

suitable order properties. (7) Pascal's theorem holds only for the usual symmetric forms and in particular requires commutativity (conic sections make a fleeting appearance at this point in the book). (8) Determination of the two-sided ideals in the ring of all linear transformations, there being one for each cardinal number.

There are two generalizations which the author calls to the reader's attention. The first is to replace the vector space by a suitable kind of module over a ring; in a series of papers the author himself has carried this program nearly to completion. The second is to replace the vector space (implicitly paired to its full dual) by an arbitrary pair of dual vector spaces; here there has been substantial work by Mackey and Rickart. Further in the distance lies the project of uniting these two generalizations by studying dual modules. In yet a different direction lie the still largely mysterious rings and lattices without minimal elements, typified by von Neumann's continuous geometry. So there is much to be done; and the coming generations of young algebraists, with this book happily tucked under their arms, will find the path well laid out.

I. KAPLANSKY

Calculus of variations with applications to physics and engineering. By Robert Weinstock. New York, McGraw-Hill, 1952. 10+326 pp. \$6.50.

This book, which appears in the International Series in Pure and Applied Mathematics, has been written to fill the need for an elementary introduction to the calculus of variations, followed by extensive applications to physics and theoretical engineering. By far the greater emphasis is placed on the applications, and the list of chapter headings will show the scope: 1. Introduction; 2. Background preliminaries; 3. Introductory problems; 4. Isoperimetric problems; 5. Geometrical optics: Fermat's principle; 6. Dynamics of particles; 7. Two independent variables: the vibrating string; 8. The Sturm-Liouville eigenvalue-eigenfunction problem; 9. Several independent variables: the vibrating membrane; 10. Theory of elasticity; 11. Quantum mechanics; 12. Electrostatics.

A book with this scope should have a wide appeal at the present time, particularly among those physicists and engineers who find variational methods tricky and evasive. For the author's aim is clarity of exposition. He goes slowly at the beginning, where slowness is essential, and he provides, at the ends of the chapters, sets of exercises which should prove very useful. He is writing for those who know the concepts and techniques of a first year calculus course,

including a smattering of ordinary differential equations, and who are also familiar with many of the matters encountered in a short course on advanced calculus. To assist the reader, the second chapter contains a list of essential formulae, with derivations partially indicated.

I regard this as a very useful book which I shall refer to frequently in the future. It contains a wealth of material, and the inclusion of elasticity and quantum mechanics in one volume marks a breaking down of barriers, greatly to be desired.

In tempering this praise with some adverse criticism, I would like as far as possible to distinguish between criticism stemming from personal prejudice and criticism called forth by actual errors in the book. Errors are easy to deal with. They creep in somehow or other, hide when the author reads the proofs, and then shame him on the printed page. The court is indulgent, merely sentencing the author to correct them in the next edition. Here are a few things that should be otherwise:

p. 9. The wronskian is shown as a determinant with $(n+1)$ rows and n columns, and it is stated wrongly (cf. E. L. Ince, *Ordinary differential equations*, London, 1927, p. 116) that the vanishing of the wronskian is a sufficient condition for the linear dependence of n solutions of a linear homogeneous differential equation of order n .

p. 44. For Schwartz read Schwarz.

p. 89. This extension of Hamilton's principle to a system without a potential function is a muddle. For a correct treatment, see E. T. Whittaker, *Analytical dynamics*, Cambridge, 1927, p. 248.

p. 149. Some clumsy work here in the replacement of a line integral by the double integral of a divergence over the contained area, confusing since ds/dx becomes infinite twice at least on going round a closed curve. A couple of pages here could with advantage be compressed into a clearer treatment contained in a few lines.

p. 164. The non-degeneracy of eigenvalues for a rectangular membrane of sides a and b depends on the irrationality of (a^2/b^2) , not (a/b) .

Let me now turn to criticism based on personal prejudice. I like simple English—the simpler the better. Therefore I criticize a sentence such as the following (p. 7):

“Quite often involved in the integrand of a line integral taken about a simple closed curve C in the xy plane is the *normal derivative* of a function $w(x, y)$.”

But, happily, this is not a typical sentence. On the whole the style is clear enough, although loaded a bit too much with long words where short ones would have done the job better.

But that is not important. In the category of prejudiced criticism I have two important and interesting matters to discuss. These issues are: (i) the δ method in the calculus of variations, (ii) mathematics by authority, or "we state without proof." The second arises in connection with expansions in terms of eigenfunctions, and plays a fundamental part in Chapters 7 and 9.

The author abjures the "vague mechanical δ method," and uses instead what may be called the ϵ method. The difference is this: in the former the variation of y is δy , and in the latter it is $\epsilon \eta$. What do we gain by the use of the ϵ method? A great deal, I readily admit, if whenever we see the symbol δ we mutter to ourselves archaically "An infinitely small quantity." But we need not do so. We can rationalize the δ of the calculus of variations in the same way as we rationalize the d of calculus, and so enjoy a notation which is precise, suggestive, and economical. It is true that in this process of rationalization we would bring in ϵ , and we would keep it permanently in the backs of our minds. But we would not have to drag it into situations already complicated enough.

Mathematical notations exist to save thought by substituting for it mechanical processes which can be carried out automatically. But we do need, at any given moment, to be able to snap out of our trance and know what we are really doing. So, in the calculus of variations the mathematical physicist needs two wives, δ to relax with and ϵ to tell him where he gets off. He will give up the latter as soon as he understands the former.

He will never give up δ , and I very much fear that the author's harsh rejection of δ will limit the appeal of his book among those who might profit most from it. With no δ , we cannot express Hamilton's principle as $\delta \int L dt = 0$; we have to make a sentence out of it. Indeed, the whole of Chapter 6, devoted to Hamiltonian theory, cries aloud for δ , not only for economy in writing but to express the basic structure of the theory. The author seems to lose sight of the simplicity obtained by taking

$$\delta \int (\sum p_i dq_i - H dt) = 0$$

as the central law, with free variations of p_i and q_i except for $\delta q_i = 0$ at the ends; and the whole of p. 87 may with advantage be compressed into two lines if we recognize the principle of least action as

$$\delta \int \sum p_i dq_i = 0, \quad \mathcal{H}(p, q) = E.$$

Only time will tell. My bet is that two centuries hence δ and d will still be going strong, unless nature happens to find out about second variations; but she has not yet, to any serious extent, and so the author very wisely excludes them from his book.

And now to the other matter—mathematics by authority. On p. 101 we read:

“We state without proof the following theorem concerning the expansion of an arbitrary function in terms of the known set of eigenfunctions:

“If the arbitrary function $g(x)$ is piecewise continuous and piecewise differentiable in $0 \leq x \leq L$, the series

$$\sum_{n=1}^{\infty} c_n \phi_n(x), \quad \text{with} \quad c_n = \int_0^L \sigma \phi_n g dx,$$

converges uniformly to $g(x)$ in every subinterval of $0 \leq x \leq L$ in which $g(x)$ is continuous. We may therefore write

$$g(x) = \sum_{n=1}^{\infty} c_n \phi_n(x) \quad \left(c_n = \int_0^L \sigma \phi_n g dx \right). \quad (29)$$

Moreover, in any subinterval in which $g'(x)$ is continuous, we may differentiate (29) term by term to obtain

$$g'(x) = \sum_{n=1}^{\infty} c_n \phi_n'(x), \quad (30)$$

and the convergence is uniform. (Possible exceptions at $x=0$ and $x=L$ are mentioned below.)”

Here is a situation not uncommon. An author wants to use a technique the validity of which he cannot establish in the text; it would take him too far afield. So he says: “We state without proof . . .” Thus he commits one sin (imposing mathematics by authority, “teacher knows best”) to avoid another (incomplete proof or proof by analogy). For my part, I would choose the latter every time. A statement is set down before me, and it is I and no one else who must put *true* or *false* opposite it in the recesses of my own mind. If the statement is too wide for me to handle in all its generality, then I shall test it in particular instances which lie within my scope.

That is precisely what I did for the statement quoted above. I took $g(x) = x$ in the range $(0, \pi)$ and expanded it in a series of sines. Then I differentiated term by term, and got a series which did not converge for any value of x in the range. Thus, by quite elementary methods, well within the scope of the potential reader of the book, I was able

to satisfy myself that either (a) the statement is false, or (b) I do not understand what it means.

I believe that there is a rather bad error here, which the author would do well to correct if he gets the chance in a later edition—maybe he will have to do quite a bit of rewriting. But even if the error is corrected, that does not answer the question: Shall we impose mathematics by authority? My own view is that we should not. Authoritative statements cannot be completely avoided, but they should be supported by plausibility-arguments and by the working out of special cases within the scope of the reader. This takes space, but it is space well used if one thereby establishes confidence and a sense of reality. In the last analysis, it is the special case that establishes confidence, not the general theorem, and this holds for every-one, high and low.

J. L. SYNGE

Les nombres inaccessibles. By É. Borel. Paris, Gauthier-Villars, 1952. 10+141 pp. \$3.72.

The author prefaces the work under review as follows:

“This little book is the result of half a century of reflections on the principles of mathematical analysis and, in particular, on the definition of numbers. Some of these reflections have already been sketched here and there in the works of this Collection, but it seemed to me that it would be useful to coordinate them in a connected account.

“The profound transformations of physics in the twentieth century, and especially the theories of relativity, quanta, and wave mechanics, have been inspired by the fundamental idea that phenomena must be observed *en eux-mêmes*, without taking account of *a priori* conceptions such as time, space, matter, or energy—conceptions with which one has associated absolute and immutable entities.

“It seems to me that mathematicians as well, while maintaining the full right to work out abstract theories deduced from arbitrary non-contradictory axioms, have an interest in distinguishing, among the objects of thought which are the substance of their science, those which are truly accessible, that is to say, have an individuality, a personality, which characterizes them without ambiguity. One is thus led to define in a precise manner a science of the accessible and of the real, beyond which it remains possible to develop a science of the imaginary and of the imagined, these two sciences being able, in certain cases, to lend each other mutual support.

“Such is the spirit in which I have written this book, which I submit to the reflections of the young mathematicians whose efforts will