UNDERSTANDING THE REMARKABLE SURVIVAL OF MULTIPLIER MODELS OF MONEY STOCK DETERMINATION

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INTRODUCTION

Assuming textbook authors reveal their intellectual and pedagogical preferences and beliefs, a careful survey of the leading intermediate textbooks in money and banking and macroeconomics reveals a uniform and virtually universal consensus—the multiplier model of money stock determination is widely viewed as the most appropriate and presumably most correct approach to the topic. In the leading seller by Mishkin (1982), for example, three chapters and a total of 64 pages (about 8½ percent of the text) are devoted to the development of the multiplier model. In justifying this extensive treatment, Mishkin argues “the complete model is the basis of much of the money supply analysis performed by practicing economists in the private sector and the government” (1982, 265). Since such consensus is not, in general, an enduring characteristic of monetary economics, one is tempted to “let sleeping dogs lie.” The problem is that the multiplier model, whether viewed from an analytical or empirical perspective, is at best a misleading and incomplete model and at worst a completely misspecified model.

The purpose of this paper is to assess the use and usefulness of the multiplier model from both the point of view of Federal Reserve policymaking, especially with regard to the operations of the Trading Desk, and with regard to ongoing theoretical and empirical work outside the Fed. The basic themes examined and developed can be simply stated: (1) ignoring various institutional and structural “details” has devastating implications for a large body of received theoretical and empirical work and the positive and normative economics which motivates and flows from it; and (2) the devastating implications relate mainly to the short-run (1-6 months) relationships among money, reserves (or the monetary base), and interest rates and do not necessarily contradict the proposition that, in general, the Fed, if it so chose, could control the growth of money within a 1-1½ percent range over a 6-12 month period.

DETERMINING VS. CONTROLLING VS. PREDICTING THE MONEY STOCK

Two familiar identities seem to dominate monetary economics: (1) \( MV = Y \) and (2) \( M = mB \), where \( M = \) the money stock (somewhat measured), \( V = \) velocity, \( Y = \) GNP, \( B = \) the monetary base, and \( m = \) the multiplier. Some have given new meaning to the term “reduced-form” by combining (1) and (2) into \( 3mB = Y \). This, along with the assumptions that \( m \) and \( V \) are “predictable” (i.e., stable stochastic processes), are orthogonal with respect to \( B \) and each other, and that \( B \) is controllable, implies that \( Y = (RB) \). Such expressions have provided the basis for a huge volume of empirical research, including the profession’s recent infatuation with vector autoregressions, on such issues as the controllability of money, the relationship between the monetary and real sectors of the economy, the relationship between monetary policy and exchange

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growth of the money stock within a relatively narrow band over a 6-12 month period. Remarkably, these results seem to hold even during periods characterized by substantial financial innovation and deregulation.

The above discussion is presumably quite familiar and needs only elaboration. The questions before us, in light of the many papers which have been written on the multiplier approach, are why does the Fed largely ignore it (the Federal Reserve Bank of St. Louis being a notable exception), and why does it survive?

Critiques of the Multiplier Approach

Critiques of the multiplier approach have seldom been in short supply. The major elements of the critique, many of which are related, are summarized below.

The Multiplier Model is Not Structural, But Rather is a Reduced-Form. First argued by Gramley and Chase [1965], and other adherents to the “New View”, such as Tobin [1965], the fundamental question raised was “Is the multiplier model a theory of money supply?” If so, a theory of money demand is needed to determine the equilibrium level of the money stock. If not, then the multiplier approach, and the asset preferences which determine its component ratios, depend directly on money and credit demand and thus implicitly and indirectly on the rate-setting and deposit- and loan-offering functions of depository institutions.

Succinctly stated, the critique emphasizes that the multiplier approach abstracts from the short-run dynamics of adjustment by banks and the public, leaves the role of interest rates implicit rather than explicit, and proceeds on the assumption that movements in the monetary base (or reserves) are orthogonal to fluctuations in the multiplier. The multiplier model, it is argued, implies that deposit expansion is **quantitatively constrained** through the Fed’s control over the sources of bank reserves (chiefly, the Fed’s portfolio of securities). One of the most forceful and articulate exponents of the critique, Basil Moore, concludes that “as a result, the money-multiplier framework is of no analytical or operational use” [1988, 70].

The consensus view of the staff and policymakers within the Federal Reserve, as revealed in numerous publications [Bryant 1983, Lombra and Kaufman 1984, and references cited therein], embraces much, if not all, of the critique advanced by Moore and others. In particular, the Fed adheres to the view that the system is equilibrated through the movement of interest rates, which, through their effects on bank reserves and costs, determine banks’ and the public’s desired asset and liability positions. In this view, money and reserves are “controlled” by using open market operations to affect interest rates which in turn affect demand and thus the uses of bank reserves (chiefly, required reserves). The implication, when combined with “the Lucas critique”, is that changes in Fed regulations, Fed operating procedures, and the resulting behavior of depository institutions and the public can be expected to alter the process generating the multiplier.

There is little doubt Brunner and Meltzer [1990], for example, recognize that the multiplier is a reduced-form outcome of their rather elegant macro model. Accordingly, they implicitly assume that the myriad of possible influences on the multiplier process turn out in practice to be rather unimportant. More specifically, the implication is that the relevant time-series models can be easily updated to capture quantitatively important changes in “the rules of the game”.
Reserves (and the Monetary Base) in Practice Have Been Endogenously Determined. This contention, which is related to the lagged reserve accounting scheme in effect from September 1969 to February 1984 and the Fed's interest rate operating procedure in effect for virtually the entire post-Accord period, implies the multiplier model is completely irrelevant for the determination of the money supply (Goldfriend, 1982; Hezel, 1987; Lombra and Kaufman, 1984; 1992; Friedman, 1990). The effect of lagged reserve accounting, regardless of the operating procedure, is easily illustrated within the following simple weekly model.

\[ R_t^e = \alpha_k + \alpha_i + \alpha_i R_t^e + \alpha_M M_{t-2}^e + u_t \]

\[ NBR_t = R_t^e - BR_t^e \]

\[ BR_t = \gamma_t + \gamma_t (R_t^e - BR_t^e) + u_t \]

\[ NBR_t = \bar{NBR} \]

\[ i_t = \bar{i}_t \]

\[ BR_t = \bar{BR} \]

\[ M_t^e = \beta_k + \beta_i i_t + \beta_Y Y_t + v_t \]

where

- \( R_t^e \): banks' demand for total reserves (during week \( t \))
- \( i_t \): "the" interest rate
- \( M_t^e \): money stock two weeks ago
- \( NBR_t^e \): banks' demand for nonborrowed reserves
- \( BR_t \): banks' demand for borrowed reserves
- \( i_t \): Federal Reserve discount rate
- \( NBR_t^e \): supply of nonborrowed reserves
- \( Y \): income (assumed exogenous)

Equation (1) is a demand function for total reserves. It reflects banks' demand for required reserves, which is a function under lagged reserve accounting, of the money stock two weeks ago (and, under contemporaneous reserve accounting, a function of the current money stock), and banks' demand for excess reserves, which may be a function of the interest rate (and perhaps other variables impounded in the constant term). Equation (2) is an identity; banks' demand for nonborrowed reserves, which reveals itself in the Federal funds market, is equal to the demand for total reserves minus banks' demand for borrowed reserves. The latter, as shown in equation (3), is determined by the difference between the market interest rate and the discount rate. Equations (4a), (4b), and (4c) depict the three operating strategies utilized by the Fed since 1970: equation (4a), fixing the Federal funds rate (1970 through September 1979, and currently); equation (4b) fixing the supply of nonborrowed reserves over a week (October 1979 through mid-1982); equation (4c) setting an objective for borrowed reserves (late 1982 through late 1983).
FIGURE 1

Time Varying Estimates of GI

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The predictive accuracy of multiplier models is considerably overstated. There are several levels to this particular aspect of the critique. First, there is some debate over what constitutes a "small" vs. "large" error. More specifically, should the one-month errors be annualized? Should the monthly errors be averaged over several months? Obviously, if the errors are not annualized and tend to cancel out somewhat over a quarter (as seems to be the case), the multiplier model will appear to perform better than otherwise.

Part of the difficulty here relates to the different time horizons or control horizons prevailing in and outside the Fed. Naturally enough, the Trading Desk, for example, is concerned with the here and now and tends to emphasize the difficulty in projecting the factors affecting reserve supply, the often erratic course of banks' demand for excess reserves and borrowed reserves within and across reserve maintenance periods, and more generally, the short-run volatility and associated unpredictability of the multiplier. Put another way, the "long run" on Wall Street, and perhaps even Liberty Street (the home of the Federal Reserve Bank of New York), often seems much shorter than the horizons conditioning the empirical work and policy evaluations conducted outside the Fed. Needless to say, such differences help to account for the frequent failure to communicate effectively. As Benjamin Friedman once pointed out, critical reactions by policymakers and their staffs to academic work, "at times give the impression that the Federal Reserve can precisely control no variable familiar in the discussions of monetary economists" (1977, 92).

The second level of this aspect of the critique is more analytical. It is argued by Lindsey, et al. (1984) and Bryant (1983), for example, that the endogeneity of the base and the multiplier raises serious questions about the robustness and reliability of the empirical work surrounding the multiplier approach. When Lindsey, et al., attempt to correct empirically for the biases introduced by assuming exogeneity and orthogonality within the confines of several alternative models, they find that multiplier models overstate considerably the precision of short-run monetary control. In addition, Bryant (1983, 78) argues against averaging out the resulting large monthly errors because it begs the question of whether the variability of the money stock, and presumably other variables such as interest rates, matter. As Bryant concedes, however, if the policy prescriptions flowing from the multiplier approach are taken to imply that the Fed can control money growth fairly closely over a year or so, then "the deed can be done" (1983, 79).

In the end, this debate is a vivid example of recent discussions about the "rhetoric of economics"—involving, in particular, the realism of assumptions, the use and misuse of significance tests, the role of priors, and the relationship between prediction and hypothesis testing. Simply recognizing this helps one distinguish among the competing arguments.

SO WHY DOES THE MODEL SURVIVE?

A decade ago Hershel Grossman wrote an informative paper on the remarkable survival of non-market-clearing models (1983). In addition to playing off his title, I found it useful to compare the basic thrust of his analysis to that presented above. Basically, he argued that non-market-clearing models survive because the market-clearing approach has not been all that successful empirically and because non-market-clearing models have evolved theoretically to the point where the natural-rate hypothesis and rational expectations are routinely included. Remarkable parallels are evident in the case of multiplier models of money stock determination as compared to structural models. First, the multiplier models, given the longer time horizons emphasized, have continued to track monetary growth reasonably well [Rausche and Johannes 1987]. Second, and perhaps more importantly, recent expositions of the multiplier model have conceded many of the points raised by the critique. For example, Garfinkel and Thornton conclude "that the multiplier is affected by policy actions suggests that money stock control using the multiplier model would be enhanced by taking the effort of policy actions on the multiplier into account" (1991, 62). At even a deeper level, consider the following from Brunner and Meltzer (1966, 333-338):

"Models in which money...is determined endogenously...cannot clarify issues about monetary control" [1966]; "...differences in monetary regimes..."
are associated with differences in supply conditions of the source base” [370]; within an interest rate operating procedure, “...the base replaces the interest rate as an endogenous variable” [380]; “Substantial and persistent reverse causation occurs most often when central banks, under fixed exchange rates or interest rate control policies, supply base money on demand” [393].

Third, the structural approach emphasizing demand-side considerations, which has dominated Federal Reserve analysis of money stock determination, did not hold up well during the late 70s and early 80s — the period of significant financial innovation and deregulation. More specifically, Karamanis and Lembra [1989] found that the root mean square error for the Fed staff’s quarterly M1 forecasts over the 1979-1982 period was 5% on average. There, more than twice as large as the projection errors for the 1970s. A major part of the difficulty was at the theoretical level. The dynamic rate-setting behavior of depository institutions, and the Fed’s policy for the multiplier model is more easily understood.

Against this background, once one takes account of the Fed’s several missions and the resulting focus on the short run, in general, and on the financial system’s “plumbing” (e.g., reserve accounting schemes, reserve carryovers, discount window administration, overdrafts, and wire transfers), in particular, the Fed’s seeming disinclination for the multiplier model is more easily understood.

On the academic side, the simplicity and tractability of the multiplier approach, like that of the Keynesian multiplier (relating changes in national income to changes in autonomous spending) are attractive pedagogically. However, unlike the Keynesian multiplier, which is typically “unkneaded” in higher level courses, the money multiplier model lives on with model-builders who are confirmed adherents to the Law of Parity, as well as to the view of the exchange rate. The omitted variable and identities are tightly linking reserves (or the base) and money over the longer run provide all the most comfort empiricists must proceed on, but concerns noted above remain plausible.

Where does this leave us? If, as Goodhart [1988, 136] claims, “the information content of the multiplier model is remarkably slight,” one might be tempted to conclude that the demise of the model as an engine of analysis—empirical as well as theoretical—is inevitable. However, as McCloskey has argued, “the doubting and falsifying method, embodied in the official version of econometric method, is largely impractical” [1985, 14]. The implication is that, whereas the discipline, it is doubtful that the ready availability of data banks, computer terminals, and regression runners, will produce definitive evidence which falsifies the multiplier model.

\[ R = s + m + n + o + p + q + r + s + t + u + v + w \]

\[ s = m + n + o + p + q + r + s + t + u + v + w \]

\[ s = m + n + o + p + q + r + s + t + u + v + w \]

1. For example, consider a simple case where money and time and savings deposits are ignored: \[ M = M_{1} + M_{2} + M_{3} + M_{4} \]


3. Different models would, of course, yield qualitatively different structures. For example, explicit considerations of the credit market, as in Brunner and Meltzer [1969], would make the demand for credit as well as the ability to obtain funds to the equilibrium solution. Similarly, incorporating a demand function for domestic currency, in the model, and expected effects rates generally would alter the solution, while in fact the solution. In the long run, the Fed must find the funds rate.

4. Within the confines of the model developed in the text, \[ M^{*} = (x + y + z) \]

5. See, for example, Goodhart [1988], and the literature cited therein. Since then, of course, the Federal Reserve Bank of New York and the Federal Reserve Board staff have been at the forefront in analyzing the implications of the observation that deposit change is a more stable source of money, and that the Fed is more successful in controlling the liabilities of depository institutions. See, for example, Muth, Rental, and Small [1968] and the literature cited therein.

REFERENCES


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ENDOGENOUS TECHNOLOGICAL ADVANCE AND POSTWAR ECONOMIC GROWTH: A PRODUCTION FUNCTION ANALYSIS

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I. INTRODUCTION

A central issue in the determination of long-term growth rates is the rate of technological advance. It is well established that on the supply side, about one-third of postwar growth in the United States is an unexplained residual — multifactor productivity in the national income accounts — which is understood to encompass technological advance, although the two measures are not coterminous. In many studies, the rate of technological advance has been approximated as a deterministic trend, either fitted to the residual or multiplying one of the other factor inputs. Deterministic models, however, are unsatisfactory for two reasons. First, as theoretical grounds, technological advance should be considered endogenous [Kohler, 1988]. Second, on empirical grounds, the existing measures of technological advance all exhibit stochastic behavior. This paper specifies a growth model in which the rate of technological advance is determined by the flow of services from the Research and Development (R&D) stock. In this framework, it is established that technology can account for a substantial share of the residual variation in output. The residual can be further reduced by taking the public sector capital stock into account. Section II examines the issue of technological advance in the production function. Section III specifies the model for the R&D stock. Section IV discusses the estimation procedure. Section V estimates the contributions of R&D and government capital to growth.

II. THE PRODUCTION FUNCTION

A useful starting point for analyzing the impact of technological advance on economic growth is the neo-classical framework originated in Solow [1957]. The production function is in two factors (\( X, L \)), but can be generalized to any arbitrary order. Let \( X \) denote the factor inputs of a \( j \)-order production function, and let \( t \) denote a deterministic time index:

\[
\ln Y_t = \ln \alpha_t + \ln X_t \beta + \mu_t = \ln (dy/dt) + \ln Y_t - \ln (dy/dt)X_t \beta
\]

where \( Y \) is real output and \( P = \text{price} \). The corresponding log-linear production function is simply:

\[
\ln Y_t = \alpha_t + \beta \ln X_t + \mu_t
\]

where \( \alpha_t \) are elasticities of output with respect to inputs.

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