reestablish the close wartime Anglo–American cooperation, and the repeated rebuffs of these efforts by the American government. These rebuffs apparently had multiple origins: just plain chauvinism; the peculiar kind of isolationism, aimed especially at foreign intellectuals, that was engendered by the Cold War; the revelations about spies; McCarthyism and its formative attitudes, and concern over potential commercial advantages in a world which apparently would eventually have to turn to nuclear power to meet its growing energy demands.

Volume 1 does not, however, claim that these rebuffs had very much to do with the British decision to build their own A-bomb and thus achieve an independent deterrent. Rather, the British A-bomb program seems to have had—in words used two decades later in connection with another related matter—a "mad momentum of its own."

It seems to have been tacitly assumed from the beginning that there would be a British A-bomb, and every time a decision point was reached in the technological program, the decision appears always to have been made in the way that such an implicit assumption demanded. The immediate postwar decision to build the "Windscale" reactors was made because it was believed that any respectable nuclear program must include the construction and operation of large reactors, but the type and size were from the beginning matched to the American reactors at Hanford and hence were capable of producing weapon-grade plutonium in amounts such that the output of one pile would be sufficient to make 15 bombs per year. The immediate Ministerial-level decision (December 1945) was to build one reactor and prepare the basis for an eventual second, but on New Year's Day in 1946, the British Chiefs of Staff issued a report saying "clearly two piles are better than one, and in the number of bombs we have will lie our strength."

It was not until January 1947 that it finally became necessary to decide whether or not to actually design and build bombs. A very small group, surrounded by a wall of secrecy apparently substantially higher than what we are accustomed to in America in peacetime, made the decision on very general and fundamental grounds, including simply "a feeling that Britain as a great power must acquire all major new weapons." Among those few persons in on making these decisions, and among the larger circle of those in a position to discuss the question even though they were not directly involved in making the decisions, there seems to have been very little opposition. Gowing names only Patrick M. S. Blackett as being firmly opposed, and only Sir Henry Tizard as expressing any doubts.

Later in 1947, William Penney was placed in charge of the bomb-development program. He had spent the war years at Los Alamos and had participated in the American tests at Bikini in 1946. Immediately after the war he had been named director of the Ministry of Supply's Armament Research Establishment in Kent. This organization was not then engaged in developing nuclear weapons, but Penney's being appointed to this post must have been in anticipation of its later expansion into this field. All of these decisions led directly and, in the main, smoothly to the 1952 test of the first British A-bomb. The only major surprise seems to have been the prior explosion of the first Soviet A-bomb in August 1949; the British had definitely expected to be number two and were quite surprised to find they weren't.

Volume 2 recounts mainly the stories of three men and the institutions they headed—John Cockcroft and the Atomic Energy Research Establishment at Harwell; William Penney and the Atomic Weapons Laboratory at Aldermaston; and Christopher Hinton and all of the production facilities including the Windscale reactors. The stories include many personal matters as well as strictly professional and technological events, and these will be of interest to anyone who either has or is importantly involved in the development and management of nuclear energy.

Three other interesting persons also appear in this volume: The spies Klaus Fuchs and Alan Nunn May and the defector Bruno Pontecorvo. The information presented in this volume on these three men is by comparison quite brief, but even so there is more here than will be found in any other equally well informed source.

Finally, I must note that Canadian scientists especially will be interested in the full and detailed account of the very important Canadian–British atomic relationship, which began during the war and has continued ever since.

HERBERT F. YORK
University of California, San Diego
La Jolla

All in Our Time: The Reminiscences of Twelve Nuclear Pioneers

J. Wilson, ed.
236 pp. Bulletin of the Atomic Scientists, Chicago, 1975. $3.45

This collection of personal reminiscences, *All in Our Time,* describes the discoveries and events that culminated in the Trinity test at Alamagordo, New Mexico. It comes at a time when the spread of nuclear-energy production poses, in many people's minds, a threat to humanity. Almost anyone reading this little volume—and it is very readable—has to be impressed with the inevitability of the successive discoveries leading up to the Bomb and to nuclear power. In the same vein, one may find cause to wonder if there really is any way to turn the clock back or to prevent the imaginative use of this available source of energy.

Jane Wilson has very ably edited the contributions of twelve nuclear scientists into a story that spans the decade that saw the birth of the "atomic age," and her own introductory contribution successfully ties together the separate essays that follow. While the authors are all highly

Metal quonset huts and wooden boardwalks made a crude backdrop for the physicists who ushered in the atomic age at Los Alamos. Photo from Fermi Documentary Film Collection, AIP Library.

successful scientists in their own rights—including one Nobel laureate—and certainly contributed significantly during this period, here they describe their youthful experiences working with some of the great physicists of that era: Ernest O. Lawrence, Leo Szilard, Enrico Fermi and Robert Oppenheimer.

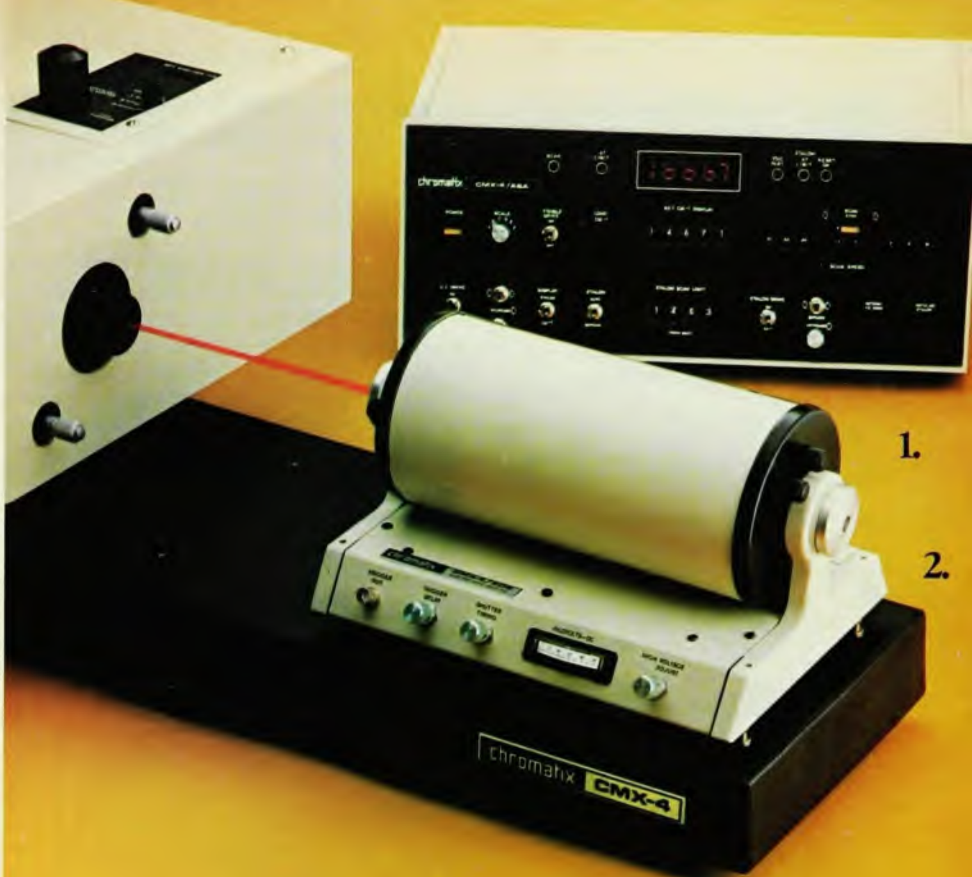
Beginning with the Berkeley laboratory of Lawrence in the 1930's, Luis W. Alvarez, Philip H. Abelson and Martin D. Kamen successively describe the atmosphere of long hours and exciting discovery in what Kamen calls "the birthplace of big science." In this connection Alvarez writes "I am convinced that Ernest Lawrence's greatest invention was not the cyclotron, but the modern way of doing physics in cooperative teams." Both the cyclotron and the team-science concept were carried to all parts of the country as the young Berkeley scientists matured and moved on.

Otto Frisch shows that the Europeans were immediately aware of the implications of the fission discovery, and he describes the formation of the Maud Committee in Great Britain for studying the possibility of a nuclear weapon. His account of how the British Commission came to be organized and moved to the United States and Los Alamos to contribute to the wartime Manhattan Project is particularly interesting.

Herbert L. Anderson and Albert Wattenberg cover the early period at Columbia, when both Szilard and Fermi were eagerly pushing the chain-reaction concept, and then on to Chicago, where the historic chain reaction was finally achieved. From there Anderson moved with Fermi to Los Alamos in time to participate in other experiments, including the final Trinity test at Alamagordo.

How the enormous jobs of building a laboratory at the remote mountain site of Los Alamos, moving equipment to it and recruiting the personnel to man it were accomplished is humorously related, with many anecdotes, by John Manley and Robert R. Wilson. Secrecy was part of the flavor of Los Alamos. So were a sense of research excitement and a oneness of purpose. Many different aspects of the Los Alamos scene are brought out in the essays by Manley, Wilson, Frederic de Hoffman, Boyce McDaniel, Val L. Fitch and Kenneth T. Bainbridge.

All in all, this was an exciting period in which to be working in physics. Those who shared the experience of the Manhattan Project, and particularly the Los Alamos adventure, were indelibly marked. The camaraderie that existed in this closely knit, isolated wartime community—where one's next-door neighbor could be—and probably was—a Fermi, Bethe, Teller, Oppenheimer, Weisskopf or Bacher, and where the younger staff were of the caliber to become the Feynmans, Wilsons, Andersons and Chews of today—had to influence deeply all par-



ticipants involved in such a major task.

These personal accounts are, of course, not history, but they do record for the benefit of historians and the rest of us insights into a few of the many facets of the Manhattan Project and the events leading up to it. And the authors have managed to convey to the reader the sense of urgency and intellectual excitement that drove the pioneering atomic scientists to initiate the atomic age.

MARY ARGO
HAROLD ARGO
Los Alamos Scientific Laboratory
New Mexico

An Introduction to Invariant Imbedding

R. Bellman, G. M. Wing
250 pp. Wiley, New York, 1975. $18.95

This text constitutes another welcome volume in the Wiley Interscience series. Richard Bellman and G. Milton Wing take their readers to the forefront of a fascinating region of research conducted in mathematics and applied disciplines, including among others physics, statistics and engineering.

The first three chapters develop the theory and are accessible to anyone with a knowledge of differential equations and some applied mathematics. These introductory chapters lay the foundation for the following more advanced subject matter, which for the most part has been available mainly in research journals up to the present. Here, invariant imbedding methods are employed to discuss problems formerly attacked by classical techniques.

Though the notation is not always standard, I do not know of any other book in this field written so lucidly and containing such a variety of material. The book develops into a remarkable text as the authors progress. The method of invariant imbedding has been discussed by others, but few have focused entirely on the subject matter as have Bellman and Wing.

The authors present their ideas most effectively by means of clear definitions and numerous applications. Theory and computational procedures are combined and given equal emphasis. From the very beginning Bellman and Wing explain precisely what they intend to do. Such topics as wave propagation, time-dependent problems, random walks, transport theory, radiative transfer and integral equations are given special emphasis. There are a number of well chosen problems, references and a summary at the end of every chapter. Among books on invariant imbedding available in English today, this book represents a precise and valuable account. In a letter to me, Bellman states his desire to use it as a text, and indeed it appears suitable for the curriculum of any inquiring university. I highly recommend *Invariant Imbedding* either as a text or as reference material.

DAGMAR R. HENNEY
George Washington University
Washington, D.C.

book notes

Topics in Applied Physics, Vol. 7: Integrated Optics. T. Tamir, ed. 315 pp. Springer-Verlag, New York, 1975. $34.40

In her article in this issue of PHYSICS TODAY, Esther Conwell surveys the development of integrated optics, or, in her words, "the use of microwave techniques at optical frequencies to perform such functions as transmission, modulation, switching, mixing and upconversion . . . [and] . . . miniaturization of components such as lasers, modulators, detectors . . . [and] . . . integration of different optical functions on the same substrate." In this book, referenced by Conwell in her article, Theodor Tamir of the Polytechnic Institute of New York, Brooklyn, has brought