

## Reply to Wright's Commentary

**Allan H. Meltzer**

**R**andall Wright has several complimentary things to say about some of my earlier work. I thank him for those remarks. Most of his comment, however, fails to discuss my current paper, and the main comments he makes about the paper are untrue. Specifically, my paper does not attack theory or oppose the development of micro-foundations for macroeconomics. It is not about economic policy. I am at a loss to understand how a reader could come away with either an idea or with a belief that I am critical of recent work on economic development and growth.

The paper proposes specific hypotheses for analyzing the role of money and uncertainty. The foundation is a micro-theory in which there is production for inventory. Uncertainty about the duration of observed changes gives rise to costs of information. The reason is that permanent and transitory changes cannot be distinguished for some time after a change occurs. In this model, money is privately and socially valuable because it reduces costs of transacting and bearing uncertainty.

I use this framework to discuss three problems related to the questions that the organizers asked me to address: (1) why some prices are set; (2) why some firms set nominal prices; and (3) why some prices are sticky—have substantially less variance weekly or monthly than prices in auction markets. I give explicit references to papers in which the framework is more fully developed or in which it has been used fruitfully in empirical studies.

I contrast this framework with others in which there are no sticky prices, no produc-

tive or useful mediums of exchange, no differences in costs of acquiring information, and no distinction between money, bonds and capital that would enlarge the role of relative price changes in the transmission of monetary and real shocks. I am critical of models with one open market interest rate and no sticky prices, and models in which money is introduced as a socially costly way to overcome frictions in an otherwise frictionless Walrasian model. I am skeptical of some of the conclusions drawn from such models.

Wright gives considerable space to methodological issues. Although the models we use influence the way we look at the world, it is a mistake to confuse the model with the world. Science, well done, does not equate the model to the world; it recognizes that all useful models generate refutable propositions. The weak testing procedure called calibration that is now fashionable is a distant substitute for serious, careful assessment of competing hypotheses.

Much recent work, including real business cycle models or overlapping generation models of money, have implications that are readily refuted. For example, it is well established that all correlations between money and income are not the result of "reverse causation" and that all unemployment is not the result of intertemporal substitution.

I have proposed an alternative model in which uncertainty and costs of transactions and information have a large role. Monetary arrangements (and other institutions) reduce these costs, but some costs are unavoidable.

Wright's comment reports on some of the research that is now under way or that has been published in the recent past. As always, some of this work will prove fruitful, some not. There is a high cost of information and great uncertainty about which will be successful.

It is my view that progress on the important issue about how a monetary economy adjusts to changes or shocks will require more attention to uncertainty about the

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permanence of shocks and to costs of information and transactions. That view may prove right or wrong, but it is neither atheoretical nor anti-theory.

I respect Wright's past work and looked forward to his comments. I regret that he avoided discussion of the issues raised in my article and my proposals for dealing with them. Such a discussion is overdue.