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.

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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.

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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

grandparents and numerous uncles are described in detail at the beginning of the book, his son Igor is mentioned only as being born. The beautiful Barbara, whose inimitable and imaginative verses are scattered through the volume, is given two paragraphs, one of them devoted mainly to a joke about Gamow's showing a policeman their wedding license instead of his driver's license.

However, Gamow was a thorough and methodical man (in spite of all appearances), and the reviewer has no doubt that the book would have been more complete if he had lived longer (he was still working on it at the time of his death). Anyway, it is very good reading as it is.

There are some revealing passages. Gamow's discovery of the mechanism of alpha decay was made in Göttingen in the summer of 1928 (and simultaneously by Gurney and Condon, as he states). He explains that although Göttingen was then full of the feverish activity of applying the new quantum mechanics to atomic and molecular problems, he did not get much involved in that (and decided to try instead to apply the new theory to the nucleus), for two reasons: First, there were far too many people engaged in that activity and he "preferred to work in less crowded fields." Second, "any new theory is almost always expressed in a very simple form, [but] within only a few years it usually grows into an exceedingly complicated mathematical structure. . . ." The same attitude appears in his complete lack of enthusiasm for the doctoral thesis topic (on the adiabatic invariants of a pendulum) that had been assigned to him in Leningrad; he found it boring. The idea of the mechanism of alpha decay occurred to him in Göttingen while he was reading an article by Rutherford, which contained an explanation that did not appeal to him. He says, with evident pride, "Before I closed the magazine, I knew what actually happens in this case." He valued simple ideas and simple explanations of things. His description of Friedman's work on cosmology emphasizes that Friedman's improvement of Einstein's cosmology involved deleting a term from Einstein's equation (the cosmological term) rather than adding new terms.

Gamow had a constant flow of original ideas (a sizable fraction of them were good ideas, and he abandoned the others quickly). It would seem that, in order to keep these ideas flowing, he studiously (though possibly subconsciously) avoided becoming involved in what he considered to be uninteresting complications. He surely *could* have learned to spell (in English, German, and in Russian) if he had wanted to, and this reviewer has seen him use mathematics (correctly) when he wanted to. But, by and large, he shied away from any subject when original ideas were no longer important and technical elaborations were accumulating instead.

If this interpretation is correct, it may partly explain Gamow's relative isolation in Boulder during his last decade. There were many good nuclear physicists and astrophysicists there, but apparently they did not seek out Gamow, nor he them. It may be that nuclear physics and astrophysics, in both of which he had played dominant roles, no longer appealed to him when million-dollar computers, elaborate theories, and volumes of data were involved. So, instead, he began to work in biology—in a part of biology where original ideas could still count. Unfortunately, this reviewer cannot assess his contributions there.

Other familiar Gamow characteristics are also evident in the book, such as his complete lack of both vanity and unnatural modesty and his complete lack of acrimony: situations that would have brought anger from others just brought jokes from "Geo." One can hardly imagine a more interesting world line to read about.

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The Causation of Behavior

Animal Behaviour. A Synthesis of Ethology and Comparative Psychology. ROBERT A. HINDE. Second edition. McGraw-Hill, New York, 1970. xviii, 876 pp., illus. $15.50.

Ethology has been generously endowed with general books by its leading practitioners, such as Eibl-Eibesfeldt, Klopfer, Manning, Marler, and Tinbergen. Robert Hinde's volume, in a revised and expanded second edition just four scant years after the first, remains in the ascendant. It is a perceptive scientist's analysis of some of the central questions about animal behavior. Although far too long and difficult to be used as a one semester's introductory text, this volume should be virtually mandatory reading for students of behavior beyond that level.

The volume is organized around the three major topics of causation, ontogeny, and evolution of behavior. It cannot, however, be considered a complete exposition of behavioral studies, for it gives little attention to behavioral problems at the population level. For instance, social systems are virtually ignored, as is much of animal communication. The chapter on evolution and behavior pays as much attention to the aid that ethology can render to taxonomy as it does to the actual evolution of behavior. Nowhere is there an analysis of how traditional behavior originates or how it is transmitted within populations. And the few pages devoted to the adaptiveness of behavior and its role in the creation and maintenance of species are more for the sake of completeness than analysis. This is, then, a book about the behavior of individual organisms.

The greater part of the volume is concerned with sifting the vast literature on what the Old World ethologists term the causation of behavior: its integrated control by environmental stimuli and internal physiological mechanisms. Hinde's *modus operandi* is to ask a question, such as whether coordinated movements are controlled by environmental stimuli, proprioceptive feedback, or endogenous central nervous mechanisms. He may take one or many chapters to frame an answer, weaving as he goes the warp of ethology with the woof of psychology. Often, the answer is not simply a choice among the alternative hypotheses but, rather, a documentation of the diversity of answers that pertain to different organisms and different behavioral patterns.

In tackling the various problems of causation—complex movements, the organization of perception, factors that produce selective attention and perception, internal drives, changes in motivational states, spontaneity of behavior, and the conflict of simultaneously activated patterns—Hinde introduces in passing many of the older ethological concepts rendered superficial by his synthesis. The chapter on animal orientation seems misplaced in these discussions, and the classification of motivational systems does not clearly enough dissect functional from physiological categories. Nor does the treatment of problems such as perception seem as penetrating as the treatment of problems on which Hinde himself