Randall Wright is a professor of economics at the University of Pennsylvania and serves as a consultant to the Federal Reserve Bank of Minneapolis. The author thanks Dan Thornton for useful comments on an earlier draft, and Lee Ohanian for several useful discussions.

Commentary

Randall Wright

Allan Meltzer raises a variety of issues, and reviews and extends some research he and his collaborators have been pursuing over the years. Some of the more or less technical points he presents, both with regard to the theory and the evidence, will undoubtedly be of interest to many macroeconomists. He does a good job of presenting these technical points, and so my plan is not to discuss them in any detail here. Instead, I want to address some more general methodological issues. That is, I plan to comment mainly on some remarks Meltzer makes on the state of macroeconomics.

To provide some motivation for the discussion, I would like to begin with a few quotations from the Introduction to his paper. Meltzer says that "For decades, macroeconomists have listened to criticism from their professional colleagues about the absence of micro-foundations for most of what they say and do...It is time to question whether this now widely accepted approach is likely to be fruitful."

He appears from his remarks to be of the opinion that the answer is no. While conceding that we may have learned one or two things over the years, the suggestion is that much of modern macroeconomics is at a dead end. For example, "Overlapping generations models of money, intertemporal substitution theories of unemployment and productivity shock theories of the business cycle *have not proved fruitful*...[and] the results to date *are not promising*" [emphasis added]. He further suggests that the current state of affairs compares to the Keynesian-monetarist debates of a generation or so ago.

Presumably, he puts forward this assertion so that the reader will be more sympathetic to the alternative approach provided in his paper. But what I want to do is question the assertion itself. To focus the discussion, I propose to debate the following position: *We have made little progress in macroeconomics since the Keynesian-monetarist debates, and existing models built on micro-foundations are neither fruitful nor promising.* I perceive this position to be a fair reflection of the view expressed in the paper. But even if this is not exactly what Professor Meltzer had in mind, I believe that it is an interesting issue to debate. I hope the reader will forgive me if it appears I am debating a straw man, and indulge me the opportunity to present some of my own views on the state of macroeconomics.

As I see it, economists have made remarkable progress in understanding things that bewildered us just two or three decades ago. I will describe this progress in four of the most important areas of macroeconomics: business cycles; the labor market; monetary economics; and growth. I will also discuss some more general methodological issues toward the end. This is not meant to say that I am totally unsympathetic to the views of Professor Meltzer, merely that I think he overstates the case when he asserts that existing macroeconomic models are neither fruitful nor promising.

BUSINESS CYCLES

Two decades ago, few would have believed the following assertion: A frictionless, competitive, non-monetary model built around the one-sector growth model, abstracting from heterogeneity, distortionary taxation, and many other features of reality, can generate time series that look like those in the data when hit by impulses that seem like a reasonable representation of stochastic technological progress. Since the work of Kydland and Prescott (1982), Hansen (1985) and others, we know that the assertion is true. But I am sure that even Kydland and Prescott would not have expected it ex ante.

Consider the original version of what I will call their dynamic general-equilibrium (GE) model. (This is a more accurate label than the more common real business cycle, or RBC, model, given that many people work with monetary versions of the model). It included many complications, such as time-to-build, non-time-separable utility, and signal extraction problems, which, while still included in some applications, are not parts of the current standard benchmark model. Why were those complications there? They thought a simple model wouldn't stand a chance. On one interpretation, the entire exercise was to see just how bad things were, so we would have some idea where to go next (for example, in terms of adding other impulses and propagation mechanisms).

To everyone's surprise, however, even very simple dynamic GE models do quite well at replicating key aspects of the macro time series. Output is more volatile than consumption, not as volatile as investment, and about as volatile as the labor input; and, the coherence of all these series is high. Furthermore, the model is consistent with these features of the data at a quantitative level, not just a qualitative level.

Traditional macroeconomists, especially Keynesians, reacted to these findings with much suspicion, and virtually every aspect of the analysis was called into question. In retrospect, many controversial issues turned out to be red herrings, including the following:

(1) Abstracting from heterogeneity (that is, focusing on a representative agent) is an assumption that, depending on the questions, is sometimes appropriate and sometimes inappropriate, but is never good or bad as a matter of principle.

(2) Abstracting from market failures, frictions and money is a proper first step—no one should advocate complication for its own sake—and the fact that simple models can be solved efficiently by exploiting the welfare theorems does not mean that users of these models believe the real world is "first best" nor that policy is unworthy of discussion.

(3) Calibration is a way of taking models to the data that avoids many complications; although these days many of us are estimating dynamic GE models using traditional econometric methods (see, for example, McGrattan, 1994).

(4) The HP filter is simply a convenient tool, and obsessing over its merits or demerits is like debating whether the mean, median or mode is the "correct" measure of central tendency.

The consensus today is that the dynamic GE models are useful tools for studying business cycles. Of course, this does not mean that business cycle research is a solved problem. There are many unanswered or partially answered questions, such as the correlation between employment hours and productivity, the equity premium, and the relations between real and nominal variables. Much work has been done to address these questions with some, but not total, success. There are still interesting puzzles out there—but this is why working in the area is exciting. The point is that we now have a *standard model* of the business cycle, a base case from which to generalize when the situation warrants it.

The dynamic GE approach is a tool for macroeconomics the way that the supply-and-demand approach is a tool for microeconomics. One should not ask: "Is the model true?" but only: "Is it useful?" Have we made progress understanding business cycles? Yes. Are these models based on microeconomic foundations? Yes. They are based on the standard economic principles of constrained optimization (which, in a dynamic context, obviously concerns intertemporal substitution) and a coherent concept of equilibrium. Can the base model accommodate frictions, money, heterogeneity, private information and so on? Yes. Do we need to throw out dynamic GE theory in favor of new micro-foundations or a retrograde macro approach? No, no more than we need to throw out supply-and-demand curves.

Of course, a base model is always simplistic. In the case of supply and demand, for example, suppose we want to know what will happen to the price of orange juice after a frost in Florida or a Vitamin C craze. Is it OK to abstract from private information, strategic issues, reputation and so on, and proceed by shifting the supply or demand curve? I don't know the answer definitively, but I think, provisionally, yes. Similarly, if we want to ask something basic about business cycles, it seems reasonable to use the basic dynamic GE framework as the benchmark. To readers interested in studying this in more detail, I recommend the book *Frontiers of Business Cycle Research*, edited by Thomas Cooley.

THE LABOR MARKET

It is commonly believed that unemployment is a major economic and social problem. We have not come up with a definitive model that explains unemployment or gives us a panacea to cure unemployment. That is, we have not *solved* the problem. But we know something about it and can study it scientifically. We know that incentives matter, whereby I mean things like unemployment insurance, dismissal restrictions, tax policy and so on. These things can be built into economic models built on standard micro-foundations (constrained optimization and coherent equilibrium concepts), and analyzed both qualitatively and quantitatively.

Some of the issues are more or less static: for example, the incentive effects of unemployment insurance on layoffs and hours per worker (see, for example, Burdett and Wright, 1989). Others are intrinsically dynamic. A major recent success concerns the application of search models of labor market dynamics to worker and job flow data. Combining the job creation-job destruction data analysis of Davis and Haltiwanger (1990) with the theoretical framework laid out, for example, in Pissarides (1990) has proved fruitful. These authors have used dynamic GE models based on search theory to account for the main empirical features of the labor market, like the job creation and job destruction data (see, for example, Mortensen, 1994). These models can be used to study policy interventions qualitatively and quantitatively.

Of course, as I stated earlier, there is more than one model of unemployment. This is as it should be. There is more than one type of unemployment. Efficiency wage considerations, insider-outsider considerations, multiple equilibrium considerations, and so on, each may have some elements of truth to them. Moreover, these models are not mutually inconsistent, but are complimentary special cases of a general framework (see Mortensen, 1989). We should not look for a simple single answer.

Are frictions in the labor market important, as Meltzer suggests? Yes. Can private information be a relevant consideration, as Meltzer suggests? Yes. Does this mean a move away from micro-foundations, or a move to new micro-foundations, is the answer? No. Many researchers have been working on incorporating frictions and informational considerations into the standard paradigm for years. It has been successful. Given this success, I do not see any reason to argue to return to a reduced-form Phillips Curve approach. My preferred alternative is to learn search theory and forge ahead.

MONETARY ECONOMICS

Not so long ago, there did not exist in the literature a serious formal model of a dynamic monetary economy. The overlapping-generations (OLG) model, invented by Samuelson (1958) and developed by many people (see, for example, Wallace, 1980), has remedied this deficiency. That model has been and continues to be an extremely useful framework within which to illustrate theoretical properties of monetary economies, to interpret episodes in economic history, to shed light on policy debates, and to discover new things about economics generally. Concerning the latter, it is worth remarking that many technical discoveries, such as the possible inefficiency of competitive equilibrium, or the potential for endogenous limit cycles and sunspot equilibria, revolved closely around the analysis of OLG models (see, for example, Azariadis, 1993). These discoveries seem important for macroeconomics.

When Meltzer criticizes the OLG model, perhaps what he has in mind is that there are certain phenomena for which it is ill-designed to explain. One could belabor the obvious and argue that money in the OLG model is only a store of value and not a medium of exchange. But a model need not capture every feature or nuance of money in order to teach us something about monetary theory or policy. More to the point, we now have theoretical models in which money clearly and indisputably is a medium of exchange. Some of these models are built around search frictions that capture Jevons' famous "double coincidence of wants" problem with direct barter; see, for example, Kiyotaki and Wright (1989) or Trejos and Wright (1995). Others are built around private information problems; see, for example, Williamson and Wright (1994).

Allan Meltzer, along with Karl Brunner, is on record as saying that private information is the driving force behind the use of money in modern economies. He reiterates this position in the current paper. Some of us who work in monetary theory have taken his position to heart and have attempted to formalize these ideas. We do not think of ourselves as abandoning micro-foundations; the models are built on search or private information frictions incorporated into microeconomic models with optimizing agents and coherent equilibrium concepts.

I agree with Professor Meltzer when he argues that we need to develop theories that incorporate not only money, but also other aspects of the real world, like brokers, dealers, market makers, intermediaries and so on. Given this, it seems that search-based models of the sort analyzed by Rubinstein and Wolinsky (1987), for example, are promising. Like much of the search-based monetary theory, these models are primitive, but they do address many of the issues that Meltzer correctly identifies as important.

Due to their rudimentary nature, the models to which I am referring are not yet very good at providing policy guidance. They do not answer, "What should we do at the discount window next week?" I for one do not think that this is the most interesting question in monetary economics. Even if one is interested mainly in policy, there is potential value in building qualitative models that help edify us and our students regarding more basic issues. At the same time, monetary dynamic GE models currently exist that, although not as well-grounded in terms of first principles of microeconomics, can be brought to bear on more mundane policy affairs. We have seen some of them discussed at this conference.

Is it a problem that pure and applied monetary economics have not converged? In any science, it should not be too surprising that progress in pure and applied theory proceeds in counterpoint and not in unison. That is why monetary economics today is vibrant and flourishing.

GROWTH

It was only about a decade ago that macroeconomists were relatively uninterested in growth theory and in policy directed toward economic growth. One reason may be that our attention was directed toward other issues—business cycles, unemployment and money. Another reason is that we were looking at the wrong models. Although models with perpetual growth have been around for decades, the standard Solow model in the textbooks is in the unfortunate position of not explaining growth, except as a transitory phenomenon on the way to a steady state or as the outcome of exogenous technical progress. We owe something to Romer (1986) and Lucas (1988) for redirecting our attention.

There is now a plethora of endogenous growth theories—arguably, too many. However, these models all have common threads that I hope will allow us to distill common essence. We know that growth is important as a matter of welfare. As compared to eliminating cyclical fluctuations in GNP, getting the growth rate up a few percentage points is an order of magnitude more important. Can we achieve higher growth by better policy? Can we understand why different economies grow at different rates? The jury is still out, but these are obviously interesting questions. The way to answer them is with standard economic theory.

When I say "standard economic theory," I do not mean that we should stick to the status quo when confronting issues for which the textbook model is inappropriate. But going from Solow's leading example to a model with non-decreasing returns is hardly a scientific revolution. Is there a role for private information or other frictions in modern growth theory? Potentially. The interaction of growth with financial development and intermediation may be interesting and important. Some work has been done and more is in progress. But I did not see anything in Meltzer's approach that makes me want to stray from mainstream growth theory.

GENERAL METHODOLOGICAL ISSUES

There have been many technical developments that have paved the way for these successes in macroeconomics. One obvious innovation involves computational ability. Graduate students now have machines on their desks that allow them to solve and simulate dynamic GE models as homework in a good first year macro-course. If one really thinks that heterogeneity, incomplete markets, income distribution or related issues are important, we now have the technology and the power to solve models with these complications; see Rios-Rull (1995).

There have been developments outside the domain of hardware. The publication of Stokey and others (1989) illustrates how we now all have access to a set of tools that few macroeconomists were comfortable with not so long ago. The analysis of multiple equilibria, including dynamic multiplicity, endogenous limit cycles and phenomena like sunspot equilibria have given us a new set of ways to think about the world. Game theory has provided us with new ways of posing and solving strategic questions, including bilateral bargaining problems that are central to some of the phenomena that Meltzer emphasizes (see the references in the section of monetary economics). Analysis of data, like the job creation and destruction data, or the cross-country growth data, have given us new things to think about and new ways of confronting our models with reality.

Lucas (1980) emphasized the interplay between technical developments, on the one hand, and deviations between theory and facts, on the other hand, as what leads to progress and change. He argues that this interplay was behind the emergence in the 1970s of "rational expectations" macroeconomics and the downfall of the IS-LM approach. We have been since then mostly engaged in what Kuhn (1962) calls "normal science." To be sure, there are disagreements, but there is a core of good people working on interesting and important questions and making progress.

The bottom line is that macroeconomics has made impressive advances on a large number of fronts. Today, we have models built on first principles—that is, on constrained optimization and a consistent concept of equilibrium—of business cycles, unemployment, money and growth. It is still true that good economists often have difficulty with questions like, "What should we do at the discount window next week?" This may suggest the questions are ill-posed (although I do understand and have sympathy for the many professional economists who cannot ignore such questions because they get paid to come up with answers).

Perhaps I am too sanguine. How about our failures? One thing that Meltzer emphasizes that we are not so good at explaining is sticky prices. This may be because we sometimes take the pricing aspect of the Arrow-Debreu paradigm too seriously. We know that there are many ways to decentralize a given allocation. Contracts, core-like coalitions, reputation and several other institutions are also possibilities, as Meltzer mentions. It may be that agents get the allocation right without using prices in the way that our textbooks assume. That is, in principle, prices may "look" sticky but this need not have implications for welfare or policy.

When do sticky prices matter? Sticky prices can be studied in dynamic GE models, as Ohanian and Stockman (1994), Cho and Cooley (1994) and others have shown. These authors do not explain *why* prices are sticky; rather, they investigate the implications of varying degrees of exogenous stickiness. Should we try to explain stickiness endogenously? Maybe, but I was not convinced by Meltzer's current article.

I would like to conclude by saying that I have always learned from Professor Meltzer, especially on questions in monetary economics. It is worthwhile trying to take seriously his notions of information theory as a foundation for monetary theory. In other areas, he is also posing interesting questions. Standard macroeconomic GE models provide a venue for their analysis.

REFERENCES

Azariadis, Costas. *Intertemporal Macroeconomics.* Blackwell, 1993.

Burdett, Kenneth, and Randall Wright. "Unemployment Insurance and Short-Time Compensation: The Effects on Layoffs, Hours per Worker, and Wages," *Journal of Political Economy* (December 1989), pp. 1479-96.

Brunner, Karl, and Allan H. Meltzer. "The Uses of Money: Money in the Theory of an Exchange Economy," *The American Economic Review* (December 1971), pp. 784-805.

Cho, Jang-Ok, and Thomas F. Cooley. "The Business Cycle with Nominal Contracts," working paper (1994).

Cooley, Thomas F. *Frontiers of Business Cycle Research.* Princeton University Press, 1995.

Davis, Steven J., and John Haltiwanger. "Gross Job Creation and Destruction: Microeconomic Evidence and Macroeconomic Implications," National Bureau of Economic Research *Macroeconomics Annual* (1990).

Hansen, Gary D. "Indivisible Labor and the Business Cycle," *Journal of Monetary Economics* (November 1985), pp. 309-27.

Lucas, Robert E., Jr. "On the Mechanics of Economic Development," *Journal of Monetary Economics* (July 1988), pp. 3-42.

________. "Methods and Problems in Business Cycle Theory," *Journal of Money, Credit and Banking* (November 1980, part 2), pp. 696-715.

Kiyotaki, Nobuhiro, and Randall Wright. "On Money as a Medium of Exchange," *Journal of Political Economy* (August 1989), pp. 927-54.

Kuhn, Thomas S. *The Structure of Scientific Revolutions.* University of Chicago Press, 1962.

Kydland, Finn E., and Edward C. Prescott. "Time to Build and Aggregate Fluctuations," *Econometrica* (November 1982), pp. 1345-70.

McGrattan, Ellen R. "The Macroeconomic Effects of Distortionary Taxation," *Journal of Monetary Economics* (June 1994), pp. 573-601.

Mortensen, Dale T. "The Cyclical Behavior of Job and Worker Flows," *Journal of Economic Dynamics and Control* (November 1994), pp.1121-42.

________. "The Persistence and Indeterminacy of Unemployment in Search Equilibrium," *Scandinavian Journal of Economics* (June 1989), pp. 347-70.

Ohanian, Lee E., and Alan C. Stockman. "Short Run Independence of Monetary Policy under Pegged Exchange Rates," working paper (1994).

Pissarides, Christopher. *Equilibrium Unemployment Theory.* Blackwell, 1990.

Rios-Rull, Jose-Victor. "Models with Heterogeneous Agents," in Thomas F. Cooley (ed.) *Frontiers of Business Cycle Research.* Princeton University Press, 1995, pp. 98-125.

Romer, Paul M. "Increasing Returns and Long-Run Growth," *Journal of Political Economy* (October 1986), pp. 1002-37.

Rubinstein, Ariel, and Asher Wolinsky. "Middlemen," *Quarterly Journal of Economics* (August 1987), pp. 581-93.

Samuelson, Paul. "An Exact Consumption-Loan Model of Interest With or Without the Social Contrivance of Money," *Journal of Political Economy* (December 1958), pp. 467-82.

Stokey, Nancy L., Robert E. Lucas, Jr., and Edward C. Prescott. *Recursive Methods in Economic Dynamics.* Harvard University Press, 1989.

Trejos, Alberto, and Randall Wright. "On the State of Macroeconomics," *Journal of Political Economy* (No. 1, 1995), pp. 118-41.

Wallace, Neil. "The Overlapping Generations Model of Fiat Money," in John H. Kareken and Neil Wallace, eds. *Models of Monetary Economies.* Federal Reserve Bank of Minneapolis, 1980.

Williamson, Steve, and Randall Wright. "Barter and Monetary Exchange Under Private Information," *The American Economic Review* (March 1994), pp. 104-23.