reader could ever hope to have or to use. A number of important applications are given after the exposition of the theory. These include a study of permutation groups of degree p and linear groups of degree at most p, a study in which the cyclic theory plays a prominent part; an analogue in characteristic p due to Brauer and Feit of Jordan's theorem on the existence of normal abelian subgroups of bounded index; and the Glauberman Z^*-theorem on the embedding of involutions in groups.

The writing is spare yet elegant, slanted towards the specialist, yet self-contained. Readers familiar with Feit's *Characters of finite groups* will know the style. Misprints do occur at awkward places in the text, and the lack of punctuation may distract at first. But these are minor matters and will not deter the reader from the best account of the general theory.

PAUL FONG

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Nonlinear oscillations, dynamical systems, and bifurcations of vector fields, by John Guckenheimer and Philip Holmes, Applied Mathematical Sciences, Vol. 42, Springer-Verlag, New York, 1983, xvi + 453 pp., $32.00. ISBN 0-3879-0819-6

Smooth dynamical systems, by M. C. Irwin, Academic Press Inc., London, 1980, x + 260 pp., $48.50. ISBN 0-1237-4450-4

Geometric theory of dynamical systems, an introduction, by Jacob Palis, Jr. and Welington de Melo, Springer-Verlag, New York, 1982, xii + 198 pp., $ 28.00. ISBN 0-3879-0668-1

In 1963 E. N. Lorenz wrote an article [L] discussing a meteorological model given by the system of differential equations:

$$\frac{dx}{dt} = -10x + 10y, \quad \frac{dy}{dt} = 28x - y - xz, \quad \frac{dz}{d} = -\frac{8}{3}z + xy.$$

(The constants are actually values chosen by Lorenz of certain parameters.)

It is a remarkable fact that fifty years ago it would have been impossible to make more than a superficial analysis of the behavior of the solutions of this system, while today there is an extensive literature concerning it (including numerous articles and at least one book [S]). It is also the case that no standard text on ordinary differential equations contains anything of particular value in understanding this system. In contrast, the three books under review are of considerable value, in the sense that they would go a long way toward preparing the reader to read the literature. And, in fact, the book by Guckenheimer and Holmes discusses precisely this system in considerable detail.

The reason why much more is known today concerning this system, the so-called Lorenz attractor, than would have been possible to know fifty years ago is, of course, the existence of digital computers. However, surprisingly enough the authors of the best mathematical articles concerning this system of differential equations made very little use of computers themselves, and their articles made no use of them whatsoever. In fact this body of mathematical work represents one of the best—and least controversial[1]—uses of computers in mathematics.

The history of this example illustrates several changes which the rise of computers is having on the field of dynamical systems. First, in the numerous disciplines which model natural phenomena with ordinary differential equations, researchers are no longer limited to using differential equations which are solvable. Numerical solution of equations can be used when explicit solution is impossible. This represents a major change in the way mathematics is used, one whose full implications are still impossible to gauge. It is probably safe to say that the effects on applied mathematics will, in the long run, be quite dramatic.

Of course when numerically obtained "solutions" of a system of differential equations are extremely complicated (chaotic is the current buzz word), it is difficult to know what they mean or even if they are very trustworthy. For example, when there is an apparent randomness to the solution, there is considerable concern that the observed behavior is merely an artifact of the computer or computation scheme. One way to check this is to use several computers and/or numerical methods and compare the results. However, in a chaotic system, such as the Lorenz equations above, one typically discovers the following surprising fact. An attempt to use two computation methods to find one particular solution with one set of initial conditions results in two "solutions" which have almost nothing to do with each other except for times quite close to the initial value time. Nevertheless, the overall picture, obtained for example by graphically plotting a number of solutions, is quite distinct and seems completely independent of the numerical methods used, accuracy of the computer, roundoff scheme, or almost anything else except the differential equation itself!

Under these circumstances deeper understanding must occur in the realm of pure mathematics rather than that of numerical computation. In the case of the Lorenz equations this understanding was achieved by constructing what is called a geometric model which can be completely understood in a rigorous mathematical sense. What this means is that from numerical studies of the

[1]There is now a whole spectrum of "experimental mathematics" which poses some very serious questions for mathematicians. For example, how do we react to a researcher who establishes a "mathematical truth" without giving a proof or even making use of deductive logic, but instead uses a good experimental methodology, a methodology which is quite acceptable, say, to physicists? This is a profound question for mathematicians which seems to have been given very little attention. The impact of computers on mathematics seems much more likely to come in the form of a mathematical empiricism than through computer aided proof.

system certain quite plausible "geometric assumptions" about it were formulated. Then a rigorous mathematical analysis [**G-W, W**] was carried out for all systems satisfying these assumptions. The result is a complete topological description of the geometric model differential equations, i.e. any differential equations which satisfy the geometric assumptions. The result is too complicated to describe here, but it seems quite satisfying as an explanation of the numerically observed chaotic behavior of the true Lorenz equations above.

What is still missing, of course, is a proof that these equations satisfy the geometric assumptions. The reason this has not been proven, I suspect, is twofold. First such a proof, done by hand, would be extremely laborious computationally (although Guckenheimer and Holmes suggest that "in principal [the assumptions] can be verified by numerical methods"). Secondly, it seems likely that such a proof would be rather unenlightening, and as a result no one seems sufficiently highly motivated to make the effort. This could be incorrect, of course, and the result could have an exciting proof or be false. The idea of understanding a complicated dynamical system from certain geometric hypotheses, however, is exactly what modern dynamical systems and the three books under review are all about. The idea of a geometric model provides an interface between the purely mathematical discipline and the application of mathematics. This seems to have been a fruitful concept and one which we can expect to see more frequently in the future.

A second lesson one can learn from the Lorenz equations is that except for linear equations there is little relation between the simplicity of the algebraic form of a differential equation and the simplicity of qualitative behavior of its solutions. These equations have only the simplest nonlinearities, but the solutions are extremely complicated. (I heartily recommend that anyone so inclined try plotting the numerical solutions of this system with a microcomputer to get a sense of what is meant by the term "strange attractor.") The simplest quadratic differential equation or function can have extremely complicated "chaotic" dynamics.

For this reason it makes more sense to have the taxonomy for ordinary differential equations based on complexity of the dynamics rather than the more traditional scheme based on the algebraic form of the right-hand side of the equation (linear with constant coefficients, linear with variable coefficients etc.). The traditional approach makes sense if there is a technique for explicitly solving an equation and this technique can be recognized from the algebraic form of the equation. Sadly a remarkably small number of nonlinear differential equations fall into this category.

An alternative approach which seems to have been more or less adopted by all three of the books under review is to classify ordinary differential equations (and other dynamical systems) by the qualitative complexity of their dynamics. A qualitative description of a dynamical system can be many different things, but should include, at least, a description of the long run behavior of trajectories, i.e, what will happen to all (or most) initial conditions if we wait long enough. Thus a system displaying the simplest behavior from this point of view might be one in which all solutions converge to some fixed stationary point as time tends to infinity.

Of course, the typical nonlinear ODE will have a more complicated qualitative behavior. The point is that a geometric taxonomy for dynamical systems might be the most enlightening in the world of nonlinear systems which cannot be explicitly solved. Indeed, if we could somehow magically obtain a closed form solution of the Lorenz equations for all initial conditions this would almost surely be completely unenlightening and it would still be necessary to make the same geometric analysis in order to understand the solutions. The idea of a qualitative geometric analysis is not new; it was certainly espoused by Poincaré. It has been given new currency, however, by the desire to understand easily obtained, but often very complicated, numerical "solutions" to differential equations. One can even envision modelling by finding appropriate "geometric assumptions" which are satisfied by the natural phenomena under investigation and studying the class of dynamical systems which satisfy these assumptions. In other words one might sometimes skip the step of algebraically formulating a model.

This review has emphasized the interplay between dynamical systems and applied mathematics because this interplay is currently having a strong and generally beneficial influence on dynamical systems. It provides new directions for investigation and new motivation. However, we should not lose sight of the fact that dynamical systems is first and foremost a branch of pure mathematics and has the same goals and motivations as less applicable branches of mathematics. This seems to be the approach taken in the books by de Melo and Palis and by Irwin. Neither of these works deals to any great extent with applications, though both give numerous examples. (Neither makes any mention of computers whatsoever, nor should they.) I liked both of these books and found their choice of topics and presentation quite good for an introductory course in dynamical systems, either at the advanced undergraduate or beginning graduate level.

The book by Guckenheimer and Holmes, though similar in content, is quite different in spirit and intent. According to the authors it is primarily a "user's guide" intended for "members of the engineering and applied science communities...who do not generally have the necessary mathematical background to go directly to the research liteature." The authors suggest that some parts of the book might well be read with a "microcomputer at hand, so that [the reader] can simulate solutions of the model problems."

Clearly this book is squarely on the interface between the mathematical discipline of dynamical systems and the application of mathematics. I found it a remarkable and fascinating book from the point of view of the mathematician who is generally familiar with the mathematics it contains, but less so with the applications. How well it will succeed in achieving its avowed purpose of serving the engineering and applied science communities is perhaps better judged by someone from those communities. The authors, a mathematician and an engineer, have undertaken a difficult task. My first impression is that this is a difficult book to understand without a considerable mathematical sophistication and/or some previous contact with the field, e.g. one of the other books mentioned above. It could very well be, however, that the engineering-applied science reader will be looking for something different in this book than what the mathematician is. I believe that this would be an excellent choice for a more advanced course in dynamical systems which emphasized applications. Such a course might have as a prerequisite the material of one of the other books and should be supplemented with some original sources.

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JOHN FRANKS

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Special functions in queuing theory and related stochastic processes, by H. M. Srivastava and B. R. K. Kashyap, Academic Press, Inc., New York, 1982, xii + 308 pp., $37.50. ISBN 0-1266-0650-1.

When the early mathematical models of queues were formulated in the beginning of this century, some of the main concepts in the more general field of stochastic processes were not fully developed. Hence these models were at best crude approximations of reality. As more and more tools from the theory of stochastic processes were made available, it was possible to formulate models which were more realistic descriptions of the actual phenomena under study. As a simple illustration, one may refer to the underlying parameters of a simple queu, which by and large were assumed to be fixed, but more realistically vary in accordance with some underlying random fluctuations such as the queu size. Evidently the analyses of such models were more involved with the end results not being in terms of simple functions, but often involving in some form one or more of the family of functions—known as special functions—i.e., gamma, incomplete gamma, beta, incomplete beta, Bessel, modified Bessel, hypergeometric, etc.

What then can be learned from the intersection of the vast amount of literature available in these two areas of mathematics, namely theory of special functions and queuing theory? This well-researched book provides a source for those who attempt to seek answers to this question. Its extensive bibliography lists recent contributions of many authors who have analyzed queuing models which differ from well-known early formulations in that they are often more realistic descriptions of reality. Precisely, the authors attempt to demonstrate how and where special functions appear in the analysis of special queuing models.