

FOCUSING ON COMPLEXITY AND THOSE WHO STUDY IT

Complexity: The Emerging Science at the Edge of Order and Chaos

M. Mitchell Waldrop

*Simon and Schuster,
New York, 1992. 380 pp.*

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Reviewed by Daniel L. Stein

As most readers of this magazine's Reference Frame column are by now aware, there is growing support for a new science of "complexity," and the Santa Fe Institute in New Mexico is its standard-bearer. Readers are no doubt also aware that there is controversy over whether the subject exists as a science in its own right; many critics complain that they don't even know what complexity is. At a time when funding is becoming increasingly uncertain and hard choices about scientific priorities need to be made, the resulting debate is more than an academic exercise.

The guiding principle behind complexity research is one of the basic goals for modern science: to explain the rich array of observed natural phenomena with a few fundamental laws. These laws, however, are not necessarily those governing the elementary particles and their interactions, important as they are. The hope is to find the principles behind the spontaneous emergence of order, pattern and structure—not of the quasi-permanent, static variety, as in crystals or galaxies, but rather those of a more fluid, shifting variety, as in the patterns of weather, economies or life.

These are laudable goals; the individual subjects are certainly worthy of investigation. Why, then, the debate? At its heart, the main criticisms might go something like this:

The problems people choose to work on at a place like the Santa Fe Institute are often interesting and important—adaptive computation, the origin of life, turbulence, cellular automata, time-series forecasting and so on. But why is it necessary to try to force them under a single umbrella, with the claim that all are "complex systems" governed by some overarching set of laws and principles? Is the search for these laws appropriate at this time? If the practitioners of this science can't define what a complex system is to everyone's satisfaction (or even, in some cases, agree among themselves), then doesn't complexity become little more than a slogan, with its flagship institution merely peddling some brand of Science Lite?

These are valid questions, worthy of serious discussion. The publication of Mitchell Waldrop's popularized book *Complexity* performs a valuable service in giving the uninitiated reader a very good feeling for what the subject is about, primarily by focusing on the thinking of a few of its most prominent advocates. Although the issues raised above are spread throughout the book, the reader will glean enough insight and understanding to be able to make a more informed judgment.

Waldrop, a contributing correspondent for *Science* magazine, provides an atmosphere of unrestrained excitement about the tremendous potential and opportunities that the study of complexity affords. His book is unabashedly partisan. It focuses exclusively on the Santa Fe Institute; indeed, it is as much about the institute as about complexity. The casual reader unacquainted with either might come to believe the two synonymous.

Before proceeding, I must mention that I am formally affiliated with the Santa Fe Institute and so what appears here is undoubtedly colored by this association. Despite this I feel that a major flaw in the book is the almost complete neglect of relevant work done by those with no connection to the institute. Other institutes

and centers also devoted to complexity research go unmentioned, unless a Santa Fe Institute faculty member happens to reside at one. The only real exceptions to this omission are Waldrop's discussions of Los Alamos, which is chiefly of interest because of its umbilical cord to the Santa Fe Institute. The history and background of the science of complexity also receive short shrift; this is unfortunate, not only because it robs the discussion of some anchoring context, but also because the quest described by Waldrop is one of the oldest in science, with impressive foreshadowings by some of the great thinkers of earlier eras.

Waldrop wisely avoids a frontal attack on the problem of defining complexity. Instead, he takes many passes at the subject, looking at it through the eyes of different people, so that each time it acquires a new flavor and substance. While many personalities parade through the book, the lion's share of space is devoted to four people: Brian Arthur, Stuart Kauffman, John Holland and Chris Langton. The presentation of their interactions and the interplay of their thoughts are as stimulating as the discussions of their personal histories are absorbing. These men come from fields as diverse as economics, biology, computer science and physics, but they find that their ideas have some striking commonalities. Through their discussions with each other and with the author, the reader begins to get a sense of the kinds of problems that fall under the purview of complexity.

These discussions concern the behavior of systems composed of many particle-like units—atoms, neurons, economic agents—whose interaction is often based on only a few simple rules and leads to puzzling and unanticipated phenomena, poised delicately between rigid order and chaos. As the dynamics of these systems unfold, patterns emerge—spatial and temporal structures that flicker on, persist for a while and then dissolve, only to

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arise elsewhere. These structures are often capable of storing, transferring and using information. Complex systems do exist in the physical world—for example, turbulent fluids or spin glasses—but much of the research described in the book focuses on adaptive systems found in the living world—such as the immune system or the economy.

The time evolution of a complex system is often surprising and impossible to predict from its rules and initial conditions. As Holland points out, though, the understanding to be sought is not typical of conventional science, which strives for predictability in detail; the patterns of weather or economics, for example, never settle down or repeat themselves exactly. One goal then is to understand the *structures* that repeatedly emerge, their dynamics and how they interact. We can't, for example, predict exactly when a hurricane will arise, but we can be pretty sure we'll see a few each fall, and we have a reasonable idea about how they'll behave.

Throughout the sections of the book that attempt to present the fundamental characteristics of complex systems as understood by different individuals, Waldrop achieves remarkable success in conveying the important ideas in a very accessible style with little jargon and no equations. As writers of popular science know, this is no easy task.

Much of the rest of the narrative focuses on the origins of the institute, particularly the economics program; the efforts of George Cowan, Phil Anderson, David Pines, Murray Gell-Mann, Pete Carruthers and others to realize their vision of the institute, while simultaneously trying to agree on what that vision is; and the current thoughts of some of its most active researchers as to what the future holds. Of particular interest is the search for a new set of laws that will finally elevate complexity, adaptation and emergence to something like a new thermodynamics. Needless to say, the speculation becomes more rampant as the book proceeds; it will infuriate some physicists, inspire others and amuse the rest—but it makes for good reading.

Waldrop's book is clearly patterned in many ways after James Gleick's enormously successful book *Chaos*, and it is impossible to avoid comparison. Gleick's is the broader book, certainly more history-minded and perhaps more objectively written. Waldrop's book in some parts comes dangerously close to hero-worship. Gleick also had the tremendous advantage of writing about a subject

that had already become sharp and focused. Most physicists, at least, knew precisely what the word chaos meant before reading Gleick's book.

Which brings us back to the difficulty posed at the beginning of the review. The sense of this book, which I believe to be accurate, is that (at least at the Santa Fe Institute) there does exist a broad consensus on the nature of complexity and the kinds of problems that fall under its purview. This consensus may be somewhat vague and certainly falls short of detailed agreement, but otherwise life (and science) would be boring. In trying to construct a more precise conceptual framework, one must grapple with an old question: How necessary is it for a concept to be precisely defined before real scientific progress can be made? Although one can find examples that answer the question either way, there are cases in which important progress was made based on intuition, with precisely defined terms coming later. A widely used example is the progress in our understanding of heat and energy in the early 19th century, as Doyne Farmer pointed out to Waldrop.

Perhaps part of the problem is that the term "complex," to which some wish to ascribe a scientific meaning, has a clear connotation in everyday usage. Its intended meaning is obscured when taken to be synonymous with "more difficult than other problems." This connotation may lead some to perceive more than a hint of arrogance. (One of my colleagues, a high-energy physics experimentalist, has posted on his door a sign reading, "Institute for Simple Systems.") A second difficulty for some is the aggressive interdisciplinary nature of the work done at the Santa Fe Institute. My response is that such pursuits are part of the lifeblood of the Santa Fe Institute and probably the future of science. One of the most valuable lessons a scientist can draw from Waldrop's book is that the boundaries of our traditional disciplines are dictated as much by history as by the nature of the world we try to comprehend.

Theoretical Nuclear Physics: Nuclear Reactions

Herman Feshbach

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\$150.00 hc ISBN 0-471-05750-9

It is a pleasure to have a comprehensive book on nuclear reactions by one of the world's foremost experts on the

subject. *Theoretical Nuclear Physics* is useful for advanced graduate students and research physicists—especially in nuclear physics, but also in other fields of physics—because it includes descriptions of many-body reaction theory, multiple scattering, resonance theory and related topics. However, the book is not intended to cover scattering theory per se, as is done, for instance, in the classic book by Marvin L. Goldberger and Kenneth Watson, *Collision Theory* (Wiley, New York, 1964). In his book Herman Feshbach concentrates on techniques applicable to nuclear physics, particularly at energies below roughly 1 GeV. It is therefore a disaster that the book has been priced beyond the reach of most graduate students and that its cost will discourage physicists in fields outside nuclear physics from purchasing it.

The richly illustrated book follows up on and complements Amos de Shalit and Feshbach's earlier volume, *Theoretical Nuclear Physics: Nuclear Structure* (Wiley, New York, 1974; reprinted in paperback in 1990), and there are numerous references to the earlier text throughout the latest one. Indeed, the two volumes belong together, and the reader will find it much easier to digest the newer treatise if she or he has read the earlier one or has it available for reference.

The book summarizes the development of scattering theory as applied to nuclear physics over the last 40 years. It is clear that the author is in his element when he treats such topics as projection operator techniques and doorway states, to which he made major contributions. That is not to say that there are not excellent and thorough discussions of many other topics, such as analog states, antisymmetry in direct reactions, distorted-wave approximations and the formalism of transfer reactions (including those in heavy-ion collisions). The text includes a chapter on heavy-ion reactions and two sections on relativistic and ultrarelativistic collisions. It concludes with a chapter on pion and kaon scattering.

The author states in the introduction that it is "not possible to be complete or up to date." The emphasis is on classical nuclear physics and more so on topics with which the author is familiar through his own research or that of his colleagues at MIT. Thus, some topics, such as "rainbow" scattering, are omitted, and most references are from the period of the development of the theory (from roughly 1950 to 1980). Feshbach states that his goal is to give