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DEVELOPMENTS IN THE FIELD OF DETECTION AND IDENTIFICATION OF NUCLEAR EXPLOSIONS (PROJECT VELA) AND RELATIONSHIP TO TEST BAN NEGOTIATIONS

HEARINGS

BEFORE THE

JOINT COMMITTEE ON ATOMIC ENERGY

CONGRESS OF THE UNITED STATES

EIGHTY-SEVENTH CONGRESS

FIRST SESSION

ON THE

DEVELOPMENTS IN THE FIELD OF DETECTION AND IDENTIFICATION OF NUCLEAR EXPLOSIONS (PROJECT VELA)

JULY 25, 26, AND 27, 1961

Printed for the use of the Joint Committee on Atomic Energy

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DEVELOPMENTS IN THE FIELD OF DETECTION AND IDENTIFICATION OF NUCLEAR EXPLOSIONS (PROJECT VELA) AND RELATIONSHIP TO TEST BAN NEGOTIATIONS

TUESDAY, JULY 25, 1961

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The Joint Committee met at 10 a.m., pursuant to notice, in room P–63, the Capitol, Hon. Chet Holifield (chairman of the Joint Committee) presiding.

Present: Representatives Holifield, Price, Aspinall, Morris, Van Zandt, Hosmer, Bates, and Westland.

Also present: James T. Ramey, executive director; John T. Conway, assistant director; George F. Murphy, Jr., professional staff member; Richard T. Lunger, staff consultant; and Doyle N. Northrup, technical consultant, Joint Committee on Atomic Energy.

Chairman HOLIFIELD. The committee will be in order.

In order to clarify the basis of a test ban control system, the Joint Committee, through its special Subcommittee on Radiation and Subcommittee on Research and Development, held 4 days of public hearings in April of 1960 on the "Technical Aspects of Detection and Inspection Controls of a Nuclear Weapons Test Ban." These hearings covered the entire scope of nuclear test detection, inspection and control, including the basis of the original proposed Geneva control system.

The primary objective of our hearings commencing this morning is to bring up to date the progress made in the methods of detection and identification of underground and high altitude nuclear explosions since the hearings in April 1960.

I should like to quote from the summary-analysis of our April 1960 hearings:

> It appears from the testimony that at least for the next several years it will not be possible to identify underground events whose seismic signals record the equivalent of a nonmuffled nuclear explosion of 20 kilotons or less, although they may be detected. Further, it appears that for this same time period it will not be possible to detect muffled tests of 100 kilotons or more set off deep underground in large cavities. Therefore, it seems that for the next several years and possibly thereafter there could be a race between improved means of detection and identification as against improved means of concealing and muffling nuclear tests.

The committee will be most interested to see if this estimate is still valid and in being brought up to date on the status of the race between detection and concealment.

The Department of Defense has primary responsibility for seeking ways to improve the detection system. The code name for this project

is Vela. The project is divided into three major portions. The underground portion of the program is called Vela Uniform, while Vela Sierra is the designation for the surface-based detection of high altitude detonations. That portion of the program for the space-based detection system is called Vela Hotel. The Atomic Energy Commission is responsible for implementation of that portion of the research program requiring nuclear and large chemical explosions.

Our witnesses will discuss the technical problems connected with the proposed Geneva control system as well as an overall and detailed description and discussion of Project Vela.

On Wednesday afternoon we plan to conduct a panel discussion of the results of seismic research to date. Our panel members are all eminently qualified seismologists who have been closely associated with the seismology research and seismic systems development program aspects. Later in the hearings we will receive testimony on the development aspects in the detection of space tests from ground bases referred to as Vela Sierra and the detection of tests by satellite-borne instruments referred to as Vela Hotel.

In conclusion on the third day, we plan to receive testimony on the role of the detection and inspection research program in relation to the nuclear test ban proposals and negotiations. Ambassador Arthur H. Dean, the U.S. Representative to the Conference on the Discontinuance of Nuclear Weapons Tests, will present that portion of the testimony.

It was our intention to conduct a roundtable discussion on the feasibility and capabilities of a test ban control system at the conclusion of the technical and scientific testimony. The panel members were to have been the same group from the scientific community which the committee selected last year as representing a fair spectrum of opinion in these vital questions. The bringing together of such a group would tend to pinpoint the major differences of opinion which exists today concerning the problem of providing an adequate control system.

Unfortunately, a number of those individuals invited declined on the basis that they had not been closely associated with the current research and development programs in detection and identification.* Some of the individuals concerned requested the opportunity to review the testimony which we will take in these hearings before participating in any such panel. Accordingly, this panel discussion will be postponed until sometime in the future.

I would like to emphasize once more that the purpose of these hearings is to update the progress made in research in detection and identification of nuclear tests. As in last year's hearings, we shall attempt to avoid policy questions relating to disarmament because of the sensitive nature of certain aspects of the test ban discussions. We do not intend at this time to consider such questions as to whether or when the United States should resume seismic or weapons development nuclear tests.

As in other hearings of this nature, a biographical sketch of each witness will appear at the beginning of their testimony. Also, we will place their prepared statement at the end of their oral testimony.

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*See appendix 9, p. 435, for list of scientists invited to participate in panel discussion.

Without objection I will include in the record, the Joint Committee's announcements of the hearings, together with the proposed outline of the hearings. Also, as an appendix to these hearings will be a glossary which, I believe, should be very helpful to the layman using this publication. (See app. 1, p. 289.)

(The material referred to follows:)

[From the Office of the Joint Committee on Atomic Energy. No. 325. For release Monday, July 10, 1961, p.m. papers]

HEARINGS ON U.S. NUCLEAR DETECTION CAPABILITY ANNOUNCED BY THE JOINT COMMITTEE ON ATOMIC ENERGY

The Joint Committee on Atomic Energy plans to hold public hearings on Tuesday, Wednesday, and Thursday, July 25, 26, and 27, 1961, on developments in the field of detection and identification of nuclear explosions, it was announced today by Congressman Chet Holifield, chairman of the Joint Committee on Atomic Energy.

These hearings will be held in open session in room P–63 (Old Supreme Court Chamber), the U.S. Capitol Building. Witnesses will be called from the Department of Defense, the Atomic Energy Commission, their laboratories and other appropriate agencies. Also qualified individuals from outside the Government may be called.

Particular emphasis will be placed on progress made in the methods of detection and identification of underground and high-altitude nuclear explosions, since the committee last held public hearings on the subject in 1960, according to Chairman Holifield. In making the announcement he stated:

"In April 1960, the Joint Committee held detailed public hearings on 'Technical Aspects of Detection and Inspection Controls of a Nuclear Weapons Test Ban,' at which qualified scientists and technically trained individuals were invited to discuss all aspects of the problem. In the summary analysis of our April 1960 hearings, the following observation was made:

"It appears from the testimony that at least for the next several years it will not be possible to identify underground events whose seismic signals record the equivalent of a nonmuffled nuclear explosion of 20 kilotons or less, although they may be detected. Further, it appears that for this same time period it will not be possible to detect muffled tests of 100 kilotons or more set off deep underground in large cavities. Therefore, it seems that for the next several years and possibly thereafter there could be a race between improved means of detection and identification as against improved means of concealing and muffling nuclear tests.'

"We will be interested to see if this estimate is still valid; and in being brought up to date on the status of the race between detection and concealment.

"After these hearings the President announced that about $66 million would be spent during fiscal year 1961 to attempt to improve our detection and identification capabilities. The hearings by the Joint Committee, to be held on July 25, 26, and 27, 1961, are to determine to what extent these capabilities have been improved."

The Department of Defense has primary responsibility in seeking ways to improve the detection system under Project Vela. The Atomic Energy Commission is responsible for implementation of that portion of the program requiring nuclear and large chemical explosions. The underground portion of the Defense Department's nuclear detection program is called Project Vela Uniform. The portions of Project Vela which pertain to high-altitude detection are Vela Sierra for the surface-based detection of high-altitude detonations, and Vela Hotel for the space-based detection in the same areas.

The Vela program came into being based on recommendations contained in the report of the President's Panel on Seismic Improvements (Berkner panel) made on March 31, 1959.

[From the Office of the Joint Committee on Atomic Energy, No. 327, for release Monday, July 24, 1961, a.m. papers]

JOINT COMMITTEE ANNOUNCES SCIENTIFIC WITNESSES AND OUTLINE OF PUBLIC HEARINGS JULY 25–27, 1961, FOR NUCLEAR DETECTION

A list of witnesses to appear before the Joint Committee on Atomic Energy and the topics to be covered at the hearings on the developments in the field of

detection and identification of nuclear explosions were announced today by Congressman Chet Holifield, chairman of the Joint Committee on Atomic Energy.

These hearings will be held in open session on July 25, 26, and 27, 1961, in room P–63, the Old Supreme Court Chamber of the Capitol Building.

The Joint Committee first announced these hearings on July 10, 1961. At that time Chairman Holifield stated that in these hearings—

"Particular emphasis will be placed on progress made in the methods of detection and identification of underground and high altitude nuclear explosions since the committee last held public hearings on the subject in 1960."

The committee is particularly interested in whether the progress and prospects of detection and identification of nuclear tests are as favorable as possible improvements in concealment and muffling of such tests.

Chairman Holifield also announced that a panel discussion of representative scientists would be held at the conclusion of the hearings. During the April 1960 hearings on "Technical Aspects of Detection and Inspection Controls of a Nuclear Weapons Test Ban," a panel discussion was held on the capabilities and facilities for monitoring nuclear tests during a moratorium. Consideration was also given to the significance of further clandestine tests. Panel members who testified at the 1960 hearings have been invited to a similar panel discussion at this year's nuclear detection hearings. These include Dr. Hans Bethe, Dr. Alvin Graves, Dr. Carl Romney, and Dr. Edward Teller. Among those invited to this year's panel discussion are Dr. John Foster, director of the Lawrence Radiation Laboratory, Livermore, Calif., and Dr. David Inglis, Argonne National Laboratory.

Chairman Holifield indicated that the schedule is subject to change without further notice.

PROPOSED OUTLINE FOR PUBLIC HEARINGS ON DEVELOPMENTS IN THE FIELD OF DETECTION AND IDENTIFICATION OF NUCLEAR EXPLOSIONS (PROJECT VELA), JULY 25, 26, AND 27, 1961

Tuesday, July 25, 1961—Morning session: 10 a.m.; location, room P–63, the Capitol Building

I. Introduction and General Discussion.
   Witnesses:
      Dr. J. P. Ruina, Director, Advanced Research Projects Agency (ARPA), Department of Defense.
      Brig. Gen. A. W. Betts, Director, Division of Military Application, U.S. Atomic Energy Commission.

II. Technical Problems Connected With the Proposed Geneva Control System.
   Witness:
      Dr. Richard Latter, the Rand Corp.

III. Overall Description and Discussion of Project Vela.
   Witness:
      Mr. Carlton M. Beyer, Advanced Research Projects Agency (ARPA), Department of Defense.

Tuesday, July 25, 1961—Afternoon session: 2 p.m.; location, room P–63, the Capitol Building

IV. Detection of Underground Tests (Vela Uniform):
   A. Research in Seismology.
      Witness:
         Dr. Charles Bates, Advanced Projects Research Agency (ARPA) Department of Defense.
   B. Seismic Systems Development.
      Witness:
         Dr. Carl Romney, Air Force Technical Applications Center (AFTAC).
   C. Program of Underground Nuclear and Chemical Explosions:
      1. Requirements.
         Witness:
            Mr. Theodore A. George, Advanced Projects Research Agency (ARPA) Department of Defense.

Wednesday, July 26, 1961—Morning session: 10 a.m.; location, room 362, Old House Office Building

2. AEC Program:
   (*a*) Explosion Programs.
   Witness:
   Brig. Gen. A. W. Betts, Director, Division of Military Applications, U.S. AEC.
   (*b*) Related Programs.
   Witness:
   Dr. Roland Herbst, Lawrence Radiation Laboratory, Livermore.
3. DASA Program.
   Witness:
   Col. Jack Ledford, USAF, Headquarters, Defense Atomic Support Agency (DASA).
4. Measurements and Analysis.
   Witness:
   Dr. Carl Romney, Air Force Technical Applications Center (AFTAC).

Wednesday, July 26, 1960—Afternoon session: 2 p.m.; location, room 362, Old House Office Building

D. On-site Inspection.
   Witness:
   Dr. Charles Bates, Advanced Projects Research Agency (ARPA), Department of Defense.
E. Round Table Discussion of the Results of Seismic Research.
   Panel participants:
   Dr. Hugo Benioff, Seismological Laboratory, California Institute of Technology.
   Dr. Dean S. Carder, Chief Seismologist, U.S. Coast and Geodetic Survey.
   Dr. Norman Haskell, Air Force Cambridge Research Laboratory.
   Dr. Jack E. Oliver, Columbia University, Lamont Geological Observatory.
   Dr. Carl Romney, Air Force Technical Applications Center.
   Dr. James T. Wilson, Chairman, Department of Geology, University of Michigan.

V. Detection of Space Tests From Ground Bases (Vela Sierra):
   A. General Discussion.
   Witness:
   Lt. Comdr. Donald Chandler, USN, Advanced Research Projects Agency (ARPA), Department of Defense.
   B. Work by Los Alamos Scientific Laboratory.
   Witness:
   Dr. William Ogle, Los Alamos Scientific Laboratory.

Thursday, July 27, 1961—Morning session: 10 a.m.; location, room P–63, the Capitol Building

VI. Detection of Tests by Satellite-borne Instruments (Vela Hotel).
   A. General Discussion.
   Witness:
   Maj. John Poulsen, USAF, Advanced Research Projects Agency (ARPA) Department of Defense.
   B. Detection Instruments.
   Witness:
   Dr. Harold Argo, Los Alamos Scientific Laboratory.
   C. Satellite Systems.
   Witness:
   Dr. Byron P. Leonard, Space Technology Laboratory.

Thursday, July 27, 1961—Afternoon session: 2 p.m.; location, room P–63, the Capitol Building

VII. Role of the Vela Program in Relation to the Nuclear Test Ban.
Witness:
Ambassador Arthur H. Dean, U.S. Representative to the Conference on the Discontinuance of Nuclear Weapon Tests.

Chairman HOLIFIELD. Our first witness this morning is Dr. Jack Ruina, Director of the Advanced Research Projects Agency.

Dr. Ruina, will you please come forward?

STATEMENT OF DR. J. P. RUINA,[1] DIRECTOR, ADVANCED RESEARCH PROJECTS AGENCY

Dr. RUINA. Mr. Chairman and members of the committee, I am pleased to have this opportunity to become acquainted with the Joint Committee on Atomic Energy and to preface the rather detailed, technical discussion to follow with some general remarks about the national program of research and development on the detection of nuclear tests underground and at high altitude, known as Project Vela. The Advanced Research Projects Agency, on behalf of the Department of Defense, has been charged with overall management of the program. You will be hearing directly from two of our closest associates in this endeavor, the Atomic Energy Commission and the Air Force Technical Applications Center, later on in this hearing.

One of the first major considerations of the technical problems of nuclear test detection occurred in the summer of 1958 when technical experts from the United States, the United Kingdom, Canada, and France discussed this matter with their counterparts from the Soviet Union, Poland, Czechoslovakia, and Rumania and jointly recommended a nuclear test control system.[2] The system they proposed included 180 control stations—100 to 110 on continents, 60 on islands, and 10 on ships—and recommended specific seismic equipments for installation in what has become known popularly as the "Geneva-model Station." The stations were to be spaced about 600 statute miles apart in seismically active areas and 1,000 miles apart in other regions. In addition to seismic instrumentation, the stations would be equipped with a variety of other sensing devices to detect atmospheric tests.

The detection capability of the system recommended by the experts was estimated at that time to provide a high probability of recording seismic signals from deep underground nuclear explosions in continents equivalent to 1 kiloton or above. The identification capability of the system was limited, however, in the sense that earthquakes could sometimes be identified as such, but at other times could be confused with explosions. It could not tell us with certainty that a signal came from an explosion rather than from a natural event. The

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[1] Director of the Advanced Research Projects Agency. Served as Assistant Director of Defense Research and Engineering for Air Defense. Served as Deputy for Research to the Assistant Secretary of the Air Force (Research and Development).

Born in Poland in 1923, and came to the United States in 1927. He graduated from the College of the City of New York in 1944, and received masters and doctoral degrees in electrical engineering from the Polytechnic Institute of Brooklyn. Taught at Brown University from 1950 to 1954, and is now on leave from the University of Illinois where he is professor of electrical engineering and research professor at the Coordinated Science Laboratory.

Technical activities have been concentrated in the fields of noise and communication theory, radar systems design, backscatter, and antennas and propagation.

[2] The Report of the Conference of Experts is included in the hearings before the Joint Committee on Atomic Energy entitled "Technical Aspects of Detection and Inspection Controls of a Nuclear Weapons Test Ban, April 1960 (p. 479).

experts estimated that about 90 percent of all continental earthquakes generating seismic signals equivalent to those from a buried 5-kiloton explosion could be identified. From this it was predicted that from 20 to 100 unidentified seismic events of 5-kiloton equivalent or higher would occur annually in continental areas.

These recommendations were to a large extent based on data taken from a single underground nuclear explosion, the 1.7-kiloton Rainier detonation in Nevada. This was the only underground explosion data available in mid-1958 to Western scientists. After the conference of experts adjourned, two technical developments served to reduce significantly the performance expectations of the Geneva system. First, the 5-kiloton Logan and 20-kiloton Blanca underground explosions, detonated in October 1958, indicated that the seismic signal from a nuclear explosion was smaller than expected. Secondly, and of even greater importance, calculations later confirmed by experiment demonstrated the feasibility of significantly reducing the seismic signal from an explosion by decoupling.

In the face of this situation, a number of remedial steps were initiated by the U.S. Government. The now-familiar Berkner Panel on Seismic Improvement was established at the request of the Department of State by the special assistant to the President for science and technology and it submitted a comprehensive report in late March 1959.[3] Similarly, a special Panel on High Altitude Detection was set up.[4] In April, it was decided at the highest levels to undertake implementation of the recommendations presented by the two panels. The Department of Defense was assigned primary responsibility for this national effort with the understanding that the Atomic Energy Commission would implement that portion of the program requiring nuclear explosions, and any large chemical explosions conducted at the Nevada test site, in cooperation with the Department of Defense.

The Secretary of Defense assigned the Advanced Research Projects Agency responsibility for research, experimentation, and systems development leading to a system for the detection of nuclear explosions underground and at high altitude on September 2, 1959. ARPA issued the first work order under this program to the Air Force Technical Applications Center one month later for initiation of the seismic work later called Vela Uniform.

The Vela program is unusual in that it involves many disciplines not normally associated with each other. Consequently we have had to draw upon a variety of organizations within the Government in order to assemble the necessary background and experience and apply it toward a common objective. ARPA's role is primarily to manage centrally this highly diversified effort. The more precise details of the management structure which has evolved to do the job will be presented in subsequent talks.

I would like to outline now very briefly some of the more important aspects of the Vela program. With respect to the underground portion of the program, we wish to learn more about natural seismic sigals, their particular characteristics, and the envirmonment in which they occur. Similarly, the characteristics of signals emanating from

[3] This report "The Need for Fundamental Research in Seismology" is included in the hearings before the Joint Committee on Atomic Energy entitled Technical Aspects of Detection and Inspection Controls of a Nuclear Test Ban, April 1960 (p. 643).

[4] "Report of the Technical Working Group I." Reference hearings in footnote 3 above, p. 589.

explosions must be determined. To accomplish these tasks, it has been necessary to develop and apply research techniques and instruments, some of which will also be evaluated as components for detection and identification systems. On-site inspection techniques are under study to provide means for conclusively proving that a suspected nuclear explosion has actually occurred. The high altitude portion of the program is primarily concerned with study of the frequency and pecularities of natural events which could be confused with nuclear explosions, but also includes development of instruments for detection.

Indications of our present status in the program are establishment of what will eventually be a worldwide network of standardized seismic instruments to record natural seismic events at 120 observatories; establishment and operation of the first prototype Geneva-model seismic detection station at Lawton, Okla.; and placement of some 100 research contracts which include many of this country's most notable scientists.

In the course of the hearings the technical record established during the extensive hearings conducted by the committee on April of last year will be updated. We are prepared to amplify those aspects of the program which are of most interest to the committee. I hope we can answer satisfactorily the questions which you raise.

Chairman HOLIFIELD. Could you amplify, Dr. Ruina, as to the background of ARPA in the field of detection and identification of nuclear explosions prior to the assignment to ARPA, on September 2, 1959, of the Vela program?

Dr. RUINA. Perhaps I can amplify a little by pointing out where ARPA fits into the Department of Defense and the research that we have been responsible for before.

The Advanced Research Projects Agency is a research and development agency which is organized separately from the military departments and is part of the Secretary of Defense's office. We do such research and development as Mr. McNamara and Dr. Brown deem necessary for centralized management. Project Vela is one of them.

To give you an indication of the size of ARPA, we have an annual budget of approximately $200 million.

Prior to the assignment of Vela to ARPA, ARPA was primarily the agency responsible for the space programs of the Department of Defense; these programs have since been transferred to the military departments. Examples are Discoverer, Midas, Transit, and the like.

Chairman HOLIFIELD. Are you satisfied that ARPA received adequate support, including the necessary funds to pursue the Vela program?

Dr. RUINA. Yes, I think the support is adequate. Of course, one always feels if you have more you can do more, but we cannot complain that we have an inadequate amount at this time.

Chairman HOLIFIELD. Well, we have been somewhat concerned about the fact that a number of invitations to scientific witnesses who appeared on the panel discussion last April have been declined on the ground of lack of currency with the developments on Vela programs during the last year. Are you making any effort to maintain the continued interest of such scientists as Dr. Bethé, Dr. Teller, Dr. Panofsky, Dr. Press, and others in the Vela program?

Dr. RUINA. Yes, I think that Dr. Press, for example, is currently involved in one of the programs that we are sponsoring. Dr. Bethé is generally, I think, informed of the progress in Vela, but not in particulars, perhaps, if he has not followed it in detail. Dr. Bethé is not a seismologist. I would classify him in this field as an elder statesman and a very knowledgeable and brilliant scientist, and useful for consulting purposes, but he is not directly working on Project Vela.

I can say the same thing for Dr. Teller.

Chairman HOLIFIELD. Are you keeping him informed of the progress in this field?

Dr. RUINA. Well, I personally have had only general conversations with a few of the people you have mentioned, but I am sure that they are kept informed. There is constant contact, both by our own people, as well as by our contractors working on the project, with such people as Dr. Bethé and Dr. Teller.

Chairman HOLIFIELD. Are they called in for consultation, or is this informing process very informal in nature?

Dr. RUINA. Specifically, I can say that Dr. Panofsky and Dr. Press are both in our Advisory Council in Project Vela. We have an advisory group in Project Vela, and both of these gentlemen are on it, and therefore they are both very well informed, I think, on Project Vela.

Chairman HOLIFIELD. How often have they met during the past year, with your group?

Dr. RUINA. I do not think I can tell you how often they have met, exactly, but it may be in the order of five times. And perhaps some of the later witnesses can give you details as to precisely how often they have met.

Chairman HOLIFIELD. We were somewhat concerned with some of their statements, that they did not feel that they were up to date on the progress of the program, and therefore they begged off from testifying.

Mr. Price?

Representative PRICE. Doctor, just how does the Department operate in direction of the program? For instance, what percentage of the program was conducted in the ARPA-controlled laboratories, in other Government laboratories, and what percent is handled through private operations?

Dr. RUINA. If that question can be delayed until Mr. Beyer appears, he will give you details of the ARPA management structure and go through, on each of our tasks, just exactly how it is managed. ARPA itself does not have any laboratories. We are an administrative agency that handles all or almost all of its work through other Government agencies. We might assign a certain task to the AEC, for example.

Representative PRICE. I wanted you to elaborate. You say that will be taken care of?

Dr. RUINA. That will be taken care of, yes. If there are any questions then, we will be glad to try to answer them.

Representative PRICE. Who will touch on that, did you say?

Dr. RUINA. Mr. Beyer.

Chairman HOLIFIELD. Mr. Westland?

Representative WESTLAND. I did not hear your statement, Doctor. I have read it. I would just like to ask you this: This 4.75 seismic

magnitude equivalent to above a 19-kiloton force that we say is presently undetectable—is that still true?

Dr. RUINA. Yes. If I can amplify a bit on just generally what we have learned since Vela started, let me first say that the details of this matter will emerge later on in the hearings. Many of your witnesses will testify in some detail about what has been learned specifically in different program areas. But there is no progress to date that can make us change our major conclusions of a year ago.

Representative WESTLAND. In other words, in a period of 3 years, the scientists found that we could go up to 19 kilotons without decoupling and being undetected, where they had estimated in 1958 that they could detect a 1-kiloton test. Is that correct?

Dr. RUINA. Yes.

Representative WESTLAND. But there has been nothing subsequent, no subsequent development, to the 4.75?

Dr. RUINA. Nothing to change our conclusion.

Mr. RAMEY. On page 2 of your statement, you say that: "It could not tell us with certainty that a signal came from an explosion rather than from a natural event." And we are concerned with "with certainty." Does that imply that you can to some extent make a distinction between earthquakes and explosions by means of seismic recordings?

Dr. RUINA. Well, if I can take the word literally, to some extent, the answer I would give is "yes." You can tell with certainty that the signal you receive is an earthquake. But you cannot tell with certainty that a signal you received is an explosion. In other words, the signal that an explosion generates can be confused with signals that earthquakes generate. But there are some earthquakes that generate signals which are clearly identifiable as earthquakes.

Mr. RAMEY. How about explosions? Is there anything that is clearly identified as an explosion?

Dr. RUINA. No. That is what I am trying to say here. Perhaps an observer, depending on how expert he feels, might say, "Well, this looks like it may be an explosion," or "It may not be." But he can never convince anyone else that with certainty this particular signal was an explosion. But you can in many cases say with certainty that a particular signal was an earthquake.

Chairman HOLIFIELD. On page 2 of your statement, and on the beginning of page 3, you recount the first test, the Rainier detonation, of 1.7 kilotons. Now, this was back in mid-1957. And it was upon the results of that test in tuff formation, that the so-called Geneva detection system was devised by the scientists?

Dr. RUINA. Yes.

Chairman HOLIFIELD. Who were those scientists that participated in the setting up of that system?

Dr. RUINA. Well, the names do not come to mind very quickly, but I believe——

Are you referring to the Conference of Experts?

Chairman HOLIFIELD. What did you say?

Dr. RUINA. Are you referring to the American members of the group known as Experts?

Chairman HOLIFIELD. Yes.

Dr. RUINA. I believe you will find this in your April 1960 hearings. The names do not come to me at the moment.

Chairman HOLIFIELD. They were Dr. Fisk, Dr. Bacher, and Dr. Lawrence. I remember those names. These men were all eminent scientists. What is your explanation, if you have one, of the fact that they set up a formula of detection based on the one experiment? Is that the ordinary way that the scientists perform their scientific responsibilities and duties, to have one experiment, and then from that set up conclusive sets of criteria?

Dr. RUINA. I hope not. In this particular case, I was not, Mr. Chairman, present at those meetings, and I do not presume to know exactly how they felt they should qualify what they said. But I am sure that the American participants at the time felt they had some reservation about what they said, and would have qualified their comments, I think, in general discussion. Just how it came out in the report is a different matter.

Chairman HOLIFIELD. As long as I have mentioned the Western Experts, I will read into the record the full names: Dr. James B. Fisk, Dr. Robert F. Bacher, Sir John Cockcroft, Dr. Earnest O. Lawrence, Sir William Penney, Prof. Yves Andre Rocard, Dr. O. M. Solandt. They were from the West. And then we will place in the record the names of the delegations of the Soviet Socialist Republics, the Polish People's Republic, Czechoslovak Republic, and People's Republic of Rumania. We will place those in the record at this point.

(The list referred to follows:)

Union of Soviet Socialist Republics:
E. K. Fedorov
N. N. Semenov
I. E. Tamm
M. A. Sadovski
O. I. Leipunski
I. P. Pasechnik
K. E. Gubkin
S. K. Tsarapkin

Polish People's Republic:
H. Miesowicz
L. Jurkiewicz
M. Blusztajn

Czechoslovak Republic:
C. Simáne
F. Behounek
A. Zátopek
Z. Trhlik

People's Republic of Rumania: H. Hulubei

Chairman HOLIFIELD. I might note that one of the members of the Soviet Republic was S. K. Tsarapkin, who is the head negotiator in Geneva. And he is not a scientist. He is a politician.

Representative PRICE. A diplomat.

Chairman HOLIFIELD. A diplomat; correction.

Well, hindsight, of course, is always better than foresight. But as we look back now, and as we realize the fact that these eminent scientists proceeded on the basis of one experiment and gave to our diplomatic representatives a formula upon which they would negotiate with the Soviet Union for a test ban treaty, which would rely upon the criteria developed, it is most regrettable that they did not delay their setting up of the criteria until they had enough experiments to establish at least a basis of conclusion. This procedure that was followed at that time has embarrassed us ever since in our negotiations, because, as you point out in your statement, the 5-kiloton Logan and the 19-kiloton Blanca underground explosions in October of 1958 showed that the conclusions based on Rainier were in error.

Dr. RUINA. May I say, in defense of the scientific group, that this was the best they could come up with at the time. And I think the only possible argument one can have with them is perhaps that they

did not indicate adequately the degree of confidence or lack of confidence that they had, in the formula, but that this was the best they knew at the time was certainly the case.

Chairman HOLIFIELD. And undoubtedly in further extenuation, as long as we are advancing extenuating reasons, they were under considerable pressure, emotional pressure, at least, propaganda pressure, from the world, at that time, to achieve a test ban treaty. But this still does not alter the fact that we proceeded on inadequate evidence and fallible conclusions in trying to set up a formula for negotiation.

Thank you, Dr. Ruina.

(Dr. Ruina's formal statement follows:)

STATEMENT BY DR. J. P. RUINA, DIRECTOR, ADVANCED RESEARCH PROJECTS AGENCY

I am pleased to have this opportunity to become acquainted with the Joint Committee on Atomic Energy and to preface the rather detailed, technical discussion to follow with some general remarks about the national program of research and development on the detection of nuclear tests underground and at high altitude, known as Project Vela. The Advanced Research Projects Agency, on behalf of the Department of Defense, has been charged with overall management of the program. You will be hearing directly from two of our closest associates in this endeavor, the Atomic Energy Commission and the Air Force Technical Applications Center, later on in this hearing.

One of the first major considerations of the technical problems of detection occurred in the summer of 1958 when technical experts from the United States, the United Kingdom, Canada, and France discussed this matter with their counterparts from the Soviet Union, Poland, Czechoslovakia, and Rumania and jointly recommended a nuclear test control system. The system they proposed included 180 control stations—100 to 110 on continents, 60 on islands, and 10 on ships—and recommended specific seismic equipments for installation in what has become known popularly as the Geneva-model station. The stations were to be spaced about 600 statute miles apart in seismically active areas and 1,000 miles apart in other regions. In addition to seismic instrumentation, the stations would be equipped with a variety of other sensing devices to detect atmospheric tests.

The detection capability of the system recommended by the experts was estimated at that time to provide a high probability of recording seismic signals from deep underground nuclear explosions in continents equivalent to 1 kiloton or above. The identification capability of the system was limited, however, in the sense that earthquakes could sometimes be identified as such, but at other times could be confused with explosions. It could not tell us with certainty that a signal came from an explosion rather than from a natural event. The experts estimated that about 90 percent of all continental earthquakes generating seismic signals equivalent to those from a burried 5-kiloton explosion could be identified. From this it was predicted that from 20 to 100 unidentified seismic events of 5-kiloton equivalent or higher would occur annually in continental areas.

These recommendations were to a large extent based on data taken from a single underground nuclear explosion, the 1.7-kiloton Rainier detonation in Nevada. This was the only underground explosion data available in mid-1958 to Western scientists. After the Conference of Experts adjourned, two technical developments served to reduce significantly the performance expectations of the Geneva system. First, the 5-kiloton Logan and 20-kiloton Blanca underground explosions, detonated in October 1958, indicated that the seismic signal from a nuclear explosion was smaller than expected. Secondly, and of even greater importance, calculations later confirmed by experiment demonstrated the feasibility of significantly reducing the seismic signal from an explosion by decoupling.

In the face of this situation, a number of remedial steps were initiated by the U.S. Government. The now-familiar Berkner Panel on Seismic Improvement was established at the request of the Department of State by the Special Assistant to the President for Science and Technology and it submitted a comprehensive report in late March 1959. Similiarly, a special Panel on High Alti-

tude Detection was set up. In April, it was decided at the highest levels to undertake implementation of the recommendations presented by the two Panels. The Department of Defense was asigned primary responsibility for this national effort with the understanding that the Atomic Energy Commission would implement that portion of the program requiring nuclear explosions, and any large chemical explosions conducted at the Nevada test site, in cooperation with the Department of Defense.

The Secretary of Defense assigned the Advanced Research Projects Agency responsibility for research, experimentation, and systems development leading to a system for the detection of nuclear explosions underground and at high altitude on September 2, 1959. ARPA issued the first work order under this program to the Air Force Technical Applications Center 1 month later for initiation of the seismic work later called Vela Uniform.

The Vela program is unusual in that it involves many disciplines not normally associated with each other. Consequently we have had to draw upon a variety of organizations within the Government in order to assembly the necessary background and experience and apply it toward a common objective. ARPA's role is primarily to manage centrally this highly diversified effort. The more precise details of the management structure which has evolved to the job will be presented in subsequent talks.

I would like to outline now very briefly some of the more important aspects of the Vela program. With respect to the underground portion of the program, we wish to learn more about natural seismic signals, their particular characteristics, and the environment in which they occur. Similarly, the characteristics of signals emanating from explosions must be determined. To accomplish these tasks, it has been necessary to develop and apply research techniques and instruments, some of which will also be evaluated as components for detection and identification systems. On-site inspection techniques are under study to provide means for conclusively proving that a suspected nuclear explosion has actually occurred. The high altitude portion of the program is primarily concerned with study of the frequency and peculiarities of natural events which could be confused with nuclear explosions, but also includes development of instruments for detection.

Indications of our present status in the program are establishment of what will eventually be a worldwide network of standardized seismic instruments to record natural seismic events at 120 observatories; establishment and operation of the first prototype Geneva-model seismic detection station at Lawton, Okla.; and placement of some 100 research contracts which include many of this country's most notable scientists.

In the course of the hearings the technical record established during the extensive hearings conducted by the committee in April of last year will be updated. We are prepared to amplify those aspects of the program which are of most interest to the committee. I hope we can answer satisfactorily the questions which you raise.

Chairman HOLIFIELD. Our next witness is Brig. Gen. A. W. Betts, the Director of the Division of Military Application of the U.S. Atomic Energy Commission.

General Betts, we are glad to have you before us.

STATEMENT OF BRIG. GEN. A. W. BETTS,[1] DIRECTOR, DIVISION OF MILITARY APPLICATION, USAEC

General BETTS. Mr. Chairman, members of the committee, I am pleased to have this opportunity to present to you a statement on the

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[1] Born on Nov. 22, 1912; graduated from the U.S. Military Academy at West Point with the class of 1934; master of science degree in 1938, Massachusetts Institute of Technology; graduate of the Industrial College of the Armed Forces, 1955.

Associate Director (under Dr. Norris E. Bradbury) of the Los Alamos Scientific Laboratory, Los Alamos, N. Mex., 1945.

Chief, Atomic Energy Branch of the Research and Development Division, Department of the Army; Executive to Chief of Research and Development, Department of the Army, in 1952.

Engineer of the newly created Army Ballistic Missile Agency in Huntsville, Ala., 1956.

Military Assistant (Army) to the Director of Defense Research and Engineering, 1959.

Director, Advanced Research Projects Agency, 1959.

Director of Military Application, U.S. Atomic Energy Commission, 1961–.

activities of the Atomic Energy Commission in connection with the Vela program. Dr. Ruina has already discussed the history, organization, and aims of the program in general terms. As was indicated, the Commission has from the very beginning of the Vela program been active in the three areas—Uniform, Sierra, and Hotel. With regard to Vela Uniform, our participation is primarily to furnish experimentally produced seismic signals and to do so by detonating underground nuclear explosions and one large chemical explosion. The construction of necessary detonation sites, emplacement and firing of devices, measuring the yield of these explosions, and making a portion of the close-in seismic measurements, and of course the funding for all of these are responsibilities of the Commission.

The DOD, of course, is responsible for all other close-in, and for intermediate and distant seismic measurements. Other smaller HE shots (½ to 100 tons) would be conducted in different media as a sole DOD responsibility to supplement the larger Vela Uniform detonations and to develop data for the seismic research program.

As a part of the Vela Sierra program, research and development for ground station equipment to detect and identify nuclear explosions at high altitudes and in near space, has been going on for the past 2 years at the Commission's Los Alamos Laboratory. Los Alamos Laboratory has developed two experimental instruments. One is a system for detecting direct visible light signals that may be produced by nuclear explosions at high altitudes. The other is a system for detecting the fluorescence produced in the upper atmosphere by X-rays originating from a high-altitude nuclear detonation. The effectiveness of these systems will depend on worldwide coverage, on visibility, and on whether or not signals produced by natural phenomena can be differentiated from the signals producible by nuclear explosions. These two experimental systems are now being evaluated by the DOD and LASL (Los Alamos Scientific Laboratory), and later witnesses will discuss such evaluations.

Primarily, reliance for detection of nuclear events in far space must rest on satellite-borne detection instruments. The development of such instrumentation and their integration with related satellite systems is the purpose of Vela Hotel. AEC has cooperated with the DOD in this program since early 1959. The Los Alamos Laboratory, working with the Sandia Laboratory has, in the past 2 years, been developing instruments for this purpose. They are very small but rugged neutron, gamma, X-ray, and charged-particle detectors and associated logic systems designed to select suspicious signals to be reported to earth. Considerable effort has been expended by these two Laboratories in this area. Smaller component packages have been flown on lower altitude missiles and piggyback on some of our ballistic missiles and space probes. Further piggyback rides are contemplated and there is now a program to orbit several satellites to carry a complete package or packages into space and to maintain them there for an appreciable period of time. This, in my opinion, is an important program and should result in our learning more about natural phenomena in space, as well as obtaining vital information on instrument behavior in the environment where the system would have to operate.

The LASL and Sandia participation in Vela Sierra and Hotel has been funded entirely by the AEC. In addition, however, the Com-

mission's Lawrence Radiation Laboratory at Livermore has been doing space background signal studies for Vela Hotel under DOD funding.

More specific information on Vela Hotel and Sierra will be presented by later witnesses from ARPA and the Los Alamos Scientific Laboratory.

Chairman HOLIFIELD. Thank you, General Betts, for your introductory statement. As I understand, you will be back on the list later, and we will go into detail then.

General BETTS. We will go into more detail at that time. Yes, sir.

Chairman HOLIFIELD. Are there any questions of General Betts at this time?

I might ask just one question. During your tenure as Chief of ARPA, did you feel that the Vela program received the necessary financial funding?

General BETTS. There was some struggle to obtain the necessary funding, but I believe as a result of that struggle we did in fact obtain the funds that we felt we had to have to do the job properly.

Chairman HOLIFIELD. When were you there?

General BETTS. I left ARPA on the 15th of January of this year, sir. And at that time the real problem was the question of the development of the fiscal year 1962 budget, which I think has since been resolved to ARPA's satisfaction. Of course, I would leave that up to ARPA witnesses to answer.

Chairman HOLIFIELD. There was about $60 million originally set aside for this program, was there not?

General BETTS. Not in one fiscal year in the Department of Defense. I do not remember these numbers precisely. The big problem was the question of Vela Hotel funding, which I think ARPA witnesses should speak to, Mr. Chairman.

Chairman HOLIFIELD. Thank you, General Betts.

(The formal statement of General Betts follows:)

INTRODUCTORY STATEMENT ON AEC PARTICIPATION IN PROJECT VELA BY BRIG. GEN. A. W. BETTS

Mr. Chairman, members of the committee, I am pleased to have this opportunity to present you a statement on the activities of the Atomic Energy Commission in connection with the Vela program. Dr. Ruina has already discussed the history, organization, and aims of this program. As was indicated, the Commission has from the very beginning of the Vela program been active in the three areas—Uniform, Sierra, and Hotel. With regard to Vela Uniform, our participation is primarily to furnish experimentally produced seismic signals and to do so by detonating underground nuclear explosions and one large chemical explosion. The construction of necessary detonation sites, emplacement, and firing of devices, measuring the yield of these explosions and making a portion of the close-in seismic measurements and the funding for all of these are responsibilities of the Commission.

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More specific information on Vela Hotel and Sierra will be presented by later witnesses from ARPA and the Los Alamos Scientific Laboratory.

Chairman HOLIFIELD. Our next witness is Dr. Richard Latter of the Rand Corp., who will testify on the technical problems connected with the Geneva control system.

Dr. Latter, it is aways a pleasure to have you with us. You may proceed.

STATEMENT OF DR. RICHARD LATTER,[1] THE RAND CORP.

Dr. LATTER. The control system currently being negotiated at the Geneva Conference on the nuclear test ban is known to have a very limited capability to detect and identify nuclear weapons tests. I would like to discuss what I consider this capability to be at the present time and what I think we will have to do to try to improve it.

First though, let me remind you of a few of the events that led to the proposed control system and also what the control system consists of. The control system was designed jointly by scientists of the East and West at three separate meetings in Geneva during 1958 and 1959.

The first meeting, the Conference of Experts, was in July and August of 1958. On the basis of very limited information then available, the scientists designed a control system to detect and identify nuclear explosions in the atmosphere, underwater and underground and made estimates of its capabilities and limitations. They did not, however, design means to detect space explosions.

[1] Received a Ph. D. in theoretical physics at the California Institute of Technology in 1949. From 1949 to 1957 he was a staff physicist at the Rand Corp. From 1957 to 1960 he was head of the Physics Division of the Rand Corp. Since 1960, he has been a member of the Rand Research Council. Dr. Latter was an adviser to the U.S. Delegation to the Conference of Experts, 1958, a member of the U.S. delegation to the Conference on the Discontinuance of Nuclear Weapons Tests, and a member of the U.S. Delegation to Technical Working Groups I and II, and to the Geneva Seismic Research Program Advisory Group. Dr. Latter is chairman of the ad hoc group on Detection of Nuclear Detonations (Project Vela) to the Department of Defense.

At the second meeting, Technical Working Group I, in June and July of 1959, the scientists designed control measures for space explosions. They had, however, one point of disagreement—the Soviet scientists did not agree with the inclusion of backscatter radar into the control system, claiming that it was an espionage tool.

Subsequent to the Conference of Experts, new data obtained by the United States from the underground nuclear explosions of the Hardtack II series and the discovery early in 1959 of decoupling of underground explosions cast doubt on the evaluation by the Experts of the capability of the agreed control system. These new developments led to the third meeting of scientists, Technical Working Group II, in November and December of 1959.[2] At that meeting the scientists agreed on improved instrumentation for the control posts, but they could not agree on the reduced capability of the system indicated by the new data and by decoupling.

The agreed control system—known as the Geneva system—based on these three meetings of scientists consists of a worldwide network of 160 to 170 land control posts and 10 ships. The separation of the continental control posts is to be about 600 miles in seismic regions and 1,100 miles in aseismic regions. Control posts in ocean areas are to be separated by about 1,200 to 2,200 miles. The control posts are to be equipped with acoustic, electromagnetic and radioactive debris methods of detecting atmospheric nuclear explosions, with hydroacoustic means of detecting underwater explosions and with seismic means of detecting underground explosions. Also to be included at control posts is equipment for detecting visible light, atmospheric fluorescence, absorption of cosmic noise and electromagnetic signals from space nuclear explosions. In addition to these control posts, 5 to 6 earth satellites instrumented to detect gamma rays, neutrons and X-rays from space nuclear explosions are to be placed in appropriate orbits outside the Van Allen radiation belts above altitudes of 20,000 miles. Another satellite equipped to detect electrons trapped in the earth's magnetic field is to be placed in an appropriate elliptic orbit about the earth. If considered necessary to improve detection of explosions behind the sun and the moon or to make concealment of space explosions more difficult, four solar satellites equipped to detect X-rays are to be placed in appropriate orbits about the sun. Finally, since it is recognized that underground nuclear explosions cannot be identified merely by seismic signals observed at control posts, there is agreed provision for inspection at the site of suspected underground events.

Despite the elaborateness and complexity of this control system, its effectiveness is very limited. This should not be surprising since we know that achieving any kind of effective control is very difficult. What makes control of nuclear weapons tests difficult is that the choice of physical conditions for a nuclear explosion is up to the people carrying it out. They can choose the time for the explosion. They can choose to carry it out in the atmosphere, underwater, in space or underground. And they can try deliberately to modify the explosion environment so as to make detection unlikely or impossible by known scientific means. Control is made even more difficult by the fact that the Soviet Union has tried consistently to minimize the need for control.

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[2] Report of the Technical Working Group II is included in the hearings before the Joint Committee on Atomic Energy entitled Technical Aspects of Detection and Inspection Controls of Nuclear Test Ban, April 1960 (p. 601).

We cannot say precisely what the effectiveness of the Geneva system is since data are meager and inconclusive on virtually all aspects of the problem. We can say what our best guess is.

For atmospheric tests the Geneva system appears to have a good probability of detecting and identifying nuclear explosions as small as 1 kiloton carried out at altitudes up to 6 miles and of detecting, but not identifying, nuclear explosions carried out at altitudes between 6 and 30 miles. The Geneva system has a good probability of detecting explosions set off in the deep oceans. Since underwater chemical explosions and some natural events produce signals similar to underwater nuclear explosions, identification may sometimes require inspection at the site. In some large areas of the oceans, particularly in the Southern Hemisphere, the capability of the system is degraded as a result of the great distances between control posts.

For underground tests, it is believed that the Geneva system has a good probability of detecting and locating, but not identifying, underground nuclear explosions above about ½ to 1 kiloton conducted under the same conditions as the Rainier explosion. For fully decoupled explosions in salt, locating requires a yield greater than 150 to 300 kilotons. For underground nuclear explosions above magnitude 4.75, that is for explosions above about 20 kilotons conducted under the same conditions as the Rainier explosion, approximately 50 to 70 seismic events per year out of a total of 100 to 140 per year in the U.S.S.R. will not be identified by the Geneva system. These numbers of unidentified events vary from year to year due to variations in the annual number of earthquakes. These same numbers apply to fully decoupled explosions of 6,000 kilotons in a salt medium. A 6,000-kiloton explosion is 300 times as large as the Hiroshima explosion. To decouple such a large explosion requires a very large decoupling cavity, about 300 feet in diameter. I don't know if such a large cavity can be built—but I don't know that it can't. It is generally agreed that a cavity at least 800 feet in diameter, sufficient to decouple at least 100 kilotons, is feasible.

Chairman HOLIFIELD. On that point, Doctor, of course, it is not necessary to have a hundred-kiloton explosion to obtain a great deal of scientific information in regard to the design and the feasibility of a particular weapon?

Dr. LATTER. That is certainly correct, sir.

Chairman HOLIFIELD. So when we talk about a hundred kilotons, we are not necessarily setting that as the minimum size from which we can obtain very valuable scientific information?

Dr. LATTER. That is correct, sir.

Chairman HOLIFIELD. As a matter of fact, we can obtain very valuable scientific information from explosions as low as 1 kiloton?

Dr. LATTER. Yes. Last year, Dr. Teller and Dr. Harold Brown spoke on that point, and indicated the kind of weapons developed which could be feasibly undertaken at low yields. (Hearings of April 1960 on Technical Aspects of Detection and Inspection Controls of a Nnclear Weapons Test Ban (pages 195 and 208).

Representative VAN ZANDT. Doctor, will you repeat the statement you made just prior to the chairman's question?

Dr. LATTER. I pointed out that I do not know whether decoupling cavities sufficient to decouple 6,000 kilotons could in fact be con-

structed, but I also pointed out I do not know that they cannot be constructed.

Mr. RAMEY. What size of hole would that require?

Dr. LATTER. 3,000 feet in diameter.

Chairman HOLIFIELD. Why did you pick on the number 6,000?

Dr. LATTER. The threshold magnitude 4.75 in the proposed U.S. treaty at Geneva would correspond to a decoupled explosion of 6,000 kilotons. Such an explosion produces the same strength seismic signal as a tamped explosion of 20 kilotons; that is, a signal of magnitude 4.75. Six thousand kilotons is obtained simply by multiplying the decoupling factor of 300 by the equivalent tamped explosion yield of 20 kilotons, which then gives 6,000 kilotons.

Unidentified seismic events require onsite inspections to determine what caused them. Since explosions of magnitude smaller than 4.75 are not subject to inspection according to the U.S. proposed treaty, these explosions—which can have yields from 20 kilotons under Rainier conditions to thousands of kilotons fully decoupled—cannot be identified and cannot be controlled.

For space explosions, it is believed that the Geneva system has a good probability of detecting unshielded nuclear explosions above about 10 kilotons carried out up to distances of a few hundred million miles. Due to limited knowledge of background radiations in space and consequent uncertainties in design of instruments, this capability cannot be achieved at present. More realistically, it is estimated that the system will be unable to detect unshielded explosions of about 10 kilotons at distances greater than a few million to a few tens of millions of miles—distances currently accessible by rockets. Moreover, shielded explosions of as much as tens to hundreds of kilotons are not detectable at these same distances. Since natural events may produce signals similar to space explosions, space nuclear explosions cannot be identified.

This extremely limited capability of the proposed Geneva system for detecting and identifying underground and space tests raises the question, what would we have to do to try to improve detection? There is no doubt that improving detection is possible. We already know of some major improvements; but these improvements all require major changes of the Geneva system and considerably greater access to the Soviet Union than contemplated in the current political negotiations. They require many more control stations and many more inspections than are considered politically acceptable at the present time. The real problem is to make major improvements in the Geneva system which do not require major political reorientation. This is by no means an easy job. And its success is uncertain.

For underground explosions our greatest difficult is concealment. We know that by exploding a nuclear weapon in a large underground cavity, the seismic signal can be reduced in strength about 300-fold, the so-called decoupling effect. Large decoupled explosions can be conducted without fear that the Geneva system will even detect them. Experiments have confirmed decoupling. There remains, moreover, the important theoretical possibility that decoupling can be increased even more by distributing a heat absorber such as carbon black within the decoupling cavity.

Chairman HOLIFIELD. Dr. Latter, this last paragraph of yours paints a rather pessimistic conclusion in regard to the Geneva system, which we are depending upon in our negotiations with the Soviets. You say that—

"Large decoupled explosions can be conducted without fear that the Geneva system will even detect them."

From a practical point of view, how large a shot do you expect could be exploded with decoupling methods and remain undetectable?

Dr. LATTER. Explosions of approximately 100 to 150 kilotons would go undetected. The earlier number of 6,000 kilotons which I mentioned referred to an explosion which might be detected but which is not subject to inspection according to the proposed U.S. treaty.

Representative VAN ZANDT. What were the approximate figures you used?

Dr. LATTER. For the detection capability?

Representative VAN ZANDT. Yes.

Dr. LATTER. 100 to 150 kilotons. Those are the same numbers that you heard last year before this committee.

Chairman HOLIFIELD. So if this is carried out to its logical conclusion, we would have to conclude that the Geneva system as now discussed would not give any assurance against surreptitious testing?

Dr. LATTER. The proposed U.S. treaty was designed to take just that point into account. The U.S. treaty is based on a threshold of magnitude of 4.75, rather than on a specific yield of explosion. It is based on detectability of signals.

In my discussion I have tried to indicate what yield explosions can be detected. As to whether the system is effective from the point of view of deterring the conduct of such large-yield explosions, that is a political judgment. I have nothing to say on that.

Representative VAN ZANDT. Mr. Chairman, may I ask a question? Doctor, bring me up to date on what you mean by "shielded explosions."

Dr. LATTER. Yes, sir. I intend to discuss that later in my talk.

Representative WESTLAND. Doctor, are you saying, then, that while you can detect a 6,000-kiloton fully decoupled underground explosion, because that would not produce greater force than 4.75 under the Geneva proposed treaty, it would not be subject to inspection?

Dr. LATTER. That is correct, sir. But, I want to reiterate that the technical feasibility of building such a large cavity is in question. I do not know whether it is feasible or not.

To improve detection, therefore, our most vital problem is to explore the possibility that there exist effects which might make the most effectively decoupled explosions more detectable. I do not know of any such effects, but we cannot be sure that there are none. We thought at one time that the detection of the electromagnetic signal radiated by an explosion as it pushes aside the earth's magnetic field might offer such a remote possibility. However we know now that relatively simple countermeasures can suppress entirely this electromagnetic signal. But, we must continue to look for other possible effects. Of these, seismic effects appear to offer the greatest hope.

All we know at present about the seismic signals from underground nuclear explosions has been learned from the six underground explosions which we carried out in 1957 and 1958—the Rainier shot and

the Hardtack II series.[3] In fact at the time of the Experts' conference the data from the Rainier shot was the only data on underground nuclear explosions available for designing the Geneva system. Unfortunately, all of these explosions were limited to a unique set of physical conditions. They were all tamped explosions in the same medium, in the same geological formation, and at essentially the same depth. We know that the strength and form of the seismic signals generated by underground explosions depend upon these physical conditions. We need to know the strength of the seismic signals from underground explosions in order to specify the minimum yield explosion for which seismic detection by the Geneva system is possible. We need to know the form of the signal in order to determine which features might permit the system to distinguish between earthquake and explosion signals. The Hardtack II explosions changed our estimates, which had been based on Rainier, both of the strength and the form of explosion signals and led to the revision in 1959 of our estimates of system capability deduced from the Rainier shot.

In view of this very limited knowledge, extensive research is essential. Research is needed to tell us more about the character of signals generated by underground nuclear explosions under the widest variety of conditions. It must tell us about the background noise which accompanies explosion signals, interfering with them, and making detection difficult. Research is needed to study natural earthquakes which produce signals similar to those from nuclear explosions, and research is needed to develop methods for distinguishing natural signals from explosion signals.

Our only basis now, as it was at the time of the Conference of Experts, for distinguishing earthquakes from underground nuclear explosions is the direction of the first detected earth motion. However, our data are still inadequate to determine the strength of the first motion at large distances or to determine its dependence on the explosion conditions. The data indicate that at best only limited improvements of the Geneva system can be expected from improved observation of first motion.

The greatest seismic improvements in the Geneva system for distinguishing natural signals from explosion signals probably must come from the so-called diagnostic aids which involve extensive analysis of the detailed signal form. How much improvement might be possible with these aids, we do not know. We do know that for these aids to become useful we will have to learn a great deal more about the generation and propagation of explosion signals. We will have to undertake extensive analyses of explosion signals, and, very importantly, we will have to learn considerably more about earthquake signals—where they arise and what their detailed form is. We will have to search for those subtle differences of signal forms which might distinguish an earthquake from a nuclear explosion.

In addition to studying the nature of explosion and earthquake signals, we must try to improve control system instruments to detect these signals more efficiently—so that weaker signals would be detecta-

[3] The U.S.S.R. representatives at the 1958 Conference of Experts did not offer any data on underground nuclear explosions. In 1959, during the meetings of Technical Working Group II, the Soviet representatives stated that the U.S.S.R. had conducted no underground nuclear explosions.

ble. Instrument improvement requires more knowledge about microseismic noise which interferes with detection. We must find out the principal sources of this noise and learn its detailed nature. In this way we may hope to design more optimal instruments.

Two improvements have already been suggested. One is the use of arrays of seismographs such as was proposed at the Geneva Conference of Experts and at Technical Working Group II. Another, which is still in the early research stage, is the use of deep well seismographs. Preliminary results with the deep well seismograph have been very encouraging. It has been found that at depths of several thousand feet the microseismic noise is considerably less than at the surface of the earth. Much weaker seismic signals will therefore be detectable.

In order to prove that an underground nuclear explosion has taken place, it is necessary to undertake an inspection at the site of the suspected event for the purpose of obtaining a sample of radioactive debris. It is not known, however, what the likelihood is that inspection can be carried out successfully. In fact, second only to concealment, the problem of onsite inspection is the most difficult which the control system faces. At present the most serious obstacle to improving onsite inspection is the low accuracy of locating the site of the suspected event from seismic data at distant control posts. Locating the site is possible at the present time only to within a few hundred square miles. Within such a large area it would be exceedingly difficult to locate the small regions, a few tens to a few hundreds of yards in diameter, containing the radioactive debris left by a nuclear explosion.

We can reduce the large area of uncertainty by obtaining more accurate seismic travel time data. Unfortunately, obtaining travel time data requires considerable access to the region being studied and is therefore probably politically unacceptable to the Soviets. Other methods have been proposed for reducing this area, but much study is needed before it can be decided whether they have any promise.

Within the large area of uncertainty in location of the suspected event the techniques for inspecting the site and for distinguishing between an earthquake and an explosion are also still in a very rudimentary state. These techniques include the search for unusual human activity, recent changes in surface conditions, and various geophysical techniques for locating the underground cavity left by an explosion. Our knowledge of the effectiveness of these techniques is at present extremely limited and considerable research on this difficult problem is needed.

The problem of detecting space explosions is no easier than that of detecting underground explosions. In many ways it is more difficult. We have had even less experience with space explosions than with underground explosions. We have had essentially no experience in methods of detecting this type of explosion. The control system for space nuclear explosions is based almost entirely upon theoretical considerations.

The difficulties in detecting space explosions do not come from our ignorance of the explosion signals themselves. These are reasonably well understood. The signals produced by a space nuclear explosion consist of radiations produced directly by the explosions: neutrons, gamma rays, X-rays, visible light, radio waves, and the material debris

from the weapon itself. There are also indirect signals produced by interaction of the direct radiations with the earth's magnetic field and the earth's atmosphere. The direct signals are known from experiments with atmospheric explosions combined with some well-established theoretical extrapolations. The indirect signals are less well understood because of the lack of experimental data. However, by virtue of a great deal of careful theoretical work and the little experience gained from the few U.S. high-altitude explosions,[4] the more important of these indirect signals are now also reasonably understood.

The difficulties in the space detection problem come when we attempt to design the means by which the control system is to detect these explosion signals. Some of the explosion signals are observed with ground-based instrumentation. The remaining signals are screened from the earth's surface by the atmosphere and require satellite-based instrumentation. In both cases, however, we are at present quite ignorant concerning the natural background noise which obscures the explosions. Without this information, we will be completely unable to evaluate realistically our capability to detect space explosions. In the case of the ground-based detection we expect in the near future to have measurements on background noise. Unfortunately present indications suggest that the ground-based techniques will not be very effective. The most sensitive detection techniques will be those based in satellites. However, the nature, source, and magnitude of background noise in space are still almost completely unknown. We have had very few experiments to determine the relevant space backgrounds. Moreover, experiments of this type are difficult and costly—requiring the launching of suitably instrumented satellites.

Obtaining information on background noise is our most urgent problem. Even when we obtain this information we will still face many difficult problems. We will have to design instrumentation to detect the explosion signals in the presence of background noise. For the ground-based techniques our problem will be simpler since weight is not a constraint on our instrument design. The development of satellite-based instrumentation, however, will be quite difficult. It will require a lengthy and expensive program, whose degree of success is quite uncertain. The satellite instrumentation will have to be reliable and have to have a long life otherwise the cost of the system could be prohibitive. To achieve a long instrument life is a fundamental problem of our entire space program. Simplicity is an essential prerequisite. Simplicity, however, would lead to a decrease in detection capability. A compromise will be necessary.

Furthermore, the satellite instrumentation must be designed to minimize the likelihood of a false signal arising from background noise. It is not possible to eliminate false signals entirely. But it is expected that by careful design the frequency of such signals can be made extremely small. Thus, while the occurrence of a signal would not be sufficient to identify its origin, it may be sufficient to establish a strong presumption. There may, however, be phenomena in space whose signals, like earthquakes in the underground case,

[4] Two high altitude tests designated as "Teak" and "orange" were conducted during operation Hardtack I, 1958.

are in all essential features similar to explosion signals. The existence of such phenomena is entirely unknown.

Finally, concealment of a nuclear explosion in space against detection by the Geneva system is known to be possible. The X-ray signal from a space nuclear explosion is believed to have the greatest detectable range. However, insertion of a thin metallic sheet between the explosion and the detectors will considerably degrade the X-ray detection capability. If detectors are situated only near the earth, the X-ray shield can be simple and light. If instrumented solar satellites are used, the shields become heavier and more complicated. The ultimate effectiveness of X-ray shields is uncertain.

Explosions behind the sun and the moon appear feasible and may not be detectable by earth-based instrumentation. Solar satellites, which involve difficult problems of telemetry and instrument lifetime, will probably be required by the control system.

I have mentioned only a few of the most important problems which the Geneva system faces in detecting underground and space explosions and what we have to do about them. I have omitted other problems not because they are unimportant but because there are just too many to describe in a short time. As you will hear from later speakers, these important problems are currently being worked on under Project Vela.

I have a few final comments. First, I would like to emphasize that the success of a research program for improving our capability to detect and identify underground nuclear explosions depends almost entirely on undertaking a program of experimental nuclear explosions. Chemical explosions, while helpful, cannot substitute for nuclear. I had occasion last year to describe before this committee the scientific basis for the requirement of nuclear as opposed to chemical explosions.[5] There has been no change since that time and, therefore, I will not go into the justification for this requirement.

Secondly, the success of a research program for improving detection of space explosions is almost entirely dependent on a rather costly program of satellite measurements of the background noise signals in space. Without this program, however, we will be unable to determine adequately our capability to detect nuclear explosions in space.

And, finally, although minor improvements by research are foreseen, research has as yet given no clue as to means for improving the Geneva system sufficiently to permit detection and identification of shielded tests in space or decoupled tests underground and certainly no clue how to detect and identify any underground or space test without a control system. An agreement to ban all nuclear weapons tests would have to rely, therefore, not upon control by the Geneva system but rather upon the self-control of the potential violator and on such unevaluable factors as motivation, need, and intent.

Chairman HOLIFIELD. Thank you, Dr. Latter. That is a very complete statement, although it is a very depressing statement for those who have had hopes that something of a breakthrough would occur in this field of seismological and other types of detection.

Mr. Price?

Representative PRICE. Mr. Chairman, I read an interesting article in the morning Post about detecting underground explosions by ear.

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[5] Hearings of April 1960 on Technical Aspects of Detection and Inspection Controls of a Nuclear Weapons Test Ban, p. 399.

The article stated that Dr. Speeth of the Bell Telephone Laboratories used seismic data from the Hardtack series of underground nuclear tests and earthquake data from laboratory seismometers, converting to audiotapes, and was able to train four high school students and five staff members to distinguish between earthquakes and explosions in 90 percent or more of the cases presented to them.

Would you care to comment on that?

Dr. LATTER. Yes, sir. I do not know whether the article was referring to more recent information than I have. Some time ago I visited the Bell Telephone Laboratory to hear about their work of listening to earthquake signals, and by that means distinguishing earthquakes from nuclear explosions.

At that time the work was in a very preliminary state, and I think that I would not have concluded that there was any real scientific basis for believing that discrimination of earthquakes from explosions by this means was possible.

I think, though, that it is a method which must be looked at. If we are to have any chance of a breakthrough, we have to take advantage of every possible opportunity in our research program.

Representative PRICE. At what distance can the present Geneva control system detect a shielded 10-kiloton nuclear test?

Dr. LATTER. A shielded 10-kiloton explosion, shielded against the X-rays? I think at around 100,000 kilometers, or perhaps as far as the orbit of the moon. You would probably detect it not by X-rays but by gamma rays or by visible light.

Representative PRICE. I think that is all the questions I have.

Representative VAN ZANDT. In connection with this shielding, again, when you refer to a metallic shield, you refer to fragmentation, do you not, of metal? Or is it a gas? Radiation? What is it?

Dr. LATTER. The important point in shielding against X-rays is to insert some heavy material, such as lead, between the explosion and the detectors. This can be done by making a balloon out of plastic loaded with lead. Prior to the time you detonate the explosion, you inflate this balloon in such a way that it sits between the explosion and the detectors. It acts more or less like a mirror to the X-rays. The X-rays shine on the balloon and are reflected from it in a direction away from the detectors.

Representative VAN ZANDT. It is somewhat of a decoy, then?

Dr. LATTER. Not like a decoy, more like a wall. It simply prevents the X-rays from going in the direction in which the shield is oriented and reaching the detectors.

I would say that the Geneva system does provide control, but only for big enough underground space and atmospheric tests. Since the Geneva system cannot detect all nuclear tests, a total test ban would be uncontrolled.

Representative VAN ZANDT. Have we made any improvement in seismology since we first began to talk about controlling tests?

Dr. LATTER. There have been a number of interesting instrument improvements. There have been some interesting new possibilities of discrimination. For example, instead of using the P-wave component of the seismic signal to determine first motion, using long period surface waves. This is a method first suggested by Dr. John W. Tukey.[6]

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[6] Hearings of April 1960 on Technical Aspects of Detection and Inspection Controls of a Nuclear Weapons Test Ban, p. 704.

Representative VAN ZANDT. Then the answer is that we have made some progress.

Dr. LATTER. Some progress, yes.

Representative VAN ZANDT. You would say minor progress?

Dr. LATTER. Compared to the difficult problem of detecting decoupled explosions, yes, sir.

Chairman HOLIFIELD. Would the gentleman yield?

You mentioned these deep-well seismometers and the arrays of seismometers as possible improvements for the application of this first motion criterion. Could you tell by what factor the promising developments are presently estimated to improve detection, as compared to the factor of the 300 decoupling which these improvements would overcome? Can you give us a relation there?

Dr. LATTER. My familiarity with the field of deep-well seismographs and arrays of seismographs is very limited. However, my understanding is that improvements of a factor of two to three or four over previous possibilities may be achievable. But I would prefer that the seismologists who have had direct experience in this field speak on this point.

Representative VAN ZANDT. In your statement, you have described the areas where detection is possible, and also the areas where we have to do a lot of research, and other areas where you think detection is impossible. How long do you think it would take us to put a foolproof detection system together, in years?

Dr. LATTER. Foolproof?

Representative VAN ZANDT. Yes, a foolproof detection system.

Dr. LATTER. If you base control on detectable signals, you can have a limited control system now.

Of course, that does not mean that you can have a total test ban. It means you can have a limited test ban. It will be one based on the detectability of signals. It would have a capability to control some atmospheric, some underground and some space explosions.

So I think you can have a control system now if your goal is more limited than the monitoring of all nuclear weapons tests.

Representative VAN ZANDT. Let us take one of the limited goals. Would it take us a year, 5 years, 10 years?

Dr. LATTER. There are many engineering questions involved in specifying the time. I have no special knowledge on these questions. I think, however, that we have enough scientific knowledge about some atmospheric tests, some underground tests, and some space tests, to design now a control system that would be adequate for those tests.

Representative VAN ZANDT. Would it take us a year to put it together?

The basis of my question: The last time, along with the chairman and others, we visited Geneva, we sat in on a conference that concerned the patrolling of outer space, and we heard estimates of 10 years.

Dr. LATTER. Yes.

Representative VAN ZANDT. And costing several billion dollars.

Dr. LATTER. I have certainly heard those numbers, too, sir. Determining those numbers involves a large range of nonscientific questions which I just do not feel that I am qualified to discuss. The numbers you mentioned are those which I believe the experts in this field have obtained.

Representative VAN ZANDT. May we assume, Doctor, that this overall control program—and we will agree on limited capability—will take years and will cost billions of dollars?

Dr. LATTER. Yes, I think there is a fair chance of that.

Representative VAN ZANDT. And involved in it all is a terrific amount of research and development?

Dr. LATTER. Certainly.

Representative VAN ZANDT. Now, just one more question, and I am finished.

Do you know of any contribution in the way of knowledge that the Russians have made to the development of a program, with limited capabilities, of detecting tests, such as we have from some of our projects?

Dr. LATTER. I know of nothing. No, sir. The discussions which we have had with the Soviets in Geneva have not suggested that they have made a great effort to learn about detection. However, many of them certainly are excellent scientists and very knowledgeable in their respective fields.

Representative VAN ZANDT. Is it proper to say that by their attitude they indicate that they are not interested?

Dr. LATTER. Certainly. There is every indication that the Soviets are minimizing the importance of control. This is an impression which I have gained through direct experience from the time of the experts to today.

Representative VAN ZANDT. That is all, Mr. Chairman.

Representative HOSMER. Dr. Latter, at Geneva, who first postulated the possibilities of decoupling?

Dr. LATTER. It was not postulated at Geneva. It was developed by Dr. Albert Latter at the Rand Corp. in 1959. He did it jointly with three of his colleagues.

Representative HOSMER. What date was that?

Dr. LATTER. Early in 1959.

Representative HOSMER. Thank you. That is all.

Chairman HOLIFIELD. Mr. Bates?

Representative BATES. Doctor, in discussing limited capability, that means we are dealing with a system that will be very ineffective. We have to consider something that will be total in scope to be effective, would we not?

Dr. LATTER. It is possible. What I mean when I say "limited" is that the control system does not detect all nuclear explosions. And indeed some very large nuclear explosions are not detected. The control system is therefore technically a limited system. The question of whether the system is effective and adequate for the purpose of negotiating a treaty or for whatever political goals you have, is something I simply cannot comment on. It has been stated by the President that the limited treaty currently being proposed at Geneva satisfies our needs for effective and adequate control—for those explosions which it controls, I presume.

Representative BATES. Which it what?

Dr. LATTER. Which the system controls.

Representative BATES. But the reason we are going into this is that we do not trust somebody. Now, if we are going to close one door

and leave all of the rest of the doors open, we are only kidding ourselves. Is that not what you are saying?

Dr. LATTER. Well——

Representative BATES. Well, is it not true? Is it not a fact?

Dr. LATTER. That is a very direct question. But the answer certainly involves a political judgment.

Representative BATES. I am not talking about a political question. Now, you are a scientist who has been working on this particular problem. And you say: Now, we might develop a limited capability.

Dr. LATTER. Yes, sir.

Representative BATES. Well, how about all the rest of the question that we have in reference to this problem? That is the reason we are going into it. We do not trust somebody. Now, if we develop a limited capability, and we can definitely establish within a narrow range whether or not they are testing, how about the rest of it? They are not going to test where we can detect what they are doing. Now, if they are as honorable as that and as trustworthy as that, and you can repose as much confidence in them as that, why have any system?

Dr. LATTER. The way you are posing that question makes it compelling to answer a certain way. But let me just say the following: I see no objection to an agreement which is equally limiting on the parties involved. If a control system prevents the Soviets from carrying out certain kinds of tests, and allows them to do others, as long as we have the same degree of freedom and there are no unilateral constraints on us of freedom, I see no objection to it.

Representative BATES. You see no what, sir?

Dr. LATTER. No strong objection.

Representative BATES. You have no objections to it?

Dr. LATTER. As long as all parties are equally limited and equally free.

Representative BATES. Then why do you want it at all? What are we trying to do here?

Dr. LATTER. That is a very difficult question to answer. There have been a number of statements on the subject of why we are attempting to have an agreement with the Soviets. Those statements all involve political judgments.

Representative BATES. Well, I am not talking about political judgments, Doctor. I am just talking about the practical aspects of what we are trying to do.

Now, if we want to give some scientists some exercises in a certain field, that is one thing. If it is a question of trusting or not trusting, it seems to me we must be complete in whatever we do. Otherwise, let us forget about the whole thing.

Dr. LATTER. Well, it appears to me that it might be very ambitious to try to meet the goal of complete control in any measure of arms control that you can think of. In the early days of the test ban issue which was thought to be the simplest arms control measure, we presumed that we could control all nuclear tests. That led us to propose an agreement with the Soviets to stop all nuclear tests. If we knew then what we know now, which is that only limited control is possible, we might still have attempted to have an agreement, but only on a limited basis—involving only those tests which can be controlled; in this case, both the Soviets and the United States would

have been equally limited. The Soviets could not thereby have gained an advantage over us by circumventing detection by the system. It is a question of equally limiting the two sides with no unilateral constraints on us.

Representative BATES. As far as I am concerned, that is an exercise in complete futility. If what we are trying to do is control this, it seems to me we ought to control it. Now, if we cannot control it, we ought to forget about it, unless you want a scientific adventure. Now, that is something else.

But I see no purpose served whatsoever in controlling a small segment when it is a question of trust or distrust involved. Is that an unreasonable statement?

Dr. LATTER. Only the last part of it bothers me. If you control a segment, and can really control it, without hamstringing yourself in the areas which cannot be controlled, then the question of trust does not enter into the issue.

Representative BATES. That is the only thing, it seems to me.

Dr. LATTER. We are not trusting them. If we have a system which does in fact stop them, for example, from conducting any atmospheric tests and if we have an agreement to stop only atmospheric tests, and allow them to conduct whatever other tests they wish, and we also are allowed to do the same, then the question of trust is not at stake. It is a purely objective agreement. They can do those things which they wish, and we can do those things that are necessary for us. We are not trusting them in any way with such an agreement.

Representative BATES. Then what would be the purpose of even having a limited agreement?

Dr. LATTER. That purpose has been debated over and over again by people who are knowledgeable on the political issues at stake. I am a scientist.

Representative BATES. Well, as far as I am concerned, you can do it or you cannot do it, and if you cannot do it, you are just wasting a lot of time.

Now, essentially there has been no change from a year ago. We are essentially where we were then?

Dr. LATTER. Very little progress.

Representative BATES. Nor do we really anticipate any, from what I can gather.

Dr. LATTER. Correct.

Representative BATES. I have no further questions, Mr. Chairman.

Chairman HOLIFIELD. Mr. Westland?

Representative WESTLAND. You have indicated in response to Mr. Van Zandt's questions that there would be years of effort to gain further knowledge in this. How much could you accelerate that, if you were permitted to make underground tests?

Dr. LATTER. Well, sir, I think that to conduct meaningful research in the detection of underground nuclear explosions, you have to study the object you are trying to detect. It is not too much a question of accelerating the program that is involved in the need for nuclear explosions: it is just that they are an essential prerequisite for research on detection. You must have nuclear explosions as part of your research program.

Representative WESTLAND. I have sort of gathered from your statement here that you are pretty much stymied at the present time in your research program, and since you want to study nuclear underground explosions, how can you study them without having them. Is that not about it?

Dr. LATTER. That certainly is so. You have got to have them to study them.

Representative VAN ZANDT. Doctor, do the Soviets still maintain they have no underground nuclear test experience or data?

Dr. LATTER. As of the last meeting in Geneva which I participated in with them, in May of 1960, they certainly did not indicate that they had any more data of any significance.

Representative VAN ZANDT. This was back in 1960?

Dr. LATTER. Yes. That was the last contact I have had with them.

Representative VAN ZANDT. After reading your paper, I am of the opinion you are willing to join some of us in recommending the resumption of tests.

Dr. LATTER. Yes, sir.

Representative VAN ZANDT. That is all, Mr. Chairman.

Chairman HOLIFIELD. Dr. Latter, there is one point I thought I might query you on. You spoke about the current method of distinguishing earthquakes from nuclear explosions, because of the direction of the so-called first motion of the seismic signals. Now, what depth—maybe I should ask you this: What percentage of the earthquakes which would record magnitude 4.75 on the seismograph originate at a depth below where man could ordinarily place the cavity? Is there any line that we can draw there?

Dr. LATTER. I have been trying to find out the answer to that question myself. I do not know the answer. Some opinions which I have heard indicate that the number of earthquakes that are so shallow that they might, by location of the focus of the earthquake, have been man placed, may be very small. But it is not known how to determine depth of focus accurately enough to answer your question.

Chairman HOLIFIELD. There is no clear demarcation, then, in earthquakes which originate, say, at 10,000 feet underground and those that originate at twenty or thirty or forty thousand?

Dr. LATTER. I do not know the answer. Research is needed to find the answer.

Chairman HOLIFIELD. The reason I ask this, of course, is that if a large segment of the 4.75 recordings could be automatically eliminated, because they come from a depth below that to which man could reasonably dig——

Dr. LATTER. Right. It is a very sound approach to the discrimination program—depth of focus. It is something which people are currently studying, to see if depth of focus can in fact be determined accurately enough from distant signals.

Also, there are some thoughts of techniques of inspection at the site that might be useful in determining depth of focus.

Chairman HOLIFIELD. Of course, a rupture in the earth would not necessarily occur at the point of origination. It might occur many hundreds of miles from the point of origination, at one of the faults in the earth.

Dr. LATTER. For very large earthquakes that might be the case. For very small ones, I think it less likely that the rupture, the surface disturbances, would be very far from where the earthquake is. I think it is very vital that we do pursue the problem of depth of focus. However, this problem of depth of focus concerns discrimination between earthquakes and explosions. But I think the important problem, the really important problem for the control system, is not the discrimination question, but it is, how do we detect decoupled explosions? And there, our problem is to detect very, very small signals. If we could ever detect such small signals, we would then be faced with a titanic problem of discrimination. But at present the discrimination problem is really second to the problem of detecting decoupled explosions.

Chairman HOLIFIELD. Dr. Latter, this committee is well aware of your high standing in the scientific community, and of course we value your testimony before this committee today as we have in other instances.

May I ask you this question: Do you know any body of scientific opinion that would be of an opposite viewpoint from the statements that you have made today, the scientific statements which you believe to be fact? Is there any recognized group that would contest your statements today, that you know of?

Dr. LATTER. What I have been talking about involves a great number of conjectural matters, since we have very little data and therefore conclusions depend upon personal judgment. I am sure you will find people who would differ with the emphasis which I have placed on some of these matters. I am pessimistic about the success of certain aspects of the research program. You will find scientists who are perhaps more optimistic.

I do not think there is any disagreement on the scientific facts, where they are known. However, where there is room for judgment, as in every issue—political, scientific, or what you will—there will be a spectrum of opinions. And that is certainly the situation in this issue.

Chairman HOLIFIELD. This committee hopes that if there is any difference of opinion on some of the testimony that you have given today, some of the witnesses will give that difference of opinion and state any side of the case which might be in opposition to your positions that you have taken in your statement.

And the Chair would take the liberty of saying that if there is any body of opinion that is founded on reputable scientists that needs to be expressed before this committee, we would like to have it.

Thank you very much for your appearance before the committee.

Dr. LATTER. Thank you, sir.

(Dr. Latter's formal statement follows:)

THE PROBLEM OF DETECTING NUCLEAR EXPLOSIONS

(By Richard Latter)

The control system currently being negotiated at the Geneva Conference on the nuclear test ban is known to have a very limited capability to detect and identify nuclear weapons tests. I would like to discuss what I consider this capability to be at the present time and what I think we will have to do to try to improve it.

First though, let me remind you of a few of the events that led to the proposed

control system and also what the control system consists of. The control system was designed jointly by scientists of the East and West at three separate meetings in Geneva during 1958 and 1959.

The first meeting, the conference of experts, was in July and August of 1958. On the basis of very limited information then available, the scientists designed a control system to detect and identify nuclear explosions in the atmosphere, underwater, and underground and made estimates of its capabilities and limitations. They did not, however, design means to detect space explosions.

At the second meeting, Technical Working Group I, in June and July of 1959, the scientists designed control measures for space explosions. They had, however, one point of disagreement—the Soviet scientists did not agree with the inclusion of backscatter radar into the control system, claiming that it was an espionage tool.

Subsequent to the Conference of Experts, new data obtained by the United States from the underground nuclear explosions of the Hardtack II series and the discovery early in 1959 of decoupling of underground explosions cast doubt on the evaluation by the experts of the capability of the agreed control system. These new developments led to the third meeting of scientists, Technical Working Group II, in November and December of 1959. At that meeting the scientists agreed on improved instrumentation for the control posts, but they could not agree on the reduced capability of the system indicated by the new data and by decoupling.

The agreed control system—known as the Geneva system—based on these three meetings of scientists consists of a worldwide network of 160 to 170 land control posts and 10 ships. The separation of the continental control posts is to be about 600 miles in seismic regions and 1,100 miles in aseismic regions. Control posts in ocean areas are to be separated by about 1,200 to 2,200 miles. The control posts are to be equipped with acoustic, electromagnetic, and radioactive debris methods of detecting atmospheric nuclear explosions, with hydroacoustic means of detecting underwater explosions and with seismic means of detecting underground explosions. Also to be included at control posts is equipment for detecting visible light, atmospheric fluorescence, absorption of cosmic noise and electromagnetic signals from space nuclear explosions. In addition to these control posts, 5 to 6 earth satellites instrumented to detect gamma rays, neutrons and X-rays from space nuclear explosions are to be placed in appropriate orbits outside the Van Allen radiation belts above altitudes of 20,000 miles. Another satellite equipped to detect electrons trapped in the earth's magnetic field is to be placed in an appropriate elliptic orbit about the earth. If considered necessary to improve detection of explosions behind the sun and the moon or to make concealment of space explosions more difficult, four solar satellites equipped to detect X-rays are to be placed in appropriate orbits about the sun. Finally, since it is recognized that underground nuclear explosions cannot be identified merely by seismic signals observed at control posts, there is agreed provision for inspection at the site of suspected underground events.

Despite the elaborateness and complexity of this control system, its effectiveness is very limited. This should not be surprising since we know that achieving any kind of effective control is very difficult. What makes control of nuclear weapons tests difficult is that the choice of physical conditions for a nuclear explosion is up to the people carrying it out. They can choose the time for the explosion. They can choose to carry it out in the atmosphere, underwater, in space or underground. And they can try deliberately to modify the explosion environment so as to make detection unlikely or impossible by known scientific means. Control is made even more difficult by the fact that the Soviet Union has tried consistently to minimize the need for control.

We cannot say precisely what the effectiveness of the Geneva system is since data are meager and inconclusive on virtually all aspects of the problem. We can say what our best guess is.

For atmospheric tests the Geneva system appears to have a good probability of detecting and identifying nuclear explosions as small as 1 kiloton carried out at altitudes up to 6 miles and of detecting, but not identifying, nuclear explosions carried out at altitudes between 6 and 30 miles. The Geneva system has a good probability of detecting explosions set off in the deep oceans. Since underwater chemical explosions and some natural events produce signals similar to underwater nuclear explosions, identification may sometimes require inspection at the site. In some large areas of the oceans, particularly in the Southern Hemiphere, the capability of the system is degraded as a result of the great distances between control posts.

For underground tests, it is believed that the Geneva system has a good probability of detecting and locating—but not identifying—underground nuclear explosions above about ½ to 1 kiloton conducted under the same conditions as the Rainier explosion. For fully decoupled explosions in salt, locating requires a yield greater than 150 to 300 kilotons. For underground explosions above magnitude 4.75, that is for explosions above about 20 kilotons conducted under the same conditions as the Rainier explosion, approximately 50 to 70 seismic events per year out of a total of 100 to 140 per year in the U.S.S.R. will not be identified by the Geneva system. These numbers of unidentified events vary from year to year due to variations in the annual number of earthquakes. These same numbers apply to fully decoupled explosions of 6,000 kilotons in a salt medium. Unidentified seismic events require on-site inspection to determine what caused them. Since explosions of magnitude smaller than 4.75 are not subject to inspection according to the U.S. proposed treaty, these explosions—which can have yields from 20 kilotons under Rainier conditions to thousands of kilotons fully decoupled—cannot be identified and cannot be controlled.

For space explosions, it is believed that the Geneva system has a good probability of detecting unshielded nuclear explosions above about 10 kilotons carried out up to distances of a few hundred million miles. Due to limited knowledge of background radiations in space and consequent uncertainties in design of instruments, this capability cannot be achieved at present. More realistically, it is estimated that the system will be unable to detect unshielded explosions of about 10 kilotons at distances greater than a few million to a few tens of millions of miles—distances currently accessible by rockets. Moreover, shielded explosions of as much as tens to hundreds of kilotons are not detectable at these same distances. Since natural events may produce signals similar to space explosions, space nuclear explosions cannot be identified.

This extremely limited capability of the proposed Geneva system for detecting and identifying underground and space tests raises the question—what would we have to do to try to improve detection? There is no doubt that improving detection is possible. We already know of some major improvements—but these improvements all require major changes of the Geneva system and considerably greater access to the Soviet Union than contemplated in the current political negotiations. They require many more control stations and many more inspections than are considered politically acceptable at the present time. The real problem is to make improvements in the Geneva system which do not require major political reorientation. This is by no means an easy job. And its success is uncertain.

For underground explosions our greatest difficulty is concealment. We know that by exploding a nuclear weapon in a large underground cavity, the seismic signal can be reduced in strength about 300-fold, the so-called decoupling effect. Large decoupled explosions can be conducted without fear that the Geneva system will even detect them. Experiments have confirmed decoupling. There remains, moreover, the important theoretical possibility that decoupling can be increased even more by distributing a heat absorber such as carbon black within the decoupling cavity.

To improve detection, therefore, our most vital problem is to explore the possibility that there are effects which might make the most effectively decoupled explosions more detectable. I do not know of any such effects, but we cannot be sure that there are none. We thought at one time that the detection of the electromagnetic signal radiated by an explosion as it pushes aside the earth's magnetic field might offer such a remote possibility. However we know now that relatively simple countermeasures can suppress entirely this electromagnetic signal. But, we must continue to look for other possible effects. Of these, seismic effects appear to offer the greatest hope.

All we know at present about the seismic signals from underground nuclear explosions has been learned from the six underground explosions which we carried out in 1957 and 1958—the Rainier shot and the Hardtack II series. In fact at the time of the Experts' Conference the data from the Rainier shot was the only data on underground nuclear explosions available for designing the Geneva system. Unfortunately, all of these explosions were limited to a unique set of physical conditions. They were all tamped explosions in the same medium, in the same geological formation, and at essentially the same depth. We know that the strength and form of the seismic signals generated by underground explosions depend upon these physical conditions. We need to know the strength of the seismic signals from underground explosions in order to specify the minimum yield explosion for which seismic detection by the Geneva system is pos-

sible. We need to now the form of the signal in order to determine which features might permit the system to distinguish between earthquake and explosion signals. The Hardtack II explosions changed our estimates, which had been based on Rainier, both of the strength and the form of explosion signals and led to the revision in 1959 of our estimates of system capability deduced from the Rainier shot.

In view of this very limited knowledge, extensive research is essential. Research is needed to tell us more about the character of signals generated by underground nuclear explosions under the widest variety of conditions. It must tell us about the background noise which accompanies explosion signals, interfering with them and making detection difficult. Research is needed to study natural earthquakes which produce signals similar to those from nuclear explosions, and research is needed to develop methods for distinguishing natural signals from explosion signals.

Our only basis now, as it was at the time of the conference of experts, for distinguishing earthquakes from underground nuclear explosions is the direction of the first detected earth motion. However, our data are still inadequate to determine the strength of the first motion at large distances or to determine its dependence on the explosion conditions. The data indicate that at best only limited improvements of the Geneva system can be expected from improved observation of first motion.

The greatest seismic improvements in the Geneva system for distinguishing natural signals from explosion signals probably must come from the so-called diagnostic aids which involve extensive analysis of the detailed signal form. How much improvement might be possible with these aids, we do not know. We do know that for these aids to become useful we will have to learn a great deal more about the generation and propagation of explosion signals. We will have to undertake extensive analyses of explosion signals, and, very importantly, we will have to learn considerably more about earthquake signals—where they arise and what their detailed form is. We will have to search for those subtle differences of signal forms which might distinguish an earthquake from a nuclear explosion.

In addition to studying the nature of explosion and earthquake signals, we must try to improve control-system instruments to detect these signals more efficiently—so that weaker signals would be detectable. Instrument improvement requires more knowledge about microseismic noise which interferes with detection. We must find out the principal sources of this noise and learn its detailed nature. In this way we may hope to design more optimal instruments.

Two improvements have already been suggested. One is the use of arrays of seismographs such as was proposed at the Geneva Conference of Experts and at Technical Working Group II. Another, which is still in the early research stage, is the use of deep-well seismographs. Preliminary results with the deep-well seismograph have been very encouraging. It has been found that at depths of several thousand feet the microseismic noise is considerably less than at the surface of the earth. Much weaker seismic signals will therefore be detectable.

In order to prove that an underground nuclear explosion has taken place, it is necessary to undertake an inspection at the site of the suspected event for the purpose of obtaining a sample of radioactive debris. It is not known, however, what the likelihood is that inspection can be carried out successfully. In fact, second only to concealment, the problem of onsite inspection is the most difficult which the control system faces. At present the most serious obstacle to improving onsite inspection is the low accuracy of locating the site of the suspected event from seismic data at distant control posts. Locating the site is possible at the present time only to within a few hundred square miles. Within such a large area it would be exceedingly difficult to locate the small regions, a few tens to a few hundreds of yards in diameter, containing the radioactive debris left by a nuclear explosion.

We can reduce the large area of uncertainty by obtaining more accurate seismic travel-time data. Unfortunately, obtaining travel-time data requires considerable access to the region being studied and is therefore probably politically unacceptable to the Soviets. Other methods have been proposed for reducing this area, but much study is needed before it can be decided whether they have any promise.

Within the large area of uncertainty in location of the suspected event the techniques for inspecting the site and for distinguishing between an earthquake and an explosion are also still in a very rudimentary state. These techniques include the search for unusual activity, recent changes in surface conditions,

and various geophysical techniques for locating the underground cavity left by an explosion. Our knowledge of the effectiveness of these techniques is at present extremely limited and considerable research on this difficult problem is needed.

The problem of detecting space explosions is no easier than that of detecting underground explosions. In many ways it is more difficult. We have had even less experience with space explosions than with underground explosions. We have had essentially no experience in methods of detecting this type of explosion. The control system for space nuclear explosions is based almost entirely upon theoretical considerations.

The difficulties in detecting space explosions do not come from our ignorance of the explosion signals themselves. These are reasonably well understood. The signals produced by a space nuclear explosion consist of radiations produced directly by the explosions—neutrons, gamma rays, X-rays, visible light, radio waves and the material debris from the weapon itself. There are also indirect signals produced by interaction of the direct radiations with the earth's magnetic field and the earth's atmosphere. The direct signals are known from experiments with atmospheric explosions combined with some well-established theoretical extrapolations. The indirect signals are less well understood because of the lack of experimental data. However, by virtue of a great deal of careful theoretical work and the little experience gained from the few U.S. high altitude explosions, the more important of these indirect signals are now also reasonably understood.

The difficulties in the space detection problem come when we attempt to design means by which the control system is to detect these explosion signals. Some of the explosion signals are observed with ground-based instrumentation. The remaining signals are screened from the earth's surface by the atmosphere and require satellite-based instrumentation. In both cases, however, we are at present quite ignorant concerning the natural background noise which obscures the explosion signals and which might produce signals similar to nuclear explosions. Without this information, we will be completely unable to evaluate realistically our capability to detect space explosions. In the case of ground-based detection we expect in the near future to have measurements on background noise. Unfortunately present indications suggest that the ground-based techniques will not be very effective. The most sensitive detection techniques will be those based in satellites. However, the nature, source, and magnitude of background noise in space are still almost completely unknown. We have had very few experiments to determine the relevant space backgrounds. Moreover, experiments of this type are difficult and costly—requiring the launching of suitably instrumented satellites.

Obtaining information on background noise is our most urgent problem. Even when we obtain this information we will still face many difficult problems. We will have to design instrumentation to detect the explosion signals in the presence of background noise. For the ground-based techniques our problem will be simpler since weight is not a constraint on our instrument design. The development of satellite-based instrumentation, however, will be quite difficult. It will require a lengthy and expensive program, whose degree of success is quite uncertain. The satellite instrumentation will have to be reliable and have to have a long life—otherwise the cost of the system could be prohibitive. To achieve a long instrument life is a fundamental problem of our entire space program. Simplicity is an essential prerequisite. Simplicity, however, would lead to a decrease in detection capability. A compromise will be necessary.

Furthermore, the satellite instrumentation must be designed to minimize the likelihood of a false signal arising from background noise. It is not possible to eliminate false signals entirely. But it is expected that by careful design the frequency of such signals can be made extremely small. Thus, while the occurrence of a signal would not be sufficient to identify its origin, it may be sufficient to establish a strong presumption. There may, however, be phenomena in space whose signals, like earthquakes in the underground case, are in all essential features similar to explosion signals. The existence of such phenomena is entirely unknown.

Finally, concealment of a nuclear explosion in space against detection by the Geneva system is known to be possible. The X-ray signal from a space nuclear explosion is believed to have a greatest detectable range. However, insertion of a thin metallic sheet between the explosion and the detectors will considerably degrade the X-ray detection capability. If detectors are situated only near the earth, the X-ray shield can be simple and light. If instrumented solar

satellites are used, the shields become heavier and more complicated. The ultimate effectiveness of X-ray shields is uncertain.

Explosions behind the sun and the moon appear feasible and may not be detectable by earth-based instrumentation. Solar satellites, which involve difficult problems of telemetry and instrument lifetime, will probably be required by the control system.

I have mentioned only a few of the most important problems which the Geneva system faces in detecting underground and space explosions and what we have to do about them. I have omitted other problems not because they are unimportant but because there are just too many to describe in a short time. As you will hear from later speakers, these important problems are currently being worked on under Project Vela.

I have a few final comments. First, I would like to emphasize that the success of a research program for improving our capability to detect and identify underground nuclear explosions depends almost entirely on undertaking a program of experimental nuclear explosions. Chemical explosions, while helpful, cannot substitute for nuclear. I had occasion last year of describing before this committee the scientific basis for the requirement of nuclear as opposed to chemical explosions. There has been no change since that time, and therefore I will not go into the justification for this requirement.

Secondly, the success of a research program for improving detection of space explosions is almost entirely dependent on a rather costly program of satellite measurements of the background noise signals in space. Without this program, however, we will be unable to determine adequately our capability to detect nuclear explosions in space.

And, finally, although minor improvements by research are foreseen, research has as yet given no clue as to means for improving the Geneva system sufficiently to permit detection and identification of shielded tests in space or decoupled tests underground and certainly no clue how to detect and identify any underground or space test without a control system. An agreement to ban all nuclear weapons tests would have to rely therefore not upon control by the Geneva system but rather upon the self-control of the potential violator and on such unevaluable factors as motivation, need, and intent.

(Appendix 2, page 292, contains letter from Dr. David R. Inglis, Argonne National Laboratory, dated August 7, 1961, commenting on the hearing testimony; and letter from Dr. Richard Latter, Rand Corporation, dated August 22, 1961, containing comments on those portions of Dr. Inglis' letter relating to Dr. Latter's testimony; also letter from Dr. Latter, Rand Corporation, dated August 22, 1961, containing comments on Ambassador Dean's letter.)

(Appendix 2, page 292, also contains letter from Drs. Herman W. Hoerlin and Donald R. Westervelt, Los Alamos Scientific Laboratory, dated August 8, 1961, commenting on the testimony; and letter from Dr. Richard Latter, Rand Corporation, dated August 22, 1961, commenting on letter from Drs. Hoerlin and Westervelt.)

Chairman HOLIFIELD. Our next witness is Mr. Carlton M. Beyer, of the Advanced Research Projects Agency. Mr. Beyer's testimony will include an overall description and discussion of Project Vela.

Mr. Beyer, how long will it take you to present your statement?

Mr. BEYER. Probably about 45 minutes.

Chairman HOLIFIELD. Some of us have to be on the floor today for certain duties. I think in order to have continuity of your presentation and the subsequent questioning, we will ask you to return at 2 o'clock, because we would rather give you an opportunity to make your whole presentation, as it is a very important one, and then the committee will have it fresh in their minds when they question you.

So we will adjourn the meeting at this time until 2 o'clock.

(Whereupon, at 11:46 a.m., the committee was recessed, to reconvene at 2 p.m., the same day.)

AFTERNOON SESSION

Chairman HOLIFIELD. The committee will be in order.

Our first witness this afternoon is Carlton M. Beyer of the Advanced Research Projects Agency. Mr. Beyer's discussion will include an overall discussion of Project Vela.

Please proceed.

STATEMENT OF C. M. BEYER,[1] DEPUTY ASSISTANT DIRECTOR, NUCLEAR TEST DETECTION OFFICE, ADVANCED RESEARCH PROJECTS AGENCY

Mr. BEYER. Mr. Chairman and members of the Joint Committee, I will summarize the objectives of the Vela Project, the management approach, the funding aspects, and refer briefly to the technical status of each of the three programs.

The assignment of the Vela Project to the Department of Defense (DOD) resulted from a meeting on April 23, 1959 during which Dr. Killian, special assistant to the President on science and technology, Chairman McCone of the Atomic Energy Commission, and Deputy Secretary of Defense Quarles discussed the implementation of the reports from Panels on High Altitude Detection and Seismic Improvement. At this meeting, it was agreed that—

(*a*) The specific recommendations of both panels would be implemented.

(*b*) The DOD would accept overall responsibility for implementing the High Altitude Panel recommendations with support from the Atomic Energy Commission (AEC) and the National Aeronautics and Space Administration (NASA).

(*c*) The DOD would accept primary responsibility for implementing the recommendations of the Seismic Improvement Panel but that the AEC would implement the nuclear and HE explosions in cooperation with the DOD.

After a number of studies including those by advisory groups and activities within the DOD, the Secretary of Defense assigned the responsibility for the project, later named Vela, to the Advanced Research Projects Agency. The assignment is to "perform research, experimentation, and systems development, to obtain at the earliest practicable date a system for the detection of nuclear explosions both underground and at high altitude."

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[1] Born: San Diego, Calif., Aug. 25, 1915.
Education: Public schools, San Diego, Calif.; San Diego State College, San Diego, Calif.; Graduate School, Claremont College, Claremont, Calif.
Married, two sons, 11 and 9 years old.
Experience: July 1959 to December 1959: Was Chief, Missile Engines and Propellants Branch and Saturn Project Manager in the Advanced Research Projects Agency, Office of Secretary of Defense, now Deputy Assistant Director for Nuclear Test Detection Office; November 1957 to July 1959: Principal technical assistant to the Director, Office of Guided Missiles, Office of the Director of Defense Research and Engineering, Office of the Secretary of Defense; November 1956 to November 1957: Chief of Engineering Division, Office of Guided Missiles, Office of Assistant Secretary of Defense for Research and Engineering; May 1946 to November 1956: Branch head, Scientific Department, U.S. Navy Electronics Laboratory, San Diego, Calif.; 1942 to May 1946: Research associate for the University of California Division of War Research at San Diego and Key West.
Professional societies: Senior member, Institute of Radio Engineers; member Sigma Pi Sigma (physics); member, Phi Delta Kappa (education); member and consultant, American Ordnance Association.

This assignment includes the following major technical objectives:[2]

(*a*) Conduct research to better understand the nature of the nuclear detonation signals to be detected and how to discriminate them from other signals.

(*b*) Develop and evaluate existing and new techniques and instruments for detecting and identifying nuclear detonation signals.

(*c*) Develop and evaluate systems for improving the detection and identification of nuclear detonations, and the problems in the operation of these systems.

(*d*) Perform research, development, and evaluation of on-site inspection techniques.

These technical objectives contain two basic elements:

(*a*) Upgrading the sciences involved.

(*b*) Evaluating and applying available technical information to the "Geneva-proposed system" problem.

As has been previously mentioned, the Vela Project is divided into three programs: (1) Vela Uniform, which is concerned with the detection of underground and underwater nuclear detonations; (2) Vela Sierra, the detection of high altitude nuclear detonations by instruments located on the earth, and (3) Vela Hotel, the detection of high altitude nuclear detonations by means of satellite-borne instruments.

The approach in the management sense being used includes—

(1) ARPA defines the specific problem area.

(2) An ARPA order is issued describing the work to be done, the funds available, the reporting procedure and the period during which the order is effective. These orders are directed to an agent which is a Government activity with special competence in the field. The agent itself undertakes or contracts for the work with a university, a nonprofit organization, or an industrial concern.

(3) The agent is responsible for the detailed technical supervision and administration of the assigned work. This includes the responsibility for maintaining direct contact with the scientists doing the work, receiving the reports, commenting on them, and taking the appropriate action. ARPA also receives a copy of each report for review. When ARPA is directly responsible for the work and an agent is not used it gives direct guidance to the contractor.

(4) The ARPA staff periodically reviews all work under the project which is then summarized for the ARPA Ad Hoc Group on Nuclear Test Detection, which provides further technical guidance. In addition, one general summary of the project was held in the form of a symposium and a second symposium to be held jointly with the California Institute of Technology is planned for October or November of this year.

Now I would like to show the Government organizations that have major responsibilities in the Vela Project. Mr. Theodore George of ARPA will help with the charts, here. This chart (chart 1, p. 63) shows the principal organizations participating in the project, providing advice, or receiving information from those engaged in it.

[2] These apply both to subsurface and high altitude detection.

Regular guidance is received from the Office of the Special Assistant to the President for Science and Technology, and certain aspects of the Vela Project have been reviewed by the President's Scientific Advisory Committee. The U.S. Disarmament Administration in the Department of State has been kept informed of progress on the project through the Office of the Assistant Secretary of Defense for International Security Affairs, by direct contact with ARPA and by direct contact with agents working on the project.

As General Betts mentioned this morning, the Atomic Energy Commission is active in all three programs of the Vela Project and the nature of this participation will be mentioned later by witnesses from the AEC. Within the Department of the Interior, the Geological Survey is participating through an increased crustal study program and is aiding in the planned series of research explosions that will be discussed by other witnesses. The Department of Commerce, through the Coast and Geodetic Survey, is procuring and installing a network of standard seismic stations to be located throughout the world. Also, the Coast and Geodetic Survey is one of the numerous agencies assisting in the coordinated program of extensive measurements to be made in connection with the explosion program.

The National Bureau of Standards, under the Department of Commerce also, gives major support in the Vela Sierra program. The National Aeronautics and Space Administration is participating in the joint AEC, NASA, and DOD planning effort under the Vela Hotel program.

A number of units within the Department of Defense are engaged in the Vela Project. Those principally involved are shown on this chart (chart 2, p. 63). ARPA, which is in the Office of the Director of Defense Research Engineering, is responsible for the technical direction and overall management of the project and its three programs. Also within the Office of the Director of Defense Research and Engineering, the Special Assistant for Arms Control has maintained regular contact with the project and provided policy guidance. Similarly the Office of the Assistant Secretary of Defense for International Security Affairs is kept currently informed of the project's work. The Assistant to the Secretary of Defense for Atomic Energy, who is also the Chairman of the DOD/AEC Military Liason Committee, provides important guidance for Vela.

Through the Corps of Engineers and the Signal Corps, the Department of Army participates in the Vela Sierra program.

In the Department of the Navy, the Office of Naval Research (ONR) is the overall coordinator of the Navy's technical effort in the Vela Uniform program, including the work done by the Bureau of Ships laboratories and its contractors. The Office of Naval Research, its contractors, and a Bureau of Ships laboratory are also participating in the Vela Sierra program.

Within the Department of the Air Force, the Air Force Technical Applications Center (AFTAC), serves as a principal adviser on the Vela Project and is responsible for a substantial amount of work, both inhouse and under contract, on the Uniform and Sierra programs. Under the Air Force Systems Command, the Air Force Space Systems Division is primary agent for the Vela Hotel program and provides the chairman for the joint effort in this area in conjunction with NASA and the AEC. In addition, the Air Force Office of

Scientific Research (OSR) and the Cambridge Research Laboratory (CRL) are conducting research and development on the Vela Uniform and Sierra programs.

Very substantial technical support and funding is provided by the AEC for the preparation and firing of the nuclear and the one large chemical explosion and the support for the other two Vela programs. Approximately three-fourths of the other technical work and all of the remaining funds are provided by the DOD. Since a great deal of Project Vela work underway is related to other DOD objectives, it is possible to take advantage of results obtained in other programs, to use existing DOD facilities, to avoid multiple supervision costs by using DOD agents, and to minimize the cost of recordkeeping by using existing DOD reporting and accounting procedures. Also of considerable importance to the project is the ease with which it is possible to obtain advisory and direct assistance from DOD consultants and contractors.

The DOD funding for Project Vela in fiscal year 1960 and fiscal year 1961 and expected in fiscal year 1962 is shown on the next chart (chart 3, p. 64).

And these are broken down, and the total for those 2 years is $51,438,000. Of these funds, you will note, in the next column, $47,441,000 have been committed by ARPA, and of the funds committed $36,244,000 have been obligated, according to estimates as of the 30th of June.

In the case of the fiscal 1962 budget, a total of $59 million is anticipated, and out of this $59 million, $22 million is currently in the apportionment process.

Representative VAN ZANDT. Mr. Chairman, may I ask a question at this point?

Mr. Beyer, the $51 million for 1960 and 1961 and the $59 million for 1962—it covers the cost of administration of the overall project plus the cost of R. & D. Is that correct?

Mr. BEYER. With one exeception. It does not include any AEC costs.

Representative VAN ZANDT. Any AEC costs?

Mr. BEYER. That is right.

Representative VAN ZANDT. Have you any idea what they are, in round figures?

Mr. BEYER. General Betts is prepared to discuss these, and can do so. These, however, cover only the DOD costs.

Representative VAN ZANDT. Therefore it is only proper to assume that any projects that develop from the study will be additional cost.

Mr. BEYER. This covers all of the R. & D., all administration; in fact, all of the projects under the program are covered by these costs.

Chairman HOLIFIELD. Mr. Ramey has a question.

Mr. RAMEY. How does this $51 million you have for 1960–61 compare to the $60 million that was announced somewhat precipitately at the time the Joint Committee was putting out its report last year?

Mr. BEYER. Well, in the case of the same $60 million that was announced, the DOD portion of that was approximately $33 million, and this, together with approximately $3 or $4 million additional that was picked up during the year, makes up the total of the fiscal 1961 funding.

Then if you deduct that from the $51 million for the 2 years, you get the amount which we had in fiscal year 1960. And you may recall we got the fiscal 1960 funds moderately late in the fiscal year, and they were on the order of I think $1,050,000 for Sierra and about $8 or $9 million for Uniform; that order of magnitude.

Mr. RAMEY. Were you able to obligate as much as you had planned for fiscal 1961?

Mr. BEYER. Well, as you know, in the case of the actual conduct of the research explosions, things have not moved as rapidly as we had hoped, but in the other areas I would say we have come pretty well up to schedule. We have submitted for the record a compendium of the contracts that have been issued, and these are in tabulation form for your perusal now, if you would care to look at them. (See app. 5, p. 390.)

The breakdown of contracts by programs and types of contracts has been given to Mr. Conway; I can give it to you in a few minutes.

In the Vela Uniform area, there are 80 university and industrial contracts, 20 projects with Government agencies; totaling 102 for that program. In the Vela Sierra program, there are 14 university and industrial contracts, 3 with the Government, totaling 17. The Vela Hotel program—six with industry, one project with Government, totaling seven.

So the grand total is well over a hundred at this time.

Representative VAN ZANDT. Mr. Chairman, at this point:

Mr. Beyer, is it not correct to say that under Uniform, $80 of the $92 million is to be attributed to industry and university activity, and the remainder to the Government?

Mr. BEYER. These are contracts, yes.

Representative VAN ZANDT. And under Sierra, $14 million to industry and universities, and $3 million to Government?

Mr. BEYER. These are numbers of contracts, sir.

Representative VAN ZANDT. Numbers, not millions?

Mr. BEYER. Not millions. And the number per contract is shown in this tabulation, here. It is divided by programs. And you can see how much the amount of each contract is.

However, the totals, as shown over here, do apply.

Mr. RAMEY. I notice on the list of contracts that you have, that they are all relatively small amounts compared to the normal Defense Department budgetary expenditures—I mean $75,000, $145,000, going down the line, and so on. It has often been mentioned that ARPA has sort of served as a contract brokerage outfit. Had any consideration been given to assigning large blocks of this work to a national laboratory or to one laboratory, and really giving them the ball to run with it?

Mr. BEYER. I think in the case of the crustal study program with the Geological Survey this is the case. Their amount is well over a million dollars. Also in the case of groups of contracts with CRL and with OSR, they have been given blocks of money. Also AFTAC, in the systems area, has been given a rather substantial block of money.

I think you can derive this after a study from this list of contracts, by taking the column which has to do with the agent, on the first or second page. Air Force Systems Command has a whole list of contracts. You total these up, and then you will know what the block of

money given to them was. But in general, the reason for not having a very large amount of money per contract is that we are not procuring, except in the systems case, large amounts of hardware. These are for research. Therefore the amounts given I think are somewhat in line with the ONR and OSR practice.

However, in the major universities engaged in the seismic, places such as Cal Tech, University of California, Michigan, and so on, these are at the several hundred thousand dollar level, as the tabulation shows. But again, the real reason for the very large ones not being at one particular laboratory for them to use is that we are not buying hardware.

Now, I will summarize the way in which each program is being managed and briefly refer to its status. Further details concerning each program will be presented by the manager of that program.

The Vela Uniform program: First, a little background.

The easiest way to manage a program is to concentrate the skills in one place and assign responsibility commensurate with capability. With the variety and quantity of work required in this program such an approach is not practicable. This is the case because this country has no major center or small group of centers that possess the required level and depth of technical competence required. To organize such a single center and staff would probably remove most of the limited number of "earthquake" seismologists from their universities or industrial companies. In the case of university scientists, current work on Vela Uniform tasks would probably be disrupted, and the training of graduate and undergraduate students which will ultimately alleviate the shortage of talent in seismology would be degraded. As a result of this situation, ARPA has 11 agents handling the 80 major contracts in the Vela Uniform program, each agent being assigned a technical area. This is shown on the next chart 4 (p. 64).

On this chart, OSD is shown in the box on the left. There are two or three contracts which OSD, for convenience purposes, administers directly. The first one listed there is the Vela Seismic Information and Analysis Center, which is a University of Michigan contract, which is a bibliographic and abstracting type of operation for the entire program.

As mentioned earlier, the Coast and Geodetic has the worldwide seismic net effort. The U.S. Geological Survey has a crustal study program. And, of course, AEC Division of Military Application (DMA) takes care of all nuclear shot construction, and the one large HE shot at the Nevada test site.

Off to the right, at the top, AFTAC's responsibilities are shown and they are the principal technical adviser for this program. They carry on some research projects. They are the agent for the entire systems program. They are also the agent for the entire explosion measurement program at the long-range measurement phase. They also have responsibility for offsite data derived from this measurement program. They are also the agent for the on-site inspection work.

Going on down further to the right, some of the principal agents in the research program are shown. The first one, AFCRL is the agent for applied seismology, AFOSR, is responsible for basic seismology. The Air Force office, European Office of Research, is carrying on one or two projects in Europe, which Dr. Bates can describe later. The

Air Force Rome Air Development Center is exploring the possibility of using the electromagnetic pulse for detection. As mentioned earlier, Bureau of Ships is handling some of the Navy work, as are the Navy Electronics Laboratory, Navy Radiological Defense Laboratory, and ONR. These all participate in the Navy program.

The agents shown here each handle an area in which they have special qualifications.

Now, to improve the detection, identification, location, and onsite inspection capabilities for subsurface nuclear detonations. These include both underground and underwater, and they may be either tamped or decoupled. Substantial improvements must be made if the number of "false alarms" is to be markedly reduced and small tamped or moderate size decoupled explosions are to be detected at all in an operationally acceptable number of instances. The Vela Uniform program, which is concerned with making improvements in these areas, is divided into the following search task areas.

The first one is this worldwide standard seismological network, which is being implemented by Coast and Geodetic Survey, and it will be the first larger international seismological network utilizing modern calibrated and standardized instrumentation. The data from this network will be of very high quality, and will be made available to all seismologists, regardless of nationality, for conducting broad research studies on world seismicity, wave propagation characteristics, location of earthquake epicenters, and depth of foci, seismic noise, and a better understanding of differences in seismic waves originating from all types of earthquakes.

The research program, which is task 2, is aimed at broadening our knowledge of seismic and nonseismic techniques for detecting subsurface nuclear detonations. Different research areas have been defined for the 11 governmental agencies assisting in this task.

Chairman HOLIFIELD. Mr. Beyer, we are going to ask you to excuse us for about 15 minutes, to answer a rollcall.

Mr. BEYER. Yes, sir.

(Short recess.)

Chairman HOLIFIELD. The committee will be in order.

You may proceed.

Mr. BEYER. If I may, sir, I will proceed with task 2, starting with the first sentence, again. That is on page 8 of the statement.

This task is aimed at broadening our knowledge of seismic and nonseismic techniques for detecting subsurface nuclear detonations. Different research areas have been defined for the 11 governmental agencies assisting in this task. For example, the Defense Atomic Support Agency (DASA) is responsible for an integrated theoretical and experimental research program on the conversion of contained blast energy into a seismic wave train. The Air Force Cambridge Research Laboratory is responsible for applied seismology, the Air Force Office of Scientific Research for basic seismology and allied sciences, the Air Force Rome Air Development Center for underground electromagnetic pulse investigations, and the Air Force Technical Applications Center for improved monitoring and for display equipment and techniques. Research and development on the detection of undersea explosions by hydroacoustic and radiochemical techniques is the responsibility of the Office of Naval Research and the Bureau of Ships.

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In the Department of the Interior, the U.S. Geological Survey has undertaken a major investigation of seismic propagation paths in southern California and Nevada directed toward determining how accurately seismic sources can be located and identified in a well-calibrated region. Foreign seismic research has been arranged through the European Office of the Air Force Office of Aerospace Research, the European Research Office of the U.S. Army, and the Office of the Secretary of Defense. Work is already underway in Sweden, Israel, and Canada. A wide variety of topnotch talent is working on this task. Thirty-five contractors are involved, which include such well-known names as Bell Telephone Laboratories, Standard Oil of New Jersey, Standard Oil of California, and Texas Instruments, Inc., and such noted universities as the Massachusetts Institute of Technology, the California Institute of Technology, the University of Michigan, and the University of California. Dr. Charles Bates, ARPA's Chief of the Vela Uniform program will summarize the research work underway.

(3) System development: The Air Force Technical Applications Center (AFTAC) is the agent for the major part of this task. Five types of seismic detection stations will be built for study and evaluation:

(*a*) One Geneva-type station, located at Fort Sill, Okla., has been in operation since October 1960. Construction of four more will commence this year on the Geneva-recommended 600-mile spacing in California, Oregon, Montana, and Utah.

(*b*) Construction of two advanced detection stations with the improvements recommended by the Berkner panel, each station which may include as many as 100 seismometers will be initiated this year; one will be in Arizona and the other is likely to be located in the Southeast.

(*c*) A contract is being negotiated for the development of an unmanned station.

(*d*) A simplified manned station concept will be tested at one of the Geneva or Berkner type stations.

(*e*) A minimum of two installations of seismometers in deep holes is planned.

A systems development field station will provide an installation capable of conducting prototype network and "control systems headquarters" studies and tests, and will also provide a facility for evaluating equipment likely to be used in the prototype stations.

In addition to the system effort I have just described, ARPA is planning to arrange with one or more universities for the establishment of a Vela Uniform research and evaluation center. The center will be an independent effort aimed at assessing the capability of subsurface detection systems, and have a major interest in research in the earth sciences related to the Vela Uniform program. Although the concept of this center and the method for implementing it are under consideration, it is intended that this center will be a focal point where top caliber scientists and engineers, as well as graduate students, can work independently or cooperatively with academic, industrial, and governmental organizations for the solution to research and systems problems related to the improvement in the detection, identification, and location of subsurface nuclear detonations. Later on today, or pos-

sibly tomorrow, Dr. Carl Romney of AFTAC will discuss the system task in greater detail.

Mr. RAMEY. Is this another Defense Department "think factory" that you are thinking of setting up?

Mr. BEYER. No, sir. We think it should be a think factory manned by university and industrial scientists and probably would not include Government scientists. And it would probably be operated under university management. But certainly it would be funded from the Vela Project.

Mr. RAMEY. These other ones like Rand and so on do not include Government scientists, do they?

Mr. BEYER. No, sir.

Mr. RAMEY. And are they not "think" organizations?

Mr. BEYER. Indeed they are.

Mr. RAMEY. So this would be another one?

Mr. BEYER. Well, I do not think I clearly understand your point, here. The point I am trying to make is that this is an organization which would be given a very free hand to look critically at the important research and development questions relating to the overall Vela Project insofar as ground detection is concerned.

Mr. RAMEY. I think an alternative, of course, would be for the ARPA organization itself, or AFTAC, to staff up with its own people to do this sort of evaluation. And for some reason or other, they never seem to want to do this.

Mr. BEYER. Well, I would say this: That there would probably be some considerable difficulty in getting university-type scientists and industrial-type scientists to work on a full-time basis in a center of this sort in such a way that they could return to their parent organization, namely, be on leave from the university and go back, and in the case of industry go back to their company, if it were operated by the Government. There is a great deal more freedom and attractiveness in the minds of these scientists in a university-type organization.

Mr. RAMEY. Which is to say that you can pay them more under contract?

Mr. BEYER. The salary schedule problem has not received direct and serious consideration, but there is some interest on the part of the university community to avoid having anyone offer much higher salaries to their people and make that the sole means for attracting them away, even on a temporary basis.

The real interest that we have, however, is to have their talents brought to bear on this problem on a full-time basis, and the salary question will have to be looked into somewhat later. But we do not want to upset the university community by this operation.

Chairman HOLIFIELD. These people would be on leave from the university or from industry, and subject to recall and the retention of their pension rights, and that sort of thing?

Mr. BEYER. This is one concept, sir. And as I mentioned, this is not completely worked out.

Chairman HOLIFIELD. Now, our experience in different types of organizations like this, such as Rand and others, has been that the salary scale jumps sharply. It jumps from that allowed by the Government to salaries that run to $10,000 and $15,000 and $20,000, and even higher. Can you give us any idea what the salary of the Director of this center would be?

Mr. BEYER. No, sir. This has not been worked out. What has to be determined is which university or group of universities would in fact be willing to undertake this operation, and after that has been determined, to discuss this with them in detail.

Chairman HOLIFIELD. There is a shortage of these skilled people in seismology throughout the Nation, is there not?

Mr. BEYER. Indeed there is.

Chairman HOLIFIELD. And what excuse do you have for pulling them away from their university positions?

Mr. BEYER. Well, this would have to be done on a carefully planned basis, where there is sufficient depth at a particular university, so that their academic and research program, and our own Vela tasks, are not seriously disrupted. Obviously, moving any key person away would have some effect on organization. On the other hand, such a center would bring together an interdisciplinary group. By that I mean seismologists, physicists, electronics engineers, et cetera, together as a team. It is likely to be very productive in terms of making progress toward better detection schemes and better systems.

Now, this in no way will infringe on the individual research tasks being conducted at the individual universities, except by the way in which one person is pulled away for a while on a loan. The intention, too, is not to make this a large operation. Possibly we would start with the order of three to five key people, and build gradually and slowly. So it would not disrupt not only the rest of the Vela work, but also it would not disrupt the university community and the key industrial centers from which we hope to get these people.

Chairman HOLIFIELD. Proceed.

Mr. BEYER. (4) Nuclear and chemical explosion series: Although chemical explosions can be used to substitute to some extent for nuclear explosions, inherent differences in the initial nature of the energy pulse are great enough to require a combined nuclear-chemical explosion series for testing detection theories, instruments, techniques, proposed treaty control stations, and onsite inspection methods. Present plans call for 13 completely contained underground nuclear detonations and about 23 underground chemical explosions.

A series of chemical and nuclear explosions are planned for firing at various depths, yields, media, locations, and cavity sizes in order to provide a minimum spectrum of controlled signals for research and to provide a basis for evaluating detection system design with actual signals. Six of the nuclear explosions are to be at the Nevada test site.

Representative VAN ZANDT. Mr. Chairman, may I ask a question at this point?

Mr. Beyer, the cost of these planned underground detonations is not included in the moneys you are spending now for 1960 and 1961 and will ask for for fiscal 1962?

Mr. BEYER. Well, sir, it is included in our budget.

Representative VAN ZANDT. It is included?

Mr. BEYER. Yes, sir. And the expenditures thus far in fiscal 1960 and 1961, under the explosion program, are to get the necessary equipment together and everything organized and ready to proceed.

Representative VAN ZANDT. I think, as you said a moment ago, the AEC expenditure is not included?

Mr. BEYER. Not at all. However, in their cost summary for the Vela project, they can call this out. And I will give you a breakdown of the various task costs here in just a moment.

Representative VAN ZANDT. Very well.

Mr. BEYER. All right, sir.

A series of 23 0.1 to 100-ton high-explosive detonations known as Groundhog is planned in fiscal year 1962 and fiscal year 1963 to study additional concepts of signal location and interpretation in sites where major explosions are not feasible. Funds required to initiate this series have not been approved but are anticipated in the near future.

Chairman HOLIFIELD. I think it would be pertinent at this point to have the record show that these plans are all tentative in nature and subject, of course, to the determination at the Presidential level that such tests are necessary for the national interest.

Mr. BEYER. Yes, sir.

Chairman HOLIFIELD. And so, when we speak of these plans, we should understand that this is a tentative program.

Mr. BEYER. As Brigadier General Betts indicated, the AEC is responsible for providing the nuclear devices, preparing the sites and cavities in which to fire the devices, the actual firing, and making the yield measurements. Similar support is also provided by the AEC for all chemical detonations at the Nevada test site, of which one is currently planned. The Defense Atomic Support Agency (DASA), which coordinates all DOD activities at the shot sites, manages the close-in measurement programs to observe the conversion of shock waves to elastic waves, and executes all chemical explosions required outside the Nevada test side. DASA also provides the public information service for this portion of Vela Uniform.

AFTAC prepares the technical requirements and directs the acquisition of off-site measurements by over 50 contract monitoring units by cooperative seismic observers at the universities and by the geophysical prospecting industry. AFTAC also prepares the final report which will include the analysis of all data obtained. AEC, DASA, AFTAC, and ARPA representatives will testify on the planned series of research explosions for Vela Uniform.

(5) Information collation: With the rapid growth of Vela Uniform, it is necessary to insure that all researchers are informed of the work being undertaken and the results being obtained. In addition, other technical literature must be rapidly collated, reviewed, abstracted, and disseminated. Much of this function is now being performed by the Vela Seismic Information Analysis Center (Vesiac) at the University of Michigan's Institute of Science and Technology.

(6) Special studies: ARPA has earmarked funds for conducting studies of this type as required by the executive branch of the Government.

(7) On-site inspection: Under AFTAC, a comprehensive research and development program including aerial, surface, and subsurface techniques for positively identifying the cause of a "suspicious seismic event" is well underway. Dr. Bates of ARPA will testify on this later in these hearings.

Vela Uniform funding: As to the funding history of Vela Uniform Project, in fiscal year 1960, the initial year of the program, $7,835,000

became available. About 40 percent of this went into initiating the research program and another 35 percent went into preparing for the series of chemical and nuclear research detonations. The other tasks, namely, systems development, worldwide standard seismic network, special studies, on-site inspection, and information collation, received less than a million dollars each. The amounts by task were shown on the next chart (chart 5, p. 65). Fiscal year 1961 funding of $33,700,000 permitted the program to operate on a full-fledged basis for the first time. The $10.7 million was allocated for the research explosion series, while $9.3 and $8.6 million, respectively, were allocated to the research and systems development tasks. The cost of establishing and operating the worldwide standard seismic network reaches its peak of $2.6 million during fiscal year 1961 and declines to about a million dollars a year thereafter. The amount of $2.1 million has also been made available in fiscal year 1961 for work on onsite inspection, while the amounts of $300,000 and $123,000 are assigned to special studies and information collation, respectively. The basic fiscal year 1962 Department of Defense budget for the Vela Uniform program is $25 million.

Vela Sierra, Vela Hotel programs (high altitude): The problems of detecting high altitude nuclear explosions (beyond 30 to 50 kilometers above the earth) were discussed by the Conference of Experts and Technical Working Group I at Geneva. This detection problem was considered because it is believed to be technically feasible to do useful nuclear weapons testing in space should there be a desire to do so. It should be noted that a high-altitude research program is included in the treaty draft tabled [3] at nuclear test ban treaty negotiations in Geneva by the United States and the United Kingdom.

Both of the Project Vela high altitude programs include research and development aimed at improving knowledge of natural background radiations that exist in space. In the Vela Sierra program, particular emphasis is placed on determining the effect of these natural radiations on the upper atmosphere since the Sierra program is concerned with developing and improving detectors located on the earth's surface. Under the Vela Hotel program, which is concerned with satellite detectors, there is also a need for an understanding of the background radiation problem.

Although theoretical studies indicate that the radiations are not likely to cause difficulty, both high-altitude programs include work to obtain experimental evidence to measure these backgrounds. If unexpected background radiations are present, the detection and discrimination of radiations from a nuclear detonation in space is likely to be more difficult.

Vela Sierra program: The Vela Sierra program is aimed at developing and improving instruments and techniques for detecting high-altitude nuclear detonations by instruments located on the ground. The effort on detection techniques is divided into three major categories: optical, radiofrequency, and geophysical. Another part of the Vela Sierra program is concerned with the development of specific requirements for the construction and operation of a detection system. In this connection, AFTAC is preparing a plan to equip one

[3] This is a diplomatic term denoting "introduced" or "proposed" rather than the common usage in this country.

Geneva-type station with ground-based high-altitude detection equipment.

The optical work includes research and instrument development on techniques for detecting the light signal and fluorescence induced in the upper atmosphere by a high-altitude detonation. Most of this work is being done by the AEC laboratories.

The radio frequency effort is concerned with methods for detecting the electromagnetic pulse from the detonation, the effect of a nuclear detonation on the ionosphere by means of measurements of long-range and vertical radio transmissions, and some work is also being done to optimize the effectiveness of backscatter radar for this purpose. This work is being done by the Bureau of Standards at Boulder, Colo., the Navy Electronics Laboratory, and contractors.

The geophysical work is largely with studies of perturbations in the earth's magnetic field, and electrical currents that are likely to occur when a high-altitude nuclear detonation occurs. This work is being conducted by the U.S. Army Signal Corps Laboratories.

As indicated on chart (chart 6 (p. 65)), ARPA provides technical direction and coordination and is the primary funding and action agency. The Air Force Technical Applications Center is the principal technical adviser to ARPA for this program and performs detailed supervision of work performed by contractors. Where a specific capability exists within the Government, work is assigned directly by ARPA order.

The funding made available for the execution of the Vela Sierra program was $450,000 in fiscal year 1960, $3.9 million in fiscal year 1961, and $2 million in fiscal year 1962. Lt. Comdr. D. E. Chandler, U.S. Navy, ARPA's Chief for the Vela Sierra program, and Dr. William Ogle of the AEC Los Alamos Scientific Laboratory, will discuss this work in greater detail.

Vela Hotel program: The uncertainty of trying to predict the future course of weapons development and the role space testing may play creates a need for gathering technical information and data on detection in the space environment in order to be able to evaluate the implications of future events.

Thus, the fundamental requirements for performing research and development on various detection techniques is to gain knowledge and a better understanding of physical phenomena related to detection problems.

ARPA has established the basic objectives of a proposed R. & D. program which will satisfy specific minimal requirements over the next several years as follows:

(*a*) Establish an experimental basis for performance estimates. Performance capabilities currently being assumed for the various detection techniques are based on scientific estimates only and do not represent guarantees.

(*b*) Future international negotiations on banning nuclear tests should be conducted from as enlightened a technological position as possible. Supporting information and experimental data should be available for determining control effectiveness of operational surveillance systems which might be agreed upon.

(*c*) An experimental basis for evaluating the effectiveness of such countermeasures as shielding should be available. At the present time, all evaluations of effectiveness of various countermeasures are also based on scientific estimates only.

In order to achieve the general objectives given above, it is proposed to perform studies, conduct experiments, and test detection devices in order to obtain information and experimental data for defining, and evaluating the control effectiveness of satellite detection systems which meet the Geneva Technical Working Group I performance estimates. Initially, the principal effort will be directed toward obtaining information needed to develop and evaluate a far earth satellite system.

At the present time, the primary experimental effort proposed for Vela Hotel is based upon neutron, gamma ray and X-ray detection techniques. From the chart (chart 7, p. 66) it can be noted that ARPA is proposing a sequential effort starting with piggyback flights on NASA/DOD space vehicles and balloons and phasing into experimental satellite flights specifically designed for Vela Hotel R. & D. program requirements. The piggyback program has been approved and is well underway. The satellite program has recently been approved by the Department of Defense.

Barring unexpected technological pitfalls, it is estimated that enough experimental data will be available after the 1964 flights to define and design an initial far earth operational detection system with a capability not less than that estimated by the Geneva TWG–I and to evaluate economic practicability.

By mutual agreement between the DOD and AEC, the DOD (ARPA) has overall responsibility for the Vela Hotel program. The AEC is providing substantial support by providing the detectors and associated logics developed at its laboratories.

Chart 8 (p. 66) illustrates the management and technical responsibility relationships which apply during the implementation of the program. Under the supervision of ARPA, technical supervision of the AEC and DOD portions of the program will be the responsibility of a joint AEC–DOD technical group. This group, acting through normal channels in each agency, is responsible for establishing specifications, insuring complete coordination among the various tasks of the approved program, and providing technical direction to the contractors.

The AEC is responsible for providing the detectors and associated logics and analysis of the telemetered information from these devices.

The Air Force is providing system design, system assembly and test, boosters, and satellite vehicles (spacecraft). The latter includes integrating the detectors and logics into the satellite vehicles.

Close technical liaison will be maintained with NASA throughout the program.

Maj. J. A. Poulson, USAF, who is ARPA's chief for the Vela Hotel program; Dr. Harold Argo, of the AEC's Los Alamos Scientific Laboratory; and Dr. B. P. Leonard, of Aerospace Corp., who is an ARPA consultant for the Vela Project will testify in greater detail on the program.

Considerable progress has been made in getting each of the three Vela programs solidly underway. Some useful results have already been obtained and many more are anticipated. We have been gratified with the very solid support and participation that all universities, contractors, and Government agencies have given to the Vela Project.

Chairman HOLIFIELD. Thank you, Mr. Beyer.

Can you give us the size and composition of the office staff on Vela Uniform and Sierra and Hotel?

Mr. BEYER. I will try. The Vela Uniform staff is headed by Dr. Charles Bates. Working with him full time is Dr. Joseph Berg. He is on loan from Institute for Defense Analyses. Mr. Donald Clements also assists in this program, and Mr. Theodore George, and also Major Harris of the Air Force. This comprises the professional staff on the Vela Uniform program.

In the case of the Vela Sierra program, Commander Chandler is the program chief. He is assisted full time by Dr. Benson Tucker, who is on loan from IDA.

In the Vela Hotel program, Maj. John Poulson is the program chief. He is assisted by Dr. Alois Schardt and Mr. Grady Frank.

Then, as is indicated, I am the Deputy Assistant Director, and Dr. George Bing is the new head of the Nuclear Test Detection Office, which has the direct responsibility for the Vela Project.

Chairman HOLIFIELD. There are about 10 people, then, in your director's group?

Mr. BEYER. Roughly.

Chairman HOLIFIELD. That seems to be a small group to supervise this program.

Mr. BEYER. This is the professional staff directly associated with the technical problems involved. Throughout the ARPA staff there are others who support us on fiscal matters, on policy matters, public relations matters, and the like. I would say that the total support in this area would amount to an additional three or four professional-type people.

Chairman HOLIFIELD. Dr. Latter testified that research has not yet given a clue to improving the Geneva system to permit detection and identification of shielded tests in space. And you say that by 1964 Vela Hotel will be able to specify an operational system equal to the Geneva TWG–1 capability. Do you expect that by 1964 your system will have a capability for detection and identification of shielded nuclear tests in space?

Mr. BEYER. I would like to generalize first on the question, and then answer more specifically.

I do not believe TWG–I was concerned with the shielding problem, and therefore I believe that by that time, unless, as I indicated, there are unforeseen problems, we should be able to equal or maybe even, hopefully, supersede slightly the performance estimates of TWG–I for unshielded weapons.

Now, as Major Poulson will indicate, we have some studies underway on the effects of shielding, and these are in no way conclusive, because they are not far enough along yet, but certainly if it is technically feasible to put a shield up in the proper relationship with a nuclear device, and there are no satellite-borne detection systems in direct line of sight with the unshielded portion of the weapon, the detection range will be degraded. And while it is technically feasible to put a shield up, it is not simple, and no one has yet done it. However, it could be done.

Chairman HOLIFIELD. Well, you are in the position, I am afraid, that the seismologists have been in. They will probably be struggling along to catch up with the decoupling, and you will be struggling along to catch up with shielded tests, and probably so far behind that from the standpoint of the efficiency of detection you will not be able

to detect this situation. The offensive, in other words, seems to be going a little faster than the defensive in this case.

Mr. BEYER. I would say that in the case of the high altitude tests, using shielded devices, to my knowledge, at least, no one has actually done this, whereas there have been some underground tests to explore the decoupling principle through the Cowboy [4] Experiments which the AEC supported and conducted. The problem of placing such a shield in space and at the proper position with respect to the device and the detection system is not easy, and it is not like just digging a hole, so to speak.

The people trying to counteract the system have a little bit more difficult problem in space, probably, than they do underground.

Chairman HOLIFIELD. Any questions?

Mr. Price?

Representative PRICE. How many commercial contracts did you state have been let by the Vela Project so far?

Mr. BEYER. I would have to refer to these numbers.

Representative PRICE. Did you not state an overall figure, but you did not have any breakdown?

Mr. BEYER. I do not have the breakdown on hand in the commercial ones only, sir. We have university and industry together.

Representative PRICE. You do not have them separate?

Mr. BEYER. We could do this, and it is possible with this list that we gave you to derive this rather quickly.

Representative PRICE. Are the ones allocated to industry in the same small category that most of these are in this book you have presented to the committee?

Mr. BEYER. Several are very large. One on the procurement of worldwide network equipment involves the purchase of equipment and therefore will be in the million dollar category.

Representative PRICE. Who has that contract?

Mr. BEYER. Geotechnical Corp.

Representative PRICE. Geotechnical?

Mr. BEYER. Yes, sir, and they are being assisted by several other contractors.

There are several large studies being conducted by industry. Texas Instruments Co. has one large study contract. There are also some of the very small category given to industry, too. It just depends on the nature of the work to be done.

Representative PRICE. I noticed at least two foreign contracts listed. Is that due to the fact that they have a very special area in which you have to go to these particular places to get the men who are qualified to conduct this particular research?

Mr. BEYER. Yes, sir. In the case of Dr. Böth in Sweden, he had done some previous work on crustal studies in the Icelandic area. He had a great deal of data which he wanted to analyze. Since it relates to improving the understanding of the effect of the crust on seismic signals, and we would like to look at the problem on a more international basis, we are supporting him. His contract is very small.

In the case of a South African contract, Dr. Hales, there, is working on the rock-burst question. In other words, deep in the earth, where

[4] Cowboy series: A series of chemical explosions in a salt dome in Louisiana which gave experimental proof of the theory of decoupling.

there is a cavity at an old mine or a void where the rocks cave in, they release a very major amount of energy, which acts like an earthquake or an underground explosion. From this study we are getting information on rock-burst energy, on travel times, that is, the rate at which seismic signals travel through the earth, and so on, from areas other than the United States. These are two examples that come to mind.

Representative PRICE. I am not in any way objecting to this. I just wanted to bring out the reason why you felt it necessary to go outside of our own country for the particular experiments and research projects.

Now, on page 1 of your statement, you say, "after a number of studies, including those by advisory groups and activities within the DOD." Would you elaborate a little on the primary groups involved there?

Mr. BEYER. Yes, sir. There were two advisory groups operating under Dr. York's general guidance. One was headed by Dr. Press. I do not recall the exact title of this group, but it was a group of seismologists.

Representative PRICE. I hope they were more effective than the advisory groups on the aircraft nuclear propulsion program.

Mr. BEYER. Well, sir, these are different people, and I do not know anything about that other group, but these people have been most effective.

The second group was headed by Dr. Panofsky, and it was concerned with the high altitude question.

The third study that comes to mind was made by AFTAC. They proposed a general plan for attacking the Vela Project. And a large number of the thoughts that they had in that initial program are involved in the current work that is underway.

Representative PRICE. That is all I have, Mr. Chairman.

Representative VAN ZANDT. Mr. Beyer, I am impressed by the magnitude of this organization. What results could our Government use in the event that tests are resumed? I am thinking now of the development of instruments that could be used for other purposes, and so forth. Does that apply throughout the program of this organization?

Mr. BEYER. Well, I am having a little difficulty visualizing what you mean by "if tests are resumed." I can tell you what some of the benefits are if a treaty is not signed, that kind of thing.

Representative VAN ZANDT. When tests are resumed.

Mr. BEYER. Yes, all right, there will be a very great improvement in the understanding of earthquakes, not only on the basis of those that occur in the United States, but also throughout the world. There will be better instruments for detecting earthquakes and localizing explosions or earth disturbances of major magnitude.

There will be a major improvement in the understanding of how the earth transmits signals of this sort. And I think there are certain implications of this kind of thing of interest to a number of parts of the Government.

Also, if more is known about earthquakes, it should be possible to give increased protection to structures and people insofar as earthquakes are concerned. We are not going to get into this program, but the basic data and the instruments related to these things will be directly useful.

In the case of the satellites, there will be space background radiation measurements that have time resolution which are not in anyway duplicated in any other space program, either conducted by NASA or the DOD. This may or may not be significant.

For example, if there are background radiations which are of major magnitude, this may become important to people in space vehicles and to the survival of equipment within these space vehicles.

But this is only in a very narrow range of backgrounds that might exist in space. We are exploring, so to speak. But we have to have this, so that we can determine what noise detectors might have to listen through.

In the case of the ground-based high altitude detection instrumentation, I would say that there will be improvements in the understanding of the behavior of the ionosphere. We will look at the way in which the signals from cosmic sources are affected by natural background noise fluctuations. This in turn might be of some interest to, again, radio communication.

These things are not aimed directly at that, they are just side issues. They may be very useful, or they may not. At least, the results from the project will improve our general understanding of the sciences involved.

In the Uniform case, I would say there is likely to be general improvement in the overall earthquake problem, entirely apart from any explosion.

Representative VAN ZANDT. Then we will say in general terms there will be domestic benefit.

Mr. BEYER. Yes, sir.

Representative VAN ZANDT. Have you given thought, Mr. Beyer, to the size of an organization that would be necessary to police a treaty?

Mr. BEYER. Several studies have been made of this, and I believe two were reported last year. We have not gone into any greater detail on this. But as I recall, the numbers of people required to man a 180-station system would be on the order of 20,000 people. This would not, however, include anything to do with the satellite system since it was not included in any of these estimates.

Representative VAN ZANDT. The figure of 20,000—this applies worldwide?

Mr. BEYER. Yes, sir.

Representative VAN ZANDT. Involving all nations participating as members of the treaty?

Mr. BEYER. Yes, sir.

Representative VAN ZANDT. Have you any idea of the cost, in round figures, the annual cost?

Mr. BEYER. Oh. The annual operating cost?

Representative VAN ZANDT. Yes.

Mr. BEYER. I would say on the order of a billion dollars, after it is installed.

Representative VAN ZANDT. A billion dollars after it is installed. And then if we added the policing of the outer space——

Mr. BEYER. That is certainly a very rough figure, and I probably would want to change it after I had a chance to think it through.

Representative VAN ZANDT. And of course the overall costs would be divided among the nations participating?

Mr. BEYER. That depends on the way the treaty is established. I do not know.

Representative VAN ZANDT. That is all, Mr. Chairman.

Chairman HOLIFIELD. Mr. Beyer, following up the line of questioning I gave you before on the detection of high altitude nuclear explosions by means of apparatus installed in satellites, I read from page 590 of the appendix of our hearing last year,[5] appendix 3, which was the report of the Technical Working Group No. I on the detection and identification of high altitude nuclear explosions, as follows, in paragraph 4:

> When the explosion is shielded by means of a reflector having large dimensions and a weight which is comparable to the weight of the device, the estimated detection distances of high-yield nuclear explosions in the megaton range can be reduced by one or two orders of magnitude, and by an even greater amount for low-yield nuclear explosions in the kiloton range.

This seems to indicate that the Technical Working Group thought that this was within the realm of practical feasibility.

Mr. BEYER. I think I should say that I was in error.

Chairman HOLIFIELD. I do not know as you are in error. I just thought that the record should have both sides of this, because if they are speaking of shielding, which is comparable to the weight of the device, it is very easy to compute the weight of the nuclear warhead as against the shielding, and then I think you and I would both agree that the total weights could be put into orbit at this time.

Mr. BEYER. Oh, there is no doubt about that. I am sorry, I did not recall TWG–I considering the shielding question.

Now, as to the ability to put sizable devices and shields into space, the thrust does exist to do this. I was not in error on that point, no.

Mr. RAMEY. Has your organization made any effort to obtain the interest of scientists such as Dr. Bethé—you mentioned Dr. Panofsky earlier—and others, for example, that we had on our technical panel last year, in this program?

Mr. BEYER. Well, we have certainly worked with the President's Scientific Advisory Committee, and in a number of instances provided information for them, and Dr. Bethé is a member of that group, and he is kept apprised of the program through it, but not in the detailed sense.

As to Dr. Press and Dr. Panofsky, they are both members of that same committee, and also are members of the ARPA Ad Hoc Group on Nuclear Test Detection, and they are informed in rather great detail about the program.

Mr. RAMEY. How about Dr. Jay Orear and Dr. Richard Roberts? They were on our panel last year.

Mr. BEYER. I have not talked to them, and I do not recall anyone having contacted them, but should they express an interest in the program, we would be very happy to discuss it with them.

Mr. RAMEY. How about Dr. Urey, Harold Urey?

Mr. BEYER. I think he is generally informed on the program, but not directly by us.

Chairman HOLIFIELD. Are there any further questions?

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[5] Hearings of April 1960 on Technical Aspects of Detection and Inspection Controls of a Nuclear Weapons Test Ban, Part 2.)

If not, we will excuse you, Mr. Beyer. Thank you very much. I am sorry we had to interrupt you so many times with rollcalls.

(Mr. Beyer's formal statement follows:)

STATEMENT BY C. M. BEYER, DEPUTY ASSISTANT DIRECTOR, NUCLEAR TEST DETECTION OFFICE, ADVANCED RESEARCH PROJECTS AGENCY

Mr. Chairman and members of the Joint Committee, I will summarize the objectices of the Vela Project, the management approach, the funding aspects and refer briefly to the technical status of each of the three programs.

The assignment of the Vela Project to the Department of Defense (DOD) resulted from a meeting on April 23, 1959, during which Dr. Killian, Special Assistant to the President on Science and Technology, Chairman McCone of the Atomic Energy Commission and Deputy Secretary of Defense Quarles discussed the implementation of the reports from Panels on High Altitude Detection and Seismic Improvement. At this meeting, it was agreed that—

(*a*) The specific recommendations of both panels would be implemented.

(*b*) The DOD would accept overall responsibility for implementing the high altitude panel recommendations with support from the Atomic Energy Commission (AEC) and the National Aeronautics and Space Administration (NASA).

(*c*) The DOD would accept primary responsibility for implementing the recommendations of the Seismic Improvement Panel but that the AEC would implement the nuclear and HE explosions in cooperation with the DOD.

After a number of studies including those by advisory groups and activities within the DOD, the Secretary of Defense assigned the responsibility for the project, later named Vela, to the Advanced Research Projects Agency. The assignment is to "Perform research, experimentation and systems development to obtain at the earliest practicable date a system for the detection of nuclear explosions both underground and at high altitude."

This assignment includes the following major technical objectives:[6]

(*a*) Conduct research to better understand the nature of the nuclear detonation signals to be detected and how to discriminate them from other signals.

(*b*) Develop and evaluate existing and new techniques and instruments for detecting and identifying nuclear detonation signals.

(*c*) Develop and evaluate systems for improving the detection and identification of nuclear detonations, and the problems involved in the operation of these systems.

(*d*) Perform research, development, and evaluation of onsite inspection techniques.

These technical objectives contain two basic elements:

(*a*) Upgrading the sciences involved.

(*b*) Evaluating and applying available technical information to the "Geneva-proposed system" problem.

The Vela Project is divided into three programs: (1) Vela Uniform, which is concerned with the detection of underground and underwater nuclear detonations; (2) Vela Sierra, the detection of high altitude nuclear detonations by instruments located on the earth, and (3) Vela Hotel, the detection of high-altitude nuclear detonations by means of satellite-borne instruments.

The approach being used includes—

(1) ARPA defines the specific problem area.

(2) An ARPA order is issued describing the work to be done, the funds available, the reporting procedure and the period during which the order is effective. These orders are directed to an agent which is a Government activity with special competence in the field. The agent itself undertakes or contracts for the work with a university, a nonprofit organization, or an industrial concern.

(3) The agent is responsible for the detailed technical supervision and administration of the assigned work. This includes the responsibility for maintaining direct and close contact with the scientists doing the work, receiving the reports, commenting on them, and taking appropriate action. ARPA also receives a copy of each report for review. When ARPA is directly responsible for the work and an agent is not used it gives direct guidance to the contractor.

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[6] These apply both to subsurface and high altitude detection.

(4) The ARPA staff periodically reviews all work under the project which is then summarized for the ARPA Ad Hoc Group on Nuclear Test Detection, which provides further technical guidance. In addition, one general summary of the project was held in the form of a symposium and a second symposium to be held jointly with the California Institute of Technology is planned for October or November of this year.

Now I would like to show the Government organizations that have major responsibilities in the Vela Project. This chart (p. 63) shows the principal organizations participating in the project, providing advice, or receiving information from those engaged in it. Regular guidance is received from the Office of the Special Assistant to the President for Science and Technology, and certain aspects of the Vela Project have been reviewed by the President's Scientific Advisory Committee. The U.S. Disarmament Administration in the Department of State has been kept informed of progress on the project through the Office of the Assistant Secretary of Defense for International Security Affairs, by direct contact with ARPA and by direct contact wth agents working on the project.

The Atomic Energy Commission is active in all three programs of the Vela Project and the nature of this participation will be mentioned later by witnesses from the AEC. Within the Department of the Interior, the Geological Survey is participating through an increased crustal study program and is aiding in the planned series of research explosions that will be discussed by other witnesses. The Department of Commerce, through the Cost and Geodetic Survey, is procuring and installing a network of standard seismic stations to be located throughout the world. Also, the Coast and Geodetic Survey is one of the numerous agencies assisting in the coordinated program of extensive measurements to be made in connection with the explosion program.

The National Bureau of Standards gives major support in the Vela Sierra program. The National Aeronautics and Space Administration is participating in the joint AEC, NASA, and DOD planning effort under the Vela Hotel program.

A number of units within the Department of Defense are engaged in the Vela Project. Those principally involved are shown on this chart (p. 63). ARPA, which is in the Office of the Director of Defense Research and Engineering, is responsible for the technical direction and overall management of the project and its three programs. Also within the Office of the Director of Defense Research and Engineering, the Special Assistant for Arms Control has maintained regular contact with the project and provided policy guidance. Similarly the Office of the Assistant Secretary of Defense for International Security Affairs is kept currently informed of the project's work. The Assistant to the Secretary of Defense for Atomic Energy, who is also the chairman of the DOD/AEC Military Liaison Committee, provides important guidance for Vela.

Through the Corps of Engineers and the Signal Corps, the Department of Army participates in the Vela Sierra program.

In the Department of the Navy, the Office of Naval Research is the overall coordinator of the Navy's technical effort in the Vela Uniform program, including the work done by the Bureau of Ships laboratories and its contractors. The Office of Naval Research, its contractors, and a Bureau of Ships laboratory are also participating the Vela Sierra program.

Within the Department of the Air Force, the Air Force Technical Applications Center (AFTAC), serves as a principal advisor on the Vela Project and is responsible for a substantial amount of work, both inhouse and under contract, on the Uniform and Sierra programs. Under the Air Force Systems Command, the Air Force Space Systems Division is primary agent for the Vela Hotel program and provides the chairman for the joint effort in this area in conjunction with NASA and the AEC. In addition, the Air Force Office of Scientific Research and the Cambridge Research Laboratories are conducting research and development on the Vela Uniform and Sierra programs.

Very substantial technical support and funding is provided by the AEC for the preparation and firing of the nuclear and the one large chemical explosion and the support for the other two Vela programs. Approximately three-fourths of the other technical work and all of the remaining funds are provided by the DOD. Since a great deal of Project Velva work underway is related to other DOD objectives, it is possible to take davantage of results obtained in other programs, to use existing DOD facilities, to avoid multiple supervision costs by using DOD agents, and to minimize the cost of record keeping by using exist-

ing DOD reporting and accounting procedures. Also of considerable importance to the project is the ease with which it is possible to obtain advisory and direct assistance from DOD consultants and contractors.

The DOD funding for Project Velva in fiscal year 1960 and fiscal year 1961 and expected in fiscal year 1962 is shown on the next chart (p. 64).

Next I will summarize the way in which each program is being managed and briefly refer to its status. Further details concerning each program will be presented by the manager for each program.

VELA UNIFORM PROGRAM

The easiest way to manage a program is to concentrate the skills in one place and assign responsibility commensurate with capability. With the variety and quantity of work required in this program such an approach is not practicable. This is the case because this country has no major center or small group of centers that possess the required level and depth of technical competence required. To organize such a single center and staff would probably remove most of the limited number of earthquake seismologists from their universities or industrial companies. In the case of university scientists, current work on Vela Uniform tasks would probably be disrupted, and the training of graduate and undergraduate students which will ultimately alleviate the shortage of talent in seismology would be degraded. As a result of this situation, ARPA has 11 agents handling the 80 major contracts in the Vela Uniform program, each agent being assigned a technical area. (See chart, p. 64.)

To improve the detection, identification, location and on-site inspection capabilities for subsurface (i.e., underground and underwater) nuclear detonations which may be either tamped or decoupled, substantial improvements must be made if the number of "false alarms" is to be markedly reduced and small tamped or moderate size decoupled explosions are to be detected in an operationally acceptable number of instances. The Vela Uniform program which is concerned with making improvements in these areas is divided into the following seven task areas:

(1) Worldwide standard seismological network

With the assistance of the Coast and Geodetic Survey as an agent, the first international seismological research network utilizing modern, calibrated, and standardized instrumentation is being established. The data from this network will be of high quality and will be made available to all seismologists for conducting broad research studies on world seismicity, wave propagation characteristics, location of earthquake epicenters and depth of foci, seismic noise, and a better understanding of differences in seismic waves originating from all types of earthquakes.

(2) Research programs

This task is aimed at broadening our knowledge of seismic and nonseismic techniques for detecting subsurface nuclear detonations. Different research areas have been defined for the 11 governmental agencies assisting in this task. For example, the Defense Atomic Support Agency is responsible for an integrated theoretical and experimental research program on the conversion of contained blast energy into a seismic wave train. The Air Force Cambridge Research Laboratories is responsible for applied seismology, the Air Force Office of Scientific Research for basic seismology and allied sciences, the Air Force Rome Air Development Center for underground electromagnetic pulse investigations, and the Air Force Technical Applications Center for improved monitoring and for display equipment and techniques. Research and development on the dectection of undersea explosions by hydroacoustic and radiochemical techniques is the responsibility of the Office of Naval Research and the Bureau of Ships.

In the Department of Interior, the U.S. Geological Survey has undertaken a major investigation of seismic propagation paths in southern California and Nevada directed toward determining how accurately seismic sources can be located and identified in a well-calibrated region. Foreign seismic research has been arranged through the European office of the Air Force Office of Aerospace Research, the Europen research office of the U.S. Army, and the Office of the Secretary of Defense. Work is already underway in Sweden, Israel, and Canada. A wide variety of topnotch talent is working on this task. Thirty-five contractors are involved which include such well-known names as Bell Telephone

Laboratories, Standard Oil of New Jersey, Standard Oil of California, and Texas Instruments, Inc., and such noted universities as the Massachusetts Institute of Technology, the California Institute of Technology, the University of Michigan, and the University of California. Dr. Charles Bates, ARPA's Chief of the Vela Uniform program will summarize the research work underway.

(3) *System development*

The Air Force Technical Applications Center (AFTAC) is the agent for the major part of this task. Five types of seismic detection stations will be built for study and evaluation:

(*a*) One Geneva-type station, located at Fort Sill, Okla., has been in operation since October 1960. Construction of four more will commence this year on the Geneva-recommended 600-mile spacing in California, Oregon, Montana, and Utah.

(*b*) Construction of the two advanced detection stations with the improvements recommended by the Berkner Panel, each station which may include as many as 100 seismometers will be initiated this year; one will be in Arizona and the other is likely to be located in the Southeast.

(*c*) A contract is being negotiated for the development of an unmanned station.

(*d*) A simplified manned station concept will be tested at one of the Geneva or Berkner-type stations.

(*e*) A minimum of two installations of seismometers in deep holes are planned.

A systems development field station will provided an installation capable of conducting prototype network and "control systems headquarters" studies and tests, and will also provide a facility for evaluating equipment likely to be used in the prototype stations.

In addition to the system effort I have just described, ARPA is planning to arrange with one or more universities for the establishment of a Vela Uniform research and evaluation center. The center will be an independent effort aimed at assessing the capability of subsurface detection systems, and have a major interest in research in the earth sciences related to the Vela Uniform program. Although the concept of this center and the method for implementing it are under consideration, it is intended that this center will be a focal point where top-caliber scientists and engineers, as well as graduate students, can work independently or cooperatively with academic, industrial, and governmental organizations for the solution to research and systems problems related to the improvement in the detection, identification, and location of subsurface nuclear detonations. Dr. Carl Romney of AFTAC will discuss the system task in greater detail later in this hearing.

(4) *Nuclear and chemical explosion series*

Although chemical explosions can be used to substitute to some extent for nuclear explosions, inherent differences in the initial nature of the energy pulse are great enough to require a combined nuclear-chemical explosion series for testing detection theories, instruments, techniques, proposed treaty control stations, and onsite inspection methods. Present plans call for 13 completely contained underground nuclear detonations and about 23 underground chemical explosions.

A series of chemical and nuclear explosions are planned for firing at various depths, yields, media, locations, and cavity sizes in order to provide a minimum spectrum of controlled signals for research and to provide a basis for evaluating detection system design with actual signals. Six of the nuclear explosions are to be at the Nevada Test Site.

A series of 23 0.1 to 100-ton high-explosive detonations known as Groundhog is planned in fiscal year 1962 and fiscal year 1963 to study additional concepts of signal location and interpretation in sites where major explosions are not feasible. Funds required to initiate this series have not been approved but are anticipated in the near future.

Explosion program implementation

As Brigadier General Betts indicated, the AEC is responsible for providing the nuclear devices, preparing the sites and cavities in which to fire the devices, the actual firing, and making the yield measurements. Similar support is also provided by the AEC for all chemical detonations at the Nevada Test Site. The Defense Atomic Support Agency (DASA), which coordinates all DOD activities

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at the shot sites, manages the close-in measurement programs to observe the conversion of shock waves to elastic waves, and executes all chemical explosions required outside the Nevada Test Site. DASA also provides the public information service for this portion of Vela Uniform.

AFTAC prepares the technical requirements and directs the acquisition of offsite measurements by over 50 contract monitoring units by cooperative seismic observers at the universities and by the geophysical prospecting industry. AFTAC also prepares the final report which will include the analysis of all data obtained. AEC, DASA, AFTAC, and ARPA representatives will testify on the planned series of research explosions for Vela Uniform.

(*5*) *Information collation*

With the rapid growth of Vela Uniform, it is necessary to insure that all researchers are informed of the work being undertaken and the result being obtained. In addition, other technical literature must be rapidly collated, reviewed, abstracted, and disseminated. Much of this function is now being performed by the Vela Seismic Information Analysis Center (Vesiac) at the University of Michigan's Institute of Science and Technology.

(*6*) *Special studies*

ARPA has earmarked funds for conducting studies of this type as required by the executive branch of the Government.

(*7*) *Onsite inspection*

Under AFTAC, a comprehensive research and development program including aerial, surface, and subsurface techniques for positively identifying the cause of a "suspicious seismic event" is well underway. Dr. Bates of ARPA will testify on this task later in these hearings.

VELA UNIFORM FUNDING

As to the funding history of Vela Uniform Project, in fiscal year 1960, the initial year of the program, $7,835,000 became available. About 40 percent of this went into initiating the research program, and another 35 percent went into preparing for the series of chemical and nuclear research detonations. The other tasks, namely, systems development, worldwide standard seismic network, special studies, onsite inspection, and information collation, received less than a million dollars each. The amounts by task are shown on the next chart (chart, p. 65). Fiscal year 1961 funding of $33,700,000 permitted the program to operate on a full-fledged basis for the first time. The $10.7 million was allocated for the research explosion series, while $9.3 million and $8.6 million were allocated to the research and systems development tasks, respectively. The cost of establishing and operating the worldwide standard seismic network reaches its peak of $2.6 million during fiscal year 1961 and declines to about a million dollars a year thereafter; $2.1 million has also been made available in fiscal year 1961 for work on onsite inspection, while the amounts of $300,000 and $123,000 are assigned to special studies and information collation, respectively. The basic fiscal year 1962 Department of Defense budget for the Vela Uniform program is $25 million.

VELA SIERRA, VELA HOTEL PROGRAMS—HIGH ALTITUDE

The problems of detecting high altitude nuclear explosions (beyond 30 to 50 kilometers above the earth) were discussed by the Conference of Experts and Technical Working Group I at Geneva. This detection problem was considered because it is believed to be technically feasible to do useful nuclear weapons testing in space should there be a desire to do so. It should be noted that a high-altitude research program is included in the treaty draft tabled at nuclear test ban treaty negotiations in Geneva by the United States and the United Kingdom.

Both of the Project Vela high-altitude programs include research and development aimed at improving knowledge of natural background radiations that exist in space. In the Vela Sierra program, particular emphasis is placed on determining the effect of these natural radiations on the upper atmosphere since the Sierra program is concerned with developing and improving detectors located on the earth's surface. Under the Vela Hotel program which is concerned with satellite detectors, there is also a need for an understanding of the background radiation problem.

Although theoretical studies indicate that the natural radiations are not likely to cause difficulty, both high-altitude programs include work to obtain experimental evidence to measure these backgrounds. If unexpected background radiations are present, the detection and discrimination of radiations from a nuclear detonation in space is likely to be more difficult.

VELA SIERRA PROGRAM

The Vela Sierra program is aimed at developing and improving instruments and techniques for detecting high-altitude nuclear detonations by instruments located on the ground. The effort on detection techniques is divided into three major categories: optical, radio frequency, and geophysical. Another part of the Vela Sierra program is concerned with the development of specific requirements for the construction and operation of a detection system. In this connection, AFTAC is preparing a plan to equip one Geneva-type station with ground-based high-altitude detection equipment.

The optical work includes research and instrument development on techniques for detecting the light signal and fluorescence induced in the upper atmosphere by a high-altitude detonation. Most of this work is being done by the AEC laboratories.

The radio frequency effort is concerned with methods for detecting the electromagnetic pulse from the detonation, the effect of a nuclear detonation on the ionosphere by means of measurements of long-range and vertical radio transmissions, and some work is also being done to optimize the effectiveness of backscatter radar for this purpose. This work is being done by the Bureau of Standards at Boulder, Colo., the Navy Electronics Laboratory, and contractors.

The geophysical work is largely with studies of perturbations in the earth's magnetic field, and electrical currents that are likely to occur when a high-altitude nuclear detonation occurs. This work is being conducted by the U.S. Army Signal Corps laboratories.

As indicated on chart (chart, p. 65), ARPA provides technical direction, coordination, and is the primary funding and action agency. The Air Force Technical Applications Center is the principal technical adviser to ARPA for this program and performs detailed supervision of work performed by contractors. Where a specific capability exists within the Government, work is assigned directly by ARPA order.

The funding made available for the execution of the Vela Sierra program was $450,000 in fiscal year 1960; $3.9 million in fiscal year 1961 and $2 million in fiscal year 1962.

Lt. Comdr. D. E. Chandler, USN, ARPA's Chief for the Vela Sierra program and Dr. William Ogle of the Los Alamos Scientific Laboratory, will discuss this work in greater detail.

VELA HOTEL PROGRAM

The uncertainty of trying to predict the future course of weapons development and the role space testing may play creates a need for gathering technical information and data on detection in the space environment in order to be able to evaluate the implications of future events.

Thus, the fundamental requirement for performing research and development on various detection techniques is to gain knowledge and a better understanding of physical phenomena related to detection problems.

ARPA has established the basic objectives of a proposed R. & D. program which will satisfy specific minimal requirements over the next several years as follows:

(*a*) Establish an experimental basis for performance estimates. Performance capabilities currently being assumed for the various detection techniques are based on scientific estimates only and do not represent guarantees.

(*b*) Future international negotiations on banning nuclear tests should be conducted from as enlightened a technological position as possible. Supporting information and experimental data should be available for determining control effectiveness of operational surveillance systems which might be agreed upon.

(*c*) An experimental basis for evaluating the effectiveness of such countermeasures as shielding should be available. At the present time, all evaluations of effectiveness of various countermeasures are also based on scientific estimates only.

In order to achieve the general objectives given above, it is proposed to perform studies, conduct experiments, and test detection devices in order to obtain information and experimental data for defining, and evaluating the control effectiveness of, satellite detection systems which meet the Geneva Technical Working Group I performance estimates. Initially, the principal effort will be directed toward obtaining information needed to develop and evaluate a far earth satellite system.

At the present time, the primary experimental effort proposed for Vela Hotel is based upon neutron, gamma ray, and X-ray detection techniques. From the chart (p. 66) it can be noted that ARPA is proposing a sequential effort starting with piggyback flights on NASA/DOD space vehicles and balloons and phasing into experimental satellite flights specifically designed for Vela Hotel R. & D. program requirements. The piggyback program has been approved and is well underway. The satellite program has recently been approved by the Department of Defense.

Barring unexpected technological pitfalls, it is estimated that enough experimental data will be available after the 1964 flights to define and design an initial far earth operational detection system with a capability not less than that estimated by the Geneva TWG-I and to evaluate economic practicability.

By mutual agreement between the DOD and AEC, the DOD(ARPA) has overall responsibility for the Vela Hotel program. The AEC is providing substantial support by providing the detectors and associated logics developed at its laboratories.

Chart (p. 66) illustrates the management and technical responsibility relationships which apply during the implementation of the program. Under the supervision of ARPA, technical supervision of the AEC and DOD portions of the program will be the responsibility of a Joint AEC/DOD Technical Group. This group, acting through normal channels in each agency, is responsible for establishing specifications, insuring complete coordination among the various tasks of the approved program, and providing technical direction to the contractors.

The AEC is responsible for providing the detectors and associated logics and analysis of the telemetered information from these devices.

The Air Force is providing system design, system assembly and test, boosters, and satellite vehicles (spacecraft). The latter includes integrating the detectors and logics into the satellite vehicles.

Close technical liaison will be maintained with NASA throughout the program.

Maj. J. A. Poulson, USAF, who is ARPA's Chief for the Vela Hotel program, Dr. Harold Argo of the AEC's Los Alamos Scientific Laboratory and Dr. B. P. Leonard of the Aerospace Corp. who is an ARPA consultant for the Vela Project will testify in greater detail on this program.

Considerable progress has been made in getting each of the three Vela programs solidly underway. Some useful results have already been obtained and many more are anticipated. We have been gratified with the very solid support and participation that all universities, contractors, and Government agencies have given to the Vela Project.

CHART 1

GOVERNMENTAL ORGANIZATIONS PARTICIPATING IN VELA

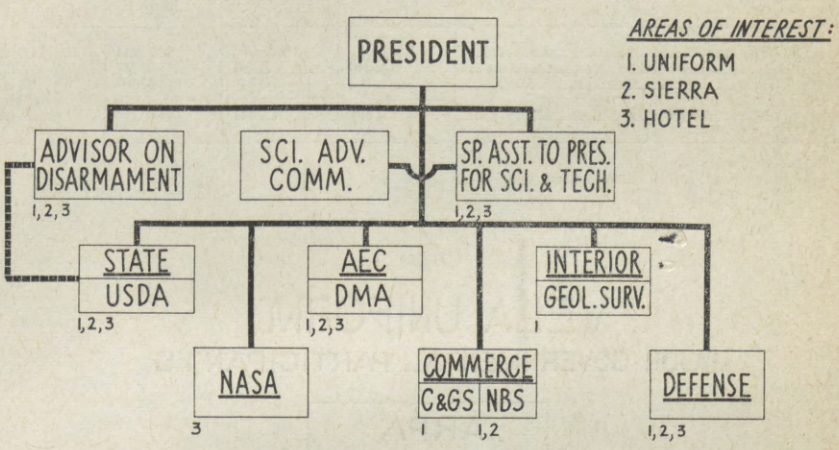


CHART 2

DEFENSE ORGANIZATIONS PARTICIPATION IN PROJECT VELA

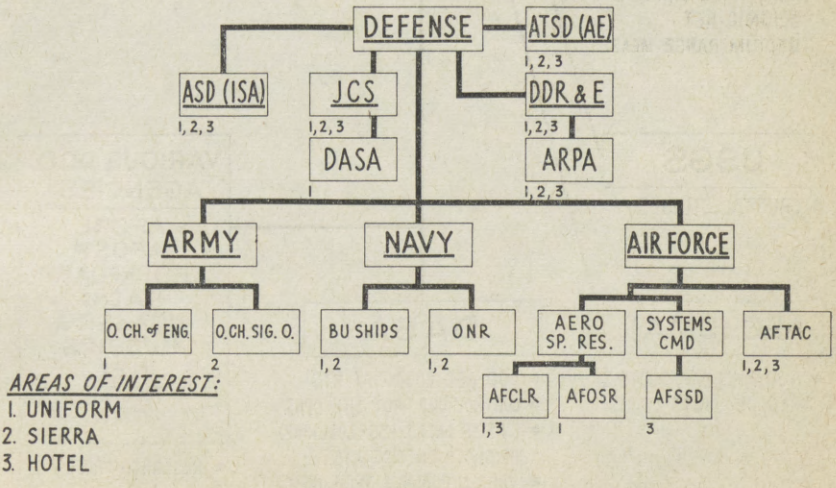


CHART 3

DOD funding for project Vela

Program	Fiscal years 1960 and 1961 funding	Committed by ARPA	Estimated obligation as of June 30, 1961	Fiscal year 1962[1][2] funding budget
UNIFORM	[1] $41,535	$39,295	$27,444	$47,000
SIERRA	4,340	2,197	3,247	2,000
HOTEL	5,563	5,349	5,563	10,000
Total	51,438	47,441	36,244	59,000

[1] Amount in thousands.
[2] Includes 22 million for use in fiscal year 1962.

CHART 4

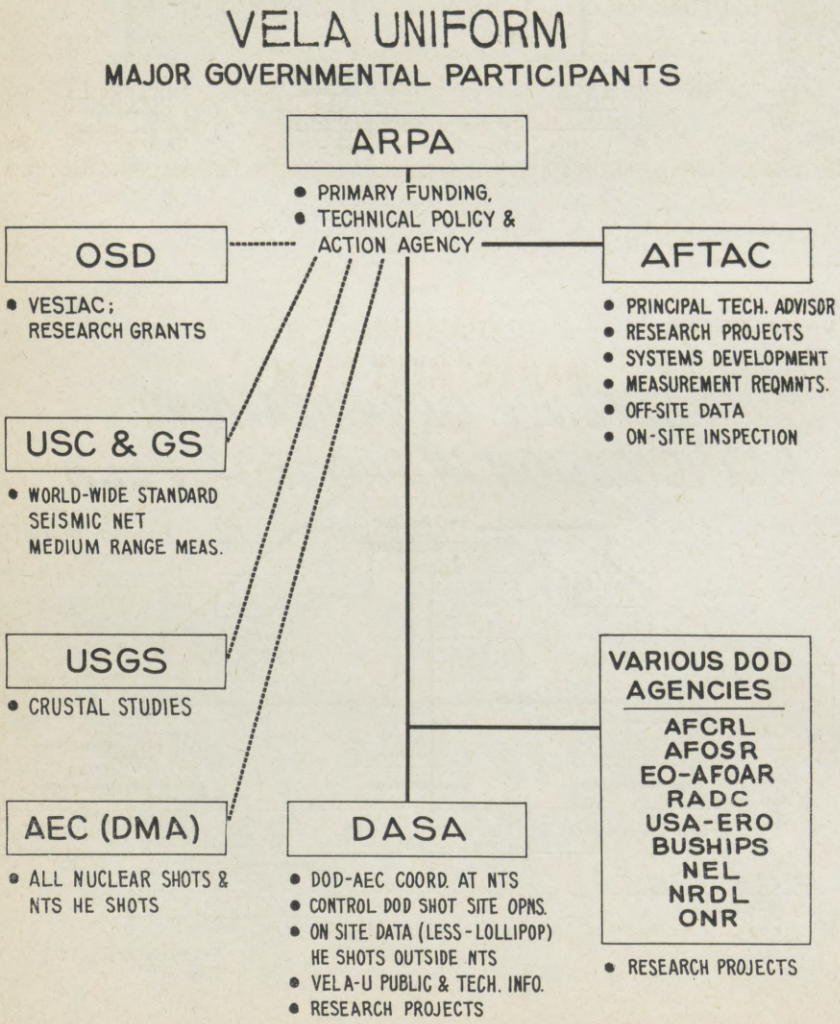


CHART 5

VELA UNIFORM TASKS

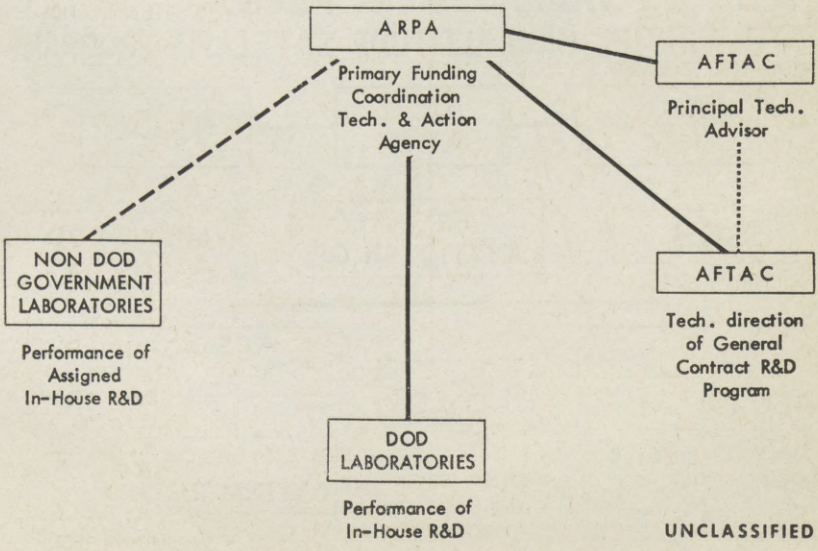
TASK	DESCRIPTION	PROGRAMMED FY 61 & PRIOR (ARPA FUNDING - Thous. $)	BUDGETED FY 62 (ARPA FUNDING - Thous. $)
1	WORLD-WIDE STANDARD SEISMOLOGICAL NETWORK	$ 3,375	$1,175
2	BASIC RESEARCH AND INSTRUMENT IMPROVEMENT	12,365	10,380
3	DEVELOPMENT AND CONSTRUCTION OF STATIONS	9,500	11,481
4	RESEARCH EXPLOSIONS	13,348	22,544
5	INFORMATION COLLATION	173	120
6	SPECIAL STUDIES	602	50
7	ON-SITE INSPECTION	2,175	1,250
	TOTALS	$41,538*	$47,000**

*378. -DOD FUNDING FROM DASA NOT INCLUDED **INCLUDES $22 MILLION FOR FY62

CHART 6

UNCLASSIFIED

VELA SIERRA
MANAGEMENT TEAM

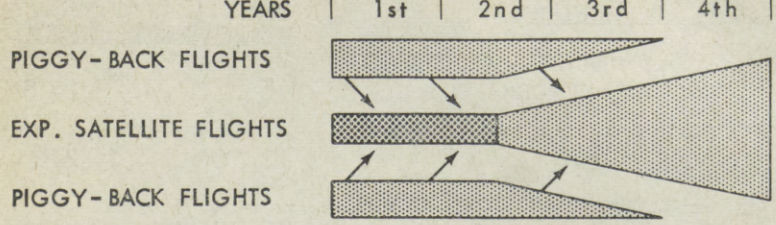


UNCLASSIFIED

CHART 7

DESCRIPTION - GENERAL

PRIMARY EXPERIMENTAL EFFORT (neutron, γ-ray, X-ray)



(Oriented to resolve the overriding question of environment effects of background)

DEVELOPMENT SEQUENCE

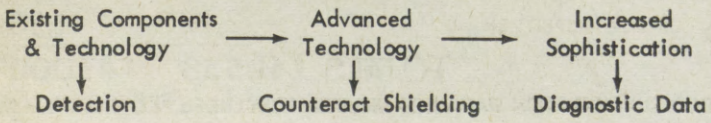
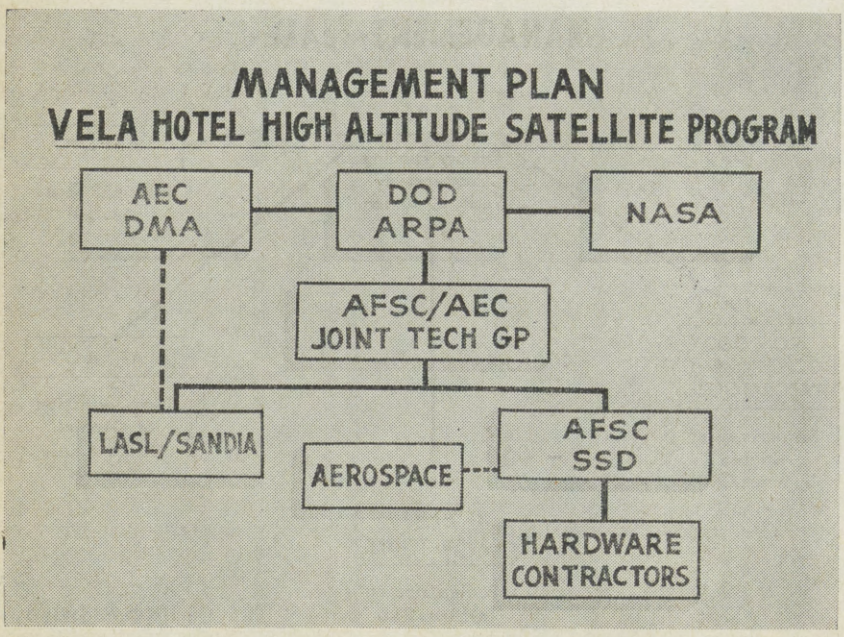


CHART 8



Chairman HOLIFIELD. Our next witness will lead off the testimony on that section of these hearings dealing with detection of underground tests, referred to as Vela Uniform.

STATEMENT OF DR. CHARLES C. BATES,[1] ADVANCED RESEARCH PROJECT AGENCY

Dr. BATES. Mr. Chairman and members of the Joint Committee: I would like to report to you concerning the research and exploratory development projects now underway in support of Vela Uniform. You will recall experience to date indicates that the most practical technique for the detection and location of underground nuclear explosions is the measurement and interpretation of the seismic waves set up by such detonations. Consequently, ARPA is placing heavy emphasis on developing and supporting a major research effort in both earthquake and explosion seismology so as to provide a comprehensive wealth of scientific knowledge for use in upgrading underground nuclear test ban control technology. As proposed in the Berkner Panel's report in 1959, entitled, "The Need for Fundamental Research in Seismology," this research effort consists of four major facets—the comprehensive study of all types of seismic source mechanisms, both natural and manmade, the determination of the number and character of the propagation paths along which such energy travels, the development of new and better methods and equipment for detecting seismic signals, and the evolution of optimum methods for processing, analyzing, transmitting, and displaying the data acquired. The task of explosion identification would be greatly simplified if there were found some other kind of simultaneous energy pulse detectable at hundreds of miles from an underground nuclear explosion capable of checking the seismic signal. Thus, a modest research effort is underway into determining whether electromagnetic

[1] Born Nov. 4, 1918, at Harrison, Ill. Received the A.B. degree (geology) from De Pauw University, Ind. in 1939; the M.A. degree (physics-meteorology) from University of California (Los Angeles) in 1944, and the Ph. D. degree (geological oceanography) from Texas A. & M. College in 1953.

Between 1939–41, Dr. Bates was employed as a geophysical trainee with the Carter Oil Co., of Tulsa, Okla. In 1945–46, he served as special assistant to the president of the American Meteorology Society and participated in the Crossroads atomic bomb tests during 1946 in the Marshall Islands as a member of the Woods Hole Oceanographic Institution's technical staff.

During World War II, Dr. Bates served as an Army Air Force weather officer and was awarded the Bronze Star Medal for forecasting wave conditions for the Normandy invasion. From 1955–57, he was commander of the research and development flight of the Washington Air Reserve Center and presently is a mobilization designee with the Air Force Office of Aerospace Research.

Dr. Bates started governmental service as an associate oceanographer with the U.S. Navy Hydrographic Office in 1946 and progressed to the position of Deputy Director, Division of Oceanography, by 1957, at which time he transferred to the Office of the Development Coordinator, Navy, to serve as Environmental Systems Coordinator. Since September 1960, he has been Chief, Vela Uniform Branch, Nuclear Test Detection Office of the Advanced Research Projects Agency, Department of Defense.

Dr. Bates is the author of a score of scientific papers in various facets of earth science. He was awarded the Navy's Meritorious Civilian Service Award in 1952 and the President's Award of the American Association of Petroleum Geologists in 1953. Between 1958–60, Dr. Bates was the Navy member of the DOD–NASA Joint Meteorological Satellite Advisory Committee, a member of the Meteorology Panel, Space Sciences Board of the National Academy of Sciences, and Chairman of the National Research Council's Advisory Committee on Postdoctoral Fulbright Awards in Geology. He is currently a member of the Seismological Society of America, Sigma XI, American Geophysical Union, Society of Exploration Geophysicists, and American Association of Petroleum Geologists.

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pulses generated by such an underground disturbance are detectable at appreciable distances.

A similarly modest effort in applied research is also underway into determining the degree of success with which underwater nuclear explosions in remote shallow or land-protected waters may be detected, identified, and located by today's techniques. This is necessary for there has been considerable question raised as to whether explosions in such areas are as simple to detect and identify as similar explosions in deep waters of the open ocean.

With the coming available during fiscal year 1961 of full-fledged Vela Uniform funding, the research effort described to the committee a year ago has accelerated into a major national undertaking. Forty-eight research contracts, grants, and interagency agreements have been made and eight renewed during the year with $9,280,000 of fiscal year 1961 funds available for this part of the program. This is in contrast to the 13 research efforts initiated during fiscal year 1960 with the $3,085,000 then available. (See table I (p. 80).) The number of ARPA agents engaged in carrying out this contracting assignment has grown from 1 to 11. The distribution of research participants by category of effort and type of background is quite well balanced, as can be seen in table II (p. 80). It may be noted that 24 of these research projects are with universities; 19 with commercial organizations; 6 with nonprofit research institutions; and 7 with governmental laboratories. Moreover, the research program is still growing in number of participants.

I would now like to describe some of the more interesting highlights of the research endeavors underway.

During fiscal year 1960, the Air Force Technical Applications Center has started work on 4 new projects; there are 21 new projects for the Air Force Cambridge Laboratories; 10 for the Air Force Office of Scientific Research; Rome Air Development Center, 4; Geological Survey, 2; Defense Atomic Support Agency, 6; Bureau of Ships, 4; Office of Naval Research, 1; Office of Secretary of Defense, 2; European Office of Aerospace Research, 1; and the Army Research and Development Office in Europe, 1.

Then there is the distribution of research participants by category of effort. Let me quickly run down who is working on what.

Under source mechanisms, University of California, Penn State, St. Louis, Columbia, and Stanford Research among the nonprofit institutions; United Electrodynamics, Sandia Corp., U.S. Bureau of Mines, American Machine & Foundry, and the Engineering-Physics Corp. among the commercial-type organizations.

On propagation path, in addition to the Geological Survey, St. Louis University, a team effort by UCLA and Cal Tech, the Uppsala University of Sweden, the University of Witwatersrand in South Africa, and the Universities of Alberta and Sasketchewan, in Canada are all being supported.

In signal detection, among the universities, University of Michigan, Columbia, Cal Tech, Rensselaer, and the University of California. Among the industrial organizations, Geotechnical Corp., Jersey Pro-

duction Research Co., Texas Instruments, Inc., Dresser Industries (SIE), Electro-Mechanics Co., Century Geophysical Corp., and Radio Corp. of America.

In signal analysis and display, we have University of Michigan, University of California (Scripps), California Institute of Technology, Columbia University, Massachusetts Institute of Technology, Texas Instruments, Inc., Bell Telephone Laboratories, Armour Research Foundation, and the Coast and Geodetic Survey.

Mr. RAMEY. How do you select your commercial contractors? Do you get competitive proposals and award on the basis of the best price, or do you select them primarily for their technical competency, regardless of the price?

Dr. BATES. In the research effort, I would say 90 percent of it is based on unsolicited research proposals, and we do go through an extensive evaluation method. We not only use inhouse evaluators, but also use a number of consultants from both universities and industry. In fact, the evaluation method is rigid enough that we throw out about three or four proposals to every one that we buy.

Reperesentative PRICE. Mr. Chairman, while we are on that subject, since they have Bell Telephone Laboratories there, I want to refer again to the article in the morning paper [2] about experiments that have to do with drawing the distinction between an earthquake and a nuclear detonation.

Are you familiar with that particular study?

Dr. BATES. Yes; we are supporting work of this type at Bell Laboratories, sir.

Representative PRICE. That is what I was getting to. You answered the second question first.

Would you care to comment on this?

Dr. BATES. Well, I could. But I do plan to go into some detail now on exactly what we are trying to accomplish in each of these technical areas.

Representative PRICE. You answered my second question first and took care of the situation.

Go ahead.

Dr. BATES. I would now like to describe some of the more interesting highlights of the research endeavors underway.

In order to understand the manner in which seismic waves are generated by buried explosive sources, a much better state of theoretical knowledge is required of the nature of the intense disturbance created in the immediate vicinity; that is, the close-in, nonelastic region, of contained explosions. This may come as a surprise considering the large amount of past effort that has gone into close-in weapon effects. Careful review, however, indicates that most of this past research has been on cratering and airburst types of detonations, rather than on contained explosions of the type that a controlled nuclear test ban would be interested in. Accordingly, the Defense Atomic Support Agency (DASA) is taking the lead within Vela Uniform to develop a research program aimed at achieving a sound theoretical description of the characteristics of the outgoing pulses from all types of contained explosions as such pulses entered the elastic

[2] Washington Post, July 25, 1961.

region; that is, become seismic wave trains. Our goal is to know how these characteristics are affected by type of medium, size, and shape of initial cavity, depth of burial, and energy yield of explosion, whether chemical or nuclear. It is anticipated this analytical work will make the results of the Vela Uniform research explosion series more comprehensible and extensible to explosions of various yields, in differing source locations, and in different earth materials.

Already of considerable interest are preliminary results of a research contract into the mechanism by which horizontally polarized shear (SH) waves are generated by small explosive sources. Comparison of theoretical and field results as to the pattern for motions of first shear from propagating surface cracks supports the hypothesis that radial cracking, a normal result of an explosion in the earth, plays an important role in the generation of this type of seismic wave. In addition, whenever the shot was near a free vertical face of a quarry wall, definite and consistent recordings were obtained of rarefactions being the sense of first motion in certain azimuths from the explosion source, rather than just the theoretically expended compressional motion on all azimuths. This may be a finding of particular significance, for the Geneva Conference of Experts placed great emphasis on the fact that only earthquakes should have well-established azimuthal patterns of first motion rarefaction and compression.

A broad research effort into a quantitative understanding of earthquake source mechanisms is also well underway. Within the coming year, it is hoped that the problem will be resolved as to whether the single couple or the double couple is the best point-source representation for initial motion along a geologic fault, as well as whether use of the plane of polarization of shear waves will provide a relatively reliable diagnostic criterion for faulting sources. Continuing investigations into the effects of finite source size and velocity of propagation of faulting are being supported, as are studies of the relationships between seismic magnitude, explosive energy yield, and elastic wave energy. Because of the complexity of the problem and previous deficiencies in seismic networks, no major results are yet available.

Although the assessment of the capabilities of any proposed seismic detection network is dependent upon having adequate knowledge of the characteristics of seismic noise as regards variation in time, geology, and geographic location, there is a dearth of good data of this type. Even today, because of limited dynamic range, practically all seismic observatories operate their equipment at magnifications low enough to keep noise from becoming an integral part of the record, thereby eliminating a logical source of raw noise data. As a consequence, Vela Uniform is supporting, under the technical direction of the Air Force Technical Applications Center (AFTAC) a major research effort at United Electrodynamics, Inc., in this area. Special slave and master recording stations of both short- and long-period seismometers have been built, and in March 1961 field measurements began along a line, Point Arguello, Calif., to Death Valley, Calif. Concurrent electronic processing of the noise data obtained is proceeding on a two-shift basis. Adequate sampling procedures are being

worked out, and first indications are that a slave station location must be occupied for about 3 weeks to obtain an adequate sample for a comprehensive noise analysis.

The wave form of the seismic signal received at long ranges is largely determined by the effect of propagation through inhomogeneous materials of the earth, particularly those in the crust and perhaps in the upper mantle. Because material properties and structure of the earth's crust and upper mantle are known only in gross outline, a deficiency of detailed information exists for determining exactly what does happen to a seismic wave train as it travels for hundreds of miles. Accordingly, the U.S. Geological Survey is conducting for Vela Uniform a comprehensive study of seismic propagation paths and regional traveltimes in the California-Nevada region. This is a region of many earthquakes that fall in the magnitude range of underground nuclear explosions. The region also contains the largest and most detailed network of permanent seismograph stations in the United States, as well as being the area in which all underground nuclear explosions by the United States have been made to date and most are planned for the future. The broad objectives of the Geological Survey's program are:

(1) To calibrate the California-Nevada region to determine the ultimate accuracy with which epicenters and focal depths of explosion and earthquake-generated seismic waves can be determined;

(2) Taking the California-Nevada region as an example of which there are geologically similar regions elsewhere in the world, to determine if such regions can be calibrated by simpler and less costly methods; and

(3) To test the results of the calibration of the California-Nevada region by locating known seismic events within the region and comparing known coordinate and depths with computed coordinates and depths.

The method used by the Geological Survey to make this study is the seismic-refraction method. High-explosive charges up to 5 tons in size are exploded at shot points at opposite ends of the profiles about 270 miles long (shown in fig. 1, p. 85). The seismic waves generated by these explosions are then recorded by at least 10 special seismic-recording units spaced along these profiles. Because of the magnitude and scope of the field program, the Geological Survey is supplementing its own personnel with recording and shot-point crews provided by a commercial geophysical contracting company.

A new all-transistorized seismic-refraction system for this work has been designed and built by a commercial manufacturer of geophysical instruments, the Southwestern Industrial Electronic Co., to meet strict performance specifications established by the Geological Survey. The new seismic system records on both magnetic tape and film, covers a very broad range of frequencies, i.e., from 1 to 300 cycles per second, has extremely low internal noise, and can record voltages, the largest of which are more than 1,000 times as great as the smallest. Magnetic recording tapes can be played back through a wide variety of filters

permitting the extraction of extremely weak signals in narrow frequency bands that could not be detected by conventional seismic-refraction instruments. We believe the new system is the most advanced seismic-refraction system in existence, and it has many potential applications in nuclear-test detection and studies of small earthquakes and after shocks.

Four of the new seismic systems are now being used for evaluation and training purposes in eastern Colorado prior to the start of scheduled fieldwork in California and Nevada about September 1, 1961. This large and complex field program is functioning smoothly, and it is anticipated that the first phase of this program, namely, the measurements of profiles 1 through 3 shown in figure 1 (p. 85), will be completed by November 1961. Providing the interpretation of these results are encouraging and funds are available, profiles 4 through 7 will then be surveyed during the summer of 1962.

As a byproduct of this study of seismic propagation paths and regional traveltimes, the Geological Survey is obtaining information of great scientific importance on the structure and composition of the earth's crust and upper mantle. This will contribute to improving our basic understanding of how mountains are built, what causes earthquakes, why volcanoes appear where they do, and even why ores of useful metals are found in certain places in the earth's crust and not in others. In addition to this governmental effort, academic investigations are also being supported into the nature of the earth's crust in such varied locations as the central United States, the Alps, the western Mediterranean region, Scandinavia, Iceland, South Africa, and western Canada.

The several theoretical and experimental studies now underway to investigate and predict the propagation characteristics on seismograms that would enable an analyst to distinguish with certainty between seismic wave trains created by underground explosions and earthquakes: To date, no single diagnostic feature has been found that is foolproof. However, certain characteristics have been noted by the California Institute of Technology that appear to be more typical of an explosion than an earthquake; for example, there is a tendency for long-period "love" waves to be more efficiently excited for earthquakes than explosions. On the other hand, Rayleigh wave spectra for earthquake and explosion sources do not show characteristic differences. In any case, work on the finalization of diagnostic aids is hampered by insufficient data on major underground explosions. In the meantime, new computer programs and digitized seismograms are becoming available in quantity and should speed up the acquisition rate of new knowledge about seismic wave propagation phenomena. Promising projects include development of a family of surface response curves generated by typical explosion wavelets with varying depths and geology, the improvement of time resolution of closely spaced or overlapping seismic wave arrivals of direct and surface-reflected waves from various sources by the application of inverse filtering techniques, and the determination of the amount of energy that should be contained in the shear-wave phases of a wave train from various types of seismic sources.

As might be expected, the research and exploratory development effort for improving seismic instrumentation is more industrially than

academically oriented. Special attention is being given to recent advances in design, materials, and components in other related areas that may be applicable to detection seismology. Dr. Romney, of AFTAC, for example, will describe the promising deephole seismometer effort wherein six contracts are now in effect, three being of a research nature and three of a systems nature. Another important effort is directed toward exploring the possibility that there may be very low seismic background noise levels on the ocean floor. To accomplish this, three different types of ocean bottom seismographs are under development, and field results will start becoming available by the end of calendar 1961. A transition is also underway from complete dependence on photographic recording to supplementary recording on slow-speed magnetic tape for greater dynamic range, ease of digitalization, immediate playback through special filters, and suitability for electronic data processing as to the nature of the signal and background noise level. Important improvements have been made in long-period seismographs so that they become stable in the field within a matter of hours, not days. Improved long-period galvanometers, filters, timing devices and amplifiers, both phototube and solid state, are also well along in the development stage.

In view of the fact that improved seismological arrays, research investigations, cooperative networks, and prototype detection stations are starting to generate seismic data at a rate many times that previously available for study, research into greatly improved data processing, analysis, and display techniques are being prosecuted under 14 contracts with university and industrial research groups. High-speed computer techniques are being attempted with good success for spectral analysis, determination of energy spectra through autocorrelation techniques, and objective analysis of the polarity of first motion. Work to date indicates that the amplitude and frequency content of the seismic power spectrum of earthquakes and buried explosions varies somewhat systematically with azimuth about the disturbance and its initial energy content.

Also of interest is that the analyses of digital seismic data taken by Texas Instruments for part of the chemical explosions in Project Cowboy suggest that the degree of decoupling is a function of frequency; that it varies with the mode of seismic propagation wherein the first motion arrival regularly appears to be decoupled less than the total energy; and that the decoupling factor increases with distance, all of which suggests a different partitioning of the elastic wave energy for coupled versus decoupled explosions. Two unusual techniques for seismic signal analysis are currently under investigation. In the first instance, advantage is being taken of the University of Michigan's Institute of Science and Technology pioneering certain optical techniques that appear capable of quickly correlating a large number of channels of analog seismic data; in the second instance, the long-term experience of Michigan and the Bell Telephone Laboratories in acoustics is being utilized to determine whether the human auditory system has the capability to distinguish between time-compressed seismic signals from underground explosions and earthquakes at comparable distances. Dr. Romney, of AFTAC, will also describe in subsequent testimony other research efforts leading to improved methods for handling seismic data from large detector arrays.

Representative PRICE. Is this the one you were talking about?

Dr. BATES. Yes.

Representative PRICE. Are you as optimistic about this as the article that appeared in the paper [3] this morning appeared to be?

Dr. BATES. Well, my assignment as a scientist in being given a problem is always to be optimistic that we can find a solution. Now, whether we can work this one through to where it has a high degree of reliability, using the ear instead of the eye, will have to remain to be found out. Our trouble with the Bell Laboratories experiment to date is that they did not have a big library of seismograms for this particular study. They were comparing earthquakes at 6,000 miles against explosions that were fairly close in.

Now, on the other hand, the University of Michigan, on very little work, as their contract has been in existence for only 2 months, has found where they have recorded at about 100 miles an earthquake magnitude of 2.25, which is very small, and a 6-ton chemical explosion of about the same size—they could distinguish by ear the difference between the explosion and the earthquake.

There is a similar situation in the Navy, where you still use the human ear generally in sonar for distinguishing between whether you have a submarine or a whale.

Whether we can be as fortunate as they are in ASW, I just do not know. But we will find out in about another 6 months or so.

Mr. RAMEY. Could you explain what you mean by ear, when you say that?

Dr. BATES. Well, you take this trace of the earth's motion, and compress it in time so that it is actually speeded up to what the ear can hear. In other words, where it may be on the order of 1 cycle a second to 10 cycles a second in nature, you speed it up to where it is in the low kilocycle frequency band.

Mr. RAMEY. Like a tape recorder when you wind it up?

Dr. BATES. Yes. In fact, Michigan noted this 2 years ago. Their technicians when editing their magnetic tapes started noticing there might be a difference between explosions and earthquakes. Professor Wilson,[4] from Michigan, will be here tomorrow.

With respect to Vela Uniform's interest in the nature of the electromagnetic signals from underground disturbances, it has been known for some time that electromagnetic pulses generated by underground explosions could be detected at the surface of the ground in the immediate vicinity of the explosion. However, it has also been assumed that the signal would not be transmitted through the earth's surface layers for useful detection distances because of the high attenuation rate that exists there. Nevertheless, the possibility does exist that such a signal might be transmitted for useful distances at depths of a few tens of thousands of feet if, as has been predicted, the rate of signal attenuation is much less than at the surface. Because so little is known about this entire phenomenon, ARPA is currently sponsoring five research projects in the area of subsurface electromagnetic propagation to determine the nature of possible source mechanisms, the possible modes and efficiency of propagation, and the

[3] Washington Post, July, 25, 1961.

[4] Dr. James T. Wilson, associate director, Institute of Science and Technology, University of Michigan (Seismic Research Panel, p. 184).

electrical properties of selected portions of the earth's crust. The study of source mechanisms is theoretical in nature; at present, two models are being investigated, each requiring interaction between products of the nuclear explosion and the earth's magnetic field. With respect to the nature of electromagnetic pulse propagation, model studies are already underway. Arrangements are also being made by Edgerton, Germeshausen, and Grier under the technical direction of the Rome Air Development Center for actually generating large electromagnetic signals later this year within a tunnel cut into granite and measuring these signals on the earth's surface at varying distances from the source. Determination of the electrical properties of selected portions of the earth's crust has been assigned to the U.S. Geological Survey and includes compiling data already existing in scattered form within commercial electrical log libraries and accumulating new information by use of several specialized field measurement techniques.

The last phase of the Vela Uniform research effort to be described pertains to marine detection techniques. At the present time, the U.S. Navy Electronics Laboratory is investigating for ARPA the feasibility of a hydroacoustic detection network of the type briefly described at Geneva. At the U.S. Naval Radiological Defense Laboratory, ARPA is also sponsoring a study into the problem of collecting samples from a suspected underwater explosion site after oceanic currents and turbulence have scattered and diffused the radioactive debris. Preliminary conclusions suggest that adequate hydroacoustic coverage of major ocean areas cannot be obtained if only stations in the proposed Geneva-type test control net are utilized. Even if the order of 30 additional hydroacoustic stations were added, there appear to be certain oceanic areas wherein underwater tests could not be detected with conventional hydroacoustic coverage.

To determine the validity of these early conclusions, a cooperative program for seismic and hydroacoustic monitoring of approximately ten 5-ton underwater explosions of the Navy-DASA Hydra II series during August 1961, near San Clemente Island, Calif., has been arranged. Present plans call for approximately 12 agencies to monitor the series at about 23 locations. Major participants include AFTAC-directed seismic crews of the U.S. Coast and Geodetic Survey and the University of Michigan. These will be stationed throughout southern California and, with the assistance of the Navy, on several of the channel islands. Extensive hydroacoustic measurements are also being made at short ranges by the U.S. Navy Electronics Laboratory and at longer ranges by the University of Hawaii and the U.S. Navy Hydrographic Office. If all goes as planned, this investigation will soon provide urgently needed data on:

(*a*) Effect of ocean bottom topography, islands and coast lines upon the transmission and reception of hydroacoustic signals.

(*b*) Effect of depth of detonation and shot distance off-bottom upon generation of seismic and hydroacoustic signals.

(*c*) Usefulness of an integrated analysis of hydroacoustic and seismic signals for detecting and identifying underwater explosions of less than kiloton yield.

I would also like to mention that in May 1961 ARPA began sponsoring, through the Office of Naval Research, a Columbia University

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program in seismic and hydroacoustic measurements within the Arctic Ocean by use of the drifting ice island, Arlis II. Equipment has been installed and data are just now becoming available.

In conclusion, I would like to note that the Vela Uniform research effort has work well underway on all the pertinent recommendations of the Berkner Panel. In addition, the program has been broadened to include other aspects of subsurface nuclear test detection not conceived of at the time the panel issued its report.

This concludes my statement. I would be pleased to answer any questions.

Chairman HOLIFIELD. Thank you, Dr. Bates.

Your statement certainly shows a great deal more activity than was occurring a year ago.

Mr. RAMEY. Do you think the principal limiting factor is the lack of underground nuclear explosions on your research work?

Dr. BATES. Well, not for the moment, certainly. We have an awful lot of hardware to build, an awful lot of ideas to get checked out. We can check them out against earthquakes about as well as we can against explosions. However, if we are going to go forward beyond the ideas that were broached a year or so ago, we definitely have to get working with the real thing. But you can see by the breadth of the people that have come into the program that they are in there primarily because they have ideas they want to try out.

Representative PRICE. Dr. Bates, in your formal statement, you describe a project which I believe you said was being conducted at St. Louis University. How much of that explanation goes to that particular project? I was not clear whether or not the next couple of paragraphs were an explanation of that.

Dr. BATES. Well, the conclusion drawn in the last sentence of that paragraph does apply to the shear waves and to the first motion concept that I discussed. Now, these are very small chemical explosions. Actually, they are of a pound or so. But it does indicate that under good experimental conditions we can do things which theory does not necessarily call for.

Representative PRICE. The reason I bring that up is not only because this is my alma mater, but also because of the fact that the modest figures contained in some of these contracts sort of startles the committee. We are not accustomed to such modest figures when we get into the research and development field. So we just wonder whether you are getting down to real basic science, or whether you can weight these projects by the amount of money that is expended on the contracts.

Dr. BATES. Well, most of our projects, with the exception of those that were started the previous year when the major seismological universities were supported more or less on open-end funding—the prices are based on what it takes to come up with a reasonable answer to a question. So that the size of a contract may vary anywhere from $5,000 to $200,000.

Representative PRICE. I am not criticizing. I am just taken aback by the small amounts involved in many of these projects. I wondered if you have some comment on the importance of the project, regardless of the small amount being invested.

What is your estimate of the probable application of using electromagnetic pulses generated by an underground explosion for detection at distances involved between control posts of the Geneva system?

Dr. BATES. Well, one theoretical study—and I might say we are still largely in theory here—because good electromagnetic measurements have never been made of either large nuclear or chemical underground explosions—indicates that we may get several hundred miles. I said "indicates." You heard Dr. Latter this morning take a different view. But what little work done with chemical explosions that I know of in the last year has suggested that the electrical signal is much larger than theory called for. And this is why we are so anxious to, (*a*) make more observations of the signal, and, (*b*) to actually put a very large electrical charge into the earth and see how far we can detect it.

Representative PRICE. In your statement, you say a modest research effort is underway into determining whether electromagnetic pulses generated by underground explosions are detectable at a considerable distance. Could this experiment be stepped up, and would it be important to step it up?

Dr. BATES. Well, it is a very argumentative field; and so we are spending now about $300,000 a year into it, as compared with seismology, where we will be spending, say, $9 million. In other words, we are spending only 3 percent in electromagnetics as in seismology. But as soon as we are sure we are either on the right track or the wrong track, we will do one thing or the other, either stop the approach, or accelerate it.

We feel we have three very good contractors in this area. We think they can come up with a new answer as to whether we should expand or disband about as easily and quickly as would putting more money into the program at the moment.

Representative PRICE. Is there any evidence that the detection of the electromagnetic pulses from underground nuclear explosions in cavities may provide a solution to the decoupling problem?

Dr. BATES. Well, this is our far-out hope—to find a signal that the more you decouple, the bigger the signal. In other words, when people speak of decoupling, they are only speaking of decoupling mechanical energy. And if there is such a thing, and it turns out that this electromagnetic signal that has been observed is a function of the size of fireball, we hope that perhaps the bigger the fireball, the bigger the signal. And so that is why we are pursuing it. Whether countermeasures can be developed against it would have to be a problem taken into account, too.

But we first want to see whether we can get a signal out of it that is actually worthwhile working with. And we do think that for a given yield, the signal should be bigger, the bigger the cavity the explosion occurred in.

Representative PRICE. You stated that already one research group, in studying a shot near a free vertical face of a quarry wall, received definite indication of rarefactions from such an event. You go on to state that this may be of particular significance in the identification of earthquakes.

In your opinion, is it possible that the Hardtack II tests, which in some instances reported the rarefactive first motion, which should be attributable only to earthquakes, may have been the result of the local geological formations in the vicinity of the Nevada Test Site?

Dr. BATES. I have not had an opportunity to study in detail the results from Hardtack II; so I perhaps should not postulate an answer.

The St. Louis work, though, indicates that there is a very close directional control, and that this rarefaction shows up in only a small part of the 360° of azimuth. I do not believe the observations on Hardtack had that much detailed coverage.

Hence, I think the rarefaction observed there is more a local-path effect than the St. Louis phenomenon.

Representative PRICE. Thank you very much.

Chairman HOLIFIELD. Do you think that the local geological and topographical features at the site of an underground test, nuclear test, at different locations around the world, can affect the direction of first motion in such an erratic fashion from one location to another as to considerably degrade the capability of the direction of first motion, as a criterion in identification of earthquakes?

That is kind of a long question. Do you understand my question?

Dr. BATES. I understand. I just do not think we have done enough shooting. This is one reason why the Geological Survey's carefully done experiment this summer will give us better data on first motion.

There is another question. If you turn up the magnification, you may find out what people have been calling precursors in the past are actually some new indication of first motion.

Thirdly, I hope within the year we may be able to use other parts of the seismogram as an indication, too, of what the motion of the source actually was.

Chairman HOLIFIELD. If there are different situations in different areas which caused a variance in reading, you would be in trouble, would you not?

Dr. BATES. We do know if you get into enough variation layers, the earth itself may act as a variable filter and do things to the first motion. We know that certainly the strength of the signal varies locally. And this is the very reason we are very anxious to get into the field and get some very closely controlled measurements.

Chairman HOLIFIELD. Would not intervention of earthquake faults between the origin of the vibration and the recording provide you with a problem? In other words, would not the vibration be carried as consistently through a fault, like the San Andreas Fault in California, as it would through the same kind of ground which would not have faults?

Dr. BATES. This is a debatable point.

We have one study aimed at measuring seismic signals transmitted through different kinds of geology, including faults, to see if it does upset the character of the signals. And I believe we also will be getting a major proposal from outside the country to take a look at this situation in a different part of the world.

Chairman HOLIFIELD. Do you think that the Vela Uniform program has the potential of developing improved detection techniques for underground explosions which would in themselves, when operated at the stations of the Geneva control system, be capable of identifying underground nuclear explosions without the need for an onsite inspection to confirm the seismic or other instrumental indications?

Dr. BATES. Well, it depends on how much energy you have to detect and identify. If we can stay at the threshold that they were talk-

ing about in 1958, and not get too much decoupling in it, I think we can perhaps get back to what our short range goal was, i.e., to recreate the 1958 capability. How much further one could go is certainly an open question.

Mr. RAMEY. That would bring it down to 5 kilotons?

Dr. BATES. That is what many of the things are designed to do, like the Berkner-type station. And whether your design pans out can only be determined by actually trying.

I mean you can put things on pieces of paper that either say you could or you can put things on paper that say you cannot. I much prefer to be "from Missouri" and see what we actually do when we get these various detection stations operational, get good seismometers down deep wells, have some better idea on how much geology upsets the signal, and this sort of thing.

Chairman HOLIFIELD. Thank you, Dr. Bates.

I think you have given us a great deal of help.

(Dr. Bates' formal statement follows:)

STATEMENT BY DR. CHARLES C. BATES, VELA UNIFORM PROGRAM MANAGER, ADVANCED RESEARCH PROJECT AGENCY

Mr. Chairman and members of the Joint Committee, I would like to report to you concerning the research and exploratory development projects now underway in support of Vela Uniform. You will recall experience to date indicates that the most practical technique for the detection, identification, and location of underground nuclear explosions is the measurement and interpretation of the seismic waves set up by such detonations. Consequently, ARPA is placing heavy emphasis on developing and supporting a major research effort in both earthquake and explosion seismology so as to provide a comprehensive wealth of scientific knowledge for use in upgrading underground nuclear test ban control technology. As proposed in the Berkner panel's report in 1959 entitled, "The Need for Fundamental Research in Seismology," this research effort consists of four major facets—the comprehensive study of all types of seismic source mechanisms, both natural and manmade, the determination of the number and character of the propagation paths along which such energy travels, the development of new and better methods and equipment for detecting seismic signals, and the evolution of optimum methods for processing, analyzing, transmitting and displaying the data acquired. The task of explosion identification would be greatly simplified if there were found some other kind of simultaneous energy pulse detectable at hundreds of miles from an underground nuclear explosion capable of checking the seismic signal. Thus, a modest research effort is underway into determining whether electromagnetic pulses generated by such an underground disturbance are detectable at appreciable distances. A similarly modest effort in applied research is also underway into determining the degree of success with which underwater nuclear explosions in remote shallow or land-protected waters may be detected, identified and located by today's techniques. This is necessary for there has been considerable question raised as to whether explosion in such areas are as simple to detect and identify as similar explosions in deep waters of the open ocean.

With the coming available during fiscal year 1961 of full-fledged Vela Uniform funding, the research effort described to the committee a year ago has accelerated into a major national undertaking. Forty-eight research contracts, grants and interagency agreements have been made and eight renewed during the year with $9,280,000 of fiscal year 1961 funds available for this part of the program. This is in contrast to the 13 research efforts initiated during fiscal year 1960 with the $3,085,000 then available. (See table I.) The number of ARPA agents engaged in carrying out this contracting assignment has grown from 1 to 11. The distribution of research participants by category of effort and type of background is quite well balanced, as can be seen in table II. It may be noted that 24 of these research projects are with universities; 19 with commercial organizations, 6 with nonprofit research institutions; and 7 with governmental laboratories. Moreover, the research program is still growing.

I would now like to describe some of the more interesting highlights of the research endeavors underway.

TABLE I.—*Status of Vela Uniform research contracts, June 30, 1961*

ARPA agent	Work started, fiscal year 1960	Work started or renewed, fiscal year 1961	Contract actions underway, July 20, 1961
Air Force Technical Applications Center	13	4	
Air Force Cambridge Research Laboratories	[1](5)	21	5
Air Force Office of Scientific Research	[1](4)	10	3
Rome Air Development Center		4	1
U.S. Geological Survey		2	
Defense Atomic Support Agency		6	
Bureau of Ships		4	
Office of Naval Research		1	
Office of Secretary of Defense		2	
European office, Office of Aerospace Research		1	
European Research Office, U.S. Army		1	1
Total	13	[2] 56	10
Funds	$3,085,000	$9,280,000	

[1] Acted as AFTAC contracting agent.
[2] Includes 2 research grants.

TABLE II.—*Vela Uniform research participants, June 30, 1961*

SEISMIC DETECTION

Source mechanisms:
- University of California (Berkeley)
- Pennsylvania State
- St. Louis University
- Columbia University
- United Electro-Dynamics, Inc.
- Sandia Corp.
- Stanford Research Institute
- APRL, U.S. Bureau of Mines
- American Machinery & Foundry Co.
- Engineering-Physics Corp.

Propagation path:
- U.S. Geological Survey
- St. Louis University
- UCLA, California Institute of Technology
- Uppsala University (Sweden)
- University of Witwatersrand (South Africa)
- University of Alberta (Canada)
- University of Saskatchewan (Canada)

Signal detection:
- University of Michigan
- Columbia University
- California Institute of Technology
- University of California (Scripps)
- Geotechnical Corp.
- Jersey Production Research Co.
- Texas Instruments, Inc.
- Dresser Industries (SIE)
- Electro-Mechanics Co.
- Rensselaer Polytechnic Institute
- Century Geophysical Corp.
- Radio Corp. of America

Signal analysis and display:
- University of Michigan
- University of California (Scripps)
- California Institute of Technology
- Columbia University
- Massachusetts Institute of Technology
- Texas Instruments, Inc.
- Bell Telephone Laboratories
- Armour Research Foundations
- Coast and Geodetic Survey

Propagation phenomena:
- California Institute of Technology
- Columbia University
- Weizmann Institute (Israel)
- Stanford Research Institute
- University of Oklahoma
- California Research Corp.
- Pennsylvania State University
- Southern Methodist University
- National Engineering Science Co.

ELECTROMAGNETIC DETECTION

- U.S. Geological Survey
- Edgerton, Germeshausen & Grier, Inc.
- Space Electronics Corp.

MARINE DETECTION

- U.S. Navy Electronics Laboratory
- U.S. Naval Radiological Defense Laboratory
- Columbia University

In order to understand the manner in which seismic waves are generated by buried explosive sources, a much better state of theoretical knowledge is required of the nature of the intense disturbance created in the immediate vicinity, i.e., the close-in, nonelastic regions, of contained explosions. This may come as a surprise considering the large amount of past effort that has gone into close-in

weapon effects. Careful review, however, indicates that most of this past research has been on cratering and air-burst types of detonations, rather than on contained explosions of the type that a controlled nuclear test ban would be interested in. Accordingly, the Defense Atomic Support Agency (DASA) is taking the lead within Vela Uniform to develop a research program aimed at achieving a sound theoretical description of the characteristics of the outgoing pulses from all types of contained explosions as such pulses enter the elastic region, i.e., become seismic wave trains. Our goal is to know how these characteristics are affected by type of medium, size, and shape of initial cavity, depth of burial, and energy yield of explosion, whether chemical or nuclear. It is anticipated this analytical work will make the results of the Vela Uniform research explosion series more comprehensive and extensible to explosions of various yields, in differing source locations, and in different earth materials. Already of considerable interest are preliminary results of a research contract into the mechanism by which horizontally polarized shear (SH) waves are generated by small explosive sources. Comparison of theoretical and field results as to the pattern for motions of first shear from propagating surface cracks supports the hypothesis that radial cracking, a normal result of an explosion in the earth, plays an important role in the generation of this type of seismic wave. In addition, whenever the shot was near a free vertical face of a quarry wall, definite and consistent recordings were obtained of rarefactions being the sense of first motion in certain azimuths from the explosion source, rather than just the theoretically expected compressional motion in all azimuths. This may be a finding of particular significance, for the Geneva Conference of Experts placed great emphasis on the fact that only earthquakes should have well-established azimuth patterns of first motion rarefaction and compression.

A broad research effort into a quantitative understanding of earthquake source mechanisms is also well underway. Within the coming year, it is hoped that the problem will be resolved as to whether the single couple or the double couple is the best point-source representation for initial motion along a fault, as well as whether use of the plane of polarization of shear waves will provide a relatively reliable diagnostic criterion for faulting sources. Continuing investigations into the effects of finite source size and velocity of propagation of faulting are being supported, as are studies of the relationships between seismic magnitude, explosive energy yield, and elastic wave energy. Because of the complexity of the problem and previous deficiencies in seismic networks, no major results are yet available.

Although the assessment of capabilities of any proposed seismic detection network is dependent upon having adequate knowledge of the characteristics of seismic noise as regards variation in time, geology, and geographic location, there is a dearth of good data of this type. Even today, because of limited dynamic range, practically all seismic observatories operate their equipment at magnifications low enough to keep noise from becoming an integral part of the record, thereby eliminating a logical source of raw noise data. As a consequence, Vela Uniform is supporting, under the technical direction of the Air Force Technical Applications Center (AFTAC) a major research effort at United Electrodynamics, Inc., in this area. Special slave and master recording stations of both short- and long-periods seismometers have been built, and in March 1961, field measurements began along a line, Point Arguello, Calif., to Death Valley, Calif. Concurrent electronic procession of the noise data obtained is proceeding on a two-shift basis. Adequate sampling procedures are being worked out, and first indications are that a slave station location must be occupied for about 3 weeks to obtain an adequate sample for a comprehensive noise analysis.

The wave form of the seismic signal received at long range is largely determined by the effect of propagation through inhomogeneous materials of the earth, particularly those in the crust and perhaps in the upper mantle. Because material properties and structure of the earth's crust and upper mantle are known only in gross outline, a deficiency of detailed information exists for determining exactly what does happen to a seismic wave train as it travels for hundreds of miles. Accordingly, the U.S. Geological Survey is conducting for Vela Uniform a comprehensive study of seismic propagation paths and regional travel times in the California-Nevada region. This is a region of many earthquakes that fall in the magnitude range of underground nuclear explosions. The region also contains the largest and most detailed network of permanent seismograph stations in the United States, as well as being the area in which all underground nuclear explosions by the United States have been made to date and most are planned for the future. The broad objectives of the Geological Survey's program are—

1. To calibrate the California-Nevada region to determine the ultimate accuracy with which epicenters and focal depths of explosion and earthquake generated seismic waves can be determined.

2. Taking the California-Nevada region as an example of which there are geologically similar regions elsewhere in the world, to determine if such regions can be calibrated by simpler and less costly methods.

3. To test the results of the calibration of the California-Nevada region by locating known seismic events within the region and comparing known coordinates and depths with computed coordinates and depths.

The method used by the Geological Survey to make this study is the seismic-refraction method. High-explosive charges up to 5 tons in size are exploded at shot points at opposite ends of the profiles about 270 miles long. (Shown in fig. 1, p. 85.) The seismic waves generated by these explosions are then recorded by at least 10 special seismic-recording units spaced along these profiles. Because of the magnitude and scope of the field program, the Geological Survey is supplementing its own personnel with recording and shot-point crews provided by a commercial geophysical contracting company.

A new all-transistorized seismic-refraction system for this work has been designed and built by a commercial manufacturer of geophysical instruments, The Southwestern Industrial Electronic Co., to meet strict performance specifications established by the Geological Survey. The new seismic system records on both magnetic tape and film, covers a very broad range of frequencies, i.e., from 1 to 300 cycles per second, has extremely low internal noise, and can record voltages, the largest of which are more than 1,000 times as great as the smallest. Magnetic recording tapes can be played back through a wide variety of filters permitting the extraction of extremely weak signals in narrow frequency bands that could not be detected by conventional seismic-refraction instruments. We believe the new system is the most advanced seismic-refraction system in existence, and it has many potential applications in nuclear-test detection and studies of small earthquakes and aftershocks.

Four of the new seismic systems are now being used for evaluation and training purposes in eastern Colorado prior to the start of scheduled field work in California and Nevada about September 1, 1961. This large and complex field program is functioning smoothly, and it is anticipated that the first phase of this program, namely, the measurements of profiles 1 through 3 shown on figure 1 (p. 85), will be completed by November 1961. Providing the interpretation of these results are encouraging and funds are available, profiles 4 through 7 will then be surveyed during the summer of 1962.

As a byproduct of this study of seismic propagation paths and regional traveltimes, the Geological Survey is obtaining information of great scientific importance on the structure and composition of the earth's crust and upper mantle. This will contribute to improving our basic understanding of how mountains are built, what causes earthquakes, why volcanoes appear where they do, and even why ores of useful metals are found in certain places in the earth's crust and not in others. In addition to this governmental effort, academic investigations are also being supported into the nature of the earth's crust in such varied locations as the Central United States, the Alps, the western Mediterranean region, Scandinavia, Iceland, South Africa, and western Canada.

The several theoretical and experimental studies now underway to investigate and predict the propagation characteristics of body and surface waves represent a broad search for characteristics on seismograms that would enable an analyst to distinguish with certainty between seismic wave trains created by underground explosions and earthquakes. To date, no single diagnostic feature has been found that is foolproof. However, certain characteristics have been noted by the California Institute of Technology that appear to be more typical of an explosion than an earthquake; for example, there is a tendency for long-period "Love" waves to be more efficiently excited for earthquakes than explosions. On the other hand, Rayleigh wave spectra for earthquake and explosion sources do not show characteristic differences. In any case, work on the finalization of diagnostic aids is hampered by insufficient data on major underground explosions. In the meantime, new computer programs and digitized seismograms are becoming available in quantity and should speed up the acquisition rate of new knowledge about seismic wave propagation phenomena. Promising projects include development of a family of surface response curves generated by typical explosion wavelets with varying depths and geology, the improvement of time resolution of closely spaced or overlapping seismic wave

arrivals of direct and surface-reflected waves from various sources by the application of inverse filtering techniques, and the determination of the amount of energy that should be contained in the shear-wave phases of a wave train from various types of seismic sources.

As might be expected, the research and exploratory development effort for improving seismic instrumentation is more industrially than academically oriented. Special attention is being given to recent advances in design, materials and components in other related areas that may be applicable to detection seismology. Dr. Romney of AFTAC, for example, will describe the promising deep-hole seismometer effort wherein six contracts are now in effect, three being of a research nature and three of a systems nature. Another important effort is directed toward exploring the possibility that there may be very low seismic background noise levels on the ocean floor. To accomplish this, three different types of ocean bottom seismographs are under development, and field results will start becoming available by the end of calendar 1961. A transition is also underway from complete dependence on photographic recording to supplementary recording on slow-speed magnetic tape for greater dynamic range, ease of digitalization, immediate playback through special filters, and suitability for electronic data processing as to the nature of the signal and background noise level. Important improvements have been made in long-period seismographs, so that they become stable in the field within a matter of hours, not days. Improved long-period galvanometers, filters, timing devices and amplifiers, both photo-tube and solid state, are also well along in the development stage.

In view of the fact that improved seismological arrays, research investigations, cooperative networks and prototype detection stations are starting to generate seismic data at a rate many times that previously available for study, research into greatly improved data processing, analysis and display techniques are being prosecuted under 14 contracts with university and industrial research groups. High-speed computer techniques are being attempted with good success for spectral analysis, determination of energy spectra through autocorrelation techniques, and objective analysis of the polarity of first motion. Work to date indicates that the amplitude and frequency content of the seismic power spectrum of earthquakes and buried explosions varies somewhat systematically with azimuth about the disturbance and its initial energy content. Also of interest is that the analyses of digital seismic data taken by Texas Instruments for part of the chemical explosions in Project Cowboy [5] suggest that the degree of decoupling is a function of frequency; that it varies with the mode of seismic propagation wherein the first motion arrival regularly appears to be decoupled less than the total energy; and that the decoupling factor increases with distance, all of which suggests a different partitioning of the elastic wave energy for coupled versus decoupled explosions. Two unusual techniques for seismic signal analysis are currently under investigation. In the first instance, advantage is being taken of the University of Michigan's Institute of Science and Technology pioneering certain optical techniques that appear capable of quickly correlating a large number of channels of analog seismic data; in the second instance, the long-term experience of Michigan and the Bell Telephone Laboratories in acoustics is being utilized to determine whether the human auditory system has the capability to distinguish between time-compressed seismic signals from underground explosions and earthquakes at comparable distances. Dr. Romney of AFTAC will also describe in subsequent testimony other research efforts leading to improved methods for handling seismic data from large detector arrays.

With respect to Vela Uniform's interest in the nature of the electromagnetic signals from underground disturbances, it has been known for some time that electromagnetic pulses generated by underground explosions could be detected at the surface of the ground in the immediate vicinity of the explosion. However, it has also been assumed that the signal would not be transmitted through the earth's surface layers for useful detection distances because of the high attenuation rate that exists there. Nevertheless, the possibility does exist that such a signal might be transmitted for useful distances at depths of a few tens of thousands of feet if, as has been predicted, the rate of signal attenuation is much less than at the surface. Because so little is known about this entire

[5] A series of chemical explosions in a salt dome in Louisiana which gave experimental proof of the theory of decoupling.

phenomenon, ARPA is currently sponsoring five research projects in the area of subsurface electromagnetic propagation to determine the nature of possible source mechanisms, the possible modes and efficiency of propagation, and the electrical properties of selected portions of the earth's crust. The study of source mechanism is theoretical in nature; at present, two models are being investigated, each requiring interaction between products of the nuclear explosion and the earth's magnetic field. With respect to the nature of electromagnetic pulse propagation, model studies are already underway. Arrangements are also being made by Edgerton, Germeshausen and Grier under the technical direction of the Rome Air Development Center for actually generating large electromagnetic signals later this year within a tunnel cut into granite and measuring these signals on the earth's surface at varying distances from the source. Determination of the electrical properties of selected portions of the earth's crust has been assigned to the U.S. Geological Survey and includes compiling data already existing in scattered form within commercial electrical log libraries and accumulating new information by use of several specialized field measurement techniques.

The last phase of the Vela Uniform research effort to be described pertains to marine detection techniques. At the present time, the U.S. Navy Electronics Laboratory is investigating for ARPA the feasibility of a hydroacoustic detection network of the type briefly described at Geneva. At the U.S. Naval Radiological Defense Laboratory, ARPA is also sponsoring a study into the problem of collecting samples from a suspected underwater explosion site after oceanic currents and turbulence have scattered and diffused the radioactive debris. Preliminary conclusions suggest that adequate hydroacoustic coverage of major ocean areas cannot be obtained if only stations in the proposed Geneva-type test control net are utilized. Even if the order of 30 additional hydroacoustic stations were added, there appear to be certain oceanic areas wherein underwater tests could not be detected with conventional hydroacoustic coverage.

To determine the validity of these early conclusions, a cooperative program for seismic and hydroacoustic monitoring of approximately ten 5-ton underwater explosions of the Navy-DASA Hydra II series during August 1961, near San Clemente Island, Calif., has been arranged. Present plans call for approximately 12 agencies to monitor the series at about 23 locations. Major participants include AFTAC-directed seismic crews of the U.S. Coast and Geodetic Survey and the University of Michigan. These will be stationed throughout southern California and, with the assistance of the Navy, on several of the channel islands. Extensive hydrocoustic measurements are also being made at short ranges by the U.S. Navy Electronics Laboratory and at longer ranges by the University of Hawaii and the U.S. Navy Hydrographic Office. If all goes as planned, this investigation will soon provide urgently needed data on—

(*a*) Effect of ocean bottom topography, islands and coast lines upon the transmission and reception of hydroacoustic signals.

(*b*) Effect of depth of detonation and shot distance off-bottom upon generation of seismic and hydroacoustic signals.

(*c*) Usefulness of an integrated analysis of hydroacoustic and seismic signals for detecting and identifying underwater explosions of less than kiloton yield.

I would also like to mention that in May 1961, ARPA began sponsoring, through the Office of Naval Research, a Columbia University program in seismic and hydroacoustic measurements within the Arctic Ocean by use of the drifting ice island, Arlis. Equipment has been installed and data are just now becoming available.

In conclusion, I would like to note that the Vela Uniform research effort has work well underway on all the pertinent recommendations of the Berkner Panel. In addition, the program has been broadened to include other aspects of subsurface nuclear test detection not conceived of at the time the panel issued its report.

This concludes my statement. I would be pleased to answer any questions.

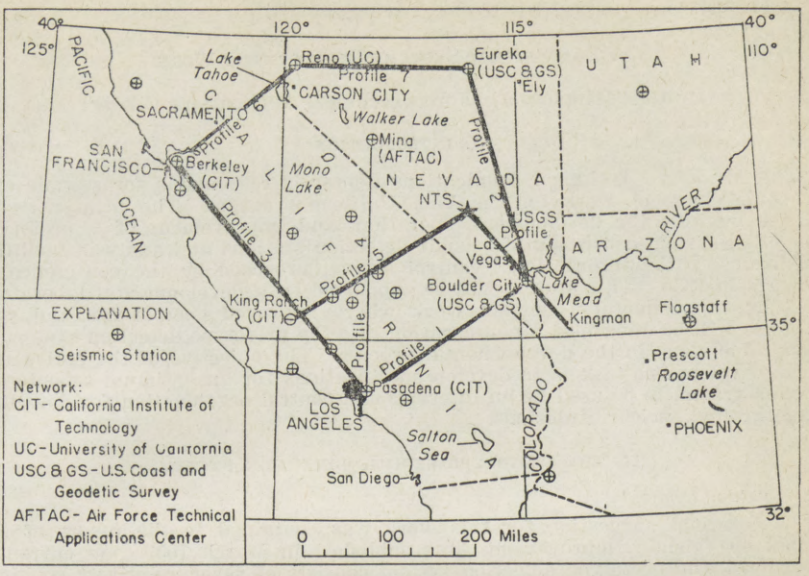


FIGURE 1.—*U.S. Geological Survey crustal study profiles*

Chairman HOLIFIELD. Dr. Carl Romney of the Air Force Technical Application Center will be our next witness. And he will present testimony on seismic systems development.

Dr. Romney, it is a pleasure to have you before the committee again. Your efforts contributed a great deal to the success of the 1960 hearings.

We are glad to hear from you today.

STATEMENT OF DR. CARL ROMNEY,[1] AIR FORCE TECHNICAL APPLICATIONS CENTER

Dr. ROMNEY. Thank you, Mr. Chairman.

May I ask if, in view of the time, you would wish me to submit my paper for the record, and give you a summary of it?

Chairman HOLIFIELD. I believe that would be good. If you could take each section and summarize it, we will print it in the record in its entirety.

Dr. ROMNEY. Yes, sir. I will be happy to do the best I can.

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[1] Place and date of birth: Salt Lake City, Utah; June 5, 1924.

Education: B.S. (meteorology), California Institute of Technology, 1945; Ph. D. (geophysics), University of California, 1956.

Employment: Meteorologist, U.S. Navy, 1945–46; research assistant in seismology, University of California, 1947–49; seismologist, later chief seismologist, Beers and Heroy (consulting geologists, geophysicists, and engineers), 1949–55; geophysicist, later Assistant Technical Director, Air Force Technical Applications Center, 1955 to present; AFTAC representative, Vela-Uniform program, 1959 to present.

Related professional experience: Member, U.S. Delegation to Geneva Conference of Experts, 1958; member, Panel on Seismic Improvement (Berkner Panel), 1959; member, U.S. delegation to Conference on Discontinuance of Nuclear Weapon Tests, Technical Working Group 2, 1959; member, U.S. delegation to Conference on Discontinuance of Nuclear Weapon Tests, Seismic Research Programme Advisory Group, May 1960; member, U.S. delegation to Conference on Discontinuance of Nuclear Weapon Tests, technical adviser, initial session, March 1961.

(The prepared statement of Dr. Romney follows:)

AFTAC Seismic Systems Development Work

(By Carl Romney, Air Force Technical Applications Center)

I. INTRODUCTION

The Air Force Technical Applications Center is responsible for seismic systems development work for the Vela Uniform program. The principal part of the work is the development, operation, and improvement of a prototype detection system consisting of seismic detection stations and analysis facilities analogous to the international control system proposed by the conference of experts in 1958. This system will be supported by a developmental laboratory and by supplementary contract work where improved components and subsystems will be developed for integration into the prototype detection system at a suitable stage in the developmental process. The objective of the entire systems development task is to develop specifications for an optimum seismic detection system to be used by an international control organization for detecting underground nuclear explosions.

II. THE PROTOTYPE SEISMIC DETECTION SYSTEM

A. System concept

The original suggestion for this work was contained in the report of the Panel on Seismic Improvement, first published in March 1959. As currently conceived, the prototype detection system consists of seven relatively sophisticated seismographic stations reporting by suitable data transmission channels to an analysis center where the data are evaluated. Five of these seven stations will employ all the seismic equipment recommended by the 1958 Conference of Experts. This consists of 21 seismometers, amplifiers, recorders, a timing system, and other auxiliary equipment. Later in my discussion, I will describe in more detail one station which has been completed and is now in operation, so that you will have a more thorough understanding of the station and how it operates.

In addition to these five stations equipped in accordance with the recommendations of the Conference of Experts, the prototype seismic detection system will have two large stations employing many more seismometers, perhaps ultimately to exceed 100, and other detection equipment of advanced design.

It has also been decided to establish a laboratory which would be charged with engineering design, calibration, and test responsibilities leading to improved components, instrumentation techniques, and operational techniques. The laboratory will also assist in developing equipment and techniques for processing and evaluating data from the detection stations.

Under the present concept, the prototype seismic detection system is expected to function as follows:

(1) First, the best stations that can now be designed will be constructed and operated to record earthquakes and large underground explosions as they occur. These stations will be continuously improved and the amount of improvement determined. The data from these stations will then be reported to an analysis unit for processing and evaluation. During periods when network operation is being simulated, this data transmission will be analogous to the various possible methods that could be employed in the proposed worldwide international control system. The epicenter of each earthquake and explosion will be determined and each seismic event studied to determine such matters as whether or not the nature of the source can be established from the recorded seismic data. In trying to establish whether the source of an event was an earthquake or an explosion, the data analysis unit will apply various identification criteria which are now known or have been suggested for trial, as well as new criteria developed by various research studies now in progress. After the accumulation of operational experience in utilizing such criteria, the detection equipment will be modified and improved so as to emphasize the most effective criteria.

(2) Similarly, the detection components themselves will be tested to identify and correct operating problems and to learn which instruments produce useful data and which do not. Analytical methods will be developed which can be applied at the detection stations, thereby reducing the amount of information

which must be transmitted. Requirements will be developed for the communications system, and the methods for reporting information from the stations to the data handling unit will be improved so that the most useful information is reported first, with less important information being assigned a lower transmission priority. Thus, through the operation of the prototype detection system, the capability of the system will be continuously upgraded.

From the nature of this process, it is apparent that the personnel responsible for the development and improvement of the prototype seismic system must be competent scientists and engineers skilled in various facets of data processing, seismology, engineering, and other specialized fields. In addition to conducting research of a highly original nature themselves, these people must be supported by, and promptly informed of, the research results and ideas developed by other participating governmental laboratories, industrial contractors, and universities who are also active in the Vela Uniform program.

B. Status of prototype seismic detection system work

(1) The initial station of this prototype seismic network has been constructed, and became operational in October 1960. The station is known as the Wichita Mountains Seismological Observatory, and is located about 15 miles northwest of Lawton, Okla. AFTAC was able to achieve this result rapidly because the station involved only a modest expansion of an existing Air Force research facility where most of the techniques and equipment employed had previously been developed.

A schematic diagram of the station is shown in figure 1 (p. 89). This station has an array of 10 short period vertical seismometers, with the instruments spaced at intervals of about 3,000 feet from the other detectors in the array. Thus, the array covers an area of about 3 by 3 kilometers (a square 2 miles on each side). The purpose of this array is to detect smaller signals than can be detected by a single instrument. Such an array is effective for detecting small signals because the noise in the earth at each detector will be partially canceled when the outputs of the 10 detectors are combined, but the summed outputs of signals will remain 10 times that obtained from a single detector. Thus the relative size of the noise is reduced with respect to that of signals of interest. Our operational experience to date shows that signals can be detected by this array which are about one-third the amplitude detectable on a single seismograph. In terms of detection of explosions, this means that if a 10-kiloton explosion could be detected by a single seismograph, a 2-kiloton explosion could be detected at the same distance by an array of 10 detectors.

In addition to the array which I have described, there are other seismographs which bring the total to 21 at this station. These additional instruments are for the purpose of recording seismic information at other frequencies than those detected by the array instruments. I should like at this point to introduce into the record Information Bulletin No. 1 of the Wichita Mountains Seismological Observatory (see p. 91). This document describes in some detail the location of the station, the manner in which the instruments are installed, the kinds of instruments in operation, and shows samples of the seismograms obtained. I would like to direct your attention to figure 18 on page 27 (p. 117) of this bulletin, which shows a sample seismogram. The 13th line from the top shows the ground motion from the array. You will note that signal which occurs in the middle of the record is recorded on a trace which is relatively noise free, compared to the traces above which are recordings of ground motion obtained from the individual seismographs. This illustrates my earlier comment to the effect that the array makes it possible to detect smaller signals than could be detected on a single instrument.

Since this station became operational in mid-October 1960, we have gained valuable experience which has resulted in several improvements to the instrumentation, and have improved the design of the vaults in which the detectors are installed. We have learned that one of the instruments recommended by the Conference of Experts contributes relatively little to the capability of the station; I refer here to the instrument described as the "intermediate band" seismometer. We have also learned that the minimum signals detectable by the station at distances of 2,000 miles and more are from magnitude four earthquakes, corresponding to underground nuclear explosions of slightly more than 1 kiloton detonated in tuff. Information on first motion cannot be obtained from such minimum signals, of course, and it is estimated that first motion, of use in identifying the nature of the source, can be obtained only from earthquakes an order of magnitude larger. Since the station is located in a very quiet area, it is likely that not all similar stations to be constructed will perform as well.

(2) Site surveys for the additional six seismic detection stations are nearing completion. These surveys, conducted at many locations throughout the United States, considered both technical and logistic factors. For example, since one of the chief requirements for these stations is that they be located at quiet sites, noise measurements were made at each site under study and many otherwise promising sites were rejected because they were too noisy. The best locations, revealed by these studies, for the four additional stations of the type just described, are: northeastern Oregon; near the southern end of the Sierra Nevada in California; northeastern Utah; and north-central Montana. The two larger stations, with arrays which may ultimately reach 100 detectors, will be built in central Arizona and central Tennessee.

The seven stations (including WMSO) will thus lie on a grid, spaced approximately as recommended by the Conference of Experts. The locations in the Western United States are about 1,000 kilometers apart, and the Eastern United States site tentatively selected in Tennessee, is about 1,100 kilometers from the station in Oklahoma.

(3) A contract has been awarded for the construction and operation of the four smaller stations. Provided there are no unforeseen administrative problems, these stations can be in operation early in 1962.

(4) The design of the two larger stations has been completed, and a study has been made of the supporting facilities and personnel needed to operate these stations. We are currently working out the administrative details necessary to construct and operate the stations.

(5) Similarly, a study has been made of the requirements for the developmental laboratory which will be charged with a major responsibility for improving the network of stations. This study is currently being evaluated by the Government and we expect to proceed with the work necessary to establish the laboratory in the very near future.

(6) The requirements for effectively analyzing and evaluating the large quantity of high-quality seismological data which will result from these stations when they become operational, is now under study. Several plans are currently under consideration, and a decision on how to proceed will be made shortly.

III. RELATED SEISMIC SYSTEMS DEVELOPMENT WORK

AFTAC is also responsible for three programs which are closely related to the development of the prototype seismic detection system. We plan to carry this work forward to the point where the major problems are solved, and then incorporate the resulting developments into the prototype system. The status of these programs is as follows:

A. Array studies

This work involves the study of the basic characteristics of seismic noise and signals, to be followed by theoretical and engineering developments leading to the specification of optimum procedures for detecting seismic signals in the presence of noise. The theoretical phase of this work has been completed, and the contractor is beginning the practical engineering work which will lead to an experimental test of the theoretical ideas revealed by the study and to the development of hardware which can be used in a detection system. The work to date is highly encouraging. On the basis of reasonable models of signals and noise, it has been concluded that an improvement on the order of $(2N)^{0.5}$ or better over a 2-to-3 octave pass band can be achieved in a practical system. (N is the number of seismometers in the array.) This means that an array having 30 seismometers could detect signals 7.5 times smaller than could be detected by a single seismometer; similarly, an array of 100 seismometers could, in principle, detect a signal 14 times smaller than could be detected by a single seismometer. Experimentation is, of course, required to verify these predictions.

B. Unmanned seismic stations

As you will recall from the Joint Committee hearings of a year ago, unmanned seismic stations have been suggested as a means for detecting and locating the source of decoupled explosions. In concept, these are rather simple stations which would be disposed in large numbers around the territory where decoupled explosions might take place. Each station would report information to one of the control posts of the international control system by means of a radio or hard wire communications channel. Initially it is planned to build and evaluate one such station. A contract is being negotiated at present, and work should start during August 1961.

C. Deep well detectors

In the report of the Panel on Seismic Improvement, I described work completed and in progress on the development of seismometers for detecting seismic waves in deep wells. An Air Force-sponsored program which was completed in the summer of 1960, showed that the theoretical predictions on the amplitude of noise at depth were correct. It was also shown that detectors, operated in deep wells several thousand feet below the surface of the earth, can detect smaller signals than are detectable at the surface of the earth, because there is less noise at great depths than at the surface. Having established that the method works in principle, the remaining work necessary to exploit this technique in a detection system is to develop satisfactory instruments which can operate efficiently at great depths for long periods of time, and to determine the optimum geological environments for installing such detectors. Under Project Vela, we have recently initiated two projects for developing operational detectors to be used in this subsurface environment. We have also initiated a theoretical study of the noise and signal amplitudes to be expected at various depths below the earth's surface in various geological environments. We expect that these three projects will have progressed where the experimental work, necessary for verification of the results, will be underway during the spring of 1962. Subsequently, deep well detectors will be incorporated into the prototype seismic detection system for operational evaluation.

FIGURE 1

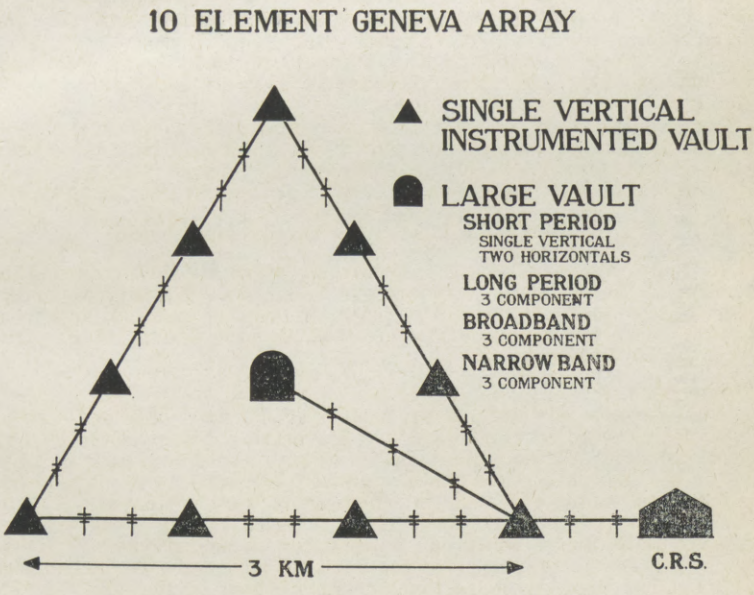
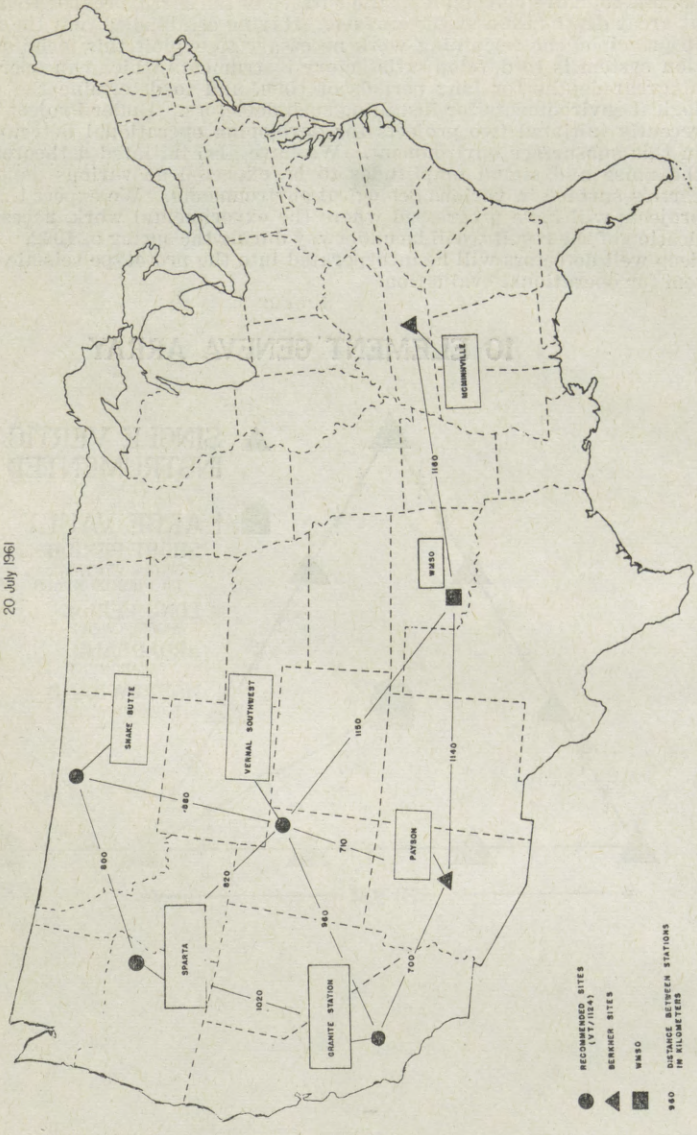


FIGURE 2

STATUS OF SITE SURVEYS
FOR
SYSTEMS DEVELOPMENT STATIONS

20 July 1961



WICHITA MOUNTAINS SEISMOLOGICAL OBSERVATORY

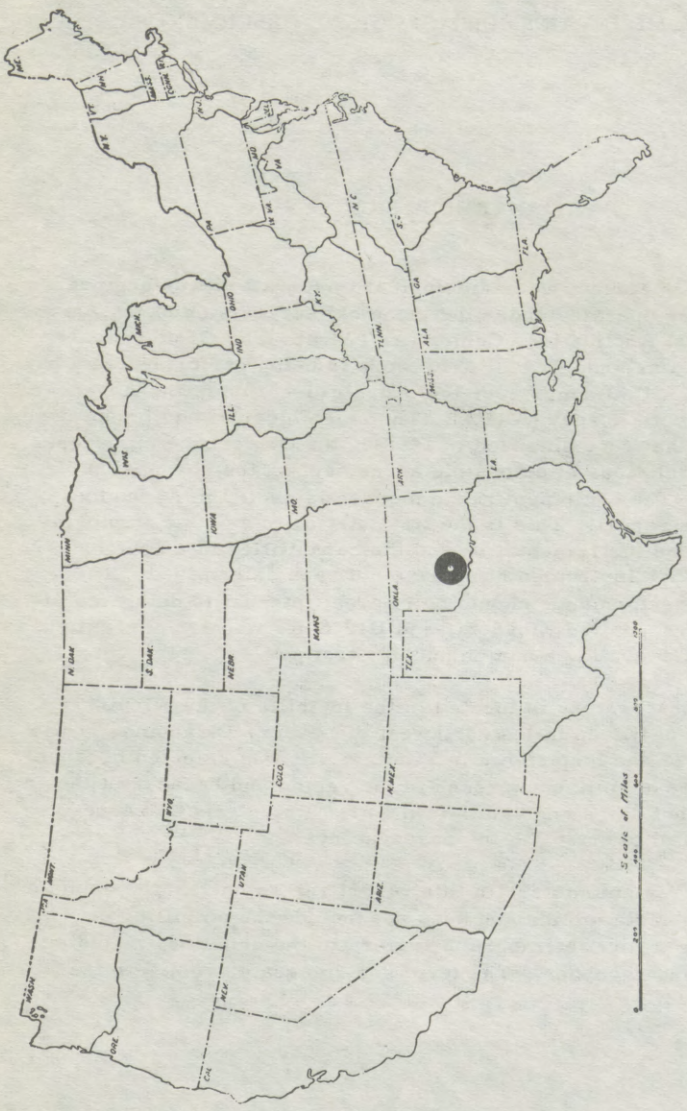
1. INTRODUCTION

1.1 The Wichita Mountains Seismological Observatory was engineered and is now being operated under the technical supervision of the Air Force Technical Applications Center (AFTAC) by The Geotechnical Corporation of Garland, Texas. The work is being performed as a part of Project VELA Uniform, under the overall direction of the Advanced Research Projects Agency (ARPA). The seismological equipment used is identical to that recommended in 1958 by the Conference of Experts for detecting violations of a possible agreement on the suspension of nuclear tests. The equipment recommended is quoted in Appendix I attached to this report. This is the first station to be built employing the recommended instrumentation and the capabilities of a control system using such instrumentation have not been determined. The observatory's performance should be of great interest to delegates of the United States, United Kingdom, and U.S.S.R. who are currently negotiating in Geneva for a ban on nuclear testing.

1.2 The new observatory is located in the foothills of the Wichita Mountains, about fifteen miles northwest of Lawton, Oklahoma. The site is located on the map shown in Figure 1. It was selected because of the low microseismic background in the region and because of the availability of buildings and vaults in the area which greatly reduced the time required to establish the observatory.

1.3 The term "seismometer" in this report refers only to the detector that senses the earth motion and does not include the amplifier or recorder. The term "seismograph" refers to the detector, including the amplifier and recorder. The term "seismogram" refers to the resulting record.

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Figure 1. Location of Wichita Mountains Seismological Observatory

2. LAYOUT OF THE OBSERVATORY

2.1 Figure 2 shows a pictorial diagram of the layout of the observatory. A total of ten vaults covered with earth can be seen in the illustration. The four large vaults shown in the upper part of the diagram are constructed of concrete and contain the intermediate-band and broad-band seismometers specified in Appendix I and four of the ten seismometers in the array. The other vaults are watertight cans located in a shallow excavation and contain the remaining six seismometers of the array. Approximately two feet of earth is piled over the cans to protect the instruments from temperature changes. The data from the seismometers in the vaults is transmitted to the amplifiers located in the smaller building, and then to the larger central-recording building.

2.2 Figure 3 shows a scale layout of the vault pattern. This pattern was selected because it was the pattern that could be constructed most easily considering existing vaults, cables, buildings, rock outcrops, accessibility, and other factors. It is thought that a pattern of ten seismometers forming a series of equilateral triangles might be preferable and the array can later be changed to this configuration. All seismometers were placed on the granitic bedrock that occurs in the area. The mean amplitude of predominant microseisms in the period range 0.2 to 2 seconds is approximately 1/2 millimicron center-to-peak (0.000,000,5 mm) at a period of 0.5 second The spacing of the seismometers in the array is approximately two thousand feet.

2.3 Figure 4 shows a block diagram of the instrumentation. The ten-element array, the three-component short-period seismograph, the three-component broad-band seismograph, the three-component intermediate-band seismograph, and the three-component long-period seismograph require a total of twenty-one seismometers. One of the vertical seismometers of the ten-element array also serves as the vertical component for the three-component short-period seismograph The seismometers are located in ten separate vaults and drive photo-tube amplifiers located in the amplifier building near the central recording building. The output of the amplifiers is routed through the data control units and recorded on 16-mm strip film and on magnetic tape. Trace, station, and date identification are printed on the film automatically at regular intervals. Precise time marks from a

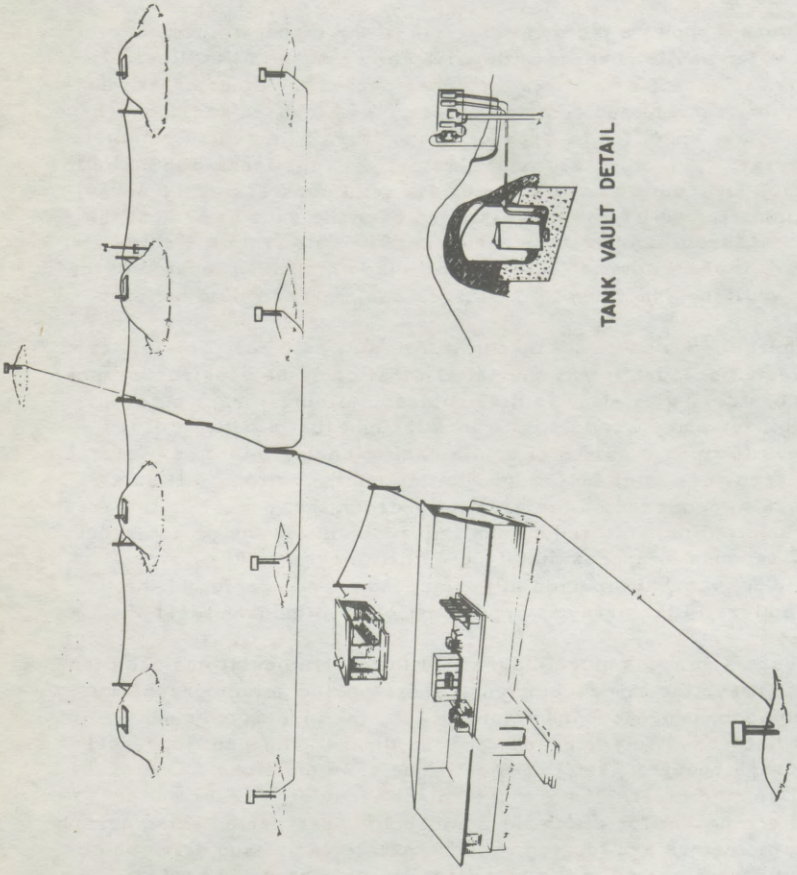


Figure 2. Building and vault layout

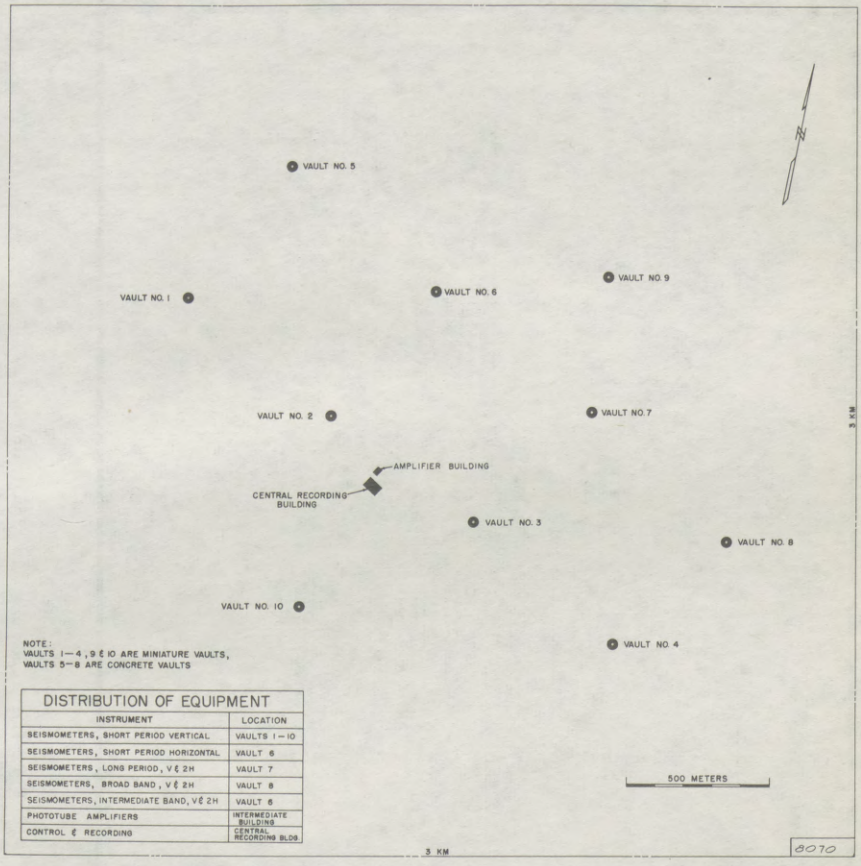


Figure 3. Array pattern

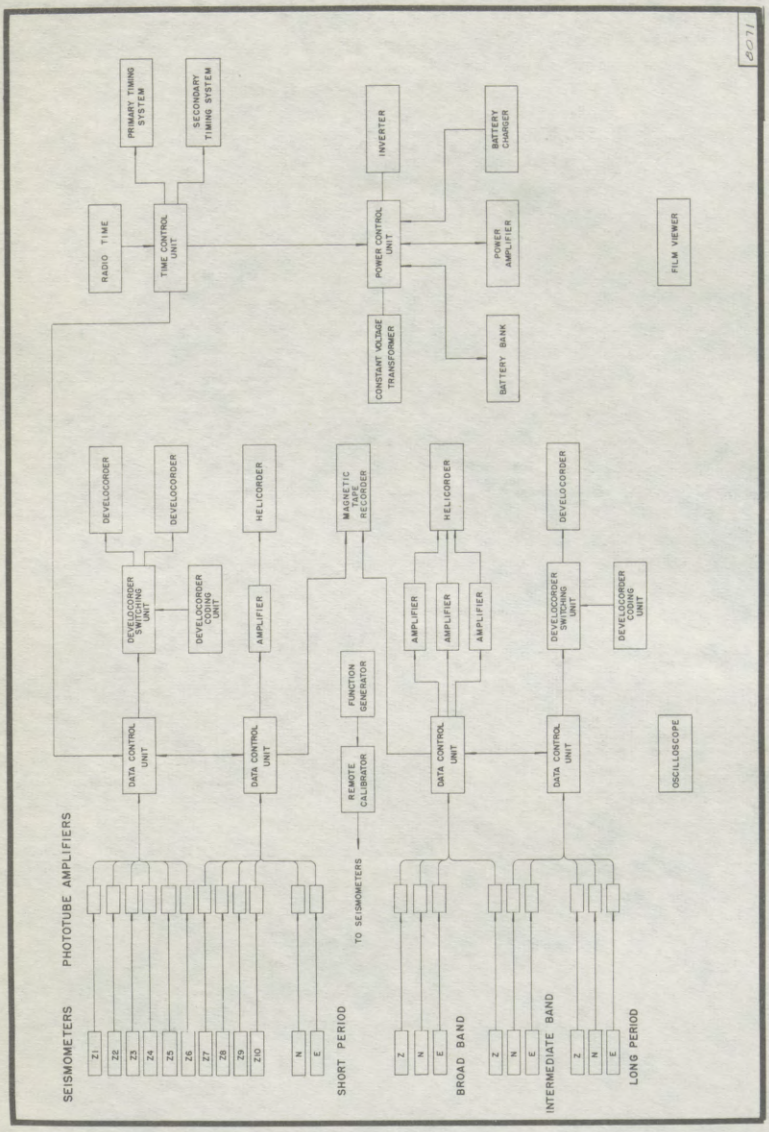


Figure 4. Block diagram of instrumentation

transistorized crystal-controlled timing system are placed on each record. The time is periodically checked against radio time signals received from Radio Station WWV in Washington, D. C. A bank of nickel-cadmium batteries provides standby power in case the main power system fails.

3. INSTRUMENT RESPONSE CHARACTERISTICS

3.1 Figure 5 shows the response characteristics of the four different types of seismographs used at the observatory. The amplitude of earth vibration in the case of the curves shown is held constant. Magnification is defined as the ratio of deflection on the record to the deflection of the earth. The ten-element array and the three-component short-period seismographs are capable of very high magnification. They can be operated with the full magnification shown only in the quietest locations.

3.2 The response of the long-period seismographs is shown to the right in Figure 5. This type of response characteristic is best suited for the detection of long-period surface waves, whereas the response of the short-period seismograph is best suited for the detection of the shorter period body waves. The intermediate-band seismograph emphasizes the period range of 1 to 3 seconds and rejects the shorter period signals. This type of response is very helpful in the study of shear waves. The magnification of the broad-band seismograph is limited by the 5- to 8-second microseismic noise always present in the earth; however, for larger disturbances it responds equally well to several wave types and provides a recording of the complete disturbance on a single trace. It should be noted that all four of the response characteristics have about the same magnification at a period of 6 to 7 seconds. This is because of the relatively high background noise in this period band. Experience has shown that the amplitude of the background noise decreases toward longer and shorter periods at about the same rate that the magnification increases on the short-period and the long-period seismographs.

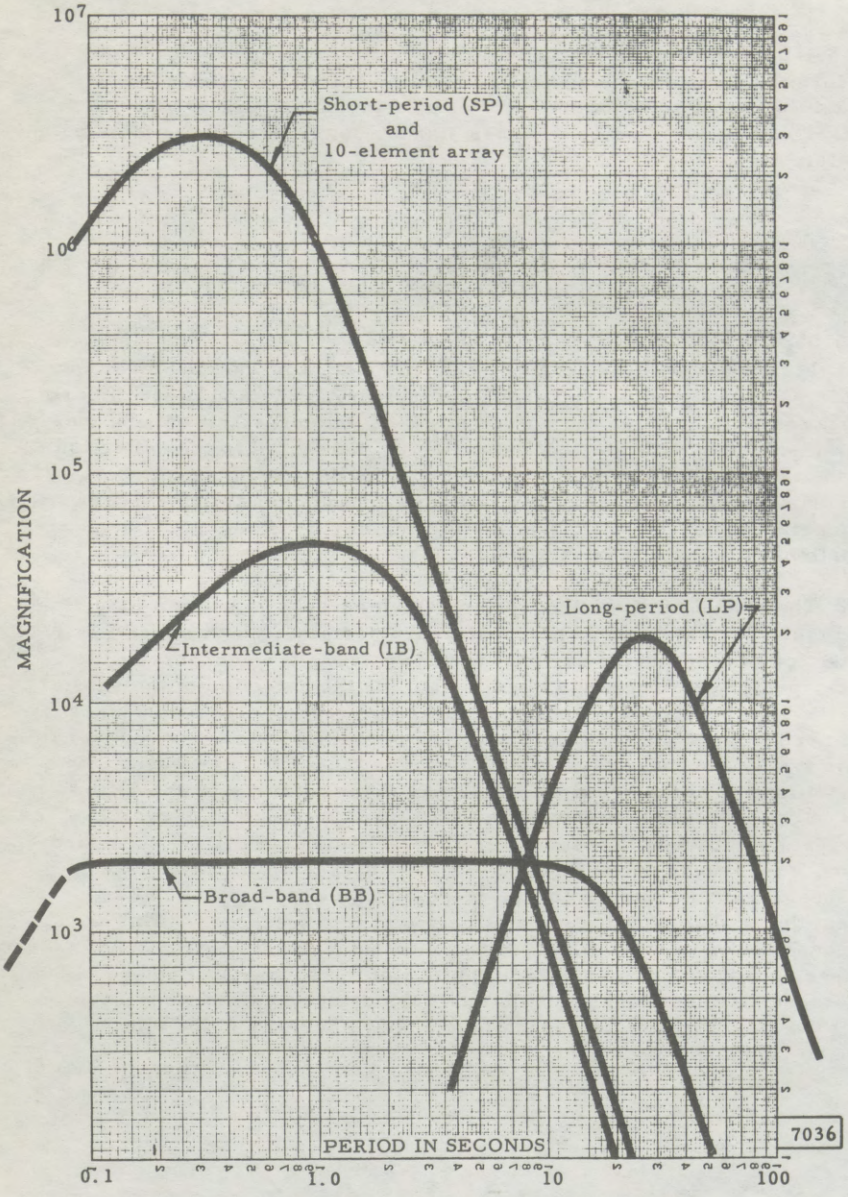


Figure 5. Response characteristics of seismographs

3.3 The characteristics of the instrumentation that result in responses shown in Figure 5 are tabulated below. In each case the galvanometer is placed in a phototube amplifier rather than recording directly on photographic film or paper as is normally done in university observatories. This enables higher magnification and more flexibility in recording.

	Short-period and array (SP)	Intermediate band (IB)	Broad-band (BB)	Long-period (LP)
1. Seismometer free period (seconds)	1.0	2.5	12.5	25
2. Seismometer damping*	1.0	0.65	0.4	1.0
3. Galvanometer free period (seconds)	0.2	0.64	0.64	30
4. Galvanometer damping*	1.0	1.5	9.0	1.0
5. Filter bandpass at 3-db cutoff points (seconds)	0.1-100	0.05-100	0.05-100	25-1000
6. Filter cutoff rate at short-period side (db/octave)	12	12	12	12
7. Coupling coefficient	0.01	0.0005(H) 0.002 (V)	0.0004(H) 0.0002(V)	0.004

* $\frac{\text{Critical circuit damping resistance}}{\text{Circuit damping resistance}}$

4. VAULTS AND SEISMOMETERS

4.1 One of the small sealed vaults is shown in Figure 6. The earth normally used to cover the top of the vault is piled to the right. The

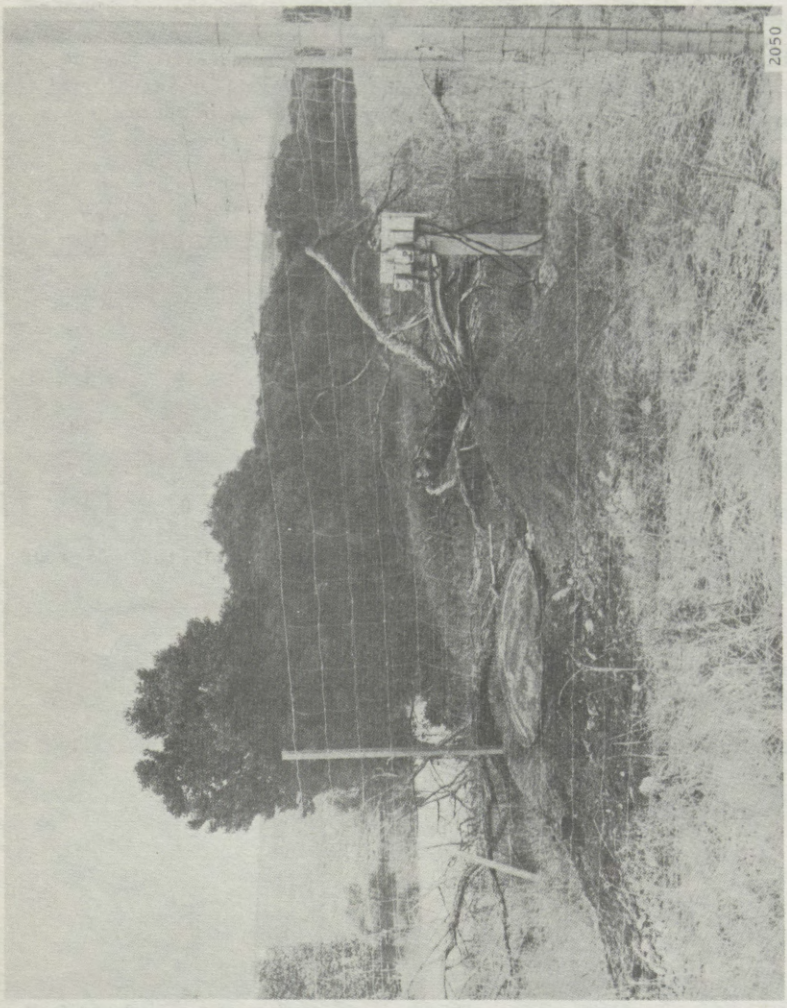


Figure 6. New type sealed vault and signal line terminations. Earth covering is removed to expose top of vault.

data line terminations and lightning protection devices for the seismometer are mounted on the post shown in the right portion of the photograph.

4.2 Figure 7 is a photograph of the Benioff vertical seismometer, which is located in the small sealed vault shown in Figure 6. This instrument has an inertial mass of 100 kilograms; it uses a variable-reluctance type transducer and operates with a natural period of one second. When connected to its associated phototube amplifier, it will resolve earth motions as small as one angstrom unit (0.000,000,1 mm) at a frequency of one cycle per second. This type of seismometer is used in seismological observatories throughout the world. It has a built-in calibration device so that calibrations can be performed without removing the cover from the vault.

4.3 Figure 8 is a view of the mound of earth that covers one of the large vaults. The vault is constructed of reinforced concrete and is located about fifteen feet below the top of the mound. This is one of four vaults which house the long-period, short-period, intermediate-band, and broad-band seismographs and four seismometers of the array. The seismometer used for the array and short-period seismograph was shown in Figure 7. The other seismometers are shown in Figures 9, 10, and 11, and described below.

a. Figure 9 shows the Sprengnether vertical seismometer, which is used as the vertical component of the long-period seismograph. It has an inertial mass of about 7-1/2 kilograms, a moving-coil type transducer, built-in calibrator, and is operated with a natural period of 25 seconds.

b. Figure 10 shows the Melton vertical seismometer, which is used as the vertical component of the intermediate-band seismograph. It has an inertial mass of about 5 kilograms, a moving-coil transducer, and built-in calibrator, and is operated with a natural period of 2.5 seconds.

c. Figure 11 shows the Press-Ewing vertical seismometer, which is used as the vertical component of the broad-band seismograph. It has an inertial mass of about 7-1/2 kilograms, a moving-coil type transducer, and built-in calibrator, and is operated with a natural period of 12-1/2 seconds.

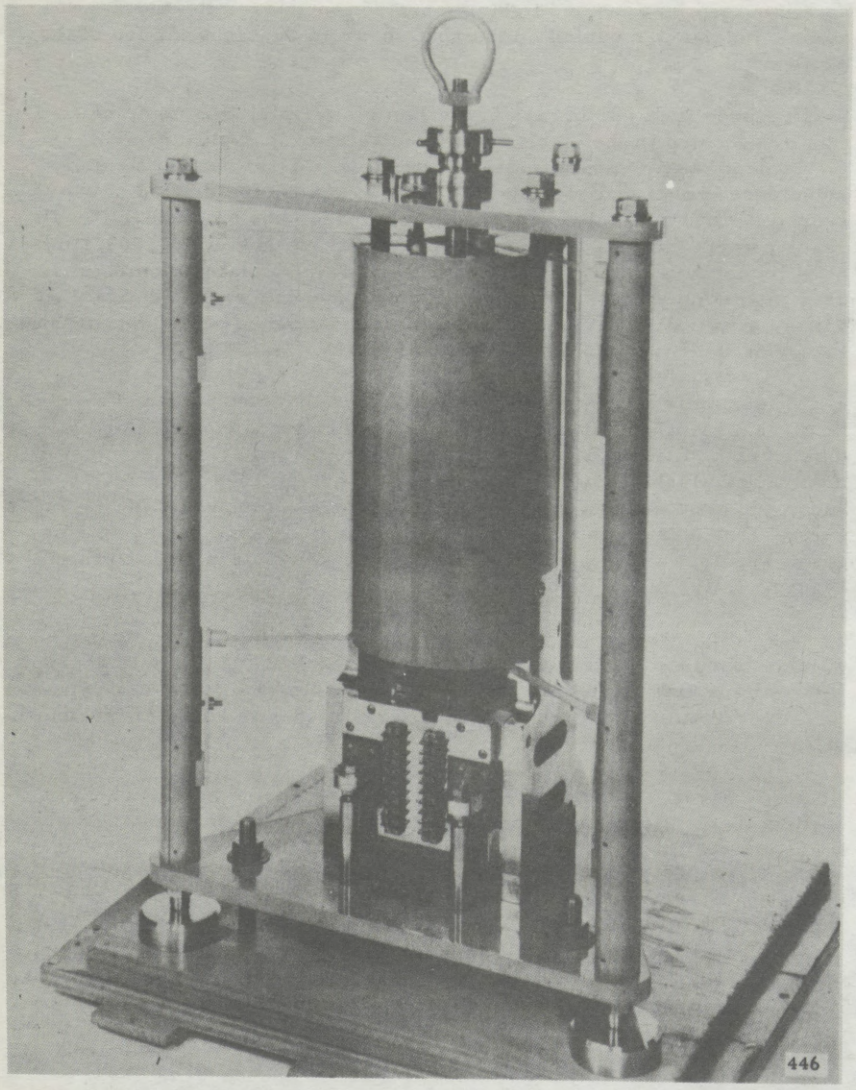


Figure 7. Benioff vertical seismometer used in 10-element array and in 3-component short-period seismograph

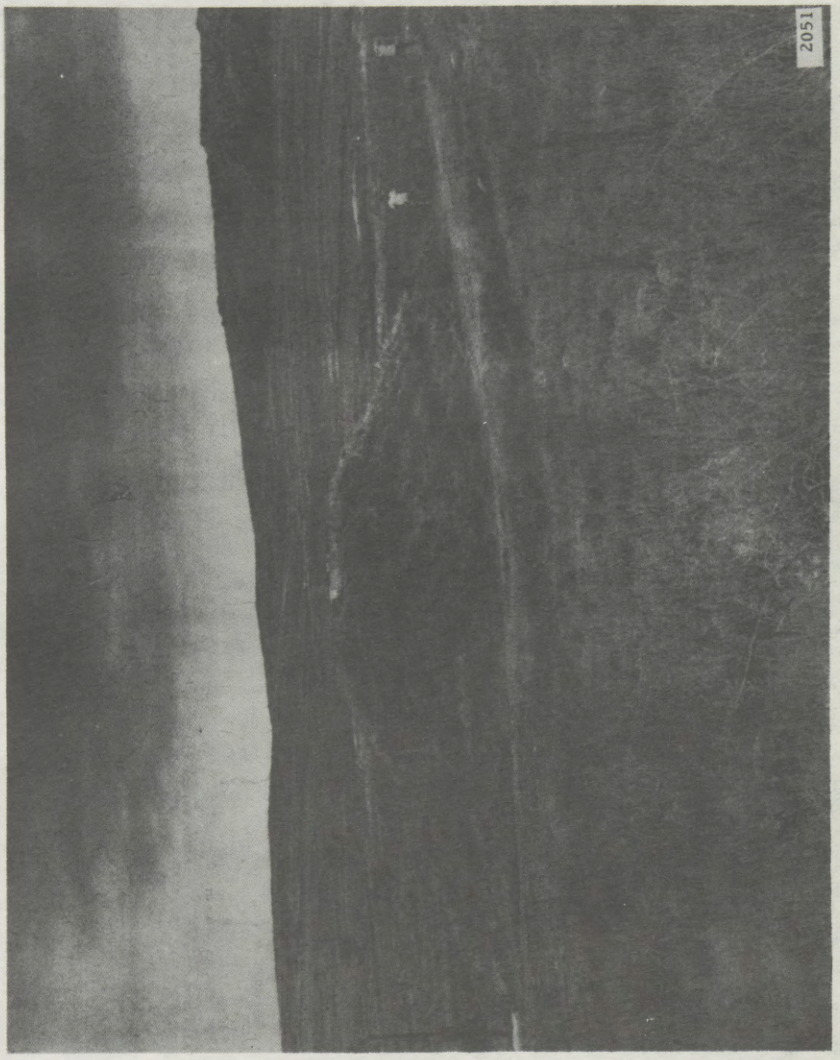


Figure 8. Typical large underground vault, which contains four types of seismometers

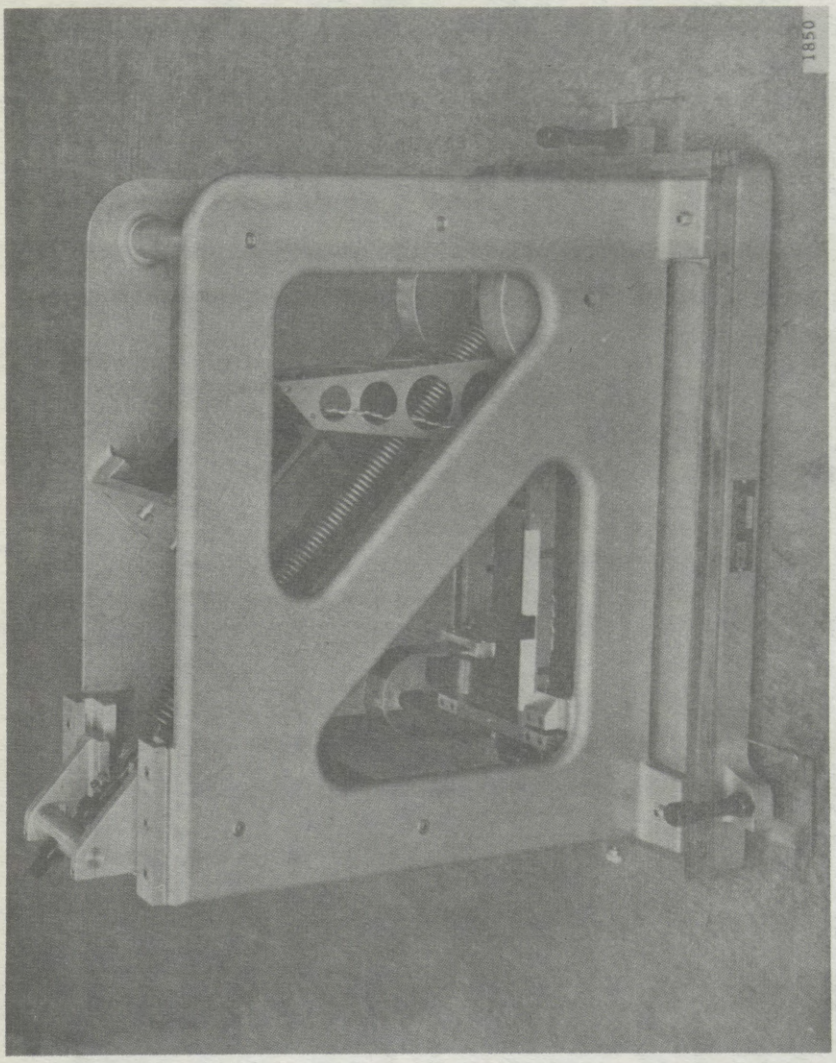


Figure 9. Sprengnether vertical seismometer used in the long-period seismograph

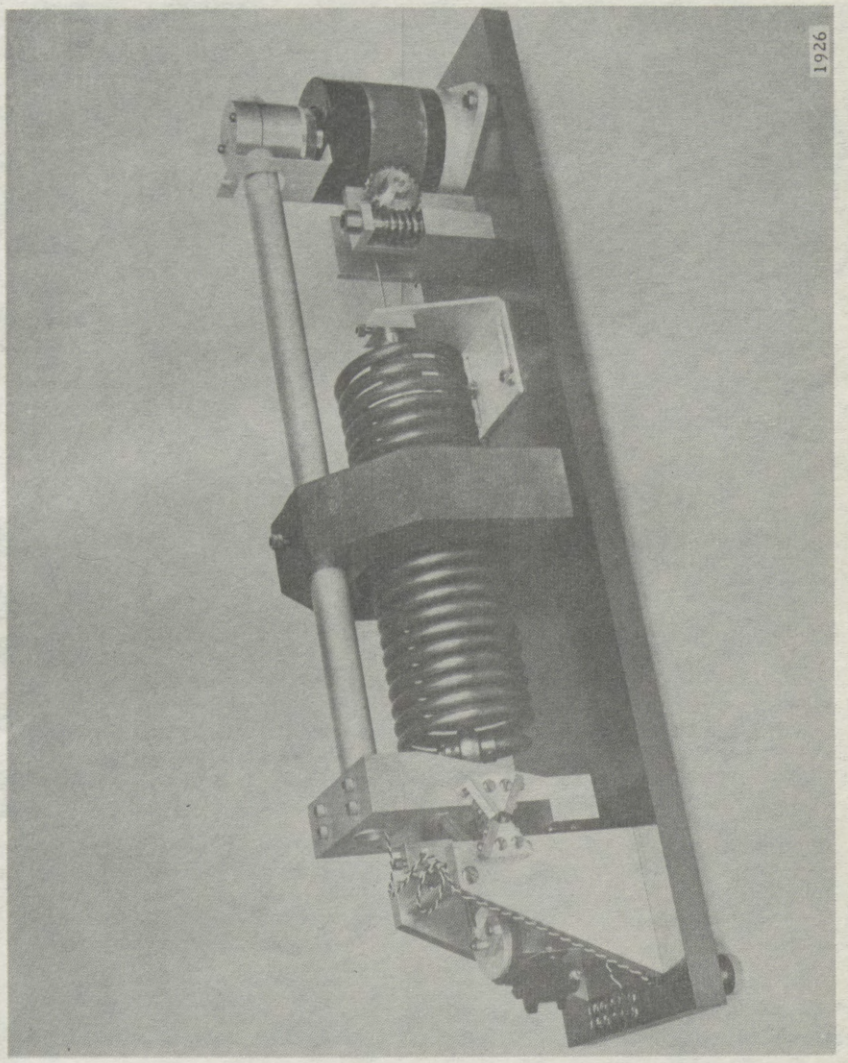


Figure 10. Melton vertical seismometer used in the intermediate-band seismograph

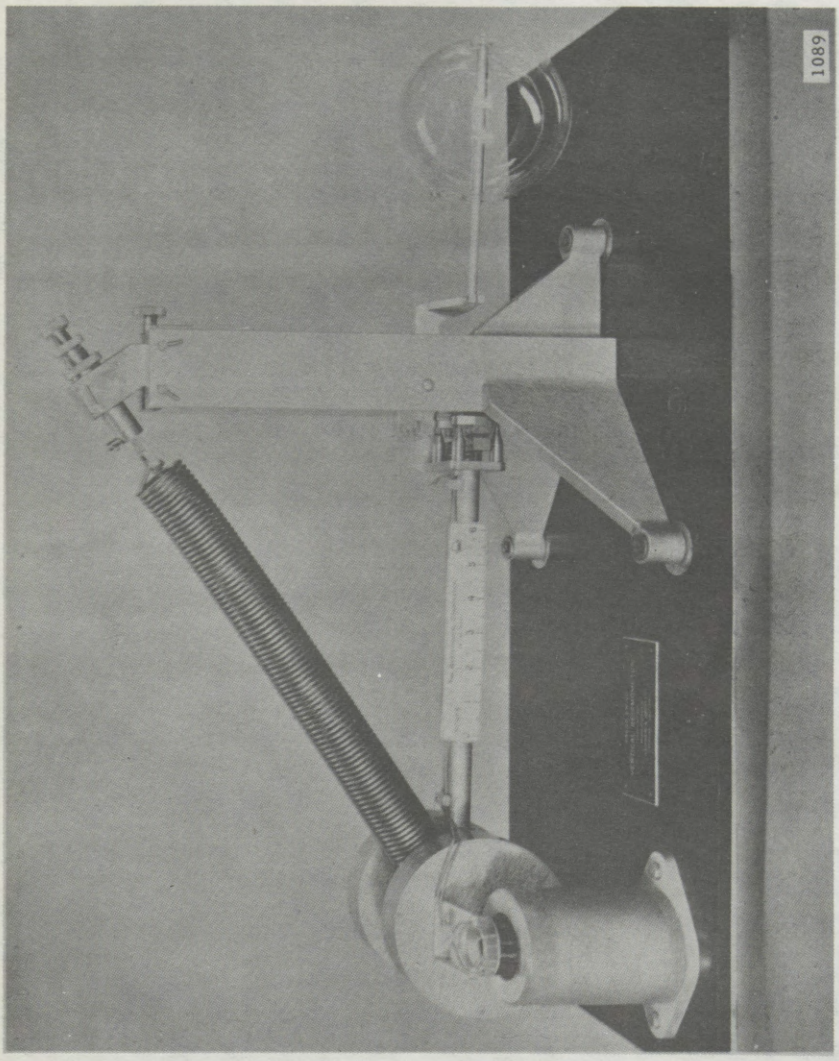


Figure 11. Press-Ewing vertical seismometer used in the broad-band seismograph

5. AMPLIFIERS

5.1 Figure 12 shows a view of the central-recording building and several supporting structures. The cables over which the electrical signals from the seismometers are transmitted are shown at the extreme left of the photograph. The cable enters the first building to the left, which houses the phototube amplifiers. Here the signals are amplified and then routed to the line that carries the signal to the central-recording building shown on the right. The four small buildings in the center of the photograph are used for storage. The main power line can also be seen in the foreground.

5.2 Two types of amplifiers are located in the amplifier building. The short-period phototube amplifier is shown in Figure 13; the long-period phototube amplifier is shown in Figure 14. In both cases the amplifiers are shown with their covers removed. The amplifiers are essentially the same except for the type and period of the galvanometer used (see table on page 9) to obtain the initial amplification. The light reflected from the galvanometer mirror falls on two photocells and the optical system is designed so that, as the galvanometer deflects, the light varies in intensity but does not change its position on the photocells. This improves the linearity. Both amplifiers have an internal noise level low enough so that the thermal agitation noise or "Johnson noise" of the seismometer coils can be seen. Two stages of cathode follower are used to match the impedance of the photocells to the lower impedance required to drive the recording devices. In each case, the galvanometer, phototubes, and other critical components are mounted in heavy metal cases that are sealed to prevent entry of moisture. Both amplifiers have a dynamic range of over 70 decibels and the short-period amplifier operates over a temperature range of from -60° to $+120^{\circ}$ F. The thermal stability of the amplifier using the long-period galvanometer is not as good because the long-period galvanometer is more sensitive to temperature changes. The use of the long-period galvanometer gives the added sensitivity and filtering required for the long-period seismograph.

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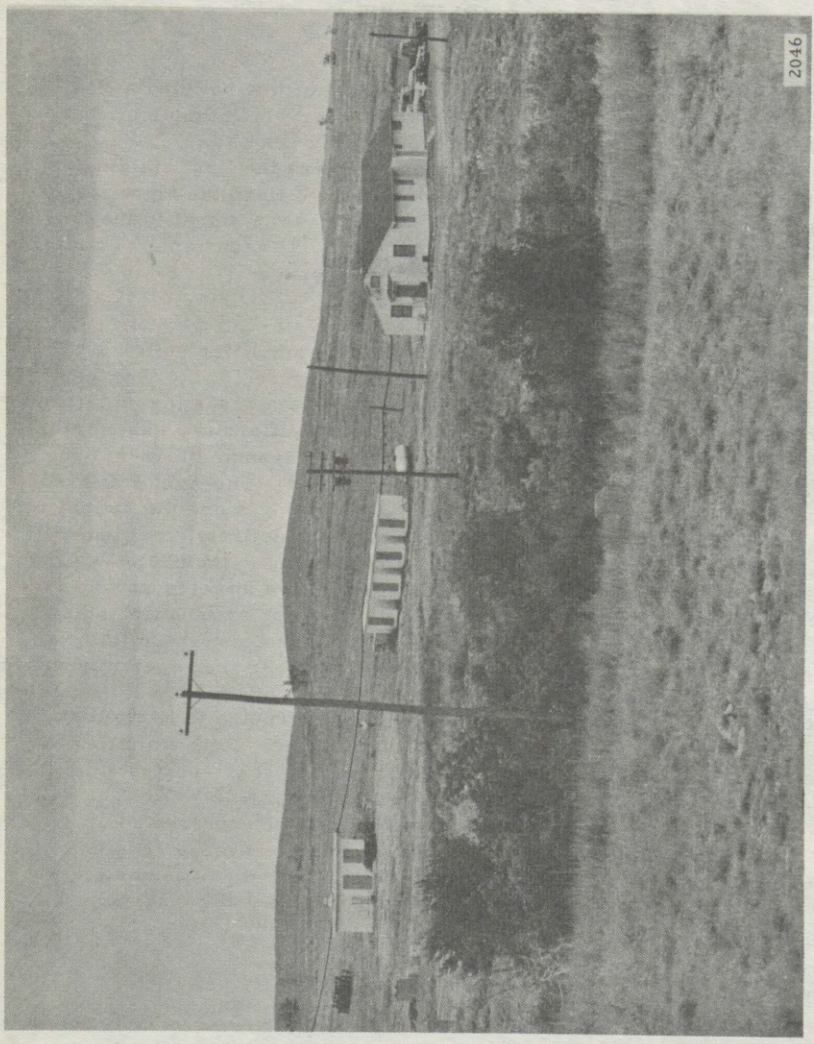


Figure 12. From left to right are shown the amplifier building, four storage huts, and the central-recording building

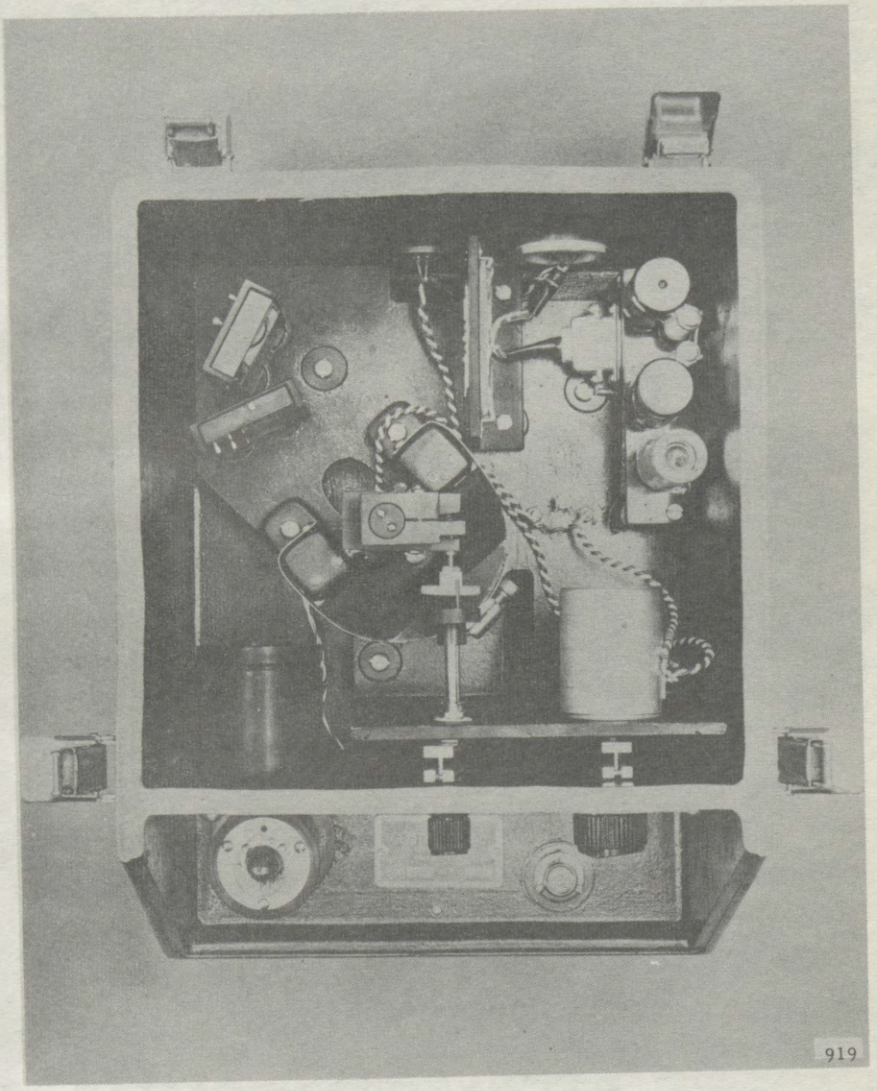


Figure 13. Short-period phototube amplifier with cover removed

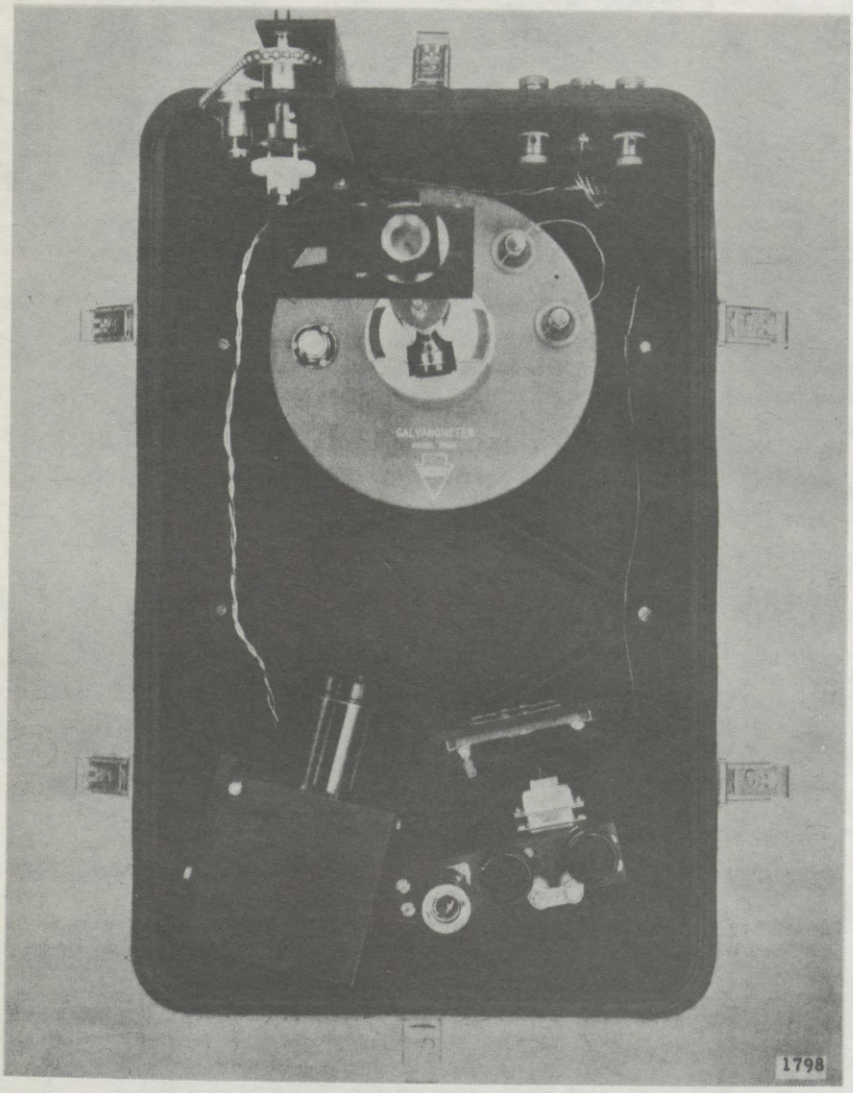


Figure 14. Long-period phototube amplifier with cover removed

6. DATA CONTROL, TIMING, AND POWER

6.1 The amplified signals are transmitted over the data line from the amplifier building to the central-recording building where they are routed to the data control panels located in the right portion of the console shown in Figure 15. The nickel-cadmium batteries used for emergency operation when the main power fails are located in the cabinet on the left. The second cabinet from the left contains a radio receiver, radio time signal converter, crystal-controlled timing system, and power circuitry. The two center cabinets contain the monitor recorders on which signals from either the array or short-period, intermediate-band, broad-band, or long-period seismometers can be recorded for monitoring or trouble-shooting. The recorder to the left has a single stylus and operates at a chart speed of 90 mm/minute The recorder on the right has three styli and operates at a chart speed of 15 mm/minute. The data to be recorded are selected by patch cords shown on the two cabinets to the right of the recorders The recorders are of the drum type using heat-sensitive paper. As the drum rotates, the stylus leads along the axis of the drum, subscribing a helix similar to the way a lathe cuts a screw thread After the recording is complete, the paper is removed from the recording drum, and when laid out flat, reads from left to right and top to bottom like the page from a book.

6.2 The two cabinets to the right of the monitor recorders contain the calibration equipment, data control modules, trace coding circuitry, oscilloscope, and accessory items. The data control modules are used to set the trace magnification levels and route the signals to the magnetic-tape and film recorders.

7. RECORDING

7.1 The magnetic-tape equipment is not shown in Figure 15 but is now located immediately to the right of the console. An Ampex FM magnetic-tape recorder is used and seven channels of data in the

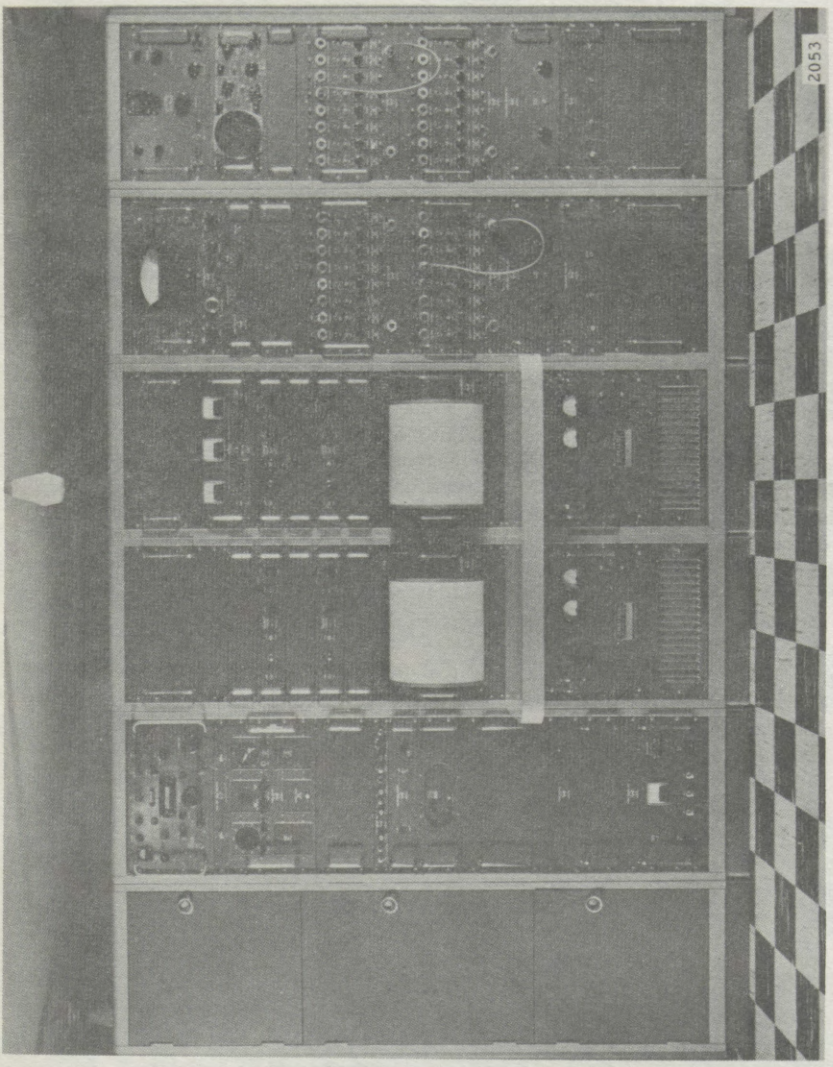


Figure 15. Data control, time and power console in the central-recording building

frequency range of from 0 to 20 cps are recorded. The tape speed is 0.3 inch per second, so a 2500-foot roll of tape will record for 24 hours. It is hoped that a 14-track FM recorder and equipment to record several channels of data in digital form can be added to the observatory at a later date

7.2 Figure 16 shows the film recorders, which in addition to the magnetic-tape recorder, are used for the primary recordings at the observatory. The recorders are located on the right in the photograph. Each recorder has 16 channels. The two recorders to the left record short-period and intermediate-band information, including data from the array; the recorder at the right records long-period and broad-band data. Galvanometers with a natural frequency of 16 cycles per second are used in all three recorders. The recordings are made on 16-mm film, which is processed automatically and projected on the viewscreen. In the short-period recorder, approximately ten minutes is required from the time the recording is made until the film is projected on the viewscreen. In the long-period recorder, approximately one hour is required.

7.3 Figure 17 shows a schematic of the automatic processing film recorder The light reflected from the galvanometer mirrors is focused by an optical system onto an opal screen. The galvanometer light spots imaged on the opal screen are then recorded on the film at about X10 reduction by a standard 16-mm camera lens. The film is fed from the supply reel across the developer, fix, and wash rollers to the film take-up reel. A meniscus action is used to feed the liquids to the bottom surface of the film. The top part of the film is not wet during the developing process Immediately after processing, the film is dried by a blower, then projected at X10 enlargement onto the viewing screen

7 4 In short-period recording, approximately 150 feet of film per day are required and the film speed is 300 millimeters per minute as viewed on the recorder screen. In long-period recording, approximately 30 feet of film per day are required, and the film speed is 60 millimeters per minute as viewed on the screen of the recorder.

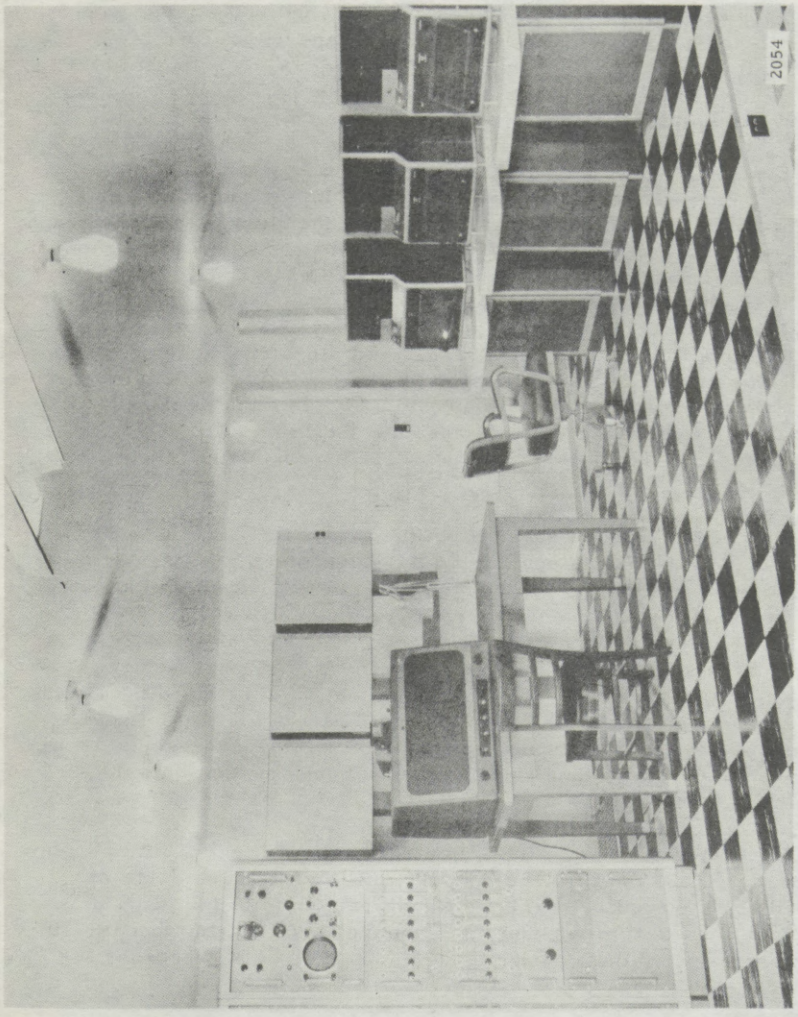


Figure 16. From left to right are shown the data control console, film viewer, and film recorders in the central-recording building

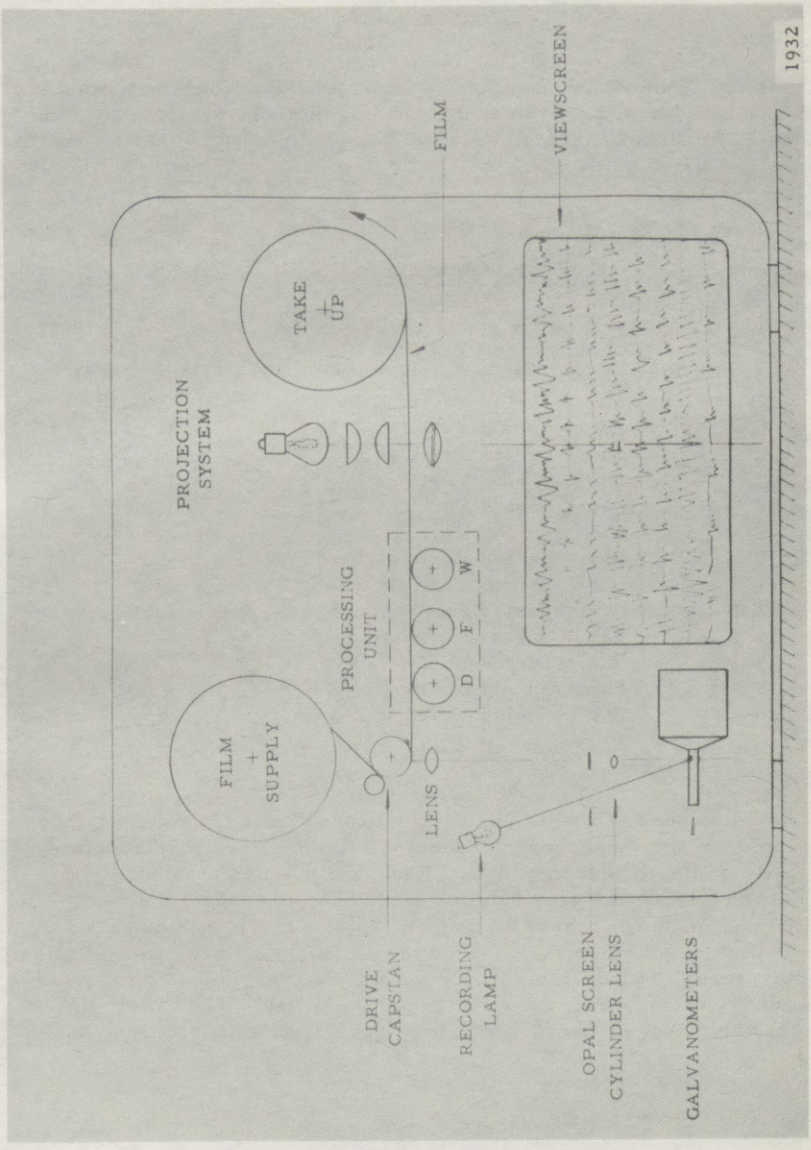


Figure 17. Schematic of automatic processing recorder (Develocorder)

8. SEISMOGRAMS

8.1 Figure 18 shows two samples of the type of recording made on the 16-mm film. The upper film is a recording of short-period data. The film speed is 30 mm/minute. Channel designations and normal magnifications are listed below.

Channel	Channel designation	Normal magnification at 1 second
1	WWV (R)	-
2	Short-period Vertical Z_1	400K
3	Short-period Vertical Z_2	400K
4	Short-period Vertical Z_3	400K
5	Short-period Vertical Z_4	400K
6	Short-period Vertical Z_5	400K
7	Short-period Vertical Z_6	400K
8	Short-period Vertical Z_7	400K
9	Short-period Vertical Z_8	400K
10	Short-period Vertical Z_9	400K
11	Short-period Vertical Z_{10}	400K
12	Short-period Summation S_4	400K
13	Short-period Summation S_{10}	400K
14	Short-period Horizontal N	400K
15	Short-period Horizontal E	400K
16	Short-period Vertical Z-LG*	10K

*Low-gain

The signal shown is from an earthquake occurring in the Kurile Islands. The numerals shown represent Greenwich time and are printed automatically on the film each minute. Each five minutes a group of symbols indicating the year, station identification, and day of year is printed on the film. Traces 12 and 13, which are the summation of four and ten seismometers, respectively, clearly show the cancellation of short-period microseisms by the array. Microseisms with a period of about six seconds are not cancelled by the array. The lower film is a recording of WWV, broad-band, long-period, intermediate-band,

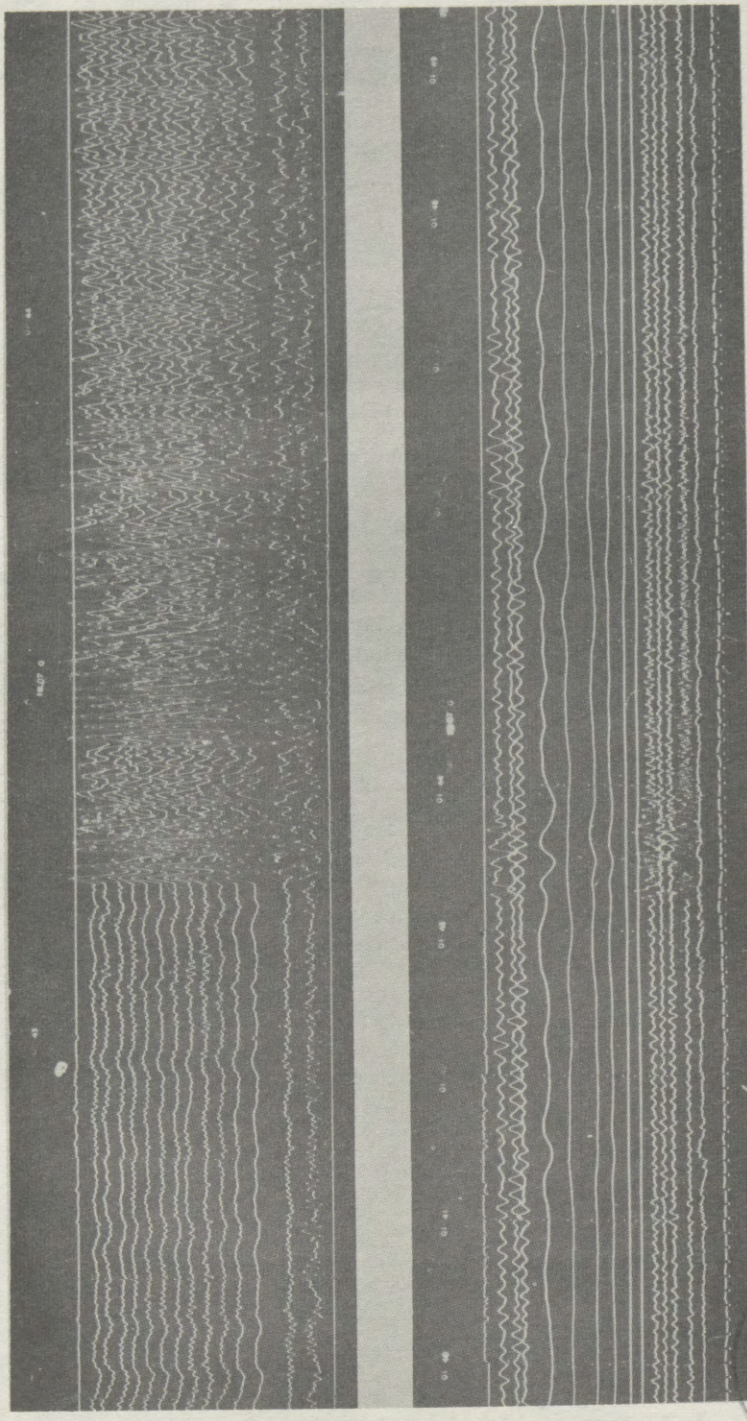


Figure 18. Enlargement of sections of 16-mm film records (X5)

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short-period summation, pressure change, and wind velocity. The film speed is 6 mm/minute. Channel designations and normal magnifications are listed below. Note that the 3-component long-period seismographs are also operated at one-fifth normal gain to avoid loss of information on large amplitude signals.

Channel	Channel designation	Normal channel magnification
1	WWV, R	-
2	Broad-band Vertical, BZ	4K at 1 cps
3	Broad-band Horizontal, BN	4K at 1 cps
4	Broad-band Horizontal, BE	4K at 1 cps
5	Long-period Vertical, LZ	3K at 25 cps
6	Long-period Horizontal, LN	3K at 25 cps
7	Long-period Horizontal, LE	3K at 25 cps
8	Long-period Vertical, LZ	0.6K at 25 cps
9	Long-period Horizontal, LN	0.6K at 25 cps
10	Long-period Horizontal, LE	0.6K at 25 cps
11	Intermediate-band Vertical, IZ	30K at 1.4 cps
12	Intermediate-band Horizontal, IN	30K at 1.4 cps
13	Intermediate-band Horizontal, IE	30K at 1.4 cps
14	Short-period Summation, S_{10}	400K at 1 cps
15	Microbarograph, M	-
16	Anemometer, A	-

8.2 Figure 19 shows the viewer that is used to view these films; the viewer is also shown in Figure 16. The film reels are placed on top of the viewer and a rapid film transport mechanism allows any point on a 150-foot roll of film to be located in about 30 seconds. A variable-speed slow transport is provided for scanning the film. Magnification is X20. The time scale on the screen is 10 millimeters per second for the short-period recordings and two millimeters per second for the long-period recordings.

8.3 By using this type of film-recording equipment, data from several seismometers can be recorded side by side and directly compared as to signal-to-noise ratio, phase shift, and amplitude response. For

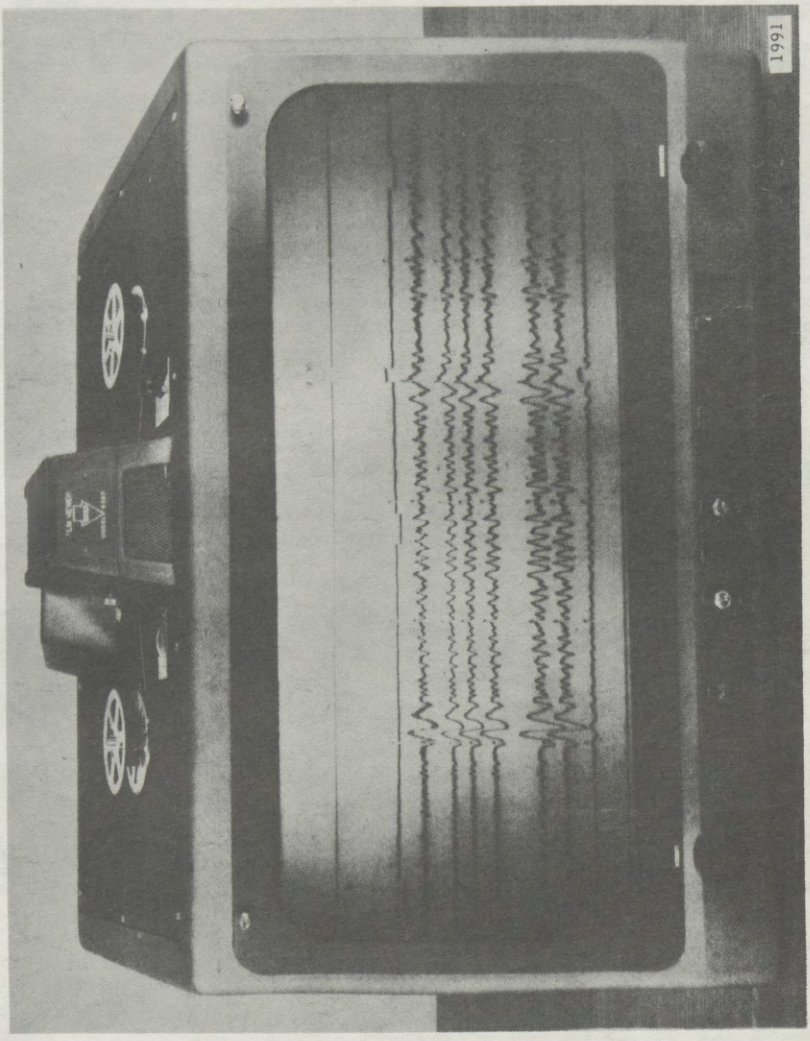


Figure 19. Viewer for use in analysis of 16-mm film records. Screen is 28" long by 12" high. Magnification is X20.

selected events, more detailed comparison will be made by analysis of magnetic-tape recordings; however, it is thought that much preliminary work can be done from the film records and that magnetic-tape records will be used only for detailed analysis of selected events.

9. CONCLUSIONS

The Wichita Mountains Seismological Observatory is well equipped and is located at a site with a low background noise. Other instrumentation characteristics can be obtained by using different plug-in filters in the phototube amplifiers and by changing the natural period and coupling of the seismometers and galvanometers. Continuous spectrum analysis of noise and signals and detailed analysis of significant seismic events recorded on magnetic tape should prove very informative. The station is located at a convenient distance from Californian and Mexican earthquakes and should provide a significant contribution to the science of detecting and identifying underground nuclear explosions and earthquakes.

APPENDIX I

Excerpt from page 16 of the Report of the Conference of Experts to Study the Methods of Detecting Violations of a Possible Agreement on the Suspension of Nuclear Tests

A. Seismic apparatus

The seismic apparatus of the control post should include:

(1) Approximately 10 short-period vertical seismographs dispersed over a distance of 1.5-3 kilometers and connected to the recording system by lines of cable. The seismographs should have a maximum magnification of the order of 10^6 at a frequency of 1 c.p.s. and a receiving band adequate to reproduce the characteristic form of the seismic signal;

(2) Two horizontal seismographs with the parameters indicated in point (1);

(3) One three-component installation of long-period seismographs having a broad receiving band and a constant magnification of the order of 10^3 - 2×10^3 in the period range 1-10 seconds;

(4) One three-component installation of seismographs with a narrow receiving band and magnification of the order of 3×10^4 when T = 2-2.5 seconds;

(5) At certain posts one three-component installation of long-period seismographs with magnification of the order of 10^4 - 2×10^4 at period of T = 25 seconds;

(6) Auxiliary equipment necessary in order to get precise records of the seismic signal; recording devices, chronometers, power supply units and apparatus for receiving automatic radio-signals giving correct time.

The seismic apparatus should be installed in places with a minimal level of micro-seismic background, away from industrial areas, and on outcrops of bedrock (where possible). The seismographs should be installed in suitable vaults.

The area required for installing the seismic apparatus should be about 3 x 3 kilometers.

Dr. ROMNEY. The two tasks which the Air Force Technical Applications Center has major responsibility for in the Vela Uniform program are the systems development task and the explosion program. And my talk at this point has to do with systems development.

The objective of our work at this time is to design a prototype detection system, which will be somewhat analogous, although on a smaller scale, to the system proposed by the Conference of Experts in 1958.

In this respect, the system will have major detection stations, some of which will be roughly limited to the equipment recommended by the experts in 1958, and others of which will be expanded beyond the experts' recommendations and will incorporate what we hope will be more advanced equipment.

In addition to these stations, we expect to have a facility which models in some ways the analysis center which would be necessary and is proposed to be located at Vienna under the current treaty. The work in establishing and evaluating the performance of this system would be supported by a developmental laboratory where new and improved equipment would be developed, and where communications procedures would be tested, and where identification criteria would be considered.

The work of the prototype system development would also be supported by some separate contract efforts, and these efforts would be carried to a suitable point in their development and then incorporated into the prototype system as improvements.

Now, at the present time, the system of stations which are being built consist of seven large stations, six of which are in the western part of the United States, and one station in the eastern part. This is shown on the second figure attached to the prepared statement which you have (p. 90).

These stations will be located on a grid which approximates the spacing recommended by the Conference of Experts for stations in a seismic area.

Five of the stations, then, are the Geneva-type stations, and two of the stations will be larger. And the two larger stations are presently planned to be in Arizona and in Tennessee.

The current status of the work is about as follows: We have one station which has been in operation since October of last year. This station was built and is operated by the Geotechnical Corp. We have certain preliminary results from the operation of this station.

For example, we have learned that the minimum signal detectable by this station at long range corresponds to about a magnitude 4 earthquake, and this in turn corresponds roughly to a 1-kiloton explosion in tuff

Chairman HOLIFIELD. For what range?

Dr. ROMNEY. These are ranges in excess of 2,000 miles, sir.

We have also learned, or confirmed would be a better term, that you must have a much larger signal than that from a magnitude 4 earthquake to attempt to identify the probable cause of the source.

Chairman HOLIFIELD. Much larger than what?

Dr. ROMNEY. Than magnitude 4, the minimum detectable signal.

Chairman HOLIFIELD. Is your point there that if you had a larger signal than 4.75, you would have a better chance of determining the difference between an earthquake and a nuclear explosion?

Dr. ROMNEY. Yes, sir. We would say, based on the evidence we have now, that stations with capabilities similar to that one now in operation could detect and locate most events equivalent to 1 kiloton in tuff; but that you would have to have explosions or earthquakes about 10 times as large before you could do any significant identification.

Now, by "10 times as large," I mean 10 times as much signal, which means about 30 times the yield. So identification would become significant at perhaps 20 to 30 kilotons.

Chairman HOLIFIELD. Now, let me try to interpret that.

Does this mean, then, that a 20,000-ton explosion, which theoretically would rest at 4.75, at a distance of 600 miles, would actually have to be 30 times that, or 600,000 tons, in order to give you the 4.75 recording?

Dr. ROMNEY. No, sir. The meaning is that while we could locate an event as small as about 1 kiloton, we could not identify an event until it approached magnitude 5, if we are confined to distances greater than 2,000 miles.

Chairman HOLIFIELD. Well, under the Geneva system, you would not be confined to that range, would you?

Dr. ROMNEY. When you extend these results to stations with the spacing recommended by the Geneva Conference, then the threshold would drop, and we would think that our previous estimates as to the identification capability at magnitude 4.75 are still approximately correct. So that I believe the experience in running this station about confirms the estimates given to the committee last spring.

We have also learned that at least one of the instruments recommended by the experts does not contribute very significantly to the mission of the station.

As far as the other stations——

Chairman HOLIFIELD. Have you learned that you can do with a smaller number of machines than you thought at one time—seismographic machines in the battery, or in the array?

Dr. ROMNEY. No, sir. We still feel that you need as many instruments as recommended by the experts, and probably to do the job that they suggested the system could do, you would actually have to have more.

Chairman HOLIFIELD. This is in effect a factor of unreliability in the individual machine, and you have to have, say, a hundred machines, in order to strike an average of dependability. Is that right?

Dr. ROMNEY. Well, the point is that with a single detector, you are more limited in the size of signal you can detect than you are if you have a multitude of detectors, and this is because the noise in the earth is partially canceled out by having a large number of detectors.

Chairman HOLIFIELD. How is that? I do not quite understand that.

Dr. ROMNEY. Well, a simple explanation would be that at the different sites where the detectors are installed, the noise may produce upward motions at one detector and downward motions at another detector at the same time. So if these outputs are added, they will partially cancel one another.

Now, we do not expect this to happen from earthquake or explosion signals, because the signals will arrive at the station more nearly simultaneously at all seismometers, and therefore the seismometers will emphasize the signals and deemphasize the noise.

Concerning the remaining stations, our sites have been selected, and at the present time a contract has been awarded for the construction and operation of these stations. The contractor will be Texas Instruments, in cooperation with the Geotechnical Corp.

The two larger stations have been designed, and we are currently working out the administrative details necessary to provide for the construction and operation of the stations. United Electrodynamics of Pasadena is the organization which has done the design work for us.

I might mention also some of the related work that we have done. We have completed a study, a theoretical study, on the specifications of optimum array procedures. This work was done by Texas Instruments, and they are currently beginning the practical engineering design of the system for the most effective use of an array.

And I would say the work to date is very encouraging. They have done theoretical work which indicates that on the basis of reasonable assumptions about noise and signal, you could expect an array of 30 seismometers to detect signals about seven and a half times smaller than could be detected by a single instrument, and an array of a hundred seismometers could in principle detect a signal 14 times smaller than could be detected by a single seismometer.

We of course feel that experimental verification is necessary.

We have initiated work on the unmanned seismograph station, which received considerable attention at the last hearings, and it is too early to say anything about results.

Since the hearing last year, we received the final report on our project on deep-well seismology, where we found that the theoretical predictions on the effectiveness of a seismometer operated in the deep well were approximately confirmed.

Having established that the method works in principle, we now believe that there is considerable work required to develop a very practical detection system, where instruments are designed which can operate for extended periods of time at depths of the order of 10,000 or more feet below the surface of the earth.

Work is also needed to determine which locations on the earth are most desirable from the standpoint of operating this kind of instrument.

Now, these array studies and deep-well studies that I have mentioned will be carried forward to the point where we believe we have a reasonably practical system, and at that time these devices will be incorporated into the prototype detection system which I described earlier.

I hope this summary has been adequate, and I believe many more details are contained in the paper which I have submitted to you.

I also have for your information a rather thorough description of the seismological station which has been constructed and is currently in operation. I would like to submit that for the record, also (p. 91).

Representative PRICE. Doctor, you have stated that an array of 100 seismometers would enable you in principle to detect a signal 14 times smaller than could be detected with a single seismometer. Does this mean that a violator would only be required to excavate a cavity large enough to provide a partial decoupling of a factor of 14 to cancel the advantage of a 100-element seismometer array?

Dr. ROMNEY. I think that is a very simple and clear statement of fact. Yes, sir. The array partially cancels the advantage of de-

coupling, but, of course, the factor of 14 improvement leaves a long way to go if we are faced with a factor of 300 in decoupling.

Representative PRICE. Would you say a factor of 300? Would you go as high as a factor of 300 for decoupling?

Dr. ROMNEY. For decoupling, yes, sir.

Chairman HOLIFIELD. Then how high would you have to go in numbers of seismographs to offset that?

Dr. ROMNEY. I do not think there is real hope that with any number you could achieve that.

Chairman HOLIFIELD. I was going to suggest that if it was a mathematical problem, here, it might be well to go into the seismometer business.

Dr. ROMNEY. Yes. I have given a little formula in my paper, where you can calculate the number, but I do not think it applies when you pursue it to this bitter end.

Representative PRICE. Now, at the hearing of this committee last year, it was reported that the array of seismometers of 30 to a hundred elements would be very expensive installations and would require such a large area of the geologically good seismometer emplacement that its application to the Geneva system might be very difficult and expensive. Have your studies of the array problem indicated any change in this situation, since it was presented to the committee last year?

Dr. ROMNEY. I think no significant change. The problem still exists that it would be very difficult to find a hundred quiet locations within a small area. And the predictions which I have given on the effectiveness of the array imply that there is a certain amount of coherence to the noise between the individual detectors.

Now, unless you could find a hundred sites where the noise was relatively coherent, you could not expect to do as well as my statement implied. I believe this is still a difficult problem, and a problem where we certainly do need to do experimental work, and there are plans to attempt to verify just how far you can push this technique by expanding to larger arrays.

So I would say our problem does not look any easier than it did last year, and there is still an experimental program required to determine how far we can go with the technique.

Representative PRICE. I understand that at the Lawton, Okla., station, they more or less extended the facilities already on that site that were operated by the Air Force. How expensive an operation is that?

Dr. ROMNEY. I did not hear your question.

Representative PRICE. How expensive an operation is the Lawton station?

Dr. ROMNEY. The annual operating budget is of the order of $200,000. This includes more than simple operations. It also includes a certain amount of work leading to the improvement of the equipment.

Representative PRICE. Do you plan to set up an actual hardware communication system between the Geneva and Berkner stations to report data into a central point on an actual prototype system basis?

Dr. ROMNEY. I think we would like to try that in at least one case to determine how effectively the signal can be transmitted over long

wire lines or radio links, and whether or not we would expand this to the entire network of stations is a question that we would answer after some experience.

Chairman HOLIFIELD. Dr. Romney, how much does the factor of 14 for a hundred-element array exceed the estimated gain from such an array a year ago?

Dr. ROMNEY. It depends on whose estimates you are talking about. It exceeds mine by a factor of 1.4. It falls short of some others by a factor of 6

Chairman HOLIFIELD. From the standpoint of its application to the system, and assuming that a deep-well seismometer installation would produce at least equal improvement to that from a hundred-element seismometer array, could you discuss the relative advantages or disadvantages of a deep-well installation in cost, with practical application to a Geneva control system and any other factors which you would care to mention?

Dr. ROMNEY. I would estimate that the cost of a deep-well installation could in some cases equal the cost of a large array, that is, an array of the order of 30 detectors, and perhaps in bad cases an array of the order of a hundred detectors. However, I would expect in general the deep-well technique would be slightly cheaper.

As far as practical application is concerned, I believe the deep-well method, if it proves to be as good as we expect, could be applied in many cases where you could not use an array. I am thinking in particular of islands, where the international control system has proposed there be stations, and on some of the islands that we have talked about, there simply would not be room for a seismometer array.

Chairman HOLIFIELD. How large is one seismometer?

The reason I ask this is: I am thinking about the drilling of a well. How large a hole would the well have to be in order to drop a seismometer or lower a seismometer to the bottm of it?

Dr. ROMNEY. We are designing our instruments to go down a 5-inch hole.

Chairman HOLIFIELD. In other words, you can lengthen the instrument out?

Dr. ROMNEY. Yes, sir. This would probably be an instrument 5 inches in diameter and 30 feet long.

Chairman HOLIFIELD. How deep would you classify a "deep well"? What does that mean in terms of thousands of feet?

Dr. ROMNEY. That means 10,000 feet or more.

Chairman HOLIFIELD. Would it be possible for you to utilize abandoned oil wells in many areas of the Nation that go to that depth?

Dr. ROMNEY. This is a problem that we are looking into. I think in some cases it would be possible. However, the difficulty is that at the present time we suspect that casing in a well causes trouble under certain circumstances—introduces noise. Also uncased wells ordinarily do not stand open very long; so we need to understand this casing problem a little more fully. But it is possible, for instances, that we might find a well cased part way down, where we could use that much of the well and drill beyond the end of the casing. So to that extent I would think we could probably count on using certain wells.

Chairman HOLIFIELD. Most of the oil wells run their casing down to some water strata in drilling through the cement. Of course, in

many cases they install a casing below that particular shutoff, water shutoff.

Are there any further questions?

Mr. Ramey?

Mr. RAMEY. Do you consider this deep-well development work, which I gather was going on before under the Defense Department, to be the most significant improvement potentially in your detection schemes?

Dr. ROMNEY. There is a theoretical possibility, as yet unverified, that this technique could provide a greater improvement in detection capability than any other technique that I know of.

Mr. RAMEY. Is this work being funded adequately, or are you being held up by lack of funds or facilities?

Dr. ROMNEY. I cannot quite answer that, because AFTAC only has part of the responsibility for the program. And our own work is going satisfactorily at the present time.

I do recommend, however, an expansion in certain areas, particularly the investigation of this technique and how it might apply to shallower wells. I believe there are possibilities that you could make a rather significant improvement by not going very deep, but not the kind of improvement that I have talked about. But the lesser improvement could have application to array problems, for example.

Chairman HOLIFIELD. Thank you very much, Dr. Romney.

Our next witness is Mr. Theodore George, of the Advanced Projects Research Agency, who will testify on the problems connected with underground nuclear-chemical explosions.

Mr. George, has your statement been distributed to the press?

Mr. GEORGE. Yes it has, sir.

Chairman HOLIFIELD. Did you distribute it?

Mr. GEORGE. No I did not distribute it personally, but I assumed it was delivered.

Chairman HOLIFIELD. You gave it to the staff? I understand from the staff that they have not distributed it.

All right. You may proceed. I notice it is a short statement. You go ahead.

STATEMENT OF THEODORE A. GEORGE,[1] VELA UNIFORM PROGRAM MANAGER, ADVANCED RESEARCH PROJECTS AGENCY

Mr. GEORGE. Mr. Chairman and members of the Joint Committee, the determination of whether it is possible to really develop an effective system for detection and identification of an underground nuclear explosion, must in the final analysis rest on a clear understanding of the characteristics of detectable signals generated by such a disturbance. No amount of theoretical research will ever completely replace the need for information which is obtainable from a carefully

[1] Born in Detroit, Mich., on Nov. 1, 1926. Employed as an aeronautical engineer in the Office of the Chief of Staff, Department of the Army 1953–58. Since then, held position as general engineer in the Advanced Research Projects Agency, first as program manager for Project Discoverer (1959–60) and since then associated with the research effort for detection of nuclear explosions. From 1946 to 1953 studied mathematics at the undergraduate and graduate levels at George Washington University; and then from 1957 to 1958 mechanical engineering (BME) and from 1959 to 1961 astronautical engineering (MAE) at the Catholic University of America. Member of the American Mathematical Association, Institute of Aeronautical Sciences, Reserve Officers Association and CU Chapter of Tau Beta Pi.

planned series of underground explosions. This principle has been recognized from the time that Project Vela was first conceived. In the original report of the Panel on Seismic Improvement, or Berkner Panel as it is frequently referred to, an entire section was devoted to the need for underground nuclear and chemical explosions. Again last year, Dr. Richard Latter testified before this committee in considerable detail as to the technical reasons for requiring a series of underground nuclear experiments. In view of these and other papers which have been widely circulated, I feel that the need for a series of nuclear/chemical explosions is now well recognized. Therefore, with the permission of the chairman, I propose to omit a repetition of the explosion program justification, and instead concentrate on progress which has been made in this area since the last hearings. I would also like to briefly summarize our present concept of the Vela underground explosion program.

On the basis of theoretical considerations and what little experimental data is available, we feel that the physical conditions of an underground explosion are likely to have a profound influence on all signals emitted. Conditions most likely to affect the signals are depth of burst, the medium in which it is fired, yield of the explosion, precise coupling conditions and type of explosive used, that is HE or nuclear. There is reason to believe that there may be ways to artifically reduce the intensity of the seismic signal produced by an underground explosion. One such method studied extensively from a theoretical standpoint is the so-called decoupling procedure. This calls for firing the nuclear device inside a cavity of sufficient size and depth to insure that the stresses on the walls do not exceed the elastic limit. Theoretical analysis and some small scale testing with the use of HE indicate that this procedure could reduce the intensity of the seismic signal by a factor as high as 300; that is a 300 kiloton nuclear device fired under the appropriate conditions might appear no larger than a 1 kiloton explosion fired under a different set of conditions.

In order to obtain the necessary experimental data, the Department of Defense, together with the Atomic Energy Commission and with the continuous advice of a group of prominent scientists headed by Dr. Richard Latter, has developed a nuclear and chemical underground explosion program which you see outlined on this cnart.

(The chart referred to follows:)

VELA UNIFORM

THE DOD/AEC NUCLEAR-CHEMICAL EXPLOSION PROGRAM

NO. OF EVENTS	NAME	TYPE	YIELD	MEDIUM	DEPTH (FT)	SITE
SHADE						
1	ORCHID	NUCLEAR	5 KT	TUFF	1350	NTS
1	CRYSTAL		1 KT			
1	LOLLIPOP		5 KT	GRANITE	1500	
1	LINEN	HE		TUFF	1250	
1	STINGRAY	NUCLEAR	25-50 KT		2000+	
1	MUSLIN		¼ KT		1350	
1	SHOAL		5 KT	GRANITE	—	—
1	PORPOISE			GRANITE	7500	NTS
DRIBBLE						
1	RECORD	NUCLEAR	100 T	SALT	2500	TATUM DOME
1	HAYRIDE		500T			
1	HERMIT		100T			
1	GAUCHO		5 KT			
1	GREENBEAN		25 KT			
1	TIPSY		5 KT			
GROUNDHOG						
1	GH I	HE	100T	SALT	2500	TATUM DOME
1	GH II		30 T	HARD ROCK	240	—
1	GH III				750	
1	GH IV				3000	
3	GH V-VII		50-100T	—	350	CALIFORNIA
3	GH VIII-X				350	NEVADA
11	GH XI - XXI		½ T	VARIOUS	110-1100	—

Mr. GEORGE. During its formulation, the program was reviewed by several competent scientific groups. In its final version the program was approved by the Executive Office of the President, the Secretary of Defense and other key officials of the Government. The number of explosions in this program has been held to a minimum because of budgetary and policy considerations. Consequently, there is no redundancy in the program; each explosion shown on the chart has a specific purpose. However, as theoretical data becomes available, we are constantly restudying the program to see whether any changes may be desirable. Furthermore, results to be obtained from

the first few experiments will undoubtedly indicate changes required in subsequent tests, in order to obtain better and more accurate information. Although the program at present calls for no repetition of any experiment, it is possible that an inadequate explosion yield could invalidate the results of some event, and thereby require an additional shot.

With your permission, I would like to discuss briefly each line item on the chart and say a few words relating to the background and justification for including that event in the program. Before I start let me state that all these experiments will be carefully instrumented with the latest available sensory equipment. The instrumentation phase of the program can generally be divided into two categories. The first consists of the so-called close-in measurements and involves a wide variety of sensory equipment all of which is emplaced within a radius of about 10 miles from ground zero. Colonel Ledford will cover this phase in more detail. The intermediate-range and long-range or teleseismic measurement program extends many hundreds of miles from the site as will be discussed later by Dr. Carl Romney.

The first item on the chart is Orchid. This is more or less a pivot of the program and should furnish a considerable amount of basic information. It will serve three specific and important purposes. First, it will provide a comparison between the signal produced in a soft medium, such as tuff, and the signal from the same size explosion fired in a hard rock. Secondly, Orchid is the first in a series of events to determine the "scaling effect," that is the variation in amplitude and frequency of seismic waves as a function of explosive yield. Thirdly, this event will be used to make a direct comparison between a nuclear explosion and a chemical explosion of the same energy level fired in the same medium.

Crystal is the second "scaling effect" shot. It is a 1-kiloton experiment to be fired at the same depth and in the same environment as Orchid.

The third line defines the Lollipop experiment. It is also a 5-kiloton explosion to be fired in granite, a hard rock with high seismic velocity characteristics. The results from Lollipop will be compared with those from Orchid. Furthermore, granite has been selected as the medium for determination of depth effect on the signals produced by an explosion. Consequently, Lollipop is also the first in a two shot series to investigate depth effect. The second event in this series is Porpoise which I will discuss in a few minutes.

Linen is a 5-kiloton chemical explosion to be fired in tuff at approximately the same depth as Orchid. This experiment will be compared with Orchid, which is also a 5-kiloton nuclear shot, in order to provide a direct measure of chemical-nuclear differences in seismic wave generation. It is conceivable that when the differences between the disturbance created by a chemical explosion and a nuclear explosion are clearly established and understood, it may be possible in some instances to substitute HE for nuclear devices in some of the subsequent tests without loss of desired information should this become necessary because of political considerations.

At this point it may be appropriate to introduce the problem of scheduling these various experiments. As you know technical preparations have been in progress for some time to fire various of the

events which you see on the chart. However, at this time for reasons which you are all aware of no decision has been made to authorize any of the nuclear experiments. In the case of Orchid all preliminary preparations have been completed and AEC and DOD are prepared to undertake this event promptly when and if the international political circumstances warrant such a decision. Crystal could then be fired 30 days after Orchid. In the case of Linen, the major HE event, our preparations will be completed for a firing in the spring of next year.

The next event is Stingray. This is intended to be the largest nuclear explosion in the series. The yield has not been established precisely as yet but will be at least 25 kilotons and possibly as high as 50 kilotons. The depth will be determined after the yield is set in order to insure complete containment. This is the only event which can be expected to provide quantitative data on the amplitude and detectability of first motion at distances greater than 1,000 kilometers.

Muslin is intended to furnish data on our ability to detect very small yield underground explosions. Consequently, Muslin should provide the lower boundary of the scaling effect curve. However, if results from Crystal indicate that effective detection of fractional kiloton explosions is not feasible, it may be decided to cancel the Muslin experiment.

Next is Shoal, a 5-kiloton nuclear explosion scheduled to be fired in an active seismic area. Its purpose is to investigate the effect of the path of propagation on the recorded seismic waveforms. The experiment should also provide direct comparison with seismic disturbances created by an earthquake. Several sites for this event are currently being studied. One of the most promising at present appears to be in the southwestern part of Nevada.

Porpoise is a 5-kiloton nuclear shot in granite to be fired at a depth of 7,500 feet at the Nevada Test Site. The experiment will furnish detailed information on the influence of depth on the amplitude spectrum and waveform characteristics of seismic waves. Theoretical arguments have been advanced which indicate that amplitude of first motion, which may be one of the principal means of distinguishing between earthquakes and explosions, could be strongly influenced by depth. It is also believed that the depth will influence the intensity and characteristics of surface waves.

Now I would like to discuss briefly six explosions which are scheduled to be fired in a salt dome. These tests will furnish us with information on the effectiveness of concealment of an underground explosion by decoupling. All of these tests will be conducted at a depth of 2,500 feet.

First we have three small underground nuclear explosions. Record is to be fired in the center of an underground cavity which will provide for complete decoupling. It will follow by Hayride having a yield five times as much as Record but fired in the same cavity. This will provide data on the effects of partial decoupling. Both of these shots will then be compared with Hermit with the same yield as Record, but fully tamped. These three events are expected to take place in the Tatum Salt Dome in Mississippi.

The next three events are also intended to furnish data on decoupling but at much higher yield level. Here we have Gaucho a 5-kilo-

ton event completely decoupled; Greenbean a 25-kiloton event partially decoupled; and Tipsy the comparison shot of 5 kilotons, tamped.

On the lower part of the chart you see a series of 21 HE underground explosions referred to as Project Groundhog. These are relatively small explosions from one-half to 100 tons of HE. They are to be fired in various media and at various depths. As you see on the chart, the depths are to vary from 110 to 2,500 feet. The Groundhog experiments are expected to furnish a large amount of data which will be processed and analyzed in order to supplement the information gathered from the large yield explosions which I have already discussed. Essentially Project Groundhog has six objectives. These are to compare coupling in various media; determine the effects of depth in a single medium; compare seismic signals from explosions with those from earthquakes; determine the spectrum and amplitude of first motion at various distances; determine the effectiveness of epicenter location techniques; and confirm the nuclear-chemical equivalance at various yield levels. The possibility of concealing one of the Groundhog shots, that is removing traces of human activity on the surface near ground zero, is also under consideration. Such a project would provide a good opportunity for testing the effectiveness of our onsite inspection techniques. The onsite inspection team would be required not only to locate the epicenter of the disturbance but also prove that it was of chemical rather than nuclear origin. It is our intent to fire the first Groundhog explosions before the end of this year. I think that the Groundhog series is indicative of the firm intention of the U.S. Government to use chemical explosives to the maximum extent practicable in collecting empirical data for Project Vela Uniform.

In conclusion let me state that there can be no doubt that the explosion series is a vital part of the Vela Uniform program. Theoretical considerations alone will never be able to confirm that an effective detection capability has in fact been achieved. Furthermore, there is a strong possibility that these tests will provide an indication of some new way of identifying an underground nuclear explosion which may have been overlooked in the course of theoretical analysis.

Chairman HOLIFIELD. Thank you, Mr. George, for your statement.

The Chair would like at this time to point out and emphasize, in order that there can be no misunderstanding, that the series of high explosive shots are permitted at this time under our international agreement. And of course, we are under the moratorium on the nuclear experiments. And, as you have stated in here, no decision has been made to authorize any other nuclear experiments at this time. You also say that you are prepared to proceed with the tests when and if the international political circumstances warrant such a decision.

The Chair calls attention to these facts in order that there will be no mistaken newspaper headlines in regard to the nature of this test work today.

Representative PRICE. I think you should make another point, that none of these shots are development shots.

Chairman HOLIFIELD. That is right. All of these tests would be for the purpose of improving seismological instruments in case they are authorized.

Are there any questions of Mr. George?

If not, we thank you, sir, for your testimony, and you are excused at this time.

(Mr. George's formal statement follows:)

STATEMENT BY MR. THEODORE A. GEORGE, VELA UNIFORM PROGRAM MANAGER, ADVANCED RESEARCH PROJECTS AGENCY

Mr. Chairman and members of the Joint Committee, the determination of whether it is possible to really develop an effective system for detection and identification of an underground nuclear explosion, must in the final analysis rest on a clear understanding of the characteristics of detectable signals generated by such a disturbance. No amount of theoretical research will ever completely replace the need for information which is obtainable from a carefully planned series of underground explosions. This principle has been recognized from the time that Project Vela was first conceived. In the original report of the Panel on Seismic Improvement, or Berkner panel as it is frequently referred to, an entire section was devoted to the need for underground nuclear and chemical explosions. Again last year, Dr. Richard Latter testified before this committee in considerable detail as to the technical reasons for requiring a series of underground nuclear experiments. In view of these and other papers which have been widely circulated, I feel that the need for a series of nuclear/chemical explosions is now well recognized. Therefore, with the permission of the chairman, I propose to omit a repetition of the explosion program justification, and instead concentrate on progress which has been made in this area since the last hearings. I would also like to briefly summarize our present concept of the Vela underground explosion program.

On the basis of theoretical considerations and what little experimental data is available, we feel that the physical conditions of an underground explosion are likely to have a profound influence on all signals emitted. Conditions most likely to affect the signals are depth of burst, the medium in which it is fired, yield of the explosion, precise coupling conditions, and type of explosive used, that is, HE or nuclear. There is reason to believe that there may be ways to artificially reduce the intensity of the seismic signal produced by an underground explosion. One such method studied extensively from a theoretical standpoint is the so-called decoupling procedure. This calls for firing the nuclear device inside a cavity of sufficient size and depth to insure that the stresses on the walls do not exceed the elastic limit. Theoretical analysis and some small scale testing with the use of HE indicate that this procedure could reduce the intensity of the seismic signal by a factor as high as 300; that is a 300-kiloton nuclear device fired under the appropriate conditions might appear no larger than a 1-kiloton explosion fired under a different set of conditions.

In order to obtain the necessary experimental data, the Department of Defense, together with the Atomic Energy Commission and with the continuous advice of a group of prominent scientists headed by Dr. Richard Latter, has developed a nuclear and chemical underground explosion program which you see outlined on this chart (p. 130). During its formulation, the program was reviewed by several competent scientific groups. In its final version the program was approved by the Executive Office of the President, the Secretary of Defense, and other key officials of the Government. The number of explosions in this program has been held to a minimum because of budgetary and policy considerations. Consequently, there is no redundancy in the program; each explosion shown on the chart has a specific purpose. However, as theoretical data becomes available, we are constantly restudying the program to see whether any changes may be desirable. Furthermore, results to be obtained from the first few experiments will undoubtedly indicate changes required in subsequent tests, in order to obtain better and more accurate information. Although the program at present calls for no repetition of any experiment, it is possible that an inadequate explosion yield could invalidate the results of some event, and thereby require an additional shot.

With your permission, I would like to discuss briefly each line item on the chart and say a few words relating the background and justification for including that event in the program. Before I start let me state that all these experiments will be carefully instrumented with the latest available sensory equipment. The instrumentation phase of the program can generally be divided into two categories. The first consists of the so-called close-in measurements and involves a

wide variety of sensory equipment all of which is emplaced within a radius of about 10 miles from ground zero. Colonel Ledford will cover this phase in more detail. The intermediate-range and long-range or teleseismic measurement program extends many hundreds of miles from the site as will be discussed later by Dr. Carl Romney.

The first item on the chart is Orchid. This is more or less a pivot of the program and should furnish a considerable amount of basic information. It will serve three specific and important purposes. First, it will provide a comparison between the signal produced in a soft medium, such as tuff, and the signal from the same size explosion fired in a hard rock. Secondly, Orchid is the first in a series of events to determine the "scaling effect," that is the variation in amplitude and frequency of seismic waves as a function of explosive yield. Thirdly, this event will be used to make a direct comparison between a nuclear explosion and a chemical explosion of the same energy level fired in the same medium.

Cyrstal is the second "scaling effect" shot. It is a 1-kiloton experiment to be fired at the same depth and in the same environment as Orchid.

The third line defines the Lollipop experiment. It is also a 5-kiloton explosion to be fired in granite, a hard rock with high seismic velocity characteristics. The results from Lollipop will be compared with those from Orchid. Furthermore, granite has been selected as the medium for determination of depth effect on the signals produced by an explosion. Consequently, Lollipop is also the first in a two-shot series to investigate depth effect. The second event in this series is Porpoise which I will discuss in a few minutes.

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of distinguishing between earthquakes and explosions, could be strongly influenced by depth. It is also believed that the depth will influence the intensity and characteristics of surface waves.

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The next three events are also intended to furnish data on decoupling but at a much higher yield level. Here we have Gaucho a 5-kiloton event completely decoupled; Greenbean a 25-kiloton event partially decoupled; and Tipsy the comparison shot of 5-kiloton tamped.

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In conclusion let me state that there can be no doubt that the explosion series is a vital part of the Vela Uniform program. Theoretical considerations alone will never be able to confirm that an effective detection capability has in fact been achieved. Furthermore, there is a strong possibility that these tests will provide an indication of some new way of identifying an underground nuclear explosion which may have been overlooked in the course of theoretical analysis.

Chairman HOLIFIELD. Due to prior commitments, this room will not be available for tomorrow's session. We will convene at 10 a.m., in room 362 of the Old House Office Building. Room 362 is known as the caucus room over there.

We plan to complete the testimony on the detection of underground tests, and to conduct a roundtable discussion on the results of seismic research. Panel members will be: Dr. Dean S. Carder, Chief Seismologist, U.S. Coast and Geodetic Survey; Dr. Norman Haskell, Air Force Cambridge Research Laboratory; Dr. Jack Oliver, Columbia University, Lamont Geological Observatory; Dr. Carl Romney, Air Force Technical Applications Center; and Dr. James T. Wilson, chairman, Department of Geology, University of Michigan.

We will convene in room 362 of the Old House Office Building at 10 a.m. tomorrow.

The committee stands adjourned.

(Whereupon, at 5:08 p.m., Tuesday, July 25, 1961, the committee was adjourned, to reconvene at 10 a.m., Wednesday, July 26, 1961, in room 362, Old House Office Building.)

DEVELOPMENTS IN THE FIELD OF DETECTION AND IDENTIFICATION OF NUCLEAR EXPLOSIONS (PROJECT VELA) AND RELATIONSHIP TO TEST BAN NEGOTIATIONS

WEDNESDAY, JULY 26, 1961

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The Joint Committee met at 10 a.m., pursuant to notice, in room 362, Old House Office Building, Hon. Chet Holifield (chairman of the Joint Committee) presiding.

Present: Representatives Holifield, Price, Aspinall, Bates and Westland.

Also present: James T. Ramey, executive director; John T. Conway, assistant director; George F. Murphy, Jr., professional staff member; Richard T. Lunger, staff consultant; and Doyle Northrup, technical consultant, Joint Committee on Atomic Energy.

Chairman HOLIFIELD. The committee will be in order.

Our last witness in yesterday afternoon's session led off the testimony on the program of underground nuclear and chemical explosions for the Vela Project.

This morning, we continue with testimony on the AEC and Defense Atomic Support Agency programs in support of Project Vela. We will also consider the seismic measurements and analysis requirements under the explosion program.

In the afternoon session, testimony on onsite inspection will be presented.

Following this, a roundtable discussion of the results of seismic research to date will be conducted by a panel of seismologists.

We will then receive testimony on that portion of the Vela Project designated "Vela Sierra"—the detection of space tests from ground bases.

Our first witness this morning is Brig. Gen. A. W. Betts, Director of Military Applications, AEC, who will testify on the AEC program in support of Project Vela underground explosion requirements.

General Betts, you may proceed.

STATEMENT OF BRIG. GEN. A. W. BETTS,[1] DIRECTOR, DIVISION OF MILITARY APPLICATIONS, USAEC

General BETTS. Good morning, sir.

I would remind you that the primary objective of the Vela Uniform explosion series, stated previously by Mr. George, is to obtain data needed for the evaluation of the proposed Geneva system and for

[1] See biographical sketch, p. 13.

the development of techniques for improving the capability of detecting and identifying underground nuclear explosions. Signals from detonations are required in order to permit such an evaluation of present and possible future instrumentation. The Atomic Energy Commission has the responsibility for providing the sites, the devices, and actually accomplishing the firing of all nuclear detonations of the Vela Uniform program. In addition, it has responsibility for firing one very large detonation of high explosive which is to be accomplished within the Nevada Test Site. Also, the AEC has responsibility for accomplishing such close-in instrumentation as may be necessary for determining that the firing yield was as forecast, and for certain detonations, determining the nature of the transition of the signal from the cavity of the detonation to the immediate surrounding environment.

Although no nuclear detonations have been approved, the program as presently recommended as necessary to satisfy technical requirements would consist of both nuclear and high explosive shots, and may be broken down into three categories based upon the medium in which the detonations are planned.

For your information, I have retained Mr. George's chart, so that you can refer to the particular shots as we go through them.

These several categories of shots are:

First, three or four nuclear shots, and one chemical high explosive shot in a soft rock, which we refer to as tuff.

The second group is that of three nuclear shots in hard rock, including one shot in an active seismic area.

And the third is a group of six nuclear shots in a salt medium. And these are in connection with the decoupling experiment.

Now, reviewing first the events in tuff: The events to be detonated in tuff at the Nevada Test Site would be at a depth of about 1,350 feet, except for Stingray, which would be at a greater depth. Now, these depths were previously given by Mr. George, and are on the chart.

The Orchid shot, which would be very highly instrumented, would be the initial shot of the Vela series, as now planned. Construction for this shot, to be detonated in a tunnel under the Rainier Mesa at Nevada Test Site with about 1,350 feet of overburden, is virtually complete.

The Crystal event would follow Orchid and would be detonated in a drift off the same basic tunnel under the Rainier Mesa, also at a depth of about 1,350 feet. Basic construction is completed and further preparations are dependent upon a firm go-ahead decision.

Linen is the 5-kiloton HE event planned at the Nevada Test Site. This shot requires 8,700,000 pounds of composition B explosive to produce the equivalent of 5 kilotons of TNT energy release. The high explosive, packed in individual 47-pound containers, requires a cavity nearly spherical in shape approximately 60 feet in diameter. Ground zero for the Linen shot would be about 1,250 feet below the surface. The basic entry to the shot area is a tunnel into the Rainier Mesa at Nevada Test Site. Near the end of a drift from the main tunnel, a shaft was constructed down to a point some 600 feet below the tunnel floor. Due to water bearing strata, it will be necessary to move the shot location up to a point approximately 500 feet below the tunnel

floor. This decision requires backfilling about 100 feet of the shaft in a particular manner so that it will have characteristics similar to the surrounding rock. Construction of the cavity has been delayed due to the backfilling required to solve the problem of water seepage. Due to the physical characteristics of this rock, our engineering experts have informed us that an unsupported cavity may slough or possibly collapse if left for an extended time period. Thus, when the cavity is constructed, high explosive loading instrumentation, arming and stemming (all of which requires about 7 months) should follow and final preparations should be completed to detonate this event. Once the high explosive is loaded in the cavity, it should be detonated within a reasonable time thereafter, since its presence would create a safety hazard for activities in the rest of the tunnel complex.

Stingray exploratory work has been completed. Like Linen, Stingray would be located a few hundred feet below the floor of a tunnel into the Rainier Mesa. The decision as to the precise yield will determine its depth between 1,600 and 2,000 feet below the surface.

As explained by Mr. George, Muslin is a contingency event. Presently there is no activity with regard to Muslin; however, a tentative location has been selected within the Nevada Test Site tunnel complex.

Now, moving to the events in hard rock: Extreme difficulty has been encountered in locating a site meeting the technical criteria for Lollipop, a 5-kiloton event in granite at a depth of 1,500 feet scheduled for Nevada Test Site. Broken and badly altered rock was encountered in a number of exploratory drill holes at depths of about 1,300 to 1,500 feet; however, promise of a satisfactory environment is indicated as a result of good granite to a depth of 1,800 feet which was found in one of the exploratory drill holes. Exploratory drilling will continue in an effort to define a suitable granite formation.

Porpoise, you will recall, is a deep shot at about 7,500 feet. It should be fired, if possible, in the same granite mass as the Lollipop shot; hence, exploratory work will be withheld until the Lollipop event has been firmly located. For this reason, exploratory work has been withheld on that basis. The badly crushed rock encountered in Lollipop exploratory drilling is not anticipated at the lower depths in the same mass.

Several areas are being considered as possible locations for Shoal, the 5-kiloton nuclear event at 1,500 foot depth in an active seismic area. Locations in several States are being considered. Once a site is tentatively chosen, exploratory work would be required to define the surface and subsurface geology and to make hydrologic investigations.

Now to move to the events in the salt domes: These are the six nuclear events desired for detonation in salt media and are all at a depth of 2,500 feet. We would expect to conduct them in a single salt dome.

After investigating and considering a large number of salt domes, the Tatum Dome, 33 miles southwest of Hattiesburg, Miss., and Bruinsburg, which is 32 miles south of Vicksburg in Mississippi, appear to most nearly meet the technical criteria we desired.

Although the Tatum Dome seems to be the more desirable of the two, final selection will depend on the results of exploratory drilling presently being conducted at both sites. The Tatum Dome has been

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adjudged technically acceptable for the conduct of phase I of the Driblet series. Those are the three smaller shots.

Further exploratory work now underway should indicate whether the Tatum Dome is technically acceptable for the conduct of the larger series, which will constitute phase II.

Excavating the cavities for the decoupled shots represents a rather extensive construction project.

Chairman HOLIFIELD. Let me ask you at that point: Were the salt domes in the Hutchinson, Kans., area investigated?

General BETTS. I cannot specifically speak to those salt domes, Mr. Chairman, but I do know that we went through a preliminary sort of map survey of all of the domes which we knew of in all the United States. And just what considerations went into the specific selection of the Mississippi area was before my time.

Chairman HOLIFIELD. Some of the members visited these salt domes in the vicinity of Hutchinson, Kans., and there are millions of feet of space in those tunnels. Some of them go as low as a thousand feet underground. The excavations are already made. These main tunnels and the laterals run for what seems like to me almost miles underground.

General BETTS. Well, I think this would be detrimental to the experiment we want to conclude. We would like an absolutely homogeneously solid dome that had not been coneycombed by any tunnels, so that there would be no anomalies introduced to the transmission of the signal by the presence of those tunnels.

Chairman HOLIFIELD. I see.

General BETTS. This would be against the selection of the site, rather than for it.

Chairman HOLIFIELD. For seismological purposes?

General BETTS. In order to get the signal tranmission——

Chairman HOLIFIELD. But if you wanted to have webbing tests later on——

General BETTS. This would be another proposition.

Chairman HOLIFIELD. Would there be any reason why parts of these lateral tunnels could not be cemented off with cement walls, and the ends of the tunnels used? I am thinking now from the standpoint of expense.

General BETTS. Just as we have used a tunnel complex in Nevada, at least in theory it could be done. There might be practical political problems that we would have to investigate more thoroughly.

Chairman HOLIFIELD. I was speaking from a practical standpoint. You say the presence of other tunnels is detrimental to your seismological recordings. How about your complex of tunnels in Nevada?

General BETTS. If we could have it our way, what we would like is a completely homogeneous medium with nothing in there but the one event. But remember: The Nevada tests—they are tamped events in the first place. I think we know something about the transmission of signals from that area. We would just as soon not have a lot of other tunnels there, but this is a fact of life that relates to the cost of doing it in some other way.

Chairman HOLIFIELD. Proceed.

General BETTS. Excavating the cavities for the decoupled shots represents an extensive construction project. In the case of a fully

decoupled event in phase I for the small-yield shots, a spherical cavity about 92 feet in diameter is required, while the larger decoupled events in phase II requires a spherical cavity of about 332 feet in diameter, which, parenthetically, is about the length of a football field.

A manmade construction of unsupported spherical cavities of this size and at this depth has never before been attempted in the United States, to my knowledge. In all, about 750,000 cubic yards of rock stone, about 1¼ million tons, must be removed. Both conventional mining and solution mining techniques are being considered.

Currently, studies relative to the salt disposal problem are being made by the U.S. Bureau of Mines and the Department of the Interior, and the Industrial Division, Business and Defense Services Administration, Department of Commerce. Cost of construction for this portion of the program would be several millions of dollars, and may vary considerably from any estimate due to the unique construction required and the magnitude of the mining operation, with which we have very little prior experience.

That completes my statement, Mr. Chairman.

Chairman HOLIFIELD. You stated that your engineers have informed you that an unsupported cavity in tuff may slough or possibly collapse if left for an extended period of time. Does this mean that a cavity several hundred feet in diameter in the Nevada tuff, if unsupported, would cave in?

General BETTS. I would have serious misgivings about whether we would even be able to create a cavity of that size in the tuff in Nevada.

Chairman HOLIFIELD. What is the consistency of this tuff formation? Is it a granite or mixed granite?

General BETTS. No; one might say a very light sandstone. It is a cemented deposit of lava outflow. At least, that is my understanding of it. And it tends to be powdery once it is crushed.

Chairman HOLIFIELD. Would this mean, then, that you would have to, in the case of constructing any hole that was several hundred feet in diameter, support it with some——

General BETTS. If we tried to make a cavity of that sort in the Nevada tuff, I think we would have to do a job of supporting the roof, as I understand the problem in that medium. This does not apply to the salt domes. I think we have a very different situation there.

Chairman HOLIFIELD. As a matter of fact, in these salt domes in Kansas, I noted when I was going through them that there was rock formation. You could see where the salt ended and the rock formation began in instances.

General BETTS. Well, in order, for experimental reasons, to put these events in a homogeneous medium, which will be our effort in the Tatum Salt Dome, we must put these wells into the dome itself, to determine that there is a homogeneous medium of the size necessary for the experiment.

Chairman HOLIFIELD. It is necessary that it have supports, but would it interfere with the principle of the decoupling?

General BETTS. I think it would introduce anomalies that would be somewhat confusing in terms of our experimental results.

Chairman HOLIFIELD. Now, you talked about a detonation of 5,000 tons of composition B explosives. I understand you have had trouble with the 500-ton tests in Nevada.

General BETTS. We did. I do not anticipate any of the same kind of troubles, since the 500-ton event problem could be traced to human error in the selection of detonators that were to be used. In this case, we will make an allout effort to use the proper detonators.

Chairman HOLIFIELD. Now, what is the extent of the damage from an underground shot? What is the nature of the reaction of the environment? The high explosive, I am speaking of.

General BETTS. It is a shock transmission through the ground that actually creates a minor compression and rarefaction of the ground as a medium. And this will vary in its magnitude in direct relationship to its distance from the source of the signal.

Chairman HOLIFIELD. Do the walls and ceilings collapse as a result of that shock or not?

General BETTS. There is undoubtedly a great deal of fracturing of the material that immediately surrounds the event.

Chairman HOLIFIELD. Is there any danger of emission of radioactive materials?

General BETTS. The depth at which we set off these events is selected to contain all of the radioactive material. And as a matter of fact, that is the reason why we found it necessary to go into further exploration for a Lollipop site.

Chairman HOLIFIELD. In the case of HE explosions, you would have poisonous gas, would you not?

General BETTS. There are poisonous fumes which come from an HE explosion.

Chairman HOLIFIELD. So you would have to take care of that to get the factor, as well as the nuclear radiation in case of a nuclear shot?

General BETTS. This is one of the problems that will face us with respect to the Linen event, since it takes place in the same tunnel complex. We will have to seal that off very carefully to prevent that problem.

Chairman HOLIFIELD. You spoke of exploratory drilling in granite. Can this be inclusive enough and exhaustive enough to give you assurance that there are no strata of broken rock?

General BETTS. We would expect to do enough drilling to have reasonable assurance, when taken in the context of the geological information which we already have in that area, that we have a sound medium in which to set off this event.

Chairman HOLIFIELD. Of course, this would be quite expensive, if you had to drill much.

General BETTS. If we had to drill exhaustively, this would be quite expensive.

Chairman HOLIFIELD. Dr. Latter testified yesterday about a cavity that might be as much as 3,000 feet in diameter to control a 6-megaton fully decoupled test. And this was also salt domes. Do you believe that you could excavate a cavity of that diameter?

General BETTS. In my opinion, a cavity of that diameter is probably a very unreliable experiment, to put it mildly.

I would say this, Mr. Chairman. You remember last year in your testimony I think it was Mr. Meade who testified that there were chambers as big as 500 feet. But I would say that in the course of examining the Vela program as it is now defined, the construction experts within the AEC complex, our own and contractor experts,

were reluctant to agree that a 600-foot unsupported chamber was reasonably feasible in terms of a safe experiment. Consequently, there is a difference of opinion.

Chairman HOLIFIELD. Is there any necessity for having that size shot underground?

General BETTS. I do not think so. I think when you get up to larger yield events, it would be much more practical to do these events in space. I think we have the capacity to do them, and I think that for chambers of that size, one could do them much more expeditiously, and probably at less cost.

Chairman HOLIFIELD. As a matter of fact, the types of tests which might be needed would not require those of that size for the proving of designs and models?

General BETTS. For the lower yield tactical weapons, we would certainly need nothing of that size to fully decouple.

Chairman HOLIFIELD. Could you not extrapolate, say, from 100,000 tons to a million, the effect that a million tons of explosives would have?

General BETTS. In terms of a testing program, looking to very large yield weapons, one can do some extrapolation from what we would call a mockup experiment. The degree of extrapolation would be a measure of the degree of confidence in the ultimate result.

Mr. RAMEY. I think Dr. Latter indicated in later testimony that he thought from 100 to 150 kilotons would be maybe the practical range in which decoupled shots might be undertaken in a clandestine way.

General BETTS. If we refer to his estimate of the 750-foot diameter sphere, necessary for full decoupling, obviously, if we came down to 400 or 500 feet, with a partially decoupled event, it would still be a very good concealment. And one who wanted to evade any kind of agreement could do that sort of thing.

Chairman HOLIFIELD. Thank you, General Betts.

(The formal statement of General Betts follows:)

AEC PREPARATIONS FOR VELA UNIFORM EXPLOSION PROGRAM BY BRIG. GEN. A. W. BETTS, USA

The primary objective of the Vela Uniform explosion series, stated previously by Mr. George, is to obtain data needed for the evaluation of the proposed Geneva system and for the development of techniques for improving the capability of detecting and identifying underground nuclear explosions. Signals from detonations are required in order to permit such an evaluation of present and possible future instrumentation. The Atomic Energy Commission has the responsibility for providing the sites, the devices, and actually accomplishing the firing of all nuclear detonations of the Vela Uniform program. In addition, it has responsibility for firing one very large detonation of high explosive which is to be accomplished within the Nevada test site. Also the AEC has responsibility for accomplishing such close-in instrumentation as may be necessary for determining that the firing yield was as forecast, and for certain detonations, determining the nature of the transition of the signal from the cavity of the detonation to the immediate surrounding environment.

Although no nuclear detonations have been approved, the program as presently recommended as necessary to satisfy technical requirements would consist of both nuclear and high explosive shots, and may be broken down into three categories based upon the medium in which the detonations are planned. These are—

1. Three or four nuclear shots and one chemical high explosive shot in a soft rock known as tuff.
2. Three nuclear shots in hard rock, including one shot in an active seismic area.
3. Six nuclear shots in a salt medium.

EVENTS IN TUFF

The events to be detonated in tuff at the Nevada test site would be at a depth of about 1,350 feet except for Stingray which would be at a greater depth (previously explained by Mr. George).

The Orchid shot, which would be very highly instrumented, would be the initial shot of the Vela series. Construction for this shot, to be detonated in a tunnel under the Rainier Mesa at Nevada test site with about 1,350 feet of overburden, is virtually complete.

The Crystal event would follow Orchid and would be detonated in a drift off the same basic tunnel under the Rainier Mesa also at a depth of about 1,350 feet. Basic construction is completed and further preparations are dependent upon a firm go ahead decision.

Linen is the 5-kiloton HE event planned at the Nevada test site. This shot requires 8,700,000 pounds of composition B explosive to produce the equivalent 5-kiloton of TNT energy release. The high explosive, packed in individual 47 pound containers, requires a cavity nearly spherical in shape approximately 60 feet in diameter. Ground zero for the Linen shot would be about 1,250 feet below the surface. The basic entry to the shot area is a tunnel into the Rainier Mesa at Nevada test site. Near the end of a drift from the main tunnel, a shaft was constructed down to a point some 600 feet below the tunnel floor. Due to water bearing strata it will be necessary to move the shot location up to a point approximately 500 feet below the tunnel floor. This decision requires backfilling about 100 feet of the shaft in a particular manner so that it will have characteristics similar to the surrounding rock. Construction of the cavity has been delayed due to the backfilling required to solve the problem of water seepage. Due to the physical characteristics of this rock, our engineering experts have informed us that an unsupported cavity may slough or possibly collapse if left for an extended time period. Thus, when the cavity is constructed, high explosive loading instrumentation, arming and stemming (all of which requires about 7 months) should follow and final preparations should be completed to detonate this event. Once the high explosive is loaded in the cavity, it should be detonated within a reasonable time thereafter since its presence would create a safety hazard.

Stingray exploratory work has been completed. Like Linen, Stingray would be located a few hundred feet below the floor of a tunnel into the Rainier Mesa. The decision as to the precise yield will determine its depth between 1,600 and 2,000 feet below the surface.

As explained by Mr. George, Muslin is a contingency event. Presently there is no activity with regard to Muslin; however, a tentative location has been selected within the Nevada test site tunnel complex.

EVENTS IN HARD ROCK

Extreme difficulty has been encountered in locating a site meeting the technical criteria for Lollipop, a 5-kiloton event in granite at a depth of 1,500 feet scheduled for Nevada test site. Broken and badly altered rock was encountered in a number of exploratory drill holes at depths of about 1,300 to 1,500 feet; however, promise of a satisfactory environment is indicated as a result of good granite to a depth of 1,800 feet which was found in one of the exploratory drill holes. Exploratory drilling will continue in an effort to define a suitable granite formation.

Porpoise is a deep shot at about 7,500 feet. It should be fired, if possible, in the same granite mass as the Lollipop shot; hence, exploratory work will be withheld until Lollipop is located. The badly crushed rock encountered in Lollipop exploratory drilling is not anticipated at this depth.

Several areas are being considered as possible locations for Shoal, the 5-kiloton nuclear event at 1,500 foot depth in an active seismic area. Locations in several States are being considered. Once a site is tentatively chosen, exploratory work would be required to define the surface and subsurface geology and to make hydrologic investigations.

EVENTS IN SALT

The six nuclear events desired for detonation in a salt medium are all at a depth of 2,500 feet and would be conducted in a single salt dome. After investigating and considering a large number of salt domes, Tatum (33 miles southwest of Hattiesburg) and Bruinsburg (32 miles south of Vicksburg) in Missis-

sippi appear to most nearly meet the technical criteria desired. Although the Tatum Dome seems to be the most desirable, final selection will depend on the results of exploratory drilling presently being conducted at both sites. The Tatum Dome has been adjudged technically acceptable for the conduct of phase I of the Dribble series (three smaller shots). Further exploratory work now underway should indicate whether the Tatum Dome is technically acceptable for the conduct of phase II.

Excavating the cavities for the decoupled shots represents an extensive construction project. In the case of the fully decoupled event in phase I (small yield shots), a spherical cavity about 92 feet in diameter is required while the larger decoupled event of phase II requires a spherical cavity about 332 feet in diameter (about the length of a football field). Manmade construction of unsupported cavities of this size and at this depth has never before been attempted in the United States. In all, about 750,000 cubic yards of rock salt, about 1¼ million tons, must be removed. Both conventional mining and solution mining techniques are being considered. Currently, studies relative to the salt disposal problem are being made by the U.S. Bureau of Mines of the Department of Interior and the Industrial Division of the Business and Defense Services Administration of the Department of Commerce.

Cost of construction for this portion of the program would be several millions of dollars and may vary considerably from any estimate due to the unique construction required and the magnitude of the mining operation.

Chairman HOLIFIELD. Our next witness is Dr. Roland Herbst, of the Lawrence Radiation Laboratory at Livermore. Dr. Herbst will testify on related AEC programs in support of the explosion requirements.

We will be particularly interested in receiving any up-to-date information you might have on decoupling.

You may proceed, Doctor.

STATEMENT OF DR. ROLAND F. HERBST,[1] LAWRENCE RADIATION LABORATORY, LIVERMORE, CALIF.

Dr. HERBST. Thank you, Mr. Chairman.

Mr. Chairman and members of the committee, today I would like to briefly review for you what we mean by decoupling, what we have learned about it since your last hearings upon this subject, and how the detection of decoupled explosions will be studied in the Vela explosion program.

Early in 1959, Albert Latter and his associates at the Rand Corp. predicted that an explosion in a large underground cavity would produce a seismic signal much smaller than the signal produced by the same explosion fully tamped; that is, detonated in a small room.

The U.S. first completely contained underground nuclear explosion was the Rainier event of 1957. This explosion had a yield of 1.7 kilotons, and was in a soft rock called tuff. All of our subsequent underground nuclear explosions have been in the same material. We would call all of these explosions tamped.

There were many measurements of the ground motion around Rainier. In particular, a measurement 371 feet above the shot showed a ground movement of 6 inches. The other measurements were consistent with this number. These measurements form the experimental basis of the decoupling theory.

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[1] Received his Ph. D. in theoretical physics at St. Louis University in 1953.
Employed at Argonne National Laboratory for 1 year. Has been at the Lawrence Radiation Laboratory since 1954 and is deputy division leader, Theoretical Division, Lawrence Radiation Laboratory.
He was a member of the U.S. delegation to the Conference on the Discontinuance of Nuclear Weapons Test in 1959.
A member of the Secretary of Defense's ad hoc panel on the Detection of Nuclear Explosions.

The decoupling theory simply consists of the realization that if the explosion had taken place in a large cavity in the same rock, and if the cavity had moved in an elastic fashion, the movement at 371 feet would have been about one-tenth of an inch, rather than 6 inches.

Now, since the seismic signal is proportional to these motions, the decoupled explosion would generate distant signals about 60 times smaller than the fully tamped explosion.

It was further noted that if the cavity were in a hard rock, the reduction would have been about 300 relative to the tamped explosion in tuff. In other words, a 30-kiloton decoupled explosion would generate a distant seismic signal equal to a one-tenth-kiloton fully tamped explosion.

Until last year the decoupling theory rested entirely upon the Rainier measurement and upon the calculation that the hole would move elastically. Has anything happened in the last year which changes or improves our understanding of these two points? Yes and no. There has been no change in the basic decoupling theory, but I believe our understanding of the problem has increased.

Cowboy was a series of nine tamped and eight decoupled chemical explosions in a salt dome. The yields ranged from 20 to 2,000 pounds. There were also the Hobo experiments; a series of four tamped chemical explosions in tuff, the Rainier medium.

The complete analysis of the Cowboy results do not differ significantly from Dr. Albert Latter's report to you last year. I would like to submit for the record a summary report on Cowboy. Cowboy indicates that our theoretical understanding of decoupling is correct. That is, as long as the decoupling cavity is large enough to prevent the medium from going into tension; or from becoming plastic; the rock moves elastically, cracks do not form, and the resultant seismic signal is small.

The high explosive Hobo experiments relate to the experimental basis of the decoupling theory; the Rainier measurements. The Hobo measurements indicate that there is no anomalous behavior of signals in tuff. The Hobo data plus a new analysis of the Rainier measurements has increased our understanding of tamped explosions in tuff. The work supports the validity of the Rainier measurement.

The work of the last year and one-half has increased our confidence and somewhat clarified our understanding of decoupling in elastic cavities.

However, the Cowboy experiment also showed something new. Dr. Albert Latter reported on this briefly last year, also. Explosions in cavities smaller than necessary to insure elastic behavior still decouple. A cavity one-tenth the volume required for full decoupling still decoupled by about twentyfold relative to the tamped salt shots.

Chairman HOLIFIELD. This would mean, then, that a cavity which was only one-tenth of the size of a full decoupling size would reduce a 20-kiloton explosion to 1 kiloton as far as seismic records were concerned?

Dr. HERBST. If you are referring to 1 kiloton in tuff, it would be slightly smaller than the 1 kiloton. It might be two-thirds of a kiloton, because it probably would be thirtyfold decoupling relative to tuff.

Chairman HOLIFIELD. But roughly speaking, it would be 20 to 30?

Dr. HERBST. Yes.

Chairman HOLIFIELD. The practical interpretation of this, then, is that a 200-kiloton shot could be discharged with a 10-kiloton seismic recording?

Dr. HERBST. Yes, sir.

Chairman HOLIFIELD. In this type of a cavity?

Dr. HERBST. Yes, sir.

Mr. RAMEY. And under the Geneva system, the United States would not be able to inspect.

Dr. HERBST. Under the U.S. draft treaty, we would have no inspection rights on the basis of that signal.

Chairman HOLIFIELD. Based on the Rainier formula which we advanced in our diplomatic negotiations to the Soviets, in which we assume that a 20-kiloton shot would cause a seismic recording at 600 miles, at 4.75—is that not right?

Dr. HERBST. Yes, sir.

Chairman HOLIFIELD. This makes completely unrealistic partly decoupled shots which might run 200 or 300 kilotons in the place of 20 kilotons, as far as registration of 4.75 is concerned?

Dr. HERBST. As far as 4.75 is concerned, only a partial decoupling would be necessary for 200 to 300 kilotons.

Chairman HOLIFIELD. Therefore, under the present proposed agreement, such tests would not be subject to inspection by international inspection teams?

Dr. HERBST. That is my understanding of the proposal.

Chairman HOLIFIELD. Then this, of course, from a scientific standpoint, makes our position completely unrealistic. And it is important for us to know whether the opponent is testing or not. It makes it a dangerous formula to depend upon. For we are depending on that kind of a formula to give us information, and if we think that information is important, to know whether our opponent is improving his weapons or not, then we are in effect leaning on a broken crutch.

Dr. HERBST. We are not leaning on the control system. That is for certain, sir.

Mr. RAMEY. I guess the idea would be to improve your system of detection, so that you would get it down to a lower level; and perhaps you could get an agreement at that point on some sort of an inspection system, because that is not a part of the present treaty arrangement.

Dr. HERBST. This presumably is the best hope of the research program, that is, that while in my opinion the research program has only a wisp of a chance of doing anything against a fully decoupled explosion, the research program should result, and in some senses possibly has to a small degree already resulted, in the ability of the United States to reduce the threshold below 4.75.

Mr. RAMEY. When you are talking about this partial decoupling on a 100- to 200-kiloton explosion, what size of a hole would you be talking about then, to reduce it twentyfold?

Dr. HERBST. For 100 kilotons the hole would have about 700,000 cubic yards of volume, if it were at a depth of about 3,000 feet. I just do not remember that diameter. And I would hate to start doing the arithmetic.

Mr. RAMEY. Maybe you could provide that for the record.

Dr. HERBST. I would provide it for the record if you would like. (The diameter is 330 feet.)

Mr. RAMEY. This would be a more practical size hole from the standpoint of construction or of deception in terms of digging, would it not?

Dr. HERBST. That is certainly true. I do not personally know where the breakpoint is between violating the practical breakpoint between underground violations and space violations. It is presumably somewhere in this region.

Shall I proceed, sir?

Chairman HOLIFIELD. Proceed, please.

Dr. HERBST. To many people this partial decoupling was a surprise. I can remember a discussion in Geneva of Technical Working Group II among several members of the U.S. scientific group. It was postulated that if the hole becomes smaller than some critical size, the signal might be larger than the signal from a tamped explosion. The Cowboy experiments proved this postulation to be untrue.

At the time of your last hearings, our understanding of the behavior of these smaller holes was very poor. I believe we now understand roughly how these overdriven holes operate, but the governing phenomena are quite complicated and difficult to analyze at the rather shallow depths of the Cowboy experiment, 800 feet.

Dr. Haskell, who I believe will be on your panel later today, has done some work on this, which might clarify it.

Fortunately, at the greater depths at which cavities for decoupling nuclear shots would probably be excavated, the phenomena seem to become somewhat less complicated. The Rand group has examined the role of plasticity on this problem. They arrive at the conclusion that in salt a cavity one-tenth the volume for full decoupling would probably decouple about thirtyfold to fortyfold relative to tuff.

In other words, while it may require a cavity with a volume of 75,000 cubic yards for each kiloton of yield to decouple three hundredfold, the volume may only have to be 7,500 cubic yards per kiloton for a thirtyfold decoupling.

These considerations greatly increase our understanding of decoupling in smaller holes, but I must emphasize that our confidence in this theory is in no way comparable to our confidence in the elastic decoupling theory. The former is quite complicated and involves poorly known physical properties of rock; the other, the full decoupling, is almost trivially simple.

This brings me to the Vela explosion program. What should we hope to learn from this program?

First, there are the detonations at the Nevada test site, the Shade series. These will involve only tamped shots and are not primarily aimed at understanding decoupling. However, we should remember that the magnitude of the decoupling factor depends mainly on the Rainier experiment. I think the history of the detection problem should cause one to be uneasy about depending upon one experiment. Shade, or even part of Shade, should correct this situation.

The Dribble series in a salt dome is of course directly aimed at the decoupling problem. The first shot in each of the two cavities is designed to test full decoupling. I look upon these as attempts to determine if there are any surprises. Are there any specific nuclear effects that we have overlooked? Does something unexpected happen

on the long-time scale of the large explosion that was missed on the short-time scale of Cowboy? Are there any nonseismic ways of detecting decoupled explosions? We hope to answer these questions by the first shot in each cavity.

The subsequent detonations will overdrive the cavities. These would have a more positive aim, since they relate to phenomena which we do not now believe we fully understand. We should know after this program how large a cavity is required for various decoupling factors. We should also learn something about the number of times a cavity can be reused.

We now believe one can decouple; after Dribble we will have a better idea of the size of cavity that is necessary for significant decoupling. We will have found if it is possible to detect such explosions.

Of course, there are questions which will not have been answered. For instance, I know of no plans to investigate methods of increasing decoupling or other methods of reducing the seismic signal or confusion methods which might cause a bomb signal to be identified as an earthquake.

However, I believe that unless we find some quite unexpected surprises, these unanswered questions are of minor importance.

Thank you, sir.

Chairman HOLIFIELD. Thank you, Doctor.

I understand from your statement that work since our last hearings, including the Cowboy tests, has done nothing to change the theory of elastic decoupling or full decoupling.

Dr. HERBST. That is correct, sir.

Chairman HOLIFIELD. I am interested in your new theory of partial decoupling, which I understand was completely unpredicted. Do you have any feeling for whether or not we will again see wholly unpredicted performance from a nuclear test in a cavity?

Dr. HERBST. Let me say this. I think this is one of the cases where the layman's seat-of-the-pants sort of feeling was better than the scientists' sophisticated feeling. I think that a number of people who were not closely involved in the decoupling problem had the natural feeling that if a big hole gave you 300 fold, the smaller hole ought to give you something smaller.

The reason for the few scientists who were involved in this problem was that we had so concentrated on this idea of elastic that we really did not know how to look at the borderline cases.

Now, I think that as far as the full decoupling is concerned, the probability of any real surprises in that field is very, very small. But there have always been surprises in the past, and there are certainly going to be some in the future.

I am quite confident that a field which is as primitive as the field that the Vela program is involved in is going to bring up a large number of surprises. This problem is in its infancy, really, sir.

Chairman HOLIFIELD. Do you think the decoupling achieved would be more likely to be less, or more, than the factor of 300 that has been indicated?

Dr. HERBST. I am saying that as far as the full decoupling is concerned, I would think that Vela might move it a little bit. It might be 250, and it might be 500. And I think the probability thit will

move more than that small amount is very small. But one cannot rule out this hope. I mean, one can hope the world will be nice to you. I think that is all it is, though—a hope.

Chairman HOLIFIELD. As a matter of fact, the proving up of this theory would be quite valuable to us, because if would have immense effect on any type of an inspection system, or detection system, rather, if it was by a factor of 500 instead of 300. It would make it even more difficult to detect a fully decoupled shot.

Dr. HERBST. Of course, there have been pointed out methods, if one really wants a factor of 500 or a thousand, there probably are methods of getting it.

Chairman HOLIFIELD. Now, with your new theory of partial decoupling, is it not true that a nuclear test of 600 kilotons could be detonated in a hole in salt 500 feet in diameter and produce seismic waves equivalent only to the magnitude of 4.75, according to Dr. Latter's calculations, at our hearings last year?

Dr. HERBST. Sir, it is not my theory. 600 kilotons in a cavity which is one-tenth of the volume—and again I would have to check that rating of the diameter—would presumably give a signal of about 4.75, the present threshold. The diameter is 600 feet.

Chairman HOLIFIELD. So any kind of test below 600 kilotons that was decoupled in this way could come within the realm of prohibition for inspection under the Geneva formula?

Dr. HERBST. Yes, sir.

Chairman HOLIFIELD. Well, now, if this is true, Dr. Herbst, is not the "theory of partial decoupling" potentially a far more practical worry for Vela than is the "theory of full elastic decoupling"?

The reason I ask that is that only one-tenth of the volume of the hole would be required for partial decoupling.

Dr. HERBST. This is purely a matter of what one expects out of the research program. I would say in my judgment the research program will improve the system sufficiently that the 4.75 will be reduced; so that partial decoupling is important, though probably not at the 600-kiloton level; more at the 100- or 200-kiloton level. I think partial decoupling is an interesting question.

Mr. RAMEY. On this matter of the threshold, how much do you thing the threshold can be reduced below the 4.75, based on present accomplishments of the Vela Uniform program?

Dr. HERBST. I am not a seismologist, sir.

There is one thing that I think, though, that a nonseismologist can say about seismology. As one reduces the noise level, one gains sort of like the square root of that number. Dr. Latter pointed out last year, I believe, to you, that reduction of noise is very important, but you do not gain as fast as one might think, because you get more and more events that you have to talk about. You see more earthquakes as you get the noise down.

Now, Dr. Romney's data of yesterday seemed to indicate already we have a gain of about a factor of two. The noise reduction of 14 for the large arrays should be compared with a reduction of about 7 that had been assumed in the past. So that there is a gain of about a factor of two in noise. You will be able to see a signal a factor of two lower, but you will see so many of them that it does not gain quite as much as you thought you gained.

I think, though, it would be much better to ask this of the panel than of me.

Chairman HOLIFIELD. We will reserve that for the panel, then. Maybe you had better participate in the panel.

Well, we thank you for your testimony. It is very valuable, and we know it comes from a background of deep study.

We have your Livermore Laboratory Report, UCRL-6165 and it is a quite valuable source document. I think it should be a part of the record. (See app. 4, p. 334.)

Thank you very much, Dr. Herbst.

(The formal statement of Dr. Herbst follows:)

STATEMENT OF DR. ROLAND F. HERBST, LAWRENCE RADIATION LABORATORY, LIVERMORE, CALIF.

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Now, since the seismic signal is proportional to these motions, the decoupled explosion would generate distant signals about 60 times smaller than the fully tamped explosion.

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The subsequent detonations will overdrive the cavities. These would have a more positive aim since they relate to phenomena which we do not now believe we fully understand. We should know after this program how large a cavity is required for various decoupling factors. We should also learn something about the number of times a cavity can be reused.

We now believe one can decouple; after Dribble we will have a better idea of the size of cavity that is necessary for significant decoupling. We will have found if it is possible to detect such explosions.

Of course there are questions which will not have been answered. For instance, I know of no plans to investigate methods of increasing decoupling or other methods of reducing the seismic signal or confusion methods which might cause a bomb signal to be identified as an earthquake.

However, I believe that unless we find some quite unexpected surprises, these unanswered questions are of minor importance.

Chairman HOLIFIELD. Our next witness is Col. Jack Ledford, of the Defense Atomic Support Agency. Colonel Ledford will testify on the Defense Atomic Support Agency's program in support of the explosion requirements in Project Vela.

Colonel, we are happy to have you with us. You may proceed.

STATEMENT OF COL. JACK C. LEDFORD,[1] ACCOMPANIED BY JOHN G. LEWIS, TECHNICAL ADVISER, DEFENSE ATOMIC SUPPORT AGENCY

Colonel LEDFORD. Mr. Chairman, may I introduce Mr. John Lewis, our technical adviser in underground shot phenomena, in the Defense Atomic Support Agency, who will assist me.

Mr. Chairman, the discussions heretofore have explained the scope of Project Vela, its objectives, organizations, and plan of operation. My remarks this morning will concern themselves with the explosion series in Vela Uniform, to continue where General Betts has already testified this morning for the AEC part of the explosion series.

The actual detonation of these series, as you have heard, is subject to authorization by the President. (See chart 1, p. 160). As the Department of Defense representative for the explosion series, DASA has the responsibility for the preparation, implementation, and coordination of all the nuclear shots when approved, and chemical shots irrespective of location. This does not include emplacement and detonation of the nuclear devices or the one large HE charge (Linen) scheduled for firing at the Nevada Proving Ground, which, as you have heard from General Betts, is a responsibility of the AEC.

DASA will provide operational support to all DOD programs at the Nevada Proving Ground. In this connection DASA will furnish to the AEC, the operator of the Nevada Proving Ground, the requirements for support of all DOD agencies participating in a particular operation. This includes such things as office space, billets, messing, and communications. DASA maintains a fleet of vehicles for use by DOD agencies operating at the proving ground.

DASA will provide a DOD program manager who exercises administrative, operational, and technical direction over all DOD agencies, personnel, and experimental programs. He acts for the AEC project manager, the individual responsible for conduct of all experiments at the proving ground, in all matters of exclusive DOD interest; and shares equal responsibility with the AEC project manager in matters concerning site selection and time of firing.

DASA is also charged with the planning and accomplishment of all close-in phenomenology measurements. This is that part of the technical program which is physically located in close proximity to the burst point, as differentiated from the Air Force Technical Applications Center experiments, which are far-out measurements. I will have more to say about this program in a few minutes.

As has already been mentioned, the Advanced Research Projects Agency (ARPA) has overall responsibility for Vela Uniform. We have just gone over the DASA responsibilities. Now I would like to show a chart (see chart 2, p. 160) indicating how the various agencies are organized for accomplishing the field operations of the explosion series. Requirements for the close-in measurements program are generated by the Air Force Technical Applications Center (AFTAC),

[1] Deputy Chief of Staff, Weapons Effects and Tests for the Headquarters, Defense Atomic Support Agency. A native of Blairsville, Ga., and a graduate (B.S. physics) of the Ohio State University, Colonel Ledford has been in the nuclear field in strategic air operations, tactical air operations, and research and development for the past 12 years of his total 20 years service in the U.S. Air Force. A combat veteran of World War II, Colonel Ledford's decorations include the Distinguished Service Cross.

approved by ARPA, and forwarded to DASA. A program is prepared and sent to DASA's field command organization, which is located at Albuquerque, N. Mex., where it is implemented in close coordination with the Atomic Energy Commission's Operations Office.

Shown here (see chart 3, p. 161) is our operational organization for conducting the explosion series. As I have mentioned before, the program manager is charged with the implementation of the technical program to include close coordination with the AEC. Under him we have a public information and visitors' bureau, of which I will have more to say in a few minutes; an administration division whose duties, I believe, are self-evident; a technical director who will be discussed later; and a support director. The support director is charged with providing logistical support to all DOD agencies operating at the proving ground. This support is either provided by DASA directly, or through the AEC, the operator of the Nevada Proving Ground.

To go back to public affairs, the Department of Defense, Department of State, and Atomic Energy Commission coordinate Vela public affairs activities closely. Within the Department of Defense, the Assistant Secretary of Defense (Public Affairs) has assigned DASA to serve as ARPA's executive agent for public affairs activities concerned with the underground detonation and associated detection station portions of the Vela Uniform program. These activities, of course, involve a considerable amount of field work. A joint information organization for work in the field has been set up jointly by the DASA field command and the AEC Albuquerque Operations Office.

Previous witnesses have described the shot program for Shade and Dribble in terms of yield, medium, and depth of burial.

I would now like to give you a typical example of a field operations of one of these events.

Our organization for conducting a nuclear field experiment is shown on this chart. (See chart 4, p. 162.) The DOD Technical Group Director utilizes the indicated staff divisions to provide the proper technical, support, and construction coordination to the experimental projects. The numbers and types of such projects are indicated on the left of the chart, under the DASA technical programs.

We give these numbers, projects 1.1, 1.2, and so forth, to differentiate and identify the various programs and their contractors.

On the other side of the chart is the Air Force Technical Applications Center offsite measurement program. I would like to say at this time there are some 28 different technical experiments going on at the same time, when the shot is fired; so that you can see that this technical director is very concerned with the scheduling of the various activities, so that all 28 of these projects will be ready when the event is fired.

By following me on your chart (chart, p. 163), we can go out to the Nevada Proving Ground. To orient you, here, we show Camp Mercury on the bottom, which is the base camp for both the Atomic Energy Commission and the Department of Defense operations. Camp Mercury is about 70 miles northwest of Las Vegas, Nev. Some 50 miles to the north of Camp Mercury are what we call areas 12 and

15, which have been selected for seven of the Shade program events. Area 12 includes those events to be fired in tuff, and area 15 those in granite.

Shown on the next chart (chart 6, p. 164) is a picture of the physical layout of the tunnel system in area 12. We are just taking the tuff area, now, to give you a picture of one of these experiments.

Up at the top you can see the expanded tunnel system of the Rainier event of 1957, which Dr. Herbst just explained. Off of this tunnel, the Linen 5-kiloton HE event will be fired. Now, that tunnel system at the top is about 850 feet below the surface of the mesa, whereas the lower portion is about 1,350 feet. So the Linen shot will be detonated about 500 feet below the tunnel, since we wish to compare the two shots at the same depth.

The next chart (chart 7, p. 165) shows the susface of the mesa above the tunnel, and the zero point for the Orchid 5-kiloton nuclear event.

Now, I will take this event and describe it as a typical measurement experiment. The remainder of the events will have similar technical programs. Indicated on this chart are positions for some of the instrumentation stations for Orchid.

The principal objective of the close-in phenomena program is to measure the motions in the earth in that zone immediately surrounding the detonation where the seismic signals are being generated. These motions are measured by use of accelerometers, and velocity and displacement gages.

Usually these gages are either firmly anchored to the surface of the earth, or inside the tunnel, or they are placed in drill holes and cemented into place, with what is referred to as a "grout," or cement.

You can see indicated on the chart the various stations where surface measurements will be made by such agencies as the Stanford Research Institute, who will measure motion along the surface as well as at various depths down a drill hole from the surface toward the burst point. We call this Project 1.2.

Edgerton, Germershausen, and Grier will measure the motion on the surface using photographic techniques. Cameras located beyond scope of the chart will photograph the motion of targets indicated on the chart as Project 1.3.

Also beyond the scope of the chart will be a project by the U.S. Coast and Geodetic Survey measuring long period accelerations and displacements on the surface and at several ranges extending from about one-quarter of a mile to 8 miles.

You might say that this is the intermediate measurement program. We have the close-in measurements. AFTAC is taking you far out. And here we are trying to fill in the gap between.

The next chart (chart 8, p. 166) shows the Orchid plan at the elevation of the tunnel, some 1,350 feet below the surface. Off of this tunnel will be drilled both horizontal and vertical holes in which instruments will be placed by the Sandia Corporation.

This project, identified as 1.1, will measure the motion in the rock in the horizontal direction and below the level of the burst, using similar gages to those used by the Stanford Research Institute on and from the surface. These gages must be properly anchored into the rock. This is accomplished by this grout that I referred to, which has the same density and strength characteristics as the rock media in

73459—62——11

which the instrumentations are placed, so that we do not get a distortion of the shock waves. This is identified as Project 9.1, under the Army Corps of Engineers Waterways Experimentation Station.

Almost all of these measurements I have described use electronic recording techniques. To accomplish this, the cables run from each gage to a recording station which is far enough away to prevent accidental damage from the shock produced by the detonation.

In addition to these electronic measurements, there will be a project to measure very accurately, both pre- and post-shot, the position of selected points along the tunnel and on the mesa surface. These measurements will give the permanent displacement caused by the event, and will be conducted by Holmes and Narver of Los Angeles.

In another project, the Space Technology Laboratories will install nonelectronic gages in the floor of the tunnel, which will respond to selected frequencies of motion. The data from these gages will indicate the predominant frequencies of the shock; or, conversely, will indicate the characteristic filtering qualities of the medium or rock surrounding the burst.

Now, once the tunnel is available and the experimental program has been designed—we are at that position now for the first event, Orchid—we still have quite a bit of work to do in the field. I would like to show the work that must be accomplished in the 10-week period after the authorization is given to go ahead with this program (chart 9, p. 167).

At D minus 10 weeks, the project agencies move to the site. Here they begin assembling and calibrating their instruments. Instrument cables are run to recording station, and the contractors complete the support construction.

At D minus 6 weeks, the users, the technical people, occupy the site and begin installing the instruments and the special grout mentioned previously. Once this process has begun, the gages, which are grouted in drill holes, are permanently committed to the event. Furthermore, these gages gradually become unusuable because of water seepage, such that if the event is not fired within 12 weeks, probably only about half of the gages will be functioning properly. Further delay could require complete redrilling and new instruments.

As you see, the D minus 6 weeks decisions are very important. Following instruments placement, electrical checkouts and dry runs begin, and usually last until shot time. Here we practice coordinating the 28 technical agencies involved so that they are all ready at shot time.

Of course, last minute changes are also occurring during this entire 10-week period. And emplacing the device is an AEC function and is accomplished during the last 2 weeks.

Mr. Chairman, this concludes my presentation. I hope I have given you a "feel" for the field activities involved in a program such as the Vela Uniform and the planning and organization that goes into a program of this type as far as the Department of Defense is concerned.

Chairman HOLIFIELD. Thank you, Colonel Ledford.

This gives us a general concept of your organizational and operational setup out there, and the time element that is involved once a decision is made.

How many individuals are required in your DASA field command?

Colonel LEDFORD. We have approximately 63 assigned at the present time.

Chairman HOLIFIELD. And how many civilian contractor people are you using?

Colonel LEDFORD. We have six contract agencies. I would have to guess at the total number of people.

I am advised it is up to 150 people in the field from the six project agencies.

Chairman HOLIFIELD. Has there been an overall estimate of the cost of preparing, firing, and recording the close-in data on a single underground nuclear test, exclusive of the initial construction cost?

Colonel LEDFORD. Yes, sir. However, all the costs have not been calculated as yet. The Orchid event so far has cost $1,651,000. That includes purchasing of instruments, and the pay and cost of contract to the research agencies conducting the measurements.

In this connection, we have been issued $4,405,000 by the Advanced Research Projects Agency, and have committed $3,400,000 total toward the Shade program.

Chairman HOLIFIELD. You consider you are adequately funded for the Shade program, then?

Colonel LEDFORD. Yes, sir, I do.

Chairman HOLIFIELD. How about the Dribble and the Groundhog?

Colonel LEDFORD. The Dribble and Groundhog program have not yet been funded.

Chairman HOLIFIELD. That would be an additional expense?

Colonel LEDFORD. Yes, sir, we have so far spent $14,000 on Dribble and $6,000 on Groundhog, mostly administrative expenses.

Chairman HOLIFIELD. Mr. Ramey?

Mr. RAMEY. I notice on page 3 that your organization is responsible for public affairs activities so far as ARPA is concerned. Who was responsible for these newspaper photographs that were shown on the tunnels out in Nevada not so long ago? There seemed to be quite a flap about that.

Colonel LEDFORD. I saw that myself. And I do not know who was responsible. It was not the Department of Defense.

Chairman HOLIFIELD. Were they old photographs, or current photographs?

Colonel LEDFORD. I do not know, Mr. Chairman. They were practically the same that I showed you here, as I recall. They were not complete photographs.

Chairman HOLIFIELD. I have been informed that some of the photographs used were photographs that were taken of the Rainier tunnels.

Colonel LEDFORD. Very likely. Some of the older photographs that were available of that tunnel complex.

Chairman HOLIFIELD. All right. Thank you very much, sir.

(The formal statement of Colonel Ledford and Mr. Lewis follows:)

VELA UNIFORM

(By Col. Jack C. Ledford and John G. Lewis)

Good afternoon gentlemen. The discussions heretofore have explained the scope of Project Vela, its objectives, organization, and plan of operation. My remarks will concern themselves with the explosion series in Vela Uniform. The actual detonation of these series, as you have heard, is subject to authorization by the President. (See chart 1.) As the Department of Defense repre-

sentative for the explosion series, DASA has the responsibility for the preparation, implementation, and coordination of all the nuclear shots when approved, and chemical shots irrespective of location. This does not include emplacement and detonation of the nuclear devices or the one large HE charge (Linen) scheduled for firing at the Nevada Proving Ground which is a responsibility of the AEC.

DASA will provide operational support to all DOD programs at the Nevada Proving Ground. In this connection DASA will furnish to the AEC, the operator of the Nevada Proving Ground, the requirements for support of all DOD agencies participating in a particular operation. This includes such things as office space, billets, messing, and communications. DASA maintains a fleet of vehicles for use by DOD agencies operating at the proving ground.

DASA will provide a DOD program manager who exercises administrative, operational, and technical direction over all DOD agencies, personnel, and experimental programs. He acts for the AEC project manager, the individual responsible for conduct of all experiments at the proving ground, in all matters of exclusive DOD interest; and shares equal responsibility with the AEC project manager in matters concerning site selection and time of firing.

DASA is also charged with the planning and accomplishment of all close-in phenomenology measurements. This is that part of the technical program which is physically located in close proximity to the burst point. I will have more to say about this program in a few minutes.

As has already been mentioned, the Advanced Research Projects Agency (ARPA) has overall responsibility for Vela Uniform. We have just gone over the DASA responsibilities. Now I would like to show a chart (see chart 2) indicating how the various agencies are organized for accomplishing the field operations of the explosion series. Requirements for the close-in measurements program are generated by the Air Force Technical Applications Center (AFTAC), approved by ARPA and forwarded to DASA. A program is prepared and sent to DASA's field command organization which is located at Albuquerque, N. Mex., where it is implemented in close coordination with the Atomic Energy Commission's Operations Office.

Shown here (see chart 3) is our operational organization for conducting the explosion series. As I have mentioned before, the program manager is charged with the implementation of the technical program to include close coordination with the AEC. Under him we have a public information and visitors bureau (of which I will have more to say in a few minutes); an administration division whose duties, I believe, are self-evident; a technical director who will be discussed later; and a support director. The support director is charged with providing logistical support to all DOD agencies operating at the proving ground. This support is either provided by DASA directly, or through the AEC, the operator of the Nevada Proving Ground.

To go back to public affairs, the Department of Defense, Department of State, and Atomic Energy Commission coordinate Vela public affairs activities closely. Within the Department of Defense, the Assistant Secretary of Defense (Public Affairs) has assigned DASA to serve as ARPA's executive agent for public affairs activities concerned with the underground detonation and associated detection station portions of the Vela Uniform program. These activities, of course, involve a considerable amount of fieldwork. A joint information organization for work in the field has been set up jointly by the DASA field command and the AEC Albuquerque operations office.

Previous witnesses have described the shot program for Shade and Dribble in terms of yield, medium, and depth of burial.

I would now like to give you a typical example of a field operations of one of these events.

Our organization for conducting a nuclear field experiment is shown on this chart (see chart 4). The DOD technical group director utilizes the indicated staff divisions to provide the proper technical, support, and construction coordination to the experimental projects. The numbers and types of such projects are indicated. Of particular importance to the technical director is the scheduling of the various activities such that all projects are ready for the event to be fired at the planned instant of firing.

This chart (see chart 5) is a map of the Nevada Proving Ground showing Camp Mercury, which is the base camp for both the AEC and the DOD operations at the site. Some 50 miles to the north of Camp Mercury are areas 12 and 15 which have been selected for seven of the Shade events. Area 12 includes those events to be fired in tuff and area 15 those in granite. Shown

on the next chart (see chart 6) is a picture of the physical layout of the tunnel system in area 12.

The tunnel system in the upper right on the chart shows the location of the Rainier event of 1957. Off this tunnel the Linen 5 kiloton HE event will be fired. This tunnel is approximately 850 feet below the surface of the mesa; therefore, Linen will be fired below the tunnel floor at a depth approximately the same as the elevation of the other tunnel system shown on the chart, which is about 1,350 feet below the mesa surface.

This next chart (see chart 7) shows the surface of the mesa above the tunnel and zero point for the Orchid 5 kiloton nuclear event. I will describe the close-in program for Orchid. The remainder of the events will have similar technical programs. Indicated on the chart are positions for some of the instrumentation stations for Orchid.

The principal objective of the close-in phenomena program is to measure the motions in the earth in that zone immediately surrounding the detonation where the seismic signals are being generated. These motions are measured by use of accelerometers, and velocity and displacement gages. Usually these gages are either firmly anchored to the surface of the earth, or inside the tunnel, or they are placed in drill holes and cemented into place with what is referred to as a "grout." You can see indicated on the chart, stations where surface measurements will be made by such agencies as the Stanford Research Institute who will measure motion along the surface as well as at various depths down a drill hole from the surface toward the burst point. This is project 1.2.

Edgerton, Germershausen, and Grier will measure the motion of the surface using photographic techniques. Cameras located beyond scope of the chart will photograph the motion of targets indicated on the chart as project 1.3.

Also beyond the scope of the chart will be a project by the U.S. Coast and Geodetic Survey measuring long-period accelerations and displacements on the surface and at several ranges extending from about one-quarter of a mile to 8 miles.

The next chart (see chart 8) shows the Orchid plan at the elevation of the tunnel some 1,350 feet below the surface. Off of this tunnel will be drilled both horizontal and vertical holes in which instruments will be placed by the Sandia Corp. This project, 1.1, will measure the motion in the rock in the horizontal direction and below the level of the burst using similar gages to those used by Stanford Research Institute on and from the surface. These gages must be properly anchored into the rock. This is accomplished by using a cement grout which has the same density and strength characteristics as the rock. The Army Corp of Engineers Waterways Experimentation Station personnel prepare and pump this grout under project 9.1.

Almost all of the measurements I have described thus far use electronic recording techniques. To accomplish this, cables run from each gage to a recording station which is far enough away to prevent accidental damage from the shock produced by detonation.

In addition to these electronic measurements, there will be a project to measure very accurately, both preshot and postshot, the position of selected points along the tunnel and on the mesa surface. These will give the permanent displacement caused by the event and will be conducted by Holmes & Narver of Los Angeles.

The Space Technology Laboratories will install nonelectronic gages in the floor of the tunnel which respond to selected frequencies of motion. The data from these gages will indicate the predominant frequencies of the shock; or, conversely, will indicate the characteristic filtering qualities of the medium surrounding the burst.

Now, several times, mention has been made of the fact that the Orchid shot was being held at a D–10 weeks' readiness status. I would now like to show the fieldwork that must be accomplished in the 10-week period after the go-ahead is given (see chart 9). At D–10 weeks the project agencies move to the site and begin assembling and calibrating their instruments. Instrument cables are run to recording stations and the contractors complete the support construction. At D–6 weeks the users (the technical people) occupy the site and begin installation of instruments using, in some instances, the special grout mentioned previously. Once this process has begun, the gages which are grouted in drill holes are permanently committed to the event. Furthermore, these gages gradually become unusable because of water seepage such that if the event is not fired within 12 weeks probably only about half of the gages will be functioning properly. Further delay could require complete redrilling and new instruments. As you see, the D–6 weeks' decisions are very important. Following instrument placement,

electrical checkouts and dry runs begin and usually last until shot time. Last minute changes are also occurring during this entire 10-week period. Emplacing the device is an AEC function and is accomplished during the last 2 weeks.

This concludes my presentation. I hope I have given you a "feel" for the field activities involved in a program such as the Vela program, as well as a better understanding of the organization and planning that goes into a program of this type. Thank you.

CHART 1

DASA RESPONSIBILITIES

1. Preparation, Implementation and Coordination of the DOD Program for all nuclear and large HE events at the Nevada Proving Ground.
2. Provide Operational Support to all DOD Programs at the Nevada Proving Ground.
3. Provide DOD Program Manager.
4. Plan and accomplish close-in phenomenology measurements.

CHART 2

ORGANIZATION FOR EXPLOSION SERIES

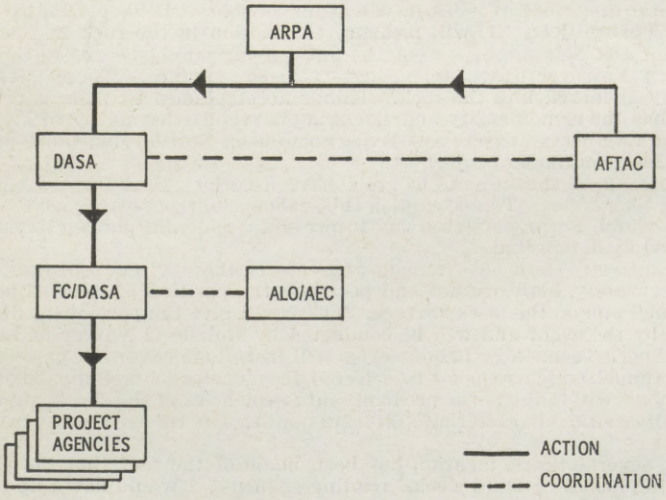


CHART 3

VELA UNIFORM FIELD ORGANIZATION

FIELD COMMAND, DASA

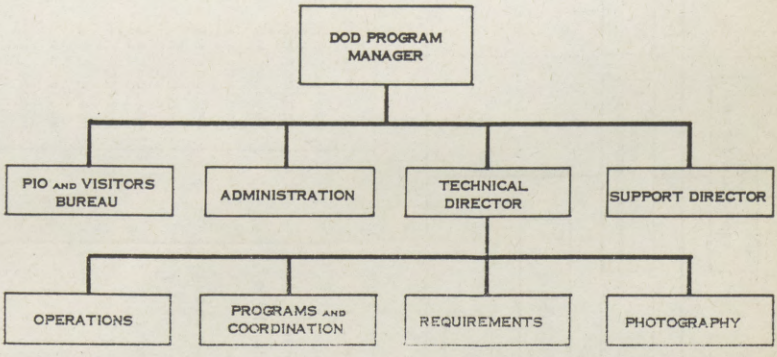


CHART 4

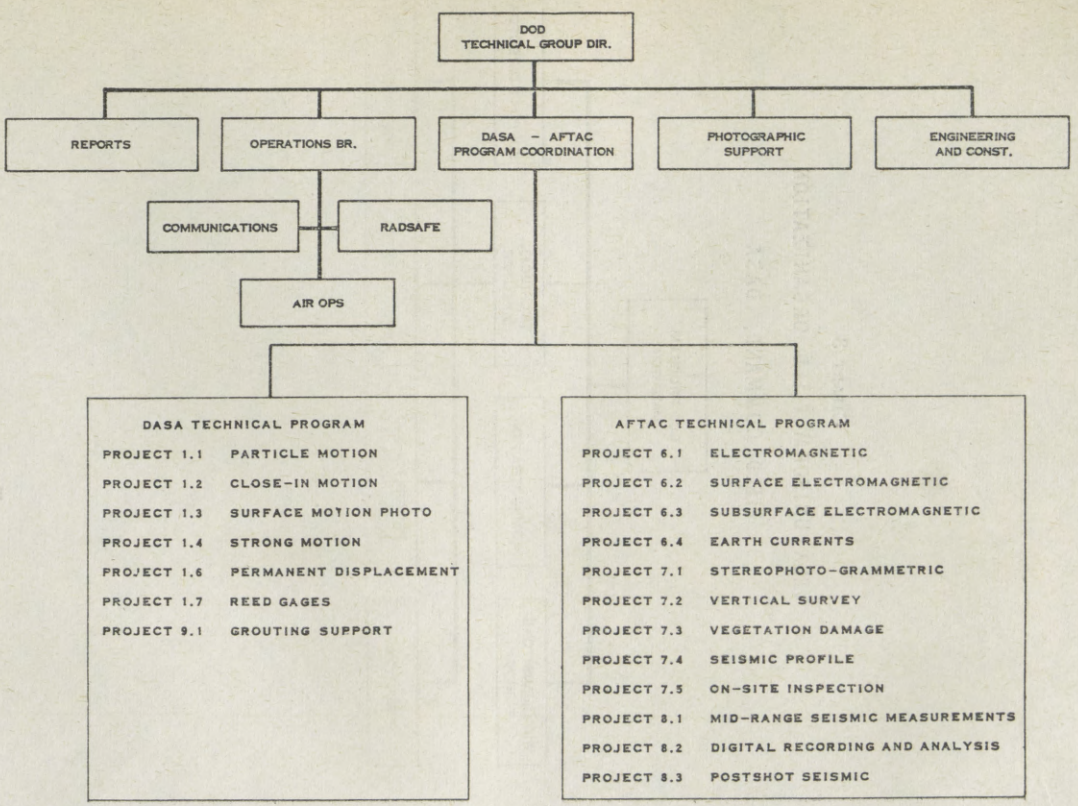
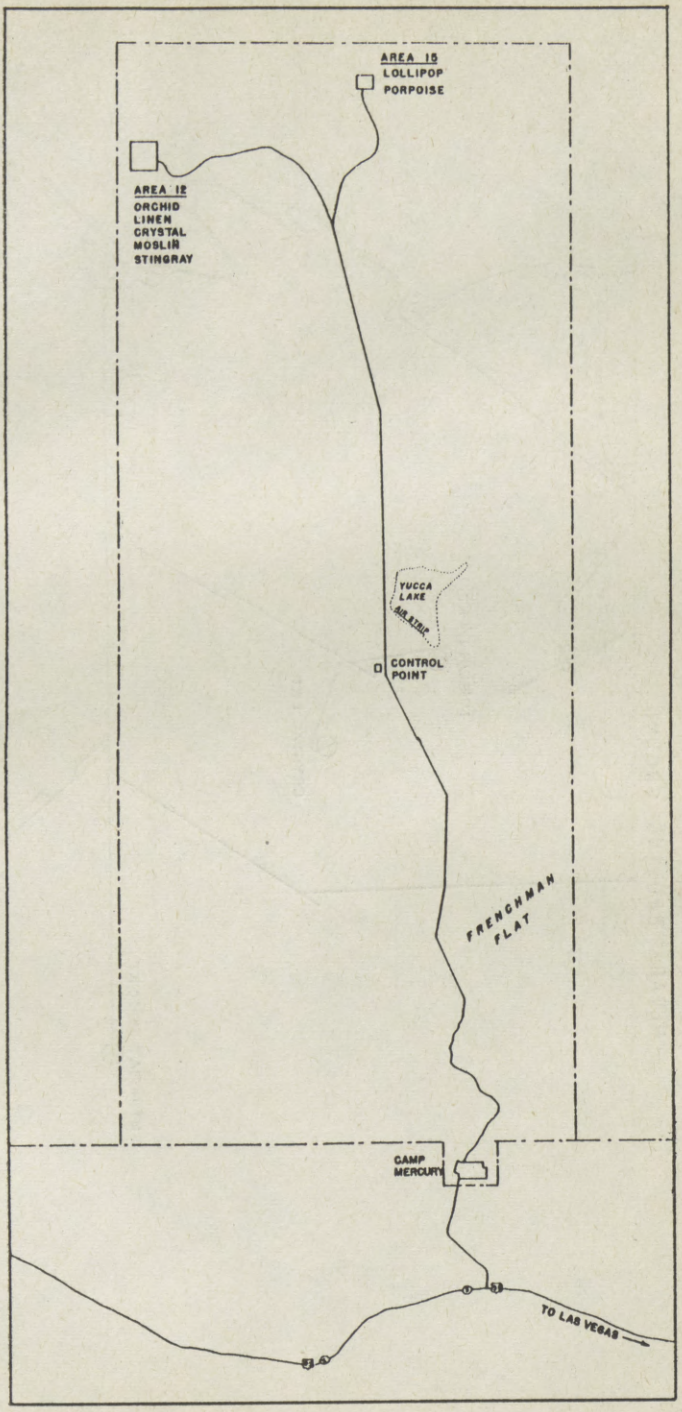

DOD
TECHNICAL GROUP DIR.
REPORTS
OPERATIONS BR.
DASA – AFTAC
PROGRAM COORDINATION
PHOTOGRAPHIC
SUPPORT
ENGINEERING
AND CONST.
COMMUNICATIONS
RADSAFE
AIR OPS
DASA TECHNICAL PROGRAM
PROJECT 1.1 PARTICLE MOTION
PROJECT 1.2 CLOSE-IN MOTION
PROJECT 1.3 SURFACE MOTION PHOTO
PROJECT 1.4 STRONG MOTION
PROJECT 1.6 PERMANENT DISPLACEMENT
PROJECT 1.7 REED GAGES
PROJECT 9.1 GROUTING SUPPORT
AFTAC TECHNICAL PROGRAM
PROJECT 6.1 ELECTROMAGNETIC
PROJECT 6.2 SURFACE ELECTROMAGNETIC
PROJECT 6.3 SUBSURFACE ELECTROMAGNETIC
PROJECT 6.4 EARTH CURRENTS
PROJECT 7.1 STEREOPHOTO-GRAMMETRIC
PROJECT 7.2 VERTICAL SURVEY
PROJECT 7.3 VEGETATION DAMAGE
PROJECT 7.4 SEISMIC PROFILE
PROJECT 7.5 ON-SITE INSPECTION
PROJECT 8.1 MID-RANGE SEISMIC MEASUREMENTS
PROJECT 8.2 DIGITAL RECORDING AND ANALYSIS
PROJECT 8.3 POSTSHOT SEISMIC

CHART 5


AREA 15
LOLLIPOP
PORPOISE
AREA 12
ORCHID
LINEN
CRYSTAL
MOSLIR
STINGRAY
YUCCA
LAKE
AIR STRIP
CONTROL
POINT
FRENCHMAN
FLAT
CAMP
MERCURY
95
95
TO LAS VEGAS

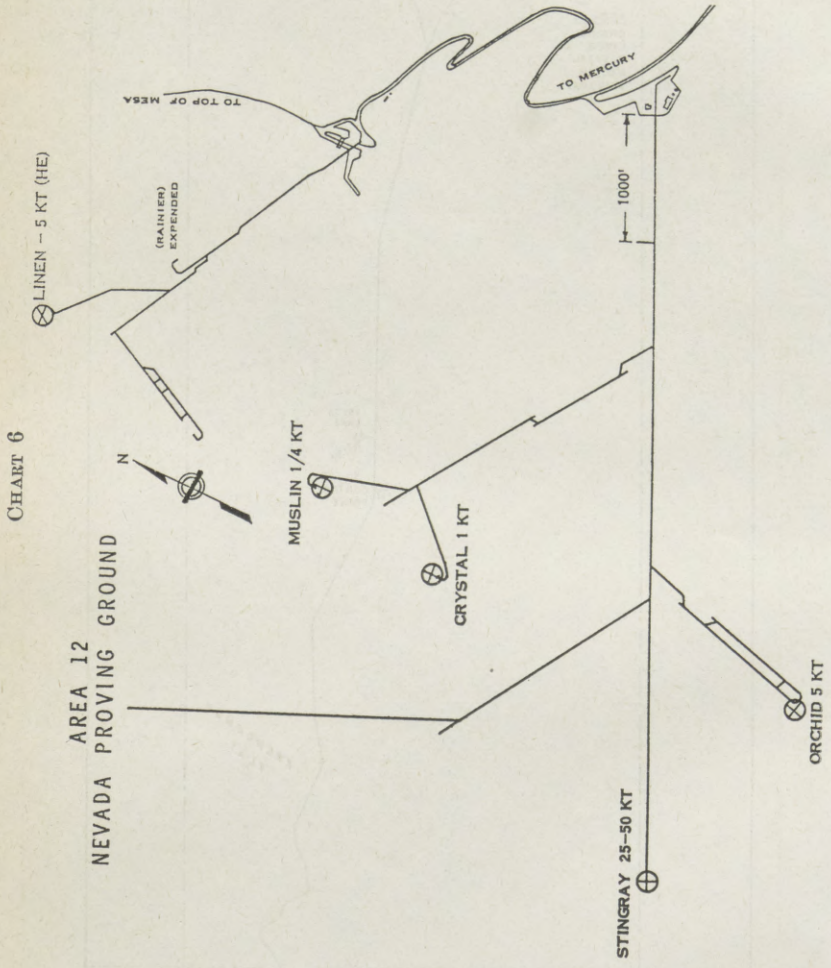

CHART 6
AREA 12
NEVADA PROVING GROUND
N
LINEN – 5 KT (HE)
(RAINIER) EXPENDED
TO TOP OF MESA
MUSLIN 1/4 KT
CRYSTAL 1 KT
TO MERCURY
1000'
STINGRAY 25–50 KT
ORCHID 5 KT

CHART 7

ORCHID - SURFACE INSTRUMENT PLAN

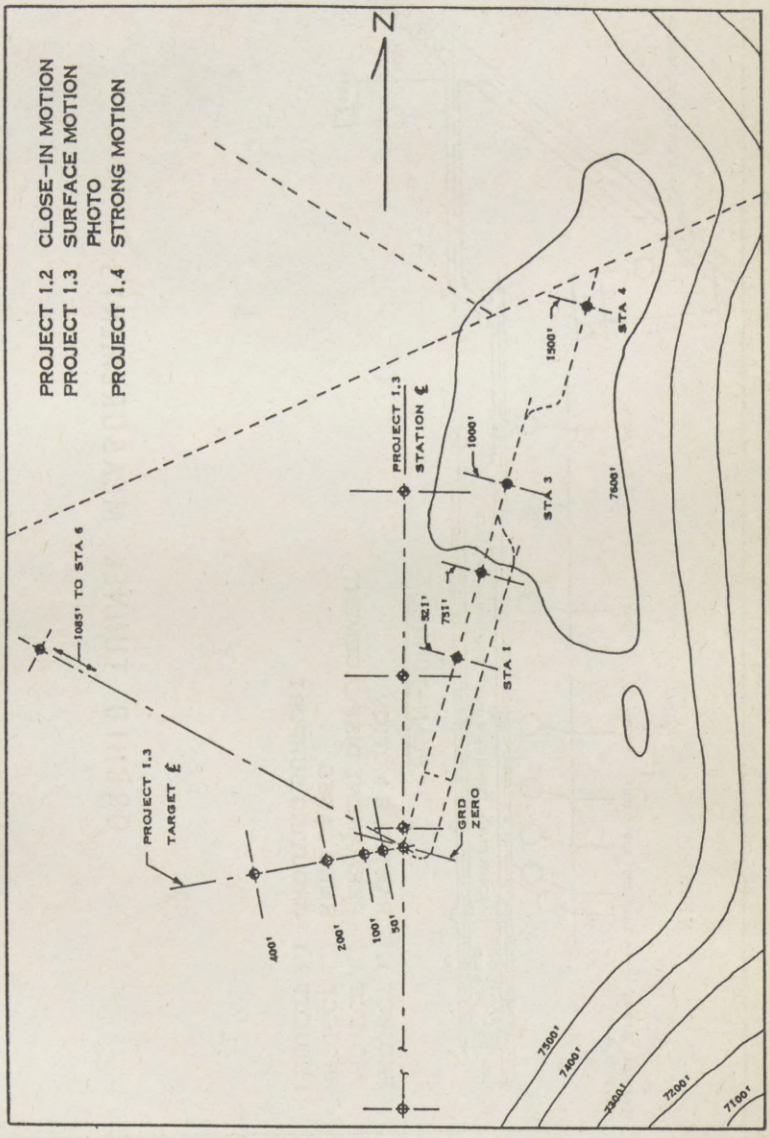
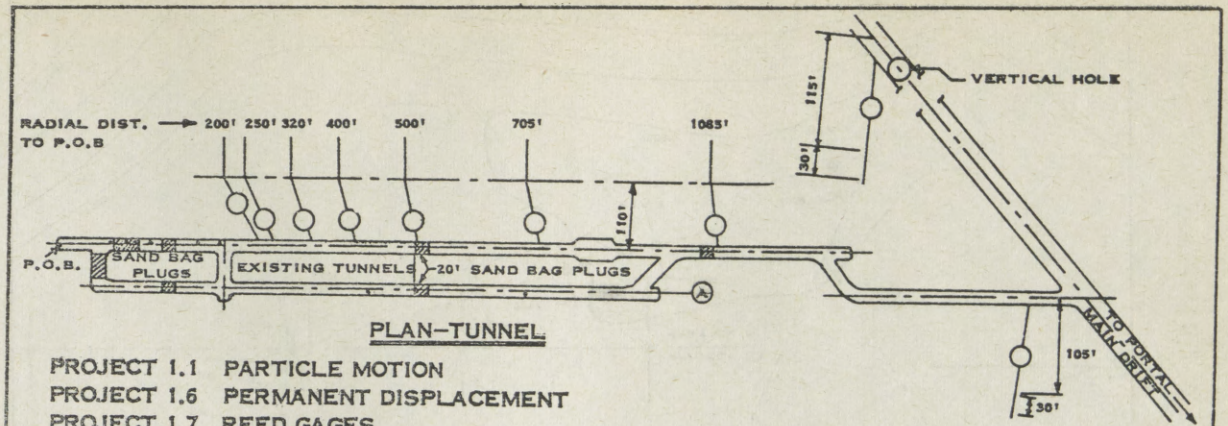


CHART 8



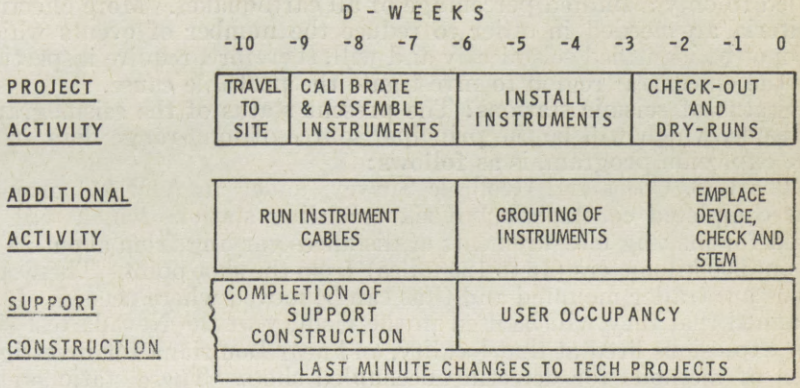
PLAN–TUNNEL

PROJECT 1.1 PARTICLE MOTION
PROJECT 1.6 PERMANENT DISPLACEMENT
PROJECT 1.7 REED GAGES
PROJECT 9.1 GROUTING SUPPORT

ORCHID TUNNEL MEASUREMENTS

CHART 9

FIELD WORK SCHEDULE



Chairman HOLIFIELD. Our next witness will be Dr. Carl Romney, of the Air Force Technical Applications Center.

Dr. Romney will discuss the seismic measurements and analysis requirements under the explosion program.

Doctor, how much time will you need?

STATEMENT OF DR. CARL ROMNEY,[1] ASSISTANT TECHNICAL DIRECTOR, AIR FORCE TECHNICAL APPLICATIONS CENTER

Dr. ROMNEY. I think approximately 15 minutes.

Chairman HOLIFIELD. Well, we are going to have to adjourn early today for some duties which some of the members have. So that is fine. Go right ahead.

Dr. ROMNEY. Perhaps I should preface my prepared remarks by reminding you again that the responsibilites of my organization on the shot program are principally concerned with the measurements and analysis of data taken at distances beyond a few miles from the shot point; I would say roughly from the boundary of the test site to greater distances.

In my testimony before this committee on the 22d of April 1960, I described a typical experimental program which would be conducted for measuring seismic effects from an explosion, and outlined the principal objectives of such a measurement program.

Briefly summarized, such a program is needed to define the characteristics of seismic waves generated from underground explosions, and to learn how these characteristics change when the size of the explosion, the conditions of emplacement, or the path between the explosion source and the recording station is changed. Such information is vital to the important task of developing identification criteria for definitely establishing whether a seismic event is of natural or explosive origin. To date, there are no effective criteria for distinguishing, with certainty, between earthquakes and explosions, al-

[1] See biographical sketch, p. 85.

though, when it is known that an earthquake occurred at great depth beneath the crust, or when it is accompanied by foreshocks and aftershocks, it can be eliminated from further consideration as a possible explosion. However, these criteria can, at the present time, be applied to only a limited percentage of all earthquakes. More effective criteria are needed in order to reduce the number of events which cannot be identified seismically and will, therefore, require inspection of the epicentral region to investigate the probable cause.

Status of seismic stations: The present status of the seismograph stations, which will be the principal source of long-range data from the explosion program, is as follows:

The U.S. Coast and Geodetic Survey, under an AFTAC project, has completed construction of six recording stations which will be used for making measurements at distances varying from about 25 to about 200 kilometers (15 to 125 miles) from the shot point. These stations are trailer mounted and thus can be moved where needed. It is planned that they will be used in the vicinity of the Nevada test site for explosions fired at that locality, and near Louisiana for investigation of methods for detecting decoupled shots. These stations employ instruments which record in the 1 to 100 cycle per second frequency range, which is the frequency range where the largest signals are recorded close to the shot point. For the first explosion, and possibly others, the Coast and Geodetic Survey stations will be supplemented by additional contractor stations of the University of Michigan and Texas Instruments, which will be arranged at about 25-kilometer intervals along a line running from ground zero toward the southeast, as shown in figure 1 (p. 174) to the prepared statement which you have.

Forty additional trailer-mounted stations are being constructed by the Geotechnical Corp., which will be used for making measurements at longer ranges. These stations will be disposed as shown on figure 2 (p. 175), for one of the 5-kiloton explosions planned at the Nevada test site. The stations will have equipment designed to record the lower frequency waves, which are the largest waves detectable at long distances. They will employ two sets of instruments: one type to record in the 1 to 10 cycle per second range, and a second type to record in the one-tenth to one one-hundredth cycle per second range. It is planned that about half of these stations will operate continuously at fixed sites during the entire explosion program, so that earthquakes will be recorded as well as explosions. The remaining stations will be moved during intervals between shots to distances in keeping with the size and emplacement conditions of succeeding shots, or to locations suitable for investigating significant new problems revealed by measurements on preceding shots. Most of these stations are now completed, and all 40 are scheduled for completion about the end of July 1961. An artist's conception of one of these stations in operation in the field is shown in figure 3 (p. 176). Each station requires an operating crew of two technicians.

If you would care to look at the third figure, you will see a van, which is the housing for the recording apparatus, the radio, the timing system, and auxiliary equipment; and then, leading out from the van, are the cables, which bring seismic information, in from an amplifier shelter, and the lines then run from the shelters to the various vaults, where the seismometers are in place. These vaults are cans

of approximately the size of a large garbage can buried in the ground, and covered with dirt to a depth of about 2 feet for thermal insulation.

Mr. RAMEY. How far apart are those placed?

Dr. ROMNEY. They will be within a very few feet of one another. Depending a little on the local circumstances, we will try to put each instrument on solid bedrock, and this may mean that they have to be separated by distances of a few tens of feet in order to find bedrock.

Arrangements have also been made to provide pertinent information on the explosion program, as it develops, to seismic prospecting crews, universities, or other volunteer groups. We expect that information obtained from such volunteer participants will be a valuable supplement to information obtained by the Government-operated field teams.

In addition to the seismic measurements just described, we will attempt to record any electromagnetic, telluric, or acoustic signals generated from selected nuclear explosions.

Theoretical studies, supported by measurements at a distance of a few miles from previous underground nuclear explosions, predict that electromagnetic waves are generated which may propagate for some unknown distance. If these signals can be detected beyond the immediate vicinity of the explosion, they could provide valuable supplementary information on the nature of this source. Several programs have been established to investigate this phenomena on the first shot.

Similarly, acoustic waves have been detected at distances as far as a few hundred kilometers from previous underground nuclear explosions. We plan to learn more about such signals, particularly for explosions in granite or other media where we have had no previous experience.

As outlined earlier in my statement, the valuable information to be derived from the analysis and evaluation of the seismic data recorded during this explosion program is the key objective and primary reason for such a program.

One of our chief problems is thus to provide for this analysis and evaluation. We have conducted exploratory investigations over the past year to define the size and nature of the problem, and believe the requirements are well understood. Our recommended solution, which has been submitted to the Advanced Research Projects Agency for approval, is that an analysis center should be established for processing the data from the explosion program, as well as from the stations of the prototype seismic detection system, which I described yesterday.

Chairman HOLIFIELD. Thank you, Dr. Romney.

You have proposed this analysis center of about 40 seismic teams, to be deployed across the entire United States. This center, as I understand it, would take care of all of the data which they accumulate. Is that right?

Dr. ROMNEY. Yes, sir.

Chairman HOLIFIELD. How long would it take to establish this analysis center, once it would be approved by the Congress?

Dr. ROMNEY. Depending in part on the tasks that you assign to the center, it could take from several months, like 4 or 5 months, up to a year, for the entire analysis job, as we see it might develop.

Chairman HOLIFIELD. If you do not have this analysis center, what position are you in?

Dr. ROMNEY. Well, the analysis center obviously must be provided somewhere within the program. There must be people trained to analyze and evaluate the data. There must be mathematical staff and computer facilities made available for the very complex analysis.

And I think that without this kind of a program, what we would learn would be very little in addition to what we already know. So it is an essential part of the program, and I believe it is well recognized by all of us that this must be done.

Chairman HOLIFIELD. Otherwise, this data would just have to be stored in some temporary place, and probably you would be lacking in the people to analyze it and coordinate it. Is that right?

Dr. ROMNEY. Yes, sir. And obviously, if we are to correct the program as we go along, if we are to learn from the first shot how to make better measurements and how to learn more on the second, we have to look at the data from the first shot before the second is fired.

Chairman HOLIFIELD. Do you have any funding at all for this function at the present time?

Dr. ROMNEY. There are funds provided for the analysis of the intermediate range measurements which I described. At the present time I do not believe we have funds for the evaluation of the data for the full program.

Chairman HOLIFIELD. It would seem to me that the long-range data would be equally important.

What percentage of earthquakes of a magnitude of 4.75 and above recorded by the Geneva control system can be eliminated from considerations as explosions on the basis of depth determination?

Dr. ROMNEY. This is a function of the area with which we are concerned. There are certain regions where a relatively high percentage could be eliminated, using present knowledge, and other parts of the earth where a small percentage could be eliminated, according to what we know now.

I believe I would agree with the remarks of Dr. Latter yesterday that probably a fairly high proportion of the earthquakes occur deeper than we could now place bombs. However, the information is not presently available as to how to exploit this characteristic of earthquakes fully. And this is one of our principal research objectives, to learn to establish the depths of relatively shallow earthquakes.

Chairman HOLIFIELD. Was it determined that this large explosion in the Kamchatka area last year was from a low depth, and therefore generally conceded to be an earthquake?

Dr. ROMNEY. I am a little uncertain as to which event you are talking about.

Mr. RAMEY. It was last fall.

Chairman HOLIFIELD. It was an unusually large one.

Dr. ROMNEY. Yes. There was an earthquake in the Kamchatka region along about October, which was subsequently shown to be deep. I have forgotten the exact depth, but I think it was greater than 20 kilometers.

Chairman HOLIFIELD. Mr. Ramey.

Mr. RAMEY. I would like to ask you the same question I asked Dr. Herbst. On the basis of the present accomplishments of the Vela Uniform program, how much do you think the threshold can be reduced below 4.75?

Dr. ROMNEY. Assuming no change in the numbers of stations, but allowing changes in the instrumentation? Would this be a fair assumption?

Mr. RAMEY. Right.

Dr. ROMNEY. Well, with the improvements that we now have in mind, I would expect that the threshold could be reduced by perhaps as much as a half magnitude.

Mr. RAMEY. So that would take it, then, down to——

Dr. ROMNEY. Oh, perhaps magnitude four and a quarter. I do not believe we have this capability in hand at the present time. I am making a guess that some of the things that now look promising will continue to look even more promising. And of course there is always the possibility of the breakthrough which could further reduce it.

Mr. RAMEY. What would that be the equivalent of in terms of a tamped shot? We talk about 4.75 being about 19 or 20 kilotons equivalent.

Dr. ROMNEY. Yes; I believe that would be around 3 kilotons.

Mr. RAMEY. That would take it down to 3. A half a magnitude?

Dr. ROMNEY. Yes, sir.

Mr. RAMEY. Have you made any calculations on, if your deep well system works out, what effect that would have?

Dr. ROMNEY. Considering the difficulties in going to great depths, I believe it is premature to make too optimistic a guess as to its effectiveness. And I had included that in my guess that we could reduce the threshold by a half magnitude.

Chairman HOLIFIELD. Thank you, Dr Romney.

Dr. Romney's testimony concludes this morning's session.

We will reconvene at 2 p.m. in this room.

(Dr. Romney's formal statement follows:)

PROGRAM OF UNDERGROUND NUCLEAR AND CHEMICAL EXPLOSIONS: MEASUREMENTS AND ANALYSIS

I. INTRODUCTION

In my testimony before this committee on April 22, 1960, I described a typical experimental program which would be conducted for measuring seismic effects from an explosion, and outlined the principal objectives of such a measurement program. Briefly summarized, such a program is needed to define the characteristics of seismic waves generated from underground explosions, and to learn how these characteristics change when the size of the explosion, the conditions of emplacement, or the path between the explosion source and the recording station is changed. Such information is vital to the important task of developing identification criteria for definitely establishing whether a seismic event is of natural or explosive origin. To date, there are no effective criteria for distinguishing, with certainty, between earthquakes and explosions, although when it is known that an earthquake occurred at great depth beneath the crust or when it is accompanied by foreshocks and aftershocks, it can be eliminated from further consideration as a possible explosion. However, these criteria can, at the present time, be applied to only a limited percentage of all earthquakes. More effective criteria are needed in order to reduce the number of events which cannot be identified seismically and will therefore require inspection of the epicentral region to investigate the probable cause.

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II. STATUS OF SEISMIC STATIONS

The present status of the seismograph stations, which will be the principal source of long range data from the explosion program, is as follows:

The U.S. Coast and Geodetic Survey, under an AFTAC project, has completed construction of six recording stations which will be used for making measurements at distances varying from 25 to 200 kilometers (15 to 125 miles) from the shot point. These stations are trailer mounted and thus can be moved where needed. It is planned that they will be used in the vicinity of the Nevada test site for explosions fired at that locality, and near Louisiana for investigation of methods for detecting decoupled shots. These stations employ instruments which record in the 1- to 100-cycle-per-second range, which is the frequency range where the largest signals are recorded close to the shot point. For the first explosion, and possibly others, the Coast and Geodetic Survey stations will be supplemented by additional contractor stations which will be arranged at about 25-kilometer intervals along a line running from ground zero toward the southeast, as shown in figure 1.

Forty additional trailer-mounted stations are being constructed by a private contracting firm, which will be used for making measurements at longer ranges. These stations will be disposed as shown on figure 2, for one of the 5-kiloton explosions planned at the Nevada test site. The stations will have equipment designed to record the lower frequency waves which are the largest waves detectable at long distances. They will employ two sets of instruments: one type to record in the 1- to 10-cycle-per-second range, and a second type to record in the one-tenth- to one-hundredth-cycle-per-second range. It is planned that about half these stations will operate continuously at fixed sites during the entire explosion program, so that earthquakes will be recorded as well as explosions. The remaining stations will be moved during intervals between shots to distances in keeping with the size and emplacement conditions of succeeding shots, or to locations suitable for investigating significant new problems revealed by measurements on preceding shots. Most of these stations are now completed, and all 40 are scheduled for completion about the end of July 1961. An artist's conception of one of these stations in operation in the field is shown in figure 3. Each station requires an operating crew of two technicians.

Arrangements have also been made to provide pertinent information on the explosion program, as it develops, to seismic prospecting crews, universities, or other volunteer groups. We expect that information obtained from such volunteer participants will be a valuable supplement to information obtained by the Government-operated field teams.

III. NONSEISMIC MEASUREMENTS

In addition to the seismic measurements just described, we will attempt to record any electromagnetic, telluric or acoustic signals generated from selected nuclear explosions. Theoretical studies, supported by measurements at a distance of a few miles from previous underground nuclear explosions, predict that electromagnetic waves are generated which may propagate for some unknown distance. If these signals can be detected beyond the immediate vicinity of the explosion, they could provide valuable supplementary information on the nature of the source. Several programs have been established to investigate this phenomena. Similarly, acoustic waves have been detected at distances as far as a few hundred kilometers from previous underground nuclear explosions. We plan to learn more about such signals, particularly for explosions in granite or other media where we have had no previous experience.

IV. ANALYSIS AND EVALUATION OF DATA

As outlined in my earlier statement, the valuable information to be derived from the analysis and evaluation of the seismic data recorded during this explosion program, is the key objective and primary reason for such a program. One of our chief problems is thus to provide for this analysis and evaluation. We have conducted exploratory investigations over the past year to define the size and nature of the problem, and believe the requirements are well understood. Our recommended solution, which has been submitted to the Advanced Research Projects Agency for approval, is that an analysis center should be established for processing the data from the explosion program, as well as from the stations of the prototype seismic detection system.

V. CONCLUSION

In summary, we have proceeded rather far toward solving the relatively easy measurement problems. We are confident that we have the know-how to proceed with the more difficult and time-consuming analytical problems. We believe the information which will result from the explosion program will fully justify the effort required.

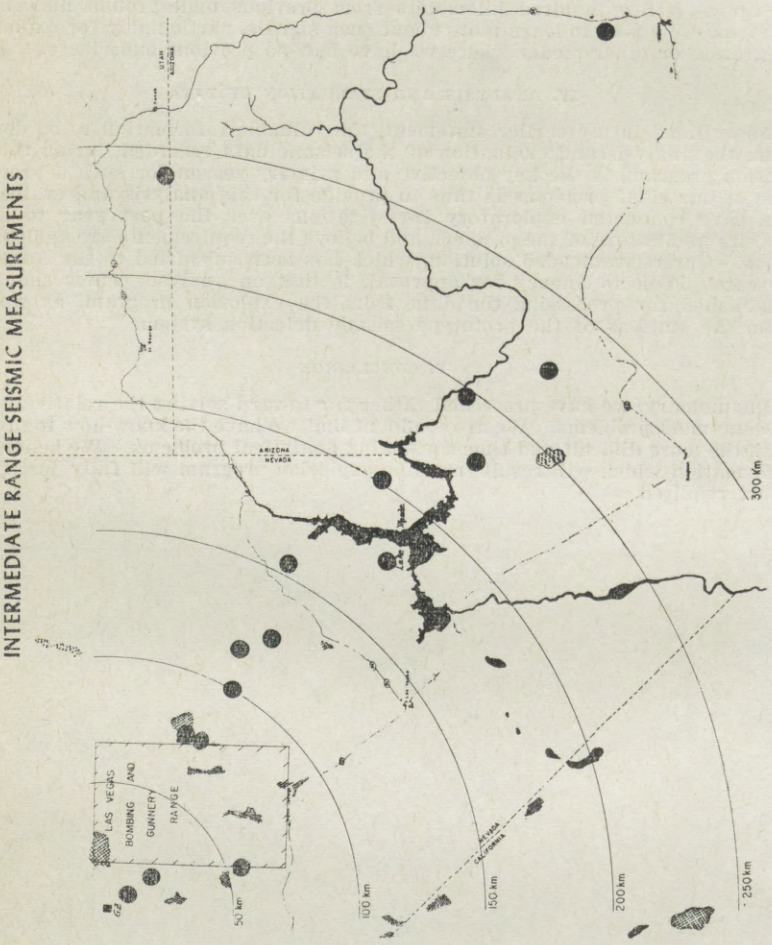

INTERMEDIATE RANGE SEISMIC MEASUREMENTS
ARIZONA
NEVADA
LAS VEGAS
BOMBING AND
GUNNERY
RANGE
50 km
100 km
150 km
200 km
250 km
300 km

FIGURE 1

FIGURE 2

VELA - UNIFORM
LONG RANGE SEISMIC MEASUREMENTS

ORCHID - PROJECT 8.4

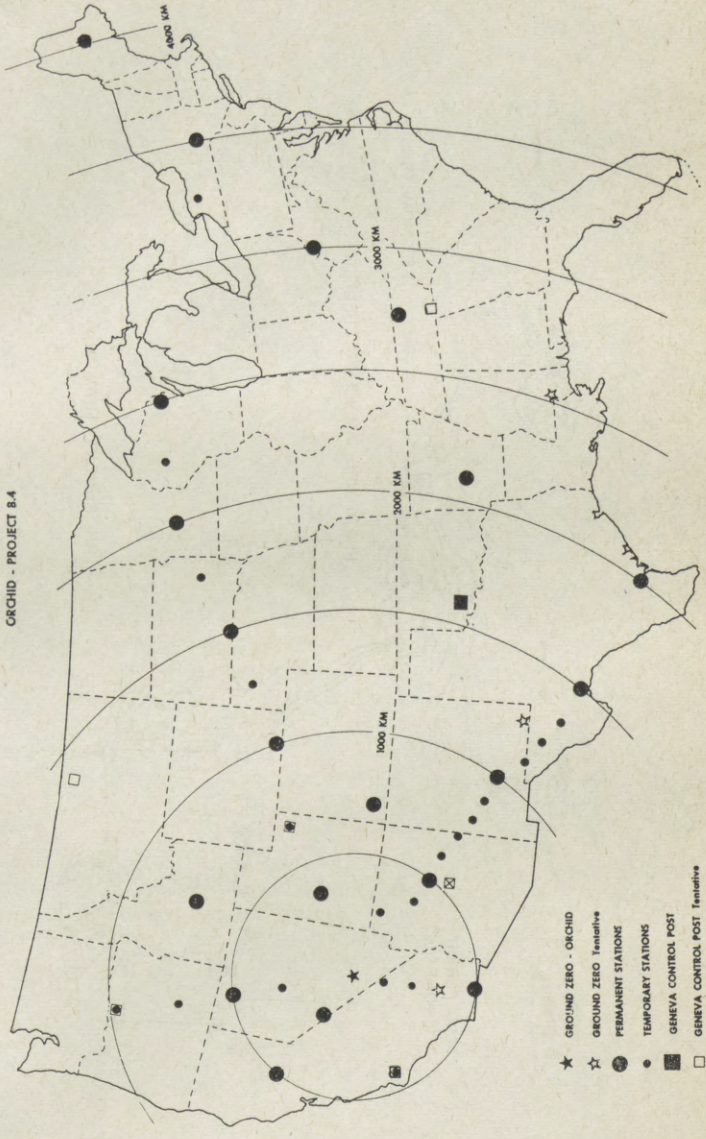
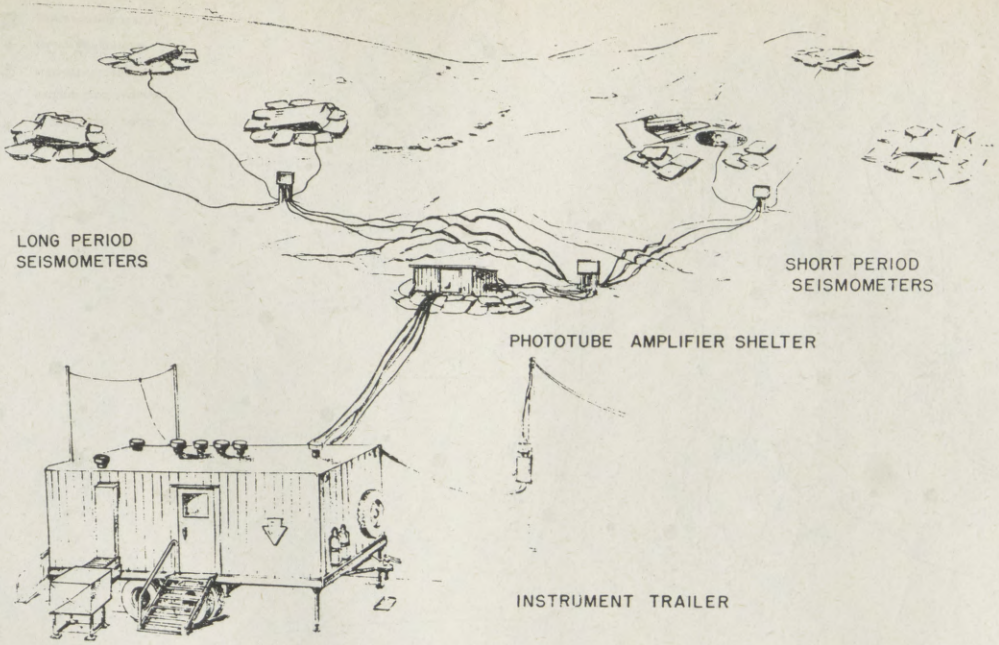


FIGURE 3



L.R.S.M. FIELD SITE LAYOUT

Chairman HOLIFIELD. Prior to a roundtable discussion on the results of seismic research by our panel of seismologists, we will receive testimony on onsite inspection.

The panel participants will be Dr. Dean S. Carder, Chief Seismologist, U.S. Coast and Geodetic Survey; Dr. Norman Haskell, Air Force Cambridge Research Laboratory; Dr. Carl Romney, Air Force Technical Applications Center; and Dr. James T. Wilson, chairman, Department of Geology, University of Michigan.

And I think we will ask Dr. Herbst to sit in, because there may be some related question on decoupling, if he will be available.

Following the panel discussion, the testimony on the Vela Sierra program will be presented. The Sierra portion of Project Vela is concerned with the detection of space tests from ground bases.

The meeting will now be adjourned until 2 p.m.

(Whereupon, at 11:31 a.m., the Joint Committee was recessed, to reconvene at 2 p.m., the same day.)

AFTERNOON SESSION

Chairman HOLIFIELD. The committee will be in order.

We open this afternoon's session with Dr. Charles Bates, of the Advanced Projects Research Agency as our first witness.

Dr. Bates will present testimony relating on onsite inspection, the final subject which will be considered under the Vela Uniform portion.

You may proceed, Dr. Bates.

STATEMENT OF DR. CHARLES C. BATES,[1] VELA UNIFORM PROGRAM MANAGER, ADVANCED RESEARCH PROJECTS AGENCY

Dr. BATES. Mr. Chairman and members of the Joint Committee, as overall program manager for Vela Uniform, it is a pleasure to discuss with you where the on-site inspection task now stands.

You will recall from Mr. Beyer's testimony the Air Force Technical Applications Center has the detailed technical responsibility for directing this effort.

Our basic premise is that the objective of an inspection group is to positively identify the cause of a "suspicious seismic event." To accomplish this, the inspection group will be faced with the problem of reducing the size of the initial inspection area from as much as 500 square kilometers to a size suitable for rigorous ground investigation including, where appropriate, drilling for a radioactive sample. Accordingly, one or more of the following three inspection phases might be used by the group, with the order of their use depending on the individual situation.

Phase I would involve the use of large-scale investigation techniques primarily suitable for use in a properly equipped aircraft. It is our hope that, by means of this inspection technique, the geographical area in which the suspicious event might have occurred can be reduced very quickly, so as to enable the inspection team to efficiently use the ground exploration techniques described below. It is our belief that sensors can be developed which would, in fact, allow this phase I program to perform its mission quickly and relatively inexpensively in most

[1] See biographical sketch, p. 67.

terrestrial regions. Our studies are aimed at finding the best combination of aerial sensors for identifying the disturbed area.

Initially, we did not know whether the airborne phase would offer much hope toward the solution of the basic time and cost problem involved in the operation of any inspection system. It now appears recent developments, which I shall describe below, may enable the phase I inspection team to quickly narrow down the area which the ground party must more closely investigate, particularly overland areas.

Phase II of our inspection system would consist of a series of rigorously applied geophysical inspection techniques of conventional and unconventional types aimed at pointing to the most likely area or areas for drilling or, for that matter, positively identifying the suspicious event as being nonnuclear in origin without the necessity for drilling. Since the very nature of the test ban agreement presupposes that the inspection team will normally find that the suspicious event is nonnuclear in origin and, moreover, since the event, if nonnuclear, is most likely to be either an earthquake or a chemical explosion, we have stressed in our program the development of techniques aimed at also meeting this portion of the requirement.

Phase III is the final phase in the inspection team's investigation; it consists of the actual drilling to the supposed explosion cavity when sufficient evidence has been collected to warrant it. It is assumed that for the purpose of establishing a test ban violation, an actual core sample containing fission products will be required.

Our presentation will now be directed at summarizing our technical progress to date, our technical plans for the future, and a short discussion of our planned utilization of the explosion program.

The technical program now underway consists of 20 research contracts and interagency agreements. The effort may be broken down into several parts:

1. In the area of overall studies, work has been completed on three major reports of interest, two by the Stanford Research Institute and one by the U.S. Bureau of Mines. Together they represent the first comprehensive and rigorous treatment of the total problem of onsite inspection from both the technical and logistics point of view and, as such, constitute a very useful addition to the state of our knowledge.

2. Any shock wave sufficiently large to be detected by the seismic net most probably should cause at least microscopic changes in the surface area at ground zero. This would include such disturbances as the occurrence of vegetation changes due to root damage resulting, in turn, in detectable changes in the reflectance characteristics of the vegetation. It is also expected that the shock wave may cause changes in the density of the soil which would result in a temperature difference around ground zero because of differential absorption of the sun's energy by the disturbed surface. Advantage is being taken of the U.S. Army Corps of Engineers' Engineer Research and Development Laboratory's (ERDL) related experience in mine detection where just such disturbances as we described above are used to physically outline minefields from the air. We have initated several projects with ERDL aimed at applying these and related airborne techniques to the on-site inspection problem. ERDL is currently engaged in

obtaining basic data to document these changes and investigating the equipment necessary to record them. Pending completion of this phase of the ERDL program, these techniques will be applied against actual test shots at the research explosion sites. The obvious potential value of these techniques cannot be overstressed for rapidly inspecting a large, suspicious ground area from an airplane and in reducing it to manageable size for the ground inspection party.

In addition, we plan to use aerial photography to very precisely document and measure physical changes around the ground zeroes of the planned research explosions. These techniques will take advantage of the latest advances in color photography, photogrammetry, and related stereophotographic techniques to measure precisely these disturbances at ground zero.

In order to enable the inspection to proceed expeditiously following a suspicious event, aerial photographic techniques utilizing the ultraviolet and infrared portions of the spectrum are also being investigated. By these means, we hope to develop equipment and techniques that will be useful over cloud-covered areas or at night.

3. Seismic listening techniques are being developed for distinguishing between natural earthquake aftershocks and those from explosions. Aftershocks from small earthquakes may continue for considerable periods after the main event whereas those resulting from explosions probably will die out in hours or at most a few days. Thus, a team could enter an area some 5 days or a week after an event takes place and listen for these disturbances using small seismometers. If some are detected, they may indicate whether the event was natural and may also pinpoint the location and depth of the focus. Much of the basic data needed for intelligent use of this technique have been hitherto unavailable. This means we must develop fundamental data on explosion aftershocks and natural earthquake aftershocks in different environments wherever possible and over considerable periods of time.

Some contract work along this line has already started with the Stanford Research Institute in studying aftershocks of small earthquakes in the San Francisco area. Preliminary indications are that small quakes, i.e., those of magnitudes 4–5.5, do have aftershocks that may extend for 2 weeks. The location of these shocks are often difficult to determine and may be some distance removed from the main epicenter. Plans for studying aftershocks at the major explosions of the research explosion series are also complete.

4. Development of seismic exploration techniques for detecting cavities resulting from underground explosions is well underway. Under a Vela Uniform contract, the United Electrodynamics Corp. has utilized seismic techniques at the Nevada test site where a few such cavities already exist and others will be created during the planned research explosion series. Although the data have not been fully analyzed, it can be stated that some explosion effects were detected. In particular, it was noted that directly over the cavities, sound velocities in the surface rocks were reduced compared to normal. However, some of this reduction may have been caused by fractures that existed prior to the explosion, for we unfortunately do not have a pre-explosion survey for comparison. Surveys at future explosions will be very helpful since preshot data for comparison will then be available.

We also plan to develop scanning techniques using geophone arrays and improved energy sources for such techniques may allow us to use seismic methods over larger areas than are now possible.

5. Normal tools of the geophysical exploration trade, particularly electrical, magnetic (magnetometer) and gravity surveys, are being evaluated as inspection tools. Because the typical commercial geophysical crew is searching for nonhomogeneous zones underground of substantial size, the application of some of these techniques to outlining a relatively small underground cavity or crushed zone at depth is not always as direct as cursory examination might indicate. As a result, we are carefully reviewing each of these tools to determine some order of priority before granting contracts for field tests and evaluations.

6. Research concerned with the identification of surface disturbances uniquely associated with underground nuclear explosions has also been initiated. In this connection, it should be mentioned that one way of looking at the inspection problem would be to include a capability which would permit searching for surface environmental discontinuities in the suspicious area and then directly categorizing such discontinuities as to source, i.e., nuclear or nonnuclear. It is our hope that, if successful, this approach could eliminate the need for widespread, expensive and time-consuming drilling operations in many cases. Obviously, a concentration of surface radioactivity above the normal background level would be such an indication. Another might be the presence of certain gases in the soil in quantities or in locations not normal to the geography. Recent observations by the U.S. Geological Survey that certain mechanically shocked mineral specimens show enhanced fluorescence also suggest that such changes in luminescence may offer another possible mechanism for location of underground explosions. Consequently, a research program with the U.S. Geological Survey has been funded, aimed at a better understanding of this phenomena, and the evaluation of its utility in the inspection program. The possibility that the plant and microorganism ecology might be uniquely disturbed is also being discussed with several organizations experienced in this field. The latter phenomena may also be of value against off-shore, i.e., underground-underwater, nuclear events where some of the other more conventional techniques may not be usable.

A large part of the on-site inspection effort is, of course, tied to the research explosion series. At the explosions, we hope to try many of the techniques previously discussed. Although the planned nuclear explosion series will not be a strictly realistic test of onsite inspection techniques, we should be able to obtain a great deal of useful data, for several will be at considerably deeper depths than anything previously detonated. As a consequence, they will provide more realistic data on surface effects and vegetation damage and will produce more difficult target cavities for seismic exploration techniques. Some of the techniques at the salt dome explosions of the Dribble series will also be tested in order to obtain experience with large cavities and in an environment substantially different from the Nevada test site.

This concludes my statement. I would be pleased to answer any questions.

Chairman HOLIFIELD. The Chair might say that during the negotiations in Geneva, the Soviets have refused to seriously consider this subject to on-site inspection within the boundaries of the Soviet Union.

This is another one of the proposals, of course, upon which we have had very little, if any, progress.

The technical difficulties, I think, are evidenced by the fact that, as I understand it, in one of the shots out in Nevada, where we knew exactly where the shot occurred underground, we tried to drill into it and missed it. Is that not true?

Dr. BATES. Yes, sir. Since then, however, we have been looking at some of the surface techniques for measuring radioactive and geochemical backgrounds on a micro-scale. Let us say they hold considerable promise. Our trouble right now is that the Nevada Test Site is too cluttered up with radioactivity on the surface to see how good we can get. But if you remember, that was quite a chimney that formed over the cavity.

Chairman HOLIFIELD. But you knew the exact location of it, and it was just a matter of mathematical computation, I would think, to get the drilling equipment over the point of explosion. And as I remember, the drilling missed the contaminated underground area.

Dr. BATES. Well, I think we are going to have to wait to get some new results to see whether we need to go right down to the cavity. We may be able to get enough radiochemistry indications at a higher level to not have to penetrate the actual cavity.

Chairman HOLIFIELD. If there is any solution outside of drilling, of course, it would certainly be helpful.

Mr. RAMEY. I notice that you mention on page 3 of your prepared statement, studies by the Bureau of Mines and the Stanford Research Institute. I think we would like to have copies of those.

(The reports referred to are on file with the Joint Committee.)

Dr. BATES. All right, sir. I have one of the reports. I can get the other two to you tomorrow, if you would like.

Mr. RAMEY. Do you know whether any of the techniques that you discuss, on on-site inspection, have been considered at all in the Geneva negotiations with the Russians?

Dr. BATES. I cannot say. I have never been to any of the Geneva discussions.

In the general material on the Geneva discussions, I have not seen much detail on how the survey should be carried out.

Chairman HOLIFIELD. I notice, in going through the list of contracts and the dates, that there was very little done in 1960. There were only six projects. It looks like you have accelerated the program of contracts considerably in the last few months. Most of the contracts, I believe, are dated in 1961. So we are hopeful that you are pushing ahead on this as fast as you can in relation to the talents that you have to employ in this field.

Thank you, Dr. Bates.

(Dr. Bates' formal statement follows:)

STATEMENT BY DR. CHARLES C. BATES, VELA UNIFORM PROGRAM MANAGER, ADVANCED RESEARCH PROJECTS AGENCY

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spection group will be faced with the problem of reducing the size of the initial inspection area from as much as 500 square kilometers to a size suitable for rigorous ground investigation including, where appropriate, drilling for a radioactive sample. Accordingly, one or more of the following three inspection phases might be used by the group, with the order of their use depending on the individual situation.

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Initially, we did not know whether the airborne phase would offer much hope toward the solution of the basic time and cost problem involved in the operation of any inspection system. It now appears recent developments, which I shall describe below, may enable the phase I inspection team to quickly narrow down the area which the ground party must clearly investigate, particularly over land areas.

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Our presentation will now be directed at summarizing our technical progress to date, our technical plans for the future, and a short discussion of our planned utilization of the explosion program.

The technical program now underway consists of 21 research contracts and interagency agreements. The effort may be broken down into several parts:

1. In the area of overall studies, work has been completed on three major reports of interest, two by the Stanford Research Institute and one by the U.S. Bureau of Mines. Together they represent the first comprehensive and rigorous treatment of the total problem of onsite inspection from both the technical and logistics point of view and, as such, constitute a very useful addition to the state of our knowledge.

2. Any shock sufficiently large to be detected by the seismic net more probably should cause at least microscopic changes in the surface area at ground zero. This would include such disturbances as the occurrence of vegetation changes due to root damage resulting, in turn, in detectable changes in the reflectance characteristics of the vegetation. It is also expected that the shock wave may cause changes in the density of the soil which would result in a temperature difference around ground zero because of differential absorption of the sun's energy by the disturbed surface. Advantage is being taken of the U.S. Army Corp of Engineers Engineer Research and Development Laboratory's (ERDL) related experience in the mine detection field where just such disturbances as we described above are used to physically outline mine fields from the air. We have initiated several projects with ERDL aimed at applying these and related airborne techniques to the onsite inspection problem. ERDL is currently engaged in obtaining basic data to document these changes and investigating the equipment necessary to record them. Pending completion of this phase of the ERDL program, these techniques will be applied against actual test shots at the research explosion sites. The obvious potential value of these techniques cannot be overstressed for rapidly inspecting a large, suspicious ground area from an airplane and in reducing it to manageable size for the ground inspection party.

In addition, we plan to use aerial photography to very precisely document and measure physical changes around the ground zeroes of the planned research explosions. These techniques will take advantage of the latest advances in color photography, photogrammetry, and related sterophotographic techniques to measure precisely these disturbances at ground zero.

In order to enable the inspection to proceed expeditiously following a suspicious event, aerial photographic techniques utilizing the ultraviolet and infrared portions of the spectrum are also being investigated. By these means, we hope to develop equipment and techniques that will be useful over cloud-covered areas or at night.

3. Seismic listening techniques are being developed for distinguishing between natural earthquake aftershocks and those from explosions. Aftershocks from small earthquakes may continue for considerable periods after the main event whereas those resulting from explosions probably will die out in hours or at most a few days. Thus, a team could enter an area some 5 days or a week after an event takes place and listen for these disturbances using small seismometers. If some are detected, they may indicate whether the event was natural and may also pinpoint the location and depth of the focus. Much of the basic data needed for intelligent use of this technique have been hitherto unavailable. This means we must develop fundamental data on explosion aftershocks and natural earthquake aftershocks in different environments wherever possible and over considerable periods of time. Some contract work along this line has already started with the Standard Research Institute in studying aftershocks of small earthquakes in the San Francisco area. Preliminary indications are that small quakes, i.e., those of magnitudes 4–5.5, do have aftershocks that may extend for 2 weeks. The location of these shocks are often difficult to determine and may be some distance removed from the main epicenter. Plans for studying aftershocks at the major explosions of the research explosion series are also complete.

4. Development of seismic exploration techniques for detecting cavities resulting from underground explosions is well underway. Under a Vela Uniform contract, the United Electrodynamics Corp. has utilized seismic techniques at the Nevada test site where a few such cavities already exist and others will be created during the planned research explosion series. Although the data have not been fully analyzed, it can be stated that some explosion effects were detected. In particular, it was noted that directly over the cavities, sound velocities in the surface rocks were reduced compared to normal. However, some of this reduction may have been caused by fractures that existed prior to the explosion, for we unfortunately do not have a preexplosion survey for comparison. Surveys at future explosions will be very helpful since preshot data for comparison will then be available. We also plan to develop scanning techniques using geophone arrays and improved energy sources for such techniques may allow us to use seismic methods over larger areas than are now possible.

5. Normal tools of the geophysical exploration trade, particularly electrical, magnetic (magnetometer), and gravity surveys, are being evaluated as inspection tools. Because the typical commercial geophysical crew is searching for nonhomogeneous zones underground of substantial size, the application of some of these techniques to outlining a relatively small underground cavity or crushed zone at depth is not always as direct as cursory examination might indicate. As a result, we are carefully reviewing each of these tools to determine some order of priority before granting contracts for field tests and evaluations.

6. Research concerned with the identification of surface disturbances uniquely associated with underground nuclear explosions has also been initiated. In this connection, it should be mentioned that one way of looking at the inspection problem would be to include a capability which would permit searching for surface environmental discontinuities in the suspicious area and then directly categorizing such discontinuities as to source, i.e., nuclear or nonnuclear. It is our hope that, if successful, this approach could eliminate the need for widespread, expensive, and time-consuming drilling operations in many cases. Obviously, a concentration of surface radioactivity above the normal background level would be such an indication. Another might be the presence of certain gases in the soil in quantities or in locations not normal to the geography. Recent observations by the U.S. Geological Survey that certain mechanically shocked mineral specimens show enhanced fluorescence also suggest that such changes in luminescence may offer another possible mechanism for location of

underground explosions. Consequently, a research program with the U.S. Geological Survey has been funded aimed at a better understanding of this phenomena and the evaluation of its utility in the inspection program. The possibility that the plant and microorganism ecology might be uniquely disturbed is also being discussed with several organizations experienced in this field. This latter phenomena may also be of value against offshore, i.e., underground-underwater, nuclear events where some of the other more conventional techniques may not be usable.

A large part of the onsite inspection effort is, of course, tied to the research explosion series. At the explosions, we hope to try many of the techniques previously discussed. Although the planned nuclear explosion series will not be a strictly realistic test of onsite inspection techniques, we should be able to obtain a great deal of useful data, for several will be at considerably deeper depths than anything previously detonated. As a consequence, they will provide more realistic data on surface effects and vegetation damage and will produce more difficult target cavities for seismic exploration techniques. Some of the techniques at the salt dome explosions of the Dribble series will also be tested in order to obtain experience with large cavities and in an environment substantially different from the Nevada test site.

This concludes my statement. I would be pleased to answer any questions.

Chairman HOLIFIELD. At this time, we would like to set up the panel, and would like to have the following individuals come forward to the witness table to participate in a roundtable discussion on the results of seismic research to date: Dr. Dean S. Carder,[2] chief seismologist, U.S. Coast and Geodetic Survey; Dr. Norman Haskell,[3] Air Force Cambridge Research Laboratory; Dr. Carl Romney,[4] Air Force Technical Applications Center; Dr. James T. Wilson,[5] Professor of geology and associate director, Institute of Science and Technology, University of Michigan; Dr. Hugo Benioff,[6] seismological laboratory, California Institute of Technology.

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[2] U.S. Coast & Geodetic Survey, Washington, D.C. Seismology. Medford, Oreg., Aug. 11, 97; m 23, 42; c. 4. B.S., Oregon State College, 21; M.S. Idaho, 25; Ph.D (seismol), California, 33. Instr. geol, Utah State College, 21–24; asst, Idaho 24–25; prin, high sch, Wash. 25–31; asst. geol, California 31–33; asst. seismologist, U.S. Coast & Geod. Surv, San Francisco, 33–39, chief, Lake Mead Seismol. Surv, Nev. 39–46, seismologist, 46–53, chief, seismol br, U.S. Coast and Geodetic Survey, 53–57, Chief Seismologist, 57– Del, Int. Union Geodesy & Geophysics 57; mem, seismol & gravity panel; Int. Geophys. Year; U.S. mem, seismol. cmt, Pan Am. Inst. Geog. & Hist. Consult, Snowy Mt. Hydroelec. Authority, New South Wales & Tasmania Hydroelec. Cmn. Australia, 58; Seis. Panel, USAF 1961; Colbert medal, Am. Soc. Mil. Eng, 56. S.A.T.C, 17–19. A.A; fel. Geol. Soc; Seismol. Soc; Geophys. Union; Pres. Seis. Soc. Am. 1961. Vibrations of structures; vibrations from explosions; major earth structure from seismic wave propagation; influence of reservoir loading on local earthquakes; origin and transmission of microseisms.

[3] Geophysics. Alton, Ill., June 30, 05; m. 33; c. 2. S.B. Harvard, 27. M.A., 35, Ph. D. (geol), 36. Observer, Rieber Lab, Calif, 36–37; computer, West. Geophys. Co, 37–38, chief computer, 38–39, res. seismologist, 39–41; physicist, div. war res, Columbia, 41–42; Nat. Defense Res. Cmt. contract. Calif. Inst. Tech. 42–45; chief geophysicist, U.S. Smelting, Ref. & Mining Co., Mass. 46–48; Physicist, Air Force Cambridge Res. Center, 48– Assoc, U.S. Naval Inst, 42– A.A.; Phys. Soc; Soc. Explor. Geophys; Seismol. Soc; fel. Geog. Soc; Asn. Petrol. Geol; Geophys. Union. Tectonophysics; seismology; geophysical exploration; underwater ballistics; mechanics of deformation of the granitic rocks.

[4] See biographical sketch, p. 85.

[5] Associate director, Institute of Science and Technology, University of Michigan, Ann Arbor, Mich.

Place of birth: Claremont, Calif.

Education: A.B., University of California, 1935; Ph.D. (seismology), University of California, 1939.

Employment: Teaching assistant, geology, University of California, 1935–39; instructor, geology, University of Michigan, 1940–43; section supervisor, Airborne Instruments Laboratory, Michigan, 1940–43; New York, 1943–45; instructor, later professor of geology and chairman of Geology Department, University of Michigan, 1945–.

Related professional experience: Chairman, Committee on Seismological Stations, National Academy of Sciences; Chairman, Air Force Office of Scientific Research Geophysics Advisory Panel.

[6] California Institute of Technology, Pasadena 2, Calif. Geophysics, Los Angeles, Calif, Sept. 14, 99; m. 28; c. 3; m. 53; c. 1. A.B, Pomona Col, 21; Calif. Inst. Tech, 21–22, 24, Ph.D. (seismol), 35. Asst, Lick Observ, California, 23–24; asst. physicist, Carnegie Institution, 24–37; asst. prof. Seismol, Calif. Inst. Tech, 37–38, assoc. prof. 38–50, Prof, 50– Res. engr, Submarine Signal Co, Boston, 39–46; res. consult, Baldwin Piano Co, Cincinnati, 46– Arthur L. Day award, Geol. Soc. With A.F; U.S. Coast and Geodetic Surv, 44. Nat. Acad; A.A; Seismol. Soc; Acoustical Soc; Phys. Soc; Geol. Soc; Geophys. Union; Royal Astron. Soc. Seismograph design; linear strain seismograph; seismic destructiveness; mecchanism of earthquakes; orogenesis; crustal structure; tectonics; seismic waves; musical instrument development.

Could you gentlemen come forward to the table?

Dr. Benioff, we would like to express our appreciation for the effort expended in getting here today. We understand that you had almost succeeded in losing yourself in the wilds of the northwest woods on a much deserved vacation, and we are glad that you finally got our message and came in.

I would like to also invite Dr. Herbst [7] of the Livermore Laboratory to participate in the panel discussion.

Dr. Herbst, we know you are not a seismologist, but if we get into the subject of decoupling, perhaps you can help us some.

We have had testimony during these hearings on two matters of detection, the array of surface seismographs and the deep hole seismographs.

Would you gentlemen for a moment discuss the advantages, if there be any, of either kind, and the disadvantages of either kind?

We will start off over on the left with Dr. Romney.

Dr. Romney?

PANEL DISCUSSION OF THE RESULTS OF SEISMIC RESEARCH TO DATE

Dr. ROMNEY. Yes, sir. Well, as you know, we have completed preliminary studies on the use of very large arrays. So far, our experience has been limited to arrays of up to 20 detectors. This work was done in the spring of 1959. We found, in our previous work, that we could accomplish an improvement in signal-to-noise ratio approximately proportional to the square root of the number of detectors. This is an improvement of about 4½ from 20 detectors.

In our more recent experience, with the seismological station at Fort Sill, we had an array of 10 detectors, and again we achieved an improvement of roughly 3, which is about the square root of 10, the number of detectors in that array.

The more recent theoretical work done for us by Texas Instruments indicates that further improvements are possible beyond those which we have achieved experimentally, and these improvements would be accomplished in part by more elaborate filtering processes in association with the array, and in part by computational techniques.

In the case of the deep hole, we had experience several years ago in operating detectors as deep as 10,000 feet. The results were rather inconclusive. This was the result of an instrumentation limitation, which did not allow us to measure the noise at the bottom of the hole.

Our more recent work, completed last summer, however, indicated that the noise at depths of a few thousand feet behaves as if it were largely Rayleigh waves, and if this situation proves to be true in general, we can also predict the improvement expected from deeper wells.

Chairman HOLIFIELD. This was a test of a single deep-hole seismograph?

Dr. ROMNEY. That is correct. The most recent work used a single deep-hole seismograph.

Chairman HOLIFIELD. You have never gotten to the point where you could have any kind of an array above the deep hole?

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[7] See biographical sketch, p. 145.

Dr. ROMNEY. No, sir, we have not.

Chairman HOLIFIELD. Would there be any value to that?

Dr. ROMNEY. In principle, there would be an advantage. Of course, the cost would incerase enormously if the holes were very deep. We do think there might be some advantage from a shallow array. That would be where the instruments are placed on the order of a hundred feet deep.

Chairman HOLIFIELD. Would it be proper for me to say that in combating this background noise, the problem is very much the same as we are having today with the microphone?

Dr. ROMNEY. Yes, indeed. I think in theory the deep-hole technique is capable of giving greater improvements than a practical surface array; or let us at least say it is capable of greater improvement than an array costing the same as the deep well.

I am not sure that we can attain the maximum improvements that the theory indicates, as a practical matter.

The advantages and disadvantages that I can see, based on what we now know, are that there probably will be places where we are limited in the amount of land available, where very large arrays are impractical. In this case, a deep well seismometer may very well be the kind of instrument that you would use for improving the signal-to-noise ratio.

Chairman HOLIFIELD. When we speak of 10 arrays, how much territory does this cover?

Dr. ROMNEY. The kind of arrays that we have been talking about cover an area of about 3 miles square, 3 miles on a side.

Chairman HOLIFIELD. For 10 instruments?

Dr. ROMNEY. I am sorry. That would be for a larger one. For 10 instruments, we consider these should be installed in an area of about 2 miles on a side.

Now, there may be areas where difficulties exist in drilling, in which case the surface array may be more useful. I believe there will be problems in keeping a large array in operation, simply because of the number of instrumental components and the wire lines which have to be maintained, and so on. I think probably both techniques will turn out to have application in the system.

This is kind of a summary, I guess. In theory, at least, the deep hole technique probably has more capability.

Chairman HOLIFIELD. Dr. Haskell, do you have any additional comment that you wish to make?

Dr. HASKELL. No, I have no firsthand experience with the deep hole work. All I know about it is what I have read, and I would agree with Dr. Romney.

Representative BATES. Mr. Chairman, it is impossible for me to hear these gentlemen. Maybe without the microphone they would do better.

Dr. HASKELL. I would agree with Dr. Romney that on the basis of the evidence we have now, the deep hole technique is probably the most important development that has come out of the research program to date, and probably more promising than the large array in areas where it is applicable.

Chairman HOLIFIELD. Dr. Wilson?

Dr. WILSON. Mr. Chairman, I think most of this has been covered.

Just one additional comment. One conceivably might be forced to put stations in fairly noisy areas, and in some of these cases, a

very large array would probably be impractical, because if you made the array big enough to make any real gain in signal-to-noise, you would begin to get parts of your array in one type of noise and another part in another type, and so on. And facing up to the practical aspects of maybe having put stations without too much control, I mean, real scientific choice in where you put them, the deep hole will probably go a lot further toward giving you a quiet station, if you have no free choice in where you are going to put it. And this, I think, is one of the hopeful aspects of the program, at the moment.

Chairman HOLIFIELD. Dr. Carder?

Dr. CARDER. I have nothing to add. I have had no experience in either, except some initial experience at the bottom of a deep mine.

However, I will say this: Even in an industrial area, the fuzz or the high frequency noise can be eliminated by operating seismometers in mines of moderate depth, a few thousand feet.

Let us suppose that the area is in a sedimentary basin. If the mine is below sedimentary rock or alluvium and in solid rock, you probably can get rid of most of the industrial noise. However, you cannot get rid of the 1-second type of signal that we are examining, unless you go to considerable depth.

I also wish to emphasize the need for exploring the possibility of operating seismometers on the bottom of the ocean. This is really a program that might have great future benefits.

Chairman HOLIFIELD. Do you have a question, Mr. Northrup?

Mr. NORTHRUP. Dr. Carder, I know that your staff at one time was doing some theoretical work on large arrays, and at the time it seemed rather promising. I remember hearing of factors of a hundred improvement from a hundred element array. I wondered if you had carried that work on any further, and what the result of it is to this time.

Dr. CARDER. We have not carried it any further during the past year. That type of array was developed by Mr. Bailey, who was a member of our staff. It showed considerable promise, provided the instrumentation was as nearly uniform as possible and the spacing was such that there was heavy weighting toward the center of the array.

His idea was to have a system of not 10 or 20 or 30, but several hundred elements in a space of a few miles square, and to have them distributed in a mathematical pattern. With such arrays, he claimed a reduction of 100 to 1 or better, much better than the square root of the number of elements.

I believe such arrays have been studied by other organizations, for example, United Electrodynamics and Texas Instruments. They have found an improvement for the first few elements, but the improvement tapered off with the large number of elements.

If this method can be perfected and if the required geological formations can be found (northwest of Austin, Tex., is suggested) I recommend field testing this array theory.

Chairman HOLIFIELD. Then in the setting up of these sites, you have to be very particular where you put them, as far as geology is concerned?

Dr. CARDER. Yes; it has to be in a relatively flat area with low relief, and on hard granite rock. These geological conditions are difficult to find.

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Chairman HOLIFIELD. This might be all right for testing, but the practical point occurs to me immediately that if you are going to have to search in an opponent's country for this type of location, you are going to pretty well have to have free access to all parts of his country in order to find out the desirable place to put your stations.

Is that not true?

Dr. CARDER. Well, that will be true. However, much of the opponent's country, I have heard, is similar to the Canadian shield area. The Canadian shield area is quite suitable for this type of an array.

Chairman HOLIFIELD. All over Canada?

Dr. CARDER. The northeastern part of Canada, or large parts of it where there are extensive exposures of granite which have been planed down by past glacial action. These areas would appear to be a suitable place for the array.

Chairman HOLIFIELD. Dr. Benioff?

Dr. BENIOFF. I just wanted to point out that to be useful, most of these arrays ought to be situated within the area of our opponent, and unfortunately, if I were he, I would be firing off these bombs in areas such as the coastal area of Kamchatka, in which we not only have many earthquakes, but we have volcanic earthquakes, which cannot be distinguished from nuclear bombs by any method we now know, and in which this same area is one in which the uniformity that is required, the homogeneity which is required, by the array method, breaks down.

So that I am not very sanguine as to the ultimate value, except for a few long distance stations that can be placed in areas such as the Canadian shield, and a few areas of the United States.

The deep hole seismograph offers considerable advantage from the theoretical point of view, but there, again, it is offset by the great difficulty of maintaining continuous operation at the depth. The temperatures are high and the pressures are high, and usually cables only last a few months or a year at a time.

So that if you set up a group of these stations, the probability that they are all working at any one time is not great.

At best, either one of them has serious drawbacks.

Dr. CARDER. And I would like to add a further statement in regard to the distance. It is not really necessary if the arrays work like they say, to go into the enemy territory. Two or three stations set off at a limited distance can view some of these events quite favorably. We can view an event of this type from a distance of twenty or twenty-five hundred kilometers better than we can at ten or fifteen hundred, because of a shadow zone which interferes. Therefore, it is not necessary to go into enemy territory to operate a good station.

Dr. BENIOFF. But you do not get the precision you want at that great distance. You would not be able to locate it with sufficient precision to be of any help in going into the region and finding it. You have to be very much closer.

Dr. HASKELL. I think if it is going to increase the capability of the Geneva network as a whole, it is going to have to be at every one of the Geneva stations.

Dr. BENIOFF. Right.

Chairman HOLIFIELD. The Geneva network is predicated on a checkerboard arrangement about 600 miles apart.

Dr. BENIOFF. Yes. And that is just the difficulty. You will not be able to find an area in the region in which the opponent is likely to fire in which you can set up an effective set of arrays of this kind. The area in which he is most likely to fire is just one which is poor, geologically speaking, in uniformity.

Chairman HOLIFIELD. So we can sum up the situation by saying that at the present state of technology, we do need to have as many stations as were heretofore designated, in order to have them checkerboarded at areas in enough diffusion to obtain adequate signals. And then on top of that, you would have to have the right kind of soil to set them on, in order to get accurate and uniform readings. Is that right?

Dr. BENIOFF. That is generally right. And you cannot expect too much from it, even then.

Chairman HOLIFIELD. Now, are you speaking both as to deep hole and to seismological arrays?

Dr. BENIOFF. Yes. Because of the fact that in the regions that are likely to be used, there will be volcanic earthquakes. They will be on coastal areas, in which you cannot get arrays situated in all directions around the source.

Most of our thinking here has been a sort of a laboratory type, in which we get into the middle of a continent and put our stations all around it. But I do not think that is where the problem is going to lie. It is going to lie with the firing of the shots, let us say, in a coastal area, such as Kamchatka, in which we have volcanic earthquakes, which simulate explosive shocks that we can produce with atomic energy, and which have many other earthquakes, as well. So that the probability of concealment can be fairly high, by virtue of the fact that we cannot make proper networks, and we have got other conflicting sources of a natural kind.

Chairman HOLIFIELD. Does any member of the panel wish to take a more optimistic viewpoint on Dr. Benioff's pessimistic viewpoint?

If not, we will take up the point that you just brought up, the earthquakes occurring in the Kamchatka area, and the problem of firing simultaneously with an earthquake in order to confuse the recording devices.

Now, we will start at this end, this time, now.

No. 1, is it possible for them to synchronize a test with an earthquake, and if so, how?

Dr. BENIOFF. Well, I am not an expert on the explosives side. I do not know precisely how they can control the time for a nuclear shot. But if that can be done, that is the way I would do it. I would sit in an area such as Kamchatka, which has very many earthquakes, and I would fire when a fairly large earthquake occurs. It would be a very simple matter to set up a seismograph which is sensitive to that. And that will practically blanket the signal all over the earth. It would be very difficult to pick it up.

There are areas, for instance, in the Hindukush in Asia, in which there have been many earthquakes per year occurring over a number of years, swarms of earthquakes. One could set up a plant there and wait for one of these shocks to occur, and fire off.

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Chairman HOLIFIELD. From a mechanical standpoint, could you coordinate the earthquake with the setting off of the test?

Dr. BENIOFF. Well, I could provide you with a signal that says, "Now is the time to fire," from a seismograph response. Whether or not the people who control the firing can set up a nuclear blast in such a manner, I do not know. That is out of my range. But if it can be done, from that point of view, we can certainly provide them with a signal.

Chairman HOLIFIELD. Well, now, what do you think about this, Dr. Carder?

Dr. CARDER. I agree with Dr. Benioff. I will say that the Russians have a big advantage over us, because Kamchatka is a wonderful place to do work of that kind. Of course that type of thing has its disadvantages, for example, a large organization is required to monitor the sensing equipment and compile the data.

Chairman HOLIFIELD. Dr. Haskell?

Dr. HASKELL. No; I have no further comments.

Chairman HOLIFIELD. Dr. Romney?

Dr. ROMNEY. Yes. I agree with Dr. Benioff.

Mr. RAMEY. I gathered from the testimony, I believe, this morning, that what happens after your initial earthquake tremor is that you get a longer series of followup tremors from an earthquake than you do from an explosion. Is that correct?

Dr. ROMNEY. You mean aftershocks?

Mr. RAMEY. Yes. So that you could mask, therefore, your explosion by not only your initial one, but your aftershock.

Dr. ROMNEY. Well, I believe that is part of what has been suggested, that these major earthquakes are usually followed by large numbers of smaller shocks.

Mr. RAMEY. But the smaller shocks would still register above 4.75?

Dr. ROMNEY. They could well, depending on the size of the main shock.

Dr. BENIOFF. In Palomar, we registered earthquake aftershocks, following a 7.5 magnitude earthquake, which is fairly common in the Kamchatka region, which occurred in California in the Tehachapi region. Following that earthquake, our sensitive instrument was so active that the motion was continuous for about 36 hours. You could not have picked up anything on that instrument for 36 hours.

Chairman HOLIFIELD. When you say it was so sensitive, do you mean to say it continued to register high?

Dr. BENIOFF. The aftershocks were coming so frequently that they were continuous. The ground motion was continuous for fully 36 hours.

Chairman HOLIFIELD. Did it continue to register that high?

Dr. BENIOFF. It continued to register high enough on sensitive instruments within 200 miles of the source for 36 hours.

Now, you get out further, and the sizes of these shocks is smaller relative to the original, and you begin to gain. But nevertheless, a large earthquake would blanket out any of our network considerations for a considerable distance and time.

Chairman HOLIFIELD. Is it possible to arrange an array of shocks to simulate earthquake signals?

Dr. BENIOFF. I think it could be possible to arrange a series of shocks to simulate an aftershock sequence of short duration. I have recorded aftershocks in California that have completed the whole cycle within 24 hours. The usual arrangement is for the aftershocks to continue with decreasing frequency for something like a year to 2 years. But that does not occur always. So that one could simulate.

Chairman HOLIFIELD. Any comment on that point?

Dr. Romney?

Dr. ROMNEY. Well, we might just mention that the Scooter shot, a 500-ton high explosive shot, conducted in Nevada last year, was in fact partly obscured by the signal from the Kamchatka earthquake that you queried me about earlier today. So there is a case where this happened accidentally, and with the result of a great loss of information on the explosion.

Chairman HOLIFIELD. Was the Kamchatka registration going on at the time you tried your Nevada shot?

Dr. ROMNEY. No, sir.

Chairman HOLIFIELD. It came on afterward?

Dr. ROMNEY. Actually, the seismic waves from the Kamchatka event arrived in the United States 10 or more minutes after the earthquake, but about the same time the explosion went off. So the event had occurred, and the people at the test site had no knowledge of it, and they went ahead and pushed the button just about as the waves got there.

Mr. RAMEY. And they were able to show that the Kamchatka was an earthquake and not a weapon shot, because it was deep, but it would still obscure some other nuclear shot, or it could have?

Dr. ROMNEY. A smaller nuclear shot? Right.

Dr. HERBST. I believe the thing was almost worse than that. The people at the test site knew there was an earthquake coming and let it pass. But they were physicists, and they did not know that after the first arrival there would be a time delay and then larger signals.

Dr. ROMNEY. I will trust Dr. Herbst's story as being more accurate than mine.

Mr. RAMEY. Perhaps this research center will coordinate this knowledge.

Chairman HOLIFIELD. It might be wise to have a seismologist and a physicist coordinating their ideas at the time of the shot.

Dr. BENIOFF. Provided they are friendly.

Chairman HOLIFIELD. Now, is there anyone who would like to express an opinion on the problem of full decoupling and partial decoupling in relation to the testimony that has been placed before this committee?

In the first place, are there any differences in the minds of any of you gentlemen in regard to the testimony that we have had so far on this?

Dr. Carder?

Dr. CARDER. Since I had experience with the Cowboy experiment I agree with Dr. Herbst's testimony on it.

Chairman HOLIFIELD. Do you have any questions on any of these four?

Mr. NORTHRUP. Well, I do have one. With respect to the point Dr. Benioff made about Kamchatka being a hard place to surround, I

am sure you have knowledge of Dr. Ewing's work, I believe it was, where a single deep oceans seismometer was reported to have been a very quiet installation. Is it possible that you could surround Kamchatka with a number of offshore installations and partially make up for the lack of information?

Dr. BENIOFF. I do not know all the problems involved, but if you can put an adequate ocean-bottom network off the coast, that would be a great help. There are many problems involved there; especially with the kind of shore that surrounds Kamchatka there may be some practical problems. But that would be a great help.

On the other hand, the instruments would not be symmetrical, and there would be some problems we have not yet worked out of identifying signals crossing the boundary of the Pacific. We would have to go in beforehand, I should think, and get travel time curves to make sure that we knew what we were doing.

Dr. HASKELL. I do not think the evidence is yet conclusive that we really do have a low noise environment on the bottom of the ocean. I think we will know this within probably the next 6 months or a year. But there have only been two or three measurements made so far. Particularly the first Columbia measurement, where they got a comparatively low noise level. There is some suspicion that the seismometer may have been lying over on its side and was not registering anything at all, and that is why it looked quiet.

Mr. NORTHRUP. And more research is needed?

Dr. HASKELL. More research is needed.

Mr. NORTHRUP. If we may change the subject just slightly, going back to the problem of on-site inspection, the localization of the area where this activity should take place, what is your judgment about the possible gain by the onsite inspection team utilizing explosions to calibrate or to doublecheck on the location of the event that he is attempting to accurately locate?

Dr. BENIOFF. I think that would be a necessary requirement, to get precise locations, because the travel time characteristics change from region to region. And though we have general travel times, I am quite sure that in a marginal area such as Kamchatka, unless you have specific information, your precision would be quite low.

Dr. ROMNEY. Of course, you are talking about very large explosions in the context of the Geneva negotiations.

Mr. NORTHRUP. About what size do you believe you would need, Dr. Romney, in order to energize the same stations of the Geneva network that had recorded the event you were looking for?

Dr. ROMNEY. How far away are the stations? I think you are talking about explosions of hundreds of tons and perhaps larger.

Chairman HOLIFIELD. Well, of course, then, if the smaller weapons were tested, they would not fit into this theory of detection; would they?

Dr. BENIOFF. You need the large ones in order to calibrate the characteristics of the ground. Once you have got that, it will work for any size shock.

Dr. HERBST. That you could see.

Dr. BENIOFF. Any size shock that you could see. Right.

Chairman HOLIFIELD. So, in effect, you would need some large nuclear shots in order to set up your calibration on the smaller ones?

Dr. BENIOFF. Well, pretty large shots.

Chairman HOLIFIELD. What is your estimate of the reduction in magnitude below 4.75 which may be possible as a result of the present status of the Vela research?

Mr. RAMEY. Dr. Romney has already commented on this.

Dr. ROMNEY. Could I clarify my comment?

Chairman HOLIFIELD. Yes, Doctor.

Dr. ROMNEY. Implicit in my statement of this morning was that we would have 20 inspections or so annually in the Soviet Union. Obviously, if we are negotiated out of inspections, then you should not reduce the threshold as far. That point I should have added earlier.

Mr. RAMEY. And that would be that you would have 20 stations, also, the Geneva number of stations.

Dr. ROMNEY. Thank you. Yes.

Mr. RAMEY. It might be possible for purposes of discussion to repeat what the threshold of lowering would be.

Dr. ROMNEY. Well, my guess was that the kind of things we can see now, with reasonably optimistic interpretations of what they mean, would imply a possible reduction of a half magnitude or so. This would roughly mean that three times as many events would be included in the gross number that the control system would have to interpret. So we not only have to detect smaller events than we can now detect; we would have to identify a much larger number to get back to 20 inspections.

This implies, then, a capability for identification of events on the order of 80 percent, and then inspecting only a sampling of the remainder.

Mr. RAMEY. And you thought this half a magnitude would have the equivalent of bringing it down to around 3 kilotons?

Dr. ROMNEY. Yes; that is fairly close.

Chairman HOLIFIELD. Then how much would the theoretical hundred events at 4.75 be increased if there was a lowering of the magnitude?

Dr. ROMNEY. Our estimates for the U.S.S.R. were that there would be approximately a hundred or slightly more events larger than magnitude 4.75 each year, subject to fairly wide fluctuations. By lowering the threshold to magnitude 4.25 then, in the same territory, you would expect the numbers of events above the threshold to increase to the order of 300 or more. So the interpretation job would be very much greater.

Chairman HOLIFIELD. You mean the dropping of one-quarter of a percent would increase the number from 100 to 300?

Dr. ROMNEY. From 4.75 to 4.25.

Chairman HOLIFIELD. Well, that is one-quarter of a percent, is it not? Seventy-five is three-quarters.

Dr. ROMNEY. Well, this is a logarithmic scale, so you cannot use percentages on it. It is a factor of three in signal size. It is a factor somewhat larger in energy.

Dr. BENIOFF. Eight and one-half, or so.

Chairman HOLIFIELD. You have got me fairly confused, I do not mind admitting.

He is getting out his slide rule, now.

Dr. HERBST. I think saying that the 4.75 corresponded to 20 kilotons, while the 4.25 corresponds to 3 or 4 kilotons, or whatever it is, may give you a better feeling.

Chairman HOLIFIELD. Well, the thing that confused me was that you would thereby get into the area of 300 recordings rather than 100 recordings.

Dr. BENIOFF. That is right. That is the price you pay for better detection. You run into smaller shocks, and it is more difficult to interpret them.

Mr. RAMEY. Then how do you identify this additional 200 events? You have it worked out as to how you are going to take care of the first hundred.

Dr. ROMNEY. Well, let me reiterate that my statement was a guess as to what the improvement might be. My guess as to how you would identify these would be that the most powerful technique that you would hope to develop for identification would be the determination of depth of focus. I think, if you could work out an adequate technique for determining the depth of small events, you would have the best possible method for identifying the nature of the sources.

Mr. RAMEY. So, then, you could distinguish them from the deeper ones?

Dr. ROMNEY. This would be my opinion.

Chairman HOLIFIELD. Any further comment on that, gentlemen?

Mr. RAMEY. We might go around the table.

Chairman HOLIFIELD. All right.

Mr. RAMEY. I think you might want to see if Dr. Wilson has any appraisal.

Chairman HOLIFIELD. Dr. Wilson, did you signify you wanted to say something?

Dr. WILSON. No.

Chairman HOLIFIELD. How much improvement in the Geneva system for detecting and identifying underground explosions might reasonably be expected to result from the research in prospect in the program Vela Uniform?

Mr. RAMEY. The last question was from results to date, I guess, with a little bit in the future; and this one is on the long-range aspects of what the Vela program might come up with.

Chairman HOLIFIELD. Dr. Benioff?

Dr. BENIOFF. I would not want to put any estimate on it at all. It is a very "iffy" question. And it seems to me that unless we get a breakthrough of some new method derived from studies of the Vela program, such as this, the probability of any great improvement is very slight.

Chairman HOLIFIELD. Would you advise us to spend the taxpayers' money for this project?

Dr. BENIOFF. Yes; because you can never predict in the scientific world what an experiment will find. Usually we get what we are looking for, but we sometimes get much better.

Chairman HOLIFIELD. Spoken like a true scientist. I have heard that one before.

I want to go around the table on that question, though.

Would you care to say anything on that?

Dr. CARDER. I will go along with Dr. Benioff.

Chairman HOLIFIELD. Do not be intimidated by this California pessimist. I am from California, too, and he is giving these California earthquakes more adverse advertising than I have allowed in any committee in years.

Dr. CARDER. I used to be a Californian, too.

Dr. WILSON. I think Dr. Benioff summarized it pretty well. I think his reasons for not wanting to get quantitative about it are pretty much the same as mine would be. I think we begin to see where we are running into our limits on the actual detection. I mean, there has been enough work on arrays, and we begin to see enough about how the deep hole seismometer matches what we would theoretically predict.

But we are still left with this very serious problem of identification, complicated by the fact that as we improve our detection mechanism, we very rapidly increase the number of events that we have to identify. And while we have not exactly run up against a dead end in the identification, we have not really made any breakthroughs in it. And a lot of the research that is going on under the Vela program at the moment is pointed in this direction.

Somebody may come up with some things that look hopeful in this. And this is why I think we have to continue it. If we can now solve the identification problem, or begin to get some clues to it, then we could begin to be a little more quantitative about what gains we might make.

Chairman HOLIFIELD. Dr. Haskell?

Dr. HASKELL. I think we will just have to accept the fact that the Vela program is to a large extent speculative. We hope we will get a breakthrough of some kind.

Chairman HOLIFIELD. It is a pioneer program in a field that has not been explored adequately in the past. It is one of the disciplines that has not been explored adequately.

Dr. HASKELL. Yes. I do not think anybody could possibly seriously say, if we work on this for 3 more years we will be 30 percent better than we are now. If anybody said that, I would assume that he was joking.

Chairman HOLIFIELD. Dr. Romney, do you have anything to add?

Dr. ROMNEY. Well, I would, of course, endorse what my colleagues have said.

I would like to add one other thought, that the best way I can think of to improve the detection system that is being negotiated would be to get more stations and more inspections.

Mr. RAMEY. That would be a little difficult politically, it seems.

Dr. ROMNEY. Yes. From where I sit, the political problems are as easy as the technical problems, however.

Dr. BENIOFF. That makes it very soft.

Chairman HOLIFIELD. There are two point in this program I have been impressed with. That is the point of accurate location, that is, the type of geology you have to have, to have efficient locations for your detection instruments. You all feel that this is a very important factor. And then the second factor is that there must be not only improvement in detection, but there must be vast improvement in identification between the recorded graphic illustrations.

Mr. Northrup.

Mr. NORTHRUP. Mr. Chairman, I must say this is the first assemblage of seismologists where I have noted such unanimity of agreement. If this is the way we get it in planning sessions, perhaps we could get you to monitor our future research planning program.

Chairman HOLIFIELD. There are not many seismologists, and they are huddling together for comfort, I guess.

Mr. NORTHRUP. I have a question for Dr. Romney, dealing with the problem of the statistics of small earthquakes. There were extensive measurements available in small earthquakes in southern California and in New Zealand, but in scarcely any other places in the world. The improvement of the seismic observatories, worldwide, with better equipment, as I understand it, will not necessarily increase their sensitivity, which is largely conditioned by their locations in somewhat noisy areas.

In your judgment, should the Vela program include additional effort to define the frequency of locations of earthquakes down to let us say magnitudes of four or four and a half throughout the seismic areas of the world?

Dr. ROMNEY. I think it is true that if you improve the equipment at existing seismic stations, you will in many cases be still limited as to the minimal size event that can be detected, because so many stations are in cities and other places where there is a lot of noise.

Now, if it is anticipated that there will be a future requirement for truly worldwide statistics on very small earthquakes, then I think a valuable supplementary approach would be the addition of a few stations, such as we have at Fort Sill at the present time, which do seem capable of detecting about magnitude four events to really long range. And this would carry the world's statistics on events down to about that level.

I am not certain, however, that the stations now planned would be capable of doing it, and I rather doubt it.

Perhaps Dr. Benioff would have an opinion on this.

Dr. BENIOFF. Well, it seems to me if you could choose your sites, even though you might not get as efficient sites, if you could choose a few of them, I feel that you would accomplish a proper survey of smaller shocks, particularly in the regions in which we are most interested, in which our present knowledge is very meager.

I would like to see it done.

Chairman HOLIFIELD. Dr. Romney proposed a research analysis center in his testimony today. Is there any feeling on this, among you gentlemen? Do you feel that this is an important recommendation and should be carried through?

As I understand it, it was to be a research center under a university that would analyze all the information that is brought in from these different stations.

Is that not right, Dr. Romney?

Dr. ROMNEY. That was not precisely my proposal.

Chairman HOLIFIELD. You had better state it.

Dr. ROMNEY. Well, there have been two suggestions. One research center was discussed by Mr. Beyer in his testimony yesterday, and I also mentioned today the need to set up somewhere a group for the analysis of the explosion data and the data from the stations of the prototype system.

Now, these, under different management concepts, could be two independent groups, or conceivably a single group. But there have been two analytical groups that have been discussed in the hearings. One was the university-operated group, which Mr. Beyer discussed, and the other, which I proposed, was an adjunct to the explosion program and the systems development program.

Chairman HOLIFIELD. Would there be any discussion on that? Do you gentlemen have strong feelings, either way?

Dr. HASKELL. I would just like to remark that I do not think any such center should do all the analysis on this. I think the record should be made quite generally available to universities or commercial research laboratories throughout the country, so that a great many people could be working on the analysis of this data from a great many different points of view. I do not think all the work should be concentrated in any one place.

Dr. ROMNEY. Yes; I would certainly endorse that. These records are just too valuable to put in one little cubbyhole.

Chairman HOLIFIELD. And they are also quite voluminous, as I understand it, and will require a lot of minds working on it.

Dr. Wilson?

Dr. WILSON. Well, I think there are two problems here. Clearly we have to be sure that the data collected in any shot program gets really worked over in detail. We have been putting an enormous amount of money and effort into a shot program, and the data is going to have to be adequately worked up. And as you point out, it is going to be very voluminous.

Some mechanism will have to be provided to store it and make it available to everyone who wants to work on it, and the money to work on it will have to be provided. And I do not think there is much doubt that some sort of central organization will have to exist to do the first runthrough on the data, and also to make sure it is made available to everybody.

There is also the problem of the systems that Dr. Romney mentions, the big stations, the stations with their big arrays, and so on. We are not going to gain too much from them unless something is done with the data that they produce day after day. And they can go on being very happy with themselves, grinding out records that may be of rather limited usefulness, unless somebody is always taking a look at, "Now, is this the kind of data you should be getting, and are you doing the things with it you should be doing," and so on.

So I think it is quite important that we do not end up doing a big data-collecting job, with a quick, casual look at it and that being the end of it. This sometimes happens in scientific projects. The amount of money that goes into using the data is sometimes very small compared to the amount of money that went into getting it.

Dr. CARDER. I have this to add, especially on these special jobs. There are several organizations involved in gathering the data. I would say that each organization should work up its own data. Then the research center should coordinate all results, and make them available to qualified scientists.

Dr. BENIOFF. Well, the data is going to be so voluminous that I would like to see some central organization that would take this data and list the number of earthquakes and the number of records and the

types of recordings we have of each, so that the research worker does not have to look through a half a million records himself, but has that data all provided for him. And thereafter, it can be analyzed, either at a central organization or several central organizations.

But the way the data will be coming in, you cannot even begin to analyze it unless somebody first of all presents it in a form in which you can, with a minimum amount of time, pull out the particular earthquakes or the peculiar characteristics you want. And it seems to me that is an extremely important preliminary.

Chairman HOLIFIELD. Now, gentlemen, I ask each one of you separately: Is there any information that you would like to put on the record which you feel has been overlooked by the committee in its questions, or by the witnesses in their testimony, at this time?

If you feel that you want to look this record over more completely—some of you may not have heard all the testimony—there are additional copies of the testimony available, and we will hold the record open for about 2 weeks for any comments or additional material that you wish to submit.

Dr. BENIOFF. And to my way of thinking, we are weak in one respect, and that is that we have very little studies and knowledge of the volcanic earthquakes of Kamchatka. And it would seem to me to be very desirable to set up as good stations as we can in the Aleutians and the northern Japanese islands, in order to get some idea as to what is going on over there. It may be being done, but it is not to my knowledge.

Chairman HOLIFIELD. I thank each of you gentlemen for your participation on the panel. We appreciate the trouble you have gone to to be before us. And we will excuse you at this time.

The remainder of this afternoon's session will be devoted to testimony on that portion of Project Vela known as Vela Sierra—the detection of space tests from ground bases.

The next witness is Lt. Comdr. Donald Chandler, of the Advanced Research Projects Agency.

Commander Chandler, will you come forward, please, and take the witness chair and proceed?

We will come out of the underground now.

STATEMENT OF LT. COMDR. D. E. CHANDLER,[1] U.S. NAVY, CHIEF, VELA SIERRA BRANCH, ADVANCED RESEARCH PROJECTS AGENCY

Lieutenant Commander CHANDLER. Mr. Chairman and members of the Joint Committee, at the hearings conducted in April 1960, Dr. Panofsky reviewed the results of Technical Working Group I at Geneva. Technical Working Group I considered possibilities for the detection of nuclear tests at altitudes greater than 30 to 50 kilometers by means of equipment located on the surface of the earth and rec-

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Married, one daughter and two sons (daughter, 14 and twin sons, 11).
Assignments: Destroyers, Pacific Fleet, 1946–49; USS *Eldorado* (AGC 11) Pacific Fleet and Korea, 1949–51; Graduate School, 1951–55; Office of Naval Research, Electronics Branch, 1955–57; Headquarters, Armed Forces Special Weapons Project (now Defense Atomic Support Agency), Radiation Division, 1957–59; Advanced Research Projects Agency, Vela Sierra Branch, Nuclear Test Detection Office, 1959 to present.

ommended that specific types of equipment should be useful for this purpose. The techniques recommended were direct optical, fluorescence, cosmic noise absorption, and electromagnetic pulse. Backscatter radar was discussed, but agreement could not be reached as to its inclusion in a system for detecting high altitude tests.

Developing techniques for the control of high altitude tests requires an understanding of two factors: detection and identification. In principle, detection can be accomplished by instruments responding to the emissions from the detonation, or by instruments responding to the effects of these emissions on the environment of the instruments. In Vela Sierra, primary attention is given to the measurement of the effects of the detonation on the atmosphere and magnetic field of the earth. Identification requires that the information obtained from the detection instructions be interpretable as resulting from a nuclear test, rather than from a natural event such as a solar flare or cosmic ray shower.

The Vela Sierra program has three major objectives:

1. Prediction of signals to be received from a nuclear test.
2. Determination of the degree of similarity between the expected test signals and those of natural origin.
3. Development of instruments and analytical techniques which will afford the maximum capability of detecting tests at high altitude, and the minimum chance of interpreting a natural event as a nuclear detonation.

High altitude test phenomena: I would like to review the phenomena which are expected to result from a detonation at high altitude, emphasizing the resulting possibilities for detection from the surface of the earth, that is, those of specific interest to the Vela Sierra program.

(Chart I, p. 211.)

As shown in this highly schematic drawing, the atmosphere above the earth is normally ionized, that is, free electrons are present. This ionized region markedly affects normal radio propagation, and, as we shall see, it is the detection of changes in the ionosphere resulting from a nuclear detonation which are the basis of many of the Sierra techniques.

(Chart II, p. 212.)

At the instant of a high altitude detonation, various electromagnetic radiations are released. These include X-rays, gamma rays, visible light, and possibly a radio signal. With the exception of the visible light, and the radio signal, the atmosphere absorbs these radiations before they reach the surface of the earth. As a result, visible light and the radio signal can be detected directly from the surface while the X-rays and gamma rays cannot. For surface based detection purposes, the most important effect of the X-rays and gamma rays is to deposit their energy in the atmosphere. The results of this deposition of significance to the Vela Sierra program are (1) that free electrons, in addition to those normally present, are released in the atmosphere, and (2) that the air is briefly excited to fluoresce, that is, to emit light.

(Chart III, p. 213.)

In this chart we see the situation a short time after the explosion has occurred. First, in the vicinity of the explosion, a cloud of fission

debris forms what may be considered as a cloud of matter in normally empty space. In the vicinity of the earth the effects of the gamma ray ionization, for all intents and purposes, have disappeared. The free electrons formed by the X-rays persist but are rapidly disappearing.

If the detonation has taken place within the magnetic field of the earth, the fission debris will interact with the magnetic field in two general ways. First, the charged debris will be guided by the magnetic field in such a way that it will interact with the atmosphere. Second, the detonation will disturb the earth's magnetic field.

VELA SIERRA PROGRAM

It is apparent that a considerable variety of possibilities for detection from the surface exist. For simplicity, these can be grouped by the measurement technique employed into the following categories: (1) Optical, (2) Radio Frequency, and (3) Geophysical. The Vela Sierra program is organized around these three general possibilities, with two additional tasks—studies of natural phenomena to increase our knowledge of the similarity of these phenomena to those expected from a nuclear test, and a study on the requirements for incorporating the various detection instruments, analytical techniques, and supporting components into a workable system.

In discussing the current status of the Vela Sierra program, unless otherwise noted, the various tasks are being supervised in detail for ARPA by AFTAC. For coherence, I will include discussion of the various supporting research efforts in context with the relevant detection area.

(Chart IV, p. 214.)

The detection of air fluorescence and direct optical signals were included in the recommendations of Technical Working Group I.

Preliminary models of direct visible and air fluorescence optical detection instruments have been constructed in accordance with design criteria established by the Los Alamos Scientific Laboratory of the Atomic Energy Commission. Field trials of the air fluorescence instrument have been conducted at Los Alamos, N. Mex., in Alaska, and at Thule, Greenland. Field trials of the direct visible instrument are being conducted at Los Alamos. The current status of these field trials, as well as the related work on optical detection problems being conducted at Los Alamos, will be discussed by the next speaker.

One of the unknown factors in the operation of optical detection instruments is the extent to which the time history of the expected signals would be modified by clouds. An experimental and theoretical study of this problem is being conducted. Although far from complete, the results to date indicate that the presence of clouds will not constitute a serious degradation of the discrimination capabilities of the instrument.

The detailed characteristics of the direct visible light signal are not as well understood as in the case of the air fluorescence detection technique. A study is underway to attempt to further define the nature of this signal.

In the design of the current optical equipments it was, of course, necessary to utilize available components. A program of component development for optical detection systems is being conducted in conjunction with studies aimed at increasing the capability of this class of instruments.

A possibility for detection in addition to those recommended by Technical Working Group I is based on the observation of sunlight scattered from the debris resulting from the nuclear detonation. There are two possibilities here depending on the distance at which the test occurs. First, observation of the light scattered from the debris in the vicinity of the detonation, and second, observation of light scattered from the debris when it has been deposited in the upper atmosphere. These possibilities are being explored theoretically to determine whether a reasonably large and distinctive signal would result. If these studies indicate sufficient promise, a pre-prototype instrument will be developed.

With regard to techniques employing radio detection equipment, three specific possibilities were considered by TWG–I. The riometer, the electromagnetic pulse generated by the detonation, and the use of backscatter radar.

The riometer is basically a sensitive radio receiver designed to measure the intensity of the continuous radio signals generated at great distances from the earth. Since these signals are observed after passing through the earth's atmosphere, changes in the state of the atmosphere result in changes in the intensity of the received signals. Analysis of the effects expected from a nuclear detonation indicate that the rapidity of onset should be less than a second while the corresponding time for natural events is 30 seconds to several minutes. As a consequence, development of instruments capable of resolving the rapid time history of the observed signals is being carried out. Prototype riometer instruments capable of adequate resolution time are being operated to determine whether in fact signals with rapid variation do occur naturally.

One result of the experiments to date has been the observation that at times of solar activity, the radio emissions originating in the sun can be sufficiently intense to saturate riometer type instruments. Therefore, it must be expected that riometer detection instruments will be completely inoperable at times.

The electromagnetic radio frequency pulse from a nuclear detonation at extreme distances from the earth is not well understood. The strength and frequency to be expected can only be arrived at theoretically. In addition, the theoretical understanding required is complicated since possible signals appear to depend to a very great extent on the details of the device being tested. A theoretical study is being pursued with the intent of trying to more closely specify the nature of the expected signal resulting from a detonation at an extreme altitude.

Radio signals of this type resulting from detonations well within the atmosphere but above 30 kilometers are more or less understood and their utility for detection and identification is somewhat similar to that existing in seismic detection of underground tests, in the sense that signals of natural origin are practically indistinguishable from signals resulting from a nuclear detonation. Consequently, the Vela Sierra program contains experimental studies of the nature of natural radio frequency signals in order that possible differences in detail can be found and utilized for discrimination.

Backscatter radar has been studied for a considerable length of time. For instance, equipments of this general type were employed during the International Geophysical Year. A fundamental difficulty

in the use of backscatter radar is in clearly defining what would be specified as a signal resulting from a nuclear test. The second difficulty lies in designing a set of equipment capable of performing the required measurements. The specific effort being carried out in the Vela Sierra program deals with a conceptual design of appropriate instruments.

In addition to the radio techniques specifically considered by Technical Working Group I, several others have been proposed.

It has been observed that the propagation time from transmitter to receiver of very low frequency radio waves is dependent on the ionospheric conditions. Analysis of the expected effects from a high-altitude nuclear detonation on the ionosphere indicate that changes in propagation time should result. Theoretical studies demonstrate that the rapidity of onset of the propagation time change should provide a discrimination against changes from natural causes. Experimental programs on rapid changes in propagation time are being conducted at the Navy Electronics Laboratory in San Diego and the National Bureau of Standards Laboratories in Boulder, Colo. The equipments in use could be considered as preprototype detection instruments although the primary purpose at present is to determine the stability of VLF* propagation on the time scale of the changes expected from nuclear detonations. The results to date are gratifying in that it does appear that discrimination is feasible.

An alternative method for determining the state of the ionosphere to those previously described is by the use of vertical sounding equipment. This can be considered as a radar measurement of the height of the ionized regions of the atmosphere. Two programs are being conducted exploring the use of this technique, one at the Bureau of Standards and one under the direction of AFTAC. To date the programs have not been in operation for a sufficient period to permit conclusions as to the capability of this technique, although preliminary theoretical analysis makes it appear quite promising.

A class of phenomena not discussed at Geneva has been grouped under the heading of "Geophysical Effects." As has been mentioned previously, a detonation in the magnetic field of the earth should perturb the earth's field with the result that magnetic and earth current variations could be measured at the surface. Phenomena of this sort have been observed in connection with previous high-altitude tests. The U.S. Army Signal Corps Laboratories are conducting a study of the possible application of this sort of measurement to the test detection problem. Since some experience has been obtained with signals of this sort, the nature of the expected signal, at least in those instances, is known. A basic problem, however, is in determining how one would distinguish this type of observation of a nuclear test from the somewhat similar signals of natural origin. The study of this class of detection techniques, therefore, includes analysis of information previously obtained on natural signals, those observed in connection with previous tests and theoretical study of the nature of this type of disturbance. Experimental measurements of magnetic and earth current anomalies are being conducted in those frequency ranges where insufficient experimental information on natural signals exists.

* Very low frequency.

There is a possibility that the natural electric field of the earth might be disturbed in a specific way by the ionizing radiation resulting from a test at an extreme distance from the earth. Preliminary theoretical analysis of the nature of the expected signals appears sufficiently promising to warrant measurement of the natural field to determine the nature of natural fluctuations. A limited program of such measurements has been initiated. The program has not been in existence sufficiently long to permit any specific conclusions.

In addition to the investigations discussed up to this point, two very attractive additional possibilities are receiving active consideration:

First, a test in the upper atmosphere or in space should result in fission debris being deposited in the upper atmosphere. Conceptually, it should be possible to collect a sample of the debris by means of a rocket-borne sampling device, with subsequent laboratory analysis of the sample.

Second, while the gamma rays from a nuclear detonation in space are absorbed by the atmosphere, this absorption occurs at quite low altitudes—below 100,000 feet. Altitudes greater than this can be reached by balloons. Therefore, it appears feasible that properly instrumented balloons could perform a useful function as part of a detection system.

The final portion of the Vela Sierra program is concerned with the development of specific requirements for the construction and operation of a worldwide detection system utilizing surface-based techniques. The initial result of a study of those problems is concerned with how such a system would be designed and operated using current developments of those techniques recommended by Technical Working Group I. As the study continues it will refine the preliminary conclusions reached with regard to the Technical Working Group I recommended methods as well as adding additional methods developed under the Vela Sierra program.

The actual operation of a system of detection stations will require the solution of a large number of engineering problems. Since a variety of sensitive instruments would be installed at a control post, some mutual interference can be expected; uniform methods of processing and interpreting the outputs of the various instruments must be developed. To explore these problems in a realistic way, active consideration is being given to the construction and operation of a complete prototype control post.

Chairman HOLIFIELD. Thank you, Commander.

You heard Dr. Latter yesterday voice a rather pessimistic view of the possible effectiveness of ground-based techniques as opposed to satellite-based techniques of space detection. In your description today of the five techniques recommended by Technical Working Group I and the five additional techniques now included in the Sierra program, I detected more than slight bits of optimism.

I am sure that Dr. Latter would not mind and probably suspects you may not share his pessimism in this regard. Would you please state in your best judgment at what ranges a ground-based system which could be expected to result from Vela Sierra might be expected to detect and identify unshielded nuclear explosions of 10 kilotons or more in space?

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Commander CHANDLER. Yes, sir. This is a somewhat difficult question to answer, in that what one means by identification is a rather complex question.

In my opinion, however, I feel that——

Chairman HOLIFIELD. Let us simplify the word "identification," then, for the sake of your answer, and say that we are talking about identifying a nuclear explosion from a natural phenomena of some kind.

Commander CHANDLER. Yes, sir. I would say that in my opinion the techniques being developed under Vela Sierra should be—I would be quite confident in predicting that they will be able to detect detonations of the size you indicated, 10 kilotons, about as far as the moon's distance from the earth. Certain of these techniques would have capability for 10 kilotons considerably beyond that distance, perhaps 5 to 10 times as great. However, the confidence would not be as great, in my opinion.

Chairman HOLIFIELD. Did you include in that detection——

Commander CHANDLER. I think detection and identification about as far as the moon.

Chairman HOLIFIELD. And how many ground bases would you have to have for that?

Commander CHANDLER. Well, actually, to provide complete surveillance of the volume of space we are talking about, we have considered the number discussed at Geneva, 170 stations. This would seem like a reasonable and adequate number.

Now, if one wanted to make the system perform with a minimum number of stations, with some reduction in certainty and with some possibility of areas of incomplete coverage, this number could be reduced.

Chairman HOLIFIELD. Would you utilize an orbiting satellite on this?

Commander CHANDLER. I am referring to the system that is not using a satellite.

Chairman HOLIFIELD. These are ground-based?

Commander CHANDLER. Yes, sir.

Chairman HOLIFIELD. And your signals would come to instruments on ground bases rather than being relayed?

Commander CHANDLER. Yes, I am not considering the satellite. I consider the two quite complementary, as far as that is concerned.

Chairman HOLIFIELD. What about the shielded explosions of 10 kilotons or more?

First, do you believe that they can be effectively shielded?

Commander CHANDLER. I am certain that they can.

Well, let me qualify it in this way. Conceptually, shielding is quite feasible.

However, when we say "shielding," in the context that has been used so far in the discussion, we are referring to shielding of X-rays. Now, in the case of those techniques which depend on the X-ray effects, their performance could be degraded considerably by shielding. Those, however, which depend, for instance, on seeing the debris itself for effects on the magnetic field, for the radiation of the direct radio frequency signal, the direct optical signal, which do not depend on the X-rays—these would probably not be affected by that type of

shield. And as far as shielding these types of effects are concerned, this would be quite complicated. I am not sure it could be done.

Chairman HOLIFIELD. You have been, of course, confined, I would assume, to theoretical predictions of the effectiveness and range of detection so far in this area of the Vela Sierra.

Is there a need for actual explosions in space to confirm your theoretical assumptions and conclusions?

Commander CHANDLER. While it is true to a large extent that we are limited to theoretical predictions, it is not completely true, in the sense that there have been a small number of explosions conducted at fairly high altitudes. We have information from these tests which can be applied to this problem.

In the case of a detonation in a vacuum, in free space, at very high altitude, I think the theoretical predictions with certain exceptions of the characteristics of the output of the weapon are sufficiently well known to be able to predict with relatively good confidence what we would see on the instruments.

Now, this is a long way of saying that I think we can design instruments which we would have a great deal of confidence in.

I think one would never say that one would not eventually, when you think you have completed a system, want to have a test to prove that your predictions were correct. In other words, a test under known conditions. However, at this time, I would not see it——

Chairman HOLIFIELD. Is most of your work based on past tests?

Commander CHANDLER. To a large extent; yes, sir.

Chairman HOLIFIELD. Were they thoroughly instrumented?

Commander CHANDLER. I say yes, to a large extent because in some cases they were not instrumented for some of the specific applications.

Chairman HOLIFIELD. Was the Johnson Island test well instrumented?

Commander CHANDLER. Yes, sir. However, I could mention one or two things, in Riometer tests. These Riometer tests did not respond with the speed we think would be necessary for an optimum detection instrument.

Chairman HOLIFIELD. And have you improved your Riometers?

Commander CHANDLER. Yes, sir.

Chairman HOLIFIELD. How much?

Commander CHANDLER. Well, the Riometer that was used during the International Geophysical Year and was used at Johnson Island was designed primarily to show the changes in the opacity of the ionosphere on a daily cycle. Consequently, they had about a 30-second reaction time. In other words, when a change in the state of the ionosphere occurred, they took 30 seconds to respond to this. They were not designed to respond rapidly.

The equipment we have in operation now will follow a change that occurs in less than a second. This is experimental work.

Chairman HOLIFIELD. By a factor of 30, then, you have increased the efficiency of your machine?

Commander CHANDLER. Yes, sir. This is not necessarily the limit. It could be speeded up beyond that.

Representative BATES. Commander, if you were given the green light now, and all the funds and facilities available, what would be the time scale to put this into operation?

Commander CHANDLER. The complete system, sir? I would say 3 to 5 years.

Representative BATES. And what percent of detection would you expect then, from unshielded explosions? What would be the degree of reliability, in your judgment, of this system?

Commander CHANDLER. Well, for tests which were conducted within the range, that is, at an altitude within the capability of this system—I do not know. This is a very hard question to answer.

If, for instance, an unusual solar flare occurred and the test were conducted at the time of that flare, then the capability of the system would be degraded by a considerable amount.

Now, for instance, I mentioned that Riometers are affected by radio emissions from the sun. During a very intense period of solar activity, about 10 percent of the time a Riometer instrument would be useless. Considering these periods, then, it would be difficult to assign a number which would apply all the time. If I had to put a number on it, I would say that with tests of 10 kilotons and larger at distances less than the distance to the moon, we would have somewhere between 80 and 90 percent probability of detecting; and as far as identification is concerned, something less.

Representative BATES. There was some system that we considered a year ago, that would have cost millions of dollars, and I think the capability of detection was in the range of 5 or 10 percent. But in any event, you think yours would be much higher than that?

Commander CHANDLER. I think it would be, eventually. But the difficulty with trying to put a number on something like this is that one is referring to an average, over time, but the times when it is not operating—perhaps when I say it would be effective on 80 percent of the shots, or 80 percent of the time, actually, there would be times, perhaps. when it would not be effective at all.

Representative BATES. Yes; I understood you to say that.

Commander CHANDLER. Yes, sir.

Representative BATES. Now, tell me: What other forms would there be to nullify the signal in addition to shielding?

Commander CHANDLER. Natural?

Representative BATES. Natural or otherwise.

Commander CHANDLER. I think probably the most significant natural event that could occur that would make the operation of the system less effective would be a solar flare.

Representative BATES. Sir?

Commander CHANDLER. A solar flare.

Representative BATES. A solar flare?

Commander CHANDLER. A large eruption on the sun.

Representative BATES. It has been suggested: How about behind the moon?

Commander CHANDLER. Yes, sir. The moon would provide a very good shield.

Representative BATES. Now, if we were to perfect what you had in mind, and someone wants to try a test, if they put it behind the moon, where are you?

Commander CHANDLER. We would be in a very poor position, sir. There is one remote possibility for detection in that case, and that is the detection of the sunlight that was scattered from the debris after

it is moved from behind the moon. This is a rather slim possibility, I think.

Representative BATES. And so any system you develop must be a complete system.

Commander CHANDLER. Certainly.

Representative BATES. Otherwise, it is not really serving the purpose intended.

Commander CHANDLER. Yes, sir.

Representative BATES. So if you were to use your system, you would have to use it certainly in conjunction with other systems.

Commander CHANDLER. By "system," you are referring, I believe, to the ground-based, satellite, and so forth. Yes, sir.

Representative BATES. So it would be a combination?

Commander CHANDLER. Depending on the objectives one was trying to reach. To obtain the maximum capability for detection, quite clearly I think one should use both ground-based and satellite-based instruments.

Mr. RAMEY. Commander, do solar flares inactivate all of your Sierra techniques?

Commander CHANDLER. No, sir. Those which depend on the optical signals, the direct optical signals, should be unaffected. The fluorescent signal—we are exploring this to see what the effects of a solar flare would be on fluorescence of the atmosphere.

The solar flares are normally accompanied by radio frequency emissions which could confuse the instruments designed to detect the direct radio signal. Solar flares are accompanied by an increase in the X-ray emission from the sun, and this would affect the atmosphere in somewhat the same way as the bomb would and thus the instruments which depend on radio propagation measurements.

Chairman HOLIFIELD. Thank you very much, Commander.

(Commander Chandler's formal statement follows:)

STATEMENT BY LT. COMDR. D. E. CHANDLER, U.S. NAVY, CHIEF, VELA SIERRA BRANCH, ADVANCED RESEARCH PROJECTS AGENCY

Mr. Chairman and members of the Joint Committee, at the hearings conducted in April 1960, Dr. Panofsky reviewed the results of Technical Working Group I at Geneva. Technical Working Group I considered possibilities for the detection of nuclear tests at altitudes greater than 30 to 50 kilometers by means of equipment located on the surface of the earth and recommended that specific types of equipment should be useful for this purpose. The techniques recommended were direct optical, fluorescence, cosmic noise absorption, and electromagnetic pulse. Backscatter radar was discussed, but agreement could not be reached as to its inclusion in a system for detecting high altitude tests.

Developing techniques for the control of high altitude tests requires an understanding of two factors: detection and identification. In principle, detection can be accomplished by instruments responding to the emissions from the detonation, or by instruments responding to the effects of these emissions on the environment of the instruments. In Vela Sierra, primary attention is given to the measurement of the effects of the detonation on the atmosphere and magnetic field of the earth. Identification requires that the information obtained from the detection instruments be interpretable as resulting from a nuclear test, rather than from a natural event such as a solar flare or cosmic ray shower.

The Vela Sierra program has three major objectives:

1. Prediction of signals to be received from a nuclear test.
2. Determination of the degree of similarity between the expected test signals and those of natural origin.
3. Development of instruments and analytical techniques which will afford the maximum capability of detecting tests at high altitude, and the minimum chance of interpreting a natural event as a nuclear detonation.

HIGH ALTITUDE TEST PHENOMENA

I would like to review the phenomena which are expected to result from a detonation at high altitude, emphasizing the resulting possibilities for detection from the surface of the earth, that is, those of specific interest to the Vela Sierra program.

(Chart I, p. 211.)

As shown in this highly schematic drawing, the atmosphere above the earth is normally ionized, that is, free electrons are present. This ionized region markedly affects normal radio propagation, and, as we shall see, it is the detection of changes in the ionosphere resulting from a nuclear detonation which are the basis of many of the Sierra techniques.

(Chart II, p. 212.)

At the instant of a high altitude detonation, various electromagnetic radiations are released. These include X-rays, gamma rays, visible light and possibly a radio signal. With the exception of the visible light and the radio signal, the atmosphere absorbs these radiations before they reach the surface of the earth. As a result, visible light and the radio signal can be detected directly from the surface while the X-rays and gamma rays cannot. For surface-based detection purposes, the most important effect of the X-rays and gamma rays is to deposit their energy in the atmosphere. The results of this deposition of significance to the Vela Sierra program are (1) that free electrons, in addition to those normally present, are released in the atmosphere, and (2) that the air is briefly excited to fluoresce; that is, to emit light.

(Chart III, p. 213.)

In this chart we see the situation a short time after the explosion has occurred. First, in the vicinity of the explosion, a cloud of fission debris forms what may be considered as a cloud of matter in normally empty space. In the vicinity of the earth the effects of the gamma ray ionization, for all intents and purposes, have disappeared. The free electrons formed by the X-rays persist but are rapidly disappearing.

If the detonation has taken place within the magnetic field of the earth, the fission debris will interact with the magnetic field in two general ways. First, the charged debris will be guided by the magnetic field in such a way that it will interact with the atmosphere. Secondly, the detonation will disturb the earth's magnetic field.

VELA SIERRA PROGRAM

It is apparent that a considerable variety of possibilities for detection from the surface exist. For simplicity, these can be grouped by the measurement technique employed into the following categories: (1) optical, (2) radio frequency, and (3) geophysical. The Vela Sierra program is organized around these three general possibilities, with two additional tasks—studies of natural phenomena to increase our knowledge of the similarity of these phenomena to those expected from a nuclear test, and a study on the requirements for incorporating the various detection instruments, analytical techniques, and supporting components into a workable system.

In discussing the current status of the Vela Sierra program, unless otherwise noted, the various tasks are being supervised in detail for ARPA by AFTAC. For coherence, I will include discussion of the various supporting research ef-

(Chart IV, p. 214.)

The detection of air fluorescence and direct optical signals were included in the recommendations of technical working group I.

Preliminary models of direct visible and air fluorescence optical detection instruments have been constructed in accordance with design criteria established by the Los Alamos Scientific Laboratory of the Atomic Energy Commission. Field trials of the air fluorescence instrument have been conducted at Los Alamos, N. Mex., in Alaska, and at Thule, Greenland. Field trials of the direct visible instrument are being conducted at Los Alamos. The current status of these field trials, as well as the related work on optical detection problems being conducted at Los Alamos, will be discussed by the next speaker.

One of the unknown factors in the operation of optical detection instruments is the extent to which the time history of the expected signals would be modified by clouds. An experimental and theoretical study of this problem is being conducted. Although far from complete, the results to date indicate that the presence of clouds will not constitute a serious degradation of the discrimination capabilities of the instrument.

The detailed characteristics of the direct visible light signal are not as well understood as in the case of the air fluorescence detection technique. A study is underway to attempt to further define the nature of this signal.

In the design of the current optical equipments it was, of course, necessary to utilize available components. A program of component development for optical detection systems is being conducted in conjunction with studies aimed at increasing the capability of this class of instruments.

A possibility for detection in addition to those recommended by Technical Working Group I is based on the observation of sunlight scattered from the debris resulting from the nuclear detonation. There are two possibilities here depending on the distance at which the test occurs. First, observation of the light scattered from the debris in the vicinity of the detonation, and second, observation of light scattered from the debris when it has been deposited in the upper atmosphere. These possibilities are being explored theoretically to determine whether a reasonably large and distinctive signal would result. If these studies indicate sufficient promise, a preprototype instrument will be developed.

With regard to techniques employing radio detection equipment, three specific possibilities were considered by Technical Working Group I. The riometer, the electromagnetic pulse generated by the detonation, and the use of backscatter radar.

The riometer is basically a sensitive radio receiver designed to measure the intensity of the continuous radio signals generated at great distances from the earth. Since these signals are observed after passing through the earth's atmosphere, changes in the state of the atmosphere result in changes in the intensity of the received signals. Analysis of the effects expected from a nuclear detonation indicate that the rapidity of onset should be less than a second while the corresponding time for natural events is 30 seconds to several minutes. As a consequence, development of instruments capable of resolving the rapid time history of the observed signals is being carried out. Prototype riometer instruments capable of adequate resolution time are being operated to determine whether in fact signals with rapid variation do occur naturally.

One result of the experiments to date has been the observation that at times of solar activity, the radio emissions originating in the sun can be sufficiently intense to saturate riometer-type instruments. Therefore, it must be expected that riometer detection instruments will be completely inoperable at times.

The electromagnetic radio frequency pulse from a nuclear detonation at extreme distances from the earth is not well understood. The strength and frequency to be expected can only be arrived at theoretically. In addition, the theoretical understanding required is complicated since possible signals appear to depend to a very great extent on the details of the device being tested. A theoretical study is being pursued with the intent of trying to more closely specify the nature of the expected signal result from a detonation at an extreme altitude.

Radio signals of this type resulting from detonations well within the atmosphere but above 30 kilometers are more or less understood and their utility for detection and identification is somewhat similar to that existing in seismic detection of underground tests, in the sense that signals of natural origin are practically indistinguishable from signals resulting from a nuclear detonation. Consequently, the Vela Sierra program contains experimental studies of the nature of natural radio frequency signals in order that possible differences in detail can be found and utilized for discrimination.

Backscatter radar has been studied for a considerable length of time. For instance, equipments of this general type were employed during the International Geophysical Year. A fundamental difficulty in the use of backscatter radar is in clearly defining what would be specified as a signal resulting from a nuclear test. The second difficulty lies in designing a set of equipment capable of performing the required measurements. The specific effort being carried out in the Vela Sierra program deals with a conceptual design of appropriate instruments.

In addition to the radio techniques specifically considered by Technical Working Group I, several others have been proposed.

It has been observed that the propagation time from transmitter to receiver of very low frequency radio waves is dependent on the ionospheric conditions. Analysis of the expected effects from a high altitude nuclear detonation on the ionosphere indicate that changes in propagation time should result. Theoretical

studies demonstrate that the rapidity of onset of the propagation time change should provide a discrimination against changes from natural causes. Experimental programs on rapid changes in propagation time are being conducted at the Navy Electronics Laboratory in San Diego and the National Bureau of Standards Laboratories in Boulder, Colo. The equipments in use could be considered as preprototype detection instruments although the primary purpose at present is to determine the stability of VLF propagation on the time scale of the changes expected from nuclear detonations. The results to date are gratifying in that it does appear that discrimination is feasible.

An alternative method for determining the state of the ionosphere to those previously described is by the use of vertical sounding equipment. This can be considered as a radar measurement of the height of the ionized regions of the atmosphere. Two programs are being conducted exploring the use of this technique, one at the Bureau of Standards and one under the direction of AFTAC. To date the programs have not been in operation for a sufficient period to permit conclusions as to the capability of this technique, although preliminary theoretical analysis makes it appear quite promising.

A class of phenomena not discussed at Geneva has been grouped under the heading of geophysical effects. As has been mentioned previously, a detonation in the magnetic field of the earth should perturb the earth's field with the result that magnetic and earth current variations could be measured at the surface. Phenomena of this sort have been observed in connection with previous high altitude tests. The U.S. Army Signal Corps Laboratories are conducting a study of the possible application of this sort of measurement to the test detection problem. Since some experience has been obtained with signals of this sort, the nature of the expected signal, at least in those instances, is known. A basic problem, however, is in determining how one would distinguish this type of observation of a nuclear test from the somewhat similar signals of natural origin. The study of this class of detection techniques, therefore, includes analysis of information previously obtained on natural signals, those observed in connection with previous tests and theoretical study of the nature of this type of disturbance. Experimental measurements of magnetic and earth current anomalies are being conducted in those frequency ranges where insufficient experimental information on natural signals exists.

There is a possibility that the natural electric field of the earth might be disturbed in a specific way by the ionizing radiation resulting from a test at an extreme distance from the earth. Preliminary theoretical analysis of the nature of the expected signals appears sufficiently promising to warrant measurement of the natural field to determine the nature of natural fluctuations. A limited program of such measurements has been initiated. The program has not been in existence sufficiently long to permit any specific conclusions.

In addition to the investigations discussed up to this point, two very attractive additional possibilities are receiving active consideration: First, a test in the upper atmosphere or in space should result in fission debris being deposited in the upper atmosphere. Conceptually, it should be possible to collect a sample of this debris by means of a rocket borne sampling device, with subsequent laboratory analysis of the sample.

Second, while the gamma rays from a nuclear detonation in space are absorbed by the atmosphere, this absorption occurs at quite low altitudes—below 100,000 feet. Altitudes greater than this can be reached by balloons. Therefore, it appears feasible that properly instrumented balloons could perform a useful function as part of a detection system.

The final portion of the Vela Sierra program is concerned with the development of specific requirements for the construction and operation of a worldwide detection system utilizing surface based techniques. The initial result of a study of these problems is concerned with how such a system would be designed and operated using current developments of those techniques recommended by Technical Working Group I. As the study continues it will refine the preliminary conclusions reached with regard to the TWG–1 recommended methods as well as adding additional methods developed under the Vela Sierra program.

The actual operation of a system of detection stations will require the solution of a large number of engineering problems. Since a variety of sensitive instruments would be installed at a control post, some mutual interference can be expected; uniform methods of processing and interpreting the outputs of the various instruments must be developed. To explore these problems in a realistic way, active consideration is being given to the construction and operation of a complete prototype control post.

CHART I

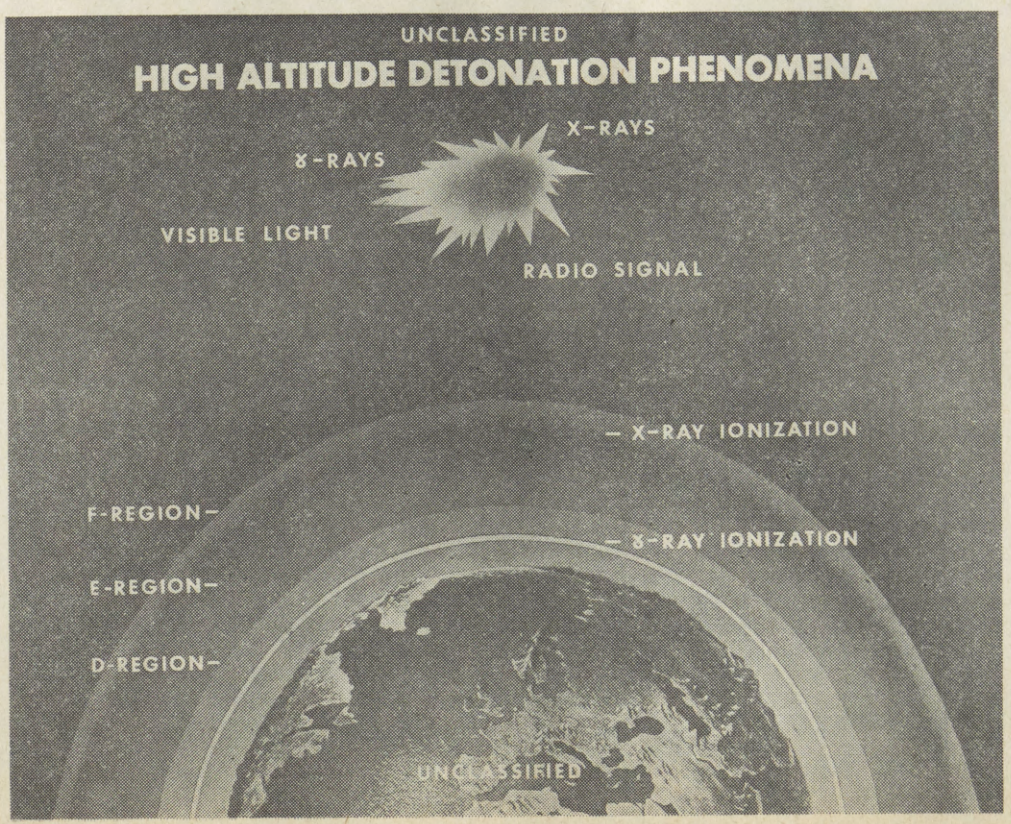

UNCLASSIFIED
HIGH ALTITUDE DETONATION PHENOMENA
X-RAYS
γ-RAYS
VISIBLE LIGHT
RADIO SIGNAL
— X-RAY IONIZATION
F-REGION—
— γ-RAY IONIZATION
E-REGION—
D-REGION—
UNCLASSIFIED

CHART II

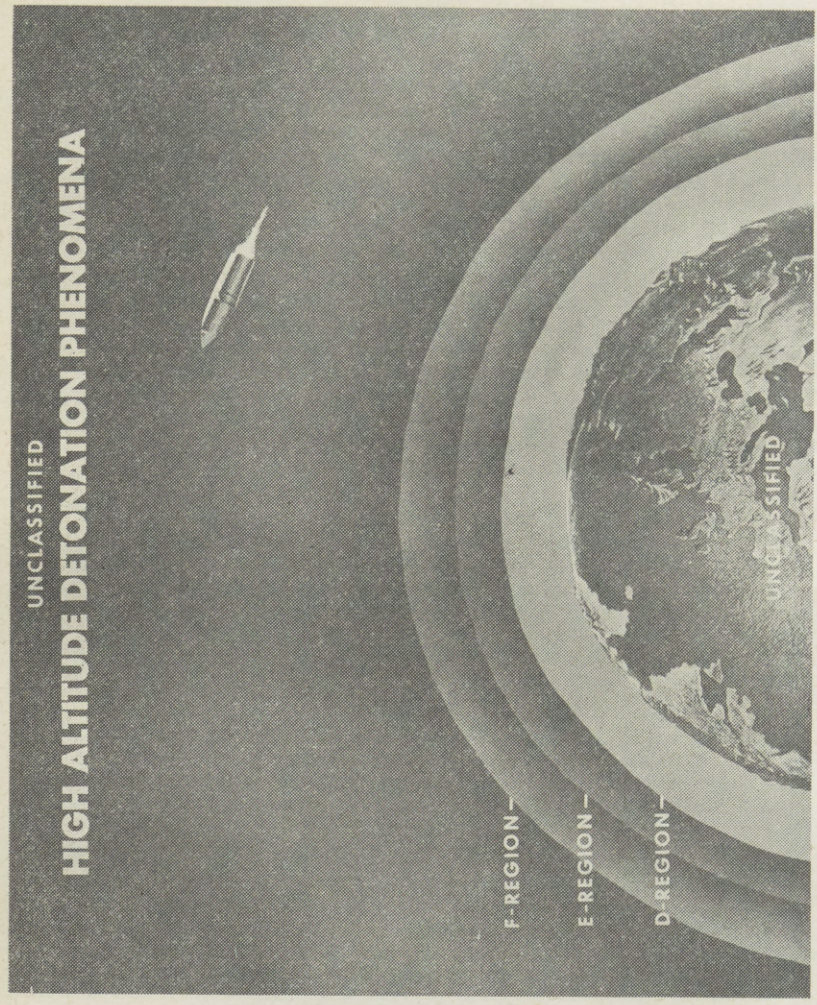

UNCLASSIFIED
HIGH ALTITUDE DETONATION PHENOMENA
F-REGION
E-REGION
D-REGION
UNCLASSIFIED

CHART III

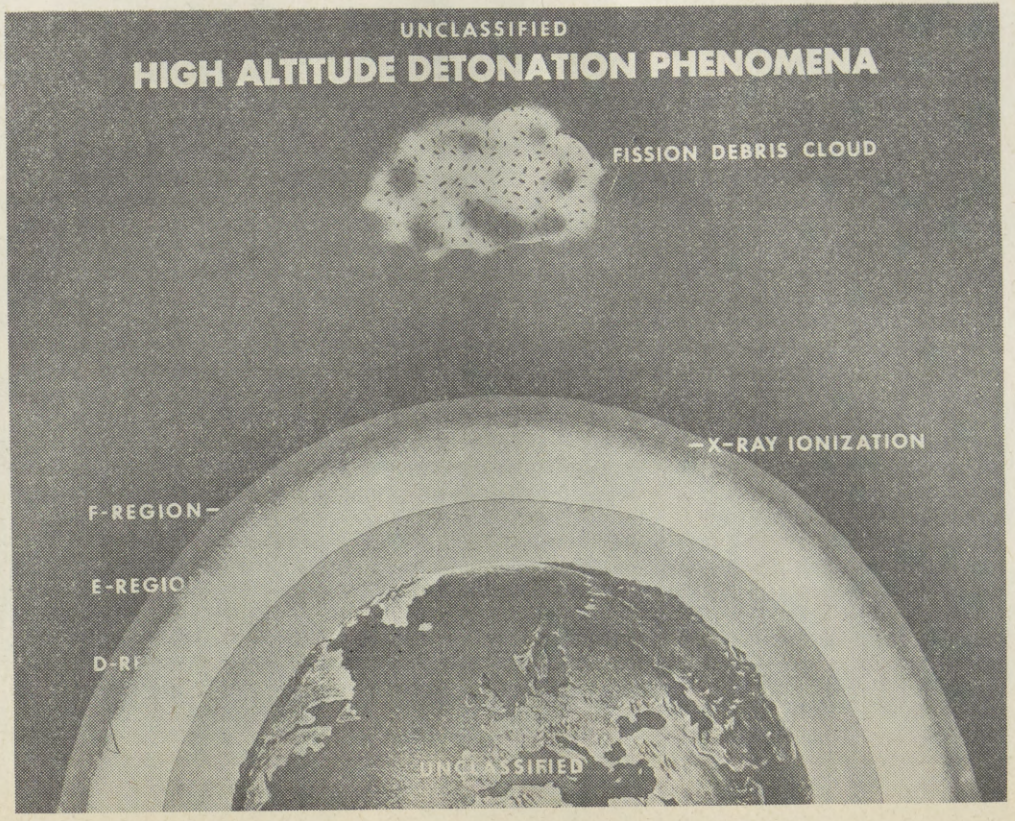

UNCLASSIFIED
HIGH ALTITUDE DETONATION PHENOMENA
FISSION DEBRIS CLOUD
—X-RAY IONIZATION
F-REGION—
E-REGIO
D-RE
UNCLASSIFIED

CHART IV

VELA SIERRA

Detection Technique	TWG-1	
Optical	Direct Visible Air Fluorescence	Scattered Sunlight
Radio	Riometer Radio Pulse (Backscatter Radar)	VLF Propagation Vertical Sounder
Geophysical		Geomagnetic Perturbation Electrostatic Gradient
System Design and Analysis		

Chairman HOLIFIELD. Our next and last witness today is Dr. William Ogle, Los Alamos Scientific Laboratory. He will present testimony on the work being conducted by Los Alamos in support of his Vela Sierra program.

Dr. Ogle, we are glad to have you with us from Los Alamos. Go right ahead, please.

STATEMENT OF DR. WILLIAM OGLE,[1] LOS ALAMOS SCIENTIFIC LABORATORY

Dr. OGLE. The Los Alamos Scientific Laboratory has studied over the last year two of the Vela Sierra proposed methods of detection. We call these the fluorescent method and the direct optical method. Allow me to take a moment to remind you again of the concepts behind these two methods. The fluorescent method is based on the fact that a nuclear device detonated in space releases a large portion of its energy in the form of X-rays which are emitted, depending upon the yield, in the range of a few kilovolts and in a time of perhaps a half microsecond to a microsecond. These X-rays travel through space unimpeded and a small fraction of them eventually hit the earth's upper atmosphere. Peak absorption of those X-rays occurs at approximately 80 kilometers altitude in the atmosphere. They

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Organizations: Lambda Chi Alpha, fraternal, Reno, Nev.; Phi Kappa Phi, honorary, University of Nevada; Sigma XI, honorary, University of Illinois; American Physical Society, fellow.
Hired Los Alamos Scientific Laboratory, Nov. 22, 1944; appointed alternate J Division leader, September 1952.
Nuclear weapons test participation:
Eniwetok Proving Ground: Crossroads, Sandstone, Greenhouse, Experimentor; Ivy, commander Task Unit 1, LASL experiments; Castle, commander Task Group 1, Scientific Task Group; Redwing, Hardtack, deputy for scientific matters to the task force commander.
Nevada test site: Ranger, Experimentor; Buster-Jangle, Tumbler-Snapper, Upshot-Knothole, Teapot, Plumbob, Hardtack, Phase II, Test Group Director for LASL experiments; variously member, Chairman of AEC Test Manager's Advisory Panels.
Family: Wife, Johanna Wilhelmina Ogle; children—William, James Weldon, Jean Marie, Mark Danacy, Mona Sue.

produce in the upper atmosphere electrons in the few kilovolt or lower energy range by the photoelectric effect. These electrons in slowing down in the atmosphere produce various excited atoms and molecules and as these atoms and molecules release their energy, they emit light, most of which is in wavelength regions that will not penetrate the rest of the atmosphere. However, some of the light, and in particular that from the excited levels of N_2^+ that is, nitrogen molecules which have lost 1 electron, is reemitted in the visible wavelength range and is transmitted through the atmosphere. Approximately ¼ percent of the energy that comes into the atmosphere as X-rays is re-emitted in the 3914 Angstrom line of N_2^+. This light may be observed by an optical system on the ground. The pulse of light observed from the ground would be perhaps a few hundred microseconds long since the apparatus observes the whole sky. When this method was proposed the numbers then available led to a predicted range of observation of approximately 100,000 kilometers for a 1-kiloton device and 3 million kilometers for a 1-megaton device. These ranges apply in daylight. At night the ranges would be about 10 times greater. Cloud cover can reduce the range by perhaps a factor of 3. A great proportion of our work over the last year has been to verify these predictions.

The direct optical method proposes to see the light emitted by the bomb materials in about the first 100 microseconds. In order to observe this light, collimated optical systems are used to reduce the background from normal sky light. The particular proposal under recent consideration would use about 200 channels, thus dividing the sky up into about 200 sections. The direct optical method has the advantage of giving the direction to an explosion but has no labeling device to prove that the signal was actually from a nuclear explosion. The range of this direct optical system is predicted to be such that it could observe an explosion of several megatons at approximately 10^6 kilometers in daylight. The equipment costs for a direct optical station of this type would be perhaps a half million to a million dollars whereas a fluorescent method station would cost approximately $100,000. These prices do not include the cost of running the station.

Most of the work on Vela Sierra over the past year at LASL has been concerned with the fluorescent method. The calculation of range for the fluorescent method depends very strongly on estimates of the actual detector sensitivities allowable in the presence of normal sky light. The only way to be sure of the calculation is to build equipment and to try it out. That has been done in the last year and the sensitivities and statistical noise backgrounds are as expected.

The success of the fluorescent method also depends upon a lack of natural backgrounds. For instance, aurora and lightning might produce signals similar to that from a nuclear detonation. The present apparatus, in addition to observing the expected 3914 Å line observes a narrow region at about 4150 Å. This latter wavelength is not present in aurora, but is present in lightning. It would not be present in the light produced by a nuclear explosion. Thus, in principle, lightning can be discriminated against by observing whether or not there is light at 4150 Å. During the past year, the fluorescent equipment has been operated at Los Alamos, where we find there is no problem in

discriminating against lightning, and there are no rapid variations of sky light intensity of the type expected from a nuclear detonation. The equipment has also been operated in the Arctic for a moderate period of time. The experience there indicates that a normal aurora—I should emphasize "normal"—does not contain signals in the 3914 Å wavelength region that could be mistaken for a nuclear detonation. That wavelength is present, but does not seem to vary in time in the manner expected from a bomb signal. The experience there also showed that over the time of observation no rapidly varying signals, due for instance to solar flares, were coming in along the earth's magnetic lines of force in such a manner as to be confused with a nuclear detonation.

During this same period of time, experimental laboratory work at Los Alamos has led to greater confidence that approximately one-fourth of 1 percent of the X-ray energy is converted to light at 3914 Å in the upper atmosphere. Thus, experimental work in the last year has shown that the fluorescent method has approximately the range predicted; that the equipment can be built and operated, and that over a moderate period of time, we do not see natural signals from aurora or the sun or lightning or the normal sky that can be mistaken for nuclear detonations.

In the near future, we expect to finish the laboratory work on conversion efficiencies. We will continue to operate the fluorescence method equipment at Los Alamos to build up data on natural backgrounds, and we propose to study for longer periods of time the signals that might be present in aurora. We feel it is not sufficient to show that there are no natural signals that could be confused with nuclear detonations over a short period of time. For the fluorescent method to be useful, one should know that over a period of some long time, perhaps a year, there are no such signals or that the signals can be identified by some other means. For instance, major solar events could perhaps produce signals in the polar regions which we would not have observed during the short period of time we were there.

Since the present fluorescent equipment is almost completely proven-in we now propose to construct larger optical systems to study sky backgrounds and statistics with the idea in mind of extending the range of the system. An extention of range is not simple. For instance, because of statistical considerations, the range varies as approximately the fourth root of the area of the aperture of the optical system. Therefore, if we want to increase the range by a factor of 10, we must increase the area of the optics by a factor of 10,000. This is not impossible but begins to get into very expensive systems. A factor of three in range does seem reasonable with newly available components, and thus is our present aim.

Over the past year four typical channels of the direct optical system have been constructed and are now at Los Alamos for evaluation. I should point out that ARPA has the technical responsibility for the direct optical system and that Los Alamos has agreed only to evaluate the systems built. However, we are helping in other phases as we are able. The four channels now at Los Alamos are in the process of evaluation. The question here of course, is that of the statistics and natural backgrounds which determine the range of

the system. In the near future, Los Alamos will also study the source in appreciably more detail than has been done in the past; that is, the amount of visible light that one might really expect from the nuclear detonation in space. At the moment, we feel that about 10^{-6} of the energy of a megaton device will come out as visible light in perhaps 30 microseconds and that this is the major signal. However, theoretically, this is a pretty weak number and is the largest uncertainty in the calculations of what the direct optical system will do. We also propose to study natural backgrounds with one extended range channel at Los Alamos; that is, a channel that has larger optics and sees a smaller proportion of the sky and hence has a higher signal to noise ratio. An actual system using the extended range channels would become pretty complex. The optics we propose to use would involve one station having something like 20,000 channels with presently available photomultipliers; however, our main point is to study the backgrounds to see if such a system is possible at all. Larger area multipliers are now being developed that would allow construction of an extended range direct optical system with a more reasonable number of channels.

Lastly, possible combinations of these two systems to increase both range and assurance of identification has been proposed. We will in the next year study some of these methods but have no promises to make at this time.

In summary, the uncertainties with respect to the fluorescent method have been tentatively removed over the last year. We are now moderately confident that the signal predictions cannot be significantly improved without actual test. Aurora, lightning, and other natural phenomena do not seem to be a serious problem but will bear further study. A prototype station is in existence, operating, and collecting background information. Data obtained under operational conditions indicate that the range of the fluorescent system may be somewhat greater than originally predicted. The direct optical systems will be evaluated over the next year. Thus, we are now turning our attention to increasing the range of these systems.

Thank you.

Chairman HOLIFIELD. Thank you, Dr. Ogle.

Now, you say here that the range of the fluorescence system may be somewhat greater than originally predicted. Can you give us any figures on that?

Dr. OGLE. That number is unfortunately dependent upon the energy that one is willing to expend on the system. It is largely a question of how many individual pulses people are willing to look at, to see what their shape is.

I would guess offhand that a factor of two is available without too much trouble.

Chairman HOLIFIELD. In your optical shielding, or optical system, rather, you mention that it has an inherent advantage in being insensitive to shielding.

Can you amplify on this point, please, and give us a range estimate also for the direct optical system?

Dr. OGLE. I feel that the calculation of the energy spectrum of radiation that comes out of a nuclear detonation in space is quite good in the X-ray region. The shielding methods that have been proposed deal with that energy region.

I have no real reason to question those calculations. They seem quite reasonable to me.

However, our knowledge of the time history of the light is extremely weak. It could be wrong by a factor of 100, easily, I think. I suspect that the optical signal may be larger than present calculations indicate. I think there is a point to be made also that the direct optical system looks for a quite long time. A shield to cut off this light would, therefore, have to be quite large as compared with the size of shield necessary to cut off the X-ray signal, and would have to be either far away from the bomb or fairly heavy. If it is close to the bomb, then presumably it also will got hot, if it is light.

So that I feel the shielding methods will not work so well in a direct optical system. However, the range of the direct optical system is, I think, in really great question.

I have quoted a number of 10^{-6} kilometers for a megaton. I think that is a reasonable number, but I certainly would not want to swear by it.

Chairman HOLIFIELD. You would need some tests in order to verify these theories?

Dr. OGLE. Yes. I think for the fluorescent system, we have confidence in the source and confidence in the measurements that have been made. With the direct optical system, we can spend a lot of effort in calculating what the light signal will be, and propose to spend that effort.

But frankly, I would not particularly trust the results without seeing a test. There are physical quantities involved in this calculation that are almost impossible of calculation.

Chairman HOLIFIELD. Mr. Ramey?

Mr. RAMEY. How did the Los Alamos program get started?

Dr. OGLE. Well, Los Alamos conducted a fairly large experiment observing the light on the Teak and Orange shots, and hence had some experience in this field.

After the moratorium began, we of course discussed these data with a number of people, including members of the Armed Forces and members of AFTAC, who, in conversation with us, raised the question, and in conversation we concluded, that this might be a way that would be useful to detect clandestine explosions. And we started thinking about it and eventually got into the system.

I suppose actually work started on it in 1959.

Mr. RAMEY. Your work started after the Johnson Island phenomenon?

Dr. OGLE. Yes, the work directed toward this purpose started after the Johnson Island experiments.

Mr. RAMEY. And I suppose that excited your curiosity in this field quite a bit—the effect upon the eyes of rabbits and also the other phenomena that was attached to it.

Dr. OGLE. The Teak shots were extremely impressive detonations, and the people that worked there on the optical data——

Mr. RAMEY. The AEC funded this very modest effort that you were making there before the Vela program was started?

Dr. OGLE. Yes, sir. That is correct.

Mr. RAMEY. And I believe that both the Los Alamos Laboratory and the AEC should be complimented for their foresight and initiative in looking into this matter.

Dr. OGLE. Yes, sir.

Mr. RAMEY. Do you think there would be any byproducts from the type of work that you have been doing on this survey work, and so on, in establishing these stations and being able to scan in this fashion for other purposes?

Dr. OGLE. Well, there are two byproducts, I suppose, that I have to admit are of great interest to us. It is pleasant for a physicist to get into work that is of some use to someone else, and is still very good physics. And there are a great number of cross sections in the atomic physics field, here, which are not known, and we are having to measure them to carry out this work. I think they will be useful in the future.

Also, the whole business of what is going on in the outer edges of the atmosphere and in space, of course, is extremely fascinating, and we hope to learn more about it by studying this problem.

Chairman HOLIFIELD. Any further questions of Dr. Ogle?

Thank you, sir, for your testimony and your appearance here.

This completes this afternoon's session.

(Dr. Ogle's formal statement follows:)

FLUORESCENT AND DIRECT OPTICAL METHODS

(By William Ogle)

The Los Alamos Scientific Laboratory has studied over the last year two of the Vela Sierra proposed methods of detection. We call these the fluorescent method and the direct optical method. Allow me to take a moment to remind you again of the concepts behind these two methods. The fluorescent method is based on the fact that a nuclear device detonated in space releases a large portion of its energy in the form of X-rays which are emitted, depending upon the yield, in the range of a few kilovolts and in a time of perhaps a half microsecond to a microsecond. These X-rays travel through space unimpeded and a small fraction of them eventually hit the earth's upper atmosphere. Peak absorption of these X-rays occurs at approximately 80 kilometers altitude in the atmosphere. They produce in the upper atmosphere electrons in the few kilovolt or lower energy range by the photoelectric effect. These electrons in slowing down in the atmosphere produce various excited atoms and molecules and as these atoms and molecules release their energy, they emit light, most of which is in wavelength regions that will not penetrate the rest of the atmosphere. However, some of the light, and in particular that from the excited levels of N_2^+ this is, nitrogen molecules which have lost one electron, is reemitted in the visible wavelength range and is transmitted through the atmosphere. Approximately one-quarter of 1 percent of the energy that comes into the atmosphere as X-rays is reemitted in the 3914 Å line of N_2^+. This light may be observed by an optical system on the ground. The pulse of light observed from the ground would be perhaps a few hundred microseconds long since the apparatus observes the whole sky. When this method was proposed the numbers then available led to a predicted range of observation of approximately 100,000 kilometers for a 1-kiloton device and 3 million kilometers for a 1-megaton device. These ranges apply in daylight. At night the ranges would be about 10 times greater. Cloud cover can reduce the range by perhaps a factor of 3. A great proportion of our work over the last year has been to verify these predictions.

The direct optical method proposes to see the light emitted by the bomb materials in about the first 100 microseconds. In order to observe this light, collimated optical systems are used to reduce the background from normal skylight. The particular proposal under recent consideration would use about 200 channels, thus dividing the sky up into about 200 sections. The direct optical method has the advantage of giving the direction to an explosion but has no labeling device to prove that the signal was actually from a nuclear explosion. The range of this direct optical system is predicted to be such that it could observe an explosion of several megatons at approximately 10^6 kilometers in daylight. The equipment costs for a direct optical station of this type would be perhaps a half million to a million dollars whereas a fluorescent method station would cost approximately $100,000.

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Most of the work on Vela Sierra over the past year at Los Alamos Scientific Laboratory has been concerned with the fluorescent method. The calculation of range for the fluorescent method depends very strongly on estimates of the actual detector sensitivities allowable in the presence of normal skylight. The only way to be sure of the calculation is to build equipment and to try it out. That has been done in the last year and the sensitivities and statistical noise backgrounds are as expected.

The success of the fluorescent method also depends upon a lack of natural backgrounds. For instance, aurora and lightning might produce signals similar to that from a nuclear detonation. The present apparatus, in addition to observing the expected 3914 Å line observes a narrow region at about 4150 Å. This latter wavelength is not present in aurora, but is present in lightning. It would not be present in the light produced by a nuclear explosion. Thus, in principle, lightning can be discriminated against by observing whether or not there is light at 4150 Å. During the past year, the fluorescent equipment has been operated at Los Alamos, where we find there is no problem in discriminating against lightning, and there are no rapid variations of skylight intensity of the type expected from a nuclear detonation. The equipment has also been operated in the Arctic for a moderate period of time. The experience there indicates that a normal aurora does not contain signals in the 3914 Å wavelength region that could be mistaken for a nuclear detonation. That wavelength is present, but does not seem to vary in time in the manner expected from a bomb signal. The experience there also showed that over the time of observation no rapidly varying signals, due for instance to solar flares, were coming in along the earth's magnetic lines of force in such a manner as to be confused with a nuclear detonation.

During this same period of time, experimental laboratory work at Los Alamos has led to greater confidence that approximately one-quarter of 1 percent of the X-ray energy is converted to light at 3914 Å in the upper atmosphere. Thus, experimental work in the last year has shown that the fluorescent method has approximately the range expected; that the equipment can be built and operated, and that over a moderate period of time, we do not see natural signals from aurora or the sun or lightning or the normal sky that can be mistaken for nuclear detonations.

In the near future, we expect to finish the laboratory work on conversion efficiencies. We will continue to operate the fluorescence method equipment at Los Alamos to build up data on natural backgrounds, and we propose to study for longer periods of time the signals that might be present in aurora. We feel it is not sufficient to show that there are no natural signals that could be confused with nuclear detonations over a short period. For the fluorescent method to be useful, one should know that over a period of some long time, perhaps a year, there are no such signals or that the signals can be identified by some other means. For instance, major solar events could perhaps produce signals in the polar regions which we would not have observed during the short period of time we were there.

Since the present fluorescent equipment is almost completely proven in we now propose to construct larger optical systems to study sky backgrounds and statistics with the idea in mind of extending the range of the system. An extension of range is not simple. For instance, because of statistical considerations, the range varies as approximately the fourth root of the area of the aperture of the optical system. Therefore, if we want to increase the range by a factor of 10, we must increase the area of the optics by a factor of 10,000. This is not impossible but begins to get into very expensive systems. A factor of 3 in range does seem reasonable with newly available components, and thus is our present aim.

Over the past year four typical channels of the direct optical system have been constructed and are now at Los Alamos for evaluation. I should point out that ARPA has the technical responsibility for the direct optical system and that Los Alamos has agreed only to evaluate the systems built. However, we are helping in other phases as we are able. The four channels now at Los Alamos are in the process of evaluation. The question here of course, is that of the statistics and natural backgrounds which determine the range of the system. In the near future, Los Alamos will also study the source in appreciably more detail than has been done in the past, that is, the amount of visible light that one might really expect from the nuclear detonation in space. At the moment, we feel that about 10^{-6} of the energy of a megaton device will

come out as visible light in perhaps 30 microseconds and that this is the major signal. However, theoretically, this is a pretty weak number and is the largest uncertainty in the calculations of what the direct optical system will do. We also propose to study natural backgrounds with one extended range channel at Los Alamos; that is, a channel that has larger optics and sees a smaller proportion of the sky and hence has a higher signal-to-noise ratio. An actual system using the extended range channels would become pretty complex. The optics we propose to use would involve one station having something like 20,000 channels with presently available photomultipliers; however, our main point is to study the backgrounds to see if such a system is possible at all. Larger area multipliers are now being developed that would allow construction of an extended range direct optical system with a more reasonable number of channels.

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In summary, the uncertainties with respect to the fluorescent method have been tentatively removed over the last year. We are now moderately confident that the signal predictions cannot be significantly improved without actual test. Aurora, lightning, and other natural phenomena do not seem to be a serious problem but will bear further study. A prototype station is in existence, operating, and collecting background information. Data obtained under operational conditions indicate that the range of the fluorescent system may be somewhat greater than originally predicted. The direct optical system will be evaluated over the next year. Thus, we are now turning our attention to increasing the range of these systems.

Thank you.

Chairman HOLIFIELD. We will convene tomorrow morning in room P–63, the old Supreme Court chamber, in the Capitol Building.

The meeting is adjourned until 10 a.m. tomorrow.

(Whereupon, at 4:20 p.m., Wednesday, July 26, 1961, the Joint Committee was adjourned to reconvene Thursday, July 27, 1961, at 10 a.m.)

DEVELOPMENTS IN THE FIELD OF DETECTION AND IDENTIFICATION OF NUCLEAR EXPLOSIONS (PROJECT VELA) AND RELATIONSHIP TO TEST BAN NEGOTIATIONS

THURSDAY, JULY 27, 1961

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The Joint Committee met at 10 a.m., pursuant to notice, in room F–39, the Capitol, Hon. Chet Holifield (chairman of the Joint Committee) presiding.

Present: Representative Holifield, Senators Pastore, and Jackson, Representatives Price, Van Zandt, Bates, and Westland

Also present: James T. Ramey, executive director; John T. Conway, assistant director; George F. Murphy, Jr., professional staff member; Richard T. Lunger, staff consultant; and Doyle Northrup, technical consultant, Joint Committee on Atomic Energy.

Chairman HOLIFIELD. The committee will be in order.

Testimony on Vela Uniform, the detection of underground tests, and Vela Sierra, the detection of space tests from ground bases, has been completed.

During this morning's session the committee will receive testimony on the Vela Hotel Project, which concerns the detection of tests by satellite-borne instruments. In addition to a general description of the Hotel phase, the testimony will include requirements for detection instrumentation and the satellite systems necessary for their support.

The afternoon session will include a discussion of the role of the Vela program in relation to the nuclear test ban by Ambassador Arthur H. Dean, U.S. representative to the conference on the discontinuance of nuclear weapons tests.

Our first witness this morning is Maj. John Poulson, U.S. Air Force, Advanced Research Projects Agency. Major Poulson will present a general description of the Vela Hotel Project.

Major, you are invited now to come forward and take the witness chair. You may proceed.

STATEMENT OF MAJ. JOHN A. POULSON,[1] USAF, CHIEF, VELA HOTEL BRANCH, ADVANCED RESEARCH PROJECTS AGENCY

Major POULSON. Mr. Chairman and members of the Joint Committee: The Vela Hotel portion of the Vela Project consists of research and development associated with space based techniques for detecting nuclear detonations which occur beyond the sensible atmosphere of the earth. Last year, before this committee, it was pointed out that the use of neutron, X-ray, and gamma ray detectors appeared to offer the best approach for early development of a practical satellite based operational detection system. That is still the situation today. At that time, ARPA presented the problem of detecting nuclear detonations in space as being dependent primarily on the following five factors:

1. The amount of radiation emitted from a nuclear detonation.
2. The distance between the detonation and the detection system.
3. The natural radiation background or environment in which the detection system is operating.
4. The sensitivity of the detection system sensors and associated electronic instrumentation.
5. Any shielding or other countermeasures which might be employed to conceal the nuclear detonation.

Over the past year significant progress has been achieved in establishing the basis of an R. & D. program which examines in detail the interrelationship of these five factors. First, the analytical effort to date has been mainly oriented to providing technical support for our international negotiations. This has culminated in the United States proposing, as part of the complete text of a nuclear test ban treaty which was recently tabled by the Western Powers at Geneva, a total ban on high altitude tests, and the installation of a control system of ground based and space based detectors generally in accordance with the recommendations of the report of the TWG–1 dated July 20, 1959. Second, extensive planning for conducting an experimental program has been accomplished and the program has been initiated.

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[1] Born: Bay Shore, Long Island, N.Y.; April 26, 1924.
Education: One and one-half years at McGill University, Montreal, Canada, prior to entering military service. Graduated from U.S. Military Academy, West Point, N.Y. in 1949. Received M.S. degree in nuclear engineering from North Carolina State College, Raleigh, N.C. in 1952.
Married, one daughter, 8, and one son, 6.
Assignments: Entered the U.S. Army Air Corps in January 1942 after 1½ years at McGill University and received pilot's wings and commission as a second lieutenant in February 1944. Between April 1944 and January 1945, flew 70 combat missions as a B–25 aircraft commander with the 12th Air Force during the Italian and southern France campaigns. Was promoted to first lieutenant and awarded the Distinguished Flying Cross and the Air Medal with seven oak leaf clusters during this period. Resigned commission and entered West Point in July 1945, graduating in June 1949. Was a flight instructor pilot in the Air Training Command until June 1950. Entered North Carolina State College and graduated in June 1952. Was then assigned to the USAF Air Research and Development Command at Wright-Patterson Air Force Base, Dayton, Ohio, until February 1958. While there, was a project and staff officer in a weapon system management office, last position being Chief, Plans Branch, Directorate of Nuclear Systems. In February 1958 was assigned to the Aircraft Nuclear Propulsion Office, Headquarters AEC, as Chief, Plans Office. In June 1958 was transferred to the Pentagon as the Assistant for Operations to the Assistant Deputy Chief of Staff/Development for Nuclear Systems, Headquarters USAF until September 1959. During this time duties included staff work on the nuclear aircraft, ramjet, rocket, and auxiliary power programs. Was assigned to ARPA in September 1959 to the present. During the first 4 months with ARPA, was the assistant program manager on the Discoverer, Midas, and Samos space programs. In January 1960 became the Chief of the Vela Hotel Branch, current assignment. In this position has been responsible for organizing and initiating the Vela Hotel R. & D. program. During the month of April of this year served as a Department of Defense representative to the Nuclear Test Ban Conference delegation in Geneva for high altitude controls.

The fundamental requirement for performing experimentation is the gaining of knowledge and a better understanding of physical phenomena related to detection problems. The immediate objective is to obtain experimental data on which to base a design of an operational detection system and a definitive evaluation of its control effectiveness. This is considered absolutely necessary since the TWG–1 estimates, which have remained essentially unchanged in the intervening months, are based solely on theoretical calculations.

Before discussing the status of the R. & D. program, I would like to briefly review the present understanding of the relationship between detection capability and the five basic factors mentioned above.

As was pointed out earlier by Dr. Latter, the first two factors are well understood.

We are quite knowledgeable regarding the neutron, X-ray, and gamma ray radiation emanating from a nuclear detonation in space and the behavior of these radiations as they travel great distances through space. However, the latter three factors are not completely understood and are subject to considerable uncertainty at present. The reason for this uncertainty is—

(*a*) The lack of pertinent experimental information on the natural radiation environment in space and the behavior of the proposed detectors in that environment; and

(*b*) The difficulty in trying to predict the actions of a potential violator when certain countermeasure techniques such as shielding offer possibilities for concealment.

I would like to review these two points in turn.

LACK OF BACKGROUND INFORMATION

The basic problem created by the existence of the natural radiation background is that the detectors, designed to sense the radiation signals from a nuclear detonation, are also sensitive to the background radiations. This creates a requirement for discriminating the nuclear detonation signals from the spurious signals caused by the background. In order to satisfy this discrimination requirement for each individual detector, stringent criteria have to be established for the nuclear detonation signals, and rather complex electronic equipment have to be added to analyze the signals sensed by the detector to determine whether or not they meet the criteria. This procedure eliminates most of the spurious signals picked up by the individual detectors. However, there still remains the definite possibility that some spurious signals will pass through this screening process on the individual detectors and will be registered as an "interesting event" or a "false alarm." By imposing additional criteria of time correlation between several detectors in one satellite and between two or more satellites, the overall "false alarm" rate for the system can be adjusted to an extremely low value, such as 1 per 100 years. This concept is based on statistical theory, and is the heart of our current technical approach.

There are basically two types of "false alarms" which can result from the natural background: pulse phenomena and statistical "bunching."

1. Pulse phenomena as used here are bursts or pulses of radiation of time duration of microseconds or less which might simulate the X-ray or gamma ray pulses from a nuclear detonation. Localized

radiation bursts or "showers" originating within the spacecraft caused by cosmic ray collisions with the spacecraft structure or integral equipment will result in "false alarms" of this nature and are expected. The frequency of occurrence of these "showers" directly affects detection capability. Obtaining data on this phenomenon is one of the primary goals of the R. & D. program.

2. Statistical "bunching" as used here results from statistical fluctuations in the so-called "steady-state" radiation background. A prime source of the "steady-state" background is the sun and the more pronounced fluctuations in solar activity are known as "solar flares." The steady-state background is thought to be composed of randomly spaced radiation quanta which vary in intensity with time. Statistical theory predicts certain finite probabilities that the radiation quanta will, every so often, be so closely spaced or "bunched" that the criteria for an "interesting event" or "false alarm" will be satisfied for individual detectors. This type of "false alarm" is expected and has been provided for in the design philosophy of the detection equipment by incorporating an automatic energy threshold adjustment on each detector.

A helpful way of looking at this might be to consider the steady-state background as a steady noise level in which all the detectors are immersed. This establishes the minimum strength which the signals we are looking for must have. During a solar flare this noise level may increase many orders of magnitude. This means that the signals we are looking for must be much more intense than before if we are to see them. By incorporating on each detector an automatic energy threshold adjustment which is synchronized with the fluctuations in the noise level of the background, the detectors can remain in operation during solar flares at a constant false alarm rate of 1 per 100 years, but with reduced sensitivity.

Our present design objective is to be able to automatically adjust to background noise fluctuations over dynamic ranges of eight to nine orders of magnitude. It is hoped that this will prevent our instruments from becoming saturated with no detection capability for any period of time. The investigation of the statistical fluctuation in the radiation background over long periods of time is another area on which great emphasis is being placed in the R. & D. program. The possibility that the steady-state background will dictate relatively high thresholds which would result in relatively low detection ranges is another source of uncertainty in our estimate of detection capability. Here again we will not be certain until experimental data are obtained.

An attractive feature of our current technical approach is that the experimental satellite flights and any subsequent operational flights will provide statistical information beginning with the initial flight from which "false alarm" rate data can be calculated. Practically speaking, this means that we will be able to calculate more and more accurately with time the probability that a suspicious event was, in fact, a nuclear detonation. Thus, operations might start with a relatively low confidence probability that the system saw a nuclear detonation if and when all the alarm criteria were satisfied. However, with favorable progress in data collection and analysis, this confidence probability could be raised to relatively high levels after a few years—high enough possibly to convince a reasonable person that a nuclear

detonation had, in fact, occurred. (NOTE.—This could be particularly true if significant correlation with other information, such as intelligence or space tracking data, were achieved.)

Before leaving the subject of "false alarms," I would like to briefly mention the possibility that certain physical phenomena might exist in nature on which there is, at present, no rational theoretical basis for anticipating their existence. I am referring to the possibility that microsecond pulses of radiation, not predicted by statistical theory, might originate outside the spacecraft somewhere in the cosmos through an undiscovered physical process. If these phenomena are found to exist, a reappraisal of our current technical approach would have to be made.

Chairman HOLIFIELD. That is just by way of saying that you would be completely wrong in your present approach and your R. & D. would be wasted?

Major POULSON. Sir, I do not believe I would go that far. Of course we would have to wait to see what the frequencies of these are, but I believe that the R. & D. program as it is now planned is quite basic in nature, and the information we are planning to get we are going to have to get in any case in order to be able to understand the performance of future detection systems. So I do not think I could agree that it is wasted.

As a matter of fact, I think that this is the type of program we have to start with in any case.

Chairman HOLIFIELD. You would be depending on orbiting satellites, a number of them, for this information?

Major POULSON. For the operational system; yes, sir. I think the R. & D. program will be explained in more detail by subsequent witnesses, and they will discuss the number of satellites being planned.

Representative VAN ZANDT. Let me ask you this question, Major. In your statement, you used terms we use in the Navy when employing sonar. I am wondering whether or not you find any of the problems in space found in underwater utilization of sonar.

Major POULSON. The propagation of waves? Well, of course, sonar is based on sound waves, which are compression-rarefaction type waves, which are different from the electromagnetic waves that we are talking about here as far as the physical phenomena causing the waves are concerned. But many of the sensing techniques, the range and position finding techniques, are quite similar.

Representative VAN ZANDT. Yes. That is what I thought. Thank you.

Major POULSON. The preceding discussion indicates that all detectors are limited, for practical purposes, to a minimum signal-to-noise ratio which establishes the detectable range for each device detonated under the specific background environmental conditions existing at that time.

The current theoretical predictions of the detection capability against unshielded megaton detonations of an early satellite-based system have not changed significantly from TWG–1 estimates and are as follows: neutron, gamma ray, and X-ray detectors provide redundant detection coverage out to about 1 million miles. From about 1 million miles to perhaps tens of millions of miles, gamma ray and X-ray detectors provide redundant coverage. Beyond this to 100 million miles or more only the X-ray detectors provide coverage.

POSSIBILITIES FOR CONCEALMENT

I would like to discuss the second major source of uncertainty and the fifth factor on the list of items affecting detection capability—the possibilities for concealment through such techniques as the employment of simple shields. By positioning mass between the device to be tested and the detectors, major reductions in the intensity and energy spectrum of the X-ray and gamma ray signals traveling in the direction of the detectors can be achieved. For proper perspective in the discussion that follows, it should be kept in mind that much more shielding is required to attenuate the gamma ray signal to the same degree as the X-ray signal.

From the estimated detection capabilities listed above, it is noted that a violator would be forced to test unshielded devices beyond 100 million miles—a formidable undertaking at least for the immediate future. Possibly a more attractive approach from the violator's point of view would be to circumvent the X-ray detectors by testing at several tens of millions of miles (beyond the range capability of the gamma ray detectors) and positioning a thin, lightweight shield between the device and the detection system. With currently projected rocket technology these distances can be reached after waiting times of 1 to 4 months, depending upon the excess velocity over escape velocity. To enhance his probability of testing without detection, a violator might also wait until a time of heavy solar activity when he knows that the sensitivity of the detectors is minimal.

Chairman HOLIFIELD. Would that not also affect the telemetering of information back from the explosion?

Major POULSON. No, sir.

Chairman HOLIFIELD. It would not?

Major POULSON. No, sir. Telemetering is different. The telemetry receivers would be on the earth and would not be significantly affected by the solar flares. There might be some minor perturbations, but it is a different situation entirely.

Chairman HOLIFIELD. So the advantage is on the part of the violator in this instance?

Major POULSON. That is correct, sir. There is a problem of being able to time the detonations to coincide with solar flares. This certainly would appear to be technically feasible. But it obviously does create problems for him in trying to synchronize his timing to this extent.

Representative VAN ZANDT. Mr. Chairman?

Establishing 100 percent for a complete return of this type of a test, where it is within reach of all your instruments, photography, and so forth, would it be proper to say that with this type of a shot, millions of miles away from the surface of the earth, he would only get 50 percent of his return in the way of evaluation?

Major POULSON. I think the best way to answer that would be to say that if he gets any information at all, it will be sufficient for the purposes of his test.

I believe last year Dr. Teller and other people testified on this point, and pointed out that in some respects testing in space entails disadvantages relative to testing in the atmosphere. In other respects, it is an advantage, in that you do not have the interactions of the radiations with the atmosphere to worry about.

You have to look at each individual test and the objectives of the test to make a meaningful statement. But in any case, basic information as to the energy release of the detonation and some of the key physical phenomena which relate to design criteria of the weapon can be obtained from a space test.

Representative VAN ZANDT. And the answer is that the return he gets will be worth while?

Major POULSON. Yes, sir.

The violator must also incorporate provisions which insure that the test operation will "fail safe" if anything goes wrong. In other words, he must prevent an inadvertent detonation which would look like a beacon in the night to the X-ray detectors if, for instance, the shield did not deploy or was not properly positioned. Although all of the above operations have not yet been demonstrated, there does not appear to be any inherent reason why the technological capability could not be established given the necessary time, money, and personnel for development.

A prime objective of any detection system is to complicate a violator's ability to conduct useful clandestine tests, to increase his cost, and to decrease his confidence of being able to test without discovery. From this standpoint, the requirements for long waiting periods, attitude control, shield deployment, data retrieval over long distances, and "fail safe" provisions which are imposed on a violator, certainly complicate his problem. In view of all the factors involved, it must be concluded that it is technically feasible for a determined violator to test clandestinely. However, he is never completely certain of success on any particular attempt, and his chances of being discovered go up with the number of attempts he makes.

Representative VAN ZANDT. What about the cost factor? Suppose he wants to set up machinery to conduct these tests?

Major POULSON. Clandestine tests, sir?

Representative VAN ZANDT. Yes. Would the costs be enormous?

Major POULSON. About all we can say now is that his costs would be greater, obviously, than if there were no detection system up there. If there were no detection system, he would be offered the easy solution of sending up a single stage rocket a few hundred miles, or a thousand miles or so, right over the middle of Siberia, and obtaining his data rapidly.

Representative VAN ZANDT. The cost would be the rocket?

Major POULSON. The cost would essentially be a simple single stage rocket. On the other hand, if you force him to go to great distances, he automatically has to go to multistage rockets, to complex telemetry and so on. It is the old question: If you spend money for detection capability, what do you impose in terms of money or economic factors on the violator?

There are no absolute numbers on which to say X is greater than Y, or Y is greater than Z.

I think philosophically speaking, if you do not have any detection capability at all, then a lot of the discussion on the underground situation is academic, because it is awfully easy to launch a single stage rocket and test a few hundred miles up.

Representative VAN ZANDT. Then the more money spent on the detection system, the more money he will have to spend testing clandestinely?

Major POULSON. Yes, up to a point. Of course, this the economic factor. There are a lot of other factors involved such as military factors and political factors. But economically speaking, it is just impossible to say. In the final analysis, you have to look at the objectives of a test program. One test will not be, overall, significant in altering the balance of nuclear technology. I think a violator would have to conduct a series of tests. Then you would have to look at the series of tests and what type of tests, and so forth. So you do get into a rather complex analysis, here.

Representative VAN ZANDT. Under present-day conditions, could he shoot a rocket and tell the world about it afterward, after setting up a barrier shield that would hide an outer space test on his part?

Major POULSON. Well, he would have had to have started preparations 2 years ago to do this today. Of course, we do not know. But, if he started today and worked forward, he definitely could have this capability in, say, 2 to 3 years.

Representative VAN ZANDT. Could this type of a rocket be considered just another test on his part?

Major POULSON. I am sorry, sir. You asked me the question: Could he launch a space vehicle undetected? Of course, as you know, the North American Defense Command has a program of trying to improve and increase our space tracking capability. Three years from now, whether or not a violator could, with impunity, launch spacecraft without us knowing about it at all, is problematical. I do not think there is a clear-cut answer to that.

Representative VAN ZANDT. What about BMEWS?

Major POULSON. I am speaking more about the space tracking system. At the present time, BMEWS is primarily an early warning system. There are plans to make modest modification to BMEWS data handling to increase its satellite intercept capability. We have in existence, and are planning to expand the coverage of, detection fences to be able to detect anything that goes into orbit. And from there, we would like to extend capability to be able to detect any spacecraft at all, whether it goes into orbit or not. This is a very severe requirement at present and is, therefore, more in the future. But I think eventually we will possess this capability.

Representative VAN ZANDT. Thank you.

Chairman HOLIFIELD. Would these stations be something like this English installation?

Major POULSON. The BMEWS station in England?

Chairman HOLIFIELD. No; the kind that you plan to put in, the tracking stations.

Major POULSON. The tracking stations would be something like—well, ARPA originated the first net, the ARPA net, a net across the southern part of the United States. This is for detecting satellites, passive satellites that do not radiate, up to maybe several hundred or so miles. We want to extend that range and then close the gap between about 30 degrees north latitude to 30 degrees south latitude by installing a similar station near the Equator.

All of these things are being studied by Norad.

Chairman HOLIFIELD. These are similar to—what is the name of the bank?

Major POULSON. The Jodrell Bank is not really part of this net, sir. It is primarily to spot active spacecraft, spacecraft that radiate information; in other words, telemeter information back to the earth. It is a single purpose type of facility.

Chairman HOLIFIELD. All right. You may proceed.

Major POULSON. In summary, while a detection system cannot be made 100 percent foolproof, neither is a violator 100 percent certain he will escape detection. Correlation of intelligence information and other data such as that available from the space detection and tracking system will increase the inherent risk faced by a violator. Under favorable circumstances, a determined violator willing to take the risk can reduce his probability of detection to a relatively low value. These statements pertain only to the present technological estimates of the capability of an early detection system, and do not attempt to predict improvements which could result from the R. & D. program.

Consideration of detection vis-a-vis clandestine testing leads to the obvious conclusion that the relative situation is never static; it changes with time and technological progress. This, of course, leads to the conclusion that a continuing R. & D. program on detection is required in an attempt to stay ahead of, or at least neutralize, the testing art. This is a convenient lead into a discussion of the present Vela Hotel R. & D. program.

Under the preceding discussion, I have attempted to explain the primary things that we are placing emphasis on. Now I would like to discuss the program itself.

The specific technical objective of the Vela Hotel R. & D. program is to experimentally confirm detector sensitivity, reliability, and system performance capability in the space environment and to investigate the effectiveness of various countermeasures such as shielding. In order to satisfy this objective, it will be necessary to conduct experiments and technical analyses in order to obtain data on the natural radiation environment in space and on the behavior of various detectors in this environment and to test preprototype detection equipment—all leading to the definition of operational surveillance systems and the specification of their capability. The program does not include the establishment of an operational capability. It is oriented initially to an applied research effort through 1964 for establishing a state of technology on which an operational far earth satellite system could be defined and designed with capabilities not less than that estimated by the Geneva Technical Working Group 1. The schedule proposed for the applied research is compatible with providing the technology for satellite systems phasing as outlined in the draft treaty recently tabled at Geneva.

An operations analysis will be conducted concurrently with the applied research program in order to evaluate the interrelationship between the technical, political, military, and economic factors which should be considered in arriving at decisions regarding the most effective means for establishing adequate control.

If the program were continued beyond this initial phase, a progressively improving state of technology should result which would form the basis for successively more effective operational surveillance

capability. The specific nature of the improvements cannot be predicted at the present time. However, possible means for detecting nuclear detonations in space other than X-ray, gamma ray, and neutron sensing will be investigated. An attractive possibility in this area is radiofrequency pulse detection.

The specific program currently underway resulted from an extensive analysis of technical problems related to detection jointly conducted by an Air Force/AEC/NASA planning team established by ARPA. The team submitted an R. & D. program plan which was reviewed by a panel of distinguished scientists headed by Dr. Latter and Dr. Panofsky and by ARPA, Headquarters AEC, and Headquarters NASA. The program consists of a sequential effort starting with piggyback flights on balloons and NASA/DOD space vehicles and phasing into experimental satellite flights at far earth altitudes (about 50,000 to 75,000 n.m.). The information and data received from the piggyback flights will be used in the design of the experimental satellite instrumentation payloads. I will not delve into the technical details of the program, since Dr. Harold Argo from Los Alamos Scientific Laboratory and Dr. Byron P. Leonard from Aerospace Corp. will cover the salient aspects in their talks.

By mutual agreement between the DOD and AEC, the DOD (ARPA) has overall responsibility for the Vela Hotel program. The AEC will support the DOD by providing the detectors and associated electronic analyzing equipment. Under the supervision of ARPA, detailed technical supervision of the AEC and DOD portions of the program will be the responsibility of a Joint AFSC/AEC Technical Group. This group, acting through normal channels in each agency, will be responsible for establishing specifications, insuring complete coordination among the various tasks, and providing technical direction to the contractors. Close technical liaison will be maintained with NASA throughout the program.

SUMMARY

From the above discussion, I believe it is apparent that there is an urgent need for conducting an applied research program to provide experimental data which will increase our basic knowledge and understanding of the physical phenomena which affect satellite-based detection of nuclear detonations in space. The effort underway to accomplish this effectively integrates the capabilities of three governmental departments and industry by utilizing skills already established under other programs.

This ends my formal presentation.

Chairman HOLIFIELD. Thank you, Major Poulson.

I notice on page 8 at the bottom of the page you say:

> The program does not include the establishment of an operational capability. It is oriented initially to an applied research effort through 1964 for establishing a state of technology on which an operational far earth satellite system could be defined and designed with capabilities not less than that estimated by the Geneva Technical Working Group 1.

Now, the date, there, 1964, for your applied research effort, interests me, and I would like to ask you how many years it would take you to establish an operational capability beyond 1964.

Major POULSON. This depends on what we do over the next several years, up to 1964. In other words, there would be two possibilities, here, sir. If we were to make a decision today, or within the next few months, and say we have to go through this basic R. & D. program to establish the technology, but at the same time we will do our planning necessary for building up and phasing into an operational system as soon as possible, then I think we could live within the 4-year time period that is stated in the draft treaty. And this in effect was the basis of our proposing it in the draft treaty.

Let me say that there is an inherent risk in this approach, in that if we come up against some unexpected pitfall, technological pitfall, in the R. & D. program, this obviously would stretch out the program. But assuming we were successful all the way through, we could meet the date of 4 years from today.

If, on the other hand, we proceeded today with the R. & D. program and await the results of that program before we made a decision to phase into an operational capability, then I think it would take about an additional 2 years after we had the initial results, if they were attractive results. It would take about 2 years after that to establish an operational capability.

Chairman HOLIFIELD. Of course, you have not been funded for operational capability.

Major POULSON. We have not been funded for operational capability.

Chairman HOLIFIELD. So you are facing 4 years, with a possibility of 5 or 6, depending on funding.

Major POULSON. A possibility of 5 or 6, depending of funding.

Chairman HOLIFIELD. So we add 6, let us say, to the 33 months, and we have a total of 8½ years on that assumption. We have a total of 8½ years in which we would be incapable of detecting——

Major POULSON. Wait a minute, sir. I am not following your arithmetic.

Chairman HOLIFIELD. The 33 months, the moratorium. I am adding the 33 months.

Major POULSON. Oh, from 1958, sir?

Chairman HOLIFIELD. Yes. The moratorium, to the estimated 5 or 6 years.

Major POULSON. Yes, sir; I understand.

Chairman HOLIFIELD. And you come up, under that type of estimate, with between 8 and 9 years of moratorium on testing, assuming that the treaty is signed.

Major POULSON. This would be a maximum. I think we could do better than that if the treaty were signed and we started in the next few months.

Chairman HOLIFIELD. Let us say we cut it to 4 years, and we have already 33 months. So that is 6 years and 9 months, if we maximize our effort and achieve operational capability by the end of 1964 or early 1965.

Major POULSON. Yes, sir.

Chairman HOLIFIELD. The committee has observed an item on page 10 in the list of Vela Hotel contracts whose purpose is obscure to me. The item in question is entitled "Environmental Test Satellite," contracted for the amount of $1,998,000. Would you kindly enlighten

the committee on the relationship of this project to the rest of the Vela Hotel program, and what have been the specific results of this program initiated about 9 months ago?

Major POULSON. First let me tell you what the program is. This is a program whereby ARPA and NASA, are providing two separate spacecraft to fly on top of three Atlas-Centaur vehicles, during the Atlas-Centaur development program, which is under the jurisdiction of NASA. The particular spacecraft, which ARPA is sponsoring, has several purposes:

Primarily, it will carry a Vela Hotel package, the initial experimental equipment on the radio frequency pulse detection idea, which I mentioned briefly in my prepared statement, sir. In addition to this, it will carry several other experiments which measure the background radiations and the fluctuations of these radiations with time in the synchronized 24-hour orbit.

This information, in conjunction with the information that is being provided by the NASA spacecraft, will give us a better understanding, a more complete understanding, of the nature of the background and its fluctuations with time. This will be certainly of direct interest to the Vela Hotel program.

And finally, it will also carry electromechanical equipment like bearings or different coatings for temperature control in satellites, things of this nature, which every satellite must have in order to operate. It will test these for the benefit of space programs in general, one of which is the Vela Hotel program.

It so happens that it will benefit most space programs. In fact, we have coordinated the individual experiments with the Air Force's Midas program, the Navy's Transit program, and the Army's Advent program, and so forth.

Chairman HOLIFIELD. Have there been any initial results?

Major POULSON. No, sir. The shots will not go for about a year. So we do not have any specific results to offer at this time.

Chairman HOLIFIELD. Are you in the planning stage now?

Major POULSON. It is in the final design stage, and environmental testing on components has started. The hardware is being fabricated.

Chairman HOLIFIELD. Yesterday Comander Chandler testified that, in his opinion, Vela Sierra and the Hotel programs are complementary. The committee notes that whereas the Vela Sierra program appears to be well underway, the list of contracts for Vela Hotel shows that in the the 20 months that have elapsed since Vela Hotel was assigned to ARPA, only about $5 million has been obligated for Vela Hotel. Mr. Beyer's testimony indicated that plans for Vela Hotel would involve satellite launchings, but he did not indicate the overall cost of the Vela Hotel program. The committee would be interested in knowing the reason for the disparity in these two complementary programs, and an estimate of the funds required for Vela Hotel to reach the stage of development of a system cited by Mr. Beyer for 1964.

Major POULSON. Well, one of the unfortunate and unhappy aspects that I have to contend with on Vela Hotel is that the experimental equipment is a relatively minor part of the cost. In other words, the lion's share of the money that I need goes into such things as spacecraft, boosters, and this type of equipment. We have approval now

for about a $64 million program over the next 3 to 3½ years. In the final analysis, the bulk of this will go into such things as the ones I just mentioned.

On the Vela Sierra program—where I think they are talking about $2 to $3 million a year—the bulk of that money will actually go into the experimental apparatus which operates on the ground. They do not have the additional cost associated with space vehicles and boosters and things of this nature.

Does that answer it, sir? I am not sure I answered the whole thing.

Chairman HOLIFIELD. I think that pretty well answers it.

Major POULSON. I will say, in addition to satellite program we have a rather extensive preliminary program, that of flying equipment piggyback on balloons, space probes, and DOD/NASA spacecraft.

Chairman HOLIFIELD. You are working right in with the existing satellites, and attaching some of your instruments piggyback?

Major POULSON. That is right, sir.

Chairman HOLIFIELD. We have also noted that large portions of Vela Uniform and Sierra are supervised in detail by AFTAC, yet none of Vela Hotel. Can you clarify for the committee the reasons for this differentiation among the programs?

Major POULSON. I am sorry, sir. Would you say that again?

Chairman HOLIFIELD. Your Vela Uniform and Sierra are supervised in detail by AFTAC?

Major POULSON. Yes, sir.

Chairman HOLIFIELD. And none of the Vela Hotel. Why is it separated from it?

Major POULSON. Well, let me put it this way. As far as the detailed technical supervision of the R. & D. program is concerned, in discussing this about a year and a half ago with the Air Force and with the Air Research and Development Command, General Schriever, it was decided that the technical supervision of the R. & D. program in space should be done by the people with the experience in this field. This would be the Ballistic Missiles Division, which is now the Space Systems Division, which is part of what is now known as the Air Force Systems Command, under General Schriever and General Estes.

At the same time, ARPA recognized that the people most interested in the results would be AFTAC. And so we have made a concerted attempt to provide them with all the information and ask them for technical recommendations as we have gone along in the program.

But as far as the detailed technical supervision of the hardware effort was concerned, it was decided by the Air Force that the Air Research and Development Command was to handle it.

Mr. RAMEY. On your instrumentation, I did not follow all of it, but has there been any consideration of the use of SNAP devices for your power supplies?

Major POULSON. Well, not serious consideration. It has been thought about it. But I would say that our power requirements are low enough so that we do not have to go to a SNAP type power supply. A solar power supply which has been proven to be reliable and operationally adequate is the direction in which we are going at the present time.

Mr. RAMEY. How about in terms of length of use?

Major POULSON. Right now, our design objective is 6 months' lifetime for the R. & D. program and there appears to be no problem with utilizing solar cells. We do not believe there will be problems with solar cells as far as reliability is concerned. We could be wrong on this. If we meet some very deleterious types of radiation background at these altitudes, it could be harmful to the solar cells, but we do not expect it. We think our reliability problems will primarily involve the breaking down of components and parts in the electronic circuitry.

Mr. RAMEY. How about in the operational stage?

Major POULSON. This could be another question, and I do not think we have considered it sufficiently to determine today whether we would go to a SNAP device or solar cells; although right now, if I were asked to make a prediction, I would say we would still go to the solar cells for the operational system.

Mr. RAMEY. That is sort of the path of least resistance. Since this is an atomic energy committee, we always ask the question about utilizing atomic devices, which would certainly seem to have a longer period of operational capability.

Major POULSON. Well, I think I am a little familiar with the SNAP program, and I think the problem really is that the basic weight involved would be very detrimental to us. We do not need an equivalent amount of weight for a solar power supply, because our power requirements are relatively low.

Mr. RAMEY. There is one of them——

Major POULSON. The radioisotope SNAP? These have a limited lifetime, do they not?

Mr. RAMEY. Five years.

Major POULSON. Well, if this particular power supply does become available, I think we would probably take another look at it.

Representative PRICE. Major Poulson, do you agree with the scientific judgments expressed by Dr. Latter in his statement to the committee either yesterday or the day before, as follows:

> And finally, although minor improvements for research are foreseen, research has as yet given no clue as to means for improving the Geneva network sufficiently to permit detection and identifications of shielded tests in space?

Major POULSON. Well, I think we are talking about individual personal judgments, of what our feelings are on trying to predict results in an R. & D. program. I guest I come from a more optimistic school, particularly from the standpoint of such things as radio frequency pulse detection. This is an approach which, if it pans out, would negate the usefulness of shielding as a countermeasure technique, in that the more shielding one used, the better, we think, the radio frequency pulse signal, or the more intense the radio frequency pulse signal, might be.

But we are really talking about purely theoretical aspects of the problem and, and certainly his judgment is as good as anybody else's in this area.

Representative PRICE. You were familiar with his statement?

Major POULSON. Yes, sir.

Representative PRICE. The committee notes on pages 1 and 2 in your statement that Vela Hotel analytical results have been mainly oriented toward providing technical support for our test ban negoti-

ations and have culminated in the United States proposing a total ban on high altitude tests, and the installation of ground-based and space-based detectors, generally in accordance with the report of Technical Working Group I, on July 20, 1959. Your statement further states that the Technical Working Group I estimates have remained essentially unchanged in the intervening months.

Since there have been a number of intervening months since July 20, 1959, would you please specify those parts of the U.S. tabled position on high altitude detection which were based upon the analytical effort you mentioned, and which were based upon the recommendations of Technical Working Group I?

Major POULSON. I think that is a rather complex question.

Representative PRICE. May I suggest, then, if you do not care to answer this, at the moment, that you submit a reply for the record?

Major POULSON. I do not want to duck it, sir.

Representative PRICE. I am not assuming that you would want to duck it, and I know that you would not. But it is a complex question, one that we would like to have treated in the official record, and we would appreciate it if you would submit a reply for the record.

Major POULSON. All right, sir.

(The material referred to follows:)

The U.S. tabled position on high-altitude detection is based on both theoretical and engineering estimates. The theoretical estimates stem directly from the recommendations of TWG I. The engineering estimates stem directly from the extensive analytical work accomplished within the Vela Hotel program during the intervening months to the present. It so happens that this analytical work also probed into the theoretical estimates presented in the TWG I report and resulted in a conclusion that the report still represented the best authoritative source of information on the subject. In addition, no new experimental data have become available in the interim to cause a change in these theoretical estimates. Since the TWG I report did not consider the engineering practicability of achieving a satellite-based detection capability, the Vela Hotel analytical effort filled this gap. As a result, current estimates of the situation on high altitude remain essentially unchanged from those presented in the TWG I report with the additional confidence that the nondetection aspects can be achieved through straightforward engineering practice.

Representative PRICE. The committee notes that you specify in the R. & D. program a specific objective to experimentally confirm present theoretical estimates, and among other things the effectiveness of various countermeasures, such as shielding. The committee would be interested to know how you plan to experimentally confirm shielding effectiveness if not by actual nuclear space tests.

Major POULSON. There are possibilities for investigating the physical processes associated with the theoretical analyses on shielding which have been done to date, by using underground tests, for instance, such things as a long evacuated tube as close to a vacuum inside as possible with thin plates of lead or other materials has been mentioned. This would verify, we hope, to a great extent, some of the shielding calculations that have been done to date.

In other words, what I am saying is that there are techniques which we will consider very closely in the next several months to determine if we can mockup or simulate a shielding experiment with an underground nuclear detonation. In order to examine shielding techniques closely, we do need a nuclear detonation somewhere. We do not necessarily need it in space.

Representative PRICE. Major Poulson, in glancing over your biographical sketch, there is a phrase in here that promptly caught my eye. I note that you have a very distinguished military record with the Air Force, an excellent combat record. I also note that you were associated with and bear the scars of the losing battle of the ANP program. You have stated a moment ago that you were more optimistic than some people on another matter. I wonder how far you would carry your optimism on the nuclear-powered aircraft. Would you care to hazard your own personal opinion whether someone someday will successfully fly a nuclear-powered airplane?

Major POULSON. I am glad you asked me for my personal opinion, because that gives me quite a bit of leeway on that subject.

I would like to make one statement for the record, sir, that I feel very strongly about. I think one of the finest Air Force officers in the technical services was General Kearn. And I think that the way he managed the ANP program was outstanding. He is retired now, so that is why I feel free to make this statement. I know he has come under a lot of criticism over the years. But knowing the time situation from the inside as it were, those criticisms were unjust.

As far as my feeling as to the ANP program is concerned, I think that the technical direction which General Kearn provided over the period of years was certainly astute and oriented in the direction in which the greatest amount of progress could be made, even though there were ups and downs in official approval and so forth.

As far as the applications of nuclear power to aircraft are concerned, there is absolutely no question in my mind that someday this will come. I just hope when it does that the United States will be in the vanguard.

Representative PRICE. You know, I am certainly glad I asked you the question. I did not know what your reply would be.

Major POULSON. Well, let me make this further statement, sir. I feel that if the Polaris concept is good, which it is, then an atmospheric extension of this concept should also be good, which it is. And this is one of the reasons why I do not understand why we can be so enthusiastic about the underwater concept and not about the atmospheric concept.

Mr. RAMEY. Well, we have the Skybolt system.

Representative PRICE. Some people think that there is no military mission for nuclear-powered aircraft. I am glad that you have named at least 1 here, and I have named about 15 of them in various statements I have made.

I also want to say that I share your opinion of the good work of General Kearn and also of General Branch. The trouble has been that these people who really knew something about it were never consulted before basic and final decisions were made.

Major POULSON. I have no comment on that one, sir.

Representative PRICE. I do not ask you to comment, but I am certain if you did, it would be in agreement with what I have just said.

Are there any questions from the staff?

Thank you very much, Major Poulson.

(Major Poulson's formal statement follows:)

STATEMENT BY MAJ. JOHN A. POULSON, U.S. AIR FORCE, CHIEF, VELA HOTEL BRANCH, ADVANCED RESEARCH PROJECTS AGENCY

Mr. Chairman and members of the joint committee, the Vela Hotel portion of the Vela Project consists of research and development associated with space-based techniques for detecting nuclear detonations which occur beyond the sensible atmosphere of the earth. Last year, before this committee, it was pointed out that the use of neutron, X-ray, and gamma ray detectors appeared to offer the best approach for early development of a practical satellite-based operational detection system. That is still the situation today. At that time ARPA presented the problem of detecting nuclear detonations in space as being dependent primarily on the following five factors:

1. The amount of radiation emitted from a nuclear detonation.
2. The distance between the detonation and the detection system.
3. The natural radiation background or environment in which the detection system is operating.
4. The sensitivity of the detection system sensors and associated electronic instrumentation.
5. Any shielding or other countermeasures which might be employed to conceal the nuclear detonation.

Over the past year significant progress has been achieved in establishing the basis of an R. & D. program which examines in detail the interrelationship of these five factors. First, the analytical effort to date has been mainly oriented to providing technical support for our international negotiations. This has culminated in the United States proposing, as part of the complete text of a nuclear test ban treaty which was recently tabled by the Western Powers at Geneva, a total ban on high altitude tests and the installation of a control system of ground-based and space-based detectors generally in accordance with the recommendations of the report of the TWG I dated July 20, 1959. Second, extensive planning for conducting an experimental program has been accomplished and the program has been initiated. The fundamental requirement for performing experimentation is the gaining of knowledge and a better understanding of physical phenomena related to detection problems. The immediate objective is to obtain experimental data on which to base a design of an operational detection system and a definitive evaluation of its control effectiveness. This is considered absolutely necessary since the TWG I estimates, which have remained essentially unchanged in the intervening months, are based solely on theoretical calculations.

Before discussing the status of the R. & D. program, I would like to briefly review the present understanding of the relationship between detection capability and the five basic factors mentioned above.

As was pointed out earlier by Dr. Latter, the first two factors are well understood. We are quite knowledgeable regarding the neutron, X-ray, and gamma ray radiation emanating from a nuclear detonation in space and the behavior of these radiations as they travel great distances through space. However, the latter three factors are not completely understood and are subject to considerable uncertainty at present. The reason for this uncertainty is—

(*a*) The lack of pertinent experimental information on the natural radiation environment in space and the behavior of the proposed detectors in that environment; and

(*b*) The difficulty in trying to predict the actions of a potential violator when certain countermeasure techniques such as shielding offer possibilities for concealment.

I would like to review these two points in turn.

LACK OF BACKGROUND INFORMATION

The basic problem created by the existence of the natural radiation background is that the detectors, designed to sense the radiation signals from a nuclear detonation, are also sensitive to the background radiations. This creates a requirement for discriminating the nuclear detonation signals from the spurious signals caused by the background. In order to satisfy this discrimination requirement for each individual detector, stringent criteia have to be established for the nuclear detonation signals, and rather complex electronic equipment have to be added to analyze the signals senses by the detector to

determine whether or not they meet the criteria. This procedure eliminates most of the spurious signals picked up by the individual detectors. However, there still remains the definite possibility that some spurious signals will pass through this screening process on the individual detectors and will be registered as an "interesting event" or a "false alarm." By imposing additional criteria of time correlation between several detectors in one satellite and between two or more satellites, the overall "false alarm" rate for the system can be adjusted to an extremely low value such as one per hundred years. This concept is based on statistical theory and is the heart of our current technical approach.

There are basically two types of "false alarms" which can result from the natural background: pulse phenomena and statistical "bunching."

1. Pulse phenomena as used here are bursts or pulses of radiation of time duration of microseconds or less which might stimulate the X-ray or gamma ray pulses from a nuclear detonation. Localized radiation bursts or "showers" originating within the spacecraft caused by cosmic ray collisions with the spacecraft structure or integral equipment will result in "false alarms" of this nature and are expected. The frequency of occurrence of these "showers" directly affects detection capability. Obtaining data on this phenomena is one of the primary goals of the R. & D. program.

2. Statistical "bunching" as used here results from statistical fluctuations in the so-called steady-state radiation background. A prime source of the steady-state background is the sun and the more pronounced fluctuations in solar activity are known as solar flares. The steady-state background is thought to be composed of randomly spaced radiation quanta which vary in intensity with time. Statistical theory predicts certain finite probabilities that the radiation quanta will, every so often, be so closely spaced or "bunched" that the criteria for an "interesting event" or "false alarm" will be satisfied for individual detectors. This type of "false alarm" is expected and has been provided for in the design philosophy of the detection equipment by incorporating an automatic energy threshold adjustment on each detector. A helpful way of looking at this might be to consider the steady-state background as a steady noise level in which all the detectors are immersed. This establishes the minimum strength which the signals we are looking for must have. During a solar flare this noise level may increase many orders of magnitude. This means that the signals we are looking for must be much more intense than before if we are to see them. By incorporating on each detector an automatic energy threshold adjustment which is synchronized with the fluctuations in the noise level of the background, the detectors can remain in operation during solar flares at a constant false alarm rate of one per hundred years but with reduced sensitivity. Our present design objective is to be able to automatically adjust to background noise fluctuations over dynamic ranges of eight to nine orders of magnitude. It is hoped that this will prevent our instruments from becoming saturated with no detection capability for any period of time. The investigation of the statistical fluctuation in the radiation background over long periods of time is another area on which great emphasis is being placed in the R. & D. program. The possibility that the steady-state background will dictate relatively high thresholds which would result in relatively low detection ranges is another source of uncertainty in our estimate of detection capability. Here again we will not be certain until experimental data are obtained.

An attractive feature of our current technical approach is that the experimental satellite flights and any subsequent operational flights will provide statistical information beginning with the initial flight from which "false alarm" rate data can be calculated. Practically speaking, this means that we will be able to calculate more and more accurately with time the probability that a suspicious event was, in fact, a nuclear detonation. Thus, operations might start with a relatively low confidence probability that the system saw a nuclear detonation if and when all the alarm criteria were satisfied. However, with favorable progress in data collection and analysis, this confidence probability could be raised to relatively high levels after a few years—high enough possibly to convince a reasonable person that a nuclear detonation had, in fact, occurred. (NOTE.—This could be particularly true if significant correlation with other information, such as intelligence or space tracking data, were achieved.)

Before leaving the subject of "false alarms" I would like to briefly mention the possibility that certain physical phenomena might exist in nature on which

there is, at present, no rational theoretical basis for anticipating their existence. I am referring to the possibility that microsecond pulses of radiation, not predicted by statistical theory, might originate outside the spacecraft somewhere in the cosmos through an undiscovered physical process. If these phenomena are found to exist, a reappraisal of our current technical approach would have to be made.

The preceding discussion indicates that all detectors are limited, for practical purposes, to a minimum signal-to-noise ratio which establishes the detectable range for each device detonated under the specific background environmental conditions existing at that time.

The current theoretical predictions of the detection capability against unshielded megaton detonations of an early satellite-based system have not changed significantly from TWG I estimates and are as follows: neutron, gamma ray, and X-ray detectors provide redundant detection coverage out to about 1 million miles. From about 1 million miles to perhaps tens of millions of miles, gamma ray and X-ray detectors provide redundant coverage. Beyond this to 100 million miles or more only the X-ray detectors provide coverage.

POSSIBILITIES FOR CONCEALMENT

Now I would like to discuss the second major source of uncertainty and the fifth factor on the list of items affecting detection capability—the possibilities for concealment through such techniques as the employment of simple shields. By positioning mass between the device to be tested and the detectors, major reductions in the intensity and energy spectrum of the X-ray and gamma ray signals traveling in the direction of the detectors can be achieved. For proper perspective in the discussion that follows, it should be kept in mind that much more shielding is required to attenuate the gamma ray signal to the same degree as the X-ray signal.

From the estimated detection capabilities listed above, it is noted that a violator would be forced to test unshielded device beyond 100 million miles—a formidable undertaking at least for the immediate future. Possibly a more attractive approach from the violator's point of view would be to circumvent the X-ray detectors by testing at several tens of millions of miles (beyond the range capability of the gamma ray detectors) and positioning a thin, lightweight shield between the device and the detection system. With currently projected rocket technology these distances can be reached after waiting times of 1 to 4 months depending upon the excess velocity over escape velocity. To enhance his probability of testing without detection, a violator might also wait until a time of heavy solar activity when he knows that the sensitivity of the detectors is minimal. The violator must also incorporate provisions which insure that the test operation will "fail safe" if anything goes wrong. In other words, he must prevent an inadvertent detonation which would look like a beacon in the night to the X-ray detectors if, for instance, the shield did not deploy or was not properly positioned. Although all of the above operations have not yet been demonstrated, there does not appear to be any inherent reason why the technological capability could not be established given the necessary time, money, and personnel for development.

A prime objective of any detection system is to complicate a violator's ability to conduct useful clandestine tests, to increase his cost, and to decrease his confidence of being able to test without discovery. From this standpoint, the requirements for long waiting periods, attitude control, shield deployment, data retrieval over long distances, and "fail safe" provisions which are imposed on a violator certainly complicate his problem. In view of all the factors involved, it must be concluded that it is technically feasible for a determined violator to test clandestinely. However, he is never completely certain of success on any particular attempt and his chances of being discovered go up with the number of attempts he makes.

In summary, while a detection system cannot be made 100 percent foolproof, neither is a violator 100 percent certain he will escape detection. Correlation of intelligence information and other data such as that available from the space detection and tracking system will increase the inherent risk faced by a violator. Under favorable circumstances, a determined violator willing to take the risk can reduce this probability of detection to a relatively low value. These statements pertain only to the present technological estimates of the capability of an early detection system and do not attempt to predict improvements which could result from the R. &. D. program.

Consideration of detection vis-a-vis clandestine testing leads to the obvious conclusion that the relative situation is never static; it changes with time and technological progress. This, of course, leads to the conclusion that a continuing R. & D. program on detection is required in an attempt to stay ahead of, or at least neutralize, the testing art. This is a convenient lead into a discussion of the present Vela Hotel R. &. D. program.

R. & D. PROGRAM

The specific technical objective of the Vela Hotel R. & D. program is to experimentally confirm detector sensitivity, reliability, and system performance capability in the space environment and to investigate the effectiveness of various countermeasures such as shielding. In order to satisfy this objective it will be necessary to conduct experiments and technical analyses in order to obtain data on the natural radiation environment in space and on the behavior of various detectors in this environment and to test preprototype detection equipment—all leading to the definition of operational surveillance systems and the specification of their capability. The program does not include the establishment of an operational capability. It is oriented initially to an applied research effort through 1964 for establishing a state of technology on which an operational far earth satellite system could be defined and designed with capabilities not less than that estimated by the Geneva Technical Working Group I. The schedule proposed for the applied research is compatible with providing the technology for satellite systems phasing as outlined in the draft treaty recently tabled at Geneva.

An operations analysis will be conducted concurrently with the applied research program in order to evaluate the interrelationship between the technical, political, military and economic factors which should be considered in arriving at decisions regarding the most effective means for establishing adequate control.

If the program were continued beyond this initial phase, progressively improving state of technology should result which would form the basis for successively more effiective operational surveillance capability. The specific nature of the improvements cannot be predicted at the present time. However, possible means for detecting nuclear detonations in space other than X-ray, gamma ray, and neutron sensing will be investigated. An attractive possibility in this area is radiofrequency pulse detection.

The specific program currently underway resulted from an extensive analysis of technical problems related to detection jointly conducted by an Air Force/AEC/NASA Planning Team established by ARPA. The team submitted an R. & D. program plan which was reviewed by a panel of distinguished scientists headed by Dr. Latter and Dr. Panofsky and by ARPA, Headquarters AEC, and Headquarters NASA. The program consists of a sequential effort starting with piggyback flights on balloons and NASA/DOD space vehicles and phasing into experimental satellite flights at far Earth altitudes (about 50,000 to 75,000 n.m.). The information and data received from the piggyback flights will be used in the design of the experimental satellite instrumentation payloads. I will not delve into the technical details of the program since Dr. Harold Argo from LASL and Dr. Byron P. Leonard from Aerospace Corp. will cover the salient aspects in their talks.

By mutual agreement between the DOD and AEC, the DOD (ARPA) has overall responsibility for the Vela Hotel program. The AEC will support the DOD by providing the detectors and associated electronic analyzing equipment. Under the supervision of ARPA, detailed technical supervision of the AEC and DOD portions of the program will be the responsibility of a Joint AFSC/AEC Technical Group. This group, acting through normal channels in each agency, will be responsible for establishing specifications, insuring complete coordination among the various tasks, and providing technical direction to the contractors. Close technical liaison will be maintained with NASA throughout the program.

SUMMARY

From the above discussion, I believe it is apparent that there is an urgent need for conducting an applied research program to provide experimental data which will increase our basic knowledge and understanding of the physical phenomena which affect satellite-based detection of nuclear detonations in space. The effort underway to accomplish this effectively integrates the ca-

pabilities of three governmental departments and industry by utilizing skills already established under other programs.

Chairman HOLIFIELD. The next witness will be Dr. Harold Argo, of the Los Alamos Scientific Laboratory. Dr. Argo will present testimony on the detection instrumentation requirements under the Vela Hotel program.

Dr. Argo?

STATEMENT OF DR. HAROLD V. ARGO,[1] LOS ALAMOS SCIENTIFIC LABORATORY

Dr. ARGO. I have been asked to review the detection instruments that constitute the payload of the forthcoming experimental Vela Hotel satellites, and what information we expect to obtain from these flights. Dr. Richard Taschek, of the Los Alamos Scientific Laboratory, covered in considerable detail the technical problems associated with the satellite detection of nuclear explosions in space at the hearings of this committee in April of 1960. I will not take up your time repeating all of these arguments, but will try to concentrate on the progress that has been made since that time. This work has been done by a group of about 25 at Los Alamos, with normal laboratory support from the shops and service groups, and a group of about 30 at Sandia Corp., working in very close and effective cooperation.

The satellite system that has been chosen has already been described very briefly by Major Poulson and will be described in greater detail by Dr. Leonard following me. I will confine myself to the instruments carried by the satellites, and to their objectives. A comprehensive study of detector designs, and the effects of anticipated radiation backgrounds upon them, has been carried out during the past 2 years. During this period we have benefited from exchanges of ideas and information with such agencies as ARPA, Aerospace Corp., and Livermore. Parallel with this effort has been the development of the associated electronic logics necessary to interpret the signals from the detectors. We are now in the final stages of design, with the present schedule calling for a design freeze by September 1.

Before describing the complement of detectors that have been chosen, I would like to remind you of some of the most important characteristics of a nuclear detonation in space. We are all familiar with the blinding flash of light, the giant fireball, and the destructive blast wave that have accompanied nuclear tests in the past. These phenomena are due entirely to interactions with the atmosphere, and will be absent when the test is conducted in space. Instead, we will find that over half of the energy of the explosion is released in the form of thermal radiation. The impressive part of this is that the case temperature of the weapon will be so high that the thermal radiation will be X-rays instead of the familiar infrared rays customarily associated with thermal radiation. The X-rays will have from 1 to 5 Kev. of energy. Because of the extremely high temperature of the bomb case (perhaps $15(10)^6$ ° K.), the energy generated by the nuclear

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Education: B.A., Whitman College, 1939; M.A., George Washington University, 1941; Ph. D., University of Chicago, 1948.
Staff member, Los Alamos Scientific Laboratory, 1948 to present.
Fellow: American Physical Society.

explosion will be radiated in a very short time, less than 1 microsecond (10^{-6} seconds). Therefore, an observer of this test, if he has an instrument that is sensitive to soft X-rays will see a short burst of X-rays, lasting less than 1 microsecond.

The intensity of this short burst is of interest, since it will determine the maximum distance over which one can observe it. A 1 megaton weapon detonated at $3(10)^8$ kilometers (twice the distance from the earth to the sun) will deposit 1.5 Mev. of X-ray energy into each $cm.^2$ of detector area. By laboratory standards, this signal will be at least 100 times larger than necessary for observation. Because of possible limitations of natural background present in a space environment, it is not entirely clear how much of this safety factor will be available in a detection system. However, it is this safety factor of 100 or more than encourages us to believe a reasonable detection capability exists.

In addition to the thermal X-rays, there are of course gamma rays and neutrons, although the combined energy carried off by these is only about 1 percent of the total.

The experimental satellite is being equipped with instruments to detect all three types of radiation. Since we know exactly how these instruments would respond if a nuclear detonation were to occur at any given distance, you might ask why this is called an experimental satellite. The reason is that we do not know what the natural environment at 50,000 miles is, and we do not know how frequently, if at all, solar eruptions can produce very short duration bursts of X-rays or gamma rays that could be mistaken for bomb bursts. However, the best available information from previous space probes and satellites indicates the environment at this altitude will be favorable for a detection system, and most solar physics experts believe that very short duration bursts (less than 1 microsecond) from the sun are highly unlikely.

I would like now to describe the detection instruments that have been selected for the satellites.

X-RAY DETECTORS

There will be 10 X-ray detectors uniformly spaced over the surface of the satellite. The detectors are thin domelike plastic fluors that extend out from the surface of the satellite. This arrangement makes a number of them visible from any direction in space. These plastic fluors have the property of efficiently absorbing the X-rays and converting their energy into visible light. The pulse of light is then collected and measured by standard photomultiplier tubes. The photomultipliers have to be shielded from the sunlight, and this is done by placing a thin cover of beryllium or aluminum over the dome-shaped plastic fluors. This shield has to be thin enough that it does not absorb the soft X-rays, yet still be opaque to sunlight. This is one of our problem areas.

A nuclear detonation would produce light pulses of less than 1 microsecond duration in the detectors facing in the proper direction. The intensity of the pulse would be directly proportional to the yield of the weapon and inversely proportional to the square of the distance from the weapon. There will be seven different bias levels, or sensitivity levels, built into the electronics in order to measure the intensity

of the pulses. By command from the ground we can select the intensity of X-ray pulse required to trigger the counters. In an actual detection satellite, one would use some high order of coincidence between detectors to separate a nuclear burst from cosmic ray background counts.

GAMMA RAY DETECTORS

There will be six gamma ray detectors spaced uniformly about the satellite, probably beneath the surface skin. Each detector will be a solid plastic fluor about 3 inches in diameter by 2 inches long, mounted on a photomultiplier tube. The detector fluor will be covered with about one-eighth inch of aluminum to shield from low-energy background radiations. The electronic circuitry will be much the same as for the X-ray detectors, with seven sensitivity levels and multifold coincidence circuits.

NEUTRON DETECTORS

There will be a neutron detector consisting of two or more adjacent BF_3 neutron counters, surrounded by a common moderator of 10 pounds of CH_2. These will be inside the spacecraft shell.

GUARD COUNTERS

Ten guard counters, probably associated with the 10 X-ray detectors, will help to identify bursts of charged particles produced in the package by cosmic rays and triggering several of the X-ray detectors. One possibility is to have a plastic fluor so arranged about each X-ray detector that most charged particles passing from within the satellite out through the X-ray detector have to pass through the guard counter.

The above combination of X-ray, gamma ray, neutron, and guard counters will not only give us information about X-ray, gamma ray, and neutron backgrounds, but will be very helpful in diagnosing what is happening when a complicated situation occurs, such as the occurrence of a high-energy plasma of protons and electrons ejected from the sun and striking the satellite.

We consider the presence of two satellites in orbit to be essential for any detection system, because there is a possibility for a cosmic-ray burst within either satellite to simulate a bomb burst. How frequently these bursts will occur is not known, but we believe the frequency can be made low, like a few per day, by requiring high-order coincidences, and adjusting the sensitivity level. By requiring both satellites to see the signal, one expects to eliminate this troublesome source of false alarms.

As Dr. Leonard will explain later, the proposed satellite system will contain two satellites for each launching, so we have high hopes of having two satellites in orbit at one time to measure the frequency at which these background coincidences are caused by cosmic ray events in the satellites themselves.

THE LOS ALAMOS AND SANDIA SUPPORT PROGRAM

I would like to mention some of the things that Los Alamos and Sandia have been doing to insure the success of the satellite program.

We have built several full-scale packages that simulate a detection satellite, and are in the process of flying them suspended from balloons at about 120,000 feet, at the top of the atmosphere. These packages contain 12 X-ray detectors of the type I have described, or interchangeably, 12 gamma-ray detectors. Within the package is the necessary electronic logics to measure the frequency with which high-order coincidences occur as a function of the sensitivity of the detectors. This information is telemetered to the ground and recorded on magnetic tapes. The coincidences we see are caused solely by cosmic ray showers, since X-rays could not get down to this altitude without absorption. The purpose of this operation is to obtain an upper limit on the frequency of high-order coincidences caused by cosmic rays, prior to launching the experimental satellite. In addition, it has enabled Sandia and Los Alamos to gain almost a year's headstart in the development of the electronic logics needed for the satellite.

Another experiment from which we hope to obtain useful information will be carried aloft in the near future by the first two NASA Ranger probes. These probes get out to moon distances and maintain a fixed orientation in space, with one axis pointing to the sun. Our experiment consists of two X-ray detectors that will look directly at the sun, and for a period of a month or so will be able to analyze the solar X-ray flux for any short duration bursts of significant intensity.

A third experiment in the Vela Hotel program is being flown on an Air Force Blue Scout, Jr., rocket this fall, with the help of the Air Force Special Weapons Command Group at Kirtland Air Force Base. This package has two X-ray detectors similar to the proposed satellite detectors in most respects, and two gamma-ray detectors. Again, these detectors will look for short bursts of X-rays and gamma rays from the sun. The flight itself will give us needed experience in the handling of the special X-ray detectors.

Representative PRICE (presiding). Doctor, the committee is impressed with the intensive effort, involving, I think you said, a group of about 25 at Los Alamos and about 30 at Sandia. You also mention early flights of full-scale packages have allowed you to gain almost a year's headstart in the development of electronics logics for the satellite.

Yesterday, the committee was informed by Dr. Ogle that the Los Alamos Laboratory started work on ground-based optical detection techniques before the Vela program came into being. The advanced state of the work you have reported in connection with Vela Hotel prompts me to inquire whether or not this joint Los Alamos-Sandia project is another example of AEC Laboratory foresight and initiative.

The ARPA list of contracts shows no fund allocations to Los Almos or Sandia, although 1½ million is shown at an unspecified date for the Lawrence Radiation Laboratory of the AEC.

Now, to what extent was this project initiated by your laboratory, and Sandia, and carried on independently of the Vela Hotel program?

Dr. ARGO. Los Alamos Laboratory and Sandia started looking at this problem prior to the name being given to it of Vela Hotel. This was in the spring of 1959.

Representative PRICE. How much work were you doing about that time?

Dr. ARGO. At that time, it started out as a study group, and there were probably 15 people involved between the two organizations. The study group continued for the summer of 1959. By the fall of 1959, we were getting support, official support, from the AEC here in Washington for our studies, and actually the next budget contained $1 million for each of the two laboratories and has continued at that support level since.

Representative PRICE. When did you officially come under the Vela program? About last August, I suppose.

Dr. ARGO. Officially, it was just about a year ago; but we had been working at it, on whatever the name was, since the spring of 1959.

Representative PRICE. The committee noted with considerable interest the management mechanism described by Major Poulson for management of the Vela Hotel program. As we understand it, ARPA has overall technical responsibility, and a joint committee of the Air Force Systems Command and the AEC are responsible for detailed technical supervision, acting through normal channels in each agency. Presumably from page 10 of Major Poulson's statement, coordination with NASA is supervised by ARPA.

Do you consider that there is a satisfactory working relationship between the AEC and the Department of Defense from the standpoint of effective prosecution of the program?

Dr. ARGO. Things seem to be going very smoothly.

Representative PRICE. They are going satisfactorily?

Dr. ARGO. At this point I would say they are satisfactory.

Representative PRICE. No cause for any concern, and as far as you can see no reason for anything different to be done than is now being done?

Dr. ARGO. Well, we were somewhat disappointed at the slow start that has been made on Vela Hotel, but as of this point we are quite happy with the progress that has been made.

Representative PRICE. Any questions?

Do any of the staff have any questions?

Thank you very much, Dr. Argo. We appreciated having your testimony.

(Dr. Argo's formal statement follows:)

DETECTION INSTRUMENTS FOR VELA HOTEL EXPERIMENTAL SATELLITES

(By Harold V. Argo, Los Alamos Scientific Laboratory)

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The satellite system that has been chosen has already been described by the gentlemen appearing before me at these hearings. I will confine myself to the instruments carried by the satellites, and to their objectives. A comprehensive study of detector designs, and the effects of anticipated radiation backgrounds

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Before describing the complement of detectors that have been chosen, I would like to remind you of some of the most important characteristics of a nuclear detonation in space. We are all familiar with the blinding flash of light, the giant fireball, and the destructive blast wave that have accompanied nuclear tests in the past. These phenomena are due entirely to interactions with the atmosphere, and will be absent when the test is conducted in space. Instead we will find that over half of the energy of the explosion is released in the form of thermal radiation. The impressive part of this is that the case temperature of the weapon will be so high that the thermal radiation will be X-rays instead of the familiar infrared rays customarily associated with thermal radiation. The X-rays will have from 1 to 5 kilo electron volts of energy. Because of the extremely high temperature of the bomb case (perhaps $15(10)^6$ degrees K), the energy generated by the nuclear explosion will be radiated in a very short time, less than 1 microsecond (10^{-6} seconds). Therefore, an observer of this test, if he has an instrument that is sensitive to soft X-rays will see a short burst of X-rays, lasting less than 1 microsecond.

The intensity of this short burst is of interest, since it will determine the maximum distance over which one can observe it. A 1-megaton weapon detonated at $3(10)^8$ kilometers (twice the distance from the earth to the sun) will deposit 1.5 million electron volts of X-ray energy in each cm^2 of detector area. By laboratory standards this signal will be at least 100 times larger than necessary for observation. Because of possible limitations of natural background present in a space environment, it is not entirely clear how much of this safety factor will be available in a detection system. However, it is this safety factor of 100 that encourages us to believe a reasonable detection capability exists.

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Chairman HOLIFIELD. The last witness this morning will be Dr. Byron Leonard, of the space technology laboratory.

I understand you will testify on the satellite systems requirements under Vela Hotel, Dr. Leonard.

STATEMENT OF DR. B. P. LEONARD,[1] AEROSPACE CORP.

Dr. LEONARD. Mr. Chairman and members of the Joint Committee, last year I had the privilege of testifying before this committee on the subject of the feasibility of performing and detecting nuclear weapons tests in space. Since that time an extensive study has been made of this subject, and I would like to review the results briefly. Following this, I will discuss the salient features of the research program currently underway to obtain experimental data on which to confirm the purely theoretical predictions upon which we rely today.

Turning first to the subject of the performance of nuclear tests in space. There is no question that both the United States and the U.S.S.R. have the capability to perform such tests. In my opinion, either country, if it chose to do so, could launch a payload into space containing a nuclear device and capsules with instruments for recording the performance of the device. At the appropriate time the capsules would be separated from the weapon. The weapon would then be fired, the data recorded and transmitted back to earth. While the amount of data obtained would be limited, it, nevertheless, would be highly significant in determining the performance of the device.

Several important advantages are associated with space tests as opposed to terrestrial tests, particularly if the tests are clandestine. They are (1) no fallout, (2) no limit to the yield of the weapon (for high yield tests, data obtained in space would be more meaningful than for the same test performed underground due to wall effects), (3) minimal possibility for a followup investigation if it is suspected that a test has been run.

Mr. RAMEY. No on-site inspection?

Dr. LEONARD. Exactly.

Since there exists both the incentive and the capability to test in space, a space detection system is an essential element of any weapons test detection system. We have, therefore, given serious attention to the techniques and capabilities for detecting such tests. Dr. Argo has described the basic detection techniques, and Major Poulson has

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[1] Born: Morgan City, La.; February 26, 1925.

Education: Bachelor's degree in physics from Southwestern Louisiana Institute and a master's and doctor's degree in nuclear physics from the University of Texas.

Experience: After obtaining his bachelor's degree in 1943, entered the U.S. Navy where he served until 1946. After his discharge he became a project engineer at the U.S. naval ordnance test station where he was responsible for range test firings of experimental rockets from aircraft.

He then became a member of the physics teaching staff at Southwestern Louisiana Institute. In 1950 he entered the University of Texas where he taught physics, carried out nuclear research for the Air Force, and subsequently received his doctor's degree.

In 1953, joined the Convair Aircraft Co. on the aircraft nuclear propulsion program. There he became head of the nuclear aircraft research facility where two experimental reactors were operated to obtain data on nuclear shielding and radiation effects to aircraft materials and components. In this capacity he was responsible for the nuclear test airplane program which entailed first flights of a test reactor aboard a B–36 airplane.

Joined space technology laboratories in 1959 and was responsible for studies on advanced nuclear systems including possible detection systems for high altitude nuclear explosions.

In 1960 joined the Aerospace Corp. and is presently group director of a major space program. He has recently been nominated as a scientific member of the ARPA ad hoc group on detection of nuclear detonations. In March of this year he served as a scientific consultant to the nuclear test ban conference delegation in Geneva.

pointed up the present major uncertainties in assessing the capabilities of a high altitude detection system. I will now describe the research and development program being established to reduce these uncertainties.

You may recall that the Technical Working Group I considered four possible satellite systems. They were—

1. An Argus-type satellite sampling trapped radiation in the earth's magnetic field.
2. Near earth satellites.
3. Far earth satellites.
4. Solar satellites.

Each of these systems has been studied in detail. The first suffers from limited coverage; that is, there are readily accessible regions of space near the earth in which testing could be carried out without detection. The near earth system (orbital altitude 300 to 900 nautical miles) requires an exorbitant number of satellites to achieve redundant and complete surveillance coverage. The far earth system provides redundant and complete coverage down to the earth's atmosphere as well as out into space with a reasonable number of satellites (approximately six). The solar satellite system is a natural extension of the far earth system which forces nuclear testing farther and farther from the earth.

These considerations have led to the decision to concentrate initially in the research effort on the detection problems related to the far earth satellites. This establishes the requirement to conduct research and experimentation beyond the trapped radiation zones because of the high radiation background present within the zones. Current estimates indicate that between 50,000 and 75,000 nautical miles should be sufficient to escape any significant background effects due to the trapped radiation.

A primary objective of the program is to conduct space-based experimentation for verifying the feasibility of detecting and identifying high altitude nuclear explosions by means of X-ray, gamma ray, and neutron detector systems. This objective can be met only by investigating the behavior of preprototype detection equipment in space and monitoring the responses to the natural radiation backgrounds over significant periods of time. Two key questions to be answered are—

1. Will the detection systems respond to natural phenomena as they would to a weapon test?
2. If so, how frequently does this occur? That is, what is the false alarm rate?

Upon the answers to these two questions rests the final assessment of the capability to detect high-altitude nuclear explosions.

The research program is divided into two phases. The first phase consists of flying experiments piggyback on various NASA and Department of Defense ballons, satellites, and probes. The AEC's Los Alamos Scientific Laboratory, Lawrence Radiation Laboratory, and Sandia Corp. are designing and fabricating the instrument packages.

In August 1960, ARPA requested the Air Force Systems Command (AFSC) and the AEC to evaluate the possibilities of flying particular experiments on planned Department of Defense satellite flights. In December 1960, after reviewing the recommendations from the

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AFSC and the AEC, ARPA approved a series of radiation detector experiments for riding piggyback on regular flights. The purpose of these flights is to make measurements of the radiation environment of particular interest to the detection art. Sensors will be flown which will provide directly X-ray, gamma ray, charged and neutral particle fluxes. Special instrumentation will be provided to detect bursts of radiation and to attempt to identify their nature. ARPA is providing funds to the AEC Lawrence Radiation Laboratory for development of the experimental equipments and for analysis of the experimental data received, and to the AFSC for integrating the equipment into the satellite systems.

The second phase of the program consists of five launches from the Atlantic Missile Range of experimental spacecraft into orbits of from 50,000 to 75,000 nautical miles. Launches will commence in the next few years.

This figure (fig. I, p. 259) shows the manner in which the desired orbit will be achieved.

The two spacecraft will be launched into a highly elliptic orbit. The entire payload package will be spun to provide stabilization for the spacecraft. They will then be separated individually and will coast to the maximum altitude (apogee). At this time a small solid rocket will inject one of the spacecraft into a nearly circular orbit. On the next pass at apogee the second spacecraft will be injected into the same orbit but lagging the first by about 140° due to the time delay. The altitude is chosen so that the spacecraft will operate well out of the high background level of the earth's trapped radiation fields. However, should the injection rocket not fire, the vehicle is designed to continue to function in the highly elliptic orbit.

Figure II (p. 260) shows a sketch of a typical spacecraft. A number of X-ray detectors will be mounted at the outer surface. Connected together they form a single system which monitor space in all directions. Gamma ray and neutron detectors will be mounted internally. Since it is impractical to be in communication with the spacecraft at all times, continuous monitoring will be achieved by processing and recording data in the vehicle. Periodically, data will be transmitted to the ground. Electrical power will be supplied in the vehicle by a combination of batteries and solar cells. The solar cells are shown mounted on the surface of the spacecraft.

The detectors mentioned above have been proposed as preprototype nuclear test sensors. If space and weight permit, these preprototype detectors may be supplemented by detectors of more sophisticated and advanced design. However, our philosophy for the initial flights is to emphasize doing those things which are simple and best understood and which will result in a relatively high level of equipment reliability. The design objective of the initial spacecraft is a useful lifetime of about 6 months. We will phase into advanced techniques gradually as our confidence in the validity of the data received increases.

Simultaneous launching of two spacecraft may well turn out to be an extremely important factor in the results of the program for the following reason. The two most troublesome radiation backgrounds in space could well be—

1. The radiation produced in the spacecraft itself (cosmic ray "showers").
2. Possible short-time bursts of radiation from the sun.

In the first case, it is possible to design detection systems such that it is extremely unlikely that signals would appear in both spacecraft simultaneously. In the second case, if the bursts of radiation emanated from the sun, alarms could appear in both spacecraft at very nearly the same time. Correlated alarms appearing in more than one satellite would almost certainly be interpreted as a weapon test. Therefore, should the latter case occur, a serious situation would develop requiring further research to distinguish the natural phenomena from weapons tests.

In closing, I would like to reemphasize that only through painstaking and extensive research upon which we are now embarked, will we be able to determine the true capability for detecting high altitude nuclear weapons tests.

Representative PRICE. Thank you very much, Doctor.

I note on page 4 of your statement the division of ARPA funds between the AEC Lawrence Radiation Laboratory for the development of payload equipments and analysis of data and the Air Force Systems Command for integrating the equipment in the satellite systems.

Does the committee understand correctly that satellite-based detectors are being developed both by the Lawrence Radiation Laboratory and by the Los Alamos-Sandia team?

Dr. LEONARD. I think that a proper statement of what is being done by the Lawrence Radiation Laboratory would be something like this: The type of instrumentation that they are flying is preliminary, in the sense that it is not a preprototype of a detection system. It is more in the category of early experimentation to support the design of the pre-prototype equipment that will go into large satellites in phase II. In that sense, it is comparable to the kinds of work that Los Alamos and Sandia are doing with balloons and the Ranger flights.

Representative PRICE. We also noted with considerable interest the management mechanism described by Major Poulson for management of the Vela Hotel program. I would like to ask you the same question I asked Dr. Argo a moment ago. Do you consider that there is a satisfactory working relationship between the AEC and the Department of Defense from the standpoint of effective prosecution of the program?

Dr. LEONARD. I am afraid that I have not been sufficiently close to the program in the last few months to be very positive on this subject. However, I have learned indirectly from the Air Force and Aerospace Corp. sources that, as Dr. Argo indicated, the program seems to be going along very well, now that the funds are available.

Representative PRICE. I notice you have also had a little contact with the nuclear propulsion program. If we got all you folks back together, maybe we could put a plane in the air. It is evident that we had considerable talent in this program. And if you took a poll of the talent, I think all of them would have been optimistic about the possibility of nuclear propulsion aircraft. I thing it speaks well for the type of people we did have in this program. And I have never known anybody who actually worked on the program to be pessimistic about it, at least as pessimistic as those who made the final decision killing the program.

Mr. Ramey, do you have a question to ask?

Mr. RAMEY. Has your group given any consideration, as to your power sources for your satellites, to the SNAP devices?

Dr. LEONARD. Yes, sir. Early in the program this was looked at, and in the case of the Vela Hotel satellite, and in the case of many satellites that are being flown today, it turns out that the power requirements come out in a few tens to a few hundreds of watts. When one looks at the various SNAP devices—he finds two ranges of power sources being developed, relatively low levels of power through radioisotopes and relatively high levels of power through reactors. It appears that the requirements on Vela Hotel tend to fall between these two levels. For that reason, and because the solar cell-battery arrangement is fully developed and flight tested, we are not planning to use a SNAP power supply.

Representative VAN ZANDT. The answer is, then, your power requirement?

Dr. LEONARD. That is right. Also, the solar cell-battery arrangement has turned out to be a very reliable power source. It is not as light as we would like it to be, but it has essentially no moving parts in it.

Representative VAN ZANDT. Mr. Chairman, of course, like other members of this committee, I am enthusiastic about this SNAP program, and we have in orbit at the present time Transit I, and it is producing power, and will do so for 5 years.

Then I recall testimony we have had before this committee from certain people, who say that solar energy gives you only a return for a percentage of a full day; is that correct?

Dr. LEONARD. Well, it depends upon what kind of power source you are talking about. When you use the solar cells in conjunction with batteries, then you get continuous power.

Representative Van ZANDT. You charge the batteries when the solar cell is accepting energy?

Dr. LEONARD. Yes.

Representative VAN ZANDT. Does it mean if the SNAP program develops to the extent it will produce a greater amount of continuous power, it could then displace the solar cells?

Dr. LEONARD. Yes, sir. At higher power level and for very long periods of time, the reactor SNAP system would certainly come into its own at some point.

Representative VAN ZANDT. Using the isotope and its weight factors, such as in Transit I at the present time, if it produces greater power, could it fit into your program?

Dr. LEONARD. If it met the power requirements, certainly. I am not familiar with the exact power being produced in that Transit satellite, but I am sure it is considerably smaller than the power requirements we have here.

Representative VAN ZANDT. I specifically asked the question to develop an answer in regard to the weight factor. It would not outlaw itself from your project because of its weight, would it?

Dr. LEONARD. Well, one would have to be a little careful in giving an oversimplified answer to this.

In the case of the radioisotopes, as the power requirements continue to increase, we get a larger amount of radioactivity required, and the

shielding requirements go up comparably. Again, I think the final answer is that in certain power regimes these SNAP devices look good, and in others they do not compete with other more conventional systems.

Representative VAN ZANDT. I have another question, Mr. Chairman.

At the close of your statement, you say, "I would like to reemphasize that only through painstaking and extensive research, on which we are now embarked, will we be able to determine the true capability for detecting high altitude nuclear weapons tests."

When you made that statement, I was reminded of what goes on at Canaveral and also at Vandenberg. They tell us that space aboard these satellites is committed for the next several years. Have you commitments already from any of these missile ranges?

Dr. LEONARD. Yes, sir. The phase II program is based upon allocation of specific launch vehicles for this program.

Representative VAN ZANDT. In other words, you have a commitment, then, for this program?

Dr. LEONARD. That is right. This is not a question of trying to go piggyback or squeeze into other space vehicles. These spacecraft will be designed specifically for this program, and have their own launch vehicles.

Representative VAN ZANDT. Now, on page 2, you say here:

> You may recall that the Technical Working Group I considered four possible satellite systems. They were:
> 1. An Argus-type satellite sampling trapped radiation in the earth's magnetic field.

Does the trapped radiation result from tests, or is there a normal amount of natural radiation in these magnetic fields?

Dr. LEONARD. There is a normal amount of charged particles trapped in the closed lines of the earth's magnetic field. And the technical working group pointed out that should a weapon be detonated in this region of closed lines, the beta particles emanating from the weapon would be trapped in the same way that charged particles from natural sources would be.

Representative VAN ZANDT. How many radiation belts have we finally discovered?

Dr. LEONARD. Well, there are two very distinct ones. There is the inner belt, that contains protons, normally referred to as the Van Allen belt, and then there is a much more extensive belt that actually encompasses that inner belt, which consists of relatively low-energy electrons trapped in the earth's magnetic field. And these are the particles that we are concerned about in the Vela Hotel program. The low-energy band extends out to as far as 50,000 to 75,000 nautical miles, and actually fluctuates in and out, depending upon the sun's activity. We are trying to get outside of that region to get away from that background problem.

Representative VAN ZANDT. This question may be beside the point, but we have refrained from testing now for nearly 3 years. It will be 3 years in October. What about the load of radiation in the atmosphere? Is it decaying, in accordance with the schedule given to us at that time?

Dr. LEONARD. Actually, we do not expect the radiation in these trapped belts to decay, so much as to fluctuate, depending on the activity of the sun. We have cases where a solar flare could either cause a big buildup in the radiation field or could actually disturb the magnetic field so that particles were ejected out of it. So that we expect wide variations, but not any decay.

Representative VAN ZANDT. Some years ago, we were told that atmosphere radiation resulting from tests of nuclear weapons would probably be with us for 10 years, and that annually there would be a certain percentage of it would settle on the surface of the earth.

Dr. LEONARD. I see. You are talking about the fission products deposited in the upper atmosphere. Yes, there is a gradual injection of these fission products into the lower earth's atmosphere.

Mr. RAMEY. It is coming down quicker than they thought it would.

Representative VAN ZANDT. That is what I was getting to. Is it true it is coming down quicker than they estimated?

Dr. LEONARD. I have heard this, but I am not familiar enough with the data to say.

Representative VAN ZANDT. Does the 10-year figure still hold?

Mr. RAMEY. That was Dr. Libby's original figure, but that has been substantially revised. Apparently it has come down faster.

Representative VAN ZANDT. In other words, it could be less than 10 years?

Mr. RAMEY. Well, there will always be some, but the rate is much faster.

Representative PRICE. Any further questions?

Thank you very much, Mr. Leonard.

Dr. Leonard's testimony completes that portion of the hearings dealing with research and development to date in all three phases of the detection program: Vela Uniform, Vela Sierra, and Vela Hotel.

With the culmination of the testimony that we have received from the technical and scientific witnesses over the past 2½ days, the committee feels that an analysis of the role of the Vela program in relation to the nuclear test ban question is most appropriate and necessary.

Ambassador Arthur H. Dean, the U.S. representative to the conference on the discontinuance of nuclear weapons tests, will present this analysis at the afternoon session.

We will reconvene at 2 p.m., in room P–63, the Old Supreme Court Chamber, in the Capitol.

Chairman HOLIFIELD. This meeting is adjourned until 2 o'clock.

(The entire statement of Dr. Leonard follows:)

STATEMENT BY DR. B. P. LEONARD, AEROSPACE CORP.

Mr. Chairman and members of the Joint Committee, last year I had the privilege of testifying before this committee on the subject of the feasibility of performing and detecting nuclear weapons tests in space. Since that time an extensive study has been made of this subject, and I would like to review the results briefly. Following this, I will discuss the salient features of the research program currently underway to obtain experimental data on which to confirm the purely theoretical predictions upon which we rely today.

Turning first to the subject of the performance of nuclear tests in space. There is no question that both the United States and the U.S.S.R. have the capability to perform such tests. In my opinion either country, if it chose to do so, could launch a payload into space containing a nuclear device and capsules

with instruments for recording the performance of the device. At the appropriate time the capsules would be separated from the weapon. The weapon would then be fired, the data recorded and transmitted back to earth. While the amount of data obtained would be limited, it, nevertheless, would be highly significant in determining the performance of the device.

Several important advantages are associated with space tests as opposed to terrestrial tests, particularly if the tests are clandestine. They are: (1) No fallout, (2) no limit to the yield of the weapon (for high yield tests, data obtained in space would be more meaningful than for the same test performed underground due to wall effects), (3) minimal possibility for a followup investigation if it is suspected that a test has been run.

Since there exists both the incentive and the capability to test in space, a space detection system is an essential element of any weapons test detection system. We have, therefore, given serious attention to the techniques and capabilities for detecting such tests. Dr. Argo has described the basic detection techniques and Major Poulson has pointed up the present major uncertainties in assessing the capabilities of a high altitude detection system. I will now describe the research and development program being established to reduce these uncertainties.

You may recall that the technical working group I considered four possible satellite systems. They were—

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A primary objective of the program is to conduct space-based experimentation for verifying the feasibility of detecting and identifying high altitude nuclear explosions by means of X-ray, gamma-ray, and neutron detector systems. This objective can be met only by investigating the behavior of preprototype detection equipment in space and monitoring the responses to the natural radiation backgrounds over significant periods of time. Two key questions to be answered are—

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Upon the answers to these two questions rests the final assessment of the capability to detect high altitude nuclear explosions.

The research program is divided into two phases. The first phase consists of flying experiments piggyback on various NASA and DOD balloons, satellites, and probes. The AEC's Los Alamos Scientific Laboratory, Lawrence Radiation Laboratory, and Sandia Corp. are designing and fabricating the instrument packages.

In August 1960 ARPA requested the Air Force Systems Command (AFSC) and the AEC to evaluate the possibilities of flying particular experiments on planned DOD satellite flights. In December 1960, after reviewing the recommendations from the AFSC and the AEC, ARPA approved a series of radiation detector experiments for riding piggyback on regular flights. The purpose of these flights is to make measurements of the radiation environment of particular interest to the detection art. Sensors will be flown which will provide directly X-ray, gamma-ray, charged and neutral-particle fluxes. Special instrumentation will be provided to detect bursts of radiation and to attempt to identify their nature. ARPA is providing funds to the AEC Lawrence Radiation Laboratory for development of the experimental equipments and for analysis of the experimental data received, and to the AFSC for integrating the equipment into the satellite systems.

The second phase of the program consists of five launches from the Atlantic Missile Range of experimental spacecraft into orbits of from 50,000 to 75,000 nautical miles. Launches will commence in the next few years. Figure I shows the manner in which the desired orbit will be achieved. The two spacecraft will be launched into a highly elliptic orbit. The entire payload package will be spun to provide stabilization for the spacecraft. They will then be separated individually and will coast to the maximum altitude (apogee). At this time the small solid rocket will inject one of the spacecraft into a nearly circular orbit. On the next pass at apogee the second spacecraft will be injected into the same orbit but lagging the first by about 140 degrees due to the time delay. The altitude is chosen so that the spacecraft will operate well out of the high background level of the earth's trapped radiation fields. However, should the injection rocket not fire, the vehicles are designed to continue to function in the highly elliptic orbit.

Figure II shows a schematic sketch of a typical spacecraft. A number of X-ray detectors will be mounted at the outer surface. Connected together they form a single system which monitors space in all directions. Gamma-ray and neutron detectors will be mounted internally. Since it is impractical to be in communication with the spacecraft at all times, continuous monitoring will be achieved by processing and recording data in the vehicle. Periodically, data will be transmitted to the ground. Electrical power will be supplied in the vehicle by a combination of batteries and solar cells. The solar cells are shown mounted on the surface of the spacecraft.

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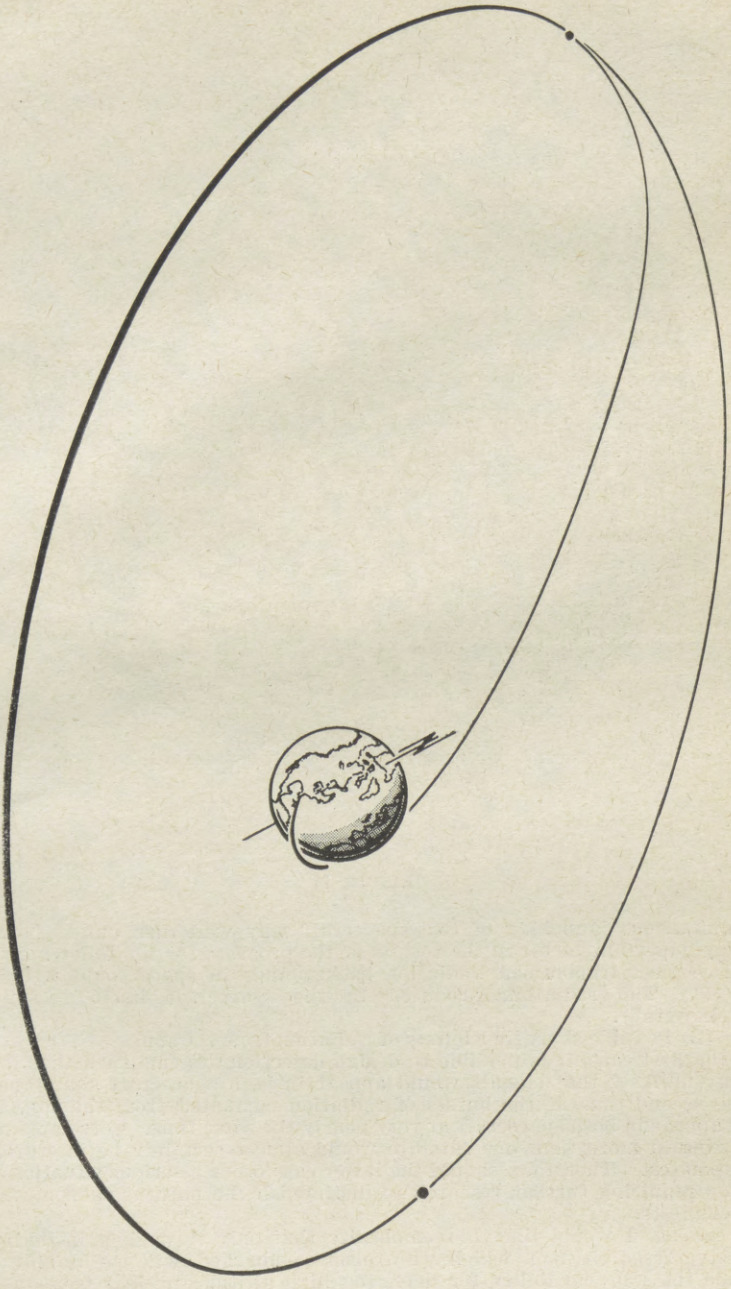


FIGURE I

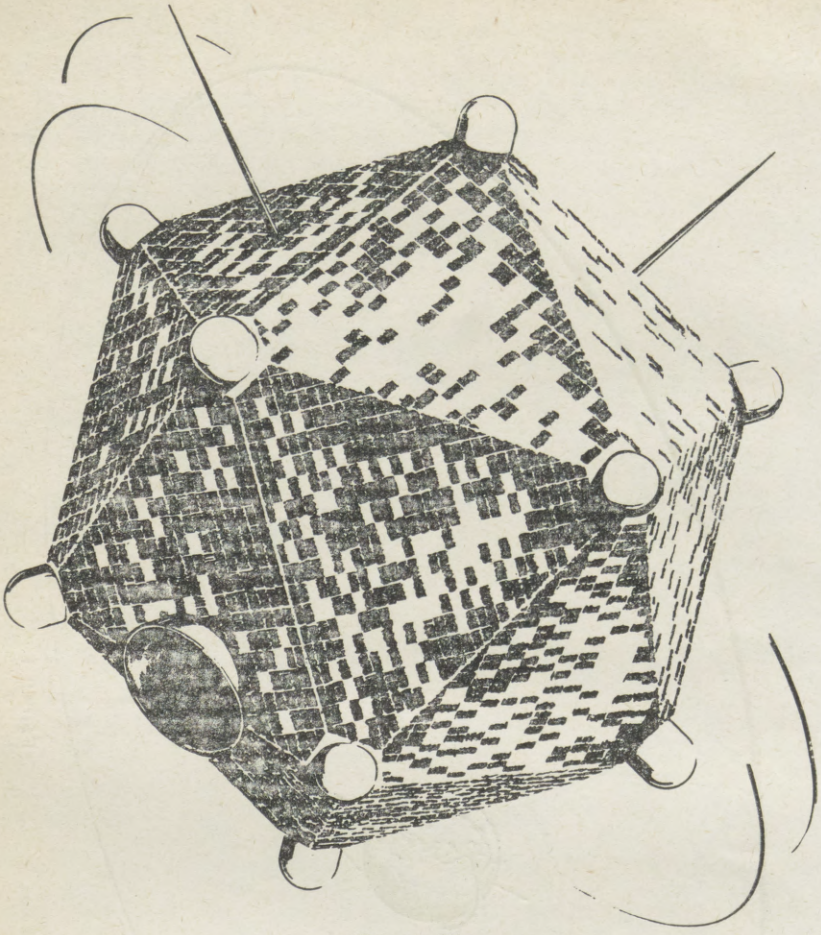


FIGURE II

Simultaneous launching of two spacecraft may well turn out to be an extremely important factor in the results of the program for the following reason. The two most troublesome radiation backgrounds in space could well be—

(1) The radiation produced in the spacecraft itself (cosmic ray "showers").

(2) Possible short time bursts of radiation from the sun.

In the first case, it is possible to design detection systems such that it is extremely unlikely that signals would appear in both spacecraft simultaneously. In the second case, if the bursts of radiation emanated from the sun, alarms could appear in both spacecraft at very nearly the same time. Correlated alarms appearing in more than one satellite would almost certainly be interpreted as a weapon test. Therefore, should the latter case occur a serious situation would develop requiring further research to distinguish the natural phenomena from weapons tests.

In closing, I would like to reemphasize that only through painstaking and extensive research upon which we are now embarked, will we be able to determine the true capability for detecting high altitude nuclear weapons tests.

(Whereupon, at 11:45 a.m., the joint committee recessed, to reconvene at 2 p.m., the same day, in room P–63, the Capitol.)

AFTERNOON SESSION

Chairman HOLIFIELD. The committee will be in order.

The Joint Committee meets this afternoon in the final session of our 3 days of hearings on the test detection problems. We have completed that portion of the testimony dealing with the technical problems on detection and identification of nuclear tests. We have received testimony during the past 3 days from the scientific personnel who are responsible for and who are doing research work in this very important area.

We have been primarily concerned with what advancements have been made since the committee last held its public hearings in April of 1960. Based on the testimony we received, it appears that the possibilities of developing an effective control system are not much better than it appeared a year ago. I believe I can speak for the committee when I state that we are quite disappointed that this be so.

Having completed the portion of the hearings concerned with the technical aspects, we will now hear from Ambassador Arthur Dean as to the role of Project Vela in relation to the nuclear test ban. Ambassador Dean is the U.S. representative to the conference on the discontinuance of nuclear weapon tests and since March 21, 1961, has been negotiating with the U.S.S.R. in an attempt to arrive at a treaty. For over 33 months Ambassador Dean, and his predecessor, Ambassador Wadsworth, have attempted to reach agreement with the Soviets.

Mr. Ambassador, we would like to pay tribute to the fine job that you have been doing in Geneva. We were fortunate to see you in action when some of us attended the test ban meetings last May. It is reassuring to this committee to know that we are so ably represented in this very important conference.

We welcome you as a witness to this committee this afternoon, and you may proceed, sir.

STATEMENT OF AMBASSADOR ARTHUR H. DEAN,[1] U.S. REPRESENTATIVE TO THE CONFERENCE ON THE DISCONTINUANCE OF NUCLEAR WEAPONS TESTS

Ambassador DEAN. Thank you, Chairman Holifield. Those are very kind words, and I appreciate them very much.

For the last 3 days, your committee has been hearing the testimony concerning the technical and scientific aspects of the Vela program, some of which, as I read it, appears to be—and I suppose that is the way it should be—rather on the cautionary or pessimistic side. But as you have seen from this testimony, knowledge in any field is never static, and you never know until you try. And I think this is par-

[1] Senior partner of the New York law firm of Sullivan & Cromwell, holds degrees of A.B. and LL.B. from Cornell University. He is chairman of the board of trustees of Cornell University, a director and member of the Committee on Studies of the Council on Foreign Relations, trustee of the Carnegie Foundation for the Advancement of Teaching, and president of the American Society of International Law. He has been Dickinson lecturer at Harvard and Chubb fellow at Yale.

He has had various diplomatic assignments, has twice served as chairman of the American delegation at Conferences on the Law of the Sea, and is currently chief negotiator for the United States at the Nuclear Test Ban Conference at Geneva.

He is currently in Washington for consultation with the President and the Secretary of State, and is working with John J. McCloy, the President's Special Adviser on Disarmament, on disarmament matters generally.

ticularly true with this new field of nuclear energy and its related field of seismology with respect to earthquakes or tests underground.

I am here today to emphasize the continued need for the scientific results of the Vela program, particularly as they relate to this country's efforts to secure an effective agreement on the cessation of nuclear weapon tests and as they bear on our ability to detect nuclear explosions underground or in outer space.

Dr. Leonard has testified here, and he helped us in the drafting of our proposed treaty provisions on outer space. Other scientists who have also testified here, including Dr. Richard Latter and Dr. Carl Romney, and Doyle Northrup and Major Poulson, have also helped us; but the drafting and responsibility is, of course, the State Department's and not the scientists'.

Let me be clear. While I favor the Vela program, I am, of course, not expressing any opinion as to whether the Vela underground nuclear detonations should take place, and if so, when; as this is, of course, a matter which must be decided by the President.

By my testimony here, I do not wish to express any opinion as to whether the very real scientific need for this program may be outweighed by other factors not related to the program. Again, this is a decision which only the President can make.

Technically, of course, there is no moratorium in existence on any nuclear explosions in this country. On December 29, 1959, at the end of a declared moratorium, which had existed for over a year, President Eisenhower said: "We consider ourselves free to resume nuclear weapons testing * * *." He then went on to say that at that time we were resuming "negotiations in a continuing spirit of seeking to reach a safeguarded agreement." And he then declared a "voluntary suspension of nuclear weapons tests," which could be terminated at any time.

So it is this voluntary suspension which is now in effect.

As the committee knows, the Vela program evolved originally from the recommendations set forth in the 1959 report of the panel on seismic improvement, otherwise known as the Berkner panel. As President Eisenhower announced on May 7, 1960, the program is designed to provide "full understanding of both the capabilities of the presently proposed detection system and the potential for improvements in this system * * *."

The Vela program was thus initiated primarily to provide the necessary scientific information to establish and improve the capability of the system for detection and identification of nuclear explosions proposed at the Geneva negotiations.

Mr. McCloy and Mr. Fisher and I have met with members of this committee several times during the negotiations at Geneva to give you status reports, and you are all well aware of the position the nuclear test ban negotiations are now in at Geneva.

Very briefly, beginning on March 21 of this year, the United States and the United Kingdom introduced at Geneva a large number of new proposals dealing with the principal unresolved issues at the conference at the top control commission, the method of appointing the executive administrator and his first deputy, the makeup of onsite inspection teams, the citizenship of the chiefs of control posts, the number of annual onsite inspections on Soviet territory, the number of control stations on Soviet territory, the criteria for locating uniden-

tified earth tremors, the length of a proposed moratorium on tests registering below 4.75 on the seismic scale of magnitude, a moratorium which is not to begin until the proposed treaty is signed. One of our most important new proposals would give the Soviet Union and states associated with it four seats—or a position of equality with the United States, the United Kingdom, and states associated with those two powers—on the top policymaking control commission. This proposal would also give three seats to nonassociated states. The top control commission would thus have a total of 11 seats, with a majority of six. The new proposals would also go as far as reasonably possible, consistent with the technical and organizational requirements of effective control, to accommodate Soviet sensitivities concerning inspection and other necessities of control on its territory. Contrary to some of the testimony you have heard here, our scientific advice—which has been reviewed by a scientific panel; and that goes for both the British and the American advisers—was that the control system set forth in our treaty would provide reasonably adequate control over those regions which the treaty purported to control. Of course, this being a new field, we won't really know whether the proposed control system will work until we have tried it; and the treaty, of course, provides for its being reviewed from time to time.

As the committe knows, the proposed treaty control organization already calls for capital expenditures of some $2 billion and an annual expenditure of $500 million and an organization of some 35,000 men.

Now, of course, the more control posts you have, the better your system will work, but it does seem to me that we ought to at least try this and then review it at the end of the phase I, rather than spending more money on more conrol posts at this time.

I have in my experience seen a good many pilot plants devised. I have never seen one yet that actually worked perfectly in practice in the field. You almost always find that you have to accommodate it to what you find in the field, and you almost always run into difficulties, and you almost always find that you are going to have to change it in some respects.

On April 18 of this year, the United States and the United Kingdom introduced a complete draft treaty text with respect to tests yielding 4.75 and above on the seismic scale of magnitude. We stated at Geneva that we were and are prepared to sign this draft treaty or to use it is a basis for further negotiation.

The Soviet Union, on the other hand, has not introduced a single positive proposal within the past year and a half. Instead, since March 21, it has retreated from agreements already reached.

The Soviet Union's most significant backward step was a new proposal that day-by-day executive authority over the international control system be exercised by a three-member administrative council (including a representative of the United States and the United Kingdom together, a single representative of the Soviet Union, and a single neutral representative), which could act only by unanimous consent.

As the committee knows, this administrative council, below the 11-man top control commission, is what we sometimes call the troika or three-headed monster.

This troika proposal retracted the Soviet Union's earlier agreement to a single administrator, who would be appointed with the concurrence of the United States, the United Kingdom, and the Soviet Union.

This administrator would carry out the provisions of the treaty and the policy directives of the top control commission, upon which, as I have said, the Soviet Union and countries associated with it would have equal representation with the West. And then in addition there would be the three neutrals in the top administration.

While a single administrator could take action rapidly, effectively, and impartially, action by the three-man administrative council could be blocked, or delayed, or hamstrung by the veto of any of the three members.

With respect to the three annual onsite inspections which the Soviets have so far offered us, Premier Khrushchev has said "if"—and this is a very big "if"—it can be proved by scientific means alone, and the scientists have told me that it cannot, that the particular suspicious event was a nuclear explosion, then within the limits of three times a year, on the entire Soviet territory, the United Kingdom and the United States would have the right to have inspections on Soviet territory, but then only if we could pinpoint the unidentified event to an area 10 miles square—that is precisely the size of the District of Columbia—which again science cannot do; and then again only if the inspection teams on Soviet territory were headed up by Soviet citizens; which, of course, is no inspection at all.

Other positions the Soviet Union takes at Geneva would also frustrate the concept of exective control. Chief among these are (*a*) the determination to limit inspection of suspicious events in the United States, the United Kingdom, and the Soviet Union, to three annually, as I have mentioned. If a suspicious event occurred in January, and you took it, you would have two left for the other 11 months. You probably would then always debate as to whether you could afford to use one of them up. If you used one up in June, you would then always debate whether you should exercise it during the summer or the fall, for fear of what they might do on Christmas Day. So, in effect, you probably would never use your third. So three, of course, is ridiculous.

Chairman HOLIFIELD. You are fearful of Christmas gifts, then.

Ambassador DEAN. I am fearful of Christmas gifts, yes, especially as borne by the Soviet Union.

Then (*b*) the demand for a delay of 4 years after treaty ratification before control operations begin; and (*c*) insistence on provisions permitting self-inspection, for example, by turning over direction of all control posts and inspection teams in the U.S.S.R. to nationals of the Soviet Union.

Now, the latest developments, apart from the actual discussions at Geneva are (1) the exchange of notes, which conluded with the United States note of July 15, and (2) the request by the United States and the United Kingdom on July 15 that an item entitled "The Urgent Need for a Treaty To Ban Nuclear Weapons Tests Under Effective International Control" be included on the agenda of the 16th session of the General Assembly of the United Nations.

I believe the committee has been given copies of the treaty and of all these documents, but if not, they will be furnished to you.

Chairman HOLIFIELD. I would say to the witness that we do intend to print a number of these documents in the appendix of our hearings. (See p. 395.)

Ambassador DEAN. Thank you, Mr. Chairman. I am very glad to hear that.

I should also like to summarize the status of the negotiations with the Soviet Union concerning the research program, included in Vela, which has been discussed at Geneva. As the Joint Committee's 1960 report points out on page 8, the Berkner Panel exposed deficiencies in the 1958 experts report which has been based upon the data available at that time—data which did not include the results of the Hardtack II series of underground nuclear explosions. In addition, calculations of Dr. Richard Latter, who has testified here, and others indicated the possibility of decoupling, that is, the setting off of nuclear explosions in a large cavity, a possibility which was proven by the Cowboy series of small chemical explosions.

As the result of this new information, the United States proposed that the treaty prohibit only those underground nuclear weapons tests which produced earth tremors of 4.75 and above seismic magnitude; that a 3-year moratorium starting from the date of the new treaty be declared on tests below 4.75; and that a research program be instituted to try to evolve effective control methods for tests covered by the moratorium. This proposal was made in a joint declaration by President Eisenhower and Prime Minister Macmillan on March 29, 1960.

At Geneva, Ambassador Tsarapkin of the Soviet Union at first appeared to agree in principle to the March 1960, proposal. Scientists from the United States, the United Kingdom, and the Soviet Union met to discuss the research program in May of 1960 and, with few exceptions, agreed upon the need for and the scope of the program.

However, the general agreement of these scientists was disowned by the Soviet Union on June 3, 1960, after the collapse of the Paris Summit Conference.

At Geneva, on March 21 of this year, I renewed the research offer and the offer to the Soviets to participate in this research program.

I said at that time, and I quote:

> Although our move, taken in order to reach agreement, involves an unprecedented step in this age of heretofore complete secrecy on nuclear devices, I am able to inform you that if agreement on other treaty provisions is in sight, the President of the United States is prepared to request appropriate action from the Congress in regard to the Atomic Energy Act so as to allow the United States to show the Soviet Union the internal mechanism of the nuclear devices to be used in the seismic research programs.

I then went on to say:

> While on this subject, both Western representatives would like to urge their Soviet colleague to have his Government reconsider its decision of June last not to implement the seismic research program planned by Soviet experts and communicated to their Western colleagues during May 1960.
>
> We are certain that the U.S.S.R. could make a valuable contribution to the seismic research efforts of the two Western countries. A Soviet seismic research program would clearly be a sign of Soviet good will and readiness to cooperate in what will have to be a great range of common endeavors involved in implementing this treaty. It could be an extension of the type of coordinated action included in the International Geophysical Year and would add much to the sum total of scientific knowledge.

I went on to say:

This is quite apart from the participation, which will occur in any case, of Soviet scientists in the carrying out of the United Kingdom and United States seismic research program, all three powers are in general agreement on what has been achieved by that program, it will help each of us to decide what to do at the end of the moratorium period.

By the moratorium period, I was referring to the new proposed 3-year moratorium period, to go into effect when and if the treaty was signed.

I think this is important, because I think it shows the lengths to which we have gone in an effort to get a nuclear test ban treaty and in an effort to get the cooperation of the Soviet Union.

Chairman HOLIFIELD. I would say that the Congress has been aware of this, especially this committee of the Congress; that this remarkable offer in this age of secrecy was made, and we offered no objection to the revealment of certain types of nuclear devices which were not weapons, but which naturally embody some of the principles of weaponry, in order to go far beyond; and many of us thought that we would receive a great deal of criticism from people in our districts, because they would not understand the complications of this matter. And, of course, they would not understand that the Russians already know these principles and have gone far beyond the principle of the device that would be used in these tests, but it would be a situation where the layman would feel we would be giving away the secrets of the atomic bomb, which we would not be, of course.

This committee has been very much concerned about having a successful test ban agreement, but we have also been concerned that it be a real agreement, one whereby we could know what the other fellow was doing and he could know what we were doing. And as we have said to you many a time, we want no agreement unless it is a real agreement.

Ambassador DEAN. I fully share the committee's views that we want no agreement unless it is a real, workable, and effective agreement.

I want to say at this time that I appreciate the very constructive manner in which the committee has worked with us. And while I, of course, understand that if we were to conclude a treaty, this committee would reserve its full judgment——

Chairman HOLIFIELD. This is a matter, as you know, that would require a change in the law. And of course this committee would not have full control over it. But we have indicated a willingness to take this matter to the Congress with a recommendation, a favorable recommendation, for congressional action, in this field, if the other terms of the treaty can be agreed to.

Ambassador DEAN. I think this constructive attitude has made a very profound impression upon other countries. While I have been in Geneva, I have taken the opportunity to brief ambassadors from other countries. I have talked to a great many of the press from other countries. I have made several broadcasts on the radio and television stations of other countries, outlining the proposed treaty, and I think that in talking to editors and the press in Europe, when I have explained to them how far we were willing to go in amending the Atomic Energy Act and what we were willing to do in inviting the scientists to come and participate in this seismic research program—I think it has been one of the most impressive things that we have done.

At Geneva, the Soviet Union has not objected to the conduct by the United States of a research program which included underground nuclear explosions at which they could be present, but it has objected vehemently and often, indeed almost daily, to the decoupled explosions which are designed to increase our knowledge concerning such explosions and methods of detecting them, and it has insisted upon inspection of the nuclear devices used in the program on the ground that such inspection was necessary to establish that we were not really—under the guise of a seismic research program—testing heretofore untested weapons.

As the committee will recall, they demanded four safeguards, to which we gave our assent.

The Soviets argue, of course, that we only want to conduct decoupling shots, these nuclear experiments, in order to learn better how to what they call cheat underground—that is, to get a much bigger yield without the explosion registering as much as 4.75 on the seismic scale of magnitude.

The Soviets use of the word "cheat" is, of course, a misnomer as to events registering less than 4.75, because the proposed treaty would not even apply to that. The Soviets then maintain that once we have produced large-yield explosions underground by the decoupling technique which would not register 4.75 or higher, we won't then extend the proposed moratorium but will try to hold the Soviet Union to the treaty not to conduct nuclear tests yielding 4.75 and above.

Thus, the Soviets demand that the proposed uninspected, uncontrolled 3-year moratorium be extended indefinitely until we agree to lower the treaty threshold on the basis of the results of the seismic research program.

We have agreed to the Soviet inspection on the terms stated during the seismic research tests, if a treaty is signed or in sight. This, of course, involves both the Presidential recommendation and congressional action. We have not agreed to eliminate the decoupling shots or to give the Soviets a veto over any part of the seismic research program, and I have on a number of occasions made this crystal clear to my Soviet colleague at Geneva.

But Mr. Tsarapkin, the Soviet negotiator in Geneva, has issued a virtual Soviet ultimatum, in which he demands that the West either accept the Soviet proposal for an administrative veto over the control organization by vesting the administration of the treaty control system below the 11-man control commission, in a triumvirate requiring unanimity before action, or else—and this is the "or else"—that the West agree to merge the nuclear test ban talks with those scheduled for general disarmament.

Of course, this is contrary to fact and contrary to commonsense.

Let us suppose there were a vote in the 11-man top control organization. Further, let us suppose it went in the West's favor, four Western votes plus two neutrals against four Soviets plus one neutral, or six to five.

Under our plan the single administrator would carry out the decision. But under the Soviet troika proposal, the three administrators at the executive level would again vote and debate—instead of carrying out the top commission's decision—and the Soviet administrator could then veto the decision; even vetoing the decision of the top body.

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Also bear in mind that under our plan the 11-man top control commission, on which the Soviet has equality, appoints the single administrator.

But under the Soviet troika plan each original party would appoint its own nominee, in violation of all well-recognized international organization rules.

Let us be clear. The Soviet troika proposal is not equality. It is anarchy. It would permit sabotage of the control system at the administrative level.

The West, for good reasons, rejected both proposals: the first because the tripartite idea would prevent the prompt and effective administration of enforcement measures or treaty rights; the second on the grounds that a merger of the test ban with general disarmament talks would postpone any test ban treaty to the indefinite future, when overall agreement could be obtained on disarmament.

I might say in this connection that I think we have proved to a good many people who were postive that the Soviets were already to sign to a nuclear test ban treaty, that by our offer to amend the Atomic Energy Act and by our working out a complete text of a treaty and offering to sign it, a good many people who had hitherto believed in the complete good faith of the Soviet Union have had their eyes opened, and they now believe that their new proposal to merge the nuclear test ban talks with general disarmament is, again, a mirage.

It seems to be very difficult to make clear to some people that we were the ones who suggested the 3 neutrals on the 11-man top control mission. Amazing as it may seem, nevertheless, I have seen it suggested that we reject the Soviet troika proposal at the administrative level because we don't want neutrals in the control organization. But as I have just demonstrated, the vote of the single Soviet administrator at the administrative level could nullify the vote of the 2 neutrals on the top 11-man control commission, so I submit it to the Soviets who wish to nullify and make void the voting rights of the 3 neutrals.

Senator PASTORE. Mr. Dean, may I interrupt you at this juncture?

I am getting the impression that you are beginning to feel now as the Joint Committee felt when you left.

Ambassador DEAN. Yes, Senator Pastore. But I think that although we haven't got a treaty, we have now convinced a very large part of the world that the United States sincerely, honestly, wants a treaty; that we have gone more than the second mile in an effort to get the treaty, and that now, I think, the United States stands before the world as the honest negotiator for a nuclear test ban treaty and the Soviet Union does not.

Senator PASTORE. I realize that, and I applaud you for that. But let me ask you this question categorically. We have been dealing with the Russians through some 300 meetings and negotiations in the hope that we could, in good faith, reach an agreement on the cessation of underground testing. You have dealt with them now over a course of months. Would you say that it is your impression that the Russians sincerely hope to reach an agreement on this subject?

Ambassador DEAN. At the present time, because I believe of conditions in Laos and Cuba and the Congo—and you definitely could note their whole change of attitude after they put their man in space; you

could definitely see quite a visible change in them—I think that as of the present time they at least don't seem to want a nuclear test ban treaty. But I remember we worked for 10 years on the Austrian Treaty. And everybody had practically given up. And then one day we were able to work out the Austrian Treaty. We worked for many, many months on the agreement with respect to the International Atomic Energy Agency.

Senator PASTORE. Yes. But would you say that the distinction between the two cases is precisely this: that during the time that we are talking and negotiating with the Russians on an agreement for cessation of underground testing, we are ourselves indulging in a self-imposed moratorium?

Ambassador DEAN. That is correct.

Senator PASTORE. And if the Russians are using this as an opportunity for conducting clandestine testing on their own, we are imperiling the security of this country and the security of the free world?

Ambassador DEAN. Well, as I think you know, Senator Pastore, the President has appointed a panel of scientific advisers to advise him on that point, and I believe that that matter is under very careful and very thorough consideration.

Senator PASTORE. I am not connecting my observation with that, Mr. Dean. I am merely making the distinction that while possibly the circumstances were such that we could afford to wait 8 years in negotiating the Austrian Treaty, here we are confronted with an entirely different situation. Here we are confronted with a situation that might imperil our own national security. If the Russians are using these tactics, these delaying tactics, for purposes of perfecting their own weapons, we are consequently taking the chance on our own progress, and the stakes are so high that we cannot afford to take a chance, in my humble opinion. I mean we cannot afford to wait unless we can at least assume that there is good faith on the part of the Russians. And that is the reason I asked you the question.

Now, bad faith in negotiating an agreement in my opinion would produce an instinctive feeling that there might be bad faith to the extent of not observing the moratorium on their side, as we are observing the moratorium on our side, because we have no way of knowing whether they are or are not.

Ambassador DEAN. That is correct.

Senator PASTORE. And for that reason, the stakes being so high, I think in this case we are dealing with a very, very sensitive situation. And I will tell you very, very frankly that my heart goes out to the President of the United States, who has to make this decision.

Ambassador DEAN. So does mine.

Senator PASTORE. But the decision has to be made. And I pray to God that he makes the right one, because the stakes are so high that they could imperil the peace of the world.

Senator JACKSON. Mr. Chairman, I just want to associate myself with the remarks of Senator Pastore.

First I want to commend you for all that you are doing for the country. I admire your patience. I do not know how you put up with it.

But it does seem to me, Mr. Dean, that we should recognize that in the cold war, talking is one of the many tactics of the Soviet Union.

I have no objection to talking with the Soviets. But I certainly think we should be careful to recognize that for the most part it is just talking. And I would always be willing to sit down and talk with them. But I do not want them to use this business of long, drawn-out discussions as a device to imperil the security of our country. I feel very strongly on that; and it seems to me that Mr. Khrushchev and his people have made it very clear that they are not going to agree to any kind of an agreement that is foolproof or rascal-proof.

It is clear that you have acted in good faith. When you have suggested certain conditions that are necessary for an agreement that can be honestly accepted, they balk. They oppose it. They do not want an agreement that will be foolproof. And it seems to me, further, that what the Soviets are really trying to do at this point—in light of their clear-cut statements that make their position unacceptable to us—is to use this as a device in the cold war. They hope to advantage their position psychologically, and possibly in the military field if they are conducting—and we have no way of knowing—clandestine tests.

This is what really concerns me. Mr. Khrushchev has announced that he is not going to allow, in effect, inspectors in his country. He has made the point time and again that one of the greatest assets that they have is their closed society. He is not about to open up their society. Mr. Tsarapkin indicated that they would not allow this agreement, at one point, I believe, to go into effect for 4 years.

I am willing to exercise, as we all are, extreme patience. But there comes a time when they do certain things that may have the effect of humiliating our country. It seems to us that this hurts us around the world. It would indicate that we are afraid of them, that we are weak.

I was particularly pleased with what you had to say about other countries being impressed now by our good faith in submitting a draft treaty. I just hope we can couple that with the fact that way back in 1946, under the Acheson-Baruch-Lilienthal proposal, we offered to turn over the bomb to the United Nations under a rascalproof system. Who stopped it? The Soviet Union vetoed it.

And I think this fact, along with all the other moves that we have made, in all honesty and good faith, should be widely disseminated by our Government. I find that many people have forgotten that we agreed, when we had a monopoly, to turn over the bomb to the U.N., under a rascalproof system of control. And every time an effort is made to get a foolproof agreement, the Soviets veto it.

It seems to me, very candidly, that we ought to recognize this Geneva meeting as talking. We will always agree to talk. But we are not going to let them use talk and discussions and conferences as weapons to imperil the security of the country. I mean our country, and the nations that rely upon us.

I feel very strongly about this. Three years have gone by, and the time has come when we should take the steps necessary to protect the security of the country by the resumption of underground testing. This decision, of course, is up to the President, as we all know.

Ambassador DEAN. Thank you for what you said initially, Senator Jackson.

As you know, this divides itself into what we have to do for our own protection and our own military security. And that, I am sure, is being very carefully reviewed by President Kennedy as of the present time.

Senator JACKSON. I was not asking you to be responsible for the latter part of my statement because that is a diplomatic problem, and you very properly stayed in your own sphere.

Ambassador DEAN. As to the other part of it, we must, of course, make whatever decisions we have to make for our own military security based on what we decide we need to have.

I am sure you will agree that you cannot completely make our own decisions with respect to our own military security without taking into consideration the health and security of mankind, explaining how we come to make those decisions.

All of the countries who are members of the United Nations also have ambassadors at the European branch, and I have taken opportunity every day to see them, and I have seen a great many representatives from religious organizations, church organizations, and other organizations, who are interested. And I think that I have been able to explain to a great many of those organizations why we have been following our present course.

I find with a matter as difficult and as complex as this, that sometimes you have to go over it with the same individual some six or seven times before he finally sees what we are striving for. I mean so many people have called me up since I have come home and said, "Why won't we accept equality with the Soviets in the control commission?" When I point out to them that we were the ones who proposed putting the three neutrals in the top commission and gave the Soviets the four coequal with the United Kingdom and ourselves, and that this Soviet proposal for a three-man administrative council, with each having a veto power, is really allowing a subordinate to reverse the top organization, they are quite surprised.

Senator JACKSON. But, Mr. Dean, I must say that it seems to me that there are times in the life of a nation when you have to make certain moves that are unpopular. We cannot run a Gallup poll and then decide what move to make. The people who made the strong arguments against nuclear testing pinned them on the poisoning of the atmosphere. All of what we are proposing, suggested underground testing—is not in that category. There is absolutely no possible injury that can occur. These same people do not go around and talk about curtailing the investigations, the research, in bacteriological warfare, which could be, maybe, more devastating.

My point is that I do not think there is anything you can do with some of these people because no matter what you do, it is not going to satisfy them. And I do believe that there are some things we have to do in life that may not be very popular if this country is going to survive. I am more and more convinced of it as time goes on.

Ambassador DEAN. Let me just point out very briefly that, of course, if we don't want the nuclear test ban negotiations merged with the disarmament, as we don't, as of the present time, by continuing the nuclear test ban negotiations at Geneva, we have a forum there, and we can point out to the Soviets and to other people at the United Nations that we are discussing this matter there, that we have worked out this complete treaty, and that we are prepared to sign it.

Now, I don't purport to know what the decision is going to be. But if, by way of chance, a decision were made to resume testing—on which I express no opinion—by continuing the Geneva conference, we can always say to people, "Well, all you have to do to have testing stopped is get the Soviets to sign the treaty."

So it does seem to me that even on the basis of our own military security there are certain advantages in continuing the discussions at Geneva. It seems to me it shows that we are still willing to do our level best to try to reach agreement with the Soviet Union on a nuclear test ban treaty.

Senator JACKSON. I am in favor of our continuing to talk with them, but I do not want to have this used as a means of impairing our own nuclear capability by stopping all testing.

Ambassador DEAN. Thank you, Senator Jackson. I have given the committee a review as to where matters now stand. As the committee knows, the contemplated underground nuclear explosions have not been authorized by the President, and a nuclear test ban treaty has not been signed and is not in sight.

But despite the lack of agreement at Geneva, an "international agreement for the discontinuance of nuclear weapon tests is and will continue to be a prime objective of the U.S. Government," as our Government said in its aide memoire to the Soviet Union on June 17 of this year, following the President's meeting with Chairman Khrushchev in Vienna.

Our draft treaty is the product of almost 3 years of effort on the part of the United States and the United Kingdom, and it constitutes an attempt to work out an effective agreement with the Soviet Union, to which we hope other governments would promptly adhere. If we got it, and a workable and effective one, it would help to stop the dangerous proliferation of nuclear weapons capabilities. It would eliminate the concern over fallout. It would provide an agreed first step toward controlled disarmament and might possibly ease East-West tensions, if indeed that is possible, in view of the Soviet's demands with respect to West Berlin.

We said in our aide memoire of June 17—

> To a world grown impatient with protracted tensions and unease, it would signify the willingness of the major powers to subordinate a narrow concept of their national interests to the higher aim of creating a more peaceful and stable world order.

And because an effective test ban agreement does hold out such a great hope to the world, President Keneedy has said that we would stand by our draft treaty at Geneva despite the Soviets' unreasonable refusal to come to terms with us.

To my mind, all of this reaffirms my belief that we should continue the Vela program with the actual nuclear testing deferred for Presidential decision.

I believe that the Vela results would be useful to complete our actual knowledge concering the detection capabilities of the control system we have proposed in our treaty at Geneva. I think the program would add to the sum total of scientific knowledge in this field. We have much theory but not many proven facts.

Its results are also necessary in order to determine what improvements can be made in the proposed treaty system. Despite some of

the negative testimony you have heard here, which I recognize as quite sound for scientists, who can't predict the outcome of the program, it seems to me that a sound basic research program is bound to bring unforseen results and may to some extent offset the suggested improvements in decoupling techniques, even though some of the scientists are currently pessimistic.

If the results are encouraging, we may be able to detect, and identify as earthquakes, and therefore not as nuclear explosions, small earth tremors which do not register as much as 4.75 on the seismic scale, the threshold established by our draft treaty.

But as I believe Dr. Romney testified, this of course increases your onsite inspection program, because you then have many more tremors to police.

At the end of the proposed 3-year uncontrolled and uninspected moratorium period—depending upon the outcome of the seismic research program and the then state of our seismic knowledge—we might then be able to lower the treaty threshold. Since this, in combination with our proposal to limit the moratorium to 3 years, is a major issue which now stands in the way of agreement, possibly we might bring agreement closer by the proposed seismic research program. At least we would know much more about what we are doing. In all basic research, as the committee knows, little can be promised in actual results or on time. And sometimes we find things of great value for which we were not looking.

In my private capacity, I am chairman of the board of trustees of Cornell University, where we concentrate on basic and not applied research. And we often find that we get unexpected results from basic research programs.

Scientists will not promise you much, but I find that if you give them the tools, they will often amaze you at what they can find.

We have made major and constructive moves at Geneva in an effort to get an effective and workable nuclear test ban treaty and moratorium. We have had the most complete cooperation and help from our British colleagues, for which we are very thankful. We are in complete agreement with the British, both on the treaty and on the methods and procedures.

We plan to follow these fundamental American precepts of plain talk and fair dealing, patient and careful research, and an ironclad determination, as President Kennedy so clearly enunciated on West Berlin, not to yield basic points merely for the sake of agreement.

We shall continue to tell our position at Geneva unashamedly in simple and straightforward fashion and repeat it again and again until it is understood, not only at the negotiating table but all over the world.

The Soviet Union has denounced these simple American tactics as unfair and has expected us to yield. For as a people we like results and not yakity-yak or talk.

Indeed, the Soviet representative has protested bitterly again and again about our having the audacity to table the complete text of a nuclear test ban treaty and to say that we are ready to sign it and to expound its provisions day after day in clear language. We have done exactly that, and we plan to continue to do it.

Thus, in conclusion, I think we should continue the Vela program, because in my judgment, it will materially add to our seismological knowledge, which is presently very limited, and will aid us in bringing about an international agreement for the discontinuance of nuclear weapons tests, which President Kennedy has authorized me to say continues to be a prime objective of our Government.

Thank you very much.

Chairman HOLIFIELD. Thank you, Mr. Ambassador.

Mr. Price, any questions?

Representative PRICE. I want to congratulate you, Mr. Dean, on a very fine statement. As has been already expressed, I think the general feeling in the Joint Committee supports your position and the fine work you have done since you have taken over the negotiations in Geneva.

Just for the purpose of clarification, when you refer to "tabling" the treaty at Geneva, do you mean you put it there for consideration?

Ambassador DEAN. That is correct.

Representative PRICE. You see, in legislative language, when you table something, you are through with it, finished with it. So I just would like to express that for purposes of clarification. It is lying on the table for the consideration of the negotiators.

Ambassador DEAN. That is correct. Thank you very much.

Chairman HOLIFIELD. Under legislative language, as you know, the use of the term "tabled" has a completely different meaning from what you have used in this presentation.

Representative PRICE. And that is the reason I wanted to make the clarification.

Ambassador DEAN. Thank you very much.

Representative VAN ZANDT. Mr. Chairman, I should like to commend Mr. Dean for the splendid representation he has given the American people at Geneva.

Ambassador DEAN. Thank you very much, Mr. Van Zandt.

Representative VAN ZANDT. We certainly go right along with you. and the more you pound the table, the happier we are, I am sure.

On page 2 you talk about the moratorium and the fact that President Eisenhower included it, and then someone came forth with this new temporary one. Do the Russians take into consideration the fact that the moratorium declared in October several years ago is no longer in existence, and that we are now functioning under a temporary one?

Ambassador DEAN. Well, I have said to them any number of times that there is no moratorium as of the present time.

In dealing with the Soviets, as I found when I was dealing with them in Korea, when they say "democracy," they mean communism. When they use certain words, they use them in the Soviet lexicon.

If you look "moratorium" up in the dictionary, it is for a limited period. They insist, however, that when they agree to the eixsting moratorium, it could only be terminated with their consent.

Now they are arguing that even if you gave them the new 3-year moratorium, we could not, even at the end of 3 years, terminate it without their consent. They argue that it has got to go on forever until such time as we are willing to lower the treaty threshold to zero. Otherwise, they claim we have got no right to terminate it.

Representative VAN ZANDT. Did they ever announce a 3-year moratorium along the lines that we did?

Ambassador DEAN. No. There is a 3-year time limitation in the moratorium proposed to be entered into by each of us when we sign the treaty. The 1-year moratorium on testing was announced by President Eisenhower on August 22, 1958, and put on a voluntary basis by him on December 29, 1959, to which the Soviets made an indirect and informal statement in Tass on November 14, 1958, in which they referred to their statement of October 30, 1958, about discontinuing weapon tests. They announced in similar language. But I have had several conversations with Mr. Tsarapkin, and he insists that the Russian understanding or the Soviet understanding of "moratorium" is different from the American understanding, and it means an unlimited period unless terminated by mutual consent.

Representative VAN ZANDT. On page 4 of your statement, Mr. Dean, you mention "to accommodate the Soviet sensitivities. I wonder if you would spell out for us a few of these areas where they are very sensitive.

Ambassador DEAN. I think from a purely scientific standpoint I do not think there can be any question that if you were dealing with normal people, wherever there was an unidentified event you could go in and inspect it, no matter how many times you went in there. They apparently have an almost pathological aversion to having foreigners come onto their territory and to make inspections. They did agree to having a certain number of these seismological control stations placed on their territory, and they did agree to having three onsite inspections per annum, provided, as I said, that you could pinpoint the unidentified event, provided the chiefs of the inspection teams are Soviet citizens, provided you can identify it as a nuclear explosion by wholly objective methods, and so forth.

Our scientists thought that there might be as many—not the same number every year, as I am sure the committee knows, but there might be approximately a hundred unidentified seismic events of magnitude 4.75 and above on the Soviet territory. It was generally thought that if you investigated 1 out of 5, that would give us 20 on-site inspections per annum.

The Soviet scientists insisted that there were no more than 60 unidentified events on their territory. So we said to them: "All right. If your scientists are correct, and there were never more than 60, on the basis of 1 out of 5, then the maximum is 12, and we will take 12, because if there aren't any more, we are not entitled to them. If, on the other hand, our scientists are correct, and there are a hundred, then we get the 20."

Well, they wouldn't even take the 12, although we had accepted their scientists' prognosis as correct.

They seemed to have an almost, as I say, pathological aversion to having people come onto their territory, even though they are scientists.

I think our scientists are agreed that for all practical purposes you probably couldn't mount more than 12 or so complete onsite inspections per annum. It is quite a big job. You have got to get a lot of electrical cables and big equipment, and you may have to have earth-moving machinery, and you may have to dig. Each one of these inspection teams might take quite a number of weeks.

I think all of our scientists were generally in agreement that somewhere between 12 and 20 was a reasonable number, but I think on a purely scientific basis, on a purely reasonable basis—I don't think there ought to be any limit on the annual number of inspections.

That is what I meant by "reasonable relations" of the sensitivities of the Soviet to these on-site inspections.

Representative VAN ZANDT. That is all, Mr. Chairman.

Chairman HOLIFIELD. Mr. Bates?

Representative BATES. I regret I did not hear all of your statement, Mr. Dean. I will read it.

We have been engaged in these discussions since October of 1958?

Ambassador DEAN. That is correct, sir.

Representative BATES. Now, it does not appear to me that we are any closer to the solution today than we were when we commenced. Is that a fair assessment of the situation?

Ambassador DEAN. Well, Senator Jackson—and I agree—was pointing out to me that we might be running very serious risks in continuing to talk. But I do think that we have narrowed the issues down to relatively few in number, and as I said earlier, they negotiated 10 years on the Austrian Treaty, and then you got it, and were a long time on the International Atomic Energy Treaty, and then we got it. I can't see any immediate possibility of an agreement, but I wouldn't say that it is impossible.

Representative BATES. Well, there is a distinct difference between Austria and this situation that confronts us now. We had a situation in China many years ago where they negotiated every time the Communists were in trouble. I do not mind talking, providing that while we are talking we are not standing still while they are advancing.

That is what disturbs me. You folks can continue talking, so far as I am concerned, forever. But in the meantime, it seems to me we must assure our own security.

Ambassador DEAN. I am sure, as you know, Mr. Bates, the President has under very careful consideration this question with respect to resumption of testing, and I am very sure that that will be a very careful decision.

I am sure you probably read the Daily News one day which said that, "If Arthur Dean wants to go back and stay at expensive hotels and eat the expensive food in Switzerland, as long as the taxpayers can stand it, we suppose it is all right." So far as I am concerned, I am very happy to negotiate in Geneva as long as the President wants me to, but my personal preference would be to stay home.

Representative BATES. I am sure that is true.

Chairman HOLIFIELD. On page 1, paragraph 3 of your statement, Mr. Dean, you state that you are not expressing an opinion on whether the Vela underground nuclear detonations should take place, and if so, when, as that is, of course, a matter which must be decided by the President.

In our discussions in negotiations with the U.S.S.R., has it not been the policy of the U.S. Government, under both the previous administration and the present administration, that nuclear detonations should take place under a research program to improve our detection program?

Ambassador DEAN. That is correct, Mr. Chairman.

Chairman HOLIFIELD. Is it not, then, more accurate to state that our negotiators, including yourself, have taken the position that if we are to improve our detection capability, then nuclear detonations should take place?

Ambassador DEAN. Yes. That is correct. And I checked all of this, I think, with the existing records. You may have some information I do not have. It is my understanding that the Vela program, and the idea of the explosions in connection with this program, were approved in the Department of Defense. It is my understanding that they were approved by the President's scientific advisers. I believe at that time they were President Eisenhower's scientific advisers. I think the program has also been approved by President Kennedy's scientific advisers.

But it is my understanding that the President himself has not yet actually authorized the taking place of the explosions. That is all I meant to say.

Chairman HOLIFIELD. So the point is that there is an agreement that if we are going to improve our detection capability, there comes a time when we must have nuclear detonations; that we have run out of the field of chemical HE-type explosions and we must go to the nuclear explosions?

Ambassador DEAN. As I understand it, underground, without any radioactive fallout, and with the Soviet observers being present as I have indicated.

Chairman HOLIFIELD. That is right. And that the President will decide if and when we get the detonation of these nuclear devices for improving our seismic detection program. The decision is left to the President.

But as to the need for it, and the method of using these nuclear explosions to check our seismic capability, that has already been a decision of the previous administration and this administration, too.

Ambassador DEAN. And there really is no difference of opinion between ourselves and the Soviets on that point, except for the fact that they keep inveighing against our inclusion in the seismic program of the decoupling shots.

Chairman HOLIFIELD. They object to any type of a test that in effect proves that the original Geneva formula was in error?

Ambassador DEAN. Well, that is a long story, as you gentlemen are aware.

Chairman HOLIFIELD. I will not start you on that.

There is a bill before the Congress, Mr. Ambassador, which has been introduced to establish a U.S. Disarmament Agency for World Peace and Security. It has been introduced by a large number in the House. And in section 32 of that bill in regard to conducting research, the Agency would be authorized "to use with their consent the facilities of other Government agencies or those of outside agencies."

Now, the question occurs on this point. Do you believe that the Disarmament Agency, if it is established, should be the Agency that would have control of the Vela program, based on the consent of the facilities? Or do you believe it might be better to place on the Disarmament Agency the power to direct other agencies to do the necessary research, rather than to be dependent on their consent?

I am not advocating this, you understand. I am asking the question.

Have you given any thought to that?

Ambassador DEAN. Yes, I have, Mr. Chairman.

I worked with Mr. McCloy and Mr. Fisher on the drafting of this bill. Our general thought was this. And it was fortified by my reading of Sherman Adams' latest book, in which he points out that disarmament is generally kicked around from pillar to post, and that when people are brought in on disarmament they are not in any department. They have no budget of their own. They have no power to pick their personnel. Their personnel are assigned to them by other departments and then withdrawn. A disarmament administrator without this bill has no power of promotion. And generally, people taken out of the other departments are sometimes taken out of the line of promotion; so they don't like this temporary assignment to disarmament.

So it was our thought that if this could be set up not as a completely independent agency, in the sense that it would have its own budget and its own table of organization, we thought as a matter of efficiency it ought to be set up within the Department of State, but we did think that the Director, while reporting to the Secretary of State, should have, like the Joint Chiefs, the power to go to the President, but he should have his own budget and be able to direct his own personnel policies.

Now, our theory was that we discovered earlier—and I should think this would only be logical—that if you were to assign, let's say, an Air Force general to be Disarmament Administrator, and one of his first proposals was that you were to cut down on the Air Force, I wouldn't think that fellow would be very popular back in the Air Force, as long as he was still depending on that Department's recommendation for promotion.

Chairman HOLIFIELD. It might interfere with his promotion?

Ambassador DEAN. Yes. We also wanted to save as much as we could on the governmental expense. We did not want to build up a huge organization with tremendous staffs or tremendous expenses. We wanted, insofar as possible, to utilize all existing organizations. We thought that the AEC or any other organizations that were in existence should continue, but that their facilities could be used by the Disarmament Director in cooperation with the existing agencies.

That was the theory of this language in 32 that the Director was authorized, with their consent, to use the facilities of other governmental agencies.

I couldn't help thinking this morning, when I was reading the report, that the international space organization could not get a carrier assigned to it by the Navy, or that they could not get a type of helicopter with sufficient power to pick up the astronaut. And I was thinking about this very provision in this bill about how should we draft it in order to make it efficient and yet still to try to get away from the idea of duplicating a lot of facilities and having duplicate personnel and duplicate budgets and all that sort of thing.

I think this is something that ought to be worked out, so that it is done with the consent of the existing agencies, but I do think that the Director ought to have some more power than merely going around

and saying "please" and having people say, "Get out. We are too busy to bother with you."

Chairman HOLIFIELD. I would say if this is going to work, at that point where the Disarmament Agency did ask for the function of a department, unless they were backed up by Presidential directive, you are just in that position where you go around holding your hand out and asking for something, and it is left up to the other agency to have the power to refuse.

I can understand your position in not wanting to build up a parallel organization in many fields.

Are there any further questions of Mr. Dean?

If not, Mr. Dean, we again express our appreciation for you being before us today, and for the work that you are doing. We wish you Godspeed.

Ambassador DEAN. Thank you very much, Mr. Chairman.

(The entire statement of Ambassador Dean follows:)

STATEMENT OF AMBASSADOR ARTHUR H. DEAN, U.S. REPRESENTATIVE TO THE CONFERENCE ON DISCONTINUANCE OF NUCLEAR WEAPONS TESTS

For the last 3 days you have heard testimony concerning the technical and scientific aspects of the Vela program, some of it which appears to be rather on the cautionary or pessimistic side, as I read it. As you have seen from the testimony, knowledge in any field is never static. This is particularly true in a new field such as nuclear energy.

I am here to emphasize the continued need for the scientific results of the Vela program, particularly as they relate to this country's efforts to secure an effective agreement on the cessation of nuclear weapon tests and as they bear on our ability to detect nuclear explosions underground or in outer space. Dr. Leonard has testified here and he helped us in the drafting of our proposed treaty provisions on outer space. Other scientists who have also testified here, including Dr. Richard Latter and Dr. Carl Romny, have also helped us—but the drafting and responsibility is the State Department's and not the scientists.

Let me be clear. I am, of course, not expressing an opinion on whether the Vela underground nuclear detonations should take place, and, if so, when, as that is, of course, a matter which must be decided by the President. By my testimony here, I do not wish to express any opinion as to whether the very real scientific need for this program may or may not be outweighted by other factors not related to the program. Again, this is a decision which only the President can make.

Technically, there is no moratorium in existence on any nuclear explosions in this country. On December 29, 1959, at the end of a declared moratorium which had existed for over a year, President Eisenhower said, "We consider ourselves free to resume nuclear weapons testing. * * *" Because, at that time, we were resuming "negotiations in a continuing spirit of seeking to reach a safeguarded agreement," President Eisenhower declared a "voluntary suspension of nuclear weapons tests" which could be terminated at any time.

It is this voluntary suspension which is now in effect.

The Vela program evolved originally from the recommendations set forth in the 1959 report of the panel on seismic improvement, otherwise known as the Berkner panel. As President Eisenhower announced on May 7, 1960, the program is designed to provide "full understanding of both the capabilities of the presently proposed detection system and the potential for improvements in this system. * * *"

The Vela program was thus initiated primarily to provide the necessary scientific information to establish and improve the capability of the system for detection and identification of nuclear explosions proposed at the Geneva negotiations.

Messrs. McCloy and Fisher and I have met with members of this committee several times during these negotiations to give you status reports, and you are all well aware of the position the nuclear test ban negotiations are now in at Geneva.

Briefly, beginning on March 21 of this year, the United States and the United Kingdom introduced at Geneva a large number of new proposals dealing with the principal unresolved issues at the conference—the top control commission, the method of appointing the executive administrator and his first deputy, the makeup of onsite inspection teams, the citizenship of the chiefs of control posts, the number of annual onsite inspections on Soviet territory, the number of control stations on Soviet territory, the criteria for locating unidentified earth tremors, the length of a proposed moratorium on tests registering below 4.75 on the seismic scale of magnitude, a moratorium which is not to begin until the proposed treaty is signed. One of our most important new proposals would give the Soviet Union and states associated with it four seats—or a position of equality with the United States, the United Kingdom, and states associated with them—on the top policy-making control commission; and it would give three seats to nonassociated states. The control commission would thus have a total of 11 seats with a majority of 6. The new proposals would also go as far as reasonably possible, consistent with the technical and organizational requirements of effective control, to accommodate Soviet sensitivities concerning inspection and other necessities of control. Contrary to some of the testimony you have heard here, our scientific advice—both British and American—was that the control system set forth in our treaty would provide reasonably adequate control over those regions which the treaty purported to control. Of course, you can always spend more money and hire more men.

On April 18, 1961, the United States and the United Kingdom introduced a complete draft treaty text with respect to tests yielding above 4.75 on the seismic scale of magnitude. We were and are prepared to sign this draft treaty or to use it as a basis for further negotiation.

The Soviet Union, on the other hand, has not introduced a single positive proposal within the past year and a half. Instead, since March 21, it has retreated from agreements already reached. Its most significant backward step was a new proposal that day-by-day executive authority over the international control system be exercised by a three-member administrative council (including a representative of the United States and the United Kingdom, a representative of the U.S.S.R., and a neutral representative) which would act only by unanimous consent.

As you know, this administrative council, below the 11-man top control commission, is what we sometimes call the Troika or three-headed monster.

The Troika proposal retracted the Soviet Union's earlier agreement to a single administrator, who would be appointed with the concurrence of the United States, the United Kingdom, and the U.S.S.R.

The Administrator would carry out the provisions of the treaty and the policy directives of the top control commission, upon which, as I have said, the U.S.S.R. and countries associated with it would have equal representation. In addition, there would be three neutrals in the top administration.

While a single administrator could take action rapidly, effectively, and impartially, action by the three-man administrative council could be blocked, or delayed or hamstrung by the veto of any of the three members.

Of course, Premier Khrushchev has said "if" and this is a big "if"—that if it can be proved by scientific means alone—which it cannot—that the suspicious event was a nuclear explosion, then within the limits of three times a year, on Soviet territory, the United Kingdom, and the United States would have the right to have inspections on Soviet territory but then only if we could pinpoint the unidentified event to an area 10 miles square—the size of the District of Columbia—which science cannot do—and then only if the inspection teams on Soviet territory were headed up by Soviet citizens—which is no inspection at all.

Other positions the U.S.S.R. takes at Geneva would also frustrate the concept of effective control. Chief among these are: (*a*) The determination to limit inspection of suspicious events in the United States, the United Kingdom, and the Soviet Union to three annually, as I have mentioned; (*b*) the demand for a delay of 4 years after treaty ratification before control operations begin; and (*c*) insistence on provisions permitting self-inspection, for example, by turning over direction of all control posts and inspection teams in the U.S.S.R. to nationals of the Soviet Union.

The latest developments are (1) the exchange of notes concluding with the U.S. note of July 15 and (2) the request by the United States and the Untied Kingdom on July 15, 1961, that an item entitled "The Urgent Need for a Treaty to Ban Nuclear Weapons Tests Under Effective International Control"

be included on the agenda of the 16th session of the General Assembly of the United Nations.

I believe the committee has been given copies of all these documents. If not, they will be furnished to you.

I should also like to summarize the status of the negotiations with the Soviet Union concerning the research program, included in Vela, which has been discussed at Geneva. As the committee's 1960 report points out (p. 8), the Berkner planel exposed deficiencies in the 1958 experts report which had been based upon the data available at that itme—data which did not include the results of the Hardtack II series of underground nuclear explosions. In addition, calculations by Dr. Richard Latter, who has testified here, and others indicated the possibility of decoupling, a possibility which was proven by the Cowboy series of small chemical explosions.

As the result of this new information, the United States proposed that the treaty prohibit only those underground nuclear weapons tests which produced earth tremors of more than 4.75 seismic magnitude; that a 3-year moratorium starting from the date of the treaty be declared on tests of 4.75 or below; and that a research program be instituted to evolve effective control methods for tests covered by the moratorium. This proposal was made in a joint declaration by President Eisenhower and Prime Minister Macmillan on March 29, 1960.

At Geneva, Ambassador Tsarapkin appeared to agree in principle to the March 1960 proposal. Scientists from the United States, the United Kingdom, and the Soviet Union met to discuss the research program in May of 1960 and, with few exceptions, agreed upon the need for and the scope of the program.

However, the general agreement of these scientists was disowned by the Soviet Union on June 3, 1960, after the collapse of the Paris summit conference.

I renewed the research offer and the offer to the Soviets to participate on March 21, 1961.

I said: "Although our move, taken in order to reach agreement, involves an unprecedented step in this age of heretofore complete secrecy on nuclear devices, I am able to inform you that if agreement on other treaty provisions is in sight, the President of the United States is prepared to request appropriate action from the Congress in regard to the Atomic Energy Act so as to allow the United States to show the Soviet Union the internal mechanism of the nuclear devices to be used in the seismic research program.

* * * * * * *

"While on this subject, both Western representatives would like to urge their Soviet colleague to have his Government reconsider its decision of June last not to implement the seismic research program planned by Soviet experts and communicated to their Western colleagues during May 1960.

"We are certain that the U.S.S.R. could make a valuable contribution to the [seismic research] efforts of the two Western countries. A Soviet seismic research program would clearly be a sign of Soviet good will and readiness to cooperate in what will have to be a great range of common endeavors involved in implementing this treaty. It could be an extension of the type of coordinated action included in the International Geophysical Year and would add much to the sum total of scientific knowledge.

"This is quite apart from the participation, which will occur in any case, of Soviet scientists in the carrying out of the United Kingdom and United States seismic research program, all three powers are in general agreement on what has been achieved by that program, it will help each of us to decide what to do at the end of the moratorium period.

While the Soviet Union has not objected to the conduct by the United States of a research program which included underground nuclear explosions at which they could be present, it has objected vehemently and often to the decoupled explosions which are designed to increase our knowledge concerning such explosions and methods of detecting them, and it has insisted upon inspection of the nuclear devices used in the program on the ground that such inspection was necessary to establish that we were not really—under the guise of a seismic research program—testing hereto untested weapons.

The Soviets argue, of course, that we only want to conduct decoupling shot nuclear experiments in order to learn better how to "cheat" underground—that is to get a much bigger yield without the explosion registering as much as 4.75 on the seismic scale of magnitude.

The use of the word "cheat" is, of course, a misnomer as to events registering 4.75 or less. Because the proposed treaty would not even apply to them. The Soviets maintain that once we have produced large-yield explosions underground by the decoupling technique which would not register 4.75 or higher, we won't extend the proposed moratorium but will try to hold the Soviet Union to the treaty not to conduct nuclear tests yielding above 4.75.

Thus, they demand that the proposed uninspected, uncontrolled 3-year moratorium be extended indefinitely until we agree to lower the treaty threshold on the basis of the results of the seismic research program.

We have agreed to the Soviet inspection on the terms stated during the seismic research tests, if a treaty is signed or in sight. This, of course, involves a Presidential recommendation and congressional action. We have not agreed to eliminate the decoupling shots or to give the Soviets a veto over any part of the seismic research program and I have on a number of occasions made this crystal clear to my Soviet colleague at Geneva.

But Mr. Tsarapkin, the Soviet negotiator in Geneva, has issued a virtual Soviet ultimatum, demanding that the West either accept the Soviet proposal for an administrative veto over the control organization by vesting the administration of the treaty control system below the 11-man control commission in a triumvirate requiring unanimity before action, or else that the West agree to merge the nuclear test ban talks with those scheduled for general disarmament.

This is contrary to fact and contrary to commonsense.

Suppose there were a vote in the 11-man top control organization. Further suppose it went in the West's favor four plus two neutrals against four Soviet votes plus one neutral, or six to five.

Under our plan the single administrator would carry out the decision. Under the Soviet troika proposal, the three administrators at the executive level would again vote and debate—instead of acting—and the Soviet administrator could veto the decision.

Also bear in mind under our plan the 11-man top control commission appoints the single administrator.

Under the Soviet "Troika" plan each original party would appoint its own nominee in violation of all well-recognized international organization rules.

Let us be clear. The troika proposal is not equality. It is anarchy. It would permit sabotage of the control system at the administrative level.

The West, for good reasons, rejected both proposals: The first because the tripartite idea would prevent the prompt and effective administration of enforcement measures or treaty rights; the second on the grounds that a merger of the test ban with general disarmament talks would postpone any test ban treaty to the indefinite future when overall agreement could be obtained on disarmament.

It seems to be very difficult to make clear to some people that we were the ones who suggested the three neutrals on the 11-man top control commission. It has been suggested that we rejected the troika because we don't want neutrals in the control organization. But as I have just demonstrated the vote of the single Soviet administrator could nulify the vote of the two neutrals on the top 11-man control commission.

That is where matters now stand. The contemplated underground nuclear research explosions have not been authorized by the President and a nuclear test ban treaty has not been signed and is not in sight.

Despite the lack of agreement at Geneva, an "international agreement for the discontinuance of nuclear weapon tests is and will continue to be a prime objective of the U.S. Government," as our Government said in its aide-memoire to the Soviet Union on June 17, 1961.

Our draft treaty is the product of almost 3 years of effort on the part of the United States and the United Kingdom to work out an effective agreement with the Soviet Union to which we hope other governments would promptly adhere. Such an agreement would help to stop the dangerous proliferation on nuclear weapon capabilities. It would eliminate the concern over fallout. It would provide an agreed first step toward controlled disarmament and might possibly ease East-West tensions if indeed that is possible in view of the Soviets' demands with respect to West Berlin.

As our Government's aide-memoire of June 17, 1961 stated:

"To a world grown impatient with protracted tensions and unease, it would signify the willingness of the major powers to subordinate a narrow concept of their national interests to the higher aim of creating a more peaceful and stable world order."

Because an effective test ban agreement holds out such a great hope to the world, President Kennedy has said that we would stand by our draft treaty at Geneva despite the Soviets' unreasonable refusal to come to terms with us.

To my mind this reaffirms my belief that we should continue the Vela program with the actual nuclear testing deferred for Presidential decision.

Its results would be useful to complete our actual knowledge concerning the detection capabilities of the control system we proposed in our treaty at Geneva, and it would add to the sum total of scientific knowledge in this field. We have much theory but not many proven facts.

Its results are also necessary in order to determine what improvements can be made in the proposed treaty system. Despite some of the negative testimony you have heard here, a sound basic research program is bound to bring unforeseen results, and may to some extent offset the suggested improvements in decoupling techniques even though some of the scientists are currently pessimistic.

If the results are encouraging, we may be able to detect, and identify as earthquakes and, therefore, not nuclear explosions, small earth tremors which do not register as much as 4.75 on the seismic scale, the threshold established by our draft treaty.

At the end of the proposed 3-year uncontrolled and uninspected moratorium period—depending upon the outcome of the seismic research program and the then state of our seismic knowledge—we might then be able to lower the treaty threshold. Since this, in combination with our proposal to limit the moratorium to 3 years, is a major issue which now stands in the way of agreement, we might bring agreement closer by the proposed seismic research program. At least we would know much more about what we are doing. In all basic research, little can be promised in actual results or on time. Sometimes we find things of great value for which we were not looking.

In my private capacity I am chairman of the board of trustees of Cornell University where we concentrate on basic and not applied research.

Scientists generally won't promise you much but give them the tools and they will often amaze you at what they can find.

We have made major and constructive moves at Geneva in an effort to get an effective and workable nuclear test ban treaty and moratorium. We have had the most complete cooperation and help from our British colleagues for which we are most thankful. We are in complete agreement.

We plan to follow fundamental American precepts of plain talk and fair dealing, patient and careful research and an ironclad determination, as President Kennedy so clearly enunciated on West Berlin, not to yield basic points merely for the sake of agreement.

We shall continue to tell our position at Geneva unashamedly in simple and straightforward fashion, and repeat it again and again, until it is understood, not only at the negotiating table but all over the world.

The Soviet Union has denounced these simple American tactics as unfair and has expected us to yield. For as a people we like results and not yakity-yak or talk.

They have protested bitterly about our tabling a complete text of a nuclear test ban treaty and saying we are ready to sign it and expounding its provisions day after day in clear language. We have done that. We shall continue to do it.

Thus, we should continue the Vela program because, in my judgment, it will materially add to our seismological knowledge, which is presently very limited, and will aid in bringing about an international agreement for the discontinuance of nuclear weapons tests, which President Kennedy has authorized me to say continues to be a prime objective of the U.S. Government.

Thank you.

Chairman HOLIFIELD. We are going to ask Major Poulson, who testified this morning to please come to the witness chair for a few questions.

Major Poulson, you may have Dr. Leonard, who was with you this morning, come to the witness table also if you wish.

Mr. Ramey has a question or two as a result of the testimony which occurred after I had to leave the room this morning.

Mr. RAMEY. This morning we discussed the use of these SNAP devices as power sources in your satellites. And as I understood it, you indicated that your requirements were such that they were sort of in between the radioisotope source and the nuclear reactor kind of SNAP sources. And I gathered you indicated your power requirements were in a range of a hundred watts.

STATEMENT OF MAJ. JOHN A. POULSON, USAF, CHIEF, VELA HOTEL BRANCH, ADVANCED RESEARCH PROJECTS AGENCY—Resumed

Major POULSON. More like 40 to 50 watts, sir.

Mr. RAMEY. We have done some checking with Colonel Anderson who was in charge of the SNAP unit at AEC, and he indicates that all you have to do is get in touch with them and set up a requirement, and they think they can supply you with a radioisotope source. They are developing radioisotope sources up to 500 watts, it is our understanding, for the Air Force. It was just about to be completed, and the requirements were canceled.

Major POULSON. I would like to state that we will look into this, very definitely, and report back to the committee. The reason why we have not really taken a serious look before in the Vela Hotel R. & D. program was because solar cells were adequate for the job. The national space effort has had more experience with solar cells, and, therefore, we have not been under much compulsion to look for something else. But because this subject was brought up, and because I personally think that the future of space power supply lies with the SNAP program, we will definitely evaluate it for the R. & D. shots, and report back to the committee the results of this evaluation.

ADVANCED RESEARCH PROJECTS AGENCY,
Washington, D.C., September 11, 1961.

HON. CHET HOLIFIELD,
Chairman, Joint Atomic Energy Committee,
House of Representatives.

DEAR MR. HOLIFIELD: During the open hearings on controls associated with a nuclear weapons test ban, in response to the question by Mr. James T. Ramey regarding the utilization of radioisotope (SNAP) power sources for the Vela Hotel experimental satellites, I indicated that I would conduct a thorough evaluation of satellite auxiliary power supply systems employing SNAP devices and solar batteries and report the results to the committee. This evaluation is now essentially completed, and I will herein discuss the highlights of the results. The backup technical report will be completed by the end of September 1961* and will be transmitted to you at that time. The Air Force Systems Command, the Los Alamos Scientific Laboratory, and the Aerospace Corp. have performed most of the compiling and analyzing of the information and data for the technical report. They were assisted by the SNAP Office, Headquarters Atomic Energy Commission, the Martin Co., and the Applied Physics Laboratory of the Johns Hopkins University.

It should be kept in mind that the discussion which follows pertains only to the requirements for the five experimental satellite flights which were described during the hearing. It in no way prejudges the case for possible follow-on operational satellites in which the technical requirements will differ considerably from those established for the first five experimental launches. The question of the operational satellites will be studied in detail as the experimental program progresses.

In establishing the technical requirements for the first five flights, the primary ground rule was simplicity of concept and operating equipment, in order to enhance the probability of obtaining experimental data which were amenable to meaningful interpretation. In other words, wherever possible we will in-

*This classified report entitled "Feasibility Study on the Use of Radioisotope Power Systems for the Vela Hotel Mission" is on file with the Joint Committee on Atomic Energy.

corporate simple state-of-the-art techniques for detectors and associated logics, use proven components and subsystems for the rest of the spacecraft, and resist the urge toward sophistication which tends to complicate the spacecraft and the task of interpreting the data received. This has led to a decision not to use SNAP devices on the first five flights for the following reasons:

(*a*) A prime benefit of the SNAP device is relatively long life expectancy. From a reliability analysis of the electronic logics and communication equipment to be installed on the spacecraft for the first five flights, it was estimated that an expected spacecraft lifetime of about 6 months was a reasonable design goal. Experience to date indicates that solar batteries can completely satisfy this requirement so that the benefit of a much longer life potential inherent with SNAP devices is not pertinent to these flights.

(*b*) A power supply system employing a SNAP device will contain a radiation source. This radiation must be shielded in order to preclude adding significantly to the natural radiation background. Due to weight and configuration restrictions and to the possibility of impurities in the radiation source, the shield design problem is developmental in nature. The question of achieving a shield design which acceptably compromises the many parameters involved introduces an unknown which is highly undesirable at this time in view of the unknowns associated with the natural background. Solar batteries circumvent this problem. Of course, if SNAP devices are used on subsequent flights, this question will have to be resolved. However, by that time it is hoped that the natural background effects will not constitute the unknown that they now do.

(*c*) A power supply system employing a SNAP device will require a capability to reject a large portion of the thermal heat generated in order to provide for adequate temperature control. An installation which minimizes the heat rejection problem and permits shadow shielding is to place the SNAP unit outside the spacecraft structure as a self-contained system several feet away. This is the current approach under consideration for most space applications. However, on the Vela Hotel experimental flights every effort is being made to minimize the effects of cosmic ray "shower" production. Under such conditions this method of installation does not appear attractive. In order to minimize "shower" effects, any SNAP unit employed would have to be positioned inside the spacecraft structure. While this is not an insurmountable problem, it does represent an additional complexity not present if solar batteries are used.

(*d*) The weight breakdown on each spacecraft has to be carefully budgeted. In order to provide the 60 watts approximately required, the power supply system employing a SNAP device and radiation shield will require more weight than one employing solar batteries. Since every pound of weight is critical and since it is desired to maintain as high a proportion of the weight as possible in the experiments involving detectors and associated logics, it is felt that this one factor alone dictates using solar batteries.

It might be beneficial to briefly mention the unique requirements which make the SNAP power source attractive for the Transit and Surveyor missions which will be roughly contemporary with the five Vela Hotel launches. The Transit mission requires long life in an environment which experiences many eclipses per day in the low altitude orbit. The resultant number of thermal and storage battery discharge-charge cycles would be very detrimental to solar battery system reliability. This led to a decision to incorporate a SNAP device even though the power requirement is relatively low, 20 to 40 watts. The Surveyor mission requires a power supply on the lunar surface which must not be dependent upon receiving solar energy. Again, a SNAP device provides a unique choice. In looking into the future, we can anticipate much higher power requirements as spacecraft grow larger and as various forms of electrical propulsion phase in. Under these circumstances, the power supply systems developed under the SNAP program constitute the most probable source.

In summary, there just does not appear to be any overriding advantage or benefit to be derived from using a SNAP power supply on the initial five Vela Hotel flights. On the contrary, it seems that the incorporation of such a power supply on these flights would give rise to additional problems and complexities which should be avoided at this time. The situation with respect to possible follow-on operational spacecraft cannot be evaluated at this time. However, the decision with respect to the first five experimental flights should not be construed as representing a prejudgment on any of the possible subsequent flights.

Sincerely,

JOHN A. POULSON,
Major, U.S. Air Force, Chief, Vela Hotel Branch, Nuclear Test Detection Office.

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C., September 14, 1961.

Gen. A. R. LUEDECKE,
General Manager,
U.S. Atomic Energy Commission,
Washington, D.C.

DEAR GENERAL LUEDECKE: During the recent Vela hearings Maj. John A. Poulson of ARPA was asked to evaluate the use of SNAP for Vela Hotel satellites. The attached letter dated September 11, 1961, from Major Poulson to Congressman Holifield reports on the highlights of the evaluation.

We would appreciate receiving the Commission's comments on the attached report. Please supply comments on the alleged shortcomings of SNAP units for this application.

Sincerely yours,

JAMES T. RAMEY, *Executive Director.*

U.S. ATOMIC ENERGY COMMISSION,
Washington, D.C., October 11, 1961.

Mr. JAMES T. RAMEY,
Executive Director, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. RAMEY: In your September 14, 1961, letter you requested comments from the Commission on the report which Maj. John Poulson sent to Chairman Holifield giving reasons why a SNAP radioisotope unit should not be considered for the first five experimental Vela Hotel satellites.

As explained in Major Poulson's letter, members of my staff and technical personnel at the Martin Co. assisted in the preparation of the report and therefore we take no great exceptions with the reasons given for not using a SNAP unit in early Vela Hotel satellites.

Polonium 210 is the only isotope which can be considered available for the experimental flights; the life of the SNAP unit would, therefore, be limited to about 6 months. The 6-month design lifetime referred to in item a on page 2 is, therefore, not considered to be a strong reason for ruling out a SNAP unit. Since the Vela project personnel are of the opinion that a solar cell battery power supply will completely satisfy this requirement, then there is no advantage to be gained in using a 6-month SNAP unit. Should results of the experimental flights indicate that a longer lived power supply is required a year or more, then the use of a Pu^{238} fueled unit would be considered. As you know, the short supply and potentially great demand for this isotope leads us to reserve it for long missions such as Transit.

As explained in item b, any radioisotope source (including Po^{210}) would exceed the radiation background levels specified for the Vela Hotel mission. The Vela project personnel are of the opinion that background data should be obtained from the first several satellites before the artificial background imposed by an isotopic source could be discriminated. Shielding the isotopic source to the levels specified by the Vela engineers for these early missions would make the SNAP unit unacceptable from the standpoint of payload weight as explained in item d. It may be possible that future systems can provide electronic circuitry to discriminate the radiation field of the SNAP units from the radiation environment to be measured.

The system integration problem cited in item c is typical of all satellite applications where the satellite vehicle is designed for solar cells and redesign of the system must be considered to incorporate a nuclear device. Additional design effort would be required on the part of the DOD contractor to integrate the SNAP unit. This extra work is more of a programmatic than a technical problem and should not be done in view of the technical reasons cited in paragraphs b and d.

We feel that Major Poulson's evaluation of the use of a SNAP unit for these early short-life missions is substantially correct and concur in the decision not to use SNAP units at this time. As Major Poulson has indicated, and we concur, this in no way prejudges the case for possible follow-on operational satellites in which the technical requirements will differ from the first experimental shots.

Sincerely yours,

A. R. LUEDECKE, *General Manager.*

Major POULSON. I am sure you understand that this is not only a technical evaluation for sizing the power requirements. There are other aspects involved in utilizing these power sources that have to be considered.

Chairman HOLIFIELD. We just wanted to be assured that you were aware of the development of both the SNAP radioisotope and reactor power sources and the inherent possibilities present, because we have had testimony before this committee which indicated certain deficiencies in the solar cell type of supply.

Major POULSON. Certainly over a period of time the solar cells will deteriorate. Their efficiency does deteriorate with time. However, the initial satellite's lifetime objective, is about 6 months, and solar cells are adequate for that period of time.

I would like to state that Colonel Anderson and I are close friends, and we have a very good relationship as far as passing information back and forth to each other.

Frankly, sir, you have raised a question which we just did not consider in detail, because solar cells were adequate for our job, and we were not looking for any other problems. But it is a point well taken, and we will look at it and report back to the committee.

Chairman HOLIFIELD. That is fine. You can be excused, now. Thank you.

It was our stated intention at the start of these hearings to avoid general policy questions in the disarmament field, as well as philosophical discussions of nuclear weapons testing. This we have tried faithfully to do.

Our witnesses selected on the basis of their scientific and technical competency have been invited to supplement their oral testimony with additional material which they feel will add qualitatively to the record.

In addition to the print of the total transcript of the hearings, we will prepare and publish an objective summary-analysis after the record is thoroughly reviewed.

I would like to express our appreciation to the expert witnesses and seismic panel members for their cooperation, completeness of preparation, and sober presentation.

We also wish to thank Mr. Doyle Northrup of the Air Force Technical Applications Center for his excellent assistance to the committee and the staff in organizing and conducting the hearings.

As I stated in my opening remarks this afternoon, and feeling that I speak for the rest of the committee, it appears from the evidence presented that the possibilities of developing an effective control system are most disappointing in light of the information brought forth before us these last 3 days.

The committee stands adjourned.

(NOTE.—By letter dated Aug. 12, 1961, Mr. James T. Ramey, Executive Director, Joint Committee on Atomic Energy, requested comments on testimony and a statement from Dr. Lloyd Berkner, Graduate Research Center of the Southwest. Mr. Ramey's letter and Dr. Berkner's response will be found in app. 2, p. 301.)

(Whereupon, at 3:30 p.m., Thursday, July 27, 1961, the Joint Committee was adjourned.)

APPENDIXES

APPENDIX 1

GLOSSARY

Accelerometer: An instrument which is so designed that it responds to the accelerations of the earth relative to the suspended mass of the instrument rather than measuring either the velocity or displacement of the earth. This instrument is usually employed to measure the close-in strong motion from an underground nuclear explosion.

Array: A group of seismometers arranged in a systematic pattern over an area of a few square miles for the purpose of increasing the detectability for small earthquake or explosion signals in the presence of background noise.

Backscatter radar: Detection of change in the characteristics of the ionosphere resulting from absorption by it or energy from a nuclear detonation. The change in ionosphere characteristics changes the intensity of the radio frequency waves emitted by the radar transmitter and reflected (scattered) back to the radar receiver.

Clear rarefactions: The first motion recorded by a seismograph as a result of a distant earthquake may be either compression or rarefaction. In the first case, the first motion is caused by a movement of the earth outward from the source and in the second case, by a motion of the earth toward the source. Clear rarefactions are those which are large enough in amplitude above background microseismic noise to permit these rarefactions being identified without question as movements of the earth toward the source of the earthquake.

Cosmic noise: Incoherent electromagnetic noise signals (static) originating in cosmic space and propagated inward through the ionosphere to the surface of the earth where they are recorded on sensitive radio receivers called riometers.

Coupling: In this report, coupling refers to the amount of the total energy released in an underground explosion which is transformed into seismic waves in the earth. The greater the coupling of a particular explosion the larger will be the seismic waves and the easier it will be to detect by seismographs at some distance from the explosion.

Cowboy series: A series of chemical explosions in a salt dome in Louisiana which gave experimental proof of the theory of decoupling.

Decoupling: The process of reducing the size of the seismic signals from an underground explosion by detonation of the explosive in a large hole underground.

Deep focus: A term used by seismologists to indicate that an earthquake originates at some depth greater than 30 miles below the surface of the earth.

DEW line: Distant Early Warning line. System of radar stations established in the most northerly areas of the North American Continent to provide early warning of possible impending attack by aircraft.

Direct optical: Detection of a nuclear explosion by use of instruments (photo multipliers) sensitive to short pulse of visible light emitted by the explosion as a result of incandescence of the matter in the device.

Electromagnetic: (Detection of) radio frequency waves caused by a nuclear explosion and propagated from the point of the explosion to great distances.

Electromagnetic induction detectors: An instrument which radiates a low-frequency electromagnetic field and is sensitive to any distortion in this field due to the presence of conducting material such as iron or copper pipe, wire, landmines, etc.

First motion criteria: Criteria (or tests) by which the nature of the source of a seismic disturbance is established. In principle, explosions will cause the first movement of the earth to be in a direction away from the source. Earthquakes, on the other hand, can cause the first movement of the earth to be in a direction toward the source. Specific criteria are phrased in terms of how much larger than the noise the apparent motion toward the source can be considered to be a reliable indication of an earthquake.

Fluorescence: Detection of a nuclear explosion by use of instruments sensitive to the short pulse of visible light emitted by the gases of the upper atmosphere in fluorescence caused primarily by absorption of energy from X-rays radiated from a nuclear explosion at high altitude or in space.

Gamma ray: Electromagnetic radiation similar to X-rays but generally more penetrating.

Hardtack II: A series of underground nuclear explosions conducted in the Nevada test area in the fall of 1958. The principal underground explosions were Blanca (19 kilotons) and Logan (5 kilotons). The seismic data from these explosions provide the basis of most estimates on the detection capabilities of the control system.

Heat-absorbing material: Materials such as powdered graphite may be distributed throughout the cavity in which a nuclear device has been mounted for the purpose of providing decoupling. Part of the energy of the explosion will be absorbed in vaporizing the powdered graphite and would theoretically reduce the pressure from the explosion on the walls of the cavity.

High frequency seismograph: A seismograph sensitive to seismic waves of from several cycles per second to about 100 cycles per second (principally used in prospecting for oil).

Hydroacoustic: (Detection of) pressure waves caused by an underwater nuclear explosion which are propagated through the ocean waters from the point of the explosion to an underwater microphone usually called a hydrophone.

Intermediate period seismograph: This term usually covers seismographs sensitive to seismic waves of periods between 2 and 10 seconds.

Ionospheric radar: See "Backscatter radar."

Kilometer: 1,000 meters (0.621 statute mile).

Kiloton: Explosive energy release corresponding to 1,000 tons of the chemical high explosive, TNT. The bomb detonated at Hiroshima near the conclusion of World War II was equivalent in size to 20 kilotons of TNT.

Long period seismograph: A seismograph sensitive to seismic waves of natural periods greater than 10 seconds.

Magnetometer: An instrument for measuring the natural magnetic field of the earth to discover any distortion in this field by manmade ferrous articles (i.e., equipment, pipes, etc.) or the permanent magnetism which may exist in manmade instruments or natural ferromagnetic materials.

Magnitude: An arbitrary scale used by seismologists according to size. The scale is so arranged that a magnitude 6 earthquake is 10 times larger than one of magnitude 5; a magnitude 5 is 10 times larger than magnitude 4, etc.

Megaton: Equivalent of 1,000 kilotons or 1 million tons of chemical explosive (TNT).

Microseisms: Very small vibrations in the earth's surface layers caused by ocean waves beating on the shore and by the action of turbulent wind on the earth.

Nuclear debris sampling: Detection of nuclear explosions by collection of fine debris particles from the atmosphere by airborne or ground-air filtering equipment and analysis by radiochemical techniques.

Neutron: One of the elementary particles found in the nucleus of all atoms except the most abundant form of hydrogen. In the chain reaction fission process of a nuclear explosion, two to three neutrons are emitted for each uranium atom fissioned and are available to induce fission of more uranium atoms. These neutrons have high initial energies and are transmitted to great distances where they can be detected with suitable equipment.

Nevada tuff: A weakly cemented, rather crumbly rock, formed from volcanic ash. The underground nuclear explosions of Hardtack II, as well as the Rainier explosion, occurred in this rock, which is somewhat elastic and produces stronger seismic signals from nuclear explosions than are produced by the same size explosions in salt and granite.

Overburden: When a hole is excavated deep underground for the purpose of decoupling a nuclear explosion, the earth or rock between the hole and the surface produces an external pressure on the walls of the hole in proportion to the depth. The amount of earth above the hole, usually measured in feet, is referred to as the "overburden."

Peak signal: The maximum amplitude of motion of the seismograph in response to a nuclear explosion or earthquake.

Piggyback: Experimental equipment for making measurements in space which is added to a missile or satellite originally planned for other types of measurements.

Rainier coupling: The Rainier shot (1.7 kilotons) conducted 900 feet underground in intimate contact with a rocklike material known as Nevada tuff on September 19, 1957, was estimated to have transmitted about 2 percent of its energy into seismic waves. This observed degree of energy transformation from the explosion to seismic waves in the earth (coupling) has been taken as a standard for subsequent calculations of seismic signals generated by underground nuclear explosions.

Reflection shooting: The process of studying the rock strata deep under the surface (20,000 feet) by recording on an array of surface seismographs the seismic waves from small chemical explosions which travel downward and are reflected back to the surface on near vertical paths.

Refraction shooting: The process of studying the detailed character of the strata underlying the earth's surface by firing small charges of chemical explosives and studying the seismic waves from the explosion which proceed downward through strata in question, are refracted (bent) along the strata and refracted again back upward to an array of seismometers where they are detected and recorded.

Seismograph: An instrument for detecting and recording small vibrations in the earth's crust. The detector portion, called a seismometer, usually consists of a mass suspended from a frame either by springs or as a pendulum, in such a manner as to tend to remain in one position while the earth-mounted frame moves with the earth. The relative motion of the frame and mass is usually magnified and recorded as a seismic disturbance either natural or manmade.

Seismometer: The detector portion of a seismograph which may be located in a deep hole or on remote outcrops as in array emplacements and its motion being transmitted as electrical pulses through many miles of cable to the recording instrument.

Shielded detonation: Detonations of nuclear explosives in space can be "shielded" by placing thin spherical or cylindrical sheets of lead or other material around the nuclear device. This shield reduces the amount of radiation from the nuclear explosion and hence reduces the range of detection. Shields are particularly effective for thermal X-rays.

Short period seismograph: A seismograph sensitive to seismic waves of about 2 seconds or less (of considerable significance in detecting nuclear explosions).

Signal-to-noise: In recording the vibrations produced in the earth by a nuclear explosion it is necessary that the size of the signature from the explosion be larger than the microseismic noise on the record to permit the signature to be identified. The ratio of the amplitude of the signal to the amplitude of the noise is called the "signal-to-noise" level.

Strategic nuclear weapons: Those nuclear weapons ranging in yield of approximately 50 kilotons and more, to include the megaton yields. This class is often referred to as "high yield weapons."

Tactical nuclear weapons: Those nuclear weapons ranging in yields below 20 kilotons. This class is often referred to as "low yield weapons."

Tamped shot: An underground explosion detonated in close contact with the surrounding earth or rock so as to transmit to the earth or rock the largest possible fraction of its energy. (Alternative to maximize coupling.)

Technical Working Group I: Of the Conference on the Discontinuance of Nuclear Weapons Tests, the Technical Working Group on the Detection and Identification of High Altitude Nuclear Explosions (June–July 1959).

Technical Working Group II: Of the Conference on the Discontinuance of Nuclear Weapons Tests, the Technical Working Group on the Detection and Identification of Underground Nuclear Explosions (November–December 1959).

Thermal X-ray: Electromagnetic radiation due to the extremely high temperature of the nuclear device fireball. Similar to normal X-rays but much less penetrating.

Threshold: A seismograph of a given sensitivity placed a known distance from an underground explosion or earthquake will record relatively large earthquakes and underground explosions. As the magnitude of the earthquake or equivalent yield of the explosion is reduced it becomes more and more difficult to detect the signature of these events on the seismograph. Eventually one reaches a "threshold" which is defined as the smallest earthquake or underground nuclear explosion which can be clearly detected.

Velocity meters: Strong motion instruments which are so designed as to record the velocity of earth motion relative to the suspended mass of the instruments rather than measure the actual displacement of the earth.

APPENDIX 2

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C., August 12, 1961.

Dr. DAVID INGLIS,
Argonne National Laboratory,
Lemont, Ill.

DEAR DR. INGLIS: In accordance with our discussion, I am sending you enclosed a set of the galley prints of the hearings before the Joint Committee on Atomic Energy on the "Developments in the Field of Detection and Identification of Nuclear Explosions," July 25–27, 1961. For the most part the galley prints contain the corrections to the stenographic transcripts of the hearings by the individual witnesses. They have not, however, been reviewed as yet for accuracy of printing which is required prior to final printing.

I want to thank you for your willingness to participate in the planned round-table discussion on the "Feasibility of a Test Ban Control System" during the hearings. I am sorry, however, that the unavailability of most of the other invited participants prevented the panel discussion from being held. I believe it would have been a very worthwhile addition to the hearings.

I would appreciate any comments you may wish to make particularly after you have had an opportunity to review the testimony. We have requested ARPA to submit comments concerning some of the newer concepts including the antineutrino and the electromagnetic methods.

Sincerely yours,

JAMES T. RAMEY, *Executive Director.*

ARGONNE NATIONAL LABORATORY,
Argonne, Ill., August 7, 1961.

Congressman CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
U.S. Capitol Building, Washington, D.C.

DEAR CONGRESSMAN HOLIFIELD: I am enclosing my comments on the hearings and trust they may be included in the record of the hearings in keeping with conversations I had with Messrs. Ramey and Conway.

I hope one result of the hearings may be to arouse interest in getting the Disarmament Agency bill passed this summer. Is there any way you can speed this up so as to avoid the logjam at the very end?

With best wishes,

Sincerely yours,

DAVID R. INGLIS.

STATEMENT OF DAVID R. INGLIS

After having had the privilege of attending (by invitation) the first 2 days of the hearings, I am submitting for the record these remarks on certain technical aspects of the testimony and on questions of emphasis.

In his interesting kickoff statement on July 25, Dr. Latter mentioned as one of the difficulties the fact that uncertainty of seismic travel times in various media makes for great uncertainty of location of a seismic event (so that the area to be inspected might be a 10- or 15-mile square). He surmised that a sufficiently detailed study of travel times in Soviet territory to improve this situation would be politically unacceptable. He may have been thinking of a complete mapping in advance in several directions around each control station.

Here and elsewhere in the hearings there has been insufficient consideration of the very important possibility of a postevent comparison shot, the political acceptability of which has not been explored. (The possibility was mentioned briefly and inaudibly in the July 26 panel discussion.) The idea here is that, after a suspicious signal has been recorded, and the event has been chosen for onsite inspection, a fairly large chemical explosion should be detonated at the estimated epicenter and its signals compared with those of the original event. In the first place, this establishes travel times over approximately the same paths and makes it possible to locate the original event considerably more accurately on the basis of the original data. Secondly, and perhaps even more im-

portantly, it is reasonable to expect that study of such comparisons will show characteristic differences between the complete seismic records from quakes and explosions, when made in this way almost identical travel paths. Thus it could provide identification in addition to more accurate location.

If the Soviets' main reason for reticence concerning inspection arises from a legitimate desire to preserve their advantage of superior military secrecy (and it is difficult to assess to what extent this is their reason), they could reasonably assent to such a procedure, for it reduces the need for roving inspectors. It both reduces the area to be explored and in some cases could eliminate the necessity for area exploration. In exchange, it would be necessary to permit some activity at, and transportation to, a given spot.

The Groundhog series as outlined in these hearings provides a valuable opportunity for improving knowledge of detection techniques solely on the basis of chemical explosions. One of its most valuable features is that it should provide and indication of the gains in accuracy of location arising from knowledge of local travel times. It is highly desirable that, in the Groundhog series, special emphasis be put on the comparison of quake and explosion signals over similar paths with a view to improving identification. There is so much to be learned from vigorously pursuing the well-conceived Groundhog program and similar nonnuclear investigations that it seems premature to insist on nuclear shots as long as there remains any political disadvantage to initiating them. In considering whether to initiate nuclear shots for detection improvement, we must remember that the purpose of improving detection (and identification) is to obtain a viable test-ban agreement and its ensuing advantages. If we don't succeed in getting a controlled test ban, improvement of control techniques is useless. It is not the sort of knowledge we can store up for use in the distant future, for time is fast running out on the utility of a test ban. It will have sharply reduced value when many nations will have achieved nuclear status. If by initiating a nuclear test-detection program we should eliminate the possibility that the pendulum of negotiation may swing to a more favorable phase in the next couple of years, we would have removed much of the reason for improving detection.

If it should turn out to be more feasible, politcally, to arrange for two nuclear shots than for the 13 outlined in the hearings, perhaps because the Soviets might agree to cooperate by inspecting a small number of test devices but not a larger number, then it should be appreciated that such a program yields diminishing returns after the first two shots. If two nuclear shots were available, they would yield the most useful information of paired with matching chemical shots, one nuclear and chemical pair tamped, one decoupled. The most relevant size would be the largest for which it is practical to provide the matching chemical explosive and the decoupling cavity, which would still probably be somewhat smaller than where the capabilities of the Geneva network cut off. If it is a sound political judgment that our initiation of even one or two nuclear shots would promote escalation of weapons testing rather than test-ban negotiation, then it would seem wiser to learn as much as possible from the chemical explosions and to continue to try, through some modifications of our negotiation position that are still reasonable, to seek a test ban.

In his first statement, Dr. Bates reported an experiment showing that a shot very near a quarry wall generates a first-motion rarifaction in the direction exactly at right angles to the wall. This was interpreted in the discussion as undermining the possibility of identifying explosions by the "Geneva system," which assumes a first-motion compression in all directions from an explosion, as distinguished from an earthquake. This experiment involved extremely small explosive charges. It was not sufficiently appreciated in the hearings that, if the same experiment were scaled up and done with a nuclear explosion of several kilotons, it would mean blowing out the face of an enormous cliff and releasing radioactivity into the atmosphere.

In the course of calculating the largest test which, by use of the "big hole" technique of concealment, could theoretically be made without being detected by the "Geneva network," Dr. Latter postulated the construction of a deep underground hole 3,000 feet in diameter. Of this he said simply that he "does not know that it cannot be made." This seems to have been taken as grounds for assuming that it would be made by a determined violator and was apparently part of the grounds for some of the pessimistic conclusions drawn in the hearings concerning the efficacy of test-ban control.

The difficulty of constructing an underground hole is not simply proportional to the volume of the hole, but increases somewhat more rapidly than the volume when questions such as roof support come into the picture. Considering, however, only the volume, we may note that the volume of a 3,000-foot hole is 738 times as great as the volume of a 332-foot hole. When faced with the actual job of constructing underground cavities, General Betts had this to say: "Excavating the cavities for the decoupled shots represents an extensive construction project. In the case of the fully decoupled event in phase I (small yield shots), a spherical cavity about 92 feet in diameter is required while the larger decoupled event of phase II requires a spherical cavity about 332 feet in diameter. (About the length of a football field.) Manmade construction of unsupported cavities of this size and at this depth has never before been attempted in the United States. In all, about 750,000 cubic yards of rock salt, about 1¼ million tons, must be removed. Both conventional mining and solution mining techniques are being considered. Currently, studies relative to the salt-disposal problem are being made * * *." The entire construction, but especially the material-disposal problem, would be more difficult under conditions of complete secrecy, even in a country as nearly closed as is the U.S.S.R.

There is a big difference between having an actual job to do and imagining its being done by others. The factor 738 just mentioned is a very large factor. If General Betts, with the job at hand, finds the construction of a 332-foot hole unprecedented and somewhat formidable, then to imagine the construction of a hole 738 times as big is to lose all sense of proportion. This seems to me to be an example of a tendency, which is too prevalent in this year's and last year's hearings and elsewhere, to expect political decisions to be made only on the basis of perfect assurance from the technical side, in spite of the fact that there is no perfect assurance in any aspect of national security. It is quite proper to distinguish between technical judgments and political judgments in questions of test-ban negotiation, as was done for example in Dr. Latter's remarks. However, they cannot be kept in separate boxes. A political judgment should be based on various technical judgments considered with a proper sense of proportion. Technical judgments of prospects of evasion should be concerned with the plausibility and difficulty of concealment measures, not merely whether they are not demonstarably impossible. The likelihood of concealment measures being discovered should be judged in the light of all sources of information which will be available after a detection system is installed, and the incentives for evasion should be judged in terms of plausible future weapons developments, not on wild hopes or fears of fantastic weapons. As has been said frequently elsewhere, it should be sufficient for a favorable decision on a test ban that the control system makes evasion either too difficult or too risky relative to the incentives provided by the prospect of further weapons development.

In commenting on the control system after Dr. Latter's statement, Congressman Bates remarked that he did not understand why we should bother to lock some doors while leaving others wide open. To leave the technical possibility of a fairly large partially decoupled test after difficult preparations which might themselves be detected and under conditions to limit what may be learned from the test is not to leave that door wide open. To be viewed in perspective, this technical possibility must be coupled with the fact that the U.S.S.R. has already conducted many tests that large, and much larger, under ideal observation conditions in the atmosphere, and with the fact that a test ban, even with this technical possibility, would provide important leverage on the many-nuclear-nation problem.

Some of the pessimism expressed in the hearings arose from the impression that little progress in control techniques has been made since last year's hearings. Official Washington should be well enough acquainted with the usual slow pace of operations as not to expect research results as soon as projects are funded. Much of the year was spent in the planning stage and in letting contracts. The actual work, now well started, should show much more significant results in the next year than in the last. If any moral is to be drawn from the slow progress, it is that arms-control negotiations require imaginative planning by a central responsible agency far in advance of apparent need, that we have already been much too long without a Disarmament Agency such as has been proposed by the President, and that it is urgent that the enabling legislation should be brought out of committee and enacted in this session of Congress.

THE RAND CORP.,
Santa Monica, Calif., August 22, 1961.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
House of Representatives, Washington, D.C.

DEAR MR. HOLIFIELD: I wish to thank you for the opportunity to comment on the statement by Dr. David R. Inglis. I shall comment only on those remarks of Dr. Inglis which relate directly to my testimony at the hearings on detection.

First, Dr. Inglis pointed out the potential value to onsite inspection of using large chemical explosions to improve epicentral location. I agree completely that this method should be explored. In fact, several months ago the Project Vela Ad Hoc Group on Detection made a recommendation to this effect; and as a result, study of this method is one of the objectives of the groundhog series of chemical explosions.

In my testimony, this method was one of those I had in mind when I said, "Other methods have been proposed for reducing this area (of uncertainty in epicentral location), but much study is needed before it can be decided whether they have any promise."

The difficulties with the method arise from the size of the explosions which would have to be used. Since the Geneva system can only locate tamped explosions of the Rainier type with yields greater than ½ to 1 kiloton, chemical explosions of at least this size would be required. Since at these low yields only three stations detect and since the detected signals are only slightly above the expected noise backgrounds, larger explosions—perhaps a few kilotons—would be required for accurate travel-time determination. Moreover, it is not yet clear that only one explosion would be adequate to provide the needed location accuracy.

Undertaking chemical explosions of perhaps a few kilotons requires the transport of great quantities of explosives and would probably require several months of preparation. Even if such an undertaking were to be acceptable to the Soviets, such a long preparation time would probably render this technique useless for onsite inspection which re quires rapid access to the entire site of the suspected event to be effective.

Second, Dr. Inglis expresses the belief that construction of large underground cavities is difficult and cites statements of General Betts to support this point. However, General Betts referred only to the cavities which are to be constructed for the Vela program. These cavities are required to meet rigid requirements of sphericity in order to satisfy the scientific needs of the research program. For concealment, such strict requirements are unnecessary. In fact, the cavity need not be sperical at all. A cavity will fully decouple even when its minimum dimension is as small as about one-fourth of its maximum dimension. The problem of constructing concealment cavities should be viewed, therefore, in the light of this considerably less stringent requirement.

While I agree with Dr. Inglis that the ultimate decision on the test ban issue should take full account of political considerations, I personally cannot weigh the difficulties of concealment against Soviet incentives for weapons development. I do not know the Soviet military needs nor the Soviet incentives to fulfill their needs.

Finally, with regard to Dr. Inglis' remarks that the Vela program is still in its early stages, I would like to refer you to my statement on this matter in my letter commenting on Ambassador Dean's letter.

Sincerely,

RICHARD LATTER.

DEPARTMENT OF STATE,
Washington, D.C., August 17, 1961.

Dr. RICHARD LATTER,
Director of Physics, Rand Corp.,
Santa Monica, Calif.

DEAR DR. LATTER: I enclose a copy of my letter to Chet Holifield, chairman of the Joint Committee on Atomic Energy, in which I make reference to your testimony and your letter of June 17, 1960, to Doyle Northrup.

If you decide to write to the committee, I would appreciate a copy of your reply and would be glad to get any other comments or suggestions you would care to make.

With kind regards, I am,

Sincerely yours,

ARTHUR H. DEAN,
Chairman, U.S. Delegation, Nuclear Test Ban Talks.

DEPARTMENT OF STATE,
Washington, D.C., August 17, 1961.

HON. CHET HOLIFIELD,
House of Representatives,
Washington, D.C.

DEAR MR. HOLIFIELD: I have been reading the very interesting testimony in the hearings before your committee on July 25–27 concerning developments in the field of detection and identification of nuclear explosions. As the result of testimony by Dr. Richard Latter and others, some may feel that those who are responsible for conducting nuclear test detection research activities are less optimistic at the present time as to the possibilities for improving detection capabilities than was the case during the last committee hearings on this subject in April 1960.

Dr. Latter's recent statement should, of course, be reviewed in the light of the Rand study, in which I believe he participated (RN–2568–AFT, "Capabilities of a Seismic System," May 6, 1960, confidential) during early 1960, as well as his testimony before the committee in April 1960. The study proposes various modifications to the Geneva control system which, it was estimated, would substantially restore the capabilities of that system to those estimated by the experts in 1958.

The principal proposals offered by this Rand study for improving system capabilities were:

(*a*) Addition of 9 to 13 control posts in the Soviet Union, and

(*b*) Relocation of the 21 controls posts of the Geneva system to more densely cluster within three regions of high seismicity in the U.S.S.R.

The Rand authors, of course, were not unaware of the difficulties of negotiating additional control posts and the possible effects of relocating posts on overall system capabilities to detect events in the atmosphere and in outer space. In short, the Rand study considered only the seismic portion of the overall system. Within this context, improved capabilities are postulated.

The views as reflected in this Rand study are, I venture to say, not more optimistic than those expressed in Dr. Latter's testimony during April 1960 which was, in large measure, based upon the study. In contrast, Dr. Latter's remarks on August 25, 1961, are entirely limited to the control system as specified in the present draft treaty and attending circumstances, some of which may change.

For example, both the earlier Rand study and testimony, as well as testimony during the recent hearings, emphasized the deficiency of seismic and space environmental data which underlies a good deal of the difficulty with which we are presently confronted. Of course, even if the necessary data becomes available, sufficient improvements of control capabilities may continue to elude us. As you are well aware, one cannot prognosticate the outcome of a very extensive and complex scientific research endeavor. Since quantitative forecasts are difficult, the fact that potential improvements are in the offing often tends to become obscured in the light of rather definitely described difficulties based on existing, imperfect knowledge. Notwithstanding, I feel it is well to remember that the Vela program is just now attaining a satisfactory momentum and therefore could not have been expected to have provided clues for improving the Geneva system to the extent required in order to overcome such fundamental difficulties as decoupling in an underground environment and shielding outer space nuclear tests.

On balance, I regard Dr. Latter's statement and those of other expert witnesses recently appearing before the committee as proper evaluations of the problems which the Vela program presently faces and which must be solved in order to achieve the required improvement of the Geneva control system. I do not know whether Dr. Latter's views have significantly changed on this matter during the last year. In this connection, I am attaching a letter dated June 17, 1960, from Dr. Latter to Mr. Doyle Northrup which bears upon a similar question raised last year, which you may wish to incorporate into the record, after giving Dr. Latter an opportunity to bring his views up to date. I am sending a copy of this letter and its enclosure to Dr. Latter and am asking that he write your committee directly in order that there will be no question about his present views.

As you know, I am anxious to get the information on this subject as technically precise as we can in an effort to make the proposed treaty both workable and effective.

It may be that we could negotiate a greater number of control stations in the U.S.S.R. or possibly some unmanned stations in an effort to improve the control system's detection capabilities, to decrease the necessity for onsite inspections and possibly to lower the treaty threshold. In this connection, Dr. Latter stated (p. 34 of the transcript for July 25, 1961) that major improvements of the Geneva system were possible but would require major changes and considerably greater access to the Soviet Union than contemplated in the current political negotiations. He notes, "The real problem is to make improvements in the Geneva system which do not require major political reorientation. This is by no means an easy job." He is quite right about the nature of the job and I can assure you I have no illusions on that score. Even if we fail in the attempt to get greater access to the Soviet Union, the effort may still be worthwhile in terms of political gains.

Sincerely yours,

ARTHUR H. DEAN,
Chairman, U.S. Delegation, Nuclear Test Ban Talks.

THE RAND CORP.,
Santa Monica, Calif., August 22, 1961.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
House of Representatives, Washington, D.C.

DEAR MR. HOLIFIELD: Ambassador Dean sent me a copy of his letter of August 17, 1961, in which he discussed my testimony at the recent hearings of your committee. I have a few comments on Ambassador Dean's letter.

Ambassador Dean suggests that my views on detection may have become less optimistic since my testimony of a year and a half ago (April 1960). At that time I described a study which we had made at the Rand Corp. indicating that it is possible to modify the Geneva system so as to improve its capability to identify earthquakes by relocation and addition of a small number of control stations within the U.S.S.R. I also considered that such a modification of the Geneva system might be politically acceptable to the Soviet Union.

Ambassador Dean interpreted my testimony on this subject as implying optimism on my part. However, I would like to point out that in the same testimony of April 1960 I made it perfectly clear that I do not regard the identification of earthquakes as the main problem for detecting underground nuclear explosions. I stated then that "* * * we know that the seismic signals from an underground nuclear explosion can, by deliberate effort, be reduced by a factor of 300 or more. With the Geneva system or any plausible modification of that system, even very large explosions might therefore go undetected. Against these explosions the control system has no capability whatsoever and cannot provide a technical safeguard." Improving identification of earthquakes is of value only for controlling explosions which are not decoupled.

It is now a year and a half later and there has been no progress toward the solution of the decoupling problem. It is true, as Ambassador Dean says, that the Vela program is only just getting fully underway. However, it must be remembered that the scientific community as a whole has been aware of the problem of decoupled explosions for about 2 years. It is becoming increasingly clear that the problem of decoupling is not one that can be solved easily. What we are asking for is a breakthrough of a kind which cannot be ordered but, as in the history of science, may be expected to come about, if at all, accidentally or as a byproduct of research possibly even into unrelated areas.

Sincerely yours,

RICHARD LATTER.

LOS ALAMOS, N. MEX., *August 8, 1961.*

Subject: Hearings on Project Vela, July 25–27, 1961.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
House of Representatives, Washington, D.C.

DEAR MR. HOLIFIELD: As active participants in two phases of the Vela Sierra program, we are respectfully submitting herewith for the record comments on several aspects of the subject matter which do not seem to have been adequately covered in the recent hearings but which are pertinent to the overall evaluation

of the usefulness of the detection systems. These comments are based on excess to several original prepared statements. We are confining our remarks to problems of outer space detection, although they may have wider implications.

First, we feel that while many details of specific detection problems were discussed, only scanty attention has been paid to the deterrent effect of an operational system on the actions of a potential violator. This aspect, we believe, deserves consideration in any realistic assessment of the overall effectiveness of any detection system. True, we are uncertain about possible Russian clandestine tests and also about Russian intentions and exact capabilities for concealment. However, a careful analysis of the problems involved shows that diagnostic space tests will be much harder to conduct than tests in the atmosphere or small and medium sized tamped underground tests. The difficulties increase significanctly if the tests have to be concealed either by going to large distances or by shielding or, say, by exploding near the far side of the moon. Thus even a limited detection capability can serve an additional important purpose, namely to make clandestine testing technically very difficult, extremely wasteful in terms of scientific manpower, and economically expensive.

As an example, reference is made to the deterrent potential of the all-sky fluorescent detection system of Vela Sierra. As described by Dr. W. Ogle, prototypes of the instrumentation have been built and operated. While the effective range of the system—which has been developed to provide an interim space detection capability—is not very large compared with the ultimate satellite system, its operation would drive a potential violator behind the moon or to distances of the order of 10^6 to 10^7 miles for megaton-size explosions or would force him to resort to shielding. In the case of a test behind the moon and probably for other tests diagnostic information must be recorded in the vicinity of the explosion and stored for later telemetry to earth. Although this method of data acquisition is feasible it is technically difficult and at very large distances operationally unreliable. Poor transmission or lack of transmission would either severely limit the value of such a test or make it almost useless.

Second, referring to that part of Dr. R. Latter's testimony in which he discusses the space test capability of the Geneva system, we cannot share his pessimism so far as this area is concerned. While we agree with Dr. Latter' detection ranges for the Geneva system and for unshielded explosions we must take exception to the following statement: "Since natural events *may* produce signals similar to space explosions, space nuclear explosions *cannot* be identified." [Italic ours.] This negative conclusion is based on an uncertainty. The fact is that while one or the other natural signal may have characteristics similar to bomb signals, there exists no scientific basis for an assumption that natural phenomena produce the unique combination of signals which are typical for a nuclear explosion (emission of neutrons, gamma rays, X-rays, light, etc.). The Geneva system as envisioned by the technical working group I should provide in the early Vela Hotel implementation detection coverage for unshielded megaton detonations by means of X-rays, gamma rays and neutrons at distances of better than 10^6 miles; for X-rays and gamma rays alone the range approaches 10^7 miles. Vela Sierra records among others a signal produced by visible bomb light. Thus the Geneva system has the capability for identification of explosions by means of the analysis of several such signals. Therefore the situation is not as bad as it appears in this testimony.

Further background studies are underway. Significant parts of the current Vela Sierra effort and the Vela Hotel activities are directed toward a better understanding of natural phenomena. This may be expected to lead to new results and all the scientists with whom we are acquainted who are actually working on developing or improving space detection techniques feel confident that further advances will be made in this realm.

Respectfully yours,

HERMAN W. HOERLIN.
DONALD R. WESTERVELT.

Encl: Biographical notes.

BIOGRAPHICAL NOTES (FURNISHED BY DRS. HOERLIN AND WESTERVELT)

Herman W. Hoerlin, Ph.D., is a Los Alamos Scientific Laboratory staff member. As group leader and program director, he participated in the following test operations: Upshot Knothole, 1953; Castle, 1954; Teapot, 1955; Redwing, 1956; Plumbbob, 1957; Hardtack, 1958. He was in charge of the Los Alamos scientific programs for the Johnston Island high altitude tests of the Hardtack operation in 1958.

Donald R. Westervelt, also a staff member of the Los Alamos Scientific Laboratory, participated as experimenter or project officer in operations: Teapot, Redwing, Plumbbob, Hardtack. He is alternate group leader in the test division and directed the Vela Sierra field tests in Alaska and Greenland last winter.

D. R. Westervelt and H. W. Hoerlin jointly with the late Dr. W. Urry are the originators of the upper air fluorescence method for detection of nuclear explosions in space.

THE RAND CORP.,
Santa Monica, August 22, 1961.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
House of Representatives, Washington, D.C.

DEAR MR. HOLIFIELD: Drs. Herman Hoerlin and Donald Westervelt sent me a copy of their letter of August 8, 1961, commenting on the recent hearings on detection. I find myself in considerable disagreement with many of their statements and conclusions. I would like to explain the basis of the disagreement in some detail.

Drs. Hoerlin and Westervelt take exception to my statement: "Since natural events may produce signals similar to space explosions, space nuclear explosions cannot be identified." They say that "there exists no scientific basis for an assumption that natural phenomena produce the unique combination of signals which are typical of a nuclear explosion." I disagree with this statement of Drs. Hoerlin and Westervelt, and I will explain why shortly. However, even if their statement were correct, it is quite irrelevant to my contention that space nuclear explosions cannot be identified.

In order to identify space nuclear explosions we would have to be certain that no natural phenomena exist which could produce the unique combination of signals which are typical of a nuclear explosion. Obviously, there is no scientific basis for certainty in this matter. Therefore, space nuclear explosions cannot be identified, and Drs. Hoerlin and Westervelt are wrong when they state, "thus the Geneva system has the capability for identification of (space) explosions by means of the analysis of several such signals."

I would like to explain now why there does exist "scientific basis for an assumption that natural phenomena produce the unique combination of signals which are typical of a nuclear explosion." First, it is known that natural solar events usually produce simultaneously direct X-ray, gamma rays, cosmic-noise absorption, fluorescence, and probably also visible light. Moreover, cosmic rays produce in detection satellites large numbers of particles which presently considered detectors could misidentify as neutrons, gamma rays, and X-rays from explosions. Since these signals are the only reliable basis for detecting distant nuclear explosions, a natural solar event combined with a cosmic-ray event could produce all the attributes of a nuclear explosion which are detectable by presently considered detection systems. Consequently, again Drs. Hoerlin and Westervelt are wrong when they state that "there exists no scientific basis for an assumption that natural phenomena produce the unique combination of signals which are typical of a nuclear explosion."

Future research will of course lead to increased knowledge of natural backgrounds and may lead to improved instrumentation capable of discerning finer features of the detected signals, which would result in better discrimination between explosions and natural events. The correct description of the detection problem is, therefore, as I stated in my testimony—"Furthermore, the satellite instrumentation must be designed to minimize the likelihood of a false signal arising from background noise. It is not possible to eliminate false signals entirely. But it is expected that by careful design the frequency of such signals can be made extremely small. Thus, while the occurrence of a signal would not be sufficient to identify its origin, it may be sufficient to establish a strong presumption."

I would like now to take the opportunity to comment on the other remarks made by Drs. Hoerlin and Westervelt. They state that "the deterrent effect of an operational system on the actions of a potential violator * * * deserves consideration in any realistic assessment of the overall effectiveness of any detection system." I agree that the deterrent value of the system should be considered. But what is the deterrent value? It is generally agreed by the U.S. scientific experts that the Soviets have the capability of carrying out shielded megaton explosions at a distance of a few million to a few tens of millions of

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miles. It is also agreed that under these conditions such tests cannot be detected by the Geneva system. One might argue that there is always a small probability that the tests would malfunction and that in spite of all the precautions that naturally would be taken the signal would be detected—and thus there is some deterrent value to the system. This deterrent value is extremely small, however, when considered in the light of my previous argument that space nuclear explosions cannot be identified.

In talking about the deterrent value of the Geneva system, we are, therefore, talking only about whether the Soviets have the incentive to develop new weapons in space. I have no way of knowing whether the Soviets do or do not have the incentive. Drs. Hoerlin and Westervelt cannot know either.

Drs. Hoerlin and Westervelt say space testing will be very difficult, wasteful of scientific manpower, and expensive. The fact is that space testing is indeed a large undertaking but no more so than many past and current United States and Soviet space projects. Both the United States and the Soviet Union have indicated in their space programs a willingness to undertake difficult and costly projects which have considerably less military importance than nuclear weapons developments. The incentive to overcome difficulty, wastefulness, and cost, depends upon the gains to be made. Only the Soviets know their needs and the importance of the gains to them.

There are a few additional technical points I would like to make. Drs. Hoerlin and Westervelt state that the "difficulties" of space testing "increase significantly if the tests have to be concealed either by going to large distances or by shielding or, say, by exploding near the far side of the moon," and "In the case of a test behind the moon and probably for other tests, diagnostic information must be recorded in the vicinity of the explosion and stored for later telemetry to earth. Although this method of data acquisition is feasible it is technically difficult and at very large distances operationally unreliable." Unfortunately, such bare statements do not put in proper perspective the "difficulties" of space testing.

First, it costs no more to send a rocket 100 million miles than it does to send it 100,000 miles. Only additional time is required. Telemetry of information becomes more difficult, but the required telemetry is considered well within current United States and Soviet Union capabilities. Moreover, the proposed Geneva detection system has even more difficult problems of telemetry than are expected for space testing.

Second, shielding of space tests against detection is not difficult. The required shield is simply a balloon which must be inflated before the test. It is a much smaller balloon than the one carried on the U.S. Echo satellite. The only problem with shielding is the reliability with which the shield can be inflated. However, this operation is no more complicated than many of the operations currently routine in the U.S. space program. It is far less complicated than many of the operations which probably will be required to place detection satellites in orbit. In fact, it is the opinion of many scientists involved in the development of the Geneva system that the detection satellites will require shields similar to those required for concealment in order to avoid adverse effects of the sun's radiations on detectors. These detection shields would be further complicated since they would require constant orientation toward the sun during the entire lifetime of the detection satellite.

Finally, storing information in a satellite for later telemetering is a routine operation on almost all U.S. satellites—it is not difficult. Moreover, the type of operation required for testing behind the moon has already been undertaken by the Soviets. They launched a satellite which traveled behind the moon, and while the satellite was on the far side, out of contact with the earth, it took television pictures of the moon's surface, stored these pictures, and, after emerging from behind the moon, telemetered them to earth.

One final point—I would like to say that I agree with the scientists referred to by Drs. Hoerlin and Westervelt who "feel confident that further advances will be made in this realm." However, the results of research into the unknown cannot be predicted. Consequently, "further advances" may make detection easier—or they may make detection even more difficult than it appears now.

Sincerely yours,

RICHARD LATTER.

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C., August 12, 1961.

DR. LLOYD BERKNER,
Graduate Research Center of the Southwest,
Dallas, Tex.

DEAR DR. BERKNER: In accordance with the request of your representative, Mr. Ralph N. Stohl, I am sending you enclosed a set of galley prints of the hearings before the Joint Committee on Atomic Energy on the Developments in the Field of Detection and Identification of Nuclear Explosives, July 25–27, 1961. For the most part the galley prints contain the corrections to the stenographic transcripts of the hearings by the individual witnesses. They have not, however, been reviewed as yet for accuracy of printing which is required prior to final printing.

I am sorry that you were not able to be present to participate in the hearings. I am pleased, however, that you are recovering from your illness. The chairman will be pleased to receive your statement for the record.

Sincerely yours,

JAMES T. RAMEY, *Executive Director.*

GRADUATE RESEARCH CENTER OF THE SOUTHWEST,
Dallas, Tex., September 6, 1961.

HON. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
U.S. House of Representatives,
Washington, D.C.

MY DEAR MR. HOLIFIELD: I deeply regret that my recent illness and hospitalization have prevented my prompt response to your invitation to attend the hearings and to testify before the Joint Committee on Atomic Energy relevant to the detection of nuclear tests. May I express my appreciation for the consideration of your committee in the action of Mr. James T. Ramey who has supplied me with a transcript of the hearings. These I have studied with great care.

Now that the Soviet Union has announced resumption of nuclear testing and the decks are cleared of any sense of commitment to previous proposals, a critical evaluation of our position in this matter seems most appropriate.

Irrespective of the political climate or potentialities at any time, certain principles relating to our national capabilities for nuclear-test detection seem invariant. Obviously we must live with the "atom" from now forever onward. Even if or when a satisfactory rapprochment might be attained with the Soviet Union or any other "atom" nation, there will arise other aggressors who would acquire the power of the atom to force their will on the world. Consequently, our national interest, and indeed the interest of all free peoples, requires that we be informed as completely as possible of any activity anywhere in the world involving the development and test of nuclear weapons. Moreover, at any time our country should have precise knowledge of the cost and geographic requirements of a detection system of defined capability. Therefore development of the best possible means of detection should be pressed unflaggingly.

I emphasize the foregoing as invariant strategic principles that should guide our basic policy toward effort on nuclear detection.

If these strategic requirements are met, then we will be prepared for a wide variety of tactical situations. Among these might be (*a*) a detection system which might provide certain advantages in the absence of any agreement or availability of any detection stations in certain geographic areas; (*b*) a sound and quantitative basis for future negotiations of a wide variety of types with a secure knowledge of geographic needs and costs; or (*c*) an ability to take prompt advantage of proposals for nuclear agreements with a sense of confidence of the real meaning and protection that any such proposals would implicitly entail.

Before commenting in detail on present U.S. position, there is one aspect of that position that deserves especial attention. This concerns the need for competent and concurrent evaluation of our position relevant to nuclear detection at any time. This point was discussed at some length by Mr. Beyer and others in the testimony before your committee and constitutes the most serious and damaging immediate defect in our national program toward detection of underground tests.

Competent state-of-the-art evaluation involves four aspects. First is the detection capability at any specified time of a definable network equipped with technologically available devices and organized in a specified way at a reasonable estimated cost. Second is the continuous central monitoring of research progress so that the romise of scientific developments growing out of widely scattered and independent researches can be recognized immediately and pushed quickly to their full potential. Third is the need for continued study and test of various combinations of alternatives in the early stage of their technical development so that their relative merits may be quantitatively appraised and their weaknesses may provide indicators for the guidance of further research effort. Fourth is supervision of those useful ideas emergent from scientific research into effective and tested technology which is ready for employment in any desired system.

Closely related to these four functons is the acquisition of a technological dexterity with respect to new developments and evolving systems during the research and development interval, so that new ideas can be employed rapidly and effectively when occasion demands.

In my opinion (as is borne out by Mr. Beyer in his testimony) these evaluations are so complex that their fulfillment requires very special measures. These evaluations are simply and absolutely beyond the capabilities of any group of experts or consultants sitting in Washington without well planned and organized mechanisms in the field to aid them.

Success in previous efforts of this kind demonstrates quite clearly the kind of organizational mechanism required to assure continued evaluation and specification of a developing system as it evolves out of combinations of a wide variety of speculative scientific endeavors. For the atomic bomb, the Los Alamos Scientific Laboratory was designed to perform this function. For radar, the MIT Radiation Laboratory was organized to bring a combination of difficult physical principles to technological fruition. For the ICBM, a succession of enterprises, culminating in the nonprofit Aero-Space Corp., has served the purpose. For the anti-ICBM, a unit of the Bell Telephone Laboratories was required. There are several others. While these represent a wide spectrum of organizational devices, certain fixed patterns are recognizable as common to all.

(1) They have been clearly charged with the four interrelated aspects of continuous evaluation that I have specified above.

(2) They have direct operational control of laboratories and personnel sufficient to examine, test, and evaluate every element of the research and technological program and to acquire a measure of technological dexterity.

(3) They are organized to aid the Government in prompt review of any proposals or combination of proposals under Government consideration.

The same general pattern of management seems essential to achieve significant technological progress on the problem of detection of nuclear testing. The size and scale of management responsibility for research and development need not, of course, be as large as in the examples I have cited. Moreover, in the examples cited, the basic research program has not been centralized completely under the management laboratory, nor should it be in the management of the research and development program for nuclear detection. Significant research requires that it be decentralized to universities, agencies, and groups best qualified by their skills, desires, and facilities to do such research. But the management organization must have ready access to such independent groups through proper arrangement, and through the necessary skills among its own scientific personnel, to appreciate fully the implications of all areas of relevant research progress.

Moreover, the management and evaluation group must have personnel, facilities, and authority to carry on such research and development under its direct supervision as necessary to further its responsibilities under the program to the extent required for successful progress and for acquisition of technological dexterity.

In my opinion, the usefulness of the program or the design of reasonably satisfactory systems for detection of nuclear tests cannot mature until measures for evaluation and management such as those recommended by Mr. Beyer are effectuated.

Further, I believe that the group set up to manage the evaluation program should not at this time be the same group as that selected to operate a full-scale detection network. Certainly the research and development evaluation management group must operate test detection stations and test network capabilities, but these are stations that must constantly evolve with introduction of improved

measures. This function of evaluation should be independent of the operations of any network utilized for actual detection activities.

May I turn then to the probability of success of a feasible operating network. In doing so, I will take the liberty of reviewing and summarizing certain widely discussed aspects of the problem in order to acquire perspective on our present activities.

To detect explosions in the earth will require some sort of grid-network of mutually supporting and connected stations whose apparatus is designed to observe phenomena within the earth, and is, therefore, somehow coupled to it. The geographic spacing of the component stations of the network is, of course, dependent upon the efficacy of any particular station in acquiring certain unique and essential information, and of the network of such stations as a whole in combining relevant information and in evaluating it.

At the moment, of the various physical effects which could be conceivably adapted for detection of underground nuclear tests, surveillance of seismic waves in the earth seems to offer the most reasonable immediate hope. Utility of other physical effects is certainly conceivable, but no technology to exploit these effects effectively has yet been devised. Even if certain of these effects emerge eventually as useful tools for detection, the auxiliary information from seismic information will, most probably, remain of great importance for confirmation and localization of events.

The efficacy of seismic detection at a particular time depends on a wide variety of physical factors, the more important of which I have listed in appendix A to this letter. Fundamentally, the energy of an underground explosion will set up complex elastic waves from earthquakes of varying intensity and occurrence in any locality. Detection is further masked by random waves produced from a variety of origins, such as the impact on the earth of wind, waves, and manmade shocks, all of which are usually categorized as "noise."

The detection capability depends on the ability of the apparatus at each station, and of the combined network, to differentiate between seismic waves originating in nuclear explosions and those generated by other effects in the earth. This differentiation must arise from the unique differences in the character of each nuclear explosion from other seismic events which cannot be circumvented in the nuclear test procedure. Consequently, the technology of detection is based on a minute scientific understanding of each aspect of the initiation, propagation, and detection of seismic waves of different origins so that all differences are identified and the most useful adapted to detection technology. Appendix A serves to summarize the wide range of physical properties and characteristics of seismological science that must be exhausted in the development of a useful detection technology.

Various nuclear events, depending upon their location, their timing, and other related physical circumstances, will produce effects which have various similarities and differences with respect to natural or manmade disturbances. Consequently, detection will always be expressed in terms of a probability that a nuclear event of a certain specification has occurred. This quantitative probability is, of course, dependent on the capability of individual stations, their distribution in the net, and the capability of the net in synthesizing data from constituent stations.

The geographic spacing of network stations and, therefore, their number and location, depends on:

(1) The quality of the data at any detection station, or of a combination of detection stations whose relevant data may be combined for evaluation.

(2) The smallest explosion for which detection is desired within a specifiable probability.

Therefore, the character of a detection network at any time depends on the specifiable nature of physical factors of appendix A and on the derived technology, as well as the defined physical feasibility of circumventing or disguising a nuclear event by attempted concealment of certain physical manifestations of a nuclear test explosion.

It is immediately apparent that the problem of detection and circumvention is simply a form of the well-known problem of measure and countermeasure which is as old as the history of warfare. In this problem, detection capability is pitted against concealment. The hand of concealment is played in this game by the atomic bomb experts who, in every country, are highly organized with adequate financing and laboratory facilities. The hand of detection is played in the United States by a group which, until Vela, was totally unorganized and

with almost no financing. Out of this unbalanced situation arises the urgent need for prompt organization of the management of the evaluation function and continued effort toward development of our detection capability.

Because of the imbalance in the strategic game of detection versus concealment, the protagonists for concealment are clearly ahead. Consequently, there are many who believe that the potentialities of detection at any future time are simply hopeless.

In my opinion, we do by no means have sufficient information to prejudge the potentialities of detection as hopeless. On the contrary, an examination of the list of physical effects to be studied shows there are many clues of considerable promise which require extensive research before they can be finally evaluated. Moreover, the means of experimentally evaluating suitable combinations of alternatives successively as the research progresses is an imperative function of a well-organized management and evaluation laboratory.

In particular, as set out in the report of the Panel on Seismic Improvement to the Department of State (1959), our knowledge of these physical elements underlying a system of a seismic detection is very inadequate and primitive. It was for this reason that the Panel recommended an extensive research program (*a*) to improve that knowledge, (*b*) to evaluate the potentialities of artificial concealment designed to circumvent detection, and (*c*) to reveal for purposes of detection those as yet undiscovered weaknesses of concealment techniques in order to circumvent circumvention. Particularly, for these reasons, the Panel recommended actual observation of underground nuclear tests, in which various forms of concealment were attempted, in order to disclose quantitatively those factors related to concealment. While small-scale conventional explosions could provide certain very useful preliminary data, it seemed unlikely that small cavities in the earth would give rise to the full range of phenomena on which either concealment or detection of much larger scale underground nuclear tests might ultimately rest.

Some aspects of this proposed program are now under way within Project Vela and a few useful results have already emerged. Among these results:

(1) The Cowboy tests, using conventional explosives, have more precisely evaluated our broad estimates of the quantitative effectiveness of cavity concealment and have disclosed the quantitative potentiality of "partial concealment" with smaller cavities against conventional seismic methods and modes.

(2) A number of potential methods of differentiating between earthquakes and explosions are emerging from the research.

This latter development is of considerable importance, since these methods offer some potential toward eventual distinction between small explosions and small earthquakes. Thus, the magic number of earthquake magnitude 4.75, so often assumed as the limiting level of effective detection, may eventually be materially reduced.

In addition, under the Vela program, nearly 80 useful research projects are going forward, each of which will add significantly to our basic knowledge of the relevant fundamental physical effects underlying detection.

Therefore, because of the previous radical imbalance in the support of the protagonists in this strategic game, I am far from being prepared to concede that effective detection cannot be effectuated with a reasonable system. Throughout the history of warfare, protagonists of powerful weapons have always been convinced that such weapons could not be countered, yet step by step they have been countered through the courage of men who were prepared to exhaust applicable physical systems systematically, and by adequate support for such men. Certainly the problem of detection is no more hopeless than the problem of the anti-ICBM on which billions are now being spent. At one time the long-range bomber was thought to be invincible. In warfare our whole power of defense has hinged on careful attention to the weaknesses of so-called invincible weapons and situations.

I would conclude then by summarizing the position of the Nation's detection capability relative to its probable ultimate success. At the moment detection is based primarily upon the relatively primitive science of seismology upon which extensive research has only begun. Out of this research will come some rich opportunities, many of which cannot now be foreseen. The next step is to organize promptly a means of effective management of the evaluation function as recommended by ARPA at your hearings. If this is done, the status of nuclear detection can be continuously evaluated, put on a sound footing, and evolve effectively toward even better and cheaper methods until an acceptable combination can be found.

The national objective should be the acquisition of a reasonable strategic position in the detection of nuclear tests. Aside from its own value, such an achievement would be a rational demonstration of safe methods toward ultimate control of weapons of mass destruction.

Sincerely yours,

L. V. BERKNER, *President.*

APPENDIX A

Appendix to letter of L. V. Berkner to Congressman Holifield on the subject of surveillance of nuclear testing

Some physical factors relating to the effectiveness of detection and identification of the generating source of seismic waves.

1. The energy of the original impulse.

2. The form of the initial impulse in times; i.e., its rate of rise and fall which determines the relative energy in each of the different wavelengths originated.

3. The form of the initial impulse in space; i.e., whether originating from a "point source" as a nuclear explosion or whether from an extended surface or extended irregular volume as an earthquake fault. (The directional pattern of propagation and the nature of wave displacement in the two cases will be different in preferred directions.)

4. The gross coupling of that impulse into the earth: i.e., the efficiency of the transformation of the energy into waves that are propagated through the earth. (This takes account of both natural and artificial decoupling and the conceivable range of properties of natural materials in the vicinity of the disturbance.)

5. The detailed coupling of the energy of each wavelength of the impulse into the earth. (This depends upon the local decoupling factors, either natural or artificial, at each wavelength and involves nature of materials, local irregularities, and nonlinear wave-frequency transformations.)

6. The range of these several characteristics of natural earthquakes at each energy level and the frequency of such natural events at the several energy levels in various geographic locations.

7. The rate of absorption of the seismic waves in the earth as they are propagated from the source to the detection station (and more particularly the absorption or alternation of each wavelength and each wave mode) since short seismic waves are in general absorbed more rapidly than long waves (and different wave modes behave differently.

8. The velocity of each wave frequency and wave mode as it is propagated from the source to the detection station. (The propagation of elastic waves in the earth is "dispersive," i.e., each wavelength in a given medium travels with a different characteristic velocity. Moreover, at different depths, with different materials under different pressures and stresses, the velocities at a given wave frequency are different. In addition, a given wavelength may travel to the detector over a variety of paths of different lengths. Finally, at the same wave frequencies, different wave modes travel in the source media with quite different characteristic velocities.) Thus, the final assembly of waves at the detection station reflects a variety of intervening conditions effecting the times of arrival of waves of each wave frequency and wave mode.

9. The coupling of the detection apparatus to the earth in the transformation of the wave energy in the vicinity of the station into signals in the apparatus.

10. The detection sensitivity of the apparatus to each wavelength and wave mode in reproducing the wave energy received at the detection station.

11. The accuracy of reproduction of the received waves, so that with appropriate phase equalization to compensate for the dispersion and path differences in propagation, the form of the initial source can be reconstructed.

12. The directional characteristic of the detection apparatus with respect to the direction of the source.

13. The energy of random unrelated noise waves at the detection station at each wavelength and in each wave mode which is superimposed on the desired waves and which tends to distort them, reduce their recognizability, or totally obscure them when their amplitude is too small.

14. The ability to resynthesize or integrate the desired waves at the detection station by a variety of methods in order to extract them from the composite signal involving high level random noise, even when the desired originals are far below that noise level in energy.

15. The ability to recognize the nature of the original event from the combination of the energy distribution among the different wavelengths as related to the form of the initial source.

To these must be added:

16. The capability of combining data from two or more stations of the network for the purpose of:

(*a*) Improving the desired signal-to-random-noise ratio.

(*b*) Acquiring additional information about the source by virtue of different energy and phase differences in different directions; e.g., differentiation of "point" from extended sources.

(*c*) Evaluating each event from a broader range of information acquired concerning the different characteristics of different paths.

APPENDIX 3

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APPENDIX 4

UCRL-6165

USE OF LARGE CAVITIES TO REDUCE SEISMIC WAVES FROM UNDERGROUND EXPLOSIONS

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Abstract. An analysis is given of an experiment designed to test the theory of seismic decoupling of underground explosions proposed by Latter, LeLevier, Martinelli, and McMillan [1961]. The amplitude of a 1.7-kiloton nuclear explosion in a hole in salt is calculated and compared with the measured value of the 1.7-kt Rainier shot in tuff at the same distance. A decoupling factor of about 300 resulted. The experiment, called Cowboy,[1] was designed to test the decoupling principle by carrying out a series of eight high-explosive shots in two spheres made in a salt dome and nine tamped shots for comparison. The seismic data reported here were obtained primarily at 14,000 and 22,000 feet from the shots at frequencies of 10 to 30 cps. A salt-to-salt decoupling factor of 100 was obtained which is consistent with the predicted tuff-to-salt factor of 300. When the sphere was overdriven so that the walls did not move elastically (which violates a condition of the theory for full decoupling), decoupling factors of 10 and 30 were measured. The seismic data are interpreted to give the dependence of decoupling on the various parameters of the experiment. The decoupling deduced from measurements made 80 feet from the shot points is found to be consistent with that deduced from the measurements at 14,000 and 22,000 feet.

[1] Project Cowboy was sponsored by the Atomic Energy Commission and carried out under the technical direction of the Lawrence Radiation Laboratory, University of California.

INTRODUCTION

This report is an interpretation of the surface seismic data obtained in Project Cowboy. This project, sponsored by the Atomic Energy Commission, was designed as an experimental test with high explosives in salt of the decoupling theory published by Latter, LeLevier, Martinelli, and McMillan [1961].

The idea, in brief, is that the coupled or tamped explosion causes the surrounding medium to flow plastically, crack, and behave nonlinearly until the wave has propagated far enough out for the stress to be reduced to a "critical stress" (dependent only on medium characteristics and depth) at which time elastic propagation begins. If, however, the explosive is placed in a hole just large enough for the critical stress to occur on the walls, the radius and volume of the hole are much smaller than the radius and volume at which the tamped shot reaches the critical stress. For the low frequencies of interest, the amplitude of the seismic waves radiated away is directly proportional to the product of stress and volume at the critical radius. The spherical cavity, having a much smaller volume at critical stress than the tamped case, radiates a much smaller seismic wave.

At large distances, Latter, LeLevier, Martinelli, and McMillan [1961] predict that the amplitude of a tamped nuclear shot in tuff is 300 times the amplitude of a nuclear shot in a cavity in salt. In the Cowboy experiment we are comparing amplitudes of tamped high-explosive shots in salt to high explosive shots in a cavity in salt. The theory used predicts that the amplitude or decoupling ratio depends on having experimental measurements from the tamped explosion. Since no such measurements were available prior to Cowboy, it was not possible to predict the Cowboy results.

Seismic records which illustrate the decoupling effect are presented in Figure 1. The six traces show signal amplitude (particle velocity) versus time for six different seismometers located 200 feet apart, starting at 44,600 feet. In the upper panel a 1000-pound tamped shot is clearly seen. In the lower panel a 1000-pound shot in the 30-foot-diameter sphere gives no visible signal despite the fact that amplifier gains have been increased by a factor of 8.

To obtain a quantitative measure of the decoupling it is necessary to use seismograms taken near enough so that the decoupled shot gives a measurable signal. In Figure 2 records taken at 14,400 feet are reproduced. The decoupled shot has been amplified by a factor of 64 relative to the tamped shot. Since the amplitude of the tamped signal (at the dominant frequency of 30 cps) is roughly 50 per cent larger than the sphere signal, a decoupling ratio of about 100 results.

In this report, we examine the dependence of decoupling on the various parameters of the experiment. We have answered the following questions. Can the different seismic waves be identified as to path traveled in the particular geologic environment of the experiment? (Yes.) Does the decoupling depend on wave type? (Yes.) What wave is least affected by local geology and shot location so that it can be extrapolated out of the particular geologic environment? (The so-called "direct" wave.) What is a reasonable range of decoupling for it? (90 to 125, although extraneous values ranging from 60 to 200 can be found for specific shots and geophones.) What causes the variation? (Geology, shot location differences, and nonreproducibility of shots.) Does decoupling change if the pressure due to the explosive is changed? (No, not as long as the walls remain elastic.) What happens at lower frequencies? (The data show that the decoupling remains essentially constant.) Are these results consistent with the measurements made close in? (Yes.) What happens

if a small sphere is used and the elastic limit is exceeded? (The decoupling reduces to 25 and is even smaller at lower frequencies.)

THE PLAN OF THE EXPERIMENT

Salt was chosen as the shooting medium because the decoupling theory predicts a large decoupling for a cavity in salt, and salt cavities are easier to excavate than cavities in other strong media. Arrangements were made with the Carey Salt Company to use its salt mine at Winnfield, Louisiana. The shots were located in the unused northeast workings of the mine which are sketched in Figure 3. These workings are 800 feet below the surface.

The spheres were made with diameters of 30 and 12 feet. The 2 1/2-ft-diameter doors to the spheres were made of steel and were sealed with rubber gaskets. The plan was to detonate high explosive located in the center of one of the spheres, record the seismic waves, wait 15 minutes, detonate the same weight of explosive in a tamped hole, and record again. These paired shots are connected by lines as shown in Figure 3.

Typically, the tamped holes were made in the following manner. An 8-inch shaft was drilled 110 feet down from the mine floor. The bottom 3 feet was reamed to a 2-foot diameter and filled with the appropriate amount of pelletol explosive. The hole was grouted to the surface. Instruments designed to measure particle velocity and acceleration were placed within the mine at distances ranging from 40 to 800 feet from the shots. The pressures in the spheres were also recorded. This work was done by Sandia Corporation [Murphey, 1961], the Bureau of Mines, and the United Kingdom.

Surface seismic stations were set up at various intervals ranging out to 30 miles. Records from stations directly over the dome are not useful because of the large shot separation in the dome. Useful records for the decoupled

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shots were obtained at station 4 at 14,400 feet and at station 5, 22,100 feet from ground zero. The instruments were supplied by the National Geophysical Company and were of two types. The "low frequency" instruments had a pass band from 2 to 20 cps. The "high frequency" instruments had a pass band from 20 to 100 cps. Benioff 1- to 3-cps seismographs were also used at these stations but the only decoupled shot that gave a signal large enough above the noise to be useful was the 2000-pound overdriven sphere shot. The surface seismic instrumentation was supervised by the U. S. Coast and Geodetic Survey [Carder and Mickey, 1960; see also Adams and Allen, 1960].

THE THEORETICAL SEISMOGRAMS

A difficult part of the interpretation was to estimate what effect the particular Winnfield, Louisiana, salt dome and surrounding geology had on the measurements. Without attention to geology, the data have large variation. With even a crude geologic model and rough calculation, much of this variation can be understood with fair assurance of reproducibility of results if the experiment is repeated elsewhere.

A discussion of the procedure for making a theoretical seismogram has been placed in the Appendix. For our purposes here, we will mention only the method and the results so that we can proceed with the interpretation of the seismic data.

The first step in making a theoretical seismogram is to construct a geologic model. Data from oil wells in the vicinity are used but are insufficient to delineate the edge of the salt dome, so we assume a "possible" salt dome configuration. Hence, the theoretical seismograms show typical behavior rather than actual behavior. Nonetheless, we believe them to be useful in the interpretation.

The calculation is simplified by first computing the response of the model to a delta function or impulse source. The results are shown in Figure 4. The series of impulses received are identified by the name of the deepest geologic formation that they have passed through.

We convolve this impulse response with assumed equivalent source functions for the tamped and sphere shots to produce theoretical records. In view of the crude geologic model, we do not deem it worth while to derive equivalent source functions from whatever information that was available concerning the actual source wave form, the attenuation, and the instrument response. The wave forms assumed for the tamped and sphere shots are given in Figure 4. The ratios of their spectra are merely illustrative and are not to be construed as a prediction of the frequency dependence of decoupling. The two bottom left panels of Figure 4 show the theoretical records obtained by convolving the impulse response with the two assumed wave forms. In the next several sections, these theoretical seismograms will be used in the interpretation of the experimental records.

PROBLEMS IN THE INTERPRETATION

Identification of the major arrivals. The experimental records were digitized. Figure 5 shows a plot of the records in the digital form. In a program of this nature, it was thought useful to cover the frequency range of interest with different instruments. It is unfortunate that the major energy band, 10 to 40 cps, straddled the pass bands of the two sets of instruments referred to as "high frequency" and "low frequency" with the crossover point at 20 cps.

Three bursts of energy can be identified on all six of the displayed records. A time correlation of these bursts with the impulse response identifies the first as that due to refractions by the Austin Chalk, Navarro, and Paluxy formations (the geologic model is shown in the Appendix.). The second burst is primarily due to the refraction by the Wilcox formation and will be called the "direct" arrival since the energy travels out essentially horizontally, directly to the receiver, merely refracting up through the weathering layer. The third burst is believed due to refractions from the Rodessa and Sligo formations. Probably purely reflected waves, which have not been calculated, also come in at this time. No easily identifiable surface waves appear on the records.

Although the energy bursts seen on the experimental records can be observed on the theoretical records, the detailed comparison is poor. This is to be expected, since we are dealing with a boundary value problem in wave propagation in which the field (seismic record) is known at a few points, the actual boundaries are unknown (the model is only a first approximation), and the source function is to be determined. Such a problem, in principle, is theoretically indeterminable.

But the experiment must be interpreted and the effects of geology considered if we wish the results to have meaning for other locations. Despite lack of detailed agreement between the experimental and theoretical records, we can still use the theoretical records profitably in the interpretation because they represent the types of complications introduced into the interpretation by the geology. The geologic model and the theoretical records do identify the wave types, do explain the structure in the spectra, do predict trace-to-trace variation of decoupling, and do aid in understanding the scaling problem.

The effect of geology on the Fourier spectra. The Fourier spectra of the field records are shown in Figure 5. In a later section we examine in some detail the spectra of the outgoing wave in the salt from both the sphere and tamped shots. No sharp, dominant peaks or troughs occur below 30 cps in either wave. Therefore, we must conclude that all the structure in the spectra analysis is caused by the geology.

Such structure is indeed evident when the theoretical records are examined by Fourier analysis (Fig. 4). The Cowboy experiment was designed to give information about the source from the distant seismic signals. Yet we find that virtually all the complexities in the spectra and the many-fold arrivals in the time domain are due to the geology. This complication in the interpretation is no surprise to seismologists.

In principle, taking a ratio of tamped and sphere shot spectra cancels out the geologic effects and leaves only the source effects, provided, of course, that the two impulse responses are identical. In Figure 4, the ratio of the assumed tamped and sphere source functions is shown, as well as the ratio of the theoretical records. In the pass band the two ratios are equal. Out of the pass band the ratios of nearly zero quantities give wild answers. Higher precision in the digital analysis undoubtedly would bring these ratios into agreement.

The ratios of the experimental records are given in Figure 5. Even in the pass band wild numbers occur. The preshot noise spectrum has been calculated and in these cases is too small to cause the wild answers. The explanation appears to be that the impulse response for the tamped shot is not identical to that of the sphere shot. Tamped and sphere shots were located some 500 feet apart in the mine (over a quarter of a wavelength at 10 cps; see Fig. 3). It is reasonable to believe that this shift in location can change

the impulse response of one shot relative to the other so that the impulse responses do not cancel out in taking a ratio. The theoretical seismograms show that the impulse response controls the structure in the spectra. A small shift of a peak or trough will yield a wild ratio.

An alternative to the Fourier spectrum analysis to obtain decoupling is narrow-band filtering. Figure 4 shows that the ratio of the two theoretical records, narrow-band filtered, is the same as that of the two filtered source functions. Filtering, being a smoothing process, does not yield wild decoupling ratios. Furthermore, filtering has an additional advantage over spectrum analysis because, if the wave forms of the filtered sphere and tamped records do not agree reasonably well, noise can be suspected and the point downgraded accordingly in the interpretation.

The high frequency records themselves are approximately equivalent to narrow-band filtering at 30 cps (see Fig. 5). The high frequency cutoff is controlled by attenuation, the low by instrumental cutoff. Direct analysis of the high frequency records (Fig. 1) is satisfactory and quite convenient.

The results at 30 cps have particular significance, since 30 cps scales to about 1 cps in going from 1000-pound to 15-kiloton nuclear explosive. (Time scales as the cube root of the yield, as we will see in a later section.) The first motion of seismic waves at about 1000 km from a 15-kt underground nuclear detonation is at a frequency of about 1 cps and was recognized by the Conference of Experts in Geneva as the principal means of both locating and identifying underground nuclear tests.

Trace-to-trace variation of decoupling. We next examine the variation of decoupling with trace or geophone location. The impulse response for each trace has been calculated for both station 4 and station 5. After convolving

these responses with the assumed source functions, we have calculated the amplitude ratio of the refraction arrivals. Of course, only the relative value or variation has been deduced.

The results are plotted in Figure 6. A large trace-to-trace variation is seen for station 4 and essentially no variation at station 5, although the absolute answer depends on what particular legs, or peak and trough, are picked for the calculation. The effect of the guided waves is more important for station 5, since it is farther away. For that reason, the impulse response at station 5 is less complete.

The experimental trace-to-trace variation is shown in Figure 7. A large variation occurs for station 4 and very little for station 5, just as was predicted theoretically.

Station-to-station variation of decoupling. The experimental data in Figure 7 point out still another problem. Station 5 decoupling values for the 1000-pound shot are significantly higher than the station 4 values. The problem is demonstrated by plotting the amplitude against charge size for the various arrivals at the various stations (Fig. 8). The data have been normalized to the 500-pound shot to eliminate trace-to-trace variation.

The decoupled shots follow a first-power scaling law rather well. In a later section we shall show that a first-power law is correct.

The scaling of the tamped shots is not as simple. We should expect shots 11, 13, and 15 (1003, 987, and 936 pounds, respectively) to scale the same way at each station, yet they do not. Shot 11 at station 5 is particularly inconsistent. The energy arriving at the various stations leaves the shot at different solid angles. It may be that the different shots produce different amounts of low-frequency energy in the various solid angles pertinent to the

measurements at the distant stations. This point will be explored further in a subsection on tamped shots.

Differences in shot location may also be partially responsible for the station-to-station discrepancies. In the Appendix it is shown that the refracted rays approach grazing angles on the dome walls. Transmission coefficients change rapidly with angle under such conditions. Shots 11, 13, and 15 are over a quarter of a wavelength apart in the dome (see Fig. 3) and, for certain dome configurations, we believe there is enough change of the angles to get a significant amplitude change with change in source location. We have already seen that location differences of 500 feet between tamped and sphere shots are important enough to produce wild decoupling ratios. Curves A and B for the tamped shots show scaling deduced from close-in measurements. Discussion of these curves is reserved until later (see section "Comparison with close-in data.")

The direct wave to station 4 should be the least affected by location differences of the shot points since the energy propagates out of the dome at normal incidence. We conclude that the station-to-station and trace-to-trace variations in decoupling are a manifestation of the particular wave path and shot location in the Winnfield geology. Decoupling ratios of paired shots are valid but apply only to the particular circumstances of the Cowboy experiment.

It is highly desirable to obtain a decoupling value independent of the Winnfield geology. Since we cannot theoretically reproduce the experimental seismograms, we attempt to average out the geologic effect before taking decoupling ratios.

DECOUPLING FOR THE DOMINANT FREQUENCY

The scaling study in the last section has pointed out the importance of studying the amplitudes of the tamped and sphere shots separately before taking ratios. Figure 9 summarizes the high-frequency data. Traces 3, 4, and 5 are used for station 4, and traces 1 and 6 for station 5. These are the most noise-free traces on all the records. The amplitudes have been normalized by trace number to the 500-pound tamped shot. Large spreads occur in the sphere shots; however, if a sphere shot had been chosen for normalization, large spreads would also occur in the tamped shots.

The decoupling is summarized in Figure 10. The circles are the ratios of the mean values of the direct wave at station 4. These are considered best values because they are least affected by changes in shot location and have the highest amplitudes. We arbitrarily define the range by taking the ratio of the two maxima and two minima for a pair of shots given in Figure 9. The results should be less dependent on the local geologic influence that affects the paired-shot method of computing decoupling. An exception to this method has been made for the 100-pound shot in the 12-foot sphere. A temporary station was set up at a distance of 5000 feet for the shot, and a direct ratio was taken.

The amplitudes and decoupling factors have been plotted against the ratio of overburden to step pressure. The overburden pressure is due to the weight of the rock above, which places the sphere in an initial preshot compression. The step pressure ($\bar{p}$) is the pressure that the hot gases exert against the walls of the sphere after the initial spike has passed. This pressure ratio is equivalent to a volume-to-yield ratio.

If there were no spike, the walls of the sphere would remain elastic for a pressure ratio greater than one (i.e., for $\overline{p} < \rho gh$) where ρ is the density, g is the acceleration due to gravity, and h is the depth. In the past a pressure ratio of two (i.e., $\overline{p} < \rho gh/2$) has been considered as safe for nuclear explosions even with the spike. However, the spike for high explosives has greater momentum than for nuclear explosives, and A. Latter (private communication) has noted that a ratio of five ($\overline{p} < \rho gh/5$) may be necessary to insure completely elastic behavior for Cowboy.

The experimental data show that there is no significant change in decoupling if the pressure ratio (or volume-to-yield ratio) exceeds the critical ratio for keeping the walls elastic. This result is in accord with the decoupling theory.

The decoupling for overdriven or nonelastic sphere shots cannot be calculated by using linear theory. Experimental values are shown in Figure 10. Since it was not practical to shoot a 2000-pound tamped shot in the mine, the nominal decoupling for the 2000-pound sphere shot was obtained by doubling the amplitude of a 1000-pound tamped shot. Until the phenomenon of the overdriven sphere is understood, the validity of any extrapolation to other situations is uncertain.

FREQUENCY DEPENDENCE OF DECOUPLING

The decoupling values given in the last section are for the dominant frequency on the high-frequency records, namely, 30 cps. It is interesting to determine what happens to these values at lower frequencies.

A low signal level prevents the use of the low-frequency records at station 5. Attempts to obtain low-frequency information from the high-frequency records at station 5 lead to problems as illustrated in Figure 11. The spectra

are cut off abruptly on the low side, partly because of the instrumental cutoff but also partly by a geologically caused minimum in the spectra.

Data from the low frequency instruments at station 4 are the best hope of obtaining decoupling at low frequencies. Spectra of various shots at station 4, low frequency, are presented in Figure 12. A problem of reproducibility is immediately apparent. Shots 11, 13, and 15 did not have identical charge configuration but, as has previously been mentioned, shots 16 and 17 did, and yet variations occur. As an explanation we offer the observation that shots 16 and 17 were located 330 feet apart, which is almost a quarter of a wavelength at 10 cps. This same effect could have been operative for the 1000-pound shots. Shots 13 and 15 are separated from shot 11 by 140 and 250 feet, respectively. As mentioned earlier, it is probably the proximity of the dome edge which makes the effect so large. The disturbing aspect is that even for the same trace number the trends are not reproducible.

The lack of reproducibility from trace to trace can be demonstrated by spectral analysis of the impulse responses for two traces located 400 feet apart. As is shown in Figure 12, maxima and minima are materially shifted.

This lack of reproducibility in the data is discouraging, but it is still worth while to see if a trend can be established for the decoupling variation with frequency. As before, we shall use narrow-band filtering and pair each of the 1000-pound tamped shots with the 1000-pound decoupled shot to produce a decoupling ratio. The result is shown in Figure 13. Only two traces of the decoupled shot are sufficiently free of noise to be valid. The data are widely scattered and give no indication that the decoupling decreases at lower frequencies. The A and B curves are deduced from close-in measurements and will be discussed in a subsection on tamped shots.

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The frequency dependence of decoupling for the overdriven sphere is shown in Figure 14. Both the 1000- and 2000-pound shots show that decoupling decreases at lower frequencies. The 2000-pound shot gave usable signals at lower frequencies because the decoupling was less. Experimentally, we see the decoupling leveling off at low frequencies to a value of about half that obtained at 30 cps.

Both the 1000- and 2000-pound shots caused the sphere to be vented through the sphere plug. The gas from the 1000-pound shot leaked through a break in the plug gasket but instruments near the plug showed that this happened sometime after 1 second from shot time and hence would not affect the signals. The 2000-pound shot destroyed the plug sometime after 0.1 second from shot time. If the sphere vented rapidly at about 0.1 second, the signal would be altered. Although this possibility cannot be ruled out, we can find no evidence of it in the records.

The fact that sizable decouplings were obtained when the sphere was overdriven is significant. If this result can be extrapolated to nuclear yields, it means that a sizable decoupling can be obtained with a volume much smaller than previously considered.

COMPARISON WITH CLOSE-IN DATA

Decoupling theory. The decoupling theory may be illustrated in the following way. Near to an explosion, but beyond some elastic radius r_e, we assume that the motion can be represented approximately by the elasticity equations for a spherically symmetric infinite material. Then a "reduced" displacement potential $A(\tau)$ can be defined by

$$u(r,t) = \frac{\partial}{\partial r} \frac{A(\tau)}{r} \qquad (1)$$

where $\tau = t - r/c$, and $u(r,t)$ is the displacement. It can be shown that $A(\tau)/r$ represents elastic outgoing waves. The Fourier transform is

$$\hat{u}(r,\omega) = -(1/r^2 + i\omega/rc)\,\hat{A}(\omega) \tag{2}$$

$\hat{A}(\omega)$ is a convenient quantity since it is independent of r.

The distant signal will be given by

$$S(\tau) = \mathbf{O}\, A(\tau) \tag{3}$$

where $\mathbf{O}$ is some operator representing the earth, after transforming

$$\hat{S}(\omega) = \mathbf{O}(\omega)\,\hat{A}(\omega) \tag{4}$$

The operator $\mathbf{O}$ corresponds to the impulse response, attenuation, and instrument effects discussed in earlier sections.

Now if there are two explosions, 1 and 2, for which the conditions inside of r_e differ but outside of r_e are the same, then

$$\frac{\hat{S}_1(\omega)}{\hat{S}_2(\omega)} = \frac{\hat{\mathbf{O}}(\omega)\,\hat{A}_1(\omega)}{\hat{\mathbf{O}}(\omega)\,\hat{A}_2(\omega)} = \frac{\hat{A}_1(\omega)}{\hat{A}_2(\omega)} \tag{5}$$

Thus we see that the operator representing the earth cancels out as was demonstrated in Figure 4. Or for a tamped and a sphere shot, the decoupling factor is

$$D(\omega) = \left| \frac{\hat{S}_t(\omega)}{\hat{S}_s(\omega)} \right| = \left| \frac{\hat{A}_t(\omega)}{\hat{A}_s(\omega)} \right| \tag{6}$$

Since $\left| i\omega\,\hat{A}(\omega) \right|$ is a better behaved function than $A(\omega)$, we shall use

$$D(\omega) = \left| \frac{i\omega\,\hat{A}_t(\omega)}{i\omega\,\hat{A}_s(\omega)} \right| \tag{7}$$

In the decoupling theory $\hat{A}_t$ is estimated from experiment and $\hat{A}_s$ from elastic theory.

Since earth motions were measured near the Cowboy explosions by Sandia Corporation, one should be able to compare the distant decoupling factors with the close-in measurements. However, since the distant decoupling factors have a considerable experimental spread and since the close-in data are incomplete, one can only show consistency.

Sphere shots. The response of the 30-foot sphere should be completely elastic. The solution of the problem of the signal from an elastic spherical cavity driven by a pressure is given by several writers [Blake, 1952; Sharpe, 1942; Jeffreys, 1931]. For the "reduced" potential, it is

$$i\omega \hat{A}(\omega) = i\omega \hat{p}(\omega)\ (a^3 K/\rho c^2)\ [1 + (1 - 2K)x^2 + K^2 x^4]^{-1/2} \qquad (8)$$

where

$$K = \frac{1}{2}\ (1 - \sigma)\ (1 - 2\sigma)^{-1}.$$

$$x = \omega \frac{a}{c}.$$

a = radius of sphere.

c = compressional velocity.

σ = Poisson's ratio.

ρ = density.

ω = circular frequency.

$\hat{p}$ = Fourier transform of the driving pressure.

For the low frequencies of interest,

$$1 + (1 - 2K)x^2 + K^2 x^4 \approx 1$$

and therefore $i\omega \hat{p}(\omega)$ contains all the frequency dependence.

The pressure in the cavity may be written as

$$p(t) = p_s(t) + \bar{p}, \qquad t > 0 \qquad (9)$$

where $p_s(t)$ is a transient pressure spike and $\bar{p}$ is the step pressure, given by

$$\bar{p} = (\gamma - 1) W/V_s$$

where

V_s = the volume of the sphere.

W = the yield of the explosion.

γ = the ratio of the specific heats of the gases in the cavity.

For both air and low-density high-explosive combustion products, γ is about 1.2.

The Fourier transform of $\bar{p}$ is $\bar{p}/i\omega$. In the case of nuclear explosions the signal due to $p_s(t)$ is small at frequencies of interest but for solid chemical explosions it is not. This is due to the large mass of explosive hitting the wall.

An approximate evaluation of $p_s(\omega)$ for low frequencies can be given by

$$\hat{p}_s(\omega) = \int p_s(t) e^{-i\omega t} dt \approx \int p_s(t)\, dt, \qquad \omega \text{ small} \tag{10}$$

or

$$\hat{p}_s(\omega) \approx I \tag{11}$$

where I is the impulse on the wall.

Now if we equate I to the momentum per unit area of the high explosive,

$$I = \frac{\sqrt{2mW}}{4\pi a^2} = \frac{\sqrt{2W}}{4\pi a^2 \epsilon^{1/2}} \tag{12}$$

where ϵ is the specific energy of detonation, then

$$\hat{p}(\omega) = \frac{(\gamma - 1) W}{i\omega V_s} + \frac{\sqrt{2W}}{4\pi a^2 \epsilon^{1/2}} \tag{13}$$

and

$$i\omega \hat{p}(\omega) = \frac{(\gamma - 1) W}{V_s} \left[1 + i\omega \frac{\sqrt{2a}}{3(\gamma - 1)\epsilon^{1/2}} \right] \tag{14}$$

For the 954-pound explosion (nominal 1000-pounds) in the 30-foot sphere (shot 10),

$$i\omega \hat{p}(\omega) = 8.4 \left[1 + i\omega (0.0053) \right] \text{ bars} \tag{15}$$

where $\gamma = 1.2$ and $\epsilon = 926$ cal/gm for low-density TNT [Jones and Miller, 1948].

Equation 15 has been substituted into equation 8 and the results for the 1000-pound shot are shown as Theory in Figure 15. The pressure was measured [Murphey, 1960] in the sphere, and its time history is given in Figure 16. The step component averages to about 8 bars and the initial spike rises to 73 bars. Later spikes are reverberations in the sphere of the combustion products. The structure of the spike is thought to be an interaction between the combustion products and the small amount of residual air in the sphere. (The sphere was partially evacuated to eliminate burn-back and to reduce the size of the spike). The Fourier transform, $\left| i\omega \hat{A} \right|$, calculated from this pressure profile is shown as Experiment in Figure 15. The agreement between the measurement and the theory deduced from the momentum of the high explosive hitting the wall is good. The measured spikes have larger momentum than that in our simple calculation because of the reverberations of combustion products.

The experimental pressure record has been used to calculate the displacement at 80 feet. John Nuckolls, of our laboratory, has devised a machine code that numerically integrates the elasticity equations. The resulting theoretical displacement is shown in Figure 16 along with the measured displacement. Again the agreement is good.

Tamped shots. Figure 17 shows the displacement at 80 feet from two 1000-pound tamped shots. These displacements were obtained by integrating the velocity as measured by Murphey. The two measurements agree rather well at early times but not at late times. The same effect takes place with measurements on opposite sides of the 500-pound shot. Evidently there is asymmetry at later times on the records.

Postshot excavation of the area around one of the 1000-pound tamped shots showed an asymmetric crack pattern. Wright and Press (private communications) have both pointed out that shear-to-compressional wave ratios are larger for tamped than for sphere shots. Asymmetric motion about a tamped shot would tend to generate shear components. This asymmetry may be responsible for the fact that two 1000-pound tamped shots measured by the Benioff seismographs in the two-cycle range at both stations 4 and 5 differed in amplitude by a factor of approximately 2.

We have sketched two possible late-time extrapolations (A and B) for the tamped shots in Figure 17. These are meant to represent some possible average motions at 80 feet. The final value is the permanent displacement as computed from the measured increases in the volume of the hole.

The increase in volume for 100-, 500-, and 1000-pound tamped shots turned out to be about 0.065 ft^3 per pound of explosive. This results in an average displacement of about 10 mils for the 1000-pound shot at 80 feet. The assumption is made that no void spaces in the medium are closed by the tamped explosion. We believe that this is a reasonable assumption since the volume which experiences pressures greater than the yield strength of salt should not contain enough void space to take up much of the 65 ft^3.

Figure 15 shows $|i\omega \hat{A}|$ for these curves, and the resultant decoupling is given in Figure 13. The two different extrapolations on the tamped data are

so labeled. The results are completely consistent with the far-out data, giving values between 100 and 180. The asymmetry problem of the close-in tamped measurements causes the same degree of uncertainty in decoupling that we had already found in the far-out data.

Scaling laws. Equation 14 shows that for sphere shots $\hat{p}(\omega)$ is proportional to W. Hence for the decoupled shots, $\left| i\omega \hat{A}(\omega) \right|$, and therefore the distant signal at any given frequency, obeys a first-power law. The first-power law is compared with surface data in Figure 8 and adequate agreement is found.

For tamped shots more involved reasoning is necessary. For point sources of energy in infinite homogeneous media, one expects lengths and times to scale as $W^{1/3}$ if there are no characteristic distances or times. To state the law more exactly, if there are two explosions W_i and W_j with displacements $u_i(r,t)$ and $u_j(r,t)$ respectively, the scaling law is

$$u_j \left[(W_j/W_i)^{1/3} r,\ (W_j/W_i)^{1/3} t \right] = (W_j/W_i)^{1/3}\ u_i[r,t] \tag{16}$$

Although no real explosions completely satisfy the stated conditions, there is much experimental evidence to indicate that the $W^{1/3}$ law is approximately satisfied close to the shot point. In the case of Cowboy all of the Sandia data for tamped shots approximately satisfy equation 16. If we set $i\omega \hat{A}(\omega) = B_i(\omega)$ for a shot of yield W_i, then these laws imply that

$$B_j \left[(W_i/W_j)^{1/3}\ \omega \right] = (W_j/W_i)\ B_i[\omega] \tag{17}$$

Then for the distant signal (from equation 5),

$$\frac{S_1(\omega)}{S_2(\omega)} = \frac{B_1[\omega]}{B_2[\omega]} = \frac{W_1}{W_2}\ \frac{B_2[(W_1/W_2)^{1/3}\omega]}{B_2[\omega]} \tag{18}$$

We now have the distant scaling law in terms of B for one reference yield.[2]

We have used equation 18 to derive the 30-cps scaling law for Cowboy from curves A and B. The comparison is made in Figure 8. The close-in and far-out data are again consistent.

Overdriven spheres. As indicated in Figure 9, the 200-, 500-, and 1000-pound explosions in the 30-foot sphere should have produced no inelastic response. However, the 1000- and 2000-pound explosions in the 12-foot sphere should have produced inelastic response.

The 100-pound shot in the 12-foot sphere is a rather borderline case. Cracking should have occurred only during the transient phase. Unfortunately, the shot was detected only at a temporary station at about 1 mile away and the decoupling factors there have a large spread. We do not know what the correct average is.

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[2] It is interesting to examine these equations for the following simple assumptions concerning the time dependence of the source. If A(t) can be considered a step function for the frequencies of interest, i.e., $B(\omega)$ is a constant, then $S \sim W$ will result; a first-power law. If A(t) approximates a delta function, i.e., $B(\omega) \sim \omega$, then $S \sim W^{4/3}$.

These scaling laws have been examined for the nuclear explosions by Latter, Martinelli, and Teller [1959], using assumptions which are equivalent to ours. O'Brien [1960] has considered a first-power scaling law for chemical explosions. However, his assumptions do not seem to be consistent with the $W^{1/3}$ law close in. He assumes an effective elastic radius that is proportional to $W^{1/3}$; but, at that radius, he assumes a sharp pressure spike whose impulse is a constant for all yields. The $W^{1/3}$ scaling, which is experimentally observed in Cowboy, requires this impulse to scale as $W^{1/3}$.

The 1000- and 2000-pound shots in the 12-foot sphere gave much useful surface data because they provided comparatively large signals. Although the sphere was intact after these detonations, many cracks were visible in the wall. The only crack which approximated a great circle was on the equator, but since the velocity gages were on the equatorial plane, we believe they cannot be used for analyzing the motion around the sphere. Hence, we cannot compare surface data and close-in data for the overdriven sphere shots.

Significance of the results for the nuclear case. In the decoupling theory worked out by Latter, LeLevier, Martinelli, and McMillan, the decoupling factor was the ratio of the amplitude of the Rainier nuclear explosion in tuff to the theoretical amplitude for a decoupled nuclear explosion of the same yield in a hole in salt. The predicted value was 300. The Cowboy experiments used small chemical explosives for the tamped shots as well as for the sphere shots in salt. Differences between chemical and nuclear explosions and the difference between tuff and salt for tamped explosions have not been theoretically calculated. Nor should one expect to be able to scale exactly tamped chemical explosions to nuclear explosions larger by factors of 10,000. For these reasons the Cowboy experiments were not expected to confirm or deny the key Rainier measurement.

However, one could hope for confirmation of the basic theoretical assumptions of the decoupling theory, namely, the conditions for the elastic behavior of the hole and for linear propagation to distant stations. Figures 16 and 13 verify these assumptions within the accuracy of the experiment.

Despite problems in scaling and differences in chemical and nuclear yield it is interesting to compare Cowboy tamped explosions with the current interpretation of Rainier.

For Rainier, $|i\omega A|$ is approximately independent of frequency below 2 cps and is approximately 3 times as large as the value obtained by scaling the low frequency limit of a 1000 pound tamped high explosive in salt to the Rainier yield.

In scaling high explosive decoupled shots to nuclear yields it must be remembered that the effects of the large spike caused the high mass-to-yield ratio of chemical explosives would be absent. The $|i\omega A|$ for a decoupled nuclear explosion in a cavity in salt would be approximately independent of frequency up to 2 cps at the very least and would have a value equal to that obtained by scaling the low frequency limit of 1000 pound tamped high explosive in salt.

Hence $|i\omega A|$ for both a tamped nuclear explosion in tuff and decoupled nuclear explosion in salt is approximately independent of frequency up to 2 cps and the ratio is equal to about 3 times the value obtained in the Cowboy experiment ~300. We therefore expect both first motions (1 cps) and crustal surface waves (18 sec periods) to decouple.

It is more difficult to deal with the overdriven sphere problem because theory is not available to guide the discussion. We believe that the measured decoupling at the low frequencies of Figure 14 should be multiplied by 3 to allow for differences of tamped shots as explained above. At 30 cps an allowance should be made for the spike, and it is our opinion that decoupling should be multiplied by something less than 3.

CONCLUSION

The Cowboy experiment verifies the prediction made by Latter, LeLevier, Martinelli, and McMillan that explosions in elastic cavities will yield a very small seismic signal compared with fully tamped shots. The surface

measurements and the close-in measurements are consistent with one another and with the decoupling theory. The experiment shows further that, at least in these circumstances, an explosion in a sphere which is smaller than necessary to insure elasticity will still decouple.

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APPENDIX. THE CALCULATION OF THE THEORETICAL SEISMOGRAMS

Formation of the geologic model. The first step is to form a geologic model, that is, a cross section of the earth showing the various rock strata extending from the Winnfield dome in a southwesterly direction along the line of seismic stations. Electric logs (E-logs) taken from oil wells along this line are presented in Figure A-1. The identification of geologic horizons and the correlations have been made with the help of a stratigraphic section (located 8 miles to the north) published by the Shreveport Geological Society. The velocity information for the lower part comes from an interval velocity log (sonic) located 45 miles to the north. The velocities of the shallow beds are estimated. The velocities have been checked against velocity-depth functions for the area supplied by the National Geophysical Company, Inc.

Ideally, we should have a section made from interval velocity logs, but they do not exist. Since velocity is primarily dependent on porosity and since the resistivity measured by electric logs is also primarily dependent on porosity, the use of E-logs in the model is not unreasonable. The division into strata is made primarily on the basis of common velocity rather than geologic formation.

Although tamped signals were received as far out as station 8, the decoupled signals (down in amplitude by a factor of 100) were not. Hence, we need a model giving the detail from the dome out to station 4 at 14,400 feet. The layering in Figure A-2 is taken from the E-log section (simplified for ease of calculation) and the distance to the edge of the salt dome from drilling records made by the Carey Salt Company. Undoubtedly, the actual geologic section is far more complicated. Faults and structural complexities surely exist in the vicinity of the dome. More log information is required to define

the dip properly. This lack of additional geologic information forces us to proceed on the basis that this model is a first approximation to the geology.[3]

Method of calculation. The model is far too complicated to allow us to entertain the idea of making an exact elastic-wave-equation calculation for the motion at station 4 due to an outgoing spherically symmetric wave originating in the dome. We use the plane wave or ray-amplitude approximation. The first step in the calculation is to find the Green's function or response of the system to an impulse or delta function source. This impulse response is then convolved with the source function, the attenuation function, and the function representing the effects of the instruments, or with a function representing the combined effects of all three.

The technique is approximate, because only an approximate impulse response is found by tracing rays. The rays start with unit amplitude. Changes in amplitudes caused by the geologic boundaries are computed by using the plane wave elastic transmission and reflection coefficients. The use of these coefficients assumes the wave fronts are locally plane when crossing boundaries. If a sufficiently large number of the higher-amplitude rays are calculated, an adequate approximation of the impulse response is obtained.

Spencer [1960] has shown how exact solutions can be expressed as the sum of a plane wave solution and a correction term. The correction term is small and can be ignored if the frequencies of interest are sufficiently high. Knopoff [1958] has obtained exact solutions and reduced them to an approximate

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[3] The Shreveport Geological Society has recently published a guide book which gives about a 20° slope (from the vertical) for the opposite side of the Winnfield dome.

solution which is the plane wave solution. The approximation is valid, provided the pulse length is short compared with the total travel time involved. Low-frequency interface waves are ignored in any plane wave approximation.

Many oil companies have used this approximation in calculating theoretical seismograms for reflection prospecting (normal incidence only). One of us (Werth) has found good agreement between the actual field record and theory, when enough is known about the geology to insure that the interfaces are plane and parallel. Several examples are given by Wuenchel [1960].

Angle dependence must be included in our calculation. Furthermore, refractions, or head waves, require special treatment, because a plane wave theory will not yield a head wave. Knopoff, Gilbert, and Pilant [1960], following Cagniard, have developed a first-motion approximation for head waves (in two dimensions), which is equivalent to the plane wave approximation used for less than critical angle events. Malinovskaya [1957] outlined the Russian method of calculating an impulse response in their equivalent approximation. She presented moduli and phase angles for the various types of head waves.

Knopoff used the approximation to calculate a single-layer two-dimensional laboratory model record. He obtained a partial confirmation of the method as applied to non-normal incidence problems, but more rays should be calculated for a more complete experimental check. We know of no field application of the technique for non-normal incidence. However, ray-time (no amplitude) procedures have had outstanding success in outlining salt domes [Musgrave, 1960].

The theoretical seismograms. Physical insight is used in coding the problem for the required machine calculations. Basically, it is a refraction prospecting problem with the source complicated by the dome. A spherically

symmetric logitudinal (P wave) source is assumed. The first arriving waves travel down the dome, spill over into the various layers, propagate in these layers, refract up, split into P and S (shear) components at each interface, and arrive at the surface, where they are recorded and reflected back into the section. These waves are followed by waves reflected from the surface above the dome and from the sides of the dome. Next come waves which have traveled more or less horizontally out from the dome in the lower-speed layers near the surface. Finally, deep refractions and reflections arrive.

Since the geologic data poorly define the dome edge, it is pointless to belabor the precise mechanisms by which the energy gets out of the dome and into the layers. Some of it undoubtedly is transmitted into low-velocity strata and then critically refracted. Other energy is transmitted directly from the dome into the refracting beds. We considered not only the ray paths in the dome shown on the model but also rays reflected from the back side of the dome and from the surface.

A complete solution should contain the wave guide effect of multiple reflections in the layers as the energy is propagated horizontally. The farther the energy is progagated, the more important the effect. We do not attempt the calculation, since the layers forming the guides do not have high velocity contrasts, and since the distances propagated in them are short (at least for station 4). Furthermore, we would have difficulty in determining how the guides are excited, since we do not know dome-edge detail.

We do calculate all the rays that leave each of the beds as a critical refraction for either P or S. Malinovskaya's curves are used to give their relative amplitude. At every interface above the refractor, the waves split into new P and S waves. The transmission coefficients are used to calculate the amplitudes of all P-S splits all the way to the surface. All possible

refractions for the actual source and for the surface and back-dome-reflected-image sources have been calculated for a total of 2937 rays with their identifications, amplitudes, angles, and times. The vertical component of motion is computed and then plotted against time in the top panel of Figure 4.

Only twelve rays have sufficient amplitude to be plotted. Six have traveled as compressional waves all the way from the source. The other six are their surface reflections above the source. Although we do not question the importance of shear waves in a refraction problem [Vasil'ev, 1957], they have too low an amplitude to be of importance in this problem.

We have reflected these twelve P waves from the surface and again at each of the interfaces physically possible in the model. None of the resultant waves have sufficient amplitude to be plotted. Since the back-dome reflection has too low an amplitude, further internal dome reflections are unimportant. No attempt has been made to calculate the purely reflected arrivals.

The impulse response that results from this calculation is shown in Figure 4 and discussed in the section on theoretical seismograms.

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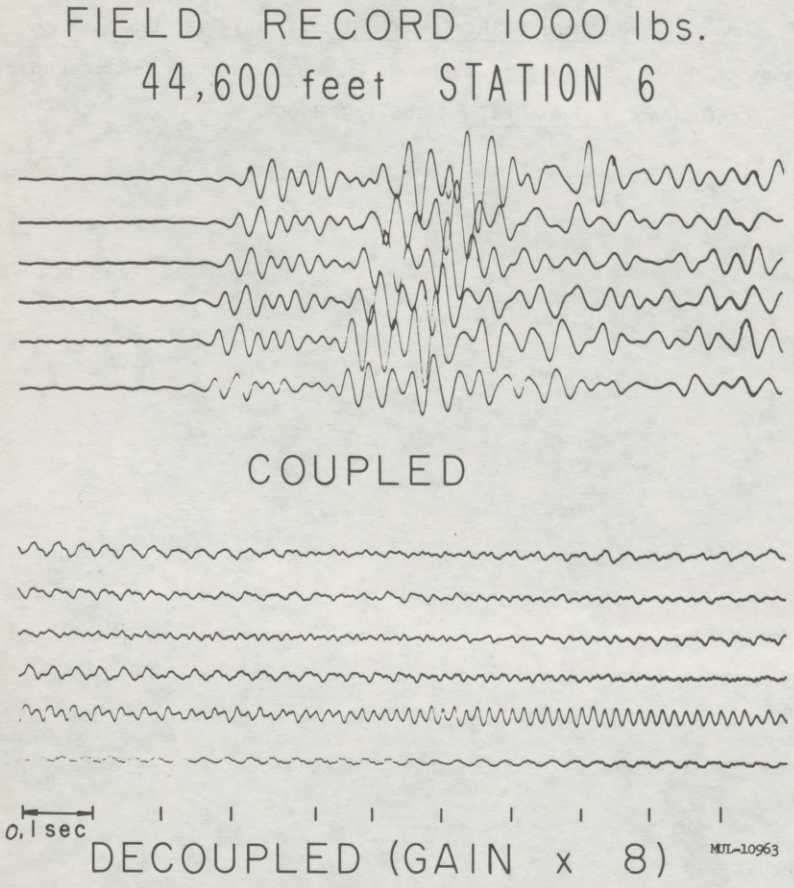


Fig. 1. Seismograms recorded at Station 6. The seismometers are 200 feet apart and are in-line with the shot point, the first one at 44,600 feet. The coupled record is shot 11 with 1003 pounds of pelletol. The decoupled record is shot 10 with 954 pounds of pelletol in a 30-foot diameter sphere in salt.

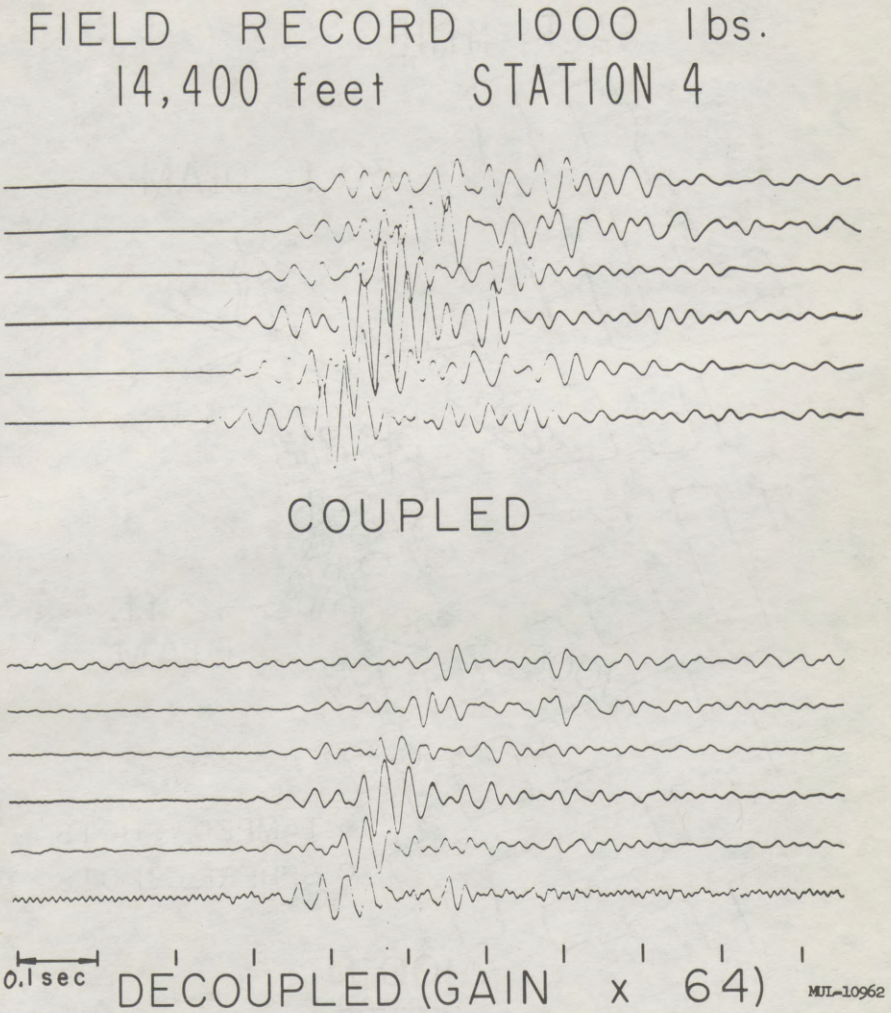


Fig. 2. Same shots as Figure 1, but recorded at Station 4 located 14,400 feet from ground zero.

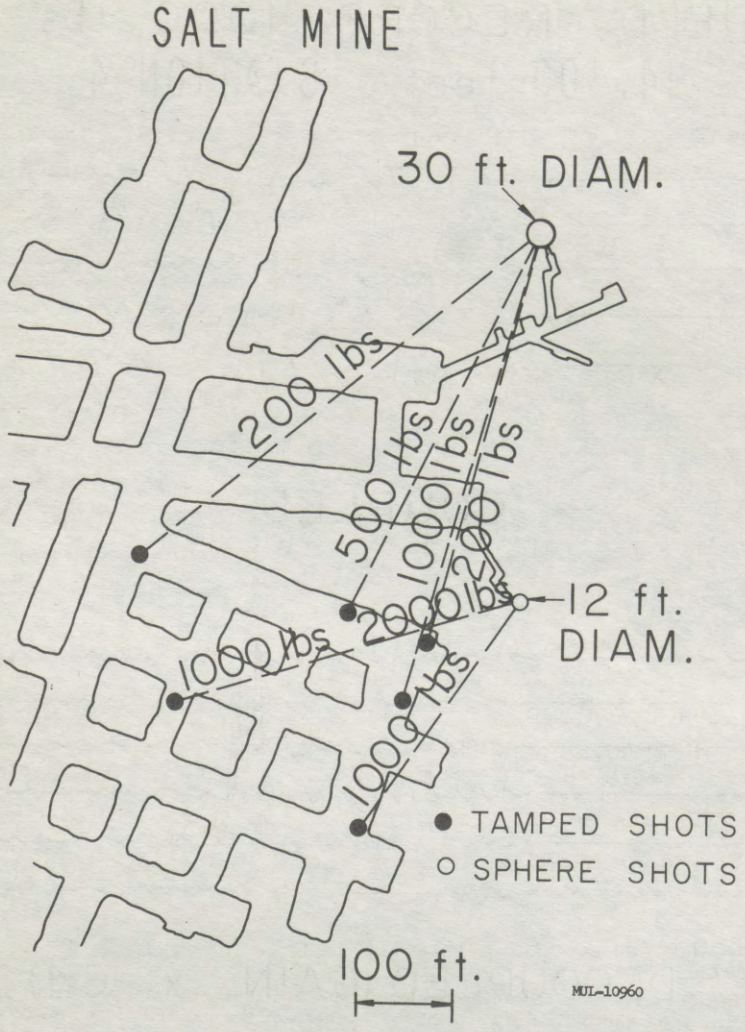


Fig. 3. Subsurface plan of the northeast workings of the Carey Salt Mine at Winnfield, Louisiana. The coupled and decoupled comparison shots are connected with arrows.

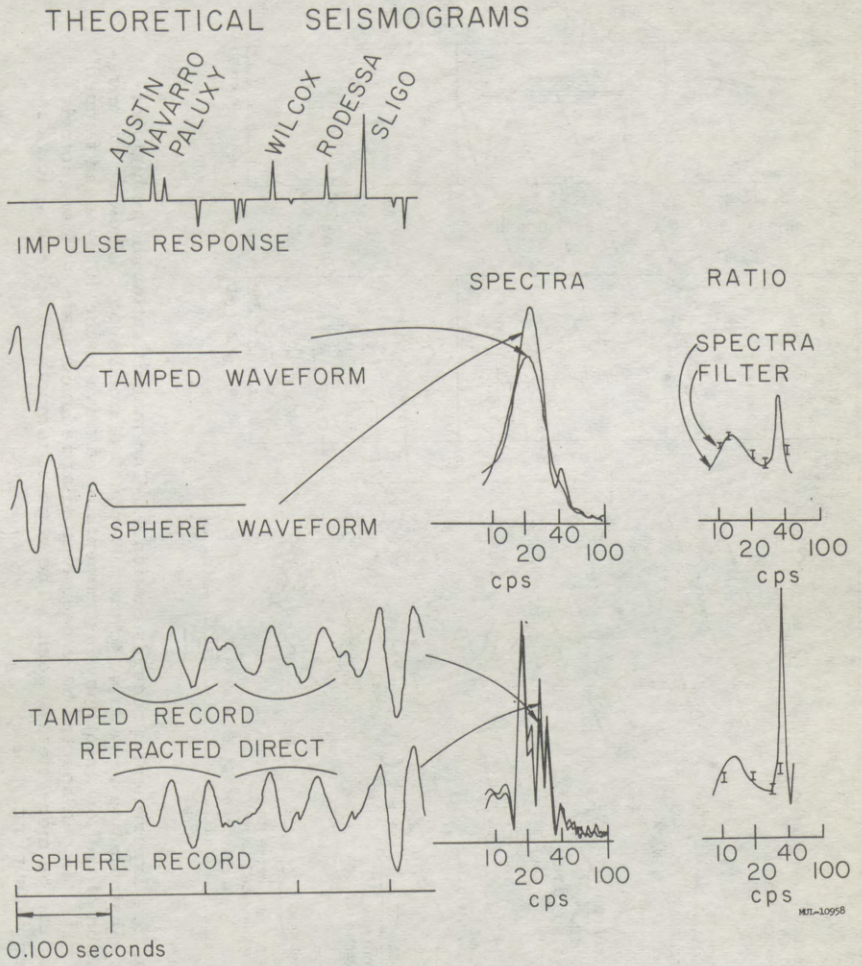


Fig. 4. Theoretical seismograms used to illustrate interpretation problems. Within the pass band the ratio of the spectra of the records is equal to the ratio of the spectra of the assumed source waveforms.

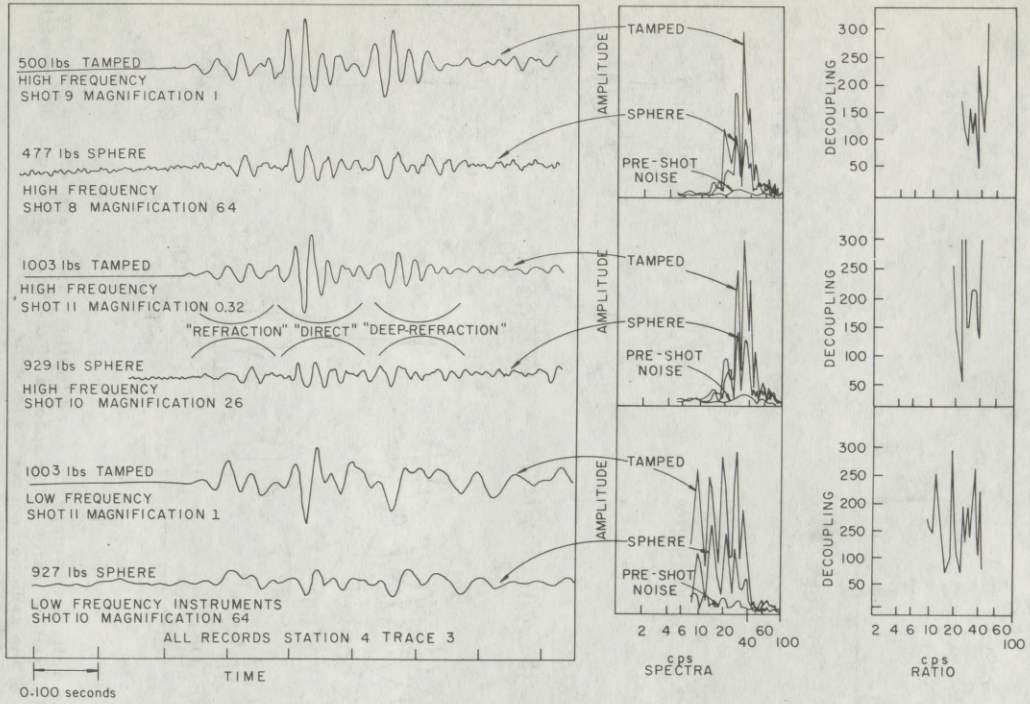


Fig. 5. Experimental records Station 4. The magnification of the high frequency instruments is relative to the 500-pound tamped shot. Likewise the magnification of the low frequency instruments is relative to the 1003-pound tamped shot. The tamped spectra have been plotted at reduced scale so that geologic effects can be more clearly seen. The ratio of tamped and sphere spectra is labeled decoupling.

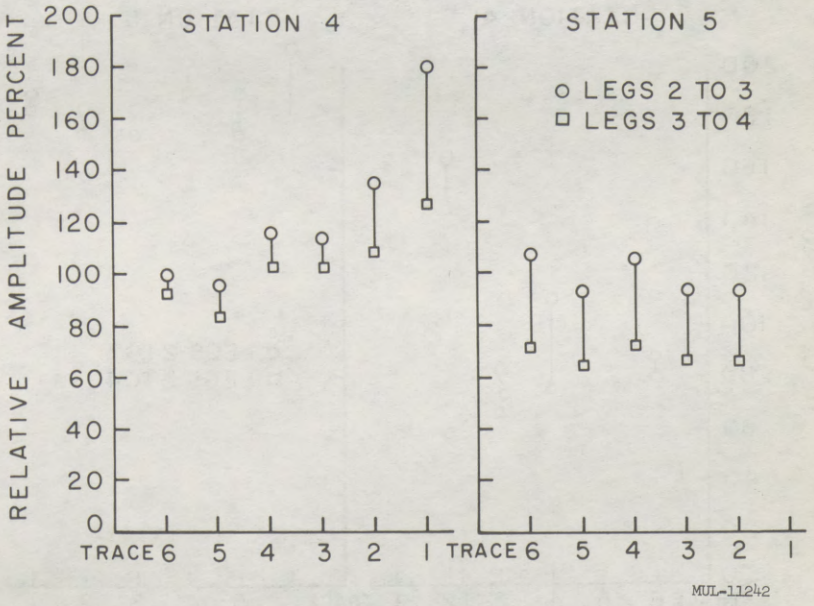


Fig. 6. Theoretical trace-to-trace variation in decoupling for the refraction arrivals. The traces are 200 feet apart. The circles are measurements from the second to third leg (peak or trough) and the squares from third to fourth. The variation is relative because actual source functions have not been used. The trends indicate the trace-to-trace variation that can be caused by geologic factors.

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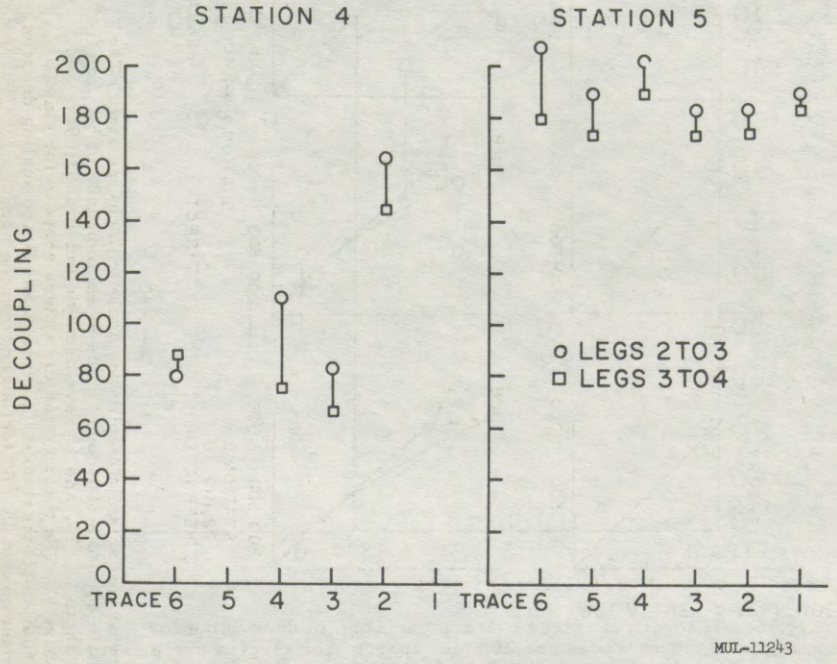


Fig. 7. Experimental trace-to-trace variation in decoupling for refraction arrivals. The decoupling is calculated from shot 11 (1003 pounds tamped) and shot 10 (954 pounds in the 30-foot-diameter sphere).

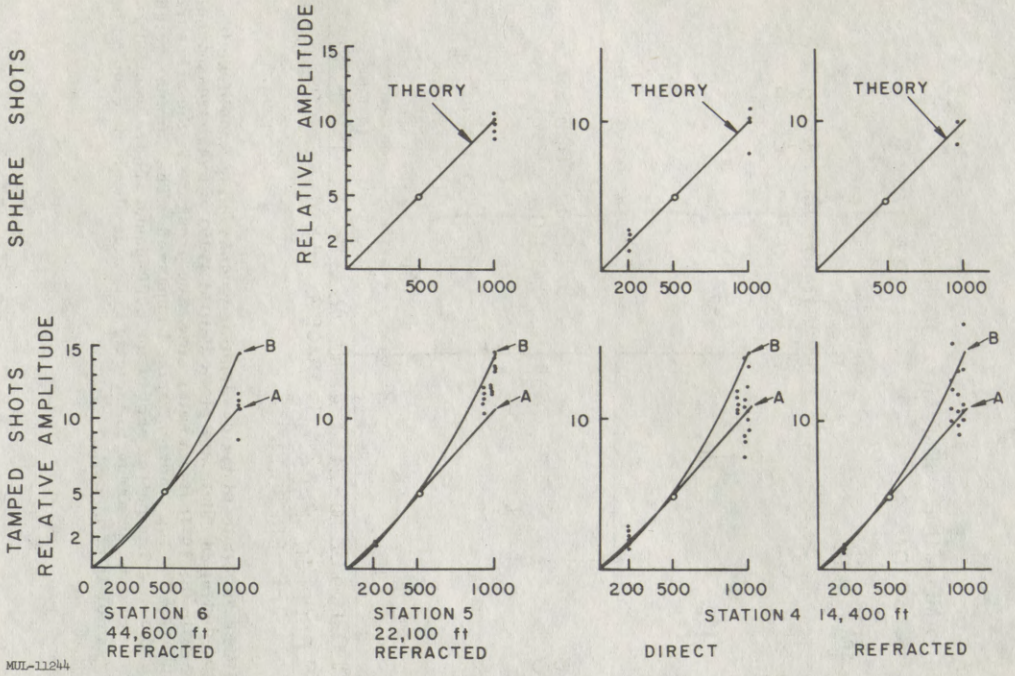


Fig. 8. Summary of how seismic amplitudes scale with charge size. The data has been normalized to the 500-pound shots at an amplitude of 5. Amplitudes from tamped shots 11, 13, and 15 have been plotted at 1003, 987, and 936 pounds, respectively. The theory cited for sphere shots is for elastic propagation away from a spherical hole. Curves A and B are scaling deduced from the measurement of shot 11, 80 feet away from the source.

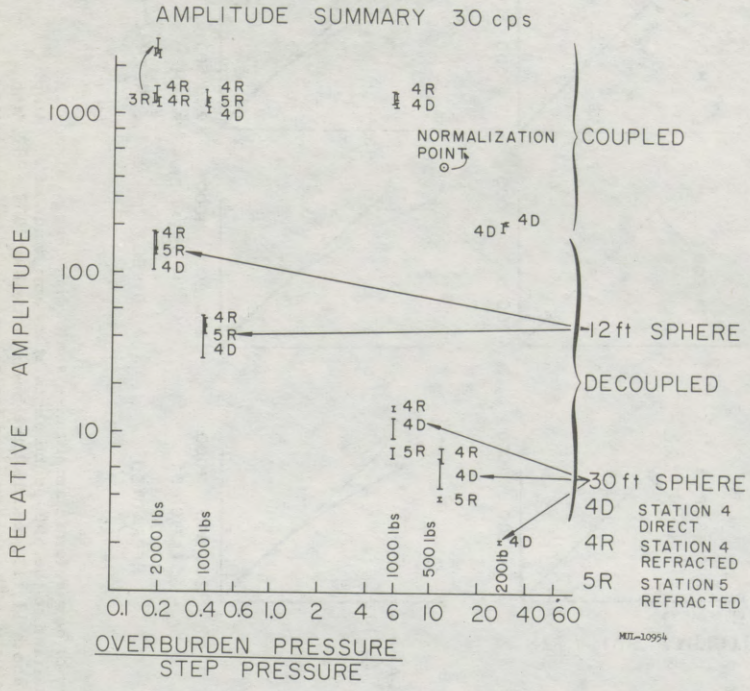


Fig. 9. Amplitude summary at the dominant frequency. The amplitude of paired coupled and decoupled shots are plotted at the ratio of overburden pressure to step pressure for the sphere shots. The step pressure is the pressure existing in the sphere after the transient spike has passed. The nominal weights of explosives are shown. The paired shot of the 2000-pound sphere was 1000 pounds and so we have used an arrow to indicate the value should be doubled before a decoupling factor is computed.

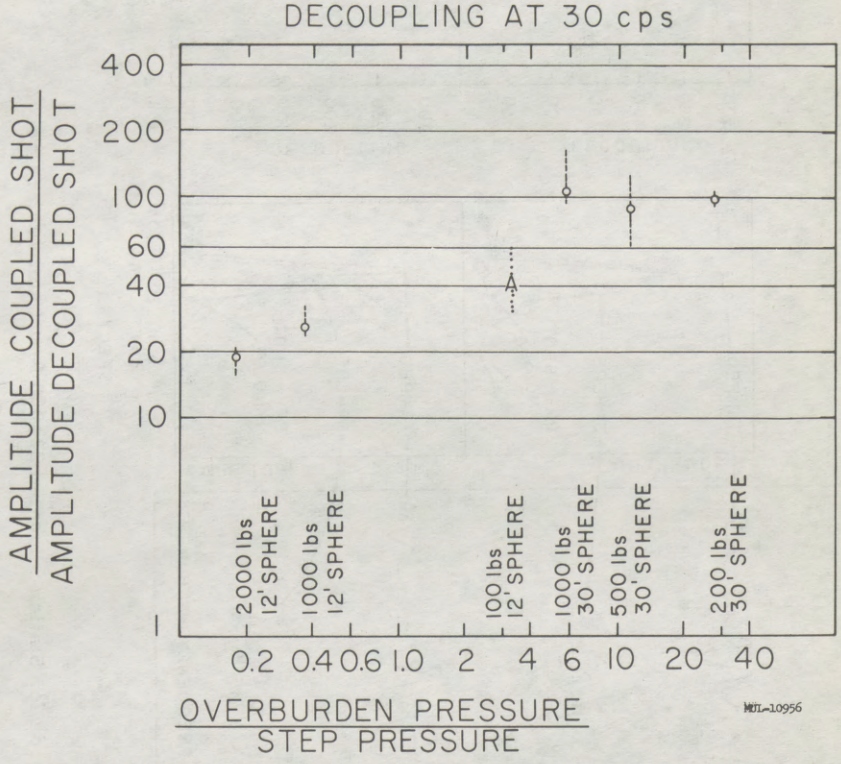


Fig. 10. Decoupling summary at the dominant frequency. The ratios of the mean values of the direct arrivals at Station 4 are given as circles indicating best values. The "A" indicates that the shot was recorded only at a temporary station, 5000 feet away. The pressure ratio must be greater than 5 for the Latter decoupling theory to apply.

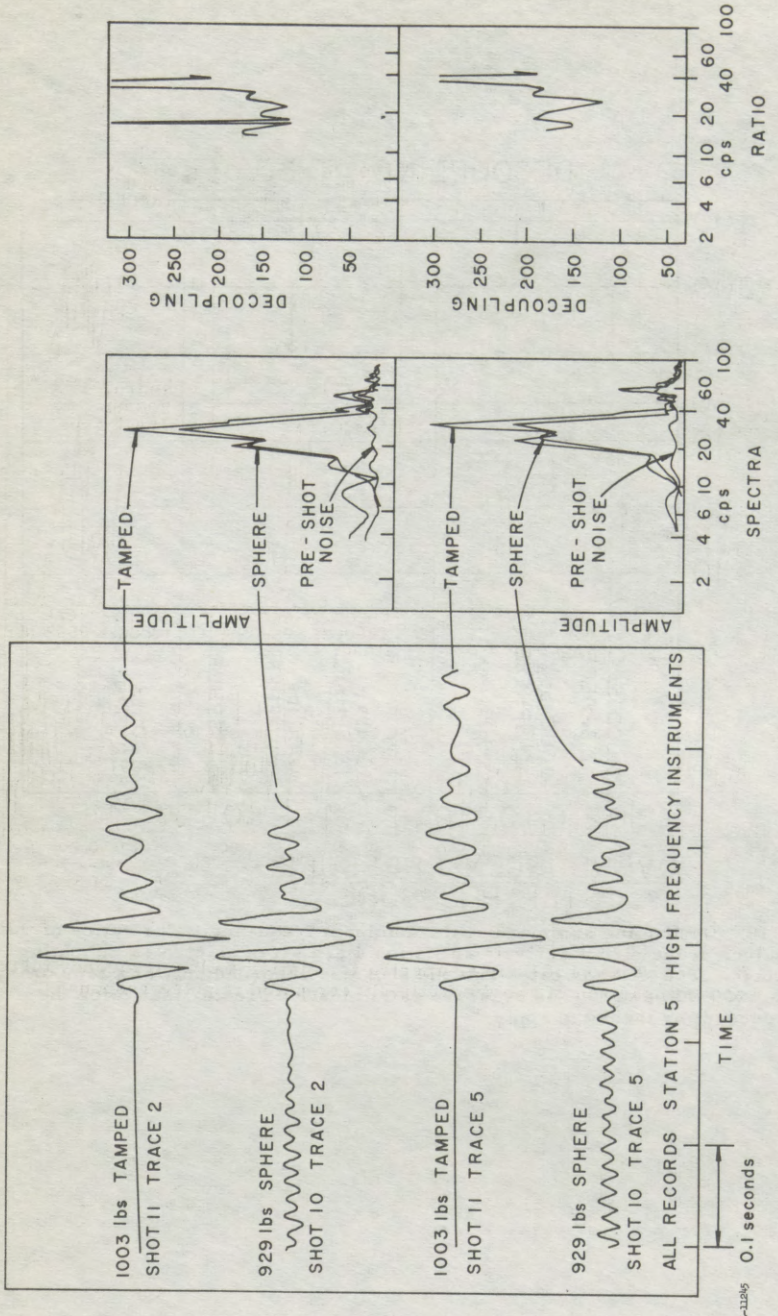


Fig. 11. Records for Station 5 located 22,000 feet from surface zero.

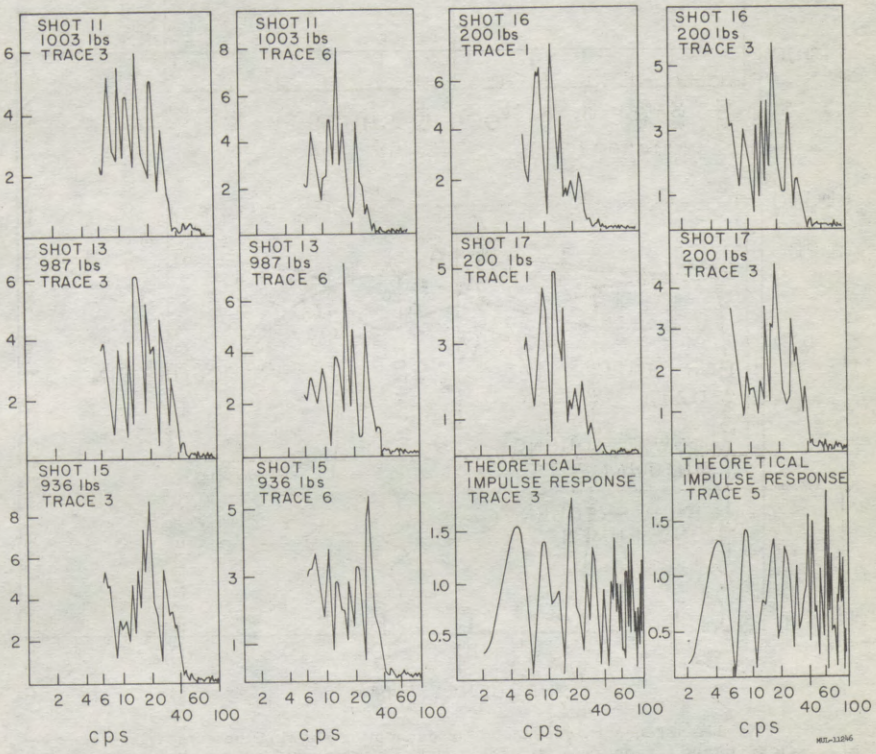


Fig. 12. Comparison of Fourier spectra for Station 4, low frequency instruments and a comparison of two theoretically computed impulse responses.

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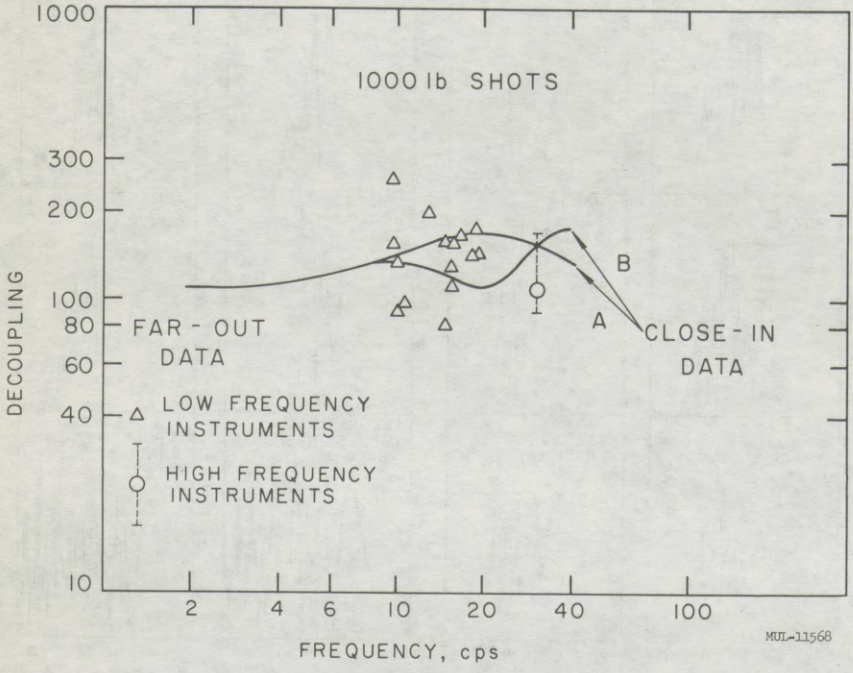


Fig. 13. Frequency dependence of decoupling showing the comparison of close-in and far-out data (shots 11, 1003 pounds and 10, 954 pounds). The high frequency instrument data has been replotted from Figure 10. The low frequency instrument data are from Station 4.

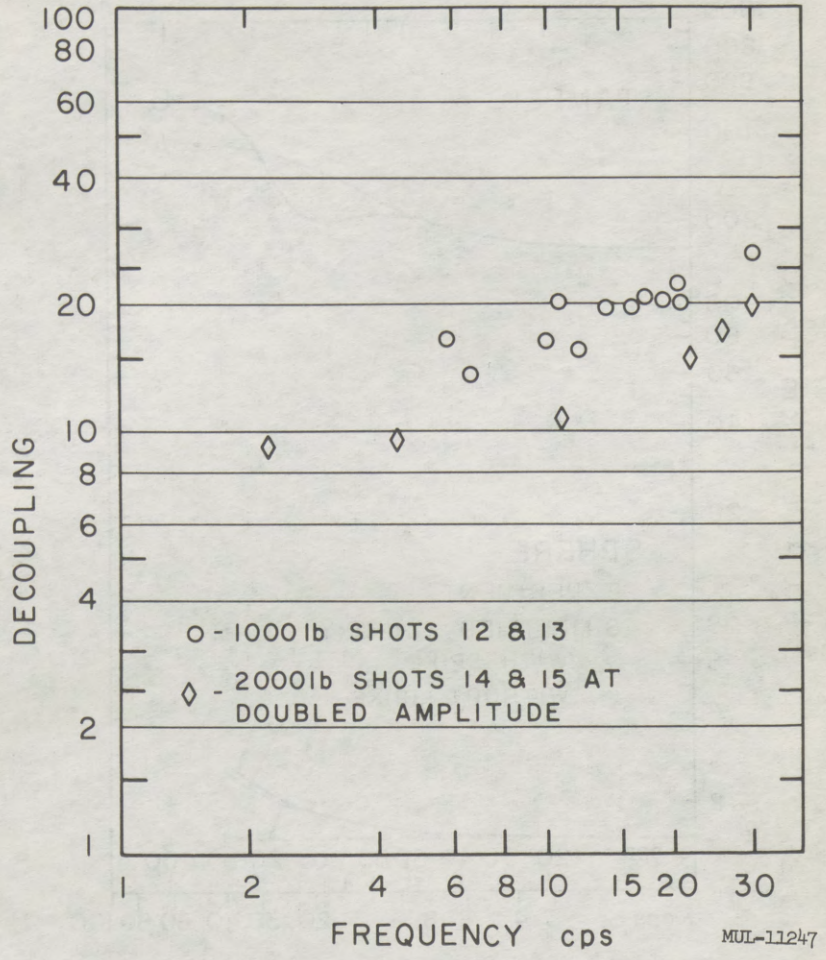


Fig. 14. Decoupling for overdriven spheres. The values at 30 cps are from Figure 9. Other values of shots 12 and 13 are from narrow band filtering of data taken from the low frequency instruments at Station 4. Values below 5 cps for shots 14 and 15 come from the Benioff seismographs at Stations 4 and 5. The remaining values are from the low frequency instruments at Stations 4, 5, and 6.

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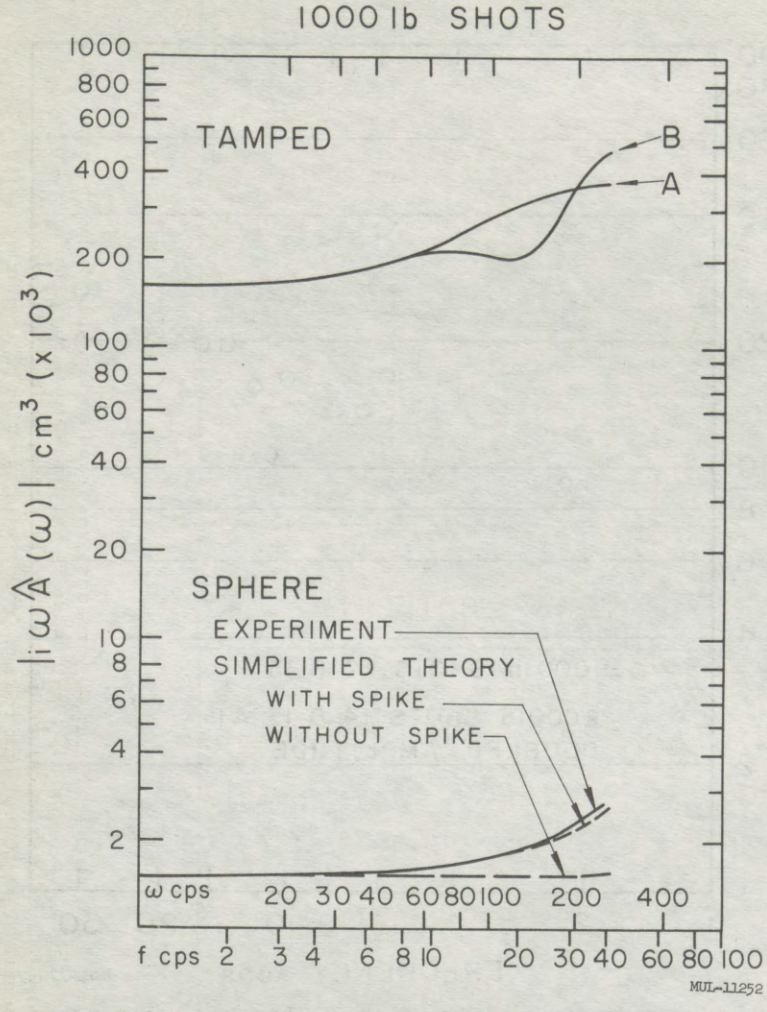


Fig. 15. $|i\omega\hat{A}|$ for the tamped and sphere explosions. The A and B curves are calculations from extrapolations of measurements at 80 feet (see Fig. 17). The 30-foot diameter sphere measurement is calculated from the pressure record at the sphere wall for shot 10. The theory is given by equations 8 and 15.

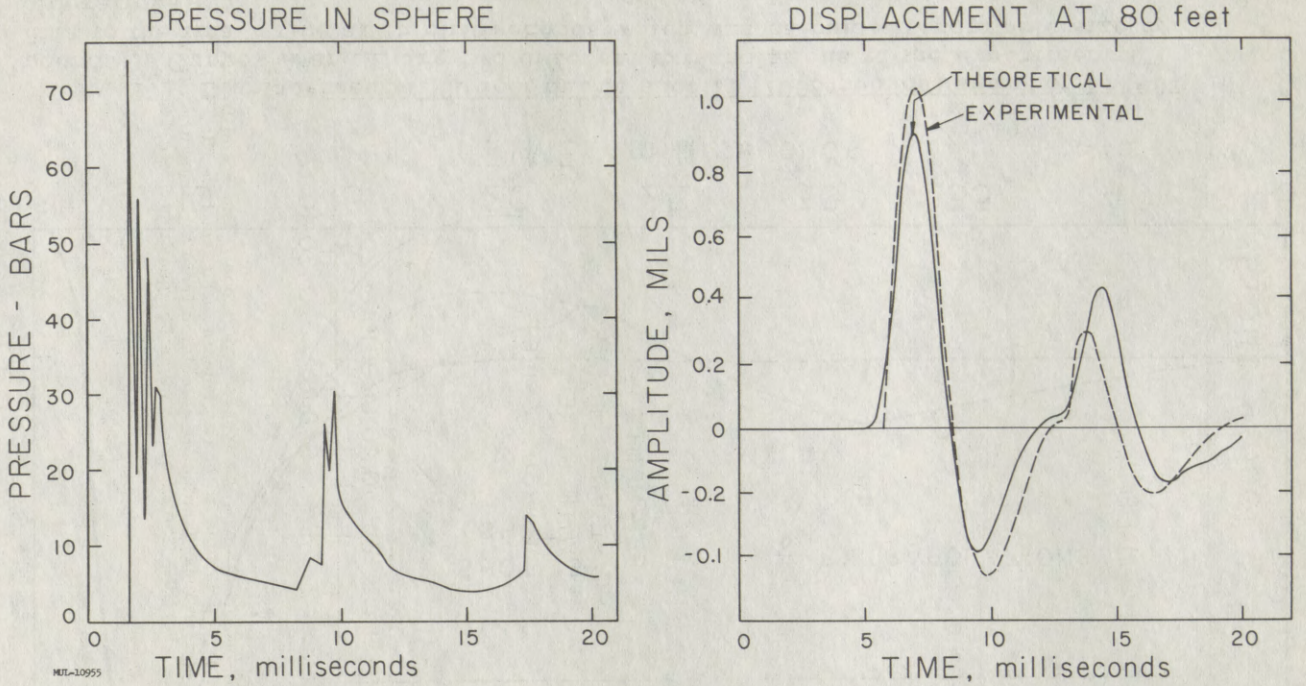


Fig. 16. The left panel shows the pressure recorded on the wall of the 30-foot-diameter sphere due to shot 10 (954 pounds). The theoretical curve in the right panel results from elastic calculations using the pressure function as input. The experimental curve at the right results from the integration of a velocity instrument record located 80 feet from the center of the sphere.

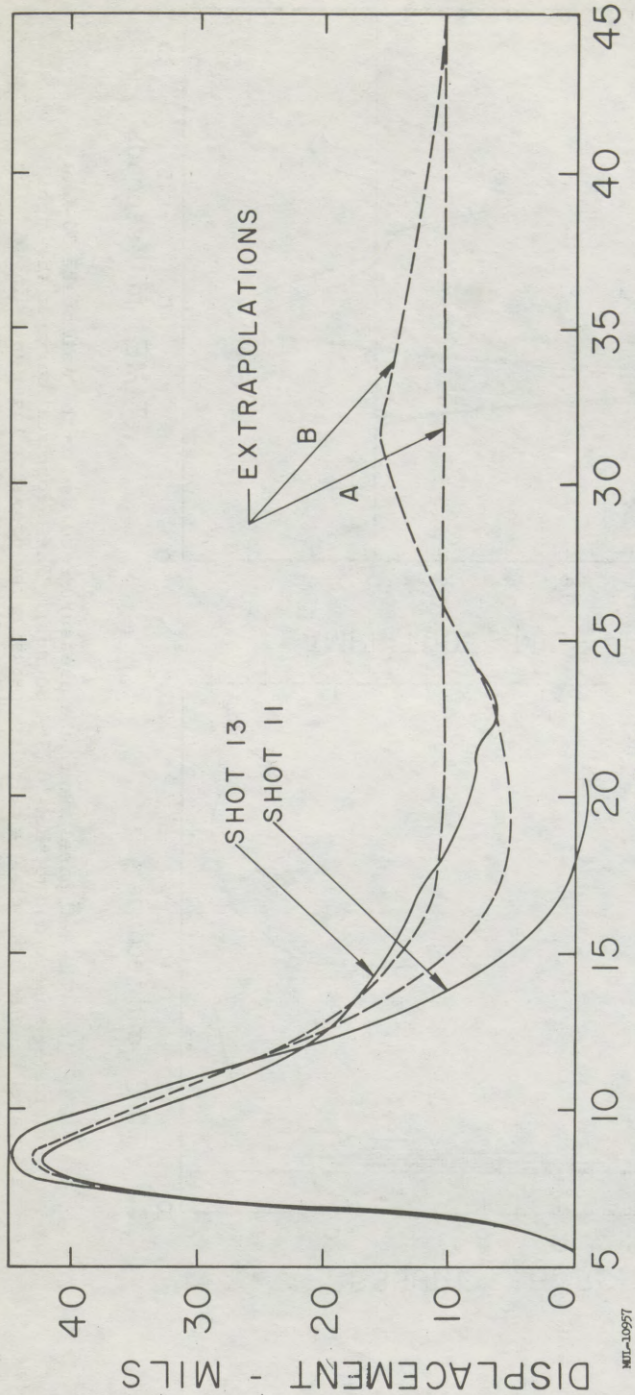


Fig. 17. Displacement at 80 feet due to shot 11 (1003 pounds) and shot 13 (987 pounds). Curves A and B are two different extrapolations of the experimental data to the final value at 45 milliseconds which was calculated from hole size considerations.

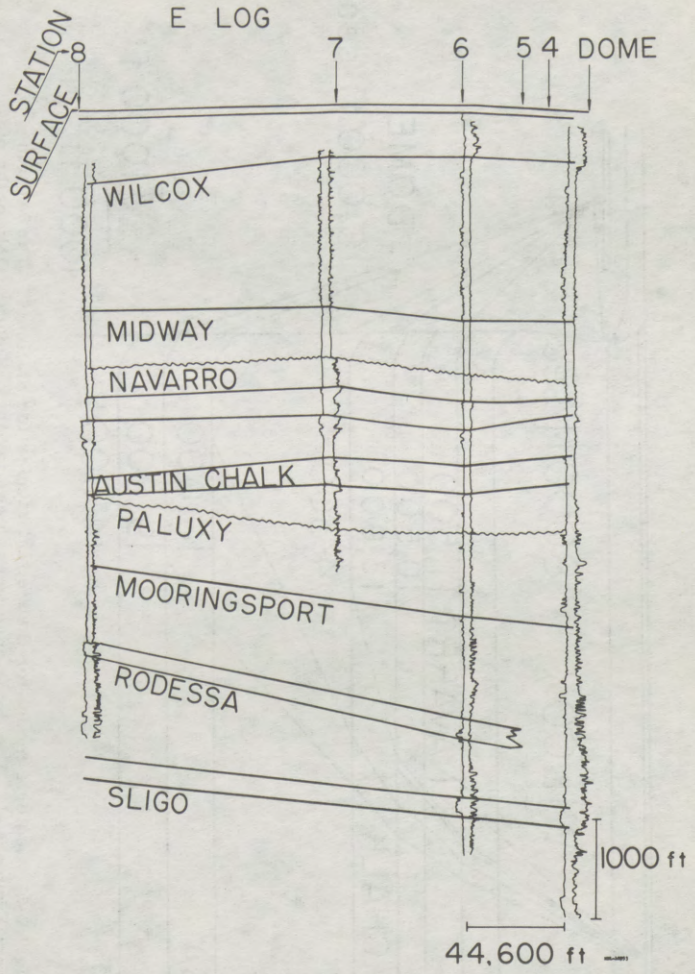


Fig. A-1. Electric Log Section along the line of seismic stations. Wells used left to right are Southern Prod. Co., #1 L. L. L. L. Co.; Fort Bend Oil Co., #1 Dunkleman; Hunt, #F-135 Goodpine; and Sinclair, #1 Louisiana Nebo. Correlations and formation identification have been drawn with reference to "Structural and Stratigraphic Cross Sections" of the Publication Committee of The Shreveport Geological Society.

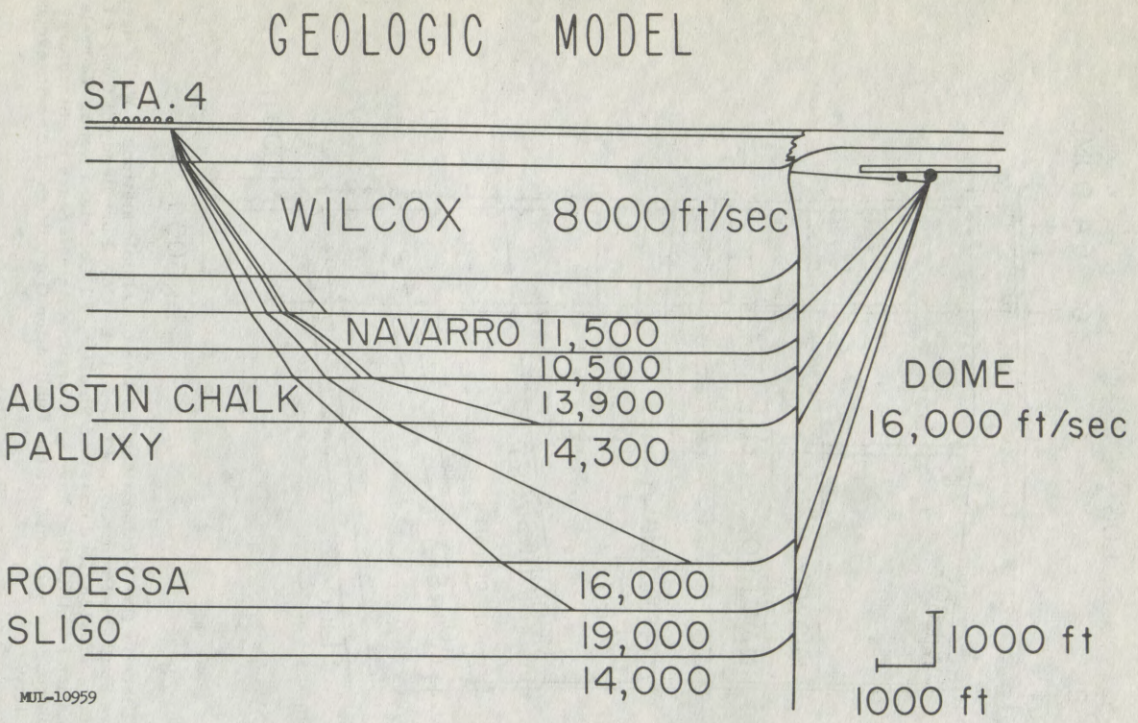


Fig. A-2. Geologic model for theoretical studies for Station 4. The layering is from Figure A-1. The dome edge is assumed for lack of data. Only paths of refracted compressional waves have been drawn. Waves that were converted to shear were also calculated. The rays eminate from the 30-foot-diameter sphere location. The dot to the left represents the tamped shot locations.

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SC, ABQ	SC-4440(RR)	Particle Motions near Explosions in Halite	B.F. Murphey et al.
USC&GS		Seismic Ground Effects from Coupled and Decoupled Shots in Salt	D.S. Carder et al.
APRL		1. Dynamic Strain Measurements in Salt – Part I 2. Dynamic Strain Measurements in Salt – Part II 3. Static Stress Measurements in Salt	L.A. Obert and/or associates
BuMines, PIT		Investigations Relating to Gas-Phase Detonations	R.W. VanDolah and/or associates
WES		Drilling and Grouting Support	J.M. Polatty
SRI	SRI-SU-2993-FR	Investigations of On-Site Inspection Techniques for High-Explosive Tests in a Salt Mine	R.B. Hoy and associates
SC, Livermore	SC-4823(RR)	High Explosives – Arming and Systematics	Lyle Hake
EG&G, BOS	EG&G-B2098	Timing and Firing of High Explosives	S.R. Hamilton et al.
LRL		Permanent Deformation	W.A. Hamilton et al.
LRL		Instrument Chart and Miscellaneous Engineering Information	R.B. Petrie et al.
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Agency	Report No.	Titles	Authors
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LRL	UCRL-6086	Seismic Decoupling for Explosions in Spherical Cavities	W.M. Adams & D.C. Allen
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APPENDIX 5

ADVANCED RESEARCH PROJECTS AGENCY COMPENDIUM OF CONTRACTS, NUCLEAR TEST DETECTION PROGRAM

Project Vela

UNIFORM

Title of work	Contractor	Agent	Project authorization No.	Contract No.	Date of contract	Contract expiration date	Amount
TASK I							
Worldwide standard seismological network	Geotechnical Corp	CQGS		USC&GS-747	Mar. 20, 1960	Oct. 5, 1962 [1]	$1,924,250
TASK II							
Focal mechanical and aftershock characteristics of small earthquakes.	University of California	AFTAC	VT-059	AF-49(638)-904	July 1, 1960	Dec. 31, 1962	405,374
Investigation of seismic wave propagation	University of Michigan	do	VT/066	AF-49(638)-911	June 1, 1960	May 31, 1962	600,000
Development and improvement of seismographs	Geotechnical Corp	do	VT/072	AF-33(600)-41824	June 15, 1960	June 30, 1962	115,304
Seismometer array data processing	Texas Instr	do	VT/077	AF-33(600)-41840	Oct. 12, 1960	Feb. 11, 1962	1,488,470
Field study of variation in characteristics of seismic noise and signals with geologic and geographic environment.	United Electro	do	VT/078	AF-33(600)-42048	Aug. 24, 1960	Aug. 24, 1962	396,481
Crustal studies of Iceland and Scandinavia	Uppsala University, Sweden	Army R. & D.		DA-91-591-EUC-1637	Oct. 28, 1960	Oct. 31, 1961	12,500
Theoretical seismology	Weizman Institute, Israel	AFSC	GRD-160474	AF-61(052)-509	Feb. 19, 1961	Feb. 19, 1962	121,122
Earthquake focus mechanisms	St. Louis University	do	GRD-160473	AF-19(604)-8054	Feb. 1, 1961	Jan. 31, 1962	6,950
Depth of seismic energy source	California Res	do	GRD-130878	AF-19-(604)-8344	Feb. 15, 1961	Feb. 15, 1963	230,000
Deep hole seismology	Jersey Prod	do	GRD-130877 130877 A and B.	AF-19(604)-8346	Apr. 1, 1961	Aug. 31, 1961	163,960
Analysis and reduction of Cowboy data	Texas Instr	do	GRD-130900	AF-19(604)-8078	Mar. 1, 1961	May 1, 1961	5,581
Ocean bottom seismometer	do	do	GRD-130891	AF-19(604)-8368	Mar. 15, 1961	Nov. 15, 1961	268,205
Electronic seismograph	Electro-Mechanics	AFSC	GRD-130893	AF-19(604)-8360	do	Mar. 15, 1962	30,493
Theoretical seismology	Columbia University	do	GRD-130892	AF-19(604)-8375	Apr. 1, 1961	Apr. 1, 1962	75,000
Dynamic properties of rocks—stress waves produced by explosions.	SRI	do	GRD-123617	AF-19(604)-8419	do	do	105,400
Ocean bottom measurements	Columbia University	do	GRD-80629	AF-19(604)-8357	do	do	173,120
Deep well seismology	Rensselaer Polytechnic Institute.	do	GRD-123616	AF-19(604)-8376	do	Mar. 31, 1963	110,300
Absorption of elastic waves	University of Oklahoma	do	GRD-123618	AF-19(604)-8416	May 1, 1961	Apr. 30, 1963	66,848
Long period seismometer	Dresser Electronics	do	GRD-123621	AF-19(604)-8472	Apr. 18, 1961	Oct. 17, 1961	18,000
Crustal study of west Canada	University of Alberta	do	GRD-123619	AF-19(604)-8470	May 1, 1961	Nov. 1, 1962	61,860
Three-dimensional seismic detection system	Century Geophysical	do		AF-49(638)-1084	June 1, 1961	Sept. 1, 1962	105,309

Pressure sensitive detection—deep hole seismology.	Century Geophysical (Tulsa)	do	GRD-80646	AF-19(604)-8454	May 1, 1961	June 15, 1961	6,066
Crustal studies in Manitoba	University of Saskatchewan	do	GRD-80648	AF-19(604)-8482	do	Nov. 1, 1961	14,000
Analysis of worldwide seismic data	Texas Instr	do	GRD-160315	AF-19(604)-8517	May 15, 1961	Aug. 14, 1962	348,234
Magnetic tape seismic net	Columbia University	do	GRD-80649	AF-19(604)-8485	June 1, 1961	June 1, 1963	292,200
Studies of earthquakes in foreign countries	University of Michigan	do	GRD-160317	AF-19(604)-8809	July 15, 1961	July 14, 1962	59,000
Classification of seismic phenomena	Armour Research	do	GRD-133725	AF-49(638)-1066	May 1, 1961	Apr. 30, 1962	65,296
Theoretical and model studies on EM signals	Space Electronics	do		AF-30(602)-2500	Apr. 17, 1961	Oct. 17, 1961	145,466
Study of earth as seismic-electric transducer	Century Geophysical	AFSC		AF-49-(638)-1085	June 1, 1961	Jan. 1, 1962	46,465
Attenuation of shock waves and spherical waves in an elastic-plastic medium.	SRI	do		AF-49-(638)-1086	June 15, 1961	June 15, 1963	222,880
Focal depth determination in distinguishing earthquakes from explosions.	Southern Methodist University	do		AF-AFOSR-61-137	June 1, 1961	June 1, 1963	63,572
Seismogram analysis	U.S. Coast and Geodetic Survey	do			June 10, 1961	June 10, 1963	
SH-type waves generated by small explosions	St. Louis University	do	80276	AF-19(604)-7402	Aug. 1, 1960	July 31, 1963	31,787
Seismic-energy distribution; model studies	Pennsylvania State	do	151706	AF-19(604)-7383	Sept. 1, 1960	Aug. 31, 1962	17,000
Seismic waves of long and intermediate periods	Columbia University	do	80274	AF-19(604)-7376	Aug. 1, 1960	July 31, 1962	400,000
Phase velocity and source determination	St. Louis University	do	80275	AF-19(604)-7399	do	do	284,084
Body-wave polarity studies	Massachusetts Institute of Technology.	do		AF-19(604)-7378	July 1, 1960	June 30, 1962	220,300
Feasibility study of underwater acoustics network.	N.E.L	BuShips			Dec. 21, 1960	July 31, 1961	145,000
Detection of radioactive particles from underwater explosions.	N.R.D.L	do			January 1961	January 1962	75,000
Hydroacoustic research—Project Jezebel	Bell Telephone Laboratory	do		Nobsr-85206	Feb. 28, 1961	Mar. 1, 1962	178,000
Crustal studies in California-Nevada area	In-house (primarily)	USGS			Dec. 30, 1960	Dec. 30, 1961	1,175,000
Electrical properties of earth crust	do	do			June 9, 1961	June 9, 1962	260,000
Crustal and upper mantle structures	do	do			Apr. 18, 1960	Continuing	310,700
Crustal structure research	University Witwatersrand, South Africa.	OSD		Grant SD-82-G1	2-year effort		148,000
Marine geophysics underwater sound from drifting Station Arlis II in Arctic Ocean.	Columbia University	ONR		Nonr-266(82)	Apr. 1, 1961	Mar. 31, 1963	124,470
Applicability of hydrodynamic theory to high-pressure flow in solids.	SRI	DASA		DA-49-146-XZ-095	June 15, 1961	Aug. 15, 1962	73,777
Research to develop a transducer system for measuring pressure in solids and rocks.	do	do		DA-49-146-XZ-096	do	do	97,660
Phenomena of explosions sources	Sandia	do		DASA-EO-300-61	May 1, 1961	May 1962	200,000
Early deformation from underground explosions	AM&F	do		DA-49-146-XZ-088	June 1, 1961	July 31, 1962	32,071
Investigation of sheer and compressional wave generation and effects of seismic impedance matching in 2 or more media.	Bureau of Mines (APRL)	do		DASA-EO-301-61	Apr. 26, 1961	Apr. 26, 1962	55,000
Emissions from underground explosion	Engineering-Physics	do		DA-49-146-XZ-089	June 1 1961	June 1, 1962	35,057
Analysis of deep-sea seismic records	University of California (Scripps).	AFSC	VT/062	AF-49(638)-905	Apr. 1, 1960	Apr. 30, 1962	418,064
Study of earth's crust in selected area	U.C.L.A.-California Tech	do	VT/064	AF-49(638)-912	June 1, 1960	May 31, 1962	100,675
Propagation of seismic waves in inhomogenous earth.	National Engineering Science	do		AF-49(638)-1082	June 15, 1961	June 15, 1963	123,744
Seismic surface wave studies	California Tech. (includes 85K, detection).	do	VT/068	AF-49(638)-910	May 19, 1960	June 30, 1962	395,193
Factors limiting seismic detection sensitivity	RCA	do		AF-49(638)-1089	June 15, 1961	Sept. 15, 1962	75,491
Study of EM signals, underground explosion	EG&G	AFSC		AF-30(602)-2546	June 1, 1961	May 31, 1962	178,293
Seismic data analysis	University of Michigan	do		AF-49(638)-1078	May 26, 1961	Dec. 31, 1961	113,000
Auditory recognition of seismic signals	do	do		AF-49(638)-1079	May 16, 1961	Jan. 1, 1963	68,000

[1] Estimated.

Project Vela—Continued

UNIFORM

Title of work	Contractor	Agent	Project authorization No.	Contract No.	Date of contract	Contract expiration date	Amount
TASK III							
Wichita Mountain Seismological Observatory	Geotechnical Corp	AFTAC	VT/036	AF-33(600)-41318	Apr. 1, 1960	May 1, 1962	$218,939
Berkner-type control post and systems development field station.	United Electro	do	VT/070	AF-33(600)-41318	Sept. 15, 1960	Continuing	238,663
Variation of seismic noise with depth and geology	do	do	VT/1123	AF-33(600)-42858	Mar. 8, 1961	Nov. 15, 1961	50,228
Geneva-type seismic detection network	Texas Instr	do	VT/1124	AF-33(600)-43486	June 15, 1961	Continuing	[1]2,700,000
Deep well modular coil inertial seismometer	United Electro	do	VT/1129	AF-33(600)-42890	Mar. 17, 1961	do	140,000
Deep well variable reluctance seismometer	Geotechnical Corp	do	VT/1139	AF-33(600)-43369	May 5, 1961	May 5, 1962	96,713
TASK IV							
Surface motion photography	Sandia and EG&G	AFTAC	VT/194		Oct. 7, 1960	Lollipop plus 90 days	42,000
Lollipop microbarographic measurements	Sandia	do	VT/185		do	do	287,000
Digital recording and analysis of data	Texas Instr	do	VT/190	AF-33(600)-42411	Oct. 24, 1960	do	31,577
Surface electromagnetic waves	Sandia	do	VT/198		Oct. 7, 1960	do	43,300
Subsurface electromagnetic waves	EG&G	do	VT/197	AF-33(600)-42403	Oct. 1, 1960	First shot	17,376
Particle motion study	Sandia	DASA		DASA 1.1	November 1960	Continuing	514,000
Close-in earth motion	SRI	do		DASA 1.2	Jan. 1, 1961	do	675,377
Photography of surface motion	EG&G	do		DASA 1.3	do	do	60,860
Strong motion seismic measurements	USC&GS	do		DASA 1.4	November 1960	do	101,046
Permanent displacement survey	Holmes & Narver, Inc	do		DASA 1.6	do	do	22,500
Spectra of strong motion—reed gage	STL	do		DASA 1.7	do	do	47,700
Grouting support	Waterways Experiment Station.	do		DASA 9.1	do	do	85,100
Documentary still photography	Army Pictorial Center	do		DASA 9.2	[2]	[2]	27,000
Hydrodynamic theory and high pressure flow in solids.	SRI	DASA		DA-49-146-XZ-095	June 15, 1961	June 15, 1962	73,777
Pressure transducer for measuring shock wave profiles.	do	do		DA-49-146-XZ-096	do	do	97,660
Nuclear and chemical explosions	U.S. Coast and Geodetic Survey.	AFTAC	VT/053		Contract for each shot.		290,000
Earth current measurements	Space Electronics	do	VT/196	AF-33(600)-42406	Oct. 20, 1960	Lollipop plus 90 days.	38,941
Long range seismic measurements	Geotechnical Corp	do	VT/074	AF-33(600)-41694	June 6, 1960	Continuing	4,267,377
Shock and seismic instrumentation	LRL	do	VT/170		July 14, 1960	June 30, 1961	522,000

TASK V							
VESIAC	University of Michigan	OSD		SD-78	Dec. 21, 1960	Dec. 21, 1961	123,000
TASK VII							
Handbook for onsite inspection techniques	SRI	AFTAC	VT/086	AF-49(638)-954	Sept. 2, 1960	Dec. 15, 1961	128,688
Techniques for measuring soil differences	ERDL	do	VT/1150		April 1961	Continuing	25,000
Visual and photographic inspection	SRI	do	VT/1109	AF-49(638)-1016	Jan. 18, 1961	1 year after Orchid.	36,970
Correlation of reflectance properties of mid-latitude vegetation.	ERDL	do	VT/1152		April 1961	April 1962	30,000
Seismic noise monitoring and surface subsidence measurements.	SRI	AFTAC	VT/1111	AF-49(638)-1053	Feb. 15, 1961	Sept. 30, 1962	[1] 25,930
San Andreas aftershock investigation	do	do	VT/1132	AF-49(638)-1071	Mar. 9, 1961	Apr. 30, 1962	163,270
Onsite geochemical and gamma ray spectral measurements.	Geophysical Service	do	VT/1156	AF-49(638)-1083	June 1961	Oct. 1, 1962	100,000
Stereophotogrametric techniques	EDRL	do	VT/181-182		Oct. 5, 1960	Oct. 5, 1961	145,000
Shock induced solid state effects	U.S. Geological Service	do	VT/1157		May 1961	Augusst 1962	120,000
Analysis of data for onsite inspection	Roland Beers	do	VT/1133	AF-33(600)-43368	April 1961	April 1962	28,000
Seismic profiling Ranier Mesa and Lollipop site	United Electronics	do	VT/179	AF-33(600)-42384	Oct. 21, 1960	Lollipop plus 90 days.	98,969
SIERRA							
Atmospheric fluorescence techniques	EG&G	AFTAC	VT/166	AF-33(600)-41920	Oct. 28, 1960	Sept. 15, 1961	$275,200
Resonance scatter of sunlight	Geophysical Corp. of America	do	VT/177	AF-33(600)-42657	Feb. 3, 1961	January 1962	49,027
Detection by absorption of cosmic noise	SRI	do	VT/178	AF-49(638)-989	Nov. 30, 1960	Nov. 30, 1961	156,298
Phase sounder	do	do	VT/178	AF-49(638)-989	Apr. 15, 1961	Apr. 15,1962	80,136
VLF ionospheric disturbance detection	do	do	VT/1148	AF-49(638)-1081	May 11, 1961	Aug. 14, 1962	72,166
Sferics studies	NBS	do	VT/184		Sept. 30, 1960	Sept. 30, 1961	90,000
Hi-altitude electrostatic and gamma measurement	ACF	do	VT/1108	AF-33(600)-42470	Apr. 18, 1961	Sept. 5, 1961	15,949
System of ground stations	GE-Tempo	do	VT/193	AF-33(600)-42748	Jan. 18, 1961	Jan. 12, 1962	233,339
ELF-VLF monitoring of tests	Aero-Geo-Astro	do	VT/1116	AF-33(600)-42595	Dec. 20, 1960	Oct. 27, 1961	75,000
Time dependent light scattering	University of Rochester	do	VT/1137	AF-49(638)-1072	Apr. 1, 1961	Mar. 31, 1962	46,000
Development of photomultipliers	CBS Laboratories	do	VT/1149		April 1961	Feb. 24, 1962	42,337
EM signals from nuclear detonations in space	Republic Aviation	do	VT/1147	AF-33(600)-43462	May 25, 1961	Nov. 25, 1962	68,000
Back-scatter antenna design study	Granger	do	VT/1110	AF-33(600)-43122	Apr. 19, 1961	Apr. 18, 1962	57,166
Geomagnetic and magnetohydrodynamic work	New York University	ASC	39522-PM-1-91-91 (8753).	DA-36-039-sc-87171	Oct. 20, 1960	Oct. 20, 1962	38,212
VLF propagation effects	NEL	BuShips	M4-1		Sept. 22, 1960	June 30, 1961	247,300
Abnormal ionospheric behavior	Central Radio Propagation Laboratory.	NBS					500,000
Earth current and magnetic field measure	University of Texas	ONR		Nonr-375(14)	Apr. 1, 1961	May 31, 1962	[1] 54,000

[1] Estimated.

[2] Not available.

Project Vela—Continued

HOTEL

Title of work	Contractor	Agent	Project authorization No.	Contract No.	Date of contract	Contract expiration date	Amount
Technical direction and systems engineering	Aero Space	AFSC			Continuing		$358,000
Piggyback experiments	LRL	AEC					1,500,000
Environmental test satellite	General Dynamics Astronautics.	OSD			Nov. 7, 1960	Dec. 30, 1963	1,998,000
Prelaunch inspection studies	NRL	OSD			Continuing		25,000
Radio frequency pulse detection	Harvard College Observatory	AFCRL			do		173,000
Piggyback systems integration	Lockheed	AFSC			do		485,000
Prelaunch inspection studies	Bendix	AFSWC			do		47,000

APPENDIX 6

THE WHITE HOUSE,
Washington, D.C., September 12, 1961.

HON. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
The Capitol.

MY DEAR MR. CHAIRMAN: Enclosed are extracts from the verbatim records of the Conference on the Discontinuance of Nuclear Weapon Tests. These records concern the seismic research program and cover the period from March 21st to April 27, 1961. Except for the discussion on August 28, 1961, these extracts disclose the United States-United Kingdom and Soviet positions on all significant aspects of the research program. I am also enclosing the entire transcript of the August 28, 1961, meeting at which the program was also discussed.

After your hearings on the VELA program, it was suggested to members of your staff that these records be included in the hearing record. I think they might be a useful reference for the members of the committee and for many others who will read the hearings.

Best regards.

Sincerely,

ADRIAN S. FISHER,
Deputy to the Adviser to the President on Disarmament.

EXTRACTS FROM THE VERBATIM RECORDS OF THE CONFERENCE ON THE DISCONTINUANCE OF NUCLEAR WEAPONS TESTS CONCERNING THE SEISMIC RESEARCH PROGRAM

March 21, 1961, to April 27, 1961

(7) Statement by the United States Representative (Dean) March 21, 1961:

"My hope about the success of our work stems from my belief that each side here is returning to work with a full recognition of the momentous issues involved. During the past two months the United States Government has had every aspect of these negotiations under the most intensive review. We considered in great detail the position of each delegation on every issue, major and minor, and then, after careful study and reflection, we reached decisions on what might be offered here in an attempt to resolve these issues. Throughout we have consulted with the United Kingdom Government and have now arrived at a common position. The specific proposals which I shall now set forth are, I am happy to say, in every sense the joint proposals of the United Kingdom and United States delegations.

"At this first meeting I shall outline our position on the major questions. The necessary elaboration, as well as a review of other problems, will follow at subsequent meetings. If we can settle these major issues in constructive ways we shall be far along toward concluding a treaty. I shall now outline these major positions, as follows.

"First, one of our first projects was to review the United States seismic research programme as put forward in Geneva last May, to see where we might reasonably accommodate ourselves to the views of the USSR. We now believe that a somewhat longer period is needed for the required research than the two years originally estimated. We are also ready to assume now that for all practical purposes the official starting date of the research programme will coincide with the signature of our treaty, assuming of course that negotiations can be concluded in the relatively near future. However, it is still possible that we may wish to seek the agreement of the USSR to an earlier starting time for the seismic research programme. As regards the content of the programme, our scientists have found no basis for changing this in any significant respect from that which we outlined last May.

"Since we have previously said that the research programme and moratorium period should be coterminous we now propose that the moratorium should have a duration of three years. As under our previous plan, the last few months of this three-year period would be utilized for consultations among the three of us to determine what should be done with regard to maintaining, changing or eliminating the threshold of seismic magnitude 4.75 after the moratorium and its attendant limitations on the freedom of action of each of us are due to expire. The moratorium would, of course, become effective on the date of treaty signature.

"Second, on a related issue, namely, on the safeguards which are to surround the explosion of nuclear devices in the seismic research programme, I can say

that the Western Governments are willing to follow in general outline the four safeguard steps proposed by the USSR at the two hundred and fourteenth meeting on 15 June, 1960.

Indeed, our two Governments have long been in general accord on some of the four steps. Please note that I use the words "in general" because the full details of these steps have not been spelled out by the USSR.

"Although our move, taken in order to reach agreement, involves an unprecedented step in this age of heretofore complete secrecy on nuclear devices, I am able to inform you that if agreement on other treaty provisions is in sight, the President of the United States is prepared to request appropriate action from the Congress in regard to the Atomic Energy Act so as to allow the United States to show the Soviet Union the internal mechanism of the nuclear devices to be used in the seismic research programme.

"This is indeed a forward step in our negotiations. In return we ask only that the Soviet representative confirm his indication made at the two hundred and fifty-fourth meeting on October 13, 1960 that, if the Soviet Government should, pursuant to a seismic research programme, decide to detonate its own nuclear research devices, it would permit us the same inspection procedure which the United States is now proposing. While on this subject, both Western representatives would like to urge their Soviet colleague to have his Government reconsider its decision of June last not to implement the seismic research programme planned by Soviet experts and communicated to their Western colleagues during May 1960.

"We are certain that the USSR could make a valuable contribution to the efforts of the two Western countries. A Soviet seismic research programme would clearly be a sign of Soviet good will and readiness to cooperate in what will have to be a great range of common endeavours involved in implementing this treaty. It could be an extension of the type of coordinated action included in the International Geophysical Year and would add much to the sum total of scientific knowledge.

"This is quite apart from the participation, which will occur in any case, of Soviet scientists in the carrying out of the United Kingdom and United States seismic research programmes. That participation will serve a useful purpose. If, at the end of the research programme all three Powers are in general agreement on what has been achieved by that programme, it will help each of us to decide what to do at the end of the moratorium period" (GEN DNT PU 274, pp. 19–21).

(8) Statement by the United States Representative (Dean), March 22, 1961

"Today I should like to consider the closely related items of a seismic research programme, safeguards for nuclear research detonations, and the moratorium on underground nuclear weapon tests producing a signal of lower than seismic magnitude 4.75.

"First, it is useful to remind ourselves of the way in which these problems came before this Conference. As representatives will recall, the United States proposed on February 11, 1960 that the treaty should not attempt to cover all underground nuclear tests since the technical capabilities of control now appear inadequate to monitor such an obligation. Therefore, in the first phase of the life of the treaty the United States would be ready to ban only those underground nuclear weapon tests which produce seismic signals equal to or greater than a seismic magnitude of 4.75. This magnitude of 4.75 was estimated to be the approximate size which the proposed control system could be expected to monitor in a reasonable effective manner. To make possible a downward extension of the treaty's scope to a lower threshold as rapidly as technically justifiable, a three-Power programme of seismic research would be instituted to develop improvements in control techniques.

"In reply the Soviet Union indicated on March 19, 1960 that it could not accept an arrangement under which some form of testing might be legally authorized at the very moment when the treaty would enter into force.

"However, it was willing to recognize United States anxiety with regard to better identification of small underground tests, and it was hence willing to join in a phased treaty having a threshold of seismic magnitude 4.75, plus a seismic research programme. The only condition to this was that all three Powers agree to a voluntary moratorium on small underground tests below the treaty threshold during the period in which the research programme would be under way. Thus the period of the research programme and the moratorium were to be coterminous.

"On 29 March 1960 Prime Minister Macmillan and President Eisenhower announced their general acceptance of the Soviet insistence on a moratorium provided, first, that research work got speedily under way, and second, that other treaty issues were satisfactorily settled.

"On 3 May 1960 the Soviet delegation declared that the USSR would agree to have the research programme include a limited number of nuclear detonations as long as proper safeguards were adopted to ensure that the country detonating the nuclear device could not use the explosion to gain militarily useful nuclear weapon information.

"Second, although Soviet scientists at the Seismic Research Programme Advisory Group in May 1960 announced plans for a separate two- to three-year seismic research programme in the USSR, we knew from Mr. Tsarapkin's statements in June 1960 that his Government did not approve of these plans, and that therefore, at least as of then, the Soviet Government would not conduct its own research programme. Although, as I stated yesterday, we sincerely hope that the USSR will reverse this decision and reinstitute a seismic research programme of its own, we must assume, until we are told to the contrary, that the only active seismic research programmes are those which are being prepared by the United Kingdom and the United States, and which we are prepared to go forward with and to complete in three years' time.

"The Soviet representative, has, however, informed his Western colleagues that the USSR expects to participate in the United States and the United Kingdom programmes from the beginning, i.e. that the USSR deems it essential that it join in the elaboration and implementation of the Western programmes and in the analysis of the data resulting from them.

"As far as the United States is concerned, we have made it clear that we welcome constructive Soviet participation in carrying out the full United States seismic research programme and not merely those portions of it relating to nuclear research detonations. Moreover, we intend to publish fully all data and analyses obtained from the seismic research programme. Further, we would want to receive all data recorded on Soviet instruments, as well as Soviet analyses and interpretations of this new information.

"We share the hope expressed last summer by Mr. Tsarapkin that this type of cooperative work will result in agreed evaluations of the technical situation revealed by the research programme and will avoid a repetition of the Soviet-Western split on these problems epitomized by Technical Working Group 2.

"Third, in relation to Soviet participation in the elaboration of the contents of the United States programme, however, we had the benefit of expert Soviet evaluation and criticism last May. Wherever we felt that criticism to be valid we made corresponding changes in details of our programme. While we welcome such Soviet participation, we do not recognize any unilateral Soviet right to insist on altering the contents of our own programme.

"Aside from this, we, for our part, are rapidly expanding our seismic research capability. A sizeable amount of research effort is actually under way at the present time. We are thus taking major steps in preparation for our full research programme so that maximum progress and speed will be possible, without loss or delay, once the programme formally begins, presumably with the signing of the treaty.

"Most of our exchanges on the subject of the programme's content have finally come down to differences of view on the nature and exact number of nuclear research detonations which should be included in the programme. We have long acknowledged that the USSR has one legitimate concern with such nuclear detonations. That is to make certain that the United States cannot obtain weapons development information from any detonations which it conducts in its seismic research programme.

"I shall deal with this matter of safeguards more fully in a few minutes, but now let me be explicit. Truly, I cannot see why Soviet concern should extend beyond this one area. After all, the proposed United States nuclear detonations are all for the sole purpose of learning more about seismic phenomena resulting from underground nuclear explosions set off under different geological and structural conditions. We are interested in the acquisition of knowledge essential for improving controls and for eliminating existing gaps in our scientifically designed controls which serve as inducements to potential treaty violators. Possible methods to overcome the effects of using decoupling techniques must be explored along with all other pertinent problems.

"Fourth, except for the protests over the contents of the United States seismic research programme—protests which I trust the Soviet delegation will soon

withdraw—I should note that the lengthening of the planned period of execution of the United States programme from two years, as formerly, to three years including an evaluation period, as I announced at the two hundred and seventy-fourth meeting (GEN/DNT/PV. 274, p. 19), makes this programme virtually identical in time span with the programme once planned by the Soviet scientists.

"The United States research programme has been developed by our scientists following the discussions of last May with scientists of the other countries represented at this Conference. Our experts advise us that three years is sufficient to accomplish the research proposed, and will result in the information needed to make decisions with regard to the appropriate threshold for control of underground tests. Mr. Tsarapkin should, therefore, be satisfied both on this point and on our simultaneous concurrence in the idea that the official start of the programme should, in normal circumstances, be timed to coincide with the signature of the treaty.

"In this connexion, it is worth mentioning, incidentally, that the fact that we are now willing to use this latter date, as a working hypothesis, for initiating research does not imply that we no longer think that it might be useful to commence research work even earlier, should that be possible. We still believe this. But we have accommodated the Soviet Union on this point too on the postulate that these negotiations are to proceed rapidly to a conclusion and that, therefore, the time of treaty signature and the coincidental start of research will not be much delayed in any case. We might, however, still wish to seek Soviet agreement to an earlier starting date for the research work.

"Fifth, it is indeed a relief for my delegation, and I presume for the others as well, to be able to assume that we have all, at last, surmounted the thorny issue of the safeguards surrounding research detonations. However, this is a matter worth looking at in some detail now so that we can be sure of exactly where we stand.

"Going back to the four requirements set forth by Mr. Tsarapkin at the two hundred and fourteenth meeting on 15 June 1960, I shall review them item by item as they would apply to a device to be exploded by the United States.

"First, the USSR wishes to have a full description and the blueprints of the structure of the device made available beforehand to representatives of the United Kingdom and the USSR. Those representatives would then be allowed to inspect the internal and external structure of the device. As to this, I can say that the United States is quite willing to allow the United Kingdom and Soviet Representatives to study blueprints for the purpose of facilitating the inspection, although not to copy them or take them away from the inspection area. In addition, those representatives will be able, at the same time, to examine the device inside and out. By this we have in mind that all separate components of the device can be examined and inspected, but not that any of them can be so handled or, indeed, hammered and hit or used in such a manner as to make it unusable thereafter in the device to be detonated.

"The second Soviet point is that United States, United Kingdom and USSR representatives must be present at the place of assembly and explosion of the device. Our understanding of this is that once the device had been broken down for inspection, as described above, it would be reassembled in the presence of Soviet and United Kingdom representatives and would not thereafter be withdrawn from their surveillance until actually exploded.

"Thirdly, the USSR wishes to be sure that instruments and measurment apparatus can be installed at the place of the explosion by the representatives of the United Kingdom and the USSR ,as well as those of the United States, and fourthly, it wishes to be sure that all of the data obtained will be made available to all three countries.

"We are, of course, quite ready to reaffirm our past agreement on these points.

"In connexion with these four points I should like to make a general observation. As I said in my statement on 21 March 1961 (GEN/DNT/PV. 274 p. 20), appropriate action with regard to the Atomic Energy Act to authorize the opening up of a United States nuclear device to internal inspection by Soviet representatives will have to be obtained from the United States Congress. Assuming that other aspects of our talks here proceed favourably, President Kennedy is prepared formally to seek this action from Congress at some time prior to the date now anticipated for officially starting the research programme.

"Sixth, the only subject remaining for me to mention today is the moratorium. As I declared on 21 March, the United States and the United Kingdom are now

ready to agree to a moratorium lasting for three years and, in normal circumstances, taking effect from the date of treaty signature. This is for a period approximately coterminous with the research programme. There is, of course, no reason for changing our already agreed understanding that the moratorium which will last for three years from treaty signature and be coterminous with the seismic research programme, will be made operative by simultaneous announcement by each of our three Governments. This would presumably happen on the date of treaty signature. I would suppose that we all also have it in mind that each other party adhering to the treaty would be encouraged to subscribe to the same moratorium when it deposited its instrument of accession." (GEN/DNT/PV 275, pp. 3–7).

(9) Statement by the Soviet Representative (Tsarapkin), the United States Representative (Dean), and the United Kingdom Representative (Ormsby-Gore), March 29, 1961.

"The CHAIRMAN (Union of Soviet Socialist Republics) (translated from Russian). I have still another question for the purpose of clarification. At the two hundred and seventy-fifth meeting the United States representative, speaking on the question of the research programme, said:

" 'Most of our exchanges on the subject of the programme's content have finally come down to differences of view on the nature and exact number of nuclear research detonations which should be included in the programme' (GEN/DNT/PV.275, p. 5).

"We know, of course, that there are such differences, and when the United States brought up for discussion at our Conference the question of the research programme it was implied—that is how we understood it and no other interpretation should or could be accepted—that the programme which the United States intended to carry out would be agreed upon with us in all those aspects about which I have already spoken in my interventions when this question was discussed. However, at the two hundred and seventy-fifth meeting from which I have taken the quotation Mr. Dean said:

" '* * * We do not recognize any unilateral Soviet right to insist on altering the contents of our own programme' (ibid., p. 4).

"I should like to have an explanation of the meaning of this sentence. Does it mean that the United States does not intend to discuss with us the substance of this programme and does not wish to reach agreement on it with us, but intends to conduct it unilaterally? We should like to have this made clear. If it turns out to be so, then the whole question of the research programme takes on a different complexion altogether. It will then appear that we have been talking different languages, and that of course cannot fail to have a deteriorating effect on our negotiations. I should like to ask Mr. Dean to clarify this point.

"Mr. DEAN (United States of America). What I said at the two hundred and seventy-fifth meeting was:

" 'Third, in relation to Soviet participation in the elaboration of the contents of the United States programme, however, we had the benefit of expert Soviet evaluation and criticism last May. Wherever we felt that criticism to be valid we made corresponding changes in details of our programme. While we welcome such Soviet participation, we do not recognize any unilateral Soviet right to insist on altering the contents of our own programme' (GEN/DNT/PV.275, p. 4).

"I went on to say further:

" 'We have long acknowledged that the USSR has one legitimate concern with such nuclear detonations. That is to make certain that the United States cannot obtain weapons development information from any detonations which it conducts in its seismic research programme.

" 'I shall deal with this matter of safeguards more fully in a few minutes, but now let me be explicit. Truly, I cannot see why Soviet concern should extend beyond this one area' (ibid., p. 5).

"I cannot possibly see why that should result in a deterioration in the negotiations.

"I had said earlier:

" 'As far as the United States is concerned, we have made it clear that we welcome constructive Soviet participation in carrying out the full United States seismic research programme and not merely those portions of it relating to nuclear research detonations' (ibid., p. 4).

"I submit that my statement, taken as a whole, is very clear and very specific. It seems to me that it should further our negotiations rather than cause them to deteriorate.

"The CHAIRMAN (Union of Soviet Socialist Republics) (translated from Russian). In this connexion, I should like to make a brief comment. Perhaps Mr. Dean, as a newcomer to our negotiations, is not fully informed of the substance of our dispute on the nature of the research programme and of the explosions.

"In this instance, when we raised this question, we had in mind the possibility of carrying out underground nuclear explosions in the research programme under decoupling conditions. But we know that the carrying out of experimental underground explosions under decoupling conditions is precisely that branch of research which is directed not towards devising more effective methods of control over underground nuclear explosions in order to ensure compliance with the treaty, but, on the contrary, towards devising ways and means of evading the treaty and best carrying out clandestine tests. We have never agreed to this branch of research, and cannot do so now. What we are working towards here is how to ensure compliance with the treaty, not how to evade it scientifically by carrying out clandestine explosions. The carrying out of underground explosions under decoupling conditions does in fact aim at devising methods of carrying out clandestine explosions. We have never agreed to this; we have not given our consent to it and will not do so. I should like you to take note of this.

"Mr. ORMSBY-GORE (United Kingdom). I should like to ask Mr. Tsarapkin a question. How will he deal with the problem if a party to the treaty decides to carry out a clandestine test by means of decoupling? How does the Soviet Union understand that the control system will catch such a violator? I know that the Soviet Union is very anxious to make the system as foolproof as possible. How will the commission and the organization be able to catch such a violator unless some research is done into the decoupling explosions?

"The CHAIRMAN (Union of Soviet Socialist Republics) (translated from Russian). The Government of the United States, the Soviet Union and the United Kingdom, the main nuclear powers, having signed the treaty are bound to comply with it honestly. That is the only way the Soviet Union conceives this matter. That is a necessary condition. If anyone wishes to violate the treaty, ways and means of evading it will always be found. But we believe that the negotiations are being conducted in good faith and that the treaty itself will be carried out in the same way.

"But if we take the path of research for the purpose of working out methods of conducting clandestine explosions so as to evade the treaty, there will never be any end or limit to such research. It would mean giving up altogether a treaty banning nuclear weapon tests.

"If the purpose of our treaty is the discontinuance of tests and our Governments sign it, we are bound to comply with it as a sacred duty. But to participate in working out scientifically based ways and means of conducting clandestine nuclear explosions is contrary to this purpose. I stress once again—there can be no limit and no end to that kind of research. New ways and means of carrying out clandestine explosions will always be found, and we do not agree to turn our efforts in that direction.

"Mr. DEAN (United States of America). What we want to do is study possible means, not of violation, but of detecting decoupled explosions. Decoupling is already technically established, and the only way to improve our system of detection is to study the problem. As I said at the two hundred and seventy-fifth meeting:

" 'After all, the proposed United States nuclear detonations are all for the sole purpose of learning more about seismic phenomena resulting from underground nuclear explosions set off under different geological and structural conditions. We are interested in the acquisition of knowledge essential for improving controls and for eliminating existing gaps in our scientifically designed controls which serve as inducements to potential treaty violators. Possible methods to overcome the effects of using decoupling techniques must be explored along with all other pertinent problems' (GEN/DNT/PV.275, p. 5).

"As you are well aware, when, as I hope, we signed this treaty, each of us would announce a moratorium on explosions below the seismic magnitude of 4.75, and it would also be a part of our seismic research programme to improve our ability to detect explosions below that yield. The whole programme would be directed to possibly lowering the threshold to which the treaty was to apply—if that seemed feasible at the conclusion of the seismic research programme. The whole programme therefore, seems to me to be in the spirit of advancing our knowledge and improving detection in this field, not that of promoting violations" (GEN/DNT/PV.280, pp. 9–12).

(10) Statements by the Soviet Representative (Tsarapkin) and the United States Representative (Dean), April 13, 1961.

"The CHAIRMAN (Union of Soviet Socialist Republics) (translated from Russian). I have only heard the explanation which has been given by the representative of the United States and the representative of the United Kingdom in the interpretation. The first impression is that this explanation more or less corresponds with the agreement which was reached between us. But before taking formal note of it I should like to study it in the verbatim record. Then, perhaps, we shall regard this question as settled.

"Today I should like to deal in somewhat greater detail with the question of the seismic research programme. We have already on a number of occasions stated the Soviet Union's position on this question. I shall repeat it briefly. The Soviet Government considers that the system of control elaborated and recommended by the technical experts in 1958 to ensure compliance with a treaty on the discontinuance of nuclear weapon tests is quite sufficient to enable us to conclude on that basis an agreement for the discontinuance of all nuclear weapon tests everywhere for all time. For this reason the Soviet Union does not see any need to carry out nuclear explosions for research purposes and does not intend, on its part, to carry out any such explosions. In view of the fact that the United States Government announced its intention to carry out such a research programme, including underground nuclear explosions, and that unless such a programme was carried out it would not agree to sign a treaty for the discontinuance of nuclear weapon tests, the negotiations being thereby threatened with collapse, the Soviet Government, in order to save the situation, consented to the United States carrying out an agreed seismic research programme, including nuclear explosions. In doing so, the Soviet Union proposed as an indispensable condition of this consent that the United States provide the other side with safeguards against these nuclear explosions being used for miiltary purposes, that is, for the purpose of improving already existing or of creating new types of nuclear weapons, and that this research would be conducted only with a view to improving methods of control over underground nuclear explosions and not with the aim of elaborating ways and means of carrying out clandestine explosions in evasion of the treaty.

"The United States Government, as can be gathered from Mr. Dean's statement, is now prepared to provide the Soviet Union with safeguards against the research detonations being utilized for military purposes and has declared its acceptance of the four conditions of control proposed by the Soviet Union over the underground explosions that will be carried out as part of the seismic research programme. But that is only one part of the question relating to the underground nuclear explosions in the seismic programme. There is yet another and also very important part of this program which the United States is obliged to agree with the Soviet Union. I have already mentioned this. It concerns safeguards against the underground nuclear explosions in the seismic programme being utilized for the opposite purposes, namely, for devising ways and means of concealing underground nuclear explosions, for devising ways and means of carrying out clandestine nuclear explosions in evasion of the treaty, in violation of the treaty. In this connexion, suspicion is aroused by the fact that the United States side is avoiding agreement on this programme with the Soviet Union and refuses to introduce into it changes intended to preclude the possibility of carrying out, within the framework of this programme, any research aimed at devising methods for the seismic camouflaging or concealing of underground nuclear explosions. The situation is just the opposite: the United States side is tenaciously insisting that nuclear explosions for the purpose of discovering methods of concealing nuclear weapon tests should be included in its proposed seismic research programme. The explosions which the United States is planning to carry out under conditions of seismic camouflaging or, as the scientists say, under decoupling conditions are designed precisely for this purpose.

"The Soviet Union cannot consent to the carrying out of nuclear explosions for these purposes. The endeavour to give such a direction to the research programme will result not in the improvement of methods of control over nuclear explosions but in the invention or discovery of ways of concealing them from control. And to tell the truth, we witnessed with mixed feelings Mr. Dean's attempt at the two hundred and eighty-seventh meeting to represent this as progress. We did not know how to take this remark of Mr. Dean's—was it a slip of the tongue on his part or a joke, a witticism, or should his observation be regarded as a pointer that the United States intends seriously to engage

in devising ways of carrying out concealed, clandestine underground nuclear explosions? Only those who intend to evade control, to evade the treaty, are striving to give such a twist to the matter. The Soviet Union cannot be a partner in this business.

"If one takes into account that the United States, on the one hand, advocates freedom of action for the nuclear Powers after the expiry of the moratorium, and, on the other hand, gives the research programme the aim of obtaining data which would confirm the attitude of those who are raising an outcry about the inefficacy of control in regard to underground nuclear explosions and for this reason are demanding freedom of action in respect of these explosions, one cannot fail to see that in this matter forces are at work which are striving to bring the negotiations to a standstill and to prevent the conclusion of a comprehensive treaty prohibiting all nuclear weapon tests for all time.

"The Soviet Government considers that the study of methods of concealing nuclear explosions—in the light of the United States demand that after the expiry of the moratorium the States are to have freedom of action—cannot be interpreted in any other way than as indicating the desire to prepare the ground for excluding certain explosions from the treaty and to bring about the conditions for the resumption of nuclear weapon tests.

"The Soviet delegation expresses the hope that the views of the Soviet Government which have been set forth above, concerning the direction and nature of the seismic research programme, will be taken into consideration by the Governments of the United States and the United Kingdom and that in future they will not insist on including in this programme nuclear detonations aimed at devising methods of seismic camouflage of underground nuclear explosions (decoupling). We consider that the carrying out of explosions under such conditions is contrary to the task of preparing as quickly as possible an effective system of control over compliance with an agreement on the discontinuance of nuclear weapon tests. In fact such explosions are aimed at justifying refusal of a comprehensive treaty banning nuclear weapon tests of all kinds.

"In concluding this exposition of our point of view on the question of an agreed seismic research programme, we cannot pass over in silence the statement which the United States representative, Mr. Dean, made at the two hundred and seventy-fifth meeting. He stated the following:

" 'While we welcome such Soviet participation, we do not recognize any unilateral Soviet right to insist on altering the contents of our own programme' (GEN/DNT/PV.275, p. 4).

"From what I have said today it should be clear to the United States representative what the considerations are which oblige us to insist on excluding from the seismic research programme nuclear explosions directed towards devising towards improving, not methods of control—that is, methods of detecting and identifying underground nuclear explosions—but methods of concealing, or camouflaging them.

"The carrying out of a programme of nuclear explosions touches upon one of the most sensitive aspects of contemporary political life; therefore we once again warn the United States of the serious consequences to which its arbitrary actions in that field may lead. We once again insist upon agreement in regard to the number and nature of the underground nuclear explosions in the seismic research programme, upon agreement in regard to its direction.

"As we have repeatedly stated in these meetings, the Soviet Union holds the view that the research programme must be a joint one, that is, it must provide necessarily for the participation of Soviet scientists in all research, from the preparation of an appropriate programme to the carrying out of research and experiments on the spot and the processing of the results.

"We consider that this should be done in our Conference here and now. We hope that the United States will display good will, and that we shall quickly be able to reach agreement on this important question."

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"Mr. DEAN (United States of America). We will, of course, study the verbatim record of the Soviet representative's remarks on the seismic research programme. However, while listening to the simultaneous interpretation it did not seem to me that he said anything new. His statement appeared to be merely a repetition of the constant what I might term political Soviet complaint against decoupling, despite the fact that the Soviet scientists readily agreed with the Unied Kingdom and United States scientists that decoupling was possible.

"I should like, however, to answer the Soviet representative's implication that I had said that the research programme would bring progress in decoupling. What I said at our two hundred and eighty-seventh meeting was:

" 'We cannot understand why, when all of us seek to make it possible to extend the area of the treaty and the control coverage the Soviet Union representative should oppose progress in this area of detection capabilities, especially when Soviet scientists have themselves recognized decoupling possibilities' (GEN/DNT/PV.237, p. 5).

"What I said at the two hundred and seventy-fifth meeting was that I regretted very much that the USSR had decided not to approve its scientists' announced plans for a separate two- to three-year seismic research programme in the USSR, and I asked that the Soviet Government give very careful consideration to a reversal of that decision in order that we might work with its scientists in a seismic research programme. It was with great regret that I heard the Soviet representative say again that it was not the intention of the Soviet Government to carry out that seismic research programme" (GEN/DNT/PV.289, pp. 7–10, p. 16).

(11) Statement by the United States Representative (Dean), April 27, 1961.

"Mr. DEAN (United States of America). I am sure that the Soviet representative noted at yesterday's meeting, the two hundred and ninety-eighth, the United Kingdom and the United States representatives concluded their explanation of the draft treaty which they tabled at the two hundred and ninety-second meeting as document GEN/DNT/110 and Corr. 1. We have now gone through all of the articles and annexes of the treaty, and we are awaiting, as we have been since 21 March last, a constructive Soviet response which, starting from the major and far-reaching provisions which we have proposed, will furnish a major impetus to our negotiations and lead to an early signature.

"While we have been focusing on the treaty text during the past ten days, we have not forgotten, of course, that there are to be certain arrangements covering matters closely related to the treaty, but dealt with in separate fashion. I am referring here to such matters as the agreed moratorium, which each of our countries is to announce unilaterally in regard to the underground environment below the treaty threshold, and the seismic research programme which is aimed at the eventual elimination of the treaty threshold. We have reviewed these questions on several occasions. The tabling of a treaty text does not of itself at all change the status of the items on which we have or have not yet reached agreement. I should like to summarize the situation as of the present time.

"First, as to the matters that have been settled at this table. Regarding the research programme to discover improvements in seismic detection and identification capabilities, the three of us are agreed that, in normal circumstances, it will start on the signature of the treaty. We all are in accord also that the programme shall contain some nuclear research detonations which will be carried out under safeguards, proposed by the USSR and concurred in by the United States as a major accommodation to the Soviet Union, even though those safeguards would involve unprecedented disclosures in the field of nuclear secrecy.

"The interpretations which the Soviet representative made at the two hundred and eighty-sixth meeting concerning the way in which the four safeguards procedures would be implemented seem generally satisfactory to my delegation. I would only make the observation that it would have to be understood that the safeguards procedures must not be implemented in any manner that would frustrate the purpose of the particular research detonation. There is also no disagreement on the idea that Soviet scientists will be welcome to co-operate in seismic research work conducted by the United States. It follows from this that they will also be welcome to collaborate in the analysis of all data obtained from the programme.

"Concerning the moratorium, we have all decided that it will start as from the date of the signature of the treaty. At that time it is to be announced unilaterally but in similar terms by each original party, and all other States adhering to the treaty will be invited to subscribe to it as well. There is agreement that the moratorium is to last for the duration of the research period; that is in our view three years from the date of the signature of the treaty. Before the moratorium expires, that is, in the last three to six months before its end, all three original parties are pledged to undertake consultations to see whether they can agree on common steps to be taken in regard to the below-threshold environment as of the time when the moratorium expires.

"Next, I must mention the matters that still have eluded settlement concerning the research programme and moratorium. A major problem has been the length of the research programme and, hence, the corresponding length of the moratorium period. We must emphasize again that the research work is being undertaken by the United States, and I submit that we are quite competent to judge how long it should take us to do our own work. The three-year period which we are now proposing is very similar to the time span suggested by the Soviet scientists to the Soviet Government for a research programme to be carried out in the USSR. Just because the USSR rejected the idea of doing research itself the force of the judgment of Soviet scientists that three years is adequate for the research programme is in no way lessened. The fact that we have allocated tens of millions of dollars and our ablest seismic researchers to our research effort is a guarantee that it will be well planned, well thought out, and complete.

"It follows from this that three years is also entirely sufficient as a moratorium period intended to be coterminous with the research work. Mr. Tsarapkin has asked why the United States seems to want to shorten this period. I shall be happy to repeat the answer which we already given him many times. By definition, the moratorium is to apply to an area below the treaty threshold of seismic magnitude 4.75 where no controls and no on-site inspections will apply and where, in our view, the control system does not now possess adequate possibilities. We are against uncontrolled obligations; we agreed to the Soviet proposal of a moratorium in March 1960 only as a means to help our negotiations move forward. There is every reason for keeping the moratorium as short as the objective requirements of research needs will allow. We have done that in as far and as scientific a manner as possible.

"Another subject on which there is disagreement is that of the content of the research programme. The USSR demands a total veto over what is contained in the United States programme. As a matter of principle we cannot accept the idea that the USSR can dictate to us what we may do with our own funds, our own scientists and our own plans. In practice, however, the real difficulty relates not to the great bulk of the programme—which was, after all, generally concurred in by the Soviet scientists in Geneva in May 1960—but only to the schedule of nuclear research detonations worked out by the United States.

"In particular, argument has persisted over whether explosions should take place under conditions of so-called seismic decoupling, which is a well accepted scientific fact. Mr. Tsarapkin seems unwilling to accept one simple fact. The procedure by which large underground cavities can be used as the sites for nuclear detonations so as to muffle drastically the resulting seismic signals is no longer just a theory: it has been proved in practice with small chemical detonations. Moreover, some natural cavities already exist which could be used for such explosions. In those circumstances it is clear that this method is already available to any potential treaty violator. Our research task now is to find control measures which stand a good chance of thwarting any such attempted violations. Once those measures have been found and are incorporated into the control system a significant measure of deterrence will have been created in this area also.

"I have been informed that United States scientists have a number of possibilities in mind for overcoming the control problem posed by decoupling. Some of these are the investigation of certain properties of long-period seismic waves, research into high-frequency seismic signals, and an examination of the propagation of shear waves, which may be eliminated by decoupled shots. In addition there is some chance that this type of detonation will disturb the earth's magnetic field or cause variations in existing tellurie currents. Moreover, it is desirable to examine the possibility of detecting at significant distances electro-magnetic pulses which may be generated by the blast.

"None of these research projects can be carried out and tested without detonating some nuclear devices under decoupled conditions. We simply cannot understand why the Soviet Union is opposed to scientific progress in control methods in this area. In view of the great role Soviet scientists play, I would urge Mr. Tsarapkin to adopt a realistic and scientific position in this matter and to eliminate it from the list of controversial questions" (GEN/DNT/PV.299, pp. 3–6).

Provisional
Private
GEN/DNT/PV.337
English

CONFERENCE ON THE DISCONTINUANCE OF NUCLEAR WEAPON TESTS—VERBATIM RECORD OF THE THREE HUNDRED AND THIRTY-SEVENTH PLENARY MEETING

Held at the Palais des Nations, Geneva, on Monday, 28 August 1961, at 3 p.m.

(Chairman: Mr. Tsarapkin (Union of Soviet Socialist Republics))

It is requested that any corrections to the originals of speeches contained in this record be submitted in triplicate (preferably incorporated in three of the single-sided mimeographed copies with which delegations are supplied) by the authorized Liaison Officer of the delegation concerned to the Documents Office, Room A.302. If not corrections are received within five days from the date of distribution, the final version will be prepared and issued on the basis of the provisionel record.

The time limit for corrections to this record will be 0900 hours on 3 September 1961.

LIST OF MEMBERS OF DELEGATIONS PRESENT AT THE TABLE

Union of Soviet Socialist Republics:
Mr. S. K. Tsarapkin
Mr. P. F. Shakhov
Mr. Y. M. Vorontsov

Union Kingdom:
Mr. D. Ormsby Gore
Sir Michael Wright
Mr. H. T. Morgan
Mr. D. H. Brinson
Mr. R. W. Snelling

United States of America:
Mr. A. H. Dean
Mr. C. C. Stelle
Mr. D. E. Mark
Mr. J. E. Goodby

Representative of the Secretary-General: Mr. T. G. Narayanan

The CHAIRMAN (Union of Soviet Socialist Republics) (translated from Russian). The three hundred and thirty-seventh meeting of the Conference is open. Does any representative wish to speak?

Mr. DEAN (United States of America). Although I have not attended meetings of the Conference for about two months I have followed its activities in detail from Washington, where I have been able to conduct extensive consultations on all of the aspects of these negotiations, and I take pleasure in greeting my old colleagues at this conference table.

Needless to say, every official in the United States Government who is connected with these talks, from President Kennedy down, has been deeply disappointed at the complete lack of acceptance by the Soviet Union of the very constructive proposals put forward by the United Kingdom and the United States in an effort to get a workable nuclear test ban treaty.

The world has noted and continues to note with approval the sincere desire of the Western Powers to conclude a sound nuclear test ban treaty as rapidly as possible—a desire dramatically evidenced by their far-reaching moves on and since 21 March last and by their repeated statements of readiness to enter into serious give-and-take negotiations on still unresolved questions—and, on the other hand, has noted and continues to note with disapproval the totally negative and obstructive policy of the Soviet Union towards a nuclear test ban agreement.

Neither the American people nor its Government, nor the peoples of the world, can find any sense or purpose in this Soviet policy which can only have the effect of increasing world tensions still further.

Even the Soviet Government has not denied the benefits that would flow from a sound agreement banning further nuclear weapon tests.

For instance, in his letter of 4 April 1958 to President Eisenhower, Premier Khrushchev wrote:

"Hardly anyone will deny that the discontinuance of experiments with atomic and hydrogen weapons would greatly improve the international political atmosphere as a whole and would create more favourable conditions for the settlement of other unsolved international problems."

In the same vein, Mr. Tsarapkin himself has also stated on a number of occasions at these meetings that a test ban could play a useful role in inhibiting the further spread of independent nuclear weapon production capabilities and that it would also mark a great step forward in relations between East and West.

Moreover, as I noted at the three hundred and twelfth meeting, a test ban would constitute a significant measure of "anticipatory" disarmament which

could prevent the appearance on the world scene of still more destructive weapons.

Despite the lip service given by the Soviet Government to these very worthwhile aims, it has nevertheless in practice followed a policy which not only has made agreement between East and West extremely difficult but must indeed, I regret to say, have been calculated to make agreement here impossible. For the course of the Soviet Union in recent months at this Conference has not only been to reject or belittle the major accommodations made by the United States and the United Kingdom to Soviet demands in order to get an effective nuclear test ban treaty. The USSR has also moved backwards to a major extent. It has done this by expressly reversing the position of the Soviet Union on a major question, namely, the appointment of a single administrator and his major deputies, on which agreement between the sides had been worked out only on 6 July 1960 after many months of labourious effort.

On top of that, the Soviet statement in its 4 June 1961 aide-mémoire that all controls at the present time involve an espionage risk has led the Soviet Union, in effect, to repudiate a control system of the type recommended by the experts at Geneva in 1958 as being of too broad a scope, in present circumstances, and, therefore, as being incompatible with Soviet security requirements.

In adddition, because of its narrow and outmoded view of its own inordinate security needs as a closed and secret society, the Soviet Union has stressed that, regardless of the cost to humanity, its position is totally inflexible upon a number of other questions still at issue regarding essential control machinery for a nuclear test ban treaty.

Finally, after having deliberately created these new obstacles to agreement and after having thus gone far to remove the possibility of finding any mutually acceptable basis for a treaty, the Soviet Union has seized upon the resulting deadlock as a pretext for proposing that these negotiations be terminated altogether and that the test ban question be merged into a consideration of general disarmament problems without any agreement as to when it would actually come up for discussion.

Indeed, the last Soviet note of 9 August to the United States implies that the Soviet Union does not now propose even to discuss the problem of a test ban at a general disarmament conference. It indicates rather that once an agreement on general disarmament has been reached, whenever that may be, the question of a test ban will have become moot because of the general elimination of military activities. Therefore, it seems to say that no special agreement on a nuclear test ban will ever be required at all.

As I have already said, the United States Government cannot concur in the view that a treaty to terminate nuclear weapon tests is no longer important to the world at large. Whatever may happen in the next few years in the field of general disarmament negotiations, the problem of banning nuclear tests under effective international control arrangements is an immediate one.

Without a properly functioning control system, there is no assurance whatsoever that the current so-called *de facto* moratorium on tests actually is being carried out in the closed society of the Soviet Union, although the Soviet Union knows full well that the United Kingdom and the United States, with their open societies, are indeed observing it by not testing.

There is no guarantee that clandestine nuclear weapon tests, which are perfectly possible from a technical point of view, are not already being carried out in secret on the territory of the Soviet Union, in violation of the current informal moratorium on testing, to its advantage and to the disadvantage of the world.

So, at the request of President Kennedy, and as evidence of his deep sincerity in seeking a nuclear test ban treaty, I have returned to Geneva, to this conference room, because these issues are so important to us and to the peoples of the world.

I have come because President Kennedy is determined to leave no stone unturned in an effort to bring the Soviet Union to understand that its own interests in the long run will be as much served by the conclusion of a reasonable nuclear test ban treaty as will those of the United Kingdom and the United States. Whatever military gains either side could achieve in a resumption of testing, these would be a much smaller contribution to the real security of the United States or of the Soviet Union than could come about from the conclusion of a first accord in the field of disarmament.

The Western Powers are convinced that the proposals which they have put forth in their draft treaty on 18 April 1961 (document DNT/110, as amended by DNT/110/Add.1) are entirely equitable and completely sound.

Nevertheless, they have stated, and I state again today, that these terms have never been presented on a "take it or leave it" basis to the Soviet Union. What we have said and continue to say is that only we of the West have offered a well thought out total plan in treaty language for the cessation of nuclear weapon tests.

Despite our failure, at least up to now, to induce the Soviet delegation to comment constructively upon our proposals, we in the West have nevertheless examined all the Soviet Union has had to say in recent months about concrete issues that have not yet been resolved with respect to a nuclear test ban.

In the remainder of my statement today I should like to concentrate on one of these questions. I shall, of course, deal with some of the other problems at subsequent meetings. But it is the complex of issues involved in the proposed nuclear test ban threshold treaty and the proposed further moratorium concept of three years from the date of signing of the treaty with respect to nuclear weapon tests producing a seismic signal of below the threshold level of 4.75 that I intend to discuss now in some detail.

The big controversy on the proposed three-year further moratorium now no longer primarily concerns the exact length of this temporary commitment but rather whether, according to the Soviet representative, this commitment should be temporary.

Despite its previous agreement to a threshold treaty and to a moratorium coterminous with the seismic research programme, the Soviet Union now says that it will not be a party to an arrangement under which any possibility is left open that testing below the treaty threshold of 4.75 might legally be resumed at the end of three-year moratorium.

The Soviet Union goes further to charge the United States with proposing in bad faith a three-year moratorium to run from the date of signing the treaty, that is, proposing it with the aim of abandoning after three years the ban on underground tests below the threshold of 4.75 seismic scale of magnitude and of then planning to undertake such tests below the treaty threshold while the rest of the treaty and control system with respect to tests registering 4.75 and above continue to be valid and operative.

Both Mr. Stelle and I have pointed out here time and again the absurdity of this Soviet position. The moratorium arose in the first place only at the suggestion of the Soviet Union on 19 March 1960, and it was explicitly meant to be a temporary expedient to prevent underground testing registering below 4.75 while a research programme went on to seek improvements in seismic controls in an attempt to lower the treaty threshold.

We have said to our Soviet colleague that his own political experience must tell him that it would, as a practical matter, be impossible for any country cavalierly to abandon the moratorium after three years unless the research results clearly showed that treaty control improvements essential for the lowering or eliminating of the threshold were not possible.

Since the Soviet Union is confident that nothing but improvements in the treaty control system's detection and identification capabilities will result from the seismic research programme, there would seem to be no cause for its pretended alarm, especially since the United States and the United Kingdom have pledged themselves to full consultation with the Soviet Union on the revision of the treaty threshold, in the full glare of world publicity, well prior to the expiration of the three-year moratorium.

It appears to us that if the Soviet Union were honestly trying to overcome obstacles in this negotiation, instead of creating new ones, it would admit that its fears about Western good faith at the end of the three-year moratorium were completely groundless. Unfortunately, however, we are confronted with no such Soviet admission. Indeed, quite the opposite is the case.

Therefore, I have been authorized by President Kennedy to go even further in giving the USSR assurances that only the most objective and carefully considered actions with respect to the treaty threshold will be taken at the end of the three-year moratorium period.

In fact, I am about to put forward "A proposal for reducing or eliminating the threshold in the treaty at the end of the three-year moratorium."

Indeed, my Government now formally proposes that, assuming a nuclear test ban treaty is agreed upon, about six months before the expiration of the three-year moratorium, with respect to tests below the treaty threshold of 4.75, a panel of eminent scientists representing each of the eleven nations which will then be members of the control commission of the treaty organization should be convened. As with the commission itself, so would this panel consist of scien-

tists from four Western States, four Soviet bloc countries and three non-aligned nations.

This scientific panel would be charged with the task of preparing a report to the eleven-nation control commission, by at least a majority vote of the scientists in it, on the following matters:

(1) recommended improvements in the treaty control system in the light of the findings of the proposed seismic research programme in which we have asked the Soviets to participate;

(2) scientific estimates of the capabilities of the control system in the light of such improvements; and

(3) recommendations as to whether or not the treaty threshold of seismic scale of magnitude 4.75 should be lowered, and if so, to what point, and indeed whether the threshold can be eliminated entirely by scientific improvements in the treaty control system.

When this has been done, the control commission itself, composed of four Western Powers, four Soviet Powers, and three neutral or uncommitted Powers, will consider the report and agree, by majority vote, on such draft amendments to the treaty to eliminate or to reduce the threshold as may be required by such of the recommendations of the scientists as the commission may approve.

All such treaty amendments would be submitted to the next annual conference of the parties, or to a special conference if the annual conference were not scheduled until after the expiration of the moratorium, since under the agreed treaty article on amendments, article 23 in the Western draft treaty (DNT/110 and Corr. 1) it is the conference which must adopt proposed amendments.

Mr. Tsarapkin has, on occasion, told us that although his Government proposed the moratorium at the one hundred and eighty-eighth meeting on 19 March 1960, he added two days later, at the one hundred and eighty-ninth meeting, that the moratorium should not be permitted to expire "automatically", so that after that date each of the parties would then not be completely free to test registering up to 4.75.

By any normal definition of the word "automatically", I think that the formal procedure which I have proposed today excludes the possibility of any "automatic" end to the moratorium. On the contrary, our plan guarantees the most careful deliberation and review—by the majority vote of eminent scientists, including at least three scientists of neutral countries, by the control commission, and by world public opinion—of the situation at the expiration of the moratorium.

There is then clearly no loophole for the resumption at the end of the moratorium of testing below the treaty threshold of 4.75. Our proposal is a means to make sure that when the treaty obligation is extended to cover underground tests of lower yields, adequate control measures shall be available to monitor the fulfillment of that obligation, which will not be left to chance.

Surely the Soviet Union can now have no basis whatsoever for not being ready to accept the plan which I have just outlinel, which is a clear guarantee that the moratorium will not "automatically" expire at the end of the proposed three-year term from the date of signing the treaty.

However, we must take note of the statement in the Soviet aide-mémorie of 4 June 1961, that:

"There can be no exceptions to the treaty; all kinds of nuclear weapons tests must be prohibited; in the air, under water, under ground, and in outer space" (GEN/DNT/111, p. 1).

This is precisely our objective too, and it has been from the beginning.

But this particular quoted statement implies that the Soviet Union has gone back on its proposal of 19 March of last year for a moratorium to prohibit tests with yields below 4.75 for some period after the date of the signing of the treaty, and now is merely looking for some pretext to avoid the necessary implications of its statement. If this should unfortunately prove to be the case, then my Government is unconditionally prepared to meet even this possible Soviet reversal of position, in the interest of reaching agreement on a reasonable and sound treaty at once.

I will call this further proposal "A proposal for reducing or eliminating the threshold in the treaty immediately".

First let me recall a bit of history as to how the threshold ever entered into our negotiations. The *Hardtack* series of United States nuclear tests in the fall of 1958 produced new data which convinced American scientists that the con-

clusions of the Geneva experts in August 1958 that all underground tests could be controlled by the system which those experts recommended were not borne out by subsequent scientific discoveries.

The Soviet Union contested this new scientific data when it was introduced into the Conference on 5 January 1959, and the issue was fought over spasmodically in our meetings from then until November 1959 when it was finally agreed to convene a new meeting of British, Soviet and American scientists to review the situation. These experts, who were known as Technical Working Group 2, met in December 1959. They agreed on the possibility of certain control improvements, but on nothing else. The Soviet scientists rejected the American analysis of the so-called new data and they reaffirmed their support of the 1958 Geneva experts' recommendations, whereas the British and the American scientists concluded that the underground controls recommended in 1958 would not be effective, even with the possible improvements envisaged by the same Technical Working Group 2, and that only larger seismic events could be practically monitored by the 1958 experts' system.

It was to deal with the resulting Soviet-Western impasse that my Government proposed, on 11 February 1960, that, instead of an immediately comprehensive treaty, the Conference agree to a treaty in stages. For the first stage we suggested that treaty obligations in the underground environment begin only for seismic events of a magnitude of 4.75 or higher.

At the same time the United States proposed a major research programme to develop seismic control improvements which could in due course, we hoped, justify the lowering or abolition of the 4.75 threshold.

It was this American offer which the Soviet Union accepted on 19 March 1960, on the condition that the United States and the United Kingdom, as well as the Soviet Union, would pledge themselves not to conduct any nuclear tests producing a seismic signal of less than 4.75 during the period of the research programme.

As I have already noted today, this arrangement of a 4.75 threshold treaty with the Geneva control system, plus a research programme, plus a moratorium, was an ingenious and reasonable political-technical solution to the Soviet-Western deadlock.

However, I must stress and repeat that my Government has never sought either a moratorium or a threshold treaty for its own sake. As we have always said, we would infinitely prefer an immediately comprehensive treaty prohibiting all nuclear tests from the outset if scientific controls for monitoring such an obligation were made part of the treaty system. In all of this our guiding principle—which I submit is not particularly startling or unreasonable—has only been not to undertake any obligation under a treaty without some reasonable assurance that the scientific control measures accompanying the agreement would be effective in verifying the compliance of all parties with their treaty obligations. Otherwise there would be no confidence in the treaty.

If the Soviet representative looks back to the statement made by my predecessor, Mr. Wadsworth, at the one hundred and seventieth meeting on 11 February 1960, when he first introduced the threshold proposal, he will find the following sentence:

"* * * the specific threshold magnitude to be adopted under this approach would depend in large part on the level of inspection that the other delegations were prepared to accept" (GEN/DNT/PV.170, p. 8).

I should note that there was nothing new or startling in Mr. Wadsworth's statement, because much the same sort of reasoning had been accepted by the 1958 Geneva experts themselves. Indeed, the penultimate paragraph of section 2 of Annex VII of the experts' report of 1958 says much the same thing, as follows:

"The dependence between these parameters"—and here the report is referring to three parameters for a network of control posts—"is such that with an increase in the yield of the explosion or the number of control posts the probability of detection and identification increases, and the number of unidentified events suspected of being a nuclear explosion decreases. On the other hand, for the identification of the increased number of unidentified events resulting from a smaller number of control posts it would be necessary to increase the number of on-site inspections or to make greater use of information coming from sources not subordinate to the international control organ or, if necessary, of both" (EXP/NUC/28, Annex VII, p. 2).

Today, while following the same reasoning, I would go a bit further, because still other variables are also involved. Thus, the level of the treaty threshold is dependent not only on the number of inspections but also on the number of control posts, on the types of control posts, whether manned or unmanned, on their coverage of seismic areas, on their being evenly spaced throughout a territory or possibly clustered in seismic areas, on the number of seismometers at a particular post and the depths at which they are placed, on the detection and identification techniques available at any moment, and on numerous other factors.

What I am saying to Mr. Tsarapkin today is that the United States is still very flexible in this matter of controls and is quite prepared to study and discuss them with the Soviet Union on a cooperative basis and to accept any revised control arrangements in its own territory as well.

And, so I say, first, we are willing to contemplate an initial treaty threshold that would be lower than the proposed one of seismic magnitude 4.75; and further, we are even willing to consider arrangements, if this, with your cooperation, should prove scientifically possible, which would amount to virtually no treaty threshold at all.

I am suggesting to my Soviet colleague that the United States is ready and prepared to negotiate here and now for the immediate lowering or even removing of the treaty threshold of 4.75, provided that the Soviet Union is ready to explore with us and open-mindedly consider those improvements or adjustments in the control system which could so increase its scientific capabilities from the outset as to warrant the lowering or removing of the threshold.

These adjustments to be explored, which are necessary to deal with the increased number of unidentified events which would result from lowering the threshold, might include: the relocation of some of the control posts in both of our countries and other countries from relatively seismic to highly seismic areas, if that can be done without injury to the whole control system; the introduction into United States, United Kingdom and USSR territories of a number of unmanned seismic recording stations, perhaps in conjunction with some of the foregoing changes regarding the relocation or regrouping of control posts; a modification of the total number of control posts in the United States, the Soviet Union and, possibly, elsewhere; an adjustment of the fixed annual number of inspections in the quota; and the immediate adoption from the outset of certain scientific improvements in the treaty control system.

Let me emphasize the entire willingness of the United States to reexamine the scientific aspects of the entire control system. We are prepared to do everything possible to advance and to make workable a nuclear test ban treaty which would eventually and as soon as possible ban all further nuclear tests in the earth's atmosphere, in outer space, in the oceans and all tests underground, just as soon as it is scientifically possible to do so.

My Soviet colleague will notice that I am not today referring to any specific new treaty threshold level or to any specific changes in control arrangements. However, my delegation is fully prepared to confer and to negotiate on these points.

It goes without saying that agreement on some changes in the control network, along with a lower or even no initial threshold, would still necessitate our going forward with plans to conduct a large-scale seismic improvement research programme underground. The basis for this, as before, would be our hope that in the future we could either abolish the threshold altogether or, if technically feasible, improve the control system with a view to simplifying it and to reducing the annual number of on-site inspections as the identification capabilities of the treaty control system increases.

Concerning that portion of the underground environment which might still not be able to be covered by the treaty from a scientific standpoint, a moratorium would still be instituted for the duration of the three-year research programme, and all of the procedures which I outlined earlier today in my proposal for reducing or eliminating the threshold in the treaty at the end of the three-year moratorium naturally would still be applicable.

My two major and far-reaching proposals of today are further evidence of the open-mindedness and serious purpose of the United States Government in devising sound political initiatives and approaches which, however, remain fully consistent with scientific control requirements.

I trust that my Soviet colleague, in reporting this statement to his Government, will not only note this new and great evidence of Western sincerity and entire flexibility in these talks on the subject of scientific controls, but will

also point out the wide scope which we have opened for negotiation. At the same time, I must make it clear that I reiterate the complete unacceptability of the Soviet three-man administrative proposal below the top eleven-man control commission, on which the West and the East have equal voices, and the Soviet theory of self-inspection.

On the one hand, we have the Western proposal for reducing or eliminating the threshold in the treaty at the end of the three-year moratorium. This means that we are ready to give the Soviet Union extremely far-reaching guarantees for scrupulously fair dealings with the issue of lowering the threshold or even of extending the proposed three-year moratorium at the expiration of the fixed moratorium period. Under such arrangements, there would be no possibility that the way would be left free to abandon research efforts and to resume testing at yields below 4.75 unless it were clearly the opinion of a majority both of the panel of scientific experts of the eleven countries, including neutral representatives, and of the control commission, that there was no substantial scientific hope of working out satisfactory controls for the underground environment below the threshold of 4.75.

On the other hand, we have put forward an alternative proposal for reducing or eliminating the threshold in the treaty immediately. This path only calls for some imaginative negotiation on all sides, plus Soviet exploration of and agreement to reasonable adjustments in the control system which are essential, at this stage of scientific knowledge, for providing effective control capabilities for underground events yielding below 4.75 on the seismic scale of magnitude.

What I have said today I hope will convince the Soviet delegation and the Soviet Government that the Western Governments are extremely anxious to negotiate a sound and effective test ban treaty and will leave no stone unturned in an all-out effort to do everything reasonable to bring this about.

We shall continue to work for sound arrangements which will not give the illusion of control without any substance. Aside from that, however, there are no limits to our willingness to negotiate on the scientific control system.

And so I earnestly appeal to the Soviet Union to examine its conscience and, thereafter, to rejoin the United Kingdom and the United States in this urgent task of concluding a sound and effective nuclear test ban treaty which will give new hope for the complete cessation of nuclear testing to a sorely distressed world and which will constitute a major step in complete and general disarmament.

Mr. ORMSBY GORE (United Kingdom). At the beginning of his statement today Mr. Dean referred to the widespread disappointment and concern which is felt in the United States about the Soviet attitude in this Conference. The same disappointment, the same concern, are also felt, and felt more strongly, in the United Kingdom, and I believe in most of the world.

It is now nearly two months since I informed the Conference of the United Kingdom Government's reaction to the Soviet Government's memorandum of 4 June this year—I am referring to my statement at the three hundred and twentieth meeting on 16 June. I said then that my Government were most unwilling to accept the main and most obvious implication of the Soviet memorandum, which was, on the face of it, that the Soviet Government had now lost all interest in any further effort at constructive negotiation round this table for a properly controlled treaty, a treaty that would place the discontinuance of nuclear weapon tests on a firm and reliable basis.

In the ten or eleven weeks that have elapsed we have, unfortunately, had no evidence of any more positive attitude on the part of the Soviet Government. Indeed, the evidence has all gone to confirm the fears which I expressed. The Soviet delegation has still not made any effort to enter into constructive negotiation with us; there has in fact been no attempt at negotiation from the Soviet side during the whole of this year. The Soviet delegation has made no new proposals since 4 June and no constructive proposals since last year, and it has remained unable or unwilling to explain large parts of the Soviet position or to submit clear and full proposals in written form. In short, the Soviet Government has continued to face us with the same ultimatum as in its memorandum of 4 June: either we accept the Soviet proposals lock, stock and barrel, even those parts of them which are as yet unknown or unexplained, or else we and the world must wait for a properly guaranteed cessation of nuclear testing until a programme of complete and general disarmament has been agreed and, for all we know, until it has been carried out. In either case the Soviet Government is firmly rejecting the idea that the rest of the world should have any

effective means, within the foreseeable future, of assuring itself that the Soviet Government is not secretly improving its nuclear arsenal by means of clandestine testing. Under neither of the Soviet alternatives are we offered effective control and verification of a ban on testing: one alternative offers us no more than self-inspection by the Soviet Union, subject to a universal administrative veto; the other alternative offers us no control at all until some unspecified future date.

It is a very serious situation, and it is entirely the responsibility of the Soviet Government. At a time when international tensions are dangerously increasing—and they are increasing primarily as a result of the deliberate actions of the Soviet Government—at this dangerous juncture in world affairs, the Soviet Government is refusing to take a step that could easily be taken, a step that would markedly contribute towards improving the international atmosphere and increasing security, including the security of the Soviet Union itself, as Mr. Dean has so frequently pointed out. It is perfectly clear that the conclusion of an effective nuclear test ban treaty would have this result and that the only obstacles to this treaty are obstacles which the Soviet Government has deliberately erected during the last few months.

However little encouragement the Soviet Government may have given us, the United Kingdom, like the United States, remains unwilling to accept the worst or to abandon hope in a matter of such importance as this, a matter in which success could make so much difference to the whole international scene. That was our position when I spoke on 16 June of this year, and it is still our position. We want this treaty both for itself and for the good it would do in other ways, and we are determined not to abandon our effort to secure it whilst any hope remains. We have declared ourselves ready to negotiate upon our own proposals or upon any Soviet proposals, provided that negotiation is directed towards the objective that brought us here in the first place—the objective of a treaty with controls adequate to assure both sides that the treaty is being observed.

In this spirit my delegation warmly welcomes and supports the statement which has just been made by the United States representative.

I have, of course, also been following the proceedings of the Conference in my absence and I have noted the repeated emphasis which the Soviet representative has been laying upon the question of what should happen at the end of the moratorium. Mr. Tsarapkin has repeatedly declared that the Soviet Union now sees three main obstacles in the way of an effective treaty. As my colleague Sir Michael Wright recently pointed out, these obstacles are substantially different from the obstacles which the Soviet representative was talking about up to the end of last year and they are very largely obstacles of the Soviet Union's own making. Nevertheless, our object is to negotiate, and if we can reasonably do anything to reassure the Soviet Union on these matters we are prepared to try to do so.

Mr. Dean has described the specific procedures for consultation before the end of the moratorium to which the United States Government is prepared now to commit itself. My Government is certainly also prepared to commit itself to those procedures. I need not repeat what Mr. Dean has said about them, nor need I go into detail about the general United Kingdom position on the moratorium, because Sir Michael Wright has only recently restated it at the three hundred and thirty-fifth meeting. As he pointed out then, the Soviet representative has never explained in any clear or intelligible manner what he means when he says that the parties should not be automatically free to resume testing below the threshold upon the expiry of the period fixed for the moratorium. We have never suggested that it would be our intention to resume testing, automatically or otherwise. Our whole object in accepting the Soviet proposal for the moratorium has been to create the conditions, and to provide the time, in which to work out controls to justify us in agreeing that no form of testing shall ever be resumed—and to assure ourselves that this agreement will in fact be carried out. To confirm what Sir Michael Wright said at the three hundred and thirty-fifth meeting:

"The only foreseeable circumstance in which we should feel obliged not to renew our undertaking under the moratorium would be if we and our partners in the treaty had completely failed to find any reasonable means of assuring ourselves that this undertaking was being respected and would be respected by others as well as ourselves" (GEN/DNT/PV. 335, pp. 22–23).

The procedures which my United States colleague has now proposed show clearly that we have no thought of any hasty or unilateral action at the end

of the moratorium. We have always agreed that there should be consultation and negotiation before the period expires, and these procedures show exactly how this would be done. They show that there would certainly be no question of just doing nothing and letting the moratorium automatically lapse. On the contrary, we should expect to discuss the possibilities of merging the moratorium into the treaty, in whole or in part, or of continuing it in some form, in a group of scientists representing all eleven members of the control commission, and we should be willing to see this group make its recommendations to the commission on the basis of a simple majority, though we should naturally hope that the scientists could reach unanimous agreement. Having received the scientists' recommendations, the commission, again by simple majority, could make its own recommendations to the Conference. I can scarcely conceive of a situation in which my country or any other country would wilfully or unreasonably disregard the recommendations of the majority made in this way.

My delegation believes that these proposed procedures should give the Soviet Union the necessary reassurance about what is to be done at the end of the moratorium. In our view they amount to a clear indication that the moratorium would not be left automatically to lapse at the whim of any one party. As I say, the Soviet representative has never clearly explained what he wants to be done at the end of the moratorium period or what he means when he says the moratorium should not "automatically" lapse. We hope he will agree that the proposed procedures put a reasonable interpretation upon those words. If he has any doubts about that, then I would invite the Soviet representative to give us his own interpretation, and to give it specifically in writing. In that way we may at least be able to find a basis for serious negotiation upon this aspect of our work. This is a problem raised by the Soviet Union. The Soviet representative has never clearly explained his position. We have now clearly explained ours, and the next step is for Mr. Tsarapkin.

Secondly, I concur with Mr. Dean about the possibility of an alternative approach to this problem if the Soviet Union would prefer it that way. For us, as for the United States, the threshold and the moratorium have never been an end in themselves. We should much prefer to have a fully and immediately comprehensive treaty if the necessary controls could be agreed upon to assure us of its observance by all parties. That has always been our position. We have always been, and still are, ready to accept in the United Kingdom and its territories any controls that may be necessary to justify a comprehensive test ban. That goes also for the United States. If the Soviet Union would only adopt the same position, then none would be more delighted than we to forget about the threshold and the moratorium and go straight to a comprehensive treaty. The threshold treaty has never been more than a second-best course imposed by the shortcomings of available control. It is true that the threshold and moratorium have the virtue of being already agreed between us three in principle, and we believe that a satisfactory treaty can be concluded on this basis and could lead straight into a comprehensive treaty, as a result of further research, in a very few years' time. But, if the Soviet Union is willing to try to find the basis of a fully controllable comprehensive treaty with us now, we are certainly willing also.

Here, then, is an opportunity for the Soviet delegation to show us whether its Government does still retain a constructive desire for an effectively controlled nuclear test ban, and I would remind the Soviet representative of the views expressed by the Government of the Soviet Union regarding this Conference in its statement of 23 January 1959, as follows:

"The Soviet Government, consistently pursuing as it does a policy of bending every effort toward delivering mankind from the threat of a nuclear war of annihilation, has, for a number of years, been persistently pressing for the cessation of atomic and hydrogen weapon tests as a first and highly important step toward a radical solution of the disarmament problem. In doing so, the Soviet Government has proceeded and still proceeds from the premise that the question of ending nuclear tests can be solved straightaway, independently of the solution of the other problems of disarmament, given the desire of all the nuclear Powers" (GEN/DNT/26, p. 1).

I hope that the Soviet Government will revert to the attitude revealed in its statement of 13 January 1959. It is our earnest desire that the Soviet representative's response to Mr. Dean's statement this afternoon will be such as to enable us to open up an avenue of genuine negotiation upon this issue, which he himself regards as one of the three most important now confronting us.

The CHAIRMAN (Union of Soviet Socialist Republics) (translated from Russian). Today's statements by the representatives of the United States and the United Kingdom have shown us that the Western Powers are still opposed as before to solving conjointly the question of the discontinuance of tests and the question of disarmament. Mr. Dean today pointed out to us once again the imperative necessity for the Western Powers to set up straightaway a system of control which would function properly, as he termed it. But without disarmament, this would in effect be the pernicious functioning of an intelligence network. Without disarmament, under the conditions of an insane arms race and the intensive military preparations of the Western Powers it would be a control system only in name and would in effect be a system of espionage. That is what the Western Powers are striving for instead of disarmament. And when we point this out, they cry out in reply that the refusal of the Soviet Union to accept the demand of the Western Powers and thus give the Western Powers wide opportunities for carrying on intelligence work on the territory of the Soviet Union, allegedly has the effect of increasing world tensions, as Mr. Dean said today.

Mr. Dean went on to quote documents of April 1959, while Mr. Ormsby Gore referred to a document of as far back as January 1959, and they called upon the Soviet Union to return to those days. Yes, it is true that in those days, that is to say more than 2½ years ago or 3 years ago, this agreement on the discontinuance of tests might perhaps have played a useful role, since at that time it might have been a first step toward disarmament, if there had been good will on the part of the Western Powers. But the policy carried out by the Western Powers is leading the world in a directly opposite direction.

Instead of disarmament, the Western Powers are intensifying the insane arms race, increasing their land armed forces, their air forces and their navies; instead of acting in a way that would lead to reducing the danger of war, the Western Powers have developed intensive military preparations which have greatly increased the danger of a nuclear missile war. In view of these activities by the Western Powers it is rather strange to hear the Western representatives' serene speeches here on various technical themes, while shutting their eyes to the menacing situation in the world brought about by the military preparations of the Western Powers, and striving for the right to carry on intelligence work and the creation under cover of an agreement on the discontinuance of tests of an espionage system of the Western Powers on the territory of the Soviet Union.

Mr. Ormsby Gore spoke today about the Western Powers' anxiety and disappointment at the fact that the Soviet Union insists on the solution of the question of discontinuing tests in conjunction with that of disarmament. But anxiety and disappointment should be aroused in the world by the intensification of the arms race and the Western Powers' military preparations, not by the Soviet Union's insistence on solving the question of discontinuing tests in conjunction with that of disarmament.

We do not feel disposed to satisfy the claims of the Western Powers who are insisting on control without disarmament, which only means using this control to carry out intelligence work and espionage on the territory of the Soviet Union.

The present situation shows beyond all doubt that the question of discontinuing nuclear weapon tests can be solved only in conjunction with that of disarmament. The Soviet Union will be prepared, and we have already told you so repeatedly, to sign without delay a treaty on general and complete disarmament and to agree to any control over disarmament and, consequently, over the discontinuance of nuclear weapon tests, but not to any control while the arms race and intensified military preparations continue, that is the Soviet Union will not agree to control separated from disarmament.

Mr. DEAN (United States of America). I do wish there were some way by which we could truly convince the representative of the Soviet Union that we are not interested in using this control system for gathering intelligence or practicing espionage in his country. Indeed, in addition to sitting down with him and examining the changes in the scientific controls, we would be quite prepared to examine any further suggestions he may have as to ways and means which would make it completely clear that the whole control system could not be used for purposes of espionage.

We have put forward this proposal so as to make sure that the treaty threshold would be either eliminated or reduced, at the end of the three-year period, by scientific means and scientific review; and we have put forward the alternative proposal to go as far as possible immediately, if the Soviet Union prefers immediate action, provided the Soviet Union is willing to review the scientific control posts.

We are not interested in trying to get intelligence or carry out espionage in connection with this treaty control system; all we are trying to do is to get a workable and effective nuclear test-ban treaty, and we are quite prepared to go to any reasonable lengths with the Soviet Union to explore the whole treaty structure in order to bring about that result.

My Government is extremely interested in the subject of general and complete disarmament, and we earnestly and sincerely hope that the meetings which are at present scheduled to be held, in New York or Washington, between Ambassador Zorin and Mr. McCloy, Special Advisor on Disarmament to President Kennedy, will produce genuinely constructive results and that an agreement will be reached as to a forum where we can carry on these disarmament negotiations. We do regret, however, that there seems to be no way of sitting down with the Soviet representative here and working out a practical and effective nuclear test-ban treaty so that the whole world will not be obliged to wait until such time as our two Governments and the other Governments that have been working with us on disarmament achieve full agreement in the whole field of general and complete disarmament.

I do hope that the Soviet representative, despite his immediate remarks, will transmit these proposals to his Government. I would like very much to hear his definitive comment before having to come to the pessimistic conclusion that this latest constructive effort at reaching an agreement with the Soviet Union has been in vain. I am quite sure that after further reflection on this new United States offer the Soviet Government will see that we are truly and sincerely trying to reach an agreement with them on a nuclear test-ban treaty and that we are not attempting, under the guise of scientific controls to work out any arrangement for espionage or for gathering intelligence. I assure Mr. Tsarapkin that nothing could be further from our intentions and I beseech him to transmit these proposals to his Government, as I am sure that they will form a basis for reaching an agreement between us.

Mr. ORMSBY GORE (United Kingdom). I must first say that if the statement from Mr. Tsarapkin to which we have listened this afternoon is the only result of his consultations in Moscow, then it can be described only as extremely meagre. We have listened this afternoon, really, to a most preposterous and childish statement about the control system being meant for purposes of espionage, whereas, as Mr. Tsarapkin well knows, it would not for a moment be so understood by any reasonably intelligent man.

As I understand his statement, Mr. Tsarapkin is now saying that the Soviet Union is not prepared to have any control system on its territory in connection with a nuclear test-ban treaty.

I would only point out to him that the treaty which we have tabled at this Conference in no way goes beyond the provisions which were set out in the experts' report of 1958. I would like to know from Mr. Tsarapkin whether he is now saying that the Soviet Union would not accept a control system based on the experts' report of 1958.

My final comment is simply this: that we have not yet heard from the Soviet representative any comment upon the two proposals put before this Conference by Mr. Dean this afternoon. As the Soviet representative has himself said, this issue of the moratorium is one of the main obstacles to a treaty at the present time. Mr. Dean, with our full support, has outlined a procedure for consultation before the moratorium runs out and we would like to think that the Soviet Government will seriously consider this proposal. I repeat what I said in my earlier comments: if the Soviet Union has certain proposals of its own with regard to consultation at the end of the moratorium we would be very glad to hear them. If he would place before us in writing the procedures he thinks we should go through at the end of the moratorium period that would be of immense help. But I do hope that he will pass on these proposals to his Government because they are designed to try to get us over one of the obstacles which he himself, the Soviet representative, has said is a main obstacle which stands between us and the signature of a treaty.

The CHAIRMAN (Union of Soviet Socialist Republics) (translated from Russian). I should like briefly to remind both the representative of the United States and the representative of the United Kingdom that while there are armies and general staffs, while the armaments race and the intensification of military preparations continue, it is impossible to separate control from intelligence work and espionage. That is one point. Now for the second point. The speeches of the representatives of the United States and of the United Kingdom show that they are continuing to close their eyes to the situation which really exists in the world, namely the intensification of military preparations, the intensification of the armaments race and the increased danger of a nuclear

missile war—and they are continuing to talk serenely about control without disarmament, to which the Soviet Union will not agree.

If no one else wishes to speak on this or on any other question—now everything here seems to be concentrated and focused on what I have just drawn your attention to—then we shall turn to the communique.

Mr. ORMSBY GORE (United Kingdom): Mr. Chairman, you did not catch that particular point which relates to the proposals for the procedure at the end of the moratorium, and I hope we shall have the Soviet Government's comments on that before very long.

The CHAIRMAN (Union of Soviet Socialist Republics) (translated from Russian). If no one else wishes to speak we shall turn to the communique.

After an exchange of views the Conference decided to issue the following communique:

"The three hundred and thirty-seventh meeting of the Conference on the Discontinuance of Nuclear Weapon Tests was held at the Palais des Nations in Geneva on Monday 28 August 1961, under the chairmanship of the representative of the Soviet Union.

"Discussion continued on the documents and proposals before the Conference.

"The United States representative put forward two alternative proposals with regard to the threshold: 'A proposal for reducing or eliminating the threshold in the treaty at the end of the three-year moratorium', and 'A proposal for reducing or eliminating the threshold in the treaty immediately'.

"The Conference decided that its official verbatim records for July 1961 would be released under the usual procedure on 4 September 1961.

"The next meeting of the Conference will be held on Wednesday, 30 August 1961, at 10:30 a.m."

The meeting rose at 4.25 p.m.

APPENDIX 7

SIGNIFICANT DATES IN ATOMIC WEAPONS DEVELOPMENT AND SUBSEQUENT NEGOTIATIONS

DATES OF CERTAIN NUCLEAR WEAPONS EXPLOSIONS

July 16, 1945: First U.S. nuclear device test, Alamogordo, N. Mex.

August 6, 1945: First atomic bomb dropped on Hiroshima.

August 9, 1945: Second atomic bomb dropped on Nagasaki.

September 23, 1949: President Truman announced the first atomic explosion in the U.S.S.R.

October 1952: First nuclear bomb test by the United Kingdom in Australia.

November 1, 1952: Hydrogen device fired at Eniwetok by United States.

August 21, 1953: First hydrogen device tested by U.S.S.R. detected by United States.

February 13, 1960: First French atomic test.

DATES OF NEGOTIATIONS ON DISCONTINUANCE OF NUCLEAR WEAPON TESTS

June 14, 1946: U.S. proposal for international control of atomic energy (Baruch plan).

June 19, 1946: U.S.S.R. proposed alternate plan including insistence on retention of Security Council veto power over any control system.

March 24, 1957: Bermuda declaration—joint declaration by the United States and the United Kingdom to conduct nuclear tests in such a manner as to keep world radiation from rising to more than a small fraction of the level that might be hazardous, to continue to announce test series, also expressed willingness to announce tests to the U.N. and permit international observation if the U.S.S.R. would do the same.

November 14, 1957: General Assembly Resolution 1148 (XII): Regulation, limitation, and balanced reduction of all armed forces and all armaments; conclusion of an international convention (treaty) on the reduction of armaments and the prohibition of atomic, hydrogen, and other weapons of mass destruction. Among its provisions, this resolution urged the immediate suspension of testing of nuclear weapons with prompt installation of effective international control, including inspection posts equipped with appropriate scientific instruments located in the United States, the Soviet Union, and the United Kingdom and at other points as required.

December 10, 1957: Soviet proposal that U.S.S.R., United States and United Kingdom discontinue all tests as of January 1, 1958.

March 31, 1958: Decree of the Supreme Soviet concerning the discontinuance of Soviet atomic and hydrogen weapons tests.

April 28, 1958: President Eisenhower by letter to Khrushchev proposed that both nations have the technical experts start to work on the practical problems involved in disarmament, particularly working toward the suspension of nuclear testing. President Eisenhower stated "I reemphasize that these studies are without prejudice to our respective positions on the timing and interdependence of various aspects of disarmament."

May 9, 1958: Letter from Khrushchev accepting Eisenhower's proposal of April 28 to have experts study the problems involved in an agreement on the cessation of atomic and hydrogen weapons tests as far as inspection and control are concerned.

July 1, 1958: Conference of Experts from the West (United States, United Kingdom, Canada, and France) and East (U.S.S.R., Czechoslovakia, Poland, and Rumania) met in Geneva.

August 21, 1958: Conference of Experts adopted a final report for consideration by Governments. Conference of Experts recommended the so-called "Geneva System" of detecting nuclear explosions. This system recommended a network of 180 control points. It should be noted that the American representatives, during this conference, had taken the position that 650 control points would be necessary to have adequate protection down to 1 kiloton. Through compromise with the Soviets, they settled on the 180 stations, but then had to point out the weakness between the area of 1 kiloton and 5 kilotons.

August 22, 1958: President Eisenhower announced that based on the Conference of Experts' report, the United States was prepared to negotiate an agreement with other nations which have tested nuclear weapons for suspension of nuclear weapons tests and the establishment of an international control system. The President also indicated that the United States would withhold further testing on its part of atomic and hydrogen weapons for a period of 1 year from the beginning of the negotiations unless testing is resumed by the Soviet Union.

October 31, 1958: First meeting in Geneva of the Conference on the Discontinuance of Nuclear Weapons Tests.

November 4, 1958: General Assembly Resolution 1252 (XIII): The discontinuance of atomic and hydrogen weapons tests. Among its provisions, this resolution urged the parties involved in the test-ban negotiations not to undertake further testing of nuclear weapons while these negotiations are in progress. It expressed the hope that the Geneva Test-Ban Conference would be successful and lead to an agreement acceptable to all. It also requested the parties concerned to report to the General Assembly the agreement that might be the result of their negotiations; and requested the Secretary General to render such assistance and provide such services as might be asked for by the conference commencing at Geneva on October 31, 1958.

November 7, 1958: President Eisenhower announced that the United States had detected additional tests by the Soviets subsequent to October 31, 1958.

December 28, 1958: The President appointed a panel on seismic improvement to review technical problems and to recommend methods of improving seismic detection.

January 5, 1959: United States released data showing many underground tests could not be detected by Geneva experts system recommended in 1958. Indicated Geneva system applicable at 20 kiloton rather than 5 kiloton threshold.

February 22, 1959 to March 2, 1959: Macmillan meeting with Khrushchev. During this meeting Macmillan and Khrushchev discussed the establishment of quotas for numbers of onsite inspections in countries where suspicious events have taken place.

April 13, 1959: United States proposed phased testing ban limited in first phase to atmospheric tests below 50 kilometers, with simplified control system, if Soviet Union continued to insist on veto for onsite inspections.

April 23, 1959: Soviets reject U.S. proposal to stop only atmospheric tests and said numerous onsite inspections would not be necessary for complete ban.

June 22, 1959 to July 10, 1959: Technical Working Group No. I met in Geneva to study high-altitude detection problems. On July 10 Geneva Technical Working Group I proposed establishment of system of earth satellites and installation of additional equipment at control posts to detect high-altitude explosions.

August 26, 1959: United States extended unilateral suspension to end of 1959.

August 27, 1959: United Kingdom said it would not resume tests as long as Geneva negotiations showed prospect of success.

August 28, 1959: U.S.S.R. pledged not to resume testing unless Western Powers did so.

November 21, 1959: General Assembly Resolution 1402 (XIV): Suspension of nuclear and thermonuclear tests. Among its provisions this resolution expressed the hope that the countries involved in the test-ban negotiations at Geneva would intensify their efforts to reach an agreement at an early date; it further urged the countries concerned in these negotiations to continue their voluntary ban on testing nuclear weapons; it also requested the countries concerned to report to the General Assembly the results of their negotiations.

November 25, 1959: Technical Working Group II met in Geneva with the Soviets and the British. This group met to consider data from the Hardtack series of nuclear explosions and the findings of the Berkner Panel. On December 18, 1959, at the conclusion of the meetings held by Technical Working Group II, U.S. members of Geneva Technical Working Group II reported that a large number of seismic events could not be identified without on-site inspection, even with improved techniques. The Soviet members of Geneva Technical Working Group II disagreed with U.S. finding.

December 29, 1959: United States said it was free to resume testing after end of 1959 but would not do so without giving advance notice.

February 11, 1960: United States proposed phased agreement, first phase to provide for cessation of tests in atmosphere, oceans, and outer space, to greatest height that could be effectively controlled; underground tests above 4.75 seismic magnitude (estimated by United States to equal explosion of about 20 kilotons) would also be covered; the 4.75 threshold would be lowered as capabilities of detection system were improved, 20 or 30 percent of unidentified seismic events above threshold should be inspected; U.S. experts estimated that this would mean about 20 inspections per year in U.S.S.R.

March 19, 1960: Soviets offered to conclude treaty on cessation of tests, together with moratorium on underground tests below magnitude 4.75, and to agree to joint research program on understanding that weapons tests would be halted during program.

March 29, 1960: United States and United Kingdom said they would agree to voluntary moratorium on underground weapons tests below magnitude 4.75 after treaty was signed and arrangements were made for coordinated research program.

December 20, 1960: General Assembly Resolution 1577 (XV): Suspension of nuclear and thermonuclear tests. This resolution urges the countries involved in the Geneva test-ban negotiations to seek a solution for the few remaining questions so that a test-ban agreement could be achieved at an early date; it further urges the countries concerned in these negotiations to continue their present voluntary suspension of the testing of nuclear weapons; it also requests the countries concerned to report the results of their negotiations to the Disarmament Commission and the General Assembly.

March 21, 1961: First meeting under the new administration of the Geneva Conference on Discontinuance of Nuclear Weapons Tests. U.S. proposal presented by Ambassador Arthur H. Dean, Soviet Union introduced its troika proposal on this date.

April 18, 1961: United States and United Kingdom introduced draft treaty to the Geneva Conference.

May 5, 1961: Statement by President Kennedy on the Geneva test-ban negotiations made at his news conference. Mention is made of the new United States and United Kingdom proposals and the introduction of the troika proposal by Russia.

June 4, 1961: Khrushchev delivers Soviet aide-memoire concerning disarmament and nuclear weapons tests to President Kennedy at Vienna. Insists that the question of control hinges on Western Powers accepting proposals on general and complete disarmament.

June 6, 1961: Kennedy reports to American people on his Vienna talks with Khrushchev.

June 16, 1961: Khrushchev reports to Russian people on his talks with President Kennedy. (Tass report) topics covered: General and complete disarmament, banning of nuclear weapons, cessation of tests, question of control, Hammarskjold, the German question (peace treaty).

June 17, 1961: U.S. aide-memoire to Soviet Russia concerning Geneva test-ban negotiations. Repeated new proposals offered by the United States and the United Kingdom on March 21, 1961.

June 28, 1961: President Kennedy announces appointment of Committee of Scientific Experts to advise him on test-ban problem.

July 5, 1961: Soviet note replying to U.S. note of June 17, 1961, concerning suspension of nuclear weapon tests. Says Soviet proposals have been distorted.

Brings up again supervision of inspection and control by equal representatives of three basic groups; Socialist states, capitalist states in Western military bloc, and neutral states (troika).

July 15, 1961: U.S. note to Soviet Union referring to the Soviet note of July 5, 1961, on the Geneva test-ban negotiations. Says Soviet note contains a multitude of irrelevant and unwarranted comments. Confines its reply to the central issue: Is the Soviet Union prepared to reach an accord which would halt nuclear tests under effective international control?

July 15, 1961: United States and United Kingdom request to United Nations to place on the agenda of the 16th General Assembly an item entitled "The Urgent Need for a Treaty To Ban Nuclear Weapons Tests Under Effective International Control."

July 20, 1961: President announces membership of nuclear test study group.

August 10, 1961: President announces he has reviewed report of Scientific Committee and is sending Ambassador Dean back to Geneva.

August 30, 1961: Soviets announce plans to resume nuclear testing. Among the reasons cited by the Soviets for taking this step were the turn-down of the troika proposal, the nuclear tests carried out by the French beginning February 13, 1960, and the Berlin situation.

August 30, 1961: White House statement on the Soviet's announcement that they planned to resume nuclear testing. This statement expressed concern and resentment in regard to the Soviet decision to resume nuclear testing. It added that the Soviet decision presented a threat to the entire world. It denounced the Soviet pretext for resumption of weapons testing by mentioning that the Berlin crisis was created by the Soviets themselves. It also mentioned that the Soviet Union bears heavy responsibility before all humanity for this decision which was made in complete disregard of the United Nations. It concluded by announcing that Ambassador Arthur Dean was being recalled immediately from his post as chief negotiator at the nuclear test-ban meetings.

September 1, 1961–November 4, 1961: The Soviet Union conducted a series of approximately 50 atmospheric nuclear tests with a total yield of about 120 megatons. The tests were conducted at three different locations in the Soviet Union: Semipalatinsk, Novaya Zemlya, and east of Stalingrad. The series was highlighted by a 55–60 megaton detonation on October 31, 1961, despite a resolution adopted October 27, 1961, by the United Nations appealing to the U.S.S.R. to refrain from carrying out their stated intention to explode a device of this yield.

September 3, 1961: President Kennedy, in a joint statement with British Prime Minister Macmillan, proposed that the Soviet Union agree immediately to discontinuing testing nuclear weapons in the atmosphere. The note suggested that the United States, United Kingdom, and U.S.S.R. representatives meet in Geneva not later than September 9 to record the agreement to cease nuclear testing in the atmosphere and report it to the United Nations.

September 5, 1961: President Kennedy announced that the United States would resume nuclear testing. He ordered the tests carried out in the laboratory and underground "with no fallout." This decision was made after the Soviets set off their third nuclear test in the atmosphere in 5 days. President Kennedy, in referring to the Kennedy-Macmillan statement of September 3 on banning nuclear testing in the atmosphere, said the offer remains open until September 9, 1961.

September 15, 1961: The United States detonates its first underground nuclear device at the Nevada test site.

November 2, 1961: The President announces that the policy of the United States will be to proceed in developing nuclear weapons to maintain a superior capability for the defense of the free world against any aggressor. This statement indicated that the United States would make necessary preparations in case it becomes necessary to test in the atmosphere.

December 22, 1961: A joint communique was issued by President Kennedy and Prime Minister Macmillan following a 2-day meeting in Bermuda. They agreed that it was necessary as a matter of prudent planning for the future, that pending the final decision [to resume atmospheric testing] preparations should be made for atmospheric testing to maintain the effectiveness of the deterrent.

January 29, 1962: Geneva Conference on the Discontinuance of Nuclear Weapons Tests breaks up at 353d meeting. The United States proposed an adjournment, and Soviet negotiator Tsarapkin said, "This is the end."

February 7, 1962: President Kennedy and British Prime Minister Macmillan said they have proposed to Soviet Premier Khrushchev that another "supreme effort" to halt the nuclear arms race be made by raising next month's 18-nation general disarmament conference to the foreign minister's level.

February 14, 1962: President Kennedy urged Premier Khrushchev not to press his proposal for an 18-nation summit meeting on disarmament. However, he assured the Soviet leader that he was ready to participate "at any stage of the conference when it appears that such participation could positively affect the chances of success."

February 21, 1962: Premier Khrushchev replied to President Kennedy's letter of February 14 still insisting on a summit conference on disarmament.

February 24, 1962: Letter from President Kennedy to Premier Khrushchev. President Kennedy replied to Premier Khrushchev's letter of February 21, 1962, stressing that heads of state participation at the Geneva Conference should be reserved until a later stage in the negotiations after preliminary agreements have been reached at the foreign minister's level.

March 2, 1962: President Kennedy announced that he had ordered a resumption of nuclear tests in the atmosphere in late April unless the Soviet Union agrees before then to an ironclad treaty banning all tests. The President held out to Khrushchev the promise of a summit conference at which such a treaty could be signed, and also said that a satisfactory treaty would be offered by the West at the disarmament conference opening in Geneva on March 14, 1962.

March 4, 1962: The Soviet Government sent the United States a message delivered to the State Department advising that Foreign Minister Gromyko would go to Geneva. The Kremlin message was reported to have said that Khrushchev had "reluctantly" accepted the foreign minister proposal.

March 14, 1962: Seventeen-nation disarmament conference opened in Geneva. (Originally 18-nation conference, but France did not attend.)

March 15, 1962: The United States, during the Geneva Disarmament Conference clearly indicated its willingness to drop the 4.75 degree threshold and to make the Test Ban Treaty, from the outset, complete in its coverage by banning all tests in the atmosphere, outer space, underground and in the oceans. The response of the Soviet Union to this proposal indicated an unwillingness on their part to accept a treaty with or without the U.S. proposed amendment.

March 16, 1962: Premier Khrushchev announced that Soviet scientists had developed a global rocket invulnerable to antimissile weapons and that it rendered obsolete the early warning system of the United States.

April 10, 1962: The White House released a joint United States-United Kingdom statement on nuclear testing appealing to the Soviet Union to agree to a nuclear test ban with adequate safeguards including the principle of international verification. This statement indicated that if such an agreement was not successful then the test series scheduled by the United States for the latter part of April would go forward.

April 10, 1962: Prime Minister Macmillan added a personal message to the joint Anglo-American note to Premier Khrushchev on a nuclear test ban asking him to accept an inspection procedure and "fill all the peoples of the world with a new sense of hope."

APPENDIX 8

Evaluation of the "Audit" and "Antineutrinos" Proposals Under Project VELA

Congress of the United States,
Joint Committee on Atomic Energy,
Washington, D.C., August 14, 1961.

Dr. J. P. Ruina,
Director, Advanced Research Projects Agency,
Department of Defense, the Pentagon, Washington, D.C.

Dear Dr. Ruina: This will confirm a telephone request, dated August 1, 1961, by Lieutenant Colonel Lunger, Joint Committee Staff, to Dr. George Bing.

We would appreciate ARPA's evaluation of two particular proposals under Project VELA.

1. Use of underground radio waves to detect clandestine nuclear explosions—Raytheon.

2. The use of antineutrinos in detecting underground nuclear explosions.

In addition your comments on any other current proposals in the detection program would be most useful to the committe.

Sincerely yours,

James T. Ramey, *Executive Director.*

ADVANCED RESEARCH PROJECTS AGENCY,
Washington, D.C., August 23, 1961.

Mr. JAMES T. RAMEY,
Executive Director, Joint Committee on Atomic Energy,
The Capitol, Senate Post Office, Washington, D.C.

DEAR MR. RAMEY: The following comments are submitted as a result of the recent telephone conversation between yourself and Dr. George Bing from this office in connection with two proposed detection schemes of possible interest to the VELA program.

(*a*) The Raytheon "audit" proposal. This specific detection scheme has received recent publicity through an Aviation Week article of July 17, 1961. The magazine article is based on a public talk by Mr. Clark Abt of Raytheon. The talk was essentially a summary of the Raytheon "audit" proposal dated December 12, 1960 and submitted to ARPA for consideration. This proposal has not been supported by ARPA.

The essential features of the audit proposal are:

(1) A dense net of unmanned detection stations (i.e. 3,000 stations worldwide as opposed to the 170–180 Geneva type manned stations).

(2) Incorporation of detectors to record a possible electromagnetic signal from underground explosions as well as the seismic signal. In theory this would give data characteristic of nuclear explosions but not earthquakes. Also in theory the electromagnetic signal might actually be enhanced from a decoupled explosion relative to a tamped explosion.

(3) Development of an elaborate "tamper-proof" communication system incorporating certain unconventional features.

The actual Raytheon proposal was to perform further studies of the feasibility of the "audit" scheme.

It has been recognized, at least since the Berkner Panel report of 1959, that a denser seismic network, manned or unmanned, would greatly enhance any detection network's capability. Unmanned seismic stations might be both politically and technically acceptable methods of improving the "system." However, no serious negotiations have been pursued on this matter. ARPA has been and is now sponsoring research on the general problem of unmanned stations.

The possibility of detecting an electromagnetic signal from underground explosions had been recognized prior to the Raytheon proposal. At present a number of fundamental studies of this problem are under ARPA sponsorship. Ultimately the feasibility of this detection scheme must be determined by measurements in conjunction with actual nuclear explosions.

In summary ARPA considers it premature to support a systems study such as the Raytheon audit proposal described prior to establishing the feasibility of the fundamental components of such a system, that is, the unmanned seismic stations and the practicality of underground electromagnetic signal detection.

(*b*) The use of antineutrinos in detecting underground nuclear explosions.

Suggestions have been made from time to time that the antineutrinos, originating in the beta decay of fission products produced, in a nuclear explosion might provide a useful detection scheme. A recent Rand Report, RM–2641, by R. O. Hundley discusses the problem in some detail. The conclusion of this report is that the method is not feasible. In particular the size of the detectors required for even very limited ranges of detection is impractically large. (In one example an array of liquid scintillation detectors with a volume equal to 5–10 Pentagon buildings would be required to detect a kiloton explosion at a distance of about 100 kilometers.)

Subsequent to the Rand Report[1] ARPA has received a proposal in which it is suggested that certain possible antineutrino interactions might be somewhat easier to detect than those on which the Rand study was based. Such interactions have not been observed experimentally and further have little theoretical basis. Even granting the hypothetical reactions, the detectors would still be unreasonably large. ARPA does not consider the use of antineutrinos for nuclear test detection feasible or worthy of support.

If we can be of further help to you in this or any other matters, please call on us.

Sincerely,

J. P. RUINA, *Director.*
(Signed) GEORGE W. RATHJENS.

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[1] See Rand Report, p. 423.

MEMORANDUM
RM-2847-ARPA
October 1961

ARPA Order No. 189-61

NOTE ON ANTINEUTRINO CROSS SECTIONS AND ON DETECTION OF ANTINEUTRINOS FROM NUCLEAR EXPLOSIONS

W. J. Karzas and R. Latter

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SUMMARY

Cross sections for various antineutrino reactions are estimated. Based on these estimates it is concluded that no antineutrino reactions are useful as a basis for detecting nuclear explosions at great distances.

RM-2847
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NOTE ON ANTINEUTRINO CROSS SECTIONS AND ON DETECTION OF ANTINEUTRINOS FROM NUCLEAR EXPLOSIONS

The fission products from a nuclear explosion provide a strong source of antineutrinos which some people have suggested could be used for detecting nuclear explosions. Antineutrinos have the advantage for detection that they are produced by all nuclear explosions, even decoupled ones. They have the disadvantage that they interact extremely weakly with matter and consequently their detection is difficult.

The only known means for detecting antineutrinos are based upon observing one of the following reactions:

(a) $\bar{\nu} + P \longrightarrow N + \beta^+$ Inverse β Decay,

(b) $\bar{\nu} + e \longrightarrow \bar{\nu} + e$ Scattering,

(c) $\bar{\nu} + \text{Nucleon} \longrightarrow \bar{\nu} + \text{Nucleon}$ Scattering (Elastic or Inelastic).

Reaction (a) has been observed by Reines and Cowan[1] to have a cross section $\sigma_{RC} = 10^{-43}$ cm^2 at fission antineutrino energies. The magnitudes of the other cross sections are unknown.

Detection of Nuclear Explosions

At a distance of R kilometers from a nuclear explosion with fission yield Y kilotons, the total number of antineutrinos per cm^2 from beta decay

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[1] F. Reines and C. L. Cowan, Jr., Phys. Rev. 113, 273 (1959).

of fission products is

$$\frac{Y}{R^2} \times 10^{12} \text{ antineutrinos/cm}^2 .$$

Nearly all the fission-product antineutrinos are produced within the first few seconds after the explosion.

For a detector containing N target particles, the expected number of antineutrino reactions of cross section σ is

$$\frac{N\sigma Y}{R^2} \times 10^{12} .$$

Observation of these reactions is in general limited by extraneous backgrounds. However, for detecting nuclear explosions we shall make the extreme assumption that there are no extraneous backgrounds. In this case detection is limited by the probability that a single reaction is observed. If it is required for detection that at least one reaction is observed with a probability of .95, then on the average 3 reactions produced by the explosion must be observed, or

$$\frac{N\sigma Y}{R^2} \times 10^{12} = 3 .$$

The maximum detection range of a nuclear explosion of fission yield Y kilotons would then be

$$R = 1.8 \times 10^{-16} \sqrt{\frac{\sigma}{\sigma_{RC}} NY} \text{ km} .$$

For an explicit estimate of the feasible detection ranges of antineutrinos from nuclear explosions it will be assumed that a detector similar to that used by Reines and Cowan for the measurement of reaction (a) is used. This detector consisted of 400 liters of water (containing a small amount of cadmium salt) as the target, and 5400 liters of liquid scintillator material to observe triple coincidences among the pair-annihilation γ rays and the neutron-capture γ ray. The target contained 10^{29} protons. The total weight of target and scintillator liquids (i.e., excluding walls, electronics, shielding, etc.) was about 6 tons. For this detector, where it is assumed that antineutrino reactions are observed with 100 per cent efficiency and that there are no extraneous backgrounds, the maximum detection range would be

$$R = 5.8 \times 10^{-2} \sqrt{Y} \sqrt{\frac{\sigma}{\sigma_{RC}}} \text{ km} \simeq 60 \sqrt{Y} \sqrt{\frac{\sigma}{\sigma_{RC}}} \text{ meters}.$$

Even if 1000 Reines-Cowan-type detectors ($N = 10^{32}$, about 6000 tons of scintillator material) were used, where again the extreme assumption of no extraneous backgrounds is made and if it is assumed that the detection efficiency is 100 per cent, the maximum detection range would be

$$R = 1.8 \sqrt{Y} \sqrt{\frac{\sigma}{\sigma_{RC}}} \text{ km},$$

and a 100-kiloton bomb, under ideal conditions, would be observed by reaction (a) with 95 per cent probability at only 18 kilometers.[2]

[2]See also R. O. Hundley, The RAND Corporation, Research Memorandum RM-2641.

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Obviously, only if reactions (b) and (c) had cross sections many orders of magnitude larger than reaction (a) is there any hope of using antineutrinos for detection purposes. However, even if reactions (b) and (c) had larger cross sections, they probably would still be less useful than reaction (a), since reaction (a) leads to simultaneous emission of three γ rays. And a triple coincidence of γ rays can effectively suppress backgrounds — unlike the non-unique products of reactions (b) and (c).

For the remainder of this note the evidence on the magnitudes of the cross sections for reactions (b) and (c) is discussed.

($\bar{\nu}$, e)-Scattering Cross Section

If the coupling of antineutrinos and electrons is the same as in ordinary beta decay, then theoretically the ($\bar{\nu}$, e)-scattering cross section[3] would be (for 3 Mev antineutrinos) $\sim 10^{-44}$ cm^2, that is, somewhat smaller than the cross section of reaction (a).

It is possible, however, that a different ($\bar{\nu}$, e)-coupling exists with an as yet unknown coupling constant. Thus to estimate the magnitude of the ($\bar{\nu}$, e)-cross section experimental data are needed. Such data exist.

Using the same scintillation detectors which they used for measuring the cross section of reaction (a), Reines and Cowan measured the "singles" counting rate from reactor antineutrinos. If the entire "singles" counting

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[3] R. P. Feynman and M. Gell-Mann, Phys. Rev. 109, 193 (1958).

rate which they observed is ascribed to electrons scattered by antineutrinos (reaction (b)), an upper limit on the ($\bar{\nu}$, e)–scattering cross section can be determined. Reines and Cowan's[4] first experiments gave an upper limit of $\sim 10^{-39}$ cm^2. Reines and Cowan interpreted this cross section in terms of an upper limit of 10^{-7} Bohr magnetons for the antineutrino magnetic moment. Later experiments[5] by Reines and Cowan using larger detectors gave an upper limit of 10^{-9} Bohr magnetons for the magnetic moment of the antineutrino. This magnetic moment is equivalent to an upper limit of 10^{-43} cm^2 for the fission antineutrino–electron scattering cross section.

Thus, the detection range of nuclear explosions using the ($\bar{\nu}$, e)–scattering process (reaction (b)) is no greater than using inverse beta decay (reaction (a)). Moreover, using the ($\bar{\nu}$, e)–scattering process has the disadvantage that the reaction products are not unique to the specific antineutrino reaction.

($\bar{\nu}$, nucleon)–Scattering

In the absence of a direct ($\bar{\nu}$, nucleon)–interaction, ($\bar{\nu}$, nucleon)–scattering can theoretically still occur by means of an electromagnetic interaction between a nucleon and a virtual electron pair coupled to an antineutrino. A theoretical estimate of the cross section of this process is $(e^2/\hbar c)^2 \approx 10^{-4}$ times the ($\bar{\nu}$, e)–cross section, or less than $\sim 10^{-47}$ cm^2.

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[4] C. L. Cowan, Jr., F. Reines, and F. B. Harrison, Phys. Rev. 96, 1294 (1954).

[5] F. Reines and C. L. Cowan, Jr., Nature, 178, 446 (1956).

However, it is possible that a direct ($\bar{\nu}$, nucleon)—interaction exists, and therefore experimental data are required in order to determine the cross section of this process.

Direct experimental information on the ($\bar{\nu}$, nucleon)—elastic scattering cross section is available. The high energy accelerator at CERN produces Bev—π mesons which, when they decay, produce along with μ mesons an intense neutrino and antineutrino beam with an energy $\sim$ 1 Bev. Using this beam, preliminary measurements have been made which put a definite upper limit of 10^{-36} cm^2, and a possible upper limit of 10^{-37} cm^2, on the ($\bar{\nu}$, nucleon)—cross section at 1 Bev.[6] Unfortunately, to apply this result to the detection problem requires extrapolation of the measured cross section to energies of Mev's. This extrapolation must be based on theory and therefore could be wrong. Nonetheless, theoretically, the most general form for the ($\bar{\nu}$, nucleon)—scattering cross section (consistent with present knowledge about neutrinos) is

$$\sigma_{(\bar{\nu},\,N)} = \frac{2}{\pi}\left\{G_1^2\left(\frac{E_\nu}{M}\right)^2\left(1+\frac{2E_\nu}{M}\right)^{-1} + \frac{1}{6}\,G_2^2\left(\frac{E_\nu}{M}\right)\left[1-\left(1+\frac{2E_\nu}{M}\right)^{-3}\right] - G_1G_2\left(\frac{E_\nu}{M}\right)^2\left(1+\frac{2E_\nu}{M}\right)^{-2}\right\},$$

where E_ν is the antineutrino energy and M is the nucleon rest energy

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[6] S. Berman, private communication.

($\sim$ 1 Bev). G_1 and G_2 are (unknown) coupling constants which determine the mixture of vector and axial vector couplings for the nucleon. Choosing the ratio of G_1 to G_2 so as to make the low energy cross section as large as possible, the cross section (using 10^{-36} cm^2 as the upper limit at 1 Bev) is

$$\sigma_{(\bar{\nu}, N)} < 4(E_\nu)^2 \times 10^{-42} \text{ cm}^2,$$

where E_ν is measured in Mev. Thus for fission antineutrinos ($E_\nu \sim 3$ Mev), the ($\bar{\nu}$, nucleon)—elastic scattering cross section is theoretically less than $\sim 4 \times 10^{-41}$ cm^2.

For detection of elastic scattering it should be noted, however, that detectors are very inefficient for counting the low energy (few kilovolt) recoil protons arising from fission antineutrinos. Such low energy protons are equivalent in ionizing power only to electron volt electrons. Thus, if only elastic scattering of nucleons occurs, then antineutrinos would not be useful for detecting nuclear explosions.

So far as inelastic scattering is concerned, the ($\bar{\nu}$, nucleon)—cross section measured at CERN is an upper bound since any scattering process requiring a threshold has a smaller cross section than that corresponding to complete elastic scattering. Nuclear matrix elements imply a further reduction in the inelastic scattering cross section compared to the cross section measured at CERN. The CERN experiments therefore only lead to a reasonable theoretical upper limit on the ($\bar{\nu}$, nucleon)—inelastic scattering

cross section at fission energies.

More direct information on the actual inelastic scattering cross section is provided by the Reines–Cowan measurements.[4] In these measurements, if a ($\bar{\nu}$, nucleon)–interaction gave rise to an inelastic scattering which left an excited nucleus, then the decay gamma rays (in the Mev range) could have been efficiently detected by the liquid scintillators. To overestimate the cross section for the ($\bar{\nu}$, nucleon)–inelastic scattering it will be assumed that the "singles" counting rate observed by Reines and Cowan was due entirely to the ($\bar{\nu}$, nucleon)–inelastic scattering.

The Reines–Cowan scintillator material was composed mostly of carbon and hydrogen, with small parts of nitrogen, oxygen, and phosphorus. The first excited level in carbon is at 4.4 Mev, so fission antineutrinos are energetically able to have excited this level. In addition, there were in the walls, earth, etc., surrounding the scintillator other nuclei, such as silicon, aluminum, lead, and iron, which could have been excited by fission antineutrinos. The mean free path for γ rays of a few Mev is about 40 g/cm^2; thus the detector of 1400 liters could efficiently have detected gamma rays from about 3×10^6 grams of target, that is, about 2×10^{30} nucleons, or a few times the number of target particles in the scintillator itself. The actual detection efficiency of Mev–gamma rays was perhaps one–third to one–half. If all the Reines–Cowan background counts are ascribed to the excitation of some particular element in or near the scintillator, the ($\bar{\nu}$, nucleon)–inelastic scattering cross section

would be less than a few times 10^{-43} cm^2.

A possibility which cannot entirely be ruled out is that the "singles" counts measured by Reines and Cowan were in fact due to a nuclear excitation of an element present only in very small amounts. This could lead to a large cross section for that particular interaction. This appears to be an entirely unreasonable interpretation since it requires that reaction (c) be forbidden for all those elements which were present in appreciable amounts in or near the scintillator.

Discussion

Even in the absence of backgrounds, the detection range of reaction (b) appears to be the same as for reaction (a) and the range of reaction (c), less than a few times that for reaction (a). However, products of reactions (b) and (c) are not unique to antineutrino reactions. Backgrounds are probably large. This additional complication probably makes reactions (b) and (c) even less useful than the (impractical) reaction (a).

In summary, the cross sections of reactions (a), (b), and (c) at fission antineutrino energies are

(i) σ (reaction (a)) $\sim 10^{-43}$ cm^2,

(ii) σ (reaction (b)) $\lesssim 10^{-43}$ cm^2,

(iii) σ (reaction (c), inelastic) $\lesssim$ few x 10^{-43} cm^2, for a wide variety of nuclei,

(iv) σ (reaction (c), elastic) $\lesssim 4 \times 10^{-41}$ cm^2, based on theoretical extrapolation from 1 Bev, but probably not observable for fission antineutrinos.

The maximum detection range for antineutrinos is small and this method of detecting nuclear explosions is apparently not useful.

Acknowledgment

We wish to thank Mr. S. Coleman for calculating the theoretical cross section of ($\bar{\nu}$, nucleon)–scattering.

APPENDIX 9

TELEGRAM OF INVITATION AND LIST OF SCIENTISTS INVITED TO PARTICIPATE IN A PANEL DISCUSSION ON FEASIBILITY AND CAPABILITIES OF A TEST BAN CONTROL SYSTEM

TELEGRAM

"The Joint Committee on Atomic Energy will hold a public hearing July 25, 26, and 27 in room P–63, U.S. Capitol Building, Washington, D.C., on developments in the field of detection and identification of nuclear explosions (particularly underground and high altitude), since the committee last held public hearings in April 1960 on "Technical Aspects of Detection and Inspection Controls of a Nuclear Weapons Test Ban." You are invited to participate in a panel discussion on July 27 as to the feasibility and capabilities of a test ban control system. The committee is particularly interested in updating information since the last panel discussion. You are also invited to be present at as many of the hearings as convenient. The committee will reimburse travel and per diem expenses. Please telegraph collect in care of Joint Committee on Atomic Energy, room F–88, U.S. Capitol Building, if you can be present."

CHET HOLIFIELD, *Chairman.*

ADDRESSEES

Dr. Roland Beers, Post Office Box 1019, Troy, N.Y.
Dr. Hans Bethe, professor of physics, University of Cornell, Ithaca, N.Y.
Dr. John Foster, director, Lawrence Radiation Laboratory, University of California, Livermore, Calif.
Dr. Dean S. Carder, Geophysics Division, Coast and Geodetic Survey, U.S. Department of Commerce, Washington, D.C.
Dr. Alvin C. Graves, Los Alamos Scientific Laboratory, Los Alamos, N. Mex.
Prof. Harrison Brown, division of geological sciences, California Institute of Technology, 1201 East California Street, Pasadena, Calif.
Dr. Jay Orear. Cornell University. Post Office Box 451, Ithaca, N.Y.
Dr. Jack E. Oliver, Columbia University, Lamont Geological Observatory, Palisades, N.Y.
Dr. Richard E. Roberts, department of terrestrial magnetism, Carnegie Institute, 5241 Broad Branch Road NW., Washington, D.C.
Dr. Edward Teller, University of California, Berkeley, Calif.
Dr. Harold Urey, Scripps Institute of Oceanography, University of California, La Jolla, Calif.
Dr. David Inglis, Argonne National Laboratory, Lemont, Ill.
Dr. Lloyd V. Berkner, Graduate Research Center of the Southwest, Dallas, Tex.

APPENDIX 10

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C., March 15, 1962.

Hon. WILLIAM C. FOSTER,
Director, U.S. Arms Control and Disarmament Agency,
Washington, D.C.

DEAR MR. FOSTER: The Joint Committee on Atomic Energy on July 25, 26, and 27, 1961, held hearings on the developments in the field of detection and identification of nuclear explosions (Project VELA).

Printed copies of the hearings and a summary-analysis are about to be released by the committee. Advanced copies of the publications have been made available to the U.S. Arms Control and Disarmament Agency.

I would appreciate it if you would advise me of any significant change that has occurred within the knowledge of your Agency of developments in the field of detection and identification of nuclear explosions since the committee's hearings July 25–27, 1961, so that this may be included in the committee's publication.

Thank you for your assistance in this matter.

Sincerely yours,

CHET HOLIFIELD, *Chairman.*

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C., March 16, 1962.

Dr. GEORGE F. BING,
Assistant Director, Nuclear Test Detection Office, Advanced Research Projects Agency, Department of Defense, Washington, D.C.

DEAR DR. BING: The Joint Committee on Atomic Energy on July 25, 26, and 27, 1961, held hearings on the developments in the field of detection and identification of nuclear explosions (Project VELA).

Printed copies of the hearings and a summary-analysis are about to be released by the committee. Advanced copies of the publications have been made available to the Advanced Research Projects Agency.

I would appreciate it if you woud advise me of any significant change that has occurred within the knowledge of your Agency of developments in the field of detection and identification of nuclear explosions since the committee's hearings July 25–27, 1961, so that this may be included in the committee's publication.

Thank you for your assistance in this matter.

Sincerely yours,

CHET HOLIFIELD, *Chairman.*

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C., March 21, 1962.

Dr. GEORGE F. BING,
Assistant Director, Nuclear Test Detection Office,
Advanced Research Projects Agency,
Department of Defense, Washington, D.C.

DEAR DR. BING: Reference is made to my letter of March 16, in which I requested you to advise me of any significant change that has occurred in the field of detection and identification of nuclear explosions since the Joint Committee on Atomic Energy held public hearings on this subject July 25–27, 1961.

Recently newspaper articles have appeared containing various statements attributable to British courses reflecting technical breakthroughs in the field of seismic detection and identification.

In responding to my March 16 letter, I would appreciate it if you would advise me as to any significant new technical advances that have been made or technical changes that have occurred, particularly with regard to the basis for the British statements.

Thank you for your assistance in this matter.

Sincerely yours,

CHET HOLIFIELD, *Chairman.*

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C., March 21, 1962.

Hon. WILLIAM C. FOSTER,
Director, U.S. Arms Control and Disarmament Agency,
Washington, D.C.

DEAR MR. FOSTER: Reference is made to my letter of March 15, in which I requested you to advise me of any significant change that has occurred in the field of detection and identification of nuclear explosions since the Joint Committee on Atomic Energy held public hearings on this subject July 25–27, 1961.

Recently newspaper articles have appeared containing various statements attributable to British sources reflecting technical breakthroughs in the field of seismic detection and identification.

In responding to my March 15 letter, I would appreciate it if you would advise me as to any significant new technical advances that have been made or technical changes that have occurred, particularly with regard to the basis for the British statements.

Thank you for your assistance in this matter.

Sincerely yours,

CHET HOLIFIELD, *Chairman.*

ADVANCED RESEARCH PROJECTS AGENCY,
Washington, D.C., March 26, 1962.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
House of Representatives.

DEAR MR. CHAIRMAN: Your letters of March 16 and 21, 1962, requested information on any significant changes in the technical aspects of nuclear test detection that have occurred since the Joint Committee on Atomic Energy hearings of July 1961. I will attempt to summarize what in my judgment are the major developments of the past 8 months. Also included is a brief summary of recent British research efforts in underground nuclear test detection and my evaluation of their significance.

Although there has been steady progress in VELA HOTEL and VELA SIERRA research, there have been no substantial technical changes since last July. We have selected the VELA HOTEL spacecraft contractor and are well on our way toward a first launch in the summer of 1963. This letter addresses itself only to underground test detection, or VELA UNIFORM.

Resumption of underground nuclear testing by the United States has had a major impact on the VELA UNIFORM program. Although we had planned a VELA nuclear explosion program, there had always been an air of unreality about it. Testing has given us the opportunity to obtain extensive data on underground explosions of various yields and in different media and locations. Since September 15, 1961, we have had over 20 underground explosions and are perhaps as embarrassed by the overabundance of data at this time as we were by its paucity before. Our ability to obtain this data was a direct consequence of the capital investment in detection equipment made prior to resumption. It must be admitted that data acquisition is far more rapid than data analysis; at this time, only the highlights of the results from tests to date are available.

In summary they are:

(*a*) Explosions in volcanic tuff give results similar to Rainier and Hardtack II experience.

(*b*) Explosions in desert alluvium on the Nevada test site give signals substantially less than those from tuff for the same yield. Alluvial shots are typically followed by cavity collapse some minutes later giving rise to a fairly strong and distinct seismic signal and leaving a characteristic surface crater several hundred feet across.

(*c*) A single explosion in granite gave signals about 1½ times the amplitude of those expected from tuff. The character of the signals was somewhat different, also.

(*d*) The GNOME explosion near Carlsbad, N. Mex., was perhaps the best observed seismic event in history. Signals from this explosion in salt were detected over most of the United States and at many foreign seismic observatories. Perhaps as interesting as the detection at distant stations was the fact that many equally good stations saw nothing. This is apparently attributable to a strong azimuthal variation in propagation conditions. Several determinations of epicentral location of GNOME have been made using a simulated "Geneva" spacing of stations and conventional traveltime curves. In each case, the epicenter was about 20 miles from the actual point. This dramatizes the very real need for accurate local traveltime data.

(*e*) Investigations of on-site inspection techniques have been made following many of the explosions. Many of the techniques appear quite promising and clearly indicate that erasing all signs of human activity is far from a trivial task. Such things as unexpected radioactive venting and the craters following alluvial shots are rather gross phenomena that the potential violator would have to contend with. At the same time it should be remembered that, except for attempting to minimize radioactive venting, no attempts are being made to hide explosions.

(*f*) Extensive measurements on the electromagnetic signal generated by underground explosions have been made. For tamped explosions in the low to moderate kiloton range, the method offers no promise for conventional detection beyond 20 or 30 miles. It has been conjectured that explosions in large cavities might generate larger signals detectable at greater distances. This has not been tested experimentally as yet but, on the basis of experience to date, appears unpromising.

In summary, the explosion program has given a variety of results, sometimes expected, sometimes unexpected. It is difficult to say at this time just what these results imply for the overall test detection problem. However, no single result or any obvious combination implies that test detection is either more or less difficult than previously believed.

In areas of research not directly related to the explosion program there have been no significant changes, but rather steady progress continues within the research effort. At this time deep-hole and ocean-bottom seismometers are undergoing field test and evaluation as is our first unmanned seismic station. It is too early to evaluate their performance at this time, although advantage is being taken of recording appropriate underground shots of the present nuclear test series.

The British have a small, highly centralized, and very aggressive program (TABOR PLUTO) in underground nuclear test detection which has paralleled in time the U.S. VELA program. There have been regular and reasonably comprehensive exchanges of information so that we are well aware of the status of each other's programs.

British research currently centers in three areas:

(*a*) Development of phased arrays and associated data analysis techniques.

(*b*) A modest deep-well program.

(*c*) Development of improved basic instrumentation, particularly long-period seismometers.

It is primarily the phased array work that shows most promise and is being vigorously pursued by the British scientists. Using a fairly simple crossed linear arrangement of seismometers, it is possible to determine the velocities of various seismic waves and to eliminate somewhat the effects of unwanted microseismic noise. It should be noted that these features are not unique to the British arrays. One can hope using array techniques to increase the detection capability of a station and perhaps eventually use the arrays to better characterize the total seismic signal so that discrimination between explosion and earthquake generated seismic signals may be improved.

The single station depth of focus determination method which British scientists have recently been investigating offers an interesting application of array techniques. In regions of reasonably simple geology and at distances up to about 1,000 kilometers, the seismic records from deep and shallow events should be different depending on the relative arrival times of certain seismic waves traveling with different velocities along different paths. The practical problem of recognizing the various waves (or phases) in a complicated seismogram limits the utility of the method when only visual interpretation of the record is possible. The phased array, however, is particularly useful in improving the recognition of waves of varying velocity. The ultimate effectiveness of this method for determining depth of focus has not been evaluated as yet, but the phased array is an essential element in improving the method.

Following the resumption of testing by the United States, a site near Laramie, Wyo., was made available to the British. They have installed a phased array on the site and have obtained seismic data on a number of underground explosions and some earthquakes. As with ourselves, only a limited amount of data has been analyzed at this time and any final evaluations would be premature.

I know of no single development in British seismic detection research that could be termed a breakthrough, nor do I feel that the British scientists would claim that there has been. They have a vigorous research program with many hopeful possibilities but only limited experimental results. As is the case with the U.S. VELA program, it is still too early for final evaluations of the research results.

I expect that many of the VELA research efforts will reach fruition in the course of the next 6 months and that, in particular, the implications of the explosion program will become more clear, ARPA will, as we have in the past, inform the Joint Committee of all interesting results as they occur. If I can provide further information to you at this time, please let me know.

Sincerely,

GEORGE F. BING,
Assistant Director, Nuclear Test Detection Office.

U.S. ARMS CONTROL AND DISARMAMENT AGENCY,
OFFICE OF THE DIRECTOR,
Washington, March 27, 1962.

HON. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy.

DEAR MR. CHAIRMAN: This is in reply to your letters of March 15 and 21 in which you requested that we advise you of any significant developments that have occurred in the detection and identification of nuclear explosions since your Committee held public hearings on this subject on July 25–27, 1961. Your letter of March 21 indicates your particular interest in the detection and identification of underground explosions in view of press statements which imply breakthroughs in this field. Our information in this area stems from recent discussions with several U.S. scientists including Mr. Doyle Northrop and Dr. Carl Romney, AFTAC; Dr. Frank Press, CIT; and Dr. George Bing, ARPA.

So far as we are aware, there have been no major breakthroughs since your hearings although a great deal of new experience has been gained. British statements concerning improvements in the detection and identification of underground events are based in large measure upon a technique utilizing a phase-correlated, 12-element, seismic array. It was tested at an experimental seismic station operated by the British near Laramie, Wyo., which recorded some of the recent U.S. underground tests, particularly GNOME, and one earthquake. U.S. scientists who have studied this technique agree that seismic arrays, in general, tend to improve the signal-to-noise ratio and therefore detection capability, but do not agree that there is evidence to support the contention that they will substantially improve the capability to identify the source of a detected disturbance. Our scientists have found that only very strong earthquake can be identified as natural events by present techniques. The large number of small, shallow disturbances which occur annually in the U.S.S.R. and other countries will inevitably remain in the questionable category unless investigated by on-site inspection teams.

As the result of over 20 underground nuclear explosions conducted by the Atomic Energy Commission since the date of your hearings, considerable experience has been gained with detection and identification methods. These tests were extensively instrumented as part of Project VELA UNIFORM and an examination of the site of each test was made after the explosion. Although much of data thus produced is still being analyzed, certain significant conclusions can be formed at this time.

The long-period seismic waves which were at one time expected to provide a method of identifying a nuclear explosion were found to be so small as to suggest that they will be barely detectable even from short distances. In the case of the GNOME shot it also was established that seismic signal velocity varies azimuthally around the energy release point. This leads to the conclusion that in some instances this velocity is dependent to an unexpected extent on the path of propagation. This conclusion could have a serious effect on the technique of locating the disturbance by triangulation until signal travel times in various regions become better known.

It also appears that signal attenuation rates vary with the path of propagation. Moreover, the strength of the seismic signal is strongly influenced by the medium in which the explosion occurs. This was suspected before the recent tests but we now have evidence concerning several different media. For example, it is now known that the intensity of a seismic signal from an explosion in salt is two or three times as strong as the same yield explosion in Nevada tuff, but that in certain other media the signal can be severalfold smaller than that in tuff.

Nonseismic techniques so far have failed to provide any encouragement for application to detection of underground nuclear explosions. For example, it now appears that the electromagnetic signal resulting from small underground nuclear explosions probably will not be detectable for more than tens of kilometers from the site of the explosion.

In a general way research on techniques of on-site inspection has been encouraging. For example, the difficulty of containment of radioactive products from underground explosions has been demonstrated by the fact that unexpected venting of small amounts of radioactivity has taken place. In addition, following explosions in certain media, large and fairly characteristic de-

pressions have formed on the surface. These would simply the on-site inspection problem for explosions in these media provided the event is located by seismic means.

Techniques for determining the depth of focus of earthquakes and explosions hold some promise. If, as expected, many earthquakes occur at depths well below those attainable for manmade explosions, a useful technique for discriminating between explosions and deep earthquakes may become available. However, to date depth of focus determinations have had associated with them such large uncertainties that in many cases it has not been possible to say with much certainty that the origin was at a depth below that reasonable for a nuclear test.

The British and other techniques for arranging arrays of seismometers and correlating the data from the different seismometers in such arrays also hold promise of improving epicenter and depth of focus determinations, though those improvements will probably be strongly dependent on properties of the materials in the earth's crust. Inevitably, technical improvements such as the use of deep wells for seismometer installation will gradually improve our capability by raising the overall state of the art.

Our scientists have concluded that our present capability to detect and identify underground nuclear explosions remains at substantially the level that was predicted for this time when Project VELA began. Much new knowledge has been acquired. Some improvements seem probable but new difficulties have been revealed. Various techniques hold promise for the future but no major breakthroughs have occurred.

It is for these reasons that Secretary Rusk's March 23 speech to the Geneva Disarmament Conference contained the following statement:

"Although American scientists have for the past several years been actively seeking new methods of detection and, even more, of identification of possible nuclear explosions, and although there are some promising avenues of investigation which may be proven in the next few years, the fact is that very little has been discovered up to date to justify a significant modification of the conclusions and recommendations of the Geneva scientists of 1958."

Sincerely,

ADRIAN S. FISHER, *Acting Director.*

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