Empirical Monetary Macroeconomics: What Have We Learned in the Last 25 Years?

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Monetary economics conveys the impression of great disagreement within the economics profession, and indeed the professional debates have often been heated. But behind the debates over policy there is a great deal of consensus on the importance of monetary variables for the working of national economies and on the mechanisms through which they exert their influence. The title of this session reminds us that twenty-five years ago this was not so. When Howard Ellis wrote "The Rediscovery of Money," the postwar revival had just begun. There was general skepticism about the ability of monetary policy to influence the economy, and the Oxford surveys were widely cited as the empirical basis for disbelief in the effect of monetary policy on investment. Despite the emergence in the intervening years of wide agreement about the importance of monetary policy, the empirical basis for many of our beliefs, and a fortiori for distinguishing among our differences, has remained weak.

In casually accepting our present assignment, we failed to appreciate just how complex the question posed in the title is. This is true even when the subject is confined, as here, to the monetary economics of the business cycle, leaving aside both microeconomic and steady-state growth considerations. What does it mean to say we have "learned" something? And to whom does the "we" refer?

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We interpret the first question to refer to how much we now know, compared with twenty five years ago, about managing the economy with instruments of monetary policy. Empirical knowledge is embodied in reliable quantitative economic relationships. This perspective does not, of course, encompass all of monetary economics, but it does encompass most of the professionally salient issues.

Who are "we"? There are often marked differences in the "knowledge" embodied in the most advanced work, in conventional professional wisdom, in ordinary textbooks, and in monetary officials. To what group should we apply our test of knowledge? Certain individuals are at the forefront of the profession, some with a pack of followers, others in splendid isolation; some are recognized at once, others with a short lag, and still others drop into oblivion to be rediscovered much later. For example, Irving Fisher explored what we now know as the Phillips curve in 1926 with an econometric sophistication which rivals all but the most recent work in the area. Diligent search can usually find a precursor to any idea. As Robert Wohlstetter discovered in her study of the Pearl Harbor attack, the problem is not the inadequacy of information; rather, it is to separate the true signal from the noise. In an active intellectual discipline, this is always done with much greater clarity long after discoveries are first made. For this reason, while it might seem most appropriate to concentrate our attention
on the most advanced work, it is neither easy nor fully appropriate to do so, since we may not even know where the true frontiers of the subject are at the present time.

In some respects, monetary economics as a whole was just being "rediscovered" as an important field in the late 1940's and a twenty-five year survey starts near a low point. The outpouring of empirical work in monetary economics during the past two decades—and especially during the past decade—has been vast. A simple bibliography of professional articles alone would exhaust the space allotted to us. We must be highly selective and, therefore, perforce, somewhat impressionistic. We first present some quantitative information on professional articles on empirical monetary economics in the late 1940's and in the early 1970's, along with a brief characterization of these articles. We then discuss the chain of monetary influence as embodied in empirical macro models of the U.S. economy. Finally, based on these comparisons and on an impressionistic sampling of other literature, we offer some generalizations about improvements in our knowledge and about areas where our ignorance remains great.

We are concerned mainly with the endpoints of our period. Limitations of space prevent consideration of many important milestones within the period, such as Gurley-Shaw, the Radcliffe Report, the Commission on Money and Credit, Friedman-Schwartz.

I. Quantitative Assessment

One measure of the relative importance and the character of empirical work in monetary economics now as compared to the late 1940's can be obtained by surveying articles in the professional journals. We have counted the articles (including notes and replies) on monetary economics in four journals, representative of the best current research by the profession and published at both the beginning and end of our period: the American Economic Review, the Journal of Political Economy, the Quarterly Journal of Economics, and the Review of Economics and Statistics. The results confirm the general impression of a great increase in work on monetary economics: in the period 1948–50 there were twenty-six articles in the four journals, of which fifteen were theoretical and eleven were empirical. In the period 1970–72, there were eighty-seven articles on monetary economics, of which fifty-one were theoretical and thirty-six were empirical. This undoubtedly understates the growth since at least one major journal, devoted entirely to research in the monetary area, was created during this period, the Journal of Money, Credit and Banking. Thus, there was a sharp increase in both theoretical and empirical work on monetary economics. Moreover, there was a marked shift in the character of the empirical articles: in the earlier period three were historical-analytical and eight involved graphical analysis of two variables. None contained regressions. In the latter period, by contrast, thirty-three involved regression analysis, two involved simulations, and one contained only graphical analysis.

This great increase in quantitative empirical work, of course, reflects the increased availability of better data and of high speed computers permitting their analysis, but also greater appreciation of the intrinsic complexity of monetary relationships, calling for multivariate and

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1 Examples of empirical articles from 1948–50 are E. Marcus, Kenneth Roose, James Tobin, Clark Warburton, and Charles Whittlesey.
2 These findings may have reflected in part a change in editorial policy regarding regressions. But regression analysis was used in other areas of economics, especially in the Review of Economics and Statistics. And Econometrica also contained no regression analysis on monetary economics during 1948–50.
simultaneous equation analysis.

In certain respects, the sharp growth in quantification itself represents a substantial gain. It compels more exact thought, both as regards the relationships among variables and as regards the variables themselves. Theoretical formulations can speak of "the" interest rate and "the" money supply, for example, but the quantifier must choose precisely what interest rate and what definition of the money supply he is going to use. This is a useful discipline, if only to indicate that our theoretical concepts are not quite as operationally sharp as we would like them to be. Second, quantification permits some assessment of the quantitative importance of the variables in question; effects of negligible practical importance often command disproportionate theoretical attention. On the other hand, a preoccupation with quantification may result in neglect of important but not easily measured variables. And quantification is itself no assurance against misspecification.

One of the most dramatic changes during the period is the increase in the number of working econometric models purporting to describe the behavior of the U.S. economy. No less than eleven models are participating in the National Bureau of Economic Research-National Science Foundation (NBER-NSF) continuing seminar on the comparison of econometric models, and since 1970 at least four different models have been used for regular quarterly forecasts one to five quarters ahead. Models of this type have the advantage of embodying concrete quantitative information about empirical linkages in the economy.

In 1950, econometric models were in their infancy. Few thought of them as a practical tool for forecasting the economy or the impact of policy; rather the concern was with the extent to which a model could capture the salient features of the economy and its cyclical behavior, and with problems of econometric methodology. Nevertheless, comparison of the difference in the treatment of the monetary sector in these models and their modern counterparts gives dramatic testimony to the cycle in the profession's perception of the importance of monetary factors in the determination of income and prices. As reflected in the models, the recovery of money from the Great Depression was protracted. When Marc Nerlove did his survey of the six models estimated from postwar data in 1966, for example, only two had more than two endogenous financial variables, and three had no monetary sector at all. This contrasts sharply with the remarkable model by Jan Tinbergen (1939), which contains an elaborate monetary sector. By the end of the period most of the models included a substantial monetary sector. Perhaps the most sophisticated and most widely emulated is the financial sector of the MIT-PENN-SSRC (MPS) model. In it monetary policy has powerful effects on income and works through a rich and complicated set of linkages. While it has been the subject of criticism by monetarists and nonmonetarists alike, it has gone a long way towards resolving some of their differences.

At the same time that structural models were in the process of reintroducing money, there was an effort, stimulated by Milton Friedman and David Meissler, to determine whether simple single-equation Keynesian or monetarist models provide a better explanation of money income. In retrospect, it appears clear that it is not possible to discriminate between competing macro models simply on the basis of in-sample fit. Given the highly serially and cross-correlated nature of economic time series, the range of choice of variables for use in any model, and the flexibility in the specification of lags, marvelous fits of historic data can be ob-
tained by models with widely different implications for the behavior of the economy and its response to policy. Out-of-sample forecasts provide a better test, but it appears difficult to discriminate between the simple Keynesian and monetarist models on this basis. Although out-of-sample forecasts do not provide a clear verdict on which of these single-equation models predicts best, they do show persuasively that these models, taken as a class or individually, cannot compete successfully with the more complicated models, structural or otherwise, nor for that matter with the median of judgmental forecasts. Today, most economists would probably agree with William Poole and Elinda Kornblith when they say (p. 915), "neither the simple Keynesian nor the simple quantity-theory models provide an adequate understanding of business cycle fluctuations... there simply is not much empirical content in the single-equation approach as employed in the studies examined here."

Given the fact that economic data do not distinguish sharply between competing views, the plausibility of theoretical explanation will continue to bear heavy weight in distinguishing among alternative hypotheses as well as in guiding our exploration of the data. For that reason, students of monetary economics will want to know how money works. They will want to know how specific components of demand are connected to money markets and the accuracy of predictions about the behavior of the links in that causal chain.

II. Channels of Monetary Policy

Earlier monetary writings are very rich in their verbal treatment of the influence of monetary variables on economic activity and at a general level our knowledge has not advanced beyond them. No one has discovered a brand new channel of influence, and indeed some of the monetary effects identified earlier, such as the importance of business and consumer expectations, or "confidence," have not yet been satisfactorily incorporated into quantitative models.

We will organize our discussion of linkages from the perspective of the policymaker. From his view, the instruments of policy are directly controlled. Thus, the direction of economic causation runs from his instruments to the rest of the economy. A simplified rendition of the chain of influence is as follows:

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\begin{align*}
CB & \rightarrow H \rightarrow MM \rightarrow Y \\
\text{(1)} & \quad \text{(2)} \quad \text{(3)}
\end{align*}
\]

where \( CB \) = the central bank, \( H \) = high-powered money, \( MM \) = money markets, \( Y \) = money income, \( Q \) = the volume of output, and \( P \) = the overall price level. The important empirical questions concern the magnitude and the reliability of each linkage. From the econometrician's point of view, of course, policy however measured is unlikely to be exogenous. This fact creates problems of estimation and interpretation of both reduced form and structural equations. Similarly, the influence of exogenous variables and the feedback from price and output greatly complicate the estimation of this chain.

In this scheme, what is the instrument of control? Taken literally, the central bank controls its portfolio of assets, the discount rate, reserve requirements, and other regulations which restrict and influence the behavior of banks and other institutions. But effective control may extend much further into the private realm. What effective control is, of course, depends upon the objectives of policy and the horizon in question.

Early discussion tended to focus on the quantity of currency plus demand deposits as the important element in \( MM \). The
central bank controlled this quantity, which was tied directly to $Y$ by a velocity or demand for money function. Recently a number of studies suggest that control of $M_t$ over periods as short as a month may be difficult. Given other, longer lags in the system, it does not seem to us that this possibility is itself a cause for alarm. It is also apparent that over a cycle in economic activity there is a difference between controlling $H$ and controlling money, even if money is defined narrowly. Earlier generations of students were trained in increasingly elaborate mechanics of the money multiplier, which tied the volume of bank deposit liabilities directly to the supply of high-powered money. The ability of the banking system to affect this multiplier by borrowing or altering excess reserves has long been recognized. But while this slippage may decrease the magnitude of response to changes in $H$, it is not likely to be so rapid or so dramatic as to prevent control of a particular $M$ over the medium term.

Over longer periods, postwar experience with the development of the federal funds market, the emergence of certificates of deposit, and the development of ties between American banks and the Eurodollar market demonstrate how institutional adaptation to profit-making opportunities and regulation may change the relationship between $H$ and money, no matter how defined. Such endogenous institutional changes suggest that it may be difficult to specify any simple rule for monetary policy applicable to the long run.

It would be possible to tighten the linkage between $H$ and a particular $M$ by implementation of proposals such as that for 100 percent money (Lloyd Mints). However, the growth during the period in bank liabilities other than demand deposits, in deposit liabilities of financial intermediaries, and in other near monies makes clear the difficulty of settling on any single definition of money for policy purposes. Improving control over one variable is likely to increase slippages elsewhere in the system.

While there is still much disagreement about the best way to describe empirically the third linkage, connecting money markets and national output, monetary economists have come a long way towards agreement about general outline of the process. Such diverse writers as Tobin, Allan Meltzer and Karl Brunner, Friedman, and Franco Modigliani stress the importance of recognizing that a broad spectrum of assets and commodities are affected by monetary disturbances, and of distinguishing between the rates of interest, nominal or real, on financial assets and the required rate of return on capital or durable goods. This latter point, which can be found in Tinbergen but was neglected in virtually all models until the $MPS$, can be formulated in a variety of superficially different ways. According to all these authors, discrepancies between the required rate on physical assets and their marginal productivities, or equivalently, between the current valuation of these goods and their replacement costs, stimulate or discourage their production.

One important implication of this more complex view of the modus operandi of monetary policy is that the second and third linkages fuse, and changes in policy can influence $Y$ without necessarily going through any particular monetary aggregate.

Traditional concern with the variability of "velocity" involves the third linkage. At a theoretical level it was long assumed that the essential issue was the interest elasticity of the demand for money, an
empirical issue on which the relative effectiveness of monetary and fiscal policy was assumed to hinge. In models which incorporate a wide variety of financial markets, the role played by the demand for any particular definition of money is displaced by the demand for high-powered money, $H$. The now widely accepted view that the demand for money depends on interest rates then becomes only one reason for some elasticity in the demand for $H$.

It is not difficult to construct models of financial and commodity markets which honor the distinctions among real and financial assets and rates, but the simultaneous adjustment of financial markets and imperfections in markets for existing physical assets make it difficult to utilize them econometrically. The builders of the $MPS$ model, for example, did not find it possible to identify separate supply and demand equations for most of the wide range of financial assets represented in the model. Instead, they rely heavily on a "term structure" equation to tie the markets together. Among other things, this simplifying assumption ignores the cumulative impact of net government borrowing on the relative supplies of bonds, capital, and other assets. As a result, the effect of government debt on the required rates of return on capital cannot be captured in the model. This assumption also restricts the ability of monetary disturbances to affect the demand for real assets without going through a long chain of financial markets. This weakness is partially remedied in the recent Bosworth-Duesenberry model of flow of funds and other models currently under construction, which include a complete set of supply and demand equations for financial assets; but it seems unlikely that such models will be reliably estimated from time series data alone.

During the period under discussion an enormous amount of quantitative em-
pirical work has been done on various sectors of the economy. This work suggests that monetary variables have a substantial effect on expenditures. Interest rates appear to have an important influence on expenditures for plant and equipment and for purchases by state and local governments. In the early 1950's much weight was put on the "availability" of funds, and we know that this is especially important today in influencing housing and perhaps other expenditures as well.

Another linkage between financial markets and expenditures arises from interest-induced changes in asset prices and from changes in the real value of financial wealth in terms of goods and services. In the $MPS$ model, for example, interest rates can have a very large effect on stock market valuations and, even with small marginal propensities to consume capital gains, are responsible for making consumption very responsive to monetary policy. The magnitudes of these effects are open to question. We do not yet understand how much is permanent and how much is regarded by wealth holders as representing temporary vicissitudes in monetary policy, with negligible influence on spending.

There has been much debate over the fourth linkage, with Keynesians emphasizing the impact of increased money income on real output and monetarists emphasizing the impact on prices. Both schools of thought have acknowledged that both effects are generally present in the short run, and the discussion has come to focus on the relative magnitudes of the short-run effects and on whether any effect on output endures in the long run. Of the linkages identified above, this one is the least well understood; forecasting errors on price changes are large, we have virtually no policy instruments to influence the division between $Q$ and $P$, and we have virtually no dynamic adjustment theory to indicate what division to expect
at any moment of time. What empirical evidence we have suggests that we can forecast \( Q \) better than \( V \). That is, in the short run prices are influenced by factors not well captured by the models. Furthermore, the effect of changes in demand on prices appears to be slow.

The above chain of influence assumes, as most economists do, that the central bank is an autonomous agent and that it can determine \( H \), high-powered money. Both assumptions are subject to question, at least under some important circumstances.

In democratic societies, central bank behavior is really endogenous to a model of the political process, so we need to close the circle of causation with a fifth linkage, running from \( P \) and \( Q \) to \( CB \). This is more the domain of political scientists than of economists, but, as noted above, it affects the descriptive value of macro models and introduces simultaneous equation bias into econometric estimates. Moreover, it suggests that economists should not pretend that central banks can pursue freely their own preferences or those of their advisers, and should direct greater professional attention to enlarging and improving the range of instruments, nonmonetary as well as monetary, for influencing target variables.

The extent to which central banks can influence high-powered money can also be questioned. Work on the monetary approach to the balance of payments has reversed the whole chain of influence, at least for a small country with a fixed exchange rate. \( Q \) is determined by real factors, and \( P \) is determined by the exchange rate and the world price level. These together determine \( V \), which determines the demand for money, which in turn influences public behavior to assure adequate supply, if not directly from the central bank, then through the balance of payments. Attempts by the central bank to supply more or less than the desired amount of money will simply result in deficits or surpluses in the balance of payments. David Hume has been rediscovered, and the money supply becomes completely endogenous.

This line of reasoning is less applicable to a large country, to a country whose currency is a reserve currency, and to a world of flexible exchange rates. But it does not become wholly inapplicable under any of these circumstances as they are likely to obtain in reality. Moreover, the use of a national currency as an international currency in private transactions raises serious questions about operational meaning of \( M \): are foreign-held dollar balances related to money income in the United States? Or in the country of the holder? Or both? The substantial size of the Eurocurrency market gives rise to a host of measurement problems which in turn reflect some looseness in our concepts of money.

III. Predictive Accuracy

Better knowledge should lead to better forecasts of the economy. Forecasting out-of-sample offers one of the few genuine tests of whether or not our knowledge of reality has improved and of the influence of policy actions on the economy. In fact, there seems to have been a substantial improvement in our ability to forecast short-term economic developments. The median forecast in the early 1950’s had a mean error of about 2 percent in forecasting nominal GNP four quarters ahead (Victor Zarnowitz). Formal macroeconomic models had a mean error of only about 1 percent in such forecasts in the early 1970’s (S. McNess). It is also clear, however, that forecasting performance is not a powerful tool for testing alternative model specifications. Although the more complicated models appear to have overtaken judgmental forecasts and on aver-
age outperform single-equation or extrapolative forecasts, it is not clear how important elaborate specification of structure is in achieving this improvement. A substantial amount of judgment is typically used in the operation of the more complicated models. Even so, Ray Fair's relatively simple 20-equation model, containing very little structure and used mechanically, performs nearly as well as the more complicated models and appears to outperform them when they are also used mechanically.

These short-term out-of-sample forecasts on aggregate economic variables do not test the model's ability to predict the response of the economy to major changes in policy. In the absence of experiments in policy, our only guide to performance in this regard is the ability of the models to predict well the large number of endogenous variables which transmit the effects of policy actions to the target variables. Unfortunately the record so far is not outstanding (Charles R. Nelson).

One of the problems which plagues macro-model builders is the difficulty of distinguishing adjustment lags, expectations, and serial correlation of errors. These phenomena lead to a high degree of autocorrelation in many economic series, which is one of the reasons that naïve autoregressive predictions do so well in short-term forecasting. They presumably are also the reason that it is typically quite difficult to pass tests of causation which call for strong relationships between movements in economic variables purified of autocorrelation. (See Christopher A. Sims 1972 and C. W. J. Granger.)

Treatment of lags in the early models was very primitive. Part of this reflected the absence of data on a more frequent than annual basis, and it seems clear that annual data do not provide very much information about the lags in most economic relationships. Quarterly models, starting with James Duesenberry, Otto Eckstein, and Gary Fromm, provide a substantial improvement in this respect, but confidence in the reliability of estimated lag structures is still quite low. In addition to all the other difficulties in estimating lag structures, recent work by Sims (1971) and J. Geweke suggests that, due to "stroboscopic" effects, estimation of continuous processes by the use of discrete time observations may yield a fitted lag structure very different from the true lag structure.

A closely related weakness in the models—and in our empirical knowledge generally—concerns the formation and the alteration of expectations. The frequent and convenient use of adaptive expectations implicitly assumes nonrational behavior on the part of the economic actors. Over time, rational expectations will take into account the actions of the policymakers, and this, in turn, can lead to an alteration of the structure of the economy itself, rendering any given model obsolete. Policy-induced institutional change, which has been very important in the U.S. financial sector over the past two decades, is one example of this; regulations create incentives to evade them. Another example is the alleged decline in money illusion and a tendency for investors and savers alike to respond to real rather than nominal interest rates. Under the circumstances of evasive changes in expectations and in economic structure, policymakers might well find that an optimizing strategy would involve deliberately introducing some ambiguity—for example, through randomizing—into their countercyclical actions. Close students of monetary policy might even say that the Federal Reserve has been using this principle from game theory all along.
REFERENCES