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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 macro-

economic 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 macro-theory? 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.*

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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.

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Response from V. V. Chari and Patrick J. Kehoe

We welcome this opportunity to respond to the comments of Robert Solow on our 2006 *JEP* essay. Solow eloquently voices the commonly heard complaint that too much of modern macroeconomics starts with a model with a single type of agent. In our response, we clarify that modern macroeconomics does not end there—and may not end too far from where Solow prefers. Most of macroeconomic research over the last 20 years has precisely been about incorporating the heterogeneity and the rich interactions that Solow seems to think it needs. Solow also seems to think that essentially the only way that modern macroeconomists confront the data is through calibration. To the contrary, a key characteristic of modern macroeconomics is the heterogeneity in empirical strategies, including estimation, that are used to discipline the models using data. Finally, Solow questions our claim that modern macroeconomics is firmly grounded in economic theory. We disagree and explain why.

Before we elaborate on our assertions, we must acknowledge, with gratitude, that the way we build models and use data—what might be called the *style* of modern macroeconomics—owes much to Solow's seminal contributions to our profession. When he wrote down a single production function with aggregate labor and capital in his growth model, he sacrificed realism for an abstraction that has proven invaluable. In his growth accounting, he showed us how to use this abstraction in order to provide quantitative answers to economic questions.

In his comments on our essay, Solow provides a beautiful illustration of the struggle that engages academic macroeconomists every day. On the one hand, Solow says: "My general preference is for small, transparent, tailored models, often partial equilibrium, usually aimed at understanding some little piece of the (macro) economic mechanism." On the other hand, he also says:

> 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.

Clearly, it is impossible to have a small model which incorporates all the richness that Solow sees in a modern economy.

So model builders need to be selective, to try to capture in their models only what is essential in order to study the issue at hand. To do so, we design models to answer specific questions, not to reproduce the entire modern economy. Building a model to study a specific question requires first understanding the economic mechanism required to provide an answer—and that is easier to do, of course, when the mechanism and the model are simple. In this sense, we share Solow's preference for "small, transparent, tailored" models. However, answering the kinds of macroeconomic questions that we ask typically requires the use of general equilibrium models.

Solow seems to think that using that sort of model requires ignoring all the rich heterogeneity which he sees in the modern economy. While that may have been true many years ago, today it is not. Most macroeconomists today work hard to examine economic mechanisms based on the kinds of myriad interactions that Solow seems to have in mind, and they incorporate into their models whatever heterogeneity is needed to answer their particular questions.

We offer just a few recent examples. Ríos-Rull (1996) develops a life-cycle model with consumers, workers, and pensioners and uses it to ask questions about the quantitative sources of business cycle fluctuations. Krusell and Smith (1998), building on Aiyagari's (1994) important contribution, develop an incomplete markets model in which heterogeneous consumers have conflicting desires and use it to ask questions about business cycle fluctuations. Rogerson and Wallenius (forthcoming) develop a life-cycle model in which agents have different capacities for supplying labor and use it to ask questions

about tax rates and average employment rates across countries. Bernanke, Gertler, and Gilchrist (1999) and Cooley, Marimon, and Quadrini (2004) develop models with investors, entrepreneurs, and bankers who have conflicting desires and use these models to study the role of financial constraints over the business cycle.

Macro research has thus evolved in the direction Solow might recommend. Yet that does not rectify what seems to be his principal complaint, which has to do with the order in which we do things. Modern macroeconomists generally start with a model with a single type of agent and then enrich it with the details necessary to answer the question at hand. Solow prefers to start with a model with eight types of agents and then trim away the unnecessary details, in order to end up with a small model. To answer any particular question, though, does it really matter that we start with a single type of agent and boost it to three types while he starts with eight types of agents and cuts back to three? Analogies about school colors and carrots aside, there does not seem to be much of substance here to argue about.

Solow is also critical of how modern macroeconomists use data to construct models. Specifically, he seems to think that the only way our models encounter data is through calibration. Again, while this may have been true years ago, today it is not. Modern macroeconomic research today takes a wide variety of econometric approaches to confront both the micro aspects and the macro implications of general equilibrium models with data. These approaches include calibration, but also maximum likelihood estimation, Bayesian estimation, case studies, and natural experiments on both micro and aggregate data. We think this big-tent approach to data analysis serves macroeconomics well: it allows us to look for clues about the quantitative magnitudes of various mechanisms in a wide variety of sources using a wide variety of methods.

Solow also takes issue with the claim that modern macroeconomic models are firmly grounded in economic theory. What distinguishes modern macroeconomics is its method: building models at the level of individual households and firms and using these models to attempt to answer aggregate questions. Solow argues that any aggregate excess demand functions that are homogeneous of degree zero and satisfy Walras' Law are just as firmly grounded in economic theory as any modern macroeconomic model. This argument implies that building macroeconomic models from the ground up—that is, from the level of individual households and firms—has no special virtue over writing down systems of behavioral equations. Solow's argument is based on an appeal to the Sonnenschein–Mantel–Debreu result, which implies that if we have only aggregate data, then theory imposes little discipline on how we model aggregates. Fortunately for macroeconomics, the Sonnenschein–Mantel–Debreu result notwithstanding, discipline is available elsewhere. If we have microeconomic data on how individual households and firms behave, then theory imposes discipline on the behavior of aggregates over and above Walras' Law and zero-degree homogeneity.

The way macroeconomists use microeconomic data to discipline their models is still developing. Solow approvingly cites the work of Hansen and Heckman (1996), who suggest ways to improve the process of using micro evidence to build macro models. Interestingly, Hansen and Heckman argue that for this process to succeed, *microeconomists* must change the way they do business. Indeed, Hansen and Heckman (1996, pp. 100–101) contend:

> Much recent micro research is atheoretical in character and does not link up well with macro general equilibrium theory. . . . A redirection of micro empirical work toward providing input into well-defined general equilibrium models would move discussions of micro evidence beyond discussions of whether wage or price effects exist, to the intellectually more important questions of what the micro estimates mean and how they can be used to illuminate well-posed economic questions.

We agree with Hansen and Heckman's decade-old proposal. Their proposed redirection of micro empirical work is now well under way, and it will be useful once empirical microeconomics is as firmly grounded in the principles of economic theory as modern macroeconomics has been. For promising recent examples of this redirection, see the work of Lee and Wolpin (2006) and their references.

We don't mean to suggest that the challenges facing modern macroeconomics are small. Macroeconomists are still at the stage of figuring out which mechanisms are likely to be quantitatively promising for answering specific questions. Long before one formalizes a mechanism by writing down a detailed model and estimates it, it is desirable to use statistical procedures to determine if the mechanism is promising. Which procedure is the best for this purpose is the subject of heated debate. While the most popular procedure

currently is vector autoregressions, we prefer another—business cycle accounting—because it relies more on economic theory (see Chari, Kehoe, and McGrattan, 2007). Regardless of the specifics, because it is firmly grounded in economic theory, macroeconomics is poised to make major advances on these challenges.

Near the end of his comments, Solow wonders why bright and enterprising economists are attracted to modern macroeconomics. We think the answer is simple: the attractions of modern macroeconomics are similar to the attractions that led Robert Solow to develop the growth model and James Tobin to develop portfolio theory and Paul Samuelson to develop the overlapping generations model. These economists, like others before and since, were attracted to using what was then the frontier of economic theory in an attempt to shed light on the day's challenging macroeconomic questions.

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