"The Demand for Money: A Cross-Section Study of Business Firms": Reply

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This essay provides the kind of concise and incisive survey that we have learned to await with anticipation from the ever-active pen of Professor Johnson. This time he is discussing the role of monetary policy as a stabilization device. The paper, originally prepared for the Canadian Royal Commission on Banking and Finance, deals mainly with Canadian problems. But the author ranges over the pros and cons of a number of issues of general interest, and there are useful comments on a variety of open questions.

The emphasis is on the principles of policy operation, as the title indicates, but the author is aware that principles and goals cannot be neatly separated from the instruments or procedures available to attain them. Most of the latter half of the essay therefore discusses the advisability of employing techniques or instruments, most of which the Bank of Canada has not been empowered to use, and the results to be expected if each of the items in the enlarged bag of tools is employed. Problems of selective credit controls, controls on financial intermediaries, and moral suasion are discussed. This section of the report would seem to be useful to a central banker, commission, or legislature trying to appreciate the merits of conflicting claims about the effectiveness of selective controls or moral suasion. The author does not take the position that selective controls are discriminatory and therefore "bad." Instead, he considers both the loss of economic efficiency and the gain in the power of stabilization policy likely to result from limited, temporary use of such instruments. Contrary to widely accepted opinion, based largely on U.S. experience, Johnson recognizes a role for moral suasion in Canada. This role is dependent on the greater centralization of deposit banking and the resultant increase in the effectiveness of administrative pressure in such a system.

The reader will be troubled by the absence of any discussion of Canadian mone-

tary policy beyond the general condemnation that appears. Doubtless, the composition of the original audience influenced this choice. But such information might have furnished stronger foundation for what I regard as the author's principal—though tentative—conclusion: Monetary policy (in Canada) should be directed toward "providing a stable long-run monetary environment, the responsibility for short-run stabilization being transferred to other instruments of economic policy."

This conclusion is one of the three alternatives discussed. The others are (1) retain monetary policy as an instrument for influencing the price level and employment in the short-run, but reduce the expected level of achievement; and (2) maintain the expectations about the performance of policy, but improve policy operations by eliminating sources of error. The author is not sanguine about achieving the second aim, although he regards a combination of alternatives (1) and (2) as the best prospect if Canada decides to retain a fixed exchange rate.

This reviewer is not yet ready to abandon the propositions that central bank policy can be placed on a more rational foundation, and that doing so does not require either abandonment of the type of money market stabilization that is desired by much of the financial community, or a reduction in short-run stabilization objectives. Moreover, I would not expect a central bank to be able to carry out a long-run policy (or adhere to a rule) without a better understanding of money supply theory than is now possessed.

Of course, theory cannot resolve the conflict between domestic and international goals that becomes extremely important when the exchange rate is fixed. It can only assist in making the consequences of particular choices more explicit. In the present state of knowledge, we can do little more than point out the general, qualitative consequences of particular choices. This is what Professor Johnson has done admirably.

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REPLY

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Since Whalen accepts my statistical results as “accurate,” and Maddala and Vogel accept my model as “true to a first approximation,” there is perhaps a greater need for discussion among the three of them than with me. Nevertheless, some of their comments require a reply. I shall discuss Whalen’s comments first.

Mr. Whalen’s objections fall under two headings. First, noting that there may be precautionary and liquidity “motives” as well as transaction “motives” for holding cash, he argues that the “economies for scale in holding transactions cash balances may be offset by more than proportionate increases in cash balances held for liquidity-preference motives.” His dissent is with Baumol as the following quotation from Baumol suggests:1 “there is little that we can say about the latter [speculative demand for cash], important though it may be, except that it seems unlikely that it will work consistently in any special direction.” Baumol’s statements clearly indicate that

he would expect the elasticity of money balances with respect to sales or transactions to be less than one, a statement that my evidence fails to support. Moreover, Baumol recognizes — as Whalen does not — that the Baumol model has no content unless some statements about the other "motives" are made to complete the hypothesis.2 Brunner and I have reported a substantial volume of evidence showing that hypotheses based on the "motives analysis" are rejected in comparisons with any of a number of alternative demand functions for money.3 This would seem to support my procedure.

Second, Whalen argues that: (1) the yield on wealth (ρ) is positively correlated with sales (S); and (2) this correlation "might have" (sic) reduced the estimated elasticity of cash with respect to sales. Application of Theil's procedure for analyzing the bias imparted by omitting ρ indicates the opposite conclusion.4 In short, if my assumption that ρ is a constant for the industry is replaced by Whalen's assumption that ρ and S are positively correlated, the elasticity of money balances with respect to sales is understated in my paper. Contrary to Whalen, this would support the Friedman — but not the Baumol — hypothesis.

Maddala and Vogel try to make a number of points. First, they challenge the relation of the evidence to the hypothesis by arguing that manufacturing firms hold only 10 per cent of the money supply. They claim that there is therefore no a priori reason for the time series demand function to be applicable to the cross-section data. Of course. If the cross-section hypothesis were true a priori there would be no point to the more than 126 tests that I made. But my statement that the time series and cross-section data should give the same estimated wealth elasticity is not based on a priori speculation. It is the result of the derivation of the cross-section demand function from the time series function with the aid of an auxiliary assumption. The derivation implies that the coefficient of wealth from the time series and the coefficient of sales from the cross-section data are the same if the hypothesis is correct. That is what I tested.

2. This comment does not apply to Tobin's "Interest Elasticity of the Transactions Demand for Cash," Review of Economics and Statistics, XXXVIII (Aug. 1956). The demand function in his paper can be joined to the demand function in his paper "Liquidity Preference as Behavior Towards Risk," Review of Economic Studies, XXV (Feb. 1958), at least in principle. If the liquidity demand is not constant for business firms, my results are not relevant for the completed Tobin model.


Maddala and Vogel go further and argue, citing Kuh,\(^5\) that “cross-section estimates are usually expected to be higher than time series estimates.” This is a misinterpretation of the Kuh paper. Kuh did not show that the two sets of estimates must differ. He stated some conditions under which this situation could occur. A major point of the Kuh argument is that cross-section estimates may more closely approximate long-run parameter values while time series estimates may reflect shorter-run, cyclical effects. Readers of my time series paper \(^6\) will recall that it is explicitly presented as a long-run model. Moreover, the cross-section data that I used were averages, a procedure that Kuh uses to reduce the “individual firm effect” in cross-section estimates. Further, I noted that the error terms from my cross-section analysis are generally free of serial correlation.\(^7\) This further suggests the absence of an “individual firm” or “average firm” effect that is of consequence for the relevance of the Kuh analysis in the present context.

Second, Maddala and Vogel question the comparability of my results to Baumol-Tobin and Friedman. I have acknowledged above that my results may not apply to the Tobin model since I did not join his liquidity preference and transaction hypotheses. But this is not the Maddala and Vogel point. The claim that I use sales as (1) “a surrogate for wealth” and that this is “inconsistent” with the use of sales as (2) “a surrogate for transactions” and (3) “for permanent income.” They are mistaken. I do not use sales as a “surrogate for wealth.” I introduce a specific hypothesis that relates sales to wealth, viz., \(S_{ij} = K_{ij} W_{ij}\).\(^8\) It is implicit in my discussion that the average sales for a size group is a measure of the group’s “permanent income.” This reduces the differences between the Friedman, Baumol and wealth hypotheses to the size of the elasticity in the cross-section regressions. I fail to understand the objection.

Third, Maddala and Vogel are correct when they state that I claimed that the hypothesis implies that interest rates are an important determinant of the demand for money. That is precisely what the hypothesis implies and the evidence supports both the cross-section and time series forms of the hypothesis in comparison with

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7. Ibid., p. 416, fn. 3.

8. \(S\) and \(W\) are sales and nonhuman wealth; \(\rho\) is the yield on wealth; \(K\) is a proportionality factor; \(i, j\) refer to firms and industries respectively.
alternative hypotheses, some of which deny the relevance of interest rates.

Fourth, Maddala and Vogel present some evidence of their own that is said to show that it is possible to "draw stronger conclusions from the same set of data." (1) I have no quarrel with their findings about the quadratic. Their evidence is an extension of the data in my Table IV, page 417 of the original. It is useful to have the additional support that their evidence provides, but I do not see how it "strengthens" my conclusion that "economies—or diseconomies—of scale may be significant in some industries." Unfortunately, their results provide no evidence on the open question of why the quadratic term is negative in some industries and positive in others.

(2) I would be most happy to accept their evidence, obtained by replacing sales with total assets in the regression equation. This would provide a more direct test of the hypothesis and would avoid my auxiliary postulate that sales are related to wealth. Unfortunately, the use of total assets as the variable by which firms are classified and the simultaneous use of total assets as an "independent variable" in the regression substantially reduces any "scatter" of points in the cash-total assets plane. It is not surprising that their correlation coefficients are high as a consequence of this procedure, but the meaning of the results is questionable.

(3) Isn't it inconsistent to reject my auxiliary postulate that sales are related to wealth (what Maddala and Vogel call a surrogate) and then discuss the high collinearity of sales and total assets, their measure of wealth?

To conclude, I would remind the reader that there is an observed association between cash balances and sales in more than 126 cross-section regressions. The results deny an important implication of Baumol's hypothesis and of Friedman's hypothesis. They are consistent with the implications of the wealth hypothesis, and as such they are additional support for that hypothesis. I claimed no more in the earlier paper.

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9. It is difficult to understand the reasoning behind their alternative approach—taking logarithms of an equation that is a function of both sales and the square root of sales.

1. Had they chosen the food industry, I suspect they would have found even "stronger" results, but it would have been more interesting to investigate the textile industry more fully. For that industry I was unable to find any significant, positive effect of the quadratic in any of the nine years for which I computed cross-section estimates.