Description of document:	US Air Force (USAF) Oral History Program Interview # K239.0512-685, Mr. Doyle Northrup, 24 July 1973, 2015
Appealed date:	24-March-2015
Release date:	07-July-2015
Posted date:	12-January-2026
Source of document:	Headquarters Air Force/AAII (Mandatory Declassification Review) 1000 Air Force Pentagon Washington, DC 20330-1000 Email: SAF.AA.MDR.Workflow@us.af.mil

CLASSIFICATION: **UNCLASSIFIED**

WASHINGTON DC 20330-1000

July 7, 2015

SAF/ AAII (MDR)
1000 Air Force Pentagon
Washington DC 20330-1000

This is in response to your 24 March 2015 appeal of 14-MDR-166 pertaining to IRIS document 00904802 Oral History Interview.

Attached is the Oral History responsive to your MDR request. This review found information still requiring continued classification in the interest of national security.

Should this decision be appealed, write to the address listed below within sixty (60) calendar days from the date of this letter. Please include your reason for reconsideration and attach a copy of this letter:

Interagency Security Classification Appeal Panel
700 Pennsylvania Avenue, NW
Room 100
Washington DC 20408

Address Questions concerning this review to the undersigned at (703) 693-2560 and refer our case number 14-MDR-166.

PENNY JENKINS
Mandatory Declassification Review
Program Manager

Attachment
Final Document

CLASSIFICATION: **UNCLASSIFIED**

K239.0512-685

Cat 2

DECLASSIFIED

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

Cite or quote only
with permission from
Interviewee

CLASSIFIED BY ____________
SUBJECT TO GENERAL DECLASSIFICATION
SCHEDULE OF EXECUTIVE ORDER 11652
AUTOMATICALLY DOWNGRADED AT TWO YEAR
INTERVALS DECLASSIFIED ON DECEMBER
31 ____1981____ MICROFILMED BY TIM

Debin #
0560830
DECLASSIFIED

00904802

3-8531-4

1. Pearl Harbor attack (SH)
2. Torpedoes (SH)
3. Nuclear detonation (SH)
4. Nuclear detonation - Dectection and Reporting (SH)
5. Nuclear detonations, 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)

DECLASSIFIED

CLASSIFIED BY
SUPERSEDED BY
SCHEDULE
AUTOMATICALLY DOWNGRADED AT TWO YEAR
INTERVALS DECLASSIFIED ON DECEMBER
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

DECLASSIFIED

"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 - Doyle 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 25 May 1951)

COPY

EXCEPTIONAL SERVICE AWARD

CITATION

Mr. Doyle 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 Doyle 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. [illegible] 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.

Page 1


INDIAN
OCEAN
RLD

(SECURITY CLASSIFICATION AS REQUIRED)

~~SECRET~~

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 1973.
Day Month Year

(Please initial the appropriate spaces.)

Security Classification

UNCLASSIFIED_____CONFIDENTIAL_____SECRET__X__TOP SECRET_____

_____(1) The transcript(s) or tape recording(s) may be read, audited, quoted, or cited by researchers upon presentation of proper credentials to an Air Force-designated custodian of the document.

__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)_____

CLASSIFIED BY _____
EXEMPT FROM GENERAL DECLASSIFICATION SCHEDULE OF EXECUTIVE ORDER 11652
AUTOMATICALLY DOWNGRADED AT TWO YEAR INTERVALS DECLASSIFIED ON DECEMBER 31, _____

Taylor K. Mattingly
Interviewee's Signature

24 July 1973
Date

Lynn K. Gibbon
Witness' Signature

24 July 1973
Date

~~SECRET~~

3 Jan 73 (SECURITY CLASSIFICATION AS REQUIRED)

~~SECRET~~

UNITED STATES AIR FORCE

ORAL HISTORY PROGRAM

Interview #K239.0512-685

of

Mr. Boyle Northrup

By

Lt Col Lyn R. Officer

Date: 24 July 1973

Location: Patrick AFB FL

NOTE: Pen and ink changes made by editor.

~~SECRET~~

[redacted]

~~SECRET~~

Oral History Interview #685
24 July 1973
SECRET
Taped Interview with Mr. Doyle Northrup
Conducted by Lt Col Lyn K. 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 battle-ship 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 sheet 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 30 caliber machine guns, and we had to go over the side several times when this happened on the way back.

D: 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 inshore 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,

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.

O: 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 87 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.

Q: 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.

O: 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.

O: 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 aw 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

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.

Q: 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.

Q: 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

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

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.

Q: 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.

Q: 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

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

O: 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.

O: Who headed the American team?

N: Jim Fisk.

O: 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

this." 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

Panofsky did a lot of good work, and he and Dick Latter from Rand Corporation and the screamer--what the hell was his name?--Sterling 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.

O: 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.

O: 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.

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

N: Oh, yes.

O: . . . 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

N: 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 Natione. 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." Carl got up and said, "Well, gentlemen, Dr. Leipunski"--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.

O: 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

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

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

safe? Are they safe or are they unsafe?

N: Monitor Russian?

O: 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 those 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?

N: No, 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 Stell. 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,

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 <u>without</u> 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?,

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.

Q: 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

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.

O: 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.

O: 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

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.

O: 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 those 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.

O: 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)