BOOK REVIEWS

Dragon's Teeth

The Los Alamos Primer. The First Lectures on How To Build an Atomic Bomb. ROBERT SERBER, RICHARD RHODES, ED. University of California Press, Berkeley, 1992. xxiv, 98 pp. + plates. $23.

The *Los Alamos Primer* is a legendary document in the literature of nuclear weaponry, summarizing what was known and what was unknown in April 1943 when the Los Alamos laboratory began its work. It consists of lecture notes written up by E. U. Condon from five lectures given by Robert Serber. It is here displayed to the public, with extensive explanatory comments by Serber, with a historical introduction by Richard Rhodes, and with an appendix containing the text of two memoranda written by Rudolf Peierls and Otto Frisch in England in 1940. The Peierls-Frisch memoranda were addressed to the British government to call attention to the possibility of nuclear weapons. The Serber lectures were addressed to the scientists who came to Los Alamos three years later to translate this possibility into hardware.

When I visited Berkeley in the summer of 1948 and talked with the physics students, I found that many of them had on their desks some mimeographed pages discussing various physics problems with great succinctness. These pages came with the heading "SERBER SEZ" in large letters. That was my first contact with Robert Serber. He had then recently taken over Oppenheimer's position as professor and chief theorist in the Berkeley physics department. The "SERBER SEZ" notes gave the students the benefits of his knowledge with as few words and as few equations as possible. He knew how to make things plain and simple. In later years I sometimes wondered whether the "SERBER SEZ" papers had survived and whether they might be worth collecting and publishing as a monument to Serber's clear thinking. I wish they had been published. Instead we have this primer, the forerunner of "SERBER SEZ," provided to the bomb designers when they first assembled at Los Alamos. This is an earlier product of the same mind and the same style, exhibiting the same brevity and clarity, but engaged in an enterprise of more dubious value. While recognizing the importance of this primer as a historical document, I still wish that it had been allowed to languish in obscurity for another century or two.

To me, the most revealing passage in the book comes near the end, when Serber describes an ingenious trick for simulating the behavior of a supercritical fast-neutron chain reaction with a subcritical slow-neutron chain reaction. This meant that the explosive events occurring in an unsymmetrical half-assembled bomb could be simulated in a laboratory experiment without risk of destroying the laboratory. This was Serber's own idea, and he is justly proud of it. It is clever and, as physicists like to say, neat. The neatness lies in the non-obvious fact that there is a simple mathematical relation between the rate of divergence of the supercritical chain reaction and the rate of decay of the subcritical reaction. It was probably this idea that made the risk of failure acceptable when the decision was made to explode the sole existing gun-type bomb over Hiroshima without any full-scale test. Serber may well be proud that his neat idea shortened the war. And yet, and yet, this neat idea also killed a hundred thousand people. Is it wise, human nature being what it is, to let everybody know about this neat idea? If I were Robert Serber, would I want everybody to know about it?

I am not advocating secrecy. The argument is about seduction, not about secrecy. It is important to understand that secrecy and seduction are entirely separate issues. The technical information contained in this primer was rightly declassified in 1965. It would have done no harm if it had been declassified much sooner. There is nothing here that would have been technically useful to a Russian bomb designer in 1950 or to an Iraqi bomb designer in 1990. But the primer contains much more than technical information. It conveys a powerful message that bomb designing is fun. The primer succeeds all too well in recreating the Los Alamos mystique, the picture of this brilliant group of city slickers suddenly dumped into the remotest corner of the Wild West and having the best time of their lives building bombs. It helps to perpetuate the myth. Serber, in his preface written 48 years later, sketches the background to the technical debates: "Bob Wilson collected the job-lot crew of young people he'd brought from Princeton and mounted them on horses. Charlotte and I would go too, galloping across the field, Wilson's kids falling off right and left. Dust flying. The wild and woolly West." This is what I mean by seduction—the myth, unfortunately containing an element of truth, that building bombs is a wild, consciousness-raising adventure.

"The secret wartime laboratory established in 1943 to build the first atomic bombs took over the Los Alamos Ranch School, on a mesa northwest of Santa Fe, New Mexico, for its isolated site and core of existing buildings. The new volunteers called the lab 'the Hill.' " [From *The Los Alamos Primer*, courtesy Robert Serber]

There is perhaps an analogy between building nuclear weapons and manufacturing an illegal drug such as LSD. Nuclear weapons and LSD are both highly addictive. Both have been manufactured extensively by bright young people seduced by a myth and searching for adventure. Both have destroyed many lives and are likely to destroy many more if the myths are not dispelled. In both cases,

technical secrecy is not the issue. The chemical formula and the recipes for the manufacture of LSD are in the public domain, as are the scientific facts and most of the engineering principles underlying the manufacture of nuclear weapons. In both cases, what we need to do is not to keep the technical processes secret but to make them boring. We need to make it clear to everybody that the manufacture of LSD and nuclear weapons is now a routine commercial business, no longer offering a serious intellectual challenge to bright young people. Books that present either LSD or nuclear bombs as a romantic adventure can be a danger to public health and safety.

We are here confronting an ethical dilemma that is at least 350 years old, the same dilemma that John Milton confronted in his historic battle for the freedom of the press in 17th-century England. Milton in his famous appeal with the title "Areopagitica," addressed to the English parliament in 1644, conceded to his enemies the point that books "are as lively and as vigorously productive as those fabulous dragon's teeth, and being sown up and down, may chance to spring up armed men." He conceded that the risks of letting books go free into the world could be lethal as well as irreversible. He argued that the risks must still be accepted, because the censorship of books was the greater evil. He lost the argument, and in his day the censors prevailed. In our day, the censors have lost their grip, but the ethical dilemma remains. Books have not lost their power to spring up armed men, to seduce and to destroy. The fact that this primer was declassified 26 years ago does not mean that we can spread it over the world without some responsibility for the consequences.

Perhaps I am making a mountain out of a molehill. If Serber should ever read this review he would probably say, "Shucks. It's not such a big deal." And perhaps he would be right. I hope so. With luck, this charming little book will be read only by elderly physicists and historians, people who can appreciate its elegance without being seduced by its magic.

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Macfarlane Burnet

The Seeds of Time. The Life of Sir Macfarlane Burnet. CHRISTOPHER SEXTON. Oxford University Press, New York, 1992. x, 301 pp. + plates.

This is a biography of one of the 20th century's great biologists, whose influence was felt far beyond the bounds of his chosen fields of endeavor, virology and immunology. It is an authorized biography, but unlike many of its genre it does not attempt to idolize or to deify its subject; the warts and blemishes are presented fairly. Indeed, Macfarlane Burnet's scientific accomplishments need no hyperbole. Trained in medicine in 1920s Australia, Burnet, innately shy and lacking in social graces, shrank from contact with patients and sought the isolation of the research laboratory. He early decided that he would accomplish great things, and hard work and a genius for generalization beyond the narrow bounds of the immediate problem vindicated this view. If Burnet had stopped with his work in virology and especially with influenza virus, he would have earned a place in the pantheon of biomedical scientists. His work on the genetic recombination of influenza virus and on bacteriophage in lysogeny helped to start the molecular biological revolution. For this work Burnet, still in his 40s, was knighted and received the Order of Merit and, among other awards, the Royal Medal of the Royal Society and the Lasker award.

Interest in the practical applications of his research led Burnet to an interest in the immune response to viral infections, and the Darwinian biologist quickly became unhappy with the Lamarckian immunochemical theories of antibody production then extant. As early as 1941 he sought to bring biology back into immunology with his book *The Production of Antibodies*. But it was his 1949 revision of this book with Frank Fenner that placed him in the forefront of immunology. In it, with great imagination and prescience, he integrated into an ontogenetic theory the recently described (but not yet named) phenomenon of immunological tolerance, with broad predictions that Peter Medawar and colleagues verified experimentally. For this Burnet and Medawar shared the Nobel Prize in 1960. In a 1955 paper by Niels Jerne, Burnet saw the seeds of a grand solution to the problem of antibody production, which took the form of his clonal selection theory of 1957–59. Like so many of Burnet's speculations, this one stimulated a generation of researchers, and clonal selection has become the dogma of modern immunology.

The author of this book is a lawyer by vocation and a biographer by avocation. This

Vignettes: Problems in Teaching

The University of Avignon, in 1650, found itself faced by a candidate for the doctorate who had capacity but who had applied himself less closely to the pursuit of knowledge than to less exacting and more exciting extra-curricular activities. After some hesitation, it conferred the doctoral degree on him with the notation *sub spe futuri studii*, which I am told can be translated as "in the hope of future study."

—*Jacob Viner*, as quoted by William G. Bowen and Neil L. Rudenstine in *In Pursuit of the PhD (Princeton University Press)*

When I retired . . . I was asked to teach in two universities . . . . I seemed to be a big success. But I was a "big success" in a way I found extremely dangerous. The students saw me, at the end of my life, working on very general problems and making observations about every discipline, be it social, medical, or scientific. Immediately they wanted to do the same thing, to come to grips from the start with problems on a worldwide scale, without being willing to work before thinking. Yet I'd tell them every day, "I want to stress that for forty years I was the most disciplined microbiologist possible, and not until I realized that I'd mastered that discipline could I permit myself to look at it from the outside." But they simply wouldn't accept my explanation. I believe that from then on I began to feel that I was a bad influence on them. Because my courses were going too well, I gave up teaching and from then on gave no more courses of that sort.

—*René Dubos*, in *The World of René Dubos: A Collection from His Writings* (Gerard Piel and Osborn Sergerberg, Jr., Eds.; Holt)

Even in this blasé age, bright young students in elementary physics classes, after hearing about electromagnetic waves, will ask the rude question; "What is waving?" and the better the instructor the less answer is provided.

—*Lawrence B. Slobodkin*, in *Simplicity and Complexity in Games of the Intellect* (Harvard University Press)