AN INTERVIEW

WITH

REID DENNIS MAYO

JUNE 4, 9 and 24, 1981

CONDUCTED BY

[redacted]

HISTORIAN, NAVAL RESEARCH LABORATORY

TRANSCRIPT PREPARED BY

[redacted]

THE NAVAL RESEARCH LABORATORY
SPACE TECHNOLOGY HISTORY PROJECT

On June 4, 9, and 24, 1981 I, Reid D. Mayo, the interviewee, voluntarily participated in a taped interview with [redacted] representing the Naval Research Laboratory. The interview was made in connection with the NRL Space Technology History Project.

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[redacted]

Reid D. Mayo
Interviewee

9/4/81
Date

Accepted for the Naval Research Laboratory by [redacted]

9/4/81
Date

*This and other signatures must be identical with the name of the interviewee as it appears in the first paragraph, page 1.

PREFACE

The following manuscript is the transcript of an oral history. As such, it has a number of qualities that must be borne in mind by the reader. The manuscript presents recollections of one individual. They are subjective and are largely unsupported by reference to official documentation. The participants in this interview did make use of documentation -- in fact, more so than is the norm in oral history interviews -- and during the editing process, further checks were made on dates and names. Nonetheless, the transcript is limited in its perspective and accuracy. It is not the result of comprehensive historical scholarship.

The lack of objectivity and completeness is in some ways a defect. Yet it also has advantages. The historian who attempts universality and objectivity in his coverage, who attempts to write from "God's point of view" as it were, always adds an air of unreality to his story. No one involved in the course of events is able to see things from this perspective, nor is anyone able to maintain the historian's lack of involvement. Each major player in the drama of events was subjective and did have a particular point of view. Moreover, each player acted on the basis of this personal knowledge and interpretation of events and not on the basis of the universal knowledge the historian seeks in retrospect. The subjectivity of an oral history, therefore, captures a sense of realism that is often lacking in standard historical accounts. Even though oral history does not present completely faithful expression of a participant's perspective -- recollections are mellowed and changed by the passage of time and events -- it comes closer than anything a historian could produce solely on the basis of documentary scholarship.

This transcript, then, should be read and evaluated in terms of what it purports to be: a set of recollections of one man who has a valuable perspective on the development of an important program in the U.S. Navy from the time of the inception of the program until the present. Certainly there will not be universal agreement on many of the interpretations of events presented here. This is to be expected. There was not agreement at the time the events occurred, and the passage of years only buries differences, it does not resolve them. The preparation and publication of this document by the Naval Research Laboratory does not indicate that Mr. Mayo's point of view is the truth, or even that it is the official point of view of the Laboratory. It only indicates that NRL believed his point of view was well worth recording.

Since the principal subject of this oral history is the origin and development of the first Navy satellite surveillance program, the reader may wonder at the attention given to Mr. Mayo's personal background, his years as a Navy enlisted man, and his years working on other projects prior to his involvement with space. The reason is that the interviewer, the NRL Historian, intended the document to have multiple purposes. It was an opportunity to capture not only a view of the history of a major defense project, but also a record of how a significant individual in the history of NRL developed intellectually and was attracted to the institution, what life was like at the Laboratory for uniformed personnel during World War II, what the character was of the early post-war years in the NRL counter-measures branch, and so on. These varied purposes made the transcript somewhat more disunified than it might otherwise have been. Only the thread of Mayo's career holds it together. Consequently readers are encouraged to use the table of contents to identify topics of particular interest to them. The biographical sketch that preceeds the transcript should also be of some help.

Finally a word about editing. Oral history transcripts are usually intended to be as faithful as possible to the original taped conversation from which they are derived. Changes are made only to improve readability or clarify expression. In this instance, however, the editing was much more extensive. Both the interviewer and the interviewee carefully reviewed the rough draft, and in many places, what was said in the original discussion was substantially changed. Usually the alterations corrected misstatement or clarified cryptic expressions, but in some cases, the original interpretation was modified substantially. Both interviewer and interviewee, however, believed that the changes increased the quality and usefulness of the transcript.

BIOGRAPHICAL SKETCH

Many research engineers become tied to a single form of technology early in their careers and follow it throughout several phases of development. Often the path leads to a dead end, and the engineers are forced to either migrate to a new subject or resign themselves to fine tuning and fiddling. Occasionally, however, an idea or a technology proves so fertile that exploitation continues to prove challenging and rewarding year after year, decade after decade. Such as been the case for Reid Dennis Mayo and the development of crystal video radar detection systems at the Naval Research Laboratory.

Reid Mayo was born on February 18, 1925. Years on a farm during boyhood and interest in mathematics in school gave him the practical aptitude and the intellectual ability necessary for a career in science and engineering. When he joined the Navy as a boy of 18 in the midst of World War II, he was quickly assigned to study electronics maintenance and repair. Training at the Radio Material School at the Naval Research Laboratory gave him his first acquaintance with the institution that later would become the focus of his entire career as a research engineer. After schooling at NRL and elsewhere, Mayo spent six months, during the last year of World War II, roaming from ship to ship in the Pacific and teaching sailors to use and repair counter-measures equipment. On several occasions, he was involved in naval engagements.

After the war, Mayo returned to Washington, attended George Washington University, and graduated in February, 1949, as a Bachelor of Engineering. Jobs were tight, but his practical experience with Navy countermeasures equipment during the war eventually helped him land a position in the Countermeasures Branch at NRL.

His first significant assignment was analyzing the German ATHOS system, a body-mounted crystal video radar detector for use by watchmen on surfaced submarines. He was to design an American equivalent. Although he succeeded, the unit was never put into production.

Next he moved to a group that was designing an airborne crystal video detector. In this case, several forms of equipment he helped develop did go into operational use. Then in late 1957, at the special request of Admiral Grenfeld, the crystal video group at NRL hastily designed a crystal video detector system for installation in submarines. This equipment (R-467(XB-3)/ALR) proved extremely useful during U.S. reconnaissance on a major Soviet fleet exercise. It was NRL's most successful application of crystal video technology so far.

While improvements on the submarine detector were still being made, Mayo conceived an idea that would shape the remainder of his career. The time was the spring of 1958. The field of outer space was just emerging as an area that was ripe for military exploitation. In October, 1957, the Russians had shocked the world by launching SPUTNIK I. Other Soviet satellites followed quickly. Reacting to the furor that ensued, Chief of Naval Operations Admiral Arleigh Burke had ordered the Navy research comunity to give serious consideration to the uses the Navy might make of outer space in the future. NRL, hard at work on the VANGUARD satellite program, was already involved in space, but everyone realized that this was just a beginning.

All these matters turned over in Mayo's mind as he headed back to the Laboratory one day in March during a heavy snowstorm. He was stopped by weather. Huddled in a restaurant, his family asleep around him, Mayo turned over a placemat and began calculating. What would happen, he wondered, if one "raised a periscope" into outer space? That is, would a modification of the crystal video system that had just proved so successful when mounted on a submarine periscope function in outer space? The calculation indicated that it would. Moreover the extreme height would give the system coverage that was far greater than that not only of the submarine system but also of airborne crystal video systems.

Upon returned to NRL, Mayo explained the idea to Howard Lorenzen, head of the Countermeasures Branch, and found him receptive. So were other Laboratory leaders. A modified VANGUARD satellite would carry the crystal video detector, it was decided, and the output could be relayed to small collections stations located along the orbital path around the earth. The idea was so simple that the cost would be minimal, when compared to other proposals for space systems being considered by the Department of Defense. The idea was fleshed out further and then sold throughout the Navy and ultimately to the President himself. Numerous problems arose in getting the system designed and built, but on June 22, 1960, the first satellite of NRL's program, DYNO I, was successfully placed in orbit. It was the nation's first operational electronic intelligence (ELINT) satellite. Its cost had been only 1.1 million dollars.

Not surprisingly after its striking initial success, NRL was ordered to continue the development of the satellite system. This became a major task of the Couuntermeasures Branch for many years. For Reid Mayo and a number of associates, it was a full-time job.

Initially the capability provided by the system was designated as a national resource. Data collected was used for determining the electronic order of battle of major enemy strategic systems. Hence when the National Reconnaissance Office (NRO) was established in 1962 under the Secretary of the Air Force, the NRL effort was placed under it and became part of "Program C." By 1965, however, new processing techniques and the use of a computer at a collection site had begun to indicate that POPPY, as the program was now called, might be able to provide near-real-time information on emitters on ships at sea. This capability, plus POPPY's high vantage point, made it an [redacted]

Plans led to action. On January 14, 1971, a Navy Space Project office (PM-16) was established within the Naval Material Command and the Naval officer in charge of Program C was also named its head. The aim was to build an organization that could develop space resources for specific Navy use. [redacted]

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Throughout the development of POPPY [redacted] Reid Mayo had been a key player for both program planning and generating new technical ideas. Consequently he had been given continually increasing responsibility and authority. A section head when the project began, he rose to branch head in 1971 during a project reorganization related to the establishment of the Navy Space Project. When the Special Project Office had been established at NRL in July 1972, Mayo was detailed there to head the Space Systems Division and oversee spacecraft production by NRL. In 1973, when NRL established the Advanced Projects Office [redacted] It is from this position also that Mayo retired on January 10, 1981.

A career in classified research and development yields little formal recognition. Mayo -- by nature humble, self-effacing, and a team player who, even as a leader, saw no job that needed to be done as beneath his own personal attention -- did not require personal recognition to maintain his commitment and drive. During his career, he did receive a handful of rewards that signify at least partial acknowledgement of what he achieved. In 1961, he was included in an incentive award given to the NRL team that had conceived and developed DYNO I. In 1963, he was accorded the Navy Distinguished Civilian Service Award for "the exceptionally outstanding development of a new scientific method to secure critical military knowledge for the United States." For his role in [redacted] he received part of three different group awards. His record also shows outstanding performance ratings, quality salary increases, and numerous letters of thanks and praise. Yet undoubtedly Mayo receives his greatest sense of accomplishment not from these, but rather when he turns his eyes upward and thinks of the satellites passing silently overhead, keeping watch on potential enemies and transmitting to the Navy and the nation vital information that makes the world a safer place for America.

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Side 1 June 4, 1981

A Now, let's just start at the beginning. You were born on the 25th of February, 1925 and that was in Albuquerque.

M The 18th of February.

A You've misstated on your 171!

M I did? I did.

A Shame on you (laughter), the 18th of February 1925 and that's Albuquerque, New Mexico.

M Yes.

A Now, the other day you mentioned a number of things off tape to me about your background and I thought I would summarize just a few of them here. Correct me if I've made a mistake.

As I understand you were raised by your parents until age 8 at which age your mother passed away. At that point, your father was ill, and you lived for a while with some aunts and uncles. This turned out not to be a good arrangement, so you and your brother were adopted by a farm family. This period in your life continued for some years, but was not an altogether happy period. At another age, which I don't remember, you moved to live with relatives in California.

M That was the end of my junior year in high school.

A You stayed there, I guess, for a year.

M For one semester.

A Your father had, by that time, come out of the hospital, and you and your brother joined your father in Ohio, where you completed your senior year in high school.

One thing I did want to discuss with you on tape was your early education and whether during your high school years you had developed any particular interest in science and engineering. Or was that a later development?

M In high school, both my brother and myself were interested in getting advanced courses in mathematics beyond what was normally presented at high school in Idaho. They did run a special class for about 8 of us in algebra, in the introduction to calculus, and in analytic geometry. As far as school was concerned, it was easy and enjoyable. The "fun" part of our life was in school. We always got good grades, and were among the top five or six in the class. There was quite a competition between my brother and I. We were in the same class because he was just 13 months older than I. As far as science, I don't think that we had chemistry -- maybe an introduction to it or something -- but nothing....

A Nothing that you would call particularly significant.

M No.

A Did you do any tinkering around at home with science or engineering?

M No....

A ...or did you do basically your school work.

M Just at school -- and of course all the farm things you had to do ....engines and mechanical things of all different kinds.

A Did you actually work on engines at home?

M Some. We had a one-lung gasoline engine that ran the pump that was always breaking down. We had to fix the govenor on it and things of that type. It was mostly farm machinery. One summer I had a job assembling the McCormack Deering farm machinery. It's a standard brand and is thought to be of fairly high quality. But those who assemble it find out that the equipment is massed produced and the holes don't match up. You have to force things. You just get a bigger hammer (laughter).

A Do you remember if your interest in other subjects at school was as high as your interest in mathematics, or was that a particular field of interest for you?

M That's the one I remember being of most interest. I didn't have any trouble with subjects requiring memory, and I could read fairly well. I expected to go to college, but I didn't know where or how. The [farm] family we were living with was not interested in advancing my education. I wanted to go to a larger high school, and that's why I left after my junior year. That was somewhat disillusioning though because when I attended Mark Keppel High School in Alhambra [California] they must of had 4,000 students. That was three times as large as the whole town [in Idaho] in which I had lived!

A So your high school previously had only been 100 or so?

M 100-150, something like that. We had 1,000 people in the town, and students were brought in from the countryside to a consolidated school. The large high school, though, was so foreign to me that it was almost a wasted semester. I remember taking physics from a tiny old professor there, a doddering old man. He'd forget what he was saying in the middle of a sentence. It was not a rewarding experience.

A So after you graduated from high school were you drafted?

M I was drafted almost immediately.

A And this was 1943.

M Yes, that would be 1943, the spring of 1943. I got out of school in June, and within a month I was in [the Navy

Training Center in] Great Lakes [Illinois]. I selected the Navy. My interest in the Navy went way back to boyhood chums in Idaho. There were three brothers, and their father had been in the Navy during World War I. All they ever talked about was the Navy. They were strong advocates of the Navy. Well here we were in the south central desert part of Idaho, with no water around hardly at all, and the Navy seemed to have a great lure.

A They probably knew about as much about the Navy as you did in reality (laughter)!

M Yes, but the contrast was so extreme. We went through Great Lakes and I was in the Great Lakes Choir which got me exempt from some of the more distasteful duties they assigned to boots. I remember [Eddie] Peabody the banjoist. He had a regular weekly program and two or three times during our boot camp, the choir sang on his program on the radio.

A Did you go directly into electronics school at Great Lakes?

M Well, they put your through an aptitude test, and in those days with the Navy installing so much electronic gear aboard their ships, right in the World War II heyday, they needed maintenance people desperately. Anyone that could spell algebra and knew which end of the soldering iron got hot was automatically a candidate to try out for this school. They had a pre-radio school at Wright Junior

College in Chicago. It was really quite a shock to me to go from high school, to boot camp, to this very intensive curriculum. It started early in the morning and went in a regimented, hour by hour [manner, continuing] all day long until about 10:00 o'clock at night. Every time there was a break, I had a choice of laying down and taking a nap or going to eat. Quite often, I would lay down and take a nap! I just was not used to that intensive schedule, and really I was not all that interested. I was very poorly motivated. They didn't do a sales job on us like they could have.

A Was that typical of your class? Was there a lot of discontent?

M Oh yes. Everyone was griping. We didn't see any advantage to it at all. I was looking forward to flunking out of the class and getting on with the Navy! I didn't want to work that hard -- starting at 5:30 in the morning and working until ten at night. Classes, exercise programs, all kinds of things.

A How soon after you got into the Navy did you end up there? Had you been in boot camp six weeks when this happened?

M I'm not sure of the length of time. I know I went immediately from Great Lakes to Wright Junior College. It probably was a period of a week's leave or something after boot camp, then I reported to Wright Junior College for about a two month period. Because I was flunking out of that, I was put on a work party for about a month. Well, I could see after that work party that school was a whole lot better alternative! There were many things worse than Wright Junior College (laughter).

A What kind of things were you doing in that work party.

M Well, we were installing equipment and [it was] heavy work. It was all hammer and crowbar and lugging. We did many things the hard way, the "Navy way" (laughter), not particularly well thought out. They were not making it easy for us. It was, in a sense, a kind of punishment to those of us who had goofed off. But, I did going through [the curriculum] the second time, and got high enough grades that I was able to select the school that I wanted. I was in the group that showed up here at the Naval Research Lab., which was called Bellevue in those days. I showed up at Bellevue two or three days before Christmas in 1943.

A Let me just ask you, did you have to repeat the whole course?

M I took the whole course the second time. There were a few, 10-15% of those that flunked out who, if they showed some interest in giving it a better shot, had a chance to repeat. They were desperate to get maintenance personnel, and they didn't want to wash you out because you were poorly motivated.

A If you had the aptitude, they really wanted to develop it.

So, you came here to NRL. What led you to choose to come to NRL?

M Well, the reunion with my father had been so satisfactory after so many years of being separated that I wanted to be as near to Toledo as I could. The train service from Washington to Toledo was very good. I expected to visit there on holiday weekends and things like that. And I did to some extent. The alternatives weren't very good either. Treasure Island in San Francisco was one, and I had been in Los Angeles, in southern California, and didn't look forward to going back there. I think the other alternative was Corpus Christi, Texas. What I knew about Texas wasn't very accurate -- I thought it was all sagebrush and sand, and I'd lived in Idaho with that! Bellevue looked like something I didn't know about, and it would be a whole lot closer to Toledo.

Now the curriculum in Bellevue was divided into two parts. The [primary] part was about three months. I was in the last class through that curriculum here at the Lab. After we graduated, all the equipment was moved to Little Rock, Arkansas. In fact, we jokingly said that if you dropped a screw driver in the lab during our class, somebody would pick it up put it on a truck to go to Arkansas. They were moving things out underneath us every day. The secondary school was where you took specialized training in sonar, radar, communications, and also a lot of electronic engineering type courses. The curriculum was, again, very intensive, even more so than Wright Junior College. But by then we were sort of committed and used to it.

Nelson N. Cook was the head of the school. The Cook slide rule and Cook textbook [on electronics] were what he was famous for. He also set the standard of comparison in the physical obstacle course that they had out here in front of the Lab over beyond the street [now route 295] up the hill. They had a full blown obstacle course with cargo nets and logs and barrier walls and all kinds of things. I remember that the sailors would run out the front gate on Saturday afternoon after inspection and they would have to go over the obstacle course.

A Every Saturday?

M Well, not every Saturday, but once a month, I think, it came your turn. It was a remarkable cooincidence that the foot of the hill was the sick bay (laughter)! If you fell, you slid in the right direction. It was a normal kind of an obstacle course but with unnatural hazards because the dogs in Southeast used to wait for us (laughter). It was really an exciting trip!

Nelson Cook is the one that set the [elapsed] standard for time through that course that we all had to achieve. If you achieved it, then you were exempt for the next month. About every other month we had to do it. I didn't have any problem with it, but some of the fat fellows and older fellows were having a great deal of difficulty. We also had to go through a swimming test in what is now the NRL recreation club pool. That was one that worried me because I couldn't swim very well. As it turned out, they put so many sailors in the pool for the swimming test that you couldn't sink! It was sort of wall to wall humanity in there so I passed.

A In floating across, you were pushed by others.

M You just had to stay alive in the pool for a certain number of minutes. You didn't have to swim any distance or anything. It was just to be in there and get out alive.

A I take it NRL had a much different atmosphere during the war with so many military people around and I guess quite a number of young people in the service here.

M Well, as I understand it, a lot of the young engineers [that] came in as civil servants were converted to chief petty officers and junior officers. They weren't very military. They were [just] in uniform. The school was not all that military either. I know we stood guard duty around the Laboratory periodically. I remember standing out on the end of the NRL dock with a clip board fastened to my belt with orders to "Shoot to Kill", but I was not armed (laughter). Something wrong there (more laughter)! We had to man a post at the rail line where it enters the Lab campus and where it leaves. There was [then] a railroad bridge across the [Potomac] river. Trains did go through here, and we had to watch very carefully when a train went through that no one got off. We had to apprehend anyone trying to enter the Lab.

A I take it very few people tried to....

M No, I never had an occasion to "shoot to kill" (laughter).

A Do you have an idea of how many people were in this school at the time you went through it, was it upwards of a 1,000 or less than that?

M You know, I don't have a feel for that. I would guess maybe 700 or 800, but I'm not sure. I think the school size was determined by the size of the barracks. There were two people in a room; they were small rooms. If your roommate was unfortunate enough to leave his bed unmade and the room was put on report, you both were put on report. I was on report all the time (laughter). I had a Marine roommate who was not very congenial. He was about twice my size and mean.

A So you didn't tell him to make his bed.

M No, I didn't take any correctivve action at all (laughter). But for extra duty, for being on report -- so many times on report -- you went to Captain's Mast and they had you work off extra duty. We used to scrub the floor of the cafeteria. To this day I'm uneasy walking in the [NRL] cafeteria. It's left a lasting impression. We called it "Duffy's Tavern" because Duffy, an old time Navy man, was the Chief Master at Arms. He probably was the most military man in all of NRL, and he wouldn't tolerate any breach of Navy etiquette by any person in his mess hall. He was the one who had to supervise our extra duty and you just never could please him. On several occasions, we had to scrub the mess hall floor with toothbrushes, -- toothbrushes! Can you imagine 30 or 40 guys on their hands and knees, with each one with a toothbrush, scrubbing the floor of the cafeteria? It can take hours, and it did. It took all day Saturday. You had to do it over and over and over again.

I was courting my wife in those days and found it very easy to escape from the Lab at night without a leave pass, but they were very expert at catching you coming back in. Very few succeeded in getting back without being apprehended. I had to debate whether I wanted to see my girlfriend during the week or weekends.

A Did you have much interaction with NRL researchers in your school?

M Only during our guard duty stands. I remember on several weekends during my tour here, I was assigned to the guard duty detail out in Blue Plains, and on those occasions, we saw the engineers that were working on the weekend. I think the guard duty detail there was for 12 hours. So, you got to see the place at night and also during the day. I remember Mr. [Jim] Trexler and Dick Libbey were two of the engineers I met then whom I later knew when I came back here after the war. There was very little interaction. They did not try to enlist my assistance....

A Or interest you?

M No. We just kept out of the way and watched for unauthorized personnel.

A How long of a period did you have here, going to the school?

M Well, I came in December 1943 and I left in December 1944. The school was nine months -- the primary school and secondary school. After secondary school, they selected the personnel for various duty. Some would go to ships, some would go to additional schooling. In my case, I was assigned to a "special projects" school. It was up in the penthouse of a building that was then located in the parking lot on the east side of the cafeteria building. It was way up in the roof, one big room. The lieutenant commander who welcomed us as the second group to go through the course, told us that we were a very select group of young men. Most of us [he said] would get the Navy Cross posthumously (chuckle)!

A Did you look for the door?

M No. I looked for the dictionary to be sure I knew what "posthumously" meant (laughter). My plans then, unbeknownst to my fiancee, changed from waiting to get married after the war to getting married as soon as we could, because I didn't think there was going to be any after the war. Our wedding took place the day the NRL chapel was commissioned. Chaplain Hemphill performed the ceremony there in the afternoon. Our wedding is the first

NRL entry in the chapel's log book. We had about two or three weeks together. Then I went to Ocracoke, North Carolina.

The first group through the Special Projects school had been deployed into the European area. Off the coast of Germany somewhere, the Germans dropped the first eighteen or so radio con- trolled bombs. The first group through the "Special Projects" school I was assigned to [had the job of decoying those bombs and they] successfully redirected all of them away from every target! So, the Germans were rather disillusioned about the success of this device.

A The allies had some advance knowledge of this?

M Yes, in fact the countermeasures group here at the Lab had, through British assistance, gotten the schematic and some of the hardware, and they developed a countermeasure for it, which the school was training us how to use.

A Before we get on to your assignment, I'd like to pick up a little bit about your education in the school. Was it an all day curriculum, concentrated studies as you had had in Chicago?

M Well, it was made up of three types of activity: labora- tory, lecture, and study. They exposed you, through hearing in the lectures. They worked through your ears to pound all this information into you and in the laboratory, through your eyes, and in study also through your eyes, and in lab through your hands as well. So, you were exposed to the same curriculum, really, in three different ways. It started very early in the morning, with exercise, calisthenics and things like that. There were three different times in the day you could do that. You could do that early in the morning, or at noon, or in the evening. You would be assigned to the morning exercise period for a month, then the lunchtime exercise period, and then the evening one. We did calisthenics out in what we called "Narcissus Stadium", kind of a grassy plot out among the tanks in the sewage disposal plant. It was not very conducive to appetite, exercise, or anything else. [But at least] it was kind of pituresque out there -- it was green and the flowers grew very well, and so on. The class activity started, again, at 5:30-6:00 in the morning. You marched everywhere you went.

A Was it principles of radio you were studying -- vacuum tube design, circuit configuration?

M In pre-radio you had all the principles of radio. They taught you how to build a radio, and you did that in lab. They exposed you to the fundamentals of electronics in the vacuum tube era. When you got into the next part of the curriculum and actually were studying sonar, you knew enough about the fundamentals so that you could tell this was an amplifier and that was an oscillator, and that was a power supply. You knew how they worked and, most importantly, when they didn't work, you knew how to fix them. That was the whole name of the game. It was to build your fundamental knowledge of electronics to the point where you could troubleshoot and repair equipment in the fleet.

A Did you have a lot of actual work with Navy equipment? Would they say, "Here's a problem, go and find out what it is and repair it."

M Yes, there was actual electronic gear, of same type used aboard ship, and they would, in a regularized manner, put troubles in it. The team would then go out and find them. The team was usually two or three students. You didn't usually do it alone. One guy had the instruction book open and he sort of led the thought process of how you go from here to there to isolate the problem. The other fellow had the test equipment. Then you changed places. I teamed up through the school with a 6'4'' farm boy from the Terre Haute area [of Indiana] and we seemed to work very well together. He could reach the high places in the gear and I could get down underneath it (laughter). We seemed to get along well and could find the problems readily and could fix them readily. The two of us were sent on to the Special Projects school as well together.

A How long did this special projects training last?

M Let's see. It was only about six weeks, I think. But they held us longer because the first team had experienced the first operational activity. They were trying to find out if there was going to be more of it or whether the

Germans had truly given up using the device. So, when our formal class experience was over, they graduated us and sent us to a beach jumping school down in Ocracoke, North Carolina. It was kind of a holding action.

A Now I wanted to ask, what kind of equipment were you trained to use?

M It was a transmitter and modulator that had been built here at the Laboratory. Again it was a maintenance as well as an operator function. They would put bugs in the equipment and we would have to find them. Not only that, there was some kind of a time sequence. You had to do things rather rapidly when the bomb was heading towards you. There was a very deliberate and careful sequence that you had to go through, and it had to be error free. This big tall guy from Indiana and I didn't have any problem at all, it was duck soup. We went to Okracoke and worked with the Marines out at Parris Island. They had PT boats where two of the motors had been removed to make room to install countermeasures equipment. We were running jamming and deception exercises against the shore mounted radar as the Parris Island Marines were practicing their landing operations. It seems that the surf, sand, and shoreline were similar to many of the islands in the south Pacific. They were training the Marines to get on the beach in spite of all the obstacles. Our counter-measures part of it was just to assist in avoiding the radar. Japanese simulated radar [was installed] on the beach.

A This was what year now?

M Late November, early December 1944.

A So the war with Germany was still going on.

M Oh, yes.

A But you were planning for operations in the Pacific?

M Evidently the plan of the Parris Island marines was that if the requirement of our services to redirect the bombs didn't materialize, then they would put us in a beach jumper support service.

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A Did you end up being deployed to the Pacific?

M It turned out that I got to spend Christmas Eve with my wife.

A In 1944.

M Then late on Christmas Eve, I had to get on a train and go to Treasure Island [California] to be shipped out on into the Pacific. I think I took the USS NEW MEXICO out of San Francisco harbor. [We encountered] enormous ground swells. I had an assignment topside, way up in the tower -- 80 to 100 feet above the water line -- and with those ground swells, that tower would roll 40 feet, back and forth. It was a very slow roll. After having survived all of that PT boat stuff down in Ocracoke, I didn't think I'd get seasick, but I did! I remember drinking coffee and getting so sick I was afraid I was going to live. To this day I don't like coffee (laughter). It was that bad.

But we did get out to Hawaii. From there we were attached to the Destroyer Squadron for the Northern Pacific. In our class there were either six or eight training teams established: an officer, a maintenance man and two countermeasure gear operators. They deployed two [teams] to the Northern Pacific, two to the Central Pacific, and two to the South Pacific and two somewhere else I believe. The ones in the South Pacific got clobbered, they were pretty heavily riddled with casualties in their operation. In the North Pacific it really didn't get too bad.

A So you were the single maintenance man in a four man team. Was this when you were moving from ship to ship?

M Yes. We were assigned temporary duty on each ship. Our officer had a list of the ships in the fleet that had countermeasures equipment installed on them. Now, a few of them had [the gear] in the bowels of the ship as ballast -- they hadn't bothered to hook it up. We would ride that ship, with nothing to do, until the next refueling date and then would swing via a "breeches buoy" highline from the ship we were on to the one delivering mail or delivering fuel. We would walk across the deck on that ship, if it happened to be a tanker, and go on a high line to the destroyer that was delivering mail and he would take us to the next ship on our list. This way, we would go from ship to ship. There were about 65 ships in a period of about 8 months [on which] we gave extensive training onboard by standing watch with the ship's company. I was able to tell my maintenance counterpart from each ship, how to repair, how to align, and how to make these countermeasures equipments work right. The two operators and I would then stand watch with each ship's operators instructing them on how to differentiate between an image and a real signal, how to determine the signal frequency, and how to tune the jammers.

The jammers were the biggest problem in those days, because they had a split anode glass magnetron, and they ran ethylene glychol for coolant. When you would tune these things up, quite often the ethylene glychol would escape and you would burn out a magnetron tube. Now, ethylene glychol is kind of slick. One drop of it covers about one acre. So, wherever that jammer was located was a treacherous slick area. We always had ethylene glychol around, and it was miserable stuff.

In the colder parts of our tour, when we would go up north, we would sleep around the electronic equipment because that was the only place you could get warm. We were only on the ship for 4 - 8 days, so they would say, "Well, you don't need a bunk, you're not going to be here long enough". We didn't even have a locker; we lived out of a seabag -- an honest to goodness seabag -- and slept on the floor most of the time. We didn't get any mail. That was the worst part of it. It was an intensive training tour. [That was how we] coordinated the countermeasures activity for that segment of the fleet to which we were assigned.

A So you were not working with the radio controlled bombs. Despite all that training, you ended up not doing that at all. Didn't the Japanese have radio controlled bombs?

M They didn't have bombs that were radio controlled that we knew about. When we went through Camp Catlin in Hawaii they did train us bit a more in the latest countermeasures gear. We'd had a small part of that in secondary school, and again in the Special Projects school, but there was nothing to do with radio controlled bombs in the Pacific at all. We were on "hold". If the Germans should start using them again, I suspect we would have been recalled very quickly.

A What type of countermeasures gear was standard on the ships?

M Well, they had several search receivers, one had tuning heads. I remember it had a "butterfly" capacitor in it. That was a picturesque term. It was a wide band search receiver and had plug-in tuning heads. You had one tuning head that would go (now this is from memory) from several hundred megacycles up several hundred more. They had panoramic adapters, a cathode ray tube device that displayed the frequency spectrum. As you tuned the receiver past a signal, the presence of the signal was indicated on the cathode ray tube as a signal moving across its face. It would move in one direction if it was an "Image", and the opposite direction if it were a "Real" signal. You had to be very careful that you got the right signal so that you could tune your jammer on the signal and not the image.

A Was it mostly communications jamming or radar jamming?

M All of it was radar. We didn't do any communications stuff at all. The frequency of the Japanese radars was 70 or 100 megacycles, and they had big wide pulses, as I remember. They had a very low pulse repetition frequency. Most of it was kind of crude. They had some airborne radar that operated on a higher frequency and indicated a little bit more development.

A What kind of reception did countermeasures gear receive by the ships? I take it must not have been too good if you found some of it stowed below.

M Well, that was the exception, really. I would think maybe one ship in twenty had it improperly installed or not installed at all.

A So it [was recognized] that this was useful equipment.

M They knew it was there for a good purpose. They were not sure how well it was going to be used. I think it wasn't until the team came on board that they began to get some comprehensive picture of what the program was all about.

For instance, when the fleet would go in to do a shore bombardment of the Japanese homeland, as they did three or four times (and we attended three of those), there was a very specific countermeasures routine that they would follow. They would assign the various jobs to the members of this armada. It involved jamming the search radars as you approached; it involved setting up decoys -- balloons and kites and things like that -- as you departed from the area. It was a fairly well thought-out activity and we played the part of teacher and messenger in carrying out that ECM policy, and on specific exerecises we were actual participants.

A Did you end up seeing action?

M Oh yes. I think the first action that I saw was on Easter Sunday at Okinawa when they started the bombardment. I remember showing up on deck, we were on the LEUTZE, a destroyer, and we were lined up for chow. Suddenly, over the PA system, there was a general quarters signal for a submarine attack. So everybody forgot about chow and went to their general quarters station. We zipped around the area and dropped depth charges, and a few hours later they painted a small figure of a submarine on the side of the Bridge on our ship. They had killed a small baby submarine of some kind. The Japanese had subs that were thirty to eighty feet long or two man subs or something. The LEUTZE got one on the first day. When we went back to chow we were a little closer to the shore. I remember seeing water spouts as shells were dropping in the water closer and closer -- not to our ship, but to a big cargo vessel that was anchored nearby, and heading directly for us. These water spouts were coming closer, and about every thirty or forty seconds another one would come in. The cargo ship proceeded full speed ahead without raising its anchor! Well, it came out to the end of its anchor chain and started to go in a circle, but it didn't get hit. It's a wonder it didn't collide with one of our own ships! It was quite an exciting morning.

The LEUTZE was sent on the next day to go back to a harbor in the south and escort up several of the other ships that had arrived too late for the initial day. We steamed back down there and then escorted [them]. Among the ships there were two battleships, one of them was the Arkansas -- I came to know it initmately because I had spent a good deal of time on the Arkansas. It was a ship that was very ancient. Everything was worn out except the main gun batteries. The main guns in this battleship were practically new and, because of those new guns, it was an excellent platform to sit out there a few miles off shore and lob shells at various targets on the beach. We would sit there, hour after hour, day after day. We ran out of everything but ammunition. Spam and rice was the main course. I remember the drinking water fountains. They had a tin cup hanging there. You would fill the cup with water and then blow the oil off the top of it in order to keep from getting dysentary, because the water and oil were mixed. It was a ship in its last months of use, but they did employ it extensively for shore bombardment. They had a bell that would go off just a few seconds before the main batteries would fire. Day and night that bell would go off. You would tense yourself and the concussion would hit you. You were tense instead of being relaxed, as you should have been. It was much more fatiguing and devastating mentally because of that blasted bell.

A So you did see quite a bit of action in the Pacific.

M The Okinawa campaign and the Japanese homeland bombardments in the latter part of the war were the extent of it.

A Then you came back to the United States and got out of the service as soon as possible?

M Well, it was a whole lot easier said than done. Coming back we had to get detached from many commands and many of the fleet elements that we had been assigned to. We had to go and find them and get our orders detaching us so we could get back. It took us two months to get out of the West Pacific and back in Hawaii. We should have been in no hurry. When we got back to Hawaii, they put us in school studying the same things that we'd been teaching, which was boring.

A So you had to stay in the service for some time after that?

M During the war, you accumulated points for the discharge system. The longer service you had, the more points you had. If you had a wife, that helped. I was doubly thankful that I was married, she was worth ten points. It was a rather boring time, but Hawaii could have been really nice if we had not been trying to get home. I have been to Hawaii five or six time since then but have never enjoyed it because of that stigma that remains.

A You were there for nine or ten months?

M Let's see. The bomb at Hiroshima was August 1945, I believe. We were out in the West Pacific in August, when the bombing took place. Then we started to get ourselves detached, and in two months we were back in Hawaii. I stayed in Hawaii until December. Then I went on a transport back to the States. I had to go to separation center in Toledo, Ohio to get detached finally. That was the last day in 1945, December 31st.

A Your wife was living in Washington when you came back here?

M Yes. She was with the telephone company, she had a very good job with them. She didn't come to Ohio to welcome me out of the service, I came to Washington. Then she took leave and we went on a six week honeymoon.

It was February then, and the newspapers in Washington had articles in the papers every day from George Washington University about their school, trying to interest people in college. So I remember this one Monday morning getting up and riding the streetcar down to GW and standing in line and seeing the "Closed" sign put up. It had been opened when I arrived, but the line didn't move fast enough for me to get served that morning. I waited in line all day long until it opened at four o'clock that afternoon. GW was oriented considerably towards day classes for the full time students and night classes for the civil servants of the area. When they opened their admissions office that evening, I was still waiting in in line. The line moved pretty fast then because it had been depleted by the less tenacious people who had left during the day. I really had no strong commitment to become an engineer [at that time].

A You just wanted to go to school, I guess.

M I just wanted to see what they had. I wanted to consider the alternatives. But the man that interviewed me was from the electrical engineering department, was very positive, and he knew just from my being there that I <u>must</u> want to be an electrical engineer. An hour later I found myself in my first class!

A The first class the same night?

M Same night, yes. Classes were already going on. This was late registration, so I was in a class of analytic geometry, registered as a student in an electrical engineering school with a specialty in electronics.

A Had you seriously had some doubt about going in that direction?

M I really didn't know. I thought that I could probably do better by going to school. My mother-in-law was very definite about it. My wife also encouraged me to go. Everything seemed to indicate that I should go to school.

A [But as to] your career options, the area that you wanted to go in, you were really uncommitted.

M I just hadn't decided. I wanted to consider the alternatives. If there was any consideration given, it was only a very few minutes.

A Then you were in class (laughter)!

M I was in line, and they were pushing. But I am a rather stubborn person, I guess. Once committed to do this, I went to school day and night, winter and summer. Except for the death of my father in my second year, I attended without a break. I would go to summer school and winter school. Anytime I could take a course that would help toward my degree, I would take it. I graduated in three years. They accepted credits from my radio material experience for amost of the electives.

A For the technical electives, but did you have humanities classes as well?

M No.

A You ended up basically doing all technical, engineering type of work.

M Well, they had English, literature and civics, economics and things like that. But it also was a very intense period. All the students were trying to double time to make up for years that they had lost in the service.

There were two activities that were not academic in which

I engaged. One was the Theta Tau engineering society, [and the other] was the IEEE, IRE it was called in those days. I was a student member and an officer in the IRE branch at GW.

A Did you like your studies or do you just remember them as something that you had to do?

M I remember them as something to get behind me. It was not a particularly happy period. My wife and I would pass each other on the bus every once in a while. She would be coming home and I would be going to school. It was a hectic affair, but we were both stubborn enough to hang in there. I graduated in 1949, and that was a very bleak period for people being hired. The economy was down and opportunities for getting a job were rather minor.

A Even for engineers?

M Yes. I intereviewed at the FAA, CAA, Interior, Commerce, Naval Ordnance Lab., place after place after place and I didn't even get a nibble.

A Was your record good?

M Well, I was probably in the middle of a class of about 200. Not a particularly good academic record, but not bad.

A It was certainly passable and reputable. It was just that the economy made it difficult.

M There just were very few opportunities. If you didn't already have a job, chances of getting one were pretty poor. I had, through a personal acquaintance, contacted Charlie Price, who works here today in my organization. We were at a party at their home, and I asked him if there was any chance of working at NRL. The answer was, "No way, they're still getting rid of World War II excess baggage." This was probably late 1948, so I didn't make arrangements for an NRL interview until the last resort. I'd failed all the other opportunies. Then I did call up and established six or seven interviews here with various divisions.

A Just by calling each individual division?

M No, I called Personnel and they set up a series of them by ascertaining what my specialties and interests were. The last interview was with the Countermeasures Branch, when in fact it should have been the first one in the whole parade.

A I take it the others hadn't amounted to much?

M No. Again, they had more people than they had need of and they were not looking for new recruits. If I had been in the top dozen in a class, they might have hired me, but I wasn't that high academically. But when the countermeasures people found out that I had experience in the fleet with the training team on the equipment they had designed during the war, they very quickly brought in their Branch Head, Mr. Lorenzen, to carry on with the interview. He interrogated me extensively about the specifics of the equipment. It became a debriefing of me and my fleet experience, much more than an interview! It was kind of exciting to me at the time to find such interest. They suggested that would like to have me, but they didn't have a ceiling point. "Could I make myself available for a few days?" They thought they had someone that was leaving the division, and I think four days later, on the 15th of February, [redacted], who was Mr. Gebhart's administrative officer, left the Laboratory and went to work for the Office of Naval Intelligence and I got his ceiling point. [redacted] was one of the continuing participants in our program over the last 25 years. So we have had an interesting parallel.

A Had you met him before you started here?

M No, I hadn't met him at all. In fact, I didn't know him until several years later, but I remembered that it was [redacted] departure and his ceiling point that permitted my entry into NRL.

A It was his warm chair that your were occupying (laughter). How do you remember Lorenzen? Did you have a first impression that is worth noting here? Did he seem knowledgeable and a leader, someone that you would like to work for?

M Oh, yes! A very positive person. Well, he was of German extraction. He didn't eqivocate. Everything he said and did was very positive. He was an inspiring guy and quite tall. He could yell real loud. Dynamic, forceful -- you were kind of in awe of him. He was well known, he moved in important circles. He had a team that was dedicated to him. They weren't terrified of him, but they were very much in awe of him.

In the first several years I had little association with Mr. Lorenzen. I had more to do with people like Jim Trexler and Ray Gleason. Gleason, who is still here at the Lab., was my supervisor for a while . It was an interesting group to get into. There were a lot of things going on and there was a lot of freedom too.

A So you came, you say, in February 1949. Your first assignment was to do what? Under whom did you work?

M Trexler was my first supervisor. Ray Gleason worked for him and I worked for Ray Gleason. So, I was in the unit with Ray Gleason. The first significant activity I recall of was working on applying the German World War II concept of the crystal video receiver to our fleet. Crystal video, I guess I should say, is a receiving system that detects the RF energy immediately, right after the receiving antenna. Then, the rectangular wave of the detected pulse is amplified in a video amplifier, which has a relatively narrow band width and high gain. It gives the advantage of a very simple, low power, small receiver that is quite reliable. It's a receiver that's called a "wide open" receiver. It looks at all frequencies that are capable of being intercepted by the antenna, or by the band pass filters that are immediately following the antenna. The Germans had used this technique by equiping the lookouts on some of their submarines with something they called the ATHOS system. It was a crystal video receiver with an antenna mounted to the helmet of the lookout. As the lookout put the binoculars to his eyes and scanned the horizon, his head would turn and he would scan the antenna in the same manner that he directed his binoculars.

A We are showing a picture of this system as Figure 1 for this interview. Is this a piece of captured equipment that came to NRL?

M No. This particular picture shows what we simulated from the concept that the Germans had used on their submarines. This is a U.S. built set, one that I built here at the Laboratory. It's called an AN/ULR-3 (XB-1). That means its built at NRL, and it is a crystal video receiver. It has a gain control, a head phone output, and an RF input. You connect the RF from the amplifier box to either of two antennas that are mounted on the helmet. This particular helmet is an armored steel, talker's helmet. It is large enough in size so the operator could wear headphones inside the helmet and the helmet continued down over the back of your head. I remember that the machinists here at the Lab had a great deal of difficulty in penetrating that helmet in order to mount the two bolts to fix the antennas to it outside. They burned up drill after drill because

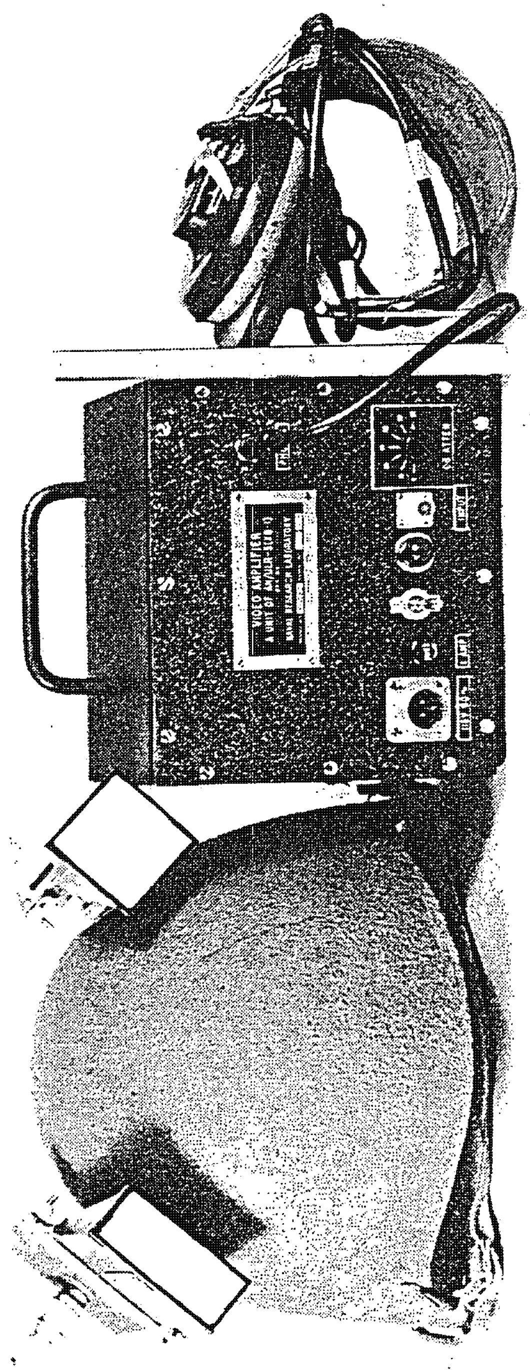

Figure 1

that steel is very, very tough. I had no idea it was that hard. So this is a picture of the U.S. equivalent of the concept of the ATHOS system.

A Do you build this just for experimental purposes or was it planned to make a U.S. system of almost an identical design to the German system?

M Well, the idea was to exploit that concept in our own fleet, either for surface ships for for submarines. This equipment never went aboard a submarine. We used it down at Chesapeake Bay mounted on a small landing craft that left the dock near North Beach (the Rod and Reel Club -- there's a Navy dock there) -- we would leave there and go out into the Bay and then depart towards Baltimore up the Bay towards Tilghman Island, and try to see how far we could go and still receive the radars that were working with us cooperatively out of CBD [Chesapeake Bay Division]. We could get ranges of 18-2Ø miles, something like that, with this thing mounted eight or nine feet from the surface of the Bay.

A Your part in this was to actually build the equipment?

M I built the box, the electronics in it, and then took it aboard this little landing craft and wrote a report on how far you could see the signals.

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M Ray Gleason was my immediate supervisor. He would give me fairly detailed instructions, and then when I'd get in trouble he would help me out. One of the best texts that was available at the time, was the 1948 Radiation Laboratory Series. It was about two years old, and there were all kinds of information in those 20 or 30 volumes about the crystal video receiving [techniques]. There were things on how to build high gain amplifiers, how to avoid microphonics in them -- all kinds of hints. Figure 1 shows one of the first systems that we built.

A Do you remember how long it took from getting this assignment to actually developing this equipment?

M I would guess we probably had the system by 1951. [It took about two years].

A So, it was a full time project for you.

M Yes, pretty much. There were several other things going on that I was helping with . Ray Gleason had his full assignment and occasionally I would be asked to assist him in some area where he needed another pair of hands.

A Was this also radio countermeasures?

M Yes. He was doing direction finding. That was the section we were in, direction finding. In another unit, they had done a submarine direction finder, a spinning antenna superheterodyne type. What I recall most vividly about that is that the rotating joint on the antenna had a mercury-wetted platinum contact. It worked fine as long as it didn't tip too much, but if you tilted it too much the mercury would all go to one side, it would short out, and the mercury would get all over the place! Now in those days we weren't worried very much about environmental protection, and the mercury that was contaminated was flushed down the toilet! You would just get rid of it. You could not even think of doing that today.

There were several projects going on. Mr. Trexler had other ones and occasionally we would be tasked to do another job. I know they were doing a direction finding antenna for the Martin Mariner aircraft, (I can't think what the alphabetic notation is for it). I built a little test device for that. They had to inject a test signal into it and I built an oscillator in the 50-80 megacycle region. It was calibrated across the frequency band and had a calibrated attenuator. It was a fairly demanding task in that they didn't want it to be very large. The smaller the better. [redacted], one of the engineers I mentioned having met years before at the NRL Blue Plains site, was the engineer in charge of that task and I helped him build the test device for it.

- P7-6 - Jet seaplane Seamaster? CO2 LCO2 Stufflebeam NATC

A So you ended up having a few odd assignments but this become the major principal focus of your activities.

M I would say my principal activity was crystal video. The other direction finding activity was not that -- it was antennas, or superheterodyne receivers.

In the branch they had signal analysis activity. Down the hall, a big group was working on 5-gun cathode ray tube analyzers for showing the various parameters of radar signals that were intercepted. The activities were functionally distributed in the branch. I worked on the second floor of Bldg. 56 on the south end of the building. I had a room to myself and quite a large degree of freedom.

Another task of this same type that I had was building a high gain receiver that did not use the helmet. It was crystal video also, but had a separate mounted antenna that you would put aboard a ship. This was one of the first amplifiers that I built. It was right out of Radiation Laboratory Series. The thing was so microphonic that if you would just thump it with your finger, it would overpower everything with the microphonics! So, the first couple of stages we had to cover with a shield of lead, and then we had to mount it on a shock mounted base so that the thing was isolated from external vibration. In order to to assure ourselves that this was going to be suitable for working in that landing craft on the Chesapeake Bay, I got a small gasoline powered 115 volt power source, mounted it in the back of a jeep, put this amplifier on the back seat, and drove up the railroad tracks here the Laboratory -- not on the rails, on the wooden ties!

A You actually had it operating?

M Yes, I had it operating and when I could get the microphonics down to the point that it would survive this trip up and down the railroad ties, then I knew it was ready to be taken down to Chesapeake Bay and tried on the ship. When we took it down there, it worked the first time. Outside of the greatest impact, when the landing craft would rise up on a steep wave and then slam back down on the valley between the waves -- except for those rare occasions -- it didn't give us any problems at all.

A Was that equipment done about the same time as this was?

M What is shown in Figure 1 actually followed that and was a smaller version. The one that was out of Radiation Laboratory was about 18 inches long and maybe 12 inches high and 12 inches wide. It was a big, heavy, brute force amplifier with a very high voltage gain. It used the large vacuum tubes. Figure 1 represents a slightly more advanced amplifier design, where we were striving for small size, light weight, and low power consumption. I guess the step between the first one from the Radiation Laboratory series and this one was a series of emperical chassis that we built up. This Figure 1 used a twin triode, what we called a cascode amplifier -- cathode coupled twin triodes. Very reliable and very low power.

A It was for picking up radar signals only?

M Yes. Radar only. A waveguide type of antenna was used.

A That's all microwaves?

M All microwaves. The antenna on the left is an X-Band, up to 10-12,000 megacycles, and the one on the right is S-Band. It would go down maybe as low as 2,000 megacycles. 2000 to 12,000 perhaps, thus it covered in two bands, the most common radar frequency bands of the day.

A Did you say it never became operational?

M No it was too clumsy. It seemed like the submarine service was getting snorkels and they were able to charge their batteries without fully surfacing and having a lookout stationed up on the surface. The opportunities of using this in the submarine service were considerably less after World War II.

We went on to exploit a wide variety of other antenna opportunities. We built a four channel antenna system that had one antenna that looked forward, another one to the right, another one to the aft, and another one to the left. Each one of these quadrants was connected through its own amplifier to a deflection plate of a cathode ray tube. So, you had what we called a four channel system. For each pulse that was received, the spot on the center on the cathode ray tube would be driven toward the perimeter in the direction from which the signal emanated. If you had ten pulses, you had 10 strobes written, one on top of the other. If you had one pulse, there would be one strobe written, even though the brightness of the cathode ray tube was probably insufficient to show a single pulse. You needed several to paint a discernible pattern on the face of the tube. As a radar would sweep by this antenna you would get a "beep" of 30-40-50-60 pulses, and that would be sufficient to paint this pattern. The strobe would point the direction of signal arrival. So that was the wide-open, crystal video, four channel direction finding system, AN/ALD-A.

A Your work on this system began 1952 or 1953?

M Oh, the mid-fifties I would think. It was going on, this was Mr. John Gruber's project. It seems that my career is punctuated with people who are over six foot six, and he was another one, being over six foot eight. A very tall, very bright, personable young man and very dedicated. I remember on one occasion we had the four channel antenna below the belly of the DC3 aircraft (Figure 2) down at Patuxent. We had a new pilot on one of the flights. We went down there in the morning and got the plane ready to take off and were speeding down the runway. All of a sudden one of the engines quit. It was a two engine aircraft, and we were two or three feet off the ground when that engine quit. [The pilot] made a hard left hand turn. The wing barely missed the runway, and he set it down going about 30 degrees off from the direction of the runway in the pasture. The first comment that [Gruber] made was, "It's a good thing we were off the ground or we might have lost our antenna!" (Laughter). I wasn't

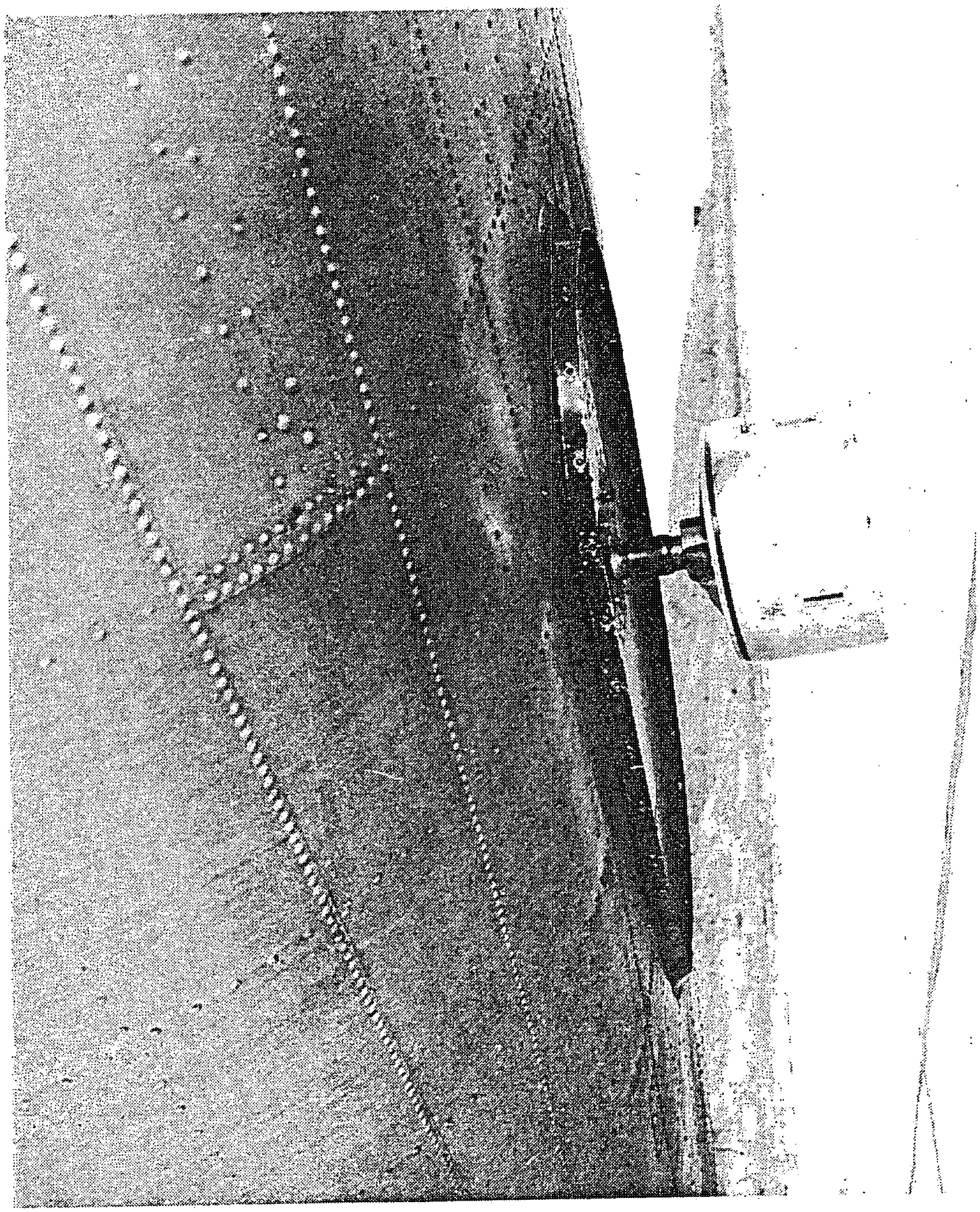

Figure 2

thinking at all about the antenna. This young man was really dedicated (more laughter)! He made a lasting impression on me.

Figure 3 shows the interior view of the antenna with the crystal detectors built near the back end of each of the four waveguide assemblies. Below the wavaeguide antenna is a dipole sticking out. The dipole is for vertical polarized reception and the waveguide is for horizontal polarized reception. In this single antenna system, we could pick up either polarization in the band from about 2000 to 5000 megacycles.

A You had designed the crystal video components of this?

M I had little to do with the design of this. This was Mr. Gruber's project and he was responsible for the whole design. But again it was aimed at exploiting the opportunities of doing different types of antenna jobs with the crystal video receiver.

A You had been moved then from your other project to this one when that one was washed out?

M Pretty much, yes. I was assigned to Mr. Gruber's unit and the thing he was working on then was this four channel system. We did mount the four channel system on a DC3, as I mentioned, and on an AD1 "Sky Raider", I think it was called -- it was the single engine weight lifting champ -- a single engine, propeller aircraft. We flew our four — later renamed A-1

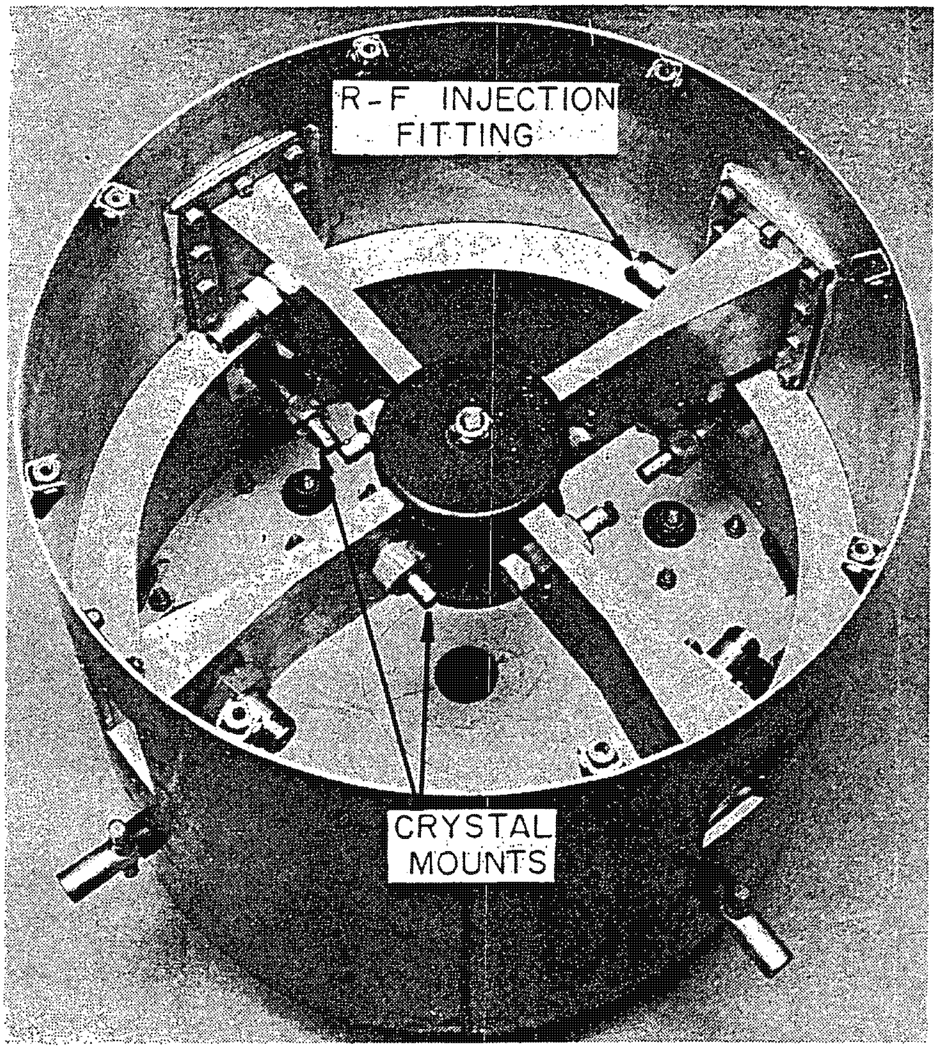

Figure 3

channel system onboard that, and we had it on a P2V, a patrol aircraft. On one of these, I think it was the DC3, we actually did go out into the the air identification zones (ADZ) in the Atlantic where a submarine was operating. The sub had a routine of coming up every ten or fifteen minutes and rotating its radar antenna three times and then turning the radar off. We were supposed to locate the sub, which we did with ease. No problem at all. The only problem was that the airplane overheated. One of the motors overheated when we were about 100 miles off the coast. We had to limp back to land before the plane ditched. It was a successful, trip but a bit exciting.

A You of course, worried about the antenna (laughter)!

M Not a bit! I was worrying about the absence of life preservers.

We took a single antenna and mounted it on the back of an aircraft, on the rudder structure so that we could detect planes coming in on the tail. This was called a tail-warning system. We built up a system that would have dispersed antennas on the wing tips, nose, and tail as opposed to this Figure 3, which had nested antennas. The nested antenna was a great problem in high speed aircraft because you didn't have any place you could locate a big antenna like this. But there were several places where you could put a small (aperture) window in the skin. We capitalized on those design opportunities and had several systems of the dispersed type. I think two of them actually went into production, the tail one and the dispersed antenna type system. The four channel system also went into production and Mr. Gruber went to work for the company that got the contract -- AVCO in Cincinnati. They built [the equipment] for the ASW search planes. The Canadians were doing much the same thing up in Canada. Mr. Gruber had worked with them extensively.

One of the worst problems we had was quality of crystals. The art of building exceedingly high-sensitivity detector crystals was coming along very slowly. We found good crystals by just wading through hundreds of samples until we found some that had the properties that we wanted. All of them looked and were marked exactly alike, but their properties weren't the same. We went up to the manufacturers in New England who assembled the crystals and watched how they made them. We could see then that [problems arose with] the cat-whisker inside the crystal, which penetrated the oxide coating to make contact with the crystal itself. The crystal was mounted on an anvil which was hammered with a hammer (about a one-half pound hammer). Now, the person doing this was watching the cat-whisker through some kind of a shadow-graph optical system and was also seeing a presentation of the noise and other characteristics of the crystal as he was hammering it! Here was a device that was so fragile you couldn't drop it on the floor for fear it would change its characteristics, being manufacturered with this abuse! It just didn't make sense at all, but that's the way they were doing it. It meant that the reproducibility of characteristics from sample to sample was very very poor. You just had to get a lot of them and find the good ones.

A Did you continue to work on this system?

M We continued experimental work on the antennas for the four channel direction finder. We also worked on the tail warning system. The Bureau [of Aeronautics] had funded this project and we wrote regular progress reports. now NAVAIR SYS COM

A Did you go from this then to submarine detection?

M Yes. In the fall of 1957, Adm Grenfeld, I believe it was, came in from the Pacific, and it is my recollection that his comments were something like: He had written his last blankety-blank memo. He wanted hardware and he wanted it right now! He had priority and money and everything else to get it, but what he wanted was somebody with a "can do" attitude. Well, he came to Mr. Lorenzen. I remember being called into the conference room, along with Vince Rose. We listened to what he wanted and went back to see what we could come up with. Later on in the afternoon, after they had gone to lunch, they came to our lab and we showed them a Type R-467 amplifier and some antennas, various types, and he said, "Yes, the amplifier would do.", but the antennas wouldn't work, they were too large and grotesque. He needed something that was like an earring, some small antenna.

A Was the R467 amplifier just a stock amplifier?

M It was kind of a stock amplifier we had produced in the laboratory. The electronic model shop had built a dozen of them and one of their electronic manufacturing contractors in the area of Maryland had also built another couple of dozen. They were deployed into the Navy. They were used with various types of antennas, for various single channel crystal video systems. This submarine crystal video system was described in an NRL report No. 5155, dated June 13, 1958. [That [was written] several months after we had installed the ~~eight~~ three of them that we built. The first one was put on the DOGFISH in New London [Conn.]]

The technique that we used was to remove the radar antenna waveguide from inside the periscope and the radar antenna itself from inside the persicope. In its place, we put in a small spiral antenna that covered from about 1000 to 12,000 megacycles, and two coaxial lines -- one that went to this ELINT spiral antenna and another that went out of the top of the periscope to a small sleeve dipole that was mounted vertically on the back of the periscope. (See Figure 4). The sleeve dipole was an antenna that covered the other range down into 100 kilocycles or lower. They coupled several multicouplers and receivers to that whip or monopole, and at the same time, the R467 was coupled to the spiral antenna through a crystal video detector.

The DOGFISH was exceedingly successful in its deployment. It worked, unbeknownst to the Soviet fleet, for several hours in a snow storm and we learned a tremendous amount

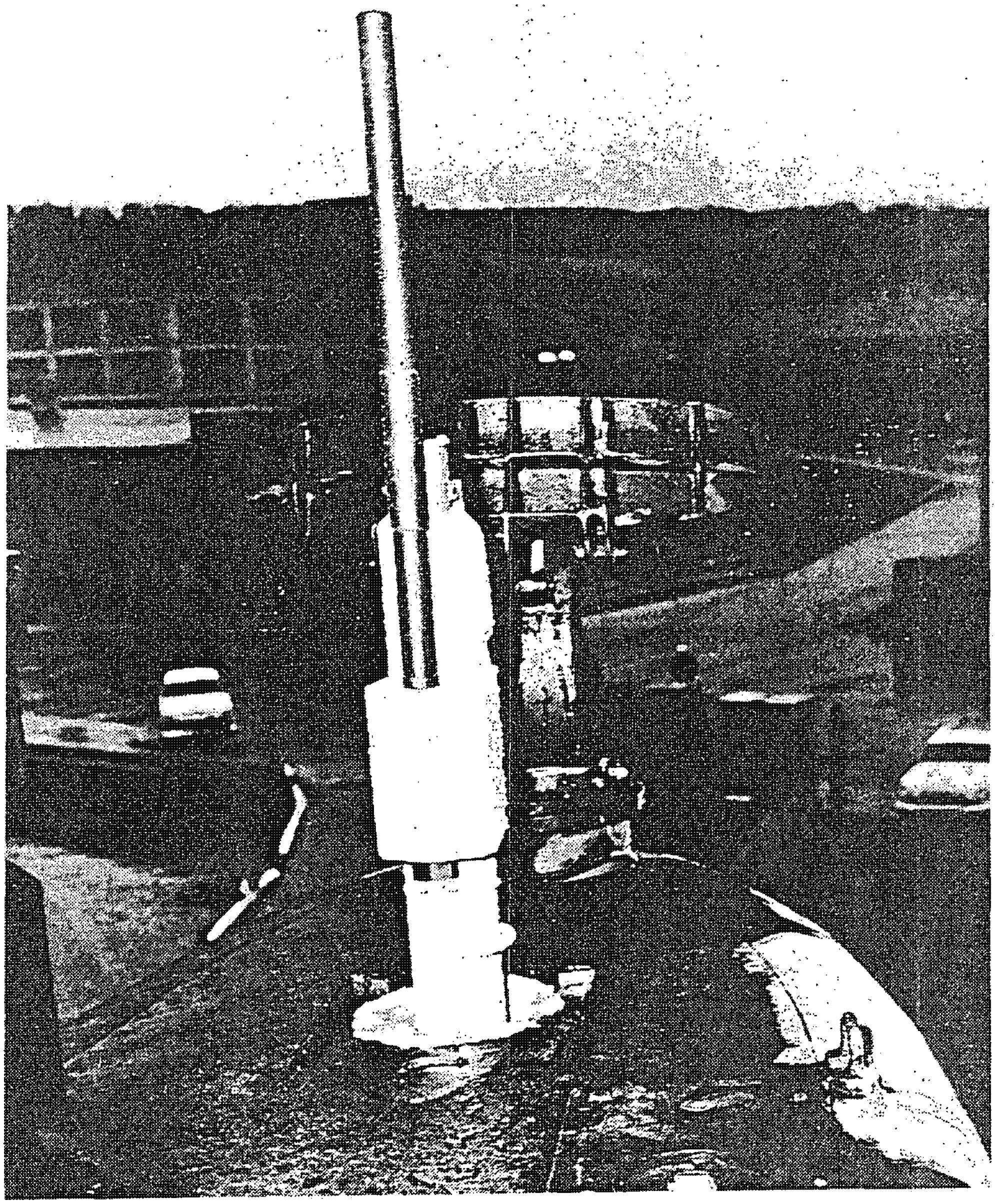

Figure 4

about the way the Soviets refuel [and about other] operations. Toward the end of their sojurn with the fleet, some member of the Soviet fleet asked when they went into operation with the submarine. There was a linguist on the DOGFISH and of course he could understand what was being said. The man in charge of the Soviet fleet exercise said, "There was no sub operation, there's no submarine! Where?" Well, that was the last they saw the DOGFISH (laughter). It proceeded to come back home very quickly. The skipper was so pleased with the operation, it was such a tremendous coup, that he came back to the Lab and thanked us, first of all, for saving their lives, for having given the ability to hear this threat and to avoid it. Then he gave us a lot of constructive criticism -- how we could do the job better, and what kind of problems they had with it. We took those criticisms and improved the system. The second [system] went into the WAHOO out of [redacted].

When I was out there putting that installation aboard the WAHOO, my father-in-law passed away,* so I came back to Washington, got my family, and went to Michigan for the funeral. I was coming back from Michigan, in the latter part of March, I think it was the 28th, [1958] and they had a terrible snowstorm....

A I know where you are going but I want to stop for a minute and pick up a little bit more on this submarine equipment. Now, this equipment was built in a crisis situation because the Admiral that had come back knew that the Soviet exercise was going to take place.

* 12/16/97: Reid checked with his sister-in-law. Father-in-law passed away in 1955. Occasion of this visit was mother-in-law's stroke on 2/22/58. She ~~Hd~~ died in July 1958. Visit to Grand Rapids, Mich. was mid-March, they stayed with sister-in-law's family 4-5 days.

M Affirmative. Yes.

A It was a joint communications/radar intercept equipment. You had been responsible for the radar intercept equipment, which was a crystal video system and basically an adaptation of what you had been using on the aircraft.

M Exactly. Yes. In looking on page 29 of the report, it shows the communications intercept system went from 15 KC to 6ØØ KC in one receiver, and from 5ØØ KC to 32 megacycles on another. Then from 54 megacycles to 265 on a third receiver. The ELINT (radar intercept) system covered from 1ØØØ to 12,ØØØ. That development continued. The submarine desk of the Bureau of Ships took the development and perfected the system in a number of ways. They went to a transistorized amplifier located in the base of the periscopes, so you didn't have to disconnect the amplifier from the periscope every time it was raised. That simplified things considerably. Kolmorgen Optical Company, who manufactures many of the periscopes, got the contract to do this development. We were consultants on it for a number of years and watched it proceed in a fairly orderly way. When we first did it, it was called an 8A periscope; when the Bureau of Ships did it, it was an 8B, I believe. They moved that periscope physically from one sub to another. There were only a few periscopes and they would share the periscope when they went out on an operational deployment. Now Naval Sea Systems CMD

A So this, certainly, was one of the most important developments to come out of your crystal video work, in some ways more important that what you did on the aircraft.

M By far it was, yes. It allowed the surveillance type submarine to have not only a small optical device above the waves, but an ELINT and COMINT device up there too without a great big snorkel or something like that showing.

A Was the COMINT intercept also wideband?

M Yes. It was considerably wider than was normal.

A Was that also a crystal video type?

M Negative. Crystal video works only on pulsed signals. They were standard communications type search receivers. They had their operators manning each of the three positions at the COMINT terminal and another operator would man the position for the ELINT system. He could employ S-band, X-band or both. The ELINT was directive, it looked in the direction that the optics did, through the same window the optics viewed the world. The communications intercept system was pretty much omni-directional.

A Let's skip then back to the middle of a snowstorm. ? where from?

M We were coming back from the funeral in Michigan [March

1958] and were stranded in a restaurant in the mountains of Pennsylvania, snowbound, with traffic stopped on the Pennsylvania Turnpipke. Several hundred of us got in this restaurant to spend the night until the snow plows got through. I remember turning the paper placemat upside down and doing some range calculations to see how far we could elevate the periscope and still receive fairly powerful radars, say a TOKEN radar which had a megawatt power. I estimated the gain of the TOKEN antenna, and I calculated that you should be able to receive that kind of a signal from an elevation of something over 6ØØ miles. If you could put this same kind of an ELINT receiving system in a spacecraft, you could intercept the radar at any height below about 67Ø miles when illuminated by the radar.

A What made you think about spacecraft?

M Well, before Adm Grenfeld had talked to us, Sputnik had already happened. That was late 1957 and I had actually participated in some of the direction finding activity with the Wullenweber over at the Hybla Valley Coast Guard Station. I stood watches over there and did intercept work on the Sputnik.

A You might say for the record that the system was one of the first in the United States that was able to pick up the Sputnik in its regular passes over the United States.

M Sputnik radiated on a frequency of around 20 megacycles, and with that direction finding system, we could receive the voice of Sputnik two-thirds the way around the world. You could hear the signal over the Pole on the other side of the earth when it was in the northern latitudes. We were providing these bearings to NASA. They were unhappy with us because the direction of the signal was going from [east to west]. It's only supposed to go from the west towards the east, but if you view it over the Pole, you get the reciprocal. It looks like it is going backwards. They discounted our effort largely because we were seeing it going in the wrong direction and they knew it wouldn't go that way. Moreover, they couldn't believe we were receiving the signal over that long range.

Since Sputnik had happened, ADM Arleigh Burke, Navy CNO, had encouraged the electronic designers of the Navy to consider doing some functions from space. It was largely the thought of that challenge from CNO that suggested to me that maybe this submarine ELINT system would work in space.

A So, how did you hear of ADM Burke's comments?

M There was talk about it but presumably there was a memo inviting the Navy inventors to look towards space. I don't remember seeing the memo.

Side 4 June 4, 1981

A Was there much talk of space activities?

M Yes, it was a very exciting time when Sputnik first went. Everyone was excited by it. The race had been on, but we didn't even know that the Russians had been competing, -- at least at my level! I think Mr. Trexeler knew, and some of the other ones perhaps, but Sputnik completely surprised me personally, and I think a large part of our community. I remember my introduction to celestial orbital mechanics was trying to figure out, when we lost the Sputnik signal with the Wollenweber, where we were going to have to train our antenna to find it the next time it appeared. What we used was the large electric incandescent bulb from a yard light in the building over there in Hybla Valley Coast Guard Station. It was maybe 6 inches in diameter with a neck on it. I remember climbing a pole and unscrewing and bringing it down very carefully, and then sketching on it, with grease pencil, the continents -- Australia, North America, Africa, South America -- and then taking a piece of solder to represent an orbit and rotating the bulb inside this ring of solder. When we'd lose it here, and then we would say, "OK, in twelve hours, it would turn this far, and in three hours it will turn this far, and in an hour and on half, it will turn this far." [That is how we got] an estimate of where we should search during the next one half hour when we were expecting the signal to reappear. We didn't have predicts and without them, since the antenna was so very very directive, it was quite hard to find the signal unless you knew precisely where to aim your antenna and let it sit there until a signal came on.

? predictions

A You didn't even buy a globe (chuckle)?

M Well, we did! However, this was the first night, when we were called over there right off the bat, when it was first up. We didn't bring a globe with us. In fact, I had an inflatable globe in my office, which we used later on. A balloon type globe. We made a brass ring, like an orbit, with an offset at the equator that would step over the amount that the earth had moved during the time of one orbit.

Yes, it was an exciting time and the talk about space was everywhere and everyone was trying to get up to speed. It was a very important event in the fall of 1957.

A Had you had much connection with the ongoing VANGUARD project here at NRL prior to that time or was it really the advent of the Russians in space that excited

M The advent of the Russians entirely. Now VANGUARD was going on but as I recall when the Russians flew, VANGUARD hadn't even attempted a flight yet. It was somewhat later when that embarrassment happened -- the explosions of the the rocket and so on. I was excited by the Russian Sputnik success and having a part of watching that thing with the Wollenweber. Yes, I was anxious to think of how we could do other things from space.

A It was really your experience and your being in the intelligence community already, [your thinking about] the Russians and their capabilities that was really the motivating factor -- is that right?

M That's largely it. I think there may have been one other factor. We were handicapped with our crystal video work aboard ship trying to maximize intercept range in spite of the curvature of the earth. We got some relief when we could put this crystal video set in an aircraft and see, not 18 miles, but several hundred miles. That's a big advantage and if we could get our receiving systems up higher than aircraft, we could see further. So, the opportunity of a wide open system being useful to look into the heartland of the Soviet Union was pretty obvious, if you could get it up high enough.

A What led you, while you were on a trip - a vacation - to think about work? Were you with your family when you were doing this?

M My family was asleep around the table. Two of the kids were on the floor, my wife was sitting at the table with her head on the table. I was sort of watching her purse and, you know, ...

A Sketching satellites (laughter)!

M No, I just did some range calculations -- that was all it was. These indicated that, yes, it would be possible if we could get that periscope 100 miles, 200 miles, 300 miles, 400, 500, 600, 700... no, 700 miles [was too high].

A The gain wasn't high enough.

M The sensitivity was not adequate to get the TOKEN radar with the values of antenna gain and radiated power figures that I assumed. Now, when I came back to Washington several days later, I brought the placemat and gave it to Mr. Lorenzen. I think that might have given rise to rumors that we've heard on some occasions that the engineers at the Lab had been noted to do very good work on the back of placemats (laughter). Not quite as formal as we could be, but innovative, nevertheless.

A What was Lorenzen's response? Do you remember?

M Not precisely, except that in the weeks that followed, Mr. Trexler, Bruce Wald, and Lorenzen were busy conversing and trying to figure out a sales campaign -- how one would establish a program in this arena. The part that I played was to help with the graphics. I worked with the Technical Information Division (TID) in getting the illustrations and the sales pitch together.

A You were ready when you came back to sell your Division on doing a satellite program even though you'd had no experience in this?

M Well, I had confidence that you could intercept the radar signal from orbit. I really didn't have the perspective that would say, "Yes, it will sell and it will go." I just gave it to Mr. Lorenzen who drew the conclusion,"Yes, it's time, VANGUARD's going, there's a move afoot to move VANGUARD out of the Navy, maybe there will be room here at the Lab to do that job." I think all of these things probably jelled in his very active, fruitful mind. I didn't play much of a part in the sales of it at all. I was too much of a junior engineer.

A Lorenzen was responsible for seeing the programmatic aspects of developing this idea.

M Now, there could have been other participants. In reviewing the records, I see Dr. Wald's name and Mr. Trexler's name regularly in the briefing team. How they interacted and what part they played and so on I don't know.

A Did you have any sense of whether this same or a similar idea had already been considered? Was Lorenzen's response such that it looked like a brand new idea?

M I have no recollection and the record doesn't show anything preceeding it [except the Air Force programs for reconnaissance].

A In any case, once he saw the idea and became convinced of it, he began going around and selling it throughout the Navy through a series of briefings. He put together, I take it, a package to do that. Do you have a sense of what he said in those briefings?

M Yes. He used a packaged brief that was put together by the TID group here at the Lab. Nancy Monacelli was a very junior member of that in those days. I worked with them in getting the ideas across -- what the bird would look like. Figure 5 the artist's estimate of what the spacecraft would look like, using the technology of VANGUARD. In fact, if you look at that, the same internal construction is proposed for this spacecraft that was being used in the VANGUARD satellite.

A Before you get into too much detail -- was working out the ideas for this proposal primarily your responsibility? Who came up with the whole systematic thinking behind this plan?

M Lorenzen, as far as I know.

A So you and Lorenzen were in close contact on this and you worked with the artists?

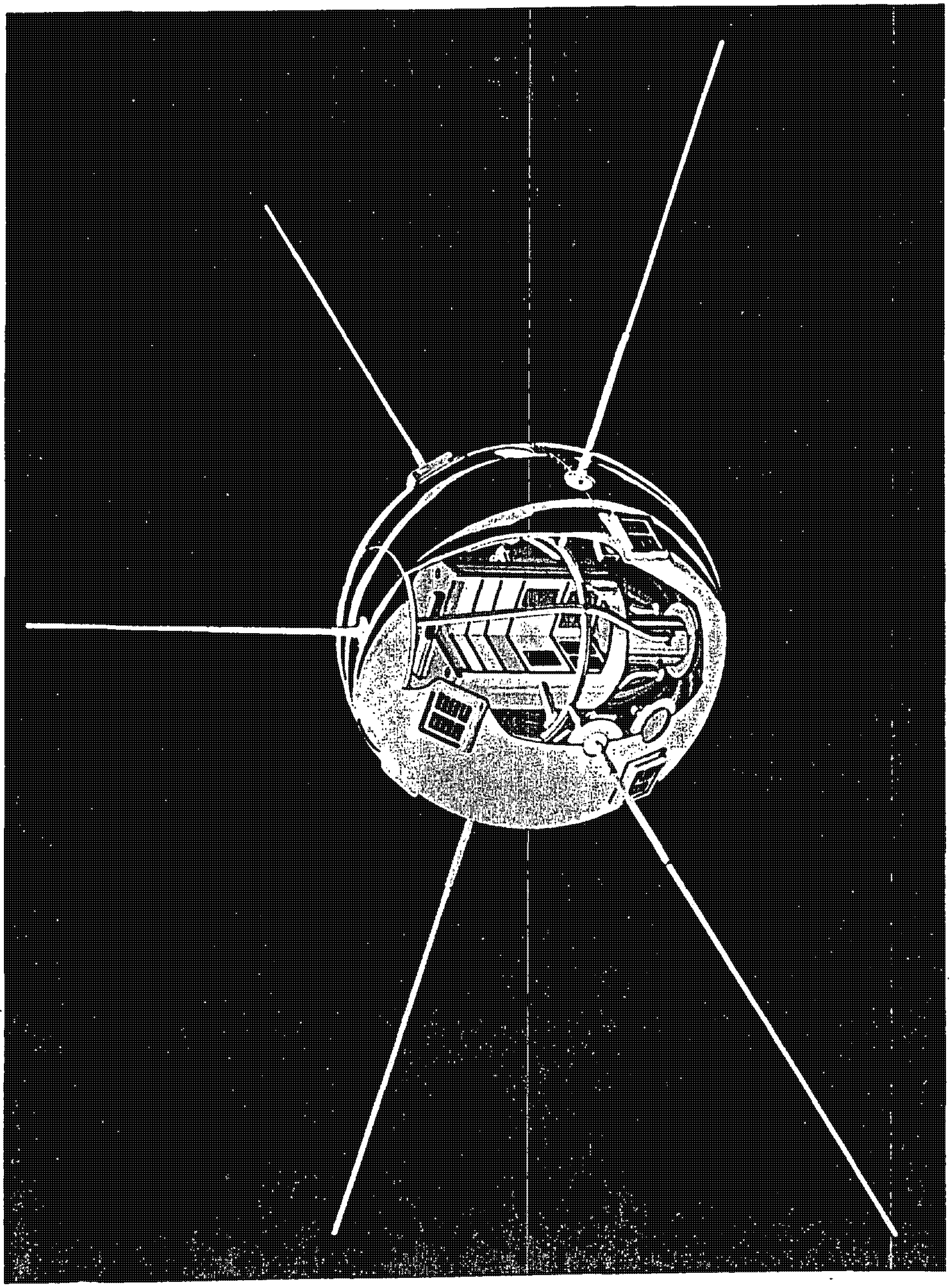

Figure 5

M He would task me in getting a graphic made to support an idea. "How do you intercept a radar? What does it look like above the earth? How do you show the horizon from a collection site? How wide a swath can the spacecraft see from orbital altitude?" We had to do the calculations that showed the range. Nancy and her boss and I worked in getting proposals back to Howard. When we had an idea that seemed to be accurate and adequate, we would go ahead and have TID render it in a 30 x 40 illustration on a cardboard base. I remember that 10 or 12 of these 30 x 40 cards were contained in a canvas bag. My job was to carry the heavy end of the canvas bag (laughter) and try to keep up with all those fellows with the long legs as we ran through halls of the Pentagon and other places with the briefing. We also had them rendered in 35 mm slides, which we still have today. Mr. Lorenzen and I, after the program was approved, were required to go to all the overseas collection sites with that 35 mm slide briefing and indoctrinate the operations personnel at the site.

A Let's get back to the details now. We've mentioned that Figure 5 is the concept that you came up with for this briefing package of what the satellite would look like. The idea was to take the VANGUARD technology that had been developed here at NRL and adapt it to this new use. Do you want to describe what's shown in the cutaway?

M Yes. We show little solar cells on the exterior of the 20 inch diameter sphere. You will note that the solar cells are very very small, they are more like the size that was used in the little 6 diameter inch VANGUARD satellite. We had no concept of how large a solar cell was required, having never experienced any before. It turned out that these were not scale models at all. The solar cells used in our satellites were each 9 inch circular patches , and six were mounted on the sphere (three on the top and three on the bottom hemisphere). We show a turnstyle antenna around the equator of the sphere, which we proposed to use as the transmitting antenna for the tracking signal for the scientific experiment on board. The vertical whip on the top was supposed to be the data link transmitting antenna. Now, we don't show anything else. There's no ELINT antenna shown in here at all. We knew what we wanted to cover in that band but we didn't know what it was going to look like -- whether it be waveguide, aperture, or a monopole or what. We had a contract, ultimately, with Farnsworth Division of ITT to develop that antenna system.

Inside the central section of the spacecraft you see little wafers inside the instrumentation. The wafers are printed circuit boards that are foamed in place, and between each layer is a beryllium copper shield to keep the energy from moving unpredictably from one receiver to the transmitter or vice versa.

A Now this satellite was going to have a dual function, was it not? Didn't it have a scientific cover?

M Well, it became essential, if we were going to launch out of Florida, that we have a reason for the launch. If we were going to put a collection capability in [redacted] we had to have a reason, to have it there, a reason that was acceptable to the local community in that environment. So Dr. Page ruled that Dr. Friedman's group would put a working scientific experiment onboard this spacecraft. In fact, right in the front of the spacecraft you can see a circular aperture. Over here is another circular aperture.

A That's on the top left.

M The top part, about 45 degrees lattitude, and over on the right hand side, another one. These were estimates of what an antenna might look like, either an ELINT antenna or a "Dr. Friedman" sensor.

A Did you feel that this was going to constrict the quality of hardware that you could stick inside?

M Not at all. You can see the volume is not totally used. There is lots of space inside the spacecraft. Volume was not our constraint. The use of onboard power was the most severe constraint.

A Was there going to be a conflict in the scientific payload and the ELINT payload in terms of power consumption?

M Absolutely. Guaranteed. They had a requirement, as their experiments proceeded, for very low noise. They didn't want any disturbance on the power line; they didn't want to have pulses that would disturb their system at all. We didn't care. They didn't do anything to bother us except consume power on board, but we did interfere with their experiment.

A As I remember, you told me earlier, there was not too much interest in this space group to use this satellite for [a scientific] purpose. It was basically dragging them into a program that was originated for your purposes.

M My perception is that the lack of success early on in the VANGUARD program didn't speak very well for the long range hope of Dr. Friedman using this satellite. He was having good success with the V-2 rocket flights out of White Sands, and they were predictable. They would get several hundred minutes of activity and then they would analyze it for a long period time. It looked like with VANGUARD's failures that these satellites were destined to be "pyrotechnic" events instead of scientific opportunities. Dr. Friedman's group was not a strong advocate of this technology prior to the first flight. As soon as it succeeded, they recognized the opportunity -- it wasn't hundreds of minutes, it was nearly continuous. Everytime you could get within receiving range of a ground station, you could get 15 or 20 minutes worth of solar background information, and they had data like they never dreamed of having. They became strong advocates for space then.

A Who was the person representing their interests, the principal person representing their interests in this initial satellite?

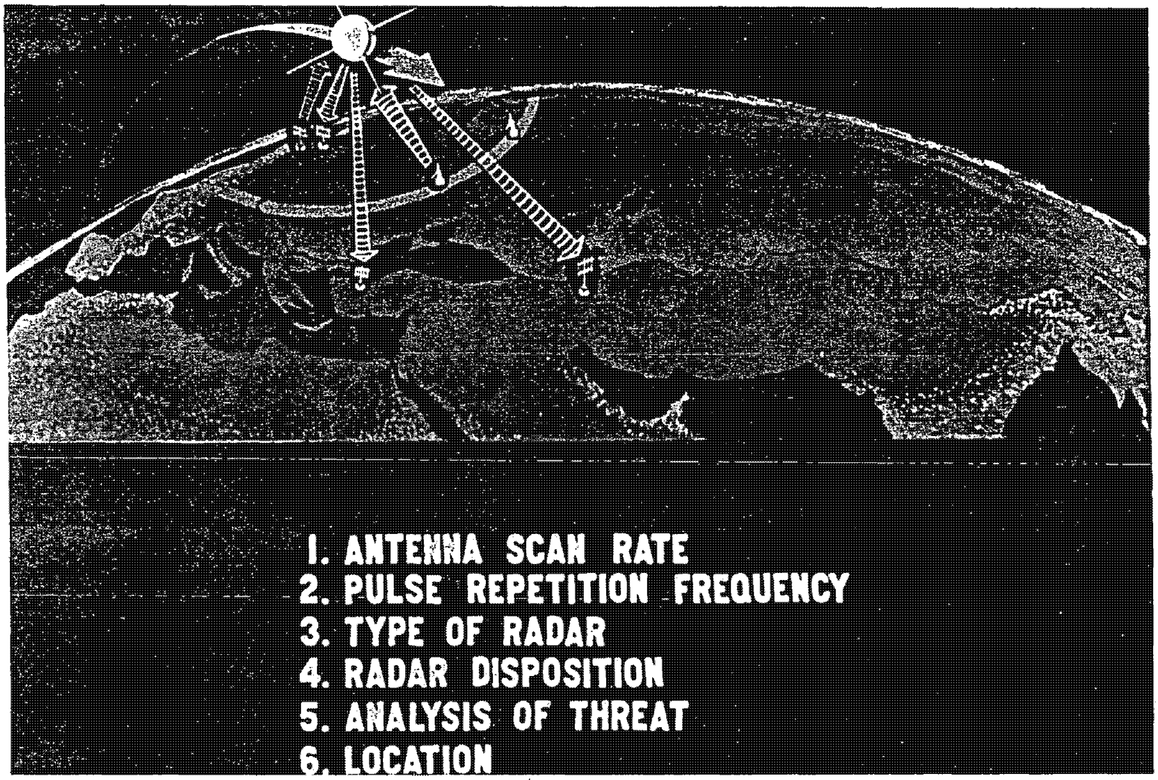

Figure 6

TOP SECRET
HANDLE VIA BYEMAN CONTROL SYSTEMS ONLY

M Paul Lester is the only one I interfaced with. He and I each worked with the Satellite Technology Branch to integrate our hardware subsystems into the satellite. We both were present at the launch site for the preflight preparations that go on. I think Paul was probably a GS-5, 6 or 7, a very capable young technician with good experience and background. But it was obvious that they didn't believe that this satellite was going to be worth a whole lot.

A [Let's return now to] what the briefing looked like as you went around to sell it.

M All right. The second viewgraph that we used, Figure 6, shows how the military mission experiment was expected to operate. In this graphic, we have the spacecraft coming up over western Europe. There is a pink arrow from Bremerhaven up to the spacecraft indicating an electronic command interrogation. The communications station at Bremerhaven would be turning the onboard timer of the spacecraft into the "powered-on" mode. Then the military ELINT collection would continue to operate for about 4Ø minutes - equivalent to the longest pass across the Soviet Union. During this 4Ø minute time, radars situated out near the horizon as they scanned, would cyclicly illuminate the spacecraft, laying on a few hundred pulses every time they scanned past the spacecraft. The crystal video receiver in the spacecraft and its associated transmitter repeated [these signals] pulse for pulse. [There was] no storage on board. [The spacecraft] would stretch the pulses. Say it picked up a one microsecond pulse in S-band, it would retransmit in the 150 megacycle region as a 600 microsecond pulse. These we could have on the ground, a fairly narrow band receiver with a high sensitivity, and a fairly large antenna in order to collect any transponded signals from the satellite. So, in essence, it was a repeater. It picked up a pulse and retransmitted it on another frequency and stretched it in time.

We were able to preserve the antenna scan rate by noting the time between one illumination and the next as the radars would cyclicly scan. We could measure the time between one pulse and the next. We could also get the pulse repetition frequency (PRF) of the emitter, and from these scan and PRF rates, we could determine the type of radar. We could tell how the radars were distributed around the Soviet Union and thereby analyze the threat. Now as far as location is concerned, this early sales pitch promised a lot more than it delivered initially. It wasn't until several years later that we got a real capable location scheme.

Let's go on to Figure 7, keeping in mind that this was only the initial sales briefing. [The figure shows] a view sliced through the center of the earth on out to altitudes of 1200 miles, and the shape in the vertical plane of main beams from two threat radars. The GAGE radar has a cosecant-squared pattern, which provides a fairly fat vertical pattern which illuminate our spacecraft to altitudes of something like 950 miles. You

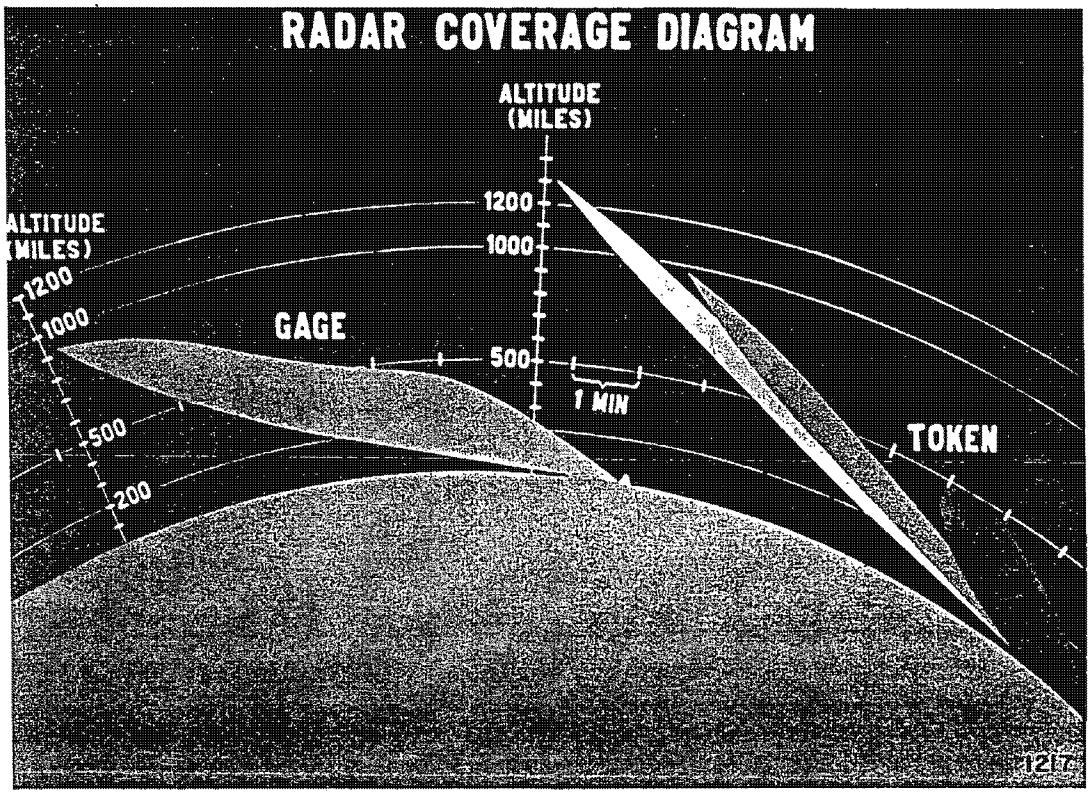

Figure 7

TOP SECRET [redacted]
HANDLE VIA BYEMAN CONTROL SYSTEMS ONLY

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would see it once as you approached the GAGE and again as you were departing beyond the GAGE radar.

The second radar that's shown here is the TOKEN and that was an early warning V-Beam radar. It had several beams; some spaced vertically one above the other, and three others off at 45 degree inclination relative to the vertical, so they were in the form of a "V". With this particular emitter, when we were lower than 5ØØ miles altitude, we would be illuminated by the slant beams. If we were illuminated with both vertical and slant beams, we could tell at what angle, back toward the nadir, the emitter lay, whether it was close in beneath the satellite or further out toward the horizon. As we approached it we would see it, as we departed we'd see it. If you bisect those times, you can say the emitter lies on a line perpendicular to the flight line and how far out from that nadir it is. It would give you some rough idea -- it would either be to the right or left, close in or out further. So, we could get rough geolocation on the V-Beam emission of TOKEN. It was an unexpected boon. We knew it was a V-Beam, but the thought of geolocating by this process didn't take place until we were up to our ears in data after the launch.

A So what you just said was not part of your original selling package. The original concept of locating was to determine very roughly when the beam swept past the bird.

M Well, you'd note at what point on the trajectory across the Soviet Union you first saw the emitter; you'd note at what point you last saw it as it departed from the area, and you'd know the emitter had to lie on the line equally between those two points. You didn't know how far out -- you had no idea whether it was close in or way far out near the horizon. [If you looked at it the next day] the line would be a different direction and you might, after a period of several days and several intercepts, be able to find the intersecting point. That was our initial thought. The inverse height finding scheme turned out as a unexpected bonus.

A In this early plan, was the idea only to get the main beam of the radar?

M Absolutely. We did not want to get the side lobes. In fact, we very carefully tailored our sensitivity so that we would not get side lobes, because we wanted to preserve what we felt to be one of the most important paramters of the radar, that is determining its scan characteristics.

If you were to intercept a threat radar, the first question you wanted answered was, "What is the radar designer doing with his antenna?" Is he scanning the horizon in a 360 degree fashion? If so, you'd say, "that's an early warning set."

"Is he sweeping it in a tight cone?" [If so, you'd say,] "that's a gun laying radar."

"Is it a fan beam that moves back and forth?" [If so, you'd know] "that's watching the end of a runway or something like that."

"If it moves vertically that is a height finder." Once you have answered those fundamental questions, the answers being discernible from the scan characteristics, then you were well on your way in determining the type of threat. To preserve the antenna scan characteristics was absolutely imperative in our initial thinking and largely continues to be even to this date. We did not want sidelobes, because they would obscure the scan characteristics.

A Were you planning to preset your sensitivity, or were you planning to have it changeable from the ground through telemetry?

M Well, initially we flew with all the sensitivity we could get, because the crystal video system is noted for its low sensitivity, and we wanted to be sure we had adequate sensitivity. Later on we tailored the sensitivity as well as the frequency spectrum toward a threat emitter family.

A You felt that with the crystal video system you wouldn't have too much trouble. The side lobes would almost automatically be excluded.

M Yes, unless we had added an RF pre-amplifier. If we had an RF pre-amplifier, and if the antenna were looking down, say, into the throat of a height finder radar, then we would have enough reduction in range and increase in sensitivity to get side lobes. But, we generally have not been bothered with side lobes.

A Was the technology involved in the satellite ELINT collector essentially the same as the technology that you had in the submarine set? Or was miniaturization a problem?

M Transistor technology had taken a firm hold of our community by then and with them, we were able to develop low-voltage amplifiers with high gain and acceptable band width. The crystal video principle was still the same. We had an antenna, a band definition filter, a detector crystal, and an amplifier. From the early 1950's to the late 1950's, the amplifiers went from tubes to transistors first using discrete components. They had very very low power consumption, on the order of 20 or 30 milliwatts for an amplifier on a 12 volt power supply.

A Were you able to use stock amplifiers for your purposes or did you have to design them specially?

M We didn't design them here at the Lab., there were so many commercial sources. With our experience in the crystal video technology, we had in mind one source, a company up in New England, for the filters, and another company in Philadelphia for the crystal detectors. But we felt almost anybody could build the amplifier. We were obliged by the rules of procurement to go out on a competitive contract in the first instance, and Farnsworth Co. in Indiana, a division of ITT, won the competition -- by price primarily. They underbid everybody else. They promised that they could meet the

A For the whole system?

M Yes, for the whole system. Well the antenna was a separate contract, but they planned to supply the band pass filter, detector, and amplifier.

A We are getting a little bit ahead here. But your plan, then, was to get all these components on contract and to have them fit together. You planned for the system to be a transistorized, low-weight and low-power consumption. And you knew because of the advent of solid state electronics that this would be feasible.

M That would be achievable, yes.

A Let's go on then to the ground stations.

M Figure 8 is the artist's concept of what we thought the ground station would look like. It shows a small electronic shelter hut that is maybe 7 feet tall, 8 or 9 feet long, and 6 feet wide. Inside, through the open door, you can see a steering wheel through a work table. The steering wheel has a vertical mast up through the roof. There is a bearing at the floor, there is a bearing at the roof, and the steering wheel will rotate the mast. On this mast

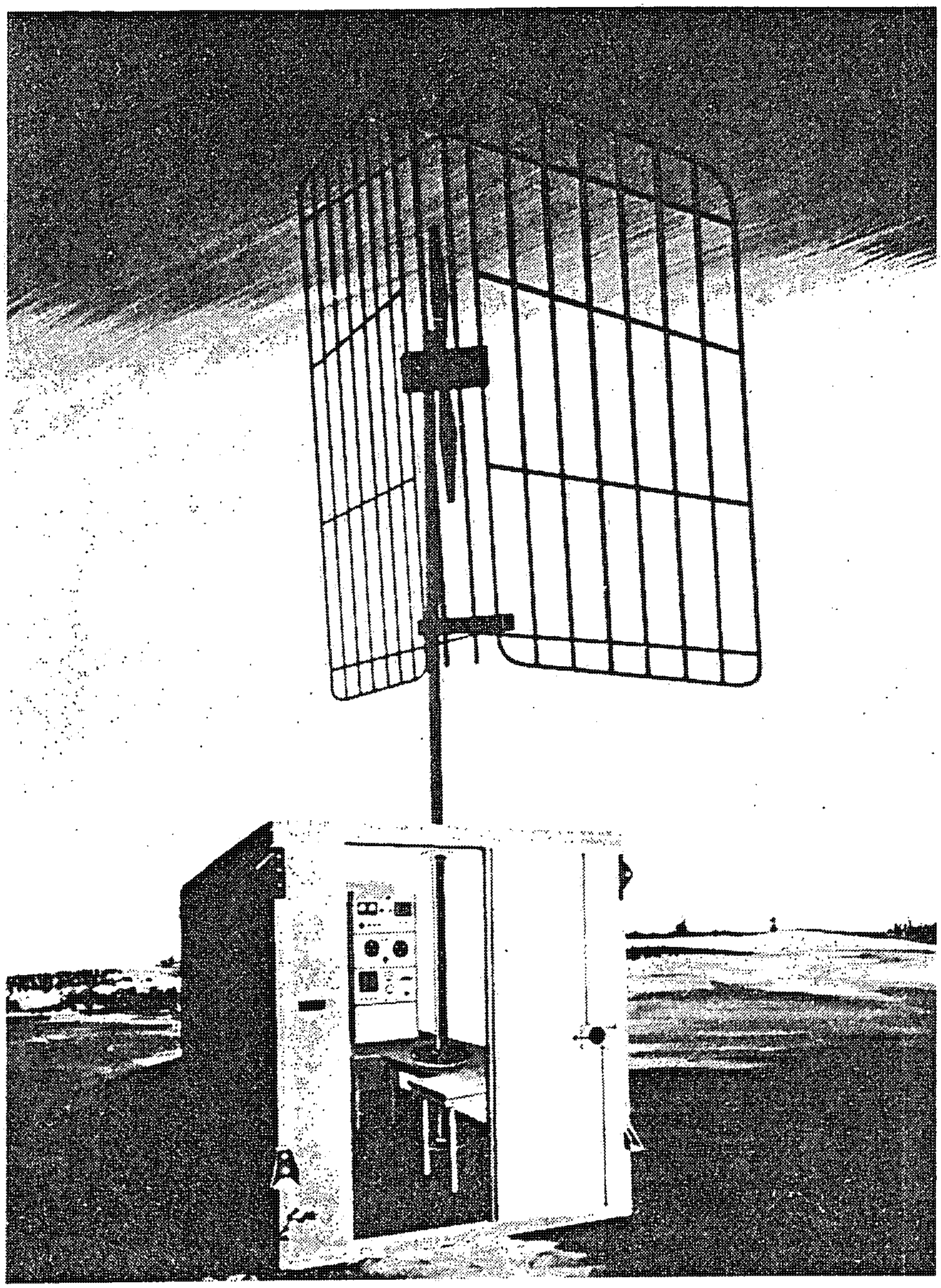

Figure 8

we have a corner reflector antenna. It seemed that we needed something with reasonable directivity in the azimuth and which was fairly fat in the vertical plane. This was an antenna that we thought we might be able to use. It turned out we didn't use the corner reflector at all, we used Yagis. We did retain the mast and the bearing, but instead of it being centered along one wall, it was centered on the far end (the back end) of the hut. So, we moved things around about 90 degrees. This is what you call "poetic license" (laughter). We had all the equipment but didn't know how it was going to be situated when the artist concept....

A How it would be configured was a minor point.

M The philosophy was to engineer the system in such a way that the electronic shelter hut was a shipping container, containing everything. The power cord was coiled up inside and the hut was shipped to a site. NRL would send two men over to erect it at sites of the Naval Security Group, or another member of the cryptologic community. They would send semi-skilled operators by the hut several times a day to make a tape, which would be sent back to NSA. That was the concept.

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M Figure 9 is an illustration of the earth with the Soviet land mass in pink, the rest of the land area in green, and the water in a blue gray. Here we have centered about each one of the proposed sites a horizon circle with a radial range line. The radial line comes out and it says, "200, 400, 500, or 670". That indicates the radius from the site and where you could see a satellite at altitudes of 200, 400, 500, or 670 miles. The circle that we have drawn is 500 miles, because that was to be the design altitude of the first spacecraft. We have a circle around [redacted]

Page Denied

A Was there any problem in planning where these sites would be or was it a fairly straightforward process?

M Well, the process was done by a multi-service steering committee -- Army, Navy, Air Force, [redacted] NSA. Each one wanted to have a piece of the action. They felt that they would benefit by being able to see, a few times a day, deep into the heartland of the Soviet Union, and to learn what electro-magnetic ELINT spectrum was. Each one of the services could gain through ELINT training of their personnel. In fact, that did materialize and it was one of the most prolific sources of ELINT experience that the communities ever had.

The other thing that this illustration shows is a yellow line going up to the right through the center, obliquely at 45 degrees. There is a dot on the line where the spacecraft is at the first instant where it can be interrogated from [redacted] If the spacecraft is turned on from [redacted] at this instant, it can see radars clear out to the horizon. And, as the spacecraft proceeds along towards the northeast, across the Soviet Union, the ground based data collection coverage moves from one station to another to another on across. There is only one little place in the middle which is beyond the range of all of the intercept sites. That does not mean that radars in that area are unavailable for intercept, because you have already seen that area when the bird was back here [indicates with hand] and you will see it again here. There is a gap in the coverage, but it is not below the bird, it is out at the horizon, and you already have opportunity of examining that area on other passes. So this is a graphic that shows how the sites worked together. The idea was to have more than one site viewing the bird at the same instant in time.

We were very worried about the danger in reducing the community confidence in this system, and one way to raise that level of confidence was to have two sites recording data from the bird simultaneously. If they both came up with the same answer, then it must be from the spacecraft. We were able to hear other things in the local environment -- a movie projector that had a sparking motor, a meat grinder [redacted] These things plagued us during the early years of the program by giving signals that looked like ELINT -- they were cyclic, they had periodicity, but they were not from the spacecraft. The proof was that neighboring collection sites were not seeing those local signals. Moreover, the site that we are talking about could hear the signals even when there was no spacecraft around. Later on, we were able to drop the alternate sites, and just depend on one site watching the bird at a time.

A I wanted to pick up a little bit more on your inter-service planning. Prior to the time this briefing was put together then, the working group was in operation?

M Yes.

A Had Lorenzen been mostly responsible for getting that group together?

M I would think Lorenzen, [redacted] and Hank DeCourt at Ft. Meade. Ft. Meade then was called the NTPC (National Technical Processing Center). It was their mission to do all the reporting and processing of ELINT. They assumed that responsibility for this program as well, not knowing what a terrible burden it would become in the years to follow. Yes, the steering committee was made up of representatives of each of the service organizations, plus NSA [redacted] Lorenzen was a prime mover there. He wasn't the chairman in the early days, I think the chairman was one of the people of the Naval Security Group, which was the one of the three service crytologic agencies that manned more of the stations than any other group.

A I'm trying to pick up the chronology. Presumably what happened is that after this idea was sold to Lorenzen, he then went out and talked to people at the Naval Security Group, [redacted] and then somebody put this working group together in order to plan such things as the collection stations. At the same time the concept was being developed, the briefing package was put together and eventually came to the form that we have been discussing.

M There is one element that we haven't mentioned. For a program to get started in a community, you need funding. Most of the other activities we have talked about -- crystal video development and so on -- were normally funded by a bureau. Here we are talking about something new. The initial lump of money to get this off of top dead center had to come from the Advanced Research Projects Agency (ARPA). Getting them sold was the first imperative. Once that happened, getting the Navy and the cryptologic community behind it to pick up and sustain the program in the follow on years was essential. That is why you needed more than just Navy, you needed the cryptologic community, which is multi-agency.

A Was this briefing subsequent to the time that ARPA was involved?

M I think that elements of this briefing, and some of the early graphics were used with ARPA. After they got ARPA convinced, then further graphics were done.

Figure 10 is, again, an early graphic before we had the full program approval. It shows the intelligence which we thought would be able to be supplied by the program, which was then called TATTLETALE. It shows that you could get information concerning the characteristics and location of air defense emissions, new S-band equipment, research and development testing activity, electronic manufacturing areas, and information on the ambient electronic atmosphere within the S-band throughout the Soviet Union. We were proposing to cover about 750 megacycles of S-band: 2500 - 3250 megacycles. That was the spectrum of the TOKEN radar and the GAGE radar that I spoke of earlier. But, we thought that there were probably other emissions

SPECIFIC INTELLIGENCE WHICH "TATTLETALE" CAN SUPPLY

1. INFORMATION CONCERNING CHARACTERISTICS AND LOCATION OF AIR DEFENSE EQUIPMENT
2. EVIDENCE OF NEW "S" BAND EQUIPMENT
3. INFORMATION CONCERNING LOCATION OF RESEARCH, DEVELOPMENT AND TESTING ACTIVITY
4. INFORMATION CONCERNING LOCATION OF ELECTRONIC MANUFACTURING AREAS
5. INFORMATION CONCERNING AMBIENT ELECTRONIC ATMOSPHERE WITHIN "S" BAND THROUGHOUT USSR

Figure 10

that we did not know about. From these you could determine or at least estimate the type of R&D and manufacturing activity. Now, some of our ideas were wishful thinking. We were premature, clairvoyant of the years that followed. All this did not happen on the first launch.

Figure 11 is a summary of the briefing that emphasizes the essence of the program: simple techniques, the fact that the components existed in that time frame, the fact that the launch vehicles were available. We were predicting one year life in orbit. We could collect the data with existing ELINT cryptologic community stations. The processing would be done by the National Technical Processing Center at Ft. Meade. We predicted that it would only take six months to be ready for launch. That did not materialize. We said it would be economical and through the first launch we had spent 1.1 million dollars on the program. So, if that is economical then we were right.

A Economical relative to what other space projects?

M There was at least one other being advertised in the literature then: the SAMOS WS/117-L Air Force program. That was a <u>multi</u>-million effort. This program that we are talking about was contemporary with the early Air Force intelligence collection ELINT programs, and as luck would have it, we flew a successful flight before they did. So, ours was the first successful ELINT or SIGINT collector that this country had in space.

SUMMARY

1. SIMPLE TECHNIQUE
2. COMPONENTS EXIST
3. EXISTING VEHICLES CAN LAUNCH
4. LIFE ONE YEAR
5. COLLECTION OF DATA BY EXISTING ELINT STATIONS
6 DATA PROCESSING BY NTPC
7. READY TO LAUNCH IN 6 MONTHS
8. ECONOMICAL

Figure 11

A Let me pick up a couple of dates. I should mention first a Bureau of Aeronautics project from which some of the initial money came, prior to the time the Advanced Research Projects Agency got involved. The money was given to NRL in March 1958 for another purpose, but some of it was taken to pay for some of the initial planning of your project. In December of 1958, the idea was presented to the Chief of Naval Operations, [ADM] Arleigh Burke, who approved it. In January 1959, it was presented to the Secretary of the Navy, Gates, who also approved it. In April 1959, it was presented to the Advanced Research Projects Agency, which recommended that the Secretary of Defense approve it. That approval from the Secretary of Defense came in July 1959, and the Secretary of Defense recommended that the President of the United States approve the project, which was necessary. Presidential approval came in August 1959. That, then, is a brief description of some of the high level approvals that this project TATTLETALE received in its early period.

M That was President Eisenhower.

A Did you get any significant opposition to the project during this approval period?

M None that I recall. If there was, it could have occurred without me being aware of it. I was not on the sharp, cutting edge of the briefing team or the selling of the program. I was down in the ranks, in the trenches as it were. There could have been opposition that I was not aware of. I detected none.

A There was no opposition from the Air Force at this period, even though this was, in some sense, competitive to the 117?

M Weapons System 117L SAMOS was their major ELINT mission, but it was more than just ELINT, and they did not really see us as a competitor.

The way they were running their program, much of the information became available in unclassified literature. And when they learned about Project TATTLETALE, it wasn't but a short while later that it showed up in The New York Times. The disclosure of TATTLETALE in The New York Times infuriated President Eisenhower. In fact, he wrote a Presidential Directive killing the program! TATTLETALE was killed. Eisenhower brought in the senior Admiral at ONI (Office of Naval Intelligence), ADM Frost, and lectured him at some length about the indiscretion of The New York Times. It was at Eisenhower's insistence that ADM Frost initiated a system of security oaths that were to be executed for everyone who was knowledgeable about the recreated program. Also, some member of the Presidential staff began reviewing the list to see how many people were being briefed in. Eisenhower wanted it to be kept to a minimum. The security agreement was for Project CANES, and there was a clearance called WALNUT that was executed when you were knowledgeable about the entire program. At the time we flew the first satellite, there were less than three hundred people in the country, and overseas that had signed statements and were knowledgeable about the program!

A So what essentially happened when Eisenhower killed TATTLETALE is that he killed it under that name. It was recreated under the name CANES.

M Well, CANES was in the security domain. The project was called GREB (unclassified) or GRAB. GRAB was an acronym meaning Galactic Radiation And Background. That was the classified word.

A Do you remember about when that disclosure to the *Times* and the killing of TATTLETALE occurred?

M Well, it had to be in the fall of 1959.

A It was fairly soon after Eisenhower approved it.

M Very soon after he approved it, yes. It was only a matter of a month or so after he approved it that it was killed. We were all heartbroken, and we were just livid, to say the least. We thought the leak might have been deliberate, but I don't believe, in retrospect, that it was. It was a standard procedure for the Air Force programs in those days. You could read all about their programs in the *Times* and all of the magazines, it was fairly wide open. But the reaction was the beginning of the compartmented, very carefully controlled activity that followed.

A Prior to the time, what was the classification?

M Secret, I believe, in our realm. Most of the viewgraphs were not even marked.

A So, it really did mean a change in how you operated on the project.

M Yes! Very definitely, yes. Things became TS (Top Secret) after that.

A Once the project was reconstituted, and you had your approvals, did this become a full time effort for you?

M The activity in preparing for this first launch involved two major areas: (1) spacecraft and (2) ground stations for collecting data. In Mr. Lorenzen's branch, under my auspices, we did the military mission experiment; the ELINT sub-system of the spacecraft. That was largely a contractor effort with Farnsworth ITT, Ft. Wayne. They had a very good dialogue with us.

It looked for a while like they were going to be late in delivery. They had a bright young engineer who was designing the bandpass filter. All he seemed to do sit at his desk with a slide rule and, really, we wanted to see hardware. This was only a couple of months before delivery was due. Finally, he got up from his desk, went to the shop, cut a piece of waveguide, drilled some holes and soldered some things on it. This became the interdigital filter using a waveguide cavity that we are still using today. It was a classic! He made it work, and it worked beautifully.

The activity in the spacecraft area was under Marty Votaw, one of the dozen and one half or so NRL employees that decided not to go to work for NASA, when the VANGUARD team left the Lab. There were 15-20 that stayed here at the Lab. to continue their careers with the Navy. Votaw was one of those, and he took the responsibility of building the spacecraft.

Under my leadership, the electronic system for the ground collection huts was done. Mr. Charles Price did the mechanical design for the antenna and steering mechanism. He used a Chrysler emergency brake as a device to clamp on the mast amd park it so the wind would not turn it. He used a Mack truck steering wheel which only cost twelve dollars. He bored out the inside and mounted it on to the mast.

A Were these actually constructed here at NRL?

M The huts were fitted here at NRL, yes, and checked-out here. The tubing of the vertical mast was stressed so that it could take a 150 MPH wind, if the hut didn't tip over. (When one did later on, they righted the hut, welded the antennas back on it and were still in business. It was really a rugged design.) Price pioneered the concept of instrumenting small electronic shelter huts, here in the Laboratory, and deploying them to a site to do special jobs. That is a technique that has since been used by the Lab. many times. It minimizes interface problems. You uncoil a power cable and connect it up and run operators by and the results are fairly predictable.

A third area I have not mentioned is the interrogation function. At one of the ground stations, [redacted] it was essential that we have a transmitter and an antenna to send signals to the bird to turn it on and off and do other, similar, kinds of things. That was engineered in close collaboration with the command receiver people in the spacecraft group. We wanted to be sure that there was nothing about the command signals that we did not understand. We wanted to have all of those facts clear. So there was very very close collaboration between the command receiver people and the ground station command transmitter operation -- both ends of the same link. That was done under Mr. Ed Withrow for the ground station, and Mr. Ed Dix for the spacecraft.

A These people worked under you?

M No. Dix was second in command under Marty Votaw, and Withrow worked for Lorenzen in another group. He was at my level, or maybe a little higher in the organization, but he was responsible for the transmitter and its interrogation function.

A Was Votaw under Lorenzen?

M No. He was not even in our division. Lorenzen was a branch head; Votaw was a branch head in a different Division. It meant a cooperative job. It was a much more formal arrangement than was ultimately achieved years later. We got a lot more informal as we worked together over the years.

A In your Division, Lorenzen was the prinipal coordinator?

M Yes.

A You had some functions but they were coordinated at his level rather than yours?

M With the ELINT experiment, 90% of the dialogue was between myself or Vince Rose, my subordinate, and the satellite techniques people. About 10% of it was budget, schedules, and things like that that Lorenzen entered into.

A So he left technical work almost entirely to you.

M Yes. But, he insisted upon being kept informed, even though he was not our immediate senior in the chain of command. He was two steps over us. We were a unit in a section and he was the branch head, two steps above us. We had instant access, day or night, to him. We had a good communication link.

A So, the communication among the various people working on the system was good. You had no problems in that area.

M Correct. Since I was on the receiving end of others assistance, I had no problem at all. In fact, they probably were not always happy about being distracted from what they thought to be their main line of activity, to help solve this new problem of the moment in Mayo's area. It did not result in bad feelings, as far as I could detect. They all worked for Lorenzen, we were all his resources, and we knew it.

A This was a high priority project for him?

M It certainly had his attention, because whatever he might be working on in another area, if a problem came up in this one, he almost, without exception, took this one for top priority.

A Now, in trying to pick up the chronology, the approval came in August 1959, and then there was the mix up about security. When was your estimate for a launch?

M In the sales pitch, we said in six months and that would be from Presidential approval, I presume -- so probably early 1960. Financial troubles were the first thing that delayed us. We ran out of money. But, ADM Tom Connolly, who had participated in some CNO study of space, was adamant about continuing the program even though we were short of dollars. The Navy money came from the TRANSIT program, which was well endowed with funding (it was a POLARIS associated activity). So, funding came off of TRANSIT through the Bureau of Aeronautics. Actually, ADM Connolly insisted that four organizations each ante up fifty thousand dollars to make a $200,000 kitty that would allow us to go through the launch phase. He anteed up his $50K and so did each of the other three. NRL was one of them. It might have been discretionary funds here or it could have been money from the Office of Naval Research, I don't know. At least NRL got tapped for $5ØK of it. This spoke very definitely of Connolly's belief in space and it allowed us to proceed on with the final launch preparations.

It was imperative before we even approached the launch that we have an isolated area, remote from the Soviet Union, where we could try the system out. We identified a Naval communications station in Hawaii, in the center of the island of Oahu. We equipped a hut with not only the interrogation function, but the receiving and recording function as well. This hut was twelve feet long and it had an extra transmitting antenna on top. We had that ready to go as launch time approached. Now, the first of May in 196Ø was when Gary Powers was shot down and the "overflight" issue became extremely important at the highest levels of government. President Eisenhower had even earlier insisted that we have the ability to turn this program off instantaneously. If there should be an incident, we should be able to cease and desist almost instantaneously. That's why we put the interrogation capability at the Naval Communications station [redacted] Well, with the U2 being shot down, the opportunities of us exploiting this program were diminishing very rapidly.

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A The U-2 incident did not affect your program directly because a different agency was running it -- it was an Air Force program. It did affect the utility of capability once it was operational. Is that your assessment?

M Yes. That is exactly right.

A Let's turn then to what the first launch consisted of, its schedule, configuration, and so on.

M All right. Figure 12 shows the THOR/ABLE/STAR within the gantry at Cape Canaveral.

A This is the launch vehicle?

M This is the launch booooster. The first stage was a THOR and the second stage was the ABLE/STAR.

A Let's get the date of that first launch.

M June 22, 1960

A As we said, that was delayed a little bit because of funding problems, but Connally had worked that out. It is now June and it is at Cape Canaveral. This is the launch vehicle.

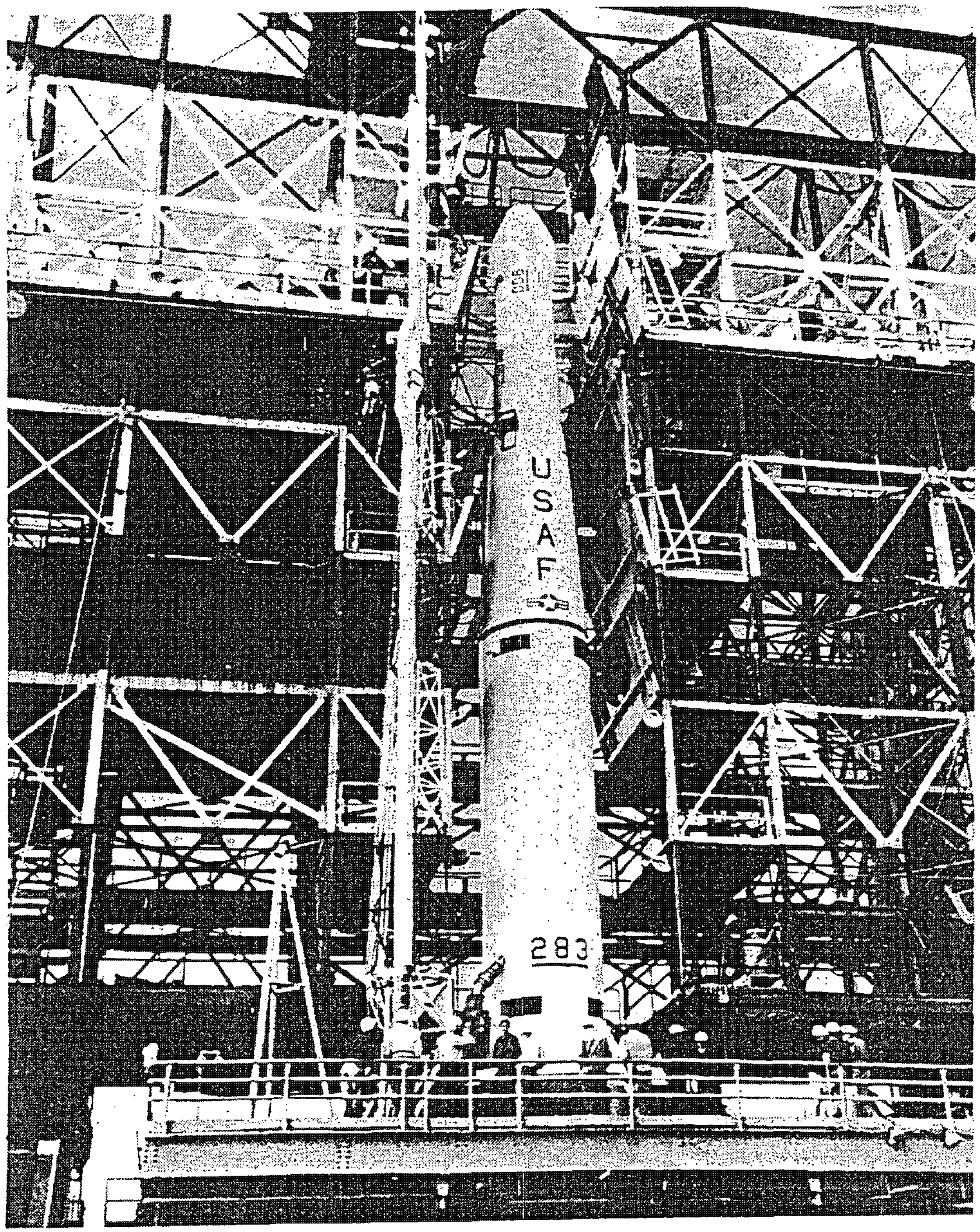


Figure 12

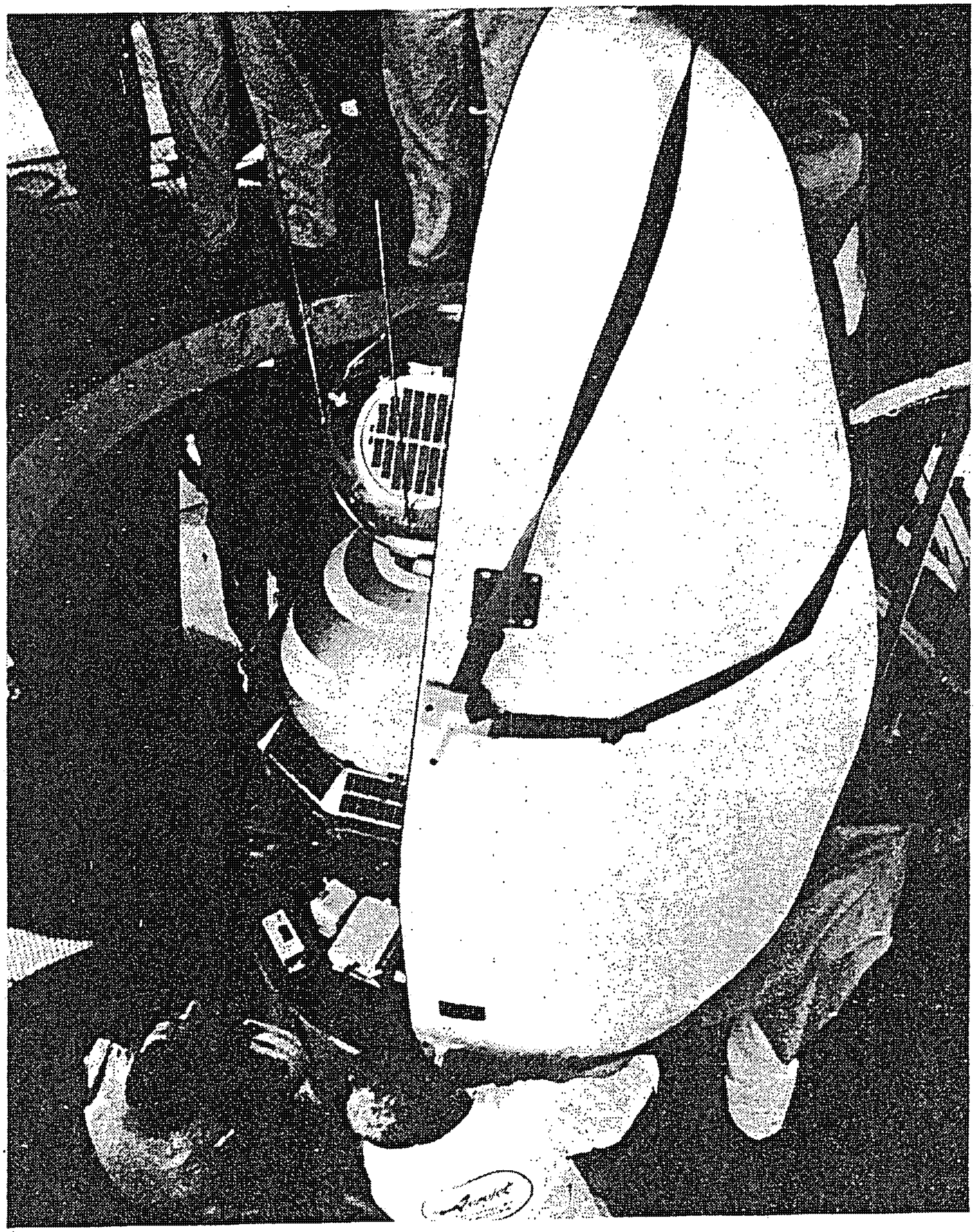


Figure 13

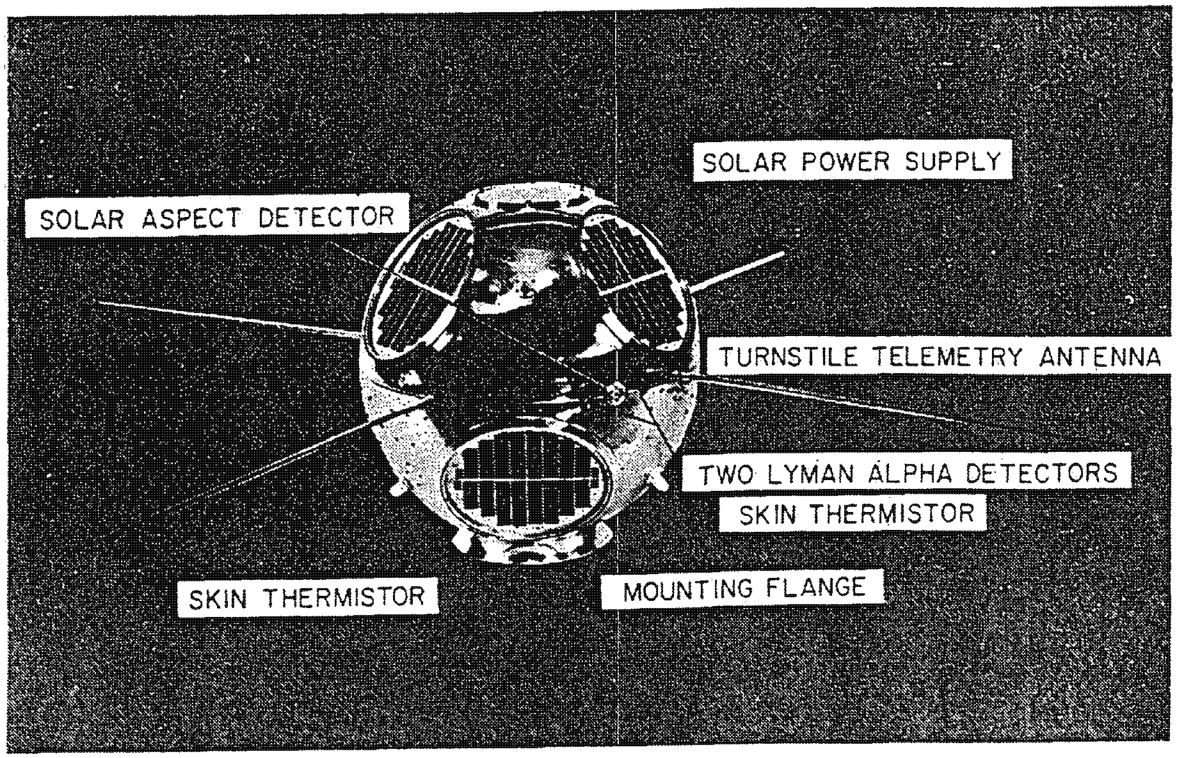

SOLAR POWER SUPPLY
SOLAR ASPECT DETECTOR
TURNSTILE TELEMETRY ANTENNA
TWO LYMAN ALPHA DETECTORS
SKIN THERMISTOR
SKIN THERMISTOR
MOUNTING FLANGE

Figure 14

M That is the precise launch vehicle in Figure 12, it was number 283. The ABLE/STAR was an Aerojet General upper stage. Figure 13 shows the TRANSIT spacecraft -- a 36 inch diameter sphere -- as the lower of the two payloads affixed one on top of the other. Ours was the 20 inch diameter up on the top. This figure shows that within the clam shell nose fairing, they are making last minute checks, getting the nose fairing in place. You can see people standing around on the gantry.

A Did you know any of those people?

M No, not really. I don't recognize the knees up top (laughter). They are Aerojet General contractor personnel because they are fitting the nose fairing on the second stage.

A Was there any concern about a dual launch? Did you worry wether the satellites would separate appropriately? Was that a worrisome aspect of your program?

M Not particularly, no. We knew that TRANSIT wanted to get shed of us. No risk about that. They wanted to get us off their backs so they could go ahead an do their job. We did not see a threat there at all.

Figure 14 shows the closeup view of the spacecraft with some descriptive terms. The solar-aspect detectors of Dr. Friedman are shown around the equator. You can see the solar cells. The solar power array is circular in configuration and much much larger than the artist's concept that we looked at earlier.

A turnstyle antenna array is still quite similar to the one shown earlier in the artist's concept. There are two Lyman Alpha detector apertures in the equator. What they called "skin thermisters" were just to allow this picture to be reproduced in the unclassified literature. They were really nylon caps over S-band ELINT receiving antennas. The mounting flange is a conical flange at the bottom. We had a Marman clamp with an explosive bolt which upon command or a timer, (I'm not sure which), would allow this spacecraft to be released from the one below it. There is a spring between the two that pushes ours off rather smartly away from the larger, heavier TRANSIT 2A.

Figure 15 shows a more detailed view of the internal electronics of the spacecraft. It is in a fixture for handling -- you see a handle up on the top. There are command receivers and this is a transmitter on the top section, a timer here, and a command receiver at the bottom, I believe.

A	This is essentially what ITT delivered to you?

M	No. This is what Marty Votaw's satellite engineering team bought from the industry in various components. The ITT ELINT hardware subsystem is shown in the next viewgraph, Figure 16. On the right hand side is the video amplifier with three or four cables coming out of it. This would, in

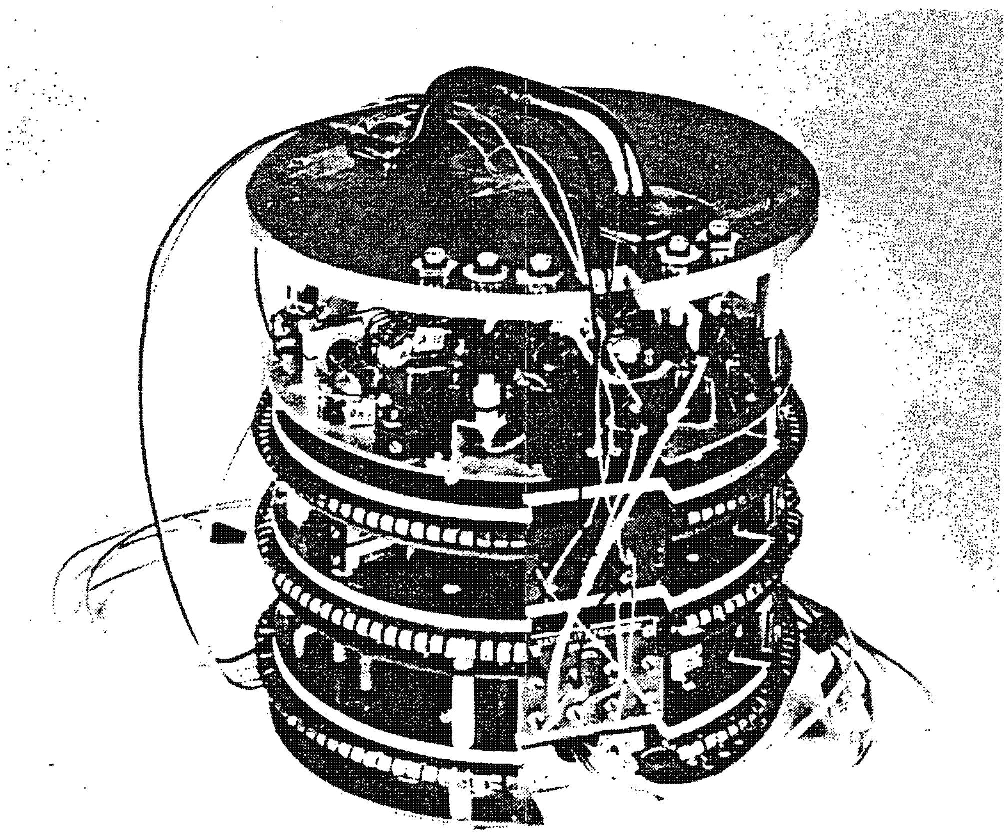


Figure 15

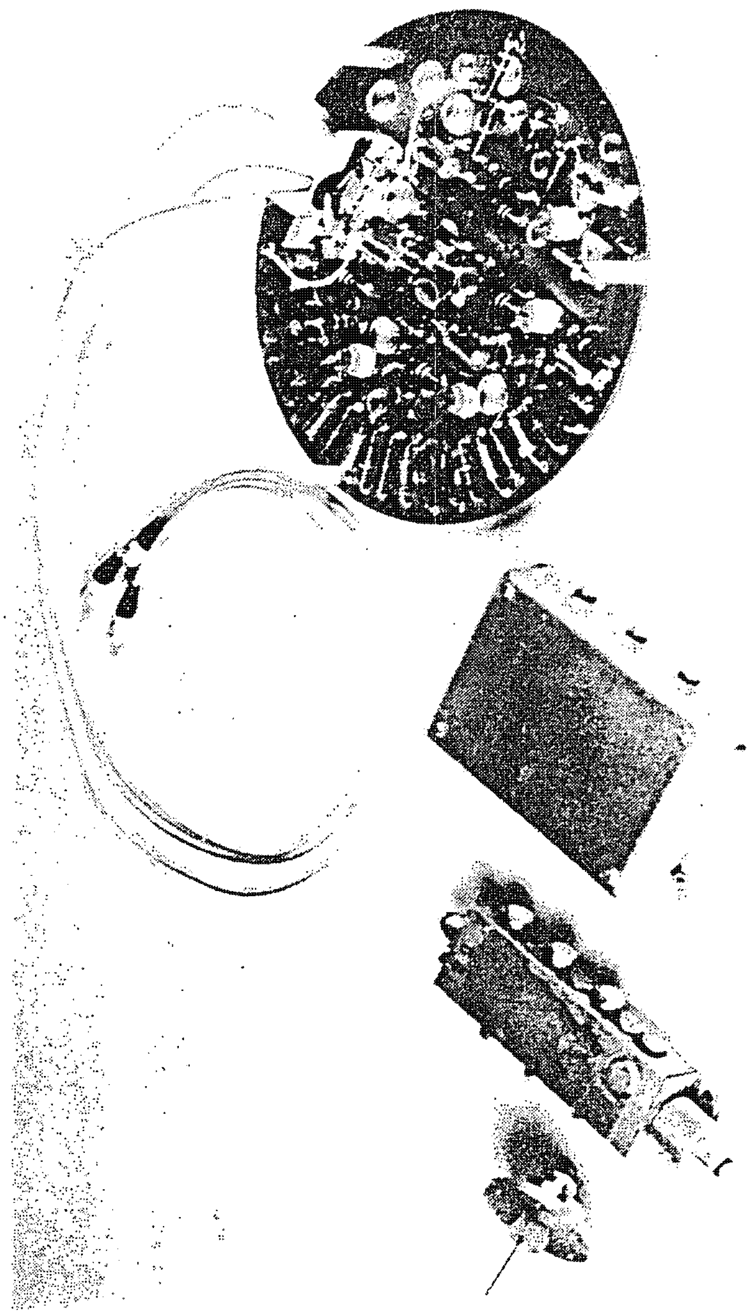


Figure 16

 BYE-58,Ø45-81

its final form, have been completely encapsulated in a foam potting material. It had one video input that came from the box on its left and that box had six inputs. Each one of those six inputs was connected to the filter detector, and still further to the left, each of the six channels went to an antenna. There were six antennas, all at right angles to one another on the surface of the 2Ø inch diameter sphere, so that we had an omnidirectional pattern. Each one of the antennas had a filter for determining the RF band. They were coupled together in this mixer box, which is in the center of the third rectangular box from the left. That box would connect to the video amplifier. From the video amplifier you went to the modulator and then on out to the transmitter and the transmitting antennas.

A Is that embedded in what we saw in Figure 15 [viewgraph]?

M Well, the transmitter probably would be the bottom unit of the stack.

A So, what we saw in Figure 15 <u>is</u> the entire internal package of the spacecraft and this is a portion of that?

M The military ELINT subsystem portion of it. Figure 15 also showed the "housekeeping" type subsystems of the spacecraft.

Figure 17 shows the RF spectrum that was covered by our first launch. The mission was called DYNO 1. DYNO was the classified name of the program given by the community at

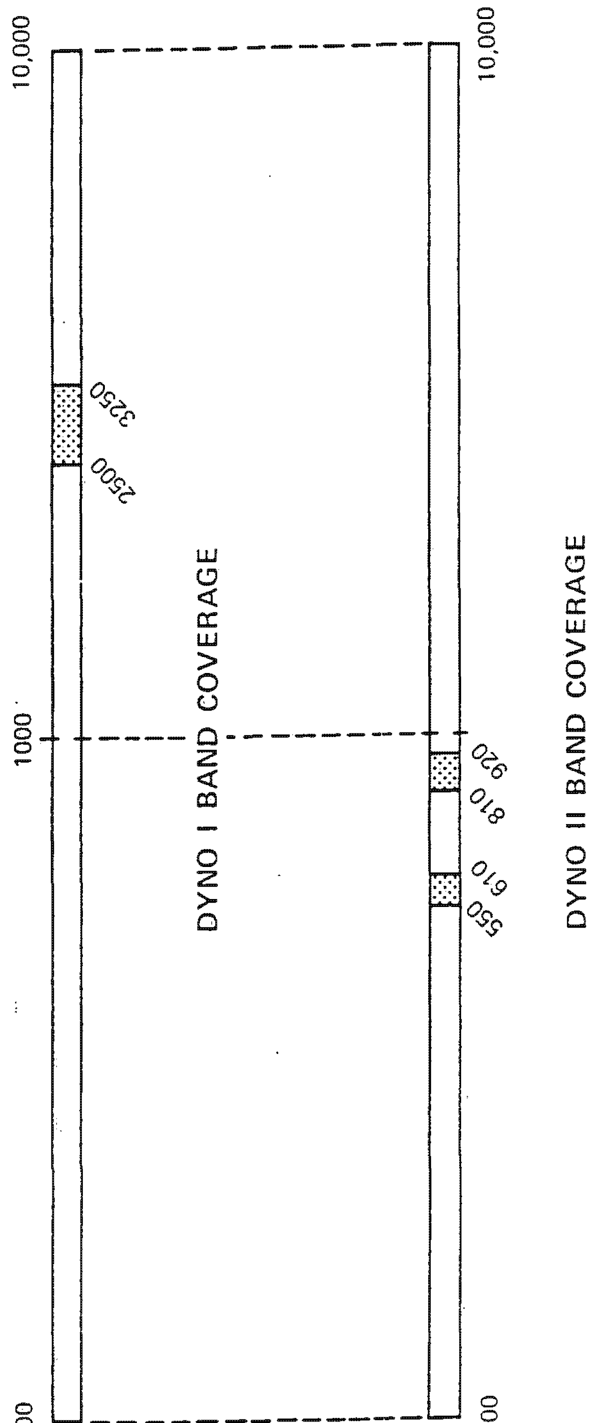

10,000
3250
2500
1000
100
DYNO I BAND COVERAGE
10,000
920
810
610
550
100
DYNO II BAND COVERAGE

Figure 17

NSA. The figure shows the spectrum covered from 2500 to 3250 megacycles. That was the RF portion embracing the radars of TOKEN and GAGE. There were some other radars that we found later on in the data. In the lower part of the picture, the frequency coverage of DYNO 2 is shown, and you will notice there are two spectrums -- 550 to 610 and 810 to 920. The data from the spacecraft coming from the lower band used a narrow pulse width, and the data coming out of the spacecraft in the upper band was a wider pulse. So, we used pulse-width coding and thereby could determine, in the data stream on the ground, from which part of the spectrum each one of the pulses emanated. This allowed us to do two jobs with one spacecraft. This was our first step toward making this, not a single purpose spacecraft, but multiple purpose collection platform.

Figure 18 shows the photograph of the interrogation and collection hut that we put over in Hawaii. It was at this spot on July 4, 1960 that we first heard the voice of our spacecraft in operation in space.

A The first one worked?

M The first one. The first interrogation did not work, but 30 or 45 seconds later it did. We had heart failure in between (laughter). We pushed the button and nothing happened.

A Where were you, were you in Hawaii?

~~TOP SECRET~~
HANDLE VIA BYEMAN CONTROL SYSTEMS ONLY

~~TOP SECRET~~
HANDLE VIA BYEMAN CONTROL SYSTEMS ONLY

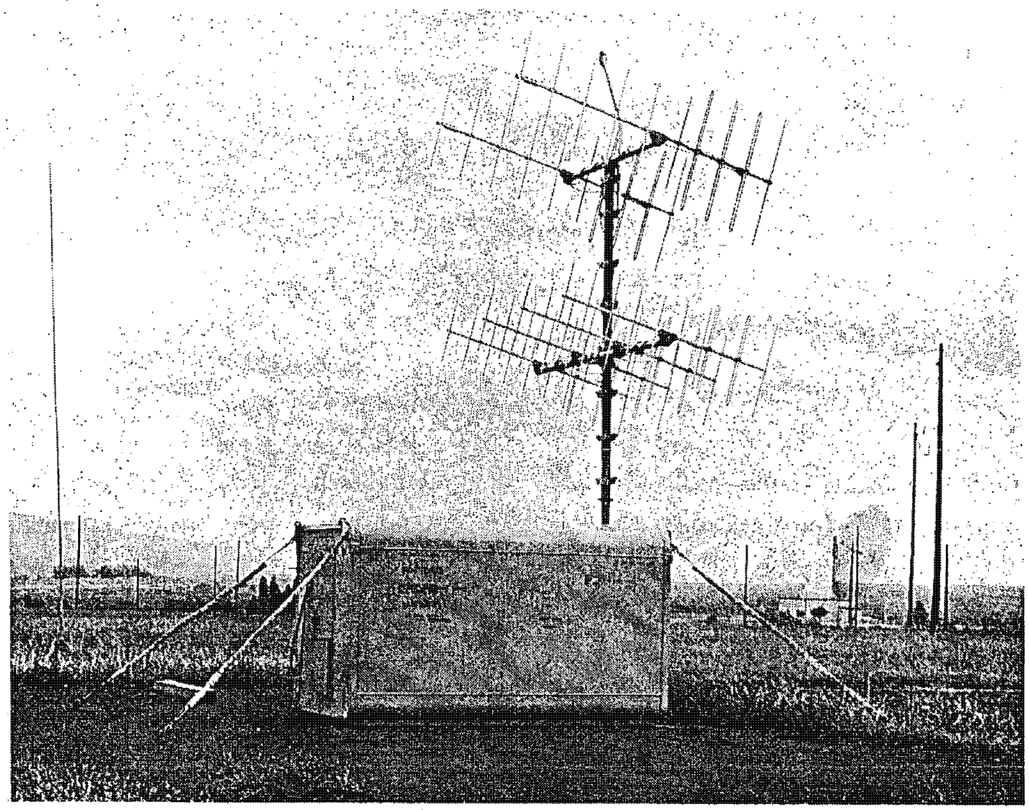


Figure 18

M Yes. The team that was in Hawaii was Mr. Lorenzen, who headed the team; Mr. Withrow, who built the command capability in this hut; Mr. Edgar Dix, who was the spacecraft engineer in charge of the command receiver in the spacecraft; Vince Rose; and myself.

A You were all standing around watching?

M We were all huddled inside here waiting for some sound to occur. When it happened, we all let out a shout of glee that it worked! Then I think most of us then went outside and celebrated with relief (laughter)! It was really a very, very emotional evening. We turned it on and we could hear radars as far away as [redacted] The horizon was near Chicago or somewhere in the [redacted]. We could also hear San Diego, [redacted]

A I didn't get your reaction on the launch. Where were you at <u>that</u> time?

M Oh! We went down to [Cape] Canaveral and participated in testing out the military subsystem before launch. In other words, we would measure the sensitivity to make sure everything worked well. The transportation of the spacecraft to [Cape] Canaveral was a very informal trip in the back of Ed Dix's station wagon! We had it accompanied the whole trip. He took it into the hotel with him that night.

A I hope he paid for the extra bed (laughter)!

M He and his wife loaded it up the next morning and drove on into [Cape] Canaveral with it. I don't think we had a spare. It was one of a kind. The launch preparations were rather straightforward. [It was] new to me, because I had never been at a launch site before, but it went on according to the schedule that we had worked out. Because of the military mission and my presence in the ELINT community, I was asked to keep a pretty low profile. If any visitor would show up in the area, Mr. Rose and I would disappear. He was responsible for checking out the ELINT mission. He did all of the work on that subsystem. We both kept a very low profile. We were not in the countdown process at all. We were not in any of the pictures. In fact, a rumor existed -- I think [redacted] told me -- that we might have had some religious feelings about being photographed.

A Pariahs?

M Yes. We always ran whenever any cameras came out. It was because we did not want to be associated with space, after having a fairly wide recognition in the countermeasures arena.

A Was Lorenzen fairly visible?

M He was not at the site. Oh, no! They would not let him go to the site. He wouldn't go anyway. In fact, I don't think he ever attended a launch. As far as the launch is concerned, the visible aspects of it are awsome, but the most phenomenal surprise I got was the sound. It was by far the biggest impact, I mean the sound and vibration the booster gives as it lifts off and goes into orbit. This particular orbit was about 35Ø, plus or minus 15Ø, miles in altitude.

A It wasn't exactly what you hoped, because you were shooting for a circular orbit.

M No, but it was very good. It made orbit and it worked the second time we pushed the button.

We had accompanyment in our celebration on the island of Oahu with all the fireworks that were going off that evening. It was their first 4th of July as an American state [but we pretended] that they were celebrating the birth of a new space program (laughter).

A You flew directly from [Cape] Canaveral to Hawaii?

M Oh, no. Launch was the 22nd of June, so we came back to Washington. We didn't delay long. We were packed and ready. We may have spent a day or so here, but then we went right on over to Hawaii.

A It took a while, then. Was there any reason for that?

M Well, we had to get the hut checked out and working, and get our heads screwed on right.

A That must have been a whole period of nervous worry, knowing it was up there and wondering whether it worked or not.

M When I say we celebrated with relief, it was just about that. It was really a climax.

A So, the real moment of truth was interrogating that bird?

M That's right.

A Far more so than the launch.

M There were so many necessary, but not sufficient steps for success. Many of them acieved ahead of launch, and they had to go and go right, but, this one was the one where you knew if it was going to do what it was supposed to do.

In the spacecraft, we had designed a "System Self-checking Emission (SSCE)". When you turn the data transmitter on for a period of 1Ø-3Ø seconds, it puts out a burst of [redacted] that allows you to zero beat your receiver in the hut and thus get it on-frequency. From there, you accommodate doppler in a manual manner. You know the direction of the pass that it is coming on, and you can change your frequency so much every minute or two minutes to accommodate doppler shift and still have the signal remain in the center of your very narrow band pass. So you have to zero-beat your receiver on the Self-Checking Mission. If you are off 2Ø or 3Ø kilocycles, you won't hear the signal.

We had that kind of a problem the first instant we pushed the button. The bird probably was O.K. but we didn't hear it. In each of the commands, the first part of it is a reset command which turns the bird off so the next part of the command turns it on in the way you want it.

Figure 19 gives an interior view of the hut. You can see the Chrysler emergency brake handle underneath the workbench to the right of the mast. The Mack truck steering wheel is centered around the mast. The housekeeping telemetry and the data for Dr. Friedman's cover experiment came down on the R-39Ø receiver on the left in the lower position. A vacuum tube type time-code generator is above it with those vertical columns. On the right hand position, the data link receiver is in the lower portion and the next two pieces of equipment above that are the Magnecord PT6 tape recorder. [In the] lower left below the work table are the low noise converters -- front end of both the telemetry and wide band data link channel. It was all very straightforward and very trouble free, except that the tape would stretch at the drop of a hat. It was quarter inch tape and if you stretched the tape, the spacing between pulses was disturbed. It was not a gentle system. Regularly it tied into knots, was broken, and so on. One of the first major changes in the design of the system was to get a real gentle tape recorder, a half-inch, instrumentation-type tape recorder in the system.

A It would stretch tape because the recorder was constantly going on and off?

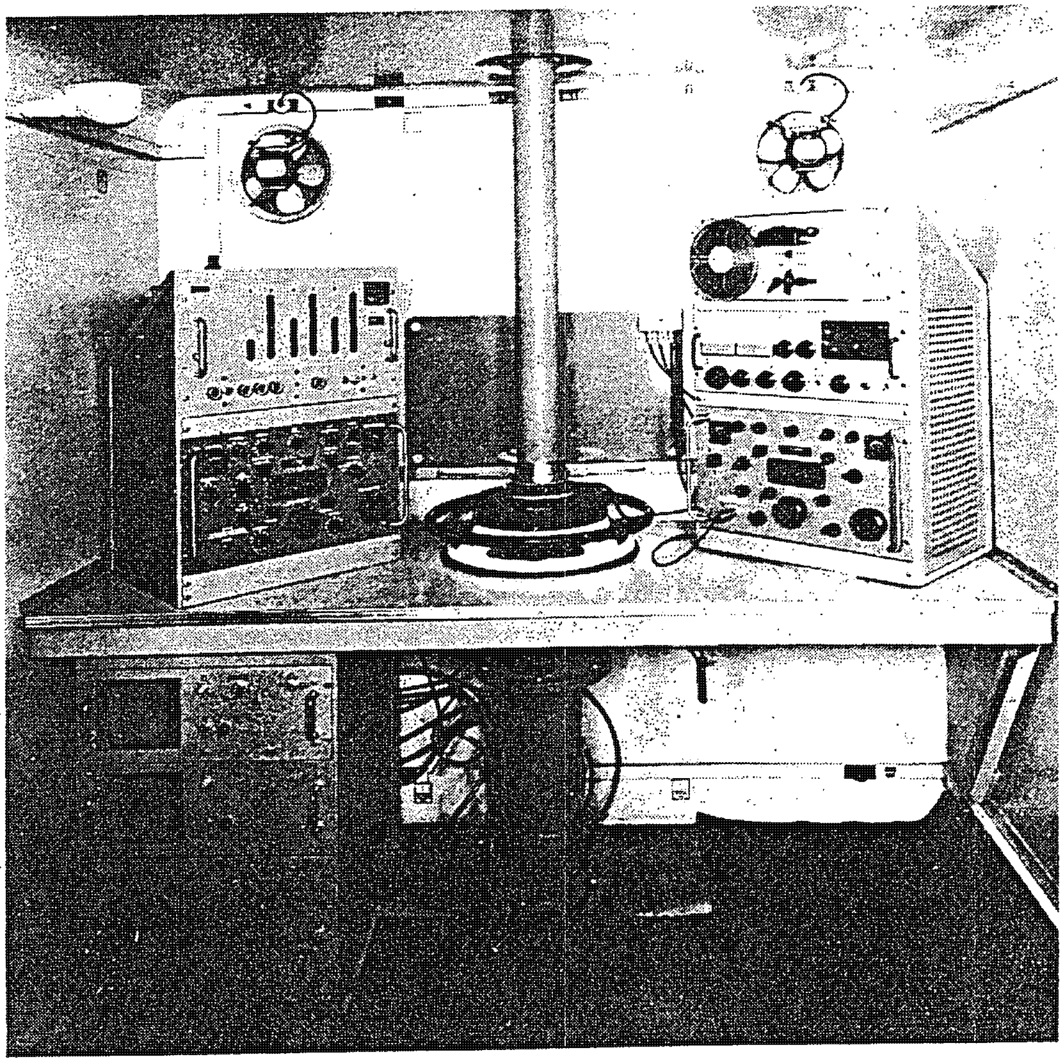


Figure 19

M Well, it happened mostly as you were playing the tape back, trying to figure out what you had recorded. You were trying to listen to what data you had received to try to do analysis. During the pass, it was almost impossible, so you would go back and listen -- "Is that a friendly or is that a Soviet signal?"

A Because of your trained ears you would be able to make that determination?

M We thought we could (laughter).

A You would <u>like</u> to think so (more laughter).

M There were, in the cryptologic community, ELINT operators who were very, very skilled at this. There were none at the scene in Hawaii where we did not have any military helping us, but at the other sites, in the weeks that followed, the operators there -- many of whom were not fully briefed in -- said, the minute they heard the first few signals, "<u>That's</u> not solar radiation background information, <u>that's</u> a TOKEN. <u>That's</u> a Soviet radar! You've got an <u>ELINT</u> mission up there!" It became very obvious to them, because they had been operating the classic ELINT positions, looking 5Ø - 1ØØ miles into the Soviet zone from their particular vantage point. Here, suddenly, they were able to see 3ØØØ to 4ØØØ miles into the Soviet heartland for about 12 times a day, if we were allowed to turn it on that often. Unfortunately, as it turned out, President Eisenhower's concern about the "overflight" and the "U2 shoot down" etc. were such that he personally had to approve everytime we turned this mission on. Consequently, we only used it operationally 22 times over the Soviet Union!

A Out of how many useful passes?

M Well, 90 days times 14 -- about 14 passes a day, over 1300 I think. We only got 90 days of life from the 24 volt power supply. We had a power supply failure at 90 days.

A That's a lot of passes!

M A lot of them, yes. Dr. Friedman's experiment worked for something like 13 or 14 months. He did not use the 24 volt power supply that we bought from Ft. Monmouth that failed! (I guess I'm showing a little prejudice).

A I want to ask a little bit more about the first operation. You mentioned your immediate reaction, you got some recordings from your second command -- what did you do after that? Did you sit there as a group and listen back over and try to figure out what you had heard? What was it like that first day?

M When the shouting passed and tranquility prevailed -- it wasn't until after the pass, really, that we had a chance to think what had really happened and to go back and listen. We actually recorded probably 15 or 20 passes in Hawaii. Not being very skilled ELINT operators, we were unable to make much sense out of it except to say, "There is sure a lot more data there than we expected. It's very busy."

A What did it sound like?

M From memory, it sounded just like it had seven years before when, in 1953, Vince Rose and I took an early crystal video receiver down to the dock along the Chesapeake Bay as the midshipmen steamed down the bay on their way out for their summer cruise with the U.S.Fleet. Five or six large destroyers, cruisers and what not, went down the Bay and we proceeded to record with an S-Band receiver, the emissions from these ships. They were so nearby, we were getting the back lobes and side lobes. It was just a continuous cacophony of sound. With this particular spacecraft, it was similar, but more periodic. You would hear, "Beep .... Beep .... Beep .... Beep", you know, that kind of scan rate, a very fast scan signal. An then there would be a, "Boong .... Boong" as if it were a "big horse" sweeping slowly by, with a lot of pulses. Each radar has its own tone, determined by the space between the pulses, and its own periodicity, determined by the scan rate. And once in a while we would hear one that you go, "b-r-r-r-r-r" something like that -- a conical scanning emission while we had flown through, or "beep-beep, ... beep-beep ... beep-beep", a book scan. You could hear those, and it was exciting.

A You had enough to know you really were getting different kinds, different sites?

M Oh, yes. By virtue of our crystal video work done on the Chesapeake Bay and on board aircraft, we knew what radars sounded like through a crystal video system. Another transmitter and stretching the pulse didn't make any difference at all. It sounded just like you were manning an ELINT position on board a ship or a plane, probably more like a plane.

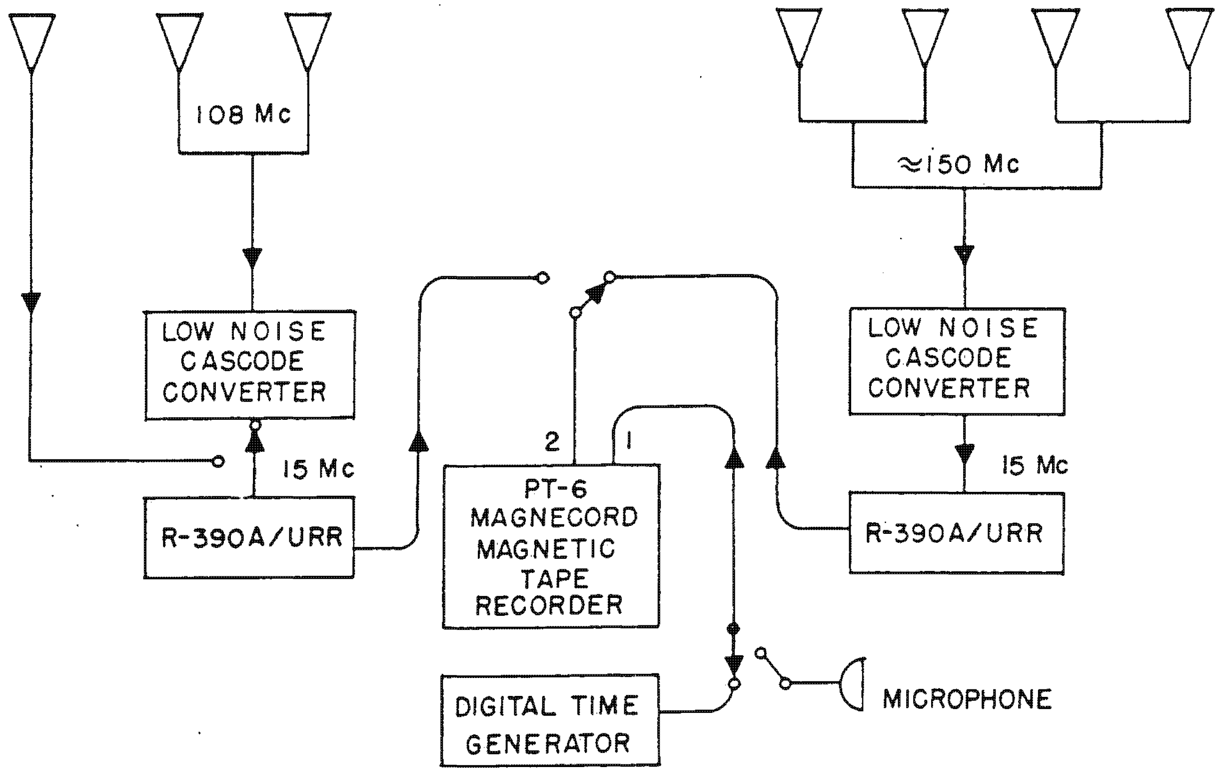
A You must have thrown a big party after that.

M Well, with Lorenzen you did not really throw a party (laughter)! You celebrated there on the job being pleased that it worked as you thought it would, and then you dashed for the airplane to get home to sell the next phase of it.

A That's like worrying about the ELINT receiver when the airplane's going down!

M I think [during] that trip to Hawaii, we did not even get in the ocean! We went to dinner twice in a fancy restaurant (laughter), and the rest of the time it was "catch as catch can". It was a very intensive trip. It was not a fun trip at all, but it was much like the dozens of them that followed.

We have in Figure 2Ø a viewgraph that shows the block diagram of the electronic system inside the hut. It is painfully simple. On the left is a 1Ø8 megacycle channel that receives the narrow-band data from Dr. Friedman's experiment plus the engineering housekeeping data from the space-



SIMPLIFIED BLOCK DIAGRAM FOR INTERCEPT AND RECORD

Figure 20

craft. It goes through a low noise cascode converter to an R39Ø receiver. We could switch that R39Ø receiver to a whip antenna out in the field and get WWV or one of the standard HF time broadcast stations. So we could get pretty good local time. In that mode, it went from the R39Ø to a switch that allowed the recorder on track 2 to either record that channel or the channel shown over on the right, which was the military mission data, a 15Ø megacycle channel from the spacecraft. Track 1 on the tape was devoted to either voice annotation through a microphone or a digital time code. With that time code you had the hour, minute, second, written on tape every second in a, "brrrrh, brrrrh," type fashion. That's what it sounded like when you listened to it. This was a quarter-inch, two-track tape, very unsophisticated. However, it was something we could put together cheaply and demonstrate the concept. It was not a precision type system at all.

A What was the response to this first successful mission and the data that you were able to bring back from it?

M Generally, the community that got to hear the data and see the first results was just startled by the tremendous quantity of data. It had many many more pulses per unit of time than anyone ever expected to see. The quarter inch magnetic tape record that we had was rather fragile and couldn't stand any abuse. We had to treat it very carefully to preserve the time information that was on it. In order to get it into some kind of a computer system for processing, it had to be digitized -- it was all analog.

[redacted] at the Lab and Bruce Wald, who worked for him then, were responsible for digitizing the data in a kind of a manual way. They put it through an audio spectrograph filter, then on to a wet paper-tape record for recording. Then with a large Gerber analog device, they were able to digitize the values of PRF that were displayed by the distribution of pulses across the width of the tape and the scan rate as that was displayed along the length of the paper record. They did this manually. They took the paper -- long rolls of it -- and ran it across the Gerber machine in what we referred to as "Operation Eye Strain." It was an extremely tedious process. NSA sent servicemen here to the Laboratory. I think Dr. Wald recently referred to one of them by name. This man was capable of doing that kind of a procedure hour after hour, all day long. Everyone else would go crazy.

A Let me try to get a sense of the magnitude of this problem. Now, we were talking about 22 passes of information. Was that all collected and sent back before any analysis was attempted?

M No, because there was only a 90 day period where we had collection opportunity. Those 22 passes were collected at several sites for each pass. In other words, each instant in time was probably recorded by more than one site. So we had duplication. We had dual records in many cases, one record from site A, another from site B. A certain pass, say pass number 698, might cross [redacted] and have four collection sites all used on portions of that one pass. We would try to make a composite -- a single best record of the contribution of each of those four sites on this one trip across the Soviet Union.

Once they had that, then they would try to digitize it. We would send the digital record into the computer at NSA and they would try to reconstruct bursts -- single illuminations as the radar would sweep by. They would try to associate all those pulses of a burst with a single emitter. Then they would try to find that emitter later on in the data stream for the next scan, and the next scan, and so on for the next scan. They would try to plot the signals by varying the speed of the paper to show the scan lines as being parallel across the paper -- in a diagonal fashion but parallel. The parallels indicated pulses from a single emitter. You could measure the distance between the parallel lines and say, "That's the scan rate". They would try to measure the PRF in a similar way. To me, it was "black magic" what [redacted] Wald, Don Christman, and these service men from NSA were doing to get it into digital form. It was extremely demanding.

A There were two aspects involved then, one is the difficulty of actually doing the operation and the other was handling the quantity of data. You are saying that both were extraordinary. The actual process of changing these fragile magnetic tapes into usable data was by itself difficult, and it was compounded by the raw quantity of information, which had to be analyzed by hand.

M Yes, they were accustomed to looking 50 or 200 miles into the Soviet Union from some vantage point from the ground or from an airplane. Here, suddenly, they were able to see 3000, 3500, 4000 miles and the magnitude, just the massive quantity of signals that were available in one short instant in time was staggering. They just were unprepared and the response was a continuous cry of, "Turn the gain down!". It was one that we were forced to contend with mission after mission after mission. The gain was set to get only main lobes but, nevertheless, they were just inundated with millions and millions of pulses.

The response in the community to this new space program was varied. The Secretary of State, ~~John Foster~~ Allen Dulles, remarked upon hearing that we were operating and collecting data from the deep heartland of the Soviet Union, "...what a remarkable coincidence to have lost the U2 and in less than two months to have in the wings, an alternate system."

A Actually, it could do much more, because the U2 couldn't fly across the heartland.

M No, but it could fly out of [redacted] up into Norway, diagonally across. But from the altitudes of 80,000 feet, they could not see nearly as far as we could. So after being denied the occasional look provided by the U2, they were given this program, which could, 14 or 15 times a day, get a good look at the interior, in one single piece of the radar spectrum.

<u>Side 7</u>　　　　　　　　　　　　　　　　　　　　　　<u>4 June 1981</u>

A　This was the first ELINT satellite the United States put in orbit wasn't it?

M　It was the first successful one.

A　Had attempts been made before?

M　The Air Force program had had several flight attempts, but [had] not successfully recovered a package.

A　Mission 117?

M　Yes, I think that is what it was called. Their first success was August 196Ø, following our June success. They did recover a capsule and also got ELINT data out of it.

A　But this was the first sucessful SIGINT mission the United States had put into orbit?

M　Yes.

A　How was NRL's reputation affected by this success?

M　The opportunity for me to make any judgement on that is rather minor because I was not in the main stream of the Lab prior to this program. I was doing a small job way down in the organization. I had little standard for comparison. I know the intelligence community members that dealt with us on this program were elated by the opportunity of something that would work as well as this did right off the bat. [redacted] particularly, became a lifelong advocate of space surveillance, mainly because of our success in the first and succeeding missions. It was a step by step process.

A What was Lorenzen's feeling? Did you get any feedback from him on how this success was affecting his status and his position?

M I know he was very proud of it, personally. I know also that the cautions that President Eisenhower and ADM Frost had imposed on the community, relative to the classification and sensitivity of the mission, made him keep things extremely close to his vest. There was no attempt to derive any personal glory out of it. We were admonished to be extremely careful, to keep a low profile and to keep out of sight relative to space activities. The engineering team responsible for the collection site, and the military ELINT subsystem in the spacecraft, were organizationally a unit in a large Direction Finding section, where the section head was not even fully cleared in the security of the program. The Branch Head ran the whole show and kept this whole thing way buried down deep in the organization with minor visibility. Only at times of stress, or when we were running out of money, or at some kind of a crunch would we have to go forward to Mr. Lorenzen's boss, Dr. Cleeton, or Dr. Schooley, or to Dr.

Page and solicit their assistance in getting over some kind of a hurdle. So, we had little visibility within the Lab. in general, and rarely was there visibility in the senior heirarchy.

A So part of the muted response was due to the fact that it was so closely held that very few people could stand up and cheer.

M That's true. When the launch was announced, it was a solar radiation launch, not an ELINT satellite launch. Dr. Friedman took his bows properly. They had a very creditable program and were doing a fine job. It turned out, though, that it wasn't totally their funding which supported their effort. Initially, they rode on the funding from our program as a coop job out of my shop. They were gracious about it, but we took no credit at all in the public eye for what had happened. The satellite was a scientific satellite for monitoring solar radiation. It probably caused a much greater stir in the scientific community than it did in the intelligence community.

A That was because this was the first time anyone had seen the sun for sustained periods of time outside the atmosphere.

M It was a good combined effort of both the intelligence and scientific teams.

A Let us go on to the second launch [29 June 1961]. Was the payload significantly different from what had been used earlier?

M It had the same technology [but there were differences.] The filters, particularly, covered not just one piece of the spectrum but two. As I mentioned earlier, I think it was 55Ø to 65Ø megacycles or something like that -- and a second band from 82Ø to 92Ø megacycles. Signals from the lower piece of the spectrum came out of the transmitter as narrow pulses. Those signals intercepted in the higher part of the spectrum came out as wide pulses. Or vice versa, I am not sure which. At least they were pulse width coded so that we could unscramble on the ground, which frequency band each of the pulses came from. Now, if they were simultaneous, the wide pulse would, of course, obscure the narrow one and we would lose the identity.

Figure 21 shows the three-satellite complex of our second successful launch. By the way, there was a November 196Ø launch failure, where the booster stopped burning prematurely. The empty booster landed in Cuba, which caused a great deal of consternation! Every flight after that flying out of [Cape] Canaveral was different. We did not want to overfly Cuba again. We had to exercise a "dog-leg" and avoid Cuba. Ultimately we transferred our lanuch activities from the Cape to fly out of the west coast complex.

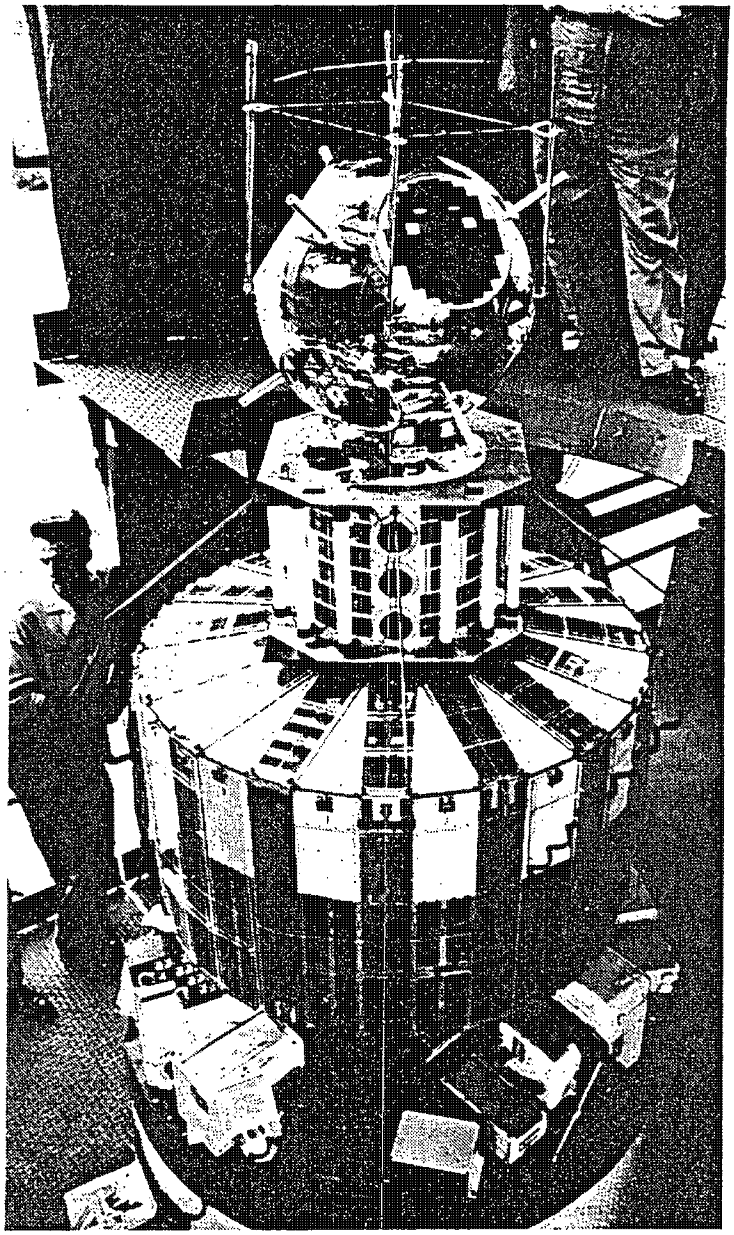


Figure 21

The second successful flight, called DYNO 2, flew on 29 June 1961. There were three satellites, one on top of the other like a three-dip ice cream cone -- with the lower one being TRANSIT 4-B, I believe. It was cylindrical, not spherical, like the first TRANSIT was. The spacecraft in the center was Dr. James Van Allen's INJUN experiment. Figure 21 shows that [satellite] with what appear to be neon bulbs around the center. Those are some kind of signal excitation test jig. (I don't believe that it flew that way because the solar cells ran behind and it would obscure the solar cells. This was a test set up where he plugged those things in and ran it to calibrate his system. He took them off for flight.) Ours was the top spacecraft, again 20 inches in diameter, and it was known as GRAB II or DYNO 2. Notice that what we referred to in the first launch as "skin thermisters", are now five or six inches long. They are also called "skin thermisters" -- as a cover story for our ELINT antenna. The solar radiation sensors are still mounted in the equator.

Both DYNO 2 and DYNO 1 were free to tumble in space. They were not attitude controlled. The solar radiation community was very happy when their sensors were looking at the sun. The intelligence community was very happy when their omnidirectional pattern picked up the signal. Whether the satellite was looking at the earth or the sun didn't really matter to us. However, in future development, we wanted to concentrate on directing our antennas at the earth, and the solar radiation community wanted to concentrate on aiming their sensors toward the sun. So, a very severe incompatibility developed. It was not surprising that this DYNO 2 was the last successful ride that the solar radiation and the ELINT collectors shared. There was one more attempt to share a ride, but it also resulted in a launch failure. The two programs then split, each one in their own right developed their own sponsorship, schedule, and flight opportunities.

A As I remember, the second mission was not completely successful from the point of view of the orbit configuration.

M Well, this one had a fairly decent orbit. The problem was that the Lab. had, in innocence (or ignorance perhaps,) assigned the responsibility for engineeering the separation mechanism to the TRANSIT program. We tested the mechanism several times, and it worked the way it should, but it was subject to human failure. The TRANSIT team, through mechanical failure or inadvertant error, separated the bolts between their package and Dr. Van Allen's first. That released the two birds above TRANSIT and ended any opportunity after that of ever getting them to come apart.

A So yours was supposed to separate first?

M First, yes, then, Van Allen's. But they just avoided the first step. So, we were fastened to Van Allen's spacecraft for the life of the mission. As it turned out, he [being] the gentleman that he is, we were able to negotiate using our bird on the odd days of the month and he got to use his [bird] on the even days of the month. It was an alternate scheme that we worked out. If he had some target of opportunity that he wanted to look at on one our days, he would negotiate with us in advance and get two or more days in a row. Later on, he would give up one or more of his days. We were not too concerned, because we were still under direction to exercise extreme caution in operations as a result of the U2 disaster. The overflight issues still made us somewhat uneasy, so we were not using the spacecraft mission on every orbit possible. We used it every second time, or third time. We were still conscious of the threat of this thing becoming a political issue in the international community.

A Was the concern that the Soviets would pick up some of your data transmission and realize that they were being broadcast to the world?

M Yes, that was a threat. Dr. Wald wrote two or three reports during this period of time explaining the probability for an uninformed enemy to detect this signal from a satellite. Later on we had a very elliptical flight during which we were able to task our ELINT community to try to pick up, "the voice of one of our ELINT satellites" using our standard "garden variety" ELINT positions. It [redacted] was unable to get the signal. However, the little site up in [redacted] whenever the satellite was lower than 300-350 miles in altitude, could intercept it. Now, the satellite was designed to fly at 500 miles. [redacted] couldn't get it when it flew at 500 with their standard garden variety gear, but they could get it when it was considerably lower than design altidude. Based on that, the National Reconnaissance Office (NRO), did ultimately allow us to go on unencrypted.

A Your information went on to prove, then, that the fear of detection was unjustified in the early days.

M At that point in time it was not justified. Later on it was found that the signal could be intercepted, as the technology of the classic ELINT system got better and better and as experience in picking up satelliates got better and better. We were vulnerable, and ultimately we had to encrypt.

A Did this second mission also give very full results, in terms of the quantity of data? Or did you "turn the gain down?"

M There was an interesting aspect to this. I think in the lower spectrum -- 550 to 650 region -- there were really only four emitter families that we were seeing. One was an Air Force emmitter that was located along the Arctic Circle in Greenland, three or four installations there, an FPS -- something or other. We were seeing these. There was also the [redacted] There was a Polish NYSA-C. [redacted], but I don't remember what it was called. [redacted] Polish and [US] Air Force were the families. Gradually, we found that in this lower spectrum, each of these families disappeared one after the other into the data. We reasoned that we were losing sensitivity in the spacecraft. The last signal that we got out of that collection experiment was the NYSA-C, the Polish radar. We felt that it must be a fluke, that it had to be the Air Force radar [because] they had all kinds of fancy things on it. It must be the finest and stronggest radar! Well, later on, in the years to follow, we did engineer a parametric measurement experiment to monitor the incoming signal amplitude. Then we <u>could</u> measure the level of the signal seen by the spacecraft. When we chose the spectrum on which to place this experiment, we chose this particular DYNO 2 band to solve that question seen in 1961, "Was the earlier result truly a manifestation of relative peak effective radiated power from the four families or not?" It turned out it was. The NYSA-C had significantly more power than the other emitters in the band! So in 1961 we had had a comparison of a peak effective radiated power but hadn't really believed it. We thought it was a fluke. We did not prove it for six years or so.

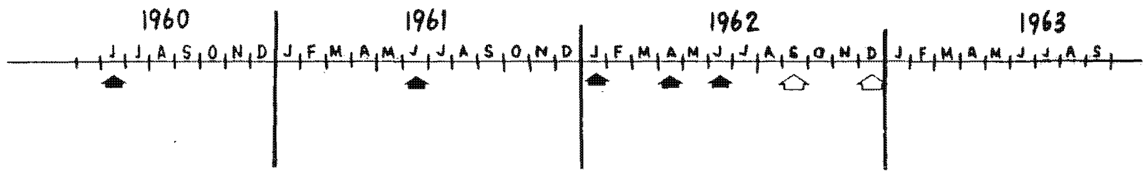
You will notice in Figure 21, that the turnstyle antennas of this DYNO bird are raised up on top and are tied together vertically with a string. The string goes through a string cutter -- a power-driven chisel -- and upon separation the string is cut, and the antennas are allowed, under spring tension, to fly down and lock in the horizontal turnstyle position. That's a VANGUARD invention, and it worked beautifully for us. [redacted]

Looking at the launch schedule, (Figure 22) for the first several missions -- DYNO 1 is shown in June 1960, DYNO 2 in June 1961. There was a failure of a solar radiation mission, I believe, in November 1960. That's the one that killed the "Cuban Cow". [We talked about that earlier]. Then there were two other failures in a row for the ELINT military mission [after the second success]. One was a composite satellite, where we tried to fly four or five spacecraft on the front end of one booster rocket.

A Were these all ELINT spacecraft?

M [redacted]

LAUNCH SCHEDULE



PAYLOAD DESIGN OBJECTIVE - 1 YEAR LIFE

Figure 22

~~TOP SECRET~~
HANDLE VIA BYEMAN CONTROL SYSTEMS ONLY

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A What was the reaction at NRL to the several rocket failures that you had? You must have been extremely frustrated and unhappy.

M Yes, we were. The thing that I recall is on the BLUE SCOUT flight out of Point Arguello on the west coast. I was watching the doppler indicator as it was connected with the flight profile. They had a chart plotted and with little dots going along showing where you were by virtue of your doppler measurement. The chart should track what you wanted to follow. We were right on track for something like 182 seconds. Suddenly, [there was] no data. That was the first indication I had of trouble. I thought someone had pulled the power plug! Something was wrong, there was no data! What I didn't know was that [for] 91 of those 182 seconds the booster was going up, and [for] the other 91 [seconds], it was coming down. The rocket had lost the attitude control mass -- the hydrogen peroxide, which had been improperly loaded.

There was a very clumsy process of loading it. To tell when they had the tank full, they would look at the out gassing vent from a tank through binoculars from a side of a hill. It would outgas when it was only partly full! But, when they saw fumes coming out, they said, "That's it! That's enough, it's full, don't run it over." Well, it actually had just started to go in the spacecraft, so they only had 91 seconds of attitude control during the powered part of flight. Then, the rocket pitched over and went right into the ocean. It was a very clumsy exercise.

We were seen by the launch community on the West Coast as a maverick. We were unsophisticated, and an unimportant perturbation on their "important work." They were doing the Air Force programs and we using their AEGENA, in our later flights, at just a fraction of its full capability -- we just needed it to burn twice. The AEGENA is a very sophisticated device and for the Air Force flights, they used many of the features that were just not required for us.

A Was that true in the early period as well?

M Not on the east coast, at [Cape] Canaveral. The launch flight down there was given as high a priority as anything because we were riding the TRANSIT. TRANSIT's priority prevailed. When we got out on the West Coast, it was Program C. Not Program A which was the Air Force, and the difference was very distinct.

A Is that where some of the animosity between the Air Force and the Navy got started?

M I wouldn't say it got started there. It was extended there. Point Arguello, when we first went out there, was a Navy base. We did not have to concern the Air Force with our testing. We did not have to ask the Air Force Range Safety, or the Air Force this or the Air Force that, if we could radiate or what not. It was a Navy base! There were some incompatabilities. The Lab. was unique in going out there and saying, "To hell with the Air Force, this is a Navy program. Let them go off and peddle their own papers, they have no authority over this program at all." But, the next time we went back, they had taken over the test range clear out to Kwajalein! The Air Force had walked off with the whole act!

We suffered from the competition with the Air Force. But we also enjoyed their successes, as members of the intelligence community, just as they enjoyed ours. They used our program. SAC [Strategic Air Command] for instance, was one of our strongest advocates. They used the the data from the early DYNO satellites. It was vastly superior to anything they had from <u>any</u> source for plotting the in-flight penetrations into the Soviet Union for their SAC missions! They were analyzing our data, somewhat in competition with NSA. They were using it for their own purposes because it was better than anything they had. So, it wasn't all light blue being antagonistic to us. There were some rice bowl issues, but SAC was perfectly happy to use our data when <u>it</u> was best, and perfectly happy to use the Air Force data when <u>it</u> was best.

In Figure 23, we have a summary of the results of the first two DYNO missions. It shows intercepts made, locations found radar site, locations confirmed, and new ones, and V-beam geolocations. This is kind of a box score of the "goodies" that were achieved by the final analysis of the data of these two missions. The first DYNO 1 lasted 90 days, with 22 tasked missions, and the second one, I think, lasted several months -- 9 or 14 months, something like that.

RESULTS . . . . . .

- INTERCEPTS READOUT:

TOKEN	118	ROCK CAKE	2
[redacted]	62	[redacted]	2
STONE CAKE	29	FRIENDLY	14
[redacted]	12	SINGLE HITS	373

TOTAL - 612

- INTERCEPTS LOCATED (THUS FAR) — 42
- SITES LOCATED — 35
  - CONFIRMED — 23
  - NEW — 12
- ACCURACY "V" BEAM (GCI) 25-50 NM - [redacted] - ?

Figure 23

A The length of time was 14 months.

M Fourteen months then for DYNO 2. One of the unique things was the ability to do geolocation with the V-beam radar. This was no a design goal in the concept of the program, but an opportunity that developed after we had data. We were able to capitalize on it. We did not get a tremendously imposing set of results, but certainly we had a good beginning with better things to come.

A How long did it take you to analyze all the data that had come back from the mission? Was it a year or a matter of months?

M I think in the sales pitch somewhere, someone very ambitiously indicated that we could anlayze it in real time. No, it was much longer than one to one. If the pass took fifteen minutes, you might spend a week analyzing the data. It was an empirical process in the beginning. We learned, quickly, things that wouldn't work and didn't try them again. Those things that did work were time consuming and laborious, but doable. I would think the data on DYNO 1 was still being examined and re-examined eight or nine months after the mission stopped being tasked. It was really loaded with more information than we ever expected. Since DYNO 2 had two different pieces of the spectrum and lasted over a year, they could be a little more selective. They were seeing height finders in the L-band, (those two pieces of the spectrum being under 1000 megacycles.) There didn't seem to be near the variety that we had expected. In DYNO 1, for instance, we expected to see the GAGE radar. That was one of the threat signals that we used in the sales briefing. The GAGE radar was seen regularly from the windows from the Embassy in Moscow and thought to be widely deployed. But, in the analysis of our data, the <u>only</u> place we saw the GAGE was in Moscow. It was not deployed elsewhere. There were other radar emitters -- TOKEN was much more widely deployed than we expected. In fact, one unique TOKEN was, some years later, hooked up to a 6Ø cycle power supply that the Koreans had captured from us. They put a TOKEN radar on it and ran the radar at six/fifths its design (it was designed to be 5Ø cycle, they used our 6Ø cycle power supply.) So, it had a six/fifths PRF and a six/fifths scan. It ran faster than the normal ones having a 5Ø cycle supply. This was a unique TOKEN. It was a calibration benchmark that we used and that NSA used, in the early part of the program in trying to work with the signal repeatedly and improve the process for geolocation. It turned out the TOKEN was on an island, [redacted] It made the geolocation very precise. We knew where the island was and most of the locations that came out of the computer were in the water near the island.

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A The success of your program must have had some impact on how easy it was to get money. Did it?

M Yes, indeed, once we had a bird that was up there and working. Through the flight of DYNO 1, we had spent, as I mentioned, $1.1 million. As we turned to the community to seek funds for the next development, three million was forthcoming. Just like that!

A From whom did this come, ARPA?

M It came from the TRANSIT program, ARPA, and the Navy. It was a shared program. Three million was the initial [sum] going in. I do not know the exact numbers going in off the top of my head, but whenever we needed money, it was available. Money and priority. The fact that the first one worked really reduced that whole [fiscal] worry completely into nothing.

If we look at the other changes between DYNO 1 and DYNO 2, we found, in an engineering sense, that some compromises that were made for DYNO 1 were no longer necessary for DYNO 2. We [had] used a very cheap, home quality, tape recorder for DYNO 1. For DYNO 2, we had a high quality unit that was very gentle -- I emphasize gentle because you had to treat that tape extremely carefully to keep from stretching it. We had a big instrumentation tape recorder installed in each of the huts.

A standard operating procedure was developed to adjust those recorders so that the tapes made in all the sites, on a world wide basis, came back in remarkably uniform quality. This was not the case in DYNO 1. In DYNO 1, every tape was unique, every operator did it differently; we did not have a standard operating procedure. But a very capable Naval Security Group officer came in and wrote the Standard Operating Procedure for the program during the time between DYNO 1 and DYNO 2. He wrote it up and it was promptly classified where he could not read it (laughter). It was done very well and served us for years.

We changed the timing system. We changed a thousand-dollar oscillator and digital time display that was a vacuum tube unit into a solid state unit, which cost just about the same amount but was more reliable and accurate.

Remember that in these huts, the antenna only trained in azimuth. It did not move in elevation. So, you could not collect data when the spacecraft was overhead. Every one of the collection sites, in this period of time, had a "holiday" overhead when the satellite would pass above them. You could not record from it then. The choice of this design was a compromise -- we did not want it to become apparent that we were tracking satellites. At the site [redacted], for instance, [the local officials] did not want us to park our antennas looking south toward their capital city. They felt the antennas were a threat to [redacted] They wanted us to look toward the east, towards their enemy India! If those "rockets" went off, or whatever was going to fire from that hut -- rays, rockets, bullets -- they would go toward their enemy not toward their capital city! So, we had to change our standard operating procedure [redacted] and to park the antennas always looking east. You never knew what the local culture and community was going to impose on the program. We did not contemplate that one at all.

A The third major change that took place in this time frame was that the Army lost a site. Is that true?

M The Army and Air Force shared the responsibility for the collection site at [redacted] up on the outer most reaches of the [redacted] They had co-existed up there, jointly on a mission where they had a large radar. It was supposed to be looking for missiles coming out of the Soviet Union and heading toward our homeland. The radar had such a high power -- on power lines and everywhere -- that we were reading something like 4Ø volts of radar signal on a 11Ø volt power line. We could not make decent recordings there at all. Because this was a combined Army and Air Force installation, they shared the commands -- six months for each. When we moved out of [redacted] the Army lost their only opportunity to contribute toward and to benefit from the experience in this program. The other services could bring their servicemen to get experience in ELINT, they could observe in a few minutes with the pass of one of these satellites, those signals that are classics -- signals they probably wouldn't see in a lifetime! They could observe it just a few minutes of our data, all kinds of signals that they would never see from a classic ELINT collection site. They needed that kind of experience. They needed to have a cadre of people knowledgeable in ELINT and students of that art. This was a training ground for it.

A So they really had a significant loss.

M They felt handicapped, and in retrospect, we were sympathetic and at the earliest opportunity brought them back into the program. Some years later, they picked up a site when the Navy moved out of [redacted] The Army did get back into the program at the first opportunity.

A I think that gives a pretty good summary of the first several missions.

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A We will be continuing here the history of the POPPY reconnaissance satellite system. This morning we are going to be talking about the change of the structure of the program once the National Reconnaissance Office (NRO) was formed. Maybe we should just begin by talking a little bit about the background for the formation of the NRO.

M As far as I am aware, there had been a lot of official dialogue in the Pentagon relative to this National Reconnaissance Office being formed. As it happened, almost the total emphasis was Air Force. Dr. Charyk was selected to head it up, and a Major Istvan was his Chief of Staff. The Air Force programs were the 989; the multi-group, interferometer program; and several others with which I am not familiar. They were doing their job to satisfy the national intelligence requirements. Evidently, Dr. Charyk had to make some kind of a report in late 1962 to Congress relative to the strides that they had made in establishing their organization. It was at this time that his testimony implied that the Air Force had the only competent, capable program. This was challenged from two points of view. One came from SAC (Strategic Air Command), who had been using the data from the Navy satellite program -- and using it very well -- to plan their strikes into the interior of the Soviet Union. They had gotten the virgin tape and were able to do certain analytic things with it to suit their needs and were very pleased with it. Their testimony, evidently, was very strong in defense of a continuation of this Navy satellite program.

The second person who came to our defense was from Adm. Frost, who was DIRNSA (Director, National Security Agency). He had moved, from being Director of ONI (Office of Naval Intelligence), to NSA to head that organization. He had intimate knowledge of the program and was very articulate. He spoke strongly in defense of continuing [the program] as a component of the National Reconnaissance Program. As a result of these two points of view, Dr. Charyk did agree that the Navy would be a component of the National Reconnaissance Program and that whenever the Navy had a spacecraft to launch they would give us a ride and support it financially.

A To your recollection, this debate took place in Congress, in front of some Congressional committee, and there really was a serious threat that the Air Force would become the whole National Reconnaissance Program, with Navy being squeezed out entirely.

M Exactly.

A Were you kept appraised of this debate as it went on, or was the whole formation of NRO -- the Air Force power play, if we can call it that -- something that you only became informed about later here at NRL?

M As I recall, we had contact with members of the intelligence community in the USIB committees. They would relay the elements of the power struggle to Lorenzen, who, in turn, would inform myself and my staff. It was a rumor more than actual printed material being presented. We did not have anyone from the Navy satellite team present in the struggle. Adm. Frost, of course, being a previous director of our Program, might have given information to his relief, Adm. Lorance. I don't really know. The information came down to me from Mr. Lorenzen, but I am not really sure of his source.

A Another thing in the administrative area that we want to talk about in this period was the implementation of the BYEMAN security system. Do you want to say a word about why that was implemented?

M From my point of view, which may be incomplete, the BYEMAN system was formed by the CIA on the occasion of two employees from NSA, Martin and Mitchell, defecting to the Soviet Union. I do recall that this hit very hard, because they were extremely well informed. When they defected, Adm. Frost, Director of NSA, took a great deal of heat. Of course, we had known him for several years in his earlier role at ONI in the program. We felt very close to him and suffered with him this particular agony. He took the brunt of the official criticism in the defense of the how this could happen -- how two employees could leave and carry with them all the information they had gathered for years. The BYEMAN system did result, as I recall, from that incident.

A What effect did this have on your program?

M Oh, tremendous! We had been TOP SECRET, limited distribution, "code word", program required signed oaths for access. A very minor amount of things went into print. It had been more meetings for verbal information exchange rather than being printed. When printed material would come into the Lab., it had been hand carried in. It was all very "spooky". There had been no evident accountability. However, with the BYEMAN system, everything became stamped in red ink, accountable, inventoried every year. There were all kinds of checks and balances to insure that nothing was left unattended. It was a much more formal system for accountability, really; this was the major result.

A Was there more documentation, or better control of the documentation that you had?

M There was much, much better control. There was also a little bit more [documentation], particularly in the management side of the program, where schedule, budgets, and things like that were concerned. Now, with the NRO being the overseer of the program, and with the BYEMAN system being present, we were forced to do things in writing and under a new and very tight security control. To talk to somebody in another agency, you had to be sure that your clearance was held by the other agency. This meant great difficulty in pre-planning. You could not call a meeting in three hours notice, as was formerly done, it would take a week or ten days. This was to insure that the people you wanted at the meeting, if it was a multi-agency meeting such as this program required, all had their clearances in order. It was a much more strenuous control.

A Working in a classified area on a classified subject like this, as you have for so many years, what reactions do you have to the constraints that it put on you as a scientist and engineer? What is it like to have to deal with material that you can only talk about to a very limited number of people, when many of your friends, collegues, family, and so on can know almost nothing about your work and activities?

M Very early on in the program, of course, we had to get special intelligence COMINT clearances. So, there were already special access caveats in existence for COMINT. It wasn't totally new to us.

A I understand that. I just want to get what your overall reaction was to working with that kind of subject.

M Each person, as they decide whether they want to enter this type of profession where severe security control is imposed, has to decide for himself if he can achieve or work and get proper compensation without being able to tell his family, minister, friends, etc. what he is doing at work. That's just axiomatic, it's the way of life. You accept it or not. If you do not accept it, you are out very quickly. There are enough checks and balances to be sure that those who chafe under this retraint are detected and ushered into other types of work.

A Did this place any particular constraint on you, as an individual?

M Yes, somewhat. But, I think, in the final analysis, it is how you feel about yourself that is important, not how the rest of the uninformed community may feel about you.

A So you feel there has been sufficient encouragement and reward to keep you going.

M Yes I do.

A Let us go on to the next launch of your satellite. The launch took place in December 1962, and was still given an NRO mission number, 7101. It came fairly rapidly after some of your earlier launches. How did you get enough equipment ready to go at this date?

A That was extremely difficult. We had the composite launch in January 1962, a Blue Scout launch out of the west coast in April 1962, and then we had a launch scheduled for December 1962. As you suggest, we had a shortage of components. They were all in the drink! Not quite all of them, but almost. However, the satellite techniques group here at the Laboratory had two young engineers, John Poole and Bob Eisenhauer, who had just joined them as graduate engineers. They were very innovative, creative, young men.

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But they were so junior, in 1962, I don't believe they were even listed in the NRL telephone book! They had just barely arrived here and they were tasked with building a spacecraft model for the museum. Well, with each failure, it became more and more questionable whether their "model" was going to be a museum model or a flight model. As it happened, we converted both museum models to flight units. About that time, the clearances came through and the two men were then ushered into the full knowledge of the program. Subsequently they participated in pre-launch activities out at Vandenberg and Point Arguello.

This was the first time we had the opportunity to fly on an NRO AGENA on top of a THOR booster. The AGENA is an extremely capable, versatile, and highly developed piece of hardware that the NRO used for most of their missions. It was being diverted toward our use in an extremely mundane manner. It was to be used as a second and third stage, on top of a THOR. Not a demanding role at all for such a fine piece of hardware. We just had two satellites sitting side by side, inside the nose fairing. The launch proceeded on schedule. I do not remember exactly the type of quandries we got into during this launch countdown, but there always were some. There were, of course, the normal Air Force vs. Navy conflicts. This was an NRO bird launched from the Air Foce Base at Vandenberg. We had some ground station activity on the Point Arguello Navy side of the road and I remember some difficulty in getting clearance to turn our bird on -- we had to ask permission of the Air Force. There was a lot of clumsy interplay between the two commands during this period.

A What was the general feeling that prevailed? I take it there had been some reluctance even to include the Navy program in the NRO charter, and here is your first launch under that. You say the booster was not really developed for your use, in fact, it did not have to be near as sophisticated as it was. As I remember, AGENA was a Lockheed rocket. What was the feeling of Lockheed, seeing their equipment go for this rather unsophisticated use?

M It was a little hard for us to detect that, really. There seemed to be a nonchalance on the part of the teams who prepared for this launch. If it was an important job, it did not show. That was the bottom line that I got. The AGENA, on this particular instance, was ignited, fired, and thrusted properly but it did not shut off at the point that would have allowed the payload to be boosted out to 500 mile altitude and be restarted. Instead, it burned until it ran out of fuel and that gave it a very elliptical orbit -- something like a 1485 mile apogee by 120 mile perigee. It was a very elliptical orbit.

A It must have been a great disappointment.

M Not really. After the two previous failures the fact that it worked at all allowed us a sigh of relief (laughter). I guess, in retrospect, it gave the community an opportunity to examine what was, from then on, called the POPPY Program. Prior to that it was not POPPY. POPPY started with the NRO. But the POPPY program was examined under a wide range of altitudes, having this elliptical orbit.

The NRO was somewhat concerned about the fact that we were not encrypting the data, that it was not stored and dumped out at a remote safe site. In fact, every pulse was being repeated in near real time. They were worried about it becoming a "public utility." They were worried that anybody who wanted to listen to ELINT, could tune in, and get it. So, they tasked the classic ELINT collection community of this government, throughout the world, to listen. The voice of POPPY was put in as a high priority target, and they waited to see how many people could intercept it. The [redacted] the ocassions when the bird was going by and when it would be sending data. They had all the information they needed, but they never got a smell of the signal -- they did not get a pulse. And one of the other stations, to our knowledge, ever got any data at all from POPPY except the little Navy site [redacted] The only reason they got [data] is that the fellow who wrote the Standard Operating Procedure for the ground collection of POPPY was the Operations Officer at [redacted] He knew exactly how you had to do it. You put a frequency meter on your receiving system, tuned it to the precise frequency that you wanted to listen to, and then aimed the antenna in the direction that you could guarantee the bird was going to appear. You locked everything solid, and put a Chief Master at Arms in position to prevent anybody from changing anything. Sure enough, the POPPY bird would fly through this receiving antenna beam, and with the receivers properly tuned, when the bird was lower than 300-350 nautical miles in altitude, they could hear it at [redacted] And they did on a number of occasions. But, since the design altitude was 500 nautical miles, we argued convincingly to the NRO that we did not need to encrypt and that the classic ELINT community would be unable to get the POPPY bird when we flew at our design altitude. But we did not task it very often in the low altitudes.

A The reason they could not get it at the higher altitudes was that the gain on the receivers wasn't high enough?

M They did not have sensitivity to do it, even though they had the Nems/Clark receivers, the best that money could buy in those days, and a reasonably good antenna. It seemed that an uninformed enemy would have great difficulty in picking POPPY out unless they had some information about it.

The fact that the altitude varied so much and that the satellite processed, gave us a great deal of difficulty in predicting where we were going to be able to see the satellite on the next collection mission. Was it going to be low? Was it going to be high? We had to change the little plotting board overlays that we gave the sites on a regular basis every couple of weeks, because the perigee was shifting, it was getting earlier and earlier. It gave us some problems. But, since this was our third successful launch, we had a good deal of experience under our belt and were able to accommodate most of it.

This launch had one big impact on the ground station. Since there were two spacecraft, we had to build and equip two collection operations at each ground station. As long as the two birds were going by in close proximity, we had to have a second "hut" at each of the ground stations to adequately collect the data from both birds at the same time.

A Did they have the same design as the earlier huts?

M They were a little bit improved. We had a new and very gentle, one-half inch instrumentation type tape recorder in use at that time. It had enough channels on it to record the data from both birds. That gave us no problem. I think we also went to a new time code generator. The huts were basically the same, but there were two of them.

A Were the huts constructed here at NRL, as well?

M Yes. They were instrumented here. Again, the theory was to have a minimum impact on the service cryptologic site where they were to be deployed. The hut was both a shipping container and an operating space. We sent a couple of NRL guys over when the hut arrived there. They would unlock the door, uncrate the equipment, install it, cable it up, unroll the power line and connect it up to municipal power at the site. There was very very minimum impact at the site itself. We were a little sensitive to the voice of POPPY being heard by people near to the hut. So, we kept them somewhat isolated. They were in a locked compound with a big fence around it. No one could get within one hundred yards of the hut for fear they would hear "bleeps" and "blips" from the voice of POPPY escaping through the relatively thin walls of the hut.

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A Did the fact that you had two satellites up at once make much difference in processing the signals?

M Well, it allowed us to collect twice as much of the frequency spectrum as we had been able to do with one spacecraft. We had both wide and a narrow pulse coming down from each transmitter. There were two transmitters in each satellite. So, we could collect eight unique pieces of the spectrum at one time. We tried to cover the radar frequency spectrum generally from one end to the other. Of course, with only eight receiver chunks, we couldn't cover it all at any one time. I think the highest frequency we collected in that mission was 3250 megacycles. The lowest was 165 megacycles.

We had a tiny bit of unintentional overlap several places in the spectrum. This led to an interesting opportunity. One of the President's scientific advisors, Dr. Dick Garwin, was out at NSA reviewing their processing program, when it occurred to him that the eight megacycle overlap which was covered in common between the A and B bird, say from 192-200 megacycles, might allow radar emitters in that range to be intercepted simultaneously by both birds. He suggested that they try to do some kind of an inverse LORAN mathematical analysis. [redacted]

A [So he suggested] taking the type of location used in the LORAN navigation system, and applying it to the satellite system that you had developed.

M Exactly. There were several NSA people at the Presidential Scientific Advisory Review of the program that heard this suggestion. Two of the most innovative ones were [redacted] a British subject who was assigned to NSA for a number of years, and Dr. Getchel. These two NSA men first tried to do this inverse LORAN type geolocation analysis on our data. They were able, after a while, to locate the TALL KING emitter. This emitter was a big, boisterous, high powered, emitter than scanned very slowly. It laid a lot of pulses on our spacecraft and statistically NSA could [redacted] This was the first step in providing precision geolocation from the POPPY program.

A So, you had not really planned this technique at all, it was something of an accident.

M Indeed, a very importanat coincidence.

A Had you given any thought to geolocation using the satellites prior to this time?

M [redacted] was a manifestation of not having a precision way of designing the skirts of the band pass filters more precisely. We would have preferred to have only one megacycle of common coverage. We were not really trying to get commonality of coverage. It was sort of coincidental. But it did lead to one of the most valuable areas of exploitation the program has ever known.

A Once this was up and running, what kind of response did you get from the people that were involved in the NRO and the community? Was it a good response? Did you get more respect, if I could put it that way, than you had when the effort had been made to put it under the NRO initially?

M Well, at first, the thing that came to mind when you asked the questions was that the response we got from the analysts was typical, and it happened every time we launched: "Turn the gain down". They were seeing so much more data from a POPPY pass than they were able to see from any other collector that they had, that it startled them. They were unprepared for the enormous quantity of data that was available. As far as the professional respect in the community was concerned, I think we were referred to as "Munchkins at the work bench at NRL" because we really took the program forward for each launch, from the concept. Generally, before each lauanch we would go out to NSA and give them a "no holds barred" briefing to answer all of their questions about this upcoming launch. We would work very hard at getting a simplified, complete description of the system documentation before launch. The theory was that we would calibrate each spacecraft, as carefully as we possibly could, before the flight. So, if we experienced high voltage, low temperature, or some unusual phenomenon in flight, we would be able to have calibration data in our repository which would accurately show how the performance of the bird would result in this new unexpected condition.

There were very few of us at the Lab doing this, so we became known on a first name basis by the members of the intelligence community and we had a very good rapport with them. If they did not like one particular aspect of the design that we had up there flying, they made it known very quickly and we took their criticism into account. Sometimes we agreed with them, sometimes we did not. Sometimes their criticism was motivated by the fact that signal analysis was just a hard job. But our mission was not to make life easy for the analysts necessarily, our mission was to satisfy the USIB collection requirements.

A Did you have fairly complete freedom in how you designed your spacecraft to meet USIB requirements?

M Yes. We would listen to the counsel of [redacted] particularly, as he would explain to us the evolving threat in the Soviet radar environment. He would have at his office, the very latest information from defectors who would tell about their radar community. He also had information from literature that might indicate a new family of magnetrons. He had all kinds of the very hottest, and latest dope on what the future threat was going to be in the radar world, as you looked across at the Soviet Union. So, we had this one individual, [redacted] who made a consistent effort to give us guidance on how we could do a better job in isolating data from a highly important emitter from some of those that were rather trivial. The key became the architecture of this band-pass filter business -- how we could get a filter that would isolate the signal of greatest interest from all the others of very low interest.

A Let's go on to talk a little bit about the requirements you began to get to identify unknown signals for high priority processing at the ground station. Was this in line with the information that [redacted] passed on to you?

M The question in the days of 7101 and other early NRO missions was how to get an emitter that was thought to be important. For example we might look at the signal acquisition set for a surface to air missile family. [redacted] wanted to isolate that to see if it used the same magnetron that the "garden variety" [redacted] were using all over the Soviet world. You do not do it based only on RF frequency. You can't do it with just a band-pass filter. [redacted]

So you had to isolate it based primarily on the scan characteristics. We became increasingly concerned with the problem of isolating one emitter family from another. At first, the steps involved separation based only on RF frequency -- you do that in the front end of the receiver in the spacecraft. After that is achieved, you may have to distinguish one signal from another by other characteristics: how the antenna is scanned, what the PRF is or time between bursts of pulses, etc. We used help from the analysts, members of the intelligence community, and those that were inputing to the new USIB requirements. We had feedback a number of sources here in the Washington area, and we were on a first name basis with many of the active participants in the intelligence community.

A Did you begin at your ground stations, doing some preliminary analysis after 71Ø1, or did you just continue to send the tapes, without any analysis whatsoever, back to NSA for processing?

M The main part of local site processing did not start until about Mission 71Ø5. Prior to that, there had been an prohibition for on-site analysis. Starting with Mission 71Ø1, it was very clear, "Thou shalt not ever rewind a POPPY tape. You record it and send it back to NSA in virgin form." They did not want it stretched, they did not want it packed improperly on the reel. They wanted it to come right off the recorder, be placed in the shipping carton, assigned a BYEMAN number, wrapped, and sent back to NSA. The less amount of disturbance at the site, the better. The only opportunity which you had to do on-site analysis was during the pass, when the data actually was being received and recorded. There were tools given to the operators to detect signals of high interest at that time. The operator could clearly hear a signal that went, "Brrrrruuuuhhhh", which indicated a conical-scanning, indicative of a gun-laying system. He might indicate in his log (there was a written log that went with each tape), "At this time, suspect conical scan signal was heard." Those logs went back to NSA and were useful in the prioritization of the analysis at NSA. The people at NSA who had to wade through this massive amount of tape that was showing up from the POPPY sites very quickly suggested that maybe the people at the sites could be of great assistance to NSA if they were permitted to scan the tapes at the site. Gradually, the prohibition against doing anything at the site was eased. NSA then became innovative in allowing us new tools, which we would procure and put over there, and new procedures.

NSA regularly visited the sites, and helped build a cadre of very skilled analysts at the site which made the job of NSA much easier and much quicker. NSA was under a great deal of pressure to expedite this extremely difficult and time-consuming analysis job. They wanted to turn it around quicker. Truly, it was an extremely difficult job and I suffered with them.

It did not matter how well they did, there were always hundreds, and hundreds, and hundreds of tapes awaiting them. They were always behind the eight ball. At every one of the steering committee meetings, they would have to take their lumps. It was not a rewarding period for Ft. Meade and that NSA team. Remember --they had to examine all of the pulses on every tape.

One of the first steps that they took in the processing was to digitize each tape. They had a device to do this called AUDICO. On that device, they had an operator who would monitor it and manually adjust the gain of a fixed voltage level slicer. They would note the time when the pulse went above a set voltage level. Of course throughout a pass, the signal would vary in amplitude by virtue of the Faraday rotation in the receiving path. All kinds of phenomena affected it. The operator might be moving the antenna. He might be tuning the receiver. Each change affected the signal amplitude that appeared on a recorded tape. So, the fixed level slicer was a very rough tool. It did not give precise analog to digital conversion without disturbing the time delay.

A Let's go on then and talk a little bit about 7102. As I recall, this came in June 1963, and was the first time you launched three satellites at once. What else is significant about this launch?

M Well, the thing that sticks in my mind is that once again, the AGENA let us down. In 7101, it had burned until it ran out of fuel. In 7102, it shut off at the right velocity, so that the spacecraft went up to the design flight altitude of 500 miles. But, once it got up there, it did not restart as it should have, and burn for a few seconds to circularize the orbit. Instead, we got an orbit that was 500 miles on the apogee and 91 miles on the perigee!

Figure 24 is a view of the National Geographic map that we used as an aid to operators for aiming their collection antenna at each of the sites. We had drawn on these maps, a "site circle diagram", and using another map that was centered about the site, we could plot these lines of constant azimuth. Mission 7101 and Mission 7102 are both shown on this Figure, so it shows the two most elliptical missions that we had in the whole Program. The overlay that we provided to the operators at the site, had to be changed regularly, because perigee was moving from one point on the orbit to another, and to another. It became a little bit laborious to keep the operators informed adequately. Keep in mind, that with the 91 mile perigee, this Mission 7102 did not last very long. It only survived for about 45 days. With the low altitude on the perigee, it was under the influence of the atmospheric drag and that quickly reduced the apogee and perigee. In about a month and a half, the satellite was drawn in and burned up in the atmosphere upon reentry. This caused the evaluation of [Mission] 7102 to be accelerated. It was placed into operational use very very quickly. Whatever data we could get we knew had to be [obtained] within the month and a half lifetime. The low frequency ELINT collection part of the spectrum on this Mission was about 90 megacycles; the high frequency end was up to about 4100 megacycles.

Here we had for the first time, three spacecraft. On 7101, we only had a pair of them, mainly because that was all we could get together in time for a launch. 7102 had three [spacecraft] sitting side by side on the front end of the

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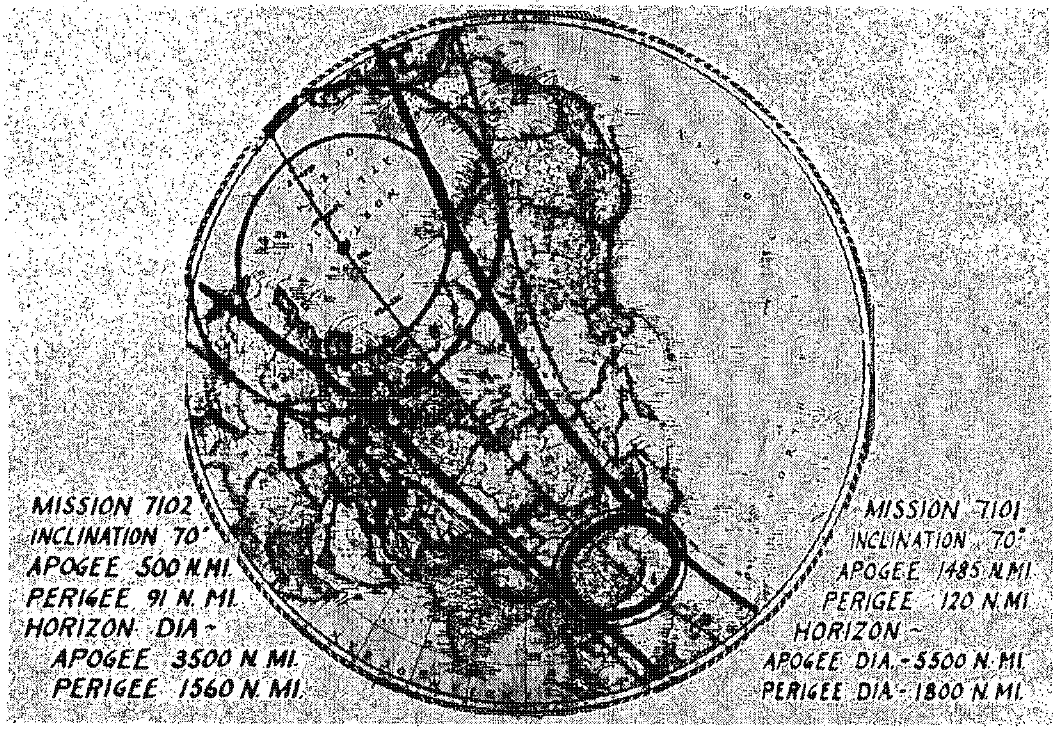

Figure 24

AGENA. This was an attempt to do two things: (1) To more efficiently utilize the capability of the AGENA, and (2) to give a wider spectral coverage in the ELINT mission. The AGENA was actually capable of carrying more than just three, but that was all we used.

In the program, we had started out building small birds like VANGUARD's 20 inch one. The pre-POPPY Navy program had 20 inch birds. These three were similar, except that the ALPHA bird had a 24 inch diameter. It weighed about 85 lbs. All three satellites were small and spherical with the surface mounted solar (recharging) array mounted about the spheres so that the spacecraft could tumble. We had continuous power for recharging the batteries which was independent of spacecraft attitude.

A [redacted]

M [redacted]

A It wasn't clear that it was a really good idea.

M No. It took a while for it to develop.

A You had two launches now in which the AGENA had failed to perform as advertised. This must have been something of an embarrassment to Lockheed. Was it?

M My impression was that it did not bother the people at the launch pad. They were very nonchalant about our mission out there in both of those launches. I am sure that the Director of Program C officially complained. I do not know what form it took, but there was some kind of an adjustment in the attitude. The next time we went out, for Mission 7103, there was much greater attention to detail. They evidently had raised the perceived importance of the Program considerably. The pad chief, who had previously slept in his Cadillac most of the time, was gone. They now had a fellow who was really attending to business. It was obvious to us that the NRO had raised the priority of this Program in launch phase.

Mission 7103, the next one, was extremely successful. It was one of the most circular orbits we ever had. It flew in January 1964, again having three spacecraft on the front end of the AGENA. The upper frequency of use in the ELINT band had grown to about 5100 megacycles. Gradually, we became capable of building adequate sensitive receivers at higher and higher frequency. Mission 7103 had one 24 inch spacecraft, and two 20 inch ones. It was noted, primarily, because one of the three spacecraft had a two-axis gravity gradient stabilization scheme. With this design we could extend a magnetic anchor out about 30-40 feet above the spacecraft on a thin, extendable boom, so that the spacecraft would hang with that boom pointing toward or away from the center of the earth. It consumed no power in the process. This really ushered in a very very important design opportunity to the Program. By capitalizing on the knowledge that the center of the earth was constant relative to thee spacecraft, you could design an ELINT collection system to point straight down at the center of the earth or off toward the horizon. We were still unstable in yaw with this two axis system. In other words, sometimes the spacecraft would be pointing off to the left, sometimes it would move around to the right, sometimes it turned backwards. It was unstable in yaw. But, as long as the horizon of the earth remained predictible, we could do a much much better job in predicting the ELINT system capability before launch. The gravity-gradient system gave us an opportunity to do a much more precise job in the engineering of ELINT experiments.

A You might say a word about the problem of power aboard the spacecraft and how important it was that this stabilization used no spacecraft power.

M Power was the most important parameter in the total design of these spacecraft from the very outset. The average power budget that one had in the DYNO 1 bird, was something less than three-quarters of a watt. If you had a half or five/eights watt drain on the batteries, 24 hours a day, that was about all the power you had available. Now, a small Christmas tree light bulb requires, maybe, eight watts. So, it was a very very small amount of power that we are talking about. The selection of an acceptable electronic subsystem design for these spacecraft was based heavily on power consumption. So, when I emphasize that the first gravity gradient system was used no power from the batteries, it was not a casual reference. It was very very important that we conserve this precious power and not use it for something that was going to take power 24 hours a day, day after day. By the time we had gotten up to Mission 7103, our average power budget had grown, perhaps, to a watt and a-quarter.

A	Is that per hour?

M	On the average, yes. If you drew a watt out of the power supply, 24 hours a day, day after day -- that was the average power budget for the whole spacecraft. The figures are from memory, they may not be precise. The point is that as the solar patches on the exterior surface of the spacecraft got larger, we could put more power into the batteries on the average. The 20 inch spacecraft, spherical in shape, allowed, I think, a 9 inch diameter solar cell patch. The 24 inch diameter spacecraft allowed an 11 inch diameter circular patch for solar cells. Each of these had its unique average power capability. Keep in mind, also, one important fact. The solar patch that I am talking about is made up of little shingles of material. If you shade or cross one of those shingles with a shadow, it opens circuits in the whole string of shingles. So, there was an inherent and very desperate incmpatability between antennas for the ELINT system that stuck outside the satellite and would cast shadows across the solar patches. The solar design engineers were very unforgiving of our ELINT antennas, if they cast shadows on the exterior of the bird. Of course, we also became sensitive to that, so many of our antennas are low profile. A short squat antenna may cover a larger surface area than one would think necessary, but it was not a casual design emphasis.

A The gravity gradient system worked, as I recall, on the magnetic field of the earth, using it to dampen out the oscillating motion of the spacecraft as the magnet moved through the field.

M Actually, the magnet up there that is inside the magnetic anchor, is free to align itself with the earth's field. Initially, the spacecraft is swinging over quite an arc. It may swing 45 or 90 degrees back and forth below the magnetic anchor. The magnetic anchor is surrounded by a magnetic repelling material which keeps it centered in the sphere, and immediately inside that is a sphere of copper. This copper sphere, which is connected through the boom to the spacecraft, is rotated very slowly as the spacecraft swings back and forth. The copper, then, cuts the magnetic lines of force, and resulting in an eddy current damping that takes place -- just a minute, microscopic amount of damping takes place. So, it takes three or four days for the spacecraft to settle down and gradually damp down the oscillating to where the boom hangs pointing toward or away from the center of the earth. There is a preferred attitude. We can, by pulling the boom in a short distance, cause the spacecraft to turn upside down or right side up. Thus you can invert the spacecraft. We have done this with retractable boom systems repeatedly over a number of years. It is very predictible and it is the ultimate technology for controlling the attitude of a low powered spacecraft.

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A Let's talk now about the evolution of the next system, 7104. As background to that, how did the concept of evolving the spacecraft -- the number of birds that you used, the design of each one -- how did that evolution of design philosophy occur?

M Each design was a compromise. From the very beginning, we wanted to cover the entire radar spectrum, but [we] could not. Crytal video receiver technology would not permit us to go above about 3000 megacycles in the very earliest launch. Gradually, that was elevated to 4000, 5000, and so on. By the time 7104 came along, we had a crystal video technology that we developed through ITT in Ft. Wayne, Indiana that would allow us to fly a sensitivity of something approaching -100 dbm at 9500 megacycles. So, for the first time, X-Band became capable of being received on a POPPY type spacecraft.

We also had a problem of power consumption that we have alluded to. You could not operate at one time in the spacecraft, enough ELINT experiments to cover the whole spectrum, or there would not have been power enough to have a transmitter, or power enough to have a command receiver, or power enough to have a telemetry transmitter. Each of these had their own essential part of the power budget.

Gradually, the technology permitted us to orient the spacecraft relative to the earth. We were then able to have a predictible look angle toward the earth, and put more ELINT collecting antennas which would look toward the earth. We could calibrate them better and they were down in the area where they would not obstruct or shadow the solar cells.

When we needed more power, we had to build a larger bird, or a bird that had a larger percentage of its exterior surface covered with the solar cells. The AGENA, at this time, was capable of boosting heavier payloads. But, if you had more than three spacecraft going by a collection site, you could not put the data on one tape. And having two tapes was no good because you could not relate things on two separate tapes. The number of channels on a tape recorder was limited. We were able to use only four channels for data on POPPY tape recorders. The other three tracks were for recording the time, housekeeping functions, verbal annotation, etc. So we had some fairly rigid restraints on our design, but gradually we were able to overcome these in one way or another.

With the stabilization of the third bird in the 71Ø3 launch -- gravity gradient stabilizaiton -- we finally were able to really go up into X-Band. Once you could cover X-Band, and the lower parts of the spectrum, this meant you had to use even bigger frequency steps for each collection band or more steps. The whole philosophy was to design a collection band that embraced one known family of radars. You wanted to optimize against that one threat emitter, if you could. Now, it could happen that a magnetron that is used in this one family of radar is the same magnetron used, for example, for a height finding job in another family, and so it had the same operating frequency. It might be that the same magnetron was also used in an airborne application. You did not find airborne and land radars using the same hardware very often actually, but you would see some things afloat (aboard ship) that were the same types of radar transmitters that were used on shore.

A So the idea was to have one channel for each task in order to make processing easier?

M Exactly. Now, with Mission 7104, which flew in March 1965, we were able to go to four spacecraft for the first time. Each of these had the larger 24 inch diameter spherical design. They weighed in excess of 100 lbs. each. Prior to this, we had never had a bird over 85-88 lbs. We were able to put a two-axis gravity gradient system on one, and, for the first time, an attempted three-axis gravity gradient system on another satellite. This stabilized it in yaw as well as pointing the satellite toward the center of the earth. Because we could now get X-Band with the crystal video technology, we had to fly more collection segments, more discrete collection experiments or bands, if you will. With Mission 7104, the whole ELINT capability of the bird was designed into 32 discrete ELINT subsystems. They covered, without a gap, the frequency range from 155 megacycles to 9500 megacycles for the first time in a single launch.

A That is, really, almost the whole spectrum.

M That was the major spectrum of the Soviet radar in 1965. They had a few things that were known, up above that in the the 12000 to 15000 [megacycle] range and a few in the 35000 range which we could not receive from space in those days. But, with the crystal video receiver now incorporating tunnel-diode RF preamplifiers, we were able to raise our collection sensitivity and thus elevate the RF frequency to embrace X-Band.

The 32 collection subsystems in 7104 really were a handful. You could not operate all 32 at one time. On each bird there was a group of four called the "primary" group, and there was another "alternate" group of four. One group was not more important than the other. So, the 32 bands were divided that way. Now, we did attempt to design some wide dual-coverage for the first time in order to capitalize on the [redacted] opportunity that had been developed at NSA, primarily in the frequency range of the TALL KING emitter. That was 165-184 megacycles. We had [redacted]

A So, this was the [redacted] been a design objective?

M Yes. We had other areas of common coverage, but they were not too important. We had some in the 550-660 megacycle region, some in the 2650-3200 megacycle, and some in the 4100 megacycle region.

Figure 25 shows the four spacecraft ELINT complement for Mission 7104. Blue is for spacecraft A, green for spacecraft B, red for spacecraft C, and tan for D spacecraft. This shows the 32 collection subsystem available for that mission.

Figure 26 gives an artist's concept of what the three axis gravity gradient stabilization system looked like on this mission. It had two tip-masts that extended down and forward, one out to the right and one out to the left. They had weights at the end of them. Much as wire walker uses a long, horizontal pole that bends down at the tips as he walks along the tight wire, these two tip-masts stabilized the spacecraft in yaw. It established two definite points on the spacecraft; one point would be along the flight line, forward and the other point would be along the flight line, toward the aft. This ushered in an opportunity to engineer into the spacecraft a micro-thruster capability to add energy to the orbit, raise it into a higher orbit, move a spacecraft forward, closer to its mate thus parking it in flight. That became a very important design feature later on in the missions that followed 7104. So, the pioneering of the three axis system was important because it allowed us to have knowledge of where, along the periphery of the spacecraft, was going to be the "spot" forward along the flight line. You could then engineer, in that spot, a little micro-thruster which would add or detract energy from the orbit.

A I think we want to talk some about processing. We have

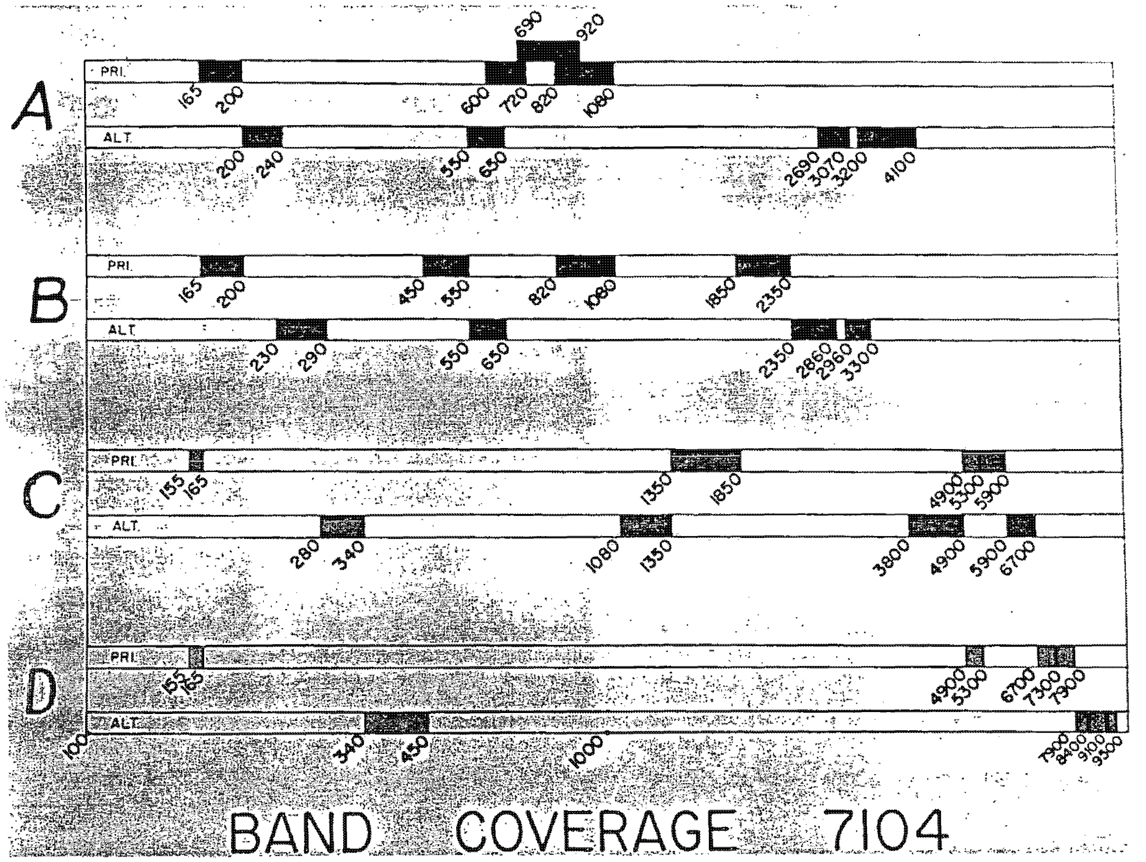

Figure 25

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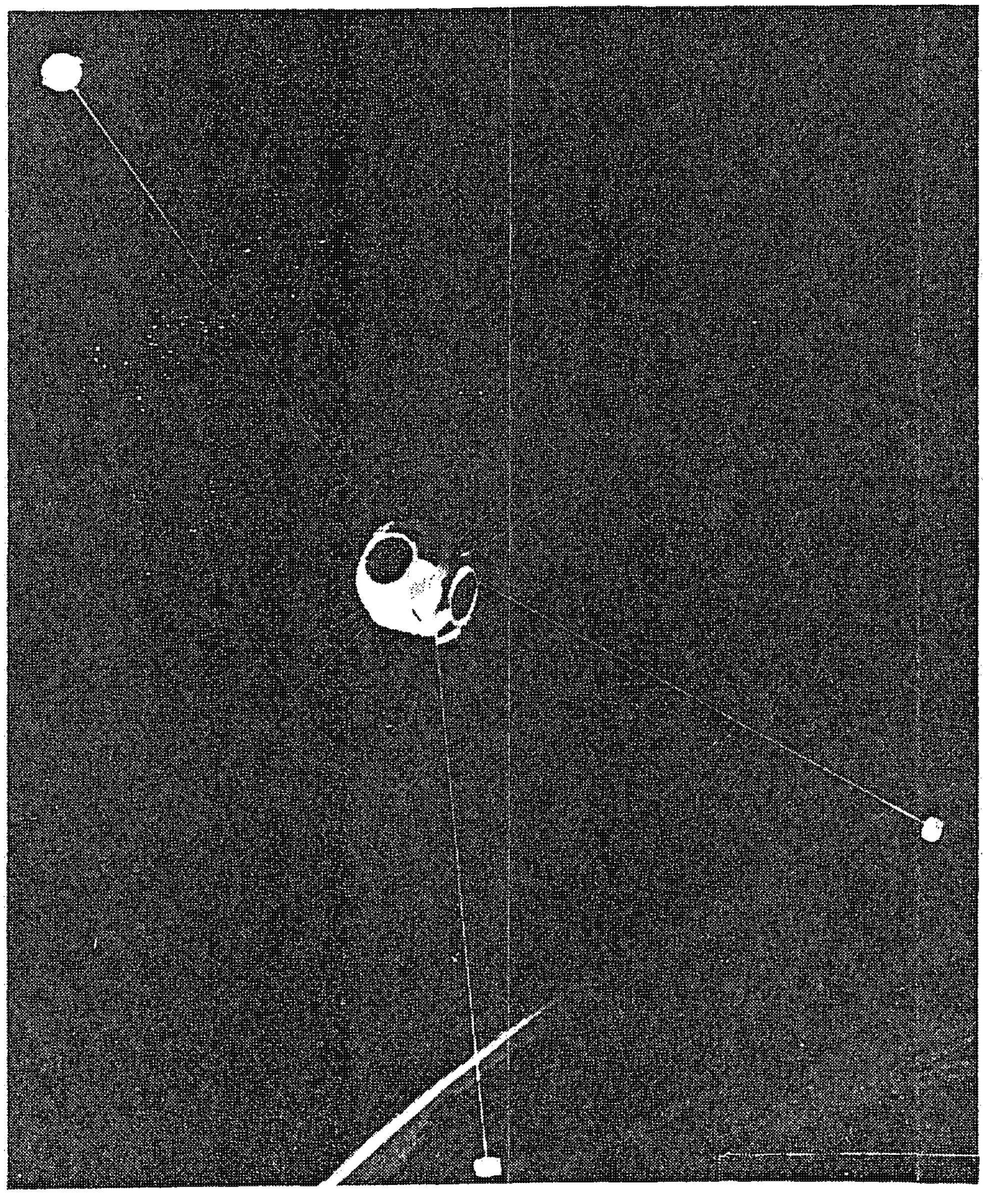

Figure 26

said before that processing of information from the spacecraft was always one of the most important things that paced the utility of the data that you got back. By the time of 7104, what was the status of processing through NSA?

M [redacted]

In Mission 7104, we were able still using the old receivers at the sites. We had not yet designed the new solid state receivers yet. But, we recognized this was a limitation.

It was 1965 when we started the development of solid state linear phase receivers.

A Let me ask a little bit about that development. That was when you brought HRB Singer in as a contractor. How did that occur?

M Within the Mr. Lorenzen's Branch, where this work was being done, a number of very important projects were going on. During our crisis times, he was able to bring in some assistants from other sections in that Branch. But he did not have people that he could put in there and use full time to design a new ground station. We could see the penalty that we were paying by using old communication type receivers to do this pulse reception job. The pulses were coming out with a large variation -- plus or minus 1000 micro-seconds -- in the timing. The variation depended upon who built the receiver and how it was tuned at the instant. It was just atrocious. You just could not maintain any time consistency through that receiver. So, Mr. Lorenzen inquired of the NRO if they would object to us using a contractor in the POPPY team. The answer came back that we could use one for the spacecraft segment or the ground segment, but they advised us not to use the same one in both. They were very happy with NRL having the prime responsibility to cover both of those. But they said we could get assistance if we wanted it. So, Mr. Lorenzen and the seniors at the Lab. agreed upon HRB Singer as the company to come in and help us engineer additional facilities for the ground segment.

A Was that done through a bidding process or was it a sole source action?

M I think they looked across the intelligence community at companies that had clearance -- because BYEMAN was going on -- talent, and a location near a college where they could obtain renewed talent. I am not all that familiar with how HRB was selected.

HRB had won their spurs in World War II in the intelligence community and were well thought of in the 1964-65 time-frame. Dr. Musser was the president of the company and he was well known in the community. The first two individuals that HRB sent down here were Lee Hammarstrom, who is still with us as a Branch Head and Jim O'Connor, who is one of the most innovative and creative people we have ever had on the program. They both converted to NRL civil servant jobs in the early seventies.

The cost to the program for using HRB talent at the Naval Research Laboratory, was reduced tremendously by a favorable "away" overhead rate that HRB would bid in their contracts. A contract person who gave the type performance of a GS-15 civil servant, would cost the program only what a GS-5 technician civil servant would cost. We were getting exceedingly fine talent for a very minor cost. It was a real good bargain.

This arrangement allowed us to have an influx of new ideas and innovative solutions to problems that had been building up in the ground segment for a number of years. The solid state receiver, for example. That was the challenge that brought Mark Van de Walle and Paul Osterling down from State College. They designed and built three of those and had them at the [redacted] in operation, in a period of 90 days. They demonstrated the capability of the new solid state technology. [It was] of tremendous improvement in the reduction of this time variation of the data passing through the receiver. The standard deviation became less than a micro-second through the receiver. The standard deviation of the R-390s was almost impossible to measure -- plus or minus 100 micro-seconds or something.

HRB was not always the "idea" team, but they were the ones that solved the problems in many instances. When we had polarization problems on the data link between the satellite and the ground station, they very quickly assisted us in coming up with a circular polarized antenna on the ground that saved the data from the mission that was already flying. We did get surprised in that one instance. We had been using a linear receiving array, vertically polarized or horizontally, and we had to switch very quickly to the circularized polarized system. [Jim] O'Connor then built a device which would sample the signal when seen through one linear polarization or another, and would switch the recorders in the receiving array at that instant that was having the best signal/noise reception. Thus, we did not have to keep the operators switching back and forth just to see which was the best. This was done automatically in a sampling system that had very few discontinuities in the data, almost none.

A So that was the kind of problem then that HRB really helped you solve. What about the NSA role at this point? Were they still being overwhelmed with data or were they beginning to make some headway in processing the information more rapidly?

M They were being harangued, I know that very definitely. In the steering committee meetings they took their lumps regularly from all elements of the community. I felt that they were being chastised unfairly, because the job was extremely difficult and endless. If they did their job perfectly, every pulse that was received at the ground stations would be analyzed appropriately at NSA. The job was of horrendous magnitude.

[redacted]

We and others from the intelligence community, were incensed that NSA was not giving equal attention to the other emitter families. There were other new, unique emitters. You did not know where the signal originated, but you saw them regularly. Sometimes, they were given a "no-threat" connotation, simply because they were a harmonic of a U.S. crystal. Sometimes, they were given no military significance because they were thought to exist at the end of commercial airport runways. Many of these assumptions were found, later on, to be false. So, what happened was that some of the known emitters in POPPY, and many of the unknowns in POPPY, resulted in later years in being very key emitters for one type of a threat or another. The surface to air missile systems were seen for years in POPPY before their full meaning was understood. It wasn't until Russia exported their SA-4 or SA-6, to Aswan in Egypt, that NSA recognized, that data that we had been seeing from POPPY, was really a tremendous surface to air missile threat.

A I guess in a certain sense, from your standpoint, it was troublesome not to see even further data analysis. But it was easy enough to be self-rightous once you were getting data to come down, because your job was done and you could stand back and point a finger.

M Yes. We were impatient. They were not as successful in solving their very difficult problem as we had been in innovating new features in the design of the spacecraft. The new features in the design were juicy jobs, which we enjoyed. Many of those were brought about by USIB collection requirements. Also attendant with them were corresponding processing requirements, which were very difficult to do.

A It was left up to NSA then. Let us go on.

It was about this time, as I recall, that a new set of requirements came down to concentrate on ABM (Anti-Ballistic Missile) or AES (Anti-Earth Satellite) systems. This was to affect your program fairly profoundly. Do you want to talk about how that came about?

M There were really a couple of things that were happening in late 1965 and throughout 1966, that really brought this into focus. [redacted] at CIA had attempted, courageously, to develop some kind of a cost effective yardstick; a yardstick that would compare the various satellite programs. They struggled with that for about a year. Those struggles, I think, could best be characterized by stating that the participants on his team were all very prejudicial. They all had vested interests. They represented the Air Force program, in many cases, and in the case of Mr. Lorenzen, the Navy Program. Each one wanted to make doubly sure that their particular rice bowl or their program got a fair shake in the development of this (cost effective) yardstick.

A This was an attempt to measure the Navy against the Air Force programs because they were the major competitors.

M I really think that [redacted] view was to try, as a taxpayer, to develop some kind of a yardstick of cost effectiveness that would allow CIA and NRO to spend their money more judiciously. The day of free and easy money was gradually coming to a close. The competition was building. Some of the larger, more expensive programs were being conceived. [redacted] being from CIA, was not party to the Air Force-Navy partisanship. He was separate from that. But the team with which he had to work came mostly from the Air Force side of the house. The effort caused a tremendous drain on the part of my staff because as the Navy would be threatened on one aspect the cost effectiveness yardstick development, we would have to prepare Mr. Lorenzen with the latest argument against that particular aspect and show how he could twist the logic of it to be more advantageous to us. This took about a year and was an extremely demanding period. It did not result in very much, except to accentuate the differences between the various programs.

A Did it build even greater animosity, do you think, between the two services?

M I think it probably did. Prior to [redacted] study, there were some opportunities for the Program A and Program C people to exchange technical ideas. There were a few first name relationships between the two teams and they dried up very quickly after that.

The second thing that took place during this period, is what I refer to as the Harry Davis "Hysterical Review." It might really be called a historical review of the ABM/AES posture of the Soviet Union. How were they prepared to defend themselves against our incoming atomic warhead? That was the bottom line of that study. The Soviets had to have some tools that could tell the decoys from the real warhead. They had to have other tools that were sufficiently precise to detect a warhead at maximum range. The whole ABM threat, anti-ballistic missile threat, if you will, was aimed at determining what the overhead intelligence community could do to build better tools -- collection tools, processing tools -- to isolate and identify, at the earliest possible time, the pieces of Soviet weapons system that they must have to defend themselves against our potential atomic attack. This was a study that took place in the fall of 1966. It culminated with some extremely political briefings at the NRO. It did not look like, for a while, that Mr. Lorenzen would be a party to those briefings. He only got in by some clever manipulation. He got to make a presentation before the NRO community, not by invitation but by skulduggery.

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A ...Nobody was thinking about POPPY.

M POPPY was lost in the noise. Howard couldn't get their attention at all. The solutions to the problem that were available to the POPPY team were so miniscule in cost and potential impact to the program that Harry Davis was not even going to include it in the final presentation to the NRO. I think that Howard was in the hallway or outer office of where this briefing was going to be given when he saw Gen. Stewart, head military officer at the NRO at that time. Stewart had been to our field site locally and he had been to the Lab. several times. He walked by and asked Howard how he was doing and Howard said, "Well, not very well." He said he didn't feel that they wanted the Lab. on their team effort. The General said, "Well, nonsense Howard, you should not feel that way. Come on into the office and talk to me for a minute." So they got out of the more public scene to a private office where Howard then laid out for the General, what the POPPY solution would be.

A He just happened to have it in his pocket (laughter).

M Yes. The General said, "My goodness, has Dr. McMillan heard this?" Howard said, "No, I can't get in to see him. All those industrial types are so busy selling their solution to the problem, no one wants to hear what the one government team member is proposing." The General said, "Come, with me. I have to go in there anyway. We will see if we can get you a hearing." So, on the elbow of General Stewart, Howard marched into the inner sactum where thirty or so of the hungriest sharks in the industrial waters were congested. The General took him right up to the front of the room and introduced him to the Director of the NRO and asked Dr. McMillan if he had heard this presentation. The head of the NRO said, "No, I have not, do you think I should?" The General said, "By all means, I think you ought to hear it right now." So, in front of this antagonistic partisan group, Howard proceeded to tell the Director of the NRO, what POPPY was prepared to do to help them solve their problem, and what it would cost. In essence, it would cost a seven week delay in the launch of 71Ø5, and it would cost $382,ØØØ. The impact would be that the POPPY team would modify the spacecraft so that they had [redacted] opportunity across the ABM spectrum -- 15Ø to 32ØØ megacycles. It meant only three or four changes in the spectrum of the spacecraft.

Figure 27 shows the ELINT complement of the four spacecraft in Mission 71Ø5. It flew on the 31 May 1967. So, we are talking about four or five months before the launch.

A We are getting ahead of the story a little bit. He then

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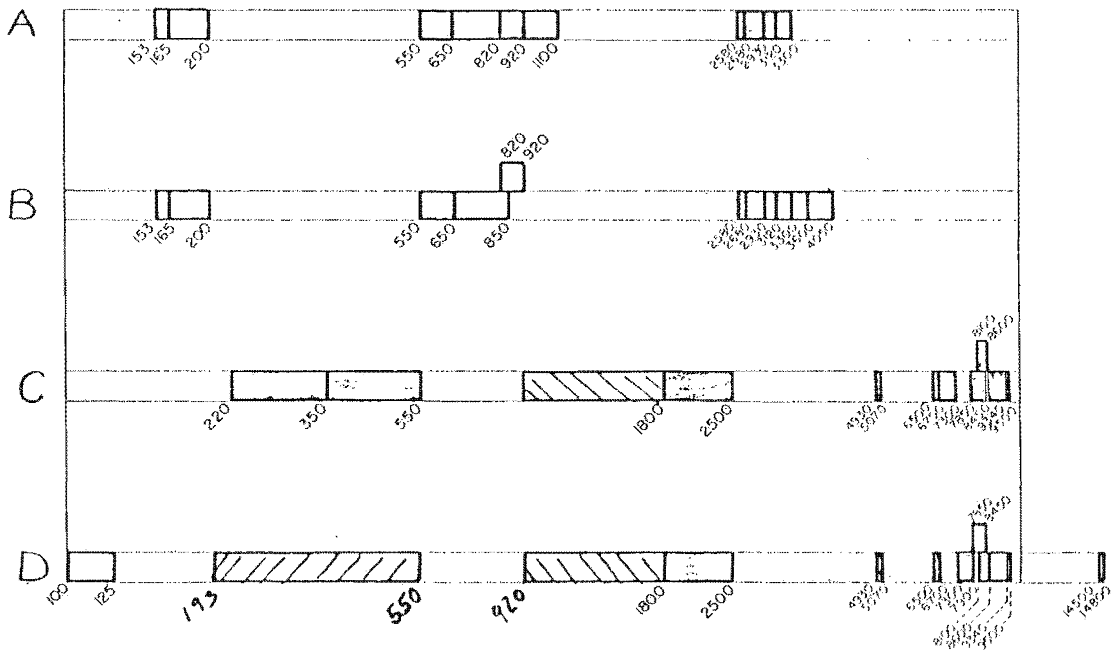


BAND COVERAGE 7105

Figure 27

165a

BYE-58,045-81

got approval to make his modifications on the next POPPY mission?

M He proceeded to discuss, not only the modification to the ELINT part of the mission, but what would be done in the data processing at the POPPY site [redacted] in answer to the major problem of the ABM threat. The three bands that were modified are shown in Cross-Hatch in Figure 27. The mandate that we were seeking was to be able to do an analog to digital (A to D) conversion of the data at the field site to improve the precision of measurement. We also proposed that if we did an A to D conversion, we must have a small computer there so we could assure high quality and no loss of quality throughout the duration of the mission. We would assess every pulse that we received on POPPY Mission 7105 to determine the liklihood of its having emanated from one of the high priority pieces of concrete the USIB judged as ABM sites in the Soviet Union. There were six or seven ABM sites, or what they thought were ABM sites. What they wanted was to find out the signature of the radar family at each of these sites. We would examine each pulse to determine its potential of having emanated from there. That is almost like real time geolocation sorting of our data, which was not a trivial proposal. All this for $382,000! The next smallest item in the industrial proposal was a half-million dollar <u>study</u> somewhere. They did not get down to $382,000, so it was lost in the noise. To get Howard out of the debate, they approved our plan. "Go ahead and do it!" they said. So on 21 December 1966, we got the green light. We got the funding to buy a computer to do an A to D conversion of the data at the site and got it all in there for the flight of Mission 71Ø5. We modified the ELINT spacecraft package at the same time.

A You must have had this briefing pretty well prepared before Lorenzen got to General Stewart.

M Oh, indeed! We had dreamed it up over the five months of the review.

A You had done a lot of thinking about this, it was just a question of getting a hearing.

M Oh, yes!

A Once in, and through this pure effrontery, Howard bulled his way in and got his approval.

M I should not imply that he did it at the expense of the audience there. These industrial tycoons, each got a big program and a big slice of the bucks. It was a mammouth undertaking. As I mentioned, [redacted]

A Was this basically SALT related?

M No. It was all ABM. It was trying to do the most effective job they could to detect how the Soviets were going to protect themselves against our incoming warheads. That is where the emphasis was centered.

Now, Mission 7105 was reconfigured slightly, as I mentioned. We went to the launch site and had our normal amount of problems in preparing a complex system of this type for launch. Three of the spacecraft had the new multi-faced design shown in Figure 28. It shows the four spacecraft on the front of the AGENA. Three of them, B, C, and D, are the multi-faced design and spacecraft A is the old 24 inch diameter spherical spacecraft that had a fairly wide equatorial band to give additional height and volume. It was a power conservative spacecraft. You will notice it has four eleven inch diameter solar panels around the top hemisphere.

Figure 29 is a view of the lower hemisphere of the of the B spacecraft. It is in this lower part of the spacecraft that we mounted all the ELINT subsystems. The numbers in this Figure refer to each of the collection subsystems. The data link modulator is shown in the lower left, where we have the wide/narrow modulator determined.

A Is this Figure a fairly typical representation of what the inside of these spacecraft tended to look like?

M In the POPPY birds, it was characteristic to have the lower half of the spacecraft devoted to the ELINT subsystems. The top half would have the housekeeping, telemetry transmitters, command receivers, memory, flywheels, anhydrous ammonia tank and things like that. The batteries would be in the lower cylindrical support tube in the center of the spacecraft. There is an instrumentation deck that goes

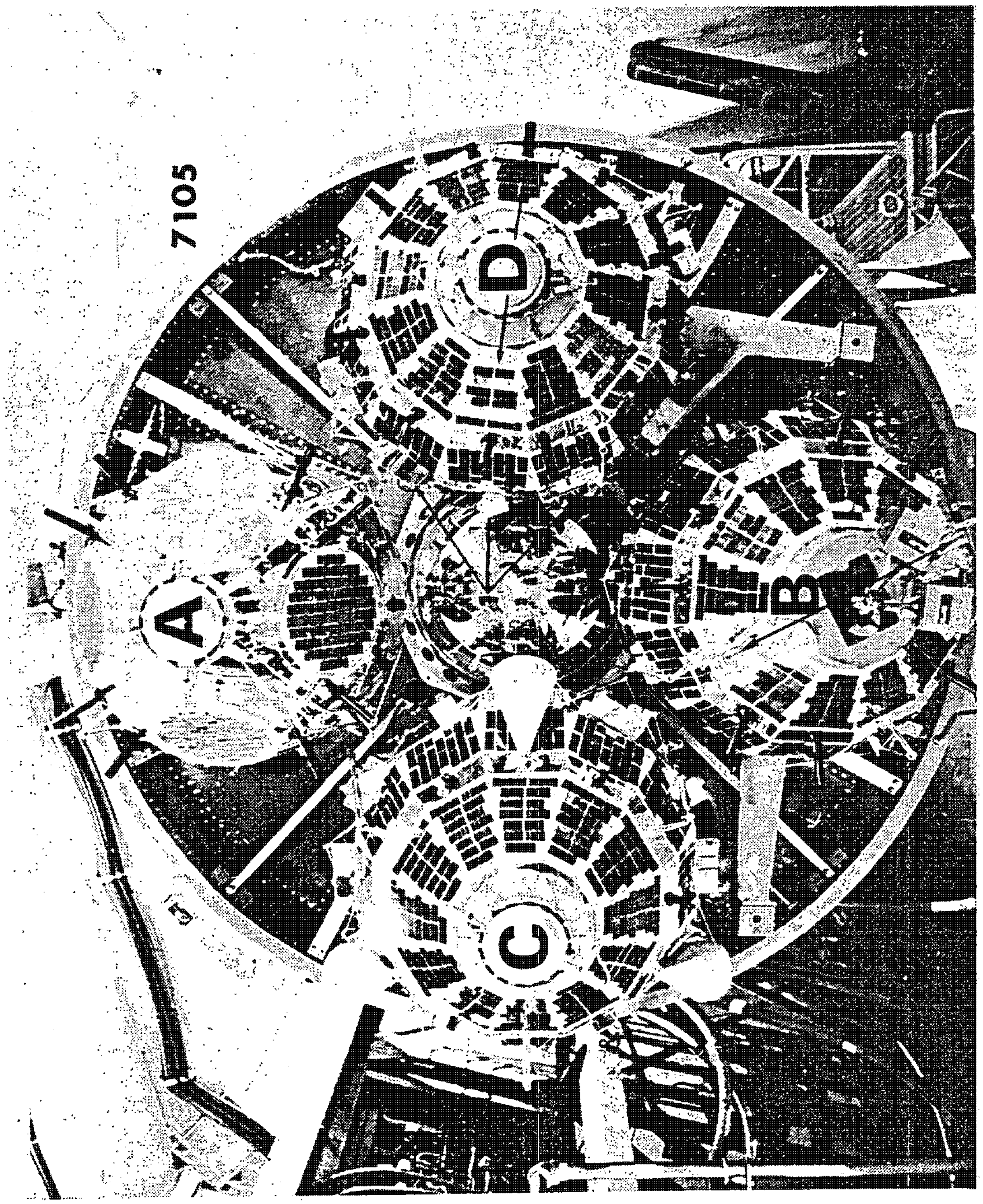


Figure 28

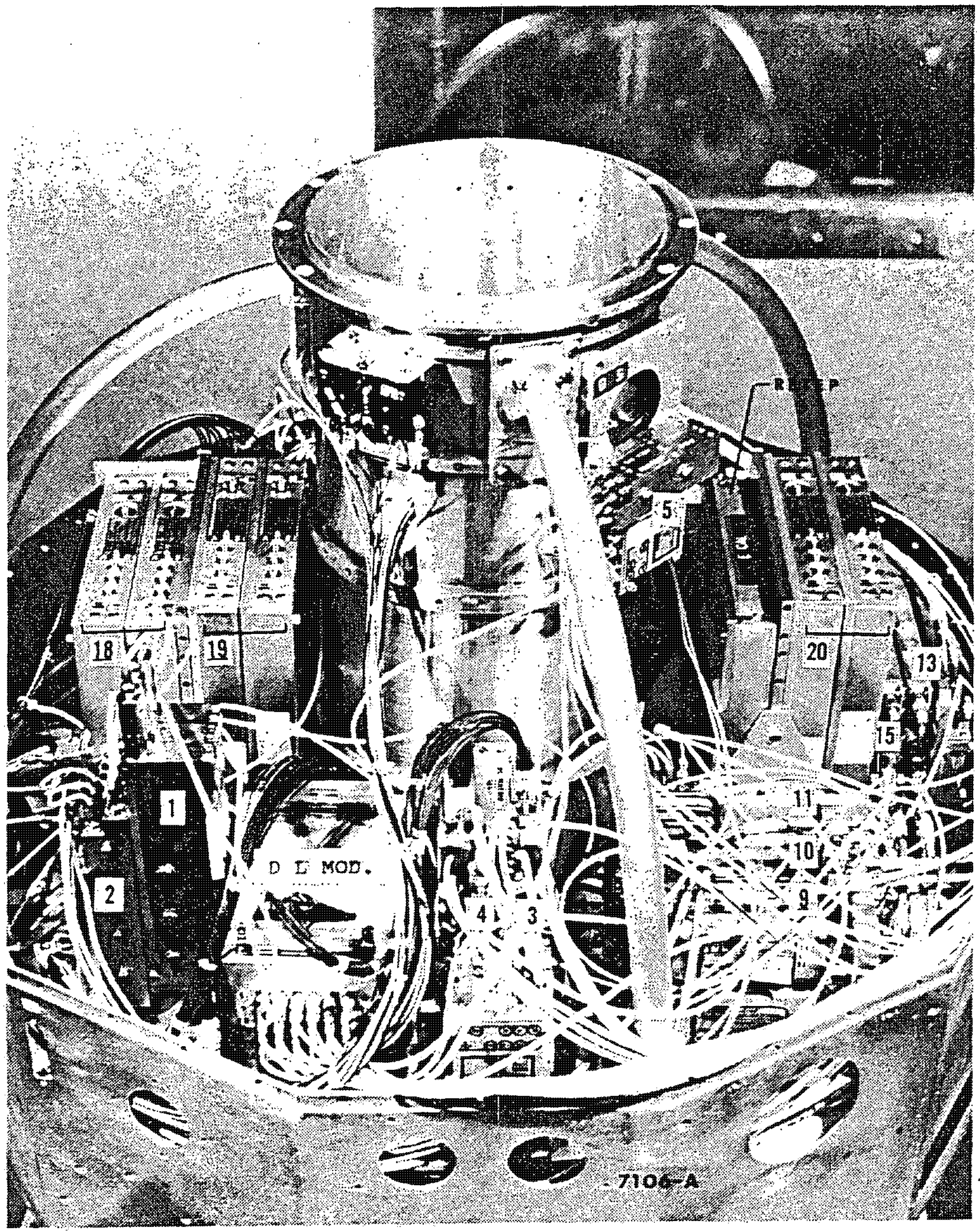


Figure 29

across at the equator of the spacecraft. You would bolt things to both top and bottom of that deck.

A I take it there was some skepticism from the big industrialists as to how well this "peanuts" program would work against the ABM systems. What kind of results did you get?

M It was normal for the POPPY program, with each new launch, to spend about two weeks examining all the design features of the spacecraft from a domestic engineering site, near the Laboratory, at [redacted] Hybla Valley. Then, we would go overseas with our Lab representatives to each one of the POPPY collection sites. We would do an extensive evaluation, [which took] about two weeks, running the payloads and spacecraft through all of their design features, and intensively using them. We used them much more strenuously than would be experienced in the operational phase of the mission.

This first month of experience with the 71Ø5 spacecraft showed us that one of the parametric measurement options was extremely valuable. The BRAVO spacecraft, 71Ø5, had available, upon command, nine bands which could utilize a

[redacted]

we used another transmitter channel for the 1-2-3-8 binary coded pulse which described the incoming amplitude in the steps.

Now, during the engineering evaluation at the [redacted] site, where I was situated, they had two unknown signals about which they had very little information -- where they were emanating, what value, what kind of a threat they were etc. They were in about the same part of the spectrum, had about the same scan and pulse rate characteristics. The minute we turned on the [redacted] with the 71Ø5 BRAVO bird, it was very apparent that one of these unknown signals came through with a level 12 -- which is a very powerful signal. The other one came through with a level 3 -- a very modest radar. It became immediately evident that the level 12 [signal] was a greater threat, it had a much higher peak effective radiant power, was capable of detecting a much smaller target at greater range than the second one. They thus were relegated to two different categories of importance. The signal level parameter, right off the bat, had shown a value that would be of interest to the intelligence community. Later on, it became a tool that the SALT talks people were interested in. We had the tool, in flight, that would help them detect when the Soviets were upgrading one of their radars to look at warheads as opposed to what it had been originally designed for, say the B52s. If they raised the peak effective radiated power, lengthened its unambiguous detectable range, and so on, those factors were discernible in the [redacted]

[redacted] So, the SALT community was able to use, later on, this parameter measurment tool.

A What kind of measurements did you get against the ABM systems? Did you get good results?

M Yes. We could tell about the vertical structure of the antenna beams of some of the radars. The ABM signals were not precisely known at that time. They were being sought. But, we could tell if we had heard a high looking emitter, as opposed to one that was oriented out toward the horizon and very narrow vertically. We could tell things about how it was scanning in azimuth. We could tell if it had the same power at one azimuth as it did at another. Things of this sort were discernible from the [redacted]

A Did you get this results fairly rapidly?

M It turned out that the analysts required some meticulous examination in the intelligence community back here in the States. It took some months to unfold. But of almost equal importance to us was to determine how potent, in peak effective radiated power, some of the emitters were in the new parts of the spectrum, which 7105 had opened for us. We did the best job we could in designing the spacecraft, and the BRAVO bird covered up to 4050 megacycles. So we could measure the signal amplitude of any of the parts of the collection spectrum included in the BRAVO bird.

You asked how well we had done in solving the ABM problem. That was the Harry Davis committee's major requirement. Well, on the <u>second</u> day that we were operational, after the one month test period, on that second day we got the first ABM intercept. I think it was DOG HOUSE. On the fourth day we were operational, we got the first intercept of the HEN EGG at Sary Shagan. Those were two of the big pieces of ABM concrete that they were worried about. So we had an element -- and I do not mean to imply that we had every signature of that hunk of concrete called DOG HOUSE -- but we had, at least, the first indication of it the <u>second day</u> we were operational. We had another piece of the HEN EGG at Sary Shagan, which was another one of the big, important centers.

It sounds easy now, to have made the changes necessary, to have done the design and modification of the ground stations, but it wasn't easy then. The little computer that we had put in there worked something like this. As the pass progressed across the site, the computer determined that at this instant in time, a signal emanating from [redacted]

Well, we came back and told the community that we had done what we had set out to do. What we had found did not turn out to be an ABM signal, but at least the system worked. It was clumsy, it was a first step, but we were pleased to say that we have been vindicated, what we had set out to do, we had done. Now, we asked, "Where do you want us to concentrate?" After this report, I knew how Mr. Lorenzen had felt when he was trying to get into that room full of very aggressive industrial community vendors, because now we all felt the same way. The reception we got kind of a shrug, like, "so what".

A You set out to do something, you did it, did it with a very small amount of money, you had gotten results rapidly, and then there was not much appreciation.

M No, what I had really expected them to do was say, "Based on this initial success, why don't you capitalize on it and try this first, then that and that". I expected some guidance and management. If not from NRO, then from NSA. But, I think the rice bowls [redacted] and the synchronous programs were too alluring. The POPPY program, in this scene, was shrugged off and only because we had a dark blue Navy loyalty, a Navy team at a Navy site at [redacted] the lack of official guidance and tasking permitted a certain opportunity to exploit the new capability by our own initiative.

But with this new resource, we were able to begin to experiment with doing things that could not wait to be done at Ft. Meade. In other words, the time critical aspect of the data became the thing that we started concentrating on. If the signal was mobile, it did not do any good to locate it a month afterwards. If it was a tank on the ground that was moving across the battlefield, to tell where it was a month ago does not help anybody. If it was a ship at sea, it moves around, and telling where it was a month ago is absolutely obsolete, it does not do any good at all. So we started looking to exploit POPPY in a tactical sense, a time critical sense.

A So that really was a side development of the initial focus on ABM systems. You began to have the capability because you had the processor at the site in [redacted] It let you begin thinking about some kind of near real-time analysis of selected emitters.

M Exactly. But what I assumed would happen was that the official NSA and NRO community would have tasked us to do a number of things on their behalf and we would not have had the opportunity to be innovative on our own. That did not take place. We got shrugged off. Only on our own initiative did we try to do things that were important to NSA that they could not do, things that would have been obsolete had they done them back at Ft. Meade. Out of this, came the first rudimentary, clumsy beginnings of locating ships at sea.

For the software development for the system, again, we had a very small budget. The initial $382,000 was spent to get the first computer, to get it programmed, and to get it over to [redacted] It arrived there before the launch. The software team was led by Bob Daniels of HRB Singer. He actually, was writing software on the plane flying over to [redacted] Remember, on 21 December, we got the green light to go buy it and do it. By late April, we had the [computer] enroute to [redacted]

A How did you get a computer that fast?

M We had to find one that was available. It turned out that we found a GE-owned instrument sitting on the floor and gathering dust at Systems Engineering Labs. (SEL), in Ft.

Lauderdale, Florida. They had some plans for it, but they were not speedily forthcoming, so SEL said we could have it if we didn't care that it was turquoise in color (laughter). So, we took a turquoise SEL model 810. The real-time buffer for the front end of the computer, where the pulses are buffered prior to going into memory, was designed over the telephone by Fred Hellrich working with SEL over the Christmas Holiday! In just a few hours, they figured out what they would need, and what kind of specs it should have. So it was written into the contract while it was being built. This was the major requirement for a design change to their existing system. We had to change several peripherals too.

Lee Hammarstrom led the software development team and Fred Hellrich led the ground station design team, which was responsible for changing the ground station to accommodate this new tool. Part of it was this A to D conversion I mentioned earlier, getting it installed and checked out. Vince Rose, as he had been since day one, was in charge of the ELINT subsystem for the spacecraft and he made those changes. The new spacecraft antennas were like large ice cream cones, conical shaped fat monopoles. They were ten inches long and five inches in diameter at the big end. The big end was away from the spacecraft. They were very incompatible with the solar cells on the lower surfaces of that multi-faced design due to the large shadow they cast.

A What were some of the other technical achievements of the 7105 Mission that we should note?

M [redacted] portunity to officially direct their signal toward our spacecraft. It was a latent experiment in the spacecraft that really did not amount to very much. Politically, it was overcome by events.

I think a couple of other features of this mission were that we had raised the frequency of highest collection up to 14,800 MHZ; and, in this particular collection band we had a sensitivity exceeding minus 100 dbm. It was very very sensitive.

Side 5 9 June 1981

M Another parametric experiment we had in this mission was in the A bird, 7105 ALPHA. It was similar to the [redacted] that I spoke of for BRAVO. ALPHA was equipped such that, upon command, going one band at a time in the ALPHA bird - through nine bands, you could uti- lize [redacted] But this was a lot of trouble, and it was not used that way operationally. Pulse width could [redacted] much more accurately from some of the other collection systems of the Air Force. So ours was not a very successful parametric experiment. Signal amplitude, however, was.

A What about the Navy use of this system? We have talked, in most of your earlier missions, about the input from POPPY to the national collection at NSA. Were you beginning to give some input to the Navy as of yet?

M . The answer is yes, but it was brought about by th personalities involved. We placed the small computer in [redacted] and we sent Dick Wales of HRB Singer, a very creative innovative, dedicated and hard working man, who is now a vice-president in the company, as our first technical representative at the site. He had been intimately involved with the development of the software and now was stationed there for the care, feeding, and exploitation of that computer system.

When the national community failed to capitalize on the developing opportunity in [redacted] Dick Wales and his military counterpart, ~~ENS~~ LTJG Ron Potts, collaborated on trying to do many things that would satisfy the time-critical needs of the community. They recognized that the strategic opportunities in POPPY were being fairly well dealt with at Ft. Meade and that there was not much hope in improving on that at the forward field site. So the time-critical things, signals that were mobile, became their main challenge. They were recipients of many of the documents, messages, and regular reports that are circulated within the fleet and between the various organizations.

The British, were already pioneering some fingerprinting opportunity in the Soviet emitters. They were observing that you could identify by PRF and scan, an emitter in the Soviet fleet over a period of several years, even though they were painting a different identification number on the fantail or the bow of the vessels that come through a choke point like the Bosphorus or some other harbor. I think the British were the first to recognize this.

Ron Potts was a recipient of the intelligence summaries that came out daily for the fleet relative to the Soviet threats, the aircraft, the shipping movements, and so on. He recognized that, on several occasions, these intelligence summaries reported a certain Soviet man-of-war going over here on this side of the islands, when, in fact, it went on the other side. He could tell from POPPY the geolocation of the emitter aboard that ship. After two or three times where he had detected errors, he found out who was writing the report and where it was written and got in touch with the officer, and suggested that we had an opportunity to collaborate and improve the quality of their publication. The team that was writing those reports was in ROTA [Spain]. They were overworked and crowded into a tiny basement with little space. It is not surprising that there were some errors that came out. But it was not oversight that I am reporting here. It was the fact that the tools that they were using -- HFDF and other tools such as SOSUS were imprecise. They were rough in their resolutions and had voids compared to POPPY on many occasions. So, Ron Potts did begin to work constructively with ROTA and they began to take his data experimentally. Then they found on their own that, yes, it did save them from making errors on a few occasions. Pretty soon they started demanding this data. It had been given it to them kind of under the table. But now they even set up a special link between [redacted] and ROTA so that the data would get down there in a timely fashion.

The 71Ø5 launch was 1967 and by the spring of 1968, a number -- 30, 40, or 50 -- of ships had been located by POPPY. Some of them were known, and some of them were highly suspect. The ones that were most important were those that they could not put any signature to.

Once they saw a ship leave Sevastipole and go out into the Black Sea, tour around for 4-5 days, then come back in to Sevastipole and be at dockside for three weeks and go out again. Finally, this unique emitter headed for the Bosphorus, went past Istanbul, and on out into the Mediterranean. All the cameras and eyeballs we could convene on that occasion were watching, and what we saw was the new helicopter carrier MOSCOVA on its first tour outside the inland waters of the Soviet Union. We then recognized that we had observed its TOP SAIL radar signal for four or five months. We saw it in September, again in December in the Black Sea, then, finally, in the spring, it went out into the Mediterranean.

A So you unmasked the villain, as it were.

M We were able to pin the tail on the donkey. We were able to say that, "This ELINT signature is that emitter on that ship." It was the TOP SAIL emitter on the MOSCOVA that we were seeing. It was a new family of emitters, it had never been deployed before. Some months later, the same radar was put on another helicopter carrier, and a year later on a cruiser. Each time, those signals were detected in Soviet waters, deep inside their interior, before the ships came out where you could really be sure of what they were.

In April 1968, ADM Tom Moorer, CNO, was going to have a luncheon with Dr. Eaton of the Eaton Committee and the subject was going to be SIGINT. It was thought by ADM Moorer's staff that he ought to hear about POPPY. Well Mr. Lorenzen and Capt. Weldon and I went over to brief ADM Moorer. We took our big 30 x 40 briefing charts and we also took a three-hole notebook with some copies of those graphics. We did not know what kind of a forum we were going to get with him. We had just started to brief him when we had to clear out of the room -- some bell rang. Everybody left the room except ADM Moorer. We left, and abandoned our BYEMAN briefing at the door so it couldn't get away from us. Pretty soon he came back in and apologized and said he only had a few more minutes. He asked if we could do the briefing in a less formal fashion than the 30 x 40 graphics. We said, yes, we had a three hole notebook. So, on his desk, thumbing through the notebook, we proceeded to tell him that we had seen the MOSCOVA, three-four-five months before it came out of the Bosphorus, and located it. We showed him the track -- the dots on the map. Well he got furious. He asked the question, "What Navy do we work for." He, really, was rather irate about it. We said, "Well, yours, sir, why do you ask?" (laughter). And, he said that in the last months, the President had handed him, on a number of occasions, pictures of ships at sea taken from the overhead photo world, and the question was asked, "Where are they heading? Do they have missiles onboard? Are they going to Cuba? Do they have a wide cargo hatch?" ADM Moorer said he did not have enough aviation fuel and aviator eyeballs to position them in all the areas where they might have been and he just could not find them. Then he said, "And you mean, we have a capability here of locating a ship in hours?" where he might get a pilot in a P2 several hours later and answer the question. He said he wanted to know why he had not heard of this capability before. In fact, he wanted to have a briefing on our results, and as we began to exploit this, he wanted a briefing every week!

The discussion was rather far ranging. It touched on what was called [redacted] He wanted this POPPY capability aimed at [redacted] [He said] that there was no reason in the world why they could not exploit this program in behalf of the Navy. It was a national requirement to know whether missiles were being moved into our neighborhood. So, that's how Ocean Surveillance began.

A So this was really the first time that the CNO ~~and~~ had ever been fully briefed about POPPY? Is that your sense of it?

M I believe it was the first time CNO knew of the ability to locate ships was inherent in this overhead technology. He might have known there was a Navy program doing national work for the NRO, but he didn't have the realization that it could locate ships. It could only locate ships because we had a dark blue [Navy] team at [redacted] a Navy base. We had demonstrated a capability of doing a national job, and had not been given a mandate to continue it. To exploit our capabilities in a reasonable taxpayer manner, we started looking at the mobile signals, and ships were the majority of them. Oh, we located a few aircraft, too. We even tried to optimize the location by varying altitude on one aircraft intercept, and we could show that it was some 3,000 or 4,000 feet in the air. It was a rough optimization. We did not do mobile things on land because none of them moved fast enough.

The VCNO [ADM B. Clarey] visited the Lab. some months after this April visit of the CNO. We debated, prior to his briefing, whether we ought to tell him about the first submarine location we had made in the previous several weeks. The briefing was quite informal, very interactive, with him asking questions and suggesting things, and so on. When it came to that point in the briefing to mention the [redacted] will have to learn to build our confidence as this is developed. But don't hold back on the old man!". That was the thrust.

So, we started putting in weekly reports out of the site of everything they did of interest to the CNO. I think Ron

Potts actually came back and briefed the CNO. He also briefed the SORS (Satellite Overhead Reconnaissance Subcommittee) of USIB, in an attempt on the part of the Navy to get permission to task the overhead ELINT assets against this Navy need. After those briefings, we were allowed to task certain passes, (two a day, I think), over the Atlantic for ocean surveillance. That was the first time that the national assets had been diverted for use by one of the services. But now, it is natural to do that, everyone is doing that.

A Ocean surveillane was, of course, to become extremely important later in this program. But prior to the time that it became the dominating factor, there were two other missions that continued on the POPPY series, which we should say a little bit about now. These were Mission 7106, where the spacecraft was launched on 30 September 1969 and Mission 7107 which followed on 14 December 1971. Could you say a little bit about the purpose of these missions and maybe some of the major achievements that resulted?

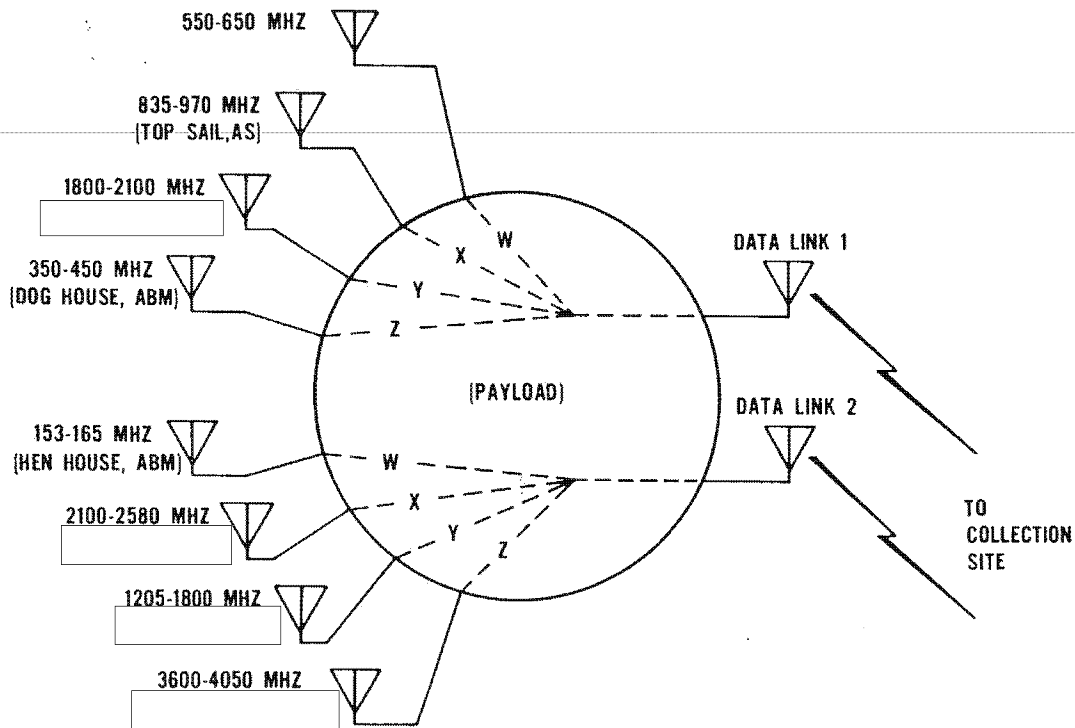
M One of the major changes in guidance from the U.S. Intelligence Board took place during the conceptualizing time for the Mission 7106. Prior to that, the entire community had been concentrating on one type of emitter or another in their design of the spacecraft ELINT systems. The USIB now advanced the philosophy of "total weapons systems" intercept and "total weapons systems" associated processing. These two thrusts were then continued for several years and did, in one way or another, influence the design of both of these POPPY missions and the data processing thereof.

In Figure 3Ø, we show four of the ELINT collection bands in 71Ø6. Inside the circle is a payload which shows a W, X, Y, and Z. Those are four different [redacted] that are used to portray, unambiguously, the data that are intercepted in each of these four collection bands. There were more than eight ELINT bands in the birds. You could operate four through one transmitter at any time, or another grouping of four at another time. There were also two transmitters in each spacecraft.

In order for us to maximize the collection of both known and unknown emitters at any instant in time, the concept had all four of the spacecraft flying in a tight group. Figure 31 shows these spacecraft about 6Ø miles apart. The illustration shows the relationship of the number and accuracy of emissions that you could geolocate. [redacted]

That is pretty well displayed in this Illustration 31.

Illustration 32 shows the spectrum of the many ELINT subsystems that were included in Mission 71Ø6. You will notice that the areas of common coverage are easy to see. In the low end, to the left, you see the TALL KING spectrum



7106 CHARLIE (TASK GROUP E 25)

Figure 30

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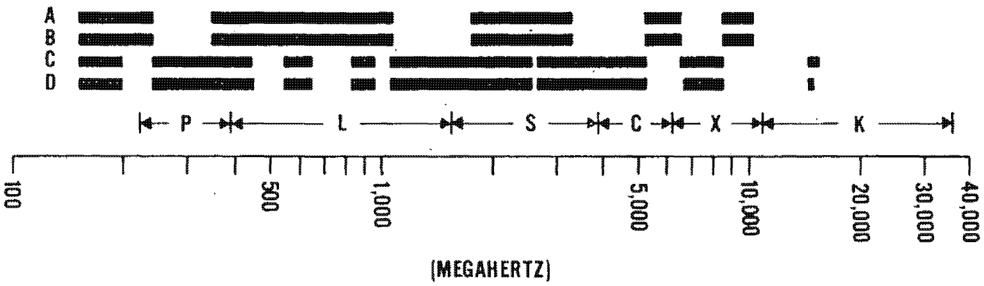

FREQUENCY COVERAGE FOR MISSION 7106

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Figure 32

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150-175 [megacycles] or thereabouts. All four birds were equipped with that. As you move up the spectrum you have some areas where other important Soviet emitters are known to be situated. Particularly, the mobile shipborne threats were very carefully equipped in all four spacecraft. We wanted the best opportunity to locate them for either high productivity or maximum accuracy or both.

In Mission 7106, we lost two of the spacecraft, BRAVO and DELTA, very early -- about 5 months into the mission. They were lost within an hour or so of one another. One was [redacted] That really was a disaster to us. We have no satisfactory explanation for it. We could not have had two of them die that way even if we had tried, but it happened. I think it was within one pass of one another, as I recall. The capability that remained after losing BRAVO and DELTA was primarily just those areas where we had designated four bird coverage initially. We had [redacted] Since the ship spectrum was so widely covered, that became one of the major uses in the mission for developing ocean surveillance effort.

Figure 33 shows one of the main changes for Mission 7107. It is that we had three transmitters in each satellite. By putting the third transmitter in there, we were able to fly the spacecraft [redacted] therefore, cut the revisit time in half. In 7106, about every 108 minutes we had another opportunity to collect.

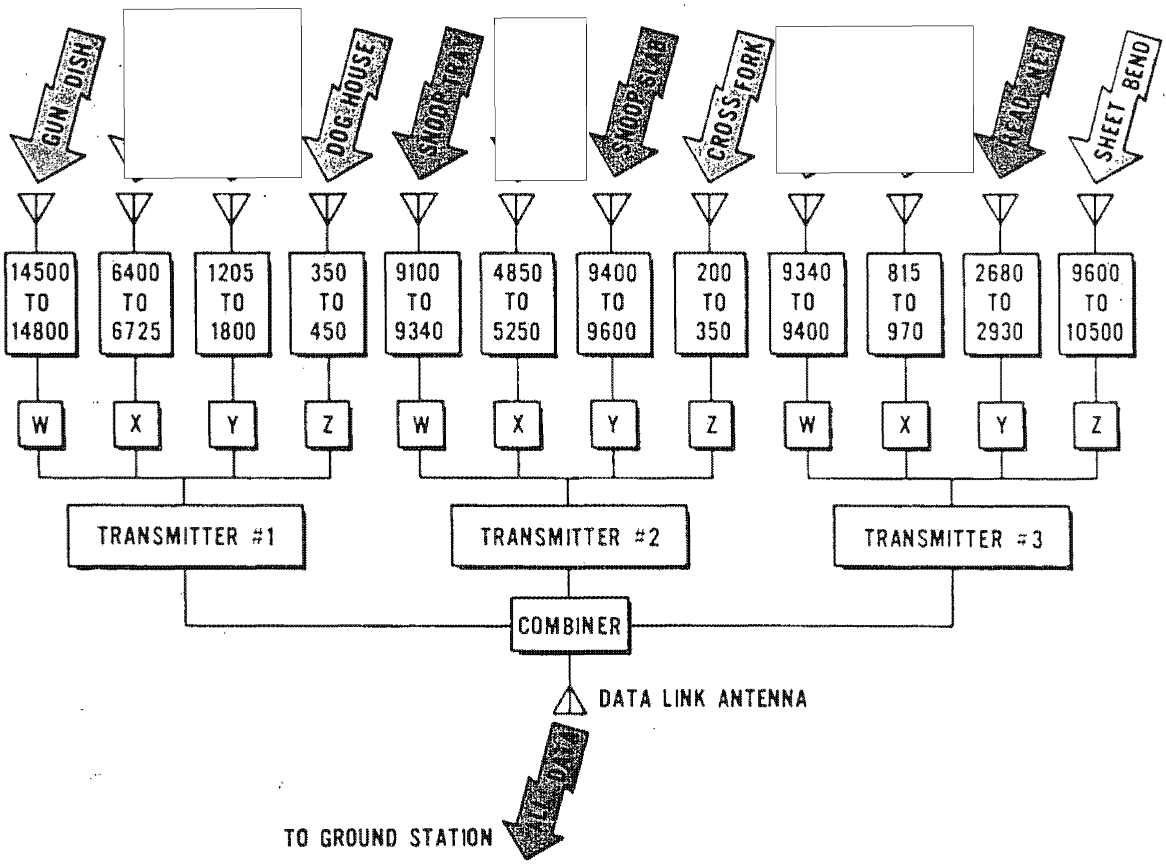
GUN DISH
DOG HOUSE
SNOOP TRAY
SNOOP SLAB
CROSS FORK
READ NET
SHEET BEND
14500 TO 14800
6400 TO 6725
1205 TO 1800
350 TO 450
9100 TO 9340
4850 TO 5250
9400 TO 9600
200 TO 350
9340 TO 9400
815 TO 970
2680 TO 2930
9600 TO 10500
W
X
Y
Z
W
X
Y
Z
W
X
Y
Z
TRANSMITTER #1
TRANSMITTER #2
TRANSMITTER #3
COMBINER
DATA LINK ANTENNA
ALL DATA
TO GROUND STATION

Figure 33

With this widely separated [redacted] we had an opportunity to collect every 54 minutes.

We did add a third transmitter in each satellite and consequently, another group of four collection bands could be tasked. That was a kind of step towards massive collection. We wanted to continue a reasonable compromise in that direction. We felt the USIB's guidance of collecting knowns and unknowns was proper, even though the analysis community had done very little towards their requirement for associated processing. They were initially charged by the USIB to look at a known emitter. At the same time, they tried to look for things that had the same scan, same up-time, same down-time, same use, in order to find unknown elements of a complex weapons system. This was to put the entire weapons system in a more accurate context. It was difficult for the analysis community to take this associative analysis guidance to heart because they were completely infatuated with looking at each signal by itself, with little regard for anything that was going on at the same time or in the same neighborhood. We continued through 7107 to build a collection capability for associated processing should the analysis community capitalize on it.

Figure 33 shows at the top, some arrows pointing down and to the left. On each of these is written a name of an emitter, a threat emitter. The band in which that emitter would be collected is shown in the rectangle immediately below, for example, 14,500-14,800 would be the band in which you would receive GUN DISH. Those that are shown in blue, at the top, like SNOOP TRAY, SNOOP SLAB, [redacted] are the primary threat emitters of the Soviet fleet. So, here you have a blue tasking with some other unknown bands also capable of being seen at the same time. Where we see a radar in peace time may not be precisely where we would see them in wartime. But both Mission 7106 and 7107 embraced the whole spectrum, up through X-Band. Mission 7107 even had a 400 megacycle piece of the 35,000 megacycle region. We never got any significant data there, but we showed that the capability could be engineered into a POPPY type crystal video ELINT system.

A As Mission 7106 and 7107 went up, was the ground station community about the same as it had been?

M Not totally. There had been an evolving change in service participation in the ground segment. In the very beginning, I mentioned that we wanted to have two sites observing the spacecraft data simultaneously. As our confidence built, it was not longer necessary to have two sites in use at a time. One at a time was sufficient. And, very early on, we moved out of the collection site with the [redacted] because the host country wanted to see what was going on in our hut. That was not permitted. We had only one choice, which was to pull out of there. We moved in with the [redacted] That was further back from the Soviet Union and a little less successful and we did not have very good maintenance on the system in [redacted] It closed after a few years.

The site at [redacted] had a number of unique issues with the local population. They did not want our antennas pointed south towards [redacted] They only wanted them aimed towards their enemy India. So, we had to leave them looking toward the east most of the time. They thought that these antennas were offensive rockets that would fire. Gradually, the [redacted] military began to co-mingle with the Air Force team in some of their Classic operations and, in order to keep them from this denied BYEMAN arena, we had to close up the operation in [redacted] Similarly, with the Army in [redacted] Eventually, they were closing up part of the operation in [redacted] The question of moving south to another installation or whether we wanted to continue it at all was raised. That was the gold flow problem. We were caught in the economies of the community. So, we completely closed up the operation in [redacted] The last one to close was the limited collection [redacted]

A What did that leave you with, after those pull-outs?

M Exclusively Navy. We had only Navy participation in Mission 7107.

A How many sites were there [redacted]

M [redacted]

A Another question comes up in the relation of these last two missions. As the overhead community had grown in terms of its numbers, and sophistication, was the POPPY system becoming less important? Was it overshadowed, or were some of its capabilities picked up by some of these other missions?

M To some extent one might perceive that this was the case. Actually the basic, fundamental, and most important attribute of POPPY was to find the signal first, to find it early, to find a signal that no one had every heard or knew about. Such signals were seen first by POPPY in most every instance. Particularly, the ABM, AES, (Anti-earth satellite) domain. But after you found the signal and cued the community, then refining it became a job for more directed activity than POPPY. The Program A Air Force team excelled here. They were able to bring higher resolution devices to bear on these signals, once they were given their basic characteristics. If they were cued to get on to it, then they would concentrate their resources to exploit it and to learn more of the fine grain details about it, and to define the threat in greater accuracy and greater precision.

The community often, I believe, downgraded the importance of the initial detection. In some Classic cases, where they looked at the evolution of a very important threat emitter from first indication the day a bulldozer was in and put a scar on the surface of the earth for the first time until the intelligence community could say, with certainty, that this ELINT signature is coming from that particular piece of construction on the ground -- there might be a period of six or seven years involved. In that six or seven years, POPPY would have intercepted it on holidays like the 4th of July, on Christmas Day, and on other holiday routines, just brief emissions, which in the final analysis, were often proven to be the first intercepts by our community.

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A We have talked about the contributions of 7106 and 7107 to the ABM threat and the AES threat. Were there other kinds of major threat that the system contributed to?

M One that comes to mind, that perhaps is not in the same order of magnitude as the ABM, is the surface to air missile threat. There was the SA-1 [redacted] system around Moscow. We saw it as we flew POPPY and we could see it in the data. But it was not deployed anywhere else. It was unique to that area. There was a ring of them around Moscow. In the Viet Nam scene, some of the other surface to air missile emissions were detected. As they deployed these for the first time out of the Soviet Union, we quite regularly had what we called the, "Ah-Ha" factor, where you could say, "That signal, by golly, is from that piece of hardware that we have been seeing for the last five or six years, in May Day parades and other occasions. We have seen the hardware but now we know what it sounds like, we know what the ELINT signature is."

POPPY played a very important role in making that connection. But once you had made the connection, then exploiting it in finer and finer grain becomes a job that was more successful in the Program A directed search community. They quickly lose sight about how difficult it is to find it first.

A Because they knew the threat they were concentrating on refining the analysis.

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A We ended our last session talking about the change in perceptions in the community about the POPPY program as it went through Mission 71Ø6 and 71Ø7. After thinking about this, you decided that maybe you should reformulate your thoughts on what that response was during this latter part of the POPPY era.

M There were a number of counterbalancing influences going on in the community. One of them was characterized by the large investment CIA, NSA, and the Air Force had made in the late sixties. Each of them had what I refer to as a rice bowl. They eaach had a commitment, a large investment, and a lot of people working in the overhead programs. The competition for budget dollars became extreme. Prior to 1969, we had almost level funding, something like six to eight million dollar a year. As the complexity of the POPPY sites and spacecraft increased, the costs increased also and the competition was getting much tougher. You had to justify [costs] at a much higher level and in much greater detail. With this came a higher level of Navy management intensity, both here at the Laboratory and in the NRO. Also in the late sixties, 1968 or 1969, Robert K. Geiger, then Captain, went to the NRO headquarters and became a presence in the management of the POPPY program. With his Navy background, Navy uniform, and Navy interest, the exploitation of the embryo ocean surveillance capability started to take place.

One of the big things that happened, was that a line item was added to our budget called, "Augmentation of Program C for Ocean Surveillance". We had about a million and a quarter dollars the first year for that and it was aimed at improving the productivity at the sites in that one realm. It bought some specific hardware and, more importantly than anything else, it allowed us to put a second generation computer at [redacted] so we could do on-line processing.

A This was an augmentation of Mission 7107?

M Yes. It was really in the ground segment only. It was a line item for the ground segment only. We beefed up the capability of the site to extract, very quickly, those signals that were mobile and the majority of which were of Navy interest. This allowed us to process signals to the point where you could geolocate them within an hour after the pass. This was done at [redacted] then about two years later at the [redacted] and another two years later [redacted]

A Was it Geiger's influence, do you think, that got the additional funding?

M I think he recognized the capability and he also was astute enough to see that the timing was right, the politics were right, and the people were right for the Navy to acquire an ocean surveillance program. He, and his very bright young junior officers, were busy exploiting the capability that was just beginning to be apparent in the POPPY output.

I should comment on the cost of the program vs. the number of reports that were turned out. NSA, for example, turned out about 100,000 POPPY reports a year from the mid-sixties up until 1969 or 1970 and then it took a jump to something like a million in one year! The next year it jumped to two million! By 1972, they were exceeding two million locations a year out of the program. That was not because NSA had some magic in their own basement turning out more reports, it was because they had very carefully calibrated the capability of the overseas sites. They agreed that the sites were accurate, using some quality control process that looked at what the sites did, so then the data from the sites was used in NSA reports. About one third of the locations were ship locations, or maybe one half. So, the cost per location dropped rather sharply as the site's output became blessed by the community and became usable.

I mentioned CIA. There was a photo mission also coming along in development stages that was a budget hog. It used every dollar that was available. They syphoned money off, whatever they could. So, the intensity of budget disputes, outside the Lab., became very very apparent. It was probably on this fulcrum that the Lab. organization began to pivot. They became sensitive to our budget procedure, the way we monitored the money, how accurate we were. Our comptroller here, on some occasions, had a large error in his monthly reports. One time he would show an error of $300,000. , and the next month it would be corrected. Some gross errors were apparent within the process here at the Laboratory. It caused us some agony with our management over at the National Reconnaissance Office. They could not understand why we were always late with our budget report, or why we were innacurate. It became embarrassing to me, to the program, to the Laboratory, and to the NRO. As the Laboratory embarrassment reached [redacted] it was cause for them to write a very strenuous memo back to Mr. Lorenzen and the POPPY team. It said that the POPPY team was going to have to raise its management visibility and become a much more standard part of the Laboratory process.

A Let me review then, what it seems to me are the differences between what we were talking about the other day and what we have talked about this morning. It seems that, perhaps, the change in perception about POPPY in the larger community was largely due, not to degradation in the importance of the data that POPPY was generating, but rather to new conditions, new competition for money, and new ideas coming in. As that pressure increased, this older but yet stable and reliable program was under increased pressure to meet a fixed budget, to be managed in a different way, and so on. That, coupled with what you said about CAPT Geiger, who appeared first within the NRO Headquarters and, as we will talk about later, then within Program C itself, led to a change in mission for the program.

You now have come to a point where we need to talk about the internal changes in the administration of the program within NRL itself. If I could just fix the date -- we are talking now around September 1970, when there was a need expressed for changing the administration of the program, first in Lorenzen's area and then Laboratory wide. Now, maybe the best way to talk about this is to summarize what the administrative situation had been prior to this change, and then talk about what did change.

M Prior to the fall of 197Ø, I had been a Section Head in the Branch of Jim Trexler. While Jim was a very fine technical person, he was extremely busy with his own project. As far as functioning as a Branch Head over my activity, his position was mostly in name only. Lorenzen fulfilled the technical management role and only the administrative part of it came under Trexler. This was an acceptable situation. I think it worried Jim more than it did me, because I had more access to Mr. Lorenzen than any of his other Section Heads or even his Branch Heads. Day and night he was available to me and vice versa. I learned that even when I was on vacation, I was available to him (laughter)! I found that out the hard way a few times. But being hidden within a Branch, isolated from the rest of the Branch, we really did not have visibility outside of Lorenzen's arena. We even borrowed Mr. Trexler's secretary for one summer because we had no cleared typist in our group. You will notice that in the records, most of the things in the mid-sixties are off of my typewriter in my own clumsy manner. We really were doing much more than we should have been in areas that really were not our speciality.

I guess what happened in the fall of 197Ø was that we had been sent down a false track by the NRO. They had told us about a year before that the next launch vehicle would be a TITAN-3B and that we should be ready to fly on the 3B. A lot of the THOR booster type hardware designs would not be consistent with using the 3B. It had a different size, shape, weight carrying capability, etc.. We began then in the fall of 1969 and spring of 1970 to develop a new series of spacecraft dedicated to the TITAN-3B. Unfortunately, the management of the NRO at that time did not have a very active dialogue with us. They knew three months before they bothered to tell <u>us</u> that it was not going to be a TITAN-3B. It was going to be another THOR! So, we were sent down this road, spending money, throwing away our manufacturing jigs and fixtures for the old THOR hardware that we really needed in the final analysis. So, we found ourselves doing a couple of things that were counter-productive to a good, cost effective program.

The other influence was that two of the spacecraft from Mission 7106 died prematurely, within the first six months. We spent a good deal of time and effort analyzing that failure, trying to figure out how it happened and to thus avoid it in the future. It turned out that one of the command frequency tones (the M tone) relay failed to unlatch. It was latched permanently for some reason or other. It was as though the lubrication in that relay, in two separate birds, had failed in the same manner at near the same time. It was implicitly held over against the stop in one direction. That meant that we could never get a a critical part of the command through the command system. Both of them failed in the same way, within an hour of one another.

A Was there feeling that the other two satellites of 7106 would fail in the same way?

M Oh, of course, there was. We had no real solid explanation for the process that caused this failure; therefore, the other two were in jeopardy for the rest of their life. The previous mission, Mission 7105, had started its life by being a "cold" running spacecraft, which is a safe condition for long life. And, those spacecraft did perform way beyond their normal life expectancy. In fact, we were somewhat embarrassed by not being able to turn them off! We would turn them off, and somewhere over the world, they would get a signal that would turn them back on. I do not know whether the bird itself generated the signal or what, but every week, after they were retired, we would find them back in a current draining mode, and have to reset them.

A They just didn't want to quit.

M That's right.

A It must have been something of an embarrassement when the next set didn't perform as well.

M That's right. So budget wise, we were expending money towards the analysis of a mission that had a significant failure. We were putting money into a booster that was not ever to be in the program, three or four months longer than was necessary, and we found ourselves running out of money. Our funding would not carry us for the year. We then went forward to the NRO with an explanation of what had happened and a request for additional funds. The request was not met with a warm welcome (laughter). We were charged with very poor management, and, to some extent, that was true. However, we felt there were extenuating circumstances. It did embarrass the program, and the Laboratory. As I have said, [redacted] insisted that we have regular cycle of management reviews. The second Thursday of every month two hours were to be devoted to what we referred to as a "blood-letting." We would go and explain the problems of the program, and solutions that we had in mind. There was a regular format that we went through. It was very constructive. In fact, we enjoyed it, because it gave us an opportunity to see [redacted] at close range, to get his thoughts, and get his emphasis of how we should go and what we should do. It was very beneficial.

A What had been [redacted] interest and involvement in the program prior to this time?

M Annually, we would go by him with an outgoing budget paper which was almost pro forma. He would just sign it and away it would go. There was hardly any discussion. Similarly, [redacted] before him. [redacted] [our Associate Director of Research] would review, and on some occasions, the distribution of the budget within the Laboratory, to make sure that each element concurred with the share that they were getting. If our overall budget was reduced then we had to make some adjustment to the other participants within the Laboratory.

Not only were the monthly meetings called for, there was also a large mandate to change the organization where the POPPY program was spawned, to change my organization from a section in Trexler's branch to a branch in its own right.

One of the hottest priorities that I recall, was that we had to get a well-qualified administrative officer on the team. Mrs. Harding was hired -- she is with us to this day. It was a good move on our part to get someone in there who could keep the burden of the administrative activity from myself and my subordinates. We also hired a secretary, who has since left the program.

There were also space problems. We had two GS-12s in a room of about 11Ø sq. ft. A real small room. I think it was 1Ø x 11 ft. There were two GS-12s and that was their only office space. And that was generous space! We had three contractors and two NRL people in a room down the hall that was just a tiny bit larger than that. We did the entire ground station development in a laboratory room that was about 2Ø ft. x 3Ø ft.! It was a "shoe horn" operation. We made up for it by working hard for long hours. This was the standard operating procedure for Lorenzen's team. You did what you had to do. When there was a crisis, you asked for help, and if he could, he would provide help. He would beg help from some other section or branch and put it at your disposal. He would assign some task to them and they would go off and do it.

In the recording area, Bob Misner's team of Don Christman, and others, would solve our recording problems. [redacted] and his group did a direction finder, lobe-switching type job so we could track the spacecraft more continuously and not have operator induced voids in the data.

Every element of Lorenzen's Branch was made available when appropriate.

A So, it had been a very informal type organization for the program itself up until this time.

M Indeed, yes.

A Was Lorenzen, do you think, embarrassed personally, by this need for a change in management? Did it reflect badly on him for not having done something prior to this time?

M It did not come across that way, to me. It came across more that under the Eisenhower/ADM Frost arrangement, when they allowed TATTLETALE to be reborn again, it had come up in a very tight security arena. That mandate remained pretty much until [redacted] said, "Wait a minute, we can't go along any further in this hidden process. It has to be much more available for the management of the Laboratory." The previous security restraint had been reinforced in a number of ways and a number of times during the previous seven or eight years.

A You mentioned some of the administrative changes, in terms of the addition of administrative personnel brought into your group, and said that this brought about high visibility more in contact with the leaders of the Laboratory. Did it make your job easier in other ways? Was it easier now to get contract personnel, or to get technical people? How else did this administrative change affect your responsibilities?

M Since 1963, we had HRB Singer people working in the ground segment of the program. Part of the plan in getting us established as a branch was to convert these key HRB men, Jim O'Connor, Lee Hammarstrom, Mark Van de Walle, Tom Lawton, to civil service employment. By the fall of 1971, a year after the initial emphasis, the first of the HRB people had converted.

A The rationale was that they had essentially been running the program so they should be under NRL control?

M They had been carrying a large share of responsibility in one segment or another. They were the designers, the implementers of major points of inflection in our ground segment. They were the nucleus of the branch team. We hired several young engineers, George Price, who is Charlie Price's son, and Terry Fisher. Vince Rose, who through all these years had done the military mission ELINT payload in the spacecraft, had brought on several people to help him, Ed Becke, [redacted] One of the main cogs in our activity deploying things overseas, making sure everything worked, the training of operators, etc. was Bill McDavit. We borrowed him for years from Bob Misner's or [redacted] sections, and now he converted to full time employment in the POPPY program. We gradually got our resources together. Many of those who had helped us in one way or another in the past, were considered for joining my branch. Some of them chose to [join], some of them did not. Running the branch caused an additional administrative burden on me that I had not had as a section head. But I had a lot of very understanding help. Mr. Lorenzen's administrative officer, Mrs. Sadie Randleman, for instance, and my administrative officer, carried a great deal of that burden. The budget process was intensive in those years. Our comptroller was late and innacurate, as I mentioned. We had to, in some sense, carry our own financial books along to keep a running account in order to have something to double check the NRL comptroller's report. After a year or so, that problem sort of took care of itself. They got a new comptroller, new procedures, a computer that was doing things right, and the accuracy problem sort of dropped off.

A Besides the more formal reporting within NRL, had there been changes in your reporting to the NRO and other outside sponsors? Had that become more formal and demanding?

M Yes.

A Was it financial pressure that tended to drive that?

M It seemed to be on that occasion we mentioned above and I would presume it was for that reason generally. Prior to the late sixties, they would have a pre-flight review where they would assess the readiness of the mission. They would, of course, examine our initial mission proposal before we started building for a new mission. Then there would be the annual budget process and that was about the extent of the interaction with the NRO. But, by the late sixties and early seventies, they were demanding monthly financial reports, and a quarterly technical review of the progress and the problems and how they were being solved. It would be attended not only by the Laboratory for the spacecraft and ground segment design, but also by the people from Ft. Meade, to be sure that they were not going to faced with any surprise when the new mission was launched. So now they got a lot of advance warning, much more than they had in previous years.

A We have talked, then, about the internal changes. A little later, a major external change within the Navy was going on in the administration of the space program as a whole and that was the creation of PM-16 (Navy Space Project Office) under the Chief of Naval Materiel. I wonder if you could skip back and give the background on that and how it came to be from your perspective and what effect it had on your program.

M Dr. John McLucas was the Director of the National Reconnaissance Office, ADM Noel Gaylor was the Director of NSA, and ADM Elmo Zumwalt was CNO. Those were the major players in 1969 and 1970. Capt. Geiger knew them all. ADM Moorer had given us a mandate to report ship locations out of the POPPY program in April 1968, and we had developed, nurtured, and kept the idea going for several years. With these beginnings of ocean surveillance, there was now a large ground swell of interest within the Navy to achieve, by overhead means, the answer to their ocean surveillance threat.

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M There were a number of players. One of them was NIPSSA. [Naval Intelligence Processing System Support Activity], the organization in the Navy that took all of the reports on ship locations, whether they be from fisherman at sea, or Lloyds of London, or sitings in a harbor from merchant interests. All this information was being focused toward computerizing the location of ships of all nations. Of course, the Soviet part of that was of most importance. The Soviets, as I think I mentioned in one of our previous recordings, had, in many instances on their capital ships, [redacted] enabled us to say with certainty, "That ship is the MOSCOVA" or whatever the cruiser's name was. We could tell them week after week, year after year, in fact. We then became an important data source for the NIPSSA data base on ship locations.

A This was happening prior to the 1970-1971 time frame?

M It was the late sixties. They became aware of it and we began to work with the players, and the management of the NIPSSA organization. They were extremely cooperative in giving us computer plots of the POPPY data alone, or the POPPY data in context with other data. We could see, for instance, on some locations, where a flotilla of several ships would go from Murmansk to Cuba and back up to the Baltic area. POPPY would show sixty or so different locations during that transit. We even had data taken from [redacted] on some of the ships that were beyond the range of our station at [redacted] so that NIPSSA was able to take the plots of the [redacted] locations as well as those from [redacted] and complete the entire voyage of the ship over all that distance.

A NIPSSA, then, was a strong supporter of making ocean surveillance a Navy only requirement on this resource.

M They became a major advocate starting in the late 1960's. Capt. Barker was in charge over there, and he recognized, early, that here was a new tool that had greater accuracy, timliness, and the potential for great productivity than any contemporary source. He became very influential in exploiting it. He also played a heavy supportive role in getting the fleet elements, (the Sixth Fleet and so on) to state their requirements for ocean surveillance in such a way that they were precisely what the community back here needed. (Actually the community back here, in some sense, had these ghost written or drafted for the Navy flags out in the forward area.) One of Lorenzen's men, in a sense his Chief of Staff, a [redacted] worked extensively with Capt. Barker and got the manifestation of the requirement for ocean surveillance into being, into the CNO level, and the USIB level. This paved the way for an activity that took place, I think in 1969, where Lt. R. Potts from [redacted] came back and [redacted] Potts, and myself went over and briefed the Satellite Overhead Reconnaissance Sub-Committee of the USIB. Our major thrust in that briefing was to try to get them to approve our request to use several of the unused POPPY off-shore passes each day for ocean surveillance tasking and to build and demonstrate our capability. They allowed us two passes each day, off in the Atlantic. These would serve us very well out of [redacted] as ocean surveillance collection opportunities. With those two passes a day, we built the capability that NIPSSA had recognized, and that the fleet soon recognized. It caused some pretty important ground swells of support related to the OKEAN exercises. POPPY was not blind to darkness at night. We could follow the Soviet ships in bad weather -- it didn't bother us a bit -- and when our fleet would lose a major element of the Soviet fleet in the Mediterranean, it wasn't long before POPPY had informed the Sixth Fleet over there where the lost Soviet ship was. Then, they would get back on it and trail it again. I think I mentioned earlier in the interview that ADM I. Kidd was running the Sixth Fleet at that time, and he recognized that POPPY was the overhead resource which was putting them back on the trail of the Soviet fleet on several occasions in these very critical military exercises.

[redacted] 0942 Space Crac 1971-72? (was also part the end of U-2 Antarctic recce)

D This is the OKEAN exercise?

M Yes, the OKEAN exercise.

5/29/92
NO?— NATIONAL WEEK exercises in the med - the intell community may have called the Russian exercises, but we (6th FLT) were (and still are) struggling with the surface surveillance. I was there. EK.

A So all this was background to the movement to create PM-16. It was pressure that led to the establishment of this office.

M Yes. VADM F. Harlfinger was the Director of Program C until the last few days of 1970 or the first few days of 1971. As a very capable manager and head of the Office of Naval Intelligence. I think that it was a good move for Capt. Geiger to get the Director of Program C assignment changed from ONI to a dedicated program office under the Chief of Naval Material. That happened, I think, the first four or five days of 1971.

Then Capt. Geiger came out here and talked to [redacted] our Commanding Officer, and told what his style would be and how he would treat the Laboratory -- I think the words still reside in my memory -- "that we would be treated as another contractor". The word "damn" was not included but it was implied. "Another damn contractor", I think was the way it came across to those of us who existed under his influence for a number of years that followed this.

A What was your reaction to <u>that?</u>

M It was a slap in the face! We were as "dark blue" Navy as he was, even though we were not in uniform! We had developed the technique of solving the Navy ocean surveillance problem in the Program without a great deal of direction. We did it on our own. We felt incensed! It was not a particularly welcome role for us to be "another contractor."

A So your sense of what he meant by that was that he would take all the control and you would just be tasked to do certain chores. The program would be run by PM-16 and Geiger.

M Yes, and that's the way it turned out in a large sense, too.

A Can you describe how that change began to take place and what form it took?

M [redacted]

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A But, NSA's role, as we look at the whole history, had changed from performer to manager, with the performance now being done at the sites entirely.

M Yes. That changed somewhat in POPPY, in the later days, as the sites' effort became more creditable in NSA's eyes. It was certainly easier for NSA to accept and bless the work of the site and turn it out as reports, which they had done in-house for Missions 71Ø6 and 71Ø7.

A We have covered, pretty much, the whole program, now, from the beginnings in old Project TATTLETALE [redacted] I wonder if there is anything in general that you might like to say about the style or approach that NRL followed in carrying out this particular ELINT satellite program. There seems, as you have talked to me, to have been a particular style of management, a particular style of running this program, that is distinctive.

M Yes, there has been. I think it goes back to Lorenzen's time in the early days of POPPY. I do not think we acted so much with an awareness of how the Air Force was doing their job and a desire to be different as we did with a deliberate intent not to turn over our spacecraft to some contractor to launch for us. We wanted to be present at every single point from concept to operational exploitation. We wanted to make sure that we were getting a fair shake, be it at the launch site or in the tasking community that determined when you turned the birds on and how intensively you utilized them, or in the processing arena with NSA, or in the contractor processing arena with [redacted] We were deliberate, careful, and consistently trying to play the part of an interested taxpayer getting our money's worth out of the program. We did not believe in the concept of writing the perfect interface control document (ICD) and then letting one semi-interested party do his job up to that interface and another semi-interested party do his job back to that interface and then assume that it would all work together. We insisted on testing and examining the performance of those two devices as they were coupled together. That goes back to day one -- all the way through both POPPY [redacted] The Air Force launch team was astounded that the Navy had anybody on the pad when the countdown was in progress, or when the birds were being prepared to fly! For them, it is all done by ICD (Interface Control Documents). But our method worked for us. I am not saying that it was the only way that would work. It will work that way when you have a small program and can have highly competent people from cradle to grave, moving with the program.

[redacted]

A It became too big for this philosophy.

M Indeed. We would have stumbled over our own feet because we would have had people out there launching when they ought to have been back here building. Ours was the same government team from the POPPY ritual.

A What about the aspects of team work in this whole system? Did the NRL team stick together fairly well throughout the history of this program or were there a lot of different disagreements among them?

M I don't recall any significant disagreements at all. I recall that in the early days, prior to 197Ø or so, we had strong personalities -- Mr. Lorenzen, particularly -- who would inspire his team vigorously to rise to whatever occasion it faced. Even though it may not be their main mission in the scheme of things, he would call on people who had a POPPY interest once every two years to drop what they were doing or schedule a down time in what they were doing, and to rise and grab a hold of the POPPY job and make a winner out of it. Every one of them worked as if that was the most important thing in their realm. They did a highly creditable job, and quite often under remote and very difficult circumstances. They were sent, for example, to ground stations. They were not noted for being the winner there, they were just crazy scientists or physicists who showed up every couple of years to do something and then go off to leave the operators to do their routine job. Lorenzen had a style of motivating people that kept the team intact and hearing a single dominant cadence.

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We had some problems, occasionally, between the contractor and the Lab.'s team and they were not all the Lab.'s problems. There were ego problems both ways.

A You mentioned that Lorenzen had a strong style of leading the program. When you came in to take over, how did you react? Did you look to provide a different kind of leadership, a different approach, a different philosopy than what his had been, and if so, could you describe how you solved your mandate to march to a different drummer?

M Yes, I think that was inevitable. Some of the things that he did I found alien to my style. My style is determined, as anyone's is, by trail and error. You try it one way and if you are uncomfortable and it is not effective, you try something else. There was a period of adjustment for me before we had gone very far [redacted] I already had a year as branch head in the POPPY domain. I had eight months in the SPO and I knew those players. I really had a unique insight into both adversaries, and I felt if I did my job right as the Manager of the Advanced Projects Office, that I would take the majority of the mud from the wheels in the outside world, and the guys down in the NRL design team, doing the work, could do it undisturbed to a large extent. I think that was recognized and appreciated in the team. I got my share of mud, I will tell you! But there was a lot of reward, too.

I think our team cohered well. I keep saying team and I do want to emphasize that, because probably the basis of it all is to have a good group, that works together and is stressed in healthy ways, so if there is a strength needed somewhere in the group, it is intuitively recognized without being official. It is not long before that individual sees the need for improving his performance and the first thing you know, the weak link is suddenly as strong as the rest of the chain. It has worked that way without a lot of arm twisting and cadence calling and so on.

I think my style differs from Lorenzen's in one major way. Every job has a lot of nasty collateral aspects to it that you do not really like. It has been characteristic of me, over the years, that before I will ask anybody to do a collateral job, I will do it along with them. I think that has made them realize that we are a team. We are not superiors and inferiors. I try to keep that aspect of it completely out of sight. We are collegues. There may be subordinates but there are no inferiors. We have had an exceptionally good team and it has been a job that you might call "juicy." It is interesting. The domain of space is new and it has a certain lure to it. We have had more money than we have needed, more priority than we needed. We have suffered in some ways that were demeaning. For example, when we had a program that was financially equal to one-twentieth of the Lab.'s budget, we had a packing density in our office spaces and lab spaces that was unacceptable. But we seemed to bear up under that and enjoy the success and not dwell at length on the problems: problems which were solved in time. I guess if I had been more aggressive, I could probably have done a tantrum on somebody's doorstep and maybe solved it a little quicker. But that is not the way I prefer to work.

A Do you have any other thing to say about the program or any other reflections on it before we end the interview?

M I do not think that there exists in all of the Department of Defense, in all of government, another place where the POPPY job could have been done in house. NRL is just that unique. The Lab. has the skills and the engineering depth to get to concepts, and to make them work and to do it in a very cost effective way. I say this for the POPPY period. [redacted] The Lab. is now supporting our sponsoring organization in some areas where maybe some adjustment should take place. We are doing things that maybe should go to industry, and industry is doing some things that should come back to the Lab. I think our middle name being Research should be indicative that we should carry the burden of advancing the technology for [redacted] I expect that the Lab. will continue to press for a larger share of that domain in the months and years ahead.

The other thing about the Lab. is that during one's career at NRL, a person probably has more influence about the direction of that career than any other place I can think of. You can, in one career, work from submarines to space. You can take a single technology like crystal video and apply it across all of those domains: air, ships, ground stations, submarines, and now finally in space.

NRL has been an excellent place to work, and I am very proud to have been a part of it and to have had it as my professional house for nearly 38 years. Supporting me through major portions of this period have been the world's finest staff and valiant technical comrades-in-arms to whom I am eternally thankful for those memories I shall cherish for all time. Thanks to each one of you for your help and counsel.