

REPLY TO DONALD HESTER

Milton Friedman and David Meiselman

Since we share Donald Hester's opinion about the importance of the issues involved, we are most anxious to have our results subjected to searching scrutiny. Hence, we welcome the attention that Hester has devoted to our paper. We hope it will prove the first in a series of critical attempts to reach agreement about what the evidence shows.¹ Unfortunately, Hester has neither gone very far nor dug very deeply. As a result, the reader of his paper who has not read ours is likely to get a misleading impression of the issues, of the nature of our research, and of the strength of our evidence. At the outset, therefore, we indicate how the scope of Hester's paper compares with that of our own. We then consider points of theory and of empirical evidence raised by Hester.

The Scope of the Two Papers

The main differences in scope are in (a) periods covered and data used and (b) consideration given to appropriate definition of autonomous expenditures.

Periods Covered

Hester uses annual data for the period 1929–1958. We use annual data for the period 1897–1958. In addition, we use quarterly data for the postwar period, 1946–1958, and experiment with lagged as well as contemporaneous relationships. Because we regard the question at issue as “mainly the short-term stability of the relations being compared,” our conclusions are based predominantly on the results for the short subperiods into which we divide the data.²

We point out that the results for 1929–1939 differ from those for the other subperiods and for the period as a whole. This is the one sub-

period for which consumption is more highly correlated with autonomous expenditures than with money. We point out further that this result is primarily due to the year 1929, and that if that year is omitted, the exception is eliminated.³

If the years of World War II are excluded, 1929–1939 is roughly half of the period for which Hester gives statistical evidence. In his table 1, 1929–1939 and 1933–1938 are the only two periods for which any of the correlations with alternative measures of autonomous expenditures are higher than the correlation between consumption and money. Had we too considered only 1929–1958, we too would have had to reach a more qualified conclusion than we did. The fact that the correlations after the war returned to the pattern before 1929 was what enabled us to designate 1929–1939 an exception. Hence, even if we waive all questions about the relevance and validity of Hester's alternative computations, the appearance of substantial difference between his results and ours derives primarily from the shorter period his calculations cover.

So far as we can see, the data are readily available to permit Hester to make his computations for the same periods as we made ours. We wish he had done so, to provide a comparable body of evidence, and are puzzled as to why he did not. He remarks, in another context, that “. . . the structure of the autonomous expenditure model has not been invariant over the past 60 years.” That is clearly no reason for restricting the time period examined; first, because we do not see how he can know that the changes have been significant without evidence; second, because the monetary model has presumably also been exposed to structural change; and third, because each subperiod can be, and should be, examined separately.

Criteria for Defining Autonomous Expenditures

We wrote in our paper:

One by-product of this investigation was the discovery that there is neither clear-cut agreement on the

¹ Another critique by Albert Ando and Franco Modigliani is scheduled for publication, along with a reply by us, in the *American Economic Review*.

² Milton Friedman and David Meiselman, “The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958,” *Stabilization Policies*, Commission on Money and Credit (Prentice-Hall, 1963), 174.

³ *Ibid.*, 186, 189.

specific statistical definition of autonomous and induced expenditures nor any well established criteria for choosing particular definitions for a particular problem or body of data. . . . In our actual empirical work, much the greatest amount of time was spent in trying to draw the appropriate boundary lines. . . . We are by no means satisfied that we have used the appropriate criteria in drawing the lines. Neither are we satisfied with the precise lines we have drawn, some of which we regard as highly tentative. Much further work remains to be done on this fundamental problem, in particular in determining statistical tests for making the best choice.⁴

Unfortunately, Hester has not contributed to such "further work." Instead, he has regrettably taken a step back — to pulling definitions of autonomous out of an alleged theoretical hat and in some mysterious unstated way being able to judge which definition is empirically preferable. Hester dismisses in a brief footnote both our suggested criterion and our statistical tests on grounds that seem to us largely irrelevant, but in any event are only asserted not argued. Aside from this footnote, Hester does not refer to the issue. In particular, he does not point out that we examined explicitly all but one of the alternative definitions he proposes (we did not consider L''' , which excludes inventory investment); that we presented statistical evidence on each; and that we explained why the evidence seemed to favor the concept we finally used.⁵

Hester gives no empirical evidence to support his own stated preferences among alternative definitions, unless it be the correlation of each with consumption. We pointed out explicitly in our paper why the size of this correlation is not by itself a valid criterion.⁶ Hester's intuition might of course yield better results than our assessment of the empirical evidence but he can hardly expect others to accept his intuition without some supporting evidence.

⁴ *Ibid.*, 180–181.

⁵ *Ibid.*, 242–257. It is especially regrettable that Hester did not explore the relevant criteria for choosing a definition of autonomous because, as indicated in the quotation from our paper, this is precisely the issue on which we are least satisfied with our own work. As noted in our paper, the most fruitful direction of further work is probably to depart from the usual global figures in national income accounts by breaking them down into more detailed categories. We experimented somewhat along this line, particularly in our examination of the government accounts, but by no means adequately.

⁶ *Ibid.*, 181–183.

The Issues of Theory

Hester asserts that we ". . . have represented the autonomous expenditure theory in a very unorthodox form," that he employs ". . . a more conventional representation," and that, if his ". . . L is a sensible measure of autonomous expenditure," we have ". . . stacked the cards against the Keynesian model . . . by ignoring the fact that taxes are a function of income." Each of these assertions is wrong. They derive partly from Hester's misunderstanding of our implicit consumption function, partly from his neglect, except for one aside, of the possibility that particular components of income may be neither induced nor autonomous but, rather, mixed. For example, our treatment of the net government deficit as autonomous does *not* imply that we regard tax receipts as autonomous rather than induced. Similarly, our treatment of the net foreign balance as autonomous does *not* imply that we regard imports as autonomous. Since we explicitly asserted the contrary and discussed the issues involved at some length, we are at a loss how Hester could have attributed to us the views he did or could have been content to conduct his own analysis at so naive a theoretical level.

With one modification, which we suspect Hester will also regard as an improvement, our equations and approach are entirely consistent with and can be derived from the model Hester outlines in his equations (7) to (12). Contrary to his belief, treating taxes as a function of income does not in any way rule out the particular concept of autonomous that we found to be supported by the empirical evidence we examined.

Hester's confusion about the theoretical issues is brought out strikingly by his assertion in a footnote: ". . . an obvious implication of Friedman and Meiselman's A is that the balanced budget multiplier is zero," as if that were proof positive that we had strayed from the well beaten conventional theoretical path. The balanced budget multipliers that are zero in our model are (1) the multiplier for consumption and (2) the multiplier for consumer after-tax income. So far as we know, both of these are also zero in the standard theoretical treat-

ments of the balanced budget multiplier. In our model, as in the standard one, the multiplier for income including government expenditures financed out of taxes is unity (see below).

Definition of Income

We are partly responsible for Hester's confusion because we left our broader model implicit, failed to spell out explicitly our consumption function, and, perhaps more important than either, failed to distinguish among different definitions of income.

Let us therefore be specific now. Using Hester's notation and equation numbering (except that we use letters to designate new equations and equations he left unnumbered), we have:

$$Y = C + A = Y_a + W \quad (a)$$

$$Y' = C + I + G + II - M \\ = C + A + T. \quad (7)$$

These equations contain three definitions of income:

1. Y_a or consumer disposable income, which is the after-tax income of consumers (or ultimate income recipients) on a predominantly cash basis.⁷

2. Y , which is (roughly) the after-tax income of consumers on a predominantly accrual basis, W consisting of two main items: the undistributed income of corporations plus a set of adjustments designed to convert wage income to an accrual basis.⁸

3. Y' , or net national product, which adds to Y that part of government purchases of goods and services financed by income taxes, corporate profits taxes, and indirect business taxes.

Y_a and Y are two among many alternative concepts that might be regarded as candidates for the independent variable in the consumption function. Both are after tax, hence seem appropriate on that ground. We chose Y instead of Y_a on the basis of our various statistical tests of what components it was best to regard as autonomous.⁹ This choice is readily

⁷ It is exceedingly difficult to get any aggregate consistently on a cash or an accrual basis from national income accounts. See *ibid.*, 256, footnote 2.

⁸ Ando and Modigliani have pointed out correctly in their paper that we overlooked net government foreign transfer payments. This small item was inadvertently included in our W . It should have been excluded.

⁹ Our choice of Y instead of Y_a happens to illustrate how our criterion departs from the criterion of maximum correlation. In testing different concepts of money and income,

justifiable on the theoretical grounds that individuals regard undistributed corporate income and wage accruals in excess of disbursements as increments to their wealth and hence as a substitute for other savings. Certainly, the evidence for the permanent income hypothesis supports the use of such an accrual concept rather than a strictly cash concept. In any event, whether right or wrong, we find it hard to see anything "unorthodox" or unconventional or "objectionable" in using Y rather than Y_a as the independent variable in a consumption function. The counterpart to using Y as the income variable in the consumption function is the designation of A as the total that individuals regard as their savings.¹⁰

Despite its wide and currently nearly unquestioned use,¹¹ Y' has no simple meaning as a value of final products,¹² though it may at

we correlated Y_a and Y with various monetary totals for several different periods using both annual and quarterly data. Y_a gave the higher correlation in 25 out of 30 comparisons. Yet we ended up using Y (Friedman and Meiselman, *op. cit.*, 244). Needless to say, we would have done the opposite if our procedure had been what an unwary reader might infer from Hester's paper.

¹⁰ *Ibid.*, 268.

¹¹ We say "currently" because the early volumes of *Studies in Income and Wealth* are filled with discussions of its meaning and relevance. The issues raised have never been resolved, only ignored.

¹² If we wish to regard government as an intermediary (like corporations) and only the ultimate income recipients as acquiring products, then how governmental expenditures should be treated depends on the character of the goods and services that government produces. Insofar as the expenditures are for the production of intermediate products (like operating costs of roads used for business) they should be omitted, being already allowed for in the value of the final products. Insofar as they are for the production of final goods and services, they should be included in income, the part corresponding to final consumer goods (like operating costs of roads used for pleasure) being treated as part of consumption, the part corresponding to final capital goods (like construction costs of new roads) being treated as part of capital formation. Along these lines, Y' can be regarded as an estimate of total product, including government final product, only on the assumptions that the part of government expenditures corresponding to final products is precisely equal to the part financed by taxes, and that these final products are worth precisely their factor costs. These are arbitrary assumptions. But unless these assumptions are made, it is hard to attribute any special significance to this total from the point of view of the ultimate income recipients.

Simon Kuznets at one time argued that *direct* taxes could be regarded as the value the community placed on that part of governmental product consisting of final consumer goods and services, and *indirect* taxes as the value of intermediate products. While the argument is not entirely convincing, it has some plausibility by analogy to market valuation. The

times be a better indicator of aggregate economic activity than Y_d or Y . In any event, the critical issue is what income concept should enter the consumption function and, so far as we know, no one has defended Y' for that purpose. The use of one income concept for specifying the consumption function does not rule out using other concepts simultaneously for other purposes.

Perhaps these brief comments will explain why we paid no special attention to Y' . Perhaps they can also explain why our approach is not inconsistent with the usual balanced budget multiplier analysis. That analysis says that an increment of government expenditure matched by an equal amount of tax revenue will leave C , A , Y_d , and Y unchanged and will raise Y' and T by the increment of government expenditure.

The Model

The one modification we would make in Hester's model is to replace his consumption function:

$$C = a + bY_d, \quad (9)$$

by the one we used,

$$C = a + bY. \quad (9a)$$

This makes it possible to dispense with Hester's equation (12), which, as he notes, was inserted solely to connect Y_d and Y' and has no theoretical basis. We can then keep his

$$T = c + dY', \quad (10)$$

or replace it by the algebraically equivalent equation

$$T = f + gY, \quad (10a)$$

where $f = c/(1-d)$, $g = d/(1-d)$.

Equations (a) and (9a) are a complete model if A is treated as the relevant total of autonomous expenditures. These yield as the solutions for C and Y :

$$C = \frac{a}{1-b} + \frac{b}{1-b} A, \quad (b)$$

identification of total governmental production of final products with total taxes does not even have this element of plausibility.

An alternative is to regard government as an ultimate income recipient strictly parallel to the class of individual consumers rather than as an intermediary serving them. On this interpretation, T is the government's income, whereas $C + A$ is the income of the private sector. Again, the total has no special significance for the class of ultimate individual income recipients.

$$Y = \frac{a}{1-b} + \frac{1}{1-b} A. \quad (c)$$

The value of T is not required for the solution but can be derived from (10a):

$$T = \left(f + \frac{ga}{1-b} \right) + \frac{g}{1-b} A. \quad (d)$$

Similarly, Y' is given by

$$Y' = \left(f + \frac{a(1+g)}{1-b} \right) + \frac{1+g}{1-b} A. \quad (e)$$

These equations demonstrate that, contrary to Hester's assertions, there is no inconsistency between our model and the treatment of taxes as induced. Our model does not ignore ". . . the fact that taxes are a function of income." Hester's impression to the contrary derives from confusion between the formal structure of a model and the empirical judgment about what magnitude is best regarded as autonomous. The same model can accommodate various empirical judgments.

To illustrate, let us follow Hester and assume that

$$L = I + G + H - M = A + T \quad (13)$$

is the relevant total of autonomous expenditures. Equations (a), (9a), (10a), and (13) are then a complete model. They yield as the solution for C

$$C = \frac{a + ag - bf}{1-b+g} + \frac{b}{1-b+g} L, \quad (16)$$

identical with Hester's (16) if g is replaced by its equivalent, $d/(1-d)$. The solution for Y is

$$Y = \frac{a-f}{1-b+g} + \frac{1}{1-b+g} L, \quad (f)$$

for Y'

$$Y' = f + \frac{(a-f)(1+g)}{1-b+g} + \frac{1+g}{1-b+g} L, \quad (g)$$

where the coefficient of L is identical with that in Hester's equation (15) if g is replaced by its equivalent, and for T ,

$$T = \frac{f(1-b) + ag}{1-b+g} + \frac{g}{1-b+g} L. \quad (h)$$

Although, as Hester asserts, ". . . equation (16) differs fundamentally from equation (1)," (which is identical with (b)), both are derivable from the same model. They differ only in the empirical judgment about which concept of autonomous is appropriate.

The Relation Between r_{CL} and r_{CA}

Hester claims to demonstrate in equations (19) and (20) that if L is autonomous, so that (16) is the appropriate solution for C ,

$$r_{CL} \geq r_{CA}, \tag{i}$$

where the equals sign applies only if d , in his solution, or g , in ours is zero or if $r_{CL} = 1$. He concluded that “. . . if L is a sensible measure of autonomous expenditure,” we “. . . have stacked the cards against the Keynesian model” by using A .

By precisely parallel mathematics, it is easy to show that (i) holds also with the same provisos about equality, if A is autonomous, so that (b) is the appropriate solution for C . Hence, it would be equally valid to say that “. . . if A is a sensible measure of autonomous expenditure,” Hester has “. . . stacked the cards” in favor of the Keynesian model by using L .

As it happens, neither the one statement nor the other is valid, because the implicit assumptions in Hester's mathematics in effect require that $r_{CL} = r_{CA} = r_{CT} = 1$. Hester got his results by replacing T by hC , which means by assuming that $r_{CT} = 1$. Since $L = A + T$, it is easy to see that r_{CL} is the correlation of C with A plus part of itself and hence will be larger than r_{CA} if $r_{CA} < 1$. But T can be validly replaced by hC only if the model is assumed to hold exactly, without disturbances. Otherwise T would have to be replaced by hC plus a disturbance term. However, if the model holds exactly, r_{CL} is unity also and, hence, so is r_{CA} . It is extremely hard to get something for nothing!

Perhaps a valid analysis introducing disturbances in all the equations would produce a determinate result. We do not know, since we have not tried. Perhaps also this example may illuminate our assertion that the “sensible” measure of autonomous is an empirical not mathematical question.

Putting all models aside, we may ask what the relation is among the three correlations, r_{CA} , r_{CL} , and r_{CT} . Since $L = A + T$,

$$r_{CL} = \frac{r_{CA}\sigma_A + r_{CT}\sigma_T}{(\sigma_A^2 + 2r_{AT}\sigma_A\sigma_T + \sigma_T^2)^{1/2}} \tag{j}$$

If all correlations are positive, it can be shown that

$$r_{CL} \geq r_{CA} \tag{k}$$

according as

$$r_{CT} \geq r_{CA} \frac{(\sigma_{A+T} - \sigma_A)}{\sigma_T}. \tag{l}$$

The fraction by which r_{CA} is multiplied on the right hand side is necessarily less than unity (if $r_{AT} < 1$). Hence, r_{CL} will clearly be larger than r_{CA} if r_{CT} is larger than r_{CA} , and may be even if r_{CT} is smaller than r_{CA} .

This result may illuminate some of Hester's empirical results. T , like C , is a relatively smooth series. A is much more erratic. Hence r_{CT} apparently usually exceeds r_{CA} . Some of our own results also indicate that this is the case. Indeed, it was partly the high correlation of T with C that, on the basis of our criterion, led us to treat A rather than L as autonomous.¹³

The Use of a Model for Forecasting

Part of the confusion about income concepts may reflect the failure to distinguish sharply between (1) the use for forecasting of a model that is accepted as valid and (2) the testing of the relative “goodness” of two alternative models.

Suppose the above model is accepted as valid and it is desired to forecast, say, next year's income. Further, let Y and Y' be the alternative income variables it is desired to forecast. Suppose, first, A is regarded as autonomous. The procedure would be:

- 1) Forecast the value of A next year.
- 2) Use equations (b) and (d) to forecast C and T next year (the parameters are assumed to be estimated from past experience).
- 3) Add A and C to forecast Y .
- 4) Add A , C , and T to forecast Y' .

Suppose, now, that L is regarded as autonomous. The procedure would be:

- 1) Forecast the value of L next year.
- 2) Use equations (16) and (h) to forecast C and T .
- 3) Add $(C - T)$ to L to forecast Y .
- 4) Add C and L to forecast Y' .

Note that in both cases, only step 2 uses the model in any essential way. Hence, to compare the “goodness” of the income-expenditure and monetary models, it is necessary to set the

¹³Friedman and Meiselman, *op. cit.*, 253-257.

monetary model the same task as the above procedure sets the income-expenditure model. Because we concentrated on Y not Y' , the task we set it was to forecast C (see step 3 when A is autonomous) and we compared how closely the two alternative autonomous variables (A and M) were related to C . Had we concentrated on Y' we could have set the monetary model the task of forecasting $C + T$ and have seen how closely the alternative autonomous variables (A and M) were related to $C + T$. Given the apparently higher correlation between C and T than between C and A , the effect would presumably have been to show an even greater superiority for the monetary model. In all comparisons, we assume that the autonomous variable (either A or M) is known (or forecast accurately), in order to isolate the "goodness" of the model. In practice, inability to forecast A or M might render even a good model of little use for forecasting.

Hester concentrates on Y' and treats L as autonomous. Hence, it is reasonable for him to concentrate on forecasting C . However, if he were to try to test the "goodness" of the two models in forecasting Y , he would have to compare the correlation of $C - T$ with the autonomous variables (L and M), not the correlation of C with them.

Note that when Hester shifts from L to L' , L'' , and L''' , he should change the dependent variable. If he is interested in seeing how good his model is in predicting Y' , he should, in each case, use as the dependent variable the difference between Y' and his autonomous concept, since this is the part of income that the model professes to predict and a comparison of the two models should set them the same task.

If the monetary model is accepted as valid, the above procedure need not be used. M can be related directly to Y or to Y' and used to forecast it. In effect, this can be regarded as using M to forecast A or L . But M itself would still have to be forecast, and inability to do so might again render the model of little use for forecasting, however closely it seemed to mirror a fundamental structural relation of the economic system.

The Concepts of Autonomous, Induced, and Mixed Components of Income

Hester writes as if our decision to treat the government surplus or deficit rather than government expenditures as autonomous means that we regard taxes as autonomous; and, similarly, as if our decision to treat the net foreign balance rather than exports as autonomous means that we regard imports as autonomous. The preceding analysis of his model demonstrates that our decisions have no such implications. Yet it may help to say a further word to explain how Hester could be misled.

We can perhaps clarify the point best by quoting from our paper. The quotation is about the foreign balance because that is the item we considered first and hence discussed most fully.

The net foreign balance, [we wrote] . . . might be autonomous in either of two ways: one because imports and exports are separately autonomous, so the difference between them is; or two on its own account, with imports induced, in which case exports would be a mixed category, being the sum of an induced and an autonomous item.¹⁴

In terms of the theoretical structure of income-expenditure analysis, it seems clear that the 'correct' treatment of the foreign balance depends on the period of time. Over a sufficiently long period of time, exports cannot possibly be autonomous. They must be paid for and they can be only by the funds earned . . . from . . . exports to the U.S., by the expenditure of reserves of liquid assets . . . , or by borrowing or selling financial assets in the U.S. . . . It seems plausible to suppose that the sum of these last two items can be regarded as autonomous over a long period and that foreign countries then spend on U.S. goods and services this total plus what they earn for [U.S.] imports. With respect to imports into the U.S., that part related to autonomous domestic expenditures (e.g., imports entering into private investment) is also autonomous. However, this part is already included in estimated private investment, all treated as autonomous, hence should not be counted again here. That part related to induced domestic expenditures is itself induced. Thus, over a long period, the proper definition of autonomous seems clearly to be the net foreign balance. For a very brief period, on the other hand, the situation may be different. Foreign countries may decide how much to buy in the U.S. and accept for the time being whatever effects these decisions may have on their reserves. Indeed, that must be the case instantaneously, so that in a very short period exports are to be regarded as autonomous. Theoretical considerations alone cannot settle the matter. The question to be decided is rather whether the periods we are interested in — roughly periods of the order of magnitude of the length of cyclical fluctu-

¹⁴ *Ibid.*, 251.

tuations, i.e., for the U.S., about three to four years — are better treated as closer to infinity or to zero.¹⁵

Precisely the same analysis applies to the government budget. We treated the net surplus or deficit as autonomous not because we regarded both tax receipts and government expenditures as autonomous, and hence the difference between them as autonomous, but because our statistical tests indicated that expenditures were best regarded as a mixed category and taxes as largely induced. The rationalization is the same as for the net foreign balance, though less compelling. We can regard the government as deciding that total expenditures shall equal what is raised by taxes plus (or minus) a specified sum to be financed by borrowing (or used to repay debt). Again, for a very short period, the situation is different.¹⁶

Incidentally, in our examination of the budget items, we tried to deal with separate categories of taxes and expenditures in order to isolate those that were most likely to be autonomous. However, we did not go nearly as far as would be desirable.

The Empirical Evidence

Hester's Table 1

As noted in the introduction, waiving all questions about the appropriateness of the alternative concepts of autonomous expenditure, the general results of this table do not differ in direction from those we presented. The appearance to the contrary derives only from the difference in period covered.

With respect to the alternative concepts of autonomous expenditures, Hester has demonstrated neither that we used erroneous criteria in choosing our concept nor that we drew the wrong conclusions from the statistical tests we made. He gives no alternative criteria and not

¹⁵ *Ibid.*, 250.

¹⁶ As an historical judgment, this seems a particularly appealing description for the period since World War II when Congress seemed willing to spend whatever the tax system would raise plus a little more. For a recent interpretation of federal expenditures along these lines precisely in the context of national income models, see Wilfred Lewis, Jr., "The Federal Sector in National Income Models," *Models of Income Determination, Studies in Income and Wealth*, xxviii (Princeton University Press for the National Bureau of Economic Research, 1964), 250.

TABLE 1. — CORRELATIONS BETWEEN FIRST DIFFERENCES OF SYNCHRONOUS VARIABLES IN NOMINAL TERMS^a

Period	Income-Expenditure Theory		Quantity Theory			
	$r_{\Delta C \Delta A}$	$r_{\Delta C \Delta A \cdot \Delta M}$	$r_{\Delta C \Delta M}$	$r_{\Delta C \Delta M \cdot \Delta A}$	$r_{\Delta Y \Delta M}$	$r_{\Delta A \Delta M}$
(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Annually</i>						
1898-1958	.097	.051	.696	.694	.576	.086
1898-1908	.006	.774	.868	.949	.868	.411
1903-1913	.263	.289	.907	.909	.803	.411
1908-1921	.520	.355	.872	.844	.782	.414
1913-1920	.182	.351	.728	.758	.534	-.080
1920-1929	.280	.110	.693	.666	.627	.295
1921-1933	.735	.518	.820	.692	.786	.610
1930-1939	.900	.711	.890	.709	.884	.789
1933-1938	.733	.582	.879	.821	.832	.576
1938-1953	-.406	-.394	.353	.339	.180	-.112
1939-1948	-.688	-.677	.163	.002	-.177	-.234
1948-1957	.484	.541	.434	.500	.256	-.008
1930-1958	.007	-.047	.627	.628	.543	.070
<i>Quarterly</i>						
1946 _{II} -1958 _{IV}	-.557	-.563	.229	.250	.148	-.038

^a Δ = Year-to-year or quarter-to-quarter change; C = Consumption; A = Autonomous Expenditures; M = Money; $Y = C + A$; r = Simple or partial correlation coefficient between variables indicated by subscripts. See Friedman and Meiselman, *op. cit.*, 259-268 for precise definitions of the variables.

a scintilla of evidence that the concepts L , L' , L'' , or L''' are, to use his word, more "sensible" concepts of autonomous than is A . Until he does one of these things, we find it hard to take his calculations seriously — except as a fishing expedition for high correlations.

Hester's Table 2

It was an oversight and a mistake on our part not to include the first-difference correlations in our original paper. As a partial remedy, we include them here in tables 1 and 2 — table 1 giving the correlations between the first differences in the nominal magnitudes, table 2, with correction for price changes. Here again, the 1930's appear to be an exception to the general weight of the evidence. There are some minor differences between the correlations we give and supposedly identical ones given by Hester which we cannot explain. However, none alters the tenor of the results.

Hester's Alternative Interpretation of the High Correlations Between C and M

Whatever may be the relative size of the quantity theory and the income-expenditure correlations, the consistently high absolute

TABLE 2. — CORRELATIONS BETWEEN FIRST DIFFERENCES OF SYNCHRONOUS VARIABLES, CORRECTED FOR PRICE CHANGE^a

Period	Income-Expenditure Theory		Quantity Theory		
	$r_{\Delta C \Delta M \Delta P}$	$r_{\Delta C \Delta A \Delta P}$	$r_{\Delta C \Delta M \Delta P}$	$r_{\Delta C \Delta M \Delta P}$	$r_{\Delta Y \Delta M \Delta P}$
(1)	(2)	(3)	(4)	(5)	(6)
<i>Annually</i>					
1898-1958	-.189	-.159	.541	.534	.336
1898-1908	.005	-.789	.868	.952	.873
1903-1913	.064	-.389	.890	.907	.770
1908-1921	-.518	-.515	.714	.713	.310
1913-1920	-.602	-.548	.802	.780	.251
1920-1929	.086	.124	-.290	-.302	-.055
1921-1933	.407	.371	.577	.558	.481
1930-1939	.837	.804	.654	.569	.584
1933-1938	.620	.606	.759	.751	.633
1938-1953	.431	.463	.111	.216	-.174
1939-1948	-.919	-.922	.128	-.229	-.298
1948-1957	.300	.430	.407	.503	.200
1930-1958	-.391	-.370	.222	.177	.021
<i>Quarterly</i>					
1946 _{II} -1958 _{IV}	-.534	-.542	.175	.203	.128

^a P = Consumer price level; other symbols same as table 1.

levels of the correlations between C and M clearly require explanation, and certainly run counter to expectations engendered by the greater part of recent literature. In the abstract, as we noted in our paper:

... a close correlation between the quantity of money and money income is consistent with either monetary changes influencing income or income changes influencing the stock of money. Even if changes in the stock of money tend to precede changes in income, the direction of influence need not be from money to income. Both changes might be the result of a common third influence, but with money reacting more rapidly than income, and there are still other possibilities. In addition to statistical studies of the kind presented in this paper, therefore, other kinds of studies are needed to judge with any confidence the direction of influence.¹⁷

Hester suggests that the explanation for the high correlations is "... a money supply function having net national product as an argument." Again he apparently relies on intuition,

¹⁷ Friedman and Meiselman, *op. cit.*, 179.

since he gives no evidence. As it happens, this possibility has been rather exhaustively investigated and the evidence currently available is nearly uniformly unfavorable to it.¹⁸

Conclusion

We remain of the opinion that there is

... a striking division among students of economic affairs about the role of money in determining the course of economic events. One view is that the quantity of money matters little; the other, that it is a key factor in understanding, and even more, controlling economic change.¹⁹

Our paper tried to present some evidence relevant to deciding between these views. The kind of evidence we gave is not the only kind that is relevant and may not be the most important or significant. And, of course, much other evidence is available from other work by us and by many others. This other evidence needs to be added to and brought to bear on the main issue that divides economists into two groups.

Hester does not quarrel with the relevance of our evidence but with the particular form of the income-expenditure theory we use. His criticism of our procedure rests primarily on a misunderstanding of the theoretical basis of our approach. He offers neither theoretical argument nor empirical evidence in support of his alternative formulation. Hence his criticism is largely beside the point. That is unfortunate. We badly need work on these problems that will clarify the issues involved. We can ill afford to waste the energy, interest, and ability that Hester displays in his paper on frivolous quibbling.

¹⁸ See Milton Friedman and Anna J. Schwartz, "Money and Business Cycles," *Review of Economics and Statistics*, XLV, No. 1, Part 2, Supplement (Feb., 1963), 48-55; and Milton Friedman, "The Monetary Studies of the National Bureau," *The National Bureau Enters Its Forty-Fifth Year*, 44th Annual Report (National Bureau of Economic Research: June, 1964), 10-19.

¹⁹ Friedman and Meiselman, *op. cit.*, 166.

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REJOINDER

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Professors Friedman and Meiselman utilize econometric research. I do not agree with a familiar and distinctive methodology in their them that “. . . the same model can accom-

moderate various empirical judgments." A model embodies a unique set of judgments. Apart from this disagreement which is basic to our exchange, a number of their points deserve comment.

I confined my elementary objection to Friedman and Meiselman's test for autonomy to a footnote; I thought the point too obvious for controversy.¹ Suppose there exist two doubtful components of autonomous expenditure, G and H . Somehow I is known to be autonomous. Then Friedman and Meiselman argue that a necessary condition for G to be autonomous is that $r_{C-(I+G)} > r_{C-I}$ and r_{C-G} . Suppose in fact G is autonomous. Assume H is also autonomous and negatively correlated with G , but independent of I . In this case, r_{C-I} may exceed $r_{C-(I+G)}$ and G will be erroneously rejected as autonomous. Their test is sensitive to the variances and covariances of I , G , and H . The Friedman-Meiselman test is ill-suited for its task; components of autonomous expenditure will not be reliably selected by their procedure. Theory or "intuition" is necessary to specify components of autonomous expenditure.

The theory reported in my comment is hardly original. It is an attempted duplication of the textbook theory which Friedman and Meiselman criticize. That theory purports to explain how the level of GNP or NNP is determined, not how the level of Friedman and Meiselman's Y or consumption is explained. I represented the theory non-stochastically in conformity to textbook discussions.

In translating the theory to an econometric model it is necessary to introduce stochastic terms, and apparently, I should have done this explicitly. However, introducing stochastic terms in every equation would have produced

needless complexity. The autonomous expenditure theory states that time series of Y' and L should be highly correlated. The implicit assumptions then in my model are that equation (15) has a stochastic term and that r_{CT} is essentially unity. In the model as expanded here, r_{CL} need not equal either r_{CA} or unity.

Data in my comment were drawn solely from the period 1929–1958 for a variety of reasons: (1) greater reliability of the data; (2) greater relevance of the data to the current environment owing to similarity of institutions; and (3) ease of acquisition. The correlations between first differences of consumption and either measure of autonomous expenditure, A or L , exceed the correlation between first differences of consumption and money in both the prewar (1930–1939) and the postwar (1949–1957) periods. Using Friedman and Meiselman's own measures of A , M , and C the reader may plainly see in table 2 that the quantity theory fared relatively poorly during peacetime years in the past three decades.

The issue about what determines the supply of money will not be resolved in this rejoinder. The supply function of money has not been successfully estimated elsewhere. Professor Friedman has alleged that changes in the money supply lead national income. This lead however provides no basis for ignoring the supply function. Solow and Karaken have reported that the lead is an arithmetic artifact which ". . . has no causal significance at all."²

Finally, someone might have construed the Friedman-Meiselman paper as saying something about the relative usefulness of the autonomous expenditure and quantity theories. I have shown that their results are inconclusive. Given the oversimplified representations of the two theories, this is all one could expect to demonstrate.

¹ Milton Friedman and David Meiselman, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958," *Stabilization Policies*, A Series of Research Studies prepared for the Commission on Money and Credit (Prentice-Hall: Englewood Cliffs, 1963), 180–185.

² Robert M. Solow, et al., "Lags in Fiscal and Monetary Policy," *Stabilization Policies*, A Series of Research Studies prepared for the Commission on Money and Credit (Prentice-Hall: Englewood Cliffs, 1963), 24.