How Much Does Money Matter? A Look at Some Recent Evidence

By RICHARD G. DAVIS
Adviser, Research and Statistics Function
Federal Reserve Bank of New York

Editor's Note: *The following is a paper presented at a Money and Banking Workshop held on May 9, 1969 at the Federal Reserve Bank of Minneapolis. It is being reprinted in this* Review *for the benefit of our readers who are interested in the current controversy surrounding the role of the money supply. The views expressed are the author's and do not necessarily reflect those of the Federal Reserve Bank of New York or of the Federal Reserve System.*

The air has been filled of late with signs of upheaval in long-established patterns of thinking regarding monetary and fiscal policy and, more generally, regarding the role of money in the economy. The basic framework used for years by most of us in analyzing these matters has come under serious challenge. Signs of intellectual disarray are evident all over, among public officials, in the business press, and among academics. Indeed, the current sense of confusion may well exceed anything witnessed since the early 1930's. Obviously the questions of "how much does money matter?" and "in what way does money matter?" are central issues underlying many specific problems of policy making, forecasting, and business-cycle analysis that are currently up for reexamination.

The change in atmosphere has been a very recent phenomenon. Only two or three years ago, there was rather general agreement that a wide variety of factors could produce fluctuations in business activity. Monetary developments were but one item in the list and, in the minds of many, by no means the most important. Money was assumed to operate through its effects on financial interest rates and through changes in the degree of credit rationing impinging on certain types of borrowers operating in imperfect capital markets.

The view that "only money matters" or, perhaps more accurately, that "mainly money matters" was the province of an obscure sect with headquarters in Chicago. For the most part, economists regarded this group—when they regarded it at all—as a mildly amusing, not quite respectable collection of eccentrics. The number of serious attempts to grapple with the Friedman view on the role of money until recently has been remarkably small. A 1960 paper by John Culbertson,[1] some work by Kareken and Solow on Friedman's approach to measuring lags,[2] and the papers surrounding the controversy over the Friedman-Meiselman work[3] comprise a nearly exhaustive list of the pre-1968 literature. The fact is that the view held by Friedman and a few others on the predominant

[1] See his "Friedman on the Lag Effect of Monetary Policy", *Journal of Political Economy* (December 1960), pages 617-21. See also Friedman's reply, "The Lag Effect of Monetary Policy" in the October 1961 issue of the same *Journal* (pages 447-66) as well as Culbertson's rejoinder in the same issue (pages 467-77).

[2] John Kareken and Robert Solow, "Lags in Monetary Policy", in *Stabilization Policies* (Commission on Money and Credit, 1963), pages 14-96.

[3] Milton Friedman and David Meiselman, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States", in *Stabilization Policies* (Commission on Money and Credit, 1963), pages 165-268. See also Donald Hester, "Keynes and the Quantity Theory: A Comment on the Friedman-Meiselman CMC Paper", and the Friedman-Meiselman reply in the *Review of Economics and Statistics* (November 1964). In addition, see Albert Ando and Franco Modigliani, "Velocity and the Investment Multiplier"; Michael dePrano and Thomas Mayer, "Autonomous Expenditures and Money"; and replies by Friedman and Meiselman plus rejoinders by Ando-Modigliani and dePrano-Mayer, all in the *American Economic Review* (September 1965), pages 693-792.

importance of money was just not given serious attention by most economists.

This whole situation has been changing of late with a rather startling abruptness. Indeed as far as the general public is concerned, much of the change in atmosphere has occurred within the last six to nine months. There can be no doubt that the principal explanation for this development has been the surprisingly exuberant behavior of the economy since the tax surcharge was enacted last June. Most forecasters who, like myself, have used the usual techniques of short-term projecting, hopefully with at least average skill, have consistently and fairly substantially underestimated the strength of the economy in the past nine months. Now of course these forecasting mistakes may not have been due to any overestimate of the potency of fiscal policy or an underestimate of the importance of the monetary growth rate. Many other explanations are possible. Nevertheless, it has been distinctly unsettling to see the projected slowdown recede further and further into the future, month after month and quarter after quarter. It is the sort of experience to make one reexamine one's "maintained hypotheses"—and perhaps such a reexamination is really in order.

The failure of conventional forecasting techniques in the wake of fiscal restraint would not, of course, necessarily send one running to the money supply for an explanation were there not a large body of research on the importance of money already waiting in the wings. This research needed only the right historical moment to bring it forth into the limelight. The post-surcharge experience has provided such a moment. Looking at the evidence in behalf of a dominant role for money presented in most of this research, however, it is not too difficult to understand why it achieved relatively little acceptance for the monetary view—on its own merits, so to speak, and without the psychological benefit of the post-surcharge trauma.

EVIDENCE FOR THE MONETARY POSITION

By far the largest mass of evidence consisted of the Friedman-Schwartz measurement and comparison of specific cycles in the monetary growth rate with reference cycles using the standard National Bureau techniques.[4] The consistency with which cycles in the monetary growth rate were found to lead reference cycles was so nearly perfect that it seemed scarcely possible that it could be due to chance alone. The existence of these timing leads was interpreted by Friedman and Schwartz to mean a dominant causal role for money, while their length and variability was taken as meaning a corresponding length and variability in the lags with which the money supply exerts its influence on business activity.

It is probably fair to say that relatively few not already in the fold were converted by these arguments. The main problem is that the evidential value of the monetary leads in trying to demonstrate a dominant causal role for money, as well as a long and variable lag in its timing, is gravely compromised by the possibility of a reverse influence of business on money. This point was made back in 1960 by Culbertson in the article mentioned earlier. At the time he wrote, little work had been done on the supply side of the money problem. Hence the existence of important reverse effects of business on money could really only be put forward as a plausible hypothesis. This situation was changed in 1965 with the publication of Phillip Cagan's book, *The Determinants and Effects of Changes in the Stock of Money*. While Cagan's book appears to be very much in the Friedman tradition, it seems to me that its results in fact tend to undercut that tradition. Cagan's work suggests rather clearly that the characteristic cyclical timing relationships between monetary rates of change and the business cycle are very importantly determined by the influence of business on money. The case is made even stronger if one takes explicit account—as Cagan does not—of the impact of the Federal Reserve's attempts at countercyclical policy. Federal Reserve behavior alone may be sufficient to explain the characteristic lead of cycles in the monetary growth rate ahead of business-cycle turning points during the postwar period without the need to posit any influence of money on business whatever. If Cagan's work is correct, then the massive evidence gathered by Friedman and Schwartz for leads in the turning points of monetary cycles seems distinctly questionable as evidence for a dominant causal role for money.

The second sort of evidence in behalf of the "mainly money matters" position, as of last June, was the so-called "reduced-form" equations turned up in the famous Friedman-Meiselman paper. As will be recalled, Friedman and Meiselman regressed first the money supply against consumption and then what they called "autonomous" spending against consumption. They regarded their results as "strikingly one-sided"[5] in support of the money

[4] Their major results are summarized in Milton Friedman and Anna Schwartz, "Money and Business Cycles", *Review of Economics and Statistics* (February 1963 Supplement), pages 32-64.

[5] Friedman and Meiselman, *op. cit.*, page 166.

multiplier over their version of the Keynesian multiplier. Again, however, it seems unlikely that many outside the fold were converted by the Friedman-Meiselman evidence. As the mass of correlation coefficients computed by the various combatants began to pile up, it became increasingly obvious that the controversy would never be able to produce a clear and decisive verdict. Results turned out to depend very much on the definition of "autonomous" spending and on the years for which the computations were made. Using definitions of autonomous spending much more akin to those of the usual textbook versions of Keynes than the one adopted by Friedman and Meiselman, little difference between the money multiplier and the autonomous spending multiplier could be discerned. Moreover, interpretation of all the results was complicated by the extreme simplicity of the "models" chosen to be tested, as well as by the inability of correlation techniques to distinguish an influence of money on business from an influence of business on money. To me, at least, the whole thing was a washout, proving nothing and making the Friedman position seem not one jot more plausible than it had seemed before.

Thus those economists, journalists, and public officials who began to take a hard look at the evidence for the monetarist position in the wake of the apparent failure of the tax surcharge found a rather mixed bag. On the one hand, Friedman and his colleagues had established beyond question a very substantial gross association between the money supply and business activity. On the other hand, the monetarists had failed to convince the majority of their professional colleagues that their claims for the importance of money had been adequately demonstrated. The reasons for their failure lay mainly in relatively esoteric matters of statistical methodology and economic theory. Such problems have obviously made much less impression on the lay public than has the simple fact of the gross association between money and business itself.

THE ST. LOUIS EQUATION

Under these circumstances, a new study of the importance of the money supply, and its importance relative to fiscal policy, was certain to be welcome. The paper by Leonall Andersen and Jerry Jordan published last November[6] in the St. Louis Federal Reserve Bank's *Review* has understandably created a good deal of interest. Their approach consists of regressing current changes in gross national product (GNP) on current and lagged changes in the money supply and in fiscal variables. Certainly their procedure is very simple but not, for that reason, necessarily invalid. My own feeling is that, right or wrong, the St. Louis article is a distinctly worthwhile contribution to the literature on the importance of money. It deserves to be taken seriously and, by the same token, it deserves careful scrutiny. I propose to use the remainder of this paper to examine the claims made by Andersen and Jordan.

Table I

THE FISCAL MULTIPLIERS IN THE BOARD-MIT AND ST. LOUIS MODELS

Elapsed time	Board-MIT		St. Louis	
	Spending	Taxes	Spending	Taxes
After 1 quarter	2.0	1.1	0.4	0.2
After 2 quarters	2.5	2.2	0.9	0.2
After 4 quarters	3.4	3.2	0.1	0.2
After 12 quarters	3.2	4.7	0.1	0.2

Note: Figures for the Board-MIT model are estimates made from the simulation presented in Charts 8 and 9 on pages 28 and 29 of the January 1968 *Federal Reserve Bulletin*. The tax simulations in the Board-MIT model are actually in terms of percentage point changes in tax rates, but it is noted on page 23 of the *Bulletin* that the .02 percentage point change used for the simulations was equivalent to about $4 billion during the period for which the simulations were conducted.

The first thing I want to note about the St. Louis equation is that it portrays a world in several respects sharply at variance with the expectations of most of us.

1. The fiscal multipliers in the St. Louis world are virtually zero. The fiscal multipliers for spending and taxes taken directly from the St. Louis equation are shown in Table I. At no point do these multipliers rise above unity, and after four quarters they have returned essentially to zero. Multipliers taken from the recent Federal Reserve Board staff-Massachusetts Institute of Technology structural econometric model are shown in the same Table I. These multipliers, by contrast, correspond roughly to expectations, rising to over 3 after a year.

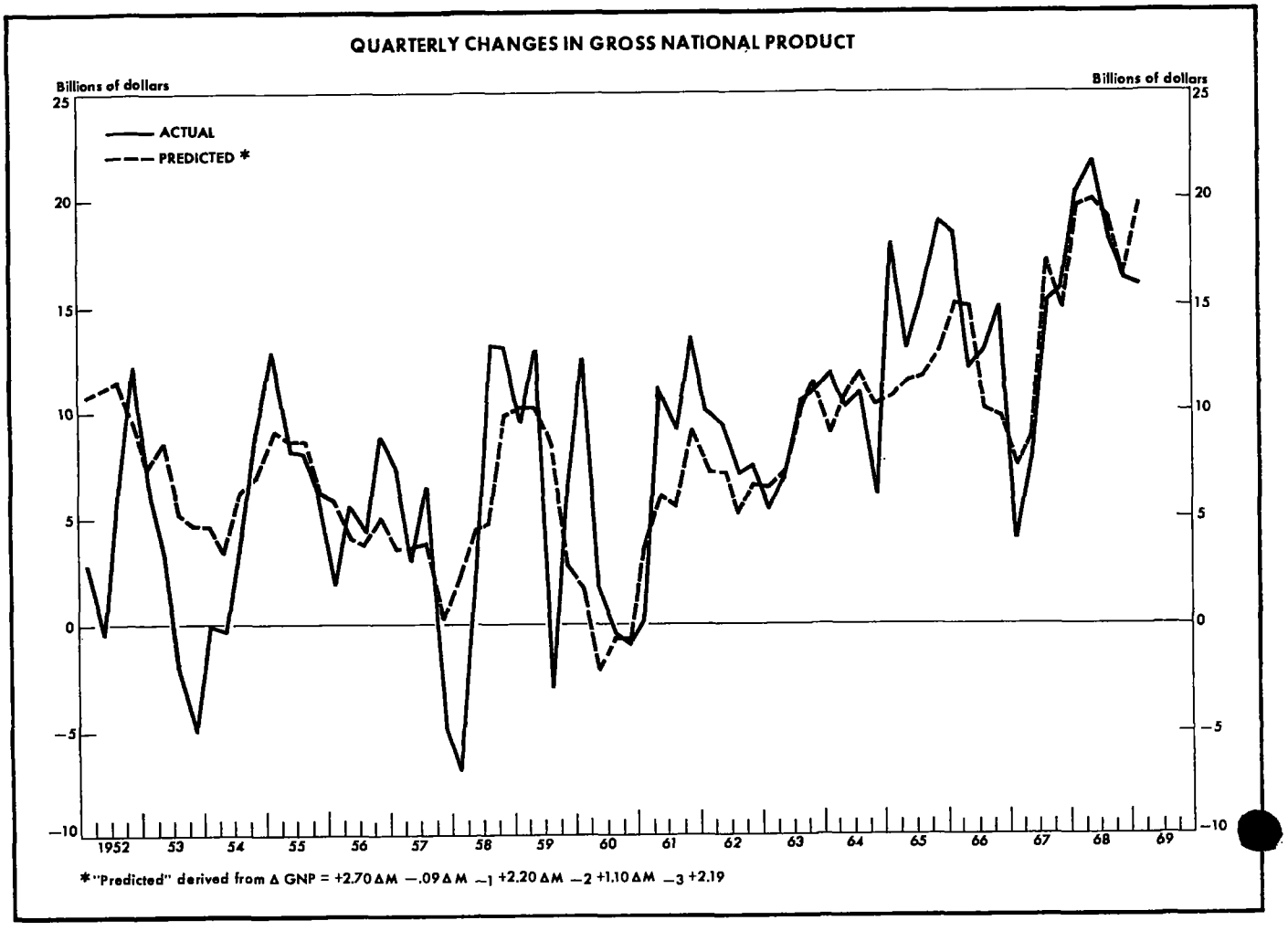
2. Current and lagged changes in M_1 (private holdings of demand deposits and currency) explain a remarkably high proportion of the variance of quarterly changes in GNP. For the 1952-68 period used by St. Louis, about 50 percent of the variance of changes in GNP is "explained" by changes in money. This leaves the remaining 50 percent to be accounted for by every other possible determinant of the course of business activity!

[6] "Monetary and Fiscal Actions: A Test of Their Relative Importance in Economic Stabilization", Federal Reserve Bank of St. Louis *Review* (November 1968), pages 11-24.

Just how well money does is illustrated in the chart. The equation used here differs from the St. Louis equation only in omitting the fiscal variables and in having an unconstrained lag structure. When it is kept in mind that the chart shows *changes* in GNP, I think the closeness is visually reasonably impressive. The only prolonged period of really serious errors is from the beginning of 1952 to about mid-1954. On the other hand, the equation's accuracy since mid-1967 has been extremely high.

3. The size of the money multiplier is much higher than might have been expected. As Table II shows, a once-and-for-all increase in the money supply of $1 billion in quarter 1 will have raised the level of GNP by $6.6 billion by quarter 4, according to the St. Louis equation. This result can be compared with the money multiplier implicit in the Board-MIT model. The money supply is of course an endogenous variable in this model, and so really has no "multiplier" as such. However, the arithmetic multiplier can be computed by dividing the GNP multiplier of nonborrowed reserves by the money multiplier of nonborrowed reserves. (This procedure includes reverse effects of business on money, to be sure, but so does the St. Louis equation. More later on this.) After four quarters, the money multiplier in the Board-MIT model is only about 0.4. Even after twelve quarters, it is still only about 2.2, or only one third the size of the multiplier implicit in the St. Louis equation.

4. To me, the most surprising thing about the world of the St. Louis equation is not so much the force, but rather the speed with which money begins to act on the economy.



This can also be seen in the results presented in Table II. If the level of the money supply undergoes a $1 billion once-and-for-all increase in a given quarter, it will already raise GNP by $1.6 billion in that very same quarter. In the next quarter, the level of GNP will have risen to $3.5 billion above what it would otherwise have been. And, as noted, it will have risen to $6.6 billion higher in the fourth quarter. In the Board-MIT model, by contrast, a once-and-for-all increase in M_1 of $1 billion in a given quarter would have almost no effect whatever on GNP in that quarter, and only very little effect by one quarter later. Even after four quarters, the level of GNP is only about $400 million higher than it would otherwise have been.

I think it is clear from this summary that what is at stake in the case of the St. Louis equation is not merely a "shade of difference", but a strikingly contrasting view of the world—at least relative to what is normally taken as the orthodox view roughly replicated and confirmed both in methods and in result by the Board-MIT model. The validity and meaningfulness of the St. Louis equation is thus a question of some importance. There seem to be two basic issues that need to be examined: First, how good are the purely statistical properties of the sort of relationship presented by Andersen and Jordan? Second, how much of the relationship they find is due to a reverse influence of business on money, the problem that has plagued all previous attempts to buttress the so-called "strong monetarist position"?

Before turning to these questions, I should note that I have concentrated all my attention in what follows on the question of the importance of money. I will have virtually nothing to say on the seeming unimportance of the fiscal variables. This latter problem is of course also of great interest, but there has simply not been time to give adequate attention to both issues.

Table II

THE MONEY MULTIPLIER IN THE BOARD-MIT AND ST. LOUIS MODELS

Elapsed time	Board-MIT			St. Louis multiplier
	ΔM_1	Δ GNP	Multiplier	
	(1)	(2)	(3)	(4)
After 1 quarter	+2.5	+ 0.5	0.2	1.6
After 2 quarters	+3.5	+ 1.3	0.4	3.5
After 4 quarters	+4.3	+ 2.0	0.4	6.6
After 12 quarters	+5.0	+11.0	2.2	6.6

Note: Changes in M_1 and GNP were estimated from charts presented on page 27 of the January 1968 *Federal Reserve Bulletin*. These charts show results for the Board-MIT model in which the effects of a $1.0 billion increase in nonborrowed reserves are simulated. The implied "money multiplier" shown in column 3 is computed by dividing the change in GNP (column 2) by the change in money (column 1). Actually only effects for demand deposits are shown—currency effects are thus assumed to be comparatively small.

The money multiplier shows the change in the level of GNP after the time period specified associated with a once-and-for-all increase in the level of the money supply of $1.0 billion. Estimates of the money multiplier for the St. Louis equation are obtained simply by summing coefficients over the appropriate number of quarters.

OTHER ST. LOUIS-TYPE EQUATIONS

Table III presents a number of St. Louis-type equations covering different time periods and using different techniques for estimating the lag structure. The first line of figures contains the original St. Louis results for their 1952 to 1968-II period. The second line reproduces these results with two differences: (1) the fiscal variables were not included and (2) a second degree polynomial was used for the Almon lag rather than St. Louis' fourth degree. (This latter adjustment was made solely to accommodate programming limitations.) A comparison of these two equations reveals that neither omission of the fiscal variables nor reduction of the degree of the polynomial has any substantial effect on the results for the monetary variables.

Concentrating on this second set of equations in Table III using the second degree polynomial, the following observations seem pertinent. First, the coefficients for the two subperiods are quite clearly different, but they are not drastically different. The overall money multiplier after four quarters is 3.6 for the earlier period and 5.3 for the later period. Both are strikingly larger than the 0.4 one-year multiplier of the Board-MIT model. The St. Louis equation does pass the Chow test for the two subperiods at the 5 percent level.

Second, the explanatory power of the monetary variables is quite low in the first half of the period ($R^2 = .18$)[7] and quite high ($R^2 = .62$) in the second half of the period. What is the reason for this difference? One possible answer is a strong common time trend in changes in M_1 and in GNP present in the 1960's but not in the earlier period. Such a trend beginning around the early 1960's is readily visible in the chart referred to earlier. (The R^2 of time alone on changes in GNP is .38 in the later period, virtually zero for the earlier period and .36 for the entire 1952 to 1968-II period.)

Now of course the mere existence of a common time trend in the 1960's does not necessarily mean that the close relationship between money and GNP is spurious.

[7] R^2 is the square of the multiple correlation coefficient.

Table III

CHANGES IN GNP REGRESSED ON CURRENT AND LAGGED CHANGES IN M_1

Quarterly changes

Period	R^2 SE of est	Sum of coefficients	t	t-1	t-2	t-3
	The St. Louis equation† Almon lag with fourth degree polynomial					
1. 1952-68*	.56 4.2	6.6	1.6 (2.2)	1.9 (3.6)	1.8 (3.4)	1.3 (1.9)
	Equations using Almon lag with second degree polynomial‡					
2. 1952-68*	.48 4.6	5.6	1.6 (3.0)	1.7 (7.6)	1.4 (4.3)	0.9 (3.0)
3. 1952-60	.18 5.1	3.6	1.7 (1.7)	1.1 (2.6)	0.6 (1.1)	0.2 (0.5)
4. 1961-68*	.62 3.2	5.3	0.8 (1.6)	1.6 (6.4)	1.7 (5.4)	1.2 (4.5)
	Equations using unconstrained lag coefficients‡					
5. 1952-68*	.50 4.6	5.9	2.7 (3.3)	—0.1 (—0.1)	2.2 (2.0)	1.1 (1.3)
6. 1952-60	.18 5.3	3.7	1.9 (1.1)	0.6 (0.3)	1.2 (0.5)	— .05 (—)
7. 1961-68*	.68 3.0	5.7	2.0 (2.8)	—0.3 (—0.3)	2.6 (2.8)	1.4 (2.0)

Note: Values of "t" statistics are indicated in parentheses.
* Through the second quarter of 1968.
† Fiscal variables included but not shown.
‡ No fiscal variables included.

One could argue that we have had relatively steady rises in quarterly GNP increments because we have had relatively steady rises in quarterly money increments. Some equations including time as an explicit variable suggest that there may have been a reasonably strong association between GNP and current and lagged changes in M_1 during the 1960's even after time is allowed for. Nevertheless, the fact remains that the degree of association between GNP and current and lagged money supply would have looked very different to Andersen and Jordan had they done their work in 1961 instead of 1968. Given an R^2 as low as prevailed in the 1952-60 period, it may be doubted that they would have felt it worthwhile to pursue the matter further.

Finally, I would like to point out the bottom three equations shown on Table III. These are simply unconstrained multiple regressions of changes in GNP on current and lagged changes in M_1. Comparison with the other lines in the table shows the extent to which imposition of Almon lags changes the results. The lag pattern present in the unconstrained equations is, of course, infected with multi-collinearity. Nevertheless, it is interesting to note that the one-period lag coefficient for the entire 1952-68 period, which is the *largest* for the Almon lags, actually becomes the *lowest* in the unconstrained equation.

PROBLEM OF TWO-WAY CAUSATION

I now want to turn to the problem of two-way causation. That there is a high "gross" association between money and business, however measured, has long been apparent from the work of Friedman-Schwartz and Friedman-Meiselman, as noted earlier. The St. Louis results generally confirm this finding—at least for the 1960's. As I noted, however, the possibility of important influences running from business to money seem to weaken substantially the evidential value of the work done by Friedman and his collaborators in trying to establish a dominant *causal* role for money. The question now is, does the St. Louis study suffer from the same fatal defect?

In a critique of the St. Louis work published in the April issue of the St. Louis Bank's *Review,* Frank deLeeuw, one of the principal architects of the Board-MIT model, and John Kalchbrenner take note of the two-way causation problem.[8] They note that the "reduced-form" approach used by St. Louis requires that the variables on the right-hand side be truly exogenous. If they are not, biased coefficient estimates may result. Conceivably, such bias could account for the surprisingly powerful and quick-acting effects of money seemingly indicated by the coefficients of the St. Louis equation. DeLeeuw and Kalchbrenner believe that Andersen and Jordan recognized the vulnerability of the money supply as an exogenous variable and that it was for this reason that they constructed an alternative version of their equation. In this alternative version, current and lagged changes in M_1 are replaced with current and lagged changes in the monetary base *(adjusted for changes in reserve requirements).*[9] This alternative version of the St. Louis equation is reproduced on the top line of Table IV. The explanatory power is similar to that of the money supply equation. The size of the multiplier, however, is naturally much larger since it represents the combined effect of the base-to-money multiplier and the money-to-GNP multiplier.

[8] Frank deLeeuw and John Kalchbrenner, "Comment", St. Louis Reserve Bank *Review* (April 1969), pages 6-11.

[9] The monetary base consists of total member bank reserves plus currency in circulation outside banks.

The second line of figures on Table IV contains a reestimate of the parameters of line 1, this time with no fiscal variables included and with the second degree polynomial rather than the St. Louis fourth degree polynomial.

The problem with the St. Louis equations using the monetary base is that, while the latter may be more exogenous than the money supply, its own "exogeneity" is still far from beyond question. In other words, a good case can be made for the view that the two-way causation problem is still present in the monetary base. First, the monetary base includes borrowed reserves. While the Federal Reserve sets the conditions for borrowing and the discount rate, actual borrowings take place at the initiative of the member banks themselves. Certainly current business conditions, interest rates, and the state of loan demand influence the demand for borrowed reserves. Second, the base includes currency. The volume of currency the public wishes to hold is an endogenous variable. The banks supply the public with currency on demand, and, during the period of the 1950's and 1960's, the Federal Reserve has more or less automatically replenished the reserves lost by the banking system through currency drains. Hence a strong case can also be made that the currency component of the base is endogenous too. In a rejoinder to deLeeuw and Kalchbrenner, Andersen and Jordan dispute the contention that borrowed reserves and currency should be subtracted from the base to obtain a more "exogenous" variable.[10] While I remain unconvinced by their rejoinder, I will not say anything more about this "exogeneity" issue since I want to pursue a somewhat different tack.[11]

The remaining equations in Table IV show the results of stripping away, successively, borrowed reserves and currency from the total monetary base. As can be seen from the table, R^2 falls and the standard error rises noticeably when the sole independent variables are the current and lagged *nonborrowed* monetary base. (Compare lines 2 and 3.) Furthermore, breaking the entire period into the two subperiods used earlier, we find no significant relationship whatever between the nonborrowed monetary base and GNP in the earlier period ended in 1960. Indeed, the overall multiplier is actually negative.

Table IV

CHANGES IN GNP REGRESSED ON VARIOUS MEASURES OF CURRENT AND LAGGED CHANGES IN THE MONETARY BASE OR NONBORROWED RESERVES

Quarterly changes

Period	R^2 SE of est	Sum of coefficients	t	t-1	t-2	t-3
	The St. Louis equation—total monetary base Almon lag with fourth degree polynomial†					
1. 1952-68*	.53 4.4	16.0	1.0 (0.5)	5.5 (3.4)	6.5 (4.1)	3.1 (1.5)
	Total monetary base Almon lag with second degree polynomial‡					
2. 1952-68*	.45 4.8	14.8	3.5 (1.8)	4.4 (7.1)	4.1 (3.5)	2.7 (2.5)
	Nonborrowed monetary base Almon lag with second degree polynomial‡					
3. 1952-68*	.32 5.3	10.8	−0.1 (−0.1)	3.2 (5.2)	4.4 (4.7)	3.3 (4.0)
4. 1952-60	.07 5.5	− 3.4	−4.1 (−1.6)	−1.0 (−0.6)	0.7 (0.4)	1.1 (0.8)
5. 1960-68*	.33 4.2	9.2	−1.0 (−0.5)	2.8 (3.0)	4.2 (3.5)	3.2 (3.3)
	DeLeeuw-Kalchbrenner—nonborrowed monetary base Almon lag with fourth degree polynomial†					
6. 1952-68*	.45 4.5	10.4	n.a.	n.a.	n.a.	n.a.
	DeLeeuw-Kalchbrenner—nonborrowed reserves Almon lag with fourth degree polynomial†					
7. 1952-68*	.42 4.7	2.4	n.a.	n.a.	n.a.	n.a.

Note: Values of "t" statistics are indicated in parentheses.
* Through the second quarter of 1968.
† Fiscal variables included but not shown.
‡ No fiscal variables included.

[10] St. Louis Reserve Bank *Review* (April 1969), pages 12-16.

[11] Andersen and Jordan find a negative correlation between changes in borrowed reserves and changes in nonborrowed reserves (*op. cit.*, page 15). From this they conclude that the Federal Reserve System automatically offsets the effects on total reserves of endogenous changes in borrowed reserves and that total reserves, rather than the nonborrowed component, should therefore be treated as exogenous. However, a negative correlation would also be found if the System used either nonborrowed reserves or borrowed reserves (or some other related money market variable such as free reserves or the Federal funds rate) as an operational target. Thus a deliberate increase in nonborrowed reserves would tend to make banks pay off borrowings. Similarly, a deliberate increase in the level of borrowed reserves would have to be engineered by a subtraction of nonborrowed reserves. In neither of these cases would total reserves or the total monetary base be the appropriate exogenous variable. Similarly, Andersen and Jordan conclude that automatic System accommodation of currency drains implies a nonborrowed reserves target; that the System has not in fact followed a nonborrowed reserves target; and, therefore, that currency cannot be endogenous (*op. cit.*, page 13). This chain of reasoning is invalid, if only because many targets other than a nonborrowed reserve target involve automatic System accommodation of currency drains, including free reserve and other money market targets.

The first deLeeuw and Kalchbrenner equation shown in Table IV also uses the nonborrowed monetary base, but differs from mine in that it includes fiscal variables. The inclusion of the latter accounts for an R^2 higher than the one in my equation. Otherwise, the results are similar to mine. (Compare lines 3 and 6.)

The second deLeeuw-Kalchbrenner equation reported in Table IV eliminates both borrowed reserves and currency from the monetary base, thus leaving nonborrowed member bank reserves. As these authors note, the effect of leaving out currency is to reduce the multiplier drastically. Indeed, the multiplier of 2.4 obtained for this equation is not very different from the 2.0 multiplier of nonborrowed reserves onto GNP obtained from the Board-MIT model (see Table II). As a result, deLeeuw and Kalchbrenner conclude that the coefficients of the St. Louis equation are in fact heavily distorted by simultaneous equations bias. Once this is removed, they argue, the results closely resemble the sort of world most of us have always believed in.

I certainly agree with deLeeuw and Kalchbrenner that, while the total monetary base is statistically superior to the money supply as an exogenous variable, it probably is not exogenous enough. Moreover, I agree that the similarity of the results for nonborrowed reserves to the Board-MIT structural model is interesting. Nevertheless, I can't help feeling that this is not quite the end of the story. Even if one were wholly satisfied that nonborrowed member bank reserves are the proper exogenous monetary variable, it must nevertheless be kept in mind that what is at stake is not a member-bank-nonborrowed-reserves theory of the economy, but rather a money supply theory of the economy. There is a substantial gap between member bank nonborrowed reserves and the money supply. The problem with the computations presented in Table IV is that they short-circuit a two-stage chain of relationships consisting of the relationship between the nonborrowed base or nonborrowed reserves and money, on the one hand, and the relationship between money and GNP, on the other. Let us suppose that the money supply *does* exert a powerful and quick-acting influence on GNP. Let us suppose also that the influence of GNP on the money supply is minimal. One might nevertheless get a rather weak and slow-acting influence of the nonborrowed base or of nonborrowed reserves on GNP simply because the relationship between the base, or reserves, and money was relatively weak. Moreover, the lag between the base, or reserves, and GNP would represent the sum of the lags in the two links of the chain. In other words, it is possible that, even though the regressions proposed by deLeeuw and Kalchbrenner and myself may be more statistically "pure" in terms of the "reduced-form" rationale of the St. Louis equations, these regressions may nevertheless fail to do justice to the real power, stability, and promptness of the causal influence of money on business.

This possibility puts us in a new dilemma. On the one hand, we can't accept the St. Louis equations at face value because neither money nor the total reserve base may be sufficiently exogenous. On the other hand, the equations using the nonborrowed base or reserves may understate the causal influence of money for the reasons just given. To separate out the influence of money on business from the influence of business on money, one would seem to need a complete structural model. But this is precisely what the "reduced-form" approach, originated by Friedman and Meiselman and carried on by Andersen and Jordan, seeks to avoid!

REDUCED-FORM EQUATIONS FOR MONEY

One possible way out of this mess is to examine reduced-form equations for money itself. If the relationship between the nonborrowed monetary base and money is not very tight, this will explain some of the relative looseness of the relationship between the nonborrowed base and GNP. Moreover, by adding current and lagged GNP to the instrument variables of policy, it may be pos-

Table V

CHANGES IN M_1 REGRESSED ON CURRENT AND LAGGED CHANGES IN THE NONBORROWED MONETARY BASE (Δ B)

Quarterly changes

Period	R^2	Δ B	Δ B_{-1}	Δ B_{-2}	Δ B_{-3}
	Unconstrained lag structure				
1952-68*	.54	.67 (2.6)	.80 (2.8)	.55 (1.9)	.27 (1.0)
1952-60	.37	.23 (0.6)	.85 (2.3)	.56 (1.5)	.90 (2.6)
1961-68*	.58	1.37 (3.4)	.64 (1.4)	.83 (1.8)	— .43 (—1.0)
	Almon lag—second degree polynomial				
1952-68*	.53	.73 (3.6)	.67 (8.4)	.56 (4.6)	.33 (3.1)
1952-60	.34	.29 (0.9)	.74 (3.8)	.84 (4.1)	.59 (3.7)
1961-68*	.54	1.31 (4.4)	.76 (5.1)	.36 (1.9)	.11 (0.7)

Note: Values of "t" statistics are indicated in parentheses.
* Through the second quarter of 1968.

ible to get some qualitative idea of how much of the oss relationship between money and GNP reflects a direct causal influence of money on GNP and how much of it reflects a reverse influence of GNP on money.

Some reduced-form equations for quarterly changes in M_1 using current and lagged values of the nonborrowed monetary base are presented in Table V. Current and lagged changes in the nonborrowed monetary base (which, it should be recalled, has been adjusted for changes in reserve requirements) explain about 54 percent of the variance of quarterly changes in M_1. What is more to the point for present purposes is that the nonborrowed base *fails* to explain fully 46 percent of the variance. Hence there is considerable looseness in the relationship between the nonborrowed base and M_1. Again, such a looseness is especially evident in the first half of the period. Moreover, the coefficients in Table V suggest that the influence of the base on M_1 does operate with a distributed lag that would contribute to the total lag of the influence of the base on GNP. Thus the results of Table V lend some support to the contention that the regressions of GNP on the nonborrowed monetary base *under*state (implicitly) the closeness of the association between money and GNP and the size of the money multiplier operating on GNP within any given period of elapsed time.[12]

If something in the neighborhood of one half the variance of quarterly changes in M_1 is left unexplained by the nonborrowed base, what accounts for the *remainder* of this variance? One factor would be the other instrument variables of policy, i.e., changes in the discount rate and in the Regulation Q ceiling. In addition, there could be various factors operating from within the banking sector of the economy, such as shifts in bank demand schedules for excess and borrowed reserves. Finally, there would be all the remaining factors summed up in the expression "the influence of business on money". Only part of the influence of business on money would be represented as an influence of, specifically, *GNP* on money. Other parts would perhaps be represented by movements in the composition of GNP, interest rates, and the various categories of credit demands.

How large a part of the unexplained variance does reflect an influence of GNP, as such, on money is the question that Table VI attempts to answer. The first equation presented for each time period covered in the table attempts to show the influence of the nonborrowed base and other policy instrument variables on changes in M_1. Actually, the only difference between these equations and those in Table V is the addition of changes in the discount rate—which does not, in fact, make much difference. As noted earlier, changes in reserve requirements are accounted for in the measure of the base. The remaining instrument variable of policy, changes in the Q ceiling, is simply not included. Some experiments on time periods when no change in the Q ceiling occurred showed essentially the same results. Hence it seemed better to omit it and save the degrees of freedom.

On balance, the results presented in Table VI do not seem to show any very strong feedback from GNP to money—which, it should be emphasized again is *not* the same thing as a feedback from "business" to money. To be sure, current and lagged changes in GNP "explain" about 32 percent of the variance of changes in M_1 over the whole 1952 to 1968-II period and a bit over 20 percent in each of the two subperiods. However, most of this impact appears to occur in the current quarter, when the direction of causation is of course ambiguous. The lagged changes in GNP contribute almost nothing.

Similarly, the addition of current and lagged GNP variables contributes almost nothing to explaining changes in M_1, once current and lagged monetary "policy" variables have already been included. For the period as a whole, the adjusted R^2 of .53 for the policy variables rises only .05 to .58 when current and lagged changes in GNP are added. In contrast, the addition of the *policy* variables to the GNP variables raises R^2 substantially, from .27 to .58. As to the subperiods, GNP does make a noticeable contribution in the earlier period. In the 1960's, however, current and lagged GNP variables give an adjusted R^2 of only .08 by themselves. When the policy variables are added, this rises to fully .67. This is an interesting result in view of the especially close relationship between changes in GNP and current and lagged changes in M_1 during the 1960's, as noted earlier.

The impression that the influence of "policy" variables on the money supply dominates any feedback from GNP to the money supply has to be modified, but only somewhat, if nonborrowed member bank reserves are used in place of the nonborrowed monetary base. The reason is, of course, that nonborrowed reserves make no allowance for the currency component of the money supply and therefore explain less of the variance of changes in M_1

[12] Essentially the same conclusions apply if member bank nonborrowed reserves are used instead of the nonborrowed reserve base. However, R^2's are of course lower since nonborrowed reserves make no attempt to explain, or to allow for, changes in the currency component of M_1. Using an unconstrained lag structure, nonborrowed reserves gives an R^2 of .34 for the full period with a total multiplier of 3.2. For the 1952-60 subperiod, the R^2 is .30, while it is .39 for the 1961 to 1968-II subperiod.

Table VI

CHANGES IN M_1 REGRESSED ON CURRENT AND LAGGED CHANGES IN GNP, THE NONBORROWED MONETARY BASE, AND THE DISCOUNT RATE

Quarterly changes

Period	Δ GNP	Δ GNP-1	Δ GNP-2	Δ GNP-3	Δ B	Δ B_{-1}	Δ B_{-2}	Δ B_{-3}	Δ R_d	R^2	$\bar{R}^2$†
1952-68*					.78 (3.0)	.85 (3.0)	.47 (1.6)	.17 (0.6)	.73 (2.1)	.57	.53
	.08 (3.9)	.01 (0.3)	— (—)	— (—)						.32	.27
	.03 (1.7)	.01 (0.3)	— (—)	— .04 (—2.3)	.95 (3.7)	.79 (2.8)	.38 (1.4)	.02 (0.1)	.56 (1.4)	.64	.58
1952-60					.46 (1.1)	.93 (2.5)	.61 (1.6)	.88 (2.6)	.49 (1.1)	.39	.29
	.05 (1.9)	— .01 (—0.4)	.01 (0.2)	— .04 (—1.7)						.23	.13
	.05 (2.2)	.01 (0.5)	.03 (1.1)	— .02 (—0.9)	.70 (1.7)	.91 (2.5)	.64 (1.7)	.73 (2.3)	— .12 (—0.2)	.55	.39
1961-68*					1.39 (3.8)	.86 (2.0)	.46 (1.0)	— .59 (—1.5)	1.54 (2.5)	.66	.59
	.09 (2.0)	.01 (0.3)	— .03 (—0.6)	.02 (0.4)						.21	.08
	.06 (1.7)	.03 (0.9)	— .05 (—1.4)	— .03 (—1.1)	1.61 (4.3)	.90 (1.8)	.21 (0.5)	— .64 (—1.6)	.86 (1.3)	.77	.67

Note: Values of "t" statistics are indicated in parentheses.
* Through the second quarter of 1968.
† Adjusted R^2.

than does the nonborrowed base. For the entire 1952 to 1968-II period, changes in nonborrowed member bank reserves (adjusted for reserve requirements) and in the discount rate have an adjusted R^2 with respect to changes in M_1 of .39. Addition of current and lagged changes in GNP raises this to .50. For the 1952-60 subperiod, the "policy" variables, so defined, give an adjusted R^2 of .20 alone, with R^2 rising to .21 when the GNP variables are added. For the 1961 to 1968-II subperiod, the "policy" variables give an adjusted R^2 of .38, which rises to .50 when GNP is included. While these results using nonborrowed reserves are less clearly one-sided than those using the nonborrowed monetary base, the conclusion that the feedback from changes in GNP to changes in M_1 may be relatively modest still seems warranted.

SUMMARY OF MAJOR STATISTICAL ISSUES

At this point a brief general summary of the major statistical issues for and against the St. Louis equation may be useful. (1) The equation shows very little explanatory power when fitted to the 1952-60 data. It seems to fit the data well only in the 1960's. Coincidentally or not, there was a significant trend in the first differences of money and GNP in the 1960's that was not present in the 1950's. In rejoinder, St. Louis might note that the period 1952-60 happens to be about the worst possible subperiod from the entire 1952 to 1968-II period. The R^2 for this subperiod, as noted earlier, is .18. For the still shorter subperiods, 1952-57 and 1955-60, it is about .32 in each case. Personally, this rejoinder does not seem very impressive to me. Consequently, I would regard the poor performance of the St. Louis equation in the 1952-60 subperiod as a distinct embarrassment.

(2) In the St. Louis equation's favor is the fact that the coefficients are reasonably stable over time, even though the two halves of the 1952-68 period show such different R^2's.

(3) If the nonborrowed monetary base or nonborrowed member bank reserves are used as the exogenous variable, rather than the money supply or the total monetary base, explanatory power drops rather substantially. So do the sizes of the multipliers. Indeed, there is no significant relationship at all between the nonborrowed base, current and lagged, and GNP in the 1952-60 subperiod. In defense of the St. Louis equation, however, one may argue that the total base is in fact a more appropriate "exogenous" variable than the nonborrowed base. Again, I myself am not at all satisfied with the St. Louis rejoinder on this point cited earlier. Nevertheless, I freely

confess that the problem of identifying a suitable exogenous monetary variable does not seem to have an entirely obvious solution.

(4) I would prefer a somewhat different defense of the St. Louis equation. This would be along the lines that the relatively poor relationship between the nonborrowed base (or reserves) and GNP does not deal directly with the question of the relationship between money itself and GNP. To deal with this question, it is worthwhile to attempt to determine how much of the relationship between changes in money and changes in GNP is a feedback relationship from GNP to money.

(5) The available evidence suggests that current and lagged changes in the nonborrowed base (or nonborrowed reserves) and other monetary policy variables leave a substantial amount of the variance in monetary changes unexplained. This therefore leaves a large potential role for all the influences wrapped up under the general rubric of "business conditions".

(6) The specific variable GNP, however, seems to contribute rather little extra to explaining the variance in monetary changes beyond what is explained by the policy variables. Hence, only a relatively modest part of the gross relationship between money and GNP exhibited in the St. Louis equation may reflect a feedback effect from GNP to money. Much of the powerful influence of "business" on money found by Cagan must be reflected by variables other than GNP (such as interest rates)—or perhaps the cyclical behavior of the monetary growth rate is simply a very different sort of variable than quarterly dollar changes in M_1.

On balance it would seem fair to say that the St. Louis equation has not been devastated by the critical scrutiny to which it has been subjected. On the other hand, I think it is equally obvious that some distinctly troublesome questions exist regarding the equation. The equation's merits do not seem to me sufficient to compel by themselves our acceptance of the world it portrays. This being the case, it seems appropriate in closing to put aside regression results and consider briefly some of the underlying economic issues.

TRANSMISSION MECHANISM

The St. Louis equation says that a $1 billion increase in the money supply this quarter will raise GNP in this same quarter by $1.6 billion and that, by the next quarter, money will have raised GNP by $3.5 billion. This is somewhat over half its ultimate influence. If the money supply were increased by means of Government handouts of newly printed dollar bills, this sort of quick, sharp reaction would certainly seem reasonable. The actual process of money creation is of course quite different, however. It works primarily through central bank open market operations and through asset purchases by commercial banks. It involves no direct effects on private income or wealth of any great magnitude.

Most people now seem to agree that monetary impulses must work their effects on GNP primarily through a chain of substitution relationships. This chain most often begins when the Trading Desk at the New York Reserve Bank makes a bid over the phone to a group of Government securities dealers who are persuaded by the terms of the offer to exchange part of their portfolio of Governments for demand deposits. Relative interest rates change and portfolio balance is disturbed. Thus further substitutions are made. Deposits are exchanged for private securities, and the rates on these securities are bid down. Ultimately, wealth holders must be persuaded that they should substitute into physical assets (whether producers' goods or consumers' durables). It is at this stage that GNP begins to be affected.

The crucial point is that, if there are no important income or wealth effects stemming from the process of money creation, then this final substitution into goods can only take place as a result of the shifts in relative interest rates that are set in motion by the monetary process. If wealth holders' net worth is unchanged, and if their income is unchanged, they will be induced to try to shift into more extensive holdings of real assets only if their demand for such assets is sensitive to the changes in relative yields. If there are no income and wealth effects whatever, the impact of monetary changes on the real economy can be no swifter or more powerful than is permitted by the interest-rate responsiveness of the demand for real capital.

As far as I can tell, incidentally, these conclusions depend in no way on the length of the chain of transactions between the original money-creating transaction and the first transaction involving nonfinancial assets. Some argue that the initial money-creating transaction may lead immediately to an increased demand for goods and that, in such cases, the interest rate elasticity of the demand for goods is irrelevant. This seems quite wrong to me. Suppose an individual is induced to exchange with the Federal Reserve some of his Government securities for deposits because the Fed's bid in the market makes such an exchange attractive to him. At this point in time, his income and wealth are unchanged, but the rate on Governments is lower. If his equilibrium portfolio composition now involves, say, fewer Governments and more of both cash and goods, it can only be because his desired holdings of both cash and of goods are sensitive to the changed yield

on Governments. Given an unchanged utility function, there is simply no other possible explanation.

To the extent that the monetary process depends upon portfolio composition effects induced by changes in relative interest rates, the monetary impulses in the St. Louis equation seem to me to influence GNP with an implausible rapidity. We are, after all, not wholly devoid of information on the response of business fixed investment, inventories, and consumer durables to changes in interest rates. Indeed, we have a large body of econometric and interview-type studies accumulated over the years on these matters. Certainly these studies are open to a variety of interpretations, and they are by no means unanimous in their findings. Nevertheless, I think there can be little question that their tenor is overwhelmingly against the sort of large short-period multipliers found in the St. Louis equation. The Board-MIT model incorporates a fairly representative sample of such econometric research, and its multipliers are much closer to what this research had in the past led us to expect.

On the other hand, it is possible that the conventional studies of the interest-elasticity of demand of the different categories of capital goods, as well as the traditional interview approach to this subject, are leaving out major elements of the transmission mechanism. The omission of these elements may explain the divergence of the St. Louis world from the world seemingly implied by the more traditional sort of research. I can think of at least three possible factors that may not be adequately accounted for in the more traditional studies.

First, the money creation process itself—and the subsequent shifting of financial portfolios—does involve bidding up the prices of a variety of financial assets, Government and private. Obviously a rise in the market price of outstanding private financial instruments has no effect on real private wealth. However, there may be a sort of "pseudo-wealth", or "distribution" effect stemming from such price rises. This could occur if private financial assets were valued by their holders at market value while the issuers valued their liabilities at maturity value or at some conventional par. How important such distributional effects may be we do not know. Certainly I would not expect that the effects of rising market prices for debt instruments related to the monetary expansion process would be of much significance. For one thing, holders of these instruments often do not value their holdings at market prices. To this extent, net worth positions as perceived by their owners would not be changed by changing market prices. When one considers rises in the price of equities, however, the possibility of a significant secondary wealth effect on the demand for goods and services seems much more real.

A second possible source of transmission from monetary changes to the real sector that may not have been given sufficient attention in the traditional empirical research is availability effects. We know that the capital market is structured so that some potential borrowers simply cannot obtain all the funds they want by raising their bid in the market. To the extent that a money supply increase is associated with a direct increase in the funds made available to these borrowers, it could have a direct, swift-acting, and powerful effect on real spending. That there exists availability effects of this kind is beyond question; that they are important enough to account for the very high short-run multipliers in the St. Louis equation is less clear.

A third possible deficiency of the conventional research may be its treatment, or lack of treatment, of the implicit rates of return on real capital. Friedman and others have argued for years that existing research on the importance of interest rates in the demand for capital goods was wholly inadequate because it failed to include own-rates of return on real capital, including rates of return on consumer durable goods. It is of course possible that a more adequate treatment of implicit real rates would turn up a much sharper and more rapid response to interest rates than has been found in past studies. At the moment, however, this is totally unexplored territory.

CONCLUDING COMMENT

In conclusion, I can summarize my overall reaction to the St. Louis equation about as follows. Andersen and Jordan have produced a monetarist equation that holds up rather better than I would have thought likely. In particular, it does not seem easy to dispose of the association between changes in money and changes in GNP by showing that it is primarily or largely a matter of "reverse causation". On the other hand, the reduced-form approach they use, which at first looks so seductively easy, turns out on closer inspection to be itself fraught with difficulties. In this particular case, it leads to an equation that produces a much quicker monetary response than seems consistent with a large part of existing research on the nature of the monetary transmission mechanism. At the moment, I find it very difficult to believe in the St. Louis equation: I just don't quite see how things could work that way. On the other hand, I am ready to concede at least the possibility that proper allowance for various secondary wealth effects, credit availability effects, and broader treatment of interest rates *might*, in principle, be able to make the St. Louis world seem plausible.

I think the onus is now clearly on the monetarists to

spell out in detail precisely how they think the transmission process works. Moreover, this description must be translated into an econometric model with a reasonable degree of structural detail. Certainly the rest of us would like to see just exactly how such a model would differ from the Board-MIT model, for example. We need to see precisely how money is supposed to produce the results it appears to produce in the Andersen-Jordan equation. I suspect that only after such a project is carried out, and carried out successfully, will most economists really be prepared to believe that money matters as much and as fast as it seems to in St. Louis.