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BOOK REVIEWS

Pedro Garcia Duarte and Gilberto Tadeu Lima, eds., *Microfoundations Reconsidered: The Relationship of Micro and Macroeconomics in Historical Perspective* (Cheltenham and Northampton: Edward Elgar, 2012), pp. 256, $125. Hard cover. ISBN 978-1-78100-409-8.

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This book contains papers originally presented at a conference held at the University of São Paulo, Brazil, in August 2009, with a follow-up session at the 2010 Allied Social Science Association meetings, chaired by John B. Davis, who wrote the foreword for the book, with Perry Mehrling as a general discussant, whose influence can be seen in some of the papers. The authors include some of the leading figures in the history and methodology of macroeconomics, mathematical economics, and econometrics. The main motive for these conferences and the book was the feeling after the crash of 2008 and subsequent recession that the models most widely used in macroeconomics that have emphasized certain forms of microfoundations have done a poor job in both predicting and analyzing what happened in 2008 and after, although little of the discussion in these papers focuses on those events. Rather, the search is for the historical roots of how these models came to be and to dominate the profession. Only one of the papers, the concluding one by one of the editors, Pedro Duarte, considers the most recently used models and the run-up to the current time. The rest cover either broad questions over long periods of time or very specific historical events thought to play an important role in the development of the microfoundations approach to macroeconomics.

My general view is that this book is very well done, with all the papers being of high quality, even if a few are a bit quirky with arguments that can be disputed. While all authors clearly are at least somewhat unhappy with the recently dominant dynamic stochastic general equilibrium (DSGE) approach, a variety of viewpoints are presented, and there is not complete agreement across the papers on some issues. The coverage is in-depth and serious, coming from several different angles methodologically and historically. All the essays are thoughtful and well informed, the net result, indeed, to make clear the foundations of the microfoundations-to-macro approach and its various forms and complications. If I have a single main criticism, it is that too little attention was paid to both more heterodox views in the past as well as possible alternatives for the future, although, arguably, this latter is not necessarily the main business of a book emphasizing history of thought. While some alternative heterodox approaches are occasionally mentioned, the argument for why they are mostly ignored is that the authors want to get at how the dominant models came to be developed, and this is not an unreasonable argument, especially given that a full coverage of all views would have substantially lengthened the book.

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An ongoing theme in the book is to cast doubt on widely believed "Whig histories," as Kevin Hoover puts it in the opening main essay, "Microfoundational Programs," after an introductory overview by the editors. He offers Robert Lucas as a key expounder of the Whig history. According to Lucas, during the Great Depression, John Maynard Keynes initiated an aggregate approach to statically modeling macroeconomies, which was further expanded by John Hicks and others in what would become the dominant "neoclassical synthesis" of the 1950s and 1960s. However, by the mid-1970s, wise economists such as Lucas developed new microfoundations based on intertemporal optimization by agents, who have rational expectations, and, eventually, Walrasian general equilibrium. The aggregation problem would be assumed away by having the economy behave as a single representative agent who exhibited all these microfounded characteristics, thereby overcoming the limitations of the earlier Keynesian models, with the new classical models imposing econometric restrictions across equations that would also help overcome the Lucas critique that earlier Keynesian aggregate models assumed that agents would not respond to policy changes by changing the behavioral parameters of estimated equations in the aggregate models. By the late 1990s, new classical and new Keynesian modelers would modify this into a new "mainstream narrative," as Olivier Blanchard would put it, or a "new neoclassical synthesis," as Marvin Goodfriend and Robert King would put it. This Whig narrative is the focus of most of these essays.

Hoover's surprise upending of this narrative is to argue that, in fact, both Keynes himself and leading Keynesian econometricians such as Lawrence Klein were very concerned with microfoundations and attempted to base their models on them as much as possible. They also attempted to provide microfounded arguments for consumption and investment behavior, even if these did not involve intertemporal optimization or rational expectations. For Keynes, the key to aggregation was the emergence of the macroeconomy out of the microfoundations. Klein attempted to break down such sectors as investment into many equations for different parts of the economy, an effort to get at microfoundations without fully being able to get to the level of the optimizing individual. However, for Hoover, both Hicks's emphasis on the average individual and the representative agent of the new classical models suffer by comparison from Sonnenschein–Mantel–Debreu aggregation problems that have also been analyzed by Alan Kirman, who showed that the representative agent may not represent anything. Considering the emphasis on econometric cross-equation restrictions of the early rational expectationists, Hoover declares (p. 49): "This strand of the new classical literature certainly paid no more attention—and, in fact, it would seem, rather less attention—to microfoundations than did the economists involved with the Brookings Model [Klein and his followers]."

The second main essay by Robert Leonard, "From Foundational Critique to Fictitious Players: The Curious Odyssey of Oskar Morgenstern," is arguably the one in the book that may stray the most from its main themes, although it does not do so entirely. As the title suggests, it focuses on Oskar Morgenstern, providing details of his history that I did not know, and showing how he moved from being a student of the Austrian School in Vienna (particularly of Hans Meyer and Ludwig von Mises) through directing the Austrian Institute for Business Cycle Research, where he had to manage economists arguing for a variety of approaches, including the Austrian, on to exile in America, where he would become fully enamored of mathematics and join with John von Neumann in developing game theory, and, in this most famous phase of his career, abandon his earlier ideas and concerns.

Where Morgenstern's tale becomes relevant to the proceedings is in his middle period, when he emphasized problems of expectations and anticipated the Lucas critique that efforts to model agent behavior would run into the problems of agents changing their expectations and behavior as circumstances and policies changed, thus making him a premonitor of more recent events and concerns. These doubts would move him away from the Austrian School, even if some of them shared such doubts, and set him on the path towards game theory, with its emphasis on agents interacting in terms of their expectations about each other's behavior. A crucial player in this period was the mathematician Karl Menger, son of the founder of the Austrian School, Carl Menger, who would feed his doubts that blossomed into a nearly full nihilism, along with initiating his interest in mathematics.

In the third main essay, by D. Wade Hands, "The Rise and Fall of Walrasian Microeconomics: The Keynesian Effect," Hands continues the effort of Hoover to turn standard stories on their heads. So, he recounts how it is standard that Hicks relied on Walrasian general equilibrium theory when he formulated ISLM, so that this influenced the neoclassical synthesis version of Keynesian macroeconomics. However, he argues that it was a two-way street; that in the 1930s, there were many competing versions of microeconomics within the broad neoclassical framework (with the old contrast between Marshallian and Walrasian approaches being just two of many). What made the Walrasian model the top dog out of this brawling compound was the influence of Keynesian analysis from the initial link made by Hicks. This influence would continue to be exhibited even as late as 1971 in Kenneth Arrow and Frank Hahn's *General Competitive Analysis,* where a large number of chapters were devoted to stability analysis and issues clearly derived from Keynesian concerns.

The key player was the main formulater of the neoclassical synthesis, Paul Samuelson, particularly in his 1947 *Foundations of Economic Analysis.* Whereas the first eight chapters are based on individual optimization, the later chapters stress market demand functions to consider stability analysis. This fits with a Walrasian-based macro analysis of dynamics based on his Correspondence Principle, also reminding that Walras himself was much concerned with stability and convergence in his various models of the tâtonnement process. The version of Walrasian equilibrium that supplanted the neoclassical synthesis would ignore these issues by simply assuming that equilibrium always holds, but then Hands argues that it was the old Keynesian concern about stability that would ultimately undermine this Walrasian foundation, even as he, like the other authors, does not suggest what will take its place.

Somewhat following the narrower focus of the Leonard chapter, the next one, by the ever-provocative Philip Mirowski, "The Cowles Commission as an Anti-Keynesian Stronghold 1943–54," arguably goes even more strongly against widely held views, given the seminal role of this institution in developing both theory and econometric methods during this important period, and the widely held view that it furthered the development of the neoclassical synthesis. While, indeed, Lawrence Klein did much of his initial work there that would lead later to the models discussed previously, he was pushed out of the commission by 1947. Most economists think that it was pro-Keynesian because so many were quite left wing; many were socialist European emigrés such as Jacob Marschak and Tjalling Koopmans, both of whom would direct

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the commission; and Klein was even a member of the US Communist Party at the time. However, Mirowski convincingly argues that many of these, particularly Jacob Marschak, were, if anything, anti-Keynesian, with Marschak arguably being the first to articulate the Lucas critique and in connection with Klein's models, and criticizing Keynes's work as early as 1940. The upshot was that there was much less solid support for Keynesian economics in the US at the time than many people have thought.

A part of Mirowski's argument that some readers may find overdone is the claim that Cold War political concerns played a crucial role at decisive moments in this drama. This would involve the expulsion of Klein from the commission in 1947, with the commission leaders beginning to seek RAND and other military funding for their research, with an eye to advising the US government on many matters. Advising the US government had always been an interest of the commission, but, with the Cold War emerging, the old anti-Bolshevik Menshevik Marschak would emphasize anti-communism with the expulsion of CPUSA (Communist Party USA) member Klein. Mirowski even cites unpublished papers by Klein from the immediately following years praising central planning that he wrote while at Ragnar Frisch's institute in Norway. While many readers may dispute his argument, Mirowski certainly provides some evidence for it, adding yet more complications to an already tangled tale.

I cannot let discussion of this chapter go without noting the at-times-fervent nature of Mirowski's argumentation. A good example is just prior to the conclusion of his essay where he declares the following (p. 160): "There is one Science, and it is our Science. There is but one God, its name is Walras, and Arrow-Debreu is his prophet. In this Passion Play, Keynes was just a minor Simeon Stylites. Journalists who proclaim 'we are all Keynesians now' are barking up the wrong pillar."

The next chapter by Michel De Vroey, "Microfoundations: A Decisive Dividing Line between Keynesian and New Classical Macroeconomics?" may be the least challenging of the Whig history of the essays. He asks whether Keynes can be viewed as a neoclassical economist and says the answer depends on how one defines the microfoundations. He distinguishes between what he calls "the Hayek–Patinkin conception" and the Lucasian one. The former simply requires that agents start out attempting to optimize, but, lacking rational expectations and not in a world where markets necessarily clear, they may not succeed in doing so *ex post*. In the Lucasian (and Finn Kydland and Edward Prescott) view, one is neoclassical and scientific only if optimization is accompanied by rational expectations and market clearing, "the equilibrium discipline." De Vroey argues that Keynes satisfies the Hayek–Patinkin condition because he assumes that firms are trying to maximize profits, but he does not satisfy the demands of Lucas.

The final chapter by one of the editors, Pedro Duarte, "Not Going Away? Microfoundations in the Making of a New Consensus in Macroeconomics?" is also the longest and covers the period from the emergence of new classical economics in the 1970s up until nearly the present, with the emergence of the new neoclassical synthesis in the late 1990s a central theme. He opens the chapter by citing Robert Hall's 1976 distinction between freshwater (Keynesian) and saltwater (classical) schools of thought. The Keynesians emphasize the demand side while the classicals emphasize the supply side, with the new classicals bringing in rational expectations and intertemporal optimization, and with the hardest line version of their approach associated with the real business cycle models (RBC) of Kydland and Prescott and such followers of theirs as V. V. Chari and Timothy Kehoe at the University of Minnesota. Eventually, Keynesians turn into New Keynesians by accepting rational expectations, but insisting on sticky prices and wages. The new neoclassical synthesis of the late 1990s, touted by Goodfriend and King along with Blanchard and Michael Woodford and others, puts these elements together, with supply-side elements more important for the long run and demand-side elements more important for the short run. A central element is the "Blanchard triangle" that sees models lying between apexes of Ramsey–Prescott optimization, Taylor nominal rigidities, and Akerlof–Yellen market imperfections. Hovering over all this as the great unifier is the DSGE approach that all future macroeconomics is supposed to use to answer all questions. However, the essay ends by quoting Gregory Mankiw to the extent that this is more of a truce between the two sides, with some holdouts still emphasizing their differences, with older Keynesian Robert Solow dismissing rational expectations in the short run, even as he likes the idea of his growth model providing the foundation for long-run analysis, while Chari and Kehoe insist that even shorter-run fluctuations are more supply-side- than demand-side-determined.

This is a compelling and mostly accurate story, although it has some loose ends, which may in turn reflect the loose ends that are very much present in the current situation of macroeconomics in the aftermath of the crash and recession that have so seriously undermined this consensus, developed with such great difficulty over such a long period of time. A curious moment of drama that happened clearly at the very last moment before this essay was completed involves a US Congressional hearing on macroeconomic modeling in 2010. Participants included Solow, David Colander, Chari, and several others. Essentially, this hearing turned into a DSGE-bashing one, with Chari playing defense. One point on which he differed from them and also from Duarte and others in this volume is on whether or not DSGE models depend on a representative agent. He claimed that they do not and have not for at least twenty years. Technically, he is correct on this point, briefly recognized by Duarte. However, the problem is that the way heterogeneous agents are allowed, aside from, for example, simply assuming the existence of two firms, is to allow for bounded continua of agents on some variable such as initial wealth. This approach, in effect, still operates like a representative agent model, only now this agent is a band rather than a point. There is no interaction between the agents as such. Given that so much is made of this issue in the whole book, it is surprising that there is so little mention of the rising efforts to use more explicit heterogeneous agent-based models for macroeconomic modeling that have become one of the main rivals to DSGE modeling.

One of the few places that Duarte or any of the authors actually spends more than a phrase on an "out of the triangle" approach is a brief and sympathetic discussion of the "corridor hypothesis" argument of Axel Leijonhufvud, dating originally from 1973, with Leijonhufvud clearly a critic of DSGE models. However, Duarte quite quickly moves on from this discussion. Leijonhufvud also receives some extended discussion and quotations in the opening overview chapter by Duarte and the other editor, Gilberto Lima, with his articulating the most clear dissent from the DSGE approach, particularly its frequent assumption of a representative agent.

However, aside from ignoring modern agent-based modeling, probably the most glaring lacuna in the entire book is the almost total absence of any discussion of the ideas of Hyman Minsky, pointed to by many economists as crucially important in the aftermath of the 2008 market crash. He does not appear in the index at all, and, as near as I can tell, his name appears exactly once in the entire book, as part of a list of seven figures who supposedly showed that Keynes's economics is incompatible with Walrasian general equilibrium, near the beginning of the essay by Mirowski. The other six are Robert Clower, Frank Hahn, John Hicks, Alan Kirman, Franklin Fisher, and Paul Davidson, but there is no further discussion of this group, Minsky in particular, who came at the end of the list. Perhaps it is viewed that Minsky was too far outside the triangle, not even worth the mention that Leijonhufvud gets, or perhaps too much a member of one of the officially established heterodox schools so carefully ignored, the Post Keynesians, although Friedrich Hayek and Ludwig von Mises received many mentions, even if there was not a full discussion of the Austrian school. I confess to being mystified by this curious lacuna involving Minsky.

I conclude by noting in Duarte's essay that one of the most prescient of established commentators he briefly mentions is the completely conventional Frederick Mishkin, author of the most widely used money and banking textbook in the US, who, in 2007, prior to the crash and while serving on the US Federal Reserve Board of Governors, offered the following list for improving monetary policymaking (p. 215): "(1) enrich estimated DSGE models so as to make them more realistic to the eyes of central bankers; (2) improve or extend the way nominal rigidities are usually incorporated in such models; (3) move from models with representative agents to ones with heterogeneity of agents; (4) incorporate (and better understand the role of) financial frictions; (5) go beyond rational expectations and embed behavioral economics into macroeconomics; (6) introduce learning into macro models; (7) keep a scent of art in monetary policymaking because economists 'can never be sure what is the right model of the economy.'"

Needless to say, all this is easier to suggest than to do, and the not-so-subtle implication of many of the authors in this book is that to do these things may well require moving beyond the DSGE models as microfounded as they have been up until this time, even though they fail to offer a clear path as to how that should be done. Nevertheless, their efforts are deeply informative and most worthy of serious attention and consideration in this well-edited and -written book.

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Alex J. Millmow, *The Power of Economic Ideas: The Origins of Keynesian Macroeconomic Management in Interwar Australia 1929–1939* (Canberra: Australian National University Press, 2010), pp. 310, $A28.00. ISBN 978-1-921-66626-1.

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This book is concerned with the role of Australian economists in the policy debates and in the formation of macroeconomic policy during the depression years and afterwards of the 1930s. Most of the focus is on the policy debate and response to the dire