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.

PHILIP KITCHER
Department of Philosophy,
University of Vermont, Burlington

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

Perrin does not hold a prominent position in the historiography of modern physics because his results were basically qualitative. There was no new "rule" to name after him.

Another historical puzzle that has invited misinterpretation is the demise of the caloric theory of heat. Count Rumford's experiments, which, perhaps, should have persuaded physicists and chemists that heat could not be a substance, in fact had no such effect. Caloric kept its supporters until the idea was almost universally replaced in the 1830's by an erroneous, briefly held, "wave theory of heat." The success of the wave theory of light in the 1820's and 1830's suggested a comparable theory for radiant heat. It was an easy step to conclude that all heat transfer except for the bulk motion of convection was accomplished by radiation. Conduction, in particular, could be explained by waves in the aether between neighboring molecules. Because the caloric theory could not easily explain heat radiation, it seemed simpler to account for all heat transfer by vibrations in the aether. The wave theory of heat served as a temporary bridge between the caloric theory and the kinetic theory, spanning a conceptual gap that most scientists had been unable to cross.

Brush also finds that scientific concepts often have extremely vague meanings while they are being developed and that the historian has to be constantly on his guard against mistaken interpretation. For instance, in reference to the wave theory of heat mentioned above, Professor Philip Kelland of Edinburgh stated his continuing support of the caloric theory by saying that recent experiments had demonstrated that the heat is transmitted by vibrations of the parts of the caloric. But according to the old caloric theory, caloric *is* heat. In referring to vibrations in the caloric, Kelland abandoned the substance of the caloric theory. He claimed to be an advocate of the caloric theory and used its terminology, but in fact he adopted the wave theory.

Another example is the so-called ergodic hypothesis, that a mechanical system left to itself will pass through every point of the phase space lying on a certain energy surface. Even with very close reading of the texts, it is difficult to tell whether Ehrenfest, for example, meant ergodic or quasi-ergodic when he discussed the hypothesis, that is, whether he saw the system as passing through every point or only infinitely close to every point. The distinction is crucial, but because it was not regarded as crucial at the time the meaning of the term "ergodic" is blurred.

The word "randomness" presents similar problems. Does a scientist describing a "random" process mean that the process itself is random or merely that it appears random because of our lack of knowledge? Often the scientist himself does not see the difference.

In a final example, Brush shows that the concept of conduction in a gas did not have a clear meaning until the advent of kinetic theory and that even then it was next to impossible to separate the phenomenon of conductive heat transfer from that of radiative heat transfer in a gas. Modern textbooks regard conduction and radiation as very different phenomena and therefore relegate them to different chapters. The history then tends to become divided along the same lines as the textbooks, but it should not be. Historically the phenomena were studied together.

In these and other examples, Brush does a great service to the history of science by emphasizing the confused state of developing concepts in physics. It is all too easy for the historian to see a familiar word like "reversibility" and attach an equally familiar meaning to it without checking to see if the original author was using the word in the same way.

Another innovation in this book is the use of referees' reports to document the reception of a new theory. The first papers on the kinetic theory by John Herapath and John Waterston were rejected by the Royal Society. Waterston's statement of the equipartition theorem was in a paper that Sir John Lubbock called "nothing but nonsense, unfit for reading before the Society." When we consider that at approximately the same time Laplace and his coterie at the French Academy were blocking papers by physicists of the caliber of Fresnel and Fourier, we begin to wonder how much the course of 19th-century science was directed by private animosities and personal prejudices. Even Clerk Maxwell, the most even-handed of critics, was not above making use of a paper by Osborne Reynolds that he had refereed and casting aspersions upon Reynolds's ideas before the author was in a position to defend himself.

I have concentrated on the more controversial and interpretative parts of Brush's book. It also contains a great deal of factual information of value to any historian of 19th-century physics. It is not a book that one is likely to read straight through, but there is much in it for specialist and nonspecialist alike to think about.

THOMAS L. HANKINS
Department of History,
University of Washington, Seattle

Sidelights on Darwin

The Collected Papers of Charles Darwin. PAUL H. BARRETT, Ed. University of Chicago Press, Chicago, 1977. Vol. 1, xviii, 278 pp., illus. $20. Vol. 2, viii, 326 pp. $20.

Hitherto it has been necessary to seek out Darwin's minor works in a variety of journals many of which are hard to come by. Barrett has rendered an invaluable service to Darwin scholarship by searching through often obscure sources, such as horticultural journals, and presenting us with many previously unknown or forgotten publications.

However, the fact that Darwin's works are more accessible does not mean that they will be read, much less read with understanding. One must realize that Darwin was as near to being a pantologist as was possible in his day. In the late 1830's he worked out a vast theoretical system. But he did not begin to publish on it until 1858, and it took the rest of his life to present his views in detail. The evolutionary content of the works he published prior to 1858 is cryptic, and even those published later are hard to follow. To appreciate the minor works requires a solid grasp of the major ones, and the general reader would best prepare himself by reading Darwin's books first. Even for a reader who has undergone such preparation a certain amount of exegesis would be helpful. Barrett provides none, and it seems appropriate that I proffer a few suggestions in this review.

Some of the papers, such as the joint publication with Wallace and the ones on heterostyly and orchids, are preliminary notices of work Darwin later presented in greater detail. The general reader will find them interesting curiosities, but would be better off using the definitive works. Specialists will find them invaluable, for, as with revised editions, point-by-point comparison manifests the evolution of the views expounded. Likewise the paper on the parallel roads of Glen Roy (of which Darwin was "ashamed") and his reply to Galton on pangenesis cast some light on Darwin's errors, but they are hard to assess unless one has an appreciation of what data were available at the time.

Many of the papers are brief communications filling in details of interest and following up on earlier publications. A few reply to criticisms and are of interest for that reason, but one needs to go back and read the critiques themselves before one can evaluate them.

There are, however, many papers of broader interest. The joint paper with