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 Institue) 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
Wiley, New York, 1992. 959 pp. $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 sufficient background to "make the current literature and review articles accessible to the reader." He succeeds admirably in this endeavor. The downside is that there is very little material related to the substructure of the nucleons. The book has numerous references to and illustrations of experimental results. Problems do not appear at the end of each chapter, but rather are scattered throughout, and they are directly related to the development in the text.

Aside from the usual misprints—not excessive—I have few quibbles. A number of facts are presented without adequate explanation. An example is the weak spin-orbit force of the Λ–N interaction, which is mentioned early in the book; this spin-orbit splitting is barely referred to in the description of hypernuclei in the book's last chapter. Also, in the depiction of the double charge exchange of pions, the explanation of the behavior of the cross section at low energies is not given. There are also a very few statements that could be misconstrued, such as blaming isospin nonconservation on the electromagnetic interaction and not mentioning quark mass differences or QCD effects.

However, these are minor reservations. Overall this is an excellent treatment of the basic scattering theory required to understand nuclear physics experiments and their results.

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Space Sailing

Jerome L. Wright
Gordon and Breach, Philadelphia, 1992. 258 pp. $24.00 pb ISBN 2-88124-842-X

The pressure exerted by solar radiation on a perfectly reflecting perpendicular surface is about 9×10^{-6} N/m^2 at 1 AU. A sail made from Kapton film 2 microns thick with a 0.1-micron-thick aluminum coating would weigh about 3 g/m^2. The acceleration of the sail itself from light pressure would then be about 3 mm/sec^2. This is about half the acceleration due to the Sun's gravitational field.

Solar sailing was repeatedly invented in the pre-Sputnik years by, for example, Frederik Tsander, Russell Saunders and Richard Garwin. Among its potential applications are spiralling out from low Earth orbit to geosynchronous orbit in tens of days or to escape in about 100 days. Solar sails can increase the number of satellites that maintain a constant longitude, which could be important when geosynchronous orbits become crowded (as proposed by Robert L. Forward). Transporting freight and passengers between orbits by "sailing ships" could be much cheaper than using chemical rockets. Yet I know of no report of any attempt to deploy solar sails in space.

In the early 1980s Jerome Wright became interested in the possibility of using solar sailing to rendezvous with Halley's Comet during its 1986 passage. This proposal was enthusiastically received by the Jet Propulsion Laboratory. Wright led a group that developed solar sailing technology in considerable detail for this mission. The group settled on the heliogyro design, which looks like a huge helicopter, with 12 rotating blades 7340 meters long held out by centrifugal force. The total surface area of the blades was 0.625 km^2, and the overall weight was estimated to be 4 tonnes (about twice the weight of the simple film sails without the operations module or the payload). The group studied and dealt with the problems of deployment, dynamics and space environment conditions (radiation, micrometeorite damage and so on). In the end, none of these hazards did them in. Space shuttle cost overruns demanded their funds, and NASA cancelled the Halley rendezvous mission.

This book also reflects enthusiasm for even more imaginative applications of space sailing. Wright outlines K. Eric Drexler's design for fabricating by vapor deposition in space aluminum films that are two orders of magnitude lighter than the 2-micron Kapton films. He discusses Forward's designs for sailing to Alpha Centauri with the aid of massive solar-driven lasers.

It is sad that the space shuttle, which has disastrously increased the cost of going into orbit, still has political power enough to displace much imaginative space science and engineering. Wright's book has captured some of the charm and creativity that should be the guiding characteristic of our space program.

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Surface Science: An Introduction

John B. Hudson
Butterworth–Heinemann, Boston, 1992. 321 pp. $59.95 hc ISBN 0-7506-9159-X

At the 1976 March meeting of the American Physical Society, Robert Schrieffer remarked in an invited talk on surface physics that "if you stay in the field a while, it's a form of masochism to continue." While he soon moved to one-dimensional systems, the field of surface science has thrived, even garnering two entire categories for March meeting abstracts. Nonetheless, remarkably few books have appeared that are suitable introductions to the subject.

Many instructors who offer special-topics courses have adopted Andrew Zangwill's *Physics at Surfaces* (Cambridge U. P., New York, 1988). When Robert L. Park reviewed that text in *Science* (30 September 1988, page 1839) he lamented the short shrift given to many experimental techniques in the book's mere 450 pages. Hence, John Hudson's book—just over 300 pages long—can hardly be expected to cover everything. And, not surprisingly, Hudson, a distinguished experimenter, emphasizes topics on which he did research during his long tenure as a materials engineer at Rensselaer Polytechnic Institute. The book developed from a course Hudson taught variously over two decades to graduate and advanced undergraduate students in physics, chemistry and engineering.

In some sense, because problems appear at the end of each of its 17 chapters, this is the first real *textbook* on surface science. (The questions typically require the student to use the information and formulas to gain a quantitative feel for specific systems.) To hold down the book's price, Hudson himself produced the many figures not taken from other publications. He has put an impressive amount of time, thought and care into this volume.

The book is divided into four parts. The first and longest provides a general introduction and deals in depth with the thermodynamics of surfaces and surface mobility. The acknowledged "special debt" to John Blakely's "pioneering book," *Introduction to the Properties of Crystal Surfaces* (Pergamon, Oxford, UK, 1973) is most evident in this part. (Unfortunately, the book contains little on progress since the early 1970s in the statistical mechanics of surfaces. One can now interpret thermodynamic measurements in terms of microscopic interactions between atoms using powerful tools from statistical mechanics and computational physics.) The second part considers interactions between gases and surfaces, with particular emphasis on beam scattering and chemical rates. In the third section, on energetic-particle probes of surfaces, Hudson deals with the topics