J Evol Econ (1998) 8: 317–327

Journal of
Evolutionary
Economics

Book reviews

Hartley, James E.: The Representative Agent in Macroeconomics
London New York: Routledge, 1997, 229 pages, ISBN 0-415-14669-0

When Alfred Marshall wrote *Principles of Economics* his chief interest was directed to the fifth chapter, in which he laid out the *Theory of the Equilibrium of Demand and Supply*. In the course of the analysis, Marshall introduced the *representative producer* or *representative firm*, the intention of which was to simplify the argument by focusing on the *normal* case of producing and trading a commodity. This happened more than one hundred years ago. In the meantime, the representative firm has led an eventful life. It was debated wildly between the two world wars and was finally abandoned from competition theory. After the second world war, it was rediscovered by aggregation theorists who made use of the conception in the macroeconomic domain. Sometime after, the representative consumer, the representative firm's twin, was born, and since then the concept of the representative agent as we know it today was completed.

After a tranquil period of more than two decades, macroeconomists began setting up microfoundation models that relied crucially on representative reasoning. These models formed the core of the New Classical Macroeconomics, which established itself as a new branch of macroeconomic theory. The writings of followers of the representative agent approach gave rise to replies from those who criticized the method. This body of literature, which began to emerge in the 1970s, has grown rapidly in recent years. Readers who did not follow this debate now see themselves confronted with a multitude of publications that can hardly be assessed. An extensive survey of the representative agent literature is overdue, and therefore Hartley's book appears at just the right point in time.

Part I of the book is entitled *Why representative agents*? It presents Marshall's original ideas about the concept and the most important points of controversy in the 1920s between early followers and opponents, which ended up with the abandonment of the model of the representative firm. Hartley then moves on to the modern literature and describes the return of the representative agent in Lucas' and Rapping's 1970 new classical macroeconomic model. The modern uses of the representative agent are then examined in greater detail in the subsequent parts of the book. While Part II focuses on some specific aspects of *The Lucas Critique*, Part III then broadens the view by dealing with The *Walrasian Tradition* in economics in more general terms.

With regard to the overall topic, Parts II and III are intermediate steps aiming steadfastly at Part IV, *Microfoundations*, which is the center of the book, comprising nearly 50% of both chapters and pages. It starts by contrasting two polar views on the issue. For Austrian economists, representative agent models are in no way an improvement on traditional macromodels because aggregates and the relations between them are not reducible to individuals, which makes them economically meaningless (pp. 113–114). By contrast, new classical economists are not concerned about the general use of macroeconomics but require that it be grounded in individual optimization. For them, the word *microfoundation* is shorthand for a model in which the starting point is a utility- or profit-maximization problem (p. 130). The subsequent chapters cover various aspects of the microfoundations literature. Chapter 10 reviews work on the aggregation problem and finds that stable consistent aggregate equations can only be derived under very strict and unrealistic assumptions. The conceptual dissimilarities of microeconomic and macroeconomic problems are then examined in Chapter 11, which concludes that representative agent models neglect the fact that the exogeneity or endogeneity of a variable may be different on both levels.

Chapter 12 is the heart of this part (as well as of the whole book) and shows that the present stage of microeconomics is much more diverse than it appears to be. Hartley demonstrates that representative agent models are only one type among many microeconomic models that explicitly consider heterogeneity issues such as asymmetric information, strategic interaction or cooperative and non-cooperative games. Current practice of basing microfoundation models on simple utility maximization subject to a production function does not represent the state of the art of modern microeconomic theory (p. 169). In Hartley's words, this a "*degenerate, simplistic, and rather banal pseudo-microeconomics*", which should be replaced (p. 175). But as long as this is not the case, representative agent microfoundation models, "*are no more or less microfoundational than the old-fashioned Keynesian consumption function*" (p. 170).

The final result from this careful and detailed analysis is devastating: The representative agent has failed as an analytical device for the study of macroeconomic problems (p. 197). The only possible conclusion is then drawn in chapter 13. Microfoundation is a myth: it not only cannot be achieved by principle, but is also not desirable, because macroeconomic problems have a nature of their own that merits academic study on the aggregative level. In addition to the overwhelming evidence already collected in previous parts of the book, chapter 13 presents even more pressing arguments regarding the inconsistencies of new classical reasoning, the aggregation literature, the theory of the firm, the notion of the macroeconomy as a social system, and equilibrium theory, all of which provide convincing reasons for the usefulness of genuinely macroeconomic theories.

The follow-up question, *Whither Macroeconomics?*, is then pursued in the last pages of the book. Hartley identifies several characteristics of the "*new macroeconomics*" that emerge "*in the wake of the death of the representative agent models*" (p. 200). From Hartley's findings it follows directly that microfoundation models of the new classical style must be abandoned. This will be also true for the Walrasian general equilibrium models: the claim to comprehensiveness cannot be maintained. Instead, Hartley argues in favor of more limited analyses of smaller parts of the economy, which take explicit regard of the heterogeneities of the agents. This requires the resurgence of bounded rationality models that quite naturally provide no point of attachment for the super-human perfect-foresight representative agent, but instead provide reasons to address heterogeneity issues such as differential access to information (pp. 200–202). This will sharpen the focus on the role of relationships between individuals in general, and on the coordination failure problem in particular. What is required, in Hartley's eyes, is a return to macroeconomics as a genuine field of inquiry that comprises both monetarist as well as New-Keynesian and Post-Walrasian elements (pp. 202–204).

Except for one qualification, which will be made later, the book is a very solid work. The flow of thought is clearly structured. The voluminous body of literature covered is broken down into pieces of readable length, which is just what is required for readers who want to get started quickly with the topics at hand. Readability is further enhanced by concise language. The main points of analysis are nicely substantiated by literal quotations from the references. The use of mathematics is kept to a sympathetic minimum, serving mainly as illustration and for support of the substantive argument. This facilitates comprehension of the economic core of the argument, and readers looking for formal hard-core expositions are free to consult the original references. Many of the chapters can even be recommended as teaching materials for graduate and Ph.D. level courses. For example, chapter 2 on the old literature, and chapters 6 and 8 on the Austrian ideas and on the aggregation problem, can be used as introductory texts for reading courses or may be put on the work list for student seminar papers as additional references supplementing the studies of the original works.

What I enjoyed most were chapters 12 and 13. The main idea of chapter 12, which has already been presented above, was to show that there is no monolithic entity that can be called micro-theory. Thus, the virtue of chapter 12 is the demonstration that the idea of establishing a link between microeconomics and macroeconomics via a representative agent model is per se bound to fail. In chapter 13, Hartley applies the new classical way of reasoning in order to reveal that it is inherently faulty and incomplete even by its own standards. By consequently and mercilessly doing so he finds that a consistent application of the new classical microfoundations arguments compels us to reject much new classical work as "*insufficiently microfoundational*" (!) (p. 185). There are some brilliant parts in chapter 13 that not only provide important substantive rationales against the new classical method but that are also demonstrations of very fine argumentation. Both chapters are strongly recommended, and the substance is worth publishing in a separate journal articles to make the ideas known to a greater audience.

However, one critical remark has to be made. There is an important author whose theoretical achievements are not considered properly in Hartley's evaluation. This is Joseph Schumpeter, who is cited only once with a short comment he made about the representative firm in an obituary to Alfred Marshall (pp. 15–16). Schumpeter's own work on the matter is completely ignored as is other work from his modern followers who gave rise to the Schumpeter renaissance, and who launched the completely new branch of evolutionary economics. This line of inquiry, which examines the role of innovations in the dynamics of single markets as well as the whole economy, has quite a lot to say about the variety of agents and why the differences of their forms of competitive conduct cannot be ignor-ed. Hartley's disregard of the innovation topic is particularly annoying since evolutionary economics does offer another way of substantive economic theorizing that should be added to the list of prospective features of the new macroeconomics. That the whole book would be a theoretically predisposed inquiry was openly stated in the introduction, when Hartley announced that the discussion would be "*solidly neoclassical*" and that criticism and suggestions would be made "*from within the family*" (p. 4). While the frankness of this statement is honorable, this self-imposed restriction regrettably narrows the topical coverage of the book without being actually compelled to do so. Notwithstanding individual preferences, which everybody may legitimately have for one or another theoretical camp, a comprehensive presentation of all the literature available on the topic would have been strongly desirable. For readers who are aware of this limitation, the book is nonetheless a very good buy.

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Witold Kwasnicki: Knowledge, Innovation and Economy
Cheltenham, UK: Edward Elgar, 1996, 221 pages, ISBN 1-85898-349-5

This book presents an excellent evolutionary analysis of core constructs of industrial economics using a simulation model targeted primarily on pricing strategy and product competition. The long-term implications for firm and industry growth outcomes are explored, with an experimental focus on the decision horizon, industry concentration, capital intensity, economies of scale, demand growth patterns and transient returns to innovation. The author also provides several chapters devoted to a taxonomy of knowledge in the economics field and a description of the knowledge development process using the biological metaphor. Overall this is a solid contribution to the stream of work focussed on the complexity of industry growth processes. The book is comprised of eight chapters which extend and refine the author's previously published work, some of which is available here for the first time in English.

The first part of the book is devoted to an exposition on knowledge development processes in the fields of physics, biology and economics. The author provides an engaging assessment of the conceptual interplay between these sciences and draws out some enlightening history of economics along the way. While there are better treatises on the history of thought in economics, this review is accompanied by the testing of some new terminology and structure, such as the terms "paragon" (conceptual basis for action) and "episteme" (conceptual perspective). The most rewarding aspect of this discussion is in seeing the potential for a richer set of conceptual tools for epistemology applied to economics, thereby enriching our discussions of organizational behavior, firm-level decision rules and the innovation process. Readers will be encouraged to apply this taxonomy in