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Rejoinder to Chase and Hendershott

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Rejoinder to Chase and Hendershott

Karl Brunner and Allan H. Meltzer

Chase makes a number of incorrect statements about our paper, and Hendershott argues that because some of the sources of the adjusted monetary base are not exogenous, the adjusted monetary base cannot be exogenous. Most of Chase's arguments do not bear, even remotely, on our paper, and Hendershott's argument involves a non sequitur, so it is important to reply to their criticisms. We will discuss Chase's comments first.

About the only points on which we agree with Chase are (1) that we regarded our paper "as more than an exercise" and (2) that "the idea of developing a quantitative gauge of monetary policy is appealing." To say that the paper is more than an exercise does not suggest that it is a definitive solution to the long-standing problem of measuring the size and direction of current or recent monetary policy operations. Chase chose to ignore our statements that we sought "to open discussion of an issue that is more frequently debated than analyzed," that "our tentative results are not presented as a resolution of the problem," and similar statements that we repeated throughout the text. However, his misinterpretation of our intent is not confined to the issue of whether or not we regard our conclusions as applicable to current policy problems. The subject and purpose of our paper escaped him completely, so that his comments are either irrelevant or wrong. We will point out his main errors after summarizing the "indicator problem."

The problem discussed in our paper can be stated succinctly. (1) Economists have incomplete knowledge about the magnitude and timing of the response of output, the rate of change of prices, and other "goal variables" to monetary policy. (2) Analysis suggests that the response of the "goal variables" occurs after a delay (or lag), and that the lag is variable, not constant. (3) There is no agreement about the appropriate measure (or measures) of the current direction of monetary policy. Two

One of us has since organized a conference on the subject of indicators and short-run targets to provide more opportunity for discussion and analysis than is available in our preliminary investigation. The papers prepared for the conference and a summary of the discussion will be published in a forthcoming volume.

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commonly used measures—market interest rates and the money supply—frequently move in the same direction, rising in periods of economic expansion and falling in recessions. The two measures provide rather different interpretations about the direction of current or recent policy, since rising money supply is interpreted as an indication of an "easier," and rising interest rates of a "tighter," policy.

We know of only two ways to resolve the long-standing, largely futile and, at times, acrimonious debate about whether changes in interest rates, money supply, bank credit, or some other variable are a more reliable indicator of the current direction of monetary policy. One way eliminates most, if not all, of the problem but requires more information than is available. The other makes use of imperfect and incomplete information to compare the relative errors that are likely to result from the use of endogenous variables as indicators.

A validated, comprehensive, fully identified theory of monetary dynamics would permit economists and policy makers to state future goals and to compute the magnitude of current operations required to achieve those goals. Under these idealized conditions many (but not all) variables could be used to indicate whether the expected effects are being achieved at the expected rate. Preferences for particular indicators would be of no consequence, since the information provided by changes in one indicator is equivalent to the information provided by another, once the appropriate parameters and the observed rates of change are known. If the theory is identified, validated, and comprehensive, each endogenous indicator would give a consistent estimate of the size of future changes in output, employment, and prices.

The contrast between this desirable state of knowledge and current information is suggested by the fact that often we cannot agree about the direction of monetary policy. We lack the means of reconciling one indicator, which is widely interpreted as suggesting that policy will have a contractive influence on output or prices, with another that suggests the opposite. Our paper, therefore, attempted to answer the question: Given our imperfect knowledge, which of the endogenous variables is the least misleading indicator of changes in the direction and magnitude of monetary policy operations?

Chase's first criticism starts with the assumption that "all the elements of equation (A) [our equation (1)] are known." In this case, all of the responses of endogenous variables to changes in policy variables are known, and the gain or loss in utility resulting from each policy change is known also. There would be no reason to construct an indicator. The policy problem would be reduced to computing the values of the policy variables that maximize utility. The assumption of complete knowledge removes any connection between his criticism and our analysis.

However, Chase adds the additional assumption that the marginal
utility of changes in each endogenous variable is constant. His utility function is linear; ours is not. Again there is no connection between the criticism and the paper. But, after a few additional assumptions, Chase reaches the absurd conclusions that:

'Good' policy presumably entails $I > 0$, 'bad' policy entails $I < 0$. Optimal policy requires maximizing $u$, which entails $I = 0$.

($I$ is the indicator of monetary policy and $u$ is social utility.)

It is either error or a strange ordering relation, indeed, that makes optimal the midpoint between good and bad. Had Chase considered the conclusion that we quote, he would have avoided this error and recognized that it resulted from his own assumptions. Chase's discussion of optimal policy appears to result from a confusion of our analysis with the analysis pioneered by Theil and others. The latter requires detailed knowledge of the trade-offs between social goals. In view of the lack of consensus about specific trade-offs in the social utility function, a main point of our paper is to provide a criteria for judging policy while avoiding a detailed statement of trade-offs.

Chase's second criticism also arises from his concern about the sign of the indicator function. He notes that the index can be rising or falling as a result of open market sales and concludes that "it seems strange to refer to monetary policy that reduces the rate of price increase during inflation as 'easing' merely because it is desirable in terms of social utility." This quotation (and several similar comments) suggest that there is a misunderstanding. The sign of the indicator has no bearing on the issues discussed in the paper. The reason is that the utility function is assumed to be unique only up to a monotone transformation. (See our footnote 4.) The indicator function provides an ordering of monetary policy, or a scale by means of which policy actions can be compared. If the utility function involves more than one argument, it must express a consensus about the effect on utility of a trade-off between various goals, as we stated. Moreover, it should be noted that the interpretation of a deflationary effect of policy as "easing" is entirely Chase's notion and is unrelated to our analysis.

The criticisms above are irrelevant because they apply to a paper that we would not consider writing; most of Chase's remaining criticisms are incorrect statements about a paper we might have written, but didn't. These comments are little more than a series of repeated assertions that our conclusions hold only for a model in which raising per capita real income is the only goal of policy and reflect some confusion about the results that would be obtained if the analysis were extended. Although we did not present the results of an analysis in which the price level is taken as the second argument of the utility function, the material in our appendix permits the extension to be made. The specific indicator, $\frac{dQ}{Q}$, that
emerges in the case presented as an illustration is also obtained if the social utility function contains both per capita real income and the price level. Chase's conjecture that the values of the partial derivatives of the utility function would have to be known is incorrect. The derivatives of the utility function remain part of the scalar and are, therefore, not needed to construct the indicator.

Chase's final criticism is that none of our conclusions have been established because we compared the endogenous indicators (rates of change of the money supply, of interest rates, and so forth) to the optimal indicator implied by our model, \( \frac{dQ}{Q} \). We would not be surprised to find on further extension of our analysis that the particular elasticities that form the weights in the index will change. But it would be surprising to learn that a solution of most systems of equations that connects monetary policy variables to output and the price level would not contain the variables that are manipulated by the policy maker, the principal components of \( \frac{dQ}{Q} \).

Nevertheless, it is important to reiterate that the nonconstancy of the scalar \( \epsilon(y,B^*) \) (the elasticity of real income with respect to the monetary base) in our indicator function restricts the use of the index. A similar restriction applies when the analysis is extended to the two-goal case. The problem is similar to the familiar "index number problem." The use of the index to compare policies at peaks and troughs or over relatively long time intervals requires more knowledge about the class of hypotheses than we have supplied thus far. We would need to know more about the structural parameters that are components of \( \epsilon(y,B^*) \) or of the scalar that is part of the solution in the multiple-goal case.

However, we did not construct the indicator to compare policies over long periods. The index was constructed to illustrate that: (1) alternative policies can be compared at a given point in time; (2) rational statements can be made about short-term changes in the direction or size of policy operations; and (3) the relative merits of variables used to appraise the current direction of policy can be appraised. If our initial effort stimulates additional work on the indicator problem and provides "impetus to a needed discussion of an important issue," we must, reluctantly, disagree with Chase. The development of "an unambiguous indicator of monetary policy" is much closer than it has been in the past.

Hendershott emphasizes that the sources of the adjusted base include endogenous variables and argues that there is, therefore, no reason to select the adjusted base as an indicator of policy action rather than some other endogenous variable. We will grant more than Hendershott asks and assume for the moment that each of the source components is an endogenous variable. How does this affect our argument? Or, to phrase
the question in another way, why would the adjusted base remain a
measure summarizing the actions taken by the monetary authority when
other monetary policy variables are unchanged?

One reason is that the stock of interest-bearing and noninterest-bearing
debt issued by the government sector, and held by the private sector, is
the cornerstone of modern monetary theory, as Patinkin has emphasized
repeatedly. This sum is obtained by consolidating the balance sheets of
the Treasury and the central bank and is the total financial issue of the
government sector to the private sector. The noninterest-bearing portion
is the adjusted base—reserves and currency held by the banks and the
public minus member bank borrowing from the central bank.

However, in our analysis, we separate the effect of changes in interest-
bearing and noninterest-bearing debt. Again, this choice is not an arbi-
trary decision. It reflects the institutional arrangements that prevail in
most countries—and certainly in the United States—which assign the pri-
mary responsibility for control of the two components to separate agen-
cies. Congress and/or the Executive determines the budget and the
amount and type of interest-bearing debt that will be issued or retired.
The Federal Reserve decides on the amount of base money that is issued.

Every dollar change in the adjusted base reveals a specific action of
the monetary authorities, an action in principle avoidable. Needless to
say, Federal Reserve officials may not view their actions in this way. They
may choose to interpret their actions as changes in the volume of free
reserves, total reserves, unborrowed reserves, and so on. From the stand-
point of monetary theory (but not of monetary policy) their interpreta-
tions are not crucial, and their actions affect the economy only if they
result in a change in the adjusted base.

A second reason for rejecting Hendershott's argument is that it is in-
sufficient for the case that he tries to make. The levels of the source com-
ponents of the base, with the exception of float and foreign deposits,
are predetermined relative to current monetary processes. Changes in
some of the components are slightly more dependent on current processes.
But even if every one of the sources responded to current monetary proc-
esses, the adjusted base would remain under the complete control of the
Federal Reserve.

Again, the reason has nothing to do with the endogeneity or exogeneity
of particular sources. It reflects the fact that the uses of the base—reserves
plus currency minus member bank borrowing—are the total emission of
the monetary authorities. As Hendershott correctly notes, the uses are

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See Don Patinkin, "Price Flexibility and Full Employment," as reprinted in F.
Lutz and L. Mints (eds.), Readings in Monetary Theory (New York: The Blakiston
Co., 1951), pp. 252-83. The definition is given on p. 264.

We abstract, of course, from actions which change reserve requirement ratios,
the rediscount rate, and so forth, which are discussed more fully in the text.
identically equal to the sum of the source components. It is the sum, and not the components, which is the critical policy variable in monetary theory.

This argument removes the alternatives that are mentioned by Hendershott. Treasury bill rate, free reserves, et hoc genus omne simply do not summarize in the same immediate and obvious manner the direct actions of the monetary authorities. Moreover, these measures require knowledge of structural detail that the policy maker does not possess. The use of the portfolio of government securities as a measure of monetary policy introduces an arbitrary dichotomy and an open invitation to irresponsible action. If the portfolio is increasing while the adjusted base is declining, monetary policy would be called expansive despite the withdrawal of base money. The tragic consequences of the early thirties, when open market operations were conducted in insufficient volume to offset the increased demand for currency, is adequate evidence on which to judge the consequences of using the Federal Reserve's portfolio of government securities as an indicator.
Comment on Daniel Brill's Interpretation of 'Predicting Velocity'

Karl Brunner and Allan H. Meltzer

Though we appreciate the interest that Dan Brill has shown in one of our earlier papers,¹ we cannot accept or even recognize the conclusion that he claims to have found there. We did not propose or even suggest that policy makers or economists could or should predict income solely from the demand and supply functions for money. Nor did we regard our description of a procedure for making such predictions as a central point of our paper.

On the contrary, we summarized our results by stating: "The major purpose of this paper has been to compare a number of different demand for money hypotheses, using identical tests . . . .,"² and concluded that "monetary policy operates on interest rates or relative prices and through wealth and substitution processes on the level of income,"³ a position quite different from the one attributed to us. Moreover, we carefully distinguished our use of the terms "prediction" and "forecast"⁴ and wrote:

These predictions are not forecasts, since in most cases the data would not have been available until after the year had passed. Our interest throughout is the comparison of the explanatory or predictive power of the various models to increase our understanding of monetary theory.⁵

Lest there be any remaining doubt about our intention, we note that only a very small part of a lengthy paper considered the problem of predicting income and an even smaller part—described as a digression—was concerned with forecasting. We leave to the reader to decide whether the cautious way in which the results were presented, the qualifications intro-

²Ibid., p. 349.
³Ibid., p. 351.
⁴Ibid., p. 323.
⁵Ibid.
duced, and the explicit reason for introducing lagged values (p. 341) warrant the scorn and the attention that Brill devoted to this section of our paper or the inference that he drew.

Finally, though we acknowledge the inadequacy of forecasts based on our one-equation method, we are disappointed to learn from Brill that policy makers have not yet given up " 'seat of the pants' judgment." We thought that they had been converted partially to econometric models of the Suits type. Since they have not been converted, one of the few justifications of our "forecasts" has vanished. Nevertheless, it may be instructive for economists and central bankers to learn that forecasts based solely on the demand and supply for money—poor as they may be—are often better than the annual forecasts reported by Suits, which deny any influence of money on economic activity.


(Continued on back cover)