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ever, that there is need for study of the effects of automated equipment upon workers in the offices and plants in which it is being introduced. It is only through knowledge acquired through such research that the necessary individual and organizational adjustments to technological change can be facilitated.

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THE ROLE OF THE SCIENTIFIC ELITE IN THE DECISION TO USE THE ATOMIC BOMB

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In his recent book, *The Power Elite*, C. Wright Mills makes important use of the concept "pivotal moment"—the moment at which important and far-reaching decisions are made by the small in-group of the power elite, institutional leaders of the military, corporations, and the state. (8) If Mills wants to clarify the governmental decision-making process by this concept, it seems to us that he succeeds only in oversimplifying the process to the point of serious distortion. Political decisions have histories: the history of any given decision is a series of prior decisions, which, depending on the matter dealt with, are often made by individuals entirely outside of the institutional network Mills considers. It may well be that many significant decisions *concern* only the big corporations, the state, and the army. To rule out consideration of other groups, institutionalized or not, that affect the political order, on such an implicit, quasi-statistical basis, however, does no service to the conceptual problems confronting the developing field of political sociology.

This point can be made clear by an analysis of one of Mill's own illustrations of a pivotal moment—the decision to drop the atomic bomb on Hiroshima. The implication of Mills' cursory, dramatic treatment of this decision is that the decision was self-contained. Once the resource—the atomic bomb—became available, the decision about whether and when to drop it was *the* significant political decision, and one in which only members of the power elite participated. An examination of the history of this decision, however, reveals a complex series of prior, consciously political decisions, made by men who were by no means members of Mills' power elite. These decisions relate not only to the final use of the bomb, but also to the early mobilization of the primary resource, the creative capacities of physicists, and their organization to produce the bomb.

There was a series of "pivotal moments," at any one of which the final availability of the resource was placed in serious jeopardy. Analysis of this case and comparison of it with the development of nuclear physics in other countries could also provide a rich source of hypotheses about the ways in which the social composition and organization of the "scientific community," one of many non-elite political groups, can affect high-level national policy-making, although a systematic attempt to make this comparison will not be attempted here.

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At the beginning, the fact of fission—or rather the special case of fission in the uranium nucleus—was known only by a very small group of scientists. Fission itself had been accomplished in 1935 by Otto Hahn and Fritz Strassmann. The fission products, however, could not be identified. When barium was definitely identified, only those persons closely following nuclear physics were able to make the critical necessary inference: i.e., that matter can artificially be converted into energy. These few nuclear physicists were working in several countries during the late '30's.

Hahn and Strassman, working at the Kaiser Wilhelm Institute in Berlin, announced the presence of barium in January of 1939. Otto Frisch passed the news on to his aunt, Lise Meitner, then in Stockholm. Almost immediately she interpreted the results correctly, as experimental verification of the Einstein matter-energy formula. For a time, she alone understood the theoretical significance of what had happened, and we can pinpoint the information at the beginning of this chronicle to a single physicist. Frisch immediately rushed to catch Niels Bohr, the Danish physicist, before he left on a lecture tour in America and succeeded in telling him, in rough outline, their conclusion. (4, p. 19) For a while, then, only these three people knew the correct implications of the Hahn and Strassmann observations. Niels Bohr was forced to spend a week on a transatlantic liner unable to pass on the single most important conclusion of modern physics. Once in the United States, Bohr lost no time. He arrived, greatly excited, at the physics laboratories at Columbia University. Enrico Fermi, then a refugee of four years' standing, happened to be away that day, and Bohr, "trembling with excitement," could talk only with Fermi's student, a certain Herbert Anderson, who immediately suggested verification on the Columbia cyclotron for his Ph.D. dissertation.* Four separate experiments, all of them in academic physics laboratories, soon established Meitner's conclusions. By February of 1939, confirmations were published by small groups at Columbia, Johns Hopkins, Carnegie Institute of Washington, and the University of California. (9, p. 25)

The implications of this discovery, and most especially of the heavy pulse of ionization that would accompany fission of the uranium nucleus, were recognized almost at once by American, British, and German scientists. In the United States, a small suppertable group at Columbia (including Fermi, V. F. Weisskopf, Leo Szilard, Eugene Wigner, and Edward Teller —the latter three all recent Hungarian refugees) decided within three months of the confirmation that the "pulse" was of great military importance. The quick release of a large amount of energy was interpreted as having important potentialities for the development of a destructive weapon. Credit is ordinarily given to Fermi and Szilard, but at this date it is impossible to determine from whom the impetus came. Nevertheless Fermi made an immediate attempt to

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*Personal interview with Herbert Anderson, Chicago, 1958.

enlist the help of the U.S. Navy and was gently turned down. (6) Szilard refused to be discouraged and suggested that Albert Einstein might have enough influence to reach the President directly. Still not trusting Einstein's prestige, Szilard suggested that a close friend of both the President and Einstein be used. He was Alexander Sachs, a New York economist and investment banker, and on October 11, 1939, the now-famous Einstein letter was delivered. (9, p. 47)

Roosevelt was impressed, and immediately appointed a three-man "Uranium Committee," with representatives from the Army, the Navy and a physicist from the Bureau of Standards. A small immediate grant was made to buy uranium for the group. The Committee made a favorable report and in June of the following year was itself merged into the National Defense Research Committee with $300,000 eventually spent for a small exploratory program. In the meantime, the group at Columbia decided that the military possibilities of fission should be kept out of the scientific journals, which, during 1939, had disseminated considerable information about the research to the scientific community.* Accordingly, in the spring of 1939 the Columbia group enlisted the support of Bohr in stopping, voluntarily, all research publication on the subject. Only one physicist disagreed, Joliot of France, and publication continued until April of 1940 when he concurred. In that month the "Reference Committee," composed of nuclear physicists, became part of a larger scientific organization, the National Research Council, and the editors of scientific journals agreed to send all papers of possible military interest to the Committee for review. This arrangement worked well and actually continued until June of 1945. (9, pp. 45-46)

Scientific communication was thereby confined—by consensus of the group—to the physicists working on this kind of project and the relevant governmental committee. These physicists were then at Columbia, the University of California, Johns Hopkins, the Bureau of Standards and a few other academic research centers. Full knowledge of the direction and possibilities of the research, however, was confined to about 20 scientists.** As the research expanded, 16 organizations subcontracted for specific aspects, without being admitted to formal knowledge of the military import of the research.

During the next few years, research on the projects continued without the raising of important political issues. Until the spring of 1945 the atomic bomb remained only a possibility; it was far from certain that such a weapon could be produced. Between 1941 and 1945, then, the physicists were working. An examination of communication patterns established during this period (both within and between projects) casts considerable light on the general nature of the social relationships existing in the group. These relationships are of interest in part because they reflect, on a grand scale, the kind of communication pattern that maximizes productivity by permitting criticism and dissent (and were thus, perhaps, a precondition for the rapid development of the bomb). Of most direct interest to us here, however, is their relevance for the attaining of political consensus within the scientific community.

The basic mechanism of communi-

*During 1939 nearly 100 papers had been published. (9, pp. 25-6)

**Actually a year later, May, 1941. (9, p. 51) cation in academic research groups is the colloquium, or seminar. Here a researcher presents his evidence and conclusions for the information and criticism of his colleagues, and in time his work is printed in accessible form and/or verbally carried to other meetings and other lectures. The "machinery of advice" set up in 1941 and 1942 to advise the government of important scientific conclusions followed this pattern closely. At the top of this formal machinery sat a Top Policy Group, consisting of the Vice-President of the United States, the Secretary of War, the Chief of the Army Ground Forces, Dr. Vannevar Bush, director of the Office of Scientific Research and Development, and Dr. James Conant. Bush acted as liaison with the S-1 Section of this Office, which was directed by Conant and included 13 scientists, each of them working directly on the "uranium problem." (9, p. 76) Contracts were let directly by the Section. (These contracts were actually research funds for small, informally organized research teams in universities.)

The S-1 Section seldom met formally, the members preferring to discuss their work informally with Conant (Bush's representative) and with Dr. Briggs, the chairman. (9, pp. 77, 81) By May of 1942 this arrangement had clearly become unwieldy, and a new committee, much larger than the first, complete with Planning Board and Executive Committee, was set up, with Conant as chairman, including five of the original atomic scientists. (9, p. 81) Either directly or through Dr. Conant's membership in a series of scientific policy groups, the recommendations of this Executive Committee went to the Top Policy Group. The task of the men on this committee was research; as problems of policy came up they were bucked upstairs. Questions of production and procurement went to the Planning Board, to a special "district" of the Corp of Engineers.

Results and suggestions were sometimes discussed by the body of the Committee; sometimes they were simply given to Conant, Briggs, or Bush. While decisions were supposedly to be made by majority vote, the Committee seldom voted and while its decisions were not binding on any of the member scientists, they were usually followed. The Committee has never been dissolved; in time it ceased to have any functions and was forgotten. (9, p. 84)

In effect, while formal channels existed, the working organization and medium of communication was still the informal meeting. The scientist-members of the Executive Committee, nearly all Program Chiefs, continued to work loosely inside channels. Bush, Conant, and Compton quite casually decided to investigate plutonium as an "afterthought" during a luncheon after a meeting of the S-1 Section (the early committee). (4, p. 71) The vital early work at Columbia and Princeton was arranged informally during the Christmas holidays, Compton simply hearing from his colleagues what ought to be done and then asking them to do it—again outside the S-1 Section. The decision to concentrate plutonium production at Chicago was made in Compton's bedroom with the advice of men not even on the Committee. (9, p. 80) Even when the Chicago "Metallurgical Laboratory" had seventy separate research groups with a staff of 5,000 workers, Compton (their director) acted on the premise that his real authority was professional, that the workers followed his instructions because the executives under his direction respected him and knew about his research work. (9, p. 84) Norman Hilberry, his administrative director, was a former student, and therefore reasonably certain to work with Compton in harmony. (9, p. 85) The closer associates were all known to Compton personally. (9, pp. 84-85) Reports to the Committee were discussed with a "laboratory council" before they were sent out. Consensus was so highly valued—and deemed so necessary to the maintenance of working relations—that when Dr. Harold Urey wished to concentrate all effort on an alternative plutonium-producing method, even after the Committee's decision had been taken, Compton embarked on a long "discussion of the points" until Urey was satisfied. (9, p. 99) When Compton was forced to push a decision through the laboratory council, he was quite disturbed. He cited the Biblical story of Gideon and invited those who could not agree with the decision to leave the project without prejudice because "a large group divided" could not accomplish as much as a "small group of united, earnest men." (9, pp. 109-110) To him, consensus on these internal political matters was of prime importance. (This community crisis, incidentally, was simply the question of whether production aspects of the giant atomic bomb production should be given to private industrial firms.)

To recapitulate, during the years between 1941 and 1945 there was an enormous expansion in the number of people working on the research projects, with continued advances both in basic knowledge and techniques. The basic means of communication between (and apparently within) projects continued to be a modification of the academic seminar. Typically a situation that maximizes dissensus, because it is seldom connected with collective action, the seminar-type meeting served a dual function for the nuclear physicists. It both permitted the kind of expression of dissensus that is essential to the advance of knowledge—free flow of communication—and gradually became a sort of "town-meeting" situation, to be used to gain consensus whenever policy issues did arise. The persistence of this pattern of communication among the physicists was facilitated not only by habit and preference but also because secrecy requirements of the research inhibited written communication.

By contrast, the German nuclear physicists' failure to make the essential basic advances in scientific knowledge despite an equal amount of scientific "talent" and a "head start" can be seen largely as the effect of a failure in communication engendered in large part by the Nazi government. Disruption of academic communities by Nazi purges probably enhanced the cohesiveness of existing cliques among the physicists, which was further enhanced by differential support of the cliques by a government insensitive to scientific canons. It is probable that the general insecurity engendered by governmental policies not only strengthened barriers to communication between cliques but also reinforced the basic authoritarian structure of the German cliques, further restricting communication and the expression of the kind of dissensus so essential to scientific advance. (7)

Among the American scientists the political potentialities of their kind of habitual social relationship became evident when they had achieved success in their research and were faced with a significant political issue. From the "town meeting" the scientists evolved political factions, which agitated for a time, and which finally resolved their differences by resorting to the vote. This issue was whether the bomb should be dropped on the Japanese.

The President had appointed an Interim Committee to advise him on "the various questions raised by the imminent readiness of an atomic bomb." On May 31, 1945, Secretary of War Stimson put the question directly to the Committee: "Gentlemen, it is our responsibility to recommend action that may turn the course of civilization." (4, p. 219) The membership was Secretary Stimson, a special consultant to Stimson, a representative of the President, the Under-Secretary of the Navy, the Assistant Secretary of State, and three scientists. A Scientific Panel of four more scientists was established to advise the Committee. In brief, the committee was advised by the scientists in unanimity that dropping the bomb on a Japanese city was advisable; the Committee in unanimity so advised the President, and after suitable consultation with other heads of states, the deed was done. Consensus among the scientists leading to the decision of the Scientific Panel, however, was not easy to obtain.

The Interim Committee itself had been organized by suggestion from the scientists. Within the Metallurgical Project at the University of Chicago, a subcommittee on social and political consequences had long been in action. It was headed by James Franck, a distinguished scientist. A. H. Compton told General Groves about the work of the Franck group, and Groves suggested the Interim Committee to Stimson. On Stimson's advice the Committee was organized. For the scientists the moral problem became acute when it became obvious that Germany would not stay in the war much longer. "Volney Wilson, now doubly troubled in mind, came to me in the earnest hope that we might avoid atomic attack on Japan. His reason was the straightforward one of Christian compassion. Could not some way be found to bring the war to a quick close without the ghastly destruction that we knew the bomb would cause?" (4, p. 223) Knowing of the report and of the imminence of the decision, the Franck subcommittee finished its report, which urged demonstration, not actual use of the bomb. Compton, on Franck's request, took the report to Stimson, who immediately presented it for study to the Interim Committee. The Committee considered it for two weeks and then answered the report in definite terms: "We can propose no technical demonstration likely to bring an end to the war; we see no acceptable alternative to direct military use."*

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*Full text is given by Stimson. (10) The Committee's reasons were those governing the use of any untried weapon with limited availability (only two bombs were available). First, the device might not go off and the demonstration would be ridiculous. Second, it *would* go off but only strengthen the hand of a determined military party, who could easily concentrate the remaining Japanese aircraft against high-level bombing attacks. Third, the Committee hoped there would be no more wars and that its use would prevent war. Fourth, the projected American casualties for the November invasion were staggeringly high. It should be noted that General George Marshall, Chief of Staff for the Army, initially opposed the use of the bomb because its secret possession would greatly enhance the future military position of the United States—in short, he disapproved of early disclosure of the weapon. The second bomb was dropped as the climax of a campaign by radio and leaflet to persuade the Japanese people to accept the terms of a settlement worked out at Potsdam. The leaflets warned of dire consequences unless the Emperor ended the war; obviously the second bomb would have to be dropped. Standard sources on the Japanese surrender should be consulted.

But the struggle for a clear-cut decision continued. One majority opinion had been rejected by the Scientific Panel; another was on the way. Leo Szilard wrote directly to the President opposing the use of the bomb on the Japanese. (He had approved of its use against Germans.) In addition, Dr. Szilard circulated a series of petitions among the laboratory groups at Los Alamos and Oak Ridge calling for "outlawing" of the bomb altogether. He got 67 signatures and turned it over to Compton for transmission. Two other petitions circulated at the same time: one urged unrestrained use of the bomb against the Japanese and the other asked for use, but with certain minor restrictions. "It was difficult," Compton says, "to get a balanced view of how our men were thinking." (4, p. 243) Accordingly, at General Groves' suggestion, Compton set up an elaborate opinion poll for the "leading scientists" at Chicago, Los Alamos, and Berkeley. The results of this poll clearly showed that this group favored military use—at least if, after other means were tried, this was found necessary to bring surrender.*

The results of this poll were ready before the Scientific Panel when the Interim Committee recommended use of the bomb. Thus they could reject a minority opinion of the Franck and Szilard variety with assurance that they were expressing a majority opinion. In effect, by the second week in June, 1945, the scientists knew that "they" wanted the bomb to be dropped. (Curiously, the executives—both military and civil—who still held the residue of the decision had serious qualms and referred back again to the scientists at least twice more for confirmation of the mood of the scientific community.) President Truman informed the allies at Potsdam of the bomb's availability and then, back in Washington, asked to see the results of the Compton poll. Compton delivered it to Truman, and then, to his amazement, was asked for his personal opinion. He gave it as follows: "My vote is with the majority. It seems to me that as the war stands the bomb should be used, but no more drastically than needed to bring surrender." (4, p. 247)**

There were no more inquiries from Washington and two weeks later the bomb was dropped.

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If we regard a significant political decision as one which determines the use of major resources in terms of a set of values which relate to important aspects of national welfare, we can see that the scientists made one crucial major political decision by themselves—the decision to exploit an esoteric scientific discovery and their own talents to produce a powerful destructive weapon. The relation of the decision to the social backgrounds of the scientists—predominantly recent refugees—is obvious. A more significant point for our purposes is that this group could (as "normal" members of the "power elite" could *not*) recognize the existence of a resource of national value. From this early decision until 1945, the government served almost exclusively to facilitate the development of this resource. The significance of the particular way it functioned vis-a-vis the national scientific community can be seen clearly in the light of the experience of the German physicists. When the resource had been developed, it became a different resource—the atomic bomb.

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*Text given in Daniels and Compton. (5)

**There is an extensive literature on the rationale of the military and diplomatic decisions made on the Japanese war. (1, 2, 3, 11)

If our government were fully rationalized, the disposition of this new weapon should properly have become the exclusive concern of the military. If Mills' general hypothesis about the ubiquity of the power elite were valid, members of the power elite alone would have participated in decisions about its disposition in a "pivotal moment." In fact, scientists were called upon not only to assist in technical decisions, such as the topographical features which would maximize the bomb's effect, but in the moral issue of whether or not to use the bomb against human beings. (That this decision related to strategic issues — details of the scheduling of the war—adds to the weight of the scientists in this decision.) In effect, the scientists in this case seemed to represent an especially well-informed segment of the population, and their opinions were used as such by the political and military officials. This instance is one of many in which only the professionals—scientists or others —working on the development of a resource are capable of or privy to the understanding of the implications of the use of that resource. In the case of the dropping of the atomic bomb, the scientific community functioned much as the legislature would (although, of course, the scientists were not elected) in a less urgent, less secret, and less technical matter. Since 1945, the discoveries and opinions of scientists have continued to play a role in national policy-making.

In conclusion, this close examination of one of Mills' illustrations of a pivotal moment reveals not only the inadequacy of the concept of pivotal moment as either an empirical or theoretical concept, but also casts serious doubt on the utility of the concept of the power elite in the understanding of complex national policy-making. The addition of the "scientific community" or one of its sub-communities to the power elite compels consideration of the nature of the political issues as such—a task not achieved by Mills. Simplicity is not the only criterion of the adequacy of a theoretical orientation; it is our opinion that in this case the simplicity of Mills' constructs is an indication of grave theoretical weakness.

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