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COMMENT ON MONEY, CREDIT AND INTEREST RATES
IN THE BUSINESS CYCLE

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Comment on Money, Credit and Interest Rates
in the Business Cycle

by Allan H. Meltzer

Near the start of his paper, Benjamin Friedman writes: "The basic theme of this paper ... is that the quantitative relationships connecting monetary and financial variables to the business cycle exhibit few if any strongly persistent regularities which have remained even approximately invariant in the context of the widespread and, in some instances, dramatic changes undergone by the U.S. financial markets..." (p. 2). In the conclusion, he repeats this theme: "[E]conomic fluctuations exhibit few quantitative regularities that have persisted unchanged across spans of time in which the nation's financial markets have undergone profound and far reaching changes." (p. 40). And he warns us not to accept "the appearance of simple and eternal verities in much of the previous literature of monetary and financial aspects of business fluctuations" (p. 33).

I find these conclusions misleading for at least two reasons. First, even if we accept Friedman's evidence, it is not clear that his evidence rejects any well established proposition. Friedman does not give any clues about the particular "simple and eternal verities" that should not now be accepted. Economists have known that numerical estimates of structural parameters and reduced form coefficients are subject to change whenever there
are changes in behavior. (This is a principal implication of the early Cowles Commission work on identification which has been more fully developed in Lucas (1976).) Second, Friedman's paper is atheoretical, and much of his evidence comes from two way comparisons of financial and real variables. Failure at this level of testing is informative but not alarming. Vector autoregressions (VARs) using five or six variables are more informative, but this technique has well-known limitations that Friedman (1983) recognizes.

My comment is in three parts. First, I reconsider some of the propositions on the role of money in business cycles developed in the sixties by Milton Friedman and Anna J. Schwartz (1963a, 1963b), Phillip Cagan (1965) and Karl Brunner and Allan H. Meltzer (1964). I report some of the evidence I find in the paper on these relations. Second, I comment on some reasons that Friedman and I draw different conclusions from his data and suggest that his work is most usefully interpreted as a test of the quantitative significance of the well-known Lucas critique of econometric practice. Third, I comment briefly on his discussions of the credit variable during business cycles.

Money and Business Cycles

G.L. Bach (1963, p. 3) summarized Friedman and Schwartz's contribution at a previous National Bureau Conference as follows:

"The Friedman and Schwartz paper, together with Friedman's other published works, provide the strongest empirical foundation for the proposition that the supply of money is a -- probably the -- dominant variable in determining the level of total spending on current output....[T]here was a general willingness to admit that the supply
of money does now appear to be an important variable in explaining the level of aggregate spending..."

Friedman and Schwartz (1963a) offered a number of propositions about the stock of money and its rate of change during business cycles, measured according to National Bureau chronology, and about the relation of changes in money growth to business cycles. They investigated cyclical patterns of velocity and demonstrated the pro-cyclical conformity of velocity -- the finding that velocity rises relative to trend during expansions and declines relative to trend during contractions. They summarized their principal findings as showing "beyond any reasonable doubt that the stock of money displays a systematic cyclical behavior. The rate of change in the money stock regularly reaches a peak before the reference peak and a trough before the reference trough, though the lead is rather variable." (1963, p. 63 Italics added.)

These and other conclusions were extended in a study of the Federal Reserve system by Brunner and Meltzer (1964), in a study of the cyclical and secular behavior of the money stock by Cagan (1965) and in Friedman and Schwartz's (1963b) Monetary History of the United States. One or more of these studies presented evidence for the following propositions that, together, constituted a major part of the foundations for "monetarism":

(1) money growth rises and falls pro-cyclically;
(2) accelerations and decelerations of money are frequently followed after a lag by cyclical expansions and contractions of real output;\(^1\)
(3) sustained money growth relative to the growth of output is a sufficient condition for inflation;
(4) market interest rates typically rise in periods of cyclical expansion and fall in contractions; and
(5) velocity growth is pro-cyclical.

Benjamin Friedman's study of postwar data supports several of these propositions. His Table 1 shows, as he notes, that money growth is higher in each expansion than in the preceding or following contraction. This finding supports proposition 1.

For each peak and trough beginning with the fourth quarter 1949, I chose the peak and trough in the quarterly rate of money growth nearest to the quarterly reference peaks or troughs recorded by the National Bureau. Friedman and Schwartz (1963a, p. 37) used peaks and troughs in monthly rates of money growth as one means of dating specific cycles in money and computing the leads of money growth at business cycle turning points. There are now more sophisticated methods of computing leads and lags, but my method permits replication of Friedman and Schwartz's work on an extended sample. Multiplying the average lead of money growth measured in quarters by three to compare to Friedman and Schwartz's monthly data shows that the average lead of money growth is 10.5 months at reference cycle troughs and 15.4 months at reference cycle peaks. This lead is one to two months shorter than the earlier estimates. Given the relatively high variability of the leads which Friedman and Schwartz note, the difference is not impressive. This quantitative proposition stands up well.

These data, and econometric studies by Barro (1978), Korteweg (1978) and others support proposition 2 on the role of changes in money growth as a dominant impulse in business cycles. Proposition 3 on the central role of money growth for inflation is now accepted by economists with many fewer reservations or disclaimers than twenty years ago. Friedman's comparison of average rates of growth of nominal and real GNP during expansions and contractions casts doubt on the relationship, however. He reports that the
Table 1

Computed Average Lead Of Specific Cycles In Money Growth ($M_1$) Before Reference Cycle Turning Points

<p>| | | |</p>
<table>
<thead>
<tr>
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<tbody>
<tr>
<td><strong>Peaks</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average at peaks 1953-1981</td>
<td>-15.4 months</td>
<td></td>
</tr>
<tr>
<td>Average all peaks 1870-1960</td>
<td>-17.6 months</td>
<td></td>
</tr>
<tr>
<td>Average all mild cycles 1870-1960</td>
<td>-16.4 months</td>
<td></td>
</tr>
<tr>
<td><strong>Troughs</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average at troughs 1949-1982</td>
<td>-10.5 months</td>
<td></td>
</tr>
<tr>
<td>Average at all troughs 1870-1960</td>
<td>-12.0 months</td>
<td></td>
</tr>
<tr>
<td>Average all mild cycles 1870-1960</td>
<td>-11.8 months</td>
<td></td>
</tr>
</tbody>
</table>
differences between the average rate of change in expansion and the average rate of change in contraction is the same (64%) for nominal and real GNP. Taken literally, this implies that the average rate of inflation has been the same for expansions as for contractions. Since average money growth is procyclical, money growth differs systematically between half cycles.

Comparing cyclical average rates of growth of money to inflation using Friedman's data shows a persistent effect of money growth on inflation. The comparative size of money growth and inflation is positively associated without any allowance for lags, supply shocks or the effects of real output growth. Rank correlations of half-cycle average rates of money growth and inflation are 0.74 for the 8 contractions and 0.89 for the seven expansions. These rank correlations are significant at better than the 5% and at the 1% level respectively.

Friedman's Table 2 shows that the average change in short-term rates is negative in each contraction and positive in each expansion, as required by proposition 4. The average change in long-term rates is positive in two of the eight contractions, contrary to the proposition, however. One of the exceptions includes the first oil shock, when interest rates were raised by the effects of the supply shock.

Friedman's discussion of Table 4 notes that monetary velocity rises in every expansion and falls in every contraction. This supports a strong form of proposition five, since no allowance has to be made for trend. Velocity growth, like money growth, is higher in each expansion than in either the preceding or following contraction.

The five propositions of monetary economics are supported by Friedman's study. Among other propositions, one is of particular interest for policy. In a recent paper, Brunner and Meltzer (1983) pointed out that covariances of
money growth and velocity growth computed from quarterly data are positively correlated at times. Friedman shows that the positive correlation of money growth and velocity growth is found also for the eight cyclical contractions and for the combined contractions and expansions. Further, Friedman finds that the variance of nominal GNP growth is dominated by the variance of money growth in both contractions and expansions. These findings imply that constant money growth would lower the variability of GNP growth by reducing or eliminating the variability of money growth and by removing the covariance of money growth and velocity growth found during expansions.

If households are risk averse, welfare increases as the variability of GNP growth declines. Friedman's estimates suggest that the static effect of constant money growth on nominal GNP growth is a reduction of more than 80% of measured variability in the cyclical averages. Or, to put the same point in another way, the data in his Table 7 imply that the variability of velocity growth would have to rise by an average of 570% in contractions and 650% in expansions to raise the variability of GNP growth following the adoption of a rule mandating constant money growth. This suggests again, that Federal Reserve's discretionary policy has lowered welfare by adding more variability to GNP growth than it removed and, at times, by maintaining positive correlation of money growth and velocity growth.

Differences In Interpretation

Friedman reports and comments on several of the relations discussed in the preceding section. In addition, he analyzes some power spectra and reports evidence of coherence that is consistent with propositions relating
money growth to the fluctuations that we call business cycles. Why, then, is his principal conclusion a warning against accepting these regularities as a reliable basis for theory or policy?

One reason is that Friedman may have been misled by the data he presents. Friedman recognizes that the bivariate relationships can be misleading because they omit relevant variables and replace partial with total responses. The vector autoregressions (VAR) have been the subject of many recent criticisms, and it is now well known that this method is sensitive to changes in the ordering of variables, the number of variables included in the VAR and that the results are subject to the Lucas critique. Elsewhere, Friedman (1983, p. 33) has recognized these criticisms and makes only modest claims about what can be learned from VARs.

The bivariate relationships are subject to other, no less trenchant, criticisms. Correlations between annual data are unlikely to yield useful information about business contractions that last less than four quarters or are spread over parts of two calendar years. Four of the eight postwar contractions are of this kind. Quarterly relations are not subject to this criticism, but they are open to two others.

First, some of Friedman's tests are not tests of plausible economic relations. Included here are bivariate tests of a relation between money growth and the growth of real income. As tests of a relation running from money to income, the tests either fail to distinguish between anticipated and unanticipated money growth or, in the case of the VARs, impose tight restrictions on anticipations. As tests of a relation running from income to money, they fail to hold prices and interest rates constant, as required for the demand for money, and fail to take account of relevant foreign variables and exchange regimes as required for the supply of money. Other tests have
problems also. Economic theory does not imply that interest rates are related in a simple way to the growth of real or nominal income or to the growth rate of money. Tests of the relation between growth of income, money growth and interest rates shed no light, and Friedman does not attempt to interpret them or provide an analytic foundation to help the reader interpret them.

Second, work on monetary relations published in the sixties, and cited earlier, made no claims that lags are constant or that numerical values are fixed. Typically, the emphasis was on the variability of lags.

The five propositions discussed in the previous section do not require constant coefficients. Earlier work using the National Bureau's business cycle method encouraged a search for common features of business cycles but also did not impose uniform leads or lags or other constant coefficients on the data. The basic unit of time in these studies is not a year or a quarter. It is a cyclical phase of varying length when measured in calendar time but assumed to be comparable to similar phases of other cycles. The Bureau also distinguished, at times, between wartime and peacetime cycles, between mild and severe recessions and between the recoveries from mild and severe recessions. While I have not found the Bureau's method attractive, I find it more useful for bivariate comparisons than Friedman's use of years or calendar quarters as units of observation.

One reason is that the seven expansions studied by Friedman vary in length from 12 to 106 months, roughly 4 to 35 quarters. The eight contractions vary from 6 to 16 months, or from 2 to more than 5 quarters. It would be surprising if the forces (including policy) influencing the length of expansions and contractions had no influence on the measured length of lags and other parameters.
An additional reason for believing that Friedman's null conclusion is misleading is that economic theory gives no reason for assuming that lags are constant. The variability of the lead of money growth at business cycle turning points has been remarked upon frequently. Recent work shows that the length of leads or lags varies directly with the ratio of the variance of permanent to the variance of transitory changes. See Brunner, Cukierman and Meltzer (1982).

Current research on policy rules or regimes, recognizing the so-called Lucas effect, makes no claim that the parameters of economic models are invariant to changes in policy rules. At times during this century, the U.S. has followed the rules of the gold standard, the gold exchange standard, the Bretton Woods system, the system of fluctuating exchange and the 1942-51 system of pegged interest rates. Other countries have experienced as many changes, and often more violent changes, in monetary regime. Each of these regimes, in principle, changes the path through which money influences economic activity or the timing of the responses of income to the stock of money or the responses of demand for money to income and other variables.

Friedman's findings are not inconsistent with this view. Although he does not mention the particular changes in policy regimes, in principle the same conclusion applies to the introduction of FDIC insurance that changed the risk of banks' deposit liabilities, the development of substitutes for money, or changes in the effective ceilings on interest rates arising from the combination of regulation and increased rates of anticipated inflation.

Friedman's findings are part of the accumulating analysis and evidence on the problems faced by policymakers who seek to control or modify economic activity using either econometric models and sophisticated feedback or control
procedures or fully discretionary policies based on judgment about average responses or are of the many mixtures of these control techniques. The results that he calls "qualitative", and the evidence I summarized in the preceding section do not rule out the relevance of economic research and economic theory for economic policy. Nothing in Friedman's work rejects such quantitative relations as:

1. the so-called Fisher equation relating nominal rates of interest to anticipated inflation;
2. approximate long-run proportionality between growth of nominal income and money growth;
3. long-run purchasing power parity.

The Role of Credit

A considerable part of Friedman's paper compares cyclical properties of credit and money and studies cyclical relations between credit and other variables. I ignored these sections in the previous comments because I do not know how to interpret the findings, and Friedman offers little guidance. A section on the credit cycle discusses some regular features of cyclical changes in public and private debt, but concludes that movements of private debt provide no information about the growth of real income that is not contained in the growth of public debt. In an earlier section, Friedman concludes that the growth rates of credit or money provides no useful information once the other growth rate is known.

This last comparison and Friedman's parallel treatment of credit and money throughout the paper suggest that he finds little basis for choosing
between the two measures. Although I am convinced that the study of intermediation is useful, I am as skeptical about Friedman's parallel treatment of credit and money as I am of his procedure for studying the role of credit or intermediation during business cycles.

Friedman defines credit as the total liabilities of non-financial borrowers -- private and public -- that have been issued in the financial markets. He computes a measure of credit velocity, defined as the ratio of GNP to credit, and compares this measure of velocity to monetary velocity, the ratio of GNP to money.

It is always possible to analyze a stock-flow relation by multiplying a particular stock by its velocity, measured in units per time, so that the product is equal to the flow. The quantity equation shows that this tradition is as old as systematic thinking about money. The initial appeal of the quantity equation, and its persistence through time, owed much to the (quantitative) empirical observation that prices and other nominal values move over time in direct proportion to money, although the correspondence may not be close during a particular year or quarter.

The relation of money to nominal GNP has been formalized in the quantity theory. Whatever reservations one may have about the content of this hypothesis, there can be no doubt about its survival or its usefulness in explaining differences in rates of inflation between countries and in the same country at different times.

There is no comparable hypothesis about domestic non-financial credit. Is there more than arithmetic behind Friedman's idea that the growth rate of credit plus the growth of credit velocity equals the growth of nominal GNP? Is the growth of nominal GNP independent of the growth of money and dependent on credit? How dependent is the postwar growth of credit relative to money on
the effects of prohibitions on interest payments and regulation in the presence of inflationary monetary policy? How dependent is the growth of the private component of credit on the growth of the public component -- the growth of the public debt?

Although Friedman does not pursue these issues, his data provide some answers. Rank correlation of his measures of the growth of public and private debt show very little relation between the two during either expansions or contractions, contrary to the complete crowding out hypothesis. The difference between the growth of private debt and money is negatively related to the short-term rate of interest. This difference is a measure of the growth of intermediation since \( M_1 \) and the monetary base grow at approximately parallel rates during half-cycles. The measure declined in both expansions and contractions as interest rates rose. The decline is dramatic, more than 50% on average, between half-cycles during which short-term market rates are below regulation ceilings and the half-cycles in which short-term rates are substantially above the ceilings. A smaller and less uniform decline in the growth of intermediation is shown by the comparison of interest rates and the difference in the growth rate of total credit and money.

The introduction suggests that the paper will explore relationships of this kind. Regrettably, the paper does not do so. Friedman is too eager to dismiss what is known and too reluctant to use his data to extend existing theories of the relation of credit and money, or the theory of intermediation, during business cycles.\(^5\)
Conclusion

This conference has produced a large number of null results, and Friedman's paper is of this kind. I am not persuaded that the null conclusions tell us as much about business cycles as they do about the method common to many of the papers. Perhaps a principal conclusion to be drawn is that you cannot get something for nothing. If we are unwilling to impose a structure on the data, by stating testable hypotheses, the data may mislead us into accepting that the world is as lacking in structure as this approach.

Benjamin Friedman has summarized ably the data for main financial variables. I find in his null results additional information about the errors that are likely to be made when policymakers rely on estimates from quarterly equations or models. The results are far less damaging -- and often supportive -- of well-known qualitative and quantitative relations between monetary and other variables. My comments try to make this distinction and to suggest the limits to the scope of reliable quantitative knowledge that economists and policymakers face.

At least since the time of Lucas' (1976) critique of econometric practice and policy simulation, economists have been aware that parameter estimates of economic models are subject to change when private or public policies change. The quantitative significance of Lucas' result has been left largely to individual judgment, and judgments differ. Friedman's work, summarized in Tables 10 and 15 and his discussion of the economic significance of his findings, can be interpreted as evidence of the quantitative significance of the Lucas' critique. Although Friedman avoids this interpretation, I find it appealing and suggestive of the way in which his study can be a useful start on the quantitative analysis of an important topic.
Appendix

Leads of Money Growth at NBER Turning Points

<table>
<thead>
<tr>
<th>Reference Cycle Dates (Quarters)</th>
<th>Money Growth Specific Cycle</th>
<th>Lead In Quarters</th>
</tr>
</thead>
<tbody>
<tr>
<td>Peak</td>
<td>Trough</td>
<td>Peak</td>
</tr>
<tr>
<td>1949-4</td>
<td>1951-4</td>
<td>1948-4</td>
</tr>
<tr>
<td>1953-2</td>
<td>1954-2</td>
<td>1953-3</td>
</tr>
<tr>
<td>1957-3</td>
<td>1958-2</td>
<td>1954-4</td>
</tr>
<tr>
<td>1960-2</td>
<td>1961-1</td>
<td>1959-1</td>
</tr>
<tr>
<td>1969-4</td>
<td>1970-4</td>
<td>1968-4</td>
</tr>
<tr>
<td>1973-4</td>
<td>1975-1</td>
<td>1972-3</td>
</tr>
<tr>
<td>1980-1</td>
<td>1980-3</td>
<td>1979-2</td>
</tr>
<tr>
<td>1981-3</td>
<td>1982-4</td>
<td>1981-1</td>
</tr>
<tr>
<td>Mean lead in quarters</td>
<td>5.14</td>
<td>3.50</td>
</tr>
<tr>
<td>Mean lead in months</td>
<td>16.4</td>
<td>10.5</td>
</tr>
</tbody>
</table>
Footnotes

1. Economists would now substitute "unanticipated money growth" for "accelerations and decelerations of money." It is not clear that the more precise restatement makes a major difference for U.S. data.

2. There is one minor exception. Money growth is higher on average during the 1969-70 contraction than in the preceding expansion. The lengthy expansion includes a period of relatively low money growth and low inflation.

3. Dating for each peak and trough is shown in the appendix. Friedman and Schwartz used $M_2$; I used $M_1$. Where the two series overlap, differences in dating are small.

4. His finding of an absence of any effect of money on real income -- other than those reported in Table 9 that are purely autoregressive -- is consistent with rational expectations.

5. One surprising claim is that financial panics "have all but vanished since the establishment of the Federal Reserve System in 1914 and especially the Federal Deposit Insurance Corporation in 1934..." This statement neglects the experience from 1929 to 1933 and particularly the waves of banking failures from 1930 until the bank holiday in March 1933.
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