In the article he contributed to the July issue of this Review, Professor Kaldor makes one central point: that changes in the money supply must be regarded as the result, not the cause, of changes in economic activity. He states this point in different ways, embellishing it with assorted illustrations, each time as if it were a profound idea that had never occurred to the well-meaning but benighted monetarists he is attacking. Strip this point from his discussion and there remains mostly rhetoric—some of it clever, some of it accurate, much of it neither. Establish this point, and his case against the monetarists is firm: pins move with the cycle; money moves with the cycle; this is evidence of neither a pin theory of the cycle nor a monetary theory of the cycle but of the pervasive influence of cyclical fluctuations.

As it happens, Professor Kaldor is a Johnny-come-lately with this point. The monetarists themselves recognized its importance from the outset, and, if they had not, their U.S. critics have for the past decade repeatedly flourished it—just as Kaldor does, with all the air of Little Jack Homer extracting a plum from his Christmas pie.

As a result, this issue has been explored intensively. The outcome is about as decisive as the answer to any such question can ever be: clearly, there are influences running from income to the quantity of money, as Professor Kaldor asserts but, equally clearly, there are strong influences running from the quantity of money to income. The latter do not and should not exclude the former.

I have summarized the evidence for the influence of money in an article reprinted in a book to which Professor Kaldor refers, but which he apparently has not read.¹ I began the summary as follows:

The specific issue I propose to consider is … whether the cyclical behavior of money is to be regarded as a major factor explaining business fluctuations or as simply a reflection of business fluctuations produced by other forces …

… The alternatives contrasted are not mutually exclusive. Undoubtedly there can be and are influences running both ways …

[Moreover], there can be and almost certainly are factors other than money that contribute to the cycle, whatever may be the role of money. The question at issue is, therefore, whether money exerts an important independent influence, not whether it is the only source of business fluctuations and itself wholly independent of them.

What kind of evidence can be cited on this issue?

I then cited five kinds of evidence:
(a) Qualitative historical circumstances, which I termed “perhaps the most directly relevant kind of evidence”; (b) the behavior of the determinants of the money stock; (c) consistency of timing on positive and inverted basis, in the course of which I emphasized that “regular and sizeable leads of the money series are themselves suggestive of an influence running from money to business but they are by no means decisive,” and cited three reasons why the timing evidence could be misleading; (d) serial correlation of amplitude of cycle phases; (e) evidence from foreign countries.

After summarizing the evidence under these five headings, I concluded:

In a scientific problem, the final verdict is never in. Any conclusion must always be subject to revision in the light of new evidence. Yet I believe that the available evidence of the five kinds listed justifies considerable confidence in the conclusion that the money series is dominated by positive conformity, which reflects in some measure an independent influence of money on business. The feedback effect of business on money, which undoubtedly also exists, may contribute to the positive conformity and may also introduce a measure of inverted conformity.

The reader can judge the weight of the casual empirical evidence for Britain since the second world war that Professor Kaldor offers in rebuttal by asking himself how Professor Kaldor would explain the existence of essentially the same relation between money and income for the U.K. after the second world war as before the first world war, for the U.K. as for the U.S., Yugoslavia, Greece, Israel, India, Japan, Korea, Chile and Brazil? If the relation between money and income is a supply response, as Professor Kaldor asserts that it is for the U.K. since the Second World War, how is it that major differences among countries and periods in monetary institutions and other factors affecting the supply of money do not produce widely different relations between money and income?

Notes

* The paper that is the subject of this comment is "The New Monetarism," by Nicholas Kaldor, Lloyds Bank Review, July 1970, pp. 1-18.