

NBER WORKING PAPER SERIES

THE HETEROGENEOUS STATE
OF MODERN MACROECONOMICS:
A REPLY TO SOLOW

V. V. Chari
Patrick J. Kehoe

WORKING PAPER 13655
<http://www.nber.org/papers/w13655>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
November 2007

We thank the NSF for financial support and Kathy Rolfe and Joan Gieseke for excellent editorial assistance. The views expressed herein are those of the authors and not necessarily those of the Federal Reserve Bank of Minneapolis, the Federal Reserve System, or the National Bureau of Economic Research.

© 2007 by V. V. Chari and Patrick J. Kehoe. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

The Heterogeneous State of Modern Macroeconomics: A Reply to Solow
V. V. Chari and Patrick J. Kehoe
NBER Working Paper No. 13655
November 2007
JEL No. E12,E13,E2,E20,E21,E22,E32,E4,E5,E52

ABSTRACT

Robert Solow has criticized our 2006 Journal of Economic Perspectives essay describing "Modern Macroeconomics in Practice." Solow eloquently voices the commonly heard complaint that too much macroeconomic work today starts with a model with a single type of agent. We argue that modern macroeconomics may not end too far from where Solow prefers. He is also critical of how modern macroeconomists use data to construct models. Specifically, he seems to think that calibration is the only way that our models encounter data. To the contrary, we argue that modern macroeconomics uses a wide variety of empirical methods and that this big-tent approach has served macroeconomics well. Solow also questions our claim that modern macroeconomics is firmly grounded in economic theory. We disagree and explain why.

V. V. Chari
Department of Economics
1035 Heller Hall
University of Minnesota
Minneapolis, MN 55455
and NBER
chari@res.mpls.frb.fed.us

Patrick J. Kehoe
Research Department
Federal Reserve Bank of Minneapolis
90 Hennepin Avenue
Minneapolis, MN 55480-0291
and NBER
pkehoe@res.mpls.frb.fed.us

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 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 con-

tribution, develop an incomplete markets model in which heterogeneous consumers have conflicting desires and use that model 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 their model 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 do include calibration, but they also include 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 that

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, using statistical procedures to determine if the mechanism is promising is desirable. Which procedure is the best for this purpose is the subject of heated debate. While currently the most popular procedure is vector autoregressions, we prefer another, business cycle accounting, because it relies more on economic theory. (See our 2007 work with McGrattan.) 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.

References

- Aiyagari, S. Rao. 1994. “Uninsured Idiosyncratic Risk and Aggregate Saving.” *Quarterly Journal of Economics*. August, 109:3, pp. 659–84.
- Bernanke, Ben S., Mark Gertler, and Simon Gilchrist. 1999. “The Financial Accelerator in a Quantitative Business Cycle Framework.” In *Handbook of Macroeconomics*, edited by John B. Taylor and Michael Woodford, Vol. 1C, 134–93. Amsterdam: North-Holland.
- Chari, V. V., Patrick J. Kehoe, and Ellen McGrattan. 2007. “Business Cycle Accounting.” *Econometrica*, May, 75:3, pp. 781–836.
- Cooley, Thomas, Ramon Marimon, and Vincenzo Quadrini. 2004. “Aggregate Consequences of Limited Contract Enforceability.” *Journal of Political Economy*. August, 112:4, pp. 817–47.
- Hansen, Lars Peter and James J. Heckman. 1996. “The Empirical Foundations of Calibration.” *Journal of Economic Perspectives*. Winter, 10:1, pp. 87–104.
- Krusell, Per and Anthony A. Smith, Jr. 1998. “Income and Wealth Heterogeneity in the Macroeconomy.” *Journal of Political Economy*. October, 106:5, pp. 867–96.
- Lee, Donghoon and Kenneth I. Wolpin. 2006. “Intersectoral Labor Mobility and the Growth of the Service Sector.” *Econometrica*. January, 74:1, pp. 1–46.
- Ríos-Rull, José-Víctor. 1996. “Life-Cycle Economies and Aggregate Fluctuations.” *Review of Economic Studies*. July, 63:3, pp. 465–89.
- Rogerson, Richard and Johanna Wallenius. Forthcoming. “Micro and Macro Elasticities in a Life Cycle Model With Taxes.” *Journal of Economic Theory*.