Journal of Economic Perspectives—Volume 22, Number 1—Winter 2008—Pages 243–249

Comments

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The State of Macroeconomics

The first sentence of the article by V. V. Chari and Patrick Kehoe in the Fall 2006 issue ("Modern Macroeconomics in Practice: How Theory is Shaping Policy," pp. 3–28) reads: "Over the last three decades, macroeconomic theory and the practice of macroeconomics by economists have changed—for the better." I think that the last phrase is a little too self-congratulatory, and the last three decades have produced rather a mixed bag. But that is ultimately a matter of opinion. The second sentence then reads: "Macroeconomics is now firmly grounded in the principles of economic theory." I think this sentence is simply false, but this time as a matter of fact, not opinion. If I am right about the second sentence, the case for the first sentence partly evaporates.

The authors also claim that this new approach to macroeconomics has been responsible for a sea-change in the practice of monetary and fiscal policy. Another dose of skepticism would seem to be in order. The Deutsche Bundesbank did not need instruction on the virtues of an independent central bank, for instance. I do not intend to pursue this issue; I am content to associate myself with the doubts expressed by Gregory Mankiw in that same issue ("The Macroeconomist as Scientist and Engineer," pp. 29–46). My business is with the relation between "modern macro" and general economic principles.

When Chari and Kehoe speak of macroeconomics as being firmly grounded in economic theory, we know what they mean. They are not being idiosyncratic; they are speaking as able representatives of a school of macroeconomic thought that dominates many of the leading university departments and some of the best journals, not to mention the Federal Reserve Bank of Minneapolis. They mean a macroeconomics that is deduced from a model in which a single immortal consumer–worker–owner maximizes a perfectly conventional time-additive utility function over an infinite horizon, under perfect foresight or rational expectations, and in an institutional and technological environment that favors universal price-taking behavior. In effect, the industrial side of the economy carries out the representative consumer–worker–owner's wishes. It has been possible to incorporate some frictions and price rigidities with the usual consequences—and this is surely a good thing—but basically this is the Ramsey model transformed from a normative account of socially optimal growth into a positive story that is supposed to describe day-to-day behavior in a modern industrial capitalist economy. It is taken as an advantage that the same model applies in the short run, the long run, and every run with no awkward shifting of gears. And the whole thing is given the honorific label of "dynamic stochastic general equilibrium."

No one would be driven to accept this story because of its obvious "rightness." After all, a modern economy is populated by consumers, workers, pensioners, owners, managers, investors, entrepreneurs, bankers, and others, with different and sometimes conflicting desires, information, expectations, capacities, beliefs, and rules of behavior. Their interactions in markets and elsewhere are studied in other branches of economics; mechanisms based on those interactions have been plausibly implicated in macroeconomic fluctuations. To ignore all this *in principle* does not seem to qualify as mere abstraction—that is setting aside inessential details. It seems more like the arbitrary suppression of clues merely because they are inconvenient for cherished preconceptions. I have no objection to the assumption, at least as a first approximation, that individual agents optimize as best they can. That does not imply—or even suggest—that the whole economy acts like a single optimizer under the simplest possible constraints. So in what sense is this "dynamic stochastic general equilibrium" model firmly grounded in the principles of economic theory?

I do not want to be misunderstood. Friends have reminded me that much of the effort of "modern macro" goes into the incorporation of important deviations from the Panglossian assumptions that underlie the simplistic application of the Ramsey model to positive macroeconomics. Research focuses on the implications of wage and price stickiness, gaps and asymmetries of information, long-term contracts, imperfect competition, search, bargaining and other forms of strategic behavior, and so on. That is indeed so, and it is how progress is made.

But this diversity only intensifies my uncomfortable feeling that something is being put over on us, by ourselves. Why do so many of those research papers begin with a bow to the Ramsey model and cling to the basic outline? Every one of the deviations that I just mentioned was being studied by macroeconomists before the "modern" approach took over. That research was dismissed as "lacking microfoundations." My point is precisely that attaching a realistic or behavioral deviation to the Ramsey model does not confer microfoundational legitimacy on the combination. Quite the contrary: a story loses legitimacy and credibility when it is spliced to a simple, extreme, and on the face of it, irrelevant special case. This is the core of my objection: adding some realistic frictions does not make it any more plausible that an observed economy is acting out the desires of a single, consistent, forward-looking intelligence. The model still imposes a sort of orderly purposefulness that has never been shown to be there. One other thing: accidentally or not, folding an imperfection into the Ramsey model is likely to push the policy implications in the laissez-faire direction.

Here I have to insert a personal note, because Chari and Kehoe innocently implicate me in this line of thought by tracing it back (in their footnote 1) to the neoclassical growth model that I helped to develop. Indeed I have often described that model as a miniature general equilibrium. I will make three exculpatory observations. First, I restricted the applicability of the model to tranquil trajectories without stormy intervals. Second, I deliberately avoided recourse to the optimizing representative agent and instead used as building-blocks only aggregative relationships that are in principle observable. Third, I immediately warned the reader of the possibility of aggregative short- to medium-run supply–demand imbalances that would not fit into the model. I feel guilty about some things, but not about "modern macro."

Suppose you wanted to defend the use of the Ramsey model as the basis for a descriptive macroeconomics. What could you say? No doubt I lack enthusiasm for this exercise, but here is what I can think of. (I take it for granted that "realism" is not an eligible defense.)

You could claim that it is not possible to do better at this level of abstraction; that there is no other tractable way to meet the claims of economic theory. I think this claim is a delusion. We know from the Sonnenschein–Mantel–Debreu theorems that the only universal empirical aggregative implications of general equilibrium theory are that excess demand functions should be continuous and homogeneous of degree zero in prices, and should satisfy Walras' Law. Anyone is free to impose further restrictions on a macro model, but they have to be justified for their own sweet sake, not as being required by the principles of economic theory.

Many varieties of macro models can be constructed that satisfy those basic requirements without imposing anything as extreme and prejudicial as a representative agent in a favorable environment. Not only can be, but have been. Someone like James Tobin, for example, as I pointed out a few years ago, was typically careful that net demand functions for assets, as well as other building blocks, should have the necessary consistency properties (Solow, 2004). Beyond that he—or anyone—could argue for further restrictions on grounds of common sense, observation, or tradition, or mere curiosity.

It seems to me, therefore, that the claim that "modern macro" somehow has the special virtue of following the principles of economic theory is tendentious and misleading. The analogy that I like to use, and may have overused, is to someone who tells you that his diet consists of carrots and nothing but carrots; when you ask why, he replies grandly that it is because he is a vegetarian. But the principles of vegetarianism offer no support to so extreme a diet. The relevant definition only requires that the diet contain no meat. Carrots-only is at best mere idiosyncrasy and at worst a danger to health.

The other possible defense of modern macro is that, however special it may seem, it is justified empirically. This too strikes me as a delusion. In fact "modern macro" has been notable for paying very little rigorous attention to data. The usual procedure, as everyone knows, is first to "calibrate" the model—that is, to choose values for the parameters that are customary in other branches of economics or, for that matter, in earlier instances of this branch of economics. It is not at all clear that this is a good idea; it tends to close off potentially interesting possibilities. I suspect that the occasional claim that this procedure is free of data-mining may be illusory.

The typical "test" of the model, when thus calibrated, seems to be a very weak one. It asks whether simulations of the model with reasonable disturbances can reproduce a few of the low moments of observed time series: ratios of variances or correlation coefficients, for instance. I would not know how to assess the significance level associated with this kind of test. It seems offhand like a rather low hurdle. What strikes me as more important, however, is the likelihood that this kind of test has no power to speak of against reasonable alternatives. How are we to know that there are not scores of quite different macro models that could leap the same low hurdle or a higher one? That question verges on the rhetorical, I admit. But I am left with the feeling that there is nothing in the empirical performance of these models that could come close to overcoming a modest skepticism. And more certainly, there is nothing to justify reliance on them for serious policy analysis.

In the Winter 1996 issue of this journal, Lars Peter Hansen and James Heckman provide a readable and far more complete and knowledgeable critique than I could possibly manage of simple "calibration" as an empirical method for real business cycle models. It is entirely consistent with my view.

Naturally, some conscientious scholars within this tradition have been dissatisfied with calibration as a method. So they have quite rightly experimented with refined methods of statistical estimation of at least some key parameters, with generally nonrobust results. Likelihood functions are often flat. I do not know whether this merely reflects the poor fit of the model, or whether there may be something about the special theoretical framework that limits identifiability and precision. Either way, one's confidence in policy conclusions is not strengthened.

Mark Watson (1993) has suggested a carefully thought-out method for checking the empirical adequacy of real business cycle models. He also shows how poor an approximation a simple model of that kind gives to U.S. business cycles. I do not know if his methods have been applied to a real business cycle model with wage and price rigidities and other market imperfections. It would be a complicated exercise. And, if the empirical approximation were substantially improved, that would be at the expense of the pristine conclusions favored by Chari and Kehoe.

For completeness, I suppose it could also be true that the bow to the Ramsey model is like wearing the school colors or singing the Notre Dame fight song: a harmless way of providing some apparent intellectual unity, and maybe even a minimal commonality of approach. That seems hardly worthy of grown-ups, especially because there is always a danger that some of the in-group come to believe the slogans, and it distorts their work.

So I am left with a puzzle, or even a challenge. What accounts for the ability of "modern macro" to win hearts and minds among bright and enterprising academic economists? I have no easy answer. Probably these fashions have no single explanation, but depend on the random (or nonrandom) conjunction of favorable factors.

There has always been a purist streak in economics that wants everything to follow neatly from greed, rationality, and equilibrium, with no ifs, ands, or buts. Most of us have felt that tug. Here is a theory that gives you just that, and this time "everything" means everything: macro, not micro. The theory is neat, learnable, not terribly difficult, but just technical enough to feel like "science." Moreover it is practically guaranteed to give laissez-faire-type advice, which happens to fit nicely with the general turn to the political right that began in the 1970s and may or may not be coming to an end.

One can imagine how this style of macroeconomics would appeal to some economists with a certain sort of temperament, especially as they are following the example of excellent and charismatic protagonists. The relaxed approach to empirical validity may simply reflect what Melvin Reder once called "tight-prior economics" in describing an earlier Chicago School. Add some active proselytizing and heresy-hunting. Is that enough to account for the current state of macrotheory? I don't rightly know. But I do think it important that a few other, more eclectic, more data-sensitive approaches to macro-theory should remain in the profession's gene pool.

I tend to resist the suggestion that I ought now to propose some particular, better orientation for macroeconomics, because I know that I have my own prejudices. My general preference is for small, transparent, tailored models, often partial equilibrium, usually aimed at understanding some little piece of the (macro-)economic mechanism. I would also be for broadening the kinds of data that are eligible for use in estimation and testing. One of the advantages of this alternative style of research is that it should be easier to accommodate relevant empirical regularities derived from behavioral economics as they become established.

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■ *I would like to thank Francis Bator, Olivier Blanchard, James Heckman, and John Solow for very useful comments on an earlier draft. There is, of course, no implication that any of them agrees with my countercultural judgments.*

References

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In an otherwise useful article on the relationship of macroeconomic theory to policy, V. V. Chari and Patrick J. Kehoe ("Modern Macroeconomics in Practice: How Theory is Shaping Policy," Fall 2006, pp. 3–28) offer some conclusions on the nature of economic advice to policymakers that should not go unchallenged. Let us focus on two such statements:

1. "Those economists caught up in the frenzy of day-to-day policymaking often view their colleagues who toil in the ivory tower of academe as having no power to affect practical policy."

2. "[T]hose economists who whisper in the ears of presidents and Congress members [view themselves] as having the ability to affect policy dramatically."

The notion of "frenzy" bears more relation to episodes of *The West Wing* than to the manner in which economists in government actually operate. The typical members of the President's Council of Economic Advisers, for example, are professors of economics at major universities on leave for government service; many of them have contributed to the professional literature. The roster of past CEA members includes several presidents of the American Economic Association and a few Nobel laureates.

Economists in government have a special opportunity to transmit the relevant work of their academic colleagues to policymakers. Thus, in the 1980s, without being physically present, Milton Friedman was channeled by colleagues in the economics profession to become an important influence on macroeconomic policymaking at the highest levels of government. It is not clear why Chari and Kehoe want to downplay the role of the economists who are in a position to serve as a transmission belt for macroeconomic—and microeconomic—thinking. After all, economists in government are involved in sharpening the design of tax and budget policies, heading off protectionist trade measures, developing benefit–cost tests for evaluating proposed regulations, and even convincing skeptical politicians of the importance of an independent Federal Reserve system.

Rather than "whispering in the ears of presidents," government economists participate in the internal debates on economic policy—along with heads of major departments, White House staff, and other advisers to the president. Economic advisers quickly learn that their colleagues don't want lectures, but do expect them to draw on their professional expertise. As for "the ability to affect policy dramatically," we economists who have served in government can only wish it were so.

The notion of a dichotomy between academic economists and economists advising governmental decisionmakers is unrealistic and unhelpful. Those who have served as presidential economic advisers or testified before congressional committees are keenly aware of the great debt that they owe to those who have built the structure of economic analysis on which they regularly draw. Thus, the role of academic economists in policymaking is three-fold: 1) to contribute to improving the formal structure of economic analysis, 2) when the opportunity arises, to insert that analysis into the process of public policy making, and 3) to train future generations of economists who will do one or more of these three interrelated tasks.