engineering or computer science. As a result, some of the applied parts seem to be comparatively more elementary and discursive than the theoretical ones.

As with any text, there will certainly be some disagreement with the authors' choice of topics. This may not so much pertain to the algebraic material as to the selection of the applications. For instance, the stress on algebraic coding theory may be questioned by some readers with interests in computer science who might want to see further discussions on, for example, the algebraic theory of automata and formal languages instead of some material on certain classes of codes. A more objective criticism might relate to the chapter on ALGOL, which is somewhat weaker than the other chapters in the book. One reason for its inclusion was probably the desire to provide a foundation for the ALGOL algorithms in later chapters and to discuss some basic aspects of formal languages. Surprisingly, ALGOL procedures are never introduced, and later on only the Gaussian elimination algorithm is written in the form of a procedure.

These comments cannot in any way detract from the considerable value of the book as a text for various courses of a new and urgently needed type. In its entirety the material can be taught as a year course on the advanced undergraduate/beginning graduate level, and the preface indicates that at Harvard University it is indeed so taught. It should also be possible to select appropriate topics for meaningful semester courses. Numerous exercises have been included throughout the book which should enhance its value as a text even further.

All in all, this is a significant addition to the mathematics text market, which deserves widespread and very thoughtful attention and, hopefully, will stimulate in many institutions the introduction of courses following its ideas.

WERNER C. RHEINBOLDT

Lebesgue's Theory of Integration by Thomas Hawkins. University of Wisconsin Press, Madison, 1970. xv + 227 pp.

A History of Vector Analysis by Michael J. Crowe. University of Notre Dame Press, Notre Dame, 1967. xvii + 270 pp.

The Development of the Foundations of Mathematical Analysis from Euler to Riemann by I. Grattan-Guinness. MIT Press, Cambridge, 1970. xiii + 186 pp.

Die Genesis des abstrakten Gruppenbegriffes by Hans Wussing. VEB Deutscher Verlag der Wissenschaften, Berlin, 1969. 258 pp.

Most mathematics has been developed since 1800, and most history of mathematics deals with the period before 1800. We can only guess at the cause of this strange situation, but one simple explanation comes to mind: to write, say, the history of Riemann surfaces you first have to understand Riemann surfaces. In other words, you must master a sizable field of mathematics before you start, whereas extra training is not needed to study the history of elementary arithmetic. In any case the imbalance exists, and has produced the widespread impression that the history of mathematics consists entirely of early calculus, Arabic numerals, and $\pi = 22/7$.

The consequence is that most mathematicians know almost nothing about the history of the subject. The only familiar source of knowledge is names attached to theorems, supplemented by passing references, gossip, and obituaries. The *Notes Historiques* in Bourbaki, brilliant but all too brief, have been almost the only reminder that modern mathematics *has* a history. But recently there have been signs of awakening interest, and the four books under review, which have all appeared in the last few years, all attempt to deal with serious parts of nineteenth century mathematics.

Lebesgue integration is one of the great success stories of modern mathematics, and Hawkins tells it very well. An introductory chapter sets the scene, describing how the first rigorous theory of integration took shape at the hands of Cauchy and Riemann. We then plunge into fifty years of ferment, as men struggle to deal with "assumptionless" functions which will not fit the theory. Differentiable functions turn up with bounded derivatives which are not (Riemann) integrable; do they satisfy the fundamental theorem of calculus? Rectifiable curves are defined without assuming differentiability; must we give up the integral formula for arc length? To prove uniqueness for trigonometric series we need a term-by-term integration of a series not converging uniformly; can it be justified? Men fall into traps through not understanding the complexity of nowhere-dense sets, and through confusing them with the sets negligible in integration. The valid theorems have complicated hypotheses and even more complicated proofs. At the end of the century Hermite exclaims "I turn away with fright and horror from this lamentable plague of functions which do not have derivatives." And then the key idea enters from a quite unexpected source.

Emile Borel is the main precursor of Lebesgue integration. The monograph developed from his thesis (1898), which lists the basic properties that a definition of measure should satisfy, includes countable additivity. What prompted this (and formed the second half of the monograph) was the study of analytic functions $\sum A_n/(z - a_n)$ where the $\{a_n\}$ form a dense subset of a circle. These were attracting attention because, in clarifying the definition of function, Weierstrass had shown that a series of rational functions could represent one analytic function inside the circle and a different one outside. Borel was able to prove that if $\sum|A_n|^{1/2} < \infty$ there is a way to define analytic continuation across the circle; in fact, given a point inside the circle and another point outside, there is an arc joining them along which the series converges uniformly. The heart of the proof is the fact that the $\{a_n\}$ can be enclosed in intervals of arbitrarily small total length, and this led to the definition of sets of measure zero.

Borel did not connect these ideas with integration, however, and the theory created by Lebesgue in his thesis (1902) rightly bears his name. As Hawkins emphasizes, the generalized definition of integral was only a small part of his accomplishment (W. H. Young thought of almost the same idea independently). What makes the thesis so impressive is the way it sweeps away difficulties which had piled up for half a century—the fundamental theorem of calculus is proved, the arc length integral is re-established, a complicated theorem of Osgood is distilled into a quick proof of the dominated convergence theorem. Within a decade the Lebesgue integral produced substantial progress in Fourier series, Fubini strengthened and simplified earlier results on multiple integrals, and Riesz introduced the L^p spaces. Here the story ends; the new integral is established as part of the analyst's toolbox, and Radon introduces the first of many generalizations to come.

Hawkins's style is straightforward and clear. Consistent notation is used as much as possible throughout the book, which makes for easy reading. I noticed only one slip, an erroneously stated theorem of Young on p. 149. This quick summary has had to omit many things, notably differentiability theorems, and anyone interested in integration should read the book for himself.

Crowe's book on vector analysis seems a little anemic in comparison, perhaps because its title is misleading. The vector differentiation operations are only mentioned in passing, and Stokes's theorem is omitted on the grounds that "it essentially lies outside the province of the history of vector analysis, for the theorems were all developed originally for Cartesian analysis." The subtitle *The evolution of the idea of a vectorial system* is a better description, but even it needs qualification: by "vectorial system" Crowe means 3-space with inner product and cross product, and everything else is disregarded. He discusses Grassmann's exterior product primarily to show how it fails to give a vector result. Differential forms are never mentioned, even though Cartan's early papers come within the period discussed. Crowe quotes from Grassmann the assertion that m linearly independent vectors in an m-dimensional space form a basis; his only comment is "This may be compared with the statement found in current vector analysis books that any vector may be expressed in the form $\boldsymbol{ai} + \boldsymbol{bj} + \boldsymbol{ck}$ where a, b, c are unique." The turn-of-the-century atmosphere becomes almost stifling when we find (p. 123) a reference to "the modern treatment of the linear vector function by means of dyadics."

Thus the book obviously has a very limited scope. It is not the place to find a full discussion of quaternions, or Grassmann, or vector spaces. On serious mathematical questions Crowe frequently chooses to quote the opinions of others rather than offer his own (perhaps wisely: on p. 30 he asserts that quaternion multiplication is anticommutative). He does succeed in his goal of tracing the genealogy of the 3-space system, concluding that it was developed out of quaternions by physicists. The great battle of the 90's was between those who wanted to preserve quaternion notation and those who wanted to deal directly with the operations on vectors. This produced a number of impassioned polemics which are quoted at length and form a piquant contrast to the book's pedestrian prose.

The development of the foundations of mathematical analysis from Euler to Riemann is a more substantial subject, and Grattan-Guinness writes about it attractively; but there, unfortunately, the praise must stop. He appears to idolize Fourier and loathe Cauchy, which makes his book rather like a history of Elizabethan England written by a partisan of Mary Queen of Scots. Cauchy is, necessarily, the major figure in most of the chapters, and the treatment of him is totally inadequate. For example, on pp. 56–58 Grattan-Guinness asserts (without references) that Cauchy always believed in infinitesimal quantities h satisfying $a + h = a$ for all ordinary numbers a. In fact Cauchy repeatedly and unmistakably says that by "infinitely small quantity" he means a variable approaching zero. The very section of the *Cours d'analyse* containing this definition is referred to on p. 61—but only for a sneer at "Carnot-style essays on different orders of infinitesimals." (Indeed, on p. 60 we are told that "Cauchy failed to understand the ideas of Carnot.") Cauchy's "essays" are of course correctly proved theorems about orders of vanishing.

Grattan-Guinness claims, confessedly without documentation, that Cauchy saw Bolzano's 1817 paper on the intermediate value theorem and took from it both the theorem and the definition of continuity. The argument for this is weakened by the admission (in a footnote on p. 53) that a similar definition appeared in the most popular French calculus book before Cauchy. In any case it begs the question to simply describe Cauchy's proof as "a version of Bolzano's." Bolzano in fact first argues for the existence of least upper bounds by repeated bisection, constructing the l.u.b. as a convergent series of reciprocal powers of 2; he then finds the smallest zero of the function by taking the l.u.b. of the numbers where it is negative. Cauchy (beginning a section on numerical solution of equations) divides the interval into m equal pieces, chooses a subinterval on which the function changes sign, and iterates the process, getting convergent sequences of upper and lower bounds for a root together with estimates for the accuracy at each stage.

Grattan-Guinness goes on to assert that the whole idea of basing analysis on the modern concept of limit was taken by Cauchy from Bolzano. The tone of the argument can be judged from a sample (p. 78): "Needless to say, the name of Bolzano appears nowhere in the *Cours d'analyse*; Cauchy would have had more sense than to make Bolzano's work known to his rivals." The assertion in fact seems unlikely, since (1) the idea of quantities approaching but not achieving limits is clearly stated in the famous *Encyclopédie* article *Limite*, which gives the same example (inscribed polygons approaching a circle) that Cauchy uses, and (2) Bolzano's paper does not mention the concept of limit.

There are several other serious mistakes. Ironically, the book spends three pages on Cauchy's construction of integrals and slides over the notorious gap (where continuity is tacitly assumed to be uniform) without the slightest comment. On p. 70 Grattan-Guinness asserts that "Euler's difficulty with series was that *he assumed that all methods of summation were regular*"—italics in the original. No reference is given, and the examples mentioned are in fact examples of perfectly regular summation methods assigning values to divergent series.

The opening discussion of Fourier series is muddled by the false statement (p. 6) that "Euler's term 'continuous' was synonymous with our 'differentiable'." Euler in fact called a function "continuous" if it was given by a single analytic expression on its whole domain; thus the function which equals x^2 for $x \geqq 0$ and x^3 for $x < 0$ is differentiable but not "continuous." Euler apparently *believed* that his "continuous" functions were differentiable, presumably because he could write down formulas for their derivatives; but he was wrong. In the case in point, Euler naturally thought that Fourier series, being "continuous," could not represent functions with corners. Grattan-Guinness' mistake is particularly unfortunate here because the old usage of "continuous" is correctly explained on p. 50—in the quotation from Cauchy.

Parts of the book can be salvaged from the wreckage, particularly some of the material on series. The importance of Dirichlet's paper on Fourier series is properly brought out, and there is an appendix on convergence tests which is very nice. Did you know, for instance, that Gauss invented an equivalent of Raabe's test when he needed it? Grattan-Guinness' comment is worth quoting: "Gauss was indeed the master; and by so much was he the master that twenty years had elapsed since the publication of his paper before anybody was able to catch up with him. In 1813 hardly anyone else understood what the convergence problem was; yet in his aloof way, Gauss concentrated only on the further, technical question of the convergence of the particular series in which he was interested."

It is a pleasure to turn to Wussing's book, a sound presentation of history *wie es eigentlich geschehen*, devoted to tracing the different lines of thought which came together in the concept of an abstract group. It comes as a surprise to find that the theory of finite abelian groups was developed independently, within number theory. There is group-theoretic reasoning in Euler, and much more in Gauss (modular arithmetic, roots of unity, and above all classes of quadratic forms); drawing on this, and prompted by Kummer's study of ideal classes, Kronecker (1870) gave a completely abstract proof of the structure theorem for finite abelian groups.

The primary topic, however, is of course the study of permutation groups. Permutations were introduced by Lagrange and Ruffini to study algebraic equations, but became a subject in their own right in the work of Cauchy. The papers of Galois involved a closer connection with equations, and the fundamental importance of *groups* of permutations became established as the implications of Galois's work were realized. This idea is basic to Jordan's monumental *Traité des substitutions* (1870). Although Kronecker knew Galois theory, it was curiously not until Netto (1882) that commutative permutation groups were seen as a particular case of Kronecker's structure theorem.

The first major generalization was from permutations to geometric transformation groups. They appeared most strikingly in Klein's group-theoretic classification of geometry, the Erlanger Programm (1872); Klein then went on to study automorphisms of the regular solids, showing that the icosahedral group is isomorphic to the alternating group of order 60 and deriving the theory of fifth-degree equations. At the same time Lie, in close contact with Klein, began his study of continuous transformation groups and their connection with differential equations. The final step seems to have been taken in an 1882 paper by W. Dyck, a student of Klein who cites Netto and gives an abstract definition of group together with some comments on generators and relations. A few years later Hölder showed the value of this abstraction by introducing quotient groups and proving the strong form of the Jordan-Hölder theorem.

This summary has omitted an episode strangely reminiscent of Holmes's curious incident of the dog in the night-time. ("The dog did nothing in the night-time." "That was the curious incident.") Dyck drew heavily on an 1878 paper of Cayley, a paper which essentially defines finite groups and shows via group tables that they can all be viewed as permutation groups. The curious fact is that Cayley had presented many of the same ideas back in 1854— and no one, not even Cayley himself, did anything with them. In a sense the entire book is an explanation of how an idea which was useless in 1854 became essential in 1882. Wussing is fully conscious of this theme and what it suggests about mathematical progress.

The topic of the book is such that less is said about group theory itself than about the subjects in which it grew. These discussions are far from perfunctory; the 13 pages on Galois, for instance, are an excellent study of the spirit of his work. Wussing always gives enough detail to let us understand what each author was doing, and the book could almost serve as a sampler of nineteenth century algebra. The bibliography is extremely good, and the prose is sometimes pleasantly epigrammatic: "Boole today has the notable fame of having been the most underestimated mathematician of the 19th century."

These four books reflect the current state of history of modern mathematics; they point out how little has been done, and exhibit the rewards and the risks of pioneering. The subject finally seems to be here to stay, since articles have also started to appear (notably in Truesdell's *Archive for history of exact sciences*). At this point an interested and critical audience is required, and undoubtedly many mathematicians will develop a taste for history now that it is available. As a start, I suggest reading Wussing or Hawkins. It won't hurt, it will supply some interesting tidbits for your lectures, and you may even decide that history of mathematics has something to it after all.

WILLIAM C. WATERHOUSE