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The higher calculus: A history of real and complex analysis from Euler to Weierstrass, by Umberto Bottazzini; translated by Warren Van Egmond. Springer-Verlag, New York, Berlin, Heidelberg, London, Paris, Tokyo, 332 pp., $39.00. ISBN 0-387-96302-2

This book is the second, much revised and augmented edition of one first published in Italian [**1**]. The first edition was good, and this one is better. The subject is not really analysis as a whole but the foundations of analysis, the origins of concepts and rigorous proofs, by no means devoid of examples to show how need for change arose and how new modes of thought developed. Of course Bottazzini makes good use of his few recent predecessors' works, for example [**2**], which covers a greater range, and [**3**, **4**], which suffer from their authors' lack of experience in mathematics itself and in mathematical ways of thinking, and [**5**], which treats only the concept of function. Bottazzini's book is much better than [**2**, **3**, **4**], for he speaks with authority, understands and treats fairly his sources, quotes neither too much nor too little, and writes compactly yet with precision. He lets his authors speak for themselves to a great part, aiding the reader to pass from one quotation or paraphrase to the next by brief yet informative transitions, and at the ends of many sections are excellent summaries in a few well chosen words, free of the pontifications in unsupported generalities that often deaden academic theses and writings by authors still close to them.

A standard defect in historical writings on mathematics comes from their authors' failure to see that the sources of pure mathematics often lie in works that today's mathematicians would consider to be "applied" mathematics or "physics". This defect damages most severely the researches of the eighteenth century, in which "applied" mathematics had not been invented, and mathematics was divided into "pure" and "mixed"; in Samuel Johnson's words, "pure considers abstracted quantity...; mixt is interwoven with physical considerations." Another is the writers' tendency to assume that rigor was sought for rigor's sake, which while true of some works of some mathematicians was not at all characteristic of the search for and achievement of rigorous procedures by, for example, Cauchy and Riemann. Bottazzini's first edition suffered somewhat from the former of these defects, just a little from the latter. In this second edition he has taken trouble to remove both. In regard to the latter, in a passage revised from the first edition we read (p. 90; translation corrected)

> Abel's arguments concerning the need for rigor in mathematics were becoming ever more widely diffused at the time and were accepted by many mathematicians. But in order to understand fully the motives for and the strength of this new attitude towards the foundations of mathematics, we must ask ourselves how and why *the methods* of doing mathematics, and consequently the accepted criteria of mathematical rigor, came to be inadequate in the eyes of mathematicians at the beginning of the century, an opinion that Abel stated most clearly.
>
> It is difficult and probably impossible to find a single answer. There were many factors both internal and extenral to mathematics that came together to form the new point of view. One of them was forcefully emphasized by Abel himself—working mathematicians encountered errors and paradoxes when they held to the point of view that had been accepted until then.

Indeed Bottazzini frequently adduces examples of what "working mathematicians" needed and what impelled them to search for good proofs and above all good definitions (p. 3):

> Nor is it purely accidental that new criteria of rigor have more often appeared in the formulation of *definitions* than in *demonstrations*. Definitions in fact appear within the complex structure of a "mature" theory and are subsequent to true mathematical discovery.Prime examples of "working mathematicians" who gave much effort to strengthening the foundations are Euler, Cauchy, and Riemann.

Prime examples of "working mathematicians" who gave much effort to strengthening the foundations are Euler, Cauchy, and Riemann.

In regard to the former common defect the following new passage explains a sounder approach (pp. 58–59; translation corrected):

> This lively discussion of the principles of mechanics in the early years of the century thus ended by entwining itself with questions about the foundations of the calculus, which were challenging and in many respects more important. This is a fundamental point if we desire to understand fully the nature of the debate on the foundations of analysis that took place at this time.
>
> It is customary in presenting the history of the infinitesimal calculus during the early years of the nineteenth century to make no reference to the problems involved in the debate about the principle of virtual velocities. In my view, this is a serious historical error. It is wrong to introduce interpretative categories that are common today, such as the distinction between "pure" and "applied" mathematics, into periods of mathematics where they do not apply. That distinction did not exist at this time, and would not do so for several decades to come. In addition, the "polytechnical" training of the French mathematicians pushed them towards a unified conception of science.

Despite his will to the contrary, Bottazzini did not succeed in writing a wholly correct, consequential, and comprehensible account of "The Elements of Analysis in the Eighteenth Century", Chapter 1. In what he did write he may have relied too much on [3], which evidences capricious, hasty consultation of the sources and reflects scant understanding of what was thought and done by mathematicians of that time. Figure 1 (p. 25), which refers to Euler's second definition of "function", is sure to confuse because it appears in the midst of Bottazzini's account of Euler's work on the vibrating string, which replaces the second definition by a third, namely that which the folklore of mathematics attributes to Dirichlet. Here should have appeared the "eel-like curve" that Euler used to construct his solution of the wave equation by periodic continuation of a curve "traced in any fashion,... not... subject to any equation..." Indeed Bottazzini's description of what Euler did with the wave equation is obscure, though not so erroneous as [3, pp. 5–13]. It was in this research that, for the first time, functions were defined *arbitrarily*, without use of an "equation", on a *fixed interval*; their periodicities required in the solution of the problem were then obtained by suitably translating and inverting the shape given on the interval.

The heroes of the book are Euler, Cauchy, and Riemann. Though important achievements of many other mathematicians are described, nobody comes close in total to any of the great three. Bottazzini quotes many of Cauchy's published criticisms of Lagrange and supports them in detail.

Cauchy here, for the first time in a history, is given his just due. His progress in analysis is traced with care and justice: his innovations, his achievements, his errors, his corrections of them, his adjustments, his changing views. We must not expect to read that Cauchy's rigor was either typical of him or a triumph. He applied it mainly in didactic works which were failures in their intended scope but later became classic. There is a sad story about his teaching at the École Polytechnique, where he was ordered to stop wasting future engineers' time by giving them rigorous proofs, for they needed only applications (p. 150)—let many an untenured teacher today take comfort! Then there were those whom he offended by his criticisms of Lagrange; of course they rejected his innovative rigor, for the old formalism was good enough for them (p. 157)—let today's mathematicians who try to clean up thermodynamics take heart, for in Cauchy's precedent they may yet hope for glory after death.

When we come to Riemann, and only the portion of his work that goes into the foundations of analysis is taken up in this book, we can only gasp in admiration.

I hope that working mathematicians today will read this book and learn from it that the truth about the creators of classical analysis is more wonderful than the folklore.

Translation is a thankless task, but nevertheless a good book deserves a good translation, which the one under review is not. The translator does not understand Italian well, and his English is slovenly. At some places a reader must know both Italian and English to get the sense. For example, on p. 46 "il geometra... geniale" is rendered "the 'genial geometer'", whence will arise, surely, the myth that Monge was a genial fellow. The statement of Riemann's great theorem on trigonometric series (p. 246) makes no sense; by consulting the first edition (p. 205), from which the text is somewhat modified, I could find what was meant. The clause (p. 133) referring to the Cauchy-Riemann equations as something "which Cauchy never even wrote" is obviously a wrong translation, what was meant being something like "which Cauchy did not write here". I have kept a list of dozens of lesser errors, but at least this translator does not fall to the depths of rendering Abel's famous statement "Cauchy est 'fou'" by "Cauchy is a fool" [3, p. 25].

The presentation of the book is scandalously bad, especially for a publisher with a great tradition of excellence. Apparently it was reproduced from a "camera-ready text," as the saying goes. The result is ugly and hard to read. The paper is flimsy and tears easily.

The foregoing review does not do justice to the book. To those mathematicians who would like to know how classical analysis developed, I can only say, Read it!

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Empirical processes with applications to statistics, by Galen R. Shorack and Jon A. Wellner, Wiley Series in Probability and Mathematical Statistics, John Wiley and Sons, New York, Chichester, Brisbane, Toronto, Singapore, 1986, xxvii + 938 pp., $59.95. ISBN 0-471-86725-X.

This is an impressive book, the result of a colossal undertaking by two people who have witnessed much of, and contributed to, the modern development of empirical processes and their applications to statistics.

In their preface, on the main objectives of their study, the authors write:

> The study of the empirical process and the empirical distribution function is one of the major continuing themes in the historical development of mathematical statistics. The applications are manifold, especially since many statistical procedures can be viewed as functionals on the empirical process and the behavior of such procedures can be inferred from that of the empirical process itself. We consider the empirical process per se, as well as applications of order statistics, rank tests, spacings, censored data, and so on.