There are many more landscapes to explore from the base camp of the Kepler problem, but I am running out of space and starting to toot my own horn. In conclusion, Singer has done an excellent job of leading the reader from the Kepler problem to a view of the growing field of symplectic geometry.

REFERENCES

1. V. I. Arnol′d, First steps in symplectic topology, *Russian Math. Surveys* **41**:6 (1986) 1–21.
2. M. F. Atiyah, Convexity and commuting Hamiltonians, *Bull. London Math. Soc.* **14** (1982) 1–15.
3. R. L. Bryant, An introduction to Lie groups and symplectic geometry, in *Geometry and Quantum Field Theory*, D. S. Freed and K. K. Uhlenbeck, eds., American Mathematical Society, Providence, 1995, pp. 5–181.
4. W. B. Gordon, A minimizing property of Keplerian orbits, *Amer. J. Math.* **99** (1970) 961–971.
5. M. Gromov, Pseudoholomorphic curves in symplectic geometry, *Invent. Math.* **82** (1985) 307–347.
6. V. Guillemin and S. Sternberg, Convexity properties of the moment map, *Invent. Math.* **67** (1982) 512–538.
7. V. Guillemin and S. Sternberg, *Variations on a Theme of Kepler*, American Mathematical Society, Providence, 1991.
8. H. Hofer and E. Zehnder, *Symplectic Invariants and Hamiltonian Dynamics*, Birkhäuser, Basel, 1994.
9. A. Knutson and T. Tao, Honeycombs and sums of Hermitian matrices, *Notices Amer. Math. Soc.* **48** (2001) 175–186.
10. J. W. Milnor, On the geometry of the Kepler problem, this MONTHLY **90** (1983) 353–365.
11. R. Montgomery, A new solution to the three-body problem, *Notices Amer. Math. Soc.* **48** (2001), 471–481.
12. L. S. Pontrjagin, V. G. Boltyanskii, R. V. Gamkrelidze, and E. F. Mishchenko, *The Mathematical Theory of Optimal Processes*, Wiley-Interscience, New York, 1962.
13. M. E. Taylor, *Pseudodifferential Operators*, Princeton University Press, Princeton, 1981.
14. D. A. Vogan, Representations of reductive Lie groups, in *Proceedings of the International Congress of Mathematicians 1986*, American Mathematical Society, Providence, 1987, pp. 245–266.
15. N. R. Wallach, *Symplectic Geometry and Fourier Analysis*, Math Sci Press, Brookline, MA, 1977.
16. R. O. Wells, *Differential Analysis on Complex Manifolds*, Prentice-Hall, Englewood Cliffs, NJ, 1973.

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Wave Motion. By J. Billingham and A. C. King. Cambridge University Press, Cambridge, U.K., 2000, 468 pp. Cloth: ISBN 0-521-63257-9, $110. Paper: ISBN 0-521-63450-4, $37.95.

Reviewed by **Jeffrey Rauch**

The mathematics of wave motion is a subject of interest to pure mathematicians, applied mathematicians, scientists, and engineers, and it has been the subject of books by people in all these disciplines. On the pure side, there are treatises on aspects of the rigorous theory of partial differential equations. Life is harder on the applied side, where one confidently uses ideas and computations that are not provably correct, and inductive reasoning from simple cases plays a large role. What, then, should be the distinction between an "applied mathematics" treatment of the subject and a "science/engineering" treatment?

In both types of books I expect to see mathematical modeling of physical phenomena involving some simplifications and approximations, followed by some analysis of the resulting differential equations and some explicit solutions. But what distinguishes the applied mathematician from the scientist is a larger set of mathematical tools and a more critical sense about the reliability of the simplifications and approximations. Thus, in a mathematics text I expect to see more care and precision in the approximations, so as to hone the skills of the reader at distinguishing results that are provably correct from those that are trustworthy but not provable and those that are downright unreliable. I also expect to see some precise results (possibly but not necessarily in the form of theorems) about the simplest models, so that at least some of the work is nailed down tight in the tradition descending from Euclid.

In a typical science text, the discussion of problems involving differential equations proceeds in three steps:

1. Derivation of the governing differential equations
2. Simplification by linearization or neglecting relatively small terms
3. Construction of some explicit solutions of the simplified equations

To justify the approximations, one often applies the following criterion:

4. Verify that for the explicit solutions the neglected terms are small compared to the retained terms.

One then proceeds to extract physical information from the solutions.

What is wrong with this? The answer is that sometimes these computations lead to incorrect conclusions. The examples collected in books usually represent successes; the failures are not reported. I think that is exactly the wrong thing to do. The weaknesses should be pointed out front and center so that users are warned of possible traps. The failures are also great pedagogic tools, as correct-sounding arguments leading to incorrect conclusions are intriguing to students with mathematical leanings. Such examples serve a role in marking the limits of applicability of methods that is analogous to the role of counterexamples in delimiting the scope of theorems.

Here is a simple realistic example showing how the standard strategy can lead to trouble. The initial value problem

$$\partial_t u + \partial_x u + u = 0, \qquad u\big|_{t=0} = f(x), \tag{1}$$

has the exact solution $u(t,x) = e^{-t} f(x-t)$; it describes damped waves moving to the right. Let us take the initial value to be a wave packet of wavelength $O(\epsilon)$—say, $f(x) = a(x)\cos(x/\epsilon)$, where $a(x)$ is smooth and vanishes rapidly as $|x| \to \infty$—and consider what happens as $\epsilon \to 0$. The solution u^ϵ is

$$u^\epsilon(t,x) = e^{-t} a(x-t)\cos((x-t)/\epsilon).$$

Suppose we follow steps 2 and 3 indicated earlier. In the limit as $\epsilon \to 0$ one finds that both $\partial_t u^\epsilon$ and $\partial_x u^\epsilon$ are $O(1/\epsilon)$, while $u^\epsilon = O(1)$ is negligibly small in comparison. Dropping this small term leads to the simplified equation for an approximation v^ϵ,

$$\partial_t v^\epsilon + \partial_x v^\epsilon = 0, \qquad v^\epsilon\big|_{t=0} = a(x)\cos(x/\epsilon),$$

whose solution is

$$v^\epsilon(t,x) = a(x-t)\cos\big((x-t)/\epsilon\big).$$

But this misses the exponential decay and thus is not a good approximation.

Two ingredients need to be added to make a careful analysis of such approximations. First, when an approximation is constructed it should be injected into the original differential equation to see how large the residual is, that is, the extent to which the approximate solution fails to solve the original problem. Second, one needs to assess the accumulation of error resulting from such residuals. In the present example, the residual is equal to

$$\partial_t v^\epsilon + \partial_x v^\epsilon + v^\epsilon = v^\epsilon = a(x-t)\cos\big((x-t)/\epsilon\big).$$

The residual is $O(1)$ as $\epsilon \to 0$. Next, for any solution u of (1) one has the simple estimate

$$\|u(t)\| \leq \|u(0)\| + \int_0^t e^{s-t}\|\partial_t u(s) + \partial_x u(s) + u(s)\|\, ds,$$

where the norms are sup norms, i.e., $\|v\| := \sup_{x\in\mathbb{R}} |v(x)|$. The error $E^\epsilon := v^\epsilon - u^\epsilon$ satisfies

$$\partial_t E^\epsilon + \partial_x E^\epsilon + E^\epsilon = v^\epsilon, \qquad E^\epsilon(0,x) = 0,$$

so it is bounded by

$$\|E(t)\| \ \leq \ \int_0^t e^{s-t}\|v^\epsilon(s)\|\, ds = \int_0^t e^{s-t}\|a\|\, ds.$$

It follows that the error tends to zero for times that tend to zero with ϵ but not for fixed times.

The analysis of error estimates is equivalent to studying the sensitivity of the solution of the original boundary value problem to perturbations. Such sensitivity analysis or stability analysis amounts to showing that the original boundary value problem is well posed in the sense of Hadamard, that is, that solutions are unique and depend continuously on the initial data. This notion is a keystone of the subject as we now know it (see the classic text of Courant-Hilbert [**1**]). Moreover, quantitative versions of well-posedness yield justifications of approximate methods.

In summary, the notions of well-posed boundary value problems, stability analysis, and residuals deserve a central position in a mathematical text on waves, along with the four themes listed earlier. They set the analysis on a solid footing, and they acquaint students with the fact that approximation methods are valid only under certain limits that are more restrictive than one might guess. (The problems of stability and well-posedness for partial differential equations invariably involve measuring the size of functions and thereby lead to norms in infinite dimensions. This makes the subject intrinsically difficult. A model for addressing these questions at a fairly elementary level is the excellent text of Strauss [**2**].)

It is interesting to note that analysis of residuals and stability is standard fare in texts on numerical linear algebra and the numerical solution of differential equations. Stability analysis is also traditional in the discussion of wave motion, but only for unstable problems (e.g., Kelvin-Helmholtz, Bénard, Turing). Including stability verifications on some stable problems would clarify what is going on in the unstable ones.

The book under review is aimed at advanced undergraduates and beginning graduate students of applied mathematics who have seen basic fluid mechanics, partial differential equations, and phase plane methods for ordinary differential equations. The authors' goal is to present mathematical ideas of general utility in the study of wave motion and to describe a wide variety of problems that fall under this heading. Unfortunately, whereas the book does a reasonable job of presenting the basic procedures 1–4 outlined earlier, it fails to take the additional steps necessary to produce a mathematically satisfactory treatment of the subject. In my opinion it lacks the clarity and precision that are key elements of mathematical thinking, and its presentation differs little from material one finds in engineering and physics texts on the same topics.

For example, problems like the example discussed earlier are solved on page 152 and the following pages under the heading of high frequency limits, but a different approach via an *Ansatz* of WKB type is employed. To my mind this is a missed opportunity to have shown that one approximation method fails and an alternative succeeds. Contrasting the failure of one with the success of the other would have taught the reader much more.

In contrast to the analytic care that I am recommending, this book has a tendency toward imprecise statements that in the worst cases are simply wrong. Here are a few examples; there are many others.

In the discussion of electromagnetic plane waves at the bottom of page 183, the reader is told that the formulas yields solutions for arbitrary complex wave vectors $\mathbf{k}$ satisfying $|\mathbf{k}| = \omega/c$. For most of us this means $\sum k_j k_j^* = \omega^2/c^2$, but the correct relation is $\sum k_j k_j = \omega^2/c^2$. The meaning of $|\mathbf{k}|$ is not specified in the text. I confess to having made the same blunder myself!

For Maxwell's equation on $\mathbb{R}^3$ (p. 185), the reader is asked to assume that the "charges and currents are confined to a finite volume and that there are no electromagnetic waves radiating energy away to infinity." It is not hard to prove using the Laplace transform that the only solutions with this property have no radiation escaping to the unbounded component of the complement of the set occupied by the currents and charges. Such bizarre situations are not those intended by the authors.

In discussing surface tension on page 94, the authors state that "All molecules in a fluid experience a force due to the presence of the surrounding fluid molecules. In the bulk of the fluid these intermolecular forces balance and the total force is zero." This is not true; the imbalance of these forces gives rise to the pressure force.

On page 58, discussing spherically symmetric solutions ϕ of d'Alembert's wave equation $\phi_{tt} - \Delta\phi = 0$ on $\mathbb{R}^3$, the authors note that $r\phi$ solves the one-dimensional analogue $u_{tt} - u_{rr} = 0$. They then assert that "Spherically symmetric waves incident from infinity are physically impossible, so the only meaningful solution is of the form

$$\phi = -\frac{1}{r} f(t - r).\text{"} \tag{2}$$

(Here f is a function of one real variable.) In the first place, it is not true that the incoming spherical pulses are not meaningful; it is just a matter of scale. Near a focal point of a nearly perfect mirror incoming spherical waves generate good approximations in the same way that the outgoing waves serve as building blocks for studying radiation from antennas. The incoming waves are equally useful for analyzing implosions.

Moreover, (2) is a solution not of d'Alembert's equation but rather of a radiation problem with a singular source:

$$\phi_{tt} - \Delta\phi = 4\pi f(t)\,\delta(x), \tag{3}$$

where δ denotes the point mass at the origin. The authors warn the reader of something of this kind by noting that the velocity field is singular as $r \to 0$. However,

$$w := -\frac{1}{r}\big(f(t-r) + f(t+r)\big)$$

is a solution of d'Alembert's equation even at $r = 0$, although w is singular at $r = 0$ at those times t that lie in the singular support of f. To put this discussion on solid ground, the authors could refer to the unique solution of

$$\psi_{tt} - \Delta\psi = F(t, r) \tag{4}$$

with F and ψ vanishing for t less than the time at which the antenna under consideration starts to radiate. If the antenna is contained in $|x| \leq R$ and F is spherically symmetric, the function ψ is a spherically symmetric solution of d'Alembert's equation in $|x| \geq R$. The radiation problem (4) is solved with the aid of the outgoing potentials from (3), and the standard multipole approximation for $|x|$ large leads to the formulas that the authors want. This improved version involves nearly the same computations as those made by the authors, but it respects the criteria of presenting a well-posed problem and then analyzing it with mathematical clarity and precision.

In summary, the book comprises a collection of wave propagation problems solved with the degree of mathematical care typical of science texts but not, in my opinion, up to the standard that we should require in teaching students of applied mathematics. A text meeting the latter criterion would be very welcome indeed.

REFERENCES

1. R. Courant and D. Hilbert, *Methods of Mathematical Physics,* 2 vols., Wiley Interscience, New York, 1953 and 1962.
2. W. A. Strauss, *Partial Differential Equations: an Introduction*, John Wiley & Sons, New York, 1992.

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