Chapter VI

The Search for Technical Agreement

I

Some Initial Compromises

President Eisenhower's letter, or more properly the formulation of a new Western negotiating position and its communication to the Soviet Union, touched off a process best described as a search between East and West for agreement on the technical issues in dispute, which lasted throughout 1959, finally ending in seeming futility. A variety of issues were at stake: whether or not the test ban treaty would be comprehensive; the extent to which the new data and the changing technological base would be taken into account; eventually, the fate of the moratorium on testing; and, to some extent, the diffusion of nuclear capabilities. President Eisenhower's letter and Ambassador Wadsworth's presentation on April 13 raised the first two issues. Given the public debate on these matters in the United States, the moratorium was obviously an important element in the background, and since one of the putative objectives of the Conference was prevention of the spread of nuclear weapons capability, this issue was inevitably involved.

The USSR Protests NATO's Nuclear Plans

The first Soviet move after the communication of the new Western position concerned the last problem. On April 21, 1959, the Soviet Foreign Ministry delivered a note to the American Embassy in Moscow protesting against plans, reported in the Western press, for the provision of missiles to NATO countries and the stockpiling of nuclear warheads there. As was described earlier, in December 1957 the NATO Council had decided that selected NATO countries were to be furnished missiles to which nuclear

warheads might be attached. Then the Atomic Energy Act had been amended in July 1958 to allow the transfer of nuclear warheads under certain specified conditions. These warheads were to remain in American custody until the President had decided that they could be used.

Under these terms an agreement was signed with the United Kingdom in 1958, and work was begun on the installation of bases in that country for Thor intermediate range rockets. A similar agreement was reached with Italy in March 1959, for the installation of Jupiter rockets, although in this case both the missiles and the warheads would remain under American control. The Western press was full of speculation that the NATO Command also had plans to establish similar bases in other NATO countries.

The Soviet note singled out Greece, Turkey, and the Federal Republic of Germany, and warned against the establishment of bases in these countries, especially in West Germany. It argued that no action should be taken in this sphere prior to the Foreign Ministers’ meeting, since to do so would constrict and perhaps foreclose the possibility of agreement.

The West replied through a statement of the NATO Council issued on May 7, 1959, and a note from the American Embassy in Moscow to the Soviet Foreign Ministry dated the following day. The essence of the Western position was to reiterate the public rationale of the December 1957 decisions; that is, that the USSR had prevented disarmament, and at the same time had introduced modern weapons into its armed forces, therefore, the West had no alternative but to take similar action.

On May 5 and 6, bilateral agreements were signed by the United States with Greece, the Federal Republic of Germany, the Netherlands, and Turkey under the provisions of the Atomic Energy Act of 1958 for the transfer to the latter countries of the non-nuclear components of nuclear weapons systems and knowledge about their use. A similar agreement was signed with Canada on May 22, 1959. At that time it was envisaged that intermediate range ballistic bases might be established in all of the countries

2Ibid., pp. 1405-9.
3See U.S. Department of State, Treaties and Other International Acts, Nos. 4292, 4267, 4277, and 4278 (1959).
4Ibid., No. 4271.
except West Germany, which according to the NATO plans then in effect was only slated to receive Matador rockets with a range of approximately six hundred miles. Eventually, only Turkey received IRBM's, under an arrangement agreed to in October 1959. Nonetheless, the May agreements established the legal framework for the transfer of nuclear weapon systems within the limitations of the 1958 Atomic Energy Act.

To a certain extent the signing of these agreements at this time merely represented the elaboration of previously agreed decisions. It may also have been an attempt by those who had strong feelings in the matter, especially the United States Department of Defense, the NATO Command, and policy-makers in the Federal Republic of Germany, to make it difficult for the Foreign Ministers of the four powers to adopt a plan for nuclear or general disengagement in Central Europe at their forthcoming meeting. These issues were relevant to the Geneva Conference since they dealt with the diffusion of nuclear weapons, one of the issues supposedly at stake in the Conference.

Chairman Khrushchev's Formal Reply: "No" to Eisenhower, "Yes" to Macmillan

The first direct Soviet reply to the new Western position came on April 23, 1959, in letters from Chairman Khrushchev to President Eisenhower and Prime Minister Macmillan. First, Khrushchev rejected the possibility of an atmospheric test ban. He argued that the aim of a test ban should be "to halt the nuclear arms race, or at the very least, to prevent the creation of new and ever more destructive types of atomic and hydrogen weapons." Since an atmospheric ban would allow testing in other environments, in his view, it would not achieve this goal. Although they evaluated it differently, in the United States, the opponents of a test ban treaty also clearly recognized this possibility. Khrushchev further asserted that nuclear weapons tests in the high atmosphere would cause fallout. He claimed that the purpose of the Conference should continue to be "to conclude an agreement providing for a cessation of all forms of nuclear weapon tests—in the atmosphere, underground, under water, and at high altitudes."

Khrushchev then suggested that it might be possible to surmount the difficulties in which the test ban negotiations were currently enmeshed by adopting Prime Minister Macmillan's proposal for an annual quota of on-site inspections. He elaborated that not many on-site inspections would be required and that they would have to be "founded . . . on objective instrument readings." It was his view that the mere possibility that an on-site inspection would be conducted would be sufficient to deter a potential violator.

Four days later, in the Geneva Conference, Mr. Tsarapkin submitted a formal proposal embodying Chairman Khrushchev's proposal, and in the next few sessions he elaborated its meaning and ramifications. He stated that if the instruments registered a suspicious event in a given location according to predetermined criteria, the opposite side could request an on-site inspection and an inspection group would be dispatched: there would be no question of voting in the Control Commission. Thus he seemed to say that the Soviet demand for a veto on on-site inspections would be dropped, if the quota proposal were accepted. Mr. Tsarapkin also stated that the size of the quota—which would have to be a "small number"—for the territories of each of the three nuclear powers would have to be fixed by agreement among them. He ruled out the possibility of fixing the quota as a set percentage of the number of unidentified events registered on seismographs. Or, as he put it to his partners in the Conference, agreement will be impossible "if you are going to talk of thousands of earth tremors a year and fix a certain percentage." Later, he asserted that it would not be necessary to consider the new seismic data prior to the adoption of the quota proposal.

Toward More Scientific Data: Projects Vela and Cowboy

Meanwhile, the United States moved to consider more seriously than it previously had the technical problems involved in detecting and identifying nuclear explosions. On April 23, 1959, the same day that Chairman Khrushchev dispatched his letters to President

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6GEN/DNT/PV. 83, pp. 3-11.
7GEN/DNT/PV. 84, pp. 10-11; PV. 85, p. 4.
8GEN/DNT/PV. 84, p. 8.
9GEN/DNT/PV. 85, p. 6.
Eisenhower and Prime Minister Macmillan, Dr. Killian, Special Assistant to the President for Science and Technology, Chairman McCone of the AEC, and Deputy Secretary of Defense Quarles met to discuss the reports of the Panofsky Panel, which had considered problems of high altitude detection, and the Berkner Panel, which had examined problems of seismic detection. They decided that the Department of Defense should assume overall responsibility for implementing the research programs recommended by these two panels and that it should receive support from the Atomic Energy Commission and the National Aeronautics and Space Administration.

In the next few months various advisory groups within the Department of Defense began a number of preliminary studies, and then the Air Force Technical Applications Center prepared specific proposals for research. In August various supervisory committees were established. Finally, on September 2, 1959, the Secretary of Defense assigned responsibility for overseeing the research—subsequently named Project Vela—to the Advanced Research Projects Agency of the Department of Defense, and the first work order was issued one month later.

During the next two years the project was expanded to full dimensions. It was divided into three aspects: Vela Uniform, dealing with underground nuclear tests; Vela Sierra, relating to monitoring nuclear tests in space from ground bases; and Vela Hotel, concerning monitoring nuclear tests in space by satellite-borne instrumentation. Various governmental and nongovernmental organizations were involved under contracts let in 1960 and 1961, and $10,000,000 was made available for the project during fiscal year 1960. From the outset it was envisaged that both conventional and nuclear explosions would be involved, and because of this the AEC was given a prominent role. Nuclear explosions were obviously desirable from a technical standpoint: without them, theories relating to detection could not be completely tested, and since it would be some time before the project would advance to the stage in which nuclear explosions would be crucial, the planners of Vela

simply ignored the question of whether or not such explosions would be politically feasible.

Curiously, the first public announcement of Project Vela was made May 7, 1960, well after the program was underway.\textsuperscript{11}

A second project, concerning decoupling, was inaugurated somewhat earlier. As soon as they were presented, Albert Latter’s theoretical calculations invited empirical testing. The Atomic Energy Commission agreed to fund such work and assigned operational responsibility to the University of California’s Lawrence Radiation Laboratory at Livermore. The RAND Corporation would also participate in the analysis of the data. These arrangements were discussed before Congressional Committees in June 1959. A series of ten or more underground chemical explosions was planned under the code name Project Cowboy and the first of these was conducted in a salt mine in Winnfield, Louisiana, on December 17, 1959, before Project Vela was even seriously under way. Thus the United States moved faster to examine through empirical tests whether or not the seismic effects of underground nuclear explosions could be muffled than it did to consider in the same way whether or not the effectiveness of the control system recommended by the Conference of Experts could be improved.

An important reason for the difference in the speed with which the two projects got underway was the fact that several policymakers felt that if the decoupling theory proved valid a ban on testing nuclear weapons underground would probably be out of the question. In addition, Project Vela was much more complicated than Project Cowboy and required considerably larger expenditures of funds. Further, most aspects of Project Vela would be executed by nongovernmental agencies, often universities. Drafting and letting contracts is time consuming, and the rhythm of academic life seldom allows the rapid commitment of large scale resources to meet external requirements. Finally, housing Project Vela within the Department of Defense meant that the responsibility for proving that a test ban could be adequately monitored was given to an agency which was less than enthusiastic about the wisdom of a test ban and which had as its basic responsibility the overwhelming

and pressing task of developing and maintaining the military capacity of the United States. When priorities were established, as they inevitably were, the latter fact was especially important.

Many Senators and scientists complained about the slowness with which Project Vela was implemented. Even Earl H. Voss, who was not among the most enthusiastic supporters of the test ban negotiations, commented in his book *Nuclear Ambush* that the project "obviously was not being pushed full speed ahead."

*Divided Counsel: A "Political" or "Scientific" Question?*

Regardless of the West's state of technical preparedness, negotiations were in progress and Chairman Khrushchev's missives called for a reply. Among other things, the USSR appeared to be gaining propaganda benefits from the seeming shift in the Western position from advocacy of a comprehensive test ban to support for an atmospheric ban, a point stressed in Chairman Khrushchev's letters. In an attempt to counter this, the White House issued a statement on April 27, 1959, asserting that the United States continued to desire a complete test ban, but the Soviet Union thus far had "been unwilling to accept the control which would make such agreement possible." If the statement had any effect at all, however, it altered the situation only slightly.

Thus on the one hand there was continued pressure by public opinion for a comprehensive test ban. Opposed to this were the arguments that such a ban could not be enforced. These contradictory pressures clashed in the formulation of the next Western move. Chairman Khrushchev's proposal for a predetermined quota of on-site inspections caused deep divisions within the United States Administration. The Atomic Energy Commission, and especially its Chairman, John A. McCone, strongly opposed the concept, arguing that a test ban including such a provision would not provide adequate safeguards for American security. The Department of State, on the other hand, gave the proposal a qualified endorsement. In addition, the British favored it; indeed, they in a sense had originated it.

In the Committee of Principals, Mr. McCone took the attitude

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that this issue was "technical," not "political," and therefore the judgment of the technically qualified experts ought to prevail. The Department of State, on the other hand, took the opposite view. In reality, both technical and political judgments were intertwined. The crucial issues involved the size of detonation which one wished to detect and the degree of assurance which one sought that such detonations were detected; in other words the old threshold problem, which had never been resolved. Such issues were partly "technical" in that they involved estimates of the capability of the proposed control network, but they were also political, in that they involved judgments about the level of acceptable risk, and weighing the risks of a less than perfect test ban against the risks of a world without a test ban. Varying estimates of the trustworthiness of the Soviet Union also entered the picture. As in the past, these issues were not settled, but a new Western position was formulated which was more or less acceptable to all of the parties involved.

The Western Response to Khrushchev: Further on the Inspection Quota

The formal Western response was expressed in letters from President Eisenhower and Prime Minister Macmillan to Chairman Khrushchev dated May 5, 1959.14 The letters narrowed the differences between East and West to a slight degree. Both Western leaders agreed to "explore," through their representatives in Geneva, the Soviet proposal for an annual quota of on-site inspections on the territories of the three nuclear powers. In particular, the Western leaders argued that questions relating to the criteria for initiating on-site inspections, timely access to areas where unidentified events occurred, and the relationship between the detection capability of the control system and the size of the quota, needed clarification. President Eisenhower also mentioned his feeling that broader problems concerning the operation of the Control Organization and system should be settled. Both Western leaders reiterated their desire for a comprehensive test ban, but went on to say that this was dependent upon the USSR's agreeing to the appropriate control measures, and both repeated the earlier suggestion for a phased agreement starting with an atmospheric ban. President Eisenhower

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wrote that the West would welcome a technical discussion by scientists to consider the feasibility of immediately extending the atmospheric ban to high altitudes and outer space.

Since the report of the Conference of Experts contained no recommendations for control measures at high altitudes and in outer space, obviously some technical decisions would have to be made for these environments. How well prepared the West was to embark on such decisions was debatable. For experimental data, there were the results of the 1958 Johnston Island and Argus shots. These data had been analyzed by the Panofsky Panel and certain tentative conclusions had been drawn. However, the first contract under Project Vela Sierra and Vela Hotel was not let until September 1960, and it would be at least another year before concrete results would be available. Apparently, though, no one on the Committee of Principals within the United States or in the West generally seriously questioned the wisdom of calling for technical talks at this time.

Interestingly neither President Eisenhower nor Prime Minister Macmillan mentioned the new seismic data in their letters to Chairman Khrushchev, bringing it in only inferentially, by requiring that the quota have some relationship to the detection capability of the control system. In the Geneva Conference the Western negotiators were again more explicit on this point, and openly insisted that the new data had to be considered. Mr. Tsarapkin, on the other hand, was equally adamant, maintaining that the technical basis of the negotiations had been established by the Conference of Experts and that these questions could not be reopened. He argued that:

There can be no doubt that either side could, if it wanted, submit at any time endless so-called new scientific data, which might differ widely in their nature according to the purpose pursued by the experimental scientists in that field. Those who wanted to prove at any cost how difficult it is to detect and identify nuclear explosions would prepare their experiments correspondingly. There are scientists who support this kind of view. We all know that there are certain such scientists in the United States also. It is sufficient to mention, for instance, Dr. Teller.

15See GEN/DNT/PV. 88 and PV. 89, passim.
16GEN/DNT/PV. 89, p. 19.
The USSR's position continued to be that the Treaty had to be negotiated on the basis of the recommendations of the Conference of Experts, which it held the three governments had "accepted"; that the Control Commission could consider new technical developments; and that the Periodic Review Article amply provided for revisions in the control system.

The USSR also took the same attitude with respect to control measures for the detection of nuclear tests at high altitudes and in outer space. Mr. Tsarapkin maintained that the report of the Conference of Experts did contain certain conclusions and that these were sufficient. He also repeated and elaborated his statement made January 8, 1959, that the USSR would be willing to launch artificial earth satellites and to establish ground stations to receive signals from the satellites for purposes of control in these environments.\(^{17}\) The technical details, he argued, could be worked out by the Preparatory Commission or the Control Commission. In his view, it would be diversionary to become enmeshed in a discussion of such technical details during the course of the political negotiations. It might also set a precedent in terms of going beyond the work of the Conference of Experts prior to the signature of a test ban treaty.

This was where matters stood on May 8, 1959, when the Geneva Conference recessed. The West requested the recess on the ground that the Foreign Ministers of France, the USSR, the United Kingdom, and the United States were scheduled to meet the following Monday, May 11, 1959, and they might hopefully resolve some of the issues which divided the Conference in the course of their meeting.

II

Technical Working Group I

*From Diplomats to Scientists: Getting Ready for Technical Working Group I*

Certain of these issues were indeed resolved at the Foreign Ministers' meeting—or it would be more accurate to say during the Foreign Ministers' meeting. On May 14, 1959, three days after

\(^{17}\)GEN/DNT/PV. 89, pp. 25-26.
the meeting opened, Chairman Khrushchev replied to President Eisenhower and Prime Minister Macmillan. Basically the letters were restatements of the Soviet position; however, there was one important exception. The Chairman agreed that there should be a brief “technical discussion of concrete measures as to methods of detecting nuclear explosions at high altitudes on the basis of the conclusion of the Geneva meeting of experts, for the purpose of including such methods in the system of control.” The Foreign Ministers confirmed this agreement.

The terms of reference of the technical discussion—or of Technical Working Group I (TWG I) as it ultimately was called—were left to be worked out at the Geneva Conference after it reconvened on June 8, 1959. This proved to be a disputatious task, and one which took the better part of six meetings. The terms of reference were not finally approved until June 15, 1959. Several issues were involved; all probably more important in terms of their implications with respect to the controversial and still unresolved problem of detecting and identifying underground tests and the American new seismic data than in their own right.

Chairman Khrushchev implicitly, and Foreign Minister Gromyko directly, both again refused to consider the new data and insisted that the negotiations had to be based on the conclusions of the Conference of Experts. The West on the other hand continued to insist that the new data had to be considered. On June 12, 1959, the United States finally introduced the findings and report of the Berkner Panel into the Geneva Conference. Having failed to obtain agreement through the Foreign Ministers, the United States now apparently hoped to force a modification in the Soviet position through a more open confrontation. Mr. Tsarapkin’s immediate response was that the conclusions of the Berkner Panel had been “broadly known” to the USSR “since last March from the magazine Fortune.”

The dispute about the terms of reference of Technical Working Group I should be viewed in the light of this continuing controversy. First, the two sides were divided as to whether or not the report of

20Ibid., p. 32.
the Conference of Experts should be mentioned. The Western powers finally reluctantly agreed that it should be. Secondly, there was a controversy concerning whether TWG I should discuss only which instruments should be used for detection, as the USSR argued, or more broadly evaluate the possible techniques of detection and identification, as the West insisted. In this case the Western position was accepted. Thirdly, the two sides differed about whether or not the tentative theoretical evaluations concerning the effectiveness of various techniques contained in the report of the Conference of Experts could be changed; the Soviet Union argued that they could not, and the Western powers that they could. A related issue was whether or not the data from the United States 1958 tests could be considered. Mr. Tsarapkin eventually agreed that the new material could be considered, and the larger issue was solved in a compromise fashion, as can be seen from the terms of reference:

The technical working group should assess the capabilities and limitations of possible techniques for the detection and identification of nuclear explosions at high altitudes (more than 30 to 50 kilometers) above the earth and, on the basis of discussions and conclusions of the Geneva conference of experts, recommend techniques and instrumentation for consideration by the Conference for incorporation in the Detection and Identification System.21

Fifthly, the Soviet Union sought to expand the terms of reference of Technical Working Group I so that it would also be directed to consider the problem of determining criteria for on-site inspections in the case of suspected underground nuclear explosions. The Western powers refused this proposal on the ground that different technical specialties were involved and, more importantly, they felt that the question of criteria could not be examined without considering the new seismic data. Finally, the Soviet Union won Western agreement that the Technical Working Group should report to the Conference on June 29, 1959, one week after it was convened, which proved to be an impossibly short time. Even though TWG I extended beyond this deadline, the scientists had an incredible, almost around the clock, working day during the Conference, as they did each time that they met during the test ban negotiations.

21GEN/DNT/PV. 95, p. 13.
Once agreement had been reached on the terms of reference, it was necessary to assemble delegations to participate in the technical discussions. A British delegation of three scientists in government employ was quickly assembled.\textsuperscript{22} None of the three had attended the Conference previously. The Soviet delegation was headed by Dr. Fedorov, who had chaired the Eastern panel at the Conference of Experts and had stayed in Geneva for the opening stages of the political negotiations. Two others of the seven-member Soviet delegation had also served on the Eastern panel at the Conference of Experts, M. A. Sadovsky and O. I. Leypunsky.\textsuperscript{23}

In the United States, several scientists had been approached on a tentative basis earlier. PSAC and the Office of the Science Adviser served as the bodies which recommended nominations to the President and the Department of State. On Tuesday the sixteenth, those selected were informed that they should fly to Geneva that Thursday and be ready for the opening of the discussions the following Monday. Wolfgang K. H. Panofsky, Director of the High Energy Physics Laboratory at Stanford University, who had headed the Panel of the President’s Science Advisory Committee which had examined this subject, was chosen as the Chairman of the American delegation.\textsuperscript{24} In creating the delegation, an attempt was made—as it had been with respect to the Conference of Experts—to maintain balance between the conflicting points of view among scientists concerning the wisdom of a test ban, and those with extreme positions were not included. Of the nine men appointed to the delegation, only two, Richard Latter and Spurgeon Keeny had at-

\textsuperscript{22}H. R. Hulme, I. Maddock, and R. Press.
\textsuperscript{23}The other members of the Soviet delegation were J. L. Alpert, A. I. Ustyumenko, O. A. Grinevsky, and S. N. Vernov. There is reason for believing that Grinevsky was not a scientist but a political representative.
\textsuperscript{24}The other members of the delegation were: Sterling Colgate, Lawrence Radiation Laboratory, University of California; Allen F. Donovan, Director, Astrovehicles Laboratory, Space Technology Laboratory, Los Angeles, California; Allen Graves, Lawrence Radiation Laboratory, University of California; Spurgeon Keeny, Jr., Technical Assistant, Office of Special Assistant to the President for Science and Technology; Richard Latter, Chief, Physics Division, RAND Corporation, Santa Monica, California; Col. Dent L. Lay, Assistant Director, Technical Operations Division, Advanced Research Projects Agency, Department of Defense; Allen M. Petersen, Head, Propagation Laboratory, Stanford Research Institute, Menlo Park, California; and Kenneth M. Watson, Professor of Physics, University of California at Berkeley.
tended the Conference of Experts. The former (like his brother Albert) was a RAND Corporation physicist and the latter, a technical assistant in the Office of the Special Assistant to the President for Science and Technology. In addition, the delegation included Allen Graves, a physicist on leave from the Lawrence Radiation Laboratory, who had been attending the political negotiations as the representative of the Atomic Energy Commission.

Professor Panofsky was actually intercepted in the midst of a cross-country trip with his wife and five children. His destination, though, was Geneva, where he planned to spend a year at CERN, the Organisation européenne pour la recherche nucléaire. He left his family, hurried to Washington—he was the only nonofficial member of the delegation to go there prior to departure for Europe—where he had a fifteen-minute conference with the Secretary of State, and then went on to Geneva, arriving somewhat later than the rest of the American delegation, and after the first meeting of Technical Working Group I.

Many of the members of the American delegation had been members of Professor Panofsky's Panel on the detection of nuclear explosions at high altitudes and in outer space, and this was the only preparation that the group had to work with. The Panel had met about eight or nine times in the winter of 1958-59. Each meeting lasted about a day, and the Panel members did various pieces of work in the interim periods. Dr. Killian had attended most of the meetings, and representatives of the Department of Defense and Atomic Energy Commission were members of the Panel. The Department of State, however, was not represented, and generally speaking the group had very little political guidance. Again, the scientists were not told what threshold of detection would be the maximum risk that the United States could accept. In this instance, in contrast to the Conference of Experts and a later technical working group, the questions of identification and "objective criteria" were carefully avoided. The scientists were not told how much money the United States would be willing to invest in a detection and identification system, nor were they told how much to assume that an evader would be willing to spend to mask his action. Thus there were no cost figures. This was the case despite the fact that the report of the Panofsky Panel had contained an approximate assessment of the cost of conducting clandes-
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time nuclear explosions in space using various methods, and of the cost of various detection systems. The scientists were not given any guidance concerning the basis for choice between various alternative techniques. They were merely given the data from the 1958 test series and asked to evaluate the capabilities and limitations of various techniques for detection and identification and to elaborate a possible system. Secretary of State Herter did not expand these instructions very much in his brief conference with Professor Panofsky. Just as in the case of the Conference of Experts various crucial problems were left for the scientists to decide.

Nor were these matters settled when the American delegation assembled in Geneva. The scientists spent most of the weekend brushing up on their technical preparation. Ambassador Wadsworth was reluctant to become involved in technical discussions, and in any case, could not make fundamental political decisions. A junior State Department member of the American delegation at the Geneva Conference was assigned to the scientists, but he did not fully understand the technical issues, nor did he attend all of the meetings of Technical Working Group I. During the course of the negotiations the scientists found it completely impossible to obtain political guidance. In other words, the United States treated Technical Working Group I, as it had the Conference of Experts, as a purely technical session which the scientists should work at by themselves.

TWG I: Toward an Adjustment in the Scientific Base

Technical Working Group I began with an argument between East and West concerning the agenda, which in several respects repeated the controversy about its terms of reference. Dr. Fedorov proposed a draft agenda which virtually would have limited the discussion to the elaboration of matters treated by the Conference of Experts. The West on the other hand preferred a broader definition of TWG I's tasks, and eventually won its point. However, the controversy continued to plague the technical discussions. Throughout TWG I the Soviet scientists tended to take the report of the Conference of Experts as a fixed document, binding the various states. They were willing to accept only evaluations which confirmed or upgraded the assessments arrived at then, and fought

any attempts to demonstrate that these assessments were too optimistic.

As in the Conference of Experts, the burden of proof tended to rest with the West since it was the Western powers which had demanded that the technical discussion be held. The Western scientists presented the bulk of the data, and it seems fairly clear that the Soviet scientists withheld considerable data which would have been useful to the discussions. This is not to say that the West made all of the data which it had available, but merely to say that it made considerably more available. Throughout the discussions, the Western scientists pressed for quantitative assessments. The Soviet scientists preferred more general formulations. The Western scientists sought to point out how violators might attempt to avoid detection. The Soviet scientists made light of these suggestions and often appeared not to have previously considered the possibilities. The Western scientists sought to indicate that although something was known about the signals of nuclear explosions at high altitudes and in outer space, very little was known about the natural background with which such signals had to compete. None of the devices which were discussed as instruments of detection existed at that time, although some of their component parts were technically proved. The Western scientists introduced data from the United States 1958 test series but argued that only the Teak and Orange shots were directly relevant and that these were conducted at relatively low altitudes. They maintained that further empirical data might invalidate their theoretical conclusions, making them appear either too optimistic or too pessimistic. The Soviet scientists argued as they had in the past that the progress of science as a whole could only result in improvements in the ability to detect and identify nuclear explosions at high altitudes.

In testimony before the Joint Committee on Atomic Energy about a year later, Professor Panofsky summed up the position and difficulties of the Western scientists.

We all realize that the Russians have very little interest in the technical reliability of control procedures because clandestine testing is essentially impossible in any of the Western countries. They do, however, have a very substantial interest in maintaining the secrecy of their country, and therefore in reducing the degree of access demanded
by the control system. Hence, it is always in their interest to minimize the extent of the control system; in order to do this the tendency is to view the performance of the control system in the best possible light. Any critical evaluation of the performance of the system, therefore, always rests upon the shoulders of the Western delegations. In turn, this makes the performance of the negotiations very arduous for the Western technical delegates since in many cases the facts which have to form the basis for a critical assessment have not been developed prior to negotiations.26

The differences between the Western and Soviet scientists in these matters were even more acute during the sessions of Technical Working Group I than they had been during the Conference of Experts. No doubt, both sides, and especially the Western scientists, constantly had the controversial history of the report produced in that meeting in mind.

Following the pattern of the Conference of Experts, and the agenda, the report of Technical Working Group I was divided into two basic sections. The first section contained a general discussion of techniques for detecting nuclear explosions at high altitudes. It was divided into two subsections; the first, dealing with detection by means of apparatus installed in artificial earth and solar satellites; and the second, dealing with detection by means of apparatus installed at ground-based control posts. The second half of the report contained recommendations for consideration by the political Conference concerning the techniques and instruments which might be incorporated in the Detection and Identification System. In this section, in contrast to the report of the Conference of Experts, alternatives were spelled out in certain areas and the choices left to others.

Other than the problems which arose from the broad differences of approach mentioned above, drafting the first part of the report was relatively noncontroversial. The only deep difference appeared to be caused by differing technical experiences; the orbit

of Soviet satellites had been such as to gather little information on the Van Allen radiation belt, and as a consequence the Soviet scientists were less inclined to take this feature of the atmosphere into account than the Western scientists. In general, both sides had very little empirical data on the level of background radiation in the atmosphere.

Not surprisingly it proved less easy to agree on recommendations for control systems, which was after all the core of the issue. Indeed, it is difficult to see why the attempt to arrive at joint assessments was necessary, except as an effort to erect an agreed point of departure for the recommendations. In formulating the recommendations, the scientists were in effect engaging in political negotiations, though the subject matter was technical and scientific. The American scientists were clearly considerably more aware of this than they had been a year earlier at the Conference of Experts. However, their political instructions were not any more complete than they had been in the previous instance. In the absence of instructions on such vital matters as threshold and cost, the American scientists tended to push for the maximum. For example, one of the problems of detection of nuclear explosions at high altitudes was the danger of false alarms; the danger that the control system might indicate that a nuclear explosion had occurred when actually none had. The Americans sought to minimize this possibility as much as possible through redundancy (having overlapping systems) and other methods; their target was a system that would not produce more than one false alarm every one hundred years. The Soviet scientists on the other hand were inclined to make more modest demands on the recommended system, but ultimately generally agreed to the American position.

The same pressure to provide for all possible contingencies led the American scientists to argue the need for a system of satellites in orbit around the sun, so that there would be no blind spots behind the sun or the moon. This recommendation, to which the

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27 For example, during the Conference Panofsky said to Fedorov: "I am more than happy to negotiate on matters of recommendations where political judgment is involved, where we must reach an agreement in order to have an agreed report concerning what steps are to be taken, but I cannot negotiate on the question of the modification of scientific facts." GEN/DNT/HAT/PV. 16, p. 46.
Soviet scientists were opposed and which would have involved substantial cost, was phrased in optional terms. Interestingly, after the conclusion of Technical Working Group I, with the advantage of further research, the American scientists also became less interested in the system of solar satellites. They eventually concluded that the costs and difficulties of conducting clandestine nuclear explosions behind the sun or the moon would be so great that it was highly unlikely that any violator would take such action.

In all, ten methods of detection were considered, and techniques involving all but one of these were recommended for inclusion in the Detection and Identification System. The one which was not recommended was a ground-based technique, backscatter radar. After over a week of irrelevant, and at times spurious technical argument, Dr. Fedorov stated that the Soviet scientists could not agree to recommend backscatter radar because it could "also be used for purposes having nothing to do with the control of high-altitude explosions." Although the American scientists were reluctant to admit that backscatter radar is an effective means of observing the launching of missiles, nevertheless, on the basis of the Argus experiments, the American scientists believed it to be an extremely useful means of detecting nuclear explosions at high altitudes. Failure to include it in the Detection and Identification System would result in an uncovered "hole" in the system under certain conditions in the area from 50 to 100 kilometers and a "half-covered hole" from 100 to 1,000 kilometers. The report merely stated that the Technical Working Group had been unable to come to an accord on this matter. That under certain circumstances there would be a "hole" in the system was not mentioned in the report.

This provides an example of the difficulty which the scientists had in obtaining political guidance. When it appeared certain that there would be an impasse on the question of whether or not to recommend backscatter radar in the control system, the American scientists felt that the most constructive move would be to agree to disagree. This being a political decision, the delegation sought guidance, but found it extremely difficult to convey the nature of the

28See Panofsky's comments before the Joint Committee on Atomic Energy, Hearings: Technical Aspects . . . of a Nuclear Weapons Test Ban, supra note 26, p. 47.
29GEN/DNT/HAT/PV. 17, p. 81.
The Search for Technical Agreement

problem to the diplomats and politicians in Geneva and Washington. In the end, the scientists had to take the decision on their own responsibility.

In formulating the recommendations the Western scientists sought to be as specific as possible, for instance listing the exact number of satellites required and their orbits. The Soviet scientists preferred more general formulations, and at one point suggested that matters involving a choice between various alternatives should be left to the Control Commission. Professor Panofsky refused to do this, on the ground that decisions on such issues should be the prerogative of the diplomatic Conference.\textsuperscript{30} In some instances he was willing to leave the choice between alternatives for subsequent decision, although in more specific terms than the Soviet scientists preferred, but he argued that the diplomatic Conference had to decide how the decisions should be made. The reason for the insistence of the American scientists on being specific was their awareness of the tentative agreement reached in the Geneva Conference that major changes in the detection and identification system would require the unanimity of the three nuclear powers.\textsuperscript{31}

The problem of identifying the violator after a nuclear explosion at high altitudes had been detected by the control system was not discussed in Technical Working Group I. Nor did the group discuss criteria for establishing evidence of detection. As had been mentioned, these issues had been carefully avoided by the politicians and diplomats in their discussions with the scientists. Technically, there is no way the violator could be identified unless the control system also had available to it information on missile launchings. Had the system been established, either some agreement would have had to have been reached on this point, or in the case of a detection of a nuclear explosion, states would have had to base their decision concerning what action to take on their unilateral intelligence systems. Since the duration article provided that the treaty could be terminated by unilateral action, this would be feasible. However, in all of the other environments the Western powers demanded incontrovertible evidence identifying the violator.

One might ask if such evidence was necessary in other en-

\textsuperscript{30} GEN/DNT/HAT/PV. 14, p. 71.
\textsuperscript{31} See Hearings: Technical Aspects . . . of a Nuclear Weapons Test Ban, supra note 26, p. 40.
DIPLOMATS, SCIENTISTS, AND POLITICIANS

environments, why not at high altitudes? Or, if not necessary at high altitudes, then why was it necessary for atmospheric, underwater, and underground explosions? Presumably the reason was that for identification it would have been necessary to have controls over another element of weaponry, the launching of missiles. But, to establish positive identification in the case of underground explosions, on-site inspections are required, which might also reveal other aspects of military strength.

Whether or not the system recommended by Technical Working Group I would have provided sufficient safeguards against the possibility of clandestine nuclear explosions in outer space was a matter of judgment. First, there was the “hole” in the system at low altitudes under certain conditions caused by the failure to recommend the backscatter radar technique. Secondly, there was the problem of identification. Finally, and perhaps most importantly, at extremely high altitudes—above 5,000,000 miles—it would only be possible to detect nuclear explosions through the soft or thermal X-ray technique, and shielding would appreciably reduce the effectiveness of this technique. Thus, if a violator were willing to spend sufficient resources to put a nuclear device, the necessary instrumentation, and a shield into outer space, and were patient enough to wait some time for the results, he could conduct undetected nuclear weapon tests.

Whether or not the possibility that a state might conduct undetected clandestine nuclear explosions in outer space constituted a serious risk was subject to debate. Professor Panofsky, in testimony before the Joint Committee on Atomic Energy expressed this opinion:

From the purely technical point of view, it appears likely that, given arbitrarily high incentives on the part of the violator, it will always be possible to devise an essentially undetectable means of carrying out the violation. I believe, however, that before that point is reached, there is the question of whether there really is sufficient incentive either for the violator to carry out tests under these extreme conditions or for the detecting system to be expanded to the maximum possible degree. 32

32Hearings: Technical Aspects . . . of a Nuclear Weapons Test Ban, supra note 26, p. 37.
On the other hand, Dr. Byron P. Leonard, of the Space Technology Laboratory, during the same hearing asserted, "It is really no trick at all to run tests outside of the X-ray detection circle ..." Edward Teller expressed the same view in a syndicated newspaper article, "Testing in space provides a loophole through which one could drive a herd of wild elephants." Again, all three scientists used the same technical data; where they differed was in their estimate of the necessary degree of security or in the threshold of detectability below which American security interests would be endangered. This was a political judgment involving an assessment of the intentions of other states, one's willingness to bear monetary and other costs involved in any type of detection system, and one's willingness to accept risks. Yet these were decisions on which the American scientists at Technical Working Group I, as their predecessors at the Conference of Experts, received little guidance from either United States politicians or diplomats.

**TWG I Reports to the Diplomatic Conference**

Technical Working Group I adopted its report on the morning of Friday, July 10, 1959. That afternoon the report was presented to the diplomatic Conference. Both sides argued that the results of the Technical Working Group supported their position with respect to the report of the Conference of Experts. In the Soviet view, the report of TWG I confirmed the findings of the earlier report, and proved that the progress of science made detection easier, while in the Western view it was conclusive evidence of the importance of looking at new data, and also proved that the demand to do this was not grounded on obstructionist motives.

The Conference agreed to release the report of Technical Working Group I immediately. This was in contrast to the situation with respect to the report of the Conference of Experts, which had first been submitted to the governments concerned and then released.

The next move with respect to the problems discussed by Technical Working Group I came one month later, on August 10,

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33Ibid., p. 345.  
35See GEN/DNT/PV. 109, passim.
1969, when Mr. Tsarapkin announced that the USSR agreed to "the inclusion of the methods and instrumentation recommended . . . for the detection of high-altitude nuclear explosions in the system of control over the cessation of nuclear tests." That same day, the British delegate, Sir Michael Wright, stated that his government accepted the report "as a correct technical assessment of possible techniques for the detection and identification of high-altitude nuclear explosions in the light of scientific knowledge available at the time of the Working Group's session." He added that his government was prepared to take part in discussions regarding the embodiment in the treaty of provisions on high-altitude controls, in the light of the recommendations contained in the report."

It was obvious that Ambassador Wadsworth, at this point, had no instructions on the issue. On August 26, 1959, however, following Sir Michael Wright's phraseology, he stated that the United States accepted "the report as a correct technical assessment of the capabilities and limitations of possible techniques for the detection and identification of high-altitude nuclear explosions in the light of presently available scientific knowledge." He also said that the United States was studying the "complex problems of timing and scope" that would be involved in preparing treaty language for a high-altitude detection system "that would be established" on the basis of "the report of TWG I." However, the United States did not discuss its proposals concerning this matter in broad outline until March 29, 1961, nearly two years later, and it did not submit specific treaty language until April 18, 1961.

The basic reason for the delay was the inability of the United States Administration to agree on the degree of risk that the United States could reasonably accept; again, it was the threshold problem. The demands of the Atomic Energy Commission and, to a lesser extent, of the Department of Defense, were considerably higher than those of the Department of State or the President's Science Advisory

36GEN/DNT/PV. 121, p. 10.
37Ibid., p. 13.
38GEN/DNT/PV. 127, p. 4.
39See GEN/DNT/PV. 280, pp. 3-7.
Committee. A related reason was the high cost of the complete high altitude system. Few were willing to countenance this cost. Thus the United States put itself in the uncomfortable position of insisting on the one hand that the “loophole” associated with the lack of high altitude coverage was sufficient reason to be an impediment toward concluding a nuclear test ban treaty, while on the other hand it did not appear to regard the “loophole” as being of sufficient importance to be willing to pay the money to plug it. Significantly, when treaty language was finally proposed, with the advantage of data gained from Vela Sierra and Vela Hotel, the United States asked for a system which was somewhat more modest and consequently considerably less expensive than that which had been recommended by Technical Working Group I. Eventually, when the United States signed the Moscow Treaty, which relied on national detection systems, it agreed to a ban on testing at high altitudes and in outer space before basic elements in its own national control system were operative.

The subsequent history of the action with respect to the report of Technical Working Group I, raises again the question of why the Western powers insisted on calling the meeting. Certainly the Western governments did not learn anything from the discussions that they could not have learned from private consultations with their own scientists. The extent of knowledge was limited, and there were risks in doing anything such as making formal recommendations for a control system that might tend to “freeze” the situation. Formulating recommendations involved political choices, which the United States government was unprepared or unable or unwilling to make at that time and for more than a year and a half after the report of TWG I was filed. Perhaps some American officials hoped that the technical discussions would in some way solve the problem for them. If they did, they both overestimated and misunderstood the qualities and powers of science. In fact, however, the Technical Working Group I did set a precedent for reconsidering matters discussed by the Conference of Experts, and perhaps this was its principal function for the West. In addition, of course, when the United States eventually decided to accept some of the recommendations of TWG I, the Soviet Union was already on record as having done so.
The Diplomatic Conference Inches Along

The Geneva Conference had been in session during the meetings of Technical Working Group I and it continued in session until August 26, 1959. The meetings, however, appeared to be mainly exercises in marking time. Neither side substantially modified its position. The Western powers continued to insist that the new data had to be considered, and the USSR continued to refuse to take this action. Mr. Tsarapkin constantly referred to the report of the Conference of Experts as "the basis" for the negotiations, while Ambassador Wadsworth held that it was merely "an agreed statement of the best scientific opinion available as of mid-1958."41

The USSR's position, though, was clarified somewhat when on June 19, 1959, Mr. Tsarapkin gave a more detailed response to the findings of the Berkner Panel.42 It was essentially negative. First of all, he alleged that to act on the recommendations of the Berkner Panel would mean that a test ban would be delayed for many years. He pointed out that no one could confidently predict how the suggestion for putting seismographs in deep holes would work out, since this technique was largely unexplored. He also objected to the suggestion of unmanned stations placed at considerably closer intervals than had been recommended at the Conference of Experts. He alleged that they would create opportunities for espionage since men would have to install the stations and check their operation.

The Soviet position was further clarified on July 9, 1959, when Mr. Tsarapkin submitted a draft article on on-site inspection.43 As he had asserted previously in response to Western questioning, the terms of the article did not require the unanimous agreement of the three nuclear powers for the dispatch of an inspection team and the inspection teams could go anywhere that the instruments indicated that an unidentified event had occurred. From the Western point of view, an unexpected feature of the proposal was that the three nuclear powers would each be subject to an equal number of inspections. Actually the proposal advanced the negotiations very little, for the most crucial element, the number of on-site inspections that

41GEN/DNT/PV. 99, p. 10.
42Ibid., pp. 14-16.
43GEN/DNT/PV. 108, pp. 3-4.
would be allowed annually, was not specified; the draft merely contained a blank space. The USSR did not formally submit a definite figure until more than a year later. However, the day before Ambassador Tsarapkin submitted the draft article, Soviet First Deputy Premier Frol R. Koslov told Michigan's Governor Mennen Williams that the USSR might accept as many as three on-site inspections each year, which is the figure that the Soviet Union ultimately proposed on July 26, 1960. Soviet representatives also mentioned the figure three in informal conversations in Geneva in the summer of 1959.

The West assumed that a much larger number would be needed, and the ensuing argument between East and West concerning whether the quota should be "politically determined," as the USSR insisted, or "scientifically determined," as the United States and the United Kingdom insisted, was in reality principally an argument about numbers. At the time very little was known about the frequency of small earthquakes and the knowledge about the relationship between the seismological signals transmitted by underground nuclear explosions and earthquakes was at best sketchy. If one accepted the estimate of unidentified events contained in the report of the Conference of Experts, the figure of three on-site inspections annually in the USSR would not be completely unreasonable even though such a small figure would pose practical difficulties. However, the West felt that in view of the new data such a figure would be clearly insufficient. At the same time, the West was not prepared to submit a specific figure, and actually did not make a definite proposal until February 11, 1960. Then it merely suggested that a fixed percentage of unidentified events should be eligible for inspection. In fact, no one knew how many unidentified events there would be. In these terms, the Western demand that the Conference consider the new data and agree that the quota of on-site inspections should be scientifically determined were really demands to get pegs on which to base claims for a larger number of inspections. The United States position also reflected the Atomic Energy Commission's insistence that this was a "technical" matter on which its views should have precedence. In somewhat different terms, the question of the number of on-site inspections again raised the

44New York Times, July 9, 1959, p. 3.
troublesome problem of the threshold. What risk could and should the United States take?

The debate on these matters continued throughout this phase of the Conference. Perhaps the most lively session occurred on July 1, when the American delegation was accompanied by the Chairman of the Atomic Energy Commission, John A. McCone, and a group of Congressional observers.45 Both sides were at their belligerent best. Exactly what purpose this episode served is difficult to fathom.

The Ending of Soviet Nuclear Assistance to Communist China

Actually, the most important events for the course of the negotiations at this stage occurred elsewhere. Perhaps the most important of these was that the USSR appears to have decided to discontinue assisting Communist China develop a nuclear capacity. The Chinese have charged that on June 20, 1959, “The Soviet Government unilaterally tore up the agreement on new technology for national defense concluded between China and the Soviet Union on October 15, 1957, and refused to provide China with a sample of an atomic bomb and technical data concerning its manufacture.”46 If this charge is true, and the USSR has not denied it, such a decision on the part of the Soviet Union had important implications for the prospects of a test ban. Like the United States, the Soviet Union apparently found it impossible to engage in a complete transfer of nuclear weapons to other states. One can only speculate about the reasons for this, but it seems plausible to assume that the USSR wanted to avoid creation of situations which might allow it to become involved in nuclear war by the actions of an ally. No doubt the USSR was also concerned about its present and potential relationship with the ally in question. China’s acquisition of a nuclear capability would strengthen that state in relation to the USSR. If these were the Soviet concerns, at this point a test ban may well have assumed greater importance for the USSR.

45GEN/DNT/PV. 106.
At that time, however, the West was ignorant of these developments. For the United States, the most crucial decision that had to be taken during this period was whether or not, in the light of the impasse in the negotiations, the one-year moratorium on testing of nuclear weapons offered by President Eisenhower in his initial proposal for the Conference should be continued. Although this was related to the question of whether or not the negotiations should be continued, the two issues were considered to be separable. That the issue of the moratorium had to be decided shows the limited significance of President Eisenhower's statement, made in November 1958 when he revealed that the Soviet Union had continued to test after the opening of the diplomatic conference, that the United States would no longer consider itself bound by its pledge to forego testing.

Even though there was a specific point at which a decision in this matter supposedly was made, like so many important decisions in the course of these negotiations—and perhaps generally—by that time prior decisions and other events almost forced the choice. Alternatives were first narrowed in the discussions and decisions relating to the Federal Budget for fiscal year 1960. As has already been mentioned, the Administration decided not to ask for funds for the testing of nuclear weapons during that period. Chairman McCone and General Starbird of the Atomic Energy Commission explained the nature and ramifications of the decisions within the Administration—which Congress in due course confirmed—in testimony before the House Committee on Appropriations on June 23, 1959. They stated that the Administration had decided to request only sufficient funds to maintain the test sites on a standby basis “except for limited tunnel construction at the Nevada test site to provide an underground test readiness capability should weapons tests be resumed.”

In the Pacific Test area, the AEC envisaged employing fewer people and spending less money than was normally spent in periods between test series; the objective was only to retard and prevent the inevitable deterioration resulting from the climate. The work on the tunnels in Nevada was in part designed to obtain

information from past tests. In addition, the program was designed to give decision-makers some flexibility. If no construction were undertaken, it would take nearly a year from the time that a decision was taken to resume testing until it could be executed.\textsuperscript{48} As it was, because of the extensive nature of the 1958 test series and the budgetary decisions for fiscal 1960, the United States probably could not have resumed testing nuclear weapons on a significant and meaningful scale in 1959, and probably not even in the first half of 1960.

A second factor narrowing the range of alternatives was that in the summer of 1959 a new round of East-West negotiations came into prospect. The Foreign Ministers of France, the USSR, the United Kingdom and the United States resumed their negotiations in Geneva on July 13 and continued in session until August 5. Among other things they agreed that a Ten-Nation Disarmament Committee should be established outside the framework of the United Nations and on the basis of parity between East and West. Thus, they finally conceded what the Soviet Union had demanded at the twelfth session of the General Assembly in 1957. They agreed that the Committee should meet sometime in 1960 and that it should report to the UN Disarmament Commission. In anticipation of this development, the Administration established a committee under the chairmanship of Charles A. Coolidge, a Boston attorney and a former Assistant Secretary of Defense, to reexamine and reevaluate the American position with respect to disarmament. The Administration again chose to rely on an "outsider" and a generalist for a reexamination and reformulation of its basic disarmament position. A related development was that in mid-July, President Eisenhower decided that he would invite Chairman Khrushchev to visit the United States and that he would agree to return the visit. Khrushchev's trip was scheduled to coincide with the opening of the fourteenth General Assembly of the United Nations in September.

Prior to his trip, Khrushchev made two important pronouncements with respect to the test ban negotiations. On August 11 he stated that the USSR would accept a pledge not to be the first nation to resume testing nuclear weapons.\textsuperscript{49} At about the same time,

\textsuperscript{48}See \textit{ibid.}, p. 168.
an article was submitted under his name for publication in *Foreign Affairs*. Although it was not published until early September, decision-makers were aware of its contents somewhat earlier. In the article he stated that the progress in the test ban negotiations justified "the hope that an agreement on the discontinuation of nuclear weapon tests will shortly be reached." He went on to state that such an agreement "would be an important step on the way to the solution of the disarmament problem and the banning of nuclear weapons in general." Among other things, the statement revealed the verbal constancy of the Soviet objective of eliminating nuclear weapons.

Thus when the Committee of Principals discussed whether or not to continue the moratorium there was really little choice. The United States was not in a position to inaugurate a significant test series. To end the moratorium might jeopardize the test ban talks and might also foreclose the possibility of fruitful negotiations on broader issues. In any case, the United States would be subjected to criticism on these grounds, and since the General Assembly was about to open, critics would have ample access to a worldwide forum. In the light of Chairman Khrushchev's pronouncement about the Soviet position, the United States could be particularly vulnerable.

The advice of the Committee of Principals and the decision of the President was first that the United States should ask for a six-week recess in the Geneva negotiations which would coincide with Chairman Khrushchev's visit to the United States and the opening of the General Assembly, and secondly that the United States should continue its unilateral suspension of nuclear weapons tests through the current calendar year. The latter aspect of this decision was made public in a statement released by the Department of State on August 26, the day the Conference recessed.

The United Kingdom took a somewhat different line. On August 27 the Foreign Office announced that the United Kingdom would not resume nuclear weapons tests as long as "useful discussions" were under way looking toward an agreement.

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The following day the Soviet Government released its statement on the matter. The statement was in accord with Chairman Khrushchev's earlier pronouncement. It stated that the Council of Ministers had resolved:

Not to resume nuclear tests in the Soviet Union if the Western Powers do not resume testing of atomic and hydrogen weapons. Only in the case of resumption by them of nuclear weapons tests will the Soviet Union be free from this pledge.\(^{52}\)

If all three governments acted according to their pledges, the moratorium would continue through 1959, and in view of the Soviet and British positions, there would be strong pressure to continue it even longer. At this point, the Soviet Union had in reality what it had always asked for, an unpoliced test ban.

**On the East River and at Camp David**

Actually, because of the British election, the Geneva Conference did not resume until October 27. Meanwhile, the question of a test ban was considered and debated in other forums, both as a separate issue and in the context of broader measures of disarmament. In particular, the matter was discussed in private meetings between President Eisenhower and Chairman Khrushchev and their advisers at Camp David and in public on the bank of the East River in the General Assembly of the United Nations. On the surface, none of the discussions appeared to advance the negotiations.

Chairman Khrushchev stole the headlines on September 19 when, during the course of his appearance before the United Nations, he offered a proposal for General and Complete Disarmament.\(^{53}\) By the terms of this proposal all states "should divest themselves of the means of waging war" within a period of four years; they would be permitted to retain only those forces required for internal security. If the Western powers were not prepared to accept this, Khrushchev offered an alternative proposal for partial measures

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52Ibid., p. 377.
53UN General Assembly, Plenary Meetings, *Official Records* (14th Session), pp. 31-38. See also UN Document A/4219.
of disarmament. Interestingly, a test ban was not included in either suggestion. However, during the course of his address, he stated that the question of a test ban was "acute and eminently ripe for solution," and asserted that negotiations on broader measures of disarmament should not delay progress on this matter. He also repeated the pledge that the USSR would not "resume nuclear explosions in its country if the Western Powers do not resume the testing of atomic and hydrogen weapons."

Actually, British Foreign Minister Selwyn Lloyd had outlined a comprehensive plan for disarmament the previous day, but because it was phrased in less dramatic terms, it received much less public attention. His was a gradual scheme, which included a test ban as an integral first step.

The General Assembly did not begin to consider either proposal in detail or other matters relating to disarmament until October 9. In the interim, Chairman Khrushchev and President Eisenhower held their tête à tête at Camp David. The two leaders discussed the test ban talks fleetingly and Secretary of State Herter and Foreign Minister Gromyko also considered the matter. Apparently all that happened was that both sides expounded their positions. In retrospect, however, it appears that the Soviet leadership became convinced of the American determination that the new seismic data would have to be considered before the United States would agree to a comprehensive test ban.

Back in the General Assembly both the Soviet and British disarmament schemes were mooted, and eventually a resolution was unanimously adopted which referred both, as well as other suggestions, to the new Ten-Nation Disarmament Committee.

In addition, the Assembly discussed the questions of banning nuclear tests and preventing the dispersion of nuclear weapons both in general terms and specifically with respect to the proposed French tests in the Sahara. In contrast to the previous session, neither the USSR nor the United States submitted draft resolutions. The pre-

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56 UN General Assembly Resolution 1378 (XIV).
vious year each protagonist had submitted draft resolutions with the obvious motivation of gaining support for their respective positions and strengthening their hands at the negotiating table. Now, however, the two superpowers seemed content to leave the matter for private talks, and it was the smaller powers who were attempting to use the Assembly. They felt strongly about the general issue and were particularly concerned about the proposed French nuclear tests. The strength of their sentiment can be explained in various terms. For one thing, nuclear weapons were glaring evidence of the disparity between their power and that of the superpowers. Thus the issue became mixed with the traditional small state-large state controversy. For another, they regarded testing, with the resultant contamination of the atmosphere all over the world, as a flagrant violation of their moral and legal rights. Their ire at the French plans stemmed from general concern about the “nth country” problem, and also from specific concern about the implications of the dispersion of nuclear weapons for their own relative strength and future plans. In addition, the fact that the French planned to test nuclear weapons in a colonial territory which was in the midst of an active revolt joined the issue with anticolonial sentiments and the anticolonial movement.

In August, India had requested that the issue of the suspension of nuclear and thermonuclear tests be included in the agenda, and after the Assembly opened it submitted a draft resolution. Subsequently, twenty-three other states, all of which except Cuba and Yugoslavia were from Africa and Asia, joined in sponsoring a version of India’s proposal. Austria, Japan, and Sweden also submitted a draft resolution. Both resolutions expressed the hope that the states participating in the Geneva Conference would soon reach an agreement and that they would continue the present voluntary moratorium. In addition, the twenty-four power resolution appealed to other states to desist from testing. In explaining the United States position, Ambassador Lodge asserted that the American objective was “the ending of nuclear weapons tests under an agreement providing effective international control,” and he argued that

57See UN General Assembly Resolutions 1402 A (XIV) and 1402 B (XIV).
"an indefinite continuation of a voluntary uncontrolled suspension of tests would not contribute to that objective."  

Both resolutions were enthusiastically supported by the Soviet bloc. The United States and the United Kingdom voted for the resolution which had been submitted by Austria, Japan, and Sweden, which was adopted by a vote of 76 to 0, with 2 abstentions, but abstained on the twenty-four-power resolution, which was adopted by a vote of 60 to 1, with 20 abstentions. The representatives of the United States and the United Kingdom never explained specifically why they took different action in the two instances, but clearly the position of France and the way that state was affected was a factor.

France abstained from voting on the three-power resolution and opposed the twenty-four-power draft. Its opposition was even more pronounced in the case of those resolutions which dealt with the dispersion of nuclear weapons. One, submitted by Ireland, asked the newly created Ten-Nation Disarmament Committee to examine the feasibility of an international agreement including appropriate inspection and control provisions which would provide that the nuclear powers would refrain from giving control of nuclear weapons to non-nuclear powers, and that the latter would refrain from manufacturing nuclear weapons. In other words, what was envisaged was a freezing of the nuclear club. The resolution probably did not contravene the NATO stockpile concept then in effect. It was adopted by a vote of 70 to 0, with 12 abstentions. The Soviet block, Peru, the Republic of China, and France abstained. Interestingly, all those states which abstained argued, basically, that the resolution was not sufficiently comprehensive, that this problem could only be handled in the context of other measures of arms control. The Soviet Union argued that the resolution was meaningless as long as states could station nuclear weapons outside of their territory, an obvious reference to United States policy in NATO and elsewhere. France argued that it would only be just to

59 UN General Assembly Resolution 1380 (XIV).
take action against the dispersion of nuclear weapons in the context of broader measures leading to nuclear disarmament. Their positions had interesting implications for the Geneva Conference, since easing the "nth country" problem was one of the putative advantages of the test ban.

The other resolutions considered by the Assembly on the subject of the dispersion of nuclear weapons dealt with France directly. In August, Morocco had asked that the question of the proposed French tests in the Sahara be inscribed on the agenda, and the day that the debate on this item opened twenty-two African and Asian states submitted a draft resolution specifically asking France to refrain from conducting its planned tests. During the course of the debate Italy and the United Kingdom submitted a substitute resolution, which merely would have requested France to take account of the views expressed in the debate and expressed the hope that France would associate itself with whatever arrangements were worked out in Geneva for the discontinuance of nuclear weapons tests. Although the Western powers generally supported this resolution, the African and Asian group and the Soviet bloc did not, and it was rejected in the First Committee by a vote of 24 to 38 with 20 abstentions. Both the First Committee and the Assembly adopted a slightly modified version of the twenty-two-power draft. In the plenary session the vote was 51 to 16 with 15 abstentions. The United Kingdom and the United States both voted against the resolution, as did France.

During the course of the debate Ambassador Lodge emphasized that the United States "had no technical information about the French experiment." The French delegate, Jules Moch, stated in extremely clear terms that France would abandon its planned tests only if the three original nuclear powers "agreed to halt, under international control, the production of fissionable materials for military purposes, to begin the reconversion of their stockpiles and

61 UN General Assembly, Plenary Meetings, Official Records (14th Session), pp. 581-82.
63 UN Document A/C. 1/L. 239 and Add. 1.
64 UN General Assembly, First Committee, Official Records (14th Session), p. 110.
to do away with nuclear weapons carriers—in short, to give up their *de facto* monopoly."^{65}

In the same debate, the Soviet delegate asserted that "the French nuclear test would not contribute to the success of the Geneva talks."^{66} This statement was made immediately after he had repeated Khrushchev’s pledge that:

> Only in the event of the resumption of nuclear weapons tests by the Western Powers, would the USSR be released from the obligation which it had taken upon itself.

The point that he did not clarify was whether or not the USSR would regard nuclear tests by France as the resumption of tests by the Western Powers. The debate indicated clearly how difficult the "nth country" problem would be, and the serious implications that the problem had for the test ban negotiations.

**Presidential Politics**

At the same time that these events were occurring, the entire test ban issue was becoming involved in the political maneuverings associated with the forthcoming Presidential election in the United States. On October 25, in a radio interview, Governor Nelson A. Rockefeller, a prominent aspirant for the Republican nomination, stated that he felt that the United States should resume testing nuclear weapons underground.^{67} Five days later, Senator Hubert H. Humphrey, a Democratic hopeful, in a speech in Pontiac, Michigan, announced his position on the issue.^{68} He favored extending the moratorium for one year. He advocated trying to negotiate a controlled agreement banning nuclear weapon tests in the atmosphere, at high altitudes, and underwater, and tests of weapons of five kilotons yield or more underground. He also proposed that a two-year moratorium should be established on tests of weapons of lesser yields. This period would be used to test Soviet good faith on inspection and for research to improve detection capabilities relating to low yield underground explosions.

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^{65}Ibid., p. 93.

^{66}Ibid., p. 113.


^{68}Ibid., October 31, 1959, p. 3.
III
Technical Working Group II

The Resumption of the Geneva Conference: Another Mission for the Scientists

The Geneva Conference resumed on October 27. For a week there was no change in the position of either side. The United States continued to insist that the new seismic data had to be considered, and the USSR, with equal adamancy, continued to refuse. Tones were moderate, but that was the only evidence of the spirit of Camp David. On November 2, Ambassador Wadsworth indicated that, if there were no change in the Soviet position, the United States would begin a unilateral presentation of the technical situation, as Americans understood it. 69

Mr. Tsarapkin—at least for the record—interpreted this as a threat to deadlock and possibly terminate the conference. Nevertheless (or perhaps, as a result), the following day, he proposed that the conference convene a technical working group to draft agreed criteria for the dispatch of on-site inspection terms and allowed that the "new seismic data" could be examined and considered within this context. 70 In making his proposal he pointed out that the Foreign Ministers had agreed some time ago that the question of criteria would have to be considered. He also proclaimed that the USSR could not agree to any revision of the Geneva Experts' report, which it viewed as the "agreed scientific and technical basis for drafting the treaty." 71 Finally, he questioned what the objectives of the Western powers were, if, as they claimed, they did not seek to increase the number of control posts or inspections.

Although the shift in the Soviet position, publicly announced as a concession to the spirit of Camp David, meant that the deadlock was broken, three weeks elapsed before the Conference could agree on terms of reference for Technical Working Group II (TWG II). Agreement was finally achieved on November 24, the day that most of the experts arrived in Geneva, and the day before they formally began their work. The central controversies concerned the status of the report of the Conference of Experts and

69 GEN/DNT/PV. 131, passim.
70 GEN/DNT/PV. 132, pp. 16-18.
71 Ibid., p. 13.
the freedom to be given the scientists in considering the new seismic data. Although the compromise formulation did not mention the new seismic data, the United States felt that it was worded so that they could be considered.\textsuperscript{72}

The Technical Working Group of Experts shall consider the question of the use of objective instrument readings in connexion with the selection of an event which cannot be identified by the international control organ and which could be suspected of being a nuclear explosion, in order to determine a basis for initiating on-site inspections. As part of their work, the experts, proceeding from the discussions and the conclusions of the Geneva Conference of Experts, shall consider all data and studies relevant to the detection and identification of seismic events and shall consider possible improvements of the techniques and instrumentation.\textsuperscript{73}

In addition, at Soviet insistence, the terms of reference included the requirement that the Group should report to the Conference by December 11; that is, in about two and a half weeks.

The note of optimism generated by this agreement was furthered by an event which occurred at about the same time outside of the framework of the Geneva Conference. After many years of disputation, on December 1, 1959, twelve states including France, the USSR, the United Kingdom, and the United States, after a brief negotiation of less than seven weeks, signed the Antarctica Treaty.\textsuperscript{74} Among other things, the Treaty provided for the demilitarization of Antarctica, with full unilateral rights of inspection, and for a ban on nuclear explosions and the dumping of radioactive wastes there. During this same period the United States and the USSR also reached agreement on the composition of the United Nations Committee on the Peaceful Uses of Outer Space. Previously, the Soviet bloc had boycotted the Committee because of disagreement on this issue. Observers began to proclaim that perhaps the meeting à deux between Chairman Khrushchev and President Eisen-

\textsuperscript{72}See GEN/DNT/PV. 137, p. 14.
\textsuperscript{73}GEN/DNT/PV. 137, p. 14.
\textsuperscript{74}For a brief account of the negotiations and the issues involved see Howard J. Taubenfeld, "A Treaty for Antarctica," \textit{International Conciliation}, Vol. 531 (January 1961), pp. 245-322.
hower had significantly altered the climate and atmosphere of international politics.

**TWG II: Differences of Motivation and Expectation**

Even within the Geneva negotiations significant progress was made. On November 30 the three powers reached agreement on Annex III to the draft treaty, setting forth the functions of the Preparatory Commission, and on December 14 the Soviet Union tabled a major compromise package proposal concerning a number of unresolved issues relating to the Control Organization.

Whether or not the Western participants in Technical Working Group II were optimistic concerning their tasks, though, was another matter. In fact, it is far from certain that they, or the Western politicians and diplomats to whom they were responsible, had a very clear concept of what the optimum or even expected outcome of this meeting would be. No one seriously expected the Soviet Union to accept the new Western assessment of the capabilities of the control system proposed by the Conference of Experts. Yet the basic instruction given to the American delegation, which was headed by James B. Fisk and included a number of other scientists who had previously been involved in one way or another in the negotiations,75 was to attempt to straighten out the technical situation.

In a press conference on November 12, Secretary of State Herter said that the United States sought the meeting "so that from the scientific point of view we would have a common understanding

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75 The United States delegation consisted of: James B. Fisk, chairman, Executive Vice President, Bell Telephone Laboratories; Hans A. Bethe, Professor, Cornell University; Harold Brown, Associate Director, Livermore Laboratory; Richard Foose, Stanford Research Institute; Richard L. Garwin, International Business Machines Corporation; Spurgeon Keeny, Jr., Technical Assistant, Office of the Special Assistant to the President for Science and Technology; Albert Latter, Physics Division, RAND Corporation, Santa Monica, California; J. Carson Mark, Director, Theoretical Division, Los Alamos Scientific Laboratory; Jack E. Oliver, Lamont Geological Observatory; Wolfgang K. H. Panofsky, Director, High Energy Physics Laboratory, Stanford University; Frank Press, Director, Seismological Laboratory, California Institute of Technology; Carl F. Romney, Assistant Technical Director, Office of Atomic Energy, Department of Defense; John Tukey, Princeton University; Anthony L. Turkevich, Enrico Fermi Institute for Nuclear Studies, University of Chicago.
The Search for Technical Agreement

as to what existing instruments were capable of doing from the point of view of detecting." 76 Twelve days later, in another press conference, he described the purpose of the technical working group as being to examine all available data and "to determine whether or not existing technological instrumentation is adequate to detect all types of underground tests or only some underground tests, and what kind, and what improvements are likely to be made in the instrumentation itself." 77 He allowed that the scientists might not agree and even envisaged the possibility of separate reports. Secretary Herter then went on to say that it would be difficult for the United States to agree to a comprehensive test ban "if our scientists, in their objective judgment, felt that the instrumentation that might be available was not good enough to be an effective deterrent from the point of view of inspection." 78 His comments indicate the range of expectations which was possible for one individual. Given the different perspectives of other individuals, one can easily see the melange of views which was involved in any discussion of this issue by the Committee of Principals.

In a sense, the American insistence on holding Technical Working Group II was a product of the divergence of opinion within the United States government and of the inability of the government to reach a decision on the basic political issues of what risks it would be willing to accept. Those who questioned the wisdom of attempting to negotiate a comprehensive test ban could see the working group as an opportunity to embarrass the Soviet Union; the disagreement which they expected to result would serve as a justification for shifting to an attempt to negotiate a partial test ban or even as a rationale for breaking off the negotiations. On the other hand, the proponents of a comprehensive test ban could see the technical working group as a device for keeping the negotiations going and could hope that some solutions to the technical problems would emerge, either from the meeting with the Eastern scientists or from time and research. Somewhat later, in describing his expectations before the Subcommittee on Disarmament, Philip J. Farley of the Department of State said: "It was,

78 Ibid., p. 865.
of course, our hope that the facts would be looked at there, that answers would be found that would provide a technical basis for concluding a comprehensive agreement." 79 For both groups and for those who were not strongly identified with either, the technical working group could serve as an excuse for avoiding decisions on the basic issue concerning the degree of risk that the United States could tolerate.

The Soviet Union viewed its agreement to hold Technical Working Group II as a fundamental concession to the West. Thus the Soviet scientists, who were again headed by Dr. Fedorov, 80 apparently had no firm instructions in a positive sense as to what should emerge from the discussions. They did, however, as will become apparent, have firm instructions as to what should not emerge. In essence, they could not agree to any joint report which would make the problem of detecting and identifying nuclear explosions appear to be more difficult than it had seemed at the time of the Conference of Experts in 1958.

As in the case of the previous technical negotiations, there was no special preparation for the American delegation. The Chairman was briefed by the Secretary of State, but the other scientists went directly to Geneva. Their technical preparation consisted of the knowledge which they had as a result of their individual work and their past experience in matters relating to the negotiations. The Vela Project was just getting underway, and so far as the data gained from Hardtack II was concerned, there had been little further analysis of it during 1959, after the preparation of the document which the United States submitted to the Conference on January 5. 81

Again, following the pattern of other sessions, Technical Working Group II opened with a disagreement about the agenda. The

80 The other Soviet scientists were K. Y. Gubkin, V. I. Keilis-Borok, D. R. Pashchink, Y. V. Riznichenko, M. A. Sadovsky, V. Shustov, and A. I. Ustyumenko.
81 See the testimony of Wolfgang K. H. Panofsky before the Joint Committee on Atomic Energy: Hearings: Technical Aspects ... of a Nuclear Weapons Test Ban, supra note 26, p. 69.
controversy really derived from the ambiguity of the terms of reference. The central issue, although it was not stated this baldly at that time, was whether or not TWG II should reevaluate the capability of the control system recommended by the Conference of Experts. In the Western view this was the essential function of the discussions. On the other hand, this was one of the things to which the Soviet scientists could not agree. Although a compromise formulation for the agenda was achieved in an informal meeting, it essentially glossed over the problem, and this fundamental disagreement continued to plague the discussions.82

Where Science Ends and Politics Begin

Although the basic reason for the Soviet refusal to agree to a reevaluation of the Experts' control system was to avoid undercutting the USSR's negotiating posture, the Soviet scientists adduced technical and philosophical reasons to support their position. Dr. Fedorov argued that even with the data gained from the Hardtack II experiments, because the data were still extremely limited, any estimate of the capability of the control system would be, as it had been in 1958, conjectural in nature; that the capability could only be properly assessed when the control system was in operation.83 He also, as will be shown, attacked the validity of the new data.

The Western position, on the other hand, was that the assessment had to be as up-to-date as possible. This position was advanced as having no political motivation; as being simply a demand that the Working Group should provide the governments concerned with, as Dr. Fisk put it, "a sound basis for taking the decision which they must take."84 In the discussions, Dr. Fisk repeatedly asserted, at times almost plaintively:

82See the agenda GEN/DNT/TWG. 2/PV. 2, p. 3. For the different way in which this agenda was interpreted by the two sides see: GEN/DNT/TWG. 2/PV. 8, p. 37; GEN/DNT/PV. 11, p. 77; GEN/DNT/PV. 13, pp. 58-60; GEN/DNT/TWG. 2/PV. 16, pp. 93-95. Panofsky commented on this problem in his testimony before the Joint Committee on Atomic Energy, Hearings: Technical Aspects . . . of a Nuclear Weapons Test Ban, supra note 26, p. 64.

83He made this point in several of the exchanges cited above. See also GEN/DNT/TWG. 2/PV. 10, p. 42, and GEN/DNT/TWG. 2/PV. 18, p. 31.

84GEN/DNT/TWG. 2/PV. 13, pp. 58-60.
I assure Mr. Fedorov once again that our sole purpose in these meetings is to set the scientific and technical facts straight. . . . Our motives are purely technical, and the spirit of our proposals is scientific. 85

But regardless of how the Western scientists perceived their motivations, their position had fundamental political ramifications. This was particularly true in that the majority among them at that time thought that the decoupling theory involved a much more serious degradation of the control system recommended by the Conference of Experts than the new seismic data and a degradation for which they could see no effective remedy. The implications of this were ominous when coupled with such statements as that made by Secretary of State Herter at his press conference the day before the Working Group began its sessions. Both in the discussions in the Working Group and in the Conference, Soviet delegates alleged that the West was motivated by a desire to prove that a comprehensive test ban could not be adequately monitored. 86 The fact that after the discussions were over the United States sent the USSR part of the terms of reference of the American delegation to TWG II may or may not have convinced Soviet leaders that this was not the American goal.

From their actions, it is easy to infer that the Soviet delegation had political instructions and guidance. They apparently could not agree to: anything that would indicate that the control system recommended by the Conference of Experts was less effective than the original estimate; anything that would imply that control over a comprehensive test ban would be impossible; or anything that would imply that there should be a greater number of control posts or on-site inspections in the USSR than originally had been thought necessary.

While the United States scientists did not have political instructions in the same sense, there were clearly political limits on their actions, although these may well have been self-imposed. 87

85GEN/DNT/TWG. 2/PV. 12, pp. 36, 37-40.
86See for example, Fedorov's statement in the Working Group and Tsarapkin's statement later in the Conference: GEN/DNT/TWG. 2/PV. 19, p. 92, and GEN/DNT/PV. 148, p. 7.
87Gilpin puts it even more bluntly, "Little did the American scientists realize, however, that the Russian scientists were actually no more political
The American scientists were clearly aware of how important the concept and issue of control over arms control agreements were to the United States. As a group, they were also much more aware of the legal and political ramifications of any document that might emerge from their discussions than the American delegation to the Conference of Experts had been. The American scientists also now had a conception of Soviet behavior which led them to believe that in formulating any joint document everything should be developed in as much detail as possible. They felt that if it were not, there would be disputes subsequently and, moreover, once an agreement was signed, nothing new could be added. As a consequence of these factors, in evaluating and analyzing data, the American scientists felt that they had to be, as Dr. Fisk told the Conference, "as careful and as conservative as we could." 88

At times, as a result of these factors the American posture became almost grotesque. For example, in a discussion of instrumentation Hans Bethe made this statement:

We believe that the experts in 1958 knew quite well about instruments, but we believe that we now know even better about them. We do not believe that the experts of the control commission will know any better than we know. 89

To argue in abutting sentences that knowledge had changed, but would not change again, posed certain logical complications, at the least.

On the other hand, because of the political position of their state, the Soviet scientists had to be as optimistic as they could. Dr. Fedorov typified their attitude at one of the final meetings, during a discussion of formulating criteria for the initiation of on-site inspections, when he asked, "What kind of scientists are we if we cannot find a solution for such a problem?" 90

There was also another difference between the nature of the

than they." (American Scientists and Nuclear Weapons Policy, p. 243). Although this may be true in terms of the end effect of the action of each group, there were important differences in method and style, which should not be ignored.

88GEN/DNT/TWG. 2/PV. 20, p. 4.
89GEN/DNT/TWG. 2/PV. 12, p. 16.
90GEN/DNT/TWG. 2/PV. 19, p. 91.
participation of the American and Soviet scientists in the Technical Working Group. The United States delegation presented by far the bulk of the empirical data which was considered. The Soviet contribution was principally in the nature of critical analysis.\(^91\) There is no way of knowing how open either side was in terms of presenting data, or whether or not all of the available data were put before the Group. During the course of the discussions the United States did submit a vast quantity of data, and the USSR did make available seismograms with signals recorded during the Hardtack series. Dr. Fedorov stated that the USSR had not carried out any underground nuclear explosions and thus could not supply data beyond that which it had gathered on American tests.\(^92\)

In terms of formal participation, the British scientists, who were again headed by Sir William Penney,\(^93\) played only a nominal role. Because of their limited experience, they could not add much empirical data. At that time the United Kingdom had not detonated a nuclear device underground. On the few occasions on which he spoke, Sir William Penney emphasized the importance of a test ban and, consequently, of attempting to reach agreement within the Working Group. This reflected both his personal preferences and the official attitude of the United Kingdom.

*On the Hardtack II Data: An Uncomfortable Uncertainty*

Carl Romney, a seismologist with the United States Air Force, made the first technical presentation in Working Group II.\(^94\) In it, he expounded the American analysis of the Hardtack II data. Although he presented more technical details, his basic conclusions were identical with those contained in the January 5 Working Paper; that is, that the magnitude of Rainier had been estimated incorrectly and consequently that there were many more earthquakes of equivalent size, and that first motion was more difficult to detect than it had previously been thought. The decoupling theory was


\(^{92}\)GEN/DNT/TWG. 2/PV. 3, pp. 103-5.

\(^{93}\)The other British delegates were H. R. Hulme, I. Maddock, F. Panton, and J. W. Wright.

\(^{94}\)GEN/DNT/TWG. 2/PV. 1., pp. 22-56.
The immediate Soviet response to both presentations was of a legalistic character. With respect to the Hardtack II data, the Soviet scientists argued that since the instrumentation used was not identical with that which had been recommended in the report of the Conference of Experts, no implications could be drawn for the recommended control system. With respect to the decoupling theory, Dr. Fedorov questioned whether it could be considered under any item of the agenda.\textsuperscript{96} After Hans Bethe and Albert Latter had presented the theory, he caustically commented that:

\ldots the contribution to our work which Dr. Latter tried to make in his report is quite similar to the contribution that he made previously to this problem; I mean the book that he published on the subject along with Dr. Teller.\textsuperscript{97}

Later in the conference, he would mention the book, \textit{Our Nuclear Future}, in even more bitter terms.

When the Soviet scientists began to comment on the substance of the issues, they did so by way of the presentation of formal papers which countered and contradicted the American findings. The American scientists professed to be puzzled and upset by the disagreement. At the tenth meeting Wolfgang Panofsky commented:

It just should not occur that objective examination and a full exchange of seismic data should lead our Soviet colleagues to one conclusion while the conclusions of our own seismologists are different.\textsuperscript{98}

At the following meeting Frank Press repeated the same sentiment and then offered the explanation and recommended solution which most of the American scientists seemed to favor:

As scientists we know that given the same basic data we should be able to arrive at similar conclusions, and yet

\textsuperscript{95}GEN/DNT/TWG. 2/PV. 7, pp. 58-110.
\textsuperscript{96}Ibid., pp. 55-56.
\textsuperscript{97}Ibid., p. 111.
\textsuperscript{98}GEN/DNT/TWG. 2/PV. 10, p. 4.
we have not been able to. This can only be because the communication and interchange between us has been imperfect. The only way to make progress, to resolve these differences, is to improve the communication, to study the data jointly step by step. 99

The solution, the American scientists thought, would be small informal meetings, and they began arguing for such meetings at an early stage in the conference. The Soviet scientists, however, did not agree to this procedure until the final week of the meetings.

Even after the informal sessions, several fundamental disagreements remained, although some differences were eliminated and others narrowed. With respect to the Hardtack II data, the Soviet scientists tended to concentrate their attention and criticisms on the problem of measuring seismic magnitude. They made a number of what can be termed procedural criticisms. They were critical of the fact that less than thirty of the seismological stations in the United States which had recorded Logan and Blanca had had seismographs which were sufficiently calibrated so that magnitude could be measured. They also criticized the fact that the American scientists gave them one relevant seismogram for study as late as December 14, during the sixteenth meeting of the Working Group.

Their more fundamental substantive criticisms centered on the statistical methods used in the computation of seismic magnitudes and on the use of magnitude scales. To deal with the issue of statistical methods first, the relevant empirical data were widely scattered. The question was whether in the computation of averages all of the figures should be used, as the Americans insisted, or whether the extremes and certain other figures should be excluded as the Russians argued. The second issue involved the question of how to relate two different scales of measurement. Some American scientists felt that what their Soviet counterparts did in this matter was unscientific and dishonest. 100

There is evidence, however, that a number of American scientists felt rather uncomfortable with respect to the whole problem of measuring the seismic magnitude of nuclear explosions because

99GEN/DNT/TWG. 2/PV. 11, pp. 87-90.
of the great uncertainties involved. Wolfgang Panofsky put it succinctly in testimony in April 1960 before the Joint Committee on Atomic Energy when he stated that:

... the matter is greatly beclouded by the inaccuracy of our seismic information concerning earthwide occurrence of small earthquakes. Specifically, a body of information on the frequency of occurrence of small earthquakes exists only for California and New Zealand, and going from this information to worldwide estimates, and particularly to estimates for the Soviet Union, involves many uncertainties; these uncertainties are in fact considerably larger than change in the situation created by the new data of the Hardtack series. This issue is, therefore, not an important new consideration, and it now appears that probably its importance has been overemphasized in the United States. ¹⁰¹

Because of these facts, many of the American scientists participating in TWG II concluded that the question of the number of on-site inspections required was essentially a political issue, as the USSR maintained. During the meetings of the Working Group, on several occasions Dr. Fisk sought to emphasize that the American delegation did not consider this aspect of the Hardtack II data the most important element.

Nevertheless, the differences between the Soviet scientists and the American scientists with respect to the problem of measuring seismic magnitude led the former to interpret the Hardtack II data as indicating that there would be fewer earthquakes which would give signals equivalent to any given size nuclear explosion rather than more, as the Americans felt would be the case. ¹⁰²

Rather than their revised estimate of the seismic magnitude of the Rainier shot, the American scientists tended to stress instead the fact that the Hardtack II data indicated that because of background noise it would be much more difficult than had been thought to detect the sign of first motion. They now estimated that the ratio of signal to noise would have to be greater than had previously been thought for the compressional first motion to be

¹⁰¹Hearings: Technical Aspects ... of a Nuclear Weapons Test Ban, supra note 26, p. 65.
detected, that is, for it not to appear as a rarefaction. In other words, first motion would only be useful for detecting substantially higher yields than had been estimated at the Conference of Experts. Here, the principal Soviet criticism was that the instrumentation used in the Hardtack II test series was not identical with that recommended by the Conference of Experts. The Americans countered by arguing that, although the Soviet charge was true, valid extrapolations could be made.

In the course of the argument, in the second week of the meeting it was discovered that the Russian and English versions of the report of the Conference of Experts were different, and that the differences would allow different interpretations of what instrumental characteristics with respect to magnification and response the Experts had recommended. The differing interpretations led the scientists from the two sides to feel that the Conference of Experts had recommended instruments which would conform to those that they had used in their national stations. This episode underscores the importance of proper translation in negotiations.

The American scientists were nonetheless convinced that their interpretation was the correct one; that instruments constructed on the basis of their interpretation would be the most efficacious in detecting first motion; and that the instruments used during the Hardtack II test series fell within the category recommended by the Experts. To prove their point, as soon as the difference was discovered they designed a seismograph based on the Soviet interpretation of the specifications and had it built. On December 9 it was installed on a pier at a location in Oklahoma alongside an instrument similar to those used in the Hardtack II series. In the first eight hours of operation there was a small earthquake, and a comparison of the signals received on the two instruments clearly indicated that the instrument based on the Soviet interpretation was considerably less effective in detecting the sign of the first motion.

Despite this evidence, however, the Soviet scientists continued to

103See Panofsky's comment before the Joint Committee on Atomic Energy: Hearings: Technical Aspects . . . of a Nuclear Weapons Test Ban, supra note 26, pp. 67-68. The only American scientist fluent in Russian at the Conference of Experts, Turkevich, left the meeting before the final texts of the report were prepared.

104See Romney's presentation, GEN/DNT/TWG. 2/PV. 17, pp. 91-96.
maintain that because the instruments used in the Hardtack II series did not conform exactly to those recommended by the Conference of Experts, the data recorded by them were not relevant.\textsuperscript{105}

\textit{On Decoupling: "Deeply Embarrassed"}

Hans Bethe and Albert Latter presented the decoupling theory during the second week of the meeting. Later, Professor Bethe referred to the task as a "doubtful honor" and said that he felt "deeply embarrassed in so doing because it implied that we considered the Russians capable of cheating on such a massive scale."\textsuperscript{106} His views of the Russian character provide an interesting contrast with those expressed by Edward Teller and Albert Latter in their book. For a variety of reasons, Professor Bethe was a logical person to be involved in the task. He had presented the relevant material at the Conference of Experts, and in the course of the discussion he had to admit that his original calculations were "not correct."\textsuperscript{107} It was also tactically expedient to have him share in the presentation, since he, in contrast to Albert Latter, was known as a proponent of a test ban.

Most American scientists who participated in the Working Group agreed with Professor Bethe's comment that "the Russians seemed stunned by the theory of the big hole."\textsuperscript{108} At first they attempted to develop counter theoretical arguments, but after various formal confrontations and informal meetings, they admitted the theoretical validity of Albert Latter's calculations concerning decoupling. They maintained, however, that there was no proof that the theory would work in practice, and, of course, at that time there was very little empirical proof. The British, who had been informed of the theory of decoupling in the summer, had conducted a limited series of experiments involving detonating small charges of TNT in cavities, and they presented the results of these experiments, which tended to confirm the theory, to the Working Group.\textsuperscript{109} The first explosion in Project Cowboy was detonated on

\textsuperscript{105}See \textit{Geneva Conference}, pp. 393-94.
\textsuperscript{107}GEN/DNT/TWG. 2/PV. 8, p. 47.
\textsuperscript{108}"The Case for Ending Nuclear Tests," \textit{supra} note 106, p. 17.
\textsuperscript{109}GEN/DNT/TWG. 2/PV. 11, pp. 32-37.
December 17, 1959, the day before the Working Group recessed, and the project was not discussed during the session.

The question of the feasibility of constructing cavities of sufficient size to allow the decoupled detonation of fairly large sized weapons was not discussed either, although the United States was prepared to go into this matter in some detail. In the fall of 1959 the Atomic Energy Commission had become concerned with this issue and had discovered that petroleum companies used large underground cavities for storing their materials. In October the Philips Petroleum Company had been commissioned to undertake certain feasibility studies, and the individual responsible for this work was brought to Geneva for the week of December 7. His work, however, was confined to assisting Albert Latter and others.

On the On-Site Inspection: à Quoi Bon?

During the course of 1959 another element of doubt, in addition to the implications of the Hardtack II data and the decoupling theory, had arisen in the American scientific community. Starting in April 1958 scientists at the University of California Radiation Laboratory at Livermore had become concerned about the matter of on-site inspection, and after preliminary discussions with geologists and geophysicists, and exploratory studies, the Laboratory let a contract to the Stanford Research Institute for the conduct of studies of the problem of on-site inspection during the Hardtack II test series. These studies indicated that the problem of conducting an on-site inspection would be quite difficult, and that the chance of actually obtaining radioactive debris and thus identifying a clandestine underground explosion would be relatively small. The American delegation was given freedom to introduce or to withhold this issue. Eventually, the delegation decided to introduce the issue, but not to stress it, and perhaps thereby detract from what they considered the more important degradations of the control system caused by the Hardtack II data and the decoupling theory. Dr. Fisk mentioned the problem in the plenary session, the reports

10Earl H. Voss has interpreted this, we think wrongly, as a deliberate attempt to gloss over the difficult problem of on-site inspection (Nuclear Ambush, p. 347). In his book he virtually accuses American scientists of attempting to mislead the American public on the issue of on-site inspections. 11GEN/DNT/TWG. 2/PV. 8, pp. 76-77.
on the work were introduced as an annex to the records of the conference,\textsuperscript{112} and the project director from the Stanford Research Institute communicated his findings to Dr. Fedorov in a private meeting. The only Soviet response was a brief denial of the American analysis and interpretation.\textsuperscript{113} The issue was not mentioned in any of the formal reports of Technical Working Group II.

\textit{On Improving the Control System: What Conclusions from Scientific Data?}

Along with their presentation of the Hardtack II data, the decoupling theory and the information relating to on-site inspection, the American scientists also presented their thoughts concerning possible improvements in the control system recommended by the Conference of Experts. They concerned both instrumental matters and improved diagnostic techniques. Interestingly, one of the more significant of these improvements was discovered by Albert Latter during the meetings of the Technical Working Group and was immediately presented at the next-to-the-last meeting.\textsuperscript{114} On these matters, there was considerable agreement between the Soviet and Western scientists.

What disagreement there was, concerned what practical conclusions to draw from the technical facts. This was illustrated by the attempts of Dr. Fedorov at the conclusion of each American presentation to elicit a formal proposal or recommendation for the use of new instruments or techniques. The American response was invariably that the state of knowledge was not sufficient to allow definite formulations, but that the presentations indicated lines of further inquiry. After one such exchange, Dr. Fedorov exclaimed, "... this is not a congress of seismologists. ... What we are interested in here ... is how this scientific contribution can be used in practice to improve the control system. ..."

\textsuperscript{115} The American scientists, however, were determined not to become involved in the elaboration of any formal texts which their scientific data and understandings could not fully support.

\textsuperscript{112}GEN/DNT/TWG. 2/PV. 9/Add. 1.
\textsuperscript{113}GEN/DNT/TWG. 2/PV. 11, pp. 51-61.
\textsuperscript{114}GEN/DNT/TWG. 2/PV. 20, pp. 31-36.
\textsuperscript{115}GEN/DNT/TWG. 2/PV. 4, p. 82.
Despite this disagreement, the Technical Working Group was able to produce an agreed report regarding possible improvements of techniques and instrumentation. In keeping with their general positions, in the preparation of this report the American scientists sought to be much more specific than the Soviet scientists.

On Criteria for On-Site Inspection: Can One Agree Without Agreement on Data?

The final substantive item on the agenda of Technical Working Group II was the formulation of objective criteria which could serve as a basis for the initiation of on-site inspections. From the outset, the American position was that there was no point in attempting to discuss this matter until there was agreement on the technical data; for example, on the effectiveness of first motion as a diagnostic technique. As it developed, the discussion of criteria began without agreement on the technical data. Indeed, there was no agreement on the data at the time that the conference adjourned.

The Soviet scientists led off at the eighth meeting on December 3 by tabling a draft proposal relating to criteria. The Americans objected to this proposal, first because they felt that it contained a number of judgments which were concerned with matters beyond the purview of the technical working group. For example, it assumed that there would be a quota of on-site inspections and that data from national seismic stations would play an important role. Moreover, it also contained stipulations on the circumstances under which on-site inspections should be discontinued. Among other things, the Soviet draft provided that if the epicenter of an unidentified event were located in a densely populated area, or if its depth of focus were beyond technological possibilities, it would be considered to be an earthquake. The American scientists felt that both of these involved political, rather than technical, considerations. However, the more important American objections to the Soviet criteria were of a technical nature. The American scientists felt that the Soviet criteria would have resulted in clandestine nuclear explosions being categorized as earthquakes: in par-

117GEN/DNT/TWG. 2/PV. 8, pp. 87-92.
118See Fisk's comments: GEN/DNT/TWG. 2/PV. 13, p. 22.
ticular, they argued that the Blanca and Logan experiments would have been so classified.\textsuperscript{119}

The American proposal on criteria was introduced on December 11.\textsuperscript{120} Soviet objections to this proposal were equally as strong as the American objections to the Soviet draft. Dr. Fedorov commented that the criteria proposed by the United States scientists turned the principle of selection "topsy turvy," the result would be "that from the total number of recorded events the greater part of them will remain open to suspicion [and thus eligible for on-site inspection] and the smaller part would be free from suspicion."\textsuperscript{121} He asserted that this was contrary to the conclusions of the Conference of Experts. The Americans agreed with his conclusions, but argued that with the present state of knowledge, they could not go farther. In the context of this debate, Dr. Fedorov remarked that science had solved more complicated problems and ought to be able to solve this one. He went on to assert that "... the purpose we have as scientists is to try to help our political officers to identify suspicious events."\textsuperscript{122} Dr. Fisk rejoined, "... science is not the servant of political expediency."\textsuperscript{123}

The American draft did list various types of auxiliary information, which it was stated might develop into criteria "to establish an event as natural in origin" at some later time after further research. Wolfgang Panofsky again summed up this problem a few months later in his testimony before the Joint Committee on Atomic Energy:

The principal problem is that these methods are potentially useful, but within the present state of knowledge there is no way of evaluating quantitatively how useful they could be nor is it possible to write specific recommendations under which they could be used by unskilled operators. In the hands of skilled seismologists, even at

\textsuperscript{119}See Fisk's comments: GEN/DNT/TWG. 2/PV. 12, pp. 57-66; and Hearing: Technical Problems and the Geneva Test Ban Negotiations, supra note 79, p. 7. See also Panofsky's comments before the Joint Committee on Atomic Energy: Hearings: Technical Aspects . . . of a Nuclear Weapons Test Ban, supra note 26, p. 72.

\textsuperscript{120}GEN/DNT/TWG. 2/PV. 14, pp. 3-21.

\textsuperscript{121}GEN/DNT/TWG. 2/PV. 15, pp. 27-30.

\textsuperscript{122}GEN/DNT/TWG. 2/PV. 19, p. 76.

\textsuperscript{123}Ibid., p. 81.
the present the use of some of these methods might very well improve the judgment which can be exercised in distinguishing earthquakes from explosions.\textsuperscript{124}

As his comments indicate, in addition to being worried about the state of knowledge and the legal and political status of their report, the American scientists were concerned about the technical competence of the personnel that would staff the control posts, and they tended to assume, probably rightly, a relatively low level of competence.\textsuperscript{125}

Although both sides made some compromises, the differences in approach proved irreconcilable, and Technical Working Group II failed to agree on criteria for initiating on-site inspections. Interestingly, the Soviet scientists did not include their proposed criteria in their report to the Conference. Whether or not the Soviet criteria were intended as a serious proposal or as a bargaining position was moot. The Soviet proposal was logically defensible only in the light of the Soviet criticisms of the measurements of the Hardtack II series, and especially the criticism of the instruments which had been used.

Although submission of an agreed report to the Geneva Conference seemed to be out of the question in view of the basic divisions between the Eastern and Western scientists, all three groups of scientists, for their own reasons, strongly wanted an agreed report, and an attempt was made. Dr. Fisk even suggested the possibility of reconvening the Working Group in January 1960 after a Christmas recess,\textsuperscript{126} but in the last analysis the scientists recognized that their differences could not be resolved by further meetings.

The attempt to write an agreed report was complicated by the fact that the scientists knew that the report of Technical Working Group I had been given immediate public release and they were concerned about the effect of any report on public opinion. In

\textsuperscript{124}Hearings: Technical Aspects . . . of a Nuclear Weapons Test Ban, supra note 26, p. 70.

\textsuperscript{125}See also Panofsky's comments before the Subcommittee of the Committee on Foreign Relations, Hearing: Technical Problems and the Geneva Test Ban Negotiations, supra note 79, pp. 9-10.

\textsuperscript{126}GEN/DNT/TWG. 2/PV. 20, p. 62.
addition, neither side wanted to have a report prejudge issues which were still in contention in the Conference. The American objections to the Soviet proposals on criteria have already been mentioned. Similarly, the Soviet scientists objected to American proposals relating to experimental explosions designed to yield data that would be helpful in improving the control system.\(^{127}\)

In the end, the many divisions proved insurmountable. The scientists could agree only on a brief procedural report to which four annexes were attached.\(^{128}\) The first of these contained the agreed recommendations on improvements, and the remaining three, the separate views of the three delegations. The British report basically agreed with the American conclusions, except in the matter of magnitude and the number of equivalent earthquakes.

IV

Deadlock

Scientists Disagree: Back to the Diplomats

Technical Working Group II recessed after twenty-one meetings at 9:20 p.m. on Friday, December 18. The following afternoon the report and the annexes were presented to the diplomatic Conference.\(^{129}\) The meeting was acrimonious. Dr. Fedorov’s statement, which Dr. Fisk subsequently labeled “incorrect, distorted, and misleading,”\(^{130}\) attacked the scientific integrity of the American delegation, among other things. The American scientists were deeply distressed by this,\(^{131}\) and by the general denouement. In addition to the substantive issues at stake, many of them felt that in the process of the discussion personal relationships with Soviet scientists, carefully built up over a number of years, had been jeopardized.

The outcome of Technical Working Group II meant that the search for agreement between East and West on a technical level had failed. The closing speeches of the diplomatic representatives

\(^{127}\)GEN/DNT/TWG. 2/PV. 21, p. 6.
\(^{128}\)See Geneva Conference, pp. 384-413.
\(^{129}\)GEN/DNT/PV. 150.
\(^{130}\)Ibid., p. 16.
\(^{131}\)See the comments by Panofsky and Fisk, Hearing: Technical Problems and the Geneva Test Ban Negotiations, supra note 79, pp. 13, 35.
in the Conference, indicated that each side saw the other’s action as precluding agreement and revealed the prevailing mutual suspicions concerning motivations. Mr. Tsarapkin went so far as to charge that there were forces trying to prevent the conclusion of a comprehensive agreement. The West clearly felt that the USSR was evading the issue of control.

Perhaps the British delegate, Sir Michael Wright, sounded the most optimistic note. He argued that the principal difficulties were caused “by the comparative scarcity of firm experimental data and by the lack of time thus far for research directed to our special problems.” He expressed the opinion that a solution might well be found through research, “jointly undertaken.” For the moment, however, the negotiations had reached an impasse.

ibid., p. 22.