Book Reviews

The Beginnings of Science Advice at the White House

Sputnik, Scientists, and Eisenhower. A Memoir of the First Special Assistant to the President for Science and Technology. JAMES R. KILLIAN, JR. MIT Press, Cambridge, Mass., 1977. xxiv, 316 pp., illus. $14.95.

On 4 October 1957 the Soviet Union put into orbit a satellite weighing 184 pounds. The timing and magnitude of this event produced a much more profound shock in the United States political community than the Administration had anticipated, and one of the actions undertaken by President Eisenhower to allay the fears of imminent Soviet technological and military superiority was to appoint James R. Killian, Jr., as his first Special Assistant for Science and Technology. Killian has documented his experiences and actions during his tenure in the office in a scholarly book produced without benefit of a diary, which, following advice from White House colleagues, he refused to keep.

It is interesting to contrast this book with that of Killian's successor as science adviser to President Eisenhower, George Kistiakowsky, who has recently published his diary under the title *A Scientist at the White House*. Killian's account is historical and detailed, and where criticism is intended it is implied delicately. Nowhere in Killian's book are the personality conflicts that swirled around such critical issues as the nuclear test ban initiatives of 1958 and the nuclear propelled aircraft presented in their full depth. In contrast, Kistiakowsky's book, being in essence an annotated diary, reflects the full feelings of the writer and contains criticisms of his adversaries in frequently harsh terms.

It emerges clearly in Killian's book that the creation of the President's Science Advisory Committee (PSAC) and the position of Special Assistant to the President for Science and Technology stemmed entirely from concern with military security. Sputnik, as may be recalled, was viewed less as a first step to the conquest of space than as a symbol of conjectured Soviet superiority in rocketry, with its consequent military implications. Only during the later phases of Killian's incumbency did PSAC become deeply involved with science education and with the general health of science, and such issues appear only in the later parts of the book.

Killian presents the role of PSAC and his own role as science adviser in essence as a success story, and he quotes proudly Eisenhower's expressions of confidence in this mechanism by a reference to "my scientists."

Killian's account gives much insight into the workings of the White House during the Eisenhower years. Since the largest part of Killian's responsibility related to decisions concerning military hardware and the beginnings of the space program, it is only natural that most of the account deals with his encounters with military personnel and members of the defense establishment. Of particular interest is his account of the role of PSAC in arms control. At that time PSAC and the office of the Special Assistant for Science and Technology were the only agencies in government focusing specifically on the means by which the evolution of military technology might be limited by international agreement. While initially the nuclear test ban was the only topic considered under the heading of arms control, it became clear to Killian and PSAC that the total amount of staff work required in support of serious arms control would soon exceed the Special Assistant's resources, and moreover that the political component in arms control demanded that science should not be the primary focus of the effort. Accordingly under Killian's leadership the seeds were sown to establish a separate agency dedicated to arms control, but it was left to Killian's successor, Kistiakowsky, to complete the difficult negotiations among the agencies of government to establish what is now the Arms Control and Disarmament Agency.

If any criticism can be made of Killian's presentation of the various events that took place during his incumbency it is that he does not find any significant fault with the role played by PSAC and the Special Assistant for Science and Technology in those years. Although I agree fully that on a large majority of counts the science advisory mechanism as initiated under Eisenhower was a success and met its original objective of introducing a voice of sanity and moderation into the heated criticisms of the American scientific, educational, and engineering endeavor that Sputnik engendered, there are specific episodes where PSAC's actions were less than perfect. Let me give but one example, an event discussed by Killian in detail.

The convening in 1958 of the "conference of experts" was to lay the technical groundwork for verifying compliance with a future treaty on the cessation of nuclear tests. The conference reached conclusions about technical means to detect and identify nuclear explosions under a variety of conditions. These conclusions naturally had to be based on only limited data from past nuclear events. Yet there remained a substantial ambiguity never understood, let alone identified in the political arena, about the meaning of the report of the conference. In later negotiations the Soviets maintained that the report was an "agreed document" intended to form the basis of subsequent political negotiation. By implication the United States treated the conference output simply as a summary of technical status, assuming that the findings would be updated as more data accumulated. Killian proudly cites in his book the much-publicized "new data" introduced in late 1958 and 1959, which turned out to be in quantitative disagreement with the information that had formed the basis for some of the conclusions on seismic detection reached by the conference. These new data were immediately transmitted by PSAC to the President with a detailing of their potentially destructive implications with respect to the possibility of reaching a test ban agreement with the Soviet Union. Consequently, the United States insisted on a reconvening of further technical working groups to update the work of the conference of experts on the detection of nuclear explosion in space and to revise the conclusions of the conference in light of the new data. It appears in retrospect that the move to revise the conclusions of the 1958 conference, which Killian recites with pride, was premature and that the implications of the new data with respect to the verifiability of compliance with a test ban treaty through seismic detection were greatly exaggerated. What should have been recognized at the time is that the variable nature of conditions and locations relating to the conduct and detection of nuclear explosions produces

large scattering in the expected data. Killian's account of this episode does not do full justice to the meaning (or lack of meaning) of these new data on the one hand, or to the raging controversy about the desirability of reaching agreement on a nuclear test ban on the other.

Killian's account does not stop with his resignation from office in May 1959. He outlines briefly the role of his successor in advising President Eisenhower, and he dedicates an entire chapter to his perception of the Eisenhower years, in particular with respect to Eisenhower's relations with the "military-industrial complex"—the famous phrase coined in Eisenhower's farewell address. It is in this summary chapter that one sees the emergence of Eisenhower's worries that "intemperate technological fantasies" would drive military and other initiatives and the recognition of his reliance on his science adviser to exert a moderating force.

The book closes with a brief summary of the science advisory situation after Eisenhower left the presidency. The decline of the science adviser's role beginning with the late Johnson years and culminating with Nixon's Executive Order abolishing PSAC, the Office of Science and Technology (OST), and the post of Special Assistant for Science and Technology altogether is well known. Killian played a substantial role in the successful persuasive effort, both in Congress and in the Executive Branch, to reestablish a science advisory at the presidential level in some form. He proudly recites the events that led to President Ford's reestablishment of a science advisory mechanism in the form of the Office of Science and Technology Policy in May 1976, and he praises the qualifications of the first incumbent of that office, his colleague Frank Press.

In his concluding chapter Killian lists the arguments for science advice at the White House level. Here again, if a fault can be found in his account it would be that he fails to mention the deficiencies in the PSAC mechanism that became apparent in its later years. In the Killian and Kistiakowsky era of science advice PSAC was a coherent deliberative body, consisting almost entirely of physical scientists and dealing with problems almost exclusively in the military and space fields. In the later years an increasing number of problems having to do with environment, medical care, and social concerns led to the introduction of a wider spectrum of disciplines to PSAC. Although this was a clear necessity, it did mean that the cogency of PSAC deliberations on any one subject suffered. Killian's account emphasizes the positive aspects of the OST-PSAC mechanism exclusively.

In summary, this book is an eminently readable historical account of the first period of the Office of the Special Assistant for Science and Technology to the President and, in less detail, of the subsequent fall and rise of the science and technology advisory mechanism in the White House. The account gives many important insights into historical events. It highly accents the positive, with respect to the achievements of the science advisory mechanism and the contributions of individuals; for this reason some of the conflicts of those years and the deficiencies in administrative leadership in technology tend to be less well presented than the constructive moves that were made in response to the gloom about U.S. science that prevailed immediately after Sputnik.

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Early Geology Reexamined

The Making of Geology. Earth Science in Britain 1660–1815. ROY PORTER. Cambridge University Press, New York, 1977. xii, 288 pp. $18.95.

Current historical ideas about the origins of geology can be traced to a great extent to views expressed by geologists of the early 19th century. Acutely aware of their membership in a recently matured discipline, geologists of the early 1800's frequently contrasted the happy new state of their science with the less fortunate antecedents it had so recently supplanted and (so it seemed) repudiated. From their judgments emerged a historical picture of earlier, would-be geologists misguided by subservience to theology or rash speculation or both. It was as if these inquirers of previous centuries stared geology in the face but failed to recognize it because they could not clear away erroneous preconceptions and inappropriate methods.

We have been learning lately to read the 19th-century histories more skeptically, to see for example that Lyell's account of geology's past in his *Principles of Geology* was designed more to justify and support his own partisan vision of the proper aims and methods of the field than for any other, more purely historical purpose. Meanwhile fresh studies of many aspects of geology's formative period have been undertaken, but with results that all too often receive little attention beyond that given by a small band of specialists in this area of the history of science, perhaps in part because these studies have not for the most part been drawn together in a form calculated to attract the attention or serve the needs of others. Porter has changed this. His book, which is both a well-informed synthesis of recent scholarship and an original interpretation with its own special insights and emphases, goes far toward providing a newly integrated historical understanding of geology's emergence as a science.

As the title indicates, Porter's study confines itself to the British, but this limitation helps make possible what is perhaps the most interesting and valuable feature of the book, its consistent focus on the social as well as the intellectual currents out of which geology materialized. With much erudition and historiographic sophistication Porter locates the various earth scientists of his period within their cultural contexts. This is no easy task, as it requires an awareness of shifting patterns in virtually all dimensions of British society (religious, philosophical, political, economic, technological, and so on), as well as familiarity with problems intrinsic to a science of the earth. By joining together social and conceptual perspectives, Porter has taken an approach that, though not new in the history of science at large, has never to my knowledge been attempted in a work of comparable scope in the history of geology. On the whole the attempt is very successful.

Among Porter's arguments a few of the more central and interesting are the following contentions: The natural history of the earth tended to move from the descriptive study of objects, through their examination in relation to specific geographic-lithological localities, toward their coordination in a larger regional-geological framework. This broadening of natural history's scale of vision was accompanied by a narrowing of the scope of "geocosmic physics" from the whole earth to just its crustal parts and by an increasing sense of understanding natural history through "an organized system of forces and products" rather than as a static inventory of things. A unified cognizance of an overall stratigraphic structure in the earth's crust was achieved only between the late 17th and the mid-18th centuries, and an increasing concentration on the significance of strata then came to be decisive in the intellectual reformulation that resulted in geology. Earth science experienced im-