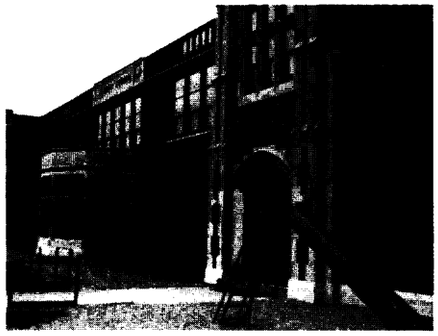
vides combined with an identification of those key concepts and ideas within a discipline that Bruner liked to call structure. The supremely contextual nature of classroom practice was underappreciated if not ignored altogether. Finally, and probably most important, there was the belief that educational reform could be achieved by being developed at the outset in the rarified atmosphere of Cambridge and then simply disseminated to schools across the country, provided, of course, that the program was accompanied by appropriate teacher training and materials. One of the participants in the MACOS project characterized this problem as getting "from Widener to Wichita."



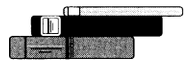
A school in Wichita. [Courtesy of Sheri Canfield]

Indeed, Dow uses that as the title of one of the chapters in his book, but that chapter is mainly an account of how the teacher training programs for MACOS were developed, how they were evaluated, and the difficulties the Educational Development Center, as it came to be called, encountered in attracting a commercial publisher for the materials that were developed. Though these elements of the story carry their own significance and are of some interest, Dow fails to provide a rigorous analysis of what has come to be called the "top-down" model of curriculum reform, and this is the book's principal weakness. Although Dow repeatedly makes a point of calling MACOS a cooperative endeavor between academic scholars and teachers, that cooperation consisted essentially of using carefully selected teachers to test certain ideas advanced by academic scholars in the crucible of specially designed classrooms.

Despite some initial success, MACOS, according to Dow's account, was sabotaged by a combination of right-wing citizens' groups and congressional suspicion that federal funds were being used to convey subversive ideas, or at least values contrary to those of mainstream America. These debates over values were a particular function of the anthropological character that MACOS assumed (including elements of cultural relativism) over the course of its development. Dow's account of this political infighting, which ultimately involved congressional inquiries into NSF's competence to manage educational programs, is genuinely intriguing in its own right, but it does not serve to explain why other curriculum reform projects of the post-Sputnik era declined almost as precipitously. Zacharias's notably successful and generously financed Physical Sciences Study Committee, for example, did not engender anything like the political controversy that MACOS did but faded just as completely.

The lessons that Dow derives from his MACOS experience revolve for the most part around his self-confessed political naïveté as well as that of his colleagues. In the context of the distorted and even vicious attacks that MACOS had to endure, however, political naïveté comes through as a virtue. It is more likely that the post-Sputnik curriculum reforms failed because of naïveté of another sort. It was a naïveté about the great cultural divide that exists between the heady but contrived atmosphere that pervaded the curriculum laboratories in Cambridge and other development sites on the one hand and the everyday realities of schooling in Wichita and the rest of the country on the other. There is no reason to believe that Dow is mistaken in identifying a politically conservative backlash as the immediate cause of MACOS's downfall, but MACOS is likely to have suffered such a fate anyway by virtue of the cultural dissonance that was almost inevitable given the "Zacharias model" of curriculum reform. By treating schools and teachers essentially as consumers of external initiatives instead of partners in a common enterprise, the curriculum reform programs of the post-Sputnik era were probably doomed from the start.

Herbert M. Kliebard
Departments of Curriculum and Instruction and Educational Policy Studies, University of Wisconsin, Madison, WI 53706



Absences from the White House

Cardinal Choices. Presidential Science Advising from the Atomic Bomb to SDI. GREGG HERKEN. Oxford University Press, New York, 1992. xiv, 317 pp. $24.95. A Twentieth Century Fund Book.

To what extent have American scientists been appropriately involved in advising the nation's leaders concerning what C. P. Snow called the "cardinal choices" of government, those "choices that in the broadest sense determine whether we live or die"? Gregg Herken, newly of the Smithsonian Institution and author of two previous books on related topics, here offers us impressive evidence that, when crucial technical issues have arisen during the last 50 years, the contribution of scientists to executive decision-making has often been inadequate. For reasons of institutional weakness and presidential ignorance or bias, representatives of the mainstream in American science have frequently been unable to reach the political leadership when that leadership needed them most.

Some presidents have done better than others. Dwight Eisenhower in particular should be given credit for having established the post of national science adviser and the President's Science Advisory Committee (PSAC) in 1957, thereby creating the first formal and systematic channel between scientists and the Oval Office. Yet in the decade following PSAC's creation, controversy over the Vietnam war and the struggle over the antiballistic missile led Lyndon Johnson and then Richard Nixon largely to turn their backs on PSAC and the presidents' scientists. Relations between the White House and the scientific community reached their low point in 1973 with Nixon's abolition of both PSAC and the office of science adviser. Since then, despite numerous proposals for reestablishing a PSAC-like entity for the chief executive, only partial and insubstantial actions have been taken to improve the way in which science advice reaches the president. Indeed, beginning with the creation of the Office of Technology Assessment by the Congress in the early 1970s, the process itself has become increasingly fragmented among competing branches of government.

Building from extensive interviewing of the scientists involved and from a careful combing of declassified records, Herken weaves his story of the science-government relationship from the Roosevelt era to the present, focusing primarily on presidential policy with regard to nuclear weapons but in later years on environmental and other issues as well. Despite the present-day fame of the initial Albert Einstein–Leo Szilard letter to FDR in 1939 about the possibility of constructing an atomic bomb, the lack of a dependable means of communication at that time made it surprisingly difficult for

those at the forefront of nuclear research to convey a sense of urgency regarding the international danger to the president. Later, after the war, the haste with which the Truman administration dismantled the Office of Scientific Research and Development (OSRD) and other wartime agencies left even such leading scientific spokesmen as Vannevar Bush and James Conant without any significant voice as to the postwar organization of atomic energy. At the end of the 1940s Truman virtually ignored the opposition of the scientific advisory committee of the Atomic Energy Commission (AEC) in making his decision to proceed with the H-bomb, listening much more seriously to the opinions of the Joint Chiefs of Staff and what Herken calls the "H-bomb lobby" led by Edward Teller.

Eisenhower, despite a sincere desire for more public candor about the developing arms race, was severely handicapped at the beginning of his administration by Robert Oppenheimer's fall from grace (the result of unsubstantiated charges) and by his enforced reliance on the extremely cautious Lewis Strauss, chairman of the AEC but a nonscientist who mirrored the views of Teller's Livermore Laboratory. By mid-decade, however, the shock of the Soviet H-bomb (November 1955), ICBM (August 1957), and Sputnik (October 1957) had reopened the door to those critical of Teller's pessimism and prompted the president to rethink the role of scientists in his government. The creation of PSAC and appointment of James Killian as science adviser (November 1957) signaled the arrival of a new dispensation, an arrangement in which the president and Killian consciously expected PSAC to serve as a counterbalance to the AEC, the national labs, and the military. Ike and the "president's scientists" set their sites on a new goal—a comprehensive nuclear test ban—and for the remainder of Eisenhower's second term the battle raged between those who believed this a manageable risk (the PSAC group) and those (like Teller) who were determined to block it.

The 1963 Limited Test Ban Treaty, though a compromise on the part of the Kennedy administration in the face of significant opposition to a comprehensive treaty in Congress, the Pentagon, and the labs, marked the culmination of a six-year effort by PSAC and three science advisers to win the public to a reformist course. Nevertheless, such newfound activism became a two-edged sword when it subsequently led scientists to oppose LBJ on such issues as defoliation, bombing, and the ABM. With the departure of Secretary of Defense Robert McNamara in 1968, PSAC lost its last real audience in the Johnson inner circle. By this time the administration was relying for advice almost entirely on professional military scientists and those in the aerospace industry.

The author's treatment of the Nixon, Ford, Carter, and Reagan years is somewhat hurried, but he makes his case nonetheless. Though Henry Kissinger talked on occasion to scientists from the civilian world, the fact is that the president's science adviser (Lee DuBridge) and PSAC were almost entirely excluded from the debate on Safeguard, MIRV, and SALT. What is more, following PSAC's demise in 1973, its alumni played little or no part in the major revision of American nuclear strategy that took place under Defense Secretary James Schlesinger. Things were not much better under Jimmy Carter, who by making his secretary of defense (Harold Brown) his "physics adviser" and selecting a science consultant who did "everything but defense" robbed himself of the institutional weight he needed to force through a Comprehensive Test Ban (CTB) or to resist a decision in favor of the land-based (rather than the smaller, air or sea-based) MX missile. With Reagan the situation was even worse, his distrust of the scientific community leading him, first, to select (at Teller's instance) a relatively unknown scientist (George Keyworth) as adviser and then, alternately, to forget about him and to expect him to act as cheerleader for such programs as the MX "densepack" deployment and "Star Wars" (SDI).

One of the most fascinating revelations of Herken's study is the way in which Edward Teller repeatedly turns up at critical moments in the history of America's involvement in the Cold War. To judge from the material here, it is not too much to describe Teller as one of the truly central personalities in perpetuating international tension between East and West during the last half century. His career and role in shaping United States foreign policy cry out for intensive investigation.

In *Cardinal Choices* Gregg Herken has produced a thoughtful and comprehensive survey of a significant relationship and at the same time a convincing plea for change. To be sure, this study is quite consistently presented from the standpoint of the "excluded" scientists; it is their testimony that Herken relies on and not the papers of, or interviews with, the "political" leadership of the successive presidential administrations. Still, it is hard to quarrel with the author's essential argument: that the expertise of independent scientists is badly needed by statesmen not only with regard to domestic issues (where George Bush would prefer to get his scientific advice) but also on matters affecting national security and defense. One must hope that there will be a clearer recognition of the need to institutionalize in an effective fashion the process by which this knowledge is proferred.

Keith L. Nelson
Department of History, University of California, Irvine, CA 92717

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Missile Defender

Teller's War. The Top-Secret Story Behind the Star Wars Deception. WILLIAM J. BROAD. Simon and Schuster, New York, 1992. 350 pp., illus. $25.

Edward was "just being himself." Repeated by friend and foe alike, this phrase seems to capture the essence of the brilliant, influential, and, more times than not, controversial nuclear scientist Edward Teller. Today in his mid-80s, Teller seems to be more active than ever, throwing out ideas at a rapid-fire rate. Taken back a bit by the revolutions of 1989 and 1991, Teller—the staunch anticommunist and technological enthusiast—now appears to have gotten his bearings in the post–Cold War world. In the past months, he has made headlines by promoting the old Baruch Plan for the international control of atomic energy, supporting increased technical trade with and economic support for the FSU (one favorite new acronym used by U.S. government officials to refer to the former Soviet Union), and postulating that nuclear explosions might be used to deflect the path of asteroids threatening the earth.

But, as William Broad points out in his entertaining, well-researched, and sometimes enlightening volume, Teller does not give up easily on some of his favorite ideas and projects. Certainly falling into this category is the primary subject of *Teller's War*, the use of promising new technologies to promote missile defense. Broad, a science reporter for the *New York Times* since 1983 and before that a writer for *Science*, is well trained to deal with such esoteric topics. Using his extensive experience and contacts, Broad has done an outstanding detective job in describing the origins and development of the Strategic Defense Initiative (SDI), or "Star Wars," as it is popularly called, and the motivations and roles of the numerous individuals involved in the program.

This is a fascinating human as well as technical story. Broad tells it well, yet his intensive focus on Teller himself sometimes distorts more than it illuminates. There is no question that Teller had a special relationship with and influence over SDI's god-