BOOK REVIEWS

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A First Course in the Numerical Analysis of Differential Equations. Arieh Iserles. 378 pp. Cambridge U.P., New York, 1996. Price: $74.95 (cloth) ISBN 0-521-55376-8; $27.95 (paper) ISBN 0-521-55655-4. (Reviewed by John Guckenheimer.)

Aimed at advanced mathematics students, this book is a text whose goal is to introduce students to numerical analysis and computation of differential equations. The book avoids the traditional "definition—theorem—proof" style of most mathematics texts at its level, but it still requires significant mathematical sophistication on the part of the reader. Mathematical questions that arise in numerical analysis of differential equations are emphasized while not ignoring the practical aspects of putting the theory to work. The book is successful in carving out a niche that is distinctly different from other books with which it might be compared. Thus this is not just another mathematical introduction to differential equations, a "cookbook" of standard methods for solving differential equations, or an advanced treatise that gives a comprehensive set of alternative methods for solving a particular class of problems. Instead, the book starts from the vantage point of a typical advanced undergraduate or beginning graduate student interested in exploring scientific computation and presents material that introduces many of the issues that arise in the creation of effective algorithms to solve differential equations.

The book has three sections devoted to ordinary differential equations, the Poisson equation, and evolution equations. Each section has a somewhat different feel and stresses different aspects of the interaction between mathematics and computation. The first section introduces multistep and Runge–Kutta algorithms for solving initial value problems. It then devotes chapters to stiff systems and to error control for adaptive step-size algorithms. The section ends with a chapter that discusses iterative solutions of systems of equations by Newton's method and its variants. In comparing this section of Iserles' text with the classic text of Henrici, one finds here lots of material based on research during the intervening years. This reflects the strength of this book in bringing the experience gained from the past 50 years of scientific computing into a setting aimed at mathematics students. I applaud Iserles for lowering the barriers between "pure" and "applied" mathematics by blending this computing experience into the mathematics.

The second section of the book, on solutions of the Poisson equation, emphasizes numerical linear algebra. Following chapters that introduce boundary value problems for ordinary differential equations and the finite-element method for solving the Poisson equation, the section devotes three chapters to methods for solving sparse systems of linear equations and one to a discussion of fast Poisson solvers. The focus of this section is on computational complexity and efficiency. The section weaves a careful path that discusses discretization of the Poisson equation near irregular boundaries and variational techniques without explicit use of infinite dimensional function spaces (e.g., Sobolev norms are introduced for finite dimensional spaces) on its path to the sparse linear algebra that is the heart of the numerical methods for these problems.

The theme of the third section is stability of algorithms for solving initial value problems for evolution equations. There are only two chapters (though each is longer than the chapters of the first two sections), and the author conveys his feeling that the material he discusses is hardly a "finished" subject. There are excellent illustrations of the consequences of instability and the essential differences between solving initial value problems for ordinary and partial differential equations. The first chapter of the section treats parabolic equations, while the final chapter of the book is devoted to hyperbolic equations. The book ends with an all-too-brief discussion of Burger's equation and methods for computing solutions with shocks.

As a mathematician who developed an interest in numerical analysis in the middle of his professional career, I thoroughly enjoyed reading this text. I wish this book had been available when I first began to take a serious interest in computation. The author's style is comfortable, and I enjoyed the way in which he expresses his personality in much of the material. As much as I like the book, I suspect that it may have difficulty attracting a sizable following. Much of the growth in scientific computation is occurring in disciplines where there is less concern for the logical structure and mathematical analysis of numerical methods than is reflected in this book. These audiences are likely to prefer texts that emphasize practice and are less demanding of mathematical background of students and teachers. That would be unfortunate because the insight that comes from understanding the mathematics underlying algorithms for solving differential equations is an excellent foundation for scientific computation. One could easily argue that the material in this book is far more important today than much of that taught in courses that introduce mathematics majors to applied mathematics. This book would be my choice for a text to "modernize" such courses and bring them closer to current practice of applied mathematics.

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Karl Popper: Philosophy and Problems. Edited by Anthony O'Hear. 297 pp. Cambridge U.P., New York, 1996. Price: $22.95 (paper) ISBN 0-521-55815-8. (Reviewed by Sam Inglis.)

It is characteristic of Sir Karl Popper's unique position in academic history that essays about his work very often take a certain common form: His influence, his originality, and his great contribution to philosophy are acknowledged; it is then regretfully pointed out that his treatment of a given issue just will not do. Sympathetic philosophers go on to try to show

how Popper might extract himself from the holes they have excavated, while others take these problems as symptomatic of the general failure of Popper's project.

What is clear is that, despite its significance, Popper's work is seen by most contemporary philosophers as seriously or fatally flawed. Though his influence rivals that of Ludwig Wittgenstein or Bertrand Russell or Willard van Orman Quine, his ideas have not been taken up in philosophy in anything like the way theirs have. This is not to say, however, that they have not been taken up elsewhere. Popper intends his philosophy of science to consist both in a description of actual scientific practice, and a statement about the methods that scientists ought to adopt in their quest for truth. Among philosophers, there is widespread agreement that Popper's theory is false as a descriptive account and irrational or incoherent as a methodology. Among scientists, however, the popularity of Popper's theory vastly exceeds that of any other philosophy of science.

Prior to Popper, it had been widely assumed that scientific theories are both generated and confirmed by so-called "inductive reasoning." An inductive argument is any argument in which the premises do not deductively entail the conclusion. Most of us, for instance, would take it that seeing a thousand swans which are all white provides some evidence both for the hypothesis that all swans are white and for the prediction that the next observed swan will be white. However, no amount of prior observation deductively entails that the next swan I see *must* be white: It is an inductive rather than a deductive inference.

Why should seeing a thousand white swans make it any more likely at all that the next observed swan will be white, rather than green or gold or grey? Because, we assume, there is some degree of regularity in nature; after all, similar processes of inductive reasoning have very often yielded true conclusions in the past. As David Hume pointed out, however, this is circular: We are justifying our use of inductive reasoning by an inductive argument. An inductive argument can only provide us with a good reason to believe a hypothesis if we have good reasons to think that there is uniformity or regularity in nature; but our belief that nature will be uniform is itself an inductive conclusion from past uniformity. That there have been regularities in the past does not entail that there will be similar regularities in the future.

If the methods of science are partly or wholly inductive, Hume's argument seems to lead inexorably to radical skepticism about science. According to Hume, no inductive argument can provide any reason whatsoever for thinking that a proposition is true; and Popper, remarkably, accepts this seemingly devastating conclusion. The central claim which is unique to Popper's philosophy, however, is that science, in fact, can and does proceed without using inductive reasoning. Popper's theory rests on a so-called "asymmetry" between confirmation and falsification. No finite number of observations ever deductively entail the truth of a universal hypothesis such as "all swans are white;" nor, according to Popper and Hume, can they ever give us any reason at all to believe such a hypothesis. In contrast, every universal hypothesis logically entails an infinite number of observational consequences (so, for instance, "all swans are white" entails "the next swan I see will be white"); and if any one of these predictions is shown to be false, it *must* be the case that the hypothesis is false. No matter how many observations we make of white swans, we can never confirm the hypothesis that all swans are white *at all*, but a single observation of a black swan is enough conclusively to disprove it.

According to Popper, then, science proceeds not by reasoning from finite sets of observations to general hypotheses, but by a process of "conjecture and refutation." His key methodological claim is that scientists should try to *falsify* their theories—that is, they should try as hard as they can to show that their theories entail false observational consequences. If a theory fails a test it should be rejected; if it passes one test it must be tested again. However many tests a theory has passed, we can have no reason at all for thinking that it is true; but we are rational in preferring it to a theory which has failed one or more tests, because we know that such a theory must be false. By rejecting falsified hypotheses in favor of unfalsified hypotheses, we hope to end up with theories that are progressively closer to the truth, though scientists can never know if or when they have arrived at a wholly true theory.

Philosophers have attacked Popper's account on every front. It is suggested that the history of science does not fit very well with the model of conjectures and refutations. It is argued that Popper's methodology could not lead to convergence on the truth, as he thinks it does. It is maintained that, despite his declaration to the contrary, Popper's account depends implicitly on all sorts of inductive assumptions, and that it is therefore irrational by his own lights.

What are we to make of this situation? Science is, after all, something we take to be a hugely successful cultural enterprise, and many of its practitioners are avowed Popperians. Are philosophers then being arrogant in rejecting Popper's views, when those views seem to be contributing to this success? Or are scientists being obstinate by adhering to Popper's theory in the face of compelling philosophical objections? In his book *The Rationality of Science* (Routledge, London; 1981), W. H. Newton-Smith suggests that the plausibility of Popper's theory stems from its not being taken seriously enough. There is, according to Newton-Smith, a level on which Popper's philosophy of science seems very attractive, but a proper examination of his theory and its consequences reveals their problematic and highly counterintuitive nature.

If this is even a partially correct explanation of the seductiveness of Popper's theory, then those who are initially attracted to that account should probably be encouraged to study it more thoroughly, and a collection of philosophical articles about Popper ought to be helpful in this respect. *Karl Popper: Philosophy and Problems* claims to "offer···the specialist and the general reader alike fresh insights into the life and work of one of the 20th century's most original thinkers." Edited by Anthony O'Hear, who provides an admirably clear and concise introduction, it contains 15 papers about disparate aspects of Popper's philosophy, all originally delivered at the Royal Institute of Philosophy in London between October 1994 and March 1995. The book begins with four excellent papers about his general philosophy of science, including one by Newton-Smith; there is also discussion of Popper's equally controversial views on determinism, probability, and quantum mechanics, and of his political philosophy.

This broad sweep is both a good and a bad feature of the book. No single volume could hope to do justice to all the issues raised by Popper in his long career, or to cover all the criticisms which have been leveled against his work, and so the overall effect of *Karl Popper: Philosophy and Problems* is rather diffuse. Many of the articles are, in themselves, interesting and persuasive; but the book as a whole does not provide (nor does it seek to provide) a balanced or comprehensive analysis of Popperian philosophy. Philosophers seeking new insights into the idea of methodology, theories of history, the concept of a scientific revolution, or the relation between Popper's philosophy and contemporary theories of knowledge will find plenty here to keep them occupied. Some of the contributors offer detailed analysis of particular problems in Popper, while others use his general philosophy as a jumping-off point for the development of their own ideas; and the fruits of both approaches will be of considerable value to those working in the field. However, the "general reader" seeking a broader understanding of Popper's theories, and of the reasons why philosophers have been hostile to them, will get less out of it.

Newton-Smith is, I think, right to argue that many followers of Popper do not take his theory seriously enough. Popper's writing is undoubtedly engaging—*Conjectures and Refutations* (Routledge, London, 1963) and *Objective Knowledge* (Clarendon, Oxford, 1972) both contain good statements of his central arguments—but it is surely wrong to endorse any theory without giving at least some thought to its consequences and to the criticisms offered by other philosophers. In this respect one would certainly profit by reading *Karl Popper: Philosophy and Problems*, but I think that there are other works which provide a more coherent view. Two books which seem to me to provide such a view are Newton-Smith's *The Rationality of Science* and Anthony O'Hear's own volume on Popper in the Arguments of the Philosophers series (Routledge, London, 1980).

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The Quark and the Jaguar: Adventures in the Simple and the Complex. Murray Gell-Mann. 392 pp. W. H. Freeman, New York, 1995. Price: $23.95 (cloth) ISBN 0-7167-2581-9; $15.95 (paper) ISBN 0-7167-2725-0. (Reviewed by David Park.)

If one of the criteria that distinguish a good book is that it helps you to change the way you think, then Murray Gell-Mann has written a good book. It consists of four long essays whose common theme, though they sometimes travel rather far from it, is announced in the Prologue. Here we are introduced to "complex adaptive systems," examples of which would be the reader or some other animal or a community of interdependent animals or a child learning to talk. It seems to be a fact that the physical laws which govern quarks, of which all members of any species are exactly alike, are also responsible for the individuality, the capacity to adapt and learn, of a complex adaptive system, and in Part I, dealing with concepts of complexity, randomness, and organization, the author begins to study how this fact can be understood.

Part II, the Quantum Universe, is concerned with superstrings and the standard model and also presents a contemporary view of quantum mechanics. I will enlarge on this a little. The section starts out with the conviction that the proper business of quantum mechanics is to tell how things actually happen, and not merely how they appear to a particular observer, and one, moreover, who has the patience to endlessly repeat the same experiment. The view that is being developed by Gell-Mann and James Hartle, along with Roland Omnès, Robert Griffiths, and several others, is based on the idea of a quantum history as represented by a path integral. It is assumed that we understand what probability is and that events in the world of our experience have probabilities associated with them. Probabilities are additive, but where histories interfere one must add amplitudes instead, and therefore one cannot assign probabilities to them. In the famous two-slit experiment (which physicists often claim not to understand) it is impossible to assign a probability to a history that starts at the source, passes through slit A, and reaches the screen at point P. But entangle that history with that of some system that interacts with it, for example an efficient counter, and sum over microscopic states corresponding to the counter's indistinguishable macroscopic states. Now probabilities can be assigned, but of course the interference pattern has disappeared. This illustrates one of the ways in which, by coarse-graining, a history can be defined roughly enough so that its interference with other histories is as small as desired and it acquires a probability. And thus, without introducing observers or classically described laboratory apparatus or "letting $\hbar$ approach zero," we can recover our ordinary experience of the world, embodied in classical physics, as a limit of quantum physics.

Coarse-grained histories are affected, sometimes in important ways, by fluctuations; a fluctuation can produce branches in a history (each with its appropriate probability) that persist after cross-graining and may even affect things on the scale of our experience. A coarse-grained history tends to have the form of a tree, and now at last comes a job for that "information gathering and utilizing system" that the Old Masters called an observer. Armed with a hatchet, it prunes the tree of histories, removing branches that might have happened but did not, a procedure distantly analogous to the collapse of the wave packet but here a matter of mental organization that is in no way a physical process.

Part III is called Selection and Fitness, and it starts with the idea of a coarse-grained history in which there has been a fluctuation large enough to produce a departure from classical behavior. Gell-Mann calls such departures "frozen accidents," and it is with this concept (or metaphor?) that he turns to a discussion of biological evolution. The general direction of evolution is governed by fitness, and he vividly illustrates the process with the concept of a fitness landscape, here imagined as a two-dimensional display of possible characteristics in which some measure of fitness, pointless to specify clearly, is plotted so that fitness increases with depth; a specially fit collection of attributes here corresponds to a local minimum, what is called a "basin of attraction" in dynamics. It is easy to imagine an organism evolving through a series of frozen accidents so as to sit in the minimum, but what then happens to the evolutionary process? And what of the unlucky organism that settles into a shallow basin when there is a deeper one nearby? Considered as a complex adaptive system, a species functions best in the presence of noise, of accidents that tend to keep it from settling down. Coelacanth and cockroach must each survey the world from a deep and quiet basin.

Part IV, Diversity and Sustainability, considers the future of animal species and human cultures and the earthly environment. Each is a complex adaptive system, and Gell-Mann