Murray tells a more somber tale. He uses all three cases to illuminate what he terms the intrinsic incompatibility of the shuttle program with the needs of unmanned planetary exploration.

Murray is particularly effective, for example, in chronicling numerous shuttle problems that delayed the Galileo launch seven years, from 1982 to 1989. During that interval, he notes, NASA officials came to identify the Galileo craft (rather than the shuttle) as the "problem," and blamed the costs for redesigning Galileo to fit the altered shuttle and for reducing the funds available for planetary exploration. This perverse confusion of means with ends, hardly unknown in large bureaucratic organizations, is a theme that Murray explores effectively.

Historians will wish that Murray had explored with similar thoroughness the difficulties he faced administering JPL. Although JPL took on more and more military research under Murray's directorship as a means of securing its institutional survival, the opposition Caltech faculty gave to this reorientation is only partially portrayed. At one point Murray also defended the Galileo mission to members of Congress in terms of its inherent contributions to military technology, without commenting on the implications—indeed, the irony—of justifying such a program under the banner of defense. Yet one appreciates Murray's attempt to footnote important documents and sources, including historical articles he has consulted in preparing his arguments.

It is in the book's final section, however, that Murray advances his most controversial argument. Here Murray turns from chronicler to advocate: To reinvigorate scientific exploration of the solar system, Murray writes, NASA should commit itself to a new Apollo-style program aimed at placing humans on Mars in the first decades of the following century. This concept, which Murray developed with Sagan and other leaders of the Planetary Society and articulated publicly in 1986, calls for a joint US–USSR undertaking to Mars, an approach calculated to reduce costs and—considering Soviet expertise in the study of weightlessness and space biology—reduce the time required to prepare such a mission. Murray argues that such a venture, by providing a vision and focus to NASA more fruitful than that embodied in the shuttle and space station, would serve to return scientific exploration of the solar system to past levels of greatness. To put it another way, manned exploration has become the only game in town.

Murray's argument (written before recent events in eastern Europe and the end of the Cold War) has gained its share of adherents. President George Bush this May endorsed this plan by announcing that the US should commit itself to sending human expeditions to Mars by the year 2020 to support "a sustained program of manned exploration of the solar system and the permanent settlement of space."

Yet one wonders if Murray has fully absorbed the lessons his own memoir provides. Given his claim that a program of manned planetary exploration would strengthen *unmanned* planetary science, it would have been exceedingly helpful had Murray examined the history of lunar science within the Apollo program. In fairness, it must be noted that Murray had little personal involvement in lunar research during the Apollo period. Yet it is precisely this comparison that has greatest relevance to Murray's argument. Recent historical studies suggest that the science–Apollo interface was often tenuous. Many scientists complained bitterly in the late 1960s that lunar science was largely confined to meeting engineering goals, illustrated by the cancellation of the final seven Rangers and the last ten Surveyor missions once practical objectives were met. Of course, complaints are a normal part of a healthy, growing field, and as NASA scientists have frequently argued, far less lunar science would have been accomplished had Apollo never come about. Nevertheless, it is clear that the relationship between science and the Apollo program was deeply troubling to lunar scientists.

Also largely missing from Murray's account is the Hubble Space Telescope, the most expensive scientific instrument constructed to date. As Robert W. Smith points out in *The Space Telescope* (Cambridge U. P., New York, 1989), Hubble's mission was expanded in the 1970s to include solar system research in order to gain support for it from planetary scientists. Indeed, the Space Telescope gained a strong constituency of scientific supporters despite its unmanned design. Unfortunately Murray is silent about what implications Hubble's development may hold for future policy decisions for planetary science.

Even if the causes Murray ascribes to the "brief golden age" of planetary exploration do not all withstand later scrutiny, his narrative illuminates planetary science policy through its formative years. The societal pressures that cause Murray and other major figures of American science to champion manned flight as the only means to fund one of this century's most visible fields of science, although an unintended lesson, are no less worthy of our attention.

Exploring Complexity: An Introduction

Grégoire Nicolis and Ilya Prigogine
W. H. Freeman, New York, 1989. 313 pp. $24.95 hc
ISBN 0-7167-1859-6

Any regular reader of the magazine *Mosaic*, published by the National Science Foundation, could get the impression that nonlinearity and complexity are a major theme of NSF-supported research. Unfortunately there does not yet exist a Federal patron to sponsor this burgeoning and promising field of research. Many, including this reviewer, believe the newly forming spectrum of ideas loosely called "complexity" to be uniquely rich in its promise of deep insights that elude the standard reductionist methodology of physics. Having been midwife to the formation of the Los Alamos Center for Nonlinear Studies (1980), the Santa Fe Institute (1984) and the University of Arizona's Center for the Study of Complex Systems (1986), I am keenly aware of both the promise and the difficulties of the subject.

Recently the scribes of nonlinearity have produced a large number of books of all sorts on the subject. Since few agree on what the "subject" really is, it is not surprising that no single text captures more than a fraction of the intellectual menu. A glance at the proceedings of the first Santa Fe Complex Systems Summer School, *Lectures in the Sciences of Complexity*, edited by Daniel L. Stein (Addison–Wesley, Redwood City, Calif., 1989), gives an indication of the vast interdisciplinary territory being opened up. It has to attract attention therefore, when a major work appears from the "Brussels School" of Ilya Prigogine, in collaboration with one of his brilliant associates, Grégoire Nicolis. It is, after all, the work of this group that played a key role in founding important aspects of the subject now known as complexity.

Many physicists of my acquaintance disapprove of certain aspects of the style of the Brussels School. To them the vision seems too grand, and as in the case of this book, the equation-to-word ratio is somewhat low to keep the attention of the physicist. With regard to the controversial aspects, however, I believe that the real crime of Prigogine was to prove that deep physical concepts can be "self-organized" through the natural evolution of dissipative systems, an affront to the particle–force–equation mentality of the T-Shirt Physics School, which nowadays designs high-energy accelerator detectors to witness only Born approximations and Clebsch–Gordan coefficients. The subsequent rise of intuitions based on computation, fractal geometry and chaos—the latter based in large part on the work of the Russian and French mathematicians and in the US particularly Edward Lorenz, Benoit Mandelbrot and Mitchell Feigenbaum—have combined to produce a widespread perception that a major restructuring of scientific concepts and perhaps disciplines is now taking place. In the words of Thomas Kuhn, a "paradigm shift" is under way.

As stated in the prologue of the book, we live in an era of transition. It is not clear that the majority of physicists are aware of the profound conceptual revolution currently in progress that classical determinism is dead (or always was); that conceptual organizational principles exist generated *ex systema*; or that these notions may illuminate areas usually considered to live at a deeper level. We physicists must learn that the route from macroscopic to microscopic is just as profound as the reverse. If the lesson is not learned soon, the restructuring of science that is occurring will no longer have physics in a leading role.

Now to the details of the book under review. Attractively presented and clearly written, it presents a very ambitious table of contents. The more extended essays, such as the chapters "Complexity in Nature" and "Dynamical Systems and Complexity," are very successful. Yet quite a number of topics, such as cellular automata and deterministic chaos, are missing or are treated superficially.

For whom is this book intended? Much of it is too technical for the elusive "intelligent lay reader" and much of it too overexplained for the professionally trained reader. The general reader is probably better off with the fine book by James Gleick, *Chaos* (Viking, New York, 1987). Nor is the book suitable as a textbook. Having recently taught a first-year graduate course called Introduction to Complex Systems, I became keenly aware of the lack of a good primer on the subject, not to mention the difficulty in constructing such a book, even for a limited audience of physicists and mathematicians. In teaching such a course, one naturally becomes aware of the omissions. The book does not properly present the main research themes that form the center of attention of much current research: computational experimentation, fractal geometry and new concepts arising in nonlinear dynamics, such as adaptation.

Basic science, which traditionally has enjoyed a major input from physics, now stands at a crossroads. The new thrust is associated with the long-overdue shift of attention to the lessons of nonlinearity, thoughtful computation, complexity and fractal geometry, and especially the realization of common intellectual themes and their validity throughout much of science and beyond. This element of synthesis is the profoundly important message of the book under review.

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Magnetic Critical Scattering

Malcolm F. Collins
Oxford U. P., New York, 1989. 188 pp. $49.95 hc
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Phase separation in fluids and ordering in alloys and magnetic materials are examples of systems exhibiting critical phenomena that have been studied for over a century. Thus there exists a wealth of experimental data that have had to be explained and have served as a test for any theory of critical phenomena. Much of the data of the last 30 years has come from neutron scattering studies on magnetic materials. These materials are ideal for studying many-body effects since they have the simplest form of microscopic interactions, which are easily modeled mathematically. Of equal importance for the experimenter, real solids can be prepared with desired dimensionalities of the spin order and the magnetic interactions. The neutron interacts very efficiently with magnetic moments in solids, and fluctuations over lengths from one to several hundred angstroms can easily be studied. It is about time for an excellent review of magnetic critical phenomena as studied by neutron scattering.

Magnetic Critical Scattering by Malcolm Collins succeeds admirably in reviewing the field. His idea to restrict the book only to neutron studies of magnetic systems lends a good focus to the text. The author, a