

This raises several questions. If one accepts the expectational aspect of interest rates (that is, rising price levels influence interest rates), does it necessarily follow that the inflation was induced by the money supply. By this I mean, that if investors react to rising prices, is this synonymous with the relationship between money and prices. Professor Fand appears to make the Quantity of Money synonymous with the expectations effect. Secondly, the question of money being a by-product is very elusive. If government finances a deficit through the banking system, which in turn has a salutary effect on business spending how would this be classified in terms of his schema? Would the newly created money be the cause of, or the result of, the increase in business activity? Since the money supply has significant aspects of endogeneity, it is difficult to see important instances of independent changes in money. This would exclude, of course, the simplistic example of money being thrown out of an airplane.¹³

Without denying the significance of the contribution made by the Monetarists in stressing the real rate of interest, it is still difficult to see the operational aspects of this concept. How is the Federal Reserve supposed to react to a variable that, despite the overall instability of this period, showed little variability. Also, how is the real rate related to concepts such as the return on capital and the return on sales. Certainly there is the problem of being able to define the concept. Even if, however, we are able to define the real rate of interest accurately, it is still accurate to state that borrowers pay the going market (nominal) rate and not the real rate. For industries experiencing a rise in the prices of its output, the deflation of the nominal rate is relevant. What of those industries, the prices of whose final products do not rise proportionately with the average price level. What if they also experience rising wage demands? These appear to be a few of the questions that may be raised to the relevancy, and accuracy, of the oft-mentioned (but seldom thoroughly analyzed) real rate of interest in Professor Fand's paper.

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REPLY

I. Professor Lucia questions the key role that I assign to expectations in analyzing the interest rate escalation in the U.S. in the latter half of the 1960's. He starts by assuming that the Fisherian price expectations effect is slow to develop and necessarily involves a long lag, and he inclines therefore to the

¹³ The growth of money does appear to be related to the size of treasury deficits. Thus the largest increase in M (of 7.6 per cent) came in 1968 after a budget deficit of \$25.1 billion in fiscal year 1967.

view that prices were relatively stable in the decade prior to 1965 and did not provide much scope for inflationary expectations to raise nominal interest rates. He writes:

"On the basis of relatively stable prices in the U.S. from the mid-1950's to 1964, it is difficult to say that interest rates from 1965 on were influenced by expectations of inflation. Whatever the merits of the theoretical framework, the facts just do not seem to support the theory".

Lucia believes that the "relatively stable prices" from the mid-1950's to 1964 should have resulted in relatively stable price expectations and could not be a major factor in the post-1965 interest rate escalation. Having ruled out any Fisherian (long lag) price expectations effect, Lucia seeks to determine whether there is any statistical evidence that "recent changes in prices influence interest rates". To test this possibility of a shorter lag he regresses interest rate changes as a function of price changes lagged over five quarters for the period 1965-1971. The 1965-1971 quarterly regression has an R^2 of .07 and not one of the coefficients is statistically significant. Lucia therefore rules out any short lag price expectations influence on nominal interest rates in the 1960's, and concludes:

"Thus a dilemma seems to exist since if we go back beyond this immediate period, the relative stability of prices during the 1955-64 period would not appear to be a good explanation of rising interest rates. If we come closer to the 1965-71 period there is little evidence of statistical relationships that explain much of the variation in interest rates via price changes".

The Yohe-Karnosky study which I cite in my article does offer some evidence for distinguishing the factors affecting price expectations in the 1950's and 1960's. They also take up and discuss a number of alternative lag structures and summarize their experiments with several kinds of lags for the price expectations variable. In addition, the two interest rate charts in my article are based on the St. Louis and the Morgan Guaranty regression equations which do incorporate a price expectations variable in the interest rate prediction equation for the 1960's. And the expectational variable does exert a substantial influence, of several hundred basis points, on market interest rates during the latter half of the 1960's in both of these monthly models.¹

I do not claim, of course, that the St. Louis and the Morgan Guaranty frameworks described in my paper, or other models with similar results, prove that inflationary expectations did raise nominal interest rates in the 1960's. Undoubtedly other substantively different models can be developed to explain the same data. Similarly, if Lucia's regression equation fails to find evidence of an inflation effect on nominal interest rates in the period 1965-1972, it

¹ See "High Interest Rates and Inflation in the U.S.: Cause or Effect?", this *Review*, March 1972, pp. 26-31.

would not disprove the price expectations effect, even if his equation was properly specified.

The regression equation presented by Lucia to estimate a price expectations effect appears to be misspecified. His interest rate prediction equation does not take account of changes in money (real) balances, the liquidity effect, or of changes in real output, the income effect. Instead he treats interest rate changes as if they were a function, solely, of price expectations, defined by the actual price changes in the preceding five quarters. His results, if taken literally, imply (1) that the long-term rate would be over 12 per cent if prices were stable, and (2) that inflation must somehow lower interest rates since long rates were considerably below 12 per cent in this period.

Lucia views his equation as suggesting that inflationary expectations were, at most, a very minor factor in raising interest rates. But his equation also suggests that they were a depressant of interest rates — a finding that is surely unacceptable to monetarists and non-monetarists alike, and indicative of a misspecification.

2. The analysis in section III, *The Natural Rate of Interest, Real Rates of Return, and Nominal Interest Rates*, offering criteria to discriminate between an interest rate rise due to inflation and inflationary expectations and an interest rate rise due to an increase in the natural rate of interest and a capital boom is questioned by Lucia. On the basis of several criteria summarized in this section, I suggest that there is no evidence of a rise in the real rates and that the available evidence on productivity and profits, though it may be inconclusive, tends to support the view that inflationary expectations were raising nominal rates relative to real rates.² Lucia questions this view that market rates were rising *relative* to, and not in *response* to, a rise in the real rate of return, and suggests instead that there may have been an investment boom. He writes:

"From *Table I* it can be seen that a rise in business profitability did occur. Using both net profits as a return on stockholders equity and as a return on sales, the profitability of investment appears to have risen from 1963 on then falling below the 1963 level by 1970, reflecting of course the recession in that year. These data together with the increasing volume of investment spending make this criticism of Neo-Keynesian doctrine spurious".

Professor Lucia questions my assessment that the productivity short-fall and the profit squeeze in the 1960's argue against a rise in the real interest rate and tend to rule out an investment boom as the factor causing nominal rates to rise. Lucia seems to believe that "a rise in business profitability did occur", and he supports this view with annual data on net profits on stockholders equity and net profits per dollar sales for the period 1963-1971.

² *Ibid.*, pp. 19-31.

While I would not have selected either of these two variables as a suitable proxy for a real rate of return on investment, both measures tend, nevertheless, to support my tentative conclusion of an inflation induced rise in market rates. Thus, both net profits and profits per dollar of sales in his *Table I* fell over 25 per cent since 1965, and these data are consistent with the finding by Okun and Perry that there was a steady decline in the corporate profits share of GNP from 10.9 per cent in 1966 to slightly over 8 per cent in 1970, even when they adjust profits to include corporate net interest payments. As indicated in my footnote 33, Okun and Perry, who carefully review corporate profits experience in the 1960's, show that profits plus net interest as a share of nonfinancial corporate gross product fell from 18.6 per cent in 1966 to 14.4 per cent in 1970 and conclude that there was indeed a profit squeeze. See their "Notes and Numbers on the Profits Squeeze".

3. Professor Lucia comments that my emphasis on the liquidity effect in rationalizing the 1969 interest rate developments is a grudging concession to the neo-Keynesian view. Lucia apparently fails to see that no one really questions either the existence, or the significance, of a *ceteris-paribus* liquidity effect and the short-run negative relation between money and interest rates. Differences center on the extent to which the positive income and price expectations effects may, at times, dominate the negative liquidity effect, even in the short run; and there are, possibly, more substantial differences concerning the secular relation between money and interest rates and the rationalization of the Gibson Paradox.

Monetarists and others who support the quantity theory of money must surely accept the *ceteris-paribus* negative relation between money and interest rates and they must also acknowledge that this inverse relation typically dominates in the short run. Where monetarists and quantity theorists may differ from neo-Keynesians and others is in highlighting the positive *mutatis-mutandis* income and price expectations effects, in suggesting that these positive effects may, at times, occur within a year of the acceleration in money growth, and in assuming that these positive effects may, in particular circumstances, dominate the negative effects even in a relatively short-run time frame.³

4. Lucia acknowledges that the recent emphasis on the real rate of interest may be a contribution, but he goes on to say,

"... it is still difficult to see the operational aspects of this concept. How is the Federal Reserve supposed to react to a variable that, despite the overall instability of this period, showed little variability".

³ See D. I. FAND, "Keynesian Monetary Theory, Stabilization Policy, and the Recent Inflation", *Journal of Money, Credit and Banking*, August 1969; "A Monetarist Model of the Monetary Process", *Journal of Finance*, May 1970; and "Money, Interest and Prices", in *Savings and Residential Financing 1970 Conference Proceedings* (Chicago, 1970).

Lucia's question suggests that he may be misinterpreting the recent discussion of real and nominal interest rates. The real rate of interest is not being proposed as an operational variable for the Federal Open Market Committee to include in its *directive* to the Account Manager in New York.

Real rates of return are introduced in order to motivate the distinction between nominal and real interest rates, to explain why rising market rates need not necessarily be associated with rising real interest rates, and to indicate why an escalation of market rates need not always result from, or reflect, tight money. Recent discussions are thus designed to show that market interest rates may be rising even if real rates are stable and even if monetary growth is accelerating.

The importance of the nominal-real distinction shows up in the evaluation of monetary policy in 1968. Analysts who assumed that nominal and real rates move together concluded on the basis of the interest rate escalation that monetary policy was tight in 1968. Other analysts who did explicitly distinguish between nominal and real rates were more inclined to view 1968 as a case of *easy* money and *tight* credit. Emphasis on the real rate of interest is sometimes designed to suggest the possibility of market rates rising relative to real rates and to motivate the analytical distinction between tight credit and tight money.⁴

The real interest rate cannot, at the present time, be measured and is not being proposed as a new operational variable for the implementation of monetary policy. But so long as the *nonobservable* real rate is not measured, it necessarily reminds us that are using an *observable* market interest rate variable as a proxy for it. And it may, in this indirect way, also focus greater attention on the monetary aggregates.

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⁴ See D. I. FAND, "A Monetary Interpretation of the Post-1965 Inflation in the U.S.", this *Review*, September 1969; "Some Issues in Monetary Economics", this *Review*, September 1969; and "Monetarism and Fiscalism", this *Review*, September 1970.