

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 differences between the average rate of change in expansion and the average rate of change in contraction is the same (6.5%) 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 with 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 .74 for the eight contractions and .89 for the seven expansions. These rank correlations are significant at better than the .05 and at the .01 level respectively.

Friedman's table 7.2 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, however, contrary to the proposition. 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 7.4 notes that monetary velocity rises in every expansion and falls in every contraction. This supports a strong form of proposition 5, 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 the 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 another way, the data in his table 7.7 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 the 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 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. He recognizes that the bivariate relationships can be misleading because they omit relevant variables and replace partial responses 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 and the number of variables included in the VAR, and that the results are subject to the Lucas critique. Elsewhere, Friedman (1983b, 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 also have problems. 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 above, 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 NBER's business cycle method encouraged a search for common features of business cycles but 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 NBER 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 NBER'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 four to thirty five quarters. The eight contractions vary from 6 to 16 months, or from two to more than five 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 frequently been remarked upon. 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 (1980).

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 United States has followed the rules of the gold standard, the gold exchange standard, the Bretton Woods system, the system of fluc-

tuating exchange, and the 1942–51 system of pegged interest rates. Other countries have experienced as many changes, and often more violent ones, 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 federal deposit 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, control procedures or fully discretionary policies based on judgment about average responses, or the many mixtures of these control techniques.⁴ 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; and (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 rate of credit or of 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 choos-

4. His finding of an absence of any effect of money on real income—other than those reported in table 7.9 that are purely autoregressive—is consistent with rational expectations.

ing 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 about his procedure for studying the role of credit or intermediation during business cycles.

Friedman defines credit as the total liabilities of nonfinancial 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 with 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 nonfinancial 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 Q 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 M1 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 Q 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, it 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 ably summarized 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's (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's results has been left largely to individual judgment, and judgments differ. Friedman's work, summarized in tables 7.10 and 7.15 and in his discussion of the *economic* significance of his findings, can be interpreted as evidence of the quantitative significance of the Lucas's critique. Although Friedman avoids this interpretation, I find it appealing and suggestive of the way his study can be a useful start on the quantitative analysis of an important topic.

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.

Appendix

Leads of Money Growth at NBER Turning Points

Reference Cycle Dates (Quarters)		Money Growth Specific Cycle		Lead in Quarters	
Peak	Trough	Peak	Trough	Peak	Trough
	1949:4		1948:4		4
1953:2		1951:4		6	
	1954:2		1953:3		3
1957:3		1954:4		11	
	1958:2		1957:4		2
1960:2		1959:1		5	
	1961:1		1959:4		5
1969:4		1968:4		4	
	1970:4		1969:3		5
1973:4		1972:3		5	
	1975:1		1973:3		6
1980:1		1979:2		3	
	1980:3		1980:2		1
1981:3		1981:1		2	
	1982:4		1982:2		2
Mean lead in quarters				5.14	3.50
Mean lead in months				16.4	10.5

Reply Benjamin M. Friedman

Allan Meltzer asks what familiar proposition the empirical evidence assembled in my paper contradicts. Meltzer's question is a useful one, and it deserves a serious answer.

This book is about business cycles. The focus of my contribution to it is the behavior, in a business cycle context, of money, credit, and interest rates. Like the book's other papers, mine follows conventional understanding in taking "business cycles" to mean aggregate-level fluctuations in real economic activity, typically lasting more than a year (for the full cycle) but well under a decade. At the same time, because money and credit are nominal variables and so may bear a stronger connection to nominal economic activity, much of the analysis in the paper focuses on both real and nominal activity measures in parallel.

Of the five *qualitative* propositions Meltzer lists on pages 442–43 of his comment, therefore, all but the third involve business cycles and

hence are of at least some interest here. Indeed, as he points out, my paper presents evidence corroborating each of them. By contrast, of the three *quantitative* relationships he lists on page 447 of his comment, none is of interest here (although the Fisher equation certainly could be).

What familiar proposition, then, does the evidence presented in my paper contradict? It is, in Meltzer's wording, "the role of changes in money growth as a dominant impulse in business cycles"—or, in Bach's even stronger wording, which Meltzer quotes, "the proposition that the supply of money is a—probably the—dominant variable in determining the level of total spending on current output."¹ This proposition has become as familiar an idea as any that macroeconomics has to offer. Although it is far from universally believed, there can be little doubt that acceptance of it—by economists, by policymakers, and by the general public—has grown enormously in the twenty years since the publication of the work Meltzer cites by Friedman and Schwartz, himself and Brunner, and others.

In his reference to his own table C7.1 and to work by Barro and by Korteweg, Meltzer treats this *quantitative* proposition about what is a (or the) driving force underlying business cycles as equivalent to the *qualitative* proposition that changes in money growth "are frequently followed" by fluctuations of real output. But the two are not the same. The issue is not whether it is possible to replicate the mean lag findings of Friedman and Schwartz, as Meltzer does, but whether the evidence warrants singling out money as playing some special, dominant role in the initiation or propagation of cyclical movements in economic activity.

In short, is there anything special about the role of money in business cycles? Given that there is no dispute about Meltzer's first and second qualitative propositions—that money growth varies procyclically and that it frequently leads cyclical variations of output growth—my paper addresses this question in three ways. The first is to go beyond the documentation of whether movements in money growth tell anything about movements in income growth by asking whether movements in money growth tell anything about movements in income growth that prior movements in income growth itself cannot say equally well. The second is to examine the importance of other variables in this context, either indirectly by asking whether what movements in money growth have to say about movements in income growth varies from one time period to another, or more directly by asking what movements in several variables (including money growth) have to say together. The third

1. The part of Bach's statement Meltzer quotes could, of course, be taken to refer only to nominal spending without any implications at all for real economic activity and hence business cycles. A reading of Bach's introduction makes clear that this is not the case, however, so that Meltzer's citation is apt in the business cycle context.

is to undertake comparisons, by asking whether what movements in money growth tell about movements in income growth differs from what movements in credit and interest rates tell.

The conclusion indicated in my paper is that the evidence does not identify anything special, or dominant, about the role of money in the business cycle context. First, although for some time periods there is evidence that movements in money growth tell something about movements in income growth that prior movements in income growth do not already say, that evidence is hardly overwhelming, and for other time periods there is no such evidence. Second, just what it is that movements in money growth tell about movements in income growth varies substantially from one time period to another,² and movements in money growth do not stand out in this context in a multivariate setting. Third, movements in other financial variables—specifically, interest rates and credit—tell about as much about movements in income growth as do movements in money growth.

No, of course these findings do not contradict the *qualitative* propositions that money growth varies procyclically and that it frequently leads real income growth. But they do cast doubt on the *quantitative* proposition that the impulse to business cycles from money growth is dominant in any ordinary sense.

Meltzer's criticism of the use of two-variable relationships to address such questions—including in part relationships between nominal money and real income—has merit, as my paper should also make clear.³ But his brusque treatment of the subject does not get to the fundamental underlying tension it involves.

At one level, what is being asserted is indeed a relationship between two variables, one nominal and one real: nominal money growth varies procyclically, nominal money growth leads real income growth, nominal money growth is a (the) dominant impulse driving real income growth, and so on. Meltzer's own table C7.1 is itself one way of examining just this bivariate relationship between nominal money and

2. It is difficult to understand Meltzer's claim that my paper does not acknowledge "changes in policy regimes" as a source of these differences. Of the three categories of change in the economy's financial structure that section 7.1 sets forth as reasons for not expecting to find unchanging business cycle relationships, the first one discussed is monetary policy and the second is financial regulation. It is also ironic in this context that Meltzer's detailed discussion of what some of my paper's findings imply for a constant money growth rule simply assumes that the variability of the velocity ratio would remain invariant to that specific regime change.

3. Meltzer also criticizes the use of natural time units rather than business cycle phases in examining these relationships, but this criticism seems misplaced. If money growth is dominant in determining income growth, why take the length of a business expansion or contraction as exogenous? Regression or vector autoregression relationships based on natural time units allow the length of each movement in income growth to be determined by the length of each movement in money growth if the data so indicate.

real income. Yet Meltzer wants to disallow evidence from bivariate relationships in addressing the three questions posed above. Why is it admissible to ask if money growth leads real income growth but not to ask if money growth leads the part of real income growth that is not already predictable from past real income growth itself? Why is it legitimate to examine the relationship of real income growth to money growth but not to credit growth or interest rates?

The tension here arises because, if other things beside money growth matter for business cycles, then a simple two-variable relationship between money growth and real income growth is fundamentally misspecified. This misspecification has significant implications both for the use of simple money/income relationships in economic forecasting and policymaking and for the investigation of hypotheses like those in question here. In both settings it is then necessary to admit that the world is more complicated, and to advance to richer representations importantly featuring variables other than money. In the research context, my paper shows that simply moving to nonstructural systems of modestly higher order does not satisfactorily represent this complexity (nor does it indicate any special role for money growth), and here I fully agree with Meltzer about the need for structural analysis. As most readers of this volume surely know, however, it is hardly the case that the evidence from structural models clearly points toward money growth as the driving force behind business cycles either.

In sum, the thrust of Meltzer's comment is that somehow economists know, presumably from the work of Friedman and Schwartz and their followers, that money growth is what matters most for business fluctuations, and that efforts to question whether this is so, or to examine the role of other variables, must accept the burden of proof. That position is untenable. One cannot simultaneously embrace the long tradition of nonstructural investigation of the relation between money growth and the business cycle—including simple lead/lag analysis early on, then straightforward regression analysis, and more recently bi- or even multivariate autoregression analysis—but ignore parallel investigations showing comparable results for other variables. One cannot accept the conclusions of whatever structural analyses indicate a unique role for money yet discard all those that do not.

Nowhere is this schizophrenic view more apparent than in Meltzer's concluding section remarking on the approach maintained throughout my paper of examining, in a way parallel to that applied to money, the role of credit—that is, of a nominal financial quantity other than money. Meltzer acknowledges that the results provide little or no empirical basis for choosing between money and credit as a (the) dominant impulse underlying business cycles if one wants to make such a claim. Instead, he says he is “skeptical” about the entire parallel treatment

of money and credit, arguing that empirical examination of business cycle relationships is somehow legitimate for money but not for credit. Meltzer motivates this presumption in favor of money by appealing to the quantity theory.

But what quantity? And what theory? The mere statement that the growth of "money" bears a relation to income growth is no more than a hypothesis subject to empirical testing. That people have believed in it for many years would not make it so if the available empirical evidence systematically contradicted it. Similarly, that people have believed in this relation for a long time does not, in the absence of evidence, make it more valid than any other relation. A long tradition of belief that money growth bears such-and-such a relation to income growth also does not make it a "theory" in the sense of a behavioral explanation that is applicable to one observed relationship but not to others.

To be sure, the theoretical literature provides many models of the demand for money for transactions purposes, as well as of the demand for asset holding. To justify Meltzer's presumption, however, it is necessary to connect the theory to the quantity in question and also to show that the theory does not connect to other quantities as well. In an earlier era, Milton Friedman's "Restatement" of the quantity theory explicitly defined money as claims "that are generally accepted in payment of debts,"⁴ yet Friedman and Schwartz's empirical work focused on an aggregate also importantly including savings balances. Today, neither M1 nor M2 readily corresponds with either the transactions or the savings models of money demand, respectively. The deposits included in M1 often serve a savings function, and these deposits and currency are hardly the only way to make payments anyway. The more comprehensive M2 certainly does not represent total financial assets, or even total liquid assets.

Failing these conditions, the theory that is needed to relate any of the familiar Ms to the business cycle is a more general theory describing *inside* asset holding, and in particular a theory relating the holding of inside assets to the determination of nonfinancial economic activity.⁵

4. Popular usage of the "quantity theory" idea notwithstanding, Friedman's "Restatement" in no way provided a rationale for describing movements of income growth in terms of movements of money growth alone. Even after all of the simplifying assumptions Friedman imposed, his final equation related nominal income not only to money but also to interest rates, equity returns, the rate of price inflation, the ratio of human to nonhuman wealth, and real income, in addition to any variables affecting tastes and preferences.

5. Models in which all assets are outside assets and (inside) liabilities do not exist have been a staple of monetary economics for decades, and they have provided valuable theoretical insights. But the world they describe corresponds to a modern economy only if the inside assets and liabilities that obviously exist do not matter much. The empirical evidence relating the monetary base to nonfinancial economic activity suggests that that is not so in this context. The base does not show relationships to income that M1 and M2 do not, and often the base shows weaker relationships than either M1 or M2.

As Tobin and others have shown, however, theories of inside asset holding are inseparable from theories of inside liability issuing. Regardless of whether credit is viewed as an aggregate of debt assets held or an aggregate of debt liabilities issued, there is no reason to presume that a satisfactory theory exists for M1 or M2 in isolation from other inside assets and liabilities, or that a comprehensive theory of inside asset holding and liability issuing would somehow point to a special role for M1 or M2.

Whether there is something special about “money” in initiating or propagating business cycles is an empirical question. The evidence presented in my paper indicates that there is not.

Discussion Summary

Christopher Sims took issue with Allan Meltzer’s claim that VARs are particularly unrobust to specification changes by noting that, first, a fortiori the same could be said of structural models. Second, while it was true that time series relationships change through time, the changes were not enormous. Third, a standard of comparison was needed before one could claim that the relationships estimated were poorly captured.

Phillip Cagan observed that the NBER research on the relation between output and money had found that from the Civil War to the end of the 1950s the qualitative evidence favored the view that variations in the supply of money from various sources did affect output and prices subsequently. This was not necessarily the case recently, since Federal Reserve behavior might have shifted, but there remained the question whether these changes affected the relationship between money and GNP in an economically significant way.

Geoffrey Moore drew attention to the fact that money and credit behavior is very different at the peaks and troughs of the cycle and that one should not expect simple time series methods necessarily to show stability over time. Stanley Fischer suggested a more general hypothesis than the one in the paper, that persistent monetary expansion is eventually followed by inflation. This, he said, seemed to be a consistent qualitative result. Anna Schwartz took exception to Friedman’s assertion in the paper that velocity is “only a ratio,” which has no relevance beyond that.

Benjamin Friedman stressed that what was important for the conduct of monetary policy along “monetarist” lines was that relationships should remain *quantitatively* stable and said he had shown that such stability does not exist. While qualitative features of the money/nominal income relationship might persist, these are of limited use for policymakers.

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