countries started in somewhat different ways, it is, to be sure, difficult to give an exact figure for how far the United States was ahead in the development of the H-bomb, but it seems to me that one should say we were a year and a half ahead (that is, from March 1954 to November 1955) or, better, that we were three years ahead (from 1954 to 1957). The contrary notion that the Russians won the first heats of the H-bomb race and that we barely eked out a tie in the finals is common; it delights Russian chauvinists, it pleases American hawks, but it is false. Moreover, this false notion does not relate merely to national pride; it involves an important political matter. The idea held in some circles that the Oppenheimer security hearings of 1954 may have been in an important sense "justified" because Oppenheimer almost caused us to lose this vitally important race is based on the assumption that the race was very close.

The part of the book dealing with Teller in the years since Mike presents a different problem. Here there are also some misconceptions, but the main fault involves what is omitted. These particular misconceptions and omissions all derive from the authors' evident failure to discuss the most important events of the last half of Teller's professional life, the years at Livermore (1952–1975), with anyone who really knew much about them, save Teller himself. Judging from the names of sources given in the preface, and from the text itself, the story of the Livermore years is based mainly on prior books, plus interviews with Teller, Ferdinand Brickwedde, and Lowell Wood. Contrary to what Blumberg and Owens say, Brickwedde never was on the staff at Livermore; he visited during the first year for a month or so only, he was involved in strictly peripheral matters, and he had relatively little personal knowledge either of the H-bomb program itself or of Teller's interaction with it. Wood only became involved in this stream of events some ten or so years after the period began. In sum, there is no evidence in the book that any members of the scientific or administrative leadership of the laboratory during its formative years, except Teller, were interviewed or otherwise consulted by the authors.

The most important misconception involves the first nuclear weapons tests made by the Lawrence Livermore Laboratory, a laboratory that was created in 1952 in part because of Teller's dissatisfaction with the leadership at Los Alamos and in part because Ernest O. Lawrence and others (including me) thought the U.S. nuclear program should be stepped up in response to the first Soviet A-test, Korea, and other Cold War events. The first Livermore tests in Nevada in 1953 gave somewhat lower than expected yields, the first Livermore test in the Pacific in 1954 was a fizzle, and what was to have been the second was canceled. Blumberg and Owens present several possible reasons for these problems and for the cancellation. They discuss the idea that the canceled bomb may have been "too big"; they suggest that the fizzle occurred because it involved especially radical or imaginative ideas, or because it was trying to achieve "too much." In fact, these first tests did not involve especially radical or bold designs. Rather, the Livermore Laboratory was trying too hard to do something that would differ substantially from what Los Alamos was doing (a general policy that was not, as the book claims it was, due primarily to Teller), and as a result Teller and the rest of us worked out a design containing a serious unanticipated fault. We canceled the second test when we realized it had the same problem. Contrary to the quotation taken from Teller's own writings, in no sense did Teller have difficulty in persuading the rest of us that the second Pacific test should be canceled.

The most serious of the many omissions involves what happened next. Despite those early poor showings, in the mid-1950's the Livermore Laboratory did manage to pull up its socks and generate some important contributions to nuclear weapons design. This was accomplished almost entirely by two groups of young men mostly in their late 20's and early 30's, just as had been the case at Los Alamos ten years earlier during the war. One of these groups was led by Harold Brown, the other by John S. Foster, Jr. Each of these groups continued to follow the Livermore precept of "doing something different from Los Alamos," but this time they were successful. They did make use of a particular suggestion by John von Neumann, and, of course, of the basic 1951 Teller-Ulam invention, but in the main the new elements were based on their own ideas. These ideas worked out very well, and while the Polaris warhead is the best-known instance, an important fraction of the current U.S. warhead designs are based on ideas that came out of the Brown and Foster groups during those early years. The failure of Blumberg and Owens to mention either of these men or any of the other laboratory leaders except in one or two relatively trivial connections (Brown as an originator of Project Plowshare, Foster in connection with helping to hasten the preparations for a 1958 test series) creates the totally false picture that the intellectual content of the Livermore program was mainly or even entirely due to Edward Teller. Moreover, this error evidently is not happenstance; it seems rather to be what the authors indeed came to believe on the basis of their interviews. They say that "Teller, by 1960, felt that Brown and a new generation of young scientists were ready to take over the responsibility of running the laboratory." In fact, this new generation had been fully responsible from the beginning, more than eight years before.

One last point. This book contains a number of references to this reviewer. I have ignored them here, but that should not be interpreted as a case of "silence gives consent."

Edward Teller is one of the most important figures in the development of 20th-century technology. The world needs a good biography of this complex and exceptionally influential scientist, and he deserves one. Unfortunately, *Energy and Conflict* does not even begin to fill the need.

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Background for a Mission

The Study of Comets. Proceedings of a colloquium, Greenbelt, Md., Oct. 1974. B. DONN, M. MUMMA, W. JACKSON, M. A'HEARN, and R. HARRINGTON, Eds. National Aeronautics and Space Administration, Washington, D.C., 1975 (available from the Superintendent of Documents, Washington, D.C.). In two parts. xxxiv + 1084 pp., illus. $11.25. NASA SP-393.

In the introduction to this book, Bertram Donn writes that the prospect of space missions to comets led members of the staff of Goddard Space Flight Center to propose an International Astronomical Union colloquium at which the cometary physics essential for mission planning would be examined. The great brightness predicted for the perihelion passage of comet Kohoutek led to some modification of the program, but the original objective has been realized in the publication reviewed here. Of the 153 participants, 39 were from countries other than the United States; it is to be regretted that there is only one contribution from the U.S.S.R.

Part 1 of *The Study of Comets* is devoted principally to observational programs and, as might be expected, the majority of the papers deal with comet Kohoutek, including two by its discoverer. (An additional score of papers on that comet appeared in *Icarus* **23**, No. 4 [1974].) Although the comet did not measure up to the expectations of extreme brilliance, this book shows that the prediction led to an unprecedented mobilization of observers and instrumentation and demonstrates that exceptional brightness is not a prerequisite for the application of new and sophisticated techniques. The observations described and analyzed range from conventional photography of the tail with small Schmidt telescopes, to ground-based photometry and spectroscopy at optical wavelengths, to radio detection of molecules and ultraviolet spectroscopy from spacecraft.

About half of the pages of the book are occupied by reviews, comments on papers, and panel discussions. It would be impractical to outline the 70-odd contributions, which on the whole complement one another without the duplication sometimes encountered in reports of meetings. The substance and spirit of the discussions (enlivened by occasional passages of vigorous disagreement) have been successfully preserved. *The Study of Comets* is recommended to anyone who wishes to become acquainted with current problems and accomplishments in cometary research. Among the many excellent papers are several on the allied problems of the nature of the nucleus and the coma. Sekanina's "A continuing controversy: Has the cometary nucleus been resolved?" is a more comprehensive review than the title suggests. It can be read in conjunction with the following 50 pages of panel discussion, with Delsemme's review, "The neutral coma of comets," and with related contributions, such as the review by Roemer, "Luminosity and astrometry of comets," "Gas phase chemistry in comets" by Oppenheimer, "The gas production rate of comet Bennett" by Lillie and Keller, and Herbig's "Review of cometary spectra." One could select similar sequences of papers bearing on cometary dust and ions, and a reader who wishes to appreciate what a campaign devoted to a single comet can yield will find 18 papers devoted to comet Kohoutek.

Part 2 concludes with five contributions on possible spacecraft missions, of which "Expected scientific results on ballistic spacecraft missions to comet Encke during the 1980 apparition" by Mumma appealed most to this reviewer. It should be read in association with "Nongravitational forces on comets" by Marsden and "Orbital error analysis for comet Encke, 1980" by Yeomans. Comet Encke is clearly the prime target for a mission. The three papers that follow Mumma's also include orbital diagrams for Encke 1980 missions, although Farquhar's contribution also looks at Encke in 1984 and three other possible targets, including Halley's comet.

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Hominid Evolution

Paleoanthropology. Morphology and Paleoecology. Papers from a congress, Chicago, Sept. 1973. RUSSELL H. TUTTLE, Ed. Mouton, The Hague, 1975 (U.S. distributor, Aldine, Chicago). xvi, 454 pp., illus. $29.50. World Anthropology.

Primate Functional Morphology and Evolution. Papers from a congress, Chicago, Sept. 1973. RUSSELL H. TUTTLE, Ed. Mouton, The Hague, 1975 (U.S. distributor, Aldine, Chicago). xvi, 584 pp., illus. + plates. $34.50. World Anthropology.

These two volumes consist of papers presented at the 11th International Congress of Anthropological and Ethnological Sciences. The differing titles notwithstanding, both books are directed mostly to questions of hominid evolution.

The papers in the books are gathered into groups, each of which is followed by a discussion ostensibly devoted to the ideas and issues raised in the papers.

Both volunteered and solicited papers are included. In the prefaces, the editor and the organizer of the sessions, R. H. Tuttle, writes that one of his aims in soliciting papers was to focus on issues of special interest. Such a strategy has the disadvantage of emphasizing the ideas of those with recognized views and encouraging the inclusion of data and conclusions that have already been published. It is successful in several sections of these volumes, however, notably those dealing with the evolution of bipedalism and the evolution of the brain and language in the primate studies volume and the very long section of the paleoanthropology volume devoted to the status of *Ramapithecus* and other Miocene hominoids.

The section of the paleoanthropology volume that concerns *Ramapithecus* includes papers by Conroy and Pilbeam, Aguirre, and Eckhardt. Their major conclusions have appeared elsewhere, yet the section as a whole is very useful, demonstrating the difficulties of evaluating the rather meager collection of scraps identified as *Ramapithecus*. The articles emphasize different aspects of the morphology and point up the equivocal nature of the bones themselves: some attributes clearly support a hominid status for this sample, whereas others suggest a short-faced pongid. The selection of papers also reflects the lack of agreement about whether all the fossils currently placed in *Ramapithecus* are in fact the same animal; some authors suggest that all the fossils ought to be considered hominid, whereas others eliminate the East African or the South Asian samples from consideration as earliest hominid. Finally, this section provides an excellent example of the disagreement about the identification of specific morphological attributes that has caused serious problems of interpretation in paleoanthropology. The first lower premolar (P_3) of the East African *Ramapithecus wickeri* is variously described as "semisectoral and bicuspid," "unicuspidate," and ape-like, and one paper denies that a P_3 from *Ramapithecus* has been reported.

In general, the papers are about evenly divided among reviews or overviews on particular subjects and presentations of original research, especially descriptions of recently discovered fossil specimens.

Among the latter, I found Sartono's description in the paleoanthropology volume of the newly uncovered *Homo erectus* skull from Java (called by Sartono *Pithecanthropus* VIII and by Jacob in the same volume Sangiran 17) particularly illuminating. His discussion of the relative stratigraphy of the Java specimens and their differing morphology clearly reveals hominid morphological change during the period from about 1.5 to about 0.5 million years ago. Sartono places the *Pithecanthropus* VIII specimen somewhat later in time than other *Homo erectus* fossils from Java, yet earlier than the Upper Pleistocene hominid sample from Ngandong (the "Solo" specimens). Sartono demonstrates that *Pithecanthropus* VIII shares morphological affinities with both the earlier *Homo erectus* fossils and the later-in-time Ngandong sample, emphasizing the temporal continuity of this Java sample.

Another article I found especially useful is Brain's succinct discussion of the South African Kromdraai australopithecine site. After summarizing the geology and the hominid and other vertebrate paleontology of this least known of the South African early hominid sites, Brain offers an interpretation of the accumulated bones from Kromdraai based on the extreme fragmentation of the individual pieces and a reconstruction of the en-