Notes and Comments

The Demand For Money in Pakistan: Reply

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I am grateful to Abe, Fry, Min, Vongvipanond, and Yu (hereafter referred to as AFMVY) [1] for obliging me to reconsider my article [2] on the demand for money in Pakistan. Upon careful examination, I find that the AFMVY results are, in parts, misleading and that, on the whole, they add very little to those provided in my study. Nevertheless, the present exercise as well as the one by AFMVY is useful in that it furnishes us with an opportunity to view some of the fundamental problems involved in an empirical analysis of the demand for money function in Pakistan.

Based on their elaborate critique, AFMVY reformulate the two hypotheses—the substitution hypothesis and the complementarity hypothesis—underlying my study and provide us with some alternative estimates of the demand for money in Pakistan. Briefly their results, like those in my study, indicate that income and interest rates are important in determining the demand for money. However, unlike my results, they also suggest that the price variable is a highly significant determinant of the money demand function. Furthermore, while I found only a weak support for the complementarity between money demand and physical capital, the results obtained by AFMVY appear to yield a strong support for that relationship.[1] The difference in results is only a natural consequence of alternative specifications of the theory and, therefore, I propose to devote most of this reply to the criticisms raised by AFMVY and the resulting reformulation of the two mypotheses.

The main AFMVY criticisms against my study are directed at (i) the use of actual prices rather than expected prices; (ii) the use of aggregate real income and real money balances rather than per capita real income and real money balances; (iii) the propriety of incorporating the index of industrial production in the substitution hypothesis; (iv) the use of net investment rather than gross investment in the complementarity hypothesis; and

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[1] I take this opportunity to point out that in my original study of the demand for money in Pakistan, *PDR* editorial staff made a minor slip in substituting R^2 for R throughout the paper. This means that in order to be comparable to R^2 provided by AFMVY, all relevant numbers should be squared. Further, note that R^2 in the AFMVY study is not adjusted for degrees of freedom.

(v) the lack of reversibility of my version of the complementarity hypothesis.[2] These five criticisms will be examined in detail in their order of listing here.

The most important difference between the results of my study and those provided by AFMVY stems from the latter's use of expected rather than actual prices. Since direct observations on expected prices are not widely available, the usual procedure to obtain expected prices is to find a proxy by postulating a scheme for generating expectations in terms of the actual data. Of course, one can generate many versions of expected price series from a single observed price series by choosing differently distributed lags, and one can always find, at least, one that would yield the results desired by the investigator.[3] The price expectations hypothesis generated in this way are therefore only conjectures, and it is one such hypothesis that is used by AFMVY.

In order to select one of the alternative price expectations models, one must take into account *a priori* considerations relevant to the particular case and empirical evidence, if any, on the formation of price expectations. AFMVY have completely failed to consider both of these facets. In the case of Pakistan, *a priori* considerations strongly suggest that during most of the period under consideration, price expectations were constant, hence the actual prices may also be regarded as the appropriate expected prices. Between 1951 and 1970, Pakistan experienced a rather low rate of inflation, averaging only 3 percent per year. Further, the yearly rise in prices averaged considerably less than 3 percent for the first fourteen years and no more than 5 percent for the last six years. Since price expectations are formed on the basis of the economic agent's awareness of the behaviour of prices (nominal as well as real prices) and such awareness is virtually non-existent if the prices are relatively stable, it is doubtful that any significant price expectations could have been formed in Pakistan, especially during 1951-64. The existence of a dominant non-monetized segment of the economy during the early period, properly minimizing the role of money and prices, appears to lend further support to this view. Another point which tends the same way is that until 1958, government controls on prices

2. Four minor points raised by AFMVY deserve brief comments. The first point is "since the rate of inflation was negative in six years between 1950 and 1970... we [AFMVY] wonder how he [Akhtar] took logarithms." Trivial ! I merely added a constant fraction to the rate of change of prices throughout the period so that logarithms could be taken of positive numbers—the procedure is quite standard. The second point is related to the lagged adjustment process in the demand for money. It may be dismissed as unimportant to the present discussion because it does not directly bear on the significance of explanatory variables of the demand for money in my study. The third point is concerned with my use of the end-of-year money stock data. While I do not believe that centred mid-year estimates would affect any of the conclusions of my study, on logical grounds AFMVY are correct in taking me to task on this point. Finally, AFMVY object to my use of consumer and wholesale prices in deflating data on income and money stock. Since more than half of the regressions reported in my study incorporated net national product at constant prices and money stock data deflated by the implicit national income deflator, my attempts to try various alternatives, whether or not they add any new information (which they do), certainly do not subtract anything from the analysis.

3. There are other well-known limitations of this procedure, but it is not necessary to appeal to them for support of the present argument. The reader may look at the reference lists provided by Carlson and Parkin [5], and Turnovsky and Wachter [10]. These two studies, based on data from the US and the UK, are exceptional in that they have attempted to measure price expectations independent of the actual or observed data on prices.

and distribution of commodities kept prices unusually stable, thereby leaving very little room for the formation of price expectations. Even in the later period, PL 480 provided a butter to maintain low prices. Note that it is not the government controls, rather the low rate of inflation brought about by the operation of such controls as acts as a deterrent to the formation of price expectations. In fact, the government and political structure may be an important influence on expectations.

Unfortunately, the empirical literature on the formation of price expectations is scanty; however, the available evidence is in *complete* agreement with the *a priori* proposition that when the rate of inflation is low price expectations tend to be constant. In the case of moderate type of inflation, the evidence favours an autoregressive scheme for measuring price expectations.[4] There is also some evidence to suggest that price expectations may be influenced independently of the actual rate of inflation, by factors such as government policy aimed at controlling inflation, changes in political structure, and changes in exchange rates.[5]

These considerations indicate that the AFMVY use of expected rate of inflation, obtained on the basis of past as well as present (observed) rates of inflation, is unwarranted and muddles the results. Its apparent significance is misleading and appears to have no economic meaning. It is possible that the expected prices used by AFMVY tend to serve as a proxy for some variable excluded from the model. In any case, whatever else they may be, they do not appear to be the relevant expected prices for Pakistan. Our analysis indicates that the most that could be expected from an appropriately specified price variable for the period 1951-70, would be to exhibit *some* awareness of the behaviour of prices. However, if the period under consideration could be properly divided into two, 1951-64 and 1965-70, then our analysis suggests the use of actual prices for the first period and the use of expected prices based on a *purely autoregressive* scheme for the second period. Clearly, the actual price variable is preferable to the expected price variable used by AFMVY.

Let us now turn to the remaining four criticisms. The question of deflating income and money stock data by population has been the subject of a long controversy in the literature on the demand for money. It is beyond the scope of this note to review the controversy; however, a few observations may serve to clarify the basic issue involved in this respect. None of the proponents of the per capita formulation has ever provided any compelling *economic* reason for assuming that the money demand function is homogeneous of the first degree in the stock of population. As noted by Meltzer [8], a plausible case can be made for including several other variables such as the distribution of population between urban and rural areas, the number of banking institutions, and the age composition of population, into the money demand function. For obvious reasons, this is especially true in the case of developing economies. Further, the inclusion of these variables may in fact suggest that the money demand function is not homogeneous of the first degree in the stock of population. Elsewhere [3], I

[4]See, for example, Turnovsky [9], and Carlson and Parkin [5]

[5]See Carlson and Parkin [5, pp. 132-35]

have tried several other variables in the case of Pakistan and the results seem to support the view that other variables may be just as (or more) important as population. Now, one can muster some *statistical* support for the use of per capita data, but that may or may not be an unmixed blessing. Recently, in a pure trend model, Jacobs [6] has provided such justification in terms of the apparent income elasticity and its standard error. However, he has concluded that deflating the aggregate data by prices and population, deflating by prices only, or using nominal data undeflated by population and prices are mathematically equivalent methods of defining the aggregate money demand function. Thus, when properly interpreted there is very little difference in the results obtained with or without deflating by population. Indeed, this is at least partially evident in the fact that the AFMVY conclusion on the significance of income is the same as that of my study.

Upon review of the passage on justification of incorporating the index of industrial production (y_1) I find that the point is quite clear and straight forward. The basic argument may be summed up by saying that in a developing economy like Pakistan, a substantial part of industrialization is independent of the private saving and the interest rate because it is financed by the government sector either directly through the budget deficit or through aggressive monetary and credit policies. Ergo, it is reasonable to try y_1 as an independent variable rather than subsume it under the rate of interest or income. The direct and indirect efforts of the public sector to finance the growth of industry lead to increases in the nominal as well as the real stock of money. The AFMVY argument that deficit financing will reduce the real stock of money by causing inflation disregards the substantial expansion of industry due to government efforts. Besides, output is highly responsive to increases in deficit financing ; the price level will either remain unchanged or may in fact go down. In a recent study of inflation in India and the Philippines [4], I found that deficit financing was not a significant influence on prices and in many cases it appeared to be negatively related to the rate of inflation. The finding may be relevant to Pakistan because between 1951 and 1970, both India and the Philippines experienced rates of inflation and deficit-income ratios either similar to or somewhat higher than those in Pakistan. Notwithstanding their criticism of the propriety of y_1 as an explanatory variable in the money demand function, AFMVY have retained its use as a proxy for the degree of monetization. There is, however, some evidence (see Akhtar [3]) which suggests that the index of industrial production may be a relatively poor proxy for the degree of monetization in Pakistan.

The AFMVY criticism of my use of net investment in the complementarity hypothesis is rather unimportant. From the viewpoint of the subject at hand, it is sufficient to point out that even a "complete reading" of McKinnon [7] cannot dismiss equation (5-2) on 45 which expresses investment as changes in the stock of capital overtime, i.e. net investment. Furthermore, it seems that in the framework of accumulating capital net investment rather than gross investment is the appropriate variable to be included in the money demand function.

The final criticism on the list deals with the lack of reversibility of the complementarity hypothesis tested in my study. I fully grant that this criticism is not trivial. However, the AFMVY reformulation of the complementarity hypothesis is plagued with, at least, three fatal problems. First, they have substituted the ratio of domestic saving to total income (S/Y) for the ratio of investment to income (I/Y) into the money demand function (equation [11]), suggesting that the former is a good proxy for the latter. Obviously, this is not correct. Investment and saving are determined by different forces and, in practice, remain in a permanent disequilibrium in the case of most developing economies. In addition, the direction of change in I/Y may or may not be the same as that in S/Y. McKinnon [7, p. 59] underlined the fact that the superiority of using I/Y is based on the existence of marginal and intramarginal differences in the rates of return in developing economies. The substitution of S/Y for I/Y usurps a part of that superiority since S/Y takes into account only a particular set of rates of return from the non-monetary sector.

Second, the decomposing of the real rate of return on money (d-p*) into two components of d and p* improperly specifies the model. In the absence of an empirical analysis of (d-p*), it is virtually impossible to assess the significance of the real rate of return on money in determining the money demand function on the basis of the results on d and p*. Besides, the decomposing of (d-p*) is likely to cause some additional statistical difficulties in the regressions. The problem is further accentuated by the AFMVY use of the inappropriate price variable. For example, the results in equation (24) appear to be inflated because of decomposing and the use of expected prices.

Third, the AFMVY reformulation does not accurately reflect the reversibility underlying McKinnon's complementarity hypothesis. More specifically, AFMVY have failed to incorporate (d-p*) as an independent variable in equation (10). The reverse complementary relationship between money demand and physical capital is based on the notion that changes in (d-p*) raise the investment-saving propensities which leads to increases in the demand for real money balances. McKinnon [7, pp. 60-66] is quite emphatic on the significance of this channel in the "conduit" effect of money. The consideration of other variables in equation (10) without any explicit analysis of (d-p*) appears to cloud the basic issue. Furthermore, the proper subject of reverse complementary relationship is I/Y, but having substituted S/Y for I/Y, AFMVY have no choice in the matter.

The preceding discussion indicates that four of the five criticisms levelled against my earlier study do not hold under a close scrutiny. On the contrary, the analysis shows serious shortcomings in the AFMVY reformulation of the two hypotheses. The fifth criticism is valid, but the alternative adopted by AFMVY poses almost as many problems as it resolves. As far as the two basic differences in the empirical results are concerned, the present analysis demonstrates that the importance of the so-called expected price variable does not represent the significance of prices in the determination of the demand for money function in Pakistan, and that the

AFMVY results on the complementarity hypothesis do not provide unequivocal support to the complementary relationship between demand for money and physical capital.

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