appears to be the case now. Others have come again from mathematical physics, in particular, as the ideas of Witten on topological quantum field theory have proven their worth in three dimensions. A four-dimensional analogue, involving Floer groups, is a topic of much current research. Work is in progress on the application of gauge theory to the geometry of a four-manifold with a fixed embedded surface in it, thus answering some old questions of Thom. These are all topics in which the authors have a great deal of experience and expertise. One can only hope that the lectures that current Oxford graduate students are experiencing will eventually surface in a form similar to *The geometry of four-manifolds.*

N. J. HITCHIN
UNIVERSITY OF WARWICK

BULLETIN (New Series) OF THE
AMERICAN MATHEMATICAL SOCIETY
Volume 28, Number 2, April 1993

0273-0979/93 $1.00 + $.25 per page

*Perturbation methods*, by E. J. Hinch. Cambridge University Press, Cambridge and New York, 1991, xi + 160 pp., $59.50. ISBN 0-521-37310-7

Differential equations can be divided into those that can be solved and those that cannot. The first class is nearly exhausted by a sophomore "cookbook" differential equations course. The study of the second class is roughly divided into three approaches: qualitative, numerical, and asymptotic. The qualitative approach (which, at least for initial value problems, more or less coincides with "dynamical systems theory") gives up the attempt to find solutions and instead seeks to describe the behavior of the solutions. (In many cases this is what one wants the solutions for anyway.) The numerical approach obtains approximate solutions in the form of tables or graphs. The asymptotic approach, otherwise known as perturbation theory, also looks for approximate solutions but obtains them as formulas. When these formulas are simple enough to comprehend, they can reveal a great deal about the solution. At the simplest level, for instance, an approximate formula for a periodic solution can immediately show the influence of each variable upon the period and amplitude. At a deeper level, it is in the very nature of an asymptotic solution that its terms are sorted into orders of importance. This forces the mathematician into a style of thinking that is reminiscent of pragmatic common sense: when faced with a complicated problem, one asks which features of the problem are most important and attempts to incorporate them into the solution first. In this way, guesswork (more politely known as heuristics) comes to play an important role in the construction of solutions, and proofs of validity (that is, proof of error bounds) get pushed to the end. Often, because the mind-set needed for heuristics differs from that needed for proofs, the proofs get ignored altogether. For many authors, if a solution "looks" asymptotic (that is, if it is "uniformly ordered", as defined below) and agrees well enough with numerical solutions, then it is good enough.

Is it good enough? Let us briefly examine what may be the paradigm case, the case that gave rise to the name "perturbation theory". Everyone knows that Newton "discovered the law of gravity". What this really means is that he unified into one theory the previously separate subjects of "gravity" (falling objects on earth) and the motions of the planets, thereby completing the breakdown (begun by Copernicus and Galileo) of the classical/medieval barrier between sublunar and superlunar phenomena. Newton was able to show that each planet individually would move around the sun on a Keplerian ellipse, but he was also aware that the planets would "perturb" each other's orbits due to their own gravitational attraction. For this reason he postulated that God would intervene occasionally to restore the planets to their proper orbits. Laplace (and others) went on to calculate the perturbations approximately, concluding that the solar system was stable and God's intervention was unnecessary. In fact, the conclusion that the solar system is stable was unjustified, being based on just the sort of heuristic approximate solution mentioned above. Clarification of the issue involves both the difficult Kolmogorov-Arnol'd-Moser (KAM) theory of invariant tori (which shows that most solutions behave as they would if the system were stable) together with the Nekhoroshev theorem about Arnol'd diffusion (which shows that the remaining solutions nevertheless behave stably for very long periods of time, much longer than the age of the solar system). So in this paradigm case, the conclusion based on heuristic approximations turns out to be essentially correct for all practical purposes, but only after some very difficult theoretical work. In some sense this evidence supports the validity of the heuristic theory, but there are two points that I think are important to make. The first is that although Arnol'd diffusion is unimportant in the solar system, there are other much more rapidly oscillating systems in which Arnol'd diffusion can take place in a short enough time to be of practical importance. The second is that several years ago (and this experience is not at all atypical) I received for review a paper that purported to prove that Arnol'd diffusion does not exist. The "proof" consisted of a heuristic approximation, essentially the same as that done by Laplace (although much shorter, because of Hamiltonian formalism that was not available at Laplace's time). Evidently there are still large numbers of people being trained in heuristic asymptotic methods who are not told any of the dangers inherent in these methods or any of the history of the subject.

Admittedly, most topics in perturbation theory are not as delicate as KAM tori and Arnol'd diffusion. For most purposes, it would be a sufficient advance if everyone learning the subject was clearly taught the distinction between *uniform ordering* and *uniform validity*. This distinction hinges on the following definitions. Consider a function $f(x, \varepsilon)$ defined for $0 < \varepsilon < \varepsilon_0$ (possibly also for $\varepsilon = 0$) and for $x$ (a scalar or vector) in a domain $D(\varepsilon)$ which may depend upon $\varepsilon$. (A typical example of such a domain would be $0 \leq x \leq \varepsilon$ or $0 \leq x \leq 1/\varepsilon$, for $x$ a scalar.) Consider also a finite or infinite series of the form

$$g_0(x, \varepsilon)\delta_0(\varepsilon) + g_1(x, \varepsilon)\delta_1(\varepsilon) + \cdots ,$$

where each $g_i$ has the same domain as $f$, and the $\delta_i(\varepsilon)$ are a sequence of monotone increasing functions of $\varepsilon$, called gauges, defined for $0 < \varepsilon < \varepsilon_0$ and satisfying

$$\lim_{\varepsilon \to 0} \frac{\delta_{i+1}(\varepsilon)}{\delta_i(\varepsilon)} = 0.$$

(The most common gauges are $\delta_i(\varepsilon) = \varepsilon^i$.) Now the series is called *uniformly ordered* if each $g_i(x, \varepsilon)$ with $i > 0$ is bounded (for $x \in D(\varepsilon)$ and $0 < \varepsilon < \varepsilon_0$). This guarantees that no term (except possibly the leading term) is of greater significance (for small $\varepsilon$) than is indicated by its gauge. Roughly, the terms are arranged in order of importance, and their importance is correctly indicated by their gauges. (Strictly speaking, we allow a term to be of *less* importance than its gauge at certain points or even everywhere, but, except possibly for the leading term, not of *more* importance. It is possible, for instance, for $g_i$ to vanish at certain points $x$ without requiring that later terms also vanish there. Under certain circumstances a different set of definitions, involving relative rather than absolute error, are more appropriate than are the definitions given above.) On the other hand, the same series is called a *uniformly valid approximation of* $f$ if, for each $k$,

$$\lim_{\varepsilon \to 0} \frac{f(x, \varepsilon) - g_0(x, \varepsilon)\delta_0(\varepsilon) - \cdots - g_k(x, \varepsilon)\delta_k(\varepsilon)}{\delta_k(\varepsilon)} = 0,$$

that is, if each truncation of the series gives an approximation to $f$ with an error of smaller order than the last gauge $\delta_k$ retained. Now it is quite clear from these definitions that *uniform ordering* is a property of the series itself, which can be easily verified by examining the terms of the series, whereas *uniform validity* is a relationship between the series itself and a function that it approximates and can only be checked by examining the error. It happens that uniform validity implies uniform ordering; in other words, failure of uniform ordering implies failure of uniform validity. This fact motivates the most common procedure in perturbation theory: one attempts to solve a differential equation by a series, rejects solutions that are disordered (not uniformly ordered), and modifies the solution procedure (by using heuristic reasoning) until a uniformly ordered series is obtained. If this procedure can be carried out successfully, one has certainly found a *reasonable candidate* for a uniformly valid approximation to a solution of the differential equation. It is entirely permissible for anyone to stop here and to test the solution against experimental or numerical data. What is not permissible is to claim that one has *actually found* a uniformly valid approximation to a solution of the differential equation. To make this stronger claim, one must first of all have in hand a proof of the existence of the solution in question. (Even this is unknown for some nonlinear partial differential equations that have been solved asymptotically.) Second, one must have a proof of error bounds. This is not the place to go into this topic, except to point out that for many classes of problems such proofs are known.

If these remarks are assumed to be understood, then it cannot be taken as a fault that the book under review presents heuristic methods without proof of validity (and without acknowledgment that validity is even an issue); this is simply the task that the author has assigned to himself. Among such books, this one is unique in that it gets very quickly into some of the more difficult and touchy parts of the heuristic theory. There are many examples here of gauges other than powers of $\varepsilon$ and particularly of logarithmic gauges. The useful suggestion is made that for exploratory purposes, when the gauges are unknown, an iteration method may reveal the correct gauges more easily than may a perturbation method. There are examples showing the failure of "Van Dyke's matching rules" (one of the heuristic methods in boundary layer theory) when there are logarithmic gauges and the superiority of the alternate (but equally heuristic) method called "matching in an overlap domain" in this situation. There is a short derivation of the asymptotic approximation to the limit cycle of a Van der Pol oscillator in the relaxation case, a complicated problem involving several different matchings between different domains. There is an example of the use of the WKB method to study an exponentially small term in a boundary layer problem, which leads to paradoxical results when it is treated by matching alone.

As these examples indicate, the strengths of the book lie in the area of boundary layers and matching. There is a short chapter on asymptotic evaluation of integrals. There is some, but very little, attention to the third major area within perturbation theory, namely, nonlinear oscillations. In contrast to the boundary layer problems, all of the oscillatory problems treated in this book are extremely simple: Duffing's equation is solved by Lindstedt's method, the Van der Pol equation (in the nearly linear case, the opposite of the relaxation case mentioned above) is solved by multiple scales, and a nonlinear wave equation is solved by an averaged Lagrangian. Aside from the last problem there is no mention of averaging, which is probably the most useful method in nonlinear oscillations. (Nonlinear oscillations include celestial mechanics, from which, as mentioned above, perturbation theory took its name; there are no celestial mechanics included in this book.)

It is not entirely clear for which audience the book is intended; it would seem to be difficult to find a reader who knows most of the things the author assumes but not most of the things that he says. The book is much too short and sketchy and hurries too rapidly into difficult examples to serve as an introduction. Yet it does treat elementary topics, which would not be required by an advanced reader. Most of the difficult examples are treated so briefly that they are best regarded as exercises with ample hints. For the Van der Pol example, the reader is expected to know immediately that $K_{1/3}$ and $I_{1/3}$ are Bessel functions (they are never identified), to know their asymptotic properties, and to know how they are used to solve Airy equations; all of this is passed over in one line. An acquaintance with fluid mechanics is also assumed. The book will probably find its greatest usefulness as a reference book for those with considerable background, but, for this purpose, one would wish for a better bibliography.

James Murdock
Iowa State University
*E-mail address*: murdock@pollux.math.iastate.edu

BULLETIN (New Series) OF THE
AMERICAN MATHEMATICAL SOCIETY
Volume 28, Number 2, April 1993

0273-0979/93 $1.00 + $.25 per page

*The Stefan problem*, by Anvarbek Meirmanov (translated from the Russian by M. Neizgodka and Anna Crowley). Walter de Gruyter, Berlin, 1992; ix+244 pp., $89.00. ISBN 3-11-011479-8

In crude terms, the Stefan problem is that of solving $\partial/\partial t[u + H(u)] = \Delta u$ where $u$ is the temperature of some material and $H$ is the Heaviside function.