sidered in several contributions. One of these (Semikhatov) gives a valuable historical perspective of stromatolite biostratigraphy in the Soviet Union, where it originated. The consensus is that "more work needs to be done"; the appearance of increasing inconsistencies with the zonation of a decade ago has cast doubt on its reliability.

The volume has a useful glossary, a list of available translations of works in Russian, a comprehensive bibliography of over 2000 references, with an index largely by geologic age, and a subject index of 18 pages.

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# On the Uses of Rigorous Proof

**Proofs and Refutations.** The Logic of Mathematical Discovery. IMRE LAKATOS. John Worrall and Elie Zahar, Eds. Cambridge University Press, New York, 1976. xii, 174 pp., illus. Cloth, $19.50; paper, $4.95.

Every beginning student is taught that, in mathematics, results are *proved*. The idea is old as Euclid. Genuine mathematics is done by beginning with immediately self-evident principles (axioms) and using these principles in chains of unimpeachable reasoning (proofs). The 20th century has seen a variety of attempts to elaborate this familiar theme. Philosophers of quite different persuasions have tried to uncover the foundations of finished mathematical theories, with the aim of showing that the theorems of those theories can be rigorously proved. Questions about mathematical discovery and about the evolution of mathematical concepts have been bypassed in favor of issues concerning mathematical truth and mathematical evidence.

Imre Lakatos's collection of essays is designed to challenge both the aims of 20th-century philosophy of mathematics and the Euclidean picture of mathematics that lies behind them. His central thesis is that the role of proofs in mathematics is misunderstood, and he suggests that this crucial misunderstanding will affect the quality both of mathematical research and of mathematical education. The mistake is to regard proofs as instruments of justification. Instead we should see them as tools of discovery, to be employed in the development of mathematical concepts and the refinement of mathematical conjectures.

This approach to mathematical proofs is elaborated and defended in the title essay (a very slightly revised version of an article published in four parts in the *British Journal for the Philosophy of Science* in 1963–64), which occupies 105 of the 154 pages of text. Lakatos had planned to amplify his suggestions, but he died in 1974, at the age of 51, and it has been left to two of his former students, John Worrall and Elie Zahar, to complete the book he envisaged. They have added one short chapter and two appendixes, all deriving from Lakatos's doctoral dissertation. These shorter pieces illuminate and extend some of the main ideas of the title essay, but they do not (and could not) tie up all the loose ends.

Lakatos proposes that a proof is "a thought-experiment . . . which suggests a decomposition of the original conjecture into subconjectures or lemmas" (p. 9). The creative mathematician begins with a conjecture and tries both to prove it and to refute it. By uncovering counterexamples he refines his proof-ideas, and by using the more refined proof-ideas he unearths new counterexamples. This process, in which proofs and refutations interact, leads from an initial conjecture, couched in vague terminology, to a body of precisely formulated results. The process is vividly illustrated in the title essay. Lakatos imagines a student discussion of Euler's conjecture about the relation between the numbers of edges, vertices, and faces of polyhedra. His imaginary students adopt, defend, and criticize the positions taken by historical figures. Their discussion formulates explicitly Lakatos's method of proofs and refutations and uses it to improve the original conjecture into sophisticated theorems.

What does the example show? I think that Lakatos has demonstrated that there are important issues about mathematical discovery that should not be neglected. The process of mathematical discovery cannot be dismissed (as it so often has been) as a series of "happy guesses." Yet we might feel that Lakatos's picture is complementary to, rather than inconsistent with, the classical Euclidean view. Perhaps the method of proofs and refutations enables mathematicians to discover mathematical truths, but they then have the duty of proving those truths by constructing rigorous derivations from uncontroversial axioms. So, while Lakatos has insightfully investigated mathematical discovery, we might suppose that issues about mathematical justification are still important and are unaffected by his suggestions. Furthermore, there are reasons for skepticism about the general applicability of his method. Conjectures about polyhedra are testable in obvious ways—we can construct or draw polyhedra. Are there analogous ways in which we can test conjectures about topological spaces or continuous functions?

Lakatos endeavors to forestall these objections in chapter 2 and appendix 1, respectively. The theme of chapter 2 is that completely rigorous mathematical proofs can be obtained only if the theorems of finished parts of mathematics are recast as "arithmetico-set theoretical tautologies" (p. 125). Thus Lakatos seems to conclude that a particular development of the Euclidean picture of mathematics is true, but rather uninteresting. (Interestingly enough, Lakatos appears to hold that logic is immune to question, a position that is reinforced by several editorial footnotes.) He offers no account of how mathematical claims could be justified during the process of developing a mathematical theory.

Appendix 1 sketches the history of the development of the concept of uniform convergence. Lakatos attempts to rebut the criticism that the method of proofs and refutations is inapplicable in areas of abstract mathematics by arguing that, in this case too, the method plays a vital role. He provides an accurate account of Cauchy's attempts to prove that the sum of a convergent series of continuous functions is continuous, and he proposes that the concept of uniform convergence was forged in an attempt to refine the proof against known counterexamples from the theory of Fourier series. Unfortunately, the rational reconstruction of the history is much less convincing here than in the case of the Euler conjecture. There are two related reasons for the difference. Lakatos has tried to detach the problem of convergence from the cluster of issues addressed in early-19th-century analysis, and the development of ideas on these other issues is relevant to the elaboration of concepts of convergence. Moreover, because Lakatos has not provided an account of how mathematical principles can be justified during the course of theoretical evolution, he is unable to explain the rationality of the process that led to the notion of uniform convergence.

To see how these issues are connected and how they expose a gap in Lakatos's approach, we should return to the contrast between the case of uniform convergence and the example of Euler's conjecture. Hypotheses about polyhedra can be tested fairly directly, through an activity akin to scientific experimentation. To test Cauchy's "theorem" that the sum of a convergent series of continuous functions is continuous one must use other parts of analysis to generate counterexamples. The thesis that there are discontinuous functions that can be expressed as the sum of a Fourier series is a consequence of principles of analysis—such as the new ideas about functions, continuity, and convergence—that could themselves be questioned. Lakatos has not explained the rationale for accept ing the principles that generate the counterexamples to Cauchy's "theorem," and he has thus failed to exhibit the reasons for criticizing Cauchy's attempts at proof.

The problem is a general one. If the method of proofs and refutations is used in developing areas of abstract mathematics then we may expect to find that favored portions of a theory are exempted from criticism and used to generate counterexamples to other parts of the theory. A rational reconstruction of the evolution of the theory through this type of criticism will have to explain why mathematicians are justified in accepting the favored principles and in using them as tools of criticism. Lakatos's logic of discovery requires an account of how some principles of a developing theory are rationally accepted. Let us call such an account a theory of interim acceptability. Armed with an account of this kind, Lakatos could show how the early-19th-century analysts were justified in adopting the new ideas about functions, convergence, and continuity and in directing their suspicions against Cauchy's "theorem." (To do so, however, he would have to probe the history more deeply, relating the new ideas to the anomalies and disputes of 18th-century analysis and rational mechanics.)

Ironically, the most obvious theory of interim acceptability would challenge directly the Euclidean picture of mathematics. It would regard mathematical principles as justified by their consequences, and by their application in the solution of scientific problems. Unfortunately, this approach is at odds with Lakatos's Popperian dismissal of inductive evidence, but it would appear to accord with his most fundamental aim, namely that of showing the irrelevance of rigorous Euclidean proofs to actual mathematics.

Lakatos's book has many merits, some of which I can only touch on here. The title essay raises, though it does not resolve, the question of when further generalization of a mathematical conjecture becomes trivial. The second appendix makes interesting recommendations about the teaching of mathematics. Lakatos points out forcefully that many of the abstract and unintuitive concepts of modern mathematics could be motivated through discussion of the historical problems and proof-ideas from which they were generated. More generally, even though one may quarrel with some of his historical discussions, one can only applaud Lakatos's method. Philosophers of mathematics should not continue to ignore the fact that mathematics has a rich and exciting history.

*Proofs and Refutations* presents a program that needs to be taken seriously by anyone who is interested in the nature of mathematics. We shall understand mathematics better when Lakatos's ideas are developed and we gain a clear idea of their merits and shortcomings. Lakatos has left to his successors the task of completing a picture—of which he has boldly sketched a (proper) part.

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## Nineteenth-Century Physics

**The Kind of Motion We Call Heat.** A History of the Kinetic Theory of Gases in the 19th Century. STEPHEN G. BRUSH. North-Holland, Amsterdam, 1976 (U.S. distributor, Elsevier, New York). In two books. Book 1, Physics and the Atomists. xiv + pp. 1–300, illus. + index. \$24.75. Book 2, Statistical Physics and Irreversible Processes. xiv + pp. 301–770, illus. + index. \$59.75. The set, \$75. Studies in Statistical Mechanics, vol. 6.

Stephen Brush began to study the history of the kinetic theory of gases in 1954 and has pursued the subject with remarkable singleness of purpose ever since. In these two volumes he has brought together 15 of his articles plus a new introductory chapter and a concluding bibliographical study. He has reworked the articles to bring them up to date and has trimmed them to create a unified narrative. The result is more than a set of collected papers. Occasionally there are awkward transitions (as between an article written for the *Dictionary of Scientific Biography* and one written for *Physics Teacher*), rough edges (as in the chapter entitled "Interatomic forces and the equation of state," where the scissors and paste are too much in evidence), and repetitions, but on the whole the articles tell a coherent story.

The first book consists of the introductory survey and a series of chapters on individuals who made major contributions to the development of the kinetic theory. The second book consists of chapters on what Brush calls "problems" in the theory. I would rather call them guiding themes or ideas that determined the course of the history. These include the wave theory of heat, statistical mechanics, interatomic forces, transport theory, conduction and radiation, randomness and irreversibility, and Brownian movement. The second book also contains a bibliography of 19th-century contributions to the subject. Brush judges it to be 99 percent complete. I think he is probably being too modest.

The work is long, but quite readable. Brush meanders through the history of the kinetic theory, pointing out ironies and paradoxes that tell us a great deal about how science functions. He ends a heavy chapter on the transport theory of Chapman and Enskog with a delightfully lighthearted portrait of Chapman originally published in the London *Observer* and a more solemn one of Enskog. Chapman admitted that reading his *Mathematical Theory of Non-Uniform Gases* was like "chewing glass," an evaluation with which the reader can readily agree. In many similar ways Brush has enlivened what could have been a terribly dry subject.

He also warns us that prejudices affect the writing of history more than we might think. We tend to give greater weight to quantitative results than to qualitative ones, even though the qualitative ones may be of greater importance. Thus a scientist who does not have his name attached to any "law," "rule," or "equation" is apt to be lost to history. For example, at the end of the 19th century the atomic kinetic theory was in decline, while Ostwald's "phenomenological" ideas were rapidly gaining ground. It was Jean Perrin's experiments confirming Einstein's theory of Brownian motion that finally proved the existence of atoms in motion. In light of Perrin's experiments, Ostwald and many of the other phenomenologists swung round to support the kinetic theory. It was a turning point in the history of atomism, and yet