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USAF ORAL HISTORY PROGRAM
Interview # K239.0512-685
Mr. Doyle L. Northrup
24 July 1973

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Interviewee

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1. Land Radio Attack (SH)
2. Torpedoes (SH)
3. Nuclear Detonation (SH)
4. Nuclear Detonation - Detection and Reporting (SH)
5. Nuclear Detonation, Russian (SH)
6. Air Force Technical applications center (T)
7. Nuclear Weapons - Testing
8. Satellite sensors proposed 1956-57 (T)
9. Electromagnetic pulse detection systems
10. Geneva Nuclear Test Ban Conference (1958) (T)
11. Limited Test Ban Treaty, 1963 (T)
12. VELA satellites (T)

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31, 1981

BIOGRAPHICAL SKETCH

Doyle L. Northrup

Doyle L. Northrup was born December 30, 1906 in the State of Washington and lived there until 1928. In 1934, he married Sybil Louise Crosby of New Bedford, Massachusetts. ~~The Northrups~~ presently reside in Alexandria, Virginia.

While an undergraduate student at Whitman College, Walla Walla, Washington, in 1928, Mr. Northrup was elected to Phi Beta Kappa. He received a Bachelor of Science degree "With Honors" from Whitman in 1929. In 1932, he was awarded a Master of Science degree from Massachusetts Institute of Technology.

From 1932 to 1940 he was a Research Associate with the Physics Department of Massachusetts Institute of Technology. He was a member of a four-man technical group which developed the Van de Graaff electrostatic generator and demonstrated its application as a valuable tool in nuclear research.

From 1940 to 1948 Mr. Northrup was associated with the Naval Ordnance Laboratory. Early in this period he served as Chief Physicist for the Pearl Harbor Magnetic Proving Ground. His brilliant work in supervising the design and installation of submarine detection defenses in Pearl Harbor and Honolulu Harbor brought him high commendation from the Navy. In 1945 the Navy awarded him its Meritorious Civilian Service Award for contributions to naval torpedo development. When

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Mr. Northrup terminated his services with the Navy in 1948, he was Acting Chief of the Torpedo Division, Naval Ordnance Laboratory, Washington, D. C.

Mr. Northrup has been with the United States Air Force since [redacted] 1948. During this period he has directed the research efforts of multiple governmental, military, and civilian agencies engaged in a highly classified scientific program of great significance to the security of the United States. The program has embraced the fields of electricity, magnetism, seismology, physics, chemistry, electro-acoustics, acoustics, nuclear physics, optics, electrical engineering and electronics engineering. The selection of Mr. Northrup as technical advisor to the United States Delegation at the 1958 and 1959 Geneva conferences on Nuclear Test Suspension is further indicative of his national recognition in the scientific community and his comprehension of national policy objectives.

Mr. Northrup has twice received the Air Force Exceptional Civilian Service Award in recognition of his scientific achievements and outstanding service to the nation (see Attachments 1 and 2). In April 1958 he was awarded the Department of Defense Distinguished Civilian Service Award (see Attachment 3). On August 15, 1958 the Honorable Lewis L. Strauss, then Chairman of the Atomic Energy Commission, wrote the following commentary on Mr. Northrup:

"I have the highest personal regard for Mr. Northrup and for his contributions to the success of numerous technical projects of highest national security. He has continuously demonstrated his cooperation and outstanding ability to translate the scientific problem into carefully planned and flawlessly executed projects ..... Mr. Northrup has earned the highest respect of the scientific elements of the commission and its associated organizations."

In January 1959 Mr. Northrup was the recipient of the coveted "President's Award for Distinguished Federal Civilian Service" (see Attachment 4). This award is the highest honor available to civilian career personnel of the United States government. The President makes no more than five such awards in any one year.

Mr. Northrup is the author of numerous technical and scientific publications (see Attachment 5). He has been a member of the American Physical Society since 1934.

6 Atchs
1. Air Force Citation
2. Air Force Citation
3. Department of Defense Citation
4. President's Citation
5. List of Publications
6. Photograph - Boyle L. Northrup

COPY

EXCEPTIONAL SERVICE AWARD

CITATION

DOYLE L. NORTHRUP has rendered outstanding service to the Department of the Air Force and to the United States Government while engaged in a project of national significance. As the scientific administrator, Mr. Northrup was responsible for the direction and coordination of the efforts of all scientists assigned to the project as well as numerous other individuals and agencies both within and outside the Department of Defense. His outstanding leadership, scientific knowledge, and zealous devotion to the task contributed immeasurably to the evolution of methods and the development of technical refinements essential to the early success of the project. Mr. Northrup's brilliant research in a scientific field hitherto unexplored resulted in the accomplishment of a mission of vital importance to the national security.

HOYT S. VANDENBERG
Chief of Staff, USAF

THOMAS K. FINLETTER
Secretary of the Air Force

(Awarded 23 May 1951)

COPY

EXCEPTIONAL SERVICE AWARD

CITATION

Mr. Boyle L. Northrup has rendered outstanding service to the United States government and brought great honor to the Department of the Air Force. He was responsible for directing and coordinating the efforts of scientists within and outside the Department of Defense and for designing and evaluating world-wide technical operations. His efforts contributed immeasurably to a program which culminated in an accomplishment of international importance in the fall of 1953.

NATHAN F. TWINING
Chief of Staff, USAF

HAROLD E. TALBOTT
Secretary of the Air Force

COPY

SECRETARY OF DEFENSE
WASHINGTON, D. C.

CITATION

The Department of Defense Distinguished Civilian Service Award is presented to Doyle I. Northrup in recognition of his pre-eminent service to the Department of Defense during the period February 1943 to November 1957. As technical director of an important Air Force organization, Mr. Northrup directed and coordinated the efforts of multiple military and civilian agencies in a scientific program of great national significance involving techniques and analyses on the frontiers of modern science. His technical knowledge, managerial skill, and selfless devotion to duty contributed immeasurably to the successful conduct of a program of vital significance to the security of the United States and warrant the highest recognition which can be given a civilian employee by the Department of Defense.

(Presented by Secretary of Defense Neil H. McElroy)

COPY

CITATION

The President's award for Distinguished Federal Civilian Service is given to Boyle L. Northrup with profound appreciation, highest esteem and great personal satisfaction. By the leadership and counsel he has furnished to the scientific efforts of military and civilian agencies in the development of our system of nuclear detection and surveillance, he has made immeasurable contribution to the security of the United States.

His expert knowledge has also provided an indispensable basis for international negotiations in the interests of world peace.

PUBLICATIONS

1. "Design, Operation and Performance of the ~~Round Hill~~ 10,000,000 Volt Electrostatic Generator by L. C. VanAtta, D. L. Northrup, C. M. VanAtta and R. J. Van de Graaff, Phy. Rev. - pp. 761-776, May 15, 1936.

2. "Design of Generating Voltmeter for Measurement of High DC Voltage" by D. L. Northrup and L. C. VanAtta, Amer. Phy. Soc., May 1937.

3. "Electrostatic Generator for Nuclear Research at M.I.T." by D. L. Northrup, L. C. VanAtta, R. J. Van de Graaff and J. S. Clark, American Physical Society (New York), February 1940.

4. "Production of High Energy Positive Ion and Electron Beams" by C. M. VanAtta, R. J. Van de Graaff, L. C. VanAtta and D. L. Northrup, Amer. Phy. Soc., N. Y. Meeting, February 21, 1940.

5. "Irradiation of Deuterium, Beryllium and Indium Nuclei by 2.0 Mev X-rays" by D. L. Northrup, L. C. VanAtta, R. J. Van de Graaff and C. M. VanAtta, Amer. Phy. Soc., Pittsburgh, June 20, 1940.

6. "Construction and Cooling of Large Vapor Traps" by D. L. Northrup, C. M. VanAtta and L. C. VanAtta, R. S. I. VII P. 207, 1940.

7. "Measurement of Roentgen Ray Production in the Range 0.8 - 2.0 Million Volts" by L. C. VanAtta and D. L. Northrup, Amer. Journal of Roentgen, V. XLI, p. 633-6, April 1939.

8. "Design, Construction and Operation of Pearl Harbor Magnetic Proving Ground" by D. L. Northrup (Classified NOL Report) January 1942.

9. "Design, Construction and Installation of Acoustic Range for Torpedoes" by D. L. Northrup (Classified NOL Report) 1945.

10. Twenty-three classified Technical NOL Memoranda on magnetometer, design, recording inclinometers, submarine detection installations at Pearl Harbor, torpedo depth recorders, torpedo exploder mechanisms, torpedo depth control problems, 1942-1946.

11. Five Comprehensive Naval Technical Mission Reports covering a Survey of German Torpedo Design and Development. 1945.

fds

DOYLE L. NORTHRUP

Biographical Sketch

Born: 1906, Colfax, Washington

Education: Whitman College, B. S. 1928, Phi Beta Kappa
Mass. Inst. of Tech., M. S. 1932

Married: 1934 to Sybil Louise Crosby of New Bedford, Mass.
Mr. & Mrs. D. L. Northrup now [illegible] on
Professional Career: South Milbrand Beach.

1933 - 1940	Res. Assoc. Physics Dept, MIT
1940 - 1948	P-5 to P-6 Naval Ordnance Lab, Submarine detection, magnetic proving ground Pearl Harbor, Torpedo Exploder Research
1948 - 1949	Deputy Technical Director, AFOAT-1
1949 - date	Technical Director, [redacted]

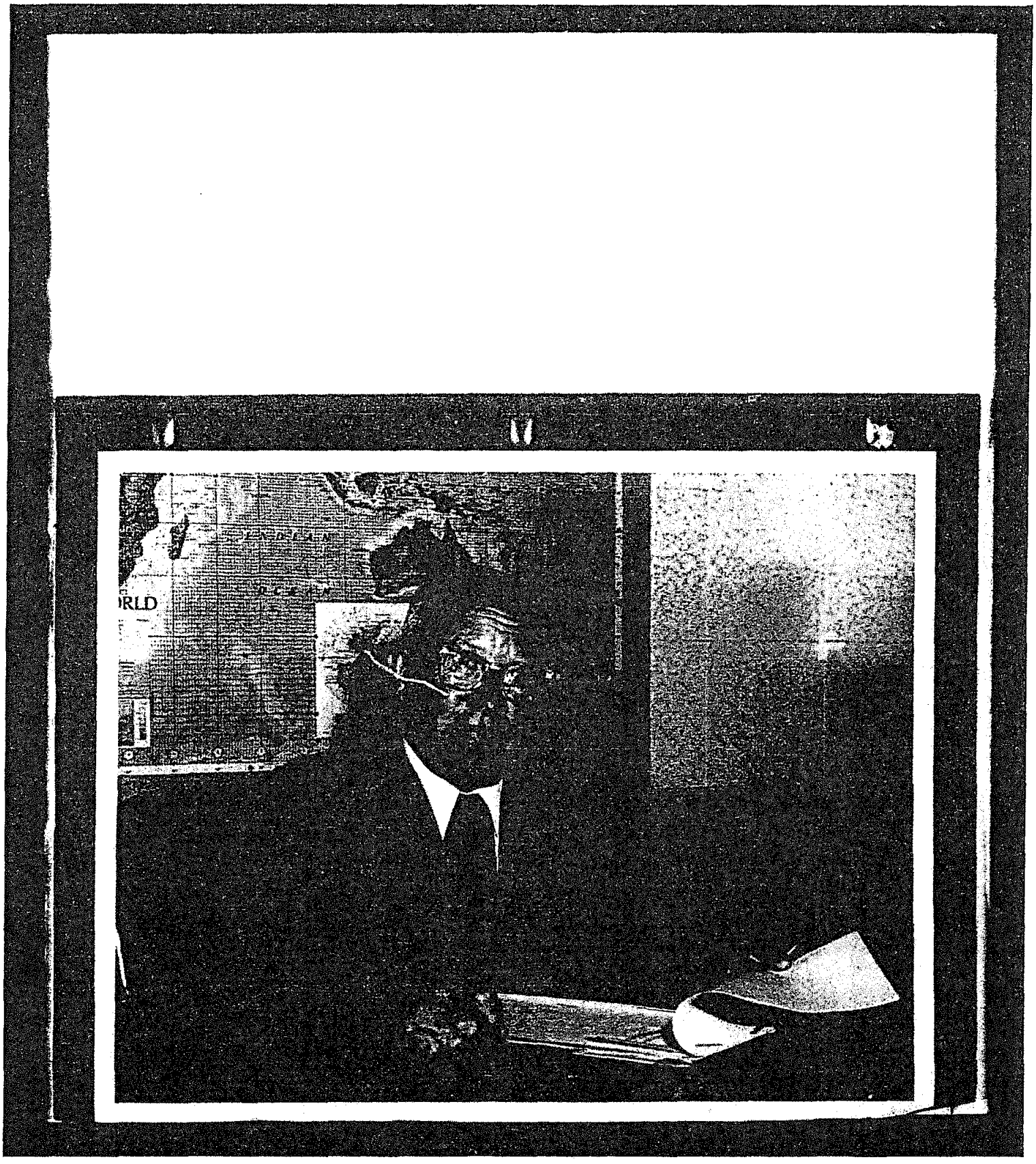
Consulting:

1958	DOD Rep Geneva - Experts Conference - Test Ban
1958-59	DOD Rep Geneva - Political Conference - Test Ban
1960	Technical Consultant to Joint Committee on Atomic Energy - Technical Aspects of Detection and Inspection Controls of a Nuclear Weapons Test Ban
1961	Technical Consultant to Joint Committee on Atomic Energy - Developments in the Field of Detection and Identification of Nuclear Explosions (Project VELA) and Relationship to Test Ban Negotiations

Awards:

1945	US Navy Meritorious Civilian Service
1950	USAF Exceptional Civilian Service
1954	USAF Exceptional Civilian Service
1958	DOD Distinguished Civilian Service
1959	President's Award - Distinguished Federal Civilian Service

Item of interest



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TAPE AND TRANSCRIPT ACCESS AGREEMENT

The following stipulations constitute the agreement regarding the tape(s) and transcript(s) resulting from the interview session(s) that I participated in with representatives of the Air Force Historical Program on 24 July 1975.
Day Month Year

(Please initial the appropriate spaces.)

Security Classification

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__X__(2) The transcript(s) and tape recording(s) may be cited or quoted only with the express permission of the interviewee.
_____Permission from the heirs also must be obtained.

_____(3) Permission must be obtained in writing from the interviewee before the transcript(s) can be examined or the tape recording(s) audited.
_____Permission from the heirs also must be obtained.

_____(4) The transcribed interview(s) and the tape recording(s) will be sealed until a time herein specified by the interviewee.

(This may be until death of interviewee or for any specified number of years.)

_____(5) Other requested action(s)_____

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Daryl L. Northrup
Interviewee's Signature

24 July 1975
Date

Kenneth K. Coffey, Jr.
Witness' Signature

24 July 1975
Date

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3 Jan 73 (SECURITY CLASSIFICATION AS REQUIRED)

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UNITED STATES AIR FORCE

ORAL HISTORY PROGRAM

Interview #K239.0512-685

of

Mr. Doyle Northrup

By

Lt Col Lyn R. Officer

Date: 24 July 1973

Location: Patrick AFB FL

NOTE: Pen and ink changes made by editor.

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Oral History Interview #685
24 July 1973
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Taped Interview with Mr. Doyle Northrup
Conducted by Lt Col Lyn R. Officer

O: Mr. Northrup, if we could, to begin the interview, would you go back and give us a little of your background leading up to the beginning of your work with the US Navy at Pearl Harbor. In other words, I'm looking for a little bit of background information, like what schools you attended, the kind of degree you acquired, this sort of thing. Some of these questions may be a little redundant, seem redundant, but we ask them because we don't have them on tape, and even though they may seem a little trite, we still like to get it on tape because it may not be recorded anywhere else. So if you would like to begin, sir, give us a little of your background leading up to your tour of duty at Pearl Harbor.

N: Okay. I got my college training in mathematics and physics at Whitman College in Walla Walla, Washington, where I served as an instructor of physics for the last two years of my undergraduate work and in fact stayed on for one additional year there in order to acquire sufficient funds to go back to MIT to pursue my graduate work. So I was an instructor of physics there for two years as well as getting my B.S. degree in mathematics and physics. Then

I went back to MIT and registered for one year toward a Master's degree in communications engineering. As it turned out, the funds situation was such that I was forced to work part-time in the department there, in the Electrical Engineering Department, instructing, and it dragged out to two years before I received my Master's degree in communications electronics. Then, as you recall, the Depression had hit, and at that time it was very difficult to find work. So I accepted a position in the MIT research facility that's down at Round Hill just south of New Bedford, Massachusetts, on the estate of Colonel E. H. R. Green, the eccentric, multi-millionaire son of Hetti Green, you know, of Wall Street fame in the 1880's. He was taking out his hobby of interest in science by providing about $20,000 a year to MIT to carry out various researches that needed a field station right on Buzzards Bay. There was an airport there so that there were all sorts of experiments possible. I was there first under Dr. Johnson doing work on high gain amplifiers, DC amplifiers. And when the Van de Graff project was moved to Round Hill, into the old blimp hangar there, where a high voltage Van de Graff generator of 10 million volts was to be constructed, I transferred then to the Physics Department, which had been my early interest at Whitman, and went on toward a Master's degree, finished the work for what was to be a Doctor's degree. I completed the thesis, which was accepted and published in the Physical Review. And for reasons of economy, of finances really, I didn't go on to get the remaining courses that I needed

for a Ph.D. I needed two years of German, I believe, and I just didn't ever bother to get that.

Q: Of interest here, sir, what was the subject of your thesis?

N: It was the development of the high voltage generator, 10 million volt electrostatic generator, the engineering that had to do with its development. This was both voltage measurement up to 10 million volts, which never had been done before, as well as the development of a new belt-charging arrangement which replaced that used by Van de Graff, which had used paper belts which are extremely sensitive to humidity. I converted that to a laminated rubber fabric belt that I got Goodyear to produce, and we got much more reliable performance out of those big generators. The generator then in the next few years, as I worked as a research associate at MIT--a research assistant and finally a research associate with the rank of assistant professor from the years 1932 to 1940. And during that time, we moved the generator from Round Hill to Vassar Street just behind the old MIT complex and built it in a large steel semi-spherical building that was built especially for the generator, combining, instead of having two separate terminals, which was the original design, each one mounted on a heavy railroad car truck that ran back and forth on rails, and each electrode was 15 feet in diameter supported by a 6-foot diameter textillite column. These two columns running up to the 15-foot spheres in which you

could actually stay when the thing was at high voltage. One would be plus 5 million volts and the other minus 5 million volts, and there were 10 million volts between. I recall on the display when we had a bunch of newspaper reporters down from Boston and Dr. Compton made an introductory address having to do with the purposes of the electrostatic generator, which was to be one of the early tools for a high energy physics research for accelerating fundamental particles to high energy. And I had the reporter from the Boston Globe up in the sphere with me, and we had little probe electrodes we would stick out of the manhole and draw a 10 million volt spark, see. When it hit this grounded terminal, that was grounded through the sphere. But the reporter and I were both at 5 million volts above ground, and this greatly reduced his curiosity. He stayed in the dead fixed center of the sphere the full time that we were putting on the demonstration. Well, after conducting a good bit of research in the subject of high voltage generator production and the measurement of high voltages, we also did some research on the acceleration of electrons down a 2-1/2 million volt vacuum tube after we moved the generator to MIT, and I published a few papers on that subject prior to my departure from MIT in 1940. An old friend of mine from MIT had been called down to Washington at the early part of 1940 to develop some means of protecting the United States naval vessels from magnetic mines which the Germans had developed in the early part of the war. He was given the Naval Ordnance Laboratory, which at that time constituted three people, Dr. Cogshell [sounds like]

and a couple of assistants who helped him develop mine mechanisms, was the entire laboratory, located in the Naval Gun Factory in Washington DC. And Ellis Johnson, who was the first technical director of the Lab, was given the job of assembling a technical staff and augmenting the ability of the Naval Ordnance Laboratory to develop naval ordnance along modern technical lines. I was one of the three or four early employees that Ellis recruited from MIT, and our job was to build up a competent laboratory for development of mine firing mechanisms, and later it included torpedo firing mechanisms, and also to work out a scheme for degaussing, it was called, or demagnetizing the steel vessels that comprised the fleet so that when a vessel passed over magnetic mines located on the bottom, they would not energize the magnetic sensors. I was given the job when I first went to the Naval Ordnance Lab of developing magnetometers that would have the necessary sensitivity. I had been there about nine months and developed two or three magnetometers that were then utilized to make a large proving ground, and the first one was to be located in the Pacific at the naval yard, Pearl Harbor, and the other one in the Atlantic--I've forgotten now--at one of the big naval bases near Norfolk in the Atlantic. These were arrays of magnetometers placed in copper tubes that were drilled 20 feet into the bottom of Pearl Harbor in the rough shape of a ship, so there were some 600 magnetometers on the bottom. A battleship could be anchored over the top of this on either a north/south range, because we had two ranges of magnetometers, or on an east/west

range and demagnetized on both of those azimuths. The first computer that I know of, practical computer, was developed under my supervision by Bell Telephone Laboratories for recording and printing the magnetic field at each of 600 points underneath the battleship in about 10 minutes. The battleship would anchor over there, and we'd have a short time to work on it because it would have to go back out to whatever fleet maneuvers were in order. So in 10 minutes this computer would completely assay the field underneath this ship, and you would then compute what current should be placed through coils that were mounted in the ship to effect a cancellation of the magnetic field it was producing at 40 feet below the water line where the magnetometers were located. I had just about completed this when, on December 7, the Japs paid us a surprise visit, and I was called in to Admiral Furlong's office since I was the only physicist at Pearl Harbor, and he asked me if there was anything we possibly could do to prevent the miniature Jap subs which had penetrated the harbor on the 7th and had fired at Navy ships, fired torpedoes at Navy ships, if there was any way we could detect them because they were still hanging around our harbor entrance and had been observed trying to penetrate the harbor entrance. So I utilized some of the equipment that I had as excess from the magnetic proving ground I had installed. And with the aid of a Navy diver and a crew of 50 sailors, we cut up lead sheets into one foot squares, rolled it onto the magnetometer cable I had, so that it would be an underwater cable, a foot at a time, and strung

effectively two coils across the thousand yard channel of Pearl Harbor side by side so that the two of them were connected in counter opposition so that the earth field variations would balance out from the two coils. But if a submarine went underneath the water right close over the top of these two large magnetic coils, the very sensitive fluxmeter that I had used on the magnetic range also would record a little signature of its passage. We designed, installed and had this first range loop operating across the channel of Pearl Harbor on the evening of December 8, the next day after the Jap raid. It was a little harrowing because we worked all hours on it, and coming home at night in a little motor whaleboat, the Marine guards were a little trigger happy and they cut loose at us with--at anything that moved out on the harbor--with 50 caliber machine guns, and we had to go over the side several times when this happened on the way back.

O: I'd like to ask you a couple of questions here. Backing up to the demagnetization field, how long would a ship stay demagnetized once you had put it over one of those fields?

N: It would stay demagnetized for a substantial time. In fact, it never completely recovered from its correction. Then there was a policy to bring the ships in, I think, about once every six months, or if they were in harbor and had the time to do it after the war started and if they were going on special missions, then they would

be degaussed at more frequent intervals. But, in any event, they were always degaussed once every six months, and we made minor changes in the currents that they were suppose to maintain in their degaussing coils.

Q: Do you know if the Navy still maintains this system?

N: I went out there about four years ago, and the young, handsome, dark-eyed lieutenant commander who took over from me as soon as I had finished developing the range was--let's see, 1940, 1965--25 years later,was old, fat and balding, and still operating the magnetic range. That was his whole career. He had spent 25 years operating the range, and as far as I know, the Navy is still degaussing its ships.

Q: You say when you strung the two lead encased cables across, could you tell about where the submarine was passing?

N: We couldn't tell as far as back and forth across the range, but we knew that it was there. The channel was only 1,000 yards wide, and the cables were only 100 feet apart, and there were three of them. There was the first one--coming in, the submarine would cross a single cable. Then it would cross two that were laid parallel, side by side, and then it would cross a third cable. Each of the two cables were connected together at the ends, and the 7 turns

of wire in the cable were connected in series. So we had effectively a seven turn coil here and another one right adjacent to it in opposite polarity so that the earth's field produced no signal but a magnetic field of this little Jap sub would cross first the first wire, then the two wires in the middle which were the common wires of the two coils and then the other one, so you could tell whether it was coming in or going out. And we frequently observed them coming in and immediately, as soon as our warning went to the in-shore patrol, and PT boats would come out and drop depth charges, then you could see the signature as the little sub went back out again, having been frightened away. Although, we were not able to get the Navy to agree to a project I had, which was to lay a line of mines along inside the coil. And if a submarine came in there, as soon as the signature went off, we would push the button and that should bring the submarine to the surface. But they were afraid, and naturally so because--what was the big carrier--I've forgotten the name of the carrier now.

O: Enterprise?

N: The Enterprise had to go back and forth over that same channel, and they were afraid to have mines that were in the hands of civilian physicists, blowing Navy carriers out of the lot.

O: Did you have an opportunity to observe the Japanese attack itself?

N: Yes. My wife and I were having an early breakfast. We had planned a trip around the island that morning and were having an early breakfast at our little cottage at the foot of Diamond Head when we saw destroyers. We could look across Honolulu Bay and see the entrance to Pearl Harbor, and we saw destroyers coming out of there under full steam and large plumes of water coming up right alongside. We didn't know it, but they were Jap bombs that were aimed at the destroyers and were near misses. And we said, "Well, there has been talk that there was going to be an M-day, a mobilization day, and a big display of our power, our defensive power," and we were saying to ourselves, "Well, that certainly is a realistic display to have those explosions so close to the ships. I wonder how they do that." And about that time, a big plume went up right 100 yards offshore, off of our little cottage. So I went in and turned the radio on. We didn't know what that was till sometime later. And it said that Jap planes were bombing Pearl Harbor and all civilians were ordered to come to the Harbor right away to help fight fires on the ships that had been hit. So I pulled my khakis on over goose bumps and got in my car. And as I went through Waikiki and on down to Honolulu, each policeman waved me faster, so I was going over 60 miles an hour through downtown Honolulu. We got out to Pearl Harbor and I just ran my car up onto a lawn of a house nearby right outside the gate and ran through the gates just as an anti-aircraft battery at Hickam Field--across a big, high hedge from us and we couldn't see it--had cut loose at some high-flying Jap

planes that were overhead. We went down--they were putting some new drainpipes in alongside of the road inside of Pearl Harbor. We just went down those drainpipes like a bunch of squirrels. Three or four of us would run through at the same time. It turned out it was a pretty good thing we dived into these because the flak from this five-inch antiaircraft fire came down and splattered on the roads, and two or three pieces hit the pipes we were in actually. When it quieted down a little, we crawled out and went on down to Ten Ten [1010] dock, where it was just a--oh, it just made tears run down your face. We had come home on that Friday, the night before, going by all the battleships which had been brought in just the day before, and it was a very spectacular show. All those battleships anchored along Ford Island, and here they were sinking, and the Arkansas on fire, the two fighting tops of the Arkansas tipped together, the Oklahoma, I believe it was, completely capsized, the Pennsylvania--I was right beside the Pennsylvania when it was hit by a 500 pound bomb, I think it was, from a Jap plane. In fact, I saw the bomb from the time it was released, followed its trajectory down, just frozen, and then the next thing I knew I was back three tiers in a pile of armor plate that the Navy stacks up like lumber in a lumberyard, in between two piles of armor plate, watching this tremendous billowing cloud of smoke and fire going up from the Pennsylvania. Well, that gave us our immediate assignment. We went down in the dry dock and helped get the shattered bodies of the sailors out,

and those that were alive, we got them on our shoulders and took them up this iron ladder on the side of the dry dock. They had hit the Pennsylvania, but the big damage was they had hit the--I mean the big hazard was that the back of the Pennsylvania was up against the dry dock and in front of it were two destroyers. They had missed the Pennsylvania, but they hit the depth charges on the destroyers, and the depth charges went up and just raised hell. So there was a big problem with trying to get the sailors out of there before the fires went so far as to explode the rest of the depth charges that were on the rear racks of the destroyers. There was a Chinese there who did a very heroic thing. In the middle of that, and realizing that the destroyer depth charges could go up at anytime, he got one of the fire engines out, a hand pump fire engine, and hooked it up and was playing water on the torpedoes, on the depth charges that were on the back of these racks, as we went down the ladders to try and get the sailors and stuff out of there. A whole bunch of civilians had responded to the call, so we spent the day doing things like that. I finally got interested in going around to the magnetometer range where my major project was. I got authority finally to go, although they wouldn't give us any firearms because we were civilians. But we managed to get Horsman--I remember, Chief Horsman, our diver--to get his old Sprint Rusty Springfield, World War I vintage, and Ellis Johnson and I, [redacted] and Horsman went around to our place and found that a Jap

bomber had been shot down. It had been shot down and was trying to land on the canefield right in front of the shack where I had a quarter of a million dollars worth of magnetometers which I was calibrating, and he hit just a little too low and hit against the 15 foot wall in the canefield. The bomber dropped down in this mess and their bodies--but their heads came off, rolled across and hit the front of this calibration shack of mine, shattered and left two sets of brains, just as if they had been taken out by a surgeon, on the top step, three-cornered pieces of skull scattered around. One of the tough majors in my shop recovered the three-cornered pieces of shell, and he later made little brass balls and threw them into the three corners and gave them away for souveniers of Pearl Harbor, ashtrays.

O: You mentioned that the battleships and all were all lined up there. They came in, I guess, on Saturday before the attack on Sunday. Is that correct?

N: They came in on Saturday, and the theory was that there was a Russian that was coming through on his way back to Russia--he had been visiting the President of the United States--and that the battleships were brought in to impress him.

O: Well, had they ever done this before, brought them all in at one time?

~~SECRET~~

N: I had never seen more than one battleship at a time from July of 1941 until the Pearl Harbor day itself.

Q: Then it appeared that the Japanese just happened to luck out to catch them all in there at once.

N: Either that or their intelligence was good enough that they knew we were planning to do this. And I suspect the latter because there was a drafting room in the top of the Navy building manned entirely by Japs, and they were, by and large I think, friendly, loyal Americans. But I am sure there were some of them that had been placed there by the Japanese. And I went down where this bomber dropped with Commander Boyd, who was our officer. When I called him and told him that it was over there, he came around, and he and I went down and picked up the bodies and took off any insignia, off their uniforms, and various of their personal belongings. And we found a map on one of the pilots of Hickam Air Force Base, and it had a parking lot on it that had only been completed two weeks before the raid and every parking space was indicated carefully by a little rectangle that was exactly accurate, and they had that kind of detail. [Mr. Northrup starts talking here about a Japanese mini-sub that was sunk and discovered on the bottom of the bay at Pearl Harbor.] They had in their chart case, chart number 87, a restricted secret chart that applied to the area of the entrance to Pearl Harbor. It was a one mile square that was not charted on

any of the posted geodetic survey charts. It was a classified chart. Secret I think it was. Had been printed in 100 copies. And they didn't have a photostat of it. They had copy number 8? they had gotten from somewhere. And I went down into the Jap sub because I was the smallest one there, the only one in the party that could get through the conning tower, and got the two--we had located this sub incidentally with our magnetometers, and then the divers had pulled it up. I went in and got the two crewmembers and pushed them out through the conning tower, their bodies, and got the chart case and all, anything else that was of intelligence value. And I was convinced of the fact, just from the little we saw there, that they had very good intelligence before the raid and probably knew exactly what was planned as far as the fleet was concerned.

O: Going back to the raid itself, did it appear to be one long, continuous raid, or did it appear that they came in waves?

N: I wouldn't know too well. I was in the process of going from my house in Waikiki to Pearl Harbor during the raid, and I got there just as the main part of the raid must have been over. I came in in time to see one torpedo plane come in and drop its torpedo. The lash-up to the plane malfunctioned so that the torpedo stern dropped but the nose didn't release and the plane was flying along with this torpedo hanging down with its motor running, and then all of a sudden

it was caught in the cross-fire of two Lewis guns that were set up on the dry docks the Pennsylvania was in, and it just exploded. And all we saw was just pieces flying everywhere. So that was about the last real action, and that one occurred something like, I'd say, 8:30 or 8:45, and I understand the first wave of planes came over about 7:30 or 7:45. We saw them going overhead. My wife and I looked up and saw what I know now must have been one flight of the bombers going in, but we didn't look at them closely enough, and they were pretty high going over Diamond Head. Didn't notice the orange insignia or have any reason to believe they were anything except military planes of US vintage.

Q: In this modern age, there is a great deal of controversy about collateral damage. Did it appear that the Japanese were not at all concerned about collateral damage? Did they actually attack targets that were not military targets?

N: As far as I could tell, they attacked no targets at all in downtown Honolulu. I understand a bomb dropped on the Punahou School. I read later reports. And I think most of us who saw the thing felt that probably was an accidental bombing. They were concentrating on Pearl Harbor as far as any of us could tell. And this plume of water that went up opposite our breakfast table out on the beach was from the Pennsylvania which was firing 5 inch shells flat trajectory at these low-flying Jap planes, and this had just looped up over 10 miles to

our place and happened to hit just in front of our house. And that was what triggered me to the fact that this wasn't just a play exercise going on.

O: Was there any damage to your project, to the one that you had, the degausser project?

N: No, no damage to that. The bomber that came in there had been hit by, I understand, the Curtiss Wright, which is a sea plane tender. It was moving out, and it was firing antiaircraft guns at this bomber as it came up from Ford Island. And they hit him, and he had circled then and tried to come in and that brought him in right on our project or right on the canefield there. He was trying to land in the canefield, I'm sure, but the canefield dropped off sharply, about a 15 foot vertical coral cliff, and he just ran into that coral cliff. Didn't quite get enough altitude to get in.

O: How long did you stay working with this project after the War started?

N: Well, after the war started, we dropped that project. It was completed anyway, and I had written up all the final things, and Admiral Nimitz had been there and okayed the installation and inspected it and accepted it. I had turned it over to this other--I was just getting my last reports in before going back to the States. And Admiral--he was the commandant of the Navy yard at the time--called

me in and asked me if I could do something about this Jap submarine hazard. So we took the next year--well, first we put in the first loop on the 8th of December, as I indicated. Then we worked on loops at two other places across the channel itself, one out at what was called Two Station, which was just a building on piles about a half mile off the main shore in front of Fort Kamehameha. We put a loop across there, and then we put one at the inshore indicator net. So there were three places where these little Jap subs could be picked up. We trained the sailors in the operation of this and turned it over to them so that they operated it completely, and then all we did was to repair it if it was damaged and to interpret the data and help them make it operate effectively. Then I was given the job from about March on of putting in the permanent nets because it turned out that the Bureau of Ships had cable and a system exactly like the one I had designed in a hurry except it was heavy, well-designed submarine cable. And I put that installation in offshore in front of the harbor entrance and got it hooked up and put in an acoustic system as well, which was really my first introduction to the whole subject of detection at short range as opposed to this long range detection that we got into with the Air Force project that I joined in 1943.

Q: Did you work with the torpedo laboratory out there also?

N: Not until I returned to Newport. When I came back to Washington

after having completed this tour at Pearl Harbor, Ellis [Johnson], who was still in charge of the Naval Ordnance Lab, sent me up to Newport in charge of a small group to look into the torpedo business because they were having reports from submarines that the torpedoes were running, arming, hitting the Jap ships and not exploding. And he put me into the job of finding out what was going on. So I went up and looked at the mechanical torpedoes and found the most fantastic thing that had happened. You could never believe. The mechanical exploder mechanism that weighed 90 pounds had been built to fire an inertia ring when the torpedo would run into a hard target. An inertia ring that was horizontal would be displaced off balance, and this would trigger a Rube Goldberg mechanical thing that would start a firing pin down a pair of rods that were perpendicular to the impact or the shock of the torpedo, and it was supposed to get down here and hit a detonator, and that would fire the 300 pound torpex head of the torpedo. And I thought to myself, "Well, if those have to be impacted and this thing has to get 4 inches down here, I'll bet it's never getting down to the thing." So I got Harold Edgerton, who was a friend of mine at Tech--he's the famous high speed photography guy--at that time he was a young student or instructor at Tech--to bring his high speed camera down. And I sat up a concrete target in front of a torpedo tube, and we mounted the torpedo with a warhead on it with the explosive replaced by simulated material that was mechanically like torpex, which was used in the torpedoes. We fired this head

with a full blast of air from the torpedo tube against this heavy, massive concrete and steel target that was just a few feet ar from the front end that simulated the conditions of a torpedo hitting the side of a heavily armed battleship. The high speed photography wasn't really needed, but it showed that what happened was that before the torpedo was collapsed more than about six inches, this entire 90 pound mechanical detonator was ejected from the warhead and fell just out of the warhead. When we inspected the mechanism afterward, just as I had guessed, this little firing pin only got halfway down the rails before it bent over under the impact. And so it never fired the detonator. So I went down to Washington and told Ellis about it and said, "Look, if you put a little ball switch in here with a pair of contacts and a little ball behind it, that's all you need, and it won't weigh more than 3 or 4 ounces." Ellis turned the job over to the Mechanical Exploder Section of the Naval Ordnance Lab that had been developed at that time. They developed a little ball switch, sent it up to Newport to be tested. I took a picture of it, took the complete set of drawings of it, and sent the picture and the drawings out to Ellis Johnson, who was at that time at Pearl Harbor working in the submarine base. He took the drawings down to the shop and had 50 of these ball switches made, mounted on Mark-14 torpedoes, and the next patrol that went out went out with Mark-14 torpedoes with the old detonator inactivated and this new little ball switch replacing it. We knew damn well you'd never get the Bureau of

Ordnance to move fast enough to get it out to the fleet. And all of a sudden our Navy submarine force started sinking Jap shipping. It was just as dramatic as that. I checked up on it. The ball switch idea was run through Newport. They made up designs and ran tests and made up more designs and ran tests, and in 1945, just as the war was coming to an end, they finally got a model that they were getting ready to send out to the fleet. And we had had these little half-baked things that the fleet made itself out at Pearl Harbor which sank all the shipping that was sunk with the contact exploder. But we did a lot of other little things at Newport, and in 1945 I was sent by Admiral Shindler, who was the head of the Naval Ordnance Lab at that time, to Paris with a Naval technical mission to go in and follow the spearheads of our Army that was moving into Germany and get into university towns and try to locate scientists who had been working on German armaments. I was particularly following torpedo developments. And I had a fantastic batch of experiences there, ending up with picking up all kinds of information on German torpedo developments and, incidentally, running into Werner von Braun and his rocket group at Peenemunde. I tried to get the Navy to move rapidly and get over and pick up that group and bring them back, but the Army had a similar technical group combing Europe and they ran into him also a short time later, and their red tape wasn't as heavy as the Navy's and they got him at Huntsville.

Q: Was there any information that you gathered in Germany on torpedoes that showed that they may have had a similar problem at anytime in the development of their torpedoes?

N: I went out about ten o'clock at night with some wavy Navy officers from the British. We were invited to go out from this Naval torpedo station in Germany on the Baltic. And we went on a ship that went out ten miles into the Baltic at nine or ten o'clock at night, and this young kid, who hardly had down on his face, had developed an acoustic torpedo. And he fired a demonstration acoustic torpedo at us as we were coming back at 35 knots in toward the firing pier. He fired this torpedo with a headlight in it so we could watch its trajectory and fired it on a course that would have passed us about a couple of hundred yards off to one side. As it got approaching us, it started curving around like this and went right under us amidships, and then it circled around and crossed under us twice more before it ran out of air. This was a homing torpedo that they had developed and had not got into the war before the end. If they had got that into the war, it would have been a devastating tool. They were far ahead of the United States Navy in its torpedo developments. And this was a young German technician that still didn't have a full beard. He was a fantastic briefer and very, very clear. He spoke excellent English and did a fine job of describing what this thing would do and how it was made and why it was made that way.

Q: Now do I understand correctly that you stayed on civilian status working for the Navy Department throughout the war as a scientist?

N: Yes I did. I did. At the time that I had finished the job and was working on this detection equipment, Dr. Johnson asked me to get a Reserve commission in the Navy. So I applied for a Reserve commission while I was out at Pearl, and I got letters from three, four people. Admiral Furlong was the commandant, or the guy that I was trying to think of a while ago. And Admiral Furlong gave me a letter that was so commendatory I never dared show it to anyone. But then I applied and I took my physical exam and everything I could do to get into the Naval Reserve before I went back. Went back in October of 1942 and found that a political opponent of Dr. Johnson's in the Bureau of Ordnance had harpooned his idea of setting up a technical corps of people to go out and work with the Navy commanders in the field during the war. So Ellis gave up and went and talked to General LeMay in the Air Force and got General LeMay to put him on his staff. And he went out to LeMay's headquarters, and he provided technical backup for their strategy sessions when they were planning the bombing of the Japanese headquarters. It was that--Ellis thought that it would be good to have operational technical teams associated with each of the military main commanders in the field. That, the Navy had shot down. So I didn't ever go ahead with my application for a commission. I stayed in civilian capacity throughout the war and spent the next

part in Newport and then came back down to Washington in 1945 after I got back from a five-month tour in Europe with the Naval technical mission and took over as Chief of the Naval Torpedo Division in the Naval Ordnance Laboratory. Held that position until Dr. Johnson left the Naval Ordnance Laboratory and took this assignment with the Air Force. He came with the Air Force in 1947, I believe, in December, and I came down with him in January or February 1948. He stayed until he locked horns with an Air Force Major General; got very abusive to him and was invited by the Air Force to leave, and they asked me to take over as Technical Director. [redacted]

[redacted]

Q: Was there any one personality that stood out in the Navy in your relationship with them as being a man who was more qualified and a better leader, more foresighted in his thinking than anyone else?

N: Well, Ralph Bennett was an electrical engineer at MIT when I was doing my graduate work. In fact, I got my degree--he was the thesis advisor for what was to be my doctorate which didn't ever turn out to be anything but a Master's degree. He was a Naval Reserve officer at the time that he was working on the staff at Tech, and he was called to active duty. And I think Ralph was one of the most farsighted technical officers in the Navy. He is the one who pushed for elevated salaries for PL-313 requirements,

something above the normal salary grade. Secretary Finletter used to say that I was the first PL-313 that he had appointed in the Air Force. Every time I would come in to see him over the years - he was the Secretary for so long - he would always remind me that he was the one that appointed me first PL-313. But Ralph Bennett awakened the Navy to the need for improving their technical facilities, improving their laboratories, and in general just bringing science much more to the fore in the Navy. The Navy was wise enough to do this, and they took advantage of the high priority that existed during wartime operations to build the Naval Ordnance Lab completely from nothing into a big facility, that now is at White Oak, to build the Naval Research Laboratory up by great leaps and bounds and several other Naval laboratories that otherwise they would never have been able to get approved.

(End Reel 1, Side 1)

Q: When we left off, we were talking about Mr. Bennett in the Navy. What happened to him?

N: Ralph left the Navy after several years as the Technical Director of the Naval Ordnance Laboratory, which we all had something to do with the formation of, and went with the General Electric Company where he built up the Vallecitos nuclear facility of the General Electric Company out near Livermore, California. It's now one

of the outstanding and is the primary atomic energy facility of the General Electric Company. I believe he retired from there and is now enjoying life in the California sunshine.

O: Were there any projects in which you were involved, say from the 1940 to the 1947 time period, that we haven't talked about that you had a special interest in that you would like to talk about?

N: Well, there was one project. While I was at the Newport torpedo station as the Naval Ordnance Laboratory senior representative, I developed, installed and put into operation an acoustic torpedo range, which was the installation of hydrophones every thousand yards out through the length of the 10,000 yard torpedo range. And, as a result of that acoustic facility, it was then no longer necessary to range electric torpedoes at night. Electric torpedoes do not make a visible wake on the surface as do air powered torpedoes. So air powered torpedoes could be ranged in the daytime or at night, either, because you could always identify when the wake passed under the rafts that were out at the 1,000 and 2,000 and 3,000 yard ranges. But the electric torpedoes would go by and you wouldn't have any way of ranging them unless you ranged at night with a headlight in them and you could see the headlight go by. But now they were able to range electric torpedoes in the daytime just as they did the air torpedoes, and another range just like that was put in by the Navy down in the Potomac south

of Washington at St. George's Island. Those were the two facilities that did all of the torpedo ranging for the Navy throughout the war. The acoustic facility came to be relied on as a much more accurate way of doing it than the way they had been doing it before by having sailors stand on rafts and when they would see a wake get out to the raft, they'd snap a stopwatch. Then each sailor's stopwatch was used to determine what the torpedo performance in velocity and so forth had been. Doing it with a little more precise method such as these sharply defined acoustic beams, then the torpedo could cross the beam 500 yards off to the side of the range, and it would still be an accurate indication of when it crossed that line. I think that's all that's really of any significance in those earlier days.

O: When did you become interested in atomic energy and that end of physics?

N: As I described a little earlier, the experience I had with Van de Graff and the two Van Atta boys who worked--we all worked as a team of four--was to build high energy generators and particle accelerators so that we could do work on bombarding fundamental particles of matter with electrons and neutrons and so on. I had always had an interest in physics from the time I got my degree in physics and math at Whitman through the periods that I was at Tech during the 1930's. And I think that is what prompted me to leap at the

opportunity to come with Dr. Johnson to the Air Force and build a system for the detection and the acquisition of intelligence on any foreign atomic weapons that might be detonated. That, combined with my experience of detecting small submarines, both acoustically and magnetically, contributed to my interest really in the subject. Plus the fact that at the time the intelligence community said, prior to 1949 when the Soviets detonated their first nuclear test, that "they couldn't possibly have a nuclear weapon prior to 1951, that 1953 was the more likely time, and it could be as late as 1955." In fact, that intelligence estimate had been briefed to us the day before in the war room. The day before we were plotting the first radio active decay curve from Russian nuclear debris that had been put into the atmosphere by the first Russian explosion and picked up by our sampling planes out in Kamchatka and taken into Alaska where a sergeant there was reading a radio active counter, sending the numbers to us in Washington, and we were plotting the decay curve. And one of the boys, who was kind of a wag, went over to this easel that had the intelligence estimate on it and picked it up and turned it upside down as an indication of the significance of what we were doing with this nuclear debris.

Q: Did you ever have any indication of where they were getting their information from?

N: The Russians?

O: No, the intelligence people that were giving you the erroneous information.

N: No. [redacted] 50X4, 6, 8 [redacted] In fact, we were just given the job by the Air Force under General Kepner and subsequently under General Hagenburger as the [redacted] 50X4, 6, 8 [redacted] to develop a system for detecting the first Russian explosion, and no one felt that there was any--I'm sure that no one in the Air Force believed that there was anything to the project except to determine when the Russians got the atomic bomb. I remember as we were walking down the hall--oh, and when we got evidence of the fact that there had been a bomb, we didn't even know anybody in the intelligence community to report it to, so we didn't know who to report it to. So I went to my friend Vannevar Bush and told him what was happening and Dr. Compton, under whom I also studied when I was at MIT, was president of MIT and in the Physics Department. He still taught courses there. I told K. T. [Dr. Karl Taylor Compton] that we had a decay curve that indicated that it was fresh fission products. He had just sent us a stop order, incidentally, to cut the project off completely. We had been under a budget cutting period that was not unlike the one that we had gone through just before I retired, and it came from 30 million to 15 to 12 to 10 to 8 to 2 to zero as of the time the

Russians fired their first bomb. And I think the Russians have to be given credit for keeping the ADS [atomic detection systems] in business.

Q: At this time, you were known as AFOAT. Is that right?

N: No. We were initially known as Armed Forces Special Weapons Project and in this capacity we were under the Air Force operations, under Chief of Staff for Operations [Deputy Chief of Staff]. Then in something like three months--it's in the history I think--we were changed to General Slater, who had the Air Force Office of Atomic Energy, and we were Atomic Energy Detection, so they called us AFOAT-1, just to not say that it was detection. And we stayed AFOAT-1 until 1958. When I was in Geneva in 1958--in January of 1959 I was given the President's Award for my work in developing surveillance systems. The citation was so directly exactly what I had been doing--and it had always been classified secret--that the Air Force practically went into a tizzy. I was in Geneva. My wife had to go up to the White House to receive the award as a proxy for me. General Rodenhouser, who was the commander at that time, was so shaken that he didn't even offer to provide any transportation for her to get to the White House. If it hadn't been for a friend of mine, who was there, a next door neighbor, that worked for me--he went over with Sybil and took her in and got her to the right place--she wouldn't have been able to get there. Rodenhouser then immediately

the night before the announcement in the papers I was asleep in the hotel and the phone rang about four o'clock in the morning. It was my wife with her usual poor arithmatic; had added five hours instead of subtracting five hours, and she thought it was the middle of the afternoon instead of the middle of the night. And she said, "You've been nominated for the President's Award this year, and I've got to go up and receive it at the White House." I said, "You must have been smoking opium. Are you sober?" And the next morning the Journal Geneve came out and said in French that the Gold Medal had been given to D. L. Northrup and Werner von Braun and Murphy, who was the State Department guy that was over with our delegation at that time. And up till then I had been just a--as far as the State Department was concerned in these negotiations--just a boy in the back room to bring out when they needed to know something technical and then take him back in the back room. But after this, boy! I got

invited to sit at the head table at all the State Department dinners. From them on I was in. [redacted] It was the Air Force concern, and I believe it was John Foster Dulles who had insisted that the real accomplishment be spelled out in this thing. He said that my development of an atomic energy detection system and the fact that it was in being when it was needed and was going to permit us to have a nuclear test ban, a ban on nuclear tests and monitor it with this system was a very important thing and that that's why I was getting the award, and he was, by God, going to see that it said why.

Q: Could you give us just a brief rundown on the history of the program and how it was when you first got in the organization and then how it progressed up to today?

N: Well, when I came to the Air Force in 1948, Dr. Johnson assembled a staff comprised of Dr. Urry [W. D.] for Nuclear Physics, J. Allan Crocker for seismology and myself for investigating new types of effects that might be used for monitoring nuclear explosions. At the time there was in existence an acoustic detection system that had been developed by Dr. Daniels in the Signal Corps of the Navy. And, as of the first Russian explosion in 1949, that system was in existence together with air sampling, which we had developed in the form of B-29 routine flights from Japan to Alaska. But our first job when we came aboard was to devise a set of experiments that

could be deployed on the Sandstone tests which were scheduled for the spring of 1948 out at Eniwetok. There were to be three nuclear tests of nominal range yields, so we deployed everything we could possibly think of. Nuclear debris fallout, we didn't know whether there would be any nuclear debris in molecular form--in other than atomic form, rather. Somebody thought that there wouldn't be debris that you could collect by flying filters through the atmosphere. That's how little we knew about it at that time. We had electromagnetic sensors, magnetic sensors, acoustic sensors, optical sensors. Anything that we thought might have any chance at all of giving evidence of being useful for long range detection we quickly assembled and through the help of the Army and the Navy and the Air Force research laboratories put teams out in the field to measure those things. Then when the Sandstone tests were over and the task force came back, we hauled all the data in to headquarters, which at that time was in the Pentagon, and analyzed it and found that there were several things that looked positive. In the first place, the Signal Corps had been successful in detecting the explosions so the acoustic system looked like it would be good. Seismology looked like it would be good because the US Coast and Geodetic Survey seismic equipment had detected these early explosions. The electromagnetic pulse technique did not respond and the magnetic technique did not respond, but we found later it was because of the rather crude apparatus that we had deployed during these first periods. B-29's that were flown in the vicinity downwind from the explosion

through the debris clouds, the visible debris clouds, turned up with radioactive oil filters, and we then knew that at least the B-29's would be able to sample, detect nuclear debris. So we put a very crude air sampling filter that was mounted on top of the B-29. It had an aperture that allowed air to get in and go through a filter paper, which would slow down and stop the particles and let the air go on through. And we mounted those on B-29's of the Air Weather Service that operated out of Japan along flight lines to Alaska and from Alaska back down to the United States and from Japan south in a round robin fashion and back to Japan, going down and covering as far as the Philippines. We decided right after the Sandstone tests before the end of 1948 to put these filters on the B-29's, and as of the beginning of 1949, we had air sampling coverage of the whole corridor from the Philippines to Alaska for any debris that might come out of Russia.

O: Was this on specific B-29's or did you put it on the entire fleet that was operating in that area?

N: We put it on specifically B-29's at that time. That was the workhorse of the Air Weather Service at that particular time. There was a flight also from Alaska, which I remember I took one of them to the North Pole and back, called the Ptarmigan flight, and it was equipped with air samplers. And they changed filters every hour all the way up and all the way back. And all this effort was

going on during 1929 [1949]. In the meantime, I was fighting the battle with the Research and Development Board, Joint Research and Development Board. The president, Dr. Compton--Dr. Oppenheimer headed up the Committee on Atomic Energy and Father Sullwarm headed up the Committee on Geophysics and Geography.

Q: Sir, you said 1949 a minute ago. You meant 1949?

N: 1949. This committee really was under--it was always laughingly referred to as--the Committee on Nuclear Physics, which was headed by Dr. Oppenheimer, was always talked about as the "tail that wagged the dog" of the Joint Research and Development Board because Dr. Oppenheimer had such prestige in those times that everybody else on the committee, as far as he was concerned, was just wasting their time. And we had the worst difficulties, programwise, with him because he felt that all that was necessary was to have airplanes flying and getting samples of debris. He had very little truck with putting in geophysical systems to detect where the explosions had occurred. It was our feeling that if you could detect the explosion in advance of the debris arriving at the flight lines that you could much better position the aircraft and use that many fewer aircraft to cover and get adequate samples of debris. [redacted] and I, as the chief of that technical group, had to spend most of my time at the Research and Development Board meetings defending our program

against cuts being recommended by the Joint Research and Development Board. This was going on all through 1949. I was spending most of my time there. My staff was busy starting research projects to look into many of the things that had looked promising as a result of the Sandstone tests. In 1949, which was the next year after we were formed, Dr. Johnson got into his little problem with the Air Force and left, and I was put in his place, I believe, in either late 1948 or early 1949. I don't remember the exact time. And I then took charge of both the research and development programs and the development of a worldwide system. A little interesting sidelight on the shortsightedness of the people looking at our program at that time was when we announced it, I went over to Bill Webster's office. Bill Webster was then Assistant Secretary of Defense for Atomic Energy and Chairman of the Military Liaison Committee. And he took me down to Secretary [Louis] Johnson, who was the Secretary of Defense, to tell him about the Russian explosion. On the way down the hall to his office--he knew all the trouble I had been having budgetwise--he said, "Well, Doyle, you can forget your problems now." He said, "You've done your job. There is no more to be done." 50X4, 6, 8

50X4, 6, 8 [redacted]

50X4, 6, 8 Well, it really didn't impress him, and it didn't

impress people. The biggest problem I had really was to get them to realize that there was some point in going on with the system after having first discovered that the Russians had a nuclear device.

O: What happened as far as the budget was concerned when you did tell Secretary Johnson?

N: Well, he was very much interested, and the budget continued to be curtailed even more severely for at least the next six months, proving one of the laws of Washington; once you get something started, the inertia is such that you can't stop a budgetary program until something in the order of months to years has gone by. Then finally it did slow down, and we started getting more funds fed in so that by the time of the second and third Russian nuclear tests we were beginning to field other more sophisticated sensor systems than we had been able to field on the first one.

O: Do you know who was specifically responsible for getting you more funds?

N: Yes. I began to recognize that we were a pretty unimportant office and undistinguished scientists running a little show here that obviously was not going to attract a great deal of attention, and, therefore, I asked Dr. [Vannevar] Bush if he would set up a panel to review our programs. And he set up the Lumas Panel with Alfred

Lumas from the Radiation Laboratory directing the panel. I've forgotten now the members of the panel, but they're also logged in the history. Then each time we had a request for funds, for either operational deployment of systems or for conduct of research on new and possibly improved systems, we would present this to the Lumas Panel and get their endorsement so that when we went back to the budget people in the Pentagon we had the Lumas signature behind it. We then had more than just "the little AFOAT-1 organization wants something, the Lumas Panel believes this would be a good idea." Now the Lumas Panel turned out not to be an answer to a maiden's prayer either, because they were very important people and had very little time to devote to this thing. Never devoted more than a few hours on each one of the meetings, so that many ideas, we would inadequately sell to the Lumas Panel, would be dead until the next meeting a year later. We would have to revive them and dress them up in better defensive clothing and trot them through the panel again and finally get them approved. Some things we just were unable to get approved because people, for reasons best known to them, felt that they didn't want to go into these other activities. One of the things that I recall as being most outstanding that was considered a real wild idea, and that was one of the later panels along in 1956-57, I tried to interest them in getting somebody to launch a satellite sensor so that we could get into the satellite business. That we just never were able to launch until after the Geneva exercise came along in 1958 where we deliberately

introduced the idea of high altitude detection. By that time, we had so many important people at the President's level, the President's Scientific Advisory Board level, contributing ideas about how you detect this and how you could detect that that the idea then went across. But we were not able to get it across on our own steam earlier. The Lumas Panel went on for about three years, and they asked to be relieved of their responsibility. By that time we had added the acoustic system and the electromagnetic pulse system, which had come about as a result of some experiments we were conducting in the Nevada proving ground. Observers of ours out there noticed that any ungrounded cable that was near a grounded apparatus--was frequently observed, on the detonation of the bomb, a spark would jump from the cable to the ground. So they put a recorder on these ungrounded cables one time and recorded tremendous numbers of amperes that were generated in these ungrounded cables laying around. So we recognized that there was an electromagnetic effect. We then developed sensors much better suited to it than we had deployed in the Sandstone tests of 1949 and found there was a real effect. And soon had a system started using the electromagnetic pulses generated by nuclear tests. The seismic system had also been developed largely through the backing of the Committee on Geophysics and Geography of the Joint Research and Development Board with strong support by the Air Force in the form of Ben Holsman who was Air Force Research and Development at that time and General Yates who took over Air Force Research and Development. And, in

fact, we then got into a battle within the Air Force. General Yates wanted to know why we should not just all move over into the research division and then he would run the whole thing himself. [redacted] 50X4, 6, 8 And we got into a lot of organizational disputes which were a nuisance. But they did take up time and prevent us from going ahead as fast as we might have otherwise.

Q: I'd like to ask one question, sir, about the detection devices; the acoustic, the electromagnetic and the air sampling. Are these all long range detection devices, or are some of them short range?

N: [redacted] 50X4, 6, 8

50X4, 6, 8 [redacted] Air sampling, of course, nuclear debris is carried around the world, and we have detected it after it has [redacted] 50X4, 6, 8 once or twice on its trips around the world. The nuclear debris just hangs in the atmosphere and is detectable at a very great range. The seismic system on very large yields, and the acoustic system as well in very large yields, has picked up waves that came from the source

all the way around the earth the short way as well as all the way around the earth the long way to the detection station. 50X4, 6, 8 [redacted] 50X4, 6, 8 I've forgotten which. The whole earth's atmosphere just vibrated for days after that tremendous explosion. So those are all long range techniques.

Restricted Data

Q: How do you distinguish on the seismic system, say, nuclear detonation from an earthquake?

N: That's one of the worst problems that we had in the detection business. And it was one of the things that deterred us somewhat in going to Geneva in 1958. Some of the scientists who were anxious for a test ban headed by Professor Bethe [Hans Albrecht], a very famous physicist and a very competent one, felt that we hadn't done enough research in seismology and a little really good research in seismology would easily reveal how you could tell the difference between an earthquake and an explosion. We have since put in, I would say, nearly a billion dollars into seismic research, and we still are working on more research to get the answer to that question. I was so disturbed about it at the time in the spring of 1958 I wrote a long letter to Dr. Killian pointing out to him that I felt that the Bethe Panel had not recognized the real problems in seismology and that I wanted him to know before he sent a

delegation to Geneva that I at least felt that the seismic problems were far from whipped. Well, he was inclined to think that probably research would solve them all right, so they went ahead with it, and the result has been that we have had a limited test ban, not a comprehensive test ban, for 10 years. And the problems still are not solved. We have made great headway, though. [redacted]

Q: I'd like to go back just a moment to about the 1949 time frame and ask you a couple of questions. The first one is about Dr. Oppenheimer. Did you know him fairly well at that time? And, if so, how would you characterize him? He has been quite a controversial figure since then. I was wondering what impressions did you have of him at that time.

N: The impression I had of Dr. Oppenheimer was that he was a very independent thinker and he had a tremendously great talent of sitting and listening to very complicated arguments advanced in meetings and when everybody had had his say and confusion reigned supreme, he would say, "Well, now, it seems to me that this is what we're all saying here," and he would say it in a way that everybody would suddenly say, "That's just what I've been trying to say." He could put in clear language very complex ideas. I think his contribution was more doing that, to people that I saw him associated with, than any independent brilliant ideas he may have had. Friends of mine who worked much more closely with him at Los Alamos have agreed that his independent contributions were not great, but his intellect and his lightning ability to follow complex arguments and to say them in language that everybody could understand was just absolutely invaluable.

O: Would you characterize him as more the manager than the pure scientist or a combination?

N: No. He was a pure scientist. I don't think as a manager he came a cropper, and Lewis [L.] Strauss crucified him for that. He happened to have a personal friend of his that he thought he could rely on, who approached him about getting information for the Russians. He made up his own mind not to turn that man in but just not to pay any more attention, not to give him the information.

And I'm sure he did not. I think he was as loyal an American and as great a contributor to our weapon program as there was anywhere. I think he was pilloried by Lewis Strauss and Gray in a way that was disgraceful. This country really has a black mark when they-- I really saw him go downhill worse after that removal of his clearance. I don't know, whatever it is they did to him, they killed him. And it wasn't but a few years. You could see him. He was a walking skeleton in his last few months. I think it was a very disgraceful performance. In a way, he was a little objectionable to the person of average intelligence in that he was very snobbish. He was very short with people who weren't as smart as he was so that you had to know what you were talking about when you were with him. If you were at all ignorant or stupid--he had no patience whatever for anybody who wasn't able to exchange ideas with him on at least a student-to-professor level. So he was an academic snob really. And I think that did him more harm. I think that made enemies. I think Strauss was really--Oppenheimer had no compunctions about telling the chairman of the commission that he thought he was crazy. He'd do it much more elegantly than that, but he would belittle him in such a way that it just infuriated Strauss. And when he got a chance at him with the security thing, he used it.

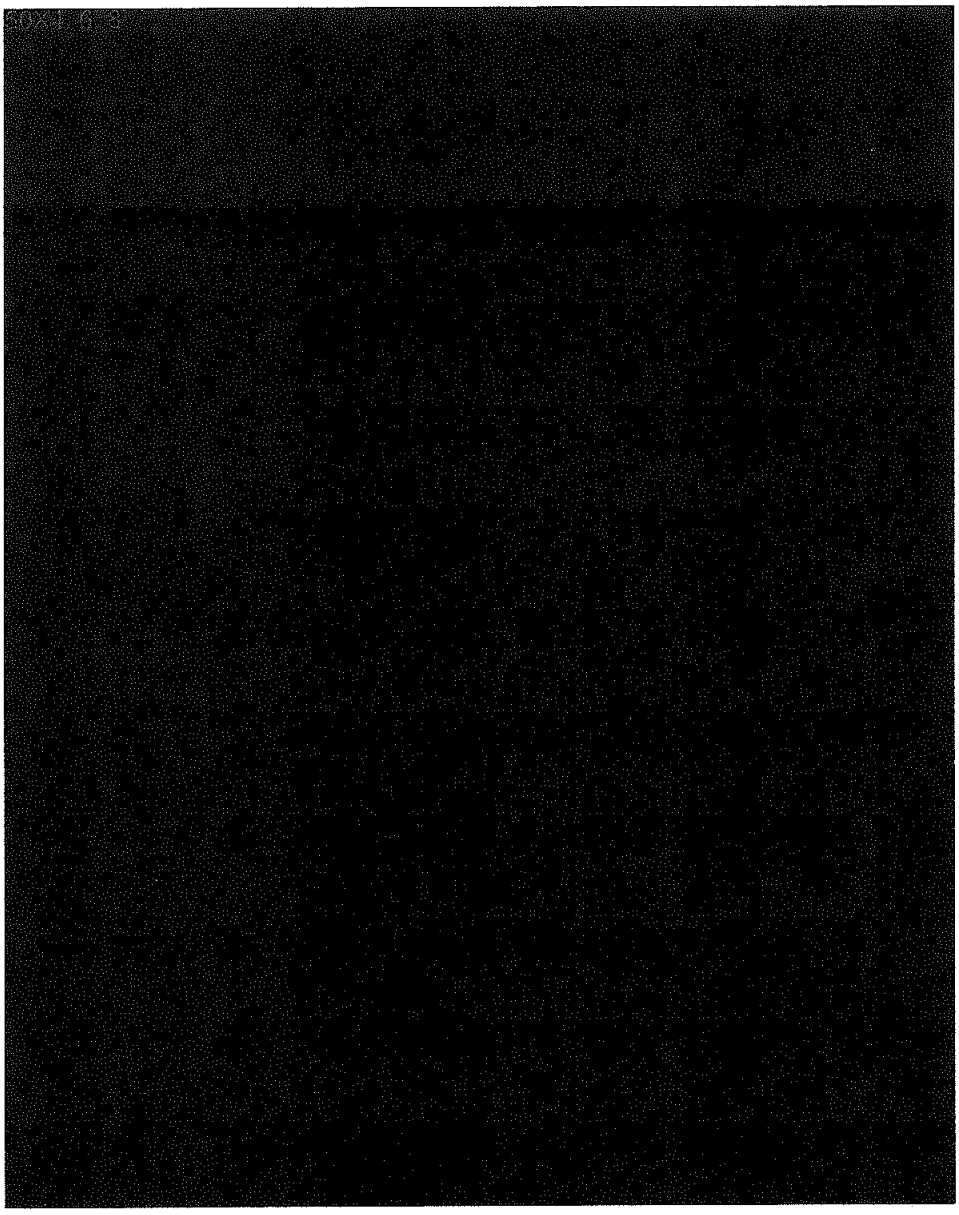
Q: My next question deals with the same time frame. I've read some old documents, board meetings, committee reports, that say that

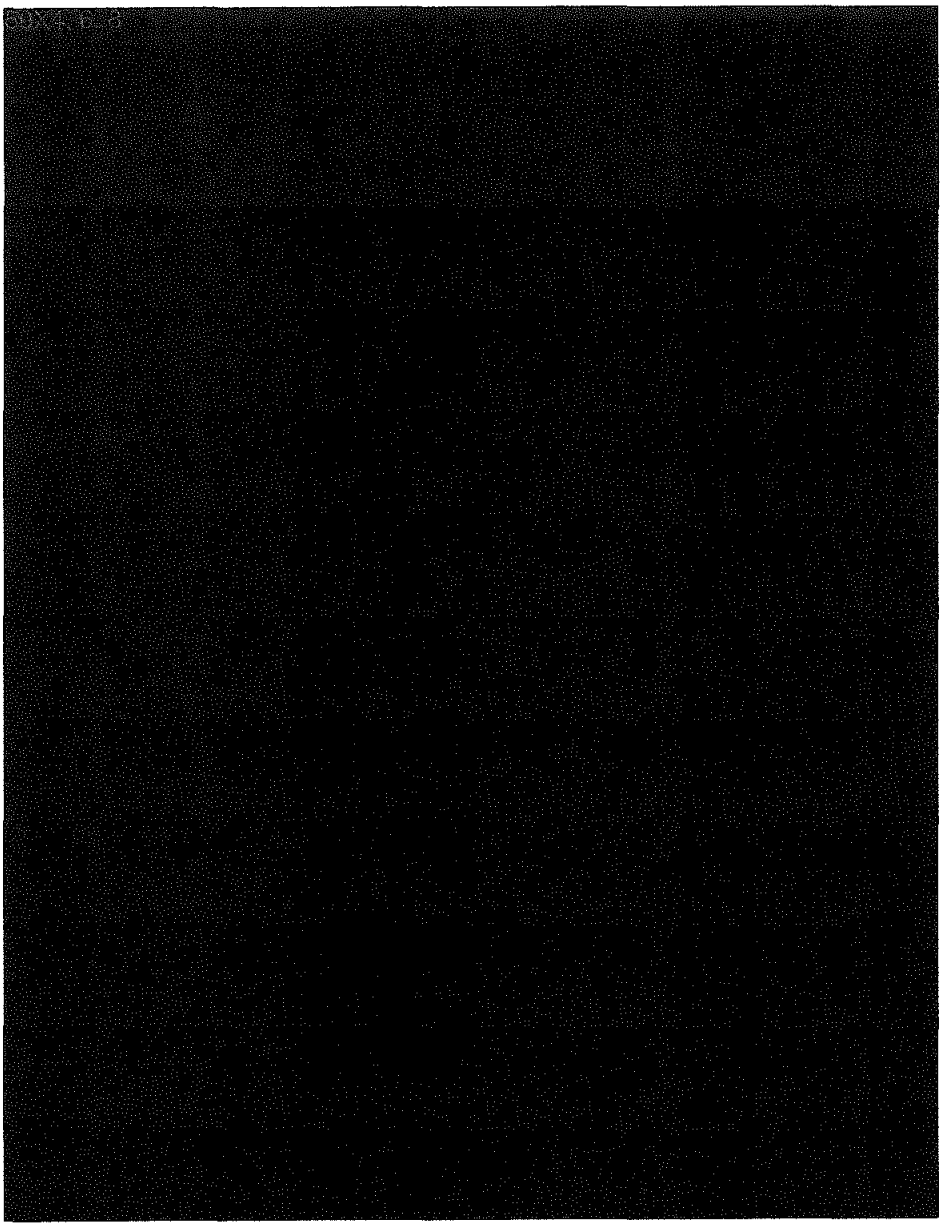
the Air Force had no doctrine at that time or no methodology really developed for the employment of nuclear weapons in wartime, and there was some controversy that developed over the employment of nuclear weapons. Were you involved in any of those committees that studied that problem?

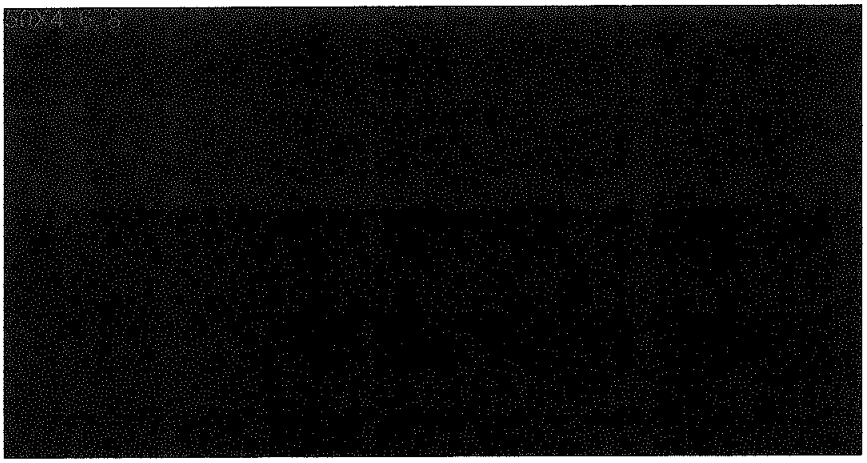
N: No, I was not. I heard the same things, however. I knew Slat Slater very well. I'm sure that if he exhibited no more foresight or insight into the weapons requirements of the Air Force than he did into our project that that might well have been true. I don't know of anyone really in those early days who really was pushing on that, but my knowledge is very limited. I was not in that area of operations. [redacted] where our utilization on other problems was not great.

Q: Am I correct in assuming then that--going back to AFTAC itself--am I correct in assuming that the AFTAC mission has not really changed much since the early days, the mission remains the same, detection?

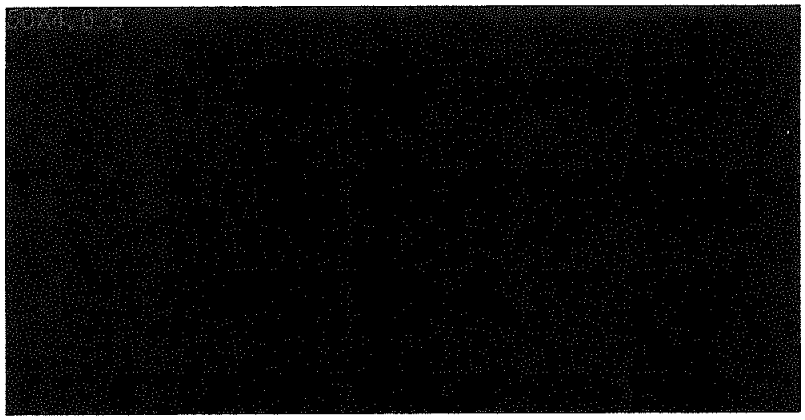
N: The mission initially was the long range detection of atomic explosions [redacted]







Q: Do we provide, or does AFTAC provide, any support to any other agencies of the government besides the Air Force, or does it all go through Air Force channels?

N: 

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Q: I understand in 1958 you were the DOD representative at the Geneva Nuclear Test Ban Conference. First of all, could you tell us a little bit about who the major participants were countrywise and who were the members of the American team?

N: The major participants were the United States, Britain and France and the Soviet Union on the other side, and the western powers were represented--United States, Britain, France and, I believe, Czechoslovakia. We can get them out of either my office or the Geneva Test Ban thing. I believe there were one or two smaller countries, but the primary ones were Britain, United States and Russia, and the chairmanship rotated amongst those three. France had a representative there and Czechoslovakia and Poland on the Russian side. But the western side was US, Britain and France.

Q: Who headed the American team?

N: Jim Fisk.

Q: Was he a scientist?

N: Yes, he was a scientist. He and I went through high energy physics together at MIT in the early days. He went to the Bell Labs. I stayed with the government. He ended up in charge of the delegation to have a scientific discussion on whether it was possible to have

a test ban, and I ended up in the back room of the same delegation with various supporters [redacted] Dr. Romney, Mr. Olmsted, Mr. Crocker, and the operations officers. Colonel Griffin was over there for a substantial period. The forerunner of that conference was in the spring of 1958. The suggestion was made, and I'm not sure how that initial one was originated, whether this was originated by the President or not, but he asked Dr. Killian to look into the possibility of monitoring a nuclear test ban. The Russians had been arguing that there should be a ban on nuclear weapons, that they were bad things and everybody should ban nuclear weapons. So Killian called in Dr. Bethe and asked him to form a group. I believe it included George [B.] Kistiakowsky and Herb York, and that's all I remember at the moment. [redacted] to talk about seismology, acoustics, electromagnetic pulse, nuclear debris, and high altitude techniques, techniques for detecting high altitude explosions, and to present what our present capability was so that they could decide whether or not we should go for a complete test ban. This we did. The upshot of it was that Dr. Bethe recommended to Killian when this group reported that we should go to Geneva. I think you have to remember that in the subsequent discussions of the test ban, where I gave the Joint Committee some assistance as a consultant, when various people came in to try to argue for the test ban, Bethe was always on the side of the test ban. I think it had to do with his conscience, that

those people who contributed to the development of nuclear devices really considered that they had done the world a disservice. And they were trying their level best to stop any further development of nuclear weapons. And I think they were--well, at least we thought they were a little blind to the security of the United States by being willing to stop even when there wasn't an adequate monitoring capability. So there was a continuing bickering between [redacted] who felt that certain techniques were not yet ready for making the agreement because we couldn't monitor them well enough and particularly underground, and they who thought that a little more research and this would all be solved. Well, as a result of the go-around for several months in the spring, it was finally concluded we should go ahead. As I told you, I wrote this letter to Killian, and he called me over to explain what it was I wanted him to do. And I told him, "Well, I want you to be damn careful that you don't get into something we can't monitor over there, and you're representing President Eisenhower. I don't believe you have been given all of the correct story." I said,

[redacted]

trip." Well, he thanked me for giving him this advice and then went ahead anyway. There was a lot of pressure on the President from other countries. Britain was pushing hard for our getting together with the Russians and having an agreement to ban all nuclear tests. So we went ahead. And I went over along with Dr. Romney, Mr. Olmsted for a while, Allan Crocker for a while, as Department of Defense representatives. There was a senior Department of Defense Representative. While I was there, it was Lieutenant General Fox, who was a very fine old gentleman, Army officer, absolutely a fair, objective--he would have been a good Supreme Court judge, I think. He was really a very intelligent and a very fair man in all of the arguments we got into, and there were many. He was always objective and fair in everything that had to do with our delegation versus the State Department or whoever we were arguing with. The most frustrating period I ever went through! I came home 60 days later and I had lost 30 pounds. My wife met me at National [Washington DC] Airport, and I decided to see if she would recognize me. So I walked right by in front of her. She didn't pay any attention at all. She didn't recognize me. We worked 18 to 20 hours a day preparing papers that never got presented. Some of them got presented, but most of the time there was so much preparation going on that you couldn't possibly present everything that we produced. We went though all of the various techniques, one by one. I did a good bit of the talking on the overall system design, the integration of all the techniques into a system. The particular

individuals, Romney talked about seismic detection and Olmsted talked about acoustic detection and so on. The procedure at the conference was which alternated chairman between the Russians and the British and the US day by day. Once in a while they would let one of the lesser countries get in, but in general it was just the three big powers that were acting as chairman. We went through, I think, first acoustics, second seismic, then nuclear, then high altitude techniques. I've forgotten. I think that was all. We ended up then within a week of the end of the conference with agreed positions that both Jim Fisk for the US delegation and Bill Penney for the British delegation agreed, with the Russians, that this was a satisfactory text to what could be done in this particular field. And there was a preamble to each one of these things. Then the problem arose as to what should we do about writing a final report of all this because we had reached agreement on each of the techniques but no agreement on the system. Well, Federov [Yevgeni K.] came in one day and said that--he was the scientific head of the Russian delegation--said that as far as the final report was concerned, he observed, that if we were to take the introduction off each of the individual technique reports and combine it with a generalized statement in front as to what we felt we could now agree to, as far as cessation of nuclear tests, that we would have a very logical and comprehensive paper. That we could all agree to. And Jim Fisk leaned forward to the microphone and said, "What's the Russian word for incredible?" Well, it ended up

That Federov was right, and we did take those individual introductions to each of the techniques, combined them, and fought for about a week on the various wordings that was in that and then hooked the other things on as separate parts of the agreement. And got a thing that within a week was agreed to. One of the greatest helps in settling semantic problems was a little Russian girl who you may have seen on television. She came over with Mrs. Kruschev and was her interpreter, Natanya something or other. I've forgotten her last name. But she sensed early in the game that the Americans and the British were really not trying to pull the wool over Russian eyes and were doing their level best really to lean over backwards and understand what was bothering the Russians. And when we were trying to negotiate some of the wording of the final thing, she would often join with us and get violently mad and argue with her Russian colleagues, that, "They don't mean this at all. Now this is what they mean. Now, goddamn it, straighten up and fly right," you know. You could almost hear her saying it. And she straightened out more semantic difficulties over just simple little words than you can shake a stick at. In general, the Russians, Tsarapkin [Semen K.] particularly, were very friendly to the US delegation. In fact, as evidence of the fact that our security cloak was not the best in the world, the day the news came out in the paper that I had received the President's Award, Federov, Sadovsky [Mikhail A.], Tsarapkin, Pasechnik [I. P.] of the Russian delegation got up, marched solumnly around the table to the back row where I

was sitting and congratulated me on having devised a system that detected the first Russian explosion. So who are we fooling with this security bit, you know? We're certainly not fooling the Russians.

Q: Was there one issue that stood out among all the rest as far as differences of opinion is concerned at the conference? Was it over a particular detection system or over the ability to detect?

N: This was a scientific meeting, and there were two areas where things were difficult. The first one I've already mentioned, the seismic area. Then we had a special seismic conference to try to resolve that, and that really didn't do much. It didn't completely. But we had a panel on underground nuclear tests, and then we had a high altitude detection meeting. Here we had no information. We had never detected a Soviet high altitude test because there hadn't been one. We had some very sketchy data on the Argus tests that were conducted in the South Atlantic, and that's about all we had. And so our understanding was primarily theoretical. [redacted] So it fell into the hands of the theoretical physicists, and there was Panofsky [Wolfgang K. H.] of Cal Tech. He is the one that built the linear accelerator out there. Very competent technical man and very lucid, very able to express himself. He is not like the usual scientist who loves to use his erudition to confuse people rather than to communicate with them.

Panofsky did a lot of good work, and he and Dick Latter from Rand Corporation and the screamer what the hell was his name? Osterday Colgate was one of them and -well, I'll think of it in a minute- were the ones who principally got into the theoretical end of things and suggested what might be possible from satellites and near earth satellites and far earth satellites and synchronous satellites and the whole bit [and] how well you could do this job. We even got into placing stations around the world in networks of 180 stations, for one proposal, networks of 640, which was the first US proposal, stations. And I worked out all three of these systems based on information on specifications from the State Department as to what size explosion we should be able to detect, and I presented those systems. The final system that was bought was the 180 station system by the Soviets. And I think the biggest argument, the biggest bone of contention there, was seismology and high altitude. We argued that we didn't know for sure what high altitude was, but we finally accepted the fact that in space computation of gamma ray intensity as a function of distance was just the inverse square law; the same way with optics; the same way with X rays. And the theoretical estimates could be relied on in space where you probably couldn't rely on them in the atmosphere. It was in general on that that we abandoned our opposition to including high altitude nuclear explosions and agreed with the Russians that you could probably work out schemes for doing it. But we did balk at the absolute minimum amount of information that we had been

able to produce on underground explosions and a complete lack of ability to identify them or know what the hell they were. After bitterly fighting for weeks, the Russians finally gave in and said they would sign a limited test ban and exclude underground tests.

Q: What part did the diplomats, the State Department, play in the conference?

N: Well, in the technical conference they sat in the back room and were pretty much subordinated because the--I think their best time was when we were putting the thing down in words as to what we would agree to. Then you got into what kinds of people you would have in the control system; whether the control mechanism should be centered in a worldwide location like in Geneva; should it be an international control system; or how many people should be on the staff--wait a minute--I'm getting on into the political conference because we didn't get into that in the technical conference. No, they were there. They had important things to do. Ambassador Fisk had a State Department guy sitting at his elbow advising him. But they did not play any kind of a leading role like they did after the political conference started. Then the technical people really went into the back room and the State Department people came out. And all of these plays back and forth as to how many on-site inspections and the real bones of contention that came up were handled by the State Department people.

Q: Was there anything about the conference itself that particularly impressed you? And I'm thinking of, for example, did you find it fairly easy to work with the Russians in working out agreements, or were they very difficult? Was there anything of that nature that made an impression on you at the conference?

N: I think, in general, you would have to say the Russians were very difficult. As I said, it was the most frustrating time I ever had. I don't know how international negotiators in State [US State Department] who have to face these kinds of intransigences continuously could ever continue to do it. But there were highlights. For example, Russians like to have everything nice and standard. But they insisted that there were large areas in the world where just water existed, or we pointed this out, and here you would have a problem covering it. Well, they said, "Well, we'll station ships in these large areas," and they went around the world and picked the large areas, and we agreed that 10 ships stationed with equipment on them could do the job. Then the argument came up. What do you put on the ships? They wanted to put the same techniques that were on land, seismic, acoustic, electromagnetic and nuclear. Well, we pointed out that nuclear could probably work on a ship because this would be an air sampler that could work on a ship as well as anywhere else. But how were you going to hook a seismometer onto the ocean bottom in 10,000 feet of water and have a stationed ship operate it? And how were you going to get an acoustic pipe 1,000 feet long

on a ship rolling and pitching? You would have so much background noise from the motion of the ship it would be an impossibility. So they decided they would have a special conference resolve this. I mean a subgroup resolve it. And they put me in charge for the Americans and Sadovsky in charge for the Russians, and we each had a guy helping us. We went out in the back room and tried to argue out why it was that we couldn't have these. And when I explained this to Sadovsky--he was a very reasonable man--he understood right away why you couldn't put an acoustic system on a ship. And from a practical standpoint, we said the same thing with seismic. We could put electromagnetic and nuclear, and that would still give you pretty good coverage in the area. So he agreed to this, not in any short time. We were out for two hours. We came back in about four o'clock, and Fisk was sitting there still arguing with Federov about how you--Federov was still arguing about how you had to have all four techniques on all the ships. So I came around to our side of the table, got up beside Jim, whispered in his ear that Sadovsky had just agreed that we could not use seismic and acoustic equipment on shipboard. It wasn't a scientifically sound principle to do it. And he leaned forward to his microphone and said, "Well, Dr. Northrup tells me that Dr. Sadovsky has just agreed with him; it's impractical to have acoustic and seismic equipment on shipboard." And Federov banged the gavel and adjourned the meeting right away. I saw him over in the corner. He had Sadovsky backed up in the corner,

and he was wiggling his finger in his face and giving him hell for undercutting him in the back room.

Q: Did they ever agree to something, a general principle, at one time and then back out on that agreement?

N: Oh, yes. And Tsarapkin had a good sense of humor, but he would say some of the most audacious things. I'll have to look up this example because it's really a good one. And he said this thing was completely foreign to anything we could possible agree to, and it was so wild that two or three or us just broke out laughing. He said, "See, even Dr. Northrup agrees." (Laughter) So there were high points like that. At cocktail parties it was very interesting to talk to them. They were very much more outgoing.

Q: Did it seem that they were more than just trying to protect their interest? Did it appear to you that they were actually trying to use the conference to gain something from the Americans that they didn't have, maybe technological know-how or something that they didn't possess before?

N: I think it was very clear to most of us that they had been sent to Geneva to come back with a complete ban on all nuclear tests everywhere, that the Russian command really wanted that and had

ordered them to do this and that everyone of their arguments was along that line, was to have this everywhere. And they were just intolerant in the early days of the conference, early part of the summer, of any thought that you could exclude tests in any region, that you couldn't ban tests everywhere forever and ever. And I think that was the one thing that must have been to Fisk the most frustrating thing. Just about the time he'd think that he had them seeing the light on some point, then they would drag this thing out, "Why, you Americans don't want it. You didn't come over here to have a nuclear test ban." And the same way with the British.

Q: I've read that one of the major issues was also the business of on-site inspections.

N: Oh, yes.

Q: . . . in which they were, I understand, diametrically opposed to that view. Is that correct?

N: They came out frankly one day and said, "All you want to get on-site inspections for is to come over here and spy on the Soviet Union. We're not about to let you come in and spy on the facilities of the Soviet Union." And it was clear that's what they were worried about. And we had a very unrealistic thing. We were

going to have we had worked out a technique--I mean a scenario for what an on-site inspection would be, and it involved train loads of equipment with drill rigs to drill down 2,000 feet and look for debris of a nuclear test underground and 2 or 300 people going into the Soviet Union and firing shots and looking to try to find disturbed things underground and in general making a nuisance of ourselves. And we wanted to do that 21 times a year, which is every two weeks, you know. It was a ridiculous request. And they were arguing for no more than one. At first they didn't want any. Then they were arguing for no more than one. And their argument was the deterrent effect of being able to conduct one on-site inspection means you could pick anyone you want. That would deter us from doing this. And the same thing, our one inspection in the United States would deter you from having an underground test, too. But it didn't ever really get settled, and it's still a big bone of contention. People are right now talking in Washington about having a comprehensive test ban treaty and limiting it to very few on-site inspections. We have recommended that if we could spend another $20 million, our seismic system could be so improved that it would [redacted]

[redacted]

Q: Were there any personalities at the conference itself that stood out above the rest, either on our side, the British side, the French side or the Russian side?

K: I think on the Russian side that technically Federov was head and shoulders outstanding, as far as the scientists in the Russian delegation. He was not a specialist in any particular field, but he led that delegation and he led the debating, and he was an absolutely superb debater. Fisk, on our side, did a not nearly as effective job, but very low key, and in protecting the best interests of the United States, I think that he was an effective guy. But he was nowhere near as impressive as Federov. On the British side, Sir William Penney, while a competent scientist, was a very poor debater and lost his temper just at the drop of a hat, and he was pretty ineffective. Going down the line, the British seismologist, Hal Thurlaway, was just average. Romney was, I think, more outstanding on the US side in seismology. He had an ability to express things, in a low key, that had sort of a devastating repercussion. I'll never forget one day we were at Room 8 in the Palais de Nations. This big plate glass window looks out on the Alps, and you can see Mont Blanc and all. On a clear day it's really a beautiful sight. And Leipunski [O. I.] had got up, a Russian theoretical seismologist I believe it was. No, it was another name. Their chief theoretical seismologist got up and argued about a semi-infinite homogeneous half space and that seismic waves going through here should do this and that and all this blackboard full of equations that almost nobody understood. I certainly didn't understand it. And when it was all through, Carl [Romney] was going to have to answer this. And I thought, "Well, by God,

I'm glad I don't have to answer." Clark got up and said, "Well, gentlemen, Dr. Lepinski"--whatever the heck his name was--"has produced a very convincing argument on what would happen in a semi-infinite homogeneous half space, but all you have to do is look out the window and you can see the world is not a semi-infinite homogeneous half space but has many discontinuities." And even the Russians broke out in laughter at that one. He was effective in that way. Dick Latter from Rand Corporation, when it came to theoretical discussions, and I think Peef Panofsky, Cal Tech, and Harold Brown--although Harold and I both got mad too quick. I remember one time we were sitting on the floor behind Fisk. We were both so mad we were scribbling notes frantically and handing them up to Fisk, "Tell the son-of-a-bitch this," and so on. But Harold was a very effective guy when he was presenting a paper. He was very logical. He has an extremely brilliant mind and, I think, had to be listed among the outstanding people at the conference.

Q: Looking back at the overall results of the conference, were you satisfied personally with the outcome of the conference?

N: Well, I was satisfied that we had been able to get an agreement that there should not be any ban on underground nuclear tests. That was the big battle that I fought, and I argued consistently in the delegation against including high altitude tests because I felt we knew so little about high altitude tests. But I was

overruled, and I really expected to be overruled by the theoretical physicists who took a very sound point that when you're out in space and you know that a certain gamma ray sensor can detect gamma rays of a certain intensity and you know that you have a source of gamma rays here--another place in space--that you can accurately calculate, then the only thing you have to do is to watch the deterioration in intensity as you go out from this source, and it's the inverse square. There is hardly anything to argue against that. It can't be far from that. So I was quite satisfied really with the whole agreement for that reason.

Q: Going back to the delegations, you mentioned that the British in some of their personalities weren't as strong as they could have been. Did it appear to you that the British really hadn't sent their first team?

N: No. The British had sent their first team. I think the British had not spent any money on long range detection. We had spent a lot. This was in 1958. I had been spending starting with $30 million which we got cut down to $2 million by 1949. 50X4, 6, 8 [redacted] 50X4, 6, 8 [redacted] 50X4, 6, 8 [redacted] And I think they were primarily providing scientists who hadn't the experience that we had. 50X4, 6, 8 [redacted]

key role in the technical backup for the nuclear tests. We had been studying it. While we didn't have the best scientists in the world, we had the best I was able to find in the Air Force, and they had the advantage of experience, which nobody else had.

Q: We're looking at the outcomes again, the results of the conference. Did you have any reservations about, for example, the limits of out testing in comparison to Russians, and I'm speaking now specifically about very large nuclear weapons? I understand that was a controversy at the time by some military people anyway that they didn't like the test ban treaty because it didn't give us the opportunity to test these very large nuclear weapons which the Russians had already tested. Did you have any reservations in that area?

N: It really wasn't an area that--I didn't know enough about our own nuclear weapon stockpile in detail from the standpoint of what types of weapons and so forth. I was aware of the fact that we have relied on lower megatonnages with the idea of multiple warheads to do the necessary damage and that it seemed unnnecessary to overkill. It struck me all along that the Russians, in going to 50, 60 megatons, had a device that was going to be impossible for them to use anywhere without getting the world down on them so badly, for having killed literally tens of millions of the civilian population in order to damage whatever they were shooting at, that

they had a nonusable weapon. And I believed what I heard from the US weapon designers who had gone through, and particularly Harold Brown, who was the expert on that subject for the delegation. He had had a lot to do at Livermore [Lawrence Laboratory] with the development of weapons. He was on our Bethe Panel for a long time, too. And when he got to Geneva, we had, I think, the expert there. Well, Hans Bethe was there, too. We had the experts on what weapons could be utilized in weapon systems. I also was fairly convinced, after listening to the Russians talk about detection, 50X4, 6, 8 [redacted] it seemed logical to me that their weapon developments were pretty crude, big, complicated, non-usable things. When you consider the weight that you have in a 50X4, 6, 8 [redacted] that requires a missile capability that is going to be extremely expensive compared to the 50X4, 6, 8 [redacted] And I felt that we really had the better solution. I think that time has proved that even nuclear weapons as small as ours are not very usable. We have never been able to use them. They have constituted some sort of a threat standoff for the Russians, I presume, as the principal

contribution of our nuclear weapons stockpile. I think it will probably continue to be until the stockpile, if ever, if they ever are mutually eliminated.

Q: I know we have talked about this a little bit, but how was the agreement to be enforced?

N: I had always thought that there was a completely irrational approach to enforcement, and this included arguments I used to have with Sir Ormsby-Gore, who was the most intelligent and volatile representative of the British over there. He was absolutely superb in debate. And he used to spend hours with Bob Press, who was his assistant, and me trying to work out staffing patterns for a control post and staffing patternsfor the headquarters in Geneva, so that there would always be a western power guy on duty with an eastern power guy and at no time would they ever be alone long enough to be able to contrive some sort of a distortion of the data to their advantage. [redacted] Nobody had ever been able to put together the kind of efficient control post, manning and operation, that we have been able to do using airmen in the United States Air Force, given special training and orders to do it this way and not to deviate from it. The minute that we sent research teams out from Livermore where Livermore scientists would work with our sergeants

running a piece of equipment on [redacted] for example, goddammedest things would happen. They would decide to tear the equipment apart the night before the test and set it up a different way so they could get this new and more exotic thing. The result was when the time for the test came, they still hadn't got it back together yet, so they got no data, you know, where in normal surveillance our guys are ordered not to tamper with the equipment or to do anything to it. And I don't think that international control system is going to be worth the powder to blow it to hell. [redacted] It was recognized that with 180 nations involved, they were all going to do their little bit. Well, let them do their little bit and have an international control station in Vienna. Let them get inputs from seismic stations, unclassified ones, all over the world. Let them put acoustic stations out at those seismic stations and have that. Let them get fallout. Let them fly their own airplanes and get debris, [redacted]

50X4, 6, 8 [redacted]

50X4, 6, 8 [redacted] Now, at that time, it was considered you could not declassify the system. As I'm leaving AFTAC now, there are moves underfoot to declassify the AEDS, and we already have declassified two stations 50X4, 6, 8 [redacted] get the final answers on underground detection. So that step has been made. We also were forced into declassifying stations in Australia by the new leftist government that came in there, labor government I guess you would call it. I think it will end up by being unclassified. But this doesn't change the situation an iota in my judgment, because as long as you do not allow them to come into the station--let it be unclassified; bring them in and let them look around, 50X4, 6, 8 [redacted]

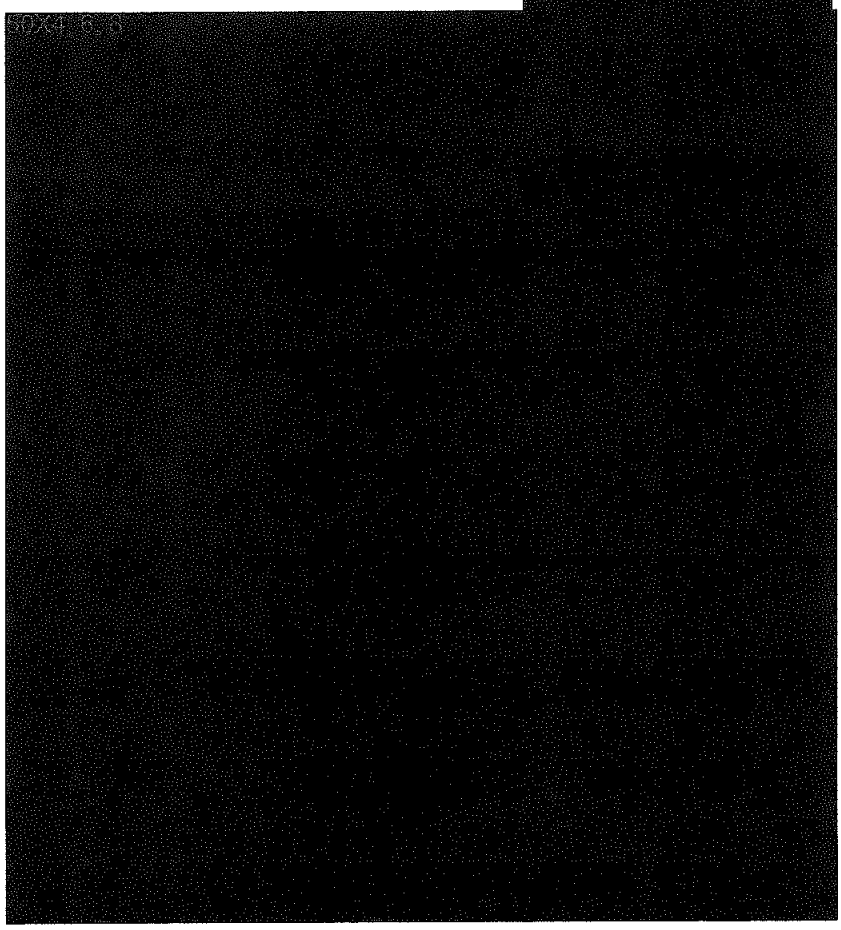
50X4, 6, 8 [redacted]

Q: But is there an international system that's set up now and working?

N: No, there isn't. Our system is as close to what comes to an international one now as it can be. 50X4, 6, 8 [redacted]

50X4, 6, 8 [redacted] There are other stations operated by friendly

governments that I cannot discuss here.

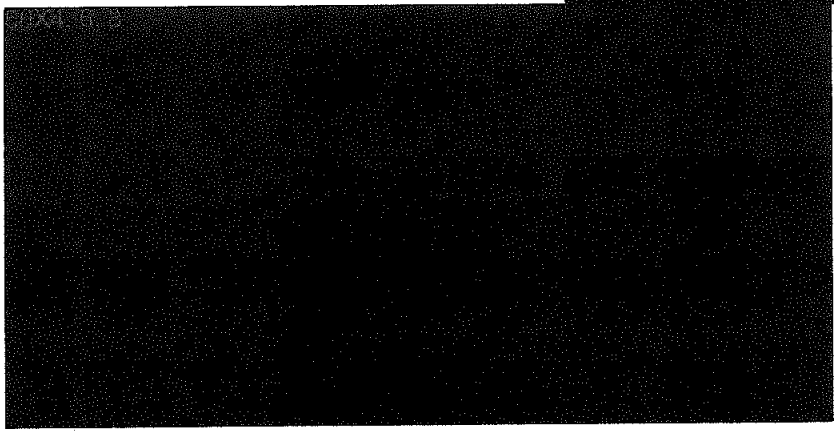


Q: Did the Pakistanis, for example, sign the agreement, test ban agreement?

N: Yes. I believe so.

I: What about on the Russian side, how are they set up to enforce the agreement? Do we know anything about that?

N: I don't believe the Russians feel it is necessary to enforce the agreement as we do. And, in any event, they don't have the geographical advantage that we have of islands and continents surrounding Russia. They would have to surround the rest of the world, and they are all localized pretty well, with a few exceptions. They couldn't get a worldwide system like we have. So I think they have just believed that they can, with their internal system, get as much as is necessary. Now we know they have a seismic system. They admitted to one in Geneva. They admitted that they were putting, or had put, microbarographs for an acoustic pickup at each one of their seismic stations,



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[redacted] So the

have a crude system, and I think they feel it's probably adequate.

[End Reel 2, Side 1]

O: We were talking about the enforcement of the agreement. Then, in conclusion, you would say that the enforcement is really just sort of left up to the good nature of the parties who signed it, that there isn't really an international control system established to enforce the agreement or to take action in case the agreement is violated.

N: No, that's right. Take a look at the Limited Test Ban Treaty. I think that was agreed to on both sides, on the basis each side would unilaterally monitor the other to the extent he thought it was necessary. There was no agreement about an international control system, and I have a hunch that idea is going to go out of consideration. There was something else I thought of as I was talking there. Well, never mind. Go on and I'll think of it in a minute.

O: Do you know if there have been any problems in this area since the Limited Test Ban Treaty was signed?

N: Oh, that reminds me what I wanted to say. We signed the Limited

Test ban Treaty and [redacted]

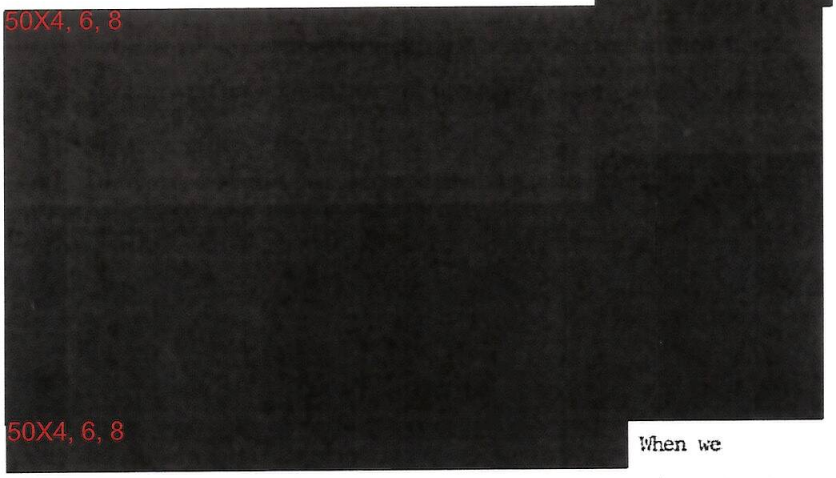
[redacted]

[redacted] So I wouldn't be at all surprised to see the next test ban agreement come up with far less stringent requirements by both sides, as to what on-site inspections might be. I think on-site inspections are highly undesirable things. They imply, right off the bat, lack of trust in another nation. And if your relationships with other nations start out with lack of trust, they can't do anything but deteriorate, in my judgment. I think you should have your own devices for checking up on what you

know about other people. And those shouldn't be widely advertised as international control systems.

Q: Have there been any accusations since the treaty was signed that there were violations by either of the major powers signing it?

N: Yes. The United States has accused the Russians, 50X4, 6, 8



When we finally convinced him of that, he then formally charged the Russians with a violation of the test ban treaty. I don't think it was such a harsh word as violation but that debris had been found outside the borders of the Soviet Union on such and such a test, and the United States was objecting to this in view of the responsibilities each had assumed in signing the test ban treaty. Then the Soviets came back and said, "Oh, no such a damn thing. We never let debris get outside the country." And that's all

there was to it.

O: The one project there where they were building that dam in the Soviet Union. I've forgotten the name of it right now. Was that before the test ban treaty or after, or do you remember?

N: Building a dam?

O: Yes. They were doing some cratering work to build a dam.

N: Oh, that's the very shot I'm talking about.

O: Oh, is it?

N: They fired off a shot to dam up a river and create a big lake, a reservoir back in the wilder sections of Siberia. I think I can say imagery, can't I, without violating the secret level, proved that it had created a reservoir.

O: Have the Soviets ever accused us of violating the treaty that you know of?

N: They said that they had evidence of debris that had leaked out of tests in Nevada, and we did have something like half a dozen cases, I believe, where there have been substantial amounts of

debris in the atmosphere. We traced them across the United States. We know they went into Canada because they were headed for Canada and then suddenly they leveled off. Our data leveled off because our planes were not allowed to cross the border and sample.

O: We have talked about this a little bit, but do you know how the violations are handled, how the machinery is set up to handle a violation?

N: No, I do not.

O: You don't know if normally it would be through diplomatic channels?

N: It's entirely, as I understand it, through diplomatic channels, and I expect it goes probably up through the White House.

O: Do you know whether the Soviets have been cooperative in implementation of the treaty as far as its provisions and all are concerned?

N: Well, from what I know of the SIPRI [Stockholm International Peace Research Institute] meetings, where they have discussed a much more extensive, and in my judgment a much more effective, peaceful use of nuclear weapons, perhaps because they have some projects in very remote unpopulated regions, which can make great changes

in their hydroelectric power resources or in their--well, they are, for example, building a dam [CANAL] between a river that empties into the Arctic and one that empties into the Caspian Sea, and it's going to be a canal some 65 feet [miles] long. We have detected several explosions where they are cutting that canal through rock and have to use something more than just TNT.

Q: I'm sorry. How long is the canal?

N: Something like 65 miles, as I recall, that they are cutting. They have got a substantial part of it finished, and that will dump water from the Arctic, that would normally go to the Arctic, divert it to the Caspian Sea. And I understand their problem in the Caspian Sea is that the water level is going down and creating difficulties all along the shoreline. So they're going to try to raise the level of the Caspian Sea. They've used it to extinguish gas fires--which I think are bonified uses--of gas wells that would just burn forever, and they have extinguished them with nuclear explosions. They have used it to activate oil shale where oil has suddenly stopped coming. They have an underground explosion. While we don't know how successful it was, our own program includes some experiments of that sort, too. But they seem to have more, and a more logical program. I think they have shown--I think that's a bonified use of nuclear weapons. I think it's one that everybody recognized in Moscow when they signed

this limited test ban agreement, that it was impossible to prevent every radioactive atom from being released from a nuclear explosion and to claim that nothing should be across the borders of the country is really not realistic. So I'm sure that there is, on both sides of the controversy, a feeling of tolerance of a certain amount of release from other activities. I think the Soviets' outgoing description of what they're planning in the SIPRI meetings is helping their case, and I would be surprised if they didn't do this deliberately to help their case, because once they've got something that the Americans can recognize as being a valid need, and the nuclear weapon is the only way they can get it, I think it makes sense for them to say so. It certainly minimizes the opposition, gives the people who want to use nuclear weapons to cut a canal through to the Pacific Ocean courage to go on and try to renew their efforts to get their project approved here.

Q: You may not know this; however, there has been a great deal of controversy in the United States in one of the areas of the application of peaceful uses of nuclear energy, and that's the power stations. Many communities, many people--most of them it appears are misinformed or not informed--it appears from what I read anyway--they are not scientists, but they are complaining that these nuclear power plants are unsafe. Do we have any programs that you know of to monitor these things to make sure that they are

[illegible] Are they safe or are they unsafe?

N: Monitor Station?

H: No. Our own.

N: The AEC, I understand, does have programs to check up on the safety or the lack of safety of nuclear reactors. We have not been brought into that pattern, and I think it's rather unlikely that we will, because this is one of the things that the AEC will keep very close to their belts in case there is ever information developed. They were very reluctant to have us check up on whether or not there were ventings in Nevada. Since we had a dual need to go in there, first to check our air sampling methods and calibrate them against actual nuclear debris as well as to find out whether there was a venting and how extensive the venting was, they were sensitive on that subject, and I'm sure they would be double sensitive on this reactor safety because that's their whole lifeblood really, is whether nuclear power can be used.

O: Well, you have quite an understanding of nuclear energy itself. Would you evaluate the safety of building these? From your knowledge of nuclear energy, would you say that you can design them where they would be safe?

N: I think that the things I have heard proposed as being hazards could be corrected but at a very high cost. I think the AEC has been unwise in going ahead with reactors where this coolant problem can be a hazard. It seems to me these things should have been recognized in the engineering. Now we have built these fantastically big plants that do contain these deficient or potentially hazardous things. Whether they really are or not, I am unable to judge. I haven't been sufficiently informed as to what--correction--I would be surprised if the thing is as bad as reported in the press. I think the AEC has done a better job than at least the press has said they have on this safety. I really have very little knowledge of this.

O: To your knowledge, have any additional agreements been made along the test ban line since the ones in 1958?

N: Well, it wasn't 1958. You remember it was 1963. A special commission to Moscow headed by--who was it?--the railroad man?

O: Harriman?

N: Harriman, yes.

O: Did you participate in that activity also?

A: Yes, and I think it contributed to the ultimate death of a very good friend of mine who was the number one guy in the State Department, the number one adviser throughout both the technical and the political, Charlie Bohlen. If anybody had deserved, by having given of his own life, three years of his own life to that--if anyone deserved to have been taken along, he certainly did. I don't know why he was left out, but he was.

Q: We talked a little bit about the Van de Graff electrostatic generator for use in nuclear research. I've read about this equipment for some time. Would you briefly describe exactly what it does and how it does it, and what did it contribute to the development of nuclear energy as we know it today?

N: The Van de Graff generator started out as a simple thing that Van de Graff made with a five gallon can and some ribbon and a motor, some silk ribbon for a belt, and a method of charging that ribbon as it goes up into the sphere and building up a high voltage on the sphere. It was just a new type of electrostatic generator. At the time that I was involved with the Van de Graff generator, we were talking about energies of 10 million electron volts. That is practically no energy at all by modern terms. The modern accelerators which went from the cyclotron, which was the primary competitor to the Van de Graff machine back in the 1930's, to these tremendous electron/proton/deutron accelerators, the linear accelerators,

and the big racetrack accelerators like the one at Bern that gets up to billions of electron volts, billions of electron volts. We were talking about 15 million. I just went through the accelerator out at Los Alamos this spring. That is a high energy machine, and it is just absolutely fantastic the things that can be done with a high energy machine like this. So the Van de Graff machine has been left long behind in the dust. One of its practical uses was developed by John Crump at MIT, who formed a corporation to build pressure insulated generators for use in hospital X-raying machines. And it did turn out, apparently, to be a useful source of high voltage and an economical source of high voltage for that purpose. But for the large machine--the Russians built, I think, the biggest electrostatic generator in the world at Kharkov, but it has subsequently been abandoned for the more sophisticated multiple acceleration, either linear or circular acceleration, machines. They certainly have taken over the entire field of high energy physics. Panofsky, at the University of California, is the expert in that area in the United States, for example.

O: So this was sort of the Model T Ford to the linear accelerators?

N: It was a Model T Ford. It was a good idea. It provided an impetus to the people who were working with cyclotrons. The cyclotron has sort of gone into the discard along with the Van de Graff generator,

and these linear accelerators have just gone miles ahead of it. And they are really unlimited. It's just a matter of getting enough geography, enough real estate, to lay down long ones. You can build one linear accelerator across the United States probably, if you wanted to, and really get some energies, but they now are at the point where they are using these machines to do research and to bombard things with heavy ions that couldn't conceivably have been accelerated in the days that I was working on high energy physics.

O: What do you consider as the major breakthrough in your particular field in nuclear detection?

N: I think the application of and development of satellite sensors is by far the most significant breakthrough.

O: What field are these sensors in? Are they the electromagnetic or . . .

N: No. They are optical and electromagnetic. [redacted]

[redacted]

50X4, 6, 8

[redacted]

50X4, 6, 8 Now that you could not possibly do with any kind--communications is our biggest problem on ground based worldwide networks, and I foresee that the whole thing is going to be done by satellites in the not too distant future, including underground explosions. 50X4, 6, 8

50X4, 6, 8

O: [redacted]

N:

Q: Are there any other systems that you know of that might prove to be just as feasible as the optical and the other one you discussed as far as satellite technology is concerned?

N: Satellites use more than just optical and EMP. I happened to mention those because they are the ones that we are utilizing most for getting data on shots in the earth's atmosphere. Now for shots in space the X-ray sensors, the gamma ray sensors and the neutron sensors on the satellites are the three primary radiations from a nuclear explosion. And the optical, for example, will not give you yield on a shot in space. You will have to get it by getting the total X ray radiation from the bomb 50X4, 6, 8

50X4, 6, 8 [redacted]

RD

Q: Would you mind explaining for us just exactly the theory behind the electromagnetic pulse?

N: When a nuclear explosion occurs in the atmosphere or in space, it occurs in the vicinity of the earth. It occurs in a magnetic field. The gamma rays and the X rays that come out from a nuclear explosion ionize the atmosphere and produce electrons which get turned around in the magnetic field. And whenever an electron is turned around in a magnetic field, it generates a signal. And this turn-

around signal, or the signal due to the interreaction of the electrons and their field and the earth's field is what produces a pulse just like you keyed a very powerful transmitter. Now if you consider what happens when you have a nuclear explosion in the atmosphere, the thing is somewhat different because the mean free path of gamma rays and X rays deep in the atmosphere is very short. As you go up in altitude, it goes down by a factor of 100, so at about three kilometers of altitude, the signal just about disappears. Then it goes on up at about 60 kilometers--60,000 feet, excuse me--the intensity of the electromagnetic wave produced is about equal to that on the surface. As you go to higher altitudes, it goes on up by another--at least factor two in our experience right now. But it is all a combination of, (A) the amount of signal produced is a mixture of the amount of mean free path of the radiations that produce these electrons and (B) the interaction of the magnetic strength of the magnetic field at a point where the electrons are produced and have their courses changed by interaction to the magnetic field.

Q: Well, is this the same phenomena that we are familiar with when they described the Pulsars in space?

N: I don't know. I really don't know. I haven't studied Pulsars well enough to know whether it's the same. I don't think so because--well, wait a minute. It could be that an interaction

between electrons and explosive releases of gamma rays in a star out in space or whatever, a plasma out there, could produce an electromagnetic signal that would radiate as far as the earth. I don't know. I think the Pulsars are--what's the distance from the earth? Aren't they millions of light years?

O: Several million light years.

N: And you wonder then whether electromagnetic radiation would be sufficiently--would be awfully diluted by the time it gets to the earth.

O: In any of your work with nuclear research, have you ever had any reservations about uses in which nuclear power has been applied?

N: Well, I personally feel that the nuclear weapon that has been developed, the use of fast fission and of fast thermonuclear reaction to produce explosive effects, is one that is probably not ever going to do the world a great deal of good. I think we could have done our peaceful uses exercises--I think we could have done without our peaceful uses exercises. I think most people who are familiar with the program feel that the world would be better off if atomic energy for explosive purposes had never been developed. I think it's just the business of can you always count on the man in charge being a sane individual? Now who was it?,

Will Rogers, who said that our Congress, the US Congress," is the best that money can buy." And somebody recently said that their concern about this Watergate business and the presidential isolation is that sufficiently large amounts of money can buy anybody. And if you buy the right people at the top of the government, you can buy the United States government if it's going to be this isolated from the people. I think that's the concern, that somebody could get absolute control, and if they had absolute control of nuclear weapons, they'd rule the world.

Q: Sir, would you give us a rough chronological review of the [redacted] operations and programs since its inception up to the present time?

N: That's a fairly large order. You mean starting back at the beginning?

Q: Right.

N: Well, we have already talked a little bit about the early operations on Operation Sandstone in 1948, which led to the setting up of a detection system which involved acoustic sensors, seismic sensors as far as the Coast and Geodetic Survey was concerned, and nuclear sampling with B-29 aircraft flown by Air Weather Service from Japan to Alaska. Now with that system, in 1949, August 29, Soviet nuclear tests, the first Soviet nuclear test, was detected.

Following the detection of this first Soviet nuclear test, certain budget limitations were ultimately removed [redacted] 50X4, 6, 8 and the system then was allowed to go into the research phase, the investigation of new techniques. The expansion was to cover larger geographical areas on a rather step-by-step basis. [redacted] 50X4, 6, 8

[redacted] 50X4, 6, 8

Following the test ban hearings of 1958 and the political conference that followed through till 1962, the need for an ability to monitor tests anywhere in the world and to monitor tests in space was

recognized [redacted] approved a proposal by General Rodenhauser and Mr. Northrup to expand the system of ground based techniques, acoustic, seismic, electromagnetic, nuclear, as well as optical and electromagnetic pulse, high altitude techniques, into the southern hemisphere and to install ground based high altitude techniques (COLT, which meant collocation of high altitude techniques) into stations [redacted] In the late 1960's, ARPA developed under the VELA program satellite techniques which even in the research stage proved to be surprisingly reliable and to have a wide range of capability for detecting nuclear [redacted] These VELA satellites are still operational and are providing excellent information on nuclear tests in the atmosphere [redacted] The DSP satellite system and the VELA system now comprise the satellite components of the atomic energy detection system. They have permitted the reduction--the almost complete elimination; not reduction--of ground based high altitude detection systems. [redacted]

50X4, 6, 8

[redacted]

Q: Who provides the command and guidance to AFTAC? Where does that come from, the Secretary of the Air Force, the Air Staff, Secretary of Defense?

N: 50X4, 6, 8

[redacted]

50X4, 6, 8

The guidance on research is provided by the Air Force Research and Development community and by DDR&E. A great deal of assistance is currently provided by a cooperative program between AFTAC and ARPA, whereby requirements

which are unable to be met by Air Force funds are often picked up by ARPA and conducted as requested by AFTAC.

Q: We have already talked a little bit about the VELA system. Who monitors that system? Is it monitored here at Patrick?

N: It is not monitored at Patrick. The VELA seismic facilities are monitored by a division of AFTAC located in Alexandria known as the VELA Seismological Center. It does work also for ARPA and is the focus of ARPA/AFTAC joint research and development programs.

Q: What happens to the information once a detection has beem made that a nuclear weapon has been detonated? What procedures are followed and who is notified?

N: [redacted]

50X4, 6, 8 [redacted]

Q: Who initiated the first program? Was there one man who said, "We need a detection program, and the Air Force will establish one"?

N: There was a committee formed by the Director of Central Intelligence, Admiral Hillenkoetter [Rear Admiral Roscoe H. Hillenkoetter] back in 1947, which involved Air Force, CIA, Navy, Army and other representatives, both technical and in the staff line, to consider suggestions initially put forth by Dr. Oppenheimer at Los Alamos that there should be some system for determining when a foreign nation obtained an atomic bomb, aided strongly by Admiral Strauss who provided residual funds from the Manhattan project for 50X4, 6, 8 [redacted] 50X4, 6, 8 [redacted] needs for money and who, in 1947, urged that a detection system be established. The recommendations of this committee were to put in acoustic, seismic and nuclear techniques and to concentrate them in some service where the system could be coordinated. Competition seemed strongest, as far as the early documentation is concerned, between the Navy and the Air Force. The Air Force won out primarily because possession of B-29's in the Air Weather Service allowed them to immediately do nuclear surveillance. This was considered to be the most important of the three techniques at that time. General LeMay, shortly after the Air Force became a separate service, assigned the project to General Kepner, who recruited General

Hagenberger [redacted] Colonel Holzman, a research officer in the Air Force, had the responsibility for the staff work in setting up all these initial arrangements for the organization, and recruiting Dr. Johnson to be the first technical director.

Q: Was there any relationship [redacted] Sandia?

N: [redacted] brought about by the original assignment of [the] mission to [redacted] which was to utilize all of the facilities available in the Army, the Navy, the Air Force and outside to do the job. Sandia has a very important capability in weapons effects and has been a contributor [redacted] research program from its very inception to the present time. They are now the agency developing sensors for use on-board satellites.

Q: Did AFTAC ever get involved in providing any policy guidance inputs to the Air Force or DOD or the civil government as far as the philosophy or doctrine for the employment of weapons or weapons effects or anything of that nature?

N: No. I do not believe so.

Q: You mentioned B-29's earlier in the air sampling program. Are

there any other air breathing type aircraft that we have used since? Would you describe them, tell us which ones are currently used for air sampling?

N: Following the B-29's, the workhorse of the air sampling team has been the WB-135, which is utilized up to altitudes of 30 to 35,000 feet; the U-2 for altitudes up to 65,000 feet and above; and the 57F's--WB, is it?, or WC; I don't remember--57F, that's the big wing modification of the 57--for high altitude surveillance. In 1972 and 1973, however, the Chief of Staff of the Air Force has urged the reduction in the use of the WC-135's and of the 57F's in the Japanese area. Those resources have been severely cut back to the point where any further cutback will have to be replaced by some other arrangement other than the use of these aircraft. It has been recently suggested and approved that we use the U-2R aircraft, which are in Southeast Asia, to be staged out of Osan [Korea] in the event they are needed in the Japanese area. Staff work on just what AFTAC sampling resources in 1974 will be is still incomplete.

(End Reel 2, Side 2)

O: Was there ever any attempt by someone else to get AFTAC reorganized into another branch of the service or come under a different organization?

N: Well, Jack Howard, who was Vice President of Sandia for Research and Development, was chairman of a panel that was reviewing AFTAC to see if they couldn't cut us back to a smaller size, or organize us more effectively to do the job, than the current organization. And Mr. Howard, in the office of Johnny Foster, DDR&E, presented a series of proposals which would remove AFTAC from the Air Force, which he felt was not giving it proper support, and place it under DDR&E, under DASA or DNA responsibility, place it in one of the other services other than the Air Force, or make it a separate agency of the DOD responsible solely for surveillance. These ideas, as advanced, showed so little knowledge of the way the Department of Defense is organized and worked that Johnny Foster came as close to losing his temper with anyone as I have ever seen him come and suggested that Mr. Howard might do better to confine himself to technical considerations and leave the organizational problems to someone else.

O: Has there ever been any interference on the part of the Congress or anyone like that to cut the program or to do away with it?

N: On the contrary, we have had absolutely the best coordination with the Joint Congressional Committee on Atomic Energy that we could possibly ask for. At times when they got wind of possible adverse actions contemplated for AFTAC, they would call for a presentation by AFTAC of what the current situation was. We have repeatedly, over the years, briefed the Joint Congressional Committee, particularly

at Mr. Polifield's instigation, on test ban problems and the ability of AFTAC to continue to monitor the test ban effectively. We have had nothing but the most outstanding support from the Joint Congressional Committee on Atomic Energy.

Q: I was reading an old report recently--went back to the 1946-47 time frame--and I was wondering if AFOAT was involved in anyway in the training of the early bomb commanders for SAC.

N: Not to my knowledge. I doubt that very much.

Q: Looking back over some almost 30 years of your association with the military services, what would you say is one of the most significant events or contributions that has been made during that period of time?

N: Well, I think that the most significant contribution was made in 1949 [redacted] the only information that was likely to have been obtained to the effect that the Russians had detonated a fission device in the central part of Asia and that [redacted] This constituted, for the first time, knowledge that the United States' monopoly on atomic weapons no longer existed and that we had a formidable competitor in the field. Other developments or other contributions [redacted] only tended to elaborate on the development

of the Soviet program. [redacted]

O: Were there any problems, any special problems of note, in the development of the different detection systems that you would care to comment on?

N: Well, I think that the most difficult problems have been the development of techniques for identifying underground explosions as opposed to earthquakes. I think that the researches that have been done have been most effective in that area. I believe the next most significant development, really, is the ability to utilize satellites and the extent to which this has made it possible to reduce the worldwide deployment of ground based stations. That is an expensive and diplomatically difficult job.

O: What about the VELA system? We talked a little bit about it. Is that an acronym? Does that mean something else?

N: No. It has to do with some constellation that was named Vela, and

it was just a word picked to identify a program which dealt with detection. In the beginning, Vela Uniform referred to underground; Vela Hotel referred to high altitude; and Vela Sierra referred to tests in the atmosphere. Most of the work in the Vela program under ARPA has concentrated in the underground and in the satellite area.

Q: In looking at all of our detection devices, would you say that the satellite system is the best system of them all?

N: No, I would not say it's the best system of them all. I say that it has advantages in that data acquisition can, in the future, be completely at continental locations, continental US locations. And that is a tremendous advantage from the standpoint of security and diplomatic intrusion into other people's real estate. Far-flung detachments on foreign soil are fraught with all kinds of administrative and diplomatic difficulties from their inception to the present day use of the station. We have lost many locations and lots of expensive installations where countries have had unstable governments.

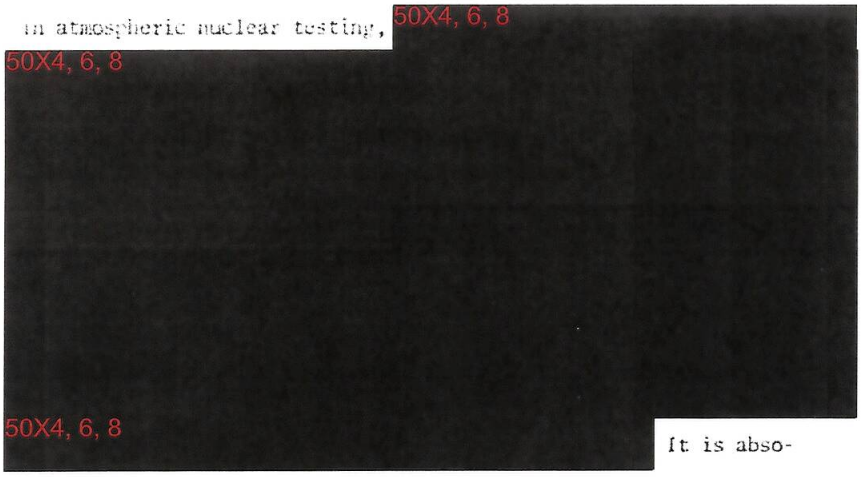
Q: This question is a little speculative, but in working with the detection area as long as you have, are there any countries in the world today that do not possess nuclear weapons that you think would be able to produce them in a very short period of time?

N: Well, I am just repeating what I know from other people's studies there. India certainly has the capability of producing a nuclear weapon, but I think the industrial resources necessary to have a substantial stockpile of weapons do not exist in India. I believe it is a possibility that under a proper stable government one or two of the South American countries have the potential of development of nuclear weapons. And, of course, one of the concerns is that the Middle Eastern powers, Israel or Egypt, might develop tactical nuclear weapons. This is certainly a realistic possibility.

O: Mr. Northrup, is there anything that we have talked about today or anything that we haven't talked about today that you would like to express an opinion on, anything about the command and control structure, the way the program has been established, the way it is run, any limitations that you think should be brought out, anything of that nature?

N: Well, I think that recent pressure from the Air Force to reduce the use of aircraft in connection with nuclear sampling has gone to the irreducible minimum if not already further than is really helpful for the future surveillance activities that I would imagine AFTAC would have. I would hope that the next Chief of Staff will find it unnecessary to apply the kind of pressure that has been applied from the Chief of Staff's office in this regard. I think that we probably can continue to do a good job with no more

resources than are now available if there is not any proliferation in atmospheric nuclear testing; however, if there is proliferation in atmospheric nuclear testing, 50X4, 6, 8



It is absolutely essential, since we are now relying so heavily on satellite sensors for coverage, that any malfunctions in those sensors should result in a launch of a new package to replace that capability.

Q: Do we have the capability today for an immediate launch of a new satellite in case one malfunctions?

N: I don't believe we do. There are satellite packages available and there are boosters available, but programming such a thing is a very time consuming and laborious process and competition with other high priority projects would, I'm sure, prevent a prompt replacement of a current DSP satellite.

Q: How long could we normally expect one of these satellites to stay in orbit and function?

N: The Vela satellites--Vela 5 launch--have now exceeded by a factor of two the estimated meantime to failure. It is now estimated that in FY 74 they will probably fail. This means they have provided something like five years or more of reliable coverage. I would hope that other satellite systems can show this same reliability. DSP has not been in being so long, and yet there have been evidences of malfunctions there, which, if they proliferate, will begin to degrade the capability. But I can't emphasize too much that in the recent budget cuts we have so severely limited ground based coverage that satellites are absolutely vital to the AEDS coverage. And should they be lost, the coverage will drop very severely.

Q: Are these satellites developed here at AFTAC?

N: The satellites are developed by Systems Command at SAMSO and interfaced with sensors that are developed at Sandia. The developments at Sandia are cooperative developments where the research people and the development people work very closely with AFTAC and do their very best to insure that the sensors are going to meet AFTAC's requirements as effectively as possible.

Q: How difficult is it to obtain qualified scientific personnel for an organization such as AFTAC?

N: Well, it's not easy. Most scientists nowadays prefer to work in an area where they can publish the results of their work and get some personal approval for it. There are only a few who are satisfied to work under the conditions as they now exist. The move of AFTAC to Patrick is not the greatest thing in the world that ever happened to AFTAC. We have been moved out of the arena where most of our contacts exist [redacted] No provision has been made for top level scientists of AFTAC to have reasonable transportation arrangements between Patrick and Washington. [redacted] for example, one must arise at five in the morning and leave by automobile for Patrick Air Force Base; then on to Orlando; take an airplane which gets into Washington before noon; and cannot then leave until 7:30 in the evening after the meeting to return; and be back in his quarters about eleven o'clock at night. [redacted] The requirement for these meetings is at least monthly, and in many cases weekly, and imposes a very severe inconvenience on personnel operating from Patrick Air Force Base. I think it's the one single most unfortunate development that has happened to AFTAC in the last decade.

Q: Looking back on your tenure as the director here, your position was sort of a scientific position as well as a managerial position. Which one of these two aspects of the job do you think would be most important for the next director, that he be a scientist or a good manager?

N: As far as the technical director is concerned, it's about 50/50, I believe. Some of the best scientists get in trouble every time they move within an organization and, though scientifically highly competent, can produce more staff difficulties than a less competent scientist with an effective knowledge of how to work with the staff.

Q: Did you ever feel that your talents as a scientist were not being fully utilized or recognized, for example, when you were presenting certain aspects of the program?

N: No. I think I have been given every opportunity to utilize whatever talents I have as a scientist. I think one of the difficulties of working in a military organization, however, is the lack of responsibility that is given to the scientific personnel, where personnel at the PL-313 level are equivalent or higher in rank to the administrative directors in AFTAC. It has frequently been necessary to spend more time convincing the officers with the authority to take the actions that I have felt were technically necessary than is really healthful for either the orgnization or for the technical director's sense

of contributing.

Q: In retrospect, is there anything about the program that you would have changed?

N: I don't think there is any other way that the organization could be--I don't believe there is any organizational change that would solve the problem, and the problem is that knowledge of what should be done resides in the technical people's mind and responsibility for carrying it out is assigned to an officer who may not be easy to deal with. This has occurred in the past. I don't know how you would correct this other than to give it entirely to a civilian organization, and I don't want to get into the business of how to reorganize AFTAC. I think it has worked pretty well. The objections that I mentioned are things that people working in a military organization should recognize before they come into the organization, but it's one which I do think will act as a deterrent to getting competent people to come to the Patrick Air Force Base and subject themselves to this kind of an undesirable position.

Q: Well, this problem you say that arises, is it because of a lack of technical knowledge on the part of the person that comes in?

N: No. Usually the person with less technical knowledge produces fewer troubles. He tends to rely pretty well on the advice of his

Technical Director. It's the fact that no one in a responsible position--how do I say this? a similar position in industry would carry a great deal more responsibility--is the best way I can say it. The responsibility does not reside with the highest ranking of the civilian scientists in the organization. It resides with the military organization and frequently PL-313 personnel find themselves forced to argue with stubborn military personnel in comparable levels in the organization but not with comparable knowledge of what the problem is.

O: Or authority.

N: Or authority.

O: Do you have anything else, sir, you would like to talk about?

N: I think I have just about talked myself out.

O: Well, sir, I certainly appreciate your taking the time to sit down and talk to us this afternoon.

N: Well, thank you. I would like to say that you are a very easy person to talk to, and I'm sure you've brought out many more things than I would ever have initiated had you not been along to catalyze the conversation.

(End Interview #685, Reel 3, Side 1)