By David Colander

This is an impressive, thought provoking, and wide ranging book that ties together much of the philosophical thinking about complex systems. It is written by a physicist whose interests have turned to philosophy, and is intended to be a non-technical, yet detailed, book. It succeeds, and, in my view, is one of the best introductions to general thinking about complex systems that I have seen. It demonstrates an enormous breadth of knowledge, using examples from economics, evolutionary biology, and statistical physics to demonstrate the foundations of complex-systems theory. While the book is non-technical, it isn't easy going; it is dense, and often the reading requires careful attention.

The book does not present a new thesis as such; instead it tries to develop the philosophical foundations of complex-system theory as a separate field that transcends specific disciplines. The book is at its best in its discussions about physics; this is where the author's background is, and this is where she shines. She also does an admirable job in surveying the other disciplines and maintaining a neutrality among them. She emphasises that the ideas in complexity have developed in various fields; the rise of complexity science is not a case of physics spreading its tentacles to all areas.

The Message of the Book

Auyang starts out by pointing out that "according to the best experimentally confirmed physical theory, all known stable matter in the universe is made up of three kinds of elementary particles coupled via four kinds of fundamental interaction" (p. 1). Yet, somehow, out of simplicity we get the cosmos—the world, society, life, and consciousness. The questions she asks are: How can this be? and: How do scientists explain the complexity that exists in the world from such simple particles and interactions? Her answer to the first is that complex-systems theory—the study of iterative processes that underlie large composite systems—can help us understand how this can be. Her answer to the second is that they don't—science is not all reductionism. Instead it is a two part approach—one reductionist in nature, and one a study of large composite systems.

The contribution of this book is its explanation of 'many bodied theories' which are scientific theories that do not rely on reductionism. She uses examples from economics, physics, and evolutionary biology to develop her points. She explains how physics has a separate branch, solid state physics, that analyses how certain aspects of reality are understood without appeal to first principles. They appear, and are in some way connected to first principles, but the connection is too complex for us to understand or model. The complex systems have emerged and exist, but cannot be understood through reductionism. They can, however, be understood in a somewhat different fashion. Specifically, we can understand how such systems are organised, and that understanding of organisation gives us insight into the nature of such systems.

Auyang argues that the organisational structure of these complex systems transcends fields, which is why a new science of complexity is developing. Although theories of large composite systems lack the "sweeping generalities of theories in fundamental physics" she argues that they are nonetheless useful. They embody emergent properties, which are fundamental to our understanding, and follow from the mathematics of the probability calculus, or what I call iterative processes. In complex systems theory one talks about distributions, not about individual elements. The concept of a large complex system allows us to talk about steady states as if they were real states when in fact they are syntheses of infinite sequences.

The essence of complexity theory is the proposition that iterative processes have their own laws, and that understanding those laws will help us understand large composite systems. If it is true that large composite systems have their own general laws, then, as Auyang states, "the recent interdisciplinary exchange is only the tip of an iceberg". Beneath all complex systems theory, on an abstract level, there is an underlying science—the science of complexity. It is a science that provides insight into emergent properties and it underlies our understanding of all large complex systems. She admits that the treatment of structure formation in large composite systems with interacting constituents is tremendously difficult, but states that "it introduces a whole new ball game in science". Auyang develops these ideas by reviewing work in physics, using examples from the three fields already mentioned to demonstrate how such complexity work is done.

Economics as an Example of Complex-Systems Theory

My comments here will focus mainly on Auyang's discussion of economics; I am not qualified to discuss the other parts from any perspective other than that of an interested lay person. While I enjoyed the book immensely, I had some problem with Auyang's discussion of economics. My problem was that I was never quite clear whether she was using economics as an example of how to go about studying large composite systems, or as an example of how *not* to go about it. In different parts of the book she does both; she alternates between wanting to say that economics is an example of the many-bodied approach, and criticising economics for its failure to deal with the complexity of the economy. I am sympathetic: economics has many strains; it is itself complex and many-bodied, and it is often hard to tell what economists are up to.

From my viewpoint, however, the current approach is an acceptance of reductionism. Any work that is not, at its core, reductionist, is considered a type of engineering work—not at the core of economic theory, or heterodox work, not part of the conventional approach to economics. That is why the economics profession has not generally accepted the complex adaptive systems work that has gone on at Santa Fe. The integration of complex-system theory into economics is still in a process of emergence. It will happen, I believe, but it has not happened yet. As I said, I am sympathetic. We economists, especially top economists at places such as MIT from whom Auyang learned her insights into economics, often sound so reasonable when talking about our theories. We interpret them broadly and sympathetically; we combine aspects of theories that cannot be combined and then proceed as if they are integrated. Alfred Marshall did this when he made the supply/demand graphical framework the basic economic framework, while skirting over the issue of increasing returns that called into question the equilibrium concept that framework provides. Paul Samuelson did it when he developed macroeconomics as a separate field from microeconomics in his path-breaking text but did not deal with how macro required a different institutional structure than micro.

The reductionist nature of economics is primarily one of stated, or unstated but deeply held, methodology as opposed to actual working methodology. This means that the difference becomes clear only when one pushes a model to its logical extreme and is forced to choose between a representation that contains practical knowledge and one that is consistent with its reductionist methodology. Based on the history of economics, especially the developments in macroeconomics from the 1970s up until quite recently, reductionist models win out when economists are forced to choose. The understanding of the need for a many-bodied approach has not yet spread through the economics profession.

This is not to say that economists have not recognised the problem, and tried to deal with the economy's complexity. There have been a number of attempts in the history of economics to deal with complexity, but when it came to formalising the theory, there was no method of doing so, and the non-reductionist methods were dropped. Thus, for example, Adam Smith and John Stuart Mill had a complex world vision, but their work was superseded by that of Ricardo and Walras, which tried to formalise economics into a single model. Thereafter the complex world view showed its face at various times—Marshall touched on it; the German historical school pushed for it; the institutionalists grappled with it, and Hayek and Keynes toyed with it. But these efforts notwithstanding, when formalisation was at issue, the complexity worldview was abandoned and the reductionist worldview adopted.

Current standard economics is built upon that reductionist worldview and hence has always shied away from any full-fledged attempt to deal formally with the economy's complexity. Why? Schumpeter provided the reason when he discussed the possibility of multiple equilibria. He said that taking that possibility seriously would undermine economics as a science, and would lead to chaos. Conventional economics has followed Schumpeter's lead and, until recently, has avoided dealing seriously with multiple equilibria and other issues that arise out of complexity. Economists' dabbling with complexity, and their partial attempts to deal with it, presents a problem for Auyang in her discussion of macro-economics, which she gives as an example of economists' attempt at a many-bodied theory. It is true that, until the 1970s, macroeconomics was separated from microeconomics, and therefore seemed to fit Auyang's view that it was separate theory. But that was only a surface distinction that did not last when economists formally tried to deal with the connection between the two. Thus, in the late 1970s and thereafter, work in microfoundations, and what is called New Classical economics, pushed for a reconciliation of micro and macro using a reductionist mode, and the need to do so was generally accepted in the profession. Auyang criticises this New Classical reductionist work, but, from my reading of the history of macroeconomics, this was a natural outcome of the neo-Keynesian research agenda, and was a natural development of pushing the neo-Keynesian models to their logical limits. (Many older Keynesians would disagree with my assessment here, but the profession did not follow their view; it followed a reductionist mode.) To see why, let me briefly review the history of macroeconomics.

A (very) Brief History of Macroeconomics

Macroeconomics began in the 1930s because 'reality' was putting great strains on standard theory. That standard theory was questioned by a variety of economists: Keynes, Hayek, and Denis Robertson, among others. There was toying with multiple equilibria theory, and the belief that the economy was complex. Denis Robertson developed sequence equilibria analysis, and Hayek worked on ways of understanding how knowledge could be built into the system and when co-ordination problems could arise.

But those aspects of what might be called standard theory questioning were quickly dropped as the Keynesian Revolution swept the field. That Keynesian Revolution was a revolution on many fronts, but it quickly gave up its theoretical aspects when the neo-classical synthesis was accepted. The neo-classical synthesis was the vehicle within which it was reined in. Keynes, himself, was not part of the theoretical revolution; he moved on into intense policy discussions that took his mind away from theory. His followers carried on the revolution.

In textbooks, macroeconomics was presented with simple models, and macro became separated from micro, but the theoretical basis of that separation was never dealt with. Instead an accord that came to be known as the 'neo-classical synthesis' was reached. This was a synthesis that combined Classical and Keynesian economics into a single model, called the IS/LM model, with the distinction between the two views dependent on the shape of the 'LM curve'. (The distinction between the two changed over time, but IS/LM's unity of approach has not.) In that accord, standard Classical economics—built upon a reductionist worldview—was the general theory; Keynesian economics was a subset of that Classical theory built upon the assumption of fixed wages. The accord also gave something to Keynesian economics; while it was a subset of theory, it was an important subset since the frictions it assumed were relevant to the economy we lived in.

The accord began breaking down in the late 1970s as the inflation of the 1970s undermined the Keynesian policy of running expansionary policy in recessions and contractionary policy in inflations. In the 1970s we had both inflation and unemployment, and conventional Keynesian economics had no policy to deal with such situations. (Actually, it did—incomes policies, but these were politically unacceptable.) As the accord broke down Keynesian economics was replaced by New Classical economics in which economists demonstrated theoretically that, if one accepted continuous market clearing and rational expectations, Keynesian economics had no valid basis in theory. New Classical economics swept the profession, but caused what was called a New Keynesian response. This New Keynesian response, for the most part, was a reaction to New Classical work; it accepted its methodological requirements of rational expectations and market clearing, but, within that, attempted to build up explanations for frictions and sticky wages and prices.

Auyang's Discussion of the Neo-Classical Synthesis

I go through all this because it contrasts with the story of macroeconomics that Auyang gives. She states that the neo-classical synthesis was an example of what she calls "synthetic micro-analysis" in which the micro is interpreted contextually with the macro. Thus she writes that "the NeoClassical synthesis puts the micro- and macro-branches of economics under the same NeoClassical label but leaves open the gap between them. Each branch has its own style, concepts, and techniques" (p. 27). For a while it did, but the differences in style were in policy models, not in theoretical models. The neo-classical synthesis was an attempt to merge the micro and macro together without coming to grips with the theoretical issue of microfoundations; it implied micro-foundations existed but did not develop them. Micro-foundations were selectively added over the next decades along reductionist modes; no discussion of emergent properties was part of the macroeconomics debate about the model. Thus, the neo-classical synthesis was not a theoretical synthesis between micro and macro; instead it was a theoretical/policy synthesis in which Classical economics won out in theory, but Keynesian economics was considered relevant for policy. Keynesian economics was a special case of neoclassical theory that depended on fixed nominal wages and prices.

Even though the Keynesian economics of the IS/LM model might not be an example of economics' acceptance of a complex systems approach, the New Keynesian response to New Classical economics might be. Auyang argues that it is. She writes that New Keynesian economics follows a "synthetic microanalytic approach" (p. 206) which elsewhere (p. 114) she defines as requiring "the differentiation of individuals for the sake of studying large systems". The problem is that, while there are many dimensions of New Keynesian economics, most of its models are partial equilibrium models which it expands to the aggregate by representative agents assumptions. It assumes identical agents, an assumption that is the antithesis of the many-bodied approach. Very little of the New Keynesian work has focussed on 'emergent characters', for that work would require different methods and a different approach.

In my own writing I have differentiated types of New Keynesians, but have finally decided that the entire Keynesian/Classical division is no longer useful, and that a new classification is needed–Walrasian captures the standard view; Post Walrasian captures the complexity view. (See David Colander, ed. *Beyond Microfoundations: Post Walrasian Macroeconomics* Cambridge: 1996; this work builds upon the work of Robert Clower and Axel Leijonhufvud, but it certainly isn't standard within the macroeconomics profession.)

Policy Models

The difficulty of fitting macroeconomics into complex systems analysis is sufficiently important to warrant further discussion, because it goes to the heart of my problems with Auyang's discussion of economics. Auyang seems to assume that all models are designed to further understanding. She assumes that all models are intended to advance theory. My understanding of economics is different. There is a general overriding theoretical model that is used in explaining (in conventional economics, this is the Walrasian general equilibrium model) and there are large numbers of policy models that most economists work on. Little thought is given to relating these policy models to the overriding model; they are engineering models to be used in policy making, but not to be integrated into the core of theory. Macroeconomic models were of this kind and could be held simultaneously with a general reductionist theoretical model. But complex system analysis requires something more of policy models; it requires that they be integrated into the theoretical core of the discipline. That has not happened in macroeconomics and therefore I believe that economics does not provide an example of the complex systems theory approach.

At issue is whether these policy models are seen as working because of frictions, and thus are consistent with a single model, or whether they are seen as part of a many bodied theory, where they are acceptable models in their own right with no need to be combined with the grand theory. Economic theories that keep at their core a belief in general equilibrium theory, and are not seen as capturing a separate emergent reality, but only as capturing certain frictions, are not examples of many-bodied theories. When pushed, they will be dumped. This was what happened to the Keynesian Revolution.

Some Concluding Comments

Early on in any scientific endeavour one must make grand leaps of faith—which allow one to swallow assumptions unpalatable at a later time. Standard science, at least the type that underlies economic science, has made the "one perspective" assumption: the assumption that the reality we observe can be reduced to something that is understandable from one viewpoint. Thus the data we observe, and science is essentially data compression, is filtered through a single switch and interpreted. When reality doesn't fit our intuitive sense of that one perspective filter, we hedge. This is a very reasonable approach, but it fails even to suggest how science can come to grips with phenomena, such as life, a functioning economy, a functioning society, or the phase transition from a liquid to a solid. Such phenomena were simply placed outside the purview of science.

Complex-system theories changes all that; it gives up the one perspective assumption, and says that many phenomena we are looking at cannot be grasped from any one perspective, but may be understandable when viewed simultaneously from severable viewpoints. We must look simultaneously from the inside out; from the outside in, and from afar. On a superficial level, there is a certain intuitiveness of doing so, but it raises innumerable new questions such as how many perspectives are appropriate, and how to reintegrate the data stream in the decompression stage of the analysis. In economics we are only now beginning to touch on such issues.

I'm not sure whether complex-systems theory offers the appropriate path for economics. It is certainly one worth exploring. But it may well require a higher level of complexity analysis to deal with it than currently exists: 'complex complexity' rather than 'simple complexity'. The reason is that the actors in economics have their own consciousness—they act purposefully, so the subject matter of social science is a synthesis of complex systems of more complex systems. This higher level complex system requires one first to come to grips with consciousness and develop a theory of cognition—how individuals process information—before one can develop a solid theory of economics. The prelude to the introduction of complexity is only now being addressed. Those who have attempted to deal with it, such as Hayek, and those who are attempting to deal with it, such as Brian Arthur and Dierdre McCloskey, have been defined as part of the heterodoxy, and not part of the standard core of economics.

Department of Economics,
Middlebury College, Middlebury,
Vermont 05753, USA.

By Stephen H. Kellert

The conglomeration of scientific inquiries known as 'complexity theory' or 'complex systems theory' has received a great deal of attention in the past few years. Auyang's work is a welcome addition to the literature which seeks to clarify the conceptual foundations of such research, delimit its promises and shortcomings, and situate it in the context of other, more established fields. *Foundations of Complex-System Theories* has the advantage of dealing with complexity theory from a well-informed perspective that takes a broad view—encompassing economics, evolutionary biology, and statistical physics. It faces the considerable challenge of attempting to deal with both these well-established fields and newer, 'cutting edge' areas such as self-organised criticality and chaos theory.

In meeting this challenge, Auyang may leave philosophers of science frustrated by the brief treatment of such long-standing issues as supervenience, the species concept, reductionism and so on. As much as we might wish that these quandaries could be swiftly resolved on one page, few will be easily satisfied. With regard to reductionism, for instance, some of Auyang's criticisms make it sound as if micro-reductionists actually believe that the deduction of large-scale phenomena from the properties of constituents is a process that can and should be accomplished in practice. But is it fair to suggest (as on p. 58) that micro-reduction was ever recommended as a good recipe for how actually to go about developing a science? Auyang's alternative account of what she calls 'synthetic micro-

