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BOOK REVIEWS

INTRODUCTION TO THE THEORY OF SMOOTH DYNAMICAL SYSTEMS

By W. SZLENK: pp. x + 369. John Wiley & Sons, 1984, ISBN 047190117-2. (≃£30)

There are several different approaches one might take in writing an introduction to the theory of smooth dynamical systems. A classical approach might deal with the analysis of mechanical systems arising in mathematical physics or might deal with ordinary differential equations arising elsewhere in science outside of mathematics. The topics covered would likely be dictated by the needs of the problems chosen for investigation. Certainly any special properties of the systems studied – for example, the fact that conservation of energy leads to Hamiltonian systems – would play a large role. The superb book on classical mechanics [A] by V. Arnold is an example of this approach.

One might also investigate dynamical systems from a much more applied perspective. The book by Guckenheimer and Holmes [**G-H**] is an excellent example. Here also the topics covered are largely determined by the choice of applied problems considered. Perhaps the greatest difference from the classical approach is an emphasis on modelling and the study of mathematical models which are somewhat less rigorously derived than the models of celestial mechanics and classical physics.

Modelling is the very difficult process of extracting from some physical phenomenon a mathematical entity with two key properties. It must reflect the essential aspects of the physical phenomenon and it must be amenable to mathematical study. How well a particular model satisfies the second of these criteria is one that (in time) mathematicians can decide. However, whether or not a particular model embodies the important attributes of a real world phenomenon is a question to which mathematics *per se* has little to contribute. It is not a question which is subject to proof or disproof.

This essentially non-mathematical issue has been the basis of some of the more celebrated controversies in mathematics, e.g. catastrophe theory and the theory of fractals. My own view is that for better or worse it will not be mathematicians who decide which models are appropriate for a given discipline but the practitioners of that discipline. This is perhaps as it should be since the mathematician's only qualification for making this decision is that he understands the models. This is a necessary but not sufficient condition (which is sadly sometimes lacking in the practitioners of the discipline). Perhaps this decision should be relegated to philosophers of science. The choice of a methodology for testing models is surely one of the central questions in the philosophy of empirical science.

The approach taken by Szlenk is neither classical nor applied but what one might call intrinsic. While the history of this point of view goes back at least to Poincaré, it owes its current popularity in no small measure to the work and influence of

Steven Smale. One wants to understand up to topological conjugacy, i.e. roughly up to orbit preserving homeomorphism, as large a class of dynamical systems as possible. Large here means in an appropriate topology on the space of dynamical systems in question. Ideally one would like to understand a dense open set or a set of second Baire category. In general this is a hopeless task, so one is quite happy to understand substantial open sets up to topological conjugacy, or to have a lesser understanding of larger sets.

The starting point, however, is internal rather than external. It is the topological structure of the space of dynamical systems which is important rather than the fact that a particular system or class of systems provide useful models for some phenomena. This is pure mathematics not applied mathematics. I heartily approve; this is the way I like to teach this material.

The book of Szlenk is not alone in this presentation. This is the approach taken by Irwin [**I**], by Nitecki [**N**], and by de Melo and Palis [**deM-P**]. However I have the impression that Szlenk's book contains more material than these other two. It might well have been titled a *Comprehensive* Introduction to Dynamical Systems. The additional material, however, is not achieved through greater length. Among the four or five treatises on dynamical systems aimed at a graduate level audience this book may well be the most densely packed. A corollary of this is that it may not be the easiest to read.

This difficulty is exacerbated by a typography which does little to clarify the exposition. This is, of course, a subjective judgement of an intangible aspect of the book. Nevertheless, even though I would be hard pressed to list the qualities of good typography in a mathematics text, I know it when I see it and its absence can be a serious flaw.

The choice of topics covered is for the most part a good one. The table of contents certainly includes most of the important topics which should be there. I have one major disagreement with Szlenk's presentation (and with the presentation of all the other texts on this topic with which I am familiar).

I would like to see a text on dynamical systems organized in the framework devised and used by the late Charles Conley. Conley observed that *every* continuous flow on a compact metric space is constructed from two ingredients. To quote from his monograph [**C**]:

Every flow on a compact space is uniquely represented as the extension of a chain recurrent flow by a strongly gradient-like flow; that is the flow admits a unique subflow which is chain recurrent and such that the quotient flow is strongly gradient-like. The unique subflow is just that on the chain recurrent set; the quotient flow is that obtained on collapsing components of the chain recurrent set to distinct points.

It has always seemed to me that this is the correct framework, at the coarsest level, for studying dynamical systems. One can present purely gradient-like systems such as Morse–Smale flows, then study chain recurrent phenomena like Anosov diffeomorphisms or the Smale horseshoe, and finally investigate the way these two ingredients fit together to form more general systems.

All of these elements are in Szlenk's book but nowhere is Conley's principle quoted above to be found (nor is it in any text on this subject of which I am aware). Szlenk, I believe, does not even define the concept of chain recurrence.

Despite these philosophical differences there is much to like in this book. The important results are all there and, by and large, the author has shown good judgement in choosing which to prove and which only to state. There are also a great many examples in this text and they seem to have been well chosen. It is even more important in dynamical systems than in other branches of mathematics to have ample and appropriate examples. The author has also provided a good selection of exercises to accompany each topic. He also includes several sections briefly summarizing prerequisites from functional analysis and differential topology just before use is made of results from these areas. This is certainly a great aid to the student who is trying to learn the subject and may have imperfect preparation.

I wish that Szlenk had also included a section of this type on the basic properties of manifolds and perhaps on some other topics. Instead he chose to insert very brief notes at the end of the text giving definitions or the statement of important results. He might, for example, have included the Jordan canonical form theorem and a brief summary of elementary results on linear ordinary differential equations by way of motivation for his section on linearizing systems.

As one final quibble, I found some minor problems with the presentation of structural stability to which one of the five chapters of this book is devoted. While it may be true as the author says that, 'dynamical systems which are actually encountered in the natural world are stable, as a rule,' he should have pointed out that essentially none of the classical mechanical systems are *structurally* stable. To cite the problem of the stability of the solar system in an introduction to and motivation for the concept of structural stability can be misleading. Also his statement of the structural stability theorem applies only to diffeomorphisms and includes the hypothesis that the system be C^2. Since the theorem is only stated, not proven, it would be appropriate to give the result for flows and to use only the hypothesis that the system is C^1 since this is sufficient.

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