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AMERICAN NATURAL SCIENTISTS IN THE POLICY PROCESS: THREE ATOMIC ENERGY ISSUES AND THEIR FOREIGN POLICY IMPLICATIONS

by

Claire M. Nader

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ABSTRACT

AMERICAN NATURAL SCIENTISTS IN THE POLICY PROCESS:
THREE ATOMIC ENERGY ISSUES AND THEIR
FOREIGN POLICY IMPLICATIONS

Claire M. Nader

This dissertation deals with the growing involvement of natural scientists in public policy problems with significant scientific-technological components. It focusses on their changing behavior as they became involved in a political process in which the veteran operators shared norms and expectations of which scientists were initially ignorant. It describes and analyzes both the constant and the developing aspects of their policy behavior as it was demonstrated in and affected by their participation in three atomic energy issues: the debate in 1945-46 surrounding the atomic energy law of 1946, the controversy of 1949-50 over the decision to build the hydrogen bomb, and the issues of peaceful uses of atomic energy during the United Nations Atoms-for-Peace conferences in 1955 and 1958.

Three central questions were posed: (1) what were the scientists' political objectives in the above issues? (2) what political strategies did they use to achieve these objectives? (3) how did the manner in which they were involved in these issues affect the kind of strategies employed?

These cases offered three different environments, over a span of thirteen years, in which scientists operated as political beings: a legislative environment, an environment of high-level executive decision-making, and a scientific environment. These settings had distinctive

effects on scientists' policy perspectives. Other factors conditioning their perspectives arose from the kinds of substantive issue handled, the time dimension of the issue, and the particular stage of the scientist's involvement in the political process.

The findings of the study indicated that scientists' political education would have proceeded more rapidly and the public interest would have been better served had they demonstrated greater awareness of the complexity of general political variables and the different manner in which political phenomena behaved, especially when perturbed by an exogenous and limited professional authority allotted these scientists. The new roles were acquired by scientists in the political process, both inside and outside of government, more easily than were the skills appropriate to a more optimum functioning of these roles.

As leading scientists increasingly shouldered joint professional and governmental responsibility, as in official advisory capacities, they began to develop a set of values, attitudes, and motivations which embraced a broader grasp and evaluation of non-scientific factors. Their hierarchy of professional values became in part a function of political realities of the decision-making process and no longer was exclusively a product of professional background.

The scope of this re-ranking of values was closely associated with the nature of their involvement in these political problems. Because atomic scientists were participants in the conflicts and deliberations over these policy issues along a broad continuum ranging from public information activities to official governmental positions, it is possible to observe the differential handling and timing of evidential and evaluative contributions by these variously positioned scientists. The situational accomodations made by scientists in how they viewed their responsibilities and commitments constituted a specialized political process which the overall polity was struggling in conscious and unconscious ways to integrate.

The findings of the study are based mostly on an analysis of the public record and archival data. Pertinent materials were found in Congressional hearings and other official documents, in writings by scientists and other individuals closely associated with the above issues. Materials about scientists, science, and government and commentaries in newspaper and periodicals were also examined. Direct interviews of scientists were conducted.

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The task of putting ideas into written form on any subject is a demanding one. It is not, however, a solitary endeavor. I have received help from many sources, some surprisingly unexpected, all gratefully acknowledged.

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a more complete knowledge and understanding of scientists' political development. His insistence on clear expression in composition was received sympathetically and did not go completely unheeded.

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In all these ways I have benefited. However, I bear sole responsibility for any inadequacies, heartened by the fact that some of these can be remedied in the process of learning.

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PART III. SCIENTISTS AND ATOMS-FOR-PEACE: THE UNITED NATIONS GENEVA CONFERENCES OF 1955 AND 1958

INTRODUCTION

I

The present confluence of science and politics in the United States has raised many problems of persistent relevance to national security questions. This study is concerned with one of them--the growing involvement of natural scientists in political issues which have important scientific-technological components. Their influential roles in domestic and foreign policy, both in clarifying objectives and in specifying means, deserve careful scrutiny. Any group which wields such large influence as scientists do today bears close surveillance. This assertion is not intended to attribute malevolent purposes to them. Rather, it is a reminder that a preoccupation by experts with their specialized knowledge can encourage narrow and inflexible viewpoints in the policy process.[1] Scientific experts are now increasingly active in policymaking, and it is therefore important to examine their approach to political problems which crucially involve scientific-technological knowledge.

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[1] On this point, see Harold A. Innis, Empire and Communications (London: Oxford University Press, 1950), pp. 215-16.

Since 1945, scientists' fundamental professional values have been threatened openly, first by the use of the atomic bomb and, subsequently, by the contingencies which spelled protracted mobilization of energies for preserving national security. By the end of World War II, scientists acutely understood the critical relationship of their work to national power. They were keenly aware that the growing scale of scientific research and development necessitated governmental support, bringing with it conditions which would impinge on professional values.

Scientists eloquently insisted on the requirements for the practice of science. These requirements, which scientists believed that they could define best, provided for free inquiry, free thought, free speech, and tolerance of different views. Existing on the basis of these shared values, the society of scientists has demonstrated the ability to nurture an impressive degree of freedom and order simultaneously.[1] It should not have been unexpected nor unusual, therefore, for the American scientific community, accustomed to ordering itself, to insist on professional autonomy.

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[1] J. Bronowski, Science and Human Values (New York: Harper and Brothers, 1956), pp. 65-94.

At the same time, scientists were deeply concerned about the use to which their work would be put. Their participation in political decision-making was often frustrating, for the confluence of science and politics forced them into public life where the synchronization of their professional goals with those of society and the demands of government has not been easy, nor always successful.

Although there have been numerous studies concerned with the general subject of scientists and government,[1] few have chosen to analyze specifically the characteristics and evolution of their political behavior. Warner R. Schilling, in his examination of the problems connected with scientists' participation in the policy process, has considered some of their policy predispositions.[2] Albert Wohlstetter has discussed

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[1]See, for example, Don K. Price, Government and Science: Their Dynamic Relations in American Democracy (New York: New York University Press, 1954); A. Hunter Dupree, Science and the Federal Government: A History of Policies and Activities to 1940 (Cambridge, The Belknap Press of Harvard University Press, 1957); James L. McCamy, Science and Public Administration (University, Alabama: University of Alabama Press, 1960): J. Stefan Dupré and Sanford A. Lakoff, Science and the Nation (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1962).

[2]See "Scientists, Foreign Policy, and Politics," in Robert Gilpin and Christopher Wright, ed., Scientists and National Policy-Making (New York: Columbia University Press, 1964).

the limitations of certain natural scientists as experts on strategic issues that have arisen since World War II.[1] Robert Gilpin in his American Scientists and Nuclear Weapons Policy has traced and examined the political positions of scientists on nuclear weapons policy from 1945 to 1962, in terms of their effect on actual policy, intra-scientific relations, and on the relation of scientific experts to political leaders.[2]

This study is about the changing behavior of certain scientists as they operated in a political process in which the old operators shared norms and expectations of which they were initially ignorant.[3] The study examines some of the factors which affect the nature of scientists' political involvement in three atomic energy issues from 1945 to 1958. These issues all had important foreign-policy as well as science-policy consequences and stimulated considerable political interest and activity on the part of scientists. In each case, there is an extensive public record of the scientists'

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[1]See "Strategy and the Natural Scientists" in ibid.

[2]Robert Gilpin, American Scientists and Nuclear Weapons Policy (Princeton, New Jersey: Princeton University Press, 1962).

[3]It is in this sense that the phrase "political socialization" is used in this study. It is meant to indicate the process by which scientists acquired knowledge and understanding of the political process in which they were involved.

activity which is available for analysis both with respect to its representatives with regard to atomic energy affairs and to public policy in general.

The first of these atomic energy issues--the establishment of a permanent postwar administrative structure for the atomic energy program in 1945-46--revolved around a policy for scientific research and development when international control of atomic energy was still believed to be an achievable objective. The second--the controversy of 1949-50 which surrounded the decision to build the hydrogen bomb--concerned weapons development in a clearly defined international political struggle. The third--the issue of peaceful uses of atomic energy during the United Nations Atoms-for-Peace Conferences in 1955 and 1958--centered on the professional relations of scientists representing their governments at two international scientific conferences, and the effect of these relations on international politics.

The substance of each of these issues was different. The times in which they arose and the stage in the evolution of scientists' relationship to the political environment were also different. How, then, did these differences affect the education of scientists about the way in which political decisions and policies were made? What was the impact on scientists' perspectives of their involvement in the three atomic energy issues under discussion: the first two of which exposed scientists to the process of political decision-making and policymaking and subjected them to unfamiliar tasks and rules of behavior, and the third of which offered them momentary relief from politics but which may have encouraged politically unrealistic attitudes toward difficult international political problems with crucial scientific-technological components.

A measure of scientists' success in contributing to coordinated policies is found in their ability to harmonize the goals of science and social objectives. But, as the needs of society impinge more directly on the needs of science, it becomes more difficult to coordinate these goals. However, some accommodation between them is necessary in the interest of national security.

One can imagine a sequence of steps by which a scientist acquires a sensitivity to problems in accommodation. This suggested sequence is useful to bear in mind for purposes of understanding some behavior of scientists. It is not meant to indicate that this progression inevitably happens, or that it is desirable.

First, a scientist may give scientific advice with minimum consideration, if any, of what is politically feasible. Second, as he works with political leaders on problems involving politics and science, he is exposed to cooperation at a policy level with members of the political leadership and, as a result of this experience, he may begin to develop a sensitivity to the kinds of problems with which policymakers have to contend and to understand and appreciate the perspectives and procedures which are an integral part of the policymaker's working equipment. Third, the scientist may become like the policymaker. He himself may now act on his knowledge and understanding of the interplay of evidence and interests in the political process, introducing his expert knowledge and balancing his view with those of other scientists and non-scientists, views which might also be important in a decision.

The purpose of this study, then, is to describe and analyze both the constant and the developing aspects of the scientists' policy behavior as it was demonstrated in and affected by their participation in the three atomic energy issues studied. Toward this end, three central questions will be asked about their participation in each issue: (1) what were the scientists' political objectives? (2) what political strategies did they use to achieve these objectives? and (3) how did the manner in which they were involved in these issues affect the kind of strategies employed?

II

Scientists are today sufficiently influential in the determination of national security policy to be interesting subjects of study in their own right. Nonetheless, this study's concert with scientists can be related to a larger concern with the "ultimate concern of advice as it bears upon and emerges in decisions."[1] This study deals only with one important policy participant, but existing patterns of behavior of all major policy participants need to be examined to know what to conserve and what to try and change in developing a combination of skills and outlooks to promote a "balanced skill state" over a "bureaucratic state" as an encompassing response to a changing environment.[2] The achievement of this objective depends, in part, on identifying the determining factors of specialists' participation in the policy process.

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[1]Arthur W. MacMahon, Administration in Foreign Affairs (University, Alabama: University of Alabama Press, 1953), p. 3.

[2]These descriptive and suggestive terms are used by Harold D. Lasswell and Myres S. McDougal in their study on "Legal Education and Public Policy: Professional Training in the Public Interest," Yale Law Journal, 52 (March 1943), 203-295.

One student of politics has observed that "the main link between a state's long-run power potential and its government's achievement of present-day policy objectives are decisions taken regarding the level and character of military preparedness."[1] Another link, perhaps equally important, may be found in the decisions taken with regard to the level and character of scientific preparedness. Foreign policies are inevitably influenced by a nation's scientific and technological level of achievement, for national power is closely associated with the power to acquire and maintain top-quality scientific research facilities, both basic and applied. In this sense, scientific preparedness encompasses all other kinds of preparedness.

Preparedness means not only a constant advance in actual scientific and technological achievements but also the integration of these with policy objectives. As the expert possessors and transmitters of scientific knowledge, scientists occupy a strategic position in this integrative process. Although they must not attempt to govern, they have the crucial function of explaining the meaning of new developments and of indicating possible new courses of action for science to those responsible for decisions.

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[1]William T. R. Fox, "Civilians, Soldiers, and American Military Policy," World Politics, VII (April 1955), 402.

Their professional competence and their responsibility for providing relevant information and advice in the pursuit of long and short-run foreign policy objectives cannot be denied; neither the vital need for political and social planning to meet political and social necessities as they arise, not after when it may be too late and probably less effective. This requires the establishment of a frame of reference in advance of the facts for the reduction of unnecessary friction.[1] This objective, in turn, presupposes a knowledge and a continuous evaluation of the part that scientists have played in public questions of the highest order.

This also presupposes knowledge of the perspectives of other major participants in policymaking. The increased interplay of scientific, political, and military factors has inevitably involved civilian officials, soldiers, scientists, and others. A determination of their specific characteristics is now appropriate and necessary in order to evaluate their contributions to coordinated policymaking.

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[1]For this point and a stimulating discussion of the tasks facing the political scientist in a world in which science and technology challenge our best talents, see Harold D. Lasswell, "The Political Science of Science: An Inquiry Into the Possible Reconciliation of Mastery and Freedom," The American Political Science Review, L (December 1956), 961-979.

An understanding of these special perspectives may help decrease the amount of friction which can arise not so much from a recognition of differences but from a lack of such recognition. The intent would not be to merge all these vital viewpoints. Rather, it would be to note their details for a fundamental grasp of problems in policymaking and possibilities for future interplay. One qualification is in order at this point. If the perspective is one which is incapable of adjusting private wants to public security requirements, then it should be substantively modified, or, failing this, discounted.

Scientific and technological developments can modify the political situation so rapidly that policies are needed to deal with current changes and anticipate and channel subsequent ones in order to achieve desired objectives. The design of such policies requires an environment which will encourage the initiative of special interest groups within a common frame of reference.

If policy is "the making of important decisions which affect the distribution of values," and if a value is "an objective of human desire," then the values of policy participants, their means of resolving problems, and their roles have to be clarified. Clarification is needed in order to develop attitudes and habits which contribute to more useful policy enterprises.

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[1]Lasswell and McDougal, op. cit., pp. 208 and 217.

III

The limitations of the study must now be indicated. First, the term "scientific community," as used here, refers to politically articulate scientists who chose to exert influence on political problems which involved them.[1] Second, the study deals with scientists who were specifically concerned about the use of science in the three issues under scrutiny. This group, which included also such scientist-administrators as Vannevar Bush, consisted mainly of the physical or atomic scientists whose wartime experience left them deeply troubled and interested in re-establishing the lines of scientific communication which had been disrupted by the war. Third, the study is limited by the policy issues selected for analysis. It is recognized that other kinds of scientists may behave differently in other issues and at other times.

The findings of this study are therefore limited both by the type of scientist whose political behavior is examined and by the issues considered. Nevertheless, since nuclear energy continues to be important to national power, and physicists

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[1] See Wallace S. Sayre, "Scientists and American Science Policy," Science, 133 (March 24, 1961), 859-63, for a helpful discussion of who the scientists are, who speaks for them, and what their goals and strategies are.

are still influential in the determination of national security policy, studies concerned with their political behavior still have policy relevance.

Finally, this study is limited by the sources which have been investigated. For the most part, written materials were used. Among these were memoirs of or writings by scientists and other individuals who were closely associated with the issues under examination, Congressional hearings, and other official government documents. Writings about scientists, science, and government by other students of the subject and commentaries in newspapers and periodicals also provided pertiment materials for the study. Direct interviews with scientists were conducted to a limited, but useful, extent. These are specifically mentioned in the acknowledgements.

Thus, the findings of this study are based mostly on an analysis of the public record and archival data. For them to have greater precision, one would have to compare the public record with information gleaned from extensive personal interviews with scientists and non-scientists who were involved in the issues being examined. In this way, for example, one could better tell whether the behavior of a scientist is representative of his profession or is merely idiosyncratic. Differences or similarities between scientists' public and private views would give important indications of their powers to advance toward their political objectives in political decision-making. Such knowledge would further illuminate the factors which deter or advance scientists' political education.

In view of these four limitations, the findings of this study can only be considered representative of atomic scientists who were politically concerned with atomic energy issues. Moreover, even these findings remain tentative pending more extensive interviewing of scientists and non-scientists who are politically involved in atomic energy questions.

PART I

SCIENTISTS AND THE ATOMIC ENERGY ACT OF 1946

Introduction

The issue of nuclear energy drew atomic scientists into the political environment at a time of war and momentous decision-making at the highest levels. World War II ended with the destruction of two major Japanese cities by an heretofore unknown power. Among others, scientists recognized that the wartime development and use of atomic energy was to affect fundamentally the military planning and political relationships of nations. It was also to affect the scientific community profoundly, for nothing served to sharpen scientists' social and political awareness more than the atomic bombings of Hiroshima and Nagasaki in August 1945. What would their wartime contribution mean for the freedom of science and for a stable world order?

A major interest in maintaining the kind of professional environment necessary to the practice of science and a deep sense of urgency that the atom be used for peaceful ends catapulted scientists into the political arena. They entered the postwar world of affairs "prepared to assist . . . in bringing about an outcome of the present crisis of humanity that is worthy of the ideals for which science through the ages has stood."[1] One scientist observed that they were bewildered by this new activity. "Need I tell you," said Selig Hecht, "that a scientist is one who practices science? The art of persuasion, the art of influencing opinion, is not his."[2] Nonetheless, scientists proved adroit in the pursuit of their ends and instrumental in shaping policy. They were keenly aware of the challenge to their professional autonomy created by the growing conflict of national security requirements and those of the scientific enterprise.

A specialized professional environment defined the nature of this group and its special interests. Heretofore an intellectual

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[1]Dexter Masters and Katharine Way, ed., <u>One World or None</u> (New York: Whittlesey House, McGraw-Hill Book Co., Inc., 1946), Foreword, "Science and Civilization," by Niels Bohr, p. X. Hecht's words may well have reflected the scientist's position in the United States but not necessarily in European countries whose capitals encompass the political and intellectual worlds and where a high level of defense mobilization was accepted. It is conceivable that the experience of the American scientist in the political environment was shaped differently from his colleagues abroad and that, therefore, characteristics attributed to scientists whose activities are described and analyzed in this study have to be limited to American scientists.

[2]What Hiroshima Did to the Scientists," p. 9, in Emergency Committee of Atomic Scientists, Inc., <u>The Social Task of the Scientist in the Atomic Era</u> (Princeton, New Jersey: 1946). The chairman of this symposium was Albert Einstein; Harold C. Urey was vice-chairman; members were Hans A. Bethe, F. R. Hogness, Philip M. Morse, Linus Pauling, Leo Szilard, and V. F. Weisskopf.

activity which had developed semi-independently from the main stream of the social process, the study of the natural world facilitated the growth of a professional society separate from other groups in society. Scientists constitute a community of investigators bound by a code of conduct which makes possible the practice of science. The vital conditions of scientific research required independence of thought and observations, a free exchange of ideas, and, in the words of Ernest Nagel, a "refining process of mutual criticism"[1] with no regard for national boundaries.

The mobilization of science for military purposes necessitated conditions of secrecy and compartmentalization of knowledge which directly contradicted the free-ranging spirit of scientific inquiry. Furthermore, while the development of atomic energy under wartime restrictions affected adversely scientists' work habits, its military use seriously undermined the idea that scientific progress benefits mankind. On both grounds scientsts tried to protect the requirements of their profession.

Optimistic about the scientific community's ability to contribute effectively to resolving political problems and to peace, they entered the political process not only

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[1]"The Place of Science in a Liberal Education," Daedalus, 88 (Winter 1959), 61.

with technical knowledge, but they had a specific interest in insuring a wide latitude of freedom for scientific research and development and helping to determine the social uses of the atom. How, then, did the professional conditions scientists insisted upon define the organization and political environment which they wanted for domestic and international control of atomic energy? In other words, what was the relation of their professional interests to their political activity?

The effort of politically oriented scientists to affect public policy relating to atomic energy legislation and the subsequent issues of this study demonstrated a strong desire to help define national security requirements in the postwar era. As a result, they became involved in a controversy over what constituted the proper combination of the military and non-military policy instruments for achieving national objectives. Agreement on the ingredients of national security policy was not easy to come by. Policymakers and their expert advisers grappled with this fundamental issue which divided both scientists and nonscientists, especially as the international crisis deepened.

In this first postwar atomic energy question, however, scientists could concentrate on their special interests without being unduly troubled by the complexities of political factors. The substance of this issue involved establishing an administrative structure for atomic energy at a time when hopes were still high for international control. In light of this, and since the problem was more a matter of science than a question of political judgment and choice, their political response remained limited by their professional interests. Some scientists, however, who carried administrative and advisory responsibilities during the war, had had their horizons enlarged beyond the professional one. In other words, actual responsibilities directed their actions. This added experience resulted in small variations in scientists' political behavior. As political choices infringed upon them unavoidably, these differences became more noticeable and are important in evaluating factors affecting their political growth. Any evaluation must begin with an account of the scientists' wartime activities, for their participation in political decision-making began before August 1945.[1]

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[1] Pp. 21-31 draw on the official account of the efforts of scientists and policymakers toward postwar planning. Richard G. Hewlett and Oscar E. Anderson, Jr., The New World, 1939-46 (University Park, Pa., Pennsylvania State University Press, 1962), Chapter 10.

Chapter I

The Wartime Experience

I

The wartime experience of natural scientists is significantly related to their political behavior in the postwar atomic energy debate. Differences among articulate scientists and the policy issues of that debate began then, emerging as important elements in scientists' participation in the atomic energy legislative process.

Scientists served essentially in two ways during the war. One group was formally responsible for organizing science for military purposes and for advising policymakers on the military, social, and political implications of atomic energy. As a result, scientists with administrative-advisory duties developed a sense of accountability for their advice and propriety in the political arena. This exposure to the cooperative process in policymaking amounted to a halting first step in their political socialization. They had not so much a real appreciation and understanding of the political process as a smoothly working relationship with political leaders. Based on a mutual respect and trust and a loyalty to these statesmen, they began perhaps to appreciate the difficult tasks of policymakers.

The other group of scientists was responsible for laboratory work leading to military effectiveness. They were officially occupied with professional activities and had no advisory duties. This specialized function did not, however, result in isolation from political problems. On the contrary, project scientists independently presented their views on the social aspects of atomic energy. There was little opportunity to develop mutual respect between them and political officials. Indeed, they suspected statesmen of failing to grasp the awesome social meaning of atomic energy and their colleagues in government of being tarnished by political superiors, therefore losing their objectivity.

These diverging points of view obstructed useful communication between the two groups and between project scientists and policymakers and presaged the nature of scientists' subsequent involvement in policymaking. Project scientists acquired a somewhat astute grasp of the political process although their understanding of political questions left something to be desired. Their effectiveness came from an undeviating concentration on relatively uncomplicated goals relating more to scientific research than balancing political objectives. On the other hand, administrative-advisory scientists (hereinafter advisory scientists) were sufficiently exposed to the policy-making process and thus began to perceive some of the political complexities. Although both groups were in a pristine state in their political development, this nascent sensitivity to political difficulties placed restraints on the political behavior of advisory scientists. It appeared that official accountability for advice tempered their movements in a way that was not characteristic of the free-wheeling activities of project scientists in the postwar atomic energy debate.

Scientists themselves were aware of these variations. Arthur H. Compton, director of the Metallurgical Laboratory at Chicago, was particularly conscious of the attitudes and ideas of his working scientists and served as a link between them, scientists in government, and policymakers[1] and encouraged cooperation between the two groups of scientists.[2]

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[1] For example, Compton asked Groves to allow these scientists "to express their views on human applications." He also favored the authorization of semi-official statements on technical matters by the Scientific Panel and project personnel who were members of the National Academy of Sciences.

[2] *Ibid.*, p. 421. See also *ibid.*, p. 342.

Vannevar Bush, director of the Office of Scientific Research and Development (OSRD), and James B. Conant, chairman of the National Defense Research Committee (NDRC) did make honest attempts to learn the views of project scientists and make them known to Secretary of War Henry L. Stimson and others. It was not always possible, however, to act upon these views. By virtue of differing functions, laboratory scientists were alienated from those in official advisory positions where more than the health of science was involved. A satisfactory liaison proved difficult.

Although divergences in approach to political questions often occurred, scientists shared the two main concerns of this period. The questions of international control and continuous atomic energy research absorbed their energies long before the war ended and became their major postwar political objectives in the animated public discourse on atomic energy legislation. They could not ignore the interrelation of these goals for, although eager to return to their professional activities, scientists recognized that the state of international relations affected the extent and direction of scientific activity.

II

Project scientists, especially those from Chicago, were vitally interested in the dispositions of these issues and insisted on some postwar planning despite wartime pressures. One of the most vocal of this group, Leo Szilard of the Chicago Laboratory, suggested as early as September 1943, that his colleagues consider carefully the political implications of their work. In January 1944, he wrote Bush about the need for international control of atomic energy. Particularly from 1944 on, the working scientists began thinking about postwar research and development and the control problem. Indeed, it was their restless pressure upon Arthur Compton, who in turn prodded Bush, which resulted in the establishment of two committees to consider these specific questions.

The laboratory scientists were not the only ones whose eyes were scanning the horizon. The advisory scientists assumed a role which could be described neither as being on top nor on tap. Instead, they were integral and equal members of a team; with their political superiors, these scientists helped significantly in the prosecution of the war. At times, they were the only ones available to think ahead about the problems of integrating atomic energy with international and national objectives. They did not wait to be asked. Recognizing that Stimson and others, while knowing the importance of such planning, could not give it the necessary advance attention, they became instrumental as planners for the immediate and long-run future.

An early example of their initiative came in the fall of 1943, when Conant, Compton, and Henry DeWolf Smyth, chairman of Section T of OSRD,[1] conceived a report summarizing the technical achievements of the wartime program. Conant believed that a technical report would provide a basis for rational public discussion and would facilitate the safeguard of essential military secrets. In March 1944, Bush arrived at a similar conclusion, and the Smyth Report became the responsibility of Major-General Leslie R. Groves, director of the Manhattan District Project. Thus the report was in hand when a public announcement was made by the president at the war's end in August 1945, and was used as a device for defining the limits of the working scientists' public remarks.

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[1]Irwin Stewart, Organizing Scientific Research for War (Boston: Little, Brown and Company, 1948), p. 123.

By the late summer and fall of 1944, more systematic attention was given to the two major issues. On the relation of atomic weapons to international relations, project scientists took their cue from Compton, who declared in August 1944, that the world would remain in a state of war until there was international control. He suggested an international research center for atomic energy where the scientific activities of all nations would be registered for general use. This was proposed as an appropriate American policy at the San Francisco Conference scheduled for April 1945. In November 1944, twenty-two leading scientists asked Compton to relay to policy officials a memorandum which was designed to guarantee the peace and avert an arm's race.[1] They believed that a statement on the new weapon was needed to erase suspicion between the United States and its allies.

Scientists, in the laboratories and in government, wanted to inform Russia about the development of atomic energy before its use so that postwar efforts toward an atomic energy arrangement could commence in an environment of confidence. At the urging of Felix Frankfurter, Niels Bohr in August 1944 advised the President that the United

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[1] Hewlett and Anderson, <u>op. cit</u>., p. 341.

States and Britain make public their efforts to produce an atomic bomb. Bohr later presented Bush with a plan. He and Bush believed that Russia should be approached while relations were still friendly. It was Bohr's view that, once the weapon was made, it would be difficult for another great nation to agree to control without having the weapon itself. The inference here seemed to be that the atomic bomb made it imperative to have some system of control. The question of whether or not to tell the Russians that the United States was developing a bomb to use against the Japanese reached a point of decision in the early summer of 1945.

In the meantime, the working scientists continued to press for some organized planning for the postwar period. In August 1944, James Franck of the Chicago Metallurgical Laboratory conveyed to Bush that the Chicago scientists were worried that nuclear research would not be continued after the war. Bush's reply indicated that the Army also did not favor the termination of this research, although postwar proposals might better be made after significant military victories in Europe or the Far East. He further assured Franck that he and Conant "were most anxious to reflect accurately the opinions of the scientists who had so deeply engaged in the enterprise."[1]

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[1] *Ibid.*, p. 324.

In an effort to allay their fears, Bush had the Military Policy Committee appoint Richard C. Tolman, special scientific adviser to Major-General Groves, and Zay Jeffries, a consultant to Compton from The General Electric Company, to head two committees. These were to give systematic thought to domestic and international control of atomic weapons, peaceful uses to benefit mankind, and postwar research and development arrangements. The reports of these committees, submitted by late 1944, provided technical and nontechnical recommendations. On the nontechnical side, the Jeffries report advocated international control through an international organization and the Tolman report recognized that military uses of atomic energy would be an important part of any postwar program, as would fundamental research and development.

Both reports were valuable working documents for future planning. However, it remained for Bush and Conant to interest those politically responsible for postwar organization. In September 1944, they themselves submitted a memorandum on international control to Stimson in the hope of getting substantive discussions underway. But, although high officials were profoundly aware of the impending decisions on the future disposition of atomic energy, the war effort took precedence, and the problem of postwar planning remained undefined even in the spring of 1945.

In December 1944, Bush urged Stimson to establish an advisory committee to consider these questions systematically; it was to be a mechanism by which Stimson and the President could begin to deal with postwar matters. Bush did not believe that the attention of scientists was sufficient. The participation of political officers was vital.

Secretary Stimson established an Interim Committee in the spring of 1945, amidst the crucial discussion of whether to use the bomb in the war against Japan. Its duties were three: to prepare a statement for delivery after the dropping of the bomb, to draft a bill for national control of atomic energy, and to recommend proposals for international control of atomic energy. Stimson wanted the subject considered not only from a military point of view but also from a scientific and political one.[1]

Through the establishment of this Committee and its Scientific Panel, scientists became advisers on political policy at the highest levels of decision-making. Although

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[1] "The Decision to Use the Atomic Bomb," Harper's Magazine, 194 (February 1947), p. 100.

both the Committee (composed of scientists and non-scientists) and its Panel (composed of scientists only) had advisory functions and no responsibility for decisions, Stimson and the President paid close attention to their recommendations.[1]

III

The establishment of the Interim Committee focused attention on Bush and Conant's concern that the restless laboratory scientists be represented on it. Project scientists were worried that without international control an armament race would commence and, at the same time, scientific research would be significantly curtailed, or restricted too greatly by national security requirements. These seemingly conflicting worries intertwined and provoked them to act.

Bush and Conant were correct in believing that these scientists would favor the Committee more if its scientific

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[1] The members of the Interim Committee were: George L. Harrison, President of the New York Life Insurance Company and special consultant to Stimson, who served as its chairman; James F. Byrnes, a private citizen at the time, who served as personal representative of the President; William L. Clayton, Assistant Secretary of State; Ralph A. Bard, Under-Secretary of the Navy; Vannevar Bush, director of OSRD; Karl T. Compton, chief of the Office of Field Service in OSRD; and James B. Conant, chairman of NDRC. The members of the Scientific Panel were Arthur H. Compton, Enrico Fermi, Ernest O. Lawrence, and Robert Oppenheimer--all involved in the actual development of the bomb.

members were "men of the stamp of Tolman and Smyth" who "though well informed . . . did not bear direct line responsibilities."[1]

In Conant's opinion, the views of scientists outside the Scientific Panel were necessary because the technical excellence of its members did not necessarily spell the best _general_ policy advice.[2] He urged Stimson to encourage the Interim Committee to seek the ideas of leading scientists about international relations and suggested that these could be expressed through the Committee or directly to the President. On the condition that Stimson would follow this approach, Conant finally accepted appointment to the Interim Committee.

Conant held it important for the government to enjoy

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[1]Hewlett and Anderson, _op. cit_., p. 337. Stimson, however, did not use Tolman, Smyth, or any others in the same category. The membership of the Interim Committee and the Scientific Panel included no project scientists despite Bush and Conant's realization of the importance of having them represented. Who, then, was responsible for the politics behind the composition of the Committee and its Panel? What was involved in Stimson's choices is an intriguing question whose answer might further illuminate the relationship of scientists and policymakers.

[2]_Ibid_., pp. 345-46. See Conant's prescription of some years later on how to deal with conflicting expert advice, _Modern Science and Modern Man_ (New York: Columbia University Press, 1952), pp. 114-18. See Don K. Price's comment on this suggestion in his _Government and Science_ (New York: New York University Press, 1954), pp. 150-51.

the full support of the scientific community and assumed that disagreements would be avoided if there were an opportunity for an exchange of views. His approach was probably prompted by the belief that experts should not disagree rather than any conviction that scientists were qualified to speak on international political problems.

Despite Compton's plea that they trust the efforts of their colleagues in government, laboratory scientists took steps to voice their views on the future of atomic energy research and on international control. In March 1945, Szilard tried to see President Roosevelt with a long memorandum on the need for international control. In April, Franck left a memorandum with Secretary of Commerce Henry A. Wallace. Franck's main point was that "statesmen who did not realize that the atom had changed the world were laying futile plans for peace while scientists who knew the facts stood helplessly by."[1] In May, Szilard, Harold C. Urey, and Walter Bartky visited James F. Byrnes in South Carolina to impress upon him, as the President's representative on the Interim Committee, that scientists ought to discuss atomic energy policy with the Cabinet. Byrnes got an unfavorable impress-

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[1] Hewlett and Anderson, op. cit., p. 342.

ion of Szilard, and Szilard thought that Byrnes did not realize the true significance of atomic energy.[1]

The Interim Committee provided a means for these scientists to channel their opinions to Stimson and the President. Compton facilitated considerably contact between working scientists and policymakers.[2] Still no satisfactory liaison between these scientists and those on advisory levels resulted from all these efforts. Conflicting responsibilities prevented totally similar approaches to the issues of the spring and summer of 1945. The question of whether or not to use the atomic bomb in the Japanese war cut across the issues of international control and postwar scientific research and development. It sharpened the belief of project scientists that both their colleagues in government and statesmen were making the wrong decisions.

IV

The atomic bombing of the Japanese cities was the final crystallization of the great and agonizing secret discussion of the first half of 1945. The process of making the decision to use the atomic bomb in the Pacific war had deeply involved scientists and other top level civilian

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[1] Ibid., p. 355

[2] Ibid., pp. 365-66, 421.

and military advisers in the Administration, although the responsibility to recommend action to President Truman fell to Stimson, and the final decision was the President's alone.[1]

Stimson was troubled by the long-range political meaning of nuclear energy in the postwar world. He was aware that any decision on the use of the bomb would have far-flung ramifications.[2] Great questions of foreign and national policy were directly tied to the military application of atomic energy. The gist of Stimson's thoughtful April 25th memorandum to the new Chief Executive was that the weapon was extremely powerful, that the United States would not maintain its monopoly for long, that control would

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[1] See Elting E. Morison, Turmoil and Tradition: A Study of the Life and Times of Henry L. Stimson (Cambridge: The Riverside Press, 1960), Chapter 32, for a discussion of these momentous and trying days. Also Alice K. Smith, "Behind the Decision to Use the Atomic Bomb," Bulletin of Atomic Scientists (hereinafter BAS), XIV (October 1958), 288-312; Karl T. Compton, "If the Atomic Bomb Had Not Been Used," Atlantic Monthly, 178 (December 1946), 54-56; Henry L. Stimson and McGeorge Bundy, On Active Service in Peace and War (hereinafter On Active Service)(New York: Harper and Brothers, 1947), Chapter XXIV; Stimson, "The Decision to Use the Atomic Bomb," op. cit., p. 98; and Hewlett and Anderson, op. cit., Chapter 11.

[2] See, e.g., his memorandum of April 25, 1945 to President Truman, On Active Service, op. cit., pp. 635-37, which presaged subsequent considerations of national and international control of atomic energy.

mean giving up some sovereignty, and that the United States had a moral responsibility to initiate such action. Stimson noted that the proper use of the new weapon would offer the opportunity to design peaceful patterns for international control, but this proved extremely difficult for scientists and non-scientists.

By June 1, 1945, the Interim Committee, after consulting with its scientific advisers, recommended the use of the bomb against Japan without advance warning.[1] The Chicago project scientists opposed this course of action. Their Committee on Social and Political Implications, headed by James Franck, produced a report by June 11. Fundamentally, it mirrored the two points of earlier reports: the importance of disseminating scientific and technical knowledge and the need for international control, particularly in view of the vulnerability of America's population and industry to attack. In order not to prejudice any attempt for international control, these scientists argued against the military use of the bomb and proposed, instead, a technical demonstration on a barren island as evidence of American power. In the event that Japan did not surrender, the weapon could still

[1] Stimson, "The Decision to Use the Atomic Bomb," op. cit., p. 101. Also Hewlett and Anderson, op.cit., pp. 358 and 360.

be employed with allied support.

Franck himself, urged by some of his colleagues who had a "distrust for scientists turned administrators," delivered the report to Stimson's office for fear the Scientific Panel would not give it timely attention.[1] The Scientific Panel issued its reply on June 16, 1945, a reply which also represented the thinking of the Interim Committee:

> Those who advocate a purely technical demonstration would wish to outlaw the use of atomic weapons, and have feared that if we use the weapons now our position in future negotiations will be prejudiced. Others emphasize the opportunity of saving American lives by immediate military use, and believe that such use will improve the international prospects, in that they are more concerned with the prevention of war than with the elimination of this special weapon. We find ourselves closer to these latter views: <u>we can propose no technical demonstration likely to bring an end to the war; we see no acceptable alternative to direct military use</u>.[2]

At the same time that the majority of the Committee

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[1] *Ibid*., p. 366.

[2] Quoted in Stimson, "The Decision To Use the Atomic Bomb," *op. cit*., p. 101. The underlined statement was italicized by Stimson. See Arthur H. Compton, *Atomic Quest* (New York: Oxford University Press, 1956), pp. 233-47 for a description of the varying views of scientists and the opinion polls which were taken in an effort to determine the majority view.

recommended dropping the bomb without warning,[1] it favored informing the Russians. At the Potsdam Conference, Churchill expressed opposition to such a move, and Stimson reconsidered after experiencing the Soviet closed system firsthand and realizing the difficulties of the problem compounded by opposing systems of government -- one closed, the other open. He wondered if the Interim Committee had been thinking in a vacuum when it made its recommendations.[2]

Bush and Conant, however, important members of the Committee, had long wanted to make a "reasonable" approach to the Soviet Union. Since they had probably thought about the problem longer than any of the other members, their views might have carried additional weight.[3]

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[1] Under-Secretary of the Navy Bard broke the Committee's unanimity by favoring the issuance of a warning. See Hewlett and Anderson, op. cit., p. 370.

[2] Ibid., p. 388.

[3] Ibid., pp.328-31. Bush had been heartened by the Yalta discussions in February and tended to be "chivalrous" with the Russians, Stimson thought. See ibid., p. 338.

In fact, the decision hinged on the question of what kind of postwar accomodations could be expected. Although both advisory and project scientists were far-sighted in suggesting the desirability of cooperation with the Soviet Union even before the divisions of the Cold War had surfaced, it is not clear that they understood the postwar balance of forces.

On July 2, 1945, after consultations with Acting Secretary of State Joseph C. Grew and Secretary of the Navy James Forrestal, Stimson recommended, in a memorandum to President Truman, that a warning be issued to Japan and that if the warning were rejected, the bomb be used. Accordingly, the President issued a warning on July 26, which was rejected on July 28. On August 6, Hiroshima was bombed; on August 9, Nagasaki suffered a similar fate; on August 10, the Japanese surrendered; and on September 2, 1945, the surrender was formally signed.[1]

V

The development of atomic weapons meant an irrevocable involvement of many atomic scientists in social and political

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[1] See Robert J. C. Butow, Japan's Decision to Surrender, (Stanford: Stanford University Press, 1954) for a well-documented history of the events which contributed to the Japanese decision to surrender.

affairs and, more pertinently, led to a division of labor among them which affected their subsequent actions in the political environment. Although pursuing similar objectives--international control and appropriate postwar atomic energy research arrangements--the advisory scientists often felt the restraints of political requirements and their responsibility for giving advice. The working scientists, on the other hand, proceeded in a single-minded and free-wheeling way toward aims which were defined more by humane and professional interests than by political considerations.

As a result of their own suggestion, advisory scientists became policy planners and led in posing the far-flung implications of atomic energy and in designing the administrative structures to consider these implications. They took the initiative in pointing to the need for international control and stimulated a continuous discussion of it among political decision-makers.

At the same time that they played a prominent role in the decision-making process, advisory scientists did recognize the ultimate responsibility for action of the civilian policymaker.[1] However, they were never really

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[1] Cf. Bush's advice to establish the Interim Committee. Also scientists' acceptance of the political decision at Potsdam not to tell the Russians about atomic energy.

overruled. Perhaps Bush, Conant, and other scientists on the Interim Committee and Scientific Panel were close enough to the decision-making process to appreciate the many factors to be coordinated in resolving political problems. Perhaps, too, they knew that, as part of the inner circle, they could continue to wield some influence in favor of international control. In short, they were able to adapt for the moment to higher political decisions.

On the other hand, project scientists, even after the decision to drop the bomb had been made, still submitted the results of their polls on the bomb question to Stimson's office.[1]

In August 1945, Groves received a complaint from John H. Manley, Oppenheimer's assistant on the Manhattan Project, of insufficient contact between the working scientists and policymakers,[2] a more or less continuous complaint. The problem of postwar atomic energy research became acute as the Manhattan District Project was nearing the completion of its specific objectives. Cutbacks on the program became clear and still there were no plans for the

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[1] Hewlett and Anderson, *op. cit.*, p. 400.

[2] *Ibid.*, p. 421.

continuation of research after the war. It was, said Groves, a problem of ways and means, but project scientists were not satisfied. The continued development of research under appropriate conditions was the main theme of their discussions for many months. Scientists were eager to settle the war uses of atomic energy and concentrate on peaceful uses. They knew that without some international control, the wartime restrictions on the conduct of science would not be lifted and, indeed, lack of international agreements would signal the beginning of an armament race. In this instance, the importance of atomic weapons would far outweigh atomic power development and free scientific research and development.

The Chicago scientists, hearing bits and pieces about the War Department's proposed bill for postwar atomic energy arrangements and about Byrnes's discouraging attitude on international control, believed that decisions would be made without proper technical knowledge. Some months earlier, however, Bush and Conant had observed that in their judgment "technical research had little bearing on the international issues."[1] Nevertheless, Bush, Conant, and others, such as

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[1] *Ibid*., p. 331.

Stimson, Groves, Harvey Bundy, and Harrison, did not underestimate the working scientists' interest in participating in decisions. Indeed, the Smyth Report was published in recognition that news of the atomic bomb would stimulate great excitement and possibly "reckless statements by independent scientists."[1] Stimson, Conant, Harrison, Bundy, and Groves agreed that a technical report on the scientific facts would avoid additional political pressure from the working scientists and circumscribe permissible areas of discussion.[2]

VI

If Bush and Conant experienced the frustrations of difficult problems, they could at least know they were being heard for they had access to top government officials, including the President. These channels of communication were not as easily available to project scientists. Neither did they have a working relationship with the responsible political officers. Nevertheless, they were impatient with Bush

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1 Ibid., p. 400.

2 Ibid. The British were reluctant to issue this document but James Chadwick, Conant's scientific opposite, "understood that the situation was different in the United States."

and Conant's efforts on international control and decided to seek direct access, ignoring Compton's advice to wait for their administrative colleagues to act. A growing number of them concluded that they were indispensable to atomic energy policymaking at the top levels and that "it was their duty to act", for advisory scientists were probably dominated by misguided individuals "who did not understand the imperatives of the hour."[1]

For them, participation did not seem to mean discussion or an exchange of ideas. It meant instead that they would educate policy officials to the scientific facts and the resulting human implications.[2] The "right" decisions would then be made. The idea that this education might prove insufficient for handling political problems was not countenanced. Perhaps the members of the Scientific Panel, in their reply to the critical views of the Franck Report, intended a gentle admonition to the laboratory scientists

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[1] <u>Ibid</u>., pp. 341-42.

[2] Hewlett and Anderson, <u>Ibid.</u>, p. 421.

when they declared:

> With regard to these general aspects of the use of atomic energy, it is clear that we, as scientific men, have no proprietary rights. It is true that we are among the few citizens who have had occasion to give thoughtful consideration to these problems during the past few years. We have, however, no claim to special competence in solving the political, social, and military problems which are presented by the advent of atomic power.[1]

Nevertheless, the project scientists, especially those at Chicago, were frustrated because they were not being heard and, when they were heard, as through the Franck Report, their advice was not heeded by the Scientific Panel. They had not been effective and their fellow scientists on the Panel had opposed them: a double frustration. This served to widen the chasm between these two groups of scientists. Operating now in different environments, divisions other than scientific arose and served to intensify a growing belief among project scientists that the advisory scientists were not representing them properly.

The termination of the war and the publication of the Smyth Report ended a period of awesome responsibility for Bush, Conant, and other high level government officials. It also unsealed the lips of the working scientists whose

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[1] Quoted in Stimson, "The Decision To Use the Atomic Bomb," op. cit., p. 101.

frustrations had run high and long. Official historians have written that the "headwaters of this torrent of public controversy lay deep in the isolated domain of the Manhattan District installations."[1] And, so it seemed. Scientists amassed their arguments for international control and appropriate structures for the conduct of scientific research and presented them forcibly in public, in Congressional testimony, and in negotiations with Administration officials.

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[1] Hewlett and Anderson, _op. cit_., p. 421.

Chapter II

An Initial Effort Toward Atomic Energy Legislation: The May-Johnson Bill

I

The wartime discussion between scientists and policy-makers on international control of atomic energy and scientific research and development in an atomic era testify to the fact that scientists were not playing completely new roles in the fall and winter after the war. Begun well before the end of hostilities, these roles now expanded and were no longer restricted to the narrow confines of the Manhattan District Project and the small circle of responsible officials. The private discussion could now become public and did.

In their initial discussions of the military use of atomic energy, laboratory and advisory scientists worried privately about the political and social impact of the new force. However, the explosions at Hiroshima and Nagasaki eloquently told the story of the secret American effort and released scientists from their vow of silence. Almost unanimously, the atomic scientists discussed the meaning of atomic energy, especially as it related to weapons, and insisted that the novelty of atomic weapons must be understood by an informed public. Furthermore, new rules were needed for the old international game of power politics, and international political institutions had to be modified. It was principally the project scientists who led the vigorous public debate and organized into political action groups. The political participation of Bush, Conant, Oppenheimer, and others closely associated with the Administration was of a quieter variety. But project scientists did not trust their colleagues in high government positions to protect professional scientific interests properly. They believed that advisory scientists were captives of statesmen who did not grasp the revolutionary meaning of atomic energy.

Stimson's successor, Secretary of War Robert R. Patterson, became impatient with this view. He distinguished "big" scientists who had governmental experience from "little" scientists who lacked this experience. Implicit in his distinction was the idea that "little" scientists lacked the proper perspective for policymaking.[1]

II

In the public discussion on national and international control of atomic energy, scientists articulated some basic

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[1] *Ibid*., p. 445.

views of their function. These persisted and affected their political behavior throughout this legislative period and beyond, hindering a sophisticated appreciation of the political process and slowing down their assimilation into the political environment. These views often reflected an underlying assumption that scientists' expert knowledge of the basic principles of the destructive potential of atomic energy enabled them, like "high priests", to know the truth in the political arena and therefore to prescribe the <u>right</u> courses of action for lasting peace. An editorial, written in the month when the McMahon bill was passed by Congress, implied that knowledge of scientific facts would provide a sufficient basis for political decision-making. It asserted that the "compelling necessity for a factual, realistic attitude as a basis of political decisions of our statesmen and political thinking of our citizens" justified the intrusion of scientists into national and international affairs.[1]

It was clear from the beginning that scientists were not going to be content merely with talking about the scientific facts of atomic energy. Their expert scientific

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[1] <u>BAS</u>, 3 (June 1946), 137.

knowledge would instead be used to support political positions which they deemed necessary for survival. Their sense of urgency had some months to mature. Scientists were eager to impart what they especially knew as scientists and as citizens. For example, project scientists in the Federation of American Scientists, established in December 1945, believed it their duty to inform not only on the basic facts of the atomic bomb but on its implications for international relations.[1] Their task arose from the social and political implications of atomic energy and they could not return to the universities before the nation and the world knew what they knew.[2] A clear understanding of the scientific facts would illuminate what needed to be done to avoid annihilation and once the vital information was divulged a rational decision would then result.

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[1] See Harrison S. Brown, Must Destruction Be our Destiny? (New York: Simon and Schuster, 1946), p. 66.

[2] See Harold C. Urey, "The Work of the Emergency Committee of Atomic Scientists," p. 15 and Selig Hecht, "The Role of the Scientists," p. 10 in Emergency Committee of Atomic Scientists Inc., op. cit.

There was an orderly way to proceed to achieve international control and the abolition of war, these scientists argued, and their arguments revealed a somewhat mechanical view of the political relations of nations, resulting in a failure to appreciate the continuous process of politics.[1]

Another view prevalent among scientists tended to discourage an understanding by them of their limitations in the political arena. They not only saw themselves as expert in prescribing rational courses of action but also believed that they could lead in effecting new departures in international relations which were required in the atomic age. In short, scientists asserted, power politics must stop or the atomic bombs would be used again.[2]

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[1]See, e.g., BAS, 1 (December 1945), 4 and also Masters and Way, op. cit., Chapter 15, pp. 78-9.

[2]Fifteen years later some scientists were still hopeful that the world would see how obsolete the old power politics had become by the advent of atomic energy and how it was making the "historical concepts of international struggle for power" meaningless. BAS, 16 (January 1960), 6. See Jacob Viner, "The Implications of the Atomic Bomb for International Relations," Proceedings, American Philosophical Society, 90 (January 29, 1946), 53-58 on the use of old frameworks since new frameworks were not yet available.

Scientists argued that a system of control could not be based on structures which had proved inadequate, such as treaties. One scientist pointed out that a system founded on treaties would have a high probability of failure, for the "treaties of the past can be viewed as experiments in which the world has been the test tube, nations the chemicals, and war the inevitable result."[1] In short, he was suggesting by analogy that an experiment which fails a number of times requires a new design. The working scientists agreed that a world authority must exercise control over atomic weapons.

Scientists in the laboratories and governmental positions believed that a free interchange of scientific information and free access to scientific laboratories everywhere--two major conditions for the scientific enterprise--would provide the bases for international control; that the international relations of scientists could rise above power politics and point the way toward peace. As Oppenheimer observed, scientists transcended "the accidents of personal or national history" in their search for knowledge about the natural world. They recognized the importance

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[1] Brown, op. cit., p. 78.

of a common approach in seeking solutions to scientific problems[1] and believed that this approach could be applied to international political problems with the same success experienced in their professional activities.

Scientists assumed that if science was above the nation-state, as Rutherford had said, that scientists also were above the nation-state. One scientist maintained that technological difficulties could be reduced if the basic concept was accepted that scientists' first responsibility is to humanity and not to any one nation.[2] In their efforts to stress the dangers of atomic weapons,[3] and to re-establish the international fraternity and freedom of science, scientists defined for themselves an ambitious role in the solution of complicated political problems. Through public lectures, press releases, official and other publications,

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[1] See Masters and Way, <u>op. cit</u>., Chapter 5, p. 24.

[2] See Brown, <u>op. cit</u>., p. 104, for the idea that scientists should be citizens of the world.

[3] Note Louis N. Ridenour's statement among others. See Masters and Way, <u>op. cit</u>., Chapter 7.

and conferences they took the initiative in educating the public and political leaders to the meaning of atomic energy and the need for its control.[1] As Frederick S. Dunn put it, scientists may have exhibited too great a tendency to expect mechanical answers to the problem of control since that was the kind of answer they were used to in their own work. But, they served an important purpose in their efforts to stress the urgency of the atomic energy problem.[2]

III

An intense public discussion of this problem began after the Japanese surrender in August 1945, while the Interim Committee was still formulating a policy for atomic energy. Truman's address to the nation shortly after

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[1] See e.g., BAS, 1 (December 24, 1945), 2 and BAS, 1 (March 15, 1946), 7. See also John A. Simpson, "The Scientists as Public Educators: A Two-Year Summary," BAS, 3 (September 1947), 243-46. This was not the situation with scientists in Great Britain where they were but one voice, among many, advocating stringent international control. Nevertheless, in a speech in Birmingham on November 3, 1945, Professor Oliphant attacked the Official Secrets Act and singled out British scientists for failing to express themselves as their American colleagues had done. See BAS, 1 (January 19, 1946), 5.

[2] See Bernard Brodie, ed., The Absolute Weapon (New York: Harcourt Brace and Company, 1946), p. 4.

Hiroshima focused the Committee's leanings toward strict atom control by the United States, Great Britain, and Canada until means for international control could be found.[1]

From August 9 to October 3, when the Chief Executive delivered his atomic energy address to the Congress, many opinions were expressed by a variety of groups. World government supporters, advocates of the extreme position of "unite or die," insisted that the concept of national sovereignty was outmoded for it had to be "one world or none."[2] Administration officials were not agreed. Secretary of Commerce Henry Wallace advocated the sharing of the weapon with the Soviet Union as a way of morally compelling that nation never to use the bomb for military purposes. In his opinion, international control would logically follow.[3] Stimson favored a Soviet-American international control

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[1] See New York Times, August 19, 1945, p. 12 for a complete text. See Hewlett and Anderson, op. cit., pp. 371-72, 388 for an account of the question of sharing the secret of the development of the bomb with the Russians at Potsdam before using the atomic bomb.

[2] See New York Times, September 16, 1945, Section E, p. 8 for a full statement of this position in a letter signed by such persons as Stringfellow Barr, John Dewey, Louis Bromfield, and Clarence Streit.

[3] New York Times, September 22, 1945, p. 1.

agreement within an effective framework. The proposal for effective control should be made directly by the United States and soon, for Stimson saw Soviet-American relations dominated and made "immediately emergent" by the atomic bomb. He argued that the manner in which the United States approached the problem of the bomb and Russia would inevitably affect future Soviet-American relations.[1] Atomic scientists and their university colleagues proposed the United Nations as the director of all atomic energy matters so that the United States would not seem to be starting an armament race.[2] The military favored a severe policy of secrecy and keeping complete control of atomic weapons in the hands of the United States, Great Britain, and Canada. This last view seemed to find approval among Congressmen, cautious in making vast proposals of uncertain implications although initially they did favor international control if it proved feasible. Their immediate concern now was the President's program for domestic control. All too soon, however, it became evident

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[1]Stimson and Bundy, <u>op. cit.</u>, p. 644. See Henry L. Stimson, "The Challenge to Americans," <u>Foreign Affairs</u>, 26 (October 1947), 5-14, in which he developed his subsequent ideas in view of demonstrated Soviet intransigence on atomic control.

[2]See <u>New York Times</u>, September 10, 1945, p. 5, for this recommendation by faculty members of the University of Chicago (arts and sciences represented) to President Truman.

that the international control problem could not be pushed aside that easily, that it had to be considered in any domestic bill.[1] A fundamental difficulty for designing atomic energy policy was rooted in the need to plan for war and peace simultaneously with an instrument as easily turned in one direction as another in a world of unstable conditions.

Inherent in all facets of this problem was the question of how to account for the needs of national security and, at the same time, maintain the objectives of a free society.[2] Although the issue of civilian-military control was loudly debated in the Congressional hearings, it was not a controlling one. As Walter Millis put it, the overriding issue

> was how to balance a proper combination of the non-strategic or non-violent with the strategic or violent components of national policy. It was with this issue, seldom clearly seen, that the history of the ensuing decade [1950s] was to be concerned?[3]

On October 3, the President sent his atomic energy message to Congress. He proposed international control of this new

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[1]See Robert Gard, "Arms Control Policy Formulation and Negotiation, 1945-1946" (Ph.D. thesis, Harvard University, 1961) for an analysis of early efforts at international control. See also Hewlett and Anderson, op. cit., Chapters 15 and 16 for the official account.

[2]See Harold D. Lasswell, National Security and Individual Freedom (New York: McGraw-Hill Book Co., 1950), pp. 50-75 for a succinct treatment of what is involved in the term "national security.

[3]With Harvey C. Mansfield and Harold Stein, Arms and the State, (New York: The Twentieth Century Fund, 1958), p. 144.

force[1] and domestic legislation to be based on the draft bill of the War Department's Interim Committee.[2]

The Congress had now to fulfill the Presidential request "for the prompt creation of an atomic energy commission to regulate all research, experimentation and operations in the new discovery for any purpose." Less than a year after Hiroshima, the United States had an atomic energy act. The international control of the atom is yet to be achieved.

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[1] The executive committee of the Chicago group of scientists quickly advocated bipartisan support of the President's direction on the international control problem. New York Times, October 5, 1945, p. 4.

[2] U. S. House of Representatives, Committee on Military Affairs, Hearings, An Act for the Development and Control of Atomic Energy, 79th Congress, 1st Session, October 9 and 18, 1945, pp. 1-2. (Herinafter Hearings, House 1945).

IV

On the same day that President Truman delivered his atomic energy message to Congress, the bill which had been drafted by the War Department[1] and which had incorporated the recommendations of Stimson's Interim Committee was introduced into the House and Senate by Representative Andrew J. May (Democrat of Kentucky) and Senator Edwin C. Johnson (Democrat of Colorado).

The May-Johnson bill provided for an Atomic Energy Commission (AEC) of nine part-time members to be appointed by the President, with the advice and consent of the Senate, and to serve for nine years without pay. They could take other employment and hold other offices. The bill explicitly enabled military officers to serve on the Commission or as administrators.[2] An administrator and a deputy administrator, the only full-time officials, were to be appointed by the Commission and responsible only to it.

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[1] The two principal individuals concerned with actual drafting of the bill were lawyers: Brigadier-General Kenneth C. Royall and William L. Marbury. See Hewlett and Anderson, <u>op. cit</u>., p. 412.

[2] The question of military participation was to become a crucial issue in the subsequent debate.

The responsibility of the AEC covered overall supervision of atomic energy matters. It could, among other duties, conduct research and production, developing processes for the release of atomic energy and for its military, industrial, scientific and medical use; it could acquire property anywhere that fissionable material was found and license all use of this material; it could make and enforce security and safety regulations, the violation of which carried severe penalties; it could make arrangements with private persons and organizations for the development of atomic energy. With certain exceptions, Patterson noted, similar general powers were conferred upon the Administrator.[1]

The particulars of this bill resulted in nationalizing a new and vital resource. In addition, freedom of information and research were drastically curtailed and civilian control overlooked. Secretary Patterson described the proposed bill as "far reaching" and the powers of the Commission as "most extensive," but insisted that nothing less than this would be adequate. The bill, he said, reflected "the views of the men who were most responsible for the wartime development of atomic energy as to the most effective method of controlling

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[1] Hearings, House 1945, op. cit., pp. 5 and 6.

and carrying forward development in this field within the United States."[1]

The bill was drafted mostly during the war when the military significance of atomic energy was uppermost in the minds of Interim Committee members, War Department personnel, and other officials who had approved the substance of the measure. Secretary Patterson testified that:

> When the interim committee had reached unanimous agreement on the scope and language of the proposed legislation, it was submitted to interested Government agencies, including the Department of State, the Department of the Interior, and the Department of Justice. With one or two minor exceptions, the revisions suggested by these departments were incorporated into the bill.[2]

The object of the May-Johnson bill seemed to be to continue the efficient system for the development and control of atomic energy that had already proven "a national asset of inestimable value."[3] Run by a small, intimate and capable team with a clear purpose before it, the Manhattan District

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[1] Ibid., pp. 8 and 4. These men were not necessarily militaristic but they had long been concerned with the difficult job of securing the national defense by military means. It is possible that their wartime experiences circumscribed their views unduly.

[2] Ibid., p. 5.

[3] Ibid., p. 1.

Project had been impressively effective. Now, however, the war was won and the "secret" was public. Responsibility for atomic energy had to be defined for the long run and purposes became varied and conflicted.

Hearings on the proposed bill opened on October 9, and were closed that same day by Representative May.[1] Four witnesses, all members of the wartime team, were heard. Patterson, Groves, Bush, and Conant especially emphasized several themes: the domestic aspect of the bill; the need for prompt legislative action, given the dangerous nature of atomic energy and project scientists' discontent with the prevailing uncertainty about the future of the Manhattan District; and the need for strong central control by a commission vested with ample powers.

The bill avoided the international aspect of atomic energy. Groves admitted that it did not allow for any international arrangements which the United States might make.[2] Bush contended, however, that a strong and healthy

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1 Hearings, House 1945, op. cit.

2 Ibid., p. 15.

domestic atomic energy set-up would be "appropriate whatever foreign policy is finally pursued on this entire subject after due consideration."[1]

Conant and Bush believed that the proposed legislation was wise in its intent to regulate and control and in its research provisions. It struck a proper balance between the requirements of national security and the necessity for encouraging an atmosphere of free scientific interchange. The consensus was that the proper control of atomic energy required a strong commission. Bush opined that he would rather err on the side of giving such a commission too much power than too little.[2] Conant reminded the Congressmen that "nothing like this has happened in the course of science or invention, unless it be the invention of fire itself in prehistoric times. This is an extraordinary bill, drawn for extraordinary circumstances. It is not just another commission."[3]

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[1] Ibid., p. 36

[2] Ibid., p. 44.

[3] Ibid., pp. 51-2.

All the witnesses agreed that unequalled peacetime powers should be conferred on the Commission. All agreed with the President that the matter was urgent, and that legislation should be made promptly so that atomic energy research and development could continue without undue interruption. All stressed that domestic control could not wait upon international control. Briefly, the proposed organization was based on stringent security regulations and isolated the national from the international control question.

V

Project scientists were quick to react to such cursory treatment of this vital subject, registering disappointment in their Washington representatives. In a letter to William Higinbotham at Los Alamos, Chicago physicist Herbert L. Anderson admitted that his confidence in the members of the Scientific Panel (Oppenheimer, Fermi, Lawrence, and A. Compton), "who enjoined us to have faith in them and not influence this legislation," had been shaken. These men, wrote Anderson, "were duped" since "they never had a chance to see this bill." He warned against "any breach of our rights as men and citizens. The war is won, let us be free again!"[1]

It was perhaps understandable that Bush, Conant, Oppenheimer, Fermi, Arthur Compton, and Lawrence would support the May-Johnson bill since it was developed on the Interim Committee's recommendations. Project scientists, on the other hand, were not consulted and had to glean what they could from rumors. Their information, which was surprisingly accurate, did not dispose them to support the Way-Johnson bill. In their opinion, it gave too dominant a voice to the military and was detrimental to the health of scientific research and development. Furthermore, project scientists did not trust their colleagues in high governmental positions to protect professional scientific interests properly.

They pointed out that this bill did not provide adequately for any future international control arrangement; gave excessive power to the military who would stifle research and development; and empowered a commission to establish the limitations on the conduct of scientific research. These limitations could well be unreasonable depending on how the phrase "national security" was interpreted.

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[1] Quoted in Hewlett and Anderson, op. cit., p. 432. As it turned out, the Scientific Panel had not carefully studied the bill in its final form. See ibid.

Oppenheimer, concerned about a costly delay in atomic energy research, persuaded Fermi and Lawrence to send a telegram on October 11 to Harrison, supporting the bill. He had minimized Szilard's criticisms of the bill's stringent provisions, declaring that they did not represent the Chicago and Oak Ridge scientists. The telegram was sent in the hope of halting, somewhat, project scientists' opposition.[1]

Oppenheimer, Fermi, and Lawrence called for immediate legislation to avoid further delay which "will cost us heavily in efficiency, in accomplishment, and in spirit." They maintained that the nature of atomic energy justified the broad powers of the Commission and expressed confidence in the wise direction of operations within the proposed framework. "We assure you that in our opinion the legislation as presented represents the fruits of well-informed and experienced consideration."[2]

Arthur Compton withheld his concurrence until he had studied the bill in detail, something which none of the Panel's members had done before sending the telegram. In a meeting on October 17, the Scientific Panel considered with

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[1] Ibid.

[2] Hearings, House 1945, op. cit., 107.

greater care the particulars of the bill and agreed that its security provisions compelled serious reservations. Other scientists such as Karl T. Compton and Lee A. DuBridge, who wrote representing 300 scientists in the Boston area, voiced similar reservations.[1]

Although scientists exercised considerable pressure on May to reopen the hearings, he refused, noting that they had had an opportunity to testify. It was true that scientists, now clamoring for a hearing, had not exercised that privilege in time. But, they indicated, their Washington representatives had described an acceptable bill. May's stand appeared to support their contention that the May-Johnson bill had dictatorial potential.

If the committee was willing to let the matter stand on five hours of hearings, the press was not so inclined. A New York Times editorial pointed out the bill's excessive provisions for secrecy and censorship and reminded Congress that science flourished by the free exchange of knowledge.[2] Opposition to the bill gained momentum. World Federalists and other "internationalists" were convinced that the bill's

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[1] Hewlett and Anderson, op. cit., p. 432.

[2] October 14, 1945, Section E, p. 9.

emphasis on national control and the military features of atomic energy did not allow a favorable condition for exploring the international control question. Scientists who had not been connected with the bomb project complained that their professional status was endangered and infringed upon by the proposed legislation.[1]

The hearings were reopened on October 18, just nine days after they had closed. The scientists had been effective, but their action did not endear them to the chairman of the House Military Affairs Committee whose acerbity permeated his initial statement. "These hearings," declared May, "have been continued for the purpose of permitting a group of interested people, known as scientists, to present their views on the questions involved in this proposed legislation."[2]

Thus, the reservations of the Scientific Panel, the criticisms of project scientists, and interest in the question of federal support of scientific research, which hearings on the bill to establish a national science foundation had generated--all these factors served to affect the

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[1] See e.g., New York Times, October 17, 1945, p. 1 for a statement by scientists from Harvard University and Columbia University.

[2] Hearings, House 1945, op. cit., p. 71. (Italics added)

decision to reopen hearings on the May-Johnson bill for another day.[1]

In the clash over the May-Johnson bill, project and advisory scientists found that their points of departure differed enough to reinforce a rift which began during the war. For example, when the Franck Memorandum was ready to be sent to Secretary Stimson's Interim Committee in June 1945, Franck's Chicago colleagues urged him to present it personally; they were not sure that their Washington counterparts would describe their views properly, or in time. Now, Anderson's letter to Higinbotham, expressing shaken confidence in the Washington scientists, and Oppenheimer's resolve to prevent a public dispute by talking to Szilard, Urey, and Anderson[2] before the hearings reopened provided fresh evidence of differences in approach.

Advisory scientists were interested in maintaining the efficient wartime system they had developed and operated for five years in cooperation with non-scientists whom they learned to work with and trust. It is perhaps not surprising that they believed themselves more qualified than the project

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[1] See Hewlett and Anderson, op. cit., p. 433. See also the hearings on the creation of a national science foundation--U.S. Senate, Committee on Military Affairs, Subcommittee on War Mobilization, Hearings on Science Legislation, 79th Cong. 1st Session, October 8, 1945-March 5, 1946.

[2] Hewlett and Anderson, op. cit., p. 433.

scientists to design an atomic energy law which would protect scientific and national security interests, nor that they tried to stem the tide of criticism from the project scientists whom they thought were not sufficiently experienced in science and politics. On the other hand, project scientists were unencumbered by governmental restraints or sobering experiences in their drive for an organization allowing the greatest freedom possible for scientific research and development.

The curious question arose of who is expert among the experts and in what? This question, which was to recur in the hydrogen bomb debate, was raised implicitly by the experts themselves. Advisory scientists thought they knew best because they were experienced in government and the problems of policymaking; project scientists thought they knew best because they were <u>not</u> caught in the web of the government's administrative-advisory hierarchy and could maintain the necessary objectivity.

VI

Project scientists' specific objections to the May-Johnson bill mirrored their professional concern for reestablishing the international scientific fraternity and the freedom of science. They also mirrored a deep social concern for establishing orderly international relations on which an international control system could be based. Scientists' testimony was pointed mainly toward three major issues: proper executive control of the commission to avoid military dominance of scientific research and development; excessive security regulations based on the erroneous idea that there can be a scientific secret; and the ramifications for international relations if the military purposes of atomic energy were emphasized exclusively.

Many scientists would have agreed with Truman's observation a decade later that the bill appeared to "set up a kind of permanent 'Manhattan District' under military control."[1] They objected to the broad powers of a part-time Commission and its lack of specific responsibility to the President. Chicago and Oak Ridge scientists especially warned that the nature of research, development, production, and use of atomic energy would be shaped entirely by the Commission and its full-time administrators in a greatly restrictive manner.

Scientists also opposed the appointment of military officers to the post of Administrator or Deputy Administrator

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[1] Memoirs, (New York: Doubleday and Co., Inc., 1956), . 2, p. 2.

since these officials would administer "both military and non-military aspects of atomic energy."[1] Scientists were not used to control and regimentation, Oppenheimer explained. They wanted explicit assurances about the "intention of the Congress to direct the Commission not to interfere with scientific work except when there is a national hazard involved."[2] At the same time, Oppenheimer did not share project scientists' concern with scientific-military relations in atomic energy questions.[3] In his view, the May-Johnson bill represented "legislation to get control of the project out of the War Department, not to put it into the War Department," to which Representative Chet Holifield (Democrat of California) replied, "I am not so sure of that."[4]

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[1] _Hearings, House 1945_, _op. cit._, p. 100.

[2] _Ibid._, p. 128.

[3] See, e.g., _Ibid._, p. 136, for Urey's statement that the control of atomic energy for military or industrial purposes by the Services would be "fatal" to development in this area.

[4] _Ibid._, p.   for an exchange between Holifield and Oppenheimer on the question of the role of the military as defined by the May-Johnson bill.

Perhaps, as Millis observed, Oppenheimer believed that the terms of the May-Johnson bill were not significant to the far-reaching issues which may arise between scientists and soldiers.[1] Perhaps his wartime experience of resolving issues with the military made him less anxious about military dominance. But Oppenheimer's position did not take into account that the relationship, which was developed during a time of war, might undergo significant changes under the duress of an uncertain peace in which conflicts over objectives and means may well arise. Project scientists, on the other hand, argued that overall coordination of governmental policy was the responsibility of the executive. Therefore, the executive should maintain control of atomic energy matters. In short, the Commission should be explicitly accountable to the President.[2] Stringent security rules also concerned scientists. "No one can predict," said Harold Anderson who represented the newly organized Atomic Scientists of Chicago, "how much harm can be caused the progress of

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[1] Millis, Mansfield and Stein, op. cit., p. 162.

[2] See Hearings, House 1945, op. cit., passim.

science through the limitation of access to information and no one can be wise enough to know what knowledge can be limited without hindrance in the advance to the final objective."[1] The scientific testimony stressed that a liberal scientific research and development program would more readily insure national security. Scientists tried to correct the impression of non-scientists that there was a "secret" to be kept. The secret was given away, observed Szilard, when the bomb exploded and with the publication of the Smyth report. Although technical know-how should be carefully guarded, basic scientific research needed a free rein to maintain scientific leadership.[2]

The questioning of Committee members revealed the extent to which they had to be educated to these ideas. It was not as clear to them that science was an open book to anyone with scientific competence. Szilard warned that unreasonable security restrictions would, in all likelihood, either be evaded or result in many "spirited men" leaving the organization.[3]

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[1] _Ibid_., p. 98.

[2] _Ibid_., pp. 80-81.

[3] _Ibid_., p. 81.

Arthur Compton also urged that the bill emphasize atomic energy development rather than "controlling those who do the work in this field. That change in emphasis seems to me to be . . . a very important matter."[1] Compton did not agree completely with other members of the Scientific Panel, who were willing to let the bill pass as it was.

Oppenheimer, for example, supported Bush, Conant, and Stimson under whose supervision the bill was drafted. He did not think that its details could be delineated in light of the rapidly changing technical situation. It was enough that the bill provided a framework for implementing policy, though it lacked specific instructions on policy formation and execution. He said:

> the May bill has been written largely from the point of view that we must have confidence in the Commission; we must have confidence in the Government of this country. We are not in a position to write detailed directives that will be binding for any reasonable period in the future. With the understanding that as the issues become clear it is appropriate to reconsider the legislation. . . . In that sense I think it should be supported.[2]

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[1] *Ibid.*, pp. 109-10, 113-14.

[2] *Ibid.*, p. 128.

Project scientists were not willing to express this kind of total confidence. They wanted the lines of responsibility clearly drawn. The two positions reflected scientists' different wartime responsibilities. Compton again served as the link between scientists who assumed administrative-advisory duties and those in the laboratories. Although he generally supported the bill, his proposals would significantly change its intent and, therefore, some of its major provisions. Compton's suggestions were more in line with what finally passed as law.

Scientists emphasized also the consequences of stringent security rules for good international relations. Anderson's statement pointed up their argument. He said:

> In the interest of international understanding which lies at the basis of any arrangement to insure a world peace, it is essential to avoid a dictated security. In the eyes of a foreign government, the enactment of legislation which shields possible military activities of a government behind a cloak of secrecy, is in itself an act of aggression. It is fraught with just the dangers of mutual suspicion which we must endeavor from the beginning to wipe out, and it will prevent the free intercourse and mutual appreciation which we have to have if we are to deal with other countries on a friendly plane.[1]

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1 *Ibid*., p. 99.

Scientists anxiously distinguished legislative goals for developing atomic power on the one hand and atomic weapons on the other. They objected to inordinate emphasis on a system stressing the atomic bomb rather than atomic power. The paradox contained in Truman's October 3rd Congressional message became explicit when Urey said flatly that "the approach to the bill should be . . . what it purports to be, namely, an atomic energy bill for power purposes and not . . . what it actually is--an atomic bomb bill."[1]

With this summation, he verbalized a useful distinction. For scientists who objected to the May-Johnson bill, clarification of goals was vital, if legislation was to account for both peaceful and non-peaceful aspects of atomic energy. A clearer discussion of the atom as a peaceful instrument of foreign policy and its recognition as a military weapon might have resulted, enhancing the probability of coordinated policymaking in the broadest sense. The scientists who made this subtle, but important, distinction were making a vital point which unfortunately was not developed.

Further comments by them in the press illustrated their determination to defeat the proposed bill. They urged

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[1] Ibid., p. 136. One of Urey's major objections was that the bill assumed an armament race which would undoubtedly hamper attempts to secure international control of atomic bombs and "create a political dictator over science." See New York Times, October 19, 1945, p. 2.

greater public participation and tighter public control over atomic energy development in general through elected representatives.[1] Scientists argued that the May-Johnson bill failed (1) to establish effective liaison with the executive for the integration of foreign policy and atomic energy policy; (2) to give the executive and Congress the proper supervisory and review controls over the Commission and the administrator and his deputy; (3) to make civilian control explicit; and (4) to provide the best conditions for conducting scientific research and exchanging information. The <u>New York Times</u> reported that the scientific testimony had stirred up enough questions and doubts so that "it appeared . . . that the bill's chances of passage in its present form had been materially lessened."[2]

On November 8, the May Committee reported the controversial bill.[3] Later on, Representative May said that his committee believed it had satisfied the desires of the armed forces. Perhaps so, and perhaps, for the time in which the bill was written, it was reasonable. However, for unexpected

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[1] See, for example, <u>New York Times</u>, October 24, 1945, p. 2 for the text of a telegram sent by scientists, educators and civic leaders to President Truman, Secretary of War Patterson, General Marshall and Representative May expressing strong opposition to the proposed legislation.

[2] <u>Ibid</u>.

[3] For the Committee's report and a dissenting view, see <u>Report No. 1186</u> to accompany <u>H.R. 4560</u>, 79th Cong., 1st Session.

postwar contingencies, the May-Johnson bill was not sufficiently flexible.[1]

During this period, when advocates of the May-Johnson bill were pushing hard to pass it, others besides scientists were working for a more flexible measure. James R. Newman of the Office of War Mobilization and Reconversion and Harold D. Smith of the Bureau of the Budget, who enlisted the aid of Don K. Price from the Public Administration Clearing House in Chicago, worried that the proposed legislation threatened executive authority. In a memorandum to Truman, Smith advised that "full control by the Executive is the most effective means to insure control by the Congress, to which the President is accountable for the administration of the Government."[2] The President was quick to see the point and privately withdrew his support of the bill.

At a White House meeting on November 7, Secretary Patterson found that he had lost Administration support of the May-Johnson bill.[3] He was opposed by members of the

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[1] Also, the problem of international control was receiving major attention. A week after the bill was reported, the United States, Great Britain, and Canada issued a statement--Truman-Attlee-King Declaration--favoring international control.

[2] Quoted in Hewlett and Anderson, op. cit., p. 438.

[3] See ibid., pp. 443-45 for the positions assumed in this question by high government officials.

President's cabinet and other advisers, by the Manhattan District laboratory scientists, by Chet Holified (Democrat of California) and Melvin Price (Democrat of Illinois) who submitted the minority report on the bill, and by senators who established a special committee for further deliberations. Envisaging "an effective and powerful commission free from political entanglements,"[1] Patterson believed that the Way-Johnson bill merited "real support from scientists rather than the well-nigh hysterical criticism it has received from some quarters." Scientists like Bush, Conant, Oppenheimer, and the Comptons with their broad governmental experience understood his argument, but the "little" scientists, awed by the destructive power they had created and full of idealism, did not comprehend.[2] In his view, the "little" scientists could not possibly grasp the problem in its whole perspective. On the other side, project scientists believed that the bill was wrong in its approach and that "other men who have an entirely different point of view about the whole program"[3] should write a new bill.

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[1] Ibid., p. 445.

[2] Ibid.

[3] Hearings, House 1945, op. cit. p. 136 for Urey's statement.

It remained for Karl T. Compton to strike a note of fairness to the sponsors of the May-Johnson bill. As a member of the Interim Committee, he was able to ascertain that "the bill was prepared and introduced with the wisest of motives and that back of it there is nothing of the sinister intent which some people, including a good many of our scientists have suspected."[1] Nevertheless, with the withdrawal of Presidential and other support, the bill no longer provided a viable basis for discussion.

VII

The dispute over the May-Johnson bill re-emphasized the difference in points of departure between project and advisory scientists and raised the implicit question of who among them was more qualified to advise on atomic energy legislation. Advisory scientists tended to trust their non-scientific colleagues in government and project scientists to mistrust them and their scientific counterparts in government. Project scientists believed that the proposed legislation threatened the proper conduct of scientific research and development. Excessive restrictions, they argued, would harm American power and discourage meaningful atomic energy re-

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[1] Quoted in Hewlett and Anderson, op. cit., p. 435.

search, if not force scientists to enter other research fields.[1] They were suspicious of an administrative structure that potentially allowed the armed forces to determine the kind of research undertaken. They were also concerned about the bill's effect on international control. As during the war, the establishment of a free scientific environment and the achievement of international control were interrelated. The idea that the reestablishment of the international scientific fraternity would provide the basis for a control system hindered a view of the actual postwar balance of forces which was fast taking shape. It encouraged an unrealistic view of their ability to contribute to the resolution of political problems.

Scientists believed that they could advise responsibly, not only on the scientific and technological implications of science and technology but also on their social and political implications. Although disclaiming any specialized skills in politics, they made policy recommendations freely and dogmatically, justifying these by the logic of the facts.[2]

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[1] See Hearings, House 1945, op. cit., p. 109 for Arthur Compton on this point. See also Hewlett and Anderson, op. cit., p. 422, for a warning by the Chicago scientists that, if the Army did not lift its restrictions, they might be forced to study the color of butterfly wings.

[2] See, e.g., illustrative statements from the testimony, Hearings, House 1945, op. cit., pp. 132-33.

For example, project scientists, in particular, proposed that a World Security Council be made the only custodian of nuclear power and declared that nations must admit to complete and periodic inspection. All relevant scientific and technological advances must be reported immediately to a technical panel of the Council. These views did not demonstrate an appreciation of the difficulties inevitably encountered in establishing smooth relationships between countries with opposing vital interests.

Scientists further believed that it was their duty to inform on the implications that science and technology had for international relations. They were confident that the facts they could offer would clarify a course of political action; that they could lead in effecting this action. Did not their international society work better than the international political society? Their intensive concern with the goodness of the scientific society and with its reestablishment enabled scientists to ignore an important and relevant question as to what elements promoted success or failure in these two societies. A consideration of this question would have pointed to the limitation of their position.

Chapter III

A Legislative Seminar:

The Senate Special Committee on Atomic Energy

Project scientists' candid objections to the proposed legislation and Administration officials' realization that it curtailed executive power pointed to the need for a new beginning. The Senate Special Committee on Atomic Energy opened hearings at the end of November with the intent of writing another bill.[1]

In learning about the development, use and control of atomic energy, senators also learned about scientists. Committee members began to grasp the intertwining of scientific and non-scientific issues and appreciate that although science was objective and impartial, scientists were not. These eloquent educators in things scientific were simultaneously eloquent advocates of special points of view. It became increasingly clear that the interests which they defended

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[1] Republican members were Senators Arthur H. Vandenberg (Michigan), Eugene D. Milliken (Colorado), Bourke B. Hickenlooper (Iowa), Warren Austin (Vermont), and Thomas C. Hart (Connecticut). Democrat members were Brian McMahon, chairman, Richard Russell (Georgia), Thomas Connally (Texas), Millard E. Tydings (Maryland), Edwin C. Johnson (Colorado), and Harry F. Byrd (Virginia).

in the political environment were, in great part, moulded by professional interests. In this respect scientists were not any different from other special interest groups. They were unique, however, for their scientific knowledge which was fundamental to atomic energy legislation. For this reason they operated from a position of strength.

Congressional hearings provided an outstanding forum for communication between scientists and Congressmen but, for a number of reasons, the educational process was handled more by scientists than senators. First, the relevant and basic subject matter was technical, new, and comprised the scientists' area of competence. Senators had to learn the elementary facts about atomic energy and scientists, as the unquestioned experts became their instructors. Second, since scientists' primary objective was the establishment of proper scientific research structures, their interest was more or less a parochial one. Despite the diffuse ramifications of atomic energy, they focused their efforts on a limited objective, and sought answers to political problems by considering primarily the factors which served it.

Members of the Special Committee, on the other hand, knew that answers to these problems required not only knowledge but the ability to make sound judgments on the basis of all the relevant facts. Their task was further complicated by the technical subject matter, its social and political implications, and by the perplexing special perspective of scientists. Had they not been so awed by the atom and by scientists, they could have performed an important educative function. If these senators instructed scientists at all to greater political perception, it took the form mostly of short skeptical remarks in reply to what seemed to them remarkably bold or naive assertions. Senator Eugene D. Millikin's (Republican of Colorado) lucid statement on the relationship of Congressmen to the "special pleader" was a salient exception. He pointed to the limitations of the scientist, and any other "special pleader," in the legislative process. In effect, he asked that scientists remember that they were men of science, not men of politics. Millikin said:

> The scientist, as I see it, and as I get it from the testimony that we have had, is an idealist. He is in a true sense--and I am not using this in a disparaging sense--an internationalist. He is, because he is accustomed to interchange and meeting people from all over the world. His speciality is science. He works in the field I have described, and therefore he does not specialize in human nature and in the causes of war and therefore underweights that when he proposes a practical solution. He proposes a solution naturally out of his own environment, out of his own way of thinking; it couldn't be otherwise. Therefore, perhaps a committee of this kind might be said to be trying to fit a jigsaw puzzle. We have got a piece from the scientists which is perhaps too big. We have got a piece from the military, which is perhaps too big. We may have to whittle those down, both of them, because we have got to make a picture that will fit the frame of human nature, that will fit the posture of the world as it

> is; and if I were to take the liberty of making a suggestion to the scientists, I don't believe they have given enough weight to the way this world is, to the way people are, to the enormous revolutions that we would have to accomplish in the way people are--not as we would like to have them, in order to make a strictly scientific program work. I don't say that in a disparaging sense. Personally, I am delighted to get the testimony of witnesses like yourself. (referring to the scientist, John A. Simpson). I myself say frankly that I have got to discount it somewhat, just as I have to discount somewhat the testimony of the military because I don't think any of the special pleaders have taken into consideration enough things that must be taken into consideration.[1]

The Senator's perceptive commentary, delivered after extensive scientific testimony, provided an excellent perspective on the way in which experts have to be used by those responsible for overall policy. It also presents a setting for examining how scientists went about pleading their case before the Special Committee and how senators tried to whittle down their position.[2]

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[1] U.S. Senate, Special Committee on Atomic Energy, Hearings, S. Res. 179, A Resolution Creating a Special Committee to Investigate Problems Relating to the Development, Use, and Control of Atomic Energy, (hereinafter Hearings, Senate 1945). 79th Cong., 1st Sess., November 27, 1945-February 15, 1946, p. 327.

[2] For a recent study of the interaction of Congressmen and scientists, see Harry S. Hall, "Congressional Attitudes Toward Science and Scientists: A Study of Legislative Reactions to Atomic Energy and the Political Participation of Scientists," (Ph.D. Thesis, University of Chicago, 1962). See also Bernard Barber and Walter Hirsch, eds., The Sociology of Science (Glencoe, Illinois: The Free Press of Glencoe, 1962), Chapter 18 for Hall's discussion of scientists and politicians.

II

Scientists did not see themselves as special pleaders, for, in their view, science and scientists were above politics. They offered their scientific and policy prescriptions in good faith and believed they were giving purely objective and <u>right</u> advice. This approach did not encourage scientists to introspect about themselves in the political process. Instead it contributed to limited perspectives in the political environment and hindered appropriate adjustment. It was therefore possible for them to be actively involved in politics and alienated simultaneously from the rules of the political game.

It was difficult for scientists to recognize that they represented a vested interest. Perhaps this was because they were not seeking political power, only an environment favorable to scientific research and development, and believed that what was good for science was good for the nation. In their early political involvement, the issues debated were more matters of science than of politics, and scientists, whether in or out of government, generally agreed on the objectives of atomic energy legislation. Differences in approach to these goals existed but unity, based on professional interests, was still possible. Later on, this united front disintegrated over issues involving science and politics. For example, in the hydrogen bomb decision, scientists had at least an opportunity to see that among themselves they were capable of pleading not one but several special cases. For the moment, however, they were not faced with such divisive conditions and organized effectively to influence the legislation.

In the late fall of 1945, the Federation of American Scientists (FAS) was established to spread basic understanding of atomic energy through the nation and establish a conducive environment for international control by re-establishing free international exchange of scientific findings.[1]

Even though scientists knew that they had to compete with other lobbies, the FAS tried to avoid political bargaining and alliances in order not to sully their distinctive cause as scientists. It was fortunate, observed official

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[1]At the same time, McMahon, Newman, and Edward U. Condon, scientific adviser to the Special Committee, prepared its members for the task of answering the War Department's position on the May-Johnson bill. Scientists and senators consulted occasionally but worked more or less separately on their different missions. Hewlett and Anderson, _op. cit_., p. 449.

historians, that their leader, William A. Higinbotham "possessed an innate feel for politics that made him an indispensable leader of the reluctant lobby."[1]

This lobby received and galvanized support from persons and groups of varying political opinion[2] but was careful not to merge with these disparate groups. "We must be sure," cautioned John A. Simpson, a young physicist from Chicago representing his colleagues in Washington, "that other groups which really have no scientific interests at heart and no more background do not join us openly."[3] Scientists helped unite them within the National Committee on Atomic Information, an organization which functioned separately from the FAS.

Besides avoiding political alliances, scientists were also careful to clarify when they were speaking as scientists and when they were speaking as citizens.[4] Two related inferences emerge from this distinction. The first

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[1] *Ibid*., p. 448.

[2] *Ibid*., pp. 447-48.

[3] Quoted in *ibid*., p. 447.

[4] See e.g., *Hearings, Senate 1945*, *op. cit*. *passim*.

is that as citizens they were not experts and therefore could present a point of view just as any other interested citizen might do. The second is that the label, "special pleader", was inappropriately applied to them as scientists, since their professional code makes the facts rule supreme and does not allow expressions of opinions on what ought to be.

These two roles--that of scientist and of citizen--were in fact difficult to keep completely separate. In discussing the inadequacies of treaties as bases for an international control system, or the establishment of power plants in China, scientists asserted that they were speaking as citizens. In effect, however, the aura of expert knowledge in one field carried over to an area in which they were not expert. Nonetheless, the distinction between their views as scientists and as citizens enabled them to deny they were special pleaders. It also confused the senators' efforts to take some measure of their special perspective.

Scientists' role in the legislative process was complicated also by the Committee's enlargement of it. Scientists were invited to comment not merely on the technical feasibility of international control of atomic energy. They were asked to discuss the political steps that the United States should take to increase world security and hasten the adoption of controls.[1] This invitation continued a trend begun during the war when responsible policymakers requested the political opinion of scientists in high advisory capacities. In turn, some of these scientists solicited similar opinions from the working scientists.

Perhaps this trend was inevitable in light of the pervasive novelty and sudden emergence of the entire atomic energy complex into political affairs. "The trouble is," declared Senator Tydings, "that we are all learning what this thing is, but nobody is telling us what to do about it." When scientists suggested that their hesitancy was caused by insufficient knowledge of the political problem, Tydings retorted that that put the scientists in the same boat as everyone else.[2]

This interchange especially illustrates the political leadership's part in creating "top billing" for scientists in the postwar period. At the same time, in any attempt to understand the play between them, it is helpful to remember the image which the political official and the scientist have of each other. For example, the political official's image

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[1] See <u>ibid</u>., p. 326 for a request by Senators Tydings and Millikin that John A. Simpson and his scientific colleagues present the Committee with a practical plan outlining the various concrete steps on prohibition, treaties, and inspection.

[2] <u>Ibid</u>.

of the scientist may determine the amount of responsibility he dispenses to him in the search for policy alternatives. Also, the scientist's views of the political official is important to the manner in which he develops political sophistication. If policymakers do not clarify the possible and the desirable in political questions, it becomes more difficult for experts to comprehend their limitations in the policy process, thus thwarting their political growth.[1]

In short, scientists' ready inclination to make pronouncements in areas that required greater skills than scientific ones can be explained only partially by a deep sense of social involvement. One must recognize the encouragement they received from the political segment of society to transcend their area of competence.

III

Scientists' participation in designing atomic energy policy and senatorial reaction to it developed around several questions: the international control question and its relation to domestic control; proper research and development conditions,

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[1] In another instance, certainly the scientist's image of the soldier's reluctance to embark on experimental research and development of weapons by no means proven stimulated a desire on scientists' part to limit military control of this field of research.

and secrecy and security. In stating their positions on these issues, scientists exhibited behavioral characteristics and assumptions about politics which hindered their political education. Their prescriptions for resolving the complicated conditions surrounding the establishment of international and domestic control appeared remarkably simple to senators and other responsible officials.

In a preliminary and unequivocal statement in December 1945 to the Senate Committee, the Federation of Atomic Scientists properly recognized the political nature of the control and inspection problem. It recognized that the overall problem of the use of atomic energy was not solved by a determination of technical feasibility of control and inspection; that this was not merely a scientific problem, nor even just a military one. It was, indeed, a political problem of the first order.[1] As the hearings proceeded, however, this distinction was often blurred. For example, the Chicago atomic scientists worried that the subject of atomic

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[1]BAS, I (January 10, 1946), 2.

energy at the Moscow Conference in December, 1945, would be combined "with an attempt to settle various other political differences between the Allies." Atomic power, they insisted, "requires a *fresh* approach and should not be encumbered by the burden of past and present conflicts and misunderstandings."[1]

Scientists advocated international control under an international organization or a world government, as if by these means atomic energy could be separated from political difficulties. Project scientists, especially, campaigned so vigorously that Secretary of State Byrnes, who was discouraged about Soviet-American relations after the London Conference of foreign ministers in the fall of 1945, came to believe that too much attention was being given to their views. He pointed out that although national boundaries may not exist for science, they were realities to Stalin and Molotov.[2]

Even before the London Conference, Byrnes was not convinced that anything could be negotiated on the international level at that time. He believed that the Manhattan

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[1] See editorial, *BAS*, I (December 10, 1945). (Italics added).

[2] Hewlett and Anderson, *op. cit.*, p. 456.

Project should continue unabated until Congress had passed legislation. A report of the Scientific Panel in August 1945 had encouraged this view. The possibility of fusion weapons of even greater force provoked the Panel to recommend unanimously and urgently international control of atomic energy developments. In reply to Oppenheimer, who had drafted the report, Byrnes maintained that the Panel's proposal for international agreements was for that time impractical and urged Oppenheimer "and the rest of the gang" to "pursue their work [on the hydrogen weapon] full force.[1] But scientists' efforts for international control coincided with a similar interest of the Allies. The Truman-Attlee-King Declaration of November 15, 1945, gave their position official backing.

Although scientists readily observed that the control question was more political than technical, they believed that atomic energy could be isolated from other international political problems. They were confident that, within a specified period of time, international friction could be settled, and work on atomic power and other aspects of atomic energy could then proceed in the usual ways of science. For example, Simpson, who represented the opinion

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1 Quoted in ibid., p. 417.

of a majority of working scientists and engineers, and who was chairman of the executive committee of the Atomic Scientists of Chicago,[1] declared that "certainly in six months man can reach some sort of understanding of the problem" if leaders were educated to the devastating qualities of the bomb.[2] The unwarranted assumption was that man's awareness of the serious consequences of the atom's misuse was a necessary _and_ sufficient condition for his doing something rational about the problem. Simpson proposed, as a first step toward control, an international prohibition of the manufacture of bombs supported by inspection. Later, when control proved workable, atomic power plants could be constructed. His position, typical of much of the scientific testimony, presumed a certain amount of trust in the world as it then was. This disturbed Committee members who were not prepared to risk the security of the country on the basis of a trust which they did not think existed. Senator Tydings queried Simpson on this point.

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[1] _Hearings, Senate 1945_, _op. cit_., p. 302.

[2] _Ibid_., p. 323.

Tydings: Would the people of America support giving up their atomic bomb plants and factories in the world as it is today under such a proposal, and if they wouldn't obviously Congress wouldn't.

Simpson: I don't know the answer to that.

Tydings: You see, this is the thing where science cannot be as exact as it might be in some other things.

Simpson: I am not talking as a scientist now. For the last hour you have been talking to me just as a citizen of this country, and not an entirely well-informed one in this field.[1]

According to Simpson's thesis, which echoed that of many project scientists, man had two choices: the eventual destruction of large segments of the population, or the development of survival methods. The underlying theme had also been stated numerous times: knowledge of the facts about the atom's destructive potential compels the peaceful solution of political problems. Scientists were willing to forego atomic power until this happened or give it up entirely if need be.[2] Their willingness to compromise

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[1] Ibid., p. 324.

[2] See e.g., the statements to this effect of Urey, Szilard, Irving Langmuir in ibid., pp. 82, 272, and 114.

the value of free inquiry to help create an environment of international cooperation provided a significant index of the profound impact that the destructive atom's implications for free science and civilization had on some scientists.

Although a laudable and serious proposal, it was not a realistic one since it required for effectiveness a foolproof inspection system. It illustrated, as did the idea that atomic energy and political differences could be considered separately, a form of wishful thinking which does not help the solution of hard problems. Prescriptions of this kind were too mechanical, too neat as answers for complex international problems. Nevertheless, they did indicate a tendency among scientists to reduce the problem to manageable proportions, not by a painstaking examination of all relevant facts and a search for viable alternatives from which to choose, but by an almost herculean effort as if to set the world right in one sweeping gesture. This characteristic led to naive assumptions about international relations.[1]

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[1]Although many non-scientists also took seriously the prospect of international control, they were more willing to go into an arms race after a significant effort for control failed. Scientists tended to think that this action became inevitable because not enough concessions were made or the United States did not try hard enough.

Underlying these assumptions was the idea that arms cause wars. Scientists believed firmly that if the United States continued to develop its atomic weapons supply, it would trigger an armament race and eventually war. According to Langmuir and Bush, the Soviet incentive to build atomic bombs would come from the United States continuing to have them, since American predominance in nuclear weaponry contributed to Russian insecurity. Scientists concluded that international control based on a good inspection system was the only alternative to war. Furthermore, the United States should demonstrate a willingness to dismantle its present bomb supply and establish an equality of levels in fissionable materials. In Urey's words, "there is no possibility of securing an agreement with any other country unless we are willing to establish a level and expect them to reach it also."[1]

Although probably a correct assessment, the proposal presupposed good faith on the part of the two great powers. However, the United States had no such faith and, presumably, neither did the Soviet Union. The idea that there was only one alternative to war--international control

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[1]*Ibid.*, p. 105. See also Langmuir's concurring opinion, *ibid.*, p. 114.

in this instance--was too simple for examining a "whole mosaic of related problems extending indefinitely into the future."[1] It did not consider the possibility of protracted negotiations without conclusive results and that these negotiations, no matter how inconclusive and unsatisfactory, might constitute an alternative to war.

Langmuir argued instead that the search for security will make control and inspection possible. Senator Vandenberg was not as sure. He questioned the scientist on the implementation of inspection.

Vandenberg: Is it your opinion that it is possible to implement inspection and control to a conclusive degree?

Langmuir: I think so, if the mutual desire for security is large enough.

Vandenberg: That is a pretty big "if".

Langmuir: No, I think it will automatically come when several nations have atomic bombs, and because of that insecurity will arise; and I think the demand for security will be such as to make a real desire for inspection. Other nations will want to insist on inspection of our country, and will have to have inspection of their own. Then, we have to find effective ways of doing it.[2]

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[1] See Bernard Brodie, op. cit., p. 6 for the words of Frederick S. Dunn.

[2] Hearings, Senate 1945, op. cit., p. 123 (Italics added)

Senator Byrd was skeptical that human nature would change for the better. Langmuir's "let's hope for something better" elicited the dry comment that "we cannot base our future on hopes alone."[1] Nevertheless, Langmuir insisted that control was <u>politically</u> feasible because nations seek security and would be willing to forego some rights to make an international control agreement work.

That the demand for security might propel nations toward bigger and more destructive devices, as in the case of the hydrogen bomb, rather than toward a dimunition of their sovereignty was not examined. The destruction of Hiroshima and Nagasaki had made a deep enough imprint on scientists to lead them to what they viewed as a rational assumption: atomic weapons would reduce, if not erase, political difficulties. Used to transcending national boundaries in the pursuit of their professional activities, they could easily suggest quite radical changes in inter-state relations. In a world which wants cooperation and good-will, observed Oppenheimer, scientists can contribute because the society of scientists crosses national boundaries;

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[1] <u>Ibid</u>., p. 140.

however, in a world which has secret armaments, scientists "will not even make good spies."[1]

Oppenheimer believed that, as soon as the United States had a "reasonable degree of conviction" that other nations would not manufacture atomic bombs and that they intended to give evidence that they were not arming with atomic weapons, "then we should no longer arm atomically ourselves."[2] Senator Millikin asked if the United States should not have "complete conviction" rather than just a "reasonable degree of conviction." Oppenheimer answered, "I would like to think that, but I doubt whether it is reasonable to ask it."[3] Toward the attainment of peace he would take great risks in connection with the destruction of the American stockpile. Oppenheimer was later to become more skeptical of Russian intentions and to understand better the postwar balance of forces.[4]

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[1] Ibid., p. 187.

[2] Ibid., p. 194.

[3] Ibid.

[4] See e.g., United States Atomic Energy Commission, In the Matter of J. Robert Oppenheimer, Transcript of Hearings before Personnel Security Board (Washington, 1954) pp. 43-45, 343-46.

Scientists recognized that a viable inspection system had to be free and unimpeded, especially because detection difficulties mounted as scientific developments increased. At the same time, they had little faith in treaties between sovereign states which history had repeatedly demonstrated to be inadequate.[1] This time, declared Alvin Weinberg, who advocated making disarmament agreements as quickly as possible, people "_will have to see_ that they really mean the agreements."[2]

Scientists argued for some higher international sovereign body to control the new force. Weinberg proposed such a body to control the nations of the world and impose the peace. When the possibility of international civil war involving atomic weapons was raised by Senator Tydings, Weinberg pleaded that he was not an expert on historical things. Tydings wryly observed: "Well, we are all just as green as you are, Doctor, in this field of world peace. There is no man, no one man, who knows the answer, and it is only by common counsel that we can learn."[3]

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[1] See e.g., _Senate, Hearings 1945_, _op. cit_., pp. 84 and 379 for Urey's view that treaties are not dependable and Ross Gunn's statement that an experiment that has failed so far should be discontinued. Gunn was technical adviser in the Naval Research Laboratory.

[2] _Ibid_., pp. 342-45.

[3] _Ibid_., p. 361.

Although clear about the desirability of international control and confident about its possibility, once nations realized the awful consequences of atomic warfare, scientists were not able to propose any effective ways to achieve it. A first step was not possible without some basis of trust, and there was no guarantee that even a first step would create the requisite trust necessary for a workable inspection system. Tydings reminded scientists that they had all stressed the bigness of the problem and its implications, but no one had really proposed a practical first step.[1]

In effect, Tydings was pointing out to scientists that recognition of the difficulty of the problem was not necessarily equivalent to its solution. There still remained the arduous task of designing steps toward international control which would be effective in political terms. Although their proposals might have an unquestionable moral basis, this was not sufficient for the political resolution of the international control issue. Furthermore, public understanding of the dangers of atomic energy was not going to lead automatically to a rational ordering of the atom's use.

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[1] Ibid., p. 187 for Oppenheimer's summary of the scientific testimony which apparently supported the Senator's observation. But an effective international control system did have to rest on strict national control for which scientists did propose a detailed plan.

Scientists admitted that heretofore they had not felt any responsibility for the rational use of the fruits of their labor; that they had willingly left this responsibility to "the people and government of our Nation." Now, in view of the powerful force they had unleashed and in view of its use in Japan, this attitude had changed.[1] But, in the process of exercising responsibility for the social implications of atomic energy, scientists confused moral

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[1] Ibid., p. 302. In contrast to English scientists, who felt a social responsibility for their work before the atomic age and had reacted vigorously against using science for war, American scientists had remained apathetic about this question until they actually got involved in the development of the atomic bomb. See Part IV, Studies in the Sociology of Science, Chapter XV, "Science and the Social Order," footnote 25, p. 545, in Robert K. Merton, Social Theory and Social Structure (Glencoe, Illinois: The Free Press, 1957). By early 1944, when it seemed as if the military project of building the bomb was well underway, some serious thought was given to postwar planning for scientific research and development and international control of atomic energy. Although engineering development problems were still acute, scientific studies basic to plutonium production were mostly completed. See Arthur H. Compton, op. cit., p. 231. See also Hewlett and Anderson, op. cit., Chapter 10, for the official account of the postwar planning of policymakers, scientists-administrators, and the working scientists.

conviction with political astuteness. For example, their solution to the "deplorable" possibility of an atomic armament race was not tenable. The argument that treaties have more often been broken than not and are, therefore, not dependable, ignored the many instances in which treaties were honored; that a treaty might be made and kept if its stipulations make "faithful participation in an international control scheme highly profitable and its evasion or violation exceedingly unprofitable."[1]

In addition their political advice often reflected their past alienation from the stream of political life and, in some instances, seemed so naive as to provoke an angry senatorial response. For instance, in response to Weinberg's assertion that the atom had made war obsolete, Senator Johnson countered that "just as soon as you don't have the atomic bomb, war comes back into style again;

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[1] See Bernard Brodie, ed., op. cit., p. 15, for a relevant discussion by Frederick S. Dunn.

it is no longer obsolete. So long as you have the atomic bomb, then war is obsolete."[1] However, in view of his assumption, Weinberg could then ignore the relation between the control problem and the development of atomic power plants. "If you have the plants and you make the bombs, he said, the war starts with atomic bombs. If you do not have the plants then the war is finished by the atomic bomb. It seems . . . that the question whether or not you have the plants is not really and completely a relevant question."[2] Weinberg obviously did not consider the advantages or disadvantages that might accrue to the United States if it had the weapon at the beginning of hostilities rather than at the end.[3]

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[1] _Hearings, Senate 1945_, _op. cit_., p. 349.

[2] _Ibid_., p. 353.

[3] For formulation of this point, I am grateful to William T. R. Fox.

Another illustration of political naivete in terms of policy proposals is found in Oppenheimer's testimony. When Senator Byrd indicated the contradiction present in the development of atomic power installations simultaneously with an attempt to control atomic weapons, Oppenheimer proposed that the United Nations should own atomic power plants in view of the difficulty of prohibiting scientific advance by legislation.[1] He indicated that China might provide a suitable location for one such plant, since "the economics involved in the use of atomic power for energy would be sound." In view of the fine line between atomic bombs and atomic power, Senator Johnson described this as a most "reckless proposal." "I think," he continued, "that our scientists instead of entering the political arena . . . should be devoting their energies to finding out what we should do <u>scientifically</u>. If there is no defense they should be developing one." Oppenheimer asserted that his purpose was only to "make available such technical information" as he had. It was not to "advise political agencies on political matters," nor was it his intention or that of any other scientist to usurp the position and function

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[1]See <u>ibid.</u>, pp. 200-01, for an interchange between Senator Hickenlooper and Oppenheimer on stopping investigation of new fields especially where work has been successful.

of statesmen.[1]

Nevertheless, Oppenheimer _was_ giving policy advice which was not strictly technical. It was not surprising since he described the atomic bomb problem as being not only one of politics and statesmanship but a problem of human values. "It is to this end that we have testified," Oppenheimer concluded.[2] He might have added that it was also a problem which impinged seriously on scientific values, provoking the scientists' involvement in the non-technical aspects of atomic energy to protect their professional interests.

If senators had difficulty absorbing scientists' blithe disregard of political realities in proposing international control, they had an equally difficult time understanding their argument for the free flow of _basic_ scientific information which had existed before the war.[3] In general,

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[1] For this interchange, see _ibid_., pp. 206-07.

[2] _Ibid_., p. 186.

[3] See _ibid_., pp. 149-50 for Bush's statement of general principles which legislation for internal control and development of atomic energy should embody and which would provide a solid basis for effective American international collaboration.

the scientific testimony drew a line between pure scientific research and development and its application to weapons systems. "I envisage," said Szilard, for example, "control as control of manufacture. I do not believe that control of knowledge is desirable." He deplored the continued compartmentalization of information, a situation which was an anathema to scientists.[1]

Where the needs of national security seemed to conflict with the needs of scientific advance, the minimum security restrictions were advocated.[2] Otherwise, any organization, declared Langmuir, should encourage scientific cooperation and responsibility for the planning and execution of scientific programs. And, since scientists understood better than any others the detrimental effects of excessive restrictions, they should participate in the stipulation of any security regulations.[3]

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[1] Ibid., p. 290. See also ibid., pp. 93-96, for Urey's attack on excessive compartmentalization and a warning that continued frustrations would lead scientists into other areas where they could work freely.

[2] Ibid., p. 114. Usually this meant the approval of security regulations, to quote Langmuir, "to cover the manufacturing processes for making the materials used in the atomic bombs and, particularly, the details used in the construction of the bombs. These, however, should not go beyond those which are common in the manufacture of war weapons."

[3] Ibid., p. 115.

Thus, the maintainance of a freedom of action on the international level became an important objective of atomic energy legislation. Since the United States was on record as favoring open doors in laboratories throughout the world, scientists believed that legislation must give substance to this declaration. Their argument for a more liberal secrecy policy as a way of obtaining greater national security may have seemed incongruous to Committee members, who had to fit atomic energy into an unsettled postwar political picture and who might well have concluded that the greater security was better achieved through greater secrecy rather than less.[1] On this point Senator Johnson, a proponent of military control of atomic energy, expounded.

> It looks to me as though you scientists have made the world extremely insecure, and now you are coming to the politicians and asking us to go about and make the world secure again by some sort of a political agreement.
>
> At the same time, you are asking that the scientists who made the world insecure be given further appropriations to discover still another and more terrible destructive element than atomic energy.[2]

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[1] See *ibid*., pp. 93-100 for an interchange of senators and Urey on the subject of secrecy in which he argued that compartmentalization could only place the United States at a disadvantage with regard to scientific progress.

[2] *Ibid*., p. 121.

Johnson also observed that human conduct was lagging behind scientific discovery and perhaps scientists ought to slow up a bit and allow society to catch up. Nevertheless, Bronowski's perception that no member of society can abdicate his particular "responsibility for making the decisions of our society by passing it to a few scientists armored with a special magic" was a valid one.[1] It still remained for Congress, after listening to many "special pleaders," to propose legislation out of its own special kind of wisdom.

IV

The establishment of the Senate Special Committee symbolized the Senate's unwillingness to define the atomic energy problem primarily as a military one. During the hearings the President himself, in a memorandum on November 30, revised his position on the May-Johnson bill and proposed its amendment to provide for civilian control.[2]

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[1] J. Bronowski, Science and Human Values (Harper and Brothers, 1956), p. 12.

[2] See Ralph Lapp, The New Force (New York: Harper and Brothers, 1953), p. 52, for his observation that "contrary to what was generally believed there was no widely supported military 'plot' to keep the heavy military fist clamped down on atomic energy. However, it was true that there was a faction that opposed civilian control of atomic energy."

At a White House conference on December 4,McMahon[1] complained that the military had refused to release information deemed vital for Congressional determination of policy for atomic energy. The President, then, re-emphasized that "the entire program and operation should be under civilian control and that the government should have a monopoly of materials, facilities, and processes."[2]

On the basis of this directive, Senator McMahon introduced his bill (S.1717) for the domestic control of nuclear energy. Its purposes were:

(1) to encourage private research and development for the maximization of scientific progress;

(2) to provide for the free dissemination of fundamental scientific information and related technical information as much as possible;

(3) to promote scientific research under federal auspices;

(4) to have a program for government supervision of the production, ownership and use of fissionable materials;

(5) to provide for the study of the political, social

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[1]Present were Secretary of War Robert R. Patterson, Secretary of the Navy James Forrestal, Edward U. Condon, Director of the National Bureau of Standards and the scientific adviser to the Committee, the Committee's counsel, James R. Newman, and General Groves. See Hewlett and Anderson, op. cit., pp. 449-53.

[2]Memoirs, op. cit., 2, p.3.

and economic impact arising from the use of atomic energy; and

(6) to devise a program which will be in harmony with international agreements adhered to by the United States and which will have the flexibility required to take care of new situations.[1]

The War Department was still to try to upset the McMahon bill. In a memorandum a week later, Secretary Patterson again urged support of the May-Johnson bill. On January 23, the President reasserted his position favoring civilian supremacy. He advised the House Military Affairs Committee chairman and other House leaders "that the Administration desires recommitment of the May-Johnson bill for purposes of amendment, or, failing this, that no steps be taken to alter the present status of the bill in the House."[2] In a letter to Senator McMahon on February 1, 1946, Truman essentially reiterated the points of his January 23rd memorandum. Thus, the President made a public commitment to the principle of civilian control.

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[1] See Hearings, Senate 1945, op. cit., p. 1 for S. 1717, Sec. 1 (b).

[2] See Memoirs, op. cit., 2, pp. 3-4 for complete text of this memorandum to Patterson and Forrestal.

V

The Senate Special Committee's initial hearings afforded scientists an opportunity to expound at length on international control and proper conditions for scientific research and development. They also afforded senators a first encounter with this new breed of "special pleaders" who, although knowing explicitly what the requirements of their profession were, demonstrated a tenuous grasp of political needs. At the same time, senators were dependent on scientists for scientific information. Because the problems posed by atomic energy were so novel and immediate, perhaps they hoped that scientists would have the political and social answers to them. It became clear, however, that the blending of science and politics would need, as Senator Tydings put it, common counsel.

Scientists, on the other hand, thought they were especially equipped for this task. Even though they acknowledged the difference between the political and technical aspects of the control problem and explicitly disclaimed any special insight to political solutions, knowledge of the technical facts remained the key to technical <u>and</u> political answers. In short, a professional appreciation of the destructive potentialities of atomic war gave them special skills in designing policies to avert catastrophe.

Scientists' admission of responsibility for the use of scientific developments implied a concern with political advice. But, their advocacy did not demonstrate sound historical knowledge. Their political assumptions that arms cause war; that the need for security will create a sincere desire for inspection among nations; that nations seek security, therefore, control is politically feasible; that atomic weapons would reduce, if not eradicate political difficulties; and that the atom has made war obsolete,were too simple for prevailing political conditions. These assumptions fed their tendency to want to create, and to think that they could, orderly and rational designs for political solutions. Illustrative of this expectation is the FAS assertion that questions of political and technical feasibility of inspection could be solved separately before "solutions are integrated to solve the major problem of complete feasibility."[1]

Underlying these political assumptions was a certainty that scientists could correct and right an unstable condition. From their lofty position above politics, they thought that

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[1] BAS, 1 (January 10, 1946), 2.

they could impose a stable system, but the foundation was not secure. Scientists did not demonstrate an understanding of political facts. This led to the senatorial observation that no practical first step had yet been stated. Thus, inadequate assumptions about international politics and unsuitable ways of approaching political objectives tended to reinforce each other and stunt the political growth of scientists.

Chapter IV

A Law is Made: The Atomic Energy Act of 1946

I

Hearings on the McMahon bill (S. 1717) began on January 22 and continued until April 8. Several themes emerged from the testimony of outstanding public administrators and other officials, representatives of the military services, educators, and scientists: the importance of (1) an administrative structure with strong executive control; (2) adequate military representation; (3) international control agreements; and of (4) the regulation of science in an age of international political instability.

All of these intertwined with the problem of civilian control. But although it dominated the debate during this period, the ultimate policy responsibility of the civilian was not really in question. Instead, the issue hinged on how much emphasis should be placed on military means of achieving policy objectives and how much on non-military means. This kind of determination depended on the estimate of the nature of Soviet-American difficulties. The civilian-military issue was raised by different estimates and views of how to resolve present difficulties, or subsequent ones. This controversy clarified the distinct but sometimes overlapping and passionate views of representatives of the scientists, the executive, the War Department, and the Congress. It also illuminated the images scientists and non-scientists had of each other's ability to meet national policy problems.

II

Public administrators who testified advocated an administrative establishment providing for overall presidential control of Commission policy, thereby assuring the proper integration of atomic energy considerations with foreign and other policy. Harold D. Smith, the Director of the Bureau of the Budget and an early advocate of executive leadership, held that the national safety and welfare depended at this time on legislative and administrative knowledge.[1] He favored an administrative official to implement Commission policy, and strong executive direction "in order that the President may coordinate the work of this agency with that

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[1]U.S. Senate, Special Committee on Atomic Energy, Hearings, A Bill for the Development and Control of Atomic Energy (hereinafter Hearings, Senate 1946), 79th Cong., 2d Sess., January 22-February 27, 1946, p. 31.

of the rest of the executive branch."[1] Smith believed that sufficient Congressional control hinged on sufficient executive control. Furthermore, Commission members should be selected for their general knowledge and stature rather than for technical competence. Engineers and scientists should assume purely technical advisory functions for, Smith opined, "technical people [as a class], in the main, are the worst people to deal with large policy issues."[2]

The FAS disagreed with this viewpoint. These scientists emphasized "not that the special interests of scientists and industrialists shall have a voice but rather that the indicated experience shall be available" to the Commission. Men experienced in pure science and industry were required. Furthermore, the Commission's members should have only the public welfare in mind. If they represent the interests of a special group, Commission policies will frequently result in unsatisfactory compromises.[3]

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[1] _Ibid_., p. 35.

[2] _Ibid_., pp. 31-35.

[3] _Ibid_., pp. 156-57.

The FAS statement reflected its desire to rise above politics, overlooking that individuals may have different ideas about the public welfare, that these might well be honest differences with no negative reflection on personal integrity. It did not recognize that an evaluation of what was in the public interest could well be colored by the particular individual's operating milieu.

The stated long-range objective of the FAS, which grew from the nucleus of the Federation of Atomic Scientists, was to inform the public about the implications of atomic energy. It was not to rally partisan support, its members claimed. Yet the Bulletin of Atomic Scientists observed that FAS success would determine "whether American scientists will be able to exercise their full influence in shaping the national and international policies in the 'atomic age'."[1]

Scientists also favored strong executive control. They testified vigorously for civilian control of atomic energy in light of its constructive and destructive potentialities. Only under civilian leadership, they believed, could the peaceful atom have a chance for development. For the

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[1] BAS, (February 1, 1946), 4.

most part, scientists were not content with a domestic bill which ignored the international side of the question.[1] Overall executive leadership, they argued, would demonstrate American sincerity regarding an international solution of the atomic energy problem. It would indicate that, in establishing a balance between the violent and non-violent instruments for achieving national objectives, the United States was interested in the greater development of the non-violent tools of policy.

For example, FAS spokesman Harrison Davies supported the McMahon bill because it encouraged international control agreements, helped establish an American attitude of peaceful cooperation, and eliminated "a feeling of military insecurity" on the part of other nations. The acceptance in good faith of the bill's intent by other important countries, explained Davies,[2] would more likely occur under a civilian commission which had no military representation. Such a commission would stress the peaceful atom and a peaceful decision in the question of peace or war, "which will be decided within

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1 John von Neumann testified that he did not think "that the time is now mature to connect this piece of domestic legislation with anticipated and desirable future developments in international politics." See *ibid*., p. 209.

2 *Hearings, Senate 1946*, p. 139.

the next six months, or a year."[1] Millikin was not convinced.

Millikin: Just how would that help to bring us to a peaceful decision? There is always a certain amount of vagueness when we get to that proposition.

Davies: By giving the signal to the rest of the world that if it is humanly possible we will not employ this phenomenon in an aggressive way, but, rather, that we stand ready to come to agreement with them to prevent the beginning of an atomic armament race.[2]

Thus, the FAS representative reiterated the view of scientists and some others of the "internationalist" persuasion[3] that it was important to demonstrate the peaceful intention and willingness of the United States to enter into international agreements. In face of atomic chaos there was no room for competing nationalities and partisan policies, declared Harlow Shapley. The question of control was "an issue for independent citizens--for independent thinkers--and the time is now."[4]

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[1] Ibid., p. 150.

[2] Ibid., [illegible]

[3] Ibid., pp. 102-05.

[4] See BAS, 1 (March 1, 1946)11 for his talk at a rally of the Independent Citizens' Committee of the Arts, Sciences, and Professions which endorsed the McMahon bill. Shapley had travelled to the Soviet Union in the summer of 1945 and been impressed by the need for international cooperation in the atomic age. (Italics added.)

If the senator was seeking operational guidance, he was not to receive it either from these scientists[1] or from their supporters who, in some instances, illustrated the same search for solutions based on hope, good intentions, and stark necessity; and the same tendency to separate the technical and political parts of the control problem. A pointed example was the testimony of Secretary Wallace.

"If," he said, "these scientists could emerge from their various compartments, and confer together, exchange information, they could in my belief, quite rapidly work out effective inspection systems."[2] Wallace refused to evaluate the political nature of the problem by taking refuge in a purely technical solution.[3] In sum, Wallace believed that the greatest security would come from an international agreement with adequate inspection safeguards handled exclusively by scientists.

The War Department strongly objected to this view. It raised the issues of military representation in atomic

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[1]What the senator was probably looking for was some indication from scientists who supported civilian control that they realized a demonstration of peaceful intentions would not automatically bring a peaceful decision on atomic energy, and that this possibility might necessitate some role for the military.

[2]Hearings, Senate 1946, p. 237.

[3]This provoked an illustrative exchange with Senator Millikin who was still troubled by the impracticality of these proposals. See *ibid*., pp. 240-41.

energy matters and appropriate regulation of science, because, as John Von Neumann observed, "nuclear physics, in combination with irresponsible or clumsy politics, could at this moment inflict terrible wounds on society."[1]

According to Secretary of War Patterson and Secretary of the Navy James Forrestal, the McMahon bill did not provide adequate military participation in developing atomic weapons.[2] Countering the provisions in S. 1717, which seemingly gave precedence to international agreement over domestic control, Forrestal declared: "You must not limit the development until the world has got the pattern we are hoping it will achieve. You must not limit or put hampering strings around the military" who are in the end responsible for adequate weapons.[3] Although the implications of atomic energy were too great to be left entirely to the military, it would be equally fantastic to give them no voice at all in light of their specific responsibility for the national

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[1] *Ibid.*, p. 206.

[2] *Ibid.*, pp. 71-4 for his recommendations to integrate the military in the atomic energy design.

[3] *Ibid.*, p. 85.

defense. Forrestal's position found expected support in Senator Johnson's observation that,

> ...in view of all the testimony of scientists, mostly, of course, your position is absolutely correct. Up to the present moment there has been no plan advanced here, even by the scientists, where the military aspects of atomic energy can be separated from its peacetime uses, except the very vague hope that maybe something can be worked out to change human behavior, and all that sort of thing, and make it safe to close their eyes to its military uses, and launch a domestic and peacetime use.[1]

Johnson and his colleagues had received ample testimony on the atom's destructive nature and its marvelous potential to benefit mankind _if_ control was had. They had listened to political proposals whose separation from political conditions made them inadequate guides to action. Johnson therefore was quite ready to accept the military's policy recipe as a practical approach to the national security question.[2]

Scientists were not. Although the FAS did not object to a mandatory liaison between the military and the Commission, nor to the Army directing the research and development of

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[1] _Ibid_., pp. 80-81.

[2] See _ibid_., p. 402 for the testimony of Secretary of War Patterson whose amendments would give the Armed Forces the right to conduct military research in atomic power, custody of bombs, and the right to be consulted on security regulations for protecting information vital to national defense.

atomic ordnance, it did "oppose the extension of Army control into the field of fundamental science under the pretext of guarding 'secrets vital for national security.'"[1] Legislation, observed Frank B. Jewett, president of the National Academy of Sciences, should protect the basic interests of the military "without putting them in position to impose these interests detrimentally on other sectors."[2] The military were not qualified to handle the administration of basic science or civil technology. Said Jewett:

> The requirement of implicit, unquestioning obedience in time of battle; the consequent deferring to higher human authority; and the necessity of making instant decisions for action on the basis of meager data, all . . . are antagonistic requirements . . . for fundamental science and for civil technology.[3]

Scientists also objected to excessive compartmentalization of scientific activities. John Von Neumann recognized the need for proper regulation of science given its present dangerous implications but warned the legislator that this regulation was a "matter of extreme delicacy." Unreasonable regulations would damage basic science with unfavorable consequences for technological developments and the American defense stature. For these reasons, Von Neumann would "pro-

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[1] *Ibid.*, p. 157.

[2] *Ibid.*, p. 420.

[3] *Ibid.*

tect the natural modus operandi of fundamental research, and specifically two of its cornerstones: freedom in selecting the subject of fundamental research and freedom in publishing its results."[1] The Bulletin further noted that unless the difference between science and technology (wherein "secret processes" are usual) was appreciated, public opinion might interpret scientific opposition to military rule and compartmentalization as a "selfish fight for a comfortable way of life, or as the defense of certain liberal ideals, which have to be scuttled in the face of the 'hard facts of life'." In fact, the stake was American leadership in scientific and technological developments.[2]

III

Just as some senators, scientists, members of the President's Cabinet, and other influential persons had frustrated efforts to clear the May-Johnson bill through Congress,

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[1] Ibid., pp. 206-7. See also the supporting statements of Louis N. Ridenour, a physicist and FAS representative, ibid., pp. 535-42, and Frank B. Jewett, ibid., pp. 412-19, and Edward Teller, Professor of Physics at the University of Chicago, ibid., pp. 274-79.

[2] BAS, 1 (March 15, 1946), 1.

they also attempted to discredit the military viewpoint before the McMahon hearings.[1] Nevertheless, the conservative majority of the Senate Committee did not accept the principle of complete military exclusion. Patterson's testimony on February 14 undermined the drive for military exclusion. He proposed a more active role for the military in military research, development, and applications.[2] On February 16, a spy ring was discovered in Canada whereby it was concluded that secret atomic energy information had been transmitted to the Soviet Union. This was disturbing news for those who believed that "the secret" meant American security <u>and</u> for those who had high hopes for international control. Higinbotham reported that public support of the McMahon bill stopped when news of the spy group had been made public. Many scientists were disappointed that this event seemed to gain support for a strict military security system. Some believed that the Army was using the spy incident to get the old May-Johnson bill through the Congress.[3]

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[1] See Hewlett and Anderson, <u>op. cit</u>., pp. 484-88 for the campaign waged against the War Department's position.

[2] <u>Ibid</u>., pp. 499-500.

[3] <u>Ibid</u>., p. 501. See the <u>New York Times</u>, February 16-21, 1946 for reports on the Canadian spy ring.

The McMahon Committee, in executive session on February 21, heard the facts of the case. General Groves testified that Alan Nunn May, a British physicist with the Canadian atomic energy program, had relayed information to Russian agents about the American project, based on three visits by May in 1944 to the Chicago Metallurgical Laboratory. In open hearings Groves pressed for military representation on a part-time Commission or for a legal right for the Joint Chiefs of Staff to survey the Commission's policies before adoption and publication.[1] In McMahon's opinion, anything less than civilian control, with military exclusion, would mark the beginning of an atomic armament race.

At the same time, scientists made a concerted effort to stem a War Department move to get the May-Johnson bill through the House. Higinbotham urged FAS locals to conduct a "strong campaign of letter-writing." Urey made speeches against the bill and the Army. Scientists not only objected to military supremacy in atomic energy, but they had a per-

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[1] See Compton, Atomic Quest, op. cit., p. 117, in which Compton wrote that the Chicago group became suspicious of May when he requested information which was not necessary for the Canadian program. The interchange of visits with Canadian scientists ended shortly thereafter.

sonal antagonism to Groves. For many of them, Groves represented a martinet who "reminded them of the wartimes restraints and indignities they wanted to forget."[1]

Against the background of these activities, the Special Committee met to draft legislation. But Groves's testimony had made its impression on Vandenberg, who favored a civilian commission but believed that the military should be consulted on military matters. The Vandenberg Amendment, an expression of this viewpoint, stimulated a controversial storm which insured that the McMahon bill would not suffer from insufficient public notice.

The President himself believed that the effect of the amendment would defeat the civilian supremacy principle. To be sure, the military had a significant part to play, but he was concerned lest the proposal undercut American efforts for international cooperation in atomic power development.[2]

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[1]Hewlett and Anderson, op. cit., p. 487, also for a description of scientists' and newsmen's meeting which further fired this sentiment. See Compton, Atomic Quest, op. cit., pp. 112-13 for Compton's observation that Groves did not understand scientists' motivation or their way of thinking.

[2]Memoirs, 2, op. cit., p. 7.

American scientists protested the amendment loudly. They opposed military determination of "national defense and security" needs. This gave the military

> the right--or rather, the duty--to try to impose <u>its</u> concept of security upon as wide an area of fundamental research as possible. . . the military advisory board is certain to attempt a continuation of the Manhattan District policy of secrecy and compartmentalization.
> . . . How can they know what field of nuclear research (or any other branch of natural science) is 'important for national defense?' After all, it was not the military who first guessed the explosive potentialities of atomic fission in 1939![1]

Scientists asserted that military personnel could no longer claim to be experts on national security. In a world with atomic arms there was no security. Yet, the military "proclaim themselves guardians of secrets in which fictitious security is supposed to reside. This is the dangerous delusion which the scientists are fighting when they opposed the apparently innocent right of the military to 'advise and consult' with the Atomic Energy Commission."[2]

Senator Vandenberg denied that his amendment, representing a consensus of the Committee's views, undermined civilian control. McMahon alone opposed the amendment.

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[1] <u>BAS</u>, 1 (March 15, 1946) 16.

[2] <u>Ibid</u>.

Senator Russell, the ranking Democratic member, supported Vandenberg. Senator Hickenlooper's remarks reflected the relationship of the Vandenberg Amendment to the discovery of the Canadian spy ring. He noted the "eulogistic remarks concerning the sanctity of civilian control and the need for such control," and at the same time, the treason charge against one of the scientists connected with the Manhattan Project who "was a civilian and a scientist." This was a reference to Alan Nunn May's role in the Canadian spy case although Hickenlooper denied any intention "to indict any class or group of men in this country."[1]

The liaison between the military, the scientists, and Vandenberg in this controversy was Thorfin R. Hogness, a chemist from the University of Chicago whom Vandenberg trusted, and who had intimate knowledge of the atomic energy project. Vandenberg told him that the scientists could "write [their] own ticket" if they insured proper liaison between the military and the civilian. He emphasized that the military aspects of atomic energy were not to be lost in

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[1] Quoted in Hewlett and Anderson, op. cit., pp. 508-09. See also Cong. Record, 79 Cong., 2 sess., pp. 2410-15.

a desire to view the postwar world through rose-colored glasses.[1] The end result of these efforts was a revised amendment.

The military clause as it now read gave the Military Liaison Committee authority to "advise and consult on all atomic energy matters which in the Committee's judgment, relate to military applications" rather than to the wide area covered in the phrase "common defense and security." If the Committee wants a Commission action or lack of action appealed, it would do so through the War and Navy Secretaries, who then could refer it to the President for final decision.[2] In less than a month the Vandenberg Amendment had been presented, rejected, and finally passed unanimously in revised form by the Senate Committee on April 2.

Senator McMahon, who had abstained, reported the bill on April 19, 1946, with the Special Committee's unanimous approval. Almost without debate, the full Senate adopted it on June 1. During the ensuing House hearings, scientific pressure continued to counter any tendency to add "crippling

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[1] Arthur H. Vandenberg, Jr. ed., with the collaboration of Joe Alex Morris, The Private Papers of Senator Vandenberg (Boston: Houghton Mifflin Company, 1952), p. 259. (Hereinafter Vandenberg Papers.)

[2] For the complete text of the revised amendment see ibid., p. 259, or BAS, 1 (April 1, 1946), 19.

amendments" to it.[1] The final series of amendments, added by the House and to which the Senate acceded, did not affect fundamentally the principle of civilian control with proper liaison for the military.[2] With these the bill passed the House. A joint House-Senate Conference Committee then considered the remaining differences.[3] By the end of July, the House passed the bill as it then stood and, on August 1st, President Truman signed the Atomic Energy Act of 1946 or Public Law 585. On December 31st of that year the Atomic Energy Commission officially assumed operation of the Manhattan District Project.

IV

In its large aspects, that is, in its intent and emphasis, the McMahon Act overcame the objections of scientists and others to the May-Johnson bill. It provided civilian supremacy with stronger executive control in the

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[1] See, e.g., BAS, 2 (July 1, 1946), 23 for two resolutions favoring the McMahon bill--one by the American Physical Society and the other by the Federation of American Scientists.

[2] See House Report No. 2478 on S. 1717, 79th Cong., 2nd Session, for further amendments which in effect provided for more severe security regulations and greater military participation.

[3] See Conference Report No. 2670, Conference Report to accompany S. 1717, 79th Congress, 2nd Session.

integration of policy, and accounted for the international atom. It explicitly defined proper military participation in atomic energy matters. Finally, it permitted greater potential freedom for scientific research. Unlike the May-Johnson bill, the Atomic Energy Act of 1946 contained ingredients allowing the United States to move toward Niels Bohr's "open world". The Act's provisions for civilian control, international arrangements, and for the encouragement of research and dissemination of scientific data, though limited, provided a more flexible framework for foreign policymaking. Subsequent deterioration in Soviet-American relations did not, however, permit an enlargement of this flexibility.

Chapter V

Scientists in the Legislative Process

I

Scientists' successful participation in the legislative process convinced them they could contribute significantly to the solution of any problem. Although novices in the political game, they influenced the legislation by knowing what constituted their best working milieu. A group with strong and long-standing points of reference, scientists had a cohesive purpose and special interests to protect. This cohesiveness and an alliance of interests, if not motivations, with members of Congress and the Administration made them politically effective.

The point is that, although scientists formed a closely-knit group regarding their work specifications and professional values, the application of scientific results created national social and political problems. These problems, in turn, intruded upon their professional code of activity. Granting the need for planned research, and even for security measures regulating weapons, scientists remained reluctant to relinquish the freedom to establish their own specifications for the conduct of science.

At the same time, the two-way impact of science and society created its own specifications; national security considerations assumed top priority over the objectives of special interest groups. At this juncture, the problem of balancing the needs of the scientific profession and those of the nation arose. Nonetheless, scientists' support of the McMahon bill was an attempt to advance the values of science which they appeared to equate with those of a democratic society; what was good for science was good for the nation.

In brief, their policy recipes were inevitably colored by their special professional background. But, although agreed on objectives in this early period, the kind and degree of balance or adjustment achieved determined in great part the variations found in scientists' political behavior. In turn, the kind of adjustment made was tempered by the role or actual responsibilities which they carried in the policy process.

II

An impressive example of consensus emerged as working and advisory scientists pursued the objectives of international and domestic control. Through action organizations, personal contacts, and by means of intellectual arguments, they, especially project scientist proceeded to enlighten the public, government officials, and elected representatives to the values of science and its scientific, political, and social implications.[1]

It is an open question whether scientists would have achieved their objective, civilian control, without the change in Administration policy, or whether they would have been as successful in pointing up the international aspect of the problem without the simultaneous concern of the Western powers. But, given that vital professional interests were at stake, scientists would probably have been influential. The fact that science needed autonomy for its development made them a different kind of an interest group. In this respect, it might be said that they were fighting for their professional life. As it was, the Administration's support of civilian control demonstrated an appreciation that executive authority was being threatened; for scientists that appropriate research conditions were being threatened both on the national and international levels. The two interests coincided.

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[1] Talented in organizing and presenting their viewpoints, they employed a variety of means: letter-writing; public speeches before different audiences; conferences with the nation's leaders and the press; and testimony before Congressional committees. The Federation of Atomic Scientists became the Federation of American Scientists and included other than atomic scientists. The Bulletin of Atomic Scientists became the official mouthpiece, especially of project scientists, and had the prestige of its creators. In these ways, this particularly vociferous segment of the scientific community opposed a nationalistic solution of the atomic energy problem.

Scientists' advocacy of civilian leadership in the atomic energy program represented a revolt against the violence at Hiroshima and Nagasaki--regardless of the positions they took in the atomic bomb decision--and against unfamiliar military restrictions on scientific work conditions which they endured during the war. Scientists questioned the wisdom of the military recipe for securing the national welfare on two grounds: that an emphasis on the atom's military aspects would discourage peaceful solutions of political problems; and that the military was incapable of conducting basic scientific research and development required for national security. In their opinion, civilian leadership would stimulate scientific research and development toward peaceful ends; it would also encourage fundamental scientific knowledge, so vital since the United States could no longer depend on European scientific talent.[1] In sum, their advocacy was rooted in the requirements of the scientific profession. Scientists urged greater executive control, as a way of preserving traditional scientific relations and preventing the

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[1] In fact, the Atomic Energy Act provided for planning in research. With its powers to support and conduct research the Commission could meaningfully expand the horizons of basic research. See Sec. 3 (a) for the range of the Act's definition of atomic energy research.

military from impinging on those relations, for free scientific interchange was seen as a means for building international trust.

Scientists encouraged the potential use of the international relations of the scientific world to solve the international control question[1] and insure proper work conditions. Toward these ends, civilian control would more likely create a healthier balance between defense and other matters. It would also tend to lessen the military importance of the atom and emphasize security through achievement, rather than through secrecy. This theme was strongly emphasized at the United Nations Atoms-for-Peace Conference in 1955 and 1958. If scientists were more willing than Congress and the military to stress the non-violent components of policy, non-scientists should not have been surprised in light of the scientific community's operational requirements.

Science and scientists provided an unusual challenge to the legislative process. The newness of the requisite

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[1] As the hearings wore on, Congress became increasingly reluctant to subordinate national policy to the possibility of an international control arrangement. It had, however, to allow for it in order not to negate American efforts in this direction. The compromise was ambivalent, but scientists remained optimistic about the prospects for international control and their contribution to it. This optimism waned as events demonstrated the intransigence of Soviet-American relations.

scientific information and scientists' confidence in the rightness of their political advocacy disturbed Congressmen even though they recognized flaws in these proposals and arguments. Nevertheless, out of their specific concerns, scientists unconsciously raised basic questions for the democratic process: national sovereignty versus internationalism; federal authority versus individual or group initiative; skepticism versus docile, unquestioning submission to national security requirements, which could be defined differently by different individuals or groups; and freedom of information. Had these and other questions of social and political import been discussed systematically by Congressmen, a clearer idea might have emerged of what was being sacrificed and for what purpose, along with an opportunity for scientists to compare their own ends with those of the larger society in which they functioned.

III

The process of balancing professional interests and objectives and public or official interests and objectives was not a simple one. How a scientist was involved in political decision-making often defined his success in this endeavor. However, despite the fact that project and advisory scientists had different responsibilities and played different roles, they shared characteristics in discussing and making proposals. Their common scientific cultural heritage could not be completely and immediately erased by different experiences in the political process.[1]

The manner in which scientists went about preserving the scientific tradition in a political context, fraught with unfamiliar imponderables, demonstrated ingenuity and, at the same time, characteristics not always helpful in resolving political problems. For example, although politically adroit, their political proposals often did not indicate a knowledge of political facts. They frequently lacked political wisdom which entails the ability to outline alternatives and live with the uncertainty intrinsic in difficult political choices. Exhibiting a need for certainty and a neat way of solving a messy problem, they tended to think that a statement of a problem was equivalent to its solution.

This was compounded when they distinguished the occasions upon which they spoke as scientists and as citizens. The inference seemed to be that as scientists they were expert and responsible and as citizens they had a right to any opinion. However desirable this strategy for preserving political objectives, it also had the effect of hindering a self-evaluation because

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[1]For a brief and lucid discourse on science and values, see J. Bronowski, op. cit.

it allowed scientists to make two different kinds of statements: one expert, the other not. At the same time, both assumed the cloak of expertness, although only one qualified. This prevented a perception of the limits of their viewpoint in the political process.

An intense focus on special professional needs diminished for scientists the importance of the intricacies of political problems. If they recognized these, their remedies were often too sweeping for effectiveness. Efforts to solve political problems in grand designs contrasted with a demonstrated patience for piecemeal analysis when planning for science.[1] For example, some of their political assumptions were restricted by a narrow professional environment. These did not undergo the process of mutual criticism which occurs in science and which often brings underlying assumptions to light for careful examination. This was not done for scientists' political proposals. Assumptions remained unexamined and therefore unrigorous.

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[1] Examples of this are found in the establishment during the early summer of 1945 of committees at Chicago to study possibilities for nuclear energy research. See Hewlett and Anderson, op. cit., p. 366. Note also the many meetings the Chicago atomic scientists had with Edward H. Levi and Byron S. Miller about the drafting of atomic energy legislation, expecially the scientific research sections, ibid., pp. 441-43,454-55, and the final Conference on Domestic Legislation by scientists held on December 27-28, 1945, ibid., p. 482, which approved the McMahon bill.

Their belief that political solutions were facilitated by the atom's destructive implications discounted the vital principle of national sovereignty. That the atom might result not in peace but in a severe intensification of the political struggle was not deemed logical nor rational. Scientists did not appear to realize that the test of rationality was defined by the context and, perhaps, their context was more a scientific than political one.

Their views on secrecy and security are especially illustrative. The discovery of Alan Nunn May's treason did not convince scientists that added security restrictions were needed. In their opinion, the long-run security goal required free scientific activities at the risk of defections. They did not consider that, in some instances involving more than scientific factors, the greater security might be obtained by the security restrictions to which they strongly objected as scientists.[1]

Because their assumptions were limited by <u>their</u> standards of logic and rationality, scientists were more in-

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[1] It is conceivable that scientists over-stressed their position in this matter for fear they would lose more of their operating freedom in the nation's search for security. Yet, there was a strong tendency among them to equate science's needs with the nation's needs.

clined to make absolute assertions than delineate alternative choices; and to believe that, as scientifically competent individuals, they could supply a factual, realistic base for resolving political problems. It was ironic that absolutes were sought by this group whose own twentieth century discoveries pointed to unlimited horizons, allowing full play to man's ingenuity.

Scientists' exhibition of a simultaneous involvement in and alienation from politics; their avoidance of entangling alliances with other interest groups; and their reluctance to admit that science had become a highly significant factor in the political game manifested a wish to remain uncontaminated by politics. The interests of science were to be kept pure and defended by members of the scientific community only. This desire for peripheral, yet influential, involvement obstructed a view of themselves as special pleaders and developed a pleasing self-image of being above politics.

The FAS position is especially illustrative. In spite of attempts to remain "pure" in its political participation, these scientists were interested in exerting influence and defining the public welfare. The effort to inform "objectively" inevitably became subjective in the choice of issues that the FAS wished to stress in its enlightment program. It is not suggested here that there is anything wrong with advocating policies which a group or person support in the national interest. It should, however, be recognized that no action is taken in a vacuum. The split in FAS thought and action arose from its failure to learn that involvement in public matters, indeed in anything, require a subjective choice of what to become involved in. A recognition of one's own special concern in public matters and how this concern expresses itself is necessary for developing political sophistication.

In summary, scientists' involvement in and alienation from politics affected their political development. It delayed an appreciation that the convergence of science and politics was permanent; that the context of political decision-making is not purely a scientific one, although science may be a crucial factor in it; that the corresponding problems would differ from scientific problems; and that this required a sustained commitment by them to participate in policymaking and perceive that an evaluation of scientific facts needs a quite different set of skills than those needed for a consideration of political facts. In brief, scientists would need to know how scientific factors related to others in the policy process.

For the moment, however, the tenacity of their response in the immediate postwar period was maintained and defined, in great part, by a deep professional interest in international control and freedom of research, rather than by this interest and one in comprehending how political factors have complicated the achievement of these objectives.

IV

A common thread ran throughout scientists' efforts to influence legislation. They did not define their part in the legislative process as one of sharing in decision-making even though they did share with senators and Administration officials in manipulating events for civilian control. Their special knowledge was basic to any atomic energy act. It was therefore easy to imagine themselves the unquestioned experts whether they were lobbyists, legislative helpers, Congressional scientific advisers and/or educators of the public and Congressmen, or as advisers in the executive branch of government. Their expression of this self-image was directly related to the manner in which scientists were involved in this political question.

For example, throughout this period wartime advisory scientists quietly supported their activist colleagues and did not campaign in the open marketplace. Wartime responsibilities oriented them to operate at the highest levels and partly determined what they considered appropriate behavior in public discussion. In addition, they still occupied advisory positions in government in the fall of 1945 and winter of 1946.[1]

At the beginning of the fall of 1945, Bush, Conant, Oppenheimer, and others were closely associated with Administration officials whose concerns were defense and military power. As they studied the May-Johnson bill more closely, however, they shifted their support to the principle of civilian control. They were not, however, as disturbed about the War Department's bill as the project scientists. During the war they had functioned effectively within a loosely structured advice system. Why not the same kind of system after the war in which case the restrictions of the May-Johnson bill can be adjusted when necessary. It is conceivable that these scientists believed that the effectiveness of interaction of scientists and government officials depended on "right thinking" men. As long as they could

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[1] E.g., Oppenheimer and Smyth were consultants to the State Department team preparing a position paper for the Moscow Conference in December. See Hewlett and Anderson, op. cit., p. 471. At this same conference Conant served as scientific adviser to Secretary of State Byrnes to the discomfort of Senators Vandenberg and Connally who did not trust "college professors" to provide an astute approach to international control. Quoted in ibid., p. 473. Oppenheimer was also a member of the Board of Consultants to the Dean Acheson committee assigned to define an American plan for international control. See ibid., p. 534. Bush and Conant served on this committee along with Groves and John J. McCloy. See ibid., p. 531.

communicate, there was no need to worry about restrictive provisions in the bill.[1]

Project scientists, on the other hand, questioned the advisability of this procedure, especially since they were outside of the official advisory circle. Unwilling to depend on "right thinking" men in decision-making, they insisted on specific institutions or mechanisms to define the participation of the military, the political leadership, and the scientific community in atomic energy matters. They lobbied widely for their objectives, unrestrained by the experience of official responsibilities.

Nonetheless, scientists in general experienced a double allegiance, and possibly still do: one to the scientific profession which pursues knowledge of the natural world and the other to the self which assumes a responsibility for the use of this knowledge and protection of the scientific activity. The form that this expression of responsibility took, however, was defined, partially at least, by the functions scientists assumed. These helped direct their political socialization.

Those serving in official advisory capacities, perhaps of necessity, have to accommodate their responsibilities as professional

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[1]See e.g., pp. 62-63 above.

scientists and those as advisers to government for most useful participation in decision-making where science is but one part of the equation. Laboratory scientists with no operating responsibilities but with a deep interest in the use to which scientific results are put do not necessarily have to seek that balance. This kind of adjustment is not needed for their role purely as scientists, or as advocates of a cause. It is required if they have official responsibilities to fulfill, or if they want to be intelligent citizens who help in articulating alternatives.

Despite differing functions and ways of participating, scientists basically agreed on objectives. Both testified before Congress and, in some instances, demonstrated similar approaches to problem-solving. Advisory scientists, no longer able to contain a wide public discussion by project scientists, allowed them the public lead in defining the conditions for domestic control and in advocating international control. Their fundamental unity on objectives during this early period has not been duplicated. Subsequent difficult and complex atomic energy issues involving more than a matter of science and administrative structures divided scientists as well as other men.

PART II

SCIENTISTS AND THE HYDROGEN BOMB DECISION:

THEIR PUBLIC AND PRIVATE DISCUSSION:

1949-1950

Introduction

Four years after the first great debate on atomic energy, many scientists found themselves deeply involved in the controversial questions surrounding the decision to build the hydrogen bomb as a response to the Soviet atomic explosion in August 1949. The substance of this issue was significantly different from the first in that it marked a decisive policy event, a turning point in political affairs. In 1945-46, scientists saw the problem more as promoting freedom of research which, for the most part, gave a unity to their advocacy. By 1949-50, somber political events inextricably committed scientists and policymakers to consider the relation of scientific innovation to defense and the postwar balance of forces. A policy decision of major import had to be made in a time of relative peace but when Soviet-American political cleavages were sharply delineated and hopes for international control were practically non-existent. These political difficulties were in stark contrast to the "purer" problems of the laboratories. They forced upon scientists a problem in choice-making of a different order of magnitude, in content and context, from their own professional problems and those of the earlier legislative debate.

The entire defense system was crucially dependent on scientific and technological progress, and scientists, more than ever before, were vital to its maintenance. Under these circumstances, a certain number of the scientific community moved, in the words of Sir Robert Watson-Watt, from the state of "dispassionate knowledge of scientific facts to passionate awareness of social needs." And, perhaps, there is truth in his further observation that, in some instances, the immediate result was to devalue their special long-range contributions in favor of short-term activities which were not unique.[1] Nevertheless, these politically articulate scientists were being exposed to new ways of thinking; new demands were being made of them; and new expectations arose about them.

The issue of whether or not to initiate a crash program to build a hydrogen bomb exposed them to their first major postwar political decision and to the trying process of making difficult choices. It presents an opportunity to examine their political behavior under stress of a momentous political decision significantly involving science. Unlike

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[1] "Science, Politics, and Citizenship," <u>Bulletin of Atomic Scientists</u> (hereinafter <u>BAS</u>) 6 (January 1950), 27.

the matter of atomic energy legislation, this was a policy question in which clearly political, military, moral, and scientific considerations were crucially interlocked. Scientists could no longer ignore the impressive interrelation of political and scientific facts. In the face of unequivocal international discord, how did their political participation compare and contrast with their 1945-46 adventure in the political environment? How did they balance professional and political responsibilities? In other words, what happened to this special interest group under the impact of hard political problems and choices? What do answers to the recurring questions of this study with regard to objectives, strategies, and roles indicate about the process of scientists' political socialization?

Chapter VI

Scientists, Politics, and Atomic Energy: 1945-1949

I

After World War II, scientists on the General Advisory Committee (GAC) of the Atomic Energy Commission (AEC) played a significant policymaking role. From the spring of 1945 to the fall of 1949, decisions based on their advice were made about atomic weapons, including fusion research. These were not substantively questioned until the United States lost its monopoly. At that point, the President sought the advice of the AEC and other agencies, which had national security responsibilities, on the question of whether or not to develop fusion weapons. In this instance, the GAC was not the sole source of scientific advice. Furthermore, scientists outside of government questioned the wisdom of its position and offered opposing advice to government officials. Before this, however, GAC scientists who had served in wartime advisory capacities continued to do so. From a seemingly invincible position, it was easy to develop a sense of "rightness" about their decisions.[1]

Although the diminished prospect of an immediate peace and international control caused divergences in scientists' political proposals, in one respect their effort remained staunch and unified. They consistently opposed excessive security restrictions on scientific research, strongly voicing this opposition during the hearings that emerged from the AEC chairman's request to answer the charge of "incredible mismanagement" made in May 1949 by Senator Bourke Hickenlooper, a senior member of the Joint Congressional Committee of Atomic Energy.[2] Probably more than anything else, these hearings crystallized for scientists the deep impingement on scientific freedom of Cold War contingencies and impressed them with the inseparability of science and

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[1] See e.g., United States Atomic Energy Commission, <u>In the Matter of J. Robert Oppenheimer</u>, Transcript of Hearing before Personnel Security Board (hereinafter <u>Oppenheimer Transcript</u>) (Washington, 1954), p. 67 for Oppenheimer's statement that the GAC's collective competence in the atomic energy program surpassed the Commission's. Also <u>ibid</u>., p. 232 for Oppenheimer's self-image of his wider experience, influence, and power than other scientists in the H-bomb controversy.

[2] U. S. Congress, Joint Committee on Atomic Energy, <u>Hearings</u>, <u>Investigation into the United States Atomic Energy Project</u>, 1949.

these political realities.[1]

II

Even before World War II ended, scientists recognized the convergence of science and politics. In the summer of 1945, the Scientific Panel of the Interim Committee reported to Secretary Stimson about the "super". Its technical assessment was not promising. Nonetheless, the Panel unanimously recommended the international control of atomic weapons, basing its advice on the possibilities of fusion weapons and the development of greater weapons.[2]

Immediately after the war, President Truman asked for the advice of Vannevar Bush and James Conant on the problem of producing a hydrogen bomb. They reported that

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[1] The investigation was directed in large measure toward the AEC fellowship program and a discussion of the pending requirement that all fellowship recipients undergo an FBI check. Previously only those concerned with classified material were subject to such an investigation. See e.g., Leo Szilard, "The AEC Fellowships: Shall We Yield or Fight," BAS, 5 (June-July 1949), 177-78.

[2] Richard G. Hewlett and Oscar E. Anderson, Jr., The New World, 1939/1946 (University Park, Pa., Pennsylvania State University Press, 1962), p. 417. See Oppenheimer Transcript, pp. 111-12, 949-50 for a discussion of H-bomb developments in middle 1945. Attached to the Panel's report to Stimson was Edward Teller's written opinion that fusion work could have proceeded.

such a project would be as complex as the development of the first atomic bomb, and would require the same kind of total priority. In light of the American monopoly of the uranium-plutonium bomb, it did not seem, in their opinion, advisable that the United States launch a peacetime effort for a hydrogen weapon.[1]

Many of the scientists who had shouldered the responsibility of developing the A-bomb were likely not receptive to a new program for developing another awesome weapon. Something of this moral revulsion might have been mirrored in Bush and Conant's report since their instrumental wartime leadership had made them sensitive to the reactions of the working scientists. At that time the dominant theme--international control--coincided with technical data on fusion which were not encouraging.[2]

Sometime later, the chairman of the Joint Committee, Sterling Cole, commented about the background of this decision. He said:

> At the end of World War II, there was a general slowdown of our entire military program, including atomic weapon development. This relaxation was due in large measure to a general belief that a lasting peace had been accomplished, that we would enjoy atomic monopoly for some years, and that there would be international control of atomic weapons.[3]

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[1]Joseph and Stewart Alsop, The Reporter's Trade (New York: Reynal & Company, 1958), p. 141.

[2]Stewart Alsop et al, The H-Bomb (New York: Didier Publishers, 1950), p. 18.

[3]Congressional Record, 83rd Cong., 2nd Session, 100, (Daily Edit., April 8, 1954, p. A2716.

Probably some of these factors also had influenced Bush and Conant's evaluation for the President. Not only poor technical data on the fusion process but a desire for greater "normalcy" in the atomic energy field, as much as this was possible, may have provided welcome support to the propensity of these scientists to avoid adding variety to atomic weapons. Thus, the Atomic Energy Commission decided against a comprehensive hydrogen bomb program, although a modest research program continued. Diversion of necessary materials from a flourishing fission program to the development of fusion weapons was judged unwise. Even in light of the Soviet atomic explosion, the AEC asserted that all that could be done in the thermonuclear field was being done.[1]

Atomic energy posed an onerous responsibility and, perhaps, it was not surprising that the Lilienthal commission sometimes sought advice from the GAC which went beyond the purely scientific and technical. During this initial period, the AEC developed a close relationship with its General Advisory Committee and relied increasingly on its advice.[2] The

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[1]This was in response to the Joint Committee's evaluation, after the President's announcement, that a workable hydrogen bomb would achieve a significant military advantage for the nation. See Morgan Thomas, Atomic Energy and Congress (Ann Arbor: University of Michigan Press, 1956), p. 88.

[2]Oppenheimer Transcript, op. cit., p. 406.

growing importance of the GAC was enforced by deteriorating communication between the AEC and the Joint Committee.[1] Lilienthal was unqualified about his trust in the advisory committee. "I had such respect," he was to say some years later, "for the wisdom of men like Conant and Oppenheimer and Fermi and other men that I certainly paid close attention to what they said on matters that were not technical."[2] Thus, in their official advisory capacities scientists were encouraged to give <u>general</u> advice. This carried consequences for the GAC's role in the hydrogen bomb controversy, affecting its members' view of their function in itself and in relation to scientists outside of government.

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[1] Thomas, <u>op. cit</u>., pp. 16-49 for a discussion of factors which contributed to this condition. For Lilienthal's view that the Joint Committee should take a more active policy role in atomic energy affairs, see his "The People, the Atom, and the Press," an address before the annual convention of the New York State Publishers Association (AEC Release, January 19, 1948). The Joint Committee was somehow reluctant to do more than survey AEC policy decisions, and, under its first chairman, Senator Hickenlooper (1947-49), it shied away from broad responsibility for policy.

[2] <u>Oppenheimer Transcript</u>, <u>op. cit</u>., p. 406.

Chapter VII

Public Reaction to the Soviet Atomic Bomb: Fall 1949

I

On September 21st, President Truman was officially informed of a Soviet atomic explosion on the Asiatic mainland between August 26 and 29. He informed the Joint Congressional Committee on Atomic Energy on September 22, and the next day the American people. The President de-emphasized the event by recalling his statement four years earlier that "the eventual development of this new force by other nations was to be expected." Thus, he pointed out, although somewhat earlier than anticipated, this event was planned for and did not require altering the program's direction.[1]

Omar Bradley, chairman of the Joint Chiefs of Staff, echoed the Presidential statement. "The calmer the American people take this the better. We have anticipated it for four years, and it calls for no change in our basic defense plan."

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[1] Harry S. Truman, Memoirs (hereinafter Memoirs) (New York: Doubleday and Co., Inc., 1956), 2, p. 307. Other key sources used in this chapter are the Oppenheimer Transcript, the BAS, and the New York Times.

[2] New York Times, September 24, 1949, p. 2. See also "General Bradley Outlines American Defense Plans," BAS, 5 (December 1949), 344.

Nevertheless, the Russian achievement provided considerable anxiety in governmental circles. It indicated substantial progress in the Soviet Union's atomic energy program and, furthermore, that a total effort for better and more bombs, aiming toward a stockpile significant for military effectiveness, could now be expected.[1]

Despite public reassurances, responsible officials knew that the political-military situation had changed radically.[2] Harold Urey put the problem succinctly. He said: "There is only one thing worse than one nation having the atomic bomb--that's two nations having it."[3] This was basically the fact that representatives of the Departments of State and Defense, the Atomic Energy Commission, the Executive Office of the President, and the Joint Committee had to face; also the problem to which articulate scientists, in and out of government, addressed themselves.

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[1] See New York Times, October 6, 1949, p. 3, for the statement of Robert F. Bacher, scientific member of the Atomic Energy Commission until May 1949.

[2] See e.g., New York Times, September 24, 1949, p. 2, for Secretary of State Dean Acheson's comments to the effect that the United States had planned accordingly and the Soviet explosion would make no change in its policy. Also ibid., for similar statements by General Dwight D. Eisenhower and General Groves.

[3] Ibid.

II

Public officials' interpretations of and reaction to the Soviet achievement reflected the positions taken in the subsequent private H-bomb debate. They touched on questions of foreign and strategic policies, and of negotiating for international control and maintaining adequate military stature simultaneously. Congress' position emphasized both horns; that the United States should try to win United Nations approval of the Baruch Plan for international control at the same time that it expressed confidence in military leaders to meet the Soviet challenge.

The President, on the other hand, placed little faith in international control negotiations. Throughout this arduous period, he remained, in his words, "firmly committed to the proposition that, as long as international agreement for the control of atomic energy could not be reached, our country had to be ahead of any possible competitor."[1] This viewpoint culminated in the Chief Executive's decision of January 31, 1950 and in subsequent implementing actions.

Secretary of State Dean Acheson concurred with the President and advocated American atomic supremacy to maintain

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[1] _Memoirs_, 2, _op. cit_., p. 306.

as flexible a bargaining position as possible.[1] The AEC emphasized what had been its basic position in the Hickenlooper inquiry: excessive secrecy measures impede technical progress and a strong defense.[2]

The military were quick to indicate that Russia would not be able to mass-produce atomic bombs. In a speech delivered in August 1949, before the atomic monopoly was broken, General Walter B. Smith predicted the Russian test within a few months. But, it would take the Soviet Union "at least ten years to get to the point of mass-production that we have now reached. I know that American techniques and industrial skills are far better than the best the Soviet can offer."[3] Scientists refuted this statement as having no basis in fact.[4] "While it is evident," said Frederick Seitz,

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[1] Chester I. Barnard, who had been a member of the board of consultants which prepared the Acheson-Lilienthal report on international control, pointed out that the 1949 situation had reduced significantly the value of some of the points debated in the past in negotiations on the control question. See his "Arms Race vs. Control," Scientific American, 181 (November 1949), 11-13.

[2] David E. Lilienthal, See his "Where Do We Go From Here?" BAS, 5 (November 1949), 294, 308.

[3] New York Times, September 27, 1949, p. 10.

[4] See BAS, 5 (October 1949), 264 for the refutation by Harrison Brown, James Franck, Joseph E. Mayer, Leo Szilard, and Harold C. Urey.

for one, "that they do not possess as vast an amount of industry as the United States, it is also evident that they possess representative segments of industrial knowledge and equipment."[1]

The Soviet news lent additional weight to advocates of strategic air power who saw a supreme weapon in the atomic bomb.[2] The Navy, in its fight for sea power and naval aviation, questioned the wisdom of a national military strategy founded on atomic bombs. Represented by its member on the Military Liaison Committee of the AEC, Rear Admiral R. Oftsie, the Navy argued that strategic bombing with atomic weapons was militarily ineffective, immoral and not in accord with American political objectives and policies.[3] In reply Bradley observed that wars were immoral but they were fought. He favored a policy allowing the United States to fight a limited war.[4]

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[1] See his article "The Danger Ahead," BAS, 5 (October 1949), 266.

[2] E.g., see Oppenheimer Transcript, op. cit., pp. 684-85, for an "airman's view" of the strategic situation.

[3] See Warner R. Schilling, "The H-Bomb Decision: How To Decide Without Actually Choosing," Political Science Quarterly, LXXVI (March 1961), 34. Also Anne Wilson Marks, "Washington Notes," BAS, 5 (December 1949), 327-28.

[4] For an expression of his views, see his article "This Way Lies Peace," Saturday Evening Post, (October 15, 1949), 32 ff.

III

In the midst of these official reactions, those scientists who did not have official responsibilities at the time and were willing to make public statements, gave their scientific and political estimates of the Soviet bomb's implications. Others, who later debated the H-bomb decision vigorously, refrained from public discussion until the decision was announced. According to Hans Bethe, they exercised meticulous discretion and respected the "secret deliberation of the Government" which was not yet public.[1]

Some familiar themes echoed in the pronouncements of scientists who did not refrain from public discussion: excessive secrecy, international control, and now the strategic and foreign policy ramifications of the prospects of atomic parity; also familiar and untenable assumptions: that weapons cause political difficulties, and that the devastating effects of atomic weapons would dictate a peaceful settlement. The persistence of these historically questionable assumptions demonstrated an utter lack of sophistication in the nature of the deep political divisions of the Cold War which had prevented international control

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[1] See his statement in Oppenheimer Transcript, op. cit., p. 333.

thus far. In the minds of some scientists, the Soviet blast made war "unthinkable." Philip Morrison's remarks are illustrative. "The myth of a cheap war, a preventive war, an absolute and one-sided war--that dangerous myth is shattered. Peace has won," he said, "but we who are her proponents cannot sit idle, but must seal her victory by building friendship between [the] two countries."[1]

This did not prove easy, and scientists were bewildered by the difficulty of the problem. As late as 1961, Linus Pauling could ask why morality could not enter into the process of choosing policies, why it took so long to agree with the Soviets in the test ban talks on the number of inspections. Admitting that he did not understand politicians and dismissing them summarily, Pauling, with unfounded optimism, looked forward to "the abolition of the great immorality of war."[2] Implicit in arguments such as Morrison's and Pauling's was

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[1]These views were expressed in a speech given at a dinner of the National Council of American-Soviet Friendship. See New York Times, October 7, 1949, p. 7.

[2]Pauling spoke at Princeton University, Princeton, New Jersey on April 20, 1961 on "World Peace and Emergence of Science." His extreme position illustrates a kind of political naivete which characterized the response of some scientists.

the belief that science would be successful in devising useful control methods, and that scientists could provide the leadership for a more moral world.

This view was vividly expressed in the objectives of the Society for Social Responsibility in Science: to develop "constructive alternatives to militarism" and to "abstain from destructive work" in the tradition of Leonardo da Vinci, who refused to make public his submarine invention "lest man put it to evil purposes."[1]

Although not a new position, the times favored its crystallization into a formal, though small and not especially effective, organization. After the Hiroshima and Nagasaki bombs were dropped, most scientists experienced a revulsion to war work. Some, however, accepted the fact of the convergence of science and politics and tried to function within this context. Others, as the Society's members and, to a certain extent, scientists who supported world government and immediate international control, in effect ignored the reality of that context.

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[1] This was an expression of the "scientific conscience" by some 600 scientists, the most illustrious of whom was Norbert Weiner, the mathematician. See his "A Scientist Rebels," BAS, 3 (January 1, 1947), 31; and "A Rebellious Scientist After Two Years," BAS, 4 (November 1948), 338-39. Other nationalities apparently expressed interest in joining this movement; none included the Soviets or their allies. See *New York Times*, September 18, 1949, p. 71

These seemingly inflexible responses persisted and remained insensitive to the fact that, as long as sovereignty was a meaningful concept in international relations, there was no easy substitute for a diplomacy which attempted to coordinate all the factors in a situation. An attitude of terror toward atomic weapons could not contribute to a calm and dispassionate attack on the problem of security;[1] nor would it, or could it, produce an automatic impetus toward world government. In short, these scientists were correct in pointing to the awesome destructive potential of atomic weapons, but they mistook the expression of lofty ideals for a reasonable guide to practical action.

However, for scientists who could not ignore the consistent intransigence of the international control discussions, now combined with a changed political situation, a wider response seemed appropriate. In the absence of a workable control system, reasoned Fermi among others, American atomic supremacy "at present seems the only sure guarantee of peace."[2]

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[1] For an early warning of this sort, see Bernard Brodie, The Atomic Bomb and American Security, Yale Institute of International Studies, Memorandum #8 (November 1, 1945), p. 28.

[2] See New York Times, October 9, 1949. See also ibid., September 24, 1949, p. 2 for John R. Dunning's view to this effect, with the additional reminder that, in the final analysis, peace is had through international cooperation and goodwill.

This was not a new realization. Basic Soviet-American difficulties were perceived shortly after the AEC began functioning in 1947 and led to the unpleasant conclusion described by Oppenheimer some years later. He observed that the GAC had decided "without debate" and "with some melancholy" that the Commission's principal task was to provide many and good atomic weapons.[1] The end of the American monopoly merely accentuated this need. These scientists believed that continuous atomic supremacy required two basic approaches: the encouragement of reasonable security regulations and political policies to meet the problems of a weak Europe, national defense, and international control.

IV

Scientists of all policy persuasions agreed on flexible security rules. Bacher summed up the crux of their statements. He said:

> One of our present difficulties is that there are just too many things about atomic energy today which are called secret to keep them all under wraps. If we persist in this direction it is inevitable that sooner or later, we are going to lose some real secrets.

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[1] Oppenheimer Transcript, op. cit., p. 69.

> A little more hardheaded thinking would show us that we are not only holding up our own development by our present policy of blanket security, but we are also jeopardizing some information that we would really like to keep secret.[1]

From the beginning they warned against complacency, pointing out that secrecy on fundamental knowledge of nuclear energy would at best provide only a few years leeway before other nations draw abreast.[2] Urey bitterly complained that

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[1] Statement of Robert F. Bacher in New York Times, October 6, 1949, p. 3. For variations on this theme see Thorfin R. Hogness's remarks, ibid., November 26, 1949, p. 7; Lilienthal's statement on the difficult task of judgment the AEC faced in fulfilling its statutory function to disseminate basic scientific and technical information, ibid., November 29, 1949, p. 4; Killian's assertion that secrecy hinders scientific advance inside and outside of the universities, ibid., December 7, 1949, p. 11; and Urey's flat observation that Russia will surpass the United States unless there is "less witch-hunting and more work," ibid., October 22, 1949, p. 5.

[2] Without an efficient international agreement, reported the Franck Committee in June 1945, the nuclear armaments race will have seriously begun no later than the day after the United States demonstrated the existence of nuclear weapons. See its memorandum to the Secretary of War, June 1, 1945 in BAS, 5 (October 1949), 262. Four years later Franck observed that the correctness of the prediction did not make scientists happy for, under the prevailing political circumstances, the implications could only be dire. In 1947, Harold C. Urey stated that the American monopoly could be broken anytime after the fall of 1948, see Oppenheimer Transcript, op. cit., p. 327. Also Frederick Seitz and Hans Bethe, "How Close Is the Danger?" in Dexter Masters and Katharine Way, eds., One World or None (New York: McGraw-Hill Book Co., Inc., 1946); also Karl W. Deutsch, "The Impact of Science and Technology on International Politics," 88 Daedalus (Fall 1959), 672.

the advice of scientists, the only ones who could legitimately claim any real knowledge about the "secret", was ignored. He warned against any attempt to seek protection "behind a scientific Maginot Line;" such an approach would continue to blight the atomic energy program.[1] A searing editorial in the Bulletin of Atomic Scientists questioned the competence of Joint Committee members to offer constructive suggestions. It asked: "How can we have respect for the judgment of a jury which refuses to listen to the most important evidence in the case because it is afraid the members might talk about it in their sleep?"[2]

Excessive secrecy was not only incompatible with "doing science" but inimical to basic American interests. In unequivocal terms, Frederick Seitz warned that the Soviet Union could surpass the United States in bomb production and

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[1] See BAS, 5, (October 1949), 265.

[2] The evidence in question was the stockpile figure and other atomic weapons statistics. Senator McMahon was the sole member of the Committee who clearly wanted to know the size of the stockpile. Others were hesitant. See New York Times, December 10, 1949, p. 5. For the editorial see BAS, 7 (December 10, 1949). See also Memoirs, 2, op. cit., p. 297 for an example of Congressional ambivalence about atomic energy information.

other parts of the atomic energy development.[1] The Soviets had exhibited a concentrated and successful drive for atomic weapons, demonstrating a keen interest in becoming an atomic power and, perhaps, explaining Russia's reluctance to negotiate an international control agreement before it had the know-how of an atomic bomb. As a result, the United States faced a dangerous situation despite its lead in numbers and atomic weapons systems. Thus far, American strategic thinking, noted Bernard Brodie, had progressed at too leisurely a pace, if at all.[2]

At the same time that these scientists supported continued American leadership in atomic energy, they were equally interested in resolving East-West political difficulties. Atomic weapons merely provided a means to deter aggression, thus allowing time for their settlement. The sustained challenge to strategic <u>and</u> foreign policy concepts had to be met. Their political proposals, for the most part unsophisticated in terms of possible achievement, were directed toward this objective. However, some were so preoccupied with the destructive potential of atomic power they tended to forget that a reasonable plan of action had to contain

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[1] Seitz listed the Russian sense of urgency, their knowledge of the Hiroshima success, and the Smyth Report as significant factors in favor of Russian achievement.

[2] "What is the Outlook Now?" <u>BAS</u>, 5 (October 1949), 268.

seeds of political possibility; that it was insufficient to describe the horrors of atomic wars and expect all nations to act magnanimously when this did not appear to be in their special national interest. Nonetheless, they enunciated policies with the assurance of individuals who are successful professionally, but who know little outside their specialty.

Although scientists were correct in their efforts to re-examine the political situation, some of their proposals for meeting the East-West conflict and the weapons problem often demonstrated a persisting naiveté about political conditions. In proposing policy, they did not examine the consequences which may be expected in view of the policy set forth. It was this more than the policy content of their proposals which was disturbing.

Szilard and Rabinowitch's arguments illustrate this characteristic especially well. For example, Leo Szilard's proposal that the Soviets and Americans agree to the neutralization of areas lying between their strategic aspirations, such as Western Europe, was designed to erase potentially disturbing areas, therefore the need for overseas bases, and include a general reduction of armaments.[1] But, although it

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[1] Almost parenthetically, Szilard considered the possibility of war in which event the United States would attack the Soviet Union through the Balkans or the Middle East. The implicit assumption here was that both nations would keep their pledge with regard to Western Europe. This was inconsistent with statements by other scientists in 1945-46 who pointed out the uselessness of treaties which are always broken. See "Shall we Face the Facts?", BAS 5 (October 1949), 269-70, 272.

may have partially allayed his discomfort with protracted and difficult Soviet-American political differences that would "obviously lead to war" when weapons were at their worst, Szilard failed to consider negative Western European reactions; assumed that Russia's sole interest was territorial independence and that, in case of war, it would not overrun Western Europe; and did not provide a realistic political and strategic context for settling international problems such as Germany and Austria.

With the same disregard for political complexities, Eugene Rabinowitch, influential leader of opinion in the scientific community, justified his position because the facts were alarming and required grave decisions and radical new departures. Scientists do not want to create public hysteria, he wrote; they have a rational and dispassionate attitude toward human problems. But, "military planning," Rabinowitch asserted, "which sees security in the acquisition of a maximum amount of arms, ships, and trained men, and political thinking which looks for security to the acquisition of the greatest number of treaty-bound allies" was an inadequate reply to the Soviet bomb.[1]

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[1]See his article, "Forewarned--But Not Forearmed," BAS, 5 (October 1949), 275.

It was not clear what did constitute an adequate reply. Rabinowitch indicated what seemed to some scientists a hopeful sign. Soviet acquisition of the bomb removed Russia's feeling of atomic inferiority and justified a thorough policy review and "an unprejudiced exploration of any new possibility which may offer itself."[1] Rabinowitch and his colleagues were perhaps correct in thinking that conditions were sufficiently different to merit another try. At least negotiations could now proceed between equals with greater possibilities for agreement.[2] But even though the Russian bomb had augmented the need for a reliable control system, the problem's basic intransigency had not diminished. A severe lack of trust between the two countries still existed. The United Nations Atomic Energy Commission's Third Report to the Security Council recognized that effective control measures rested on cooperation "in broader fields of policy." It indicated failure to obtain Soviet agreement on "those elements

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[1] Ibid.

[2] The Soviet Union had fundamentally objected to the fact that the United States, in the period of transferring its atomic capability to the United Nations, would be the only nation with an actual monopoly of fissionable materials and production know-how in addition to a preponderance of atomic energy knowledge in general. See Chester I. Barnard, op. cit., for the issues surrounding the control question and a concise discussion of the implications for an arms race which the devaluation of the bomb, as one British commentator put it, provoked.

of effective control considered essential from a technical point of view." Yet, Rabinowitch still maintained that if discussions were conducted on a "matter of fact basis," the Commission's major task to evolve effective control mechanisms would be facilitated. Clearly, he did not associate agreement "in broader fields of policy" with agreement on technical questions.[1]

The history of the United Nations attempts to cope with international control was a story of irreconcilable differences.[2] Four years of continued bad relations had had a sobering effect on the expectations of American negotiators. Nevertheless, scientists continued their efforts for control and found unexpected support in the public discussion following the President's decision from heightened Congressional interest in international control.

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[1] See Frederick Osborn, "The United Nations Faces the New Situation," BAS, 5 (October 1949), 277, and Eugene Rabinowitch, "The Narrow Way Out," BAS, 4 (June 1948), 185. Osborn was Deputy U.S. Representative on the UN Atomic Energy Commission.

[2] The discussions which had been broken off in July 1949 were again resumed in the fall of 1949 and after September 23rd the UN group had before it a different situation. Both great powers now had the bomb. But, these talks were terminated unilaterally on January 19, 1950 by the Soviet Union in protest against the lack of UN recognition of Communist China.

Urey more sensibly recognized the limitations established by seemingly irreconcilable Soviet-American differences.[1] In brief, he proposed a strong Atlantic Union to give the West unquestioned strength which might stimulate a peaceful solution despite an increase in bomb efficiency;[2] to bring control to a large part of the world; and to promote a "more revolutionary idea,"[3] thus rendering communism less attractive.

Still, Urey's security formula was based on an untenable postulate. In their counter argument, Seitz and Bethe demonstrated a more sophisticated appreciation of what motivates national action. Urey's idea that one side would not begin a war that it could not win and the other would not need to start one because the weaker side could not attack was refuted as early as 1946 by Seitz and Bethe.[4] They argued that storage

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[1] See "The Paramount Problem of 1949," <u>BAS</u>, 5 (October 1949), 283-88 for a full description of his views. See also <u>New York Times</u>, December 15, 1949, p. 24.

[2] Urey used the same argument in supporting the building of the hydrogen bomb. See <u>New York Times</u>, January 28, 1950, p. 6.

[3] Henry A. Murray recently proposed the writing of a book equal in force to Marx, to provoke a "revolutionary conversion of human nature" and "to provide the needed philosophical and moral basis for a creative foreign policy." The idea is stimulated by the belief that the war for men's minds has to be won with better ideas, not merely with better missiles. See "Unprecedented Evolution," <u>Daedalus</u>, 90, (Summer 1961), 562-63.

[4] See Frederick Seitz and Hans Bethe, "How Close Is the Danger?", in Masters and Way, eds., <u>op. cit</u>.

of more powerful bombs would add little defense value unless the United States was willing to use its bomb supply to prevent attack. Furthermore, they observed, "if history provides any lesson, it is that fear of reprisal has never prevented a war in which the chances for quick victory are as great as they would be if the adversary decided to strike rapidly and in full strength with atomic bombs."[1] Besides, highly centralized American and European industrial centers provided tempting targets.[2]

V

This brief account of scientists' ideas in the fall of 1949 for political designs looking toward peace and security demonstrates that, although most shared a certain simplicity in their approach, or a need to demarcate the problem clearly by proposing neat solutions, sufficient variations emerge to indicate a slight range of political understanding, or at least political awareness. Szilard and Rabinowitch were

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[1] Quoted in <u>BAS</u>, 5 (October 1949), 254.

[2] Eugene Rabinowitch noted that as early as 1945 atomic scientists were alerting high government officials to the especial vulnerability of American and European cities to atomic bombs. See Rabinowitch, <u>op. cit</u>. For early warnings see the Franck Report, <u>BAS</u>, 1 (May 1, 1946), 2-4, 16; also Memorandum from Leo Szilard to President Roosevelt in March 1945, <u>BAS</u>, 3 (December 1947), 351-53.

furthest away from distinguishing the possible from the desirable. Urey's acceptance of the need for American atomic supremacy to deter war enabled him to avoid Szilard's mechanistic proposal, but not totally. Seitz and Bethe's emphasis on historical precedent indicated a more knowledgable grasp of international political relations and explains, in part, their moderate reaction to the President's H-bomb decision which considered the difficulty of that choice. Reactions such as Morrison's and Pauling's, not to mention the Society for the Social Responsibility of Science, completely lacked any political sensitivity.[1]

The Soviet atomic challenge had clearly provoked scientists' concern about Western Europe's role in the new atomic context, the relationship of weapons development and adequate defense, the international control problem, and security regulations for scientific research. Their colleagues inside policy circles considered these same issues in the Government's discussion of the H-bomb question.

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[1] Such an observation does not intend a criticism of scientists' deep interest in resolving political problems; only that any such contribution must contain some elements of realistic achievement. Otherwise, it is irrelevant to practical questions.

Chapter VIII

Scientists and the Private Debate

I

The question of whether or not to proceed with a crash program to build the hydrogen bomb was a deeply troublesome one. The advice of the General Advisory Committee, its reasons for it, the use made of it, and opposition to it testified to its complexity. Advisory scientists and those outside the GAC, who sought the ear of important government officials, were in a strategic position to influence policy. Such attempts illuminated the difficulties of giving and receiving scientific advice on a question which carried profound political, military, and social implications.

II

The task of devising and implementing atomic policy was an arduous, lonely, and often thankless one. The AEC welcomed the critical and sympathetic support of the GAC, which did not share the Joint Committee's reluctance to express policy views.[1] Having dealt with atomic energy matters since their advent, GAC members were expert on all aspects of the atomic energy program.[2] This competency surpassed that of the Joint Committee and, initially, that of the Commission.

The Atomic Energy Act gave the General Advisory Committee a clear mandate to "advise the Commission on scientific and technical matters relating to materials, production, and research and development." In fulfilling the mandate, the Committee did not adhere strictly to the terms. As Oppenheimer indicated, the political background against which decisions were made could never be ignored by the GAC. "I don't want to pretend," he said, "that scientific advice

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[1] This situation was to change. After the Hickenlooper hearings, the Joint Committee became progressively more interested in playing an active part in atomic energy developments. See Thomas, op. cit., pp. 52-58 for some issues which provoked splits along party lines and general friction between the Commission and the Joint Committee.

[2] The GAC included scientists and engineers, namely: James B. Conant, at that time President of Harvard University; Lee A. DuBridge, President of California Institute of Technology; Enrico Fermi of the University of Chicago; I. I. Rabi of Columbia University; Hartley Rowe, Vice-President of United Fruit Company; Glenn T. Seaborg of the University of California; Cyril S. Smith of the University of Chicago; Hood Worthington of Du Pont and Company, and J. Robert Oppenheimer who was to be chairman of the GAC from its inception to 1952. Oliver E. Buckeley, chairman of the board of Bell Telephone Laboratories, had replaced Worthington by 1949-50.

in practical matters is like doing an experiment just for the purpose of satisfying your curiosity."[1]

The new Commission, in its initial staff recruitments, leaned on the GAC's knowledge of atomic installations at Los Alamos, Sandia, and the Argonne Laboratory. The GAC not only answered the Commission's questions but also suggested new programs.[2] As the Commissioners assumed actual direction of the program, the GAC "tended to let the questions come from them." Often these did not concern purely technical and scientific matters but such questions as security procedures, which would be fair to scientists, and the custody of atomic weapons. Also, observed Oppenheimer, "the very broad terms . . . in which the Commission addressed to us the question of the super bomb was another example . . . where it did not consult us purely on the technical problem, but asked advice in which supposed technical competence and

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[1] Oppenheimer Transcript, op. cit., p. 67.

[2] In a letter to the President at the end of the first year of operation (December 31, 1947), the GAC described the inadequate state of the program when the AEC took over. "Important questions of technical policy were undecided, and in many cases unformulated." It concluded: "It has thus been our function to assist the Commission in formulating technical programs, both for the short and for the somewhat longer term." See Memoirs, 2, op. cit., pp. 299-300.

general good sense were supposed to be blended."[1] Oppenheimer recalled that on many occasions the GAC "bowed out" on matters not technical and scientific but frequently it was "seduced" into a consideration of them.[2]

This relationship was mutually pleasing. The GAC, whose members were largely products of the wartime advisory team of scientists, developed a continuous concern about the atom's social and political implications. It was eager to help design policy for the use of science and did not believe that it was seriously violating its mandate in replying to such questions as organizational matters. The GAC view of a <u>general</u> advisory committee's scope of action became important for the evolving relationship of the scientific expert and the civilian political officer who ranked above the expert advisor in the hierarchy of decision-makers.

On the Commission side, especially under Lilienthal's chairmanship, the AEC favored the GAC's full participation for understandable reasons: the technical competence of the Committee's members in atomic energy and also the prestige of

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[1] <u>Oppenheimer Transcript</u>, <u>op. cit</u>., pp. 67-68.

[2] <u>Ibid</u>., p. 68.

the individual members, most of whom had held important administrative and advisory positions in the Manhattan District Project. Each alone could command the attention of his scientific peers and high government officials. Furthermore, the Commission found its advisory group compatible within itself. The GAC functioned with a minimum degree of friction, offering a united front to the Commission.

Thus, when in that eventful October the AEC asked the GAC for an opinion on the state of American preparedness and the feasibility of a thermonuclear crash program, it did so within a framework of trust. This was perhaps unusual with new organizations. But, in this instance, the AEC-GAC club, still small and exclusive, achieved a smoothly functioning relationship. It was, however, to be disrupted by the intrusion of other scientists and top level officials with national security responsibilities.

III

As the GAC considered a reply to the Commission's assignment, four other governmental institutions--the Joint Committee, the Departments of State and Defense, and the Office of the President--were involved. These had all been continually concerned with issues of defense strategy, international control, and of Western European recovery.[1]

In July 1949, the President asked his special committee of the National Security Council[2] to assess the atomic energy program's rate of progress, for priorities had to be set for the defense dollar. Both the Special Committee and the Joint Chiefs of Staff analyzed the problem of achieving a balanced defense system. The Committee concluded that atomic weapons production should be stepped up. Second in importance was the high priority assignment of the B-36, the long-range plane which could deliver the newest A-bomb.[3]

During this time, and previous to it, an effort was made to keep evaluations up-to-date.[4] The continuous

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[1] See Schilling, op. cit., 24-46.

[2] Memoirs, 2, op. cit., p. 302. The Committee included Secretary of State Dean Acheson; Secretary of Defense Louis Johnson; and Chairman of the Atomic Energy Commission David E. Lilienthal.

[3] Ibid., pp. 304-05.

[4] E.g., in 1948, the report of the President's Air Policy Commission, formally titled "Survival in the Air Age" and commonly referred to as the Finletter Report, was issued. See Schilling, op. cit., p. 28 for his observation and elaboration of the point that the Finletter Report did not offer a new strategy so much as an outline of the problems and choices around which the discussion of strategy was to revolve subsequently.

discussion of American national security, demonstrated in reports, evaluations, and policies was frustrating in its contradictory elements. The three major postwar objectives of American policy--international control of atomic weapons, the restoration of a balance of power in Europe, and a doctrine and strategy for atomic warfare--were not synchronized. Schilling has rightly pointed out that

> each of these three policies had the potential of pointing the American response to the Russian explosion in a different direction. With the passing of the American monopoly on the atomic bomb, the defense of Western Europe might now require a larger commitment of ground forces than had heretofore been necessary. The need to prepare for two-way atomic war, on the other hand, would seem to call for the allocation of additional resources to the weapons for air attack and defense and an expansion in the size of the nuclear stockpile. Finally, the development by the Russians of their own nuclear weapons could be seen as the proper occasion to reopen and redouble the effort to secure their control by an international agency.[1]

These national security questions were by no means new. The Soviet atomic explosion had, however, given them added urgency and poignancy. It was not surprising if the GAC recommendations exhibited a certain pressing concern about the complex choices to be made. Its professional knowledge encouraged an understandable trepidation over the Soviet achievement.

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[1] *Ibid.*, pp. 28-29.

The fall of 1949 saw an ever-expanding and successful fission development program. As a result of nine months of concentrated study, the Military Establishment and the AEC had implemented an enlarged weapons production program, which President Truman formally approved on October 19. The AEC was not only to expand its research and production facilities; it was also to improve the quality and size of the atomic arsenal.[1] Hopes were high for developing fission weapons whose destructive power could obliterate almost any target; two would certainly destroy any target. At the same time, the thermonuclear weapon was still theoretical.

This was roughly the weapons situation when Lewis Strauss, an AEC commissioner, wrote a memorandum on October 5, strongly recommending that the AEC also undertake a full program to develop the hydrogen bomb. On October 11, Lilienthal referred the matter to the GAC in a letter to Oppenheimer. The question posed was: did the current and planned program constitute "doing everything that it is reasonably possible for us to do for the common defense and security." The GAC was urged to render its "advice

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[1] See Oppenheimer Transcript, op. cit., pp. 399-400 for Lilienthal's testimony and elaboration of the exact nature of the weapons improvement program, which had been approved or was actually in operation, and the problems of delivery.

and assistance on as broad a basis as possible."[1]

On October 14, the Joint Committee heard General Hoyt Vandenberg, on behalf of the Joint Chiefs, support the development of a fusion weapon. Also, throughout October, a vigorously active group of scientists from Berkeley, who were concerned about the Soviet bomb and the subsequent challenge to American atomic leadership, lobbied in Washington for a crash thermonuclear program as the best means by which the United States could regain its lead. This group, whose core included Edward Teller, long a supporter of a thermonuclear device, Luis W. Alverez and Edward O. Lawrence, pursued a lightning campaign across the country for support of their view. As Walter Millis described it, "they buttonholed everybody, from scientists to senators, sufficiently 'cleared' to be talked to."[2]

On October 28 and 29, the GAC held its eventful meetings and emerged with its controversial report. Lilienthal was careful to indicate that, at the time of the President's September 23rd announcement and presumably

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[1] Ibid., p. 401 for complete text of the letter.

[2] With Harvey C. Mansfield and Harold Stein, Arms and the State (New York: The Twentieth Century Fund, 1958), p.251. See also Oppenheimer Transcript, op. cit., pp. 770-805 for details of these conversations.

by October 28 and 29, the Commission had not received any Defense Department request for a weapon of indiscriminate destructive power; nor did it have any evaluation of the military value of a hydrogen bomb or a bomb which did not have a limit. Furthermore, the Commission had not been given any diplomatic or political evaluation of the ramifications of such a weapon on the Cold War, American alliances, and other international relations.[1]

Apparently, although the major participants had extensive and informal exchanges of views, no clear strategic and foreign policy guidance emerged. In this charged and fuzzy environment, the GAC met to consider essentially whether or not a full-steam program to develop the "Super" would constitute the proper response to the Soviet atomic bomb. The question is more accurately stated in two parts: Was the AEC program adequate in view of the Soviet success and, if not, how should it be modified; and should a crash program to develop the "Super" be part of any new program?

IV

The first part of the question provided little difficulty for the GAC. From the beginning it had promoted

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[1] Ibid., p. 400. See also ibid., p. 77 for Oppenheimer's testimony that prior to October 29 the military had not expressed an interest in more powerful weapons and that they were interested only in quantity.

better atomic fission weapons and the integration of these with a weapons system.[1] Now, with the Soviet bomb a reality, the GAC recommended numerous measures to increase general weapons potential. On the question of the Super, however, it was unanimously opposed to a crash program by the United States. A concerted attack on the problem, as it stood then, would have a little more than a 50-50 chance of success. Subsequent work at Los Alamos supported a more pessimistic view. As a matter of fact, the Super as conceived in the fall of 1949 was never achieved. The brilliant calculation which eventually produced a hydrogen bomb came much later.[2] Thus, the technical basis for GAC opposition was upheld. Support of a crash program, it reasoned, required much greater and more concentrated attention on fusion research. It meant the construction of production facilities to provide the materials necessary for the Super, once the technical knots were untied. This,

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[1] The Sandia Laboratory was established for weapons testing. Its basic reason for existing was that radical improvement in weapons development necessitated a program of testing these weapons. Ibid., pp. 17 and 400. See also ibid., p. 18 for Oppenheimer's comment on the GAC's role in the four major expansions of AEC activities during 1946-1952.

[2] Ibid., pp. 84, 720.

in turn, pointed toward a diversion of effort from plutonium production essential for the expanded atomic fission program which had been approved by the President some nine to ten days earlier.[1]

On the basis of efficiency and logic, the General Advisory Committee thought it unwise to divert any effort from a highly promising atomic fission program[2] to a fusion weapon, whose theoretical base was thus far disappointing and, furthermore, whose deliverability was questionable. Although it would be a weapon of tremendous size and destructive capacity, its military usefulness was not evident. Thus, for the present, the GAC opposed the launching of a crash program. It proposed, however, that thermonuclear research should continue as before.

If the GAC ranged over aspects of this highly complex question which were not purely technical and scientific, it should not have been surprising. Not only did its October 11th mandate give a large leeway for advice but no event thus far had forced as wide a consideration of the issues as the

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1 <u>Ibid</u>., pp. 399-450.

2 The new program was to increase the energy of fission weapons to twenty-five times the power of the first two bombs, to increase the stockpile, to improve the production processes, and to develop various groupings of tactical atomic weapons.

Soviet atomic explosion. Before its October 29th meeting, the GAC consulted extensively with civilian and military officials from the Department of State and Defense.[1] On October 30, it submitted its findings.

The structure of the GAC report demonstrated a conscious effort to separate scientific and technical factors from military, political, and even moral ones. Added to the report itself, which consisted of two parts, one on affirmative actions to be taken regarding weapons and the other on super bombs, were a letter of transmittal and two supplementary statements which allowed for the expression of slightly divergent politico-military viewpoints. These expressed the views of the GAC and provided a take-off point for other positions by scientists. They also provide an idea of the GAC's image of its advisory function, created by the responsibility for the use of atomic weapons felt by the scientist-members and the Commission's expectations of its advisory committee.[2]

The report considered fission weapons and the super program. In line with previous GAC advice, it stated that

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[1] Oppenheimer Transcript, op. cit., p. 77.

[2] Ibid., pp. 78-83.

the AEC could do more to increase its efficiency in weapons expansion, improvement, and diversification. Some of these recommendations required new kinds of plants to accommodate further expansion.[1]

From 1947 to 1949, the GAC urged increased production of fissionable and raw material. An especial recommendation was made for the production of neutrons "which we knew would be very useful in some way or other without particularly specifying where the use would come."[2] The production of these materials, the GAC believed, might be fundamental if a Super were achieved. In March 1950, a clear go-ahead on this was given so that Rabi could point to the Savannah River project "as the way we answered the Russian success."[3]

The report described the thermonuclear device in question, what would have to be done to obtain it, and the design it would have. It explained that the Super would actually have to be built and tested in order to determine whether or not it worked. The extent of damage and the problem of deliverability were also discussed. The Super,

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[1] _Ibid_., pp. 77-78.

[2] I. I. Rabi in _ibid_., p. 457.

[3] _Ibid_.

it was concluded, would be useful only for very large targets. Otherwise, it would not be "economical in terms of damage per dollar."[1] Even then, it was uncertain whether the blast effect would merit the cost.[2] Although the GAC's estimate of feasibility at the time was negative, the report never stated that the Super was unfeasible. Some, however, found this statement of uncertainty too conservative, asserting that the thing could be done faster and with certainty.

Rabi explained a seeming disagreement on a technical question. In its report the GAC had opined that an "imaginative and concerted attack on the problem has a better than even chance of producing the weapon.[3] When asked whether this was supposed to be an accurate consensus of GAC views, Rabi replied:

> More or less. When you are talking about something as vague as this particular thing, you say a 50-50 chance in 5 years, where you don't know the kind of physical factors and theory that goes into the problem. . . . it was a field where we really did not know what we were talking about, except on the basis of general experience. We didn't even know whether this thing contradicted the laws of physics.[4]

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[1] *Ibid*., pp. 78-79.

[2] *Ibid*., p. 79.

[3] *Ibid*.

[4] *Ibid*., p. 454.

Thus, it seemed that since the problem could not be rigorously stated or since it was not theoretically valid, there was room for differing estimates. What determined them was not clear. The GAC had in mind, although not clearly enough, observed Oppenheimer, "a single design which was in its essence frozen."[1] The possibility of other ways of achieving a hydrogen bomb was apparently sidestepped. Perhaps there was a subtle unwillingness to develop a weapon with colossal destructive power. Several sentences of the technical report, which were moral in tone and content, and which Oppenheimer described as the "crux of it" evidence this.

> We all hope that by one means or another, the development of these weapons can be avoided. We are all _reluctant_ to see the United States take the initiative in precipitating this development. We are all agreed that it would be _wrong_ at the present moment to commit ourselves to an all-out effort towards its development.[2]

This moral position was, in Oppenheimer's opinion, an important component of the technical part of the questioned. He emphasized the Report's fundamental premise: that the existence of fusion weapons "would be a disadvantageous thing" from a humanitarian viewpoint. "I think," he said, "it is very clear

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[1] _Ibid._, p. 78.

[2] _Ibid._, p. 79. (Italics added)

that the objection was that we did not like the weapon, not that it couldn't be made."[1]

The military features of the problem were almost impossible to avoid. The insertion of moral concepts, along with a technically competent and accurate report, could not but affect a willingness to consider a new and more devastating weapons program. Oppenheimer himself emphasized that "lack of feasibility is not the ground on which we made our recommendations."[2] Also, lack of economy was not the major or only ground for negative arguments on the Super. The real reason for this opposition could be found in a highly explicit desire of the GAC to have a mobile and multi-faceted fission program.[3]

Beyond this, political, military, and philosophical reasons inevitably interlaced with technical considerations, giving added meaning to the technical report. The technical discussion could hardly be kept distinct. It oscillated between the two parts of the question which understandably impinged on each other: one relating to fission weapons and the other to the Super. Although the so-called majority and

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[1] Ibid., pp. 79-80.

[2] Ibid., p. 79.

[3] Ibid.

minority reports, attached to the main report, contained a more explicit exposition of the clearly non-technical aspects, the totality of the GAC recommendations was in essence a policy statement. The intensity of official reaction questioned its appropriateness as a response to the Soviet bomb and exemplified its interlocking technical, military, political, and ethical features.

Where military and political features interacted, there was a divergence of views among GAC members. Although agreed that it was not the right time to proceed with a crash thermonuclear program, they differed on a subsequent and related question: whether to establish a clear policy not to proceed with the program, or whether to declare that conceivably some future circumstances could modify the present unanimity on the crash program issue.

In their minority report, Fermi and Rabi assumed the latter position. They maintained that an attempt should be made "to outlaw the thing before it was born,"[1] arguing that this might be easier done before the bomb was developed. Since they could not see any tremendous gain from a deliverable weapon of the specific design in question, they believed that the President should make some political gesture, giving the United States a more solid moral position. They wrote in

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[1] _Ibid_., p. 395.

part:

> The fact that no limits exist to the destructiveness of this weapon makes its very existence and the knowledge of its construction a danger to humanity as a whole. It is necessarily an <u>evil</u> thing considered in any light. For these reasons, we believe it important for the President of the United States to tell the American public and the world that we think it is <u>wrong</u> on fundamental ethical principles to initiate the development of such a weapon.[1]

Such an official statement would enable the United States to proceed from a stronger ethical base if the Russians reacted unfavorably toward a mutual agreement to ban the H-bomb. Failing a reasonable agreement, Fermi and Rabi, "with considerable regret",[2] would then not oppose a thermonuclear program.

The majority opinion proposed a unilateral announcement that the United States would not in any case make the weapon under advisement. "In determining not to proceed to develop the super bomb," it declared, "we see a unique opportunity of providing by example some limitations on the totality of war and thus of eliminating the fear and arousing the hope of mankind."[3] The objection, then, was not based so much on the current inaccessibility of such a weapon as in

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[1] Ibid., pp. 79-80. (Italics added)

[2] <u>Ibid</u>., p. 395 in the words of Fermi.

[3] <u>Ibid</u>., p. 80.

a revulsion against its unlimited destruction. Later, however, Oppenheimer noted that had the theoretical basis of a hydrogen bomb been as advanced then as it subsequently became, it was speculative whether this view would have prevailed. In his opinion, these were total viewpoints which considered how good the Super was, what could be done with it, what competed with it, how the enemy might move, and the degree to which the Super's construction was an inevitable step.[1]

The majority position resisted a modification of the policy statement advocating a high priority rating for the fusion device sponsored by Teller and his Berkeley colleagues. In other words, the GAC report and the majority annex opposed not merely a crash program but the program itself.[2] On the other hand, the minority view was prepared, failing an international agreement, to go ahead "whatever going ahead were to mean," said Rabi.[3]

The basic objection of the GAC majority was to avoid a thermonuclear arms race which, in its opinion, would favor

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[1] Ibid.

[2] When the Commission questioned the GAC report on that distinction, Oppenheimer observed that "we made it quite clear that this could not be an unqualified and permanent opposition. Obviously if we learned that the enemy was up to something, we could not prevent the production of a super bomb." See ibid., p. 237.

[3] Ibid., p. 455.

American interests in several ways. In the face of super bombs requiring large targets, the United States was much more vulnerable, yet much less likely to initiate an attack. Furthermore, the uncertainty of the thermonuclear device and its unwieldy nature, should it come about, and the vastly improved and continuously improving fission weapon did not seem to provide a sound military basis for diverting resources from a successful program to an untried and unpredictable one.

In addition, there was good chance that if the United States refrained from making super bombs, the Russians would also refrain. This belief stemmed from the idea that Soviet atomic work had been imitative and spurred by knowledge of the American success. Since it was likely that their thermonuclear work would also be imitative, the United States ought not to set an example for developing fusion weapons, but to renounce such a development. Another deterring factor for the Soviets would be the prohibitive cost,[1] it was argued.

Behind all these arguments of the GAC lay the hope that this policy recommendation would somehow help channel

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1 This had been disputed earlier by some scientists. Oppenheimer later concluded that he was no longer sure of the effect on the Russian development of a thermonuclear bomb that an American decision not to proceed with a crash program would have had. This is a controversial point which cannot be resolved.

possible atomic friction into "less totally destructive methods" than those which could obliterate the great population centers. In short, bigger bombs did not comprise a valid reply to the end of the American atomic monopoly and the launching of an atomic arms race. The majority, unlike the minority view, apparently did not concede that in the event of a failure to negotiate an agreement to ban the hydrogen bomb totally, the United States would have no alternative but to proceed with a super bomb.

To summarize, the position of the General Advisory Committee was to stop, look, and reappraise the situation. It was severely aware of the incremental aspect of bigger and better weapons. It argued against the Super's military usefulness and for a continuous and ever-improving fission program to supply not only large bombs for strategic purposes but smaller tactical weapons for weening the United States away from an all-or-nothing military capability. Yet, fission weapons could be used in the same fashion as a hydrogen bomb for strategic bombing.[1] The GAC favored a balanced force with conventional weapons for a proper measure

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[1] See Hans Bethe on this point, *ibid*., pp. 339-40.

of security.[1] It favored renouncing unilaterally the making of the hydrogen weapon. In rather unspecific terms, some of its members urged another try at negotiating international control.

The underlying premise for the entire GAC position was that somehow a nuclear arms race had to be avoided, if at all possible. The development of its argument was not always consistent, nor exact, in charting a course through these rough waters. In essence, its discussion and resultant reports called for a thorough re-examination of the political and military implications of American policy. The GAC discussion and recommendations raised perplexing policy issues which the President's announcement of January 31, 1950 by no means solved.[2]

IV

The course of events moved quickly after the GAC submitted its recommendations to the Atomic Energy Commission. The AEC forwarded these to the President on November 9, together with its own report and separate statements from each

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[1] For an exposition of this idea see *ibid.*, pp. 447-48.

[2] See Schilling, *op. cit.*, pp. 36-46 for a discussion of the minimal character of President Truman's decision and its consequences.

of the Commissioners. The Commissioners agreed that this was a policy matter which only the President could decide. They did not believe that they could make a recommendation; nor that it was proper to do so since political and military factors were major considerations.

The AEC divided sharply on the question of a crash program. Strauss had favored such a program from the beginning. His purpose in calling for the GAC's opinion on October 5 was to have it consider how to obtain such a weapon, not whether it should be built. Although not as sure as Strauss, Gordon Dean favored a high priority for the thermonuclear field.[1] The other three commissioners, Henry DeWolf Smyth, Sumner Pike, and Lilienthal opposed the crash development of the H-bomb in view of the available information.

President Truman sought the advice of his Special Committee whose members, Lilienthal, Acheson, and Johnson, represented those departments most closely concerned with this problem. "I desire," he directed on November 10, "that the committee analyze all phases of the question including particularly the technical, military and political

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1 <u>Oppenheimer Transcript</u>, <u>op. cit</u>., p. 302.

factors, and make recommendations as to whether and in what manner the United States should undertake the development and possible production of 'super' atomic weapons." Truman also called for advice "as to whether and when any publicity should be given this matter."[1]

Both Lilienthal, whose position was close to the GAC majority, and Acheson understood the acute need for a broad and comprehensive review of fundamental policies. Lilienthal believed that any decision on an all-out program should be based on the results of such a review. Acheson favored the commencement of work on the H-bomb and the reappraisement of the total problem simultaneously.

Lilienthal was skeptical of this view, believing that a broad H-bomb program would prejudice the review. He staunchly supported a wide review of basic policies and another try at negotiations to obtain international control.[2]

Acheson did not see that the State Department had anything to gain from a delay in the fusion program. There were no substantial indications that the Soviet Union was

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1 <u>Memoirs</u>, 2, <u>op. cit</u>., p. 309.

2 <u>Oppenheimer Transcript</u>, <u>op. cit</u>., pp. 404-05. See also <u>New York Times</u>, October 4, 1953, Magazine Section, p. 13ff. for a discussion of his position.

interested in serious talks on the control question, and there was a real risk of losing the political initiative if the Soviets got the bomb first.[1]

Louis Johnson was anxious to embark on a program to discover if the Super could be had. A number of studies by Defense outlined some military advantages which could accrue from such a weapon.[2] This additional data probably encouraged Smyth and Pike to favor the thermonuclear development.[3]

The Joint Committee was also active. Strauss and Dean had appealed their case there and found immediate and strong support. Members of the Committee had journeyed West in October after a visit from Lawrence and Teller. They consulted with personnel at Los Alamos and Berkeley and "after talking with the scientists," Representative Melvin Price reported recently, the Committee "strongly recommended to President Truman that we accelerate this program."[4]

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[1] Note his public remarks right after the September 23rd announcement in New York Times, September 24, 1949, p. 2.

[2] Schilling, op. cit., pp. 38-39.

[3] Oppenheimer Transcript, op. cit., pp. 434-35.

[4] "Atomic Science and Government--U.S. Variety," Remarks made before the Washington Chapter of the American Nuclear Society, June 14, 1961, p. 3.

All these persuasions resulted in the Special Committee's recommendation to the President on January 31, 1950, the same day of the public announcement and a day after the Fuchs spy case broke in England.[1] It was to the effect that the Chief Executive order the AEC to proceed to a determination of whether or not a hydrogen weapon could be made and detonated. At the same time, it was recommended that "a re-examination of our foreign policy and our strategic plans both diplomatic and military" should be conducted.[2]

Truman's public statement was the first step in the achievement of a hydrogen bomb. The ensuing actions which flowed from the President's "minimal" decision were not slow in being taken. On February 24, the Secretary of Defense and the Joint Chiefs recommended acceleration of the program by "immediate implementation of all-out development of hydrogen bombs and means for their production and delivery."[3]

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[1] This betrayal by a high-ranking British scientist who gave atomic information to the Soviet Union deepened American security concerns. A tightening of security occurred, thus supporting the view of the Berkeley scientists that international control was virtually impossible in a compartmentalized world.

[2] _Memoirs_, 2, _op. cit_., p. 309. Lilienthal's agreement was obtained on this last condition.

[3] Quoted in _ibid_., p. 310.

The assumption was that the H-bomb test would be successful and it would be wise to be ready for production and use. Commissioner Pike, who was Acting Chairman (Lilienthal's resignation became effective on February 15), also agreed that plans for production and use were in order.

On March 9, the Special Committee reported to Truman that a test of the first step in developing the fusion weapon could be made in 1952. On March 10, Truman directed the AEC to plan for quantity production should the H-bomb prove feasible. This directive led to the construction of a dual purpose Savannah River plant. Along with improvements in the plutonium plants, it would provide sufficient material for the fission and fusion programs.[1]

The atomic program progressed, but it was not until April and May of 1951, during the Greenhouse tests, that some new experiments on thermonuclear reactions were made, smoothing the way for firing a megaton device a year and a half later on November 1, 1952.[2]

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[1] *Ibid.*, pp. 310-12.

[2] Tritium for these initial exploratory tests was produced at the Hanford plants. The "Super" of the H-bomb debate was not possible and was never built.

V

The decision of President Truman to proceed with a fusion program, looking toward an actual bomb, was a complete rejection of the GAC advice. The Departments of State and Defense, the Joint Committee, and the articulate group of scientists, largely from Berkeley--all combined for varying reasons to advocate an acceleration of thermonuclear work. Nevertheless, the impact of the GAC report and subsequent Commission insistence on finding answers to the troublesome questions, which were connected with the hydrogen bomb problem, were significant.

The GAC was correct in assessing the decisiveness of the fork in the road which was now reached. It essentially advocated a "holding" position. Although it supported the expansion of the fission program, it was not willing to develop a whole new family of weapons of even greater destructive capacity. Advocates of a fusion device, some scientists among them, were equally aware of the fork in the road but could see no reasonable alternative to this course of action. An affirmative decision on the H-bomb, however, did not erase the dilemma which plagued the GAC and all other participants. Termination of the American atomic monopoly seemed to spell the beginning of an arms race at the level of fission and fusion weapons. The public discussion of scientists following the Presidential decision was stirred by the implications of this dilemma.

Chapter IX

Scientists and the Hydrogen Bomb Decision

I

The awesome nature of the hydrogen bomb decision caused grave differences of judgment among high government officials. Scientists also did not escape the divisive influence of the postwar security problem. Painfully aware of the incremental aspects and implications of fission and fusion weapons, and now the possibility of mutual atomic destruction, they searched for ways to mitigate the serious political and weapons situation. Not all of them, however, agreed on means to achieve peace and security.

Scientists' public discussion after the President's decision revolved around three topics: (1) the technological feasibility, advisability, and the military worth of hydrogen bombs; (2) moral justification for building such weapons and the related theme of handling atomic weapons through international control; and (3) secrecy and scientific information, decision-making and public participation.

II

Scientists generally agreed that a fusion bomb could be had and that it would be an "open-ended" weapon with its size dependent on the amount of reacting materials in the bomb. The process of triggering this weapon, however, provided a significant obstacle to its development.

A hydrogen bomb needed a sizable quantity of heavy hydrogen and an atomic bomb to ignite it. The salient technical problem was to release a meaningful part of the energy of the heavy hydrogen before the material was scattered by the explosion. The President's decision assumes that "this development is both possible and feasible," concluded Robert Bacher, the first scientist-member of the AEC until the summer of 1949.[1] Many scientists believed that this could be accomplished although some, like Robert A. Millikan, were extremely doubtful.[2] Whatever dispute existed became academic after the Eniwetok tests in the spring of 1951 which demonstrated the likely technical feasibility of an H-bomb.

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[1] U.S. Congress, Joint Committee on Atomic Energy, The Hydrogen Bomb and International Control: Technical and Background Information, 81st Cong. 2d Session, July 1950, p. 34.

[2] See William L. Laurence, The Hell Bomb, (New York: Alfred A. Knopf, 1951), pp. 29-33. For discussions of technical considerations see Robert F. Bacher, "The Hydrogen Bomb: III," Scientific American, 182 (May 1950), 13ff; Hans A. Bethe, "The Hydrogen Bomb: II," ibid., (April 1950), 18-23. See also Bacher's speech on March 27, 1950 in Los Angeles, printed, in part, in The Hydrogen Bomb and International Control: Technical and Background Information, op. cit., pp. 32-34.

Nonetheless, some scientists questioned its technical advisability on economic and technical grounds. The hydrogen bomb was not a good investment when compared with a thriving atomic bomb program. It was not merely a matter of actual cost. The same uranium raw material and the same type of plant were required for both bombs; thus, a diversion of materials from the fission effort would be required. Also heavy hydrogen had the special characteristic of half of it disappearing every twelve years.

The GAC scientists in their Report and other scientists questioned the strategic value of an H-bomb stockpile. For them the decision to answer the Soviet success with a fusion bomb was a potentially "pyrrhic" response, since it involved a weapon uniquely suitable for destroying large urban centers, typical of the United States.[1] A single fission bomb, they argued, would be as effective for smaller industrial targets as a fusion bomb which would over-destroy at the center. Thus, although theoretically an open-ended weapon, practically its power was limited by the available supply of necessary materials, the instrument of delivery, and the design to ignite the heavy-hydrogen mixture.

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[1] See, e.g., Bacher, "The Hydrogen Bomb: III," op. cit., pp. 13-14; also Louis Ridenour, "The Hydrogen Bomb," Scientific American, 182 (March 1950), 13.

Recalling the almost extinct battleship, Bacher warned against a fascination with "the bigger the better." The hydrogen bomb did not significantly improve American military security and therefore was "not even a very good addition to the military potential." In short, Bacher was concerned lest the United States become preoccupied with the development of the hydrogen bomb, a "very dubious national policy."[1]

Scientists differed on this point. Louis Ridenour, for example, even though he also doubted the strategic value of the hydrogen bomb, reluctantly concluded that, in a "world of hotly nationalistic fear and jealousy," the decision made by the President was probably a right one. Furthermore, he observed that if it was in their interest to do so, the Russians would build an H-bomb regardless of any American example.[2]

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[1] Op. cit., p. 14; also Bacher's comments in New York Times, May 25, 1950, p. 18. See Hans Bethe in Alsop et al,op.cit. p. 69. Also New York Times, April 30, 1950, for a letter, signed by Harvard and MIT professors, criticizing over-reliance on strategic warfare based on atomic weapons to the neglect of tactical defense power founded on conventional weapons. Jerrold R. Zacharias, one of the signers, emphasized the argument in "A Citizen's View of Our National Security," BAS, 6 (July 1950), 218-19.

[2] Op. cit., p. 14.

Other scientific experts, like Teller, Lawrence, and Von Neumann, were more strongly convinced than Ridenour.[1] An H-bomb might hold the decisive lever in certain battlefield tactical situations, providing an excellent balance to the huge Russian manpower supply. Besides, for strategic, psychological, and political reasons the United States could not allow Russia to surpass it. It was not a matter of choice, the argument ran, and therefore no moral values were involved. A question which had moral values implied the possibility of choice. Since the answer to the H-bomb controversy revealed itself in the unsettled political situation, it would be gross negligence not to develop a hydrogen bomb when the Russians might. This would be immoral, these scientists believed, because it would endanger national security, which demanded any effort required for leadership.

The Air Force agreed. From its viewpoint, an H-bomb required less accuracy than an A-bomb and, since the United States was limited more by intercontinental bombers than nuclear weapons, it was important to risk as few planes as possible to accomplish any one mission. Other scientific and military experts doubted that the H-bomb constituted an absolute weapon, rendering precision bombing and a balanced

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[1] See, e.g., Edward Teller, "Back to the Laboratories," BAS, 6 (March 1950), 71-2; John von Neumann, "Can We Survive Technology?" Fortune, 51 (June 1955), 155 ff.

military force obsolete.[1] Nor, they continued, did it lessen the need for an international control system, which Teller and his colleagues thought an unlikely achievement in the current political atmosphere.

In sum, those against the President's decision often advanced technical reasons for not attempting to make the bomb. Those in favor simply said that the Cold War afforded the United States no other action. As scientists, however, all understood that technical scientific and engineering problems required a concerted effort to overcome them.

Generally, there was greater agreement on the H-bomb's technical feasibility. Differences arose in evaluating technical advisability and military value. The answers to questions such as whether or not the country's security would be enhanced better by it or by some other project (e.g., an improved guided missile system to deliver atomic bombs); whether or not the return would be greater from more efficient bombers, a wider radar defense system, or a submarine capability to launch atomic bombs; whether the United States could afford to subtract from its other military programs in order to develop an arsenal of fusion

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[1] See e.g., Hans Bethe, <u>Oppenheimer Transcript</u>, <u>op. cit</u>., pp. 339-40.

weapons were inevitably colored by the way in which scientists answered the basic question--should the bomb be made? Objections to, and support of a hydrogen bomb development, even when couched logically in technical terms, emerged largely from moral and emotional responses. In brief, differences in the interpretation of the scientific and technical facts arose from the personal orientation--moral, emotional, political, and military--of the particular scientist.

III

The rapid advance of science and technology impelled momentous decisions and created a disquieting awareness that man had to try to control this fantastic power, else, without his guidance, it would begin to dictate the future of civilization.

In 1945, the President's announcement that two atomic bombs had been used to end the war provided the first significant step toward this awareness. At that time, the American answer came in the form of the November 15th Truman-Atlee-King Declaration, recommending a UN Atomic

Energy Commission to make international control proposals.[1]

In 1950, the Presidential announcement that no new moves for agreement were necessary at this time increased the concern for international control.[2] Despite unhappy impasses and deteriorating Soviet-American relations, scientists, members of the Joint Committee, and others asserted that the United States should try again to obtain an international control agreement; that it could do no less in view of a decision which touched "the very basis of our morality," observed Oppenheimer.[3] Their position was given added ardor by the Administration's seemingly

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[1] The UN Commission was established in January 1946. From June 11, 1947, when the Soviet Union countered the Baruch Plan with proposals of its own, until January 19, 1950, when the Soviets terminated discussions by a walk-out on the question of Chinese recognition, no international agreement had been reached. John D. Hickerson, Assistant Secretary of State for UN Affairs, declared: "It has been just 2½ years now since any Russian has made, in the UN or out of it, in public or in private, officially or unofficially, a single new proposal for any phase of control or offered a single fresh criticism of any phase of the U.N. Plan." Quoted in Richard H. Rovere, "Letter from Washington," New Yorker, 25 (February 3, 1950), 52.

[2] New York Times, February 3, 1950, p. 3; ibid., February 10, 1950, p. 8.

[3] New York Times, February 13, 1950, p. 1.

intractable stand,[1] which appeared to indicate that the President had lost hope in ever achieving a workable agreement. Even some members of the President's official family were skeptical of this forthright refusal to make fresh overtures to the Soviet Union.[2] It was argued that without them the United States's moral position and hopes for a secure peace would be severely damaged.

Senator Brian McMahon, one of the strongest advocates for the hydrogen bomb, first sounded this clarion call. In a Senate speech on February 2, he proposed a "moral crusade for peace," advocating an exchange of economic aid for effective control.[3] Scientists whose support for the hydrogen bomb had been reluctant, but who had remained dis-

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[1] See New York Times, January 19, 1950, p. 12 for Dean Acheson's statement to this effect.

[2] Recall Lilienthal's view that to proceed with the hydrogen bomb without re-examining the problem with the Soviets would damage significantly the possibilities of agreement. Walter Lippmann wrote that "a new inquiry into the American policy and the American proposals" for atomic energy would have served to restore the confidence of mankind more than anything else. See Alsop et al, op. cit., p. 101.

[3] See New York Times, February 3, 1950, p. 2 for complete text. For Lippmann's criticism of the Senator's proposal, see Alsop et al, op. cit., p. 99. The Federation of American Scientists took up McMahon's economic aid proposal to Russia in exchange for inspection concessions from the Russians. For Senatorial support of McMahon's approach see e.g., New York Times, February 2, 1950, p. 7 for Connally and Vandenberg's comments.

creetly silent before the President's announcement, called for a new policy to meet a radically new situation. At least another try must be made for control and disarmament, especially since the H-bomb's unilateral renouncement, which the GAC majority report had advocated, no longer constituted viable policy advice in view of the official American decision.

Thus, these scientists reasoned, if the bomb had to be built, a search must also be continued for a _final_ solution to vexing international problems which provoke arms races. Their acute concern with the moral aspects of the H-bomb decision infused many of their attempts to meet somehow the consequences of this decision. It was the unifying element in their different arguments. These differences may at first glance appear not great enough to mention. However, the nuances of emphasis or the small adjustments that scientists make to disquieting decisions or events are important indications of how their political education was proceeding.

Near one end of the continuum were Harrison Brown, Albert Einstein, Linus Pauling, and the FAS who represented more or less the same response: that it was wrong to build the hydrogen bomb; that it was illusory to think that armaments would bring security; and that there were constructive alternatives to it. But, their advice was either politically untenable or merely a set of pronouncements with no course of action actually delineated. At the other end was Edward Teller whose position was that the United States must arm until an open world was achieved. It was equally lacking as a viable response to actual political problems which must be met in ways that might encourage a movement toward that objective.

Along the middle areas of this continuum fell Hans Bethe, Frederick Seitz, Louis Ridenour and their supporting colleagues. Their views demonstrated an acceptance of the political context in which the H-bomb decision was made. Its unpalatable necessity did not, however, prevent a passionate attempt to define the moral conditions under which the bomb could be used, conditions rooted in their ideas of what constituted moral behavior. In other words, at the same time that they recognized and reluctantly accepted the political requirements of the H-bomb decision, they tried to balance this awesome responsibility with efforts to forestall its use.

In the first week of February 1950, Bethe and eleven other leading scientists made a formal statement on the H-bomb.[1]

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[1] Statement issued at a Physical Society Meeting and signed by S. K. Allison, K. T. Bainbridge, H. A. Bethe, R. B. Brode, C. C. Laurissen, F. W. Loomis, G. P. Pegram, B. Rossi, F. Seitz, M. A. Tuve, V. F. Weisskopf and M. G. White. See New York Times, February 5, 1950, p. 3 for complete text; also BAS, 6 (March 1950), 75.

They urged that the United States unilaterally renounce first use of the hydrogen bomb in a formal pledge. Such a public pledge was conceived as a practical first step toward relieving international tension and "freedom from fear" for the world. Its results would be three-fold: to indicate a desire to avoid needless destruction; to reduce the chance that the hydrogen bomb would be used in actual war; and to eliminate to a great extent the precipitation of war by an H-bomb.[1] "What we really want," these scientists declared, "is a workable agreement, as part of our efforts toward a lasting peace." This proposed pledge would ease negotiations and make it more difficult for the Russians to use the bomb in the face of such an example.[2]

In any event, the use of the hydrogen bomb could only be justified to repel an H-bomb attack on the United States or its allies. Furthermore, the only justification for its development, these scientists concluded, would be to prevent its use. The bomb's preemptive use by the United

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[1] Laurence, <u>op. cit</u>., p. 71.

[2] <u>Ibid</u>., p. 73.

States would deny any standard of morality, and this was a moral issue. The H-bomb was not merely an improved atomic bomb; it was of "an entirely different order of magnitude," one of the signers stressed. "Even though we may be criticized for tying our hands, we must still make this declaration," he affirmed.[1] These scientists agreed with Oppenheimer's view that, although decisions to build or not build the H-bomb, to seek or not to seek international control measures were rooted in highly technical factors, they were rooted simultaneously in moral considerations.

In speeches and articles, Bethe himself deplored the development of the hydrogen bomb and opposed its use. Yet, if the effort to build one was going to be made, he hoped that the United States effort would succeed first. The dilemma was apparent. For Bethe and his colleagues it was difficult to accept a conscious peacetime policy which considered mass extermination of whole populations. This was quite different from the wartime situation. The fact that control negotiations had thus far been unsuccessful was not a valid reason to have bombs a thousand times worse, and not

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[1] See New York Times, February 5, 1950, p.3 for the words of S. K. Allison.

try again. Although the United States should not compromise on the question of effective inspection, another attempt was now doubly imperative.

This statement was later described by its spiritual father, Hans Bethe, as a desperate try to reconcile the H-bomb program ("we did not want to oppose") with a troubled conscience.[1] Although these scientists accepted the inevitability of the President's decision, they questioned its wisdom, especially on moral grounds. For Bethe, an articulate spokesman for the other eleven signers, the decision to build the bomb settled one question only and raised many others of high importance.[2] But, the most important question was the moral one.

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[1] See <u>Oppenheimer Transcript</u>, <u>op. cit</u>., pp. 326-36 for Bethe's description of his troubled conscience which showed in his refusal to work with Teller's group and of his hope that neither Russia nor the United States would be successful. He was later to join the project during its "critical" period from February 1952 to September 1952 at Los Alamos. He had begun a little earlier on an <u>ad hoc</u> basis after the Korean War broke out. See <u>ibid</u>., p. 329 for Bethe's own description of his contribution to the H-bomb development.

[2] E.g., How will the bomb affect the strategic position? Will it give the superiority in arms that was had before the Russian atomic bomb? Will it improve chances of winning the next war, if it occurs? Will it lessen the likelihood that the next war would see the annihilation of urban areas? Will it help avert or postpone war? And how will a world after an H-bomb look?

Bethe also scored the fallacy that the end justified the means. American use of the H-bomb as a threat or in actual warfare against the Soviet Union would invalidate American ideals, for the conflict with the Russians was about means. An H-bomb war would reduce civilization to barest survival. Physical destruction would bring with it moral destruction and the loss of lives and individual liberty. The hydrogen weapon, concluded Bethe, carried "mechanical warfare to ultimate absurdity in defeating its own aim," and did not provide "a fair means to win our struggle with the USSR."[1] In short, its use was morally indefensible. Furthermore, it was of doubtful military value. The United States had sufficient atomic bombs and available planes to prevent a decisive military victory if the Russians attacked with a hydrogen bomb or issued an ultimatum to surrender.

Both the statement of the twelve physicists and one by the Council of the Federation of American Scientists made at the same time and representing approximately 1500 members (mostly physicists) throughout the country decried that security would be found through a monopoly of super weapons.

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[1] *Op. cit.*, p. 21.

If the Americans built H-bombs, the Russians would also. Thus, these American scientists believed that the United States should supplement its H-bomb effort by turning "from the false security of bombs to the slow, difficult task of gaining security by a positive approach to peace by mutual agreement, to peace by gradual disarmament, to peace by world-wide economic reconstruction and development."[1]

The FAS leadership proposed a "fresh start" and the implementation of a "far-going revision which offers some *real* hope of breaking the present stubborn deadlock."[2] Alternative proposals, possibly without the far-reaching concept of international ownership and with large concessions to national interests, must be considered. For example, Hugh C. Wolfe, FAS chairman, pointed out that the possibility existed for the United States to offer concessions to the Russians in exchange for inspection concessions, in which case "we might get somewhere."[3]

The FAS recommended that a non-partisan commission of natural scientists, political scientists, and economic and foreign affairs experts consider anew formulating atomic

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[1] *Science*, III (February 17, 1950), 190.

[2] *Ibid*. (Italics added)

[3] *New York Times*, February 6, 1950, p. 1.

energy policy in light of the broader international political and economic issues. Some members of the FAS looked for completely impartial recommendations "divorced from any policy mistakes that may have been made in the past." This was apparently a reference to State Department policies of which, said William A. Higinbotham, the Congress was suspicious.[1]

In brief, the FAS statement stressed two points: (1) the United States should have no illusions of security based on a monopoly of super weapons; (2) security will come only from a stable peace. It was an expression of their hope "that there are no differences so great that they can only be solved by atomic war."[2] For the FAS the crucial question was whether the United States would seek peace through new negotiations or would rely on force and pay only lip service to the goal of international settlement. The nation could not have it both ways. But, the FAS, in its stipulation that the United States could not rely on force and negotiate a stable peace at the same time, ignored the value of strength as a basis for negotiation.

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[1] New York Times, February 6, 1950, p. 1 ff.

[2] Science, III (February 17, 1950), 190.

Again, as in 1945-46, the FAS attempted to remain above politics. It appeared to think that it was more expert than the government's science advisers. Implicit in its recommendation for a non-partisan expert commission to make impartial policy recommendations was its view that political problems can best be solved by experts outside of government, not by others making "bad" or inexpert choices. It did not consider that policy recommendations, no matter how impartial, had to be applied by "partisan" policymakers.

Einstein's position exemplified the large, humane step that must be taken to avert disaster. For him, international control represented a secondary measure. The achievement of peace was not possible, he argued, whenever action was considered and taken on the basis of a possible future conflict. The solution? A supra-national judicial and executive body to decide security questions. The first step was to abolish mutual fear and distrust through a _renunciation of violence_, for peaceful cooperation was founded primarily in mutual trust and secondarily on institutions such as courts.[1]

Einstein's first step was politically impracticable

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[1] _New York Times_, February 13, 1950, p. 1.

and represented a "naive utopianism."[1] Although the desire for an end to violence and the development of mutual trust was laudable and shared by many non-scientists, it was not easily fulfilled. The difficulty was pinned down by Louis N. Ridenour. His examination of the political conditions considered the world as it was, not as it ought to be. He observed:

> What we need is an international agreement to the resolve of men not to kill other men, and we ourselves are not ready to sign that one today. Until all nations are ready to do so, the question of whether to make superbombs can have only one answer, and that answer has been given.[2]

Harrison S. Brown categorically denied this conclusion. He doubted the wisdom of Truman's decision in view of the potentialities of what some scientists called "the many

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[1] See Warner R. Schilling, "Scientists, Foreign Policy, and Politics" American Political Science Review, LVI, (June 1962), 291-92 for his observation that the scientist has a professional propensity "to look at problems in terms of searching for a solution to them." When he considers international political problems, however, "the scientist's approach often appears open to the characterization of 'naive utopianism or naive belligerency': His approach to international relations appears simplistic and mechanistic. It is almost as if he conceives of policy being made primarily by forward-looking, solution-oriented, rational-thinking types like himself." Schilling credits Hans Speier for the phrasing of this point.

[2] *Op. cit.*, p. 15.

constructive alternatives" to the bomb. Until everything was done to live peaceably with the Soviet Union, it was wrong to develop a hydrogen bomb. Like Einstein, Brown looked to the creation of a world community with an enforceable world law as a condition for lasting peace. However, agreements cannot be piecemeal; component parts in the complex situation interlock too greatly to allow a step-by-step solution. An overall settlement must be sought to avert war, Brown declared. But his proposal was not a useful guide to political action; it did not consider the limitations of the international political situation which Brown assumed could achieve the changes he proposed.

At a rally sponsored by the National Council of Arts, Sciences and Professions,[2] Linus Pauling, in blunt opposition to the President's decision, asserted that it was a shortsighted policy which attempted to keep the peace by force and left the all-important decision of the H-bomb solely to the President. "The question of an atomic war," he said, "is not an ordinary political question" and "must not be confused by minor problems

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[1]See his two articles, "Foreign Policy for the Atomic Era," The Nation, 170 (May 20, 1950), 481-83; and "How Big Need a Big Bomb Be?" American Scholar, 19, (Summer 1950), 265-71.

[2]In attendance also were Harlow Shapley, Philip Morrison, and O. John Rugge, among others.

such as communism versus capitalism."[1] Pauling's attempt to lift atomic weapons from their political context was based on his belief that these weapons created the Soviet-American political conflict. He has consistently proposed the establishment of a research program on the causes of war and ways of preventing war under the direction of the National Academy of Sciences.[2] The assumption seemed to be that scientists were especially qualified to lead a total assault on the problem and come up with a total solution. Pauling's approach left no room for outlining alternatives from which choices could be made.[3]

At the other end of the continuum was the idea that the scientist's task was not to decide whether a hydrogen

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[1] Cf this view with the current Sino-Soviet rift.

[2] New York Times, February 14, 1950, p. 16. For a further exposition of Pauling's views see his No More War! (New York: Dodd, Mead and Company, 1958).

[3] Recall Philip Morrison's statement after the Soviet explosion that this event meant that peace was won. Now all that was needed was a mopping up operation to solidify the peace which the prospect of atomic parity had made inevitable. Events since 1947 had not substantially modified the "all or nothing" approach to politics of Morrison and his colleagues. Note his article and Robert R. Wilson's, "Half a World . . . and None: Partial World Government Criticized," BAS, 3, (July 1947), 181-82.

bomb should be built--this was a public responsibility and not a question for experts--but if it could be made and how. Teller believed that the responsibility of a scientist was to make the country militarily strong and explain "this dangerous world to his fellow-citizens."

As a citizen, however, Teller could not see any alternatives to Truman's decision.[1] Unlike Pauling, his basic premise apparently was that political differences preceded and perpetuated the arms race. Only fundamental resolutions of political difficulties would provide meaningful foundations for the control of atomic weapons. Only under the condition of world government could the arms race be contained. In other words, Teller believed that the United States could afford to stop building better weapons only when the barriers between the Soviets and the Americans had disappeared.[2]

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[1] "Back to the Laboratories," BAS, 6 (March 1950), 72.

[2] "Draft of a World Constitution," BAS, 4, (July 1948), 204. In reference to the neutron bomb Freeman J. Dyson recently combined Teller's view and Bethe's, as embodied in the pledge proposal. Dyson believes that the United States should publicly declare never to use the neutron bomb first; that it will test new kinds of weapons "only in the hope of denying military superiority to others." At the same time, he concludes that the "merciless advance of nuclear technology" can be halted only by "complete openness in nuclear research." See "The Neutron Bomb," BAS, 17 (September 1961), 271-72.

Although he was clear in distinguishing the proper functions of a scientist, Teller exerted all of his scientific prestige and personal talents to advocate an acceleration of fusion research, thus addressing himself to the question--*should* the H-bomb be made?[1] As had others before, he did not clearly separate his roles as scientist and as citizen.

Both Pauling and Teller would educate the public to the "dangerous world" around it. The hope of one was that the proper appreciation of the insanity of war would bring agreement and a stable peace. The hope of the other was that, once it was recognized that the Russians were not going to stop building better weapons, the nation would support military preparedness on any level that modern science and technology created.

Both Pauling and Teller were making proposals of action which came from "seeing things as they were." But they saw through different eyes. Both took stands which involved the question of *ought*. They shaped the selection and presentation of what *is* by their position on what *ought* to be.

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[1]Recall that Teller and his supporters had argued that no choice and therefore no morals were involved in the decision to build the H-bomb.

One of the most eloquent defenses of the fateful decision came from Frederick Seitz in a speech before the American Physical Society on February 3rd, the same gathering from which emerged the statement of the twelve physicists led by Bethe. Seitz himself had been one of the signers. Nevertheless, his plea to scientists to participate fully in military research and development of the super bomb was a call to duty such as was imposed by the demands of war. Furthermore, the issues were directly related to ideals important to the scientific community. "At stake," he declared, "is the concept of tolerance in the broadest meaning of the word. I feel it would be highly immoral not to do the best we can to preserve the state in which these ideals represent the principal goal toward which society moves. Who among us will feel sinless," queried Seitz, "if he has remained passively by while Western culture was being overwhelmed."[1]

His basic premises postulated the fundamental incompatibility of Western ideals and Russian culture, the belief that no genuinely broad compromise was possible, and the impracticability of neutrality in this conflict. Few groups were in a better position, Seitz stated, to stimulate public

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[1] New York Times, February 14, 1950, p. 2.

awareness for greatly increased international negotiations and military preparedness. No alternatives had fewer risks than an acceleration of the military program to discourage a Russian attack and increase the capability to preserve American institutions in the event of war. Scientists in general must not flinch from contributing to a situation which called for a high level of imagination and specialized knowledge.[1] Thus, Seitz called on his scientific colleagues to work within the political context as it was.

As a result of the debate on international control and the conditions under which the United States was morally justified to proceed with the hydrogen bomb, a significant part of the scientific community put pressure on the President to initiate another agreement with the Russians. It was felt that the President should have announced a new effort toward negotiations at the same time that he ordered development of the hydrogen bomb. Nonetheless, the President returned a negative reply. He told a press conference that

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[1] See Frederick Seitz, "Physicists and the Cold War," BAS, 6, (March 1950), 83-89 for a detailed development of his political assumptions which spurred his passionate plea. See also New York Times, February 1, 1950, p. 6 for Karl Compton's view that "under existing world conditions . . . it would be reckless and stupid not to make ready the most effective military weapons." Also ibid., January 28, 1950, p. 6 for Urey's reluctant acceptance of the decision even before the official announcement.

the American position was unchanged and that there was no reason to reconsider the Baruch plan.[1]

For many scientists this indicated a loss of faith in international cooperation. Despite the Administration's position, advocates of a re-examination of American atomic policy were not satisfied and deplored its intrinsic disillusionment. One editorial probably summarized the prevalent view of these advocates, some scientists, some not.[2] The tragedy of the hydrogen bomb decision, it said, was political. It demonstrated a "confession of defeatism, a failure of initiative, of imagination, of bold thinking and of moral conviction in dealing with the crucial problem of modern times." The primary reason for the obsolescence of American control proposals, it contended, could be found in their political premise "that we can place Russia on probation and when they [the Russians] proved virtuous take them in as partners." At the most a new try might

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[1] See <u>New York Times</u>, February 10, 1950, p. 8. Chester Barnard, who helped prepare the Acheson-Lilienthal report in 1946, pointed out that the chances for agreement were never very high so long as the parties were not equal in status, a prerequisite for success in bargaining. See his article, <u>op. cit</u>., p. 12.

[2] <u>New Republic</u>, 122 (February 13, 1950), 5-8.

lead to an agreement. At the very least, it would serve an educational function, both at home and abroad, on the nature and objectives of American atomic policy.

Ever since January 31st, scientists had been eager to inform the public of the facts essential to its intelligent and necessary participation in these momentous decisions. It was not surprising that they should have stimulated the public discussion. They were uniquely trained to explain the technical aspects, uniquely close to the problems, and uniquely concerned. Their insistence upon public debate of the issues and the reasons for it are considered in the next section.

IV

The problem of balancing the greatest security possible with the greatest freedom possible in a democracy has occupied thoughtful observers for a long time. Alexander Hamilton discussed it in The Federalist. He wrote:

> Safety from external danger is the most powerful director of national conduct. Even the ardent love of liberty will, after a time, give way to its dictates. . . . the continual effort and alarm attendant on a state of continual danger will compel nations the most attached to liberty to resort for repose and security to institutions which have a tendency to destroy their civil and political rights. To be more safe, they at length become willing to run the risk of being less free.[1]

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[1] Quoted in E. M. Earle, Against This Torrent (Princeton: Princeton University Press, 1941), p. 29. For a more recent discussion, see Harold D. Lasswell, National Security and Individual Freedom (New York: McGraw-Hill Book Company, Inc., 1950).

Shortly after World War II, E. L. Woodward observed that the atomic bomb had important political results, not the least of which was its bearing on the future of political liberty--the liberty to criticize authority. He wrote:

> Whether it remains for years to come only a potential source of destruction or whether it can be turned to peaceful ends, this new source of energy must remain under State control and therefore must increase enormously the power of the State over the citizen... This question is of greater significance now because every new instrument of force under State control lessens the chances of successful revolution--the last safeguard against a perpetual tyranny....[1]

In brief, the ever-present question, now sharpened by the development of atomic energy and the continuous demands of international crises, is simply how to maintain a viable democracy. For many scientists this could only occur by means of an informed public discussion. Maintaining a viable democracy required the same open and free interchange as a viable science.

The hydrogen bomb decision created a disquieting atmosphere in which it was difficult to keep a balanced judgment of the issue. Writers on science and society like to tell of a dinner conversation in Paris about eighty years

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[1] Some Political Consequences of the Atomic Bomb, (New York and London: Oxford University Press, 1946), p. 28.

ago which is recounted in the Journal of de Goncourt brothers. It illustrates well the urgency which scientists especially felt at this time. On this occasion the chemist, Pierre Berthelot, predicted that by 1969 "man would know of what the atom is constituted and would be able, at will, to moderate, extinguish and light up the sun as if it were a gas lamp." The physiologist, Claude Bernard, saw that in the future "man would be so completely the master of organic law that he would create life [synthetically] in competition with God." The Goncourt brothers added a postscript to these predictions.

> To all of this we raise no objection. But we have the feeling that when this time comes to science, God with His white beard will come down to earth swinging a bunch of keys, and will say to humanity, the way they say at five o'clock at the salon, 'Closing time, gentleman!'

The Goncourt brothers made a vivid point. Until man learns to guide the use of his discoveries with greater rationality and respect for the power he wields, the world is in grave danger of an uncontrolled chain reaction. The people, scientists insisted, must know the awesome responsibility which comes with increased scientific knowledge and its technological application. "An informed democracy," believed Louis N. Ridenour, among others, "is the strongest and most viable political form. A government does not adequately protect its citizens by taking decisions from them that they can neither know about nor take part in."[1]

Yet, despite the fact that decisions of the gravest consequences were being taken beyond the reach of public opinion, the public response to the H-bomb decision was apathetic, as it was in September when the President announced the Soviet atomic explosion.[2] Furthermore, as James B. Conant put it, "many important decisions are being made in Washington today without adequate evaluation." The government had not developed, he asserted, "even the first approximation to a satisfactory procedure for evaluating technical judgments on matters connected with national defense."[3] Conant questioned the competence of lay officials

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[1] Op. cit., p. 15. Hanson Baldwin made a similar point in the New York Times, February 6, 1950, p. 4. See also William L. Laurence, "The Truth About the Hydrogen Bomb," Saturday Evening Post, 227, (June 24, 1950), 17-19 ff.

[2] There was a noteworthy lack of any general request for more information or clarification of information on which to base better decisions of, for, and by the people. See Elizabeth Douvan and Stephen Withey, "Public Reaction to Non-Military Aspects of Atomic Energy," Science, 119 (January 1, 1954), 1-3.

[3] Conant spoke before the Harvard Club in Rochester, New York almost simultaneous with the President's announcement. Reported in "Science and the Citizen," Scientific American, 182, (March 1950), 24. See also his article "Science and Politics in the 20th Century," Foreign Affairs, 28, (January 1950), 189-202 for a suggested attack on this problem.

to decide which scientific and engineering opinions were sound.[1]

On this point, Conant's views on private scientific advice and public discussion become especially interesting for their ambivalence. One of the earliest official decisions on the hydrogen bomb was made on the basis of a confidential report to Truman in 1945 by Vannevar Bush and James B. Conant in which they advised against constructing the bomb. Subsequent decisions on this question were made by scientists in effect, and the public did not know about them. Here there was no "hearings" of experts who would be encouraged to offer alternative solutions. Indeed, the basis of the advice given appeared to be more political than technical. However, the new and inexperienced President, especially in matters where science and politics meshed, trusted these two men.[2]

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[1]It must be remembered that Conant had shared the majority view of the GAC of which he was a member at the time of the H-bomb decision.

[2]In reference to this problem of determining which scientific advice is sound, Sir Robert Watson-Watt observed that such a judgment is made not only on a man's technical skill but on the confidence and trust which is placed in him by the person making the judgment. He recalls that Harry Wimperis, Director of the Scientific Research in the British Air Ministry, had summoned him to his office one day in the middle 1930's when Britain was planning defense measures and said: I have asked you to come to see me, not in my official capacity nor in yours, (Watson-Watt was then with the Department of Scientific and Industrial Research) but because you are a personal friend in whose judgment and discretion I have complete confidence. . . . What do you think of the possibilities of a beam of damaging radiation in defense against air attack?" Quoted in an article by Sir Robert Watson-Watt, "The Truth About Churchill's Aide," Saturday Review of Literature (March 4, 1961), 51.

Their advice was apparently all he heard and, in the next few years, it was corroborated by the GAC whose members were disinclined to develop newer and more destructive kinds of weapons. Five years later, when the GAC's moral and technical objections had been overridden in a wider, albeit still private discussion, Conant observed that the worst way to make decisions was to resolve conflicts in favor of those with the loudest voice or the closest approach to political leaders.[1] At virtually the same time, Urey, who favored the H-bomb development but did not participate actively like Teller, criticized the Bush-Conant report. Decisions on the hydrogen bomb, he said, had been taken "in an almost unbelievable atmosphere," without a realistic appraisal of Soviet potential in the atomic energy field.[2]

Thus, not only was the public apathetic but, as Richard H. Rovere observed, "many in the upper strata of the government, had a feeling of ignorance, impotence, and some-

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[1] "Science and Politics in the 20th Century," <u>op. cit</u>.

[2] <u>New York Times</u>, January 28, 1950, p. 6. Conant testified later that only a board of nuclear physicists could properly determine whether or not the technical evaluation of the GAC had been "right or wrong." <u>Oppenheimer Transcript</u>, <u>op. cit</u>., p. 387.

thing approaching desperation."[1] Stewart Alsop, along with Richard Rovere, indicated that the average layman had no way of knowing how good the technical case against the hydrogen bomb was. "But neither, wrote Stewart Alsop, "it is possible to suspect, do President Truman, the non-scientific members of the Atomic Energy Commission, or the other laymen on whom rests the responsibility for making decision of great moment in secrecy.[2]

Thus, the possibility of a hydrogen bomb in the American stockpile did not reassure officials, at least, that security was increased. The public, on the other hand, might have been deluded into believing that the national security was augmented by this vastly destructive weapon. Scientific opinion, however, was fairly unanimous that the H-bomb was not just another kind of hardware, that it made an even greater different kind of a difference, and that the matter was too vital to be left to secret decision-making by any small group. Yet, scientists' reasons for wanting a public airing were not uniform. Furthermore, not all scientists exhibited an interest to inform the public. For example, the Berkeley scientists were noticeably reticent. What, then, were some of the factors which prompted those who

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[1]Op. cit., p. 50.

[2]Alsop et. al., op. cit., p. 140.

insisted on public discussion and how did they pursue this objective?

Questions of morals, politics, education and morale were involved in the plea of scientists for a wider public discussion. Unfortunately, the moral issue generated such high emotions that some scientists forgot to apply rigor to the task of informing the public. Political judgments colored their factual presentation and detracted from the special way in which scientists could contribute to public enlightenment. Instead, the atmosphere was such that, according to Rovere, "every assertion of a scientific or political character . . . quickly bred a counter-assertion."[1] Hanson Baldwin scored the executive policy to censor all thermonuclear information[2] because "the obscuration of secrecy--complicated by some extravagant, incomplete and widely varying statements about the hydrogen bomb by scientists and others--have confused the public, or hidden many of the true facts about the hydrogen bomb from them."[3]

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[1] Op. cit.

[2] See New York Times, February 1, 1950, p. 1 for the President's statement.

[3] New York Times, April 16, 1950, p. 32. See ibid., February 3, 1950, p. 3.

As a matter of fact, the GAC report gave some thought to the advisability of public discussion of the hydrogen bomb. It made, said Oppenheimer some years later,

> some comments as to what might be declassified and what ought not to be declassified and held secret if any sort of a public statement were contemplated. If the President were going to say anything about it, there were some things we thought obvious and there would be no harm in mentioning them. Actually, the secret ones were out in the press before very long.[1]

Nonetheless, even after his announcement, the President discouraged public discussion.[2] Oppenheimer made a plea in its favor, encouraging debate and criticism so that wisdom and truth could flourish. The relevant facts, he noted, serve the enemy little, yet they are basic to comprehension of policy issues.[3] Scientists generally agreed that security

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[1] Oppenheimer Transcript. op. cit., p. 79.

[2] Some officials favored an open discussion when the Alsops first reported that an H-bomb was officially under consideration and a confusion of information developed. Others maintained that, while security was not involved, such a public airing might complicate private negotiations with the Russians. The Russians walked out of the six power group on atomic energy control two days later. When closed sessions were suggested, they had accused the Americans of trying to be devious. See James Reston, "U.S. Hydrogen Bomb Delay Urged Pending Bid to Soviet," New York Times, January 17, 1950, p. 11.

[3] New York Times, February 13, 1950, p. 1. Oppenheimer appeared with other scientists on Mrs. Eleanor Roosevelt's television show.

rules surrounding atomic energy development had been so interpreted and applied that the American people knew less about atomic bomb capabilities than the Russian government.

Some scientists had considered public discussion of the H-bomb unwise because it would alert the Russians to American activities in this area. Now that the matter was out in the open, they wanted the people informed accurately.[1] But what constituted accuracy? An example of how scientific _facts_ can be interpreted according to the political and moral predispositions of scientists and the need for public support of their work is found in a University of Chicago Roundtable Conference broadcase which convened to discuss the facts about the hydrogen bomb. Four eminent scientists, Hans Bethe, Frederick Seitz, Leo Szilard, and Harrison Brown participated and succeeded in stirring a dispute about these facts among their colleagues and others. They stated that a hydrogen bomb could annihilate the Earth's population by radioactivity and that the only defense was organized urban dispersal, an idea which prevailed among many scientists.

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[1] Bethe had been one of this group. As long as it could be kept a real secret, he approved that only a few should handle the problem. It was not clear how much this procedure was in accord with the general plea of scientists that public discussion was required in the democratic process.

Thus, they concluded, the country would have to bear the expense not only of the cost of the bomb but also of such defense measures.

In a Town Hall address, Lilienthal severely censured those scientists who would spread panic.[1] What good, he asked, was it to be extravagant and sensational in picturing the horrors of atomic warfare. The idea of dispersing urban areas, he continued, was "a lot of high intellectual nonsense. It can't be done. It won't be done."[2] Szilard replied that neither the President, nor the AEC, had explained to the American people about the consequences of the H-bomb decision in terms of cost and indispensable defense measures.[3] Yet, these are the things they must know about. The same day as Lilienthal's speech, it was stated in Science News Letter

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[1] See New York Times, March 2, 1950, p. 11 for a report of the talk.

[2] New York Times, March 2, 1950, p. 11.

[3] See Ralph E. Lapp, "The Hydrogen Bomb: IV," Scientific American, 182, (June 1950), 11-15 in which he supports the idea that dispersion was the only defense. Lapp also deplored the fact that the problem of civil defense was badly neglected. Almost no facts about the effects of atomic weapons were contained in the Hopley Report (the official investigation of civil defense) of 1948. Extensive secrecy affected a crucial area of defense.

that the most gigantic H-bomb would <u>not</u> explode the atmosphere or the waters of the ocean. "That is the best judgment of scientists," the report continued, "despite the alarming statements of a few physicists. The damage that a dozen or so H-bomb could do to big cities is quite alarming enough without calling upon a chain reaction in the atmosphere or the seas."[1]

In defending his co-panelists, Harrison Brown pointed to the irksome problem which possibly contributed to the passionate tone of their analysis of the facts. Most scientists were "sick of bombs," he said, and were going through "much soul-searching." Speaking at a dinner arranged to advance the cause of world government, of which he was an active supporter, Brown continued: "I ask each and everyone of you here tonight--how would you feel if you were in our shoes? What would you, as individuals do?" Further, "the panel was not exaggerating the danger of annihilation as some have insinuated, although none of them scientists."[2]

Some months later, Robert T. Bacher and James R. Arnold's statements <u>tempered</u> Szilard's theory that a hydrogen-

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[1] "H-bomb is not End of Life," 57 (March 4, 1950), 33.

[2] This was not quite accurate, given the report in <u>Science News Letter</u>. For Brown's remarks, see <u>New York Times</u>, March 20, 1950, p. 4.

cobalt bomb could destroy the human race. Their disagreement is instructive for the problem of how to evaluate technical information, especially when presented in varying interpretations by eminent scientists.

Szilard asserted that the necessary deuterium could be produced and exploded in such a way as to yield enough neutrons which could be absorbed in an element to form a dangerous radioisotope. Radioactivity could then be distributed uniformly over the earth to irradiate all human beings, causing death.

Arnold countered that the "necessary" amount of deuterium set at a minimum of 500 tons and a maximum of 10,000 tons with a minimum cost of $4,000,000 and a maximum cost of $40,000,000 would require ten years. Second, no one knew if any such quantity of deuterium could be exploded. Third, there was no certainty that neutrons could be absorbed in an element which would become a dangerous radioactive isotope. Cobalt, the most likely element, might be required in as large an amount as 100,000 tons for bombs of contemplated size. Provision of only one-fourth of that amount would require an effort of several years, and demand would greatly increase the 1948 cost of the material, then $1.60 per pound. Fourth, Arnold did not believe that a uniform distribution of radioactivity from a bomb was possible.[1]

Bacher reinforced this statement on gamma radiation hazards by noting that the scare stories of radioactive effects postulated a complete explosion of 500 tons of deuterium. This, said Bacher, "while not impossible as far as anyone can say, is stretching probabilities a long way . . . the effectiveness of radioactive contamination from a hydrogen bomb designed and exploded to enhance that effect seems to be somewhat uncertain; and at least, in part, unpredictable."[2] This represented the most authoritative and dispassionate speech to date, in which he gave a brief but cogent analysis of "well-known scientific information" on the hydrogen bomb. Earlier that month, in commenting on the Chicago Roundtable, Bacher said that "people cannot remain for long in a state of fright and fear. After a while they turn aside and think of something else, or they turn to phantasy. Either course is a blow to our national security."[3]

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[1] New York Times, October 29, 1950, IV, p. 9.

[2] The Hydrogen Bomb and International Control: Technical and Background Information, op. cit., pp. 32-34.

[3] See New York Times, March 2, 1950, p. 11.

Thus, as this dispute demonstrates, in the use of the scientific facts and the degree of emphasis applied here rather than there, differences arose and confusion set in. From essentially the same facts one scientist could derive that the human race was in immediate danger and the other that there was no immediate danger. Arnold observed that if this discussion were repeated ten years hence, very different results might emerge.

Perhaps the most responsible public education by scientists occurred in the spring of 1950. A series of four articles on the hydrogen bomb in Scientific American, written by Louis N. Ridenour, Hans A. Bethe, Robert F. Bacher, and Ralph E. Lapp, offered a relatively dispassionate discussion of the great problem. From them one could learn what the hydrogen bomb was and what it could and could not do. In all four articles there was never any doubt where each individual author stood on the political, moral, and military implications of the hydrogen bomb. Nevertheless, the undisputed, basic scientific facts remained distinct.

The AEC objected to such candor despite the fact that this knowledge was available to anyone who knew physics. It imposed further restrictions. Dissatisfaction of scientists with AEC security policies reflected their general dissatisfaction with national policies about secrecy in science as applied, not so much as formulated. They charged that in its efforts to assure internal and external security, the government had not maintained that delicate balance between the benefits derived from exchange of scientific information and restriction of information. Only experts could make this determination. Furthermore, it would be extremely difficult to disclose scientific secrets since their value was in the working details which were difficult to transmit.[1] Also, American scientists had proven themselves singularly discreet. "Misinformed public opinion, in the present atmosphere of anxious concern," said Edward U. Condon, "may create a situation in which our progress is impeded. It is important, then, that the public be properly informed as to the problem. This is a responsibility of scientists, of our military

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[1] New York Times, May 17, 1950, p. 28 for Urey on this matter. ". . . in view of the effective competition in atomic energy development which we now have, a much more daring approach to these problems is needed. We need more self-starters in a position to make decisions, and less of the cautious administrators. . ." Compare this with Congressman Melvin Price's recent comments in which he accuses some scientists of this very cautiousness. See his address "Atomic Science and Government--U.S. Variety," delivered before the Washington Chapter of the Nuclear Society, June 14, 1961.

leaders, and of the officials of our government."[1]

The need for an informed electorate was strongly emphasized by scientists who feared that, as the Cold War continued and atomic stockpiles increased, freedom would be subverted.[2] For example, Bacher believed that the

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[1] "Science and Society," Science 107 (June 25, 1948), 664. Two years later the same concern was felt. Condon himself had come under government attack, having been described as the "weakest link" in the chain. He was subsequently cleared. See David E. Lilienthal's statement to the press at the time of the release of the fifth semi-annual report of the AEC (presented to Congress on February 1, 1949) which was an attempt to indicate "the feasibility of dissemination of a rather wide area of knowledge" for public consumption: BAS, 5 (March 1949), 94. Others also insisted that an area of secrecy be limited to the "necessities of security." See BAS, 5 (May 1949), 158-60, for a report of a House speech by Representative Melvin Price and of a speech by Senator Brian McMahon in January 1949 before the Economics Club in Detroit. See also his "Should We Reveal the Size of Our Atomic Stockpile," BAS, 5 (March 1949), 66-68 for a detailed argument for revelation of this figure, although he reserved his answer on it at that time. Walter Gellhorn illuminated and substantiated some of the fears expressed by scientists: that progress (which produced the "secrets") depended on free exchange of scientific information; that scientific teamwork was unnecessarily hindered by security regulations; that compartmentalization brought duplication of effort; that science students were obtaining imperfect training in basic subjects because access to new discoveries was curtailed. See his study, Security, Loyalty and Science, (Ithaca, New York: Cornell University Press, 1950).

[2] For a recent exposition of this view, see e.g., I. I. Rabi, "The Cost of Secrecy," Atlantic Monthly, 206 (August 1960), 39-42.

government could be much more candid with the electorate without endangering military potential. The current use of secrecy, he declared, far exceeded this minimum limit; the government's policy contributed to the erroneous public belief that quantities of hydrogen bombs would contribute qualitatively to security. Such vitally important decisions, Bacher argued, must not be left to <u>any</u> small group which did not represent the people and tended to protect its own narrow interest.[1] To do so constituted an abrogation of citizenship responsibility and a step toward authoritarian government.

In brief, scientists did not question the necessity of security restrictions for military purposes. However, they did insist that basic scientific information could and should be relayed to the public without harming the national security. If some of them confused the picture, because their political and moral predispositions had colored their presentation and interpretation of scientific facts, they remained still about the only ones who insisted

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[1] Bacher and Smyth, the first two scientists who served on the AEC, agreed that the scientist-member should serve only one year, the shortest term of all the commissioners. This would enable more leading scientists to participate in a top policy group, and thus gain an appreciation of policy problems and reduce their tendency to be doctrinaire about atomic policy. See <u>New York Times</u>, June 20, 1950, p.4.

on such a public airing. Their concern with open and free interchange whenever possible accorded with scientific traditions.

Public participation in the making of decisions, or in supporting them, was important to scientists though the call for public discussion arose from different and overlapping motivations. In general, however, scientists used this means as a way of trying to balance their professional, personal, and in some cases their governmental responsibilities.

Chapter X

Scientists In High Level Political Decision-Making

I

The Soviet atomic explosion of September 1949 provoked an important policy debate over the American defense posture. It also broke the relatively comfortable unity of objectives which scientists enjoyed in 1945-46, clarifying growing divisions among them as they attempted to accommodate unsettling political events with policies congenial to science and appropriate for the national security.[1]

The discussion surrounding the hydrogen bomb controversy indicated that scientists had come a long way from the unanimity of the immediate postwar months. As Soviet-American political difficulties sharpened, top level policy officials and their expert advisors recognized a significant turning point in strategic and foreign policymaking. Scientists

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[1] It will be remembered that the use of the atomic bomb in World War II, its destructive capability, and possible implications for their profession and civilization profoundly disturbed scientists; and that after the war in 1945 they actively supported international control as the only reasonable course for nations to pursue.

learned that professional interests were no longer necessarily sufficient to unite them in the political arena, for they used their special knowledge in the hydrogen bomb debate in different ways. In other words, the issue's divisive nature encouraged divergent evaluations not only of the scientific factor but also of other relevant features of the question.

What, then, were scientists' objectives, strategies, and rules in the hydrogen bomb controversy? How did they grapple with its meaning after the President's announcement? In other words, how did they share in decision-making and the implementation of that decision? A discussion of these questions must necessarily consider distinctions among scientists. These provide the vantage points from which scientists developed political proposals, selected means for persuading others to their views, and defined their roles in the hydrogen bomb question.

Just as distinctions could be observed among the articulate scientists in 1945-46, they could also be noted in the 1949-50 period. Immediately after the war, politically active scientists could be more or less divided into two groups--scientists with official administrative-advisory responsibilities and scientists with laboratory functions. Four years later, at least four groups who took part in the hydrogen bomb controversy could be identified: those with current and past administrative-advisory positions in government; university scientists who favored the development of fusion weapons; university scientists who deeply regretted the President's decision but, in line of duty, contributed their professional skills; and university scientists who were unalterably opposed to the new policy.[1]

For purposes of easy identification, the first group can be described as the administrative-advisory scientists, (hereinafter advisory scientists) the second as the satisfied university scientists, the third as the cooperating university scientists, and the fourth as the opposing university scientists.

In the first group were members of the General Advisory Committee such as Oppenheimer, Conant, Fermi, Dubridge, Rabi, and Seaborg, and Smith among the pure scientists, and Rowe and Buckeley among the applied scientists. Also in this category were Smyth, the scientist-

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[1] There were also the Society of Scientists for Social Responsibility (SSSR), representing the most extreme position by their refusal to work on anything contributing to war potential. Although not effective in political terms, this group is important as an example of scientists who chose alienation from society as a way of facing the hard problems of science and politics.

member of the Atomic Energy Commission at the time of the hydrogen bomb decision, and Bacher, his predecessor who had served as the first scientist-member of the AEC until May 1949.

In the second group were scientists from the Lawrence Radiation Laboratory, or elsewhere, such as Teller, Alvarez, Lawrence, Latimer, and Von Neumann.

Urey, Bethe, and Seitz represented the university element whose talents would be needed to implement the President's decision and who offered them. Their debate after the official announcement was important to the kind of support the Government could expect from the scientific community.

The fourth group also represented the university element. Its members, however, were not willing to cooperate with the President's decision. Scientists as Linus Pauling, Harlow Shapley, Philip Morrison, Eugene Rabinowitch, and those on the FAS council opposed this decision and did not operate in the context of the new policy.

II

Scientists' public and private discussions in the hydrogen bomb debate revolved around the three major objectives of American policy since 1945; the international control of atomic weapons, the restoration of a balance of power in Europe, and the design of doctrine and strategy for atomic warfare,[1] and illuminated divisions among them, which began even before September 1949, on political questions. They defined in different ways what constituted a proper response to the Soviet atomic achievement.

For some, international control remained the only proper objective for avoiding an armament race. Now that the American nuclear monopoly had been broken and a more reasonable base for conducting international control negotiations provided, as Rabinowitch and those of similar persuasion believed, another try was necessary, even though future negotiations might fail as had past attempts. Their belief that arms breed conflicts facilitated the idea that if enough scientific facts were fed into the recipe, the results would be successful. As in 1945, the political conditions required for international control were ignored and political forces not appreciated.

Other scientists supported American atomic supremacy. For them, atomic weapons provided the means to deter aggression while seeking peaceful settlement of Soviet-American

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[1]See Schilling, "The H-Bomb: How to Decide Without Actually Choosing," op. cit., for a delineation and discussion of these objectives.

differences. This required both scientific and political action. The first was a recurring and dominant theme in the fall of 1949, persisting after the Presidential decision. Regardless of political differences, all scientists argued consistently since 1945-46 that excessive secrecy was incompatible with scientific advance and national security, and pointed to the Soviet atomic blast as indisputable proof of their wisdom in this matter. Although the GAC scientists believed the Russians were imitative,[1] others like Seitz and Urey warned of their capability for leading the United States scientifically and proposed the development of reasonable security regulations as one way to meet the Soviet challenge.

On the political level, scientists like Urey and Szilard worried about the weakness of Western Europe and the vulnerability of American industrial centers. From their common premise that conflicts breed armaments, two opposite political proposals emerged. Urey supported a strong Atlantic Union and partial international control in an attempt to negotiate from strength. Szilard would dissolve NATO and neutralize Europe to erase automatically areas of political disagreement.

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[1] E.g., Oppenheimer Transcript, op. cit., p. 80.

In the fall of 1949, when the GAC was deliberating about the hydrogen bomb question, Seitz and Bethe argued that unless the United States was willing to use its atomic bomb supply, developing and storing more powerful weapons would not result in greater security. It was as if they knew what the answer was going to be and attempted to forestall it by indirect arguments.

Briefly, then, scientists' public response to the Soviet A-bomb fell into two groups--one which still adhered solely to international control as the answer to atomic weapons and the other which would maintain atomic supremacy while seeking international control. These political objectives were underlined by an overall objective--greater freedom in conducting scientific investigations--which served their professional needs and was compatible with national security goals on the basis that military strength resulted from scientific progress.

Scientists participating in the Government's secret debate and involved with an actual policy choice offered still another perspective. The enlargement of the atomic weapons arsenal to include fusion weapons and the view that international control was a lost cause until the Soviet system became an open one found substantial scientific support.

These did not, however, draw to their fold all scientists who supported American atomic fission supremacy as a response to deteriorating Soviet-American relations. Scientists on the General Advisory Committee believed that efforts toward control should be continued and provided powerful opposition to the group from Berkeley. Even those like Fermi and Rabi, who regretably concluded that the United States should proceed with the development of a thermonuclear device, if another attempt at international control did not work, were sympathetic to future negotiations. Scientists outside the governmental debate, such as Bethe, Ridenour, and Seitz reluctantly accepted the decision but insisted that international control efforts now became more important than ever. Members of Congress agreed. Although recognizing that the decision was born of necessity, they opposed Administration policy which did not contemplate fresh attempts for control.

To summarize, the positions on the continuum were filled. At one extreme was the Society of Scientists for Social Responsibility and at the other those scientists who advocated all all-out development of atomic fission and fusion weapons. In between were scientists who still clung to international control as the answer to political problems. Others decided that these problems required also the maintenance of American atomic supremacy, but in the area of a varied fission program only. In addition, they favored conventional arms for decreasing total dependence on atomic weapons. In such ways they hoped to stem the armament race. Others, who were of this persuasion before the President's decision, shifted their position after the decision. Accepting the new policy but still seeking ways to limit atomic weapons, they advocated continuous efforts for international control; a pledge not to use the hydrogen bomb first; and urged a re-examination by the Administration of its policy on atomic weapons control.

Thus, although scientists, along with others, agreed on the ultimate goal of peace and security, they differed, not unexpectedly in light of the multi-faceted issue, on middle range objectives and means leading to the long-range objective. An examination of these differences is important for understanding the development of scientists as political beings.

III

If scientists sought different objectives, if they chose different roads to peace and security, did they also exhibit different behavioral characteristics? Or were the ways that they went about achieving their goals similar?

Answers to these questions are not easy, for although scientists assumed identifiable positions, their justification of them was not always clear. The problem of harmonizing national security needs with those of science still obstructed their attempts at accommodation, more difficult for some scientists than others. Many of them recognized that the task of responding to the end of the American monopoly involved hard policy choices in which the scientific component was a highly important factor, although not the only one. Their searching efforts to coordinate political, scientific, and ethical considerations evidenced this. Representing a conscious peacetime policy on weapons, the H-bomb decision weighed heavily on some scientists. In their various proposals for proper responses to the prospect of atomic parity, they exhibited similar ways of pursuing diverse objectives, ways which appeared to brake their political growth.

Although political choices had to be made, and although scientists made them, they tended to see themselves as experts involved in solving problems and providing the right answers rather than making choices. When they, as experts, disagreed and clashed, it was a disagreement not only on objectives, which after all Lilienthal and Acheson had.

They questioned each other's intentions and good faith which raises a relevant and illuminating point. Did the GAC scientists, out of their long experience in government at the policy level, look at the scientists from Berkeley as being less expert because, after all, what did they know about science <u>and</u> politics?

Nonetheless, scientists shared a tendency to define a problem by sometimes isolating out variables important for coordinated consideration of a political question. Their search for single causes made their criteria for exclusion or inclusion of variables politically unrealistic. For example, those scientists who assumed that weapons cause political difficulties argued that if this is the problem, then get rid of weapons. Ardent advocates of international control as the answer to the Soviet atomic bomb, they believed that scientific knowledge was the key to the problem's solution. Those who assumed that political difficulties cause arms races, if this was the problem, then get rid of political conflicts. Szilard's proposal to neutralize Europe is illustrative. For those who assumed that political difficulties were not going to be resolved until the Soviet system became an open one, and that there was little use in negotiating a control agreement, if this was the case, then develop an open-ended arms system without limitation of kind or numbers. For those who assumed that negotiations must continue despite political difficulties and that a limit must be placed on atomic weapons development, in kind at least, if this is the problem, then concentrate on a varied fission program. The GAC position provided perhaps the most flexible response, looking toward greater military mobility to avoid reliance on total weapons. Yet, in its desire to limit the armament race and provide a good example for the Russians, the GAC did not consider continuing an active fission program and at the same time developing the hydrogen bomb, a possible political necessity in that the Soviets might build one.

The uncertainty inherent in resolving political difficulties awakened in scientists an old desire for an environment conducive to international control. In 1945-46, they offered to stop work on atomic energy if need be. They even compromised on the value of free inquiry. In 1950, the GAC advised a unilateral announcement that the United States would not build a hydrogen bomb as a way of providing a good example for the Soviets and putting a limit on total war. Its advice was a means to gain time for settling political problems. A significant technical advance such as the hydrogen bomb would be unsettling to political affairs.

After this approach was officially denied, cooperating university scientists proposed outlawing the bomb before its development or a pledge never to use it first. The FAS supported a fresh start and Szilard suggested a truce to provide a chance to bid for peace. Those scientists who were furthest from actual responsibility voiced a recurring theme that scientific and technological cooperation was the key to mutual trust.

Designed to demonstrate American sincerity and create a helpful atmosphere for solving political problems, all of these proposals were based on a mechanistic view of inter-state relations. They illustrated a tendency, in the face of complex and seemingly unresolvable situations, to begin afresh, or stop the race until humanity caught up with itself, reflecting a desire to uncomplicate complicated problems by an orderly and rational approach. It was as if by these means scientists could divest problems of their intricacies and deal with their essence. The satisfied university scientists, no longer hopeful for international control until the Soviet Union developed an open system, also reflected this desire to simplify political difficulties.

Interaction with an implacable enemy in a nuclear age was not easy to maintain. Some scientists assumed that a demonstration of friendship might make a friend out of the Soviet Union in which case interaction would be with a friend, or at least keep the door open. Others did not see any possibility for friendship between the two systems of government and, therefore, did not favor any interaction.

Another factor which hindered the political education of scientists was a fractional approach or consideration of factors involved in decisions to develop weapons. For example, co-operating university scientists especially attempted to reconcile a troubled conscience with the hydrogen bomb decision. Bethe's conception that means and ends had to be equally "moral" or "good" was illustrative. If the choice was to build the H-bonb, then moral justification must be found by pledging against first use and supporting international control. For Teller and Von Neumann, no choice and therefore no morals were involved in the decision; it would be immoral not to develop the bomb. For them, the end justified the means and scientists were responsible only for their scientific estimate of what could be done. Bethe and his colleagues could not be as sure. Seitz's attempt to equate scientific values with the values of a democracy eased for some scientists the conflict between the traditional conduct of science and the modifications imposed on it by the Cold War. He indicated that both the Free World and science needed to preserve the ideals of tolerance and free inquiry.

Some scientists' emphasis on the morality of developing the bomb may have prevented a consideration of possible and alternative political action, rendering them especially naive politically but highly powerful in purely ethical or ideal terms. Five years later the Archbishop of York in May 1955 discussed the dilemma of coordinating personal, professional, and public responsibility. Although in favor of progressive disarmament, he disagreed with these scientists "as to the immediate method by which war may be prevented." He said:

> Violent denunciation of the evils of war does nothing to remove the danger of its breaking out. Strong words and rhetorical resolutions may relieve your own feelings, but they do nothing to promote peace.
> . . . It is simply not true to say that our possession of the bomb implies warlike intentions against Russia or China. We shall possess it as a deterrent, in the hope that it may never be used.[1]

The emphasis on morality created a pitfall which, for the most part, scientists did not avoid. They mistook moral convictions for political astuteness.[2] Furthermore, they thought that a troubled conscience could be eased by good intentions. Here scientists failed to realize that they must assume responsibility not only for policy statements but for their consequences, and that evaluation of consequences requires the accumulation of relevant facts. As Karl W. Deutsch

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[1]Quoted in Lewis L. Strauss, Men and Decisions, (New York: Doubleday and Co., 1962), pp. 229-30.

[2]See above, pp. 223-27.

has observed, much of the discussion between statesmen and atomic scientists of international relations "hinges upon the discrepancy between the strength of the moral convictions involved and the poverty of reliable knowledge of the probable consequences of the proposed courses of action."[1] Probably scientists were frustrated because they were unable to make predictions with much assurance of accuracy and political decisions had to be made in the midst of doubt and uncertainty.

Another way scientists tried to grapple with the awesome decision to build the hydrogen bomb was to call for public participation, but their move to enlarge the public discussion was prompted by different reasons. The FAS, in the fall of 1949 and later, urged public discussion in the hope that problems would be more easily resolved if the people knew the true facts. This was an old hope from the years 1945-46 when the Federation of American Scientists lobbied for civilian control of atomic energy. However, as it turned out, the principle of civilian control was won not so much because of an informed public as of political reasons.

Arthur Compton believed that the people should decide the question of developing vastly more destructive weapons, their decision constituting the final word. This view, especially if favorable, ought to be made explicit to strengthen scientists' morale in a task alien to scientific values; in other words, for sharing a profound responsibility.

Conant's criticism that important decisions were being made without benefit of public scrutiny and his call for public discussion arose perhaps because the GAC's advice had not been accepted. Although it did not synchronize with his earlier view that experts should air disagreements privately, it did reflect a desire to gain public support for a position which lost out in official circles, but which Conant believed important. This countered Compton's view that the hydrogen bomb decision was not "a question for experts, either militarists or scientists. All they can do is to explain what the results will be if we do or do not try to develop such destructive weapons."[1]

Although scientists' response, especially those outside of government, was much more tempered than in 1945-46, some characteristics persisted. Some still tended to reduce profound questions to manageable proportions by large steps, thinking that peace can be gotten by good will. This continuous search for order resulted in somewhat utopian political proposals, for it avoided considering possible alternatives. In some instances, scientists rendered their political proposals

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[1] "Let the People Decide!" BAS, 6 (March 1950), 75.

unrealistic because they did not consider the nationalistic features of the international political system; they freely assumed that it was equipped to meet the challenges of nuclear weapons and meet them quickly. The idea that international scientific and technical cooperation would build mutual trust persisted in spite of discomforting evidence that national sovereignty and national boundaries remained potent realities.

The protracted problems of the Cold War were bound to affect scientists in the process of making foreign and strategic policies. As they moved from the state of "dispassionate knowledge of scientific facts to passionate awareness of social needs," they lost some of the innocence which they had in 1945-46 about the substance of political problems. Intransigent and frustrating Soviet-American relations had thus far prevented international control. Feeding in the right scientific facts and coming out with the right answer had not worked. Their different explanations of the failure to achieve international control facilitated divergence on middle range objectives. Nevertheless, they shared ways of proceeding toward desired ends. Attitudes and habits proved harder to change than objectives and roles.

IV

Scientists shared in the process of making and implementing the H-bomb decision in a variety of ways. Their roles and the factors which helped shape them indicated that actual advisory responsibilities which they held and the degree to which they were satisfied or dissatisfied with the decision affected their political behavior.

In the face of difficult political choices, some scientists entered another phase in the process of political socialization; they began to appreciate the problems of policymakers. For example, those who had helped direct the World War II scientific effort successfully at high policy levels, planning also for postwar atomic energy policy, became directly involved in the intricate parts of political questions with important scientific components. In their official administrative and advisory capacities, this small group of scientists came to know and respect high Administration officers. Together they planned and implemented decisions about atomic energy in a top secret environment.

Wartime conditions of secrecy developed in advisory scientists like Conant and Oppenheimer an additional sense of responsibility for advice and action and did not permit a wide discussion of issues for purposes of consensus in the scientific community. Membership in the high level policy team of World War II impressed upon them its difficult responsibilities. No longer could they function in the uncomplicated and freewheeling manner of the project scientists. Even during the war, this characteristic of administrative propriety was consistently exhibited.[1]

Atomic energy discussions frequently led to agreement since the policy group of scientists and non-scientists was small, and the division of labor relatively clean. These scientists had not had to develop mechanisms for organizing consent in the scientific community for their views. Usually they had only to explain them to their non-scientific team members. As the discussion was widened to include the working scientists, disagreements arose. Nevertheless, after the domestic control issue was resolved, these scientists retired from action except for voicing their views on international control, international relations, and other matters in the public forum. On the other hand, advisory scientists continued to participate in atomic energy decisions, for which they were asked to account in the 1949-50 airing of views.

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[1] For example, in the spring of 1945, Conant favored seeking the international relations ideas of some leading scientists outside of the policy team as a way of consolidating the opinions of scientific experts in private and forestalling public bickering. It will be recalled that the Smyth Report was issued as a book of etiquette for scientific discussions of atomic energy. In the postwar debate on domestic control these scientists refrained from an active lobbying role and testified with restraint in Congressional hearings.

In addition to a developing sense of administrative propriety, they maintained a sense of responsibility for atomic energy developments. Their wartime efforts for international control extended to the hydrogen bomb question.[1] The agonizing discussion of the spring of 1945 on the use of the atomic bomb had made its mark. Oppenheimer probably represented them correctly in defending his position on the hydrogen bomb question in 1954: "I felt, perhaps quite strongly, that having played an active part in promoting a revolution in warfare, I needed to be as responsible as I could with regard to what came of this revolution."[2]

In sum, their wartime experience critically conditioned their concept of role. It explains, at least in part, the differences between the GAC and the Berkeley scientists, who, although they had an important part in the war effort, never occupied the policy positions of the GAC scientists.

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[1] Recall that in August 1945, the Scientific Panel of the Interim Committee strongly advocated international control arrangements because of the possibilities of fusion weapons development. Two of its members, Fermi and Oppenheimer, were members of the GAC. A third member, Arthur Compton, belonged to the cooperating university scientists in 1949-50, and a fourth, Ernest Lawrence, joined the satisfied university scientists. The disillusionment of the GAC scientists with international control efforts did not keep them from further attempts.

[2] Oppenheimer Transcript, op. cit., p. 959.

If the GAC assumed a monopoly on the scientific advisory role, it is not surprising; its nucleus was formed by the intimate wartime group. From the beginning the GAC had functioned with a substantial degree of freedom, unquestioned until the hydrogen bomb debate when its advice was again sought, considered, but not taken.

The Berkeley scientists moved swiftly to counteract the GAC report. Their lobbying activities resembled the freewheeling political activities of scientists in 1945-46. A single-minded approach without consideration of policy nuances and formal channels,[1] to which Conant, Oppenheimer, and Fermi had become accustomed, characterized their activities. Teller, Lawrence, Alvarez, and others did not hesitate to organize support from Congress and other important sources. Except for Lawrence, they had not held advisory posts. Their responsibility as scientists, they asserted, was to determine what science could do, not what it ought to do. Of course, a statement of what was possible to be done on the hydrogen bomb did not necessitate the kind of drive that they conducted, except that they did <u>not</u> limit themselves to the question of

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[1]Szilard and Urey in the spring of 1945 superceded their colleagues in advisory positions and took questions of international control and postwar research to high level officials.

could the bomb be made. In short, by advising its construction they advocated a policy.

Just as Szilard and Urey believed in 1945 that Bush and Conant were dominated by officials who did not appreciate the "imperatives" of atomic energy, Teller and his supporting colleagues did not believe that the GAC, in light of its orientation, could represent their views properly. Both were outside actual policymaking and did not carry the corresponding responsibilities for recommended action. They entered the discussion not for an opportunity to exchange views but to inform influential individuals of their position. Since they advocated the development of a crash program for a hydrogen bomb, a position easy to defend when imponderables are numerous and stakes high, they carried considerable weight.

Some members of the GAC regarded with distaste their colleagues' lobbying activities. It is conceivable that the GAC objected not only to improper procedure but to the fact that the Berkeley scientists proceeded at all into this question. This image of its special competence in matters of science and policy may have been one of the factors which deterred it from conducting its own campaign for support. Members of the GAC may also have felt that, as confidential advisers, they were especially debarred from entering the public controversy. Thus

Conant and Oppenheimer might have believed that the GAC report provided the only possible course of action, and once Secretary of State Dean Acheson registered his opposition, the game was lost for any further action or it was improper to lobby in the face of defeat. The rest of the GAC might have thought it unnecessary to do so.

Teller and his group believed that the GAC was captive of its own advisory position, prompting too cautious an approach to atomic weapons development. Thus, they started their own campaign to be heard while the GAC deliberated over its report. Although theoretically limiting their advice to scientific matters, they did advocate policies. It is not clear whether or not they in fact recognized these as two separate activities. Nonetheless, this theoretical claim enabled them to accuse their GAC colleagues of presenting an incomplete argument and misrepresenting the scientific position in the hydrogen bomb question. Perhaps this accusation contained an element of truth in it. It is conceivable that although the GAC was careful to distinguish the scientific from the political estimate, its self-image of special competence left little room for others to enter legitimately.[1] At the same time, if the Berkeley

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[1] Recall e.g., Oppenheimer's view of his extra competence. See <u>Oppenheimer Transcript</u>, <u>op. cit</u>., p. 232.

scientists realized the place of <u>their</u> special advocacy and limitations in the decision-making process, it might have been possible for them and the GAC scientists to differ on objectives without questioning each other's intentions or integrity.

In general, the satisfied scientists saw their role as expressing from a scientific viewpoint a concern with the inadequacy of fusion research. Their idea of professional responsibility enabled them to speak with considerable authority. Nevertheless, they acted politically too. An increasing sense of dissatisfaction with the substantive content of policy decisions led them to want to influence the decision; their efforts resulted in widening the private debate.

The cooperating and opposing university scientists' call for public discussion after the President's decision may have been provoked, in part, by a normative conviction that something was wrong with the way in which policy was being made and, in part, by the idea that the employment of different procedures such as widening the area of discussion might result in better policies.

The first was represented by the FAS and those of the Rabinowitch, Pauling and Morrison persuasion whose interest in letting the people know stemmed from the idea that if somehow the public was aware of the facts, it could right a situation distasteful to them. Their part in awakening the public to the imminent dangers of atomic weapons was to pronounce on these dangers, not to discuss calmly all the facts and their consequences. It was to enunciate, as did Morrison, that the Soviet blast impelled peace, or to inquire plaintively why it was that morality could not influence policy choices, as was Pauling's habit.

Since they viewed themselves not as a pressure group but as impartial and knowledgeable observers, it was easy for some scientists to equate their interest with the public interest ostensibly not appreciating that they had a special interest which might not have been in the public interest. In the fall of 1949 the FAS advocated another approach to the Russians on control. Once the decision had been made, however, it proposed negotiations and a pledge against first use of the H-bomb. It still believed that somehow political difficulties could be resolved by the scientific facts. In effect, their role as "educators of the people" did not move the discussion toward greater rationality. By publicly indulging in an exposition of their private regrets over public "disasters," these scientists diminished their ability to contribute to a beneficial public understanding.

The willingness of Bethe and his colleagues to speak after January 1950, and the content of their remarks, illustrated a combination of motivations: a desire to let the people know that all was not well and a dissatisfaction with the policy decision, even though, as Bethe himself stated, they decided not to oppose it. Their public discussion was a desperate attempt to alleviate a troubled conscience, representing an effort to balance a realization of the hydrogen bomb's inevitability with a deep reluctance to enlarge the weapons system.

Frederick Seitz justified an active role for these university scientists as a way of defending the ideals of the West and science which were threatened by Russia's closed society.[1] The Cold War, he urged, was relevant to science, illustrating another attempt to equate the ideals of a democracy with those of science, and a way of rationalizing any decision to contribute professional skills to advancing weapons development.

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[1]Recall that in the fall of 1949 Seitz and Bethe argued that adding even more powerful weapons to the American arsenal would not add to the defense posture of the United States greatly unless it was willing to use the bomb supply to prevent attack.

The public discussion centered around international control and moral justification; technical feasibility, advisability, and military worth; the problem of secrecy and scientific information; and democratic decision-making. These questions were especially important to those scientists whose call for public discussion was related to the degree of their satisfaction or dissatisfaction with the content of policy. For example, Conant, who had consistently tried to limit the public discussion to private circles and who believed that the airing of disagreements among experts should be private, complained in January 1950 that important decisions were being made by a handful of men, with the loudest voices being heard. Described as being unalterably opposed to the hydrogen bomb, Conant probably experienced pulls on his idea that appropriate behavior for responsible administrative and advisory officials did not constitute going outside the established structure. At the same time, he was not satisfied with the decision. Did he then think that a wider public discussion might be necessary for decisions to go the right way in the future; that one set of procedures may lead to one decision and another set or combination of procedures could lead to another? However, in the case of the hydrogen bomb, even if the discussion had been widened to include the public, it would probably have gone against the GAC position, a much harder one to defend than that which advocated building the hydrogen bomb.

Nevertheless, Bacher, who was not satisfied with the decision,[1] believed that the public should be intelligent about how much actual security the hydrogen bomb was contributing. He worried about the level of public information on the facts of the situation. Writing in the spring of 1950, Bacher observed:

> Here we have the outcome of what can happen in a democracy when decisions of far-reaching national significance are made without public scrutiny of pertinent information. While most of the pertinent information is not all secret, some of the information the citizen should have in order to judge whether our national policy is sound is being kept secret . . .[2]

Thus, scientists' roles in the hydrogen bomb controversy, which related to their function and degree of satisfaction with the decision, indicated they were undergoing a process of political socialization which other political groups experience upon entering the political arena. Satisfied scientists did not need to conduct a wide

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[1] Bacher did not believe that the hydrogen bomb was a useful addition to American military potential. See Robert F. Bacher, "The Hydrogen Bomb: III," op. cit., pp. 11-15.

[2] Ibid., p. 13.

public discussion. They had lobbied successfully. If unsatisfied advisory scientists did go outside, their comments were still marked by the built-in restraints of long years of service in official advisory and administrative capacities. Cooperating university scientists cast themselves in the public role of telling the people how dangerous these new weapons would be at the same time that they supported the decision.

Others who had held responsible governmental posts, such as Bacher, and those who would hold such posts, such as Louis Ridenour (Chief Scientist of the Air Force in 1951), argued for greater public participation, relating this to the problem of an obstructive degree of secrecy for scientific progress. This call for an orderly public discussion of the available facts was not adhered to by all scientists. Some used the scientific facts to support their political positions without due warning. Had there been a Smyth Report for this period the discussion in some respects might have been more accurate.

Opposing university scientists continued to "let the people know" on the theory that the more facts the public had the more likely its support for international control and eventual abolition of weapons. Ineffective in terms of argument and activity, their particular sense of responsibility alienated them from participation in the Government's program. They continued to be more scientists in discussing problems of science and politics than political beings who knew much about science and its relevance to the specific problem under examination. They were motivated by a primary desire for order on the international scene and a belief that the international relations of scientists could provide the way for peacefully settling disputes among the great powers. An opportunity to demonstrate this arose at the Geneva Atoms-for-Peace Conferences in 1955 and 1958. How successful they were is another question.

In any event, scientists' insistence on wider public participation was an appropriate reminder that it is a government by discussion that "quickens and enlivens thought all through society."[1] However, their attempt at candor on basic scientific information, already available in the public domain, was stifled.

V

The hydrogen bomb question presented scientists with a difficult and unavoidable choice such as they had not had

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[1] See Walter Bagehot, Physics and Politics, "The Age of Discussion," (Boston: Beacon Press, 1956) for a measured and thought-provoking analysis of the view that an age of choice is directly related to a government by discussion.

to make in the atomic bomb decision. Their response represented the most public commitment to participate in the political process since the war and an important turning point in their political development.

The impact of this national security problem on politically articulate scientists was one that brought their diverging viewpoints into full sight. Scientists clearly derived their cohesiveness from sharing professional not political interests. Their interpretation of proper objectives and means ranged widely over the policy continuum. These differences are partially explained by their different roles in this decision-making process and partially by scientific values which were being crucially touched by political issues. The degree of accomodation that scientists achieved between their professional and political selves was affected by their governmental responsibilities, or the degree to which they were held accountable for their advice.

Extensive governmental experience modified the activities of some scientists, who may have recognized the impossibility of solving political problems by separating out variables better than their colleagues outside of government. Perhaps their combination of social responsibility, professional expertise (knowledge and methods), and governmental advisory experience encouraged a self-image of competence to choose the right answer. By the same token, scientists outside of government combined social responsibility, professional expertise (knowledge and methods) and the view that they could better provide the right answer <u>because</u> they were not part of the government and therefore could keep an uncontaminated objectivity. Both approaches resulted in the conviction that scientists knew what was right and good.

Although scientists in governmental advisory positions came to appreciate the substance of political problems more fully, in some instances they shared similar attitudes and habits with their colleagues outside government. Objectives and roles were apparently easier to change.

Scientists did not remain untouched by the political and social implications of the H-bomb decision. Whether in or out of government, all demonstrated an uneasy realization that national security problems would increasingly provide them with difficult political choices. Their ability to find an appropriate balance between professional and political objectives was, and would continue to be, related to public responsibilities and accountability.

PART III

SCIENTISTS AND ATOMS-FOR-PEACE: THE UNITED NATIONS GENEVA CONFERENCES OF 1955 AND 1958

Introduction

The first major peacetime decision on nuclear weapons development provoked a significant and lasting reaction from politically active scientists. It drew them irrevocably into the policy-making structure and discussions of military and foreign policies. For a number of them, it ended a period of ambivalence about their political involvement and pretence that affairs would somehow improve and an armaments race could still be prevented. This occurred in varying degrees of conviction. Some scientists remained unqualifiedly opposed to nuclear weapons development, insisting that there must be another way out. Persuaded that the society of scientists could devise a plan for peace, they tirelessly sought answers to political problems almost solely in the context of the professional scientific environment, with little regard for political contexts or requirements.

The hopes of all scientists for peace and for conducting scientific research and development in a more or less "normal" manner were high in 1945-56. Political events leading to the hydrogen bomb decision and subsequent policy decisions dampened these expectations considerably. They received, however, added vigor in the friendly and encouraging environment of the Geneva conferences on the peaceful uses of atomic energy held in 1955 and 1958. Scientists who had consistently argued that the world was destined for destruction looked to such scientific conclaves to stem the torrent of arms preparations threatening to engulf the world irretrievably.[1]

The United Nations sponsored two international conferences on the peaceful uses of atomic energy. Both gathered for a period of two weeks an imposing body of international scientists, the first with 1428 delegates and the second with 5000. Both dealt with a vast range of scientific subjects on the peaceful atom and had impressive exhibits, testifying to a sustained interest in developing the peaceful uses of atomic energy.[2]

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[1]See, for example, I.I. Rabi, "To Preserve the Scientific Spirit," New York Times Magazine (February 12, 1956), 14ff.

[2]For a complete story of the first conference, "Geneva I", with personalities and highlights colorfully detailed, see Laura Fermi, Atoms for the World: United States Participation on the Peaceful Uses of Atomic Energy (Chicago: University of Chicago Press, 1957). For other relevant sources describing the workings of the Conference, see the official record, Proceedings of the International Conference on the Peaceful Uses of Atomic Energy (hereinafter Proceedings "Geneva I") (New York: United Nations, 1956), 16; Robert A. Charpie, "The Geneva Conference," Scientific American, 193 (October 1955), 27-33; New York Times, August 8-20, 1955.

The first conference, "Geneva I", convened on August 8, 1955, in a glowing international environment. From July 18 to 23, the Big Four met at Geneva in a summit conference during which President Eisenhower made his open skies proposal. Although little of substance was achieved, the "spirit of Geneva" gave the international scientific meeting a favorable atmosphere for its deliberations. This was important because for the first time governments, not individual scientists, as was traditional, were gathered to discuss the peaceful atom, and scientists had to be authorized and commissioned to speak for the governments which they represented.

The second conference, "Geneva II", took place three years later under less propitious circumstances. It opened on September 1, 1958, in the midst of unhappy political rumblings created by the Quemoy-Matsu controversy, the Cyprus question, the Iraqi revolution and the landing of American marines in Lebanon. A few weeks earlier, scientists from the East and West met to discuss the possibilities of monitoring atomic explosions.

For the second, "Geneva II", see the official record, Proceedings of the Second United Nations International Conference on the Peaceful Uses of Atomic Energy (hereinafter Proceedings "Geneva II") (New York: United Nations, 1958), I; New York Times, September 1-13, 1958.

Although the political repercussions of these events were difficult for any interested citizen to ignore, scientists at Geneva were asked to do just that. "You meet today," said President Eisenhower, "as you did then [1955], in an atmosphere which is not political and where as free men of science, your only interest is in the enrichment of man's store of knowledge. May you keep this valuable and necessary attribute inviolate."[1]

Scientists welcomed the opportunity to represent the United States in the area of peaceful uses.[2] The conferences on the peaceful atom provided them and, in some ways, the political leadership with a breathing spell from the stifling nuclear weapons problem. Thus far, policy determinations had favored the development of atomic weapons creating the need for harsh strategic and foreign policy choices. For many scientists these were novel and often unsavory choices, im-

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[1] Proceedings "Geneva II", op. cit., p. 50.

[2] Eisenhower's statement was not the casual factor responsible for scientists' approval and view of "Geneva I and II." In light of their belief that the re-establishment of scientific communication was good for science and a way to peace, they would probably have proceeded in the same manner. Eisenhower's plea was a happy coincidence. This was true also of statements by other political figures. For example, Max Petitpierre's and Jules Moch's words encouraged scientists' in this view. See below pp. 307, 331. The expectations of these non-scientists that science and scientists can contribute significantly to peace placed additional demands on scientists to prove that they could do just that.

posing severe pressures on their professional environment and professional self-images. Even for those who appreciated the necessity and difficulties of these choices, the Geneva Conferences provided a chance to function once more as scientists and reassess their positions in the political world. These occasions offered an opportunity for revitalizing attitudes and expectations which were to emerge subsequentially in the debate on nuclear testing, the next serious choice to face scientific advisers and their political superiors.

In short, these scientific meetings can be viewed as an epilogue to the two major dramas in which scientists played a significant part and a prologue to future difficult political choices involving science and politics. The attitudes revived and the expectations raised at "Geneva I and II" become important for their effect on scientists' subsequent political behavior. A guide to these attitudes and expectations is found in the dominant themes of the Conferences rooted in the basic assumption which echoed throughout the two proceedings: that science and, therefore, scientists could contribute significantly to peace. Something must first be said, however, about the events which led to the atoms-for-peace conferences.

Chapter XI

Scientists, Atomic Weapons, and Candor: 1950-55

I

The destructive potential of nuclear weapons preoccupied many scientists after the H-bomb decision. The period from 1950 to 1955 was spent in search of viable policies, for although this decision constituted the American response to the Soviet A-bomb, it was not a long-range policy. Scientists played a leading role in strategic planning, a task complicated by intransigent Soviet-American relations, vested service interest, and the qualitative difference of nuclear weapons in relation to conventional weapons. In light of the President's decision to develop thermonuclear weapons, they were eager, especially those of the GAC persuasion, to provide alternative possibilities from which to choose.[1]

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[1]See U.S. Atomic Energy Commission, In the Matter of J. Robert Oppenheimer, Transcript of Hearing before Personnel Security Board (hereinafter Oppenheimer Transcript) (Washington, 1954), for a vast array of information on scientists' contribution to the design of nuclear strategy. Also Samuel P. Huntington, The Common Defense (New York: Columbia University Press, 1961), Chapter II, and Robert Gilpin, American Scientists and Nuclear Weapons Policy (Princeton: Princeton University Press, 1962), Chapter IV.

This preoccupation also led to a recommendation for greater candor with the American public about the ramification of nuclear weaponry.[1] The Administration also was concerned about the increasing momentum of nuclear weapons development. Scientists' appeal to hold down the arms race did not go unheard. It was especially sharpened by the Bikini firing, on November 1, 1952, of the first successful fusion device which left no doubt that multimegaton weapons were possible. Indeed, on March 1, 1954, a "droppable" bomb was tested. At the end of the Truman administration the issue of candor was discussed, but nothing resulted.[2]

The discussion was revived in the early days of the Eisenhower administration. Officials were disturbed by top-secret reports that the United States was vulnerable to atomic attack and urged renewed efforts for avoiding the consequences of modern war. What emerged was not a substantive policy blueprint, but a proposal that the President

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[1]In the spring of 1952, the General Advisory Committee, whose chairman was still J. Robert Oppenheimer, reported to President Truman on nuclear weapons' destructive capability. See Robert Donovan, Eisenhower: The Inside Story (New York: Harpers and Brothers, 1956), pp. 184-85. Also ibid., for the State Department's Advisory Committee on Disarmament's recommendation for candor at the end of 1952.

[2]Ibid. Members of this committee were Oppenheimer, chairman, Vannevar Bush, Allen Dulles, John Dickey, and Joseph Johnson.

should deliver a frank address on nuclear weapons.[1] The Administration's Operation Candor was allowed to lapse, despite growing stockpiles and new strategic plans for delivery systems and defense. It was renewed, however, by the Soviet hydrogen explosion on August 12, 1953, which further accentuated the need for candor.[2] What, then, was to be the American response to the prospect of nuclear parity?

II

For many scientists the need for candor was self-evident in an era of nuclear weapons. They favored telling the American people about the dangers involved and giving them a scale for measuring the potentialities and realities of the destructive atom. Oppenheimer likened the Soviet Union and the United States to "two scorpions in a bottle, each capable of killing the other, but only at the risk of his own life." He issued an eloquent call for candor,

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[1] Ibid.

[2] See Walter Millis, Harvey C. Mansfield, and Harold Stein, Arms and the State (The Twentieth Century Fund, 1958), p. 400, for the observation that "under Lewis Strauss as its new chairman, the Atomic Energy Commission was to devote itself even more earnestly than before to preventing any hint of the data essential to a rational discussion from leaking to the public." For a brief summary of the history of official candor beginning with the Smyth Report of 1945, see Ralph E. Lapp, "Atomic Candor," Bulletin of Atomic Scientists (hereinafter BAS), X (October 1954), 312-14, 336.

observing that "we do not operate well when the important facts . . . which limit and determine our choices are unknown. We do not operate well when they are known, in fear, only to a few men."[1]

For the Administration the old Candor speech was to be replaced by an optimistic appeal to the world. The President's suggestion in September 1953 that both the Russians and the Americans "turn over to the United Nations for peaceful uses X kilograms of fissionable materials" caught fire.[2] On December 8, 1953, Eisenhower delivered his now famous United Nations atoms-for-peace speech. It launched, however hesitantly, a program for the development of the peaceful atom but glossed over the stark facts of nuclear weapons and raised the hopes of underdeveloped countries unrealistically. Military obstacles compounded economic and political problems for "the science, the technology, and the industrial development involved in the so-called

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[1] "Atomic Weapons and American Policy," Foreign Affairs, 31 (July 1953), 529-30.

[2] Donovan, op. cit., p. 186. See ibid., pp. 184-86, for an account of how Operation Candor gave way to Operations Wheaties leading to a presidential address which avoided direct stress on the ominous vistas of nuclear weapons. See also Lewis L. Strauss, Men and Decisions (New York: Doubleday and Company, Inc., 1962), pp. 355-62.

beneficial uses of atomic energy appear to be inextricably intertwined with those involved in making atomic weapons."[1]

Nonetheless, for those scientists who in 1949-50 argued for limiting the development of nuclear arms and especially for those who totally opposed the hydrogen bomb development, the President's speech seemed to break the disheartening deadlock in Soviet-American attempts to settle international control problems. His atomic pool proposal was described as the "first international step toward more rational use of atomic energy."

III

Plans for an international atomic energy agency proceeded slowly and laboriously through the labyrinth of

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[1]J. Robert Oppenheimer, The Open Mind (New York: Simon and Schuster, 1955), p. 6. See also Henry L. Stimson and McGeorge Bundy, On Active Service in Peace and War (New York: Harper and Brothers, 1947), pp. 634-35, for Stimson's recognition in September 1945 that the placing of nuclear energy at the disposal of nations without first assuring an understanding between the three great powers would result in chaos.

[2]See David L. Hill's statement in BAS, X (February 1954), 59. Hill was chairman of the Federation of American Scientists Council. The chairman of the FAS Committee on Disarmament and Atomic Control expressed the general hope that "if the forthcoming negotiations should succeed in setting up the cooperative atomic development program, disarmament negotiations might follow at a protracted pace." See David R. Inglis, "The H-bomb and Disarmament," BAS, X (February 1954), 45.

Soviet-American difficulties.[1] The Administration took another initiative. On April 19, 1954, before the Los Angeles World Affairs Conference, AEC chairman, Lewis L. Strauss, announced the President's intention to convene under national sponsorship an international scientific conference on peaceful uses of atomic energy.[2] In August, I.I. Rabi, chairman of the committee for the proposed conference, conferred with Sir John Cockcroft, the director of the British Atomic Energy Establishment at Harwell, and other top scientists. It was decided that the United Nations should sponsor the meeting in Geneva. A United Nations resolution to that effect was unanimously endorsed on December 4, and the Secretary-General requested to convene an international conference on nuclear techniques by August 1955.

Although it was to be several years before the atomic pool became a reality, plans for an international scientific conference proceeded more rapidly with due regard to the then inadequate declassification of materials. Efforts were intensified to accelerate the declassification

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[1]See Robert B. von Mehren, "The International Atomic Energy Agency in World Politics," Journal of International Affairs, XIII, (1959), 57-69, in which the author discussed the Agency's political aspects.

[2]The passage of the Atomic Energy Act of 1954 on August 30 enabled the United States to participate with other governments in planning for peaceful uses.

of basic reactor data. The Secretary-General of the Conference, Walter G. Whitman, indicated that probably "within a year or so all but a small percentage of secret data on weapons will be declassified."[1] American scientists believed that the Geneva Conference offered a singular opportunity for AEC leadership.[2] Why not, queried scientists, declassify in time for insuring a successful conference which "will arouse new vigor in those who seek a way out of the impasses in which the world now finds itself." In fact, "if the conference can manage to avoid becoming entangled in irrelevant political problems, there is every reason to hope that out of it . . . may emerge a blueprint for future international collaboration in the development of nuclear energy."[3] The view that scientific collaboration could offer a means for building the mutual trust which must precede international control was an important stimulant for scientific enthusiasm for this conference.[4]

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[1]Quoted in Bernard T. Feld, "Let's Abolish Classification in the Atomic Power Field," BAS, XI (June 1955), 220.

[2]The Atomic Energy Act of 1954 permitted the Commission to determine the means and the timing for the release of information.

[3]Feld, *op. cit.*

[4]For an expression of it, see, e.g., Donald J. Hughes, *On Nuclear Energy* (Cambridge: Harvard University Press, 1957), p. 164. Hughes was a prominent participant in both Geneva conferences and one of the signers of the Franck Report in 1945.

On January 17, 1955, the Advisory Committee of the Secretary-General of the United Nations met for the first time in New York to discuss an agenda for "Geneva I".[1] Long procedural arguments were resolved by examining first the topical agenda. "As soon as they found themselves on scientific rather than on diplomatic ground, they worked rapidly and in good agreement," reported Laura Fermi, the official historian of "Geneva I".[2] In addition to a long technical agenda, a set of rules was established to keep politics out of the Conference.

By the time the Advisory Committee met again in May and June, many difficulties had been overcome and the Conference took definite shape. Also the Soviet Union announced what was interpreted as a new and propitious stand on disarmament.

The implications of nuclear weapons were not lost on the Soviet government. The existence and extension of discussions between Russian and American scientists was a conscious act of policy by the Soviet Union and the United States, in the opinion of W.W. Rostow. Their possession of

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[1]The Committee was composed of representatives from the United Kingdom, Brazil, Canada, France, the Soviet Union, and the United States.

[2]Op. cit., p. 19.

fusion weapons "led directly to a new dimension in Soviet-American relations -- a quiet dialogue of increasing political maturity among the scientists."[1] This dialogue was especially welcomed by American scientists who believed that their interaction with Soviet scientists could somehow alleviate, if not solve, the weapons problem.

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[1] The United States in the World Arena (New York: Harpers and Brothers, 1960), pp. 350-51.

Chapter XII

Scientists and Atoms-for-Peace: Geneva 1955

I

The theme of international cooperation and scientists' general belief that their profession was uniquely qualified to contribute to peace dominated the first International Conference on the Peaceful Uses of Atomic Energy.[1] Scientists at Geneva emphasized the increasingly important role of science as a unifying element in human culture. "Not only," said the distinguished Danish physicist, Niels Bohr, "is any advance of knowledge, wherever gained, of benefit to total humanity, but co-operation in scientific research offers perhaps more than anything else opportunities for the furthering of close contacts and common understanding."[2]

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[1] See Proceedings "Geneva I", pp. 45-46, for the statement of Soviet representative, A.N. Lavrishchev, that his country "attaches great importance to the development of wide international cooperation," and W.F. Libby's corresponding remarks on the U.S. side. See also ibid., p. 49, for a succinct summarization of this point by Nabor Carrillo of Mexico and Melinko Susic of Yugoslavia.

[2] Ibid., p. 61.

Indeed, since progress in international relations had not kept up with that of science and technology, scientists had a moral obligation to demonstrate that cooperation can occur for common purposes superseding international antagonisms and transcending differences.[1] The cooperation of scientists encouraged governmental cooperation, for scientific and technological advance often required collaborative efforts.[2] Furthermore, asserted Dag Hammarskjold, the UN's Secretary General, scientific cooperation will ease political tensions and have "economic, social and . . . political consequences of deep import."[3]

The fulfillment, however, of all these predictions and aspirations depended on free and broad access to

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[1]See, e.g., Max Petitpierre's welcoming address, ibid., p. 28.

[2]Ibid., p. 125, for Sir John Cockcroft's point that "the magnitude of the effort required to design and construct a nuclear reactor or a modern high-energy particle accelerator, and the unique part played in world affairs by atomic energy, have necessarily led to cooperative effort by groups invested with a formality new in science and technology." Ibid., p. 40. Gunnar Randers of Norway reminded the delegates that "actual scientific technical co-operation is, in the end, a very simple and quiet process." His example -- Norway and the Netherlands, which have cooperated since 1951. Ibid., p. 51.

[3]Ibid., p. 35. See also ibid. for the concurrence of Homi J. Bhabha, President of the Conference and theoretical physicist and chairman of the Indian Atomic Energy Commission.

information and unhampered discussion of problems of human interest. Nonetheless, despite restrictions, scientists' enthusiastic and optimistic outlook toward scientific developments, the functioning of the international scientific community, and toward what they saw as the resulting benefits for international political problems remained intact.[1]

"Geneva I" was marked by a scientific and objective atmosphere, usual at scientific meetings, but noticeable at Geneva since the delegates had come as representatives of governments. This may have influenced scientists to think that, even as political representatives of governments, they were able to maintain objectivity, remain above politics, and achieve the desired scientific and political goals. Although this meeting will have political consequences, observed Bhabha, it differed from political conferences in an important way. "Knowledge once given cannot be taken back, and in organizing this Conference the nations of the world have taken an irreversible step forward, a step from which there is no retreat." The exchange of knowledge among men of science begun at Geneva must continue.[2]

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[1] See *ibid.*, p. 53, for Bhabha's discussion of the technical contributions of "Geneva I".

[2] *Ibid.*, p. 54. See also *ibid.*, p. x, for Hammarskjold's equation of the international sharing of scientific knowledge with UN purposes and principles.

The Conference offered scientists a welcome chance to write about their work for the first time since the war. For the older scientists it meant a return to prewar days of happy exchange and a renewal of professional relationships. For those who began to work in the postwar years it meant a new experience in an old tradition. Their conceptions of the Conference's significant results are important to understanding scientists' political socialization.

II

One important result was the successful cooperation of Russian and American scientists. Even though this had to stand the test of time, it was described as

> . . . a resounding demonstration that a certain segment of the population can work together successfully, and . . . the personal bonds set up among these men, if continued and strengthened with time, may exert a significant effect on the Soviet government in the direction of peaceful coexistence.[1]

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[1]Hughes, op. cit., p. 176. See also Robert A. Charpie, op. cit., p. 33, who observed that, "at least in a limited area, it is possible to overcome formidable language barriers and <u>political precedents</u>." He hoped that a broadening of scientific cooperation would direct "nuclear energy potential toward the good of all mankind in a world at peace." Charpie was head of the unit of scientific secretaries. (Italics added.)

This initial contact, it was hoped, would lead to increased long-term cooperation which "may contribute to a better understanding between the two nations and eventually to the establishment of a secure peace."[1]

The Conference also demonstrated that Soviet science was capable of originality, of posing the proper questions and getting the correct answers. The nuclear cross-sections data[2] illustrated "that you cannot keep a secret from a man who knows the proper questions to ask of nature and who has the means of persuading nature to divulge the answers."[3]

The cross-sections data exchange was successful and reassuring. It reinforced the value of "openness" in science. As one American participant asserted, it created an atmosphere of optimism and mutual confidence, stimulating

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[1]Harrison S. Brown, "Atoms in Geneva," <u>Saturday Review</u>, 38 (September 17, 1955), 24.

[2]International declassification of cross-section data allowed its comparison before Geneva at Brookhaven National Laboratory. There was remarkable agreement in results and a closer agreement between the Russian and American data than between the American and British data. A cross-section is the quantity that expresses the strength of interaction between a nucleus and neutrons; the larger the cross-section the more likely the nucleus is to absorb the neutron, thus causing a nuclear reaction. See Hughes, <u>op. cit.</u>, for an account of the cross-section data exchange.

[3]Brown, <u>op. cit.</u>

informal seminars in which individual scientists participated with no evidence of group or national feeling.[1]

That nature could not distinguish national boundaries did not surprise scientists but came as a revelation to the lay world, including Senator Clinton P. Anderson and a majority of the Joint Congressional Committee on Atomic Energy. At a press conference Anderson stated that "many of the things we thought were secret are not as secret as we thought."[2] This elicited Harrison Brown's comment that if members of the Joint Committee really appreciated the fact that ultimately there are no secrets where nature is concerned, then, "from this point of view alone the meeting should be considered a success by the Americans."[3]

For scientists, the cross-sections experience and sophisticated Soviet knowledge of the field of nuclear power exposed the myth of security through secrecy.[4] Although

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[1]Victor Weisskopf, "A Theoretical Physicist at the Geneva Conference," BAS, IX (October 1955), 278.

[2]New York Times, August 19, 1955, p. 5.

[3]*Op. cit.*

[4]It could build reactors similar to American reactors and Russian experimentalists showed a good understanding of the theoretical background of experiments being done by big machines. It was believed that this competence derived more from an increasingly solid core of trained scientists and engineers in nuclear science than from those

agreed on the principle of security through scientific achievement, scientists disagreed about American willingness "to sacrifice the impetus to the development that full disclosure would give."[1] This was illustrated by the debate Ralph E. Lapp had with Eugene Wigner and Frederick Seitz, reactivating discussion of an old problem -- how much secrecy?[2]

Lapp believed that a nuclear power program should carry no security restrictions, permitting industrial participation without clearance.[3] Wigner and Seitz argued that the amount of technical know-how disclosed by the United States at Geneva was considerably larger than that of the Soviet Union. In those instances where the Russians appeared

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scientists who had defected to the Soviet Union. Scientists argued that the rate of Russian progress in military technology and technical competence in other fields was not hindered by American secret policies.

[1]Hughes, op. cit., pp. 219-20.

[2]See Ralph E. Lapp, "The Lessons of Geneva," BAS, XI (October 1955), 275, 308; Eugene P. Wigner and Frederick Seitz, "On the Geneva Conference: A Dissenting Opinion," BAS, XII (January 1956), 23-24; and Lapp's subsequent reply, "Nuclear Power Secrecy," BAS, XII (April 1956), 135.

[3]"The Lessons of Geneva," op. cit., p. 308.

to have reached original solutions in the reactor field, they did not describe them.[1]

Wigner and Seitz objected to the impression that atomic secrecy on nuclear reactors was foolish. They contended that not all scientists took this position and pointed out that "we must bear in mind the fact that if the other side learns all that we know, . . . we will find it very hard to stay ahead of the game" unless the exchange is reciprocal.[2] The Geneva Conference was a success not so much for the information learned as for the precedent created. Since most of the pertinent scientific information had been released before the conference, what was divulged at Geneva was technological. And Soviet reluctance to exchange freely clearly indicated they recognized what was involved in industrial secrets.

While not recommending a return to the old secrecy on reactor developments, Wigner and Seitz believed that the question of secrecy should be decided on the merits of each

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[1]See Wigner and Seitz, <u>op. cit.</u>, p. 23. This unwillingness to reciprocate makes it difficult to understand Lapp's across-the-board demand for no secrecy in the nuclear power field, particularly in view of the prestige that a nation receives for leadership in it.

[2]<u>Op. cit.</u>, p. 23.

case. These scientists were correctly suggesting that there may be some instances in which the greater security may be obtained through secrecy.

Although effort was exerted to permit normal technical and scientific cooperation, politics was not totally absent at Geneva and emerged to threaten the principle of scientific freedom. H.J. Muller, distinguished geneticist from the University of Indiana, submitted a paper to the technical staff of the AEC, which accepted the contribution. However, the Atomic Energy Commission refused it and, ostensibly, because Muller had dealt with the Hiroshima bombing, labeled it "definitely inadmissible" to a conference devoted to peacetime uses.[1]

Scientists pointed out that this exclusion was undesirable on two grounds. First, it was important to know the reasonable upper limits of radiation for human absorption, since peacetime uses of atomic energy were going to increase. They argued that it will become progressively more expensive and difficult to change inadequate precedents, once established. Secondly, authoritarian or

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[1] Science, 122 (October 28, 1955), 822. Although the paper was not admitted for oral presentation, it was printed in the proceedings of "Geneva I".

arbitrary hindrance of free discussion should be viewed with alarm, particularly when the subject concerns the existence of man.[1]

The Muller incident demonstrated that scientists were, indeed, official delegates from their countries and as such had to obey orders even though Admiral Strauss admitted at a news conference on October 3, 1955, that this controversy was a "regrettable snafu."[2] In any case, such official representation was alien to scientific beliefs and scientists objected. Where science and politics intertwine, incidents are bound to arise. Resolving them will require talent for coordinating national security requirements and those of science.

III

Scientists left "Geneva I" with an optimism ". . . not seen in scientific circles since the war." They were at the same time cognizant that "the roof may fall in at any

[1]See George W. Beadle, "H.J. Muller and the Geneva Conference," Science, 122 (October 28, 1955), 813. See also the editorial in BAS, XI (November 1955), 314-16, 343, on the Muller story.

[2]It was not clear whether he thought the incident regrettable because it had become embarrassingly public or whether he thought that the policy of turning down a contribution on a sensitive subject was unwise because it might prejudice public opinion against further testing.

time and that the situation may revert overnight to its previous status."[1] But Eisenhower's assertion of December 8, 1953, "that the atomic power plant is a weapon more powerful than any hydrogen bomb, and that the danger of atomic annihilation will be decreased in direct proportion to the sharing of beneficial uses of the atom," was particularly well-received.[2] Indeed, Kurt Kraus observed some years later that he had never before nor since seen such spirit as scientists had in "Geneva I". The general feeling was that this first postwar scientific conference broke a vicious circle of arms acceleration and of the bad taste left by the McCarthy investigations and the Oppenheimer case.[3]

The American scientific representatives at Geneva believed that the sharing by the United States of its vast store of information was a demonstration of good faith and scientific leadership, both of which were bound to bring a political advantage to this country. It was generally

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[1] Harrison S. Brown, op. cit., p. 24.

[2] See William L. Laurence on this point in New York Times, August 21, 1955, p. 1.

[3] These observations were made in an interview I had with Mr. Kraus, an Oak Ridge National Laboratory scientist, on June 26, 1963, in Oak Ridge, Tennessee.

agreed that the new spirit of international cooperation which marked the Conference "would also serve as an opening wedge for removing obstacles still standing in the way of reaching an agreement on [weapons] . . . controls,"[1] that such conferences as "Geneva I" offer opportunities for developing Soviet-American trust, thus reducing international tensions and perhaps achieving world peace in the long run.

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[1] New York Times, August 21, 1955, p. 1.

Chapter XIII

Scientists and Atoms-for-Peace: Geneva 1958

I

The theme that scientific international cooperation would lead to international cooperation in other fields and that the international society of scientists could provide the necessary leadership was a major one at "Geneva II".[1] Paul R. Jolles of The International Atomic Energy Agency noted that

> it is not the least important aspect of atomic energy that it has provided strong impetus for joint action among the nations of the world -- an impetus which derives from the realization that national self-sufficiency cannot safely nor economically be pursued in the field of peaceful uses of atomic energy.[2]

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[1] Proceedings "Geneva II", op. cit., p. 364, for I.I. Rabi's expression of it. This was a familiar note among scientists at "Geneva I" also. See, e.g., Hughes, op. cit., p. 218.

[2] Proceedings "Geneva II", op. cit., p. 373. For his discussion of the fields in which international cooperation is useful and used and the methods by which these endeavors are carried out and the way in which atomic energy developments have begun to affect patterns of international relations, see ibid., pp. 373-80.

Thus, atomic energy was again seen as a unifying force helping to achieve political cooperation and, in some instances, integration, as exemplified by CERN and EURATOM.

It was one of the conclusions of the Conference that atomic energy secrecy had been so lowered that international scientific cooperation became not only possible but "almost inevitable."[1] Scientists believed that, although political obstacles to the free exchange of scientific information still remained, their collaboration could help reduce these. As Lloyd Berkner observed:

> Since this difficulty has national origins, intimate and formal discussions and negotiations among the scientific academies and councils might easily reverse this trend that now, at least in part, isolates great nations scientifically.[2]

Similarity in fusion research proved again the universality of science, as did the cross-sections data exchange at "Geneva I".[3] The point was, said Berkner, that all scientists recognize that "original thought and methods

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[1]See New York Times, September 2, 1958, p. 1, and September 11, 1958, p. 6.

[2]Proceedings "Geneva II", op. cit., p. 435, in a general address to the Conference.

[3]See New York Times, September 6, 1958, p. 4, for the comment of Arthur E. Ruark, head of the thermonuclear program in the United States that "we have learned some new things. There are some differences in the machines, but in principle they are closely similar."

in science know no boundaries of politics, race, religion, or political philosophy -- that science flourishes best when all sources find equal opportunity to contribute."[1]

II

By the time "Geneva II" convened, three years of research had exposed the difficulties of achieving atomic power which could compete in cost with conventional sources or become useful in the economies of the underdeveloped nations.[2] Scientific eagerness to demonstrate the benefits of science at "Geneva I" made it difficult for them to tone down statements on what the atom could accomplish in the underprivileged parts of the world.

Another distinctive aspect of the Conference was the discussion of fusion power. The highly secret thermonuclear

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[1] Proceedings "Geneva II", op. cit., p. 430.

[2] The peaceful uses of atomic energy in the new nations were limited by the lack of any industrial base and the high cost of such power facilities. Many factors operated to retard economic progress, such as low rates of saving and investment and a disinclination to change. See Klaus Knorr, "American Foreign Policy and the Peaceful Uses of Atomic Energy," pp. 113-16, in Atoms for Power: United States Policy in Atomic Energy Development (New York: The American Assembly, Columbia University, 1957), for a discussion of nuclear energy and the realistic and continuous studies to determine the economic and social feasibility of nuclear energy.

research was touched upon in 1955. By 1958 the condition of nuclear parity and mutual deterrence led to a higher emphasis on weapons, delivery systems, and defense than on improvements in weapons themselves. The decline of the military need for secrecy in this area enabled the United Kingdom and the United States to announce jointly the removal of all secrecy in thermonuclear work, permitting their scientists to interact freely. The Soviet Union released its research results up to 1958 but would not commit itself beyond that year. Soviet scientists seemed eager to meet periodically to discuss results from Ogra and DCX.[1]

The declassification of data on thermonuclear reactions by the United States and the United Kingdom and to a lesser extent the Soviet Union gave the Conference a sense of expectancy. The opportunity to exchange useful ideas and "the intellectual struggle that precedes all technical success, particularly with teams working both in

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[1]Ogra is the world's largest controlled thermonuclear research device recently finished in the Soviet Union. A model of it was shown in the Russian exhibit and surprised Western observers. The DCX is the American machine for fusion research at Oak Ridge National Laboratory. American scientists estimated that the United States was about a year ahead of the Soviets in this field. See New York Times, September 1, 1958, p. 2.

unison and in competition" was a gratifying one.[1] Yet, although high-energy physics, a subject far from military significance, was declassified in Russia in 1955, there was little evidence that the exchange was completely open. It seemed as if the ideal of an open world of science was still an ideal.[2]

Upon discovering that both the Soviet Union and the United States were pursuing similar lines of research to produce a controlled thermonuclear reaction, the Russians informally proposed Soviet-American collaboration on the fusion process. In his reply, James R. Killian, Jr., special assistant to the President for science and technology, observed that while the American delegates endorsed the principle of international scientific cooperation, scientific cooperation in terms of formal exchange of scientists and information raised "matters of political significance . . . which have to be considered in Washington."[3] The question of how far the United States would wish to go in promoting periodic East-West scientific meetings could not

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[1] Eugene P. Wigner and Frederick Seitz, "Pure and Applied Nuclear Physics in East and West," BAS, 15 (March 1959), 130.

[2] Ibid., p. 131

[3] New York Times, September 12, 1958, p. 7.

be answered except tentatively in view of unpredictable political developments.

Political overtones also touched the question of explosives for peaceful purposes. An American suggestion that thermonuclear explosives could be used for peaceful purposes sharply broke the objective scientific atmosphere. Vasily S. Yemelyanov, the head of the Soviet delegation, attacked this "as a political device designed to conceal a desire in the United States to continue testing nuclear weapons."[1] The American delegation refused to be drawn into a political dispute, even though the Russian delegate made a personal attack on Willard F. Libby for trying to discover ways to continue nuclear testing.

Project Plowshare -- atomic explosives for peaceful purposes -- raised skepticism also among some Western scientists. Libby explained that there was a "good chance of controlling the radioactivity particularly of thermonuclear explosives are used."[2] These shots would be conducted under international observation to avoid any suspicion of secret development of weapons. Efforts in this area can always be reviewed, declared Wigner and Seitz, "if it turns out that

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[1] New York Times, September 4, 1958, p. 1.

[2] New York Times, September 12, 1958, p. 7.

the Soviet Union really is willing to carry through a test moratorium under conditions guaranteeing adequate inspection and controls."[1]

III

The second Geneva atoms-for-peace conferenced ended on a note of restraint about the immediate uses of atomic power. In spite of recognized lack of success in the practical application of this knowledge, scientific optimism prevailed. The control of thermonuclear reactions raised a special sense of excitement. Again scientists' expectations pointed toward large benefits accruing to all nations from developments in the peaceful uses of atomic energy. The general attitude that the scientific community was uniquely equipped to provide these benefits strengthened the assumptions that science was an international and pervasive force for peace, that it recognized no national boundaries in its search for truth, and that it could mark the way toward international cooperation in other areas -- namely political problems.[2]

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[1]Wigner and Seitz, "Pure and Applied Nuclear Physics in East and West," op. cit., p. 130.

[2]American scientists welcomed similar expressions by their Western European colleagues. Some, however, were more reluctant to believe that cooperation in one area would

For many scientists, "Geneva II" represented a successful example of how their community could cooperate in a world torn by political strife. They believed that they had begun the important process of developing mutual trust between the Soviet Union and the West. At the same time, American scientists who did not believe in extending the arms build-up indefinitely were heartened by the meeting of experts from the Soviet Union, the United States, and the United Kingdom in July 1958 to discuss the control of nuclear weapons. As its title indicates, it was a "Conference of Experts to Study the Possibility of Detecting Violations of a Possible Agreement on Suspension of Nuclear Tests." By the time "Geneva II" was convened, the discouraging political repercussions of this Conference had not yet emerged in full. It seemed to scientists that they were working successfully on two fronts to encourage a peaceful world.

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necessarily lead to cooperation in another. For example, as early as "Geneva I", Wigner and Seitz indicated an awareness that Russian scientists were not as free-wheeling as their Western colleagues.

Chapter XIV

Scientists in the Scientific Environment

I

The professional objectives of scientists dominated the two international conferences on the peaceful uses of atomic energy. Scientists met at "Geneva I and II" specifically for scientific discussions and exchange of information. This was their unambiguous concern. Nonetheless, the point of this analysis has not been to describe their scientific achievements but to examine the relation of the attitudes and expectations, which the conferences reinforced, to scientists' subsequent political behavior. Thus, the conferences become significant for the additional insights they offer into factors which helped or hindered scientists' political socialization.

An examination of the processes that stimulate scientific activities and mould the attitudes of scientists will give some measure for understanding the scientific expert in the policy process. These observations are, however, limited ones, based as they are on an examination solely of the public record. On the evidence of this record, scientists appeared to be unconscious of their limits in the political environment. If their private and public views were indeed the same, then perhaps international conferences such as "Geneva I and II" have strong limits. If their statements are taken at face value, what is the moral to be drawn from them? If the private view differs from the public one, then what motivated scientists' political judgments? Did they try to manipulate consciously for propagating their views? If so, why? What were their political purposes?

Such questions as these can better be answered by reconstructing the inner or private discussion to balance the analysis of the public record and yield more complete insights. Such an attempt, however, is beyond the scope of this study. For the moment, the following observations must remain necessarily tentative, awaiting additional work.

II

In contrast to Western scientists, especially Americans, Soviet scientists were often more restrained in their expectations about the political yield of the conferences. They described "Geneva I" carefully as "an important step for establishing personal contact among scientists of all countries"[1] and seemed much more ready than Western scientists to accept the inevitable convergence of science and politics as a fact of life.[2] American scientists, especially, expected scientific and positive political results to flow from the Geneva meetings. This expectation was based on another: that increased contact between Soviet and Western scientists will augment mutual understanding and respect and lessen tensions and suspicion.[3]

At Geneva this idea received added vigor from the strength of some assumptions that the Western scientific world treats as "givens." The assumption that science, and therefore scientists, have transcended "the limitations of

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[1]Quoted in New York Times, August 21, 1955, p. 34.

[2]Perhaps Soviet scientists representing the USSR at such a conference are required to be skilled politically as well as scientifically. For example, some years later with regard to the negotiations at the Conference of Experts, James Fisk, who had been chairman of the American scientific delegation, observed that the Russians had always been aware of the inseparability of the technical and political considerations in the question of nuclear testing. See U.S. Senate, Committee on Government Operations, Strengthening the Government for Arms Control, Senate Document No. 123, 86th Cong., 2nd Sess. 1960, p. 7. John Turkevich, the American ad hoc scientific attache in Moscow, stated in a public address on January 11, 1961, in Princeton, New Jersey, that over half of the members of the Praesidium have a technical or scientific background.

[3]This view still persists. See, e.g., I.I. Rabi, "The Cost of Secrecy," The Atlantic Monthly, 206 (August 1960), 39-42.

the particular environment in which its devotees existed"[1] was more meaningful to Western than to Soviet scientists. The Soviet delegation to "Geneva I" believed that the co-operative spirit of the conference was created not by scientific cooperation but by the Geneva Summit Conference in July 1955.[2] American scientists preferred to emphasize the universality of scientific research as demonstrated by the close comparability of results in the cross-sections data exchange of different nations.

Another assumption of Western scientists was that scientific progress requires a reduction in restrictions on international scientific exchange. Recognizing the need for secrecy in the military applications of atomic energy, scientists still faced the crucial and divisive question: how much secrecy? The problem arises in the attempt to find a modus vivendi between the values of science which insist on openness and national security needs which sometimes require a high degree of secrecy. Despite the fact that the abundant production of cheap atomic energy increased a nation's war potential, most scientists favored "openness," believing

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[1] I.I. Rabi, "To Preserve the Scientific Spirit," op. cit., p. 14.

[2] New York Times, August 21, 1955, p. 34.

that the positive results far outweighed the disadvantages of secrecy.

The underlying theme of the secrecy-security discussion was found in scientists' belief that nuclear science would benefit mankind. Insistence upon scientific freedom then became an important corollary. This theme persists. Recently, Glenn T. Seaborg, chairman of the Atomic Energy Commission, declared:

> . . . The first Geneva conference in 1955 will stand as a signal point marked by its voluminous declassification of scientific information gained during the war years. There the nations came together to witness concrete evidence of what nuclear science, shared in large measure, could do for the world's future social and economic development.[1]

The idea that science benefits mankind[2] and the enthusiasm generated by scientific interaction at the conferences facilitated another assumption. The more energy

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[1] "Toward an Open Scientific Community," The Department of State Bulletin, XLVII (October 22, 1962), 623.

[2] For a recent expression of hope in technical progress, see J. Brunowaki, "'1984' Could Be a Good Year," New York Times Magazine (July 15, 1962), 12ff. He explains the connection between science and peace by discussing the forces that scientific and technological advance can set in motion for the public good and makes a plea for using science in constructive rather than destructive ways.

directed to develop the atom's beneficial uses the less danger of an atomic war. Scientists viewed atomic energy as a unifying force, especially since international political structures were being created for its development. Scientific collaborative efforts cut across national boundaries with ease. Why not collaborative enterprises in other areas? The universal appeal of science enabled its practitioners to ignore the powerful appeal of national sovereignty.[1] They cast themselves in the role of world citizens and peacemakers, believing, according to Rabi, that "men separated by political and geographic barriers, nevertheless, can combine in a common human endeavor." He further declared:

> . . . In our continued struggle for the freedom of communication between scientists and of freedom of movement of scientists from one country to another we are engaged in a notable effort, we are fighting to realize the highest aspirations of mankind and for peace and understanding between men and nations. It is a good cause.[2]

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[1]See Proceedings "Geneva II", op. cit., p. 116, for Jules Moch's observations that "if mankind wishes to fully profit from the new conquests of science, many of the historical national partitions will have to fall."

[2]"To Preserve the Scientific Spirit," op. cit., p. 36.

III

The American initiative which resulted in the first Geneva conference came at a time when both the Soviet Union and the United States were producing increasingly more powerful weapons. It seemed propitious to ease the emphasis away from nuclear weapons to peaceful uses. Despite serious difficulties in Soviet-American relations, scientists insisted that the Geneva meetings were a positive note in an otherwise somber picture, providing a significant step toward peace.

They generally believed that the 1955 conference was a first and important move toward the reestablishment of international scientific relations, and communication among other learned groups. In their view, this kind of communication would inevitably generate better international relations, breaking down existing barriers. However, a common scientific language enabled scientists to communicate without encountering obstacles, cultural or otherwise, that might obstruct other groups in a similar exchange.

In science, frustration arises mostly from the scientific problem itself, not from what might impinge on that problem. For the most part, scientists at Geneva were able to function freely. Much professional satisfaction was received from the exchange. It was easy for them to be optimistic and think that perhaps this was indeed the way to peace; that a nuclear reactor *is* a more powerful weapon than a hydrogen bomb. But, there was a serious tendency to forget, for example, that building a nuclear reactor was a matter quite different from planning for its use in underdeveloped areas. Scientists appeared more concerned with the integrity of science than its practical application, by which its integrity is maintained in the final analysis.

In sum, recognized difficulties did not dull their enthusiasm. Since only scientific questions were discussed, for the most part, the political implications of the atoms-for-peace program were overlooked. Nevertheless, no matter how beneficial peaceful applications were, they did not remove nor really balance the threat of atomic weapons. "Geneva I and II" may have produced positive scientific results and pointed toward benefits for mankind, but many important problems remained, some scientific, some political, some economic and social. They did not contribute dramatically and significantly to these problems; on the contrary the conferences may have reckoned insufficiently with the revolutions of rising expectations. New scientific knowledge was stressed, not application of technical knowledge of more immediate value to developing nations.[1] Certainly the three years between "Geneva I and II" provided sufficient opportunity to observe that international scientific meetings are not the panacea for political problems, though they may reduce tensions momentarily.

If social, political, and economic implications of atomic energy had been discussed or at least recognized at Geneva, perhaps scientists might have developed a greater tolerance for problems of statesmen. Instead, they were concerned that their professional commitment was not understood. For example, Rabi complained, soon after the first conference ended, that few grasped "the emotional commitment we have to expand and deepen our understanding of nature, nor does the quest seem to be particularly important except under the aspect of the conquest of nature."[2] Despite this parochialism, scientists were correct in trying to wean the American course away from an accelerated arms race and in

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[1]See Caryl P. Haskins, "Technology, Science, and American Foreign Policy," <u>Foreign Affairs</u>, 40 (January 1962), 232-235, for a discussion of how best the United States can proceed to help the new nations in their scientific and technological advance.

[2]"To Preserve the Scientific Spirit," <u>op. cit.</u>, p. 14.

suggesting that the search for security not be limited purely to military means.[1]

IV

The Geneva Conferences of 1955 and 1958 occurred at a crucial time in the thinking of American scientists. Especially for those who in 1950 had concurred reluctantly with the President's H-bomb decision, 1955 represented another fork in the road. Nuclear parity and mutual deterrence were now political facts, fundamentally affecting any policy design. Still deeply conscious of the incremental features of arms development, these scientists, who were to hold important posts in the 1958 Geneva negotiations on the possibilities of monitoring atomic tests, decided that another try to stop the arms race was necessary and appropriate.[2] University scientists who had consistently

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[1]For an extensive discussion of nonmilitary possibilities, see U.S. Senate, Committee on Foreign Relations, Possible Nonmilitary Scientific Developments and Their Potential Impact on Foreign Policy Problems of the United States. A study prepared by the Stanford Research Institute, 1960.

[2]For a discussion of factors strengthening their desire for another attempt to solve the weapons problems, see Robert Gilpin, American Scientists and Nuclear Weapons Policy (Princeton, N.J.: Princeton University Press, 1962), pp. 137-50.

opposed arms development in favor of international control and suspension of tests advocated new efforts also. For these scientists, the Geneva atoms-for-peace conferences strengthened a number of ideas which were politically significant in the nuclear testing debate.[1]

On the basis of congenial interaction of Russian and American scientists at the Geneva conferences, they believed that the possibilities were substantial for Soviet-American cooperation in scientific and political matters; also that agreement was prevented by a lack of mutual trust. "Geneva I and II" represented successful and important initial means toward peaceful collaboration, just as the Conference of Experts in 1958 on monitoring nuclear tests was an important first step toward control. The impressive scientific results of the meetings contributed to the idea that science is a force for peace, and that scientists were the agents to design the peace plan for decisive solutions of political problems. These scientists also interpreted

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[1]Scientists occupying top-level research positions experienced no agonizing reappraisal. For them, it was clear, as in 1950, that the United States must maintain leadership in nuclear weaponry. Although they welcomed the conferences, they did not see them as omens of a peaceful world. See *ibid.*, pp. 217-18, for their pessimistic view that the results of the Conference of Experts to Study the Possibility of Detecting Violations of a Possible Agreement on Suspension of Nuclear Tests created more problems than it solved.

the Conference of Experts as a victory for science and its methods, and for scientists over traditional diplomacy and diplomats.[1]

Another idea emphasized at the Geneva meetings and important for subsequent events was that scientists could maintain scientific objectivity in any situation. Even the political leadership assumed that they wore this mantle of scientific objectivity. It was also assumed that the separation of technical from political considerations, usual at scientific conferences, would operate at a conference of experts on detecting nuclear tests in which political factors were vital to national security. These assumptions had unhappy consequences for the negotiations on nuclear testing.

The optimism of these scientists, such as Bethe and his supporting colleagues, ignored the minimal nature of the success of the Conference of Experts. The desire of scientists in the American delegation, on which Bethe served, to achieve a ban on nuclear testing and their belief that Russia would unquestionably pursue this rational course of action influenced their "scientific objectivity." For example,

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[1]See Eugene Rabinowitch, "Nuclear Bomb Tests," <u>BAS</u>, XIV (October 1958), 282-87, for an example of this view with reference to their belief that the 1958 Conference of Experts was successful because of the special contribution of scientists.

American scientific experts at Geneva did not sufficiently emphasize the technical difficulties of detecting nuclear explosions. As a matter of fact, the concealment problem had not, according to Freeman J. Dyson, been adequately explored.[1]

Concentrating on technical measures to solve what was essentially a political problem, scientists at the Conference of Experts defined their function as a technical one. They appeared to assume the same kind of accord with Russian scientists in the conference on monitoring nuclear tests as they had at "Geneva I and II", overlooking the fundamental political nature of this question which complicated scientific exchange and objectivity. It was doubly difficult, therefore, for them to perceive the rigidity of their means and the nonrational elements in their approach.[2]

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[1] "The Future Development of Nuclear Weapons," Foreign Affairs, 38 (April 1960), 461. See also Gilpin, op. cit., pp. 214-18.

[2] See ibid,, pp. 223-61, for an account of the technical and political consequences of the Conference of Experts and of the failure of the "first step" idea to produce a viable agreement.

These scientists also reflected President Eisenhower's deep wish to achieve a nuclear test ban.[1] Again, the political leadership, in effect, did not clarify the limits of technical expertise in politics. Instead, it may also have unconsciously looked for automatic solutions, designating scientists to find them. The political leadership was divided on this issue of whether or not to seek a test ban, and scientists, who especially favored it, stepped into the vacuum with their own views.

The conflict about a nuclear test ban clarified that not only may Soviet and American scientists interpret the scientific facts differently but that among themselves American scientists could and did disagree on the interpretation of the same set of scientific facts. They too were motivated by their basic political beliefs concerning political strategy and tactics in the Cold War. The obligation to let the fact rule was upheld, but it ruled in different ways for different scientists.[2]

The recent Soviet-American agreement on a nuclear test ban does not detract from the point of these observations.

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[1] Ibid., pp. 200-01.

[2] See ibid., pp. 262-98, on the intra-scientific conflict on the nuclear test ban question and the problem of conflicting expert advice which it raised.

There are bound to be other questions of similar substance which will require political astuteness on the part of American scientists who advise the government. Scientific conferences which deal with scientific subject matter allow scientists to function purely as scientists. They provide, more or less, an environment which is appropriate for scientific discussion. American scientists must not think, however, that the understanding which they develop with scientists from other nations at such conclaves necessarily transfers to conferences in which they participate, but where politics dictates the possible. Inevitably, political problems with important scientific components require their participation; also their respect for the limitations which political factors impose on technical solutions.

Such occasions as the Geneva conferences can be used more effectively as policy instruments if their limitations are recognized. The interaction of Soviet-American scientists can be a useful instrument to gauge Soviet intentions if American scientists appreciate the interplay of technical and political factors and political officers do not make unrealistic demands of scientists.

CONCLUSION

The principal thesis of this inquiry is that with a little more humility the political education of scientists would have proceeded more rapidly and the public interest would have been better served. The range of political factors which scientists considered as they participated in policy-making was often limited by the perspectives which they brought to their political tasks. Although these perspectives may have been an inevitable consequence of professional specialization, the scientists' contribution to political decisions would have been more rational and effective, if they had made a conscious effort to augment their understanding of the political process and their part in it. The increasingly frequent burdens of joint professional and governmental responsibility that were shouldered by leading scientists did provide an opportunity to develop a set of values, attitudes, and motivations for responsible advice and advocacy in the policy process.

Although scientists' policy predispositions were initially moulded by professional values, their hierarchy of values began changing as they assumed functions beyond the laboratory. The degree to which this re-ranking of values occurred was closely related to the manner in which they were involved in political issues. The closer scientists were to points on a continuum marking official advisory responsibilities, the greater the impact of political realities which had to be faced in the decision-making process on professional perspectives. For example, atomic scientists officially responsible for policy advice found it necessary to seek an accommodation between their professional and public responsibilities. These advisory scientists discovered early that a wholesale transfer of habits and attitudes from the scientific to the political environment was not possible, if they wished to collaborate in policymaking with other officials.

In summarizing the factors which affected scientists' political growth, it is helpful to imagine a continuum of possible involvement in policy questions for a clearer view of their differences and similarities in the political process and of the factors orienting scientists towards coordinated policies. What, then, were some of the significant elements in scientists' political behavior in the three policy issues of this study which, although they do not apply equally to all scientists, do represent some central characteristics of their participation in policymaking?

As scientists began to operate in the policy process, they exhibited a strong professional interest in separating completely the rules for the conduct of science from social and political requirements. Their political objectives and policy choices frequently represented attempts, expressed or implied, to maintain professional desires or assumptions without modification, despite difficult political situations and new public functions which continuously undermined some of their fundamental assumptions about science and themselves. But, in their eagerness to preserve a system of beliefs which they shared and believed necessary to their business, most scientists did not question how realistic these assumptions were in political terms.

As illustrations four can be distinguished. Science is international, recognizing no national boundaries and evolving for the benefit of mankind; science and, therefore, scientists are totally objective and value judgments do not affect their advice on the application of scientific discoveries; responsibility for the social use of science can be exercised peripherally--without entering too fully into the stream of politics; and, finally, there is one right answer to a problem which reason can discover.

Strong adherence to these assumptions accentuated a tendency among some scientists to confuse what is desirable with what is politically possible in the present.[1] Their considera-

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[1]See Eugene P. Wigner, "Twentieth Birthday of the Atomic Age," New York Times Magazine (December 2, 1962), 34ff for an example by a scientist of how scientific values can distort scientists' political evaluation.

tion of political problems in the framework of the scientific environment facilitated mechanical, neat answers and their insistence that they were not special pleaders substantially hindered their political growth. They did not realize that only by admitting the presence of conflicting interests was some balance between them possible. But, as they increasingly diverged on political objectives, scientists closest to policy responsibilities were especially impressed with the need for accommodating the conflicting interests of science and politics. In order to survive as effective advisers, these scientists could not afford the luxury of self-delusion. Some *modus vivendi* had to be accomplished.[1]

For many members of the scientific community, however, inappropriate assumptions and ways of approaching political problems converged to stymie their political socialization. The desire for continuing scientific activities with as little dislocation as possible, for preserving valued professional assumptions, and for political order and certainty--all contributed to unrealistic political designs and expectations and stimulated behavioral characteristics affecting the nature

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[1] Cf. the FAS's consistent position that their interests were above politics with the efforts of the GAC scientists to meet the necessities of political questions.

of scientists' political participation. Scientists shared these characteristics, more or less, depending on their political role.

Faced with hard political crises, scientists approached these issues more as scientific experts solving problems than as political beings faced with difficult choices.[1] Their experience from 1946 to 1948 encouraged the view, even among advisory scientists, that they functioned objectively, uncontaminated by the political atmosphere. During that time, for example, the GAC advised the Atomic Energy Commission to pursue a moderate fusion research program, a policy which was followed apparently without much debate outside the inner circles of the GAC or AEC. Even though this represented more a failing of the political leadership, it strengthened scientists' belief in their objectivity until the H-bomb controversy seriously undermined it.

Scientists' self-image as scientific experts solving problems was further illustrated by their staunch belief

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[1] Recall, e.g., the FAS proposal in February 1950 for the establishment of a panel of experts to consider political problems objectively.

that international control can be achieved despite repeated failures, and that science and scientists can lead the way to political solutions. To a significant degree, scientists did not understand that political problems may not have end solutions but instead may transmute themselves into other problems. They did not grasp the perpetual challenge and response and the inevitability of imponderables in the political process. This attitude relates to their view of problem-solving.

The idea that the first step toward solving problems is to believe that there is a solution, that finding it is only a matter of time, encouraged too simple a view of political problem-solving. Perhaps a laboratory problem can be controlled and solved by a single-minded concern with a limited number of variables. The variables of political problems, however, cannot be arbitrarily isolated or limited. As many of them as are relevant must be examined, for there is no one objective solution to political questions although agreement may be easily reached on the ultimate goal. In less cosmic problems, the diversity is even greater in terms of possible solutions.

Nonetheless, scientists tried to search for the essence or the "facts" of political problems to simplify them in order to deal with them. Lack of success in so delineating the numerous imponderables of arduous political problems often led to impatience and unrealistic political proposals. Detailed analysis of hard problems was bypassed for grand designs, possible only at some future date. Scientists could well have remembered Niels Bohr's distinction between those statements of truth which are "so simple and clear that the opposite assertion obviously could not be defended" and those "statements in which the opposite also contains deep truth."[1] It was Bohr's belief that the "deep truths" had to be eliminated for clarity in scientific theories. Scientists' search for this same kind of clarity in politics demonstrated they did not clearly perceive that political problems normally contain "deep truths" which most likely can never be entirely eliminated.

Their tendency to compartmentalize analysis of political problems also demonstrated these characteristics. As early as the Moscow Conference of 1945, scientists, believing that technical means could solve political difficulties, urged the separation of the discussion of atomic energy and politics. In 1949, the Rabinowitch persuasion still believed that international control negotiations could progress on a "matter of fact basis" despite demonstrated impasses in UN efforts. At the Geneva conferences in 1955 and 1958, scientists emphasized time and

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[1]Quoted in the <u>New Yorker</u>, (December 1, 1962), 52.

again that they could bridge national boundaries and direct science toward beneficial ends. In perpetuating this belief, the civilian leadership did not contribute to an understanding of the inter-connectedness of science and politics. Eisenhower's message to the delegates at these conferences stressed the purity of science, indicating the international society of scientists could contribute significantly to peace by its work. The idea that scientists could undo the knots in international relations was misleading and added to a confusion that the increase of scientific knowledge was the same as applying that knowledge.

The belief that similar scientific findings among nations helped the cause of peace ignored the arena of greater difficulty for science and politics, which was the process of application. The revitalization of respect for the objective universality of scientific inquiry at "Geneva I and II" led scientists to believe that they applied this objectivity and rationality to their political activities. It encouraged a self-image of expertness transcending politics. This hindered a sensitive appraisal of those points at which they ceased advising on scientific fact and began advocating a choice of policy alternatives.

Warner R. Schilling has suggested that scientists' propensity to approach a problem in all of its aspects prevents them from recognizing this.[1] Nevertheless, their

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[1]"Scientists, Foreign Policy, and Politics," The American Political Science Review, LVI (June 1962), 292-93.

great specialization precludes the kind of broad view which policy questions require. A scientific problem can be reduced to a point permitting a view which is really "whole" in its quality. The boundaries of a political problem necessarily remain unclear and shifting so that a "whole" view in political not scientific terms is much more difficult to achieve.

Another factor impeding scientists from appreciating the move from informing on scientific facts to political advocacy could be a problem in communication.[1] When they communicate scientific facts relevant to a political situation, they do not use precise scientific language. It may be that, in translating scientific knowledge for the layman, scientists become less precise. It becomes easier to allow political and other considerations to color technical judgments, especially if an interest in the political problem already exists. The political officer, therefore, needs to

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[1] See Henry D. Smyth, "The Place of Science in a Free Society," BAS, 6 (June 1950), 183, in which he likens the methods of eminent research scientists to those of the artist. They are often intuitive. These scientists possess a creative instinct and temperament similar to those of an artist. However, when they communicate their findings, he points out, they must use precise facts, figures and logical thought sequences. The language is largely mathematical, and experimental processes and results can be checked and conclusions accepted.

hear more than one translation.[1] Sir Robert Watson-Watt has correctly observed that "the relative tractability of their science" may direct scientists to "regard humans and human problems as being as unambiguous as their science" or to "despair of them as substantially unpredictable, and lapse into complete indifference to 'human factors'.[2] More recently, another scientist, Leland J. Haworth, director of the National Science Foundation, observed that "the scientist, accustomed to the logic of the scientific process, is sometimes impatient with the seemingly slow, and to him, often illogical and unintelligent, actions of government officials."[3]

For these reasons, scientists brought to the political sphere a confidence which complicated their

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[1] For an elaboration of this point, see the address of McGeorge Bundy at the annual meeting of the American Association of the Advancement of Science, Philadelphia, Pennsylvania, December 27, 1962.

[2] "Emotional Fall-Out," BAS, XIV (June 1958), 216.

[3] "Scientists and Society," Physics Today, 16 (July 1963), 19-22.

political education. An impressive background of cumulative professional success gave added weight to the idea that scientific facts would dictate political solutions; it nourished the attitude that perhaps they were as qualified, if not more so, than other participants in political decision-making. However, the division of labor created splits among scientists, and the question of who is more expert among them arose to cloud their roles.[1] The belief that they were uniquely qualified to handle problems of science and politics facilitated the confusion of moral conviction with political astuteness, complicating the search for viable alternatives. Scientists also tended to believe that good intentions and examples could significantly contribute to peace.[2] Their humanistic naivete often obstructed the distinction of ideals from what was politically possible.

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[1]See Lee A. DuBridge, "Science and National Policy," BAS, 1 (May 15, 1946), 12 for his reminder, while the atomic energy bill was being debated in Congress, that although scientists generally agreed on scientific matters, this agreement often disappeared where politics and science converged. At that point, he cautioned, scientists must not pretend that they are still talking as experts.

[2]Recall e.g., the argument that the United States should not build an H-bomb to set a good example for the Soviet Union.

Scientists' political behavior was characterized by still another feature. When problems became too complicated and intransigent, scientists tended to favor clearing the decks for a new beginning, a fresh start, especially those working mainly on public opinion. They searched for certainty, rationality, and order. The tendency to equate the workings of the scientific enterprise with those of a democratic government encouraged the idea that the individual or group power was the same as governmental power. If there was rationality and order in science, why not these same elements in politics? This argument was understandable since "scientists," observed Haworth, "after seeing political compromise in action, are often apt generally to conclude that government is composed of individuals whose actions are some-times stupid or self-serving."[1] They defined rational political action in terms of their own distinctive values, believing that orderly fact-finding would result in rational decisions and orderly solutions.

Scientists' belief that a problem can be kept clean and neat pointed to "purist" tendencies. Predisposed

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[1] _Op. cit_., p. 22.

toward indisputable assertions, they were reluctant to enter the political sphere as anything but scientific experts and adhered to the idea that government is best influenced by keeping independent of it. Yet scientists acted as more than scientific experts. The character of their political participation was significantly determined not only by the foregoing characteristics and attitudes but by their roles.

Scientists in advisory positions had a greater sense for formal channels through which advice proceeds than university research scientists or those trying to cultivate a particular public opinion. Advisory responsibilities, long experience in actual decision-making at high policy levels, and ready access to high political officers encouraged these scientists to work through the established structure. In a sense they could "afford" to do so since they could always be sure of a hearing.

Scientists with technical research responsibilities and an interest in related political problems ignored formal channels. Certain that they were not being correctly represented, they took their views to high policy officials. For example, in 1945 project scientists sought access to the President and his cabinet officers. In 1949, the

Berkeley scientists did not attempt to reach a consensus with their GAC colleagues. Both project scientists and the Berkeley contingent of university scientists did not trust the judgments of advisory scientists. In 1945, however, Conant and Bush, representing advisory scientists, tried to get a hearing for project scientists not only on the matter of postwar atomic energy research but also on the international question. In 1949, the GAC scientists did not appear to want the opinion of scientists who proposed a fundamentally different response to the Soviet atomic bomb; even if there had not been a classification problem. This could be explained by the fact that the 1945 atomic energy debate concerned primarily science policy; thus all scientists were competent to participate in decision-making. In 1949-50 the decision was mainly a political one. Here the GAC believed itself to be more expert.

In short, the wartime advisory experience of some scientists discouraged the organization of consent. In fact, they were the sole representatives of the scientific community, especially since the wartime situation did not permit an enlargement of the discussion. At that time the policy group was small enough for maintaining clear divisions of labor. Advisory scientists were not in the habit of consulting with their colleagues outside of official circles. Rather, they rendered scientific and political judgments without such an interchange. Their self-image as "the experts" left little room for other scientific experts to participate through formal channels.

The GAC scientists were unhappy when their dissenting colleagues succeeded in reaching receptive ears. Conant's complaint, when the decision went against the GAC, that those with the loudest voice got heard manifested this dissatisfaction. He then broke out of formal channels to join the public discussion. The Berkeley scientists also ignored formal channels when they bypassed the GAC and appealed to political officials directly.[1] But they did not need to go further since they won their point.

Thus it appeared that scientists were willing to try something else if one thing did not work. Their failure to find a consensus among themselves encouraged the vice of

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[1]This is not to say that the GAC provided the formal channel for scientific advice for all scientists, but it was a natural mechanism that scientists who were not members of the GAC might well have used had they agreed with the GAC position. The fact that they bypassed the GAC was clear indication that they did not trust GAC scientists to convey their viewpoints to top officials accurately.

immediate access to political officers and hindered the co-ordinated consideration of a political problem.[1] They saw themselves as a free-wheeling group, an image which was tempered by actual official responsibilities. Although they did not ostensibly seek political power, scientists did think they could offer the kind of "objective" advice needed for getting the "right" answers. Their technical and political advice was sought with two results: the political officer wrongly ignored the limitations of scientific experts, and scientific experts began to believe that they were capable of advising on essentially political problems. Their political participation is inevitably affected by how others define their governmental role. In a way, political officials saw scientists as problem-solvers, giving impetus to their "whole problem" approach in areas where they were not "wholly" expert.

In summary, a number of factors helped shape the behavior of scientists. The built-in restraints on advisory scientists made them conscious of what might constitute appropriate behavior in the policy process. Nonetheless, an in-

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[1] On Alfred Vagts' concept of the vice of immediacy, see William T. R. Fox, "Civilians, Soldiers, and American Military Policy," World Politics, VII (April 1955), 409-10.

creasing sense of dissatisfaction with the substantive content of policy decisions led to some lessening of these restraints and encouraged a call for public discussion. How much to widen this discussion depended on the degree of satisfaction that scientists received within the narrower circles of decision-making. It also depended on the need to mitigate the heavy burden which political decisions placed on scientific values and to share responsibility for developing greater weapons.

Another factor which added to the evolution of scientists' political roles was a more thorough understanding of the political process. A heightened sense of the elements leading to decisions has enabled some of them to play the political game better than others. Finally, their political behavior was shaped by the idea that a situation could be righted if only more people knew what was wrong. This last, in great part, influenced scientists working primarily on public opinion.

Thus, scientists' rate of progress in the political environment from 1945 to 1958 had all the characteristics of growing pains. It became apparent that new roles were easier to acquire than attitudes and habits appropriate to these new functions. Scientists with official responsibility tended to learn sooner about the workings of the political process than those on the edge of political effectiveness. Their roles determined a different kind of responsible accountability and forced the process of accommodation between scientists' professional and official governmental responsibilities. Indeed, the pervasive impact of science and technology on national and international politics continually forces a redefinition of responsibilities and development of new skills by scientists in political decision-making.

As this process continues, scientists may more readily understand that their scientific advice usually has to be modified to constitute sound policy advice, for circumstances may well dictate an altering of their special, and therefore limited, view. The irrevocable meshing of science and politics ought to make them increasingly knowledgeable about the polity, viewing themselves as integral parts of it, subject to its system of rewards and deprivations. It also is provoking the design of new structures by which scientific advice can be fed into the policy process and by which scientists may possibly learn better how to operate in the political process. How do the characteristics of these new institutions affect scientists' political development? For example, what are the assets and liabilities of part-time scientific advisory committees and full-time governmental service by scientists? What are the qualifications for a scientist-administrator or adviser? Are they the same as those required for scientific excellence?

The crucial interplay of science, technology, and politics also dictates that political leaders take as accurate a measure as possible of their expert advisers in political decision-making. How can decision-makers better understand the potentialities and limitations of science and scientists? What must they know to make a judgment about these in the political process? What kind of institutional mechanisms are needed for evaluating scientific advice and therefore scientific advisers?

The ability of political leaders to use scientific advisers to help establish and advance policy objectives may help advance the political education of scientists. Although it is their special task to develop the role of controversy for a reasonably effective discussion of public issues and to allow scientists to bring their creative imagination into play on a large and loosely-defined problem, they must not equate scientists' special competence with general competence.[1] Nor must scientists make this mistake.

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[1]On this point see James A. Perkins, "Science, Technology, and National Security," a statement before the Senate Subcommittee on National Policy Machinery on April 25, 1960, The Congressional Record - Appendix (April 29, 1960), A3666.

In short, the bridging function between science and politics should be the joint responsibility of scientific experts and their political superiors. The receiver and giver of advice each should maintain an acute awareness of his special function in order to reach for a concert of judgment. Failure to do this can mean that consequences of approved policies are not fully understood and anticipated.

Clearly, the dynamic and shifting mixture of science and politics has created an insistent need for the intelligent use of human talents and knowledge for designing foreign policies. Science and technology can contribute in many ways to an effective foreign policy, but the direction of these contributions will hinge importantly on purposes. The establishment of purposes in a complex national and international environment requires the cooperation of many kinds of statesmen whose special competence might be a vital ingredient. This would certainly include scientists whose knowledge is a powerful tool of government, with important social and political effects. Their responsible contribution in applying the results and methods of science to sound foreign policymaking depends on their ability to help statesmen set purposes. The impetus for developing procedures and perspectives for better use of scientific knowledge and experts by political officials has to come from scientific and political leadership.

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