive quarters of reductions in manufacturers' real stocks The only slight difference from 1981–1982 is the timing of these cuts in the 1974–1975 recession the inventory cuts did not begin until the last quarter of the recession and continued for two quarters after the economy as a whole bottomed out

C The 1985–1986 "Slowdown"

Ideally, we also would like to contrast our results from 1982 with those from a recession that was clearly unrelated to tight monetary policy Unfortunately, no such episode exists in the period over which our data are available The closest we can come to this ideal is to examine the two-year period 1985–1986 As we shall argue below, this seems to have been a period of quite easy monetary policy And, moreover, while it was not classified as an official NBER recession, it was also a time of notable economic weakness

As far as the stance of monetary policy goes, we view 1985–1986 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 1985–1986 period all the indicators from Figure I 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 "[4]

In terms of economic weakness, the brunt of the slowdown seems to have been borne by the manufacturing sector, which was hard hit by the high foreign exchange value of the dollar Paul

4 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 "

Volcker's testimony before Congress in July 1985 cites "marked sluggishness in the goods-producing sector" [July 17, 1985, p. 690]. More than a year later, in September 1986, *Business Week,* noting that industrial production had fallen in five of the previous six months, asserted that "the industrial sector is virtually in a recession" [September 1, 1986, p. 21]. Perhaps most interesting for our purposes, there were significant inventory cuts during this period. As can be seen in Figure II, manufacturers reduced their real stocks during five of eight quarters in 1985–1986.

III. 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 nontraded 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 firm-level data for nontraded 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 2328 U. S. manufacturing companies that had complete (i.e., nonzero and nonmissing) data on assets, sales, inventories, and cash holdings for the fiscal years ending in 1980, 1981, and 1982. We then eliminate the roughly 30 percent 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 percent of the remaining sample) whose fiscal years end in the fourth quarter, i.e., in either October, November, or December.[5] We do this for two reasons. First, this time period roughly lines up with the recession. Second, by using companies that have similar fiscal years, we ensure that all the firms in our

5. We also examined a sample of companies whose fiscal years ended in the 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.

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 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 [6]

Table I 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 subsamples, 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 subsamples, the table also includes some analogous information from the *Quarterly Financial Report for Manufacturing Corporations,* which covers virtually all 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 This phenomenon appears to be part of a long-run pattern and is not confined to the two years shown in the table Over the entire 1974–1989 period that we consider, the smaller nonrated firms had sales growth that was on average 1 6 percent faster than that of the larger, rated firms

In terms of the lending view, one suggestive (albeit very crude) comparison can be made using the numbers in Table I Firms *without* bond ratings had inventory growth that was 6 1 percent

6 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

TABLE I
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 (nominal $)		
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%
Median % change in sales (real $)		
Compustat sample	−8.06%	−3.03%
without bond rating	−6.88%	−1.67%
with bond rating	−11.84%	−6.33%
QFR sample % change in sales	−9.21%	−3.42%
Median % change in inventories (real $)		
Compustat sample	−10.91%	−4.98%
without bond rating	−10.20%	4.11%
with bond rating	−14.32%	−11.88%
QFR sample % change in inventories	−7.92%	−4.98%

Notes to table. The Compustat sample was selected following the procedure described in the text. The QFR sample comes from the *Quarterly Financial Report for Manufacturing Corporations.* Real sales and inventory changes were calculated using the consumer price index.

greater in 1985 than in 1982. For firms *with* bond ratings, the comparable 1985–1982 inventory growth differential is only 2.4 percent. 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.

IV. FIRM-LEVEL 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 using both ordinary least squares (OLS) and instrumental variables (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

A Baseline Specifications

Table II 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 year The right-hand-side variables include a constant term, the log of the inventory-sales ratio at the beginning of the year, the change in the log of firm sales over both the current and preceding years, as well as nineteen dummy

TABLE II

BASELINE SPECIFICATIONS, 1981 4–1982 4 ΔLOG(INV) VERSUS LOG(INV/SALES) ΔLOG(SALES), ΔLOG(SALES)$_{-1}$, LIQ, LIQ*B, INDUSTRY CONTROLS*

(T-STATISTICS IN PARENTHESES)

SAMPLE	LOG (INV/SALES)	ΔLOG (SALES)	ΔLOG (SALES)$_{-1}$	LIQ	LIQ*B	R^2	N
A OLS regressions							
1 All firms	−0 01	0 59	0 13	0 38	—	0 36	933
	(−0 45)	(7 88)	(2 57)	(3 52)			
2 All firms	−0 01	0 58	0 13	0 39	−0 31	0 36	933
	(−0 45)	(7 81)	(2 56)	(3 57)	(−1 69)		
3 $B = 0$	−0 01	0 58	0 14	0 41	—	0 36	802
	(−0 36)	(7 56)	(2 58)	(3 63)			
4 $B = 1$	−0 11	0 76	−0 18	−0 28	—	0 46	131
	(−2 80)	(4 90)	(−0 82)	(−1 62)			
B IV regressions (with LIQ$_{-1}$ as instrument for LIQ)							
5 All firms	−0 01	0 58	0 13	0 39	—	0 36	933
	(−0 43)	(7 85)	(2 57)	(2 67)			
6 All firms	−0 01	0 58	0 13	0 39	−0 28	0 36	933
	(−0 44)	(7 79)	(2 56)	(2 70)	(−1 45)		
7 $B = 0$	−0 01	0 58	0 14	0 41	—	0 36	802
	(−0 34)	(7 55)	(2 59)	(2 70)			
8 $B = 1$	−0 11	0 76	−0 18	−0 21	—	0 46	131
	(−2 75)	(4 88)	(−0 81)	(−1 06)			

*All regressions use White s robust errors Industry controls are dummy variables corresponding to two digit SIC codes variables corresponding to two-digit SIC codes These variables are intended to control for the nonfinancial determinants of inventories In particular, the start-of-period inventory-sales ratio and the change-in-log-sales terms can be loosely motivated by a target adjustment model of the sort seen in Lovell [1961] [7]

Row 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 0 38 and a *t*-statistic of 3 52 [8]

While this result is consistent with the notion that liquidity constraints are important for inventory behavior, 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, a nonfinancial explanation might be that LIQ is a proxy for innovations in firm profitability, i e , 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 liquidity-constrained [9]

There are two basic ways that one can address this ambiguity The first approach involves using a priori theoretical arguments to 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 Table II, and in what follows

In row 2 we add another variable to the OLS specification of

7 Alternatively, this type of specification could be motivated by appealing to a cost-minimization model that assumes that firms face quadratic costs of producing output and of deviating from a target inventory-sales ratio For instance, Kashyap and Wilcox [1993] show that this sort of setup gives rise to an error-correction equation for inventories that is similar to ours

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

row 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 I, roughly 14 percent 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. (That is, the net effect of internal liquidity for firms with bond ratings—which is given by the sum of the LIQ and LIQ*B coefficients—should be smaller.) 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 row 2 is negative and, at -0.31, is more than three quarters the magnitude of the positive coefficient on LIQ. Thus, it appears that internal liquidity is much less important for firms with access to public debt markets, which is consistent with the bank-dependence hypothesis.

Rows 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 the nonfinancial variables. For firms without access to public bond markets, LIQ is again positive, at 0.41, and strongly significant. For those with access, LIQ is actually negative at -0.28, though statistically

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 insight of Hoshi, Kashyap, and Scharfstein 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 shall also be looking for *time variation* in the LIQ coefficient for firms without bond market access. That is, we expect this coefficient to be larger during periods of tight money than at other times. In contrast, the above studies assume time-invariant coefficients. We discuss these differences in further detail below.

insignificant.[11] The *difference* between the LIQ coefficients in the two regressions is strongly significant.

In Panel B of Table II we rerun the specifications in rows 1–4, using (optimal) IV, rather than OLS. In each case, we use a firm's lagged value of LIQ (i.e., LIQ as of 1980:4) as an instrument for beginning-of-period LIQ. This instrumenting procedure should mitigate any problems that arise from LIQ proxying for recent innovations in profitability. Of course, even lagged values of LIQ may contain some information about the permanent component of firm profitability. But this should pose less of a problem, given the rest of our specification. Recall that we are essentially seeking to explain *changes* in the stock of inventories relative to sales. While it seems plausible that a secularly more profitable firm might want to maintain a higher *level* of the inventory-to-sales ratio than a less profitable firm, it is harder to imagine why such a firm would want to keep *growing* its inventories faster relative to sales on a year-in, year-out basis.

These arguments notwithstanding, we recognize that there may be situations in which there remains some residual endogeneity basis. But it must be emphasized that the use of IV is but one of three lines of defense against the endogeneity problem. The other two, which both derive from our a priori theoretical arguments, are the comparison between $B = 0$ and $B = 1$ firms seen just above, and the intertemporal comparisons that will be performed in Section V below. Even if residual endogeneity problems could explain the presence of a positive LIQ coefficient for $B = 0$ firms in 1982, it is not at all clear how they could explain the sort of systematic differences in this coefficient both across types of firms and across time periods that are predicted by our theory. Thus, while we regard the use of IV as helpful, we are not resting our entire case on the purity of our instruments.

The results in Panel B of Table II are very similar to those obtained with OLS. For $B = 0$ firms, the LIQ coefficient remains at 0.41, while for $B = 1$ firms it rises slightly to -0.21. The difference between these two coefficients is still statistically significant at the 5 percent level. Again, this is consistent with our formulation of the bank-dependence hypothesis.[12]

11. As might be expected, the much smaller sample size for the $B = 1$ firms leads to a LIQ coefficient that is less precisely estimated than that for the $B = 0$ firms.

12. 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 0.36, with a t-statistic of 1.93.

B. Economic Significance of the Results

While the LIQ coefficient of 0.41 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 I. First, the median $B = 0$ firm in our sample cut its real inventories by about 10.2 percent in 1982. Second, the median value of the LIQ variable for these firms is 5.4 percent, with a standard deviation of 13.6 percent. This means that for a $B = 0$ firm, a one-standard-deviation change in LIQ results in an increase in inventories of 13.6 percent $\times$ 0.41 = 5.7 percent. Loosely speaking, if we start with a "typical" $B = 0$ firm that is cutting its inventories by 10.2 percent, and then increase its cash holdings from, say 5 percent of assets to 19 percent of assets, we eliminate more than half of the inventory reduction.[13]

Although we fully appreciate the potential pitfalls inherent in drawing a precise structural interpretation from our reduced-form regressions, we nonetheless think that these calculations are suggestive. Even if the coefficient used is only half the size (i.e., 0.2 instead of 0.41), 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 nontrivial.

C. Robustness to Alternative Specifications

Our specifications in Table II 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 industrywide or economywide factors—e.g., interest rates—will be subsumed in the constant term and the 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.

13. Some care should be taken in interpreting these numbers. Even if inventories would not 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 economywide 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 percent 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.

Although we have controlled for the start-of-period inventory-sales ratio, as well as contemporaneous and lagged changes in 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 inventory and sales.

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 single "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 a variety of alternative specifications of the inventory-sales relationship. The results are presented in Table III. First, in Panel A we use exactly the same IV specification seen in Panel B of Table II, with one exception: we delete the start-of-period inventory-to-sales term. This term was actually insignificant in many of the regressions of Table II, so one might question whether it belongs in the specification. We chose to keep it in our baseline model because (1) there are theoretical reasons to believe it should matter, and (2) as will be seen in Section V below, it does in fact enter significantly, (with the predicted negative sign) in all the other years we consider. As can be seen from Panel A of Table III, however, the exclusion of the inventory-sales ratio has absolutely no effect on our results. In particular, the LIQ coefficient for $B = 0$ firms remains exactly the same, at 0.41.

In Panel B we modify the specification in a more substantial way. 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 exclude all sales terms from the right-hand side of the equation. This is roughly equivalent to constraining the contemporaneous change in sales term to have a coefficient of unity. Although our earlier results suggest that this is probably a very poor specification of the nonfinancial aspects of inventory behavior, it has little effect on the LIQ coefficients. For $B = 0$ firms the coefficient is now 0.33 and still significant, while for $B = 1$ firms it is still slightly negative and completely insignificant.

In Panel C we try instrumenting not only for LIQ, but also for

TABLE III
ALTERNATIVE SPECIFICATIONS OF INVENTORY EQUATIONS, 1981 4–1982 4
(t-STATISTICS IN PARENTHESES, ALL EQUATIONS ESTIMATED USING IV)

SAMPLE	LOG (INV/SALES)	ΔLOG (SALES)	ΔLOG $(\text{SALES})_{-1}$	LIQ	ΔLOG $(\text{INV})_{-1}$	LOG (ASSETS)	R^2	N
A No log(inv/sales) on right-hand side								
1 $B = 0$	--	0 57	0 14	0 41	—	—	0 36	802
		(7 22)	(2 88)	(2 75)				
2 $B = 1$	---	0 76	−0 14	−0 09	—	—	0 42	131
		(4 49)	(-0 66)	(−0 43)				
B Dependent variable is Δlog(Inv/Sales)								
3 $B = 0$	—	—	—	0 33	—	—	0 04	802
				(2 36)				
4 $B = 1$	—	—	—	−0 11	—	—	0 17	131
				(−0 50)				
C Instrumenting for ΔLog(Sales) with $\Delta\text{Log(Inv)}_{-1}$								
5 $B = 0$	−0 02	0 67	0 13	0 37	—	—	0 35	802
	(−0 70)	(3 03)	(2 59)	(2 42)				
6 $B = 1$	−0 12	1 23	−0 34	−0 19	—	—	0 34	131
	(−2 58)	(2 52)	(−1 05)	(−0 90)				
D $\Delta\text{Log(Inv)}_{-1}$ on right-hand side								
7 $B = 0$	−0 02	0 57	0 12	0 40	0 03	—	0 36	802
	(−0 54)	(7 62)	(2 14)	(2 65)	(0 47)			
8 $B = 1$	−0 12	0 74	−0 25	−0 17	0 13	—	0 46	131
	(−2 75)	(5 23)	(−1 01)	(−0 83)	(0 97)			
E Log(Assets) on right-hand side								
9 $B = 0$	−0 01	0 58	0 14	0 41	—	−0 00	0 36	802
	(−0 34)	(7 57)	(2 61)	(2 61)		(−0 25)		
10 $B = 1$	−0 11	0 78	−0 19	−0 24	—	−0 01	0 46	131
	(- 2 80)	(5 05)	(−0 86)	(−1 22)		(0 75)		

the contemporaneous change in sales This might be expected to matter if, as many theories imply, anticipated changes in sales influenced inventories differently than unanticipated changes However, this modification has a minimal impact on the LIQ coefficients For the $B = 0$ sample, the coefficient drops from 0 41 to 0 37

In Panel D we add the *lagged* change in the log of inventories to the right-hand side of the baseline specification from Table II One could motivate this either by appealing to gradual adjustment in inventories or in a more atheoretical VAR-type framework, simply as an additional control variable This variation again has almost no effect whatsoever The lagged inventory term itself has a near-zero coefficient, and the coefficients on LIQ are essentially unchanged

In Panel E we perform a somewhat different sort of robustness check We try adding a size control, the log of assets, to our baseline specification The summary statistics in Table I 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 absolutely no difference for our results Indeed, the LIQ coefficient for $B = 0$ firms is exactly the same as it was in Table II, at 0 41 [14]

In other regressions not displayed in the table, we also tested whether our results were dependent on the log-changes formulation employed for inventories and sales We used logs in Table II 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 rerun the regressions using percentage changes instead, the R^2's are much lower, on the order of 0 12 rather than 0 36 Again, however, we reach a qualitatively similar conclusion the LIQ coefficient is positive and statistically significant for $B = 0$ firms and is close to zero for $B = 1$ firms

Finally, at the suggestion of a referee, we also checked to see whether our results were affected by firms that changed their inventory accounting procedures between FIFO and LIFO They

14 A distinct question is whether size might be a better measure of public market access than whether or not a firm has a bond rating Obviously, as shown in Table I, 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 market access 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 nonbank financing Second, in the limited checking that we have done, larger firms *without* bond ratings seem to behave more like smaller nonrated 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 II) is 0 24, with a t-statistic of 1 82

were not. For example, we were able to clearly identify the 1980–1982 accounting procedures for 443 of our 802 $B = 0$ firms. For these 443 firms we reran our baseline specification, obtaining a LIQ coefficient of 0.43. We then deleted from this subsample the 31 firms (roughly 7 percent) that switched accounting procedures any time between 1980 and 1982. For the remaining 412 firms we estimated a LIQ coefficient of 0.46.

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 industries.[15]

Table IV investigates this question. The table documents the results of running separate equations (for $B = 0$ firms only) for each two-digit SIC industry for which we had more than 30 observations. Each specification is exactly identical to that in row 7 of Table II. 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, ten of the fourteen point estimates are positive. To put this in perspective, note that under the null hypothesis where positive and negative estimates are equally likely, the probability of obtaining ten or more positive values out of fourteen is only 9.0 percent.

Furthermore, in spite of the noise three of the positive estimates are significant at the 10 percent level. In contrast, none of the negative point estimates are close to being statistically significant. The median estimate is 0.50, and the unweighted mean is 0.37. The fact that these values are close to the coefficient of 0.41 obtained for the entire $B = 0$ sample suggests that the results in Table II 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 II 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

15. We also checked to see whether our results were driven 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.

TABLE IV
DISAGGREGATED RESULTS, $B = 0$ FIRMS, 1981:4–1982:4
(t-STATISTICS IN PARENTHESES, ALL EQUATIONS ESTIMATED USING IV)*

SIC #	and classification	LIQ	R^2	N
35	Machinery, ex. electrical	0.33 (1.25)	0.45	126
36	Electrical and electronic equip.	0.45 (1.26)	0.57	117
38	Instruments and related equip.	1.07 (2.89)	0.32	77
28	Chemicals and allied prod.	−0.81 (−1.18)	0.48	73
34	Fabricated metal prod.	−0.37 (−0.50)	0.29	62
37	Transportation equip.	1.11 (0.79)	0.16	50
33	Primary metal industries	0.24 (0.77)	0.22	50
30	Rubber and misc. plastic prod.	0.54 (1.18)	0.26	48
20	Food and kindred prod.	−0.11 (−0.30)	0.22	43
26	Paper and allied prod.	−0.07 (−0.36)	0.42	40
27	Printing and publishing	0.84 (1.94)	0.56	40
23	Apparel and related prod.	0.56 (1.77)	0.43	35
32	Stone, clay, and glass	0.70 (0.59)	0.25	35
22	Textile mill prod.	0.74 (0.60)	0.42	34

*Specification is identical to that used in Table II, row 7.

firms without access to public debt markets appears to be both robust and broad-based.

V. COMPARISON OF 1982 VERSUS OTHER PERIODS

We now turn to our analysis of other time periods in the 1970s and 1980s. 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 (1) a substantial coefficient for 1974 as well, to the extent that this recession also involved tight monetary policy, and (2) smaller coefficients during the 1985–1986 slowdown as well as during other times of easier money

However, it should be emphasized that we also do not necessarily expect a coefficient of *zero* during these easy money 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 V investigates this hypothesis All the specifications in the table are exactly analogous to that in

TABLE V
COMPARISON OF RESULTS FOR 1982 VERSUS OTHER YEARS
(t-STATISTICS IN PARENTHESES, ALL EQUATIONS ESTIMATED USING IV)*

SAMPLE	LOG (INV/SALES)	ΔLOG (SALES)	ΔLOG $(\text{SALES})_{-1}$	LIQ	LIQ*CCR	R^2	N
1 1982	−0 01	0 58	0 14	0 41	—	0 36	802
($B = 0$ firms only)	(−0 34)	(7 55)	(2 57)	(2 70)			
2 1974	−0 11	0 58	0 00	0 71	—	0 32	877
($B = 0$ firms only)	(−4 43)	(10 62)	(0 07)	(3 89)			
3 1985–1986	−0 20	0 57	0 14	-0 02	—	0 34	1364
($B = 0$ firms only)	(−6 64)	(6 69)	(3 02)	(−0 18)			
4 1974–1989	−0 14	0 61	0 09	0 07	0 36	0 32	13,203
($B = 0$ firms only)	(−14 01)	(19 34)	(4 04)	(1 42)	(2 78)		

*Specifications are analogous to those in Table II row 7 except that they allow for different intercept terms for each year CCR = 1 for 1982 and 1974 0 otherwise row 7 of Table II, with the one exception being that, when more than one year is included, they allow for a separate intercept term for each year in the regression (i.e., the regressions contain year dummies). Row 1 simply restates our earlier finding from 1982, namely a coefficient of 0.41 on LIQ.

Row 2 shows that in 1974, liquidity constraints were once again pronounced for $B = 0$ firms. The coefficient on LIQ is 0.71, and it is highly significant, with a t-statistic of 3.89.[16] This fits with the interpretation of 1974 as being a tight-money-induced recession year.

Row 3 focuses on our easy money control period, the two years 1985–1986. In these two years the coefficient on LIQ is actually slightly negative, although statistically insignificant. Moreover, the LIQ coefficient in 1985–1986 is significantly different from both that in 1982 and that in 1974. This lends further support to the bank lending account of inventory movements.

Not only do the coefficients from 1974 and 1982 stand out relative to 1985–1986, they stand out relative to the other years in the sample as well. This is illustrated in row 4. Here we combine all the years between 1974 and 1989, and estimate one giant regression for $B = 0$ firms. The regression contains a LIQ variable, as well as a LIQ*CCR interaction term. Here CCR is a dummy variable that takes on the value one in the "credit crunch/recession" years 1974 and 1982, and zero in the other fourteen years. The intuition is that the LIQ coefficient in this specification will capture the "average" degree of liquidity constraint, while the LIQ*CCR coefficient will capture the added effect seen in a tight money/recession year.

The results in row 4 are striking. The unconditional LIQ coefficient is only 0.07, with a t-statistic of just 1.40. However, the LIQ*CCR term has a coefficient of 0.36, with a t-statistic of 2.79. Thus, while liquidity constraints are substantial in 1974 and 1982, they are almost completely absent in other years.[17]

16. Prior to 1982 we were unable to obtain bond rating data from Compustat. Rather than laboriously collecting the data by hand, we approximated the $B = 0$ subsample in each of the earlier years as the smallest 85 percent of the sample by assets. (Recall that in 1982 firms without bond ratings comprised approximately 86 percent of the sample.) For the years in which we could do it either way—i.e., 1982 and later—using size in this way rather than bond rating to split the sample yielded virtually identical results for the "$B = 0$" category.

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 averages -0.03 over the entire 1974–1989 period, but reaches 0.36 in 1974 and (as seen earlier) 0.62 in 1982. The differential is also slightly negative in 1985 and 1986. We also tried reestimating a version of equation 4 of Table V over the shorter period

These findings that there is time-variation in the importance of liquidity constraints have parallels in the literature on fixed investment. Gertler and Hubbard [1988] essentially rerun the original investment-cash-flow equations estimated by Fazzari, Hubbard, and Petersen [1988], but allow liquidity effects to be different in recession years. As we do, they obtain significant positive coefficients on their "recession dummy." (Unlike us, however, they also find that liquidity constraints are significant for fixed investment even in nonrecession years.) Similarly, using Japanese data, Hoshi, Scharfstein, and Singleton [1993] rerun the equations estimated by Hoshi, Kashyap, and Scharfstein [1991] and find that a large part of those results are attributable to periods of restrictive policy on the part of the Bank of Japan.

VI. Conclusions

The one set of conclusions that emerges most clearly from our work is compactly summarized in row 4 of Table V and can be stated as follows. Contrary to much previous research our results demonstrate that financial factors do indeed influence inventory movements. Moreover, financial constraints appear to be much more binding during tight money/recessionary episodes. Apart from these episodes there is little systematic evidence of inventories being sensitive to financial factors.[18]

We have motivated our empirical work primarily as an investigation of the "lending view" of monetary policy transmission. Although all our results are consistent with the predictions of the lending view, it is still not clear how sharply we can distinguish 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 recessions lead to economywide declines in collateral values. This could—in the presence of information or incentive problems—

1982–1989 using fixed effects and Newey-West standard errors, with a balanced panel consisting of the 196 firms that passed our screens for each year. We reached a qualitatively similar conclusion: the LIQ*1982 coefficient is 0.68 with a t-statistic of 2.01.

18. These conclusions complement those drawn by Gertler and Gilchrist [1993] from their study of the QFR data, which allows them to examine the composite balance sheets of "small" and "large" manufacturing firms. They find that the inventories of small firms decline significantly more sharply in response to tight monetary policy than do the inventories of large firms. Results similar to ours are also reported by Carpenter, Fazzari, and Petersen [1993]. They too find that internal finance helps to explain inventory movements, and that this relationship is especially pronounced during the 1981–1982 recession.

increase the cost of bank finance, even if banks' willingness to supply loans (for a *fixed* amount of collateral) was unchanged

The one argument we have made against the "collateral shock" hypothesis involves the data from 1985–1986 In this period of easy money but pronounced economic weakness, there is no evidence of liquidity constraints in inventory behavior However, a skeptic might reasonably argue that the 1985–1986 control is not decisive Perhaps it takes a severe downturn to generate noteworthy collateral effects, and the 1985–1986 period was simply not comparable to 1974 or 1982 in terms of the magnitude of economic decline [19]

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 recessions are partially due to a cutoff in bank lending The two hypotheses only disagree about the precise 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

Finally, we are left with the following question just how economically important are the financial constraints that we have identified? The back-of-the-envelope calculations in subsection IV B suggest that these constraints might explain a substantial fraction of inventory movements during downturns However, we recognize that it is difficult to draw precise structural conclusions of this sort from the reduced-form regressions presented above Thus, while we feel quite confident in concluding that financial factors do matter for inventories, we are much less confident in assessing exactly *how much* they matter [20] Gaining a better

19 This point is underscored by a comparison of the level of stock market prices—arguably a measure of the market value of collateral—across the different periods The price-earnings ratio for the S&P 500 averaged 8 6 in both 1974 and 1982, and rose to 12 3 in 1985 and 16 4 in 1986 Given the weakness of this intertemporal test, we also attempted to differentiate between the collateral and lending hypotheses using a cross-sectional approach In particular, we tested to see whether in 1982 the LIQ coefficient was higher for firms with the most impaired levels of collaterizable assets Using a variety of proxies for firm collateral, we were unable to find any such relationship Thus, the cross-sectional tests also do not provide any positive support for the collateral hypothesis However, our collateral proxies were arguably quite noisy, so that this may again simply reflect a weakness in our ability to distinguish between the collateral and the lending hypotheses See the June 1993 working paper version of this paper for details on the cross-sectional tests described here

20 See Kashyap and Stein [1994] for a more detailed discussion of the issues involved in evaluating the magnitude of the bank lending channel of monetary policy

understanding of these economic magnitudes is an important topic for future research

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