male competition for sexual access to females.

In closing, Goodenough responds to the criticism that emic ethnography, by virtue of the importance it attaches to what is distinctive about particular cultures, is opposed to the search for cross-cultural regularities. To the contrary, he says, the two enterprises are complementary and logically related. Emic concepts provide us with what we need to know to construct valid etic concepts, and the latter, besides being the elements in terms of which comparative propositions must be framed, help to expedite discovery and description of the former. This holds true not only for propositions about the interrelations of cultural forms, but for propositions about the relations of cultural forms to extracultural variables as well. With the consideration that attention to both emic and etic concepts is indispensable for achieving the aims of scientific anthropology, Goodenough rests his case.

Description and Comparison in Cultural Anthropology is a tightly written work which, though intricate and technical in parts, is rarely obscure. It makes a substantive contribution to the theory of human society and, simultaneously, represents a timely and valuable excursion into contemporary anthropological epistemology. On both counts it is challenging and stimulating. And on both counts it merits the careful study of all professional anthropologists.

KEITH H. BASSO
Department of Anthropology,
University of Arizona, Tucson

The Post-Newtonian Period

Jean d'Alembert. Science and the Enlightenment. THOMAS L. HANKINS. Clarendon (Oxford University Press), New York, 1970. xii, 260 pp., illus. $11.25.

Not so long ago there was an inaccurate saying that Newton's achievement was of such a magnitude that a century was to elapse before other scientists could go beyond it. One suspects the myth was English in origin, for although British science subsided a bit in quality after the activity that culminated in Newton, science on the Continent continued to be vigorous. Yet even today we are scarcely flooded by books on 18th-century science, at least prior to Lavoisier. What is available is largely in articles, chapters of books devoted to longer sweeps, or volumes that are not handy—an example being Truesdell's important work on rational fluid mechanics in Euler's *Opera Omnia.*

It is therefore with a good deal of interest that one takes up Hankins's book on d'Alembert. D'Alembert is often treated in one-sided fashion, either as the coeditor of the *Encyclopédie* and literary *philosophe*, or else as a brilliant if confusing mathematician. And yet he ought to be of great interest in his entirety, since he did have a foot in each camp and since his thought was probably not as bifurcated as historians tend to see it as being. In Hankins's view, the predominant aspect of that thought was more Cartesian than anything else, an idealizing rationalistic mentality that, in spite of the vogue of English empiricism, sought to root philosophy in necessary and certain principles. To d'Alembert, rigor in concept and demonstration was the highest goal.

Such an attitude brought d'Alembert into conflict with those, like Clairaut, who were striving to match the mathematics to the phenomenal world. It also brought him into conflict with those, like Diderot, who wearied of mathematical rigor that did not take into account the foibles of humanity. Hankins's accounts of d'Alembert's disputes with his fellow *philosophes* and scientists are a necessary part of the story of the Enlightenment, when the search for a new kind of secular basis for all knowledge was a central aim.

In this light, some of the arguments between Enlightenment thinkers that seem only curious today take on a better perspective. For behind these arguments were philosophical commitments and logical difficulties that could not be resolved by mere mathematical formalization. It turns out that the common characterization of 18th-century science as "Newtonian" appears, on close inspection, to be virtually useless, and indeed misleading. Part of the trouble is due to the *philosophes* themselves, of course; it was stylish to claim to be Newtonian. That claim, however, seldom went far beyond the acceptance of Newton's law of gravity and his celestial mechanics. Furthermore, Newton had certainly not completed the study of celestial mechanics, and many questions remained that he had not envisaged. To call mechanics "Newtonian" in 1760 would be much the same as calling quantum mechanics "Planckian" today; at once the statement is a truism and empty of any deep significance.

The organization of Hankins's book is worth mentioning. He begins by discussing d'Alembert's education, his debut into the scientific community, his work with Diderot, and his eventual shift of emphasis to literature and the politics of the academies. Slowly, however, the discourse shifts away from the biographical scenario to a more topical one. Toward the end of the book are the more technical treatments of the notion of force, of the *vis viva* controversy, and of the general manner in which physical laws were conceptualized. These more technical details are discussed intelligently and, for the most part, with clarity. They form an important reprise of items discussed earlier so that both the mathematically adept reader and the one who is less so can profit. One can see the nature of the problems faced by d'Alembert and his colleagues.

In short, Hankins's effort is to be applauded. It is to be hoped that more monographs will appear in this curiously neglected period of post-Newtonian science.

J. MORTON BRIGGS, JR.
Department of History,
University of Rhode Island, Kingston

Reminiscences

My World Line. An Informal Autobiography. GEORGE GAMOW. Viking, New York, 1970. xiv, 178 pp., illus. $5.95.

This is an *informal* autobiography (as claimed by the subtitle) in the sense that it is neither a detailed historical document nor a deeply analytical account of the author's life and times, and it is somewhat sketchy. However, it is *good* autobiography, as far as it goes, for each incident gives a vivid glimpse of some aspect of George Gamow or of his environment, and the account is chronological and apparently reasonably complete, up to the time of his arrival in the United States in 1934, at the age of 30. The descriptions of his major contributions to physics in that period, though brief, are clear and even rather exciting, at least to a physicist. For the later period, they are sporadic and less satisfactory. (For example, there is a two-page account of a problem in the theory of white dwarfs which does not say what Gamow's contribution to it was.) Gamow's personal life in the United States is almost completely neglected. Although his parents and