"Four future presidents of the American Physical Society," photographed in 1938. *Left to right*, Luis W. Alvarez, J. Robert Oppenheimer, William A. Fowler, and Robert Serber. [From *Discovering Alvarez*]

story of the development of this hypothesis is recounted in both books. The extraterrestrial cause of the extinctions is widely but not universally accepted (see, for example, the article by A. Hallam in *Science* 27 November 1987). In any case, the original article by Alvarez *et al.* has stimulated an enormous amount of work and thought on the subject, and it has also raised the issue of a possible nuclear winter resulting from nuclear war.

Discovering Alvarez is a splendid book in which students and colleagues take us beyond what is printed in the technical publications to explain how a number of experiments and projects were conceived, developed, and carried out. These commentaries not only make clear what Alvarez's contributions to these programs have been, especially in basic ideas, but they also show the admiration and affection with which he is regarded by most of the contributors to this volume.

For me, this book conveys better than the autobiography the impressive nature of Alvarez's scientific work. One reason, perhaps, is that the colleagues who wrote commentaries were not inhibited from bestowing praise on their subject. Another, however, is the reluctance of Alvarez himself to include much technical matter in the autobiography. In the introduction he writes, "Parts of the book may be too technical for some readers; such parts can be easily skipped"—as he himself did when reading Abraham Pais's biography of Einstein, *Subtle Is the Lord*. But there is no comparison in this regard. Pais's book is full of highly technical material, which most readers will not really understand, but it is a marvelous book, not in spite of this material but partly because of it. In contrast, Alvarez has been overly successful in keeping his book from becoming too technical. I think if he had taken the opportunity to explain a little more of the physics behind some of his experiments, especially in the early sections, the book would have been improved for readers having an interest in science (and surely these make up the bulk of potential readers). In any case, a desire to learn more about the technical aspects of the experiments or programs can be satisfied by referring to *Discovering Alvarez*.

These books provide many insights for answering an inevitable question raised by Alvarez's career: how was one person able to accomplish so much? We obviously cannot answer that question, but some factors contributing to his success are easy to identify, besides the obvious ones of exceptional intelligence and a capacity for hard work. He has an acute observational skill coupled to a well-developed curiosity about how and why things happen the way they do. He has a great analytical ability and a willingness to use it. He is original in his thinking, questioning conventional wisdom. He knew when to leave work to others and when to do it himself. He was lucky in obtaining the requisite funding for his projects with a minimum of delay and red tape. In his autobiography he explains his disdain of our present peer review system for funding and recommends that funding decisions be based on the applicant rather than on the proposal. This is the way his programs were funded; when he asked E. O. Lawrence for help in obtaining $2.5 million for the 72-inch bubble chamber project Lawrence told him that it was too large an extrapolation and that he didn't believe in the big chamber, but "I do believe in you, so I'll help you get the money." Finally, another important factor for success was the attention paid to the advice of his father, who emphasized very early the value of thinking about the importance of what one is working on; in particular, "he advised me to sit every few months in my reading chair for an entire evening, close my eyes, and try to think of new problems to solve. I took his advice very seriously and have been glad ever since that I did." When reading these books it is interesting to speculate about which of Alvarez's ideas were a direct result of following his father's advice.

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A Biochemist in America

In Quest of Panacea. Successes and Failures of Yellapragada SubbaRow. S. P. K. GUPTA with EDGAR L. MILFORD. Evelyn, New Delhi, India, 1987 (available from Preethi Kiran, 260 Avon Road, Devon, PA). 311 pp. $15.

Yellapragada SubbaRow (1895–1948), characterized in his obituary in *Science* as an "India-born biochemist," made his way to the United States as a young man, obtained a Ph.D. at Harvard, and ultimately became the leader of a productive research group in the American pharmaceutical industry. The author of this valuable account of his career is a journalist whose interest in SubbaRow was aroused during the 1940s by Paul de Kruif's articles in the *Reader's Digest*. After SubbaRow's death, Gupta collected extensive material about him from published and archival sources and interviewed many individuals who had known him. Among these people were SubbaRow's relatives and friends in India, his colleagues at the Harvard Medical School (where he had worked from 1923 until 1940), and his associates in the Lederle Laboratories of the American Cyanamid Company, where he had spent the remaining years of his life. The book that has emerged from Gupta's painstaking efforts was long in the making, and its publication appears to have been beset by difficulties. The New Delhi address of the publisher is the same as that of the author, and the book was printed in Moscow at a time when Gupta was a press correspondent there. Clearly, for the author SubbaRow has been a source of inspiration and national pride, but hagiography has been blended with frank recognition of some of the less attractive features of SubbaRow's complex personality. Despite its modest format and somewhat disjointed organization, as well as a plenitude of typographical errors, this book merits the attention of historians of American science.

From the book we learn that SubbaRow had been a rebellious young man and that, four years after his marriage of convenience in India, he left his 16-year-old wife to study public health at Harvard, where he became associated with Cyrus Hartwell Fiske (1890–1978), a member of the medical school department of biochemistry. Fiske assigned SubbaRow the problem of improving the then-available method for the determination of the phosphate content of biological fluids. By assiduous effort, and in the face of many personal difficulties, SubbaRow succeeded and thereby won recognition as a promising young biochemist. Then followed (in 1927) the identification by Fiske and SubbaRow of creatine phosphate

and (in 1929) of adenosine triphosphate as constituents of muscle tissue; the priority for the latter discovery is still disputed, and the credit is usually assigned to Karl Lohmann. The finding of these two substances initiated a decisive change in biochemical research on the physiology of muscle contraction and on carbohydrate metabolism. In 1930, SubbaRow received his Ph.D. in biochemistry. Fiske and he then turned to the search for the curative factor in liver for pernicious anemia. In the course of this work, SubbaRow became a consultant of Lederle Laboratories, where he went when his academic prospects at Harvard proved unfavorable. The longest section of the book is devoted to the competition between SubbaRow's group at Lederle and those at other American pharmaceutical companies (notably Merck) in the elucidation of the chemical structure of new vitamins and antibiotics and in the patenting of processes for their manufacture. Gupta gives a detailed account of the important contributions of the Lederle group to the determination of the structure of folic acid (pteroylglutamic acid) and to its chemical synthesis.

In the final section of the book, Gupta tells of SubbaRow's personal relationships, his religious beliefs, his hobby of aviation, and his troubles with the U.S. immigration and naturalization authorities. These vignettes, together with those offered in earlier sections of the book, give glimpses of a man who drove himself (and others) unmercifully to achieve success and recognition in a society that regarded him as an alien.

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Private Glimpses

Simple Curiosity. Letters from George Gaylord Simpson to His Family, 1921–1970. LÉO F. LAPORTE, Ed. University of California Press, Berkeley, CA, 1988. x, 340 pp., illus. $29.95.

Through all of his vast and brilliant *oeuvre*, one searches in vain for a glimpse of the real George Gaylord Simpson. Even his autobiography, though truthfully presenting many aspects of his personal life, is primarily a "right-brained" review of his remarkable paleontological career. Only in this book of letters does one at last strip off the cloak of public narrative. Don't read this book if you want to learn more about Simpson's science; but if you wish to fathom more of the person behind the genius, get hold of it, for it goes far toward revealing the off-duty "George" and the beloved "G."

Among the most striking features of these

Simpson in the field. [From *Simple Curiosity*]

200 letters spanning 50 years are Simpson's wry wit and irrepressible creativity. More than his share of mental depressions and physical illnesses did not prevent his delivering brilliant book reviews, penetrating character analyses, original perceptions of local customs, philosophical digressions, and endless varieties of poetry, epigrams, and whimsy. The letters also bubble with illuminations, hieroglyphs, marginalia, and cartoons.

Three-fourths of these letters were to his sister Martha, four years senior to George; most of the rest, much more formal, were to his parents. Through most of his mature life he showed his love and respect for them by a stream of long epistles, most of them written on Sundays. The later years, during and after World War II, are not as well represented as the earlier years, and the contents here tend to be restricted to ordinary family business. Léo Laporte, the editor of these letters, has done an excellent job of transcribing and, when necessary, translating them. His brief introductions to the chronologically ordered sets of letters provide an excellent biographic framework.

The letters shed new light on Simpson's two marriages, the first as unhappy as the second was happy. Through half of this book Lydia, whom he married secretly while still a Yale undergraduate, is hardly mentioned until, four daughters later, during his long sojourn in Patagonia, Simpson determined to break with her. The letters reveal more of the wonderful relationship between Simpson and fellow scientist Anne Roe. Indeed, it is embarrassing to peep into the passionate coded postscripts from "G" in Montana to Anne in New York during the summer of 1935.

Although respected by colleagues, Simpson was often thought to be arrogant and aloof. Here we get flickers of how melancholic it was from the inside: "My life seems to be turning in on itself almost viciously. . . . I literally haven't a single friend in the world. . . . In spite of the fact that I write and speak with great ease and sufficient fluency, some item of my personality . . . makes intimacy on my part even with those who could eventually comprehend me and enjoy me, very slow to come by."

Simpson's early letters express great ambition to establish himself. By his postdoctoral year in London, however, he wrote his father, "It is less than two years since I published my first paper and yet it seems to me as if I had always been an accepted paleontologist." To his sister he viewed his work more cynically; for example: "I have now published two books and 53 shorter papers and all of them are lousy. I owe apologies to posterity." Likewise, "You seek beauty as I seek truth, and I fear, or rather hope, that you have the better of it. For beauty is plainly everywhere, while truth is—heaven knows where—and one has to

Sketch by Simpson from a set headed "Quelques uns de mes amis de la Floride." [From *Simple Curiosity*]

seek it with the tongue in one cheek to keep from going mad." He was equally cynical about the motives of most science: "The highest possible scientific motive is simple curiosity and from there they run on down to ones as sordid as you like." Yet he continued to enjoy his work, observing, "It is all very strange and thrilling in a way which is, I am afraid, incommunicable." To his credit, he never lost the urge to explain evolution and paleontology to nonprofessionals—perhaps to displace the "faith in Hearstian science," which he detested.

There are still many mysteries surrounding Simpson, but this book at last gives us some authentic glimpses into his formidable mind and his surprisingly warm heart.

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