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A LITTLE MORE EVIDENCE FROM THE TIME SERIES

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IN AN earlier article in this *Journal*[2] and in several other places,[3] evidence has been presented supporting a theory of the demand for money that is a part of the "wealth adjustment process." The posited demand function has successfully passed a large number of tests in competition with more than a dozen alternatives, representing the bulk of substantive work on the demand for money in the past thirty years. Though no series of tests is "definitive," the evidence from tests against alternatives is of crucial importance in establishing the *economic* relevance of the particular demand function. I regard such tests as preliminary to —and far more important than—"Chow tests," "Theil-Nagar tests," "Durbin-Watson tests," and other sophisticated statistical procedures for establishing the relevance of particular hypotheses.[4] However, the accumulating evidence suggests that the use of refined statistical procedures may now be desirable. I welcome the opportunity presented by the comments of Courchene and Shapiro to present some of the available evidence on the points that they raised.[5]

Before considering the issues raised in Courchene and Shapiro's comment, we must review a few of the assumptions that I made in the earlier paper. After assuming homogeneity of first degree in W_n, I obtain $M = f(r^*, \rho, d, W_n/P)P$.[6] I then assumed (1) that r^* and ρ are sufficiently covariated that

[1] The author is indebted to Karl Brunner whose penetrating comments contributed to the earlier paper and who has collaborated in the work on the demand function for money. Helpful comments by Michael Lovell served to clarify several issues. I would also like to express appreciation for assistance from the National Science Foundation, the Ford Foundation's Faculty Research Fellowship, and the Graduate School of Industrial Administration, Carnegie Institute of Technology for financial support that has contributed measurably to the development and testing of the hypotheses at various stages.

[2] "The Demand for Money: The Evidence from the Time Series," *Journal of Political Economy*, June, 1963.

[3] Allan H. Meltzer, "The Demand for Money: A Cross-Section Study of Business Firms," *Quarterly Journal of Economics*, August, 1963; and "Yet Another Look at the Low-Level Liquidity Trap," *Econometrica*, July, 1963; Karl Brunner and Allan Meltzer, "Predicting Velocity: Implications for Theory and Policy," *Journal of Finance*, May, 1963; and "Some Further Investigations of Demand and Supply Functions for Money," *Journal of Finance*, May, 1964.

[4] My reasons for preferring the particular research strategy can be easily summarized: (1) I see little point in investing heavily in a hypothesis that is poor relative to the alternatives available. (2) Several problems in using sophisticated tests are not generally appreciated. In particular, many such tests assume that the hypothesis is true and thereby assume that the relevant testing against alternatives has in fact been done. Further, quite frequently such tests depend on asymptotic properties of distributions while applied econometricians work with small samples.

[5] Some steps in this direction have been reported in "Some Further Investigations . . . ," *op. cit.*, presented at the December, 1963, joint meeting of the Econometric Society and American Finance Association and published in the *Journal of Finance*, May, 1964. This paper compares a number of one- and two-stage least-squares estimates for demand and supply functions for money.

[6] The variables are defined as follows: M the nominal demand for money, r^* the yield on financial assets, ρ the yield on physical assets, d the yield on human wealth, W_n the nominal value of non-human wealth, P the deflator of non-human wealth, p the income deflator, Y_h^* the expected value of income from human wealth, W_h the stock of human wealth, Y_h the value of income from human wealth. It should be noted that the development here makes a different assumption about homogeneity, but it arrives at the *identical* empirical equation. The difference in assumptions has no relevance for the present discussion.

they can be combined in a single rate, r, (2) that $d = Y_h/W_h = Y_h^*/W_h \cdot Y_h/Y_h^*$, and (3) that Y_h^*/W_h is a constant. To obtain the demand for real balances, I deflated by p to obtain

$$\frac{M}{P} = g\left(r, \frac{Y_h}{Y_h^*}, \frac{W_h}{P}\right)\frac{P}{p}.$$

If we assume that the demand equation is linear in the logarithms and that Y_h/Y_h^* may be approximated by the ratio of net national product to Friedman's permanent income, Y/Y_p, we obtain

$$\ln \frac{M}{p} = a + b \ln r + c \ln \frac{Y}{Y_p} + d \ln \frac{W}{P} + e \ln \frac{P}{p} + u. \qquad (1)$$

It must be noted that equation (1) is a more complete version of the equation that I tested in my earlier paper. Two additional assumptions were made in the earlier paper. These assumptions—*and not the wealth-adjustment model*—are the source of the principal difficulties on which Courchene and Shapiro comment. First, I explicitly noted that I was concerned with the long-run properties of the demand function for money and assumed that Y_h/Y_h^* would be equal to unity (with logarithm = 0). Second, I explicitly assumed that P/p, an index of interest rates, could be combined with r. These assumptions remove ln Y/Y_p and ln P/p from the present equation (1) and render it *identical in form* to equations (3) or (3′) of my earlier paper.

Subsequent work, some of which has been reported in a paper with Brunner,[7] suggests that the problem of serial correlation, on which Courchene and Shapiro comment, can often be avoided if it is not assumed that the long-run demand for money is independent of Y/Y_p. Moreover, *some* of the tests fail to support my assumption. At times, Y/Y_p enters with a significant negative coefficient as the hypothesis implies and my assumption denies. Omitting my second simplifying assumption, which combines P/p with r, also reduces serial correlation in some samples. But the reduction in serial correlation is obtained at a price, namely, a substantial increase in multicollinearity.[8]

In the earlier paper, I indicated[9] that one of the assumptions used to develop the model—the high covariance between r^* and ρ—does not hold for the period of World War II and the early postwar years. During that period, financial interest rates, r^*, were pegged while the yield on physical assets, ρ, was much less rigidly restricted. In long period samples, data for the war and postwar period are comparatively submerged. In very precise tests using data for short periods, like the "Chow test" that Courchene and Shapiro conducted, the failure to consider my explicit statement creates some problems of interpretation that they fail to note. The estimates for equation (1) reported here use data for 1900–1941 and 1952–58 but omit the period of pegged interest rates.

We can now consider Courchene and Shapiro's specific criticisms of my earlier paper. (1) They correctly note the problem of serial correlation; (2) they question my use of the word "stability" and deny that the hypothesis is as "stable" as I suggest; (3) they question the basis for my conclusion about Gurley-Shaw and claim to have better evidence for the position that I took. Interspersed in their note are some addi-

[7] Cf. "Some Further Investigations . . . ," *op. cit*

[8] I use a simple correlation of ½ to indicate multicollinearity in the samples reported in Table 1 below. The simple correlation of Y/Y_p with r, W/P, and P/p for the period 1930–58, omitting 1942–51, is −.84, .56, and .87, respectively. For the same period, the correlation of P/p with r or with W/P is −.70 and .76, respectively. The correlation of r and W/P is only −.19. For the years 1900–1958 (omitting 1942–51) similar problems occur. The correlation of r and W/P is only −.27 while the correlation between r and Y/Y_p or P/p is higher, −.50 and −.70. P/p is also correlated with W/P and Y/Y_p in this period at a level of .63 and .71. For 1900–1929 the principal problem of multicollinearity is between r and W/P and equals .81. This may account for the lower interest rate and wealth coefficients in the period.

[9] ". . . The Evidence from the Time Series," *op. cit.*, p. 242.

tional comments to some of which I will intersperse a reply.[10] The stability issue will be treated first. The other problems can then be dispensed with quickly.

STABILITY AND THE COMPARATIVE WEALTH ELASTICITIES

The authors correctly note that I did not define the meaning of the word "stability" though I used it frequently to describe some of the results. By "stability" I meant that the parameter estimates for the wealth-adjustment model, computed for different time periods, appeared to be drawn from the same underlying population. I invariably used the term in that sense, a meaning that is hardly original and that did not generally mislead Courchene and Shapiro.[11]

The authors note that on the basis of the Chow test, "we must reject (at the 1 per cent level) the hypothesis that the same model generated the data in both periods" (1900–1929, 1930–58). But Courchene and Shapiro fail to note that the Chow test is inapplicable when there is serial correlation of the residuals. The estimates in Table 1 for the more complete statement of the hypothesis, equation (1), pass the Chow test at the 5 per cent level. Note, however, that some serial correlation remains.

Tables 1 and 2 summarize some additional information. Table 1 reports the results for the more complete hypothesis, equation (1) of the present paper; Table 2 summarizes some relevant results for the truncated hypothesis of the previous paper. Space permits only three comments about the comparative interest rate and wealth elasticities: (1) The point estimates of the r and W/P elasticities for the two subperiods are closer to the full period estimate when the more complete hypothesis replaces the truncated hypothesis. Thus the point estimates suggest even greater "stability" than the earlier results or those presented by Courchene and Shapiro.[12] (2) The principal differences between the estimates reported in the two tables appear to result from the exclusion of the war years. For 1900–1929, the computed elasticities are virtually identical in the two tables, and the t statistics are generally larger for the more complete hypothesis. (3) The wealth elasticity of M_1/p is smaller in every period than the wealth elasticity for M_2/p.

SERIAL CORRELATION AND MULTICOLLINEARITY

The last column in Table 1 and footnote 8 provide some relevant information about these two statistical problems. My brief remarks pertain to the more complete hypothesis, equation (1), only. First, the significance of the interest rate and wealth elasticities does not appear to be substantially affected by the presence or absence of the two problems. Second, the more

[10] One such comment: Despite their interest in statistical procedures such as those noted above, the authors suggest that I "isolated a long-run trend rather than a behavioral relationship." This statement is quite puzzling since it suggests that isolation of "time trends" is an alternative to construction and testing of hypotheses (see their n. 9). The evidence to which the authors refer in this footnote may be relevant to a test of the hypothesis, and I regret that they did not present it. It would be particularly interesting to know whether they forced the free parameter to be zero in their test. If they did not, the test to which they refer is irrelevant, since it assumes that the free parameter is a function of time. The reader may easily verify this by integrating the logarithmic first differences from an equation like (1) above that includes a free parameter. I did not assume that the free parameter, a, in the demand equation is a function of time.

[11] Courchene and Shapiro are misled when they impute to me another meaning of the term "stability" that is said to be related to the neo-quantity theory. I find no support for this in my earlier paper, and they offer only a reference to some work by Friedman. To argue, as Courchene and Shapiro do, that "one believes that payment habits . . . shift in a fundamental way" without offering a hypothesis implying the "shift" is to miss completely the point of formulating and testing hypotheses (see n. 14 below).

[12] *Demand Analysis* (New York: John Wiley & Sons, 1953). Note that the comment on this point in the introduction of Courchene and Shapiro's note confuses point estimates with estimated variances.

TABLE 1

REGRESSION ESTIMATES FOR EQUATION (1) USING DATA FOR 1900–1941 AND 1952–58 ALTERNATIVE DEFINITIONS OF MONEY

TIME PERIOD	ESTIMATED ELASTICITY AND t-STATISTIC* FOR				R^2 (ADJUSTED FOR DEGREES OF FREEDOM)	DURBIN-WATSON
	r	Y/Y_p	W/P	P/p		
For M_1/p:†						
1900–1929	−0.31 (3.14)	−0.37 (2.24)	0.86 (17.54)	0.57 (2.21)	0.96	0.95‡
1930–41 and 1952–58	− .76 (5.68)	.31 (1.41)	1.14 (14.62)	.33 (1.07)	.99	1.99§
1900–1941 and 1952–58	− .64 (10.79)	.07 (.54)	1.02 (39.44)	.83 (3.90)	.99	0.89
For M_2/p:‖						
1900–1929	− .41 (4.21)	− .42 (2.64)	1.44 (30.05)	.61 (2.47)	.98	1.15#
1930–41 and 1952–58	− .67 (5.55)	.31 (1.58)	1.22 (17.33)	− .62 (2.25)	.99	2.01§
1900–1941 and 1952–58	−0.51 (7.86)	0.15 (1.07)	1.35 (48.14)	−0.44 (1.91)	0.99	0.78

* t-statistic in parentheses.
† M_1 = currency plus demand deposits.
‡ Serial correlation indeterminate at the 1 per cent level, significant at the 5 per cent level.
§ Serial correlation not significant.
‖ M_2 = M_1 plus time deposits at commercial banks.
Serial correlation indeterminate at the 5 per cent level.

complete hypothesis has less serial correlation but more multicollinearity.[13] Third, the more serious problems occur in the M_2/p regressions. For M_1/p, all significant parameters have the signs implied by hypothesis. P/p has the appropriate sign and is significant in two of the three periods. Only Y/Y_p has a sign that contradicts the hypothesis. For the longer period estimates, this "contradiction" supports the special assumption that I made, namely, that the index of transitory income derived from human wealth is without significance in the longer period estimates.

GROWTH OF FINANCIAL INTERMEDIARIES

Courchene and Shapiro criticize my interpretation of the wealth elasticities as suggestive of an important reason for the

[13] For additional findings on the absence of serial correlation in sequential estimates see "Predicting Velocity . . . ," *op. cit.*, Appendix III.

TABLE 2

INTEREST RATE AND WEALTH ELASTICITIES (WITH ESTIMATED t-STATISTICS)†

Period	Interest Elasticity (t-Value)	Wealth Elasticity (t-Value)
For M_1/p:†		
1900–1929	− 0.32 (3.0)	0.84 (16.1)
1900–1958	− .95 (21.8)	1.11 (42.0)
For M_2/p:		
1900–1929	− .42 (3.9)	1.41 (27.2)
1930–58	− .72 (10.3)	1.12 (20.6)
1900–1958	− 0.50 (10.8)	1.32 (53.2)

* Computed from the truncated hypothesis $\ln M/p = A + B \ln r + C \ln W/P + U$. The years 1942–51 were included in the relevant samples.
† I did not estimate the parameters of this equation for 1930–58.
See note to Table 1 for values of M_1 and M_2.

growth of financial intermediaries. (1) They fail to note that I clearly presented my result as an *extrapolation* to the broader definition that they call M_4, after using three alternative definitions of money or financial assets. (2) While their evidence supports my conclusion, it is of little interest unless supported by data for some other period. Their result is heavily dependent on data for periods in which some, but not all, interest rates and prices were controlled.

SUMMARY

The critical question would seem to be: Do the principal conclusions of my earlier paper remain valid? An affirmative answer seems to be indicated by the results from the more complete statement of the hypothesis. First, interest rates and real wealth remain the primary determinants of the demand for money.[14] Second, the evidence from the Chow test, if applicable, supports the asserted stability of the demand function when the years 1942–51 are omitted. Since I noted in the earlier paper that the hypothesis is not applicable to these years, I interpret this finding as support for my earlier results. Third, the demand function for currency plus demand deposits, M_1, is at least as stable as the demand functions that use alternative definitions. My earlier conclusion is reinforced by the results here, particularly the "poor" estimates for P/p in the demand function for money, inclusive of time deposits.

The present results suggest that some of the problems that Courchene and Shapiro noted were intensified by my use of simplifying assumptions that are not an integral part of the wealth-adjustment hypothesis. As is often the case, the authors seem to have jumped from the possibility of inefficient estimates to the conclusion that the inefficiency invalidates the results. Nevertheless, serial correlation may create some problems particularly for the longer period estimates. The successful completion of numerous tests against alternative hypotheses, and the results reported here, seem to indicate that the wealth-adjustment hypothesis has sufficient economic relevance to make additional work on those problems worthwhile.

[14] A major criticism of the Keynesian-type demand functions that relate real balances to interest rates and real income is that the interest elasticity of the demand for real balances is practically zero for the period 1900–1929, when income is measured by real net national product. Moreover, the sequential estimates of the interest elasticity for one form of this hypothesis are rarely negative in the period prior to 1928 as Brunner and I reported in "Predicting Velocity . . . ," *op. cit.* This contradicts an important feature of the particular hypothesis and casts serious doubt on its stability. Proponents of the hypothesis need to explain this shift in the interest elasticity. The fact that the authors obtain a lower, though still non-significant, interest elasticity for the period 1900–1929 by using GNP rather than NNP indicates that the particular hypothesis is sensitive to measurement p ocedures. Their results raise a question about why the interest elasticity is sensitive to the inclusion of depreciation as a part of income. As shown in Section II of my earlier paper, the wealth hypothesis has very similar parameter estimates for any of a number of alternative definitions of wealth.