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Ralph Emerson Beatty, Jr. Interview (MORS)

Beatty, Ralph Emerson Jr.

Military Operations Research Society (MORS) Oral History Project Interview of Dr. Ralph Beatty, Jr.

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MILITARY OPERATIONS RESEARCH HERITAGE ARTICLE

Oral Histories represent the recollections and opinions of the person interviewed, and not the official position of MORS. Omissions and errors in fact are corrected when possible, but every effort is made to present the interviewee's own words.

INTRODUCTION TO ORAL HISTORY INTERVIEW OF DR. RALPH E. BEATTY, JR.

In the summer of 1999, Ted Smyth, then Chairman of the MORS Heritage Committee and now a past President of the Society, and Gene Visco, FS, member of the Heritage Committee, interviewed Dr. Ralph Beatty, long-time Navy operations analyst and MORS participant. What follows is the result of that extended interview. Two sessions were held with Dr. Beatty, at his home in Alexandria, VA: the first in May and the second in July. The interviews were under the auspices of the MORS oral history project to capture experiences from operations analysts representing early contributions to military operations research—the heritage of our practice. The MORS Oral History Project is documenting the origins and early experiences of military operations research in the United States.

Dr. Beatty began his career in operations research in 1942 as an early member of the Anti Submarine Warfare Operations Research Group (ASWORG) under the leadership of Phil Morse. The ASWORG was one of the earliest of the US institutions to take on the title of operations research. The present Center of Naval Analyses traces its heritage to the ASWORG. Dr. Beatty spent essentially all of his lengthy operations analysis career with the Navy groups that took on different names through the years, starting with the ASWORG, then with the Operations Research Group (ORG), both during World War II, migrating to designation as the Operations Evaluation Group (OEG), and eventually as the Center for Naval Analyses (CNA).

In addition to his assignments with overseas commands, in the Far East and Europe, and his attachments to various fleets of the Navy, Ralph had considerable involvement with MORS. Among other contributions, Ralph, with two colleagues from the original ASWORG of the 1940s, participated in the first ever Heritage Program during the annual MORS Symposium of 1992, commemorating the 50th anniversary of the beginning of military operations research in the U.S.

Ralph Beatty's experiences, his applications of various scientific fields blended with operational analyses, as spelled out in the following oral history, are exciting, educational, and inspiring. The history will be read—devoured, perhaps—by both the young, new analyst and the old, experienced hand, for all will benefit from the words—truly words of wisdom. Among Ralph's closing comments are these:

> "One of the things I would stress is looking to the past and not forgetting the past. Pay attention to what's happened before. There might be some useful ideas there. Also, pay attention to the impact of technology on what you're doing. Make sure you master the tools for making good models. It's not necessarily the latest artificial intelligence program, but maybe it's a combination of some simpler things."

[Note: Messrs. Brian Engler and Robert Sheldon, FS, contributed valuable comments during the review of the following interview. Their suggestions are gratefully acknowledged.]

MILITARY OPERATIONS RESEARCH SOCIETY ORAL HISTORY PROJECT

INTERVIEW OF RALPH EMERSON BEATTY, JR., PhD. WITH GENE VISCO AND TED SMYTH

MR. VISCO: This is an interview with Dr. Ralph E. Beatty, Jr. The interview is being conducted at the home of Dr. Beatty in Alexandria, Virginia. Ralph, if you could start off with your early background, where'd you come from, and early education, and that sort of thing.

DR. BEATTY: My full name is Ralph Emerson Beatty, Junior—I always like to get the Emerson in there, and I am a PhD. That was the basic data on your form here. It says "Title." I don't know what you'd call me. Senior scientist is, I guess, the closest approximation to a title.

You've got my phone number and my E-mail address. My current professional affiliation is retired, self-employed consultant, although I'm not really doing much consulting right now. I do have an informal

contact with Admiral/Senator Denton—Jeremiah Denton—who's been a friend and close associate during much of my career in OR. He's the former senior Vietnam POW hero who became a senator from Alabama. He's now very much involved in the Kosovo relief operation. I'm having discussions with him, and keeping him in touch with what I learn that might be of interest.

He invited me to a meeting last year of the NDTA, the National Defense Transportation Association, in connection with shipping humanitarian relief after Hurricane Mitch and other crises.

I'm not really what you'd call "real active" in that. It's kind of a minor thing; I'm not paid. We're sending E-mail back and forth.

MR. VISCO: Where were you born, Ralph?

DR. BEATTY: I was born in Pittsburgh in 1920, went to Langley High School and then Carnegie Tech from 1938 to 1942. I've got these charts over here that my grandson prepared as a school project. [A copy of the charts could not be included due to printing challenges.]

Pull those out and then I'll refer to them a little bit. The first one covers the early periods of my life. He was giving this talk at school and pretending he was me.

I helped him a little bit with this, providing pictures and articles to illustrate my career, through high school, college, World War II—the ASWORG (Anti-Submarine Warfare Operations-Research Group) days—and then OEG (Operations Evaluation Group) through the '50s with emphasis on the 6th Fleet "Haystack" experiences. The last chart covers everything else since at OEG, CNA (Center for Naval Analyses), SAIC (Science Applications International Corp), TASC (The Analytical Sciences Corp.), and MORS—this lists the principal studies that I was involved in. I could go over some of that later, although I gather the emphasis is more on the early days rather than later.

MR. VISCO: More, but you can certainly touch on that because of the contribution it would make. And maybe we'll try to take a photograph of these later.

DR. BEATTY: Eventually, I'll probably put those on videotape and put it in a more useful, retrievable form. This first chart does document the high school and Carnegie Tech period. This is really, really old. I was into model airplanes, and I was a member of what they called the Junior Birdmen in those days. Langley High School was named after the famous Samuel Pierpont Langley who also built model planes. I was the commander of the Birdmen in our school group. We had a team that won the Allegheny County outdoor and indoor championships. This newspaper picture shows the little glider that flew over 30 minutes. My cabin model flew out of sight in about 12 minutes. We won the cup with two first places in three events.

I also gave flight demonstrations in the high school auditorium. Even today some children remember my flying the airplane out, over the audience, and back to the stage.

MR. VISCO: Terrific.

DR. BEATTY: I was, of course, interested in math. I had a very good algebra teacher who encouraged me. Although I was interested in science, math, and physics, my chemistry teacher had more influence on me than my physics teacher did.

Some of my teachers were friends of my mother, who was very helpful in getting me into college. Taking the Allegheny County test was critical. For some reason or other my name wasn't on the list to take the test. My mother found out about it and got some teachers excited enough to go to the vice principal to get my name on the list. It turned out that I outperformed practically all the other people from our school, placing near the top in Allegheny County. As a consequence, I got a scholarship to Carnegie Tech, which was the beginning of my career.

MR. SMYTH: What was your major at Carnegie Tech?

DR. BEATTY: At Carnegie I went into mathematics, although I was taking both math and physics. I took a lot of physics courses, but I got my degree in math. I was influenced by several good teachers, including the head of the department—Professor Dines a very kindly old man with a white goatee. One of the teachers—Professor Rosenbach—was very energetic in getting a Math Club started at Carnegie. I ended up being the president of the Math Club.

That was an activity that got me deeper into math by holding seminars on very special projects. We had a team that participated in one of the early Putnam examinations. Putnam was a famous math exam, given countrywide. Over 50 years ago it was just getting started.

At any rate, our team placed very high. I'm not sure exactly where I was on that, but Norman Painter and I were both selected to go to MIT on applied math fellowships. I think it had

a lot to do with our placement on that Putnam exam, and probably was related to our having both math and physics in our backgrounds.

Norman Painter and I started in the summer of '42 at MIT. Carnegie was the only school that had two people in the applied math group.

MR. VISCO: Is that P-a-i-s-t-e-r?

DR. BEATTY: Painter. P-a-i-n-t-e-r. He had connections with some of the other OR people. He was a close friend of Frank Bothwell's at MIT. He ended up working at the Lincoln Lab with Bothwell. That's how I first got to know Frank Bothwell.

That's also how I got to know Phil Morse. It was a long summer session where we were just taking courses. This was the first group of about ten at MIT in applied math.

Melvin Lax was one of them; he's at Cornell now. Steven Crandall who became the head of the Mechanical Engineering Department at MIT was another member. I've lost track of the others, but I do know Crandall and Lax are well-known.

It was probably October when the session was ending up. People were starting to get into war work at this point. I was already long overdue to start getting involved. The war was going on. Morse called me in and proposed my joining this new operation that was going on—I think he was calling it Operations Research even then. But, during those days it had the name, ASW Operations Research Group. So the words Operations Research were being used very early.

That's how I got started. It sounded interesting to me. I always liked the potential of traveling. Since I liked the applied type of mathematics anyway, that was very appealing. We started off working in Washington on all kinds of projects. However, until I got my clearance I worked in Morse's office doing some of the early search and screening calculations. The book on *Search and Screening* was largely Bernard Koopman's and George Kimball's work. They put it all together, but they brought together things that had been worked on by everybody else during the war. Koopman put the nicest mathematical flavor on it.

Some of the curves you see in *Search and Screening* come from the kinematics and geometric type of calculations I was working on at MIT. They were sophisticated encounter rate problems. For example, at what angle do you sight an object that's going along a certain course? What's the probability you're going to see an object in a certain direction. I won't go into the details of that.

But that was something you could do without a clearance. You could do it up there.

MR. SMYTH: Now when you say "up there," is most of that work that you're referring to up in Cambridge at MIT?

DR. BEATTY: Well, the work at MIT was a very small part of work that was mostly being started down in Washington.

There was an office in Boston also at that time. But the first few months (the end of '42 and early '43) I was down in Washington doing more routine types of data collection and analysis. The first project I remember was analyzing data from the CNO (Chief of Naval Operations) plot.

This was something that everybody got their fingers into. It was a picture taken of a big map showing the convoys and where the submarines were supposed to be. I didn't know it at the time but the sub positions were actually pretty accurate. We didn't realize till later that those submarine positions were something that had been determined by breaking the German code. I think that story may be in the OEG history book by Tidman. Jay Steinhardt was the one that found out about it by analyzing the accuracy of so-called DF fixes of submarine high frequency radio transmissions. There were quite a few submarines scattered all across the Atlantic. Analyzing the position accuracy and comparing with what you would estimate based on just knowing the basic accuracy of over-the-horizon intercept equipment. Steinhardt found that these positions were far too accurate.

There was something going on here that led Steinhardt and then Morse to take it to the Navy who admitted something else was going on there.

But, I did work with data collection. That was one of the main research activities there. They divided the off-shore area into zones and cells, where you counted activity. There was an analysis of how the activity varied off-shore, particularly how your air ASW capability pushed the subs off-shore.

You see where the activity is, and it's all ground up into various tables—numbers of units, contacts, and so forth—from which you get search rates. A search rate was a key measure of effectiveness. It was a moderately stable number despite all the variations.

You find out what the interaction rates are. This was one of the numbers which—as Morse said, they use decibel, or hemibel, thinking in looking at things. You get a "rough cut" at something. Don't worry about the last decimal point, but make sure you're in the right ball park in what you're doing.

This data collection provided the inputs to various analyses using search theory. Much of the basic work on search theory had been developed before I had gotten there. The elements of search theory were already in hand.

MR. VISCO: Where was the group physically located? The group was together by this time in Washington.

DR. BEATTY: We were down in the Navy Department—on Constitution Avenue. There were two World War I temporary buildings. Fairly large—located about where 18th Street and 19th Street come in. There's a reflecting pool in back. In back of the building there were a lot of other temporary buildings as well. There were some ordnance buildings—Bureau of Ordnance (BUORD) was back in there.

MR. VISCO: That's in the same area where what used to be called the Old Munitions Building was located, right?

DR. BEATTY: Right. The Munitions Building was the War—

MR. VISCO: War Department Building.

DR. BEATTY: Like the War Department. The two buildings were similar. Looking from the front, the Navy building was on the left and the Munitions was on the right. We were up on about the third or fourth deck. All wooden decks up there. There were big old offices. You worked in an area with a lot of desks, so there was a lot of communication back and forth between people. You could see what people were doing.

MR. VISCO: Ralph, at that time, did you know Germond—H.H. Germond?

DR. BEATTY: No.

MR. VISCO: He was still working there, even in the early '50s, in those same buildings. He was a mathematician.

DR. BEATTY: Yes I think he was with NDRC (National Defense Research Committee), or something like that. He was famous for some of the ballistic tables—all kinds of mathematical tables.

I didn't meet Germond, but I did have contact with Burrington and Torrance I don't know if you remember them or not. They were the chief scientists in BUORD, I believe.

I remember, because at Carnegie Tech our calculus book was Burrington, and I never liked that book! It was too abstruse; it wasn't applied enough. In fact I had the same feeling when I got more involved with similar kinds of work he was involved in on weapons effectiveness analyses for BUORD. I got involved in duels, back and forth with them, on which weapons you wanted to select. But that came out of my wartime work, and early post-wartime work.

As you say—what were some of your early projects? Well, search theory and ASW data. ASW attack errors was a very key thing I got involved in. In fact one of the first projects I had was one of the first studies of ship attacks on submarines—looking at the different fire control systems to do this.

Part of the worry was the submarine's going deeper, so, you had to get in close to the submarine to attack it. Particularly with depth charges, you had to go over it, and you would lose contact. Then the sub would be evading, and your chances of hitting it poor. Particularly, if your charges were sinking slowly and the submarine was deep and fast, then you would likely miss it.

We were doing some early tests using an attack trainer up in Boston. That was one of the projects for my first field assignment up there. We were studying the accuracy of different fire control systems, including "seat of the pants" type techniques where all you knew were the bearings and how the bearings were changing (the bearing rates), and you tried to guide your ship using a lead angle depending upon the bearing rates, trying to get on to a constant bearing course to head for the target to intercept it.

It was kind of crude, but they had a fairly sophisticated attack trainer, a pretty good simulator, where they trained people to go out to make their attacks. We collected a lot of data from those operations to find out how the attack errors depended on various factors. In Washington we actually set up our own first little, very simple, computer simulator. George Kimball was behind this. We had what amounted to a little box with a circular screen on it, like a PPI scope. You could move the target on there—the submarine target—and you could move your ship, and actually control it like a simple computer model to make attacks on the submarine, losing contact at a certain point. We played with that and made a lot of runs to use it as an analysis tool for investigat-

ing how attack errors would vary with different fire control systems, et cetera.

MR. VISCO: That'd be considered an analogue computer—

DR. BEATTY: It was very much an analogue computer. George Kimball got started on it, and then I was one of the key individuals running it and collecting data, getting different people to be the ship captains, trying to get a better idea of what it was all about.

MR. SMYTH: Did you use mostly Navy people to do that, or were they—

DR. BEATTY: Not all. Mostly Navy people were involved in Boston where we were analyzing the trainer data. But, in Washington, where we were doing some of our own research, it was mostly our own staff people. This led to getting involved with ahead-throwing systems—such as rockets and mortars.

There was concern about submarines going deeper, getting down to 1,000 feet or so—over 500 feet was pretty deep for a submarine in those days. There was also worry, towards the end of the war, that the submarines were going to come out and be faster. That was one of the coming problems we were trying to research.

I got involved in looking at things like patterns—large patterns of depth charges to use against the deeper submarines—and with two-ship attacks. They had one ship guide the other ship across so it didn't lose contact.

We got involved also with trying to analyze the British ahead-throwing system, which was called Squid, which was sent over in '44. The H.M.S. Hadleigh Castle came over, operating out of New London, I think it was. They were demonstrating this ship to the United States. It was sent over for us to observe and collect data on its capability.

Because it was the latest anti-submarine ship, it had special sonar—a 2D sonar (search light sonar), and a depth-determining sonar, which was something that we didn't have—a tilting 3D sonar you could use to measure the depth angle.

But, more importantly, it had new, longer-range, mortars on the bow of the ship that could shoot a pattern of three to six Squid projectiles. These mortars were pretty large—it was like firing depth charges. Previously, the ahead-thrown charges were either small mortars (Hedgehogs) or small rockets (Mousetraps).

You could fire a pattern of 24 of these small charges to about 200 yards range. This new weapon went out to 300 yards and was influence-fused, and also could be set to explode, so these were ahead-thrown depth charges. It was a much more potent attack system, on the idea that it could handle deeper and faster targets.

The ship was sent down to Fort Lauderdale for testing. There was a Test and Evaluation Command, sort of the OPTEVFOR (Operational Test Evaluation Force) of its day. The early OPTEVFOR started in Quonset. That's one of the first places we were sent to analyze some of the data from sea tests, mainly the paper tape recordings that were part of the fire control system. They had range recorders and bearing recorders for both types of sonars.

Ted was asking me about the name of the British ship that came over to demonstrate the Squid weapons system. I'm not quite sure where the name Squid comes from, but the Hadleigh Castle was their most modern anti-submarine warfare frigate.

Jim Tyson was involved in some of the early work on that ship. I came up to Quonset Point to observe the collection of some of this data. I also went out on one of the at-sea tests. This was my first exposure to Navy life. I went out in a small ship, like a PC, and then transferred to a whale boat, across to the Hadleigh Castle stationed about 150 miles off the coast in rough weather—about sea state 6.

Extremely rough. It was really scary. I came alongside the British ship, where we were going up and down about 10 or 12 feet, sitting beside the ship. You had to jump on to a ladder and scramble up, with people grabbing you and pulling you up.

MR. VISCO: You were about 23?

DR. BEATTY: I was just 23 years old. I barely managed to watch this exercise, but I mostly remember the ship's captain. He was one of those typical Scotsmen with a reddish beard. He quickly could tell that I wasn't quite "with it," and wanted to give me some strong drink to help me out.

I ended up getting totally seasick. It was something I still remember, how I felt, until I got back to shore and things started to settle down a little bit. I didn't want to do that again! That was a really rough experience. But I did see what was happening, and watched the ship perform.

Later, the ship was moved down to Fort Lauderdale for more extensive tests. They already had an air station down there and a surface development unit. It was called Surface

Anti-Submarine Development Detachment (SURASDEVDET). The surface detachment was stationed at the Coast Guard base in Port Everglades. That's totally transformed today. You wouldn't realize what this looks like today. I've been back and tried to find where our old building was, and it's pretty hard to find. I lived there in the BOQ, initially, while we were doing ASW testing. We worked over on the pier side with a bunch of Navy people. It was just Tyson and me, initially. We were doing a very systematic analysis of exercises, where the ship would go out and make attacks on a real submarine that was towing a surface buoy. So, the sub was pretty shallow.

It towed that buoy, so the submarine was marked. We knew where it was. While the ship was making attacks on the sub the CIC (Combat Information Center) personnel didn't know where that buoy was. Then they would shoot a practice charge from the Squid mortar, about 300 yards out, where it would make a splash. So it would mark your attack in relation to the buoy (submarine). Navy photographers on board ship took pictures of this, then we got involved in the photogrametry of the pictures to analyze the horizontal directions and distances of the splash and buoy.

From that data we could get a pretty good idea of what the true attack errors were—there was some ballistic error in there we had to account for too.

We had sailors, pretty well trained sailors, working for us. We had also some lieutenants that were well-educated. One was a musician and the other was—I forget. But they were both sonar people—trained in music. These were pretty sharp young people.

We also had several enlisted people. They were making the detailed measurements on all these pictures, and then converting them. We had data forms to convert all this, to make calculations.

In addition, we had a lot of other data from the fire control system on these paper tapes that measured ranges and bearings. What inputs were going into the fire control system to allow this analog computer to make the calculations.

From all that, we would do an analysis of component errors—the sonar location error, the fire control error, and the evasion error due to the submarine's movement, and so on.

So we really got into the "guts" of that. We wrote a pretty detailed scientific report on the performance of the Squid using physical and statistical analyses of all the attack errors to obtain the two-dimensional Gaussian error distributions, including bias errors.

While we were still doing that analysis, I came up to Washington, got married to Doris and brought her down to Florida. It was one of the first field assignments where we had the wife actually out on the assignment in the field. Ft. Lauderdale was a very nice place to be during the war. It was beautiful there, and there were other analysts down there too.

Jim Tyson was there, initially, but he turned it over to me in December. He went on to other things while I took over. I was the key OEG analyst there at the base.

MR. VISCO: This is September of '43?

DR. BEATTY: December of '44 now. There were a lot of other things going on there. It was not just tactics analysis. There was also work going on there with the MIT acoustics laboratory. They were doing tests of noisemakers. They had in fact invented the classic noisemaker, the parallel bars. It's like two pipes held together, closely held together, clamped at the ends. When you tow this device through the water the flow of the water through it makes these bars vibrate back and forth, making a huge racket.

The idea was to generate enough sound behind a ship that you could decoy an acoustic torpedo away from it. That was the concept. The noisemaker was being developed by an MIT professor—Fay—who worked for Morse, on the technical side of their acoustics. The MIT group included Peter Westervel who I got to know better after the war at the MIT acoustics laboratory where we both worked with Morse, getting PhD's in acoustics.

This was just an interesting little connection. There was also another group of ex-OEG scientists there in Ft. Lauderdale, including Jim Dobbie and Martin Klein. Both should be on your list for future interviews. I don't know where Dobbie is now, but Klein is a famous science historian (particularly on Einstein) who was at MIT and is now associated with Yale.

MR. SMYTH: I had a question about—kind of an administrative thing. How were you paid? Were you an employee of MIT, or were you being paid directly by the Navy? Or how was this group actually administered?

DR. BEATTY: We were paid by Columbia University, not MIT. The contract was under a Division of the NDRC (National Defense Research Committee) to provide scientist support

to the Antisubmarine Warfare Unit under Captain Baker in Boston in early 1942. Professor Phil Morse of MIT was head of the group (Research Group M—or ASWORG) and recruited MIT and other University scientists, including Bill Shockley from Bell Labs as Director of Research. When Baker was transferred to Washington most of ASWORG also moved there. At the end of the war the contract was switched to MIT. I'm not sure of the exact switchover. It's probably in that OEG history book. That situation held true up until '62 when they switched again and OEG became CNA. Then there was the interim period under Franklin Institute. That period of turmoil, followed by Rochester, then Hudson Institute, and then, finally, today CNA is a private corporation. But we were under Columbia and MIT for quite a long time.

MR. SMYTH: So during the war, when you first started to work there, you were technically a Columbia University employee?

DR. BEATTY: Yes, although there was an MIT connection, since Morse was running the group. I think the Columbia connection was something we didn't really appreciate. Columbia did have another group doing sonar research studies. As a matter of fact—before I got involved in the Squid analysis—I was involved with this group doing an analysis of sonar in New York City—in the Empire State Building.

So I spent three weeks or so during—I think this was early '44—I'm hazy on the exact dates and it doesn't show up on the field assignments [in the OEG history book]. But it was an interesting little change for a while. I was working with Conyers Herring, who's not strictly an OR type, but he had his fingers in this.

MR. SMYTH: What's his name again?

DR. BEATTY: Conyers Herring H-e-r-r-i-n-g. He's still alive. He's a fairly well-known solid state physicist.

MR. SMYTH: Yes I've seen the name.

DR. BEATTY: Besides Conyers Herring, there's also Shockley, and Charles Kittel who was a wartime member. Shockley's passed away. But Kittel is still alive and Conyers Herring is still alive. And those are two pretty famous solid state physicists. Kittel was in our group, while Herring consulted with our group and worked on physics of sound [explosive sound] in the sea. There was a lot of sonar work at other labs, such as Harvard, Scripps, San Diego, WHOI (Woods Hole Oceanographic Institute), and MIT. Primakof and Pekeris, for example, were doing studies of partial differential equations of acoustic transmission. Some pretty advanced studies of wakes and more sonar physics. So, there was interest in the utilization of sonar, and comparisons of the old searchlight sonar with the new scanning sonar that sends out a ping in all directions. It's like they're working on radar. The receiving beam's going around real fast, like the Aegis radar (electronics scan). Scanning a sector with the old narrow beam sonar was a slow physical process. You had to send out a ping and wait for the echo to get back, and then train the beam and ping again. There were questions about the optimum scanning procedure. Which way do you rotate? How big a sector?

I got involved with a more detailed analysis of that, where we modeled the searchlight sonar with a probability pattern, using a grid of cells—an analog version of current finite-dimensional computer techniques.

The sonar beam pattern is like a radio antenna beam. Near the center the sensitivity is stronger and it tapers off, so you have less than 100 percent coverage. Thus the detection probability is higher along the axis and tapers off in range and bearing. We represented this by the density of coverage of the small grid cells. We used a big diagram with a lot of little dots indicating cells with 100 percent detection. It was a laborious process. Move this pattern around, and mark your dots to get the overall coverage of the scanning process. It was the sort of "dirty work" it would have been so nice to have had a computer to do.

The project was a little thing that appears somewhere in one of the technical books. You can get a picture of the diagram that I drew up with the help of Conyers Herring. I was assigned to work with Conyers and the Columbia group in New York for almost a month. I enjoyed meeting other scientists there who were doing work in statistics. One of them—it may have been Molina—was into the Bell Labs queuing theory. Some of them belonged to this club downtown, near Wall Street or close by. I know it was a fancy place to go for lunch. They took me down there several times.

In Washington, I was mostly doing analysis of attack data and ship and aircraft tactics vs. submarines. Weapons effectiveness calculations were a major activity. I applied the same techniques to calculate optimum patterns for aircraft attacks on ships.

That was a special project I did for Shockley. He was shifting to become Director of Research for the Air Force, I think, and was interested in looking at the problem of attacking ships in the Pacific with sticks of bombs from planes. He wanted to know what would be the optimum spacing of these bombs, and what angle to attack the ship to maximize the hitting. He knew that I had been doing some similar kind of work with calculations on depth charges hitting submarines, so he had me make some quick calculations for him to come up with optimal intervalometer settings. You'd think they'd have somebody else doing this, but he wanted something quick to help the squadrons in the field. They needed an answer, and within a day or so, they had an analysis.

There was another time I used Gaussian probability paper for quick calculations. You've probably seen it. It's just a scatter diagram of a thousand points distributed normally. All you have to do is place the error distribution over the target vulnerable area and count hits. It's a "poor man's Monte Carlo" calculation. You can make calculations very rapidly that way. To account for elliptical errors, you just distort your target area. I used that approach for practically all my weapons calculations. It's a "quick and dirty" approach we developed by generating our own probability plots of normally distributed cells. Circular probability paper was also published by RAND, but I'm not sure when their charts were available to the Navy.

I used circular probability plots for calculating hit probability of different kinds of weapon patterns against submarines. That second poster board illustrates what I've been talking about here. That's a sample that's in the Operations Research book—the Squid problem. In fact, that came from my calculation. But as an example here it is (the Squid problem) in OEG Report 54, "Methods of OR." This is the classic. In fact, this is the original old soft-cover version. Here's the pattern of three Squid charges in the shape of a submarine. The whole pattern is distributed with a Gaussian distribution. This appears on the board and also in the book. Yes, that's my drawing, right there. Other pictures of mine appear in the *Search and Screening* book (OEG Report 56). The Squid problem was also featured in Morse and Kimball's article "How to Hunt a Submarine" (vol. 4 of Newman's "World of Mathematics"), which came from the Methods of OR book.

MR. SMYTH: That's the report that later became Morse and Kimball?

DR. BEATTY: Morse and Kimball; yes.

MR. SMYTH: Right. I don't know whether you're aware of it or not but MORS has just reprinted that.

DR. BEATTY: Yes. That's what I understand; yes. You had a better copy.

MR. VISCO: Yes. I think it would be useful if MORS would perhaps donate a copy of that to Ralph.

DR. BEATTY: Yes, because I've lost my later version. This copy is an older one, but I had a later (red-cover) copy that was borrowed from me. I'm missing some other classics, too. Report 51 on ASW in World War II was borrowed from me and never returned. There're several others. I think the Academy of Sciences has some of these books. I picked up a few there a number of years ago—about 20 years. Lee Hunt was there, and they had a supply of a bunch of old things over there. I got *Physics of Sound in the Sea* and a few things like that over there. They had a lot of the Division 6 classics sitting on the shelf, that were essentially free. I'm not sure whether that's true today, because that was some time ago.

MR. VISCO: Ralph, if I can, you referenced, a few months ago, that you had done some quick turnaround analysis, and I was just wondering, during the course of your work during the war, is there some type of a guesstimate that you can give as to what percentage of your time was devoted to quick turnaround type projects, because people needed answers right away, as opposed to perhaps more prolonged study efforts?

DR. BEATTY: Well, certainly it was under 10 percent; maybe even 5 percent. It's small. Most of them were longer-term projects.

MR. VISCO: What would you consider to be the average—

DR. BEATTY: These might be short projects compared to some today. Because it's a wartime period you did things as fast as you could. So, a couple of months would not be unusual. Some of those projects, like that Squid project, that's two, three, four months just for data collection and analysis. This other one I was telling you about was just a one-day project—that special project for Shockley, one of the few where I worked directly with him.

I had an interesting social contact with him, as shown on this second poster board—an after-dinner picture, labeled ASWORG SCIEN-

TISTS, with autographs. It was quite an experience for a young analyst to go out with some of these older scientists, more established, and well-known, or to become well-known. Like Shockley. I didn't know too much about him, really. Here's Bill Shockley on the left beside Foster Brooks. He's an OEG old-timer who is still around—he says he's got a copy of this picture on his desk and looks at it. Every year we invite him to our annual OEG Chinese Dinner party and he will call or send a note. That's me on Foster's left. Next is Peter Pearman, who was on assignment from an OR group in England, where he had been a prominent member of the Operational Research Section (ORS) of Coastal Command. Beside him is Cecil Reynolds, an Army Air Force Colonel who dealt with various people in our group on ASW matters. Finally, on his left, is the famous George Kimball.

MR. VISCO: Where was this dinner?

DR. BEATTY: This was one of the hotels in downtown Washington. I've forgotten exactly which one. We didn't do this too often, but I remember one dinner, in particular, with Shockley, where I found out about the magician side of his character. He did all kinds of table tricks, and tried one on me that led to my claim to fame—I won a bet from Shockley at dinner.

He had what amounted to a sucker bet that he tried to pull on me, assuming I'd fall for it. It involved picking something up leading you to do something you weren't supposed to, followed by—"Oh, I got ya." I was suspicious enough of him to not fall for his trick, so I won the bet, which was about a dollar, or something like that. I think I mostly won his respect. I saw Shockley the magician another time.

He came up to MIT, after the war, to give a talk on transistors in the physics lecture hall. It was the first I'd seen him since the war. He was demonstrating, using magic sleight-of-hand, the movements of holes and electrons in a semiconductor. He had little red and white balls in his hand that he could make appear and disappear. He was a magician. He was quite an interesting guy. His history has come out in a recent book [*Crystal Fire*]. There's a lot about him and his dealings with the Bell Labs group that invented the transistor and won the Nobel Prize in 1957. He set up a group on the west coast to make the transistor into something fancier. He pushed his people so hard that almost everybody left him. He was a driving force in starting Silicon Valley, but he made everybody mad by going into the analysis of race, and a lot of people hated it. That phase of his life came much later, however. Anyway, he's one of our more famous, infamous people, and I did have some connections with him, but not a lot. During the war years he traveled a lot. He was over in England doing work on the Battle of Biscay. Other senior people also made trips to London—I never did get there during the war. That's the one assignment I never got, that I would like to have had. I had practically every other assignment you could think of, but that was in the post-war period.

On the poster board, below the Shockley picture, is an IBM punch card that was put on there as a little sample of data analysis tools. The bulk of the data came from ship and aircraft attacks on submarines. So, IBM cards were used to analyze all the different attack conditions. The IBM data came from data forms that were religiously filled out after every attack. One of the problems was dealing with false attacks on wakes and bubbles, for example. The Navy had special boards to analyze each event. That involved many naval officers who had to decide how to evaluate each attack—whether it succeeded, or the extent of submarine damage.

These assessments appeared to be pretty reliable, but they involved special work and intelligence data. There were all kinds of statistical data which we were involved with—making the forms, and then analyzing them when they were sent in. Some of the work was done initially out in the field with squadrons, and ships. Eventually, the data all flowed back to Washington for final analysis and dissemination in statistical reports—a monthly tabulation of all the ASW activity and merchant vessel losses. The British also put out similar reports, covering their forces and areas of responsibility.

Part of the problem of putting these reports together to indicate what was happening involved statistical analysis most familiar to actuaries. We had many insurance company people in ASWORG to complement the other scientists that included physicists, mathematicians, chemists, metallurgists, and biologists. There were quite a few actuaries who had experience in handling this kind of "rough data" where they used IBM cards. Others got involved with the cards by helping with setting up the codes to use on the cards. Another person that can tell you a lot about that is Joe Neuendorffer. Joe preceded me [he was num-

ber 13 to join]. He's still around. In fact he's just one mile down the road.

He's very close. His physical condition makes it a little hard to get around, but he hasn't given up his bridge. Betty says they play bridge a lot. They took good care of me when I first came to Washington by inviting me for dinner at their house. They were one of the first ones to move into Park Fairfax, which was a new community near here. Other analysts, particularly some of the insurance people were there; Alan Hauter, for example, lived over there. He's up in Boston now; he's one I think you could probably get some dope from about the old days. Another actuary came to the OEG Chinese dinner here a couple of years ago. We hadn't seen John Boermeester for many years. I didn't know him too well. He was one of the actuaries that worked with the IBM cards doing statistical analysis.

MR. SMYTH: J.M. Boermeester?

DR. BEATTY: Yes. We have his address. I can get that for you. He made the trip from down in North Carolina somewhere.

MR. SMYTH: The reports that you gentlemen completed—were there briefings that you gave to Navy Department officials at the end of these studies? Or was it simply a written report? Or did you actually verbally brief officials within the War Department or the Navy Department?

DR. BEATTY: There wasn't much in the way of formal briefings during the war period that I can remember. From my point of view, results were communicated mostly through reports and memoranda. I'm sure that there must have been briefings at the high levels—such as the Morse-Kimball level. The top strata undoubtedly had their meetings with their counterparts, where exchanges and discussions would take place. The teams that did the study didn't go up and give a briefing. I didn't get involved in any of that until almost 10 years later, when I was in the field with the 6th Fleet. I remember well when Lt. Jeremiah Denton and I were flown back to Washington to brief 40 admirals about the new "Haystack" concept of operations we had started together [with the blessings of Admirals Felt and Brown]. That was a very special event.

MR. SMYTH: During this early period, were there uniformed serving naval officers working with you during these early studies? Or, was that a rare occurrence as well?

DR. BEATTY: Working closely with officers was not the usual case. Most of the time the scientists worked by themselves. However, in the field (in Fort Lauderdale, for example) I was working directly with naval officers. In Washington we were pretty much on our own. You didn't see many uniforms in our spaces.

MR. SMYTH: So the reports were the primary—

DR. BEATTY: The reports were the primary communications. We had many contacts with naval officers, but I don't know much about high level discussions and meetings. I would think some kinds of records must exist about what took place at some of these meetings. [About 1992, Abe Olshen turned over a lot of historical administrative information to a historical data group (including Carl Harris) at an ORSA meeting in San Francisco]. I do remember that one of the early reports that I specifically was responsible for on weapons analysis was not presented as a briefing by me.

The two diagrams (Squid Pattern and Spiral Search Plan) on the second poster board are samples of analysis I was doing during the war, and then after the war when I went back to MIT.

We moved up to Boston in 1946 to enter the MIT Physics Department. Although I was doing graduate work in physics, I was also working almost half-time as a research associate doing OR for OEG. Splitting my time like that made life difficult, but it was necessary financially, to make it a little easier to support my family. Our first child, Barbara, was born at Mass General in July, 1946.

I had a responsibility to do naval operations research, initially in Morse's office, and then later in the Math Department. [In 1946 Morse was writing the OR Methods book; I shared an office with Dave Mintzer, who made significant contributions to Morse's book—I remember particularly the Lanchester equations calculations]. The leadership of OEG was turned over to Steinhardt while Morse retained an oversight role as chairman of a small advisory committee at MIT. Morse got off on other things, and by '47, '48 he was getting involved with—I forget the actual timing—Brookhaven, RAND, and WSEG (Weapons System Evaluation Group). In 1947 I shifted offices to the Math Department where Professor Wadsworth was the point of contact for my OR work and I had a safe to handle classified documents. I had to spend my time going back and forth between

doing academic studies and doing OR too. In addition, the summers of '47 and '48 were spent away from MIT doing full-time work for OEG.

One of the OR projects I worked on at MIT is represented by the Spiral Search Plan diagram shown on the poster board. These were called "retiring search plans." I think that was perhaps Report No. 51, or fifty something—I forget what it was! [Actually Research Report 51 on Retiring Search plans]. These plans involved multiple ships that arrive late at a datum (point of lost contact). The problem is to determine what's the optimal way to search the expanding area of possible submarine positions. The search plans have a spiral shape. It was a rather straightforward type of calculation. It's one of the problems treated in search and screening that—

MR. VISCO: They called it "retiring?"

DR. BEATTY: Retiring. It's expanded. You're allowing for the motion of the submarine moving out from datum to get away from the ships that are trying to sweep. The expanding search. What starts as a Gaussian distribution of possible submarine positions becomes a donut distribution, or torus. The group of ships is trying to sweep out through this dynamic distribution of probable submarine positions in the most effective way. It turned out that each plan for different numbers of ships—three, four, five, six—has the ships in a line-abreast formation. It's almost self-evident that's the optimal way to search.

That's the best way to defeat the submarine that's trying to evade the line of ships. The more ships you have in a line, the harder it is for the sub to evade. Formulas describing this effect don't really show up specifically in the classic search and screening book, or most other U.S. sources [except for Alan Washburn's monograph on Search and Detection, which treats evasion of a single searcher]. A more complete treatment covering evasion of multiple ships in a line-abreast formation is given in a Soviet text. [The Soviets also calculate the optimal spacing of the ships, taking account of line penetration attempts when evasion is unsuccessful].

MR. SMYTH: Is that right?

DR. BEATTY: Yes, the Soviets do pay attention to submarine evasion. This became clear to me in 1982–1983 when I read unclassified books on Soviet naval OR. I was at SAIC at the time doing an analysis of potential threats to the Trident (SSBN) submarine. After developing formulas for line-abreast search I was surprised to find the same formulas I had derived. In my opinion the Soviets have done a good job revising and extending the classic U.S. texts. Their approach is very practical, with emphasis on simple analytical formulas.

I might add here that a former OEG colleague, Allan Rehm, became interested in Soviet OR some time ago, and was co-chair of a 1997 MORS Working Group on OR and Intelligence Analysis. Allan's interests were broad, but I don't think he got into the naval side as much as I did. There was another division at SAIC working in this intelligence analysis area. It was headed by—oh, what's his name? I remember Judy Grange was there. Starts with a B. He's famous for knowing all about the Soviets.

MR. VISCO: Right. I got an invitation to his retirement party, recently.

DR. BEATTY: Yes. Battilega is the name. He and Judy worked in the Denver office at SAI and consulted with our Trident group in McLean. We went even deeper than they had into the area of naval OR tactics, particularly ASW. Our group also included former naval intelligence officers who knew the people that became interested in what we had learned about Soviet mathematical models of naval tactics—such as line-abreast search ("curtain search" in Soviet terms) and sonobuoy patterns.

We have just jumped ahead in time from the war years to more recent times that are summarized on the fourth poster board—which includes the item Soviet Naval Operations Research on the list of SAIC Studies. The connection with intelligence analysis is a recurring theme that started in World War II. I have already mentioned the Steinhardt analysis of DF accuracy and the role of intelligence in attack assessment. Our group also worked with intelligence people during the war on the acoustic torpedo problem. I didn't participate directly in that analysis. That was Joe Neuendorffer, mostly. He was the expert on that, together with Ed Uehling, who's passed away now. He was a physicist from Seattle, Washington.

I didn't realize, till later, that Uehling was moderately famous in the physics community as a theoretical physicist. I saw his obituary in *Physics Today* that praised the Uehling formula. I hadn't appreciated all he had done. He was a quiet unassuming guy. His history is some-

thing that's worth looking at, and Joe Neuendorffer would know more about his ASWORG work. Joe worked closely with him on this torpedo countermeasure problem.

The Germans had come out with acoustic torpedoes—the T-5—which was a threat to our ships. The question was what's our best tactic for defeating it? How would torpedoes react to noisemakers towed behind the ship? The analysis was a laborious process of making a big plot and a relative plot, where the torpedo was moved manually in steps along a track in response to hydrophone signals from both the ship and the towed noisemaker. The torpedo rudder flip-flops back and forth to produce a course heading for the loudest target.

In some cases the torpedo might miss the decoy (noise maker) and hit the ship. You had to analyze how far back should the decoy be to provide the best protection. That's the kind of thing they were doing. But they needed information on how the torpedo worked—it's control algorithms. Information was obtained by interviewing submarine POWs that knew something about these torpedoes. The interrogator could ask—"How does this thing work?"

Here's an example. If you scratch the torpedo on the nose how does the rudder respond? From the outside alone some of these sailors or officers would know something like that. There was an exchange back and forth between our analysts and the interrogators from the Office of Naval Intelligence, who learned what technical intelligence was needed. I don't know where the prisoners were located—I guess they brought some special submarine POWs closer to Washington so they could get access to them more quickly.

We were very much involved in that interplay of the intelligence. That's something that you probably don't see too often. Some of that might be in the OEG history book. I wasn't directly involved in that aspect of the torpedo problem, but I was aware of the analysis approach and knew about the tests of these noisemakers down in Fort Lauderdale. As we proceed other examples of intelligence analysis will be brought up.

MR. VISCO: When you went back up to MIT for both graduate studies and work of an OR type nature in the Math Department.

DR. BEATTY: I worked half-time as a research associate, at first with Morse in the Physics Dept., and later administratively under the Math Department, where Professor Wadsworth was the contact point.

Wadsworth didn't know much about what I was doing, but he would come down to Washington to attend meetings from time to time. During this period OEG had a strong leader in Jay Steinhardt.

MR. VISCO: When did Jay take over?

DR. BEATTY: At the end of the war when Morse left for MIT. That was late '45, or perhaps early '46 Steinhardt was Director from that turnover until—

MR. VISCO: He stayed a long time.

DR. BEATTY: Yes. He was there through '62. He was finally pushed out by people wanting to take over with a new approach and new leaders they wanted to put in charge. During that entire period I had a lot of contact with Jay. Before I get back to that post-war period I should finish up with some more of the World War II activities.

One of the more important things I got involved with was the work with Steinhardt on the ASW aircraft gambit tactics. There's some discussion of that in the OEG history book, although it's not quite accurate.

This task arose in '43 when Steinhardt was assigned to the 4th Fleet in the South Atlantic, where he was involved with the design of barrier plans to intercept German ships that were going back and forth from their home bases to the Indian Ocean. Aircraft were used to not only intercept supply ships but also to search for submarines that were operating in the area.

Jay asked for my help in designing gambit plans to catch submarines. The scenario involved loss of contact on a surfaced submarine that dives when the aircraft approaches the contact. The problem was what to do next to regain contact. One of the techniques was to pretend that you'd gone away. Just don't keep flying around where you where—leave the area and give the submarine time to come up and look around then surface when it thinks it's safe. Then you pounce in on it, and hopefully catch it before it can dive again. That's the "gambit" concept. The same problem had come up in other operational areas and a variety of tactics had been tried.

The analysis I made was not a long, complicated study. It was a short, simple analysis. The OEG history account makes no mention of my role, and gives the impression the tactics were based on an extensive war game of the problem. I don't remember we did it that way;

it was rather a straightforward analysis of time factors using a square sweep about the lost contact datum. The size of the square depended on how much time was available on station and the estimated time for the submarine to decide to surface.

There was some educated guessing involved, but the plans I prepared with Jay were tried out. They apparently had some success. I don't remember seeing the full details of what took place, but Jay and Jack Lathrop were given credit for having caught several submarines down there. [see p. 39, OEG history] I won't question it. All I can say is, if they did, I was partly involved in designing the plans that were used. [My records list Beatty, Steinhardt as authors of ASWORG RR-1, "Tactics for Renewal of Submarine Contacts by Aircraft," 13 May 43]. I wouldn't swear how events really took place, but there appears to have been some kind of success—at least it's a story that got told and repeated enough, so people tend to believe it. Anyway, the fact that I was the initial designer of the plans has been forgotten somehow.

MR. SMYTH: Not any more.

DR. BEATTY: The OEG historian's account may have been influenced by Steinhardt's field letters, or other sources I haven't seen. I don't know how the record got distorted. I do know that Jay had a way of dominating things. When you worked for him he would tend to dominate what came out of it. But I was used to that. I was one of the people that got along with him. I got to know Jay during the war, when we had our first training session up in Boston at the radar school—in fact everybody went through this training. It was like a CIC school. It was radar school. You had to know something about radar. So, we'd go up there for—I don't know—it wasn't a week—perhaps three or four days. You were briefed on the radars, and saw them first hand. The radars were on the roof of the building on the waterfront. I remember it was cold up on that roof, but it was an exciting first experience with radar. You could look out over the Boston Harbor and see everything.

I went up to Boston in the group with George Kimball, Bernie Koopman (I think), Steinhardt, and several others. I was one of the junior guys. I was in that group and got a chance to know Steinhardt, and Kimball, in particular. We'd go out to dinner, then come back to the hotel for discussions. I remember George—you wouldn't believe what he was doing for fun in the hotel room while the rest were talking. He'd been briefed on radar and the so-called "telegraph" equation, which is about how the microwaves travel. There's George, deriving the telegraph equation and working it all out. [Laughter.]

MR. VISCO: For recreation, right?

DR. BEATTY: For recreation. Yes. He really knew his math and physics—he was famous as a quantum chemist too. He wrote one of the textbooks on quantum chemistry. I worked fairly closely with him on several projects and had a lot of respect for him. He was a very quiet man. I still remember having breakfast downstairs at this hotel in Boston with George sitting across from me, not talking much, although you know he's thinking most of the time. [Laughter.]

Another chance to meet people socially occurred later when I was stationed in Boston on a team and would come down to Washington for short meetings. Some times we would stay overnight at the Hay-Adams, where there was a special ASWORG suite that was used mostly by Morse, Kimball, and Shockley. It was another way you had contact. You had to meet them because there was a big suite with connected rooms. You might also see them for breakfast, or go out to dinner, occasionally. You had some chances for contact, although these men were often off to London and various places. They traveled a lot.

When I first came to Washington I was staying in a little place up in Northwest, right off Georgia Avenue, in the Petworth area with another OEG guy. The two of us had lived with a family there that was very kind to us. I remember, they even took me out to my first Washington Redskins game during the war—at Griffith Stadium. My roommate was Jack (John) Little. He died just recently. Jack's big project was making the only ASWORG movie. The movie, or animation, was about the complex air patrols flown around a convoy or a ship formation for protection against submarine attacks. It demonstrated the optimal pattern to fly a circuit about a moving formation.

When I came back from Boston in the summer of '43 Jack and I moved to another place out Wisconsin Avenue in the area of Tenleytown: In fact that's where I met Doris. She was working in Washington and staying with her aunt and uncle on Warren St., near Nebraska Ave. Jack Little and I were rooming there to the end of '43; in '44 Jack was away for a long time

on a field assignment to the CSF (Caribbean Sea Frontier). In late '44 I went on the field assignment to Ft. Lauderdale, but returned briefly in February '45 to marry Doris and take her back with me to Florida. I guess I've covered almost everything about World War II but—

MR. VISCO: But by this time, where are we in terms of the calendar?

DR. BEATTY: Well, I was jumping back to '43 there. And then '44. There were a lot of things going on from the last half of '43 through most of '44—various projects—it's hard for me to remember them all. I remember doing analyses of screens for task forces, and I have a list of most of the memoranda and reports covering not only the war years but later time periods as well. I've got a pretty long list of documents I don't have it at hand at the moment, but I've got it here somewhere and could show you one of the longest lists of publications of anybody in our organization. That's just an indication of some of the other projects I might mention.

For example, I remember an analysis of some Italian charts. We got some data from an Italian ASW attack chart—their fire control system. From this limited information I was supposed to analyze how it really worked. That was a short, several day study. I didn't know Italian, but was able to figure out how it worked. The analysis was an eight page report [ASWORG RR-31, Evaluation of Italian Naval Anti-Submarine Attack Charts, 25 March '44].

I also did another quick analysis of a modification of the fire-control for shooting Hedgehog projectiles. This ahead-thrown attack system fires an elliptical pattern of charges ahead of the ship to sink rapidly and hit the submarine target. Time-to-fire is gotten from a range recorder, which presents target traces on a sheet of moving paper. The slope of the range marks on the recorder can be measured using a clear, lined overlay to match the traces. The pivot point of the overlay is set using a little cam, or ruled plate, on the recorder frame that can be adjusted for submarine depth, speed, and Hedgehog characteristics. The Navy wanted a quick upgrade to the old cams and came to us for help. I made the calculations for ruling a new set of cam plates that must have been stamped out somewhere, and presumably issued to many ships. You would think these calculations would have been done in-house, but we could do it much faster than that system could. In a few days we had an answer.

There were other short tasks we undertook to come up with new tactics. [See, for example: ASWORG RR-53, Derivation of Firing Time for Two-Ship Attack on Deep Submarine, (Beatty), 25 May '44].

DR. BEATTY: John Coyle is still around somewhere, although he's hard to locate. I haven't seen him lately and I haven't tried to reach him since our last OEG get-together . . . [Both of us attended the Navy OR meeting in '97 in Quantico—at Bruce Power's invitation].

MR. VISCO: Yes. He hasn't been to any of the dinners?

DR. BEATTY: He hasn't been, recently. He's got an island up off Maine and spends a lot of time up there, I think. He has a house over in the District. I think if you'd call his house you might find someone who can at least tell you where he is—if he's out of town, for example.

MR. VISCO: Okay. Let's focus on about 10 minutes, wind up here—

DR. BEATTY: Yes; let's see. I was talking about World War II—mostly Atlantic ASW in '43 and '44 I was in Ft. Lauderdale in '45 when the war with Germany ended and interest shifted to the war in the Pacific. The development unit I was attached to was tasked with evaluation of sonar countermeasures against Japanese attacks on our submarines. We were studying the effects of countermeasure devices that are ejected when under attack—small cylinders with sonar transducers or bubble generators to jam or confuse ship sonars. Some of the loudest noisemakers generate sound mechanically. Other devices act as repeaters of active sonar signals. Usually, a large number of different types of countermeasures are launched together to make it difficult to be seen underwater. We were making tests at sea trying to simulate some simple Japanese sonar. Most of the Japanese were using listening systems that we imitated using a little barge, or platform, with an over-the-side hydrophone dipped in the water. There was a lot of dead time waiting for the submarine to come by. I remember catching sharks and fish while waiting for something to happen.

We also had some ASW ships go in to see the effects of all these jammers and decoys. These tactics are still important today for that matter. It was quite obvious that numbers were important and time was a factor, since the effects don't last forever—the submarine would

come out of the region of disturbance, and might be picked up again.

MR. SMYTH: What was your base of operation? Were you in Hawaii or—

DR. BEATTY: Oh, on this? That was at Fort Lauderdale. That was part of the surface antisubmarine development detachment. They also had the air detachment at Fort Lauderdale's airfield. That's where the famous five airplanes disappeared.

MR. VISCO: Right. The TBMs.

DR. BEATTY: I mentioned earlier that some OEG old-timers were at that air station: James Dobbie and Martin Klein were there. I know Martin is alive [saw him on TV recently], but I'm not sure what Jim Dobbie's condition is today. I haven't heard of him passing away, but I think he may have been ill and possibly not an interview candidate. The last I heard he was in New England, and I suspect that someone up there, like Dave Boodman, might know about Dobbie. It's worth a try because he's one of our more famous OEG members. His work was quite outstanding—particularly his analysis of multiple target surveillance which was published in the early ORSA journals.

The Soviets really picked that up. They liked it enough to include it in their own treatise on search theory. They rewrote it with some simplifications and modifications, but I got the impression they think Dobbie is great.

MR. VISCO: Koopman published the classic U.S. book on Search Theory.

DR. BEATTY: Yes; right. But Dobbie's contribution was substantial. We haven't heard from him in a long time. I hope he's still okay. He should be interviewed before it's too late.

Getting back to this current interview, after the war I went back up to MIT and worked on retiring search plans and surface ASW attack systems, while doing graduate work in physics. Doing both at the same time was difficult. Besides, during the summers, I was back full time doing ASW work. By this time the Fort Lauderdale detachment had moved down to Key West. It still had the same name, but it was a new group located again at an old Coast Guard headquarters—on the end of the pier. The facilities are still there today, and I think there is some Coast Guard activity there. On a recent visit to Key West I saw patrol boats, including hydrofoil boats, tied up there.

In the post-war period both the air and the surface development units were down there. After two short assignments in '47 and '49 I returned for a full year in '51 when I relieved Joe Neuendorffer and was, in turn, relieved by Fred Berghoefer. Both should be on your interview list. I think Joe Engel was also down there. No. It was Howard Kreiner. We overlapped. Howard was assigned to the air unit.

Before these Key West assignments I got involved in the ahead-thrown weapon controversy: Weapon A vs. Weapon B. These were both long range (about 1,000 yds) rockets with high rates of fire (5 sec. interval for the larger A, 1 sec for the smaller B). Late '46 to early '47 I wrote two memos comparing these alternatives ["Comments on Weapon A and the Problem of Attacking High Speed Submarines," 19 Dec '46; Comments on Weapon A and B," 21 Feb '47]. During the summer of '47 N. Shear and I participated in Attack-Teacher tests in Key West to collect attack error data. We prepared a joint memo ["Probability Calculations Surface Craft A/S Attack Weapons Against Type 21 Submarine—Preliminary Results Based on Attack Teacher Tests," 25 Aug '47]. I continued to work on this problem at MIT and in Washington during the summer of '48, resulting in a large report: OEG Report 57 ["Evaluation of Anti-Submarine Weapons for Use by Surface Craft," 28 July '48]. I consider this report to be one of my most significant pieces of work.

MR. SMYTH: Was that 57 or sixty—

DR. BEATTY: 57. I know both Steinhardt and Irwin Baumgarten (later OEG Director) thought very highly of this work. It should be unclassified. I've never gotten a copy, but I think it's unclassified by now. They should send me a copy of it. I'd like to see it again. It's an analysis of all the component errors, and it makes comparisons that influenced the weapon Alpha versus Bravo evaluation.

BuOrd was, of course, very involved in this weapon selection problem. My analysis tended to favor Weapon Alpha, partly because it had an influence fuse that gave a large damage area per shot—it's a lot like this Squid pattern shown here—but, with the advantage of much longer range. Part of the influence of my analysis was the fact it used more realistic sonar location errors than those used by the BuOrd analysts. I don't know where they got their data, but it wasn't very good. In their analysis, the location component was a relatively small part of the total error—and not too important. It turned out, however, from tests we made in Key West, that the location error was not nearly as accurate as they thought. When you got ac-

tual data from sea trials the location was poor, particularly with scanning sonar rather than searchlight sonar. Also, you didn't have depth determination on any of those sonars. You had to let the rocket fly and sink through the water as fast as possible. Although these rockets went down pretty quick, deep, fast targets could still be a significant problem.

My analysis, Report 57, became fairly influential and the Navy made the decision to go with Weapon Alpha. They built it, put it on a ship, and sent it down to Key West for tests. If I can remember correctly, it was on my first one year tour to Key West after I got my PhD in '50 that the Weapon Alpha rocket firing tests were conducted.

In fact, one of the ship's officers was a friend of mine, Commander Wallace, who I had met on the SurASDevDet staff on an earlier Key West tour. I visited him in Boston when his ship was being fitted out with the new weapon.

As I said earlier, that MIT period had been cut by several summers of assignments to Key West and Washington, doing strictly OR analysis for about three months. That cuts a big chunk out of what you're trying to do up at MIT. The last year at MIT was the exception when my research associate assignment was purely academic.

[Second part of interview]

DR. BEATTY: The open source discussion was a short digression to recent times. Otherwise, the interview covered events up through most of the post-war period when I was at MIT. I touched on parts of the post-MIT period that started in late 1950.

So probably, now, we ought to get into the '50s and '60s, and—

MR. VISCO: Yes. Just to make the point, we ended that first session at the 1947–48 period. So do you want to move on from there, then? Or does it repeat?

DR. BEATTY: No; it ended about there. As I said, while I was a graduate student at MIT I was working half-time as a research associate doing operations research. This meant I was slowed down trying to get my degree. Shifting gears between OR work and academic studies was really quite difficult. In addition, three summer periods were spent away from Cambridge on OR assignments. As I remember, the summer of '47 was a full time assignment in Key West at the Surface Antisubmarine Development Detachment.

The summer of '48 was spent full time in Washington, working at the Pentagon. I don't remember that period well enough to tell you what was going on. It probably involved ship ASW weapons. In the summer of '49 I was back in Key West working on the same kind of projects.

When I returned to MIT in the fall of '49 I quit my old half-time research associateship in order to concentrate full time on finishing my doctoral work. I was assigned to the acoustics laboratory to work on acoustics problems closely related to the subject of my PhD thesis. My thesis advisor was Phil Morse, who, as I had mentioned earlier, had started the acoustics lab during the war. I worked some with Dick Bolt, a senior member of the lab I had met during the war, but mostly I worked for Leo Beranek who had come from Harvard to be a professor in acoustics at MIT. As a matter of fact, I can show you an old MIT report where I am listed officially in the acoustics laboratory, working for Bolt and Beranek on things like transmission of sound through walls. My thesis on transmission of sound through double walls was completed at the end of '50.

I like to compare the listing—Beatty, Bolt and Beranek—with the more famous—Bolt, Beranek, Newman—(well known today as BBN). As a matter of fact, I gave serious consideration to joining up with them after getting my PhD, but the fact that I had already invested a lot of my career in operations research led me in that direction. I was comfortable with OR and knew I could go back to work for OEG at probably a better salary than I could make starting in a new field. Today I'm not sure. Maybe if I had gone with BBN I might have been a millionaire by now—or long ago! But I opted not to do that.

But anyway, I don't really regret it. Going back into operations research was really the thing to do. It meant opportunities to travel a lot. I got into many things in the coming decades that I hope we could cover. I probably will touch only lightly on the decades after the '50s and focus mostly on the 50's period.

I guess I should phase in here talking about what happened when I left MIT. I went back to Key West for a full year assignment starting at the end of '50.

I was involved in things like a sea trial comparison of scanning sonar versus searchlight sonar. That may have been a little earlier. I think we were also doing tests on the later

version of Squid, which was called Limbo. This was an ahead-thrown weapon with a much longer range, comparable to the U.S. weapon Alpha and Bravo projects that I had worked on, which came to fruition with the weapon Alpha being put on a destroyer and being sent to Key West. As noted in my earlier interview, I observed the testing of the weapon Alpha rocket that goes out about 1,000 yards. So it's fairly spectacular to watch. It was a while before they got it under control.

Weapon Alpha had a short lifetime. It was overtaken by the longer range weapon, Asroc, which could shoot out to about 10,000 yards, instead of only 1,000 yards. The Asroc payload was an acoustic torpedo with a range of about 1,000 yards, so that target coverage was a circle with a correspondingly large radius. Thus, the fire control problem was greatly simplified, and the problems I had concentrated on for shorter-range weapons were no longer considered significant.

Even though weapon Alpha doesn't exist anymore, and no one thinks much about it, I note that most Russian ships have something on them which has some similarities to weapon Alpha. These ahead thrown weapons shoot a pattern of rockets from 1,000 to 2,000 yards and thus provide a "last ditch" defense system to attack submarines. If something would happen to counter the long range attack systems they would have an alternative defense. I suspect that one of these days there might be a comeback for some of these direct attack weapons that don't depend on torpedoes!

MR. VISCO: Yes. These were rocket-launched depth charges?

DR. BEATTY: These were rocket-launched depth charges. They could be fused either magnetically or acoustically. Or, you could set them to go off at a certain depth, which means, in that case, you have to worry about knowing the depth of the target. Today, our sonar is not too good at doing that, particularly at longer range! The depth problem is not a worry when the torpedo can search in depth.

After about a year down in Key West I moved back to Washington where I was assigned to the Pentagon as head of the antisubmarine warfare team. I was also assigned as a Scientific Analyst to an OpNav ASW office (OP312), where I worked for CDR George Bullard on a War-At-Sea study.

MR. SMYTH: Now when you say you were assigned to the Pentagon, it was in fact the Department of the Navy, though?

DR. BEATTY: Oh, this is still the Navy. It was OEG. It was still under MIT; that was true until the break in 1962. Let's see. Where was I?

MR. VISCO: Were you physically in the Pentagon?

DR. BEATTY: That's where our offices were. It was some time in the late '50s that they started to make some shifts to Rosslyn. We finally moved out of the Pentagon in the early '60s. We were in the Pentagon for a long period (over 15 years).

I was there until the summer of '53, when I had an assignment in Japan. This was Jay Steinhardt's doing—he wanted me to be out there, partly because he had started a special project that required someone to coordinate operations in the field with Washington agencies. The project involved collection of intelligence data from operational flights, very similar to what are known as Ferret operations. This involved airplanes that flew along the coast to collect electronics intelligence—particularly along the coast of China.

I got involved in working with the ECM personnel whose main job was ELINT collection. We were using the word "fusion" for what we were doing back then. This was also a period when there was interest in our ability to conduct strategic strikes from the sea.

There was quite an interest in the ability of the Navy to attack with a nuclear bomb. At that time the primary attack weapons were propeller-driven ADs. The AD was slow (subsonic), but it had long range and could go low. Getting under the radar coverage was a big advantage.

I remember getting very much involved in all that. During that period we worked part time in a cave on the base at Yokosuka, which was an interesting secure place. I also flew down to the Philippines (Sangley Point), where I was scheduled to fly on one of the ECM flights. But when I got there a message had come from Washington that said, in effect: "Dr. Beatty is not allowed to go on that flight. His clearances are too sensitive to allow him on that flight."

I did get to fly in one of the same planes, but it was just on a test mission, where we calibrated the equipment by flying over the island of Corregidor many times, on different headings. I had real good views of Corregidor. The ECM plane was a somewhat unusual type

of turbo-prop, with something like a RAM jet—not a RAM jet—but a jet-assisted pod that could make the plane go very fast. The designation was P4M.

As noted earlier, this project was something that Steinhardt had gotten involved in, and was very interested in. Since I had the clearances, he wanted me to be out there to follow up, monitor, and run this project.

MR. SMYTH: Could you add a little bit more detailed information? You say you were out there, but who were the other members of the team and where were you located within the Navy organization in Yokosuka?

DR. BEATTY: In Yokosuka, I was the OEG Rep assigned to the Staff, Commander, Naval Forces, Far East. That meant I had my own little office there. There had been an office there for a number of years, particularly during the Korean War. Initially, I shared the office with Sid Shear, who preceded me as the OEG Rep. The Korean War was just ending up, and the very first things I got involved in were, acting as a liaison with the OEG people that were actually stationed at sea with the 7th Fleet and the carriers. There were several fleets out there.

That included the 3rd and 5th Fleets, and a number of carrier divisions. It was a pretty active period. I got involved with making arrangements for some of our Reps to make visits. One of the places where a lot of this got done was the Officer's Club in Yokosuka, where the staffs would come in to relax and have informal meetings. A lot of business got done that way.

I got to know a couple of the admirals very well—some that Steinhardt had also gotten to know. When Steinhardt came out to visit he rode a carrier all the way out to Yokosuka from Hawaii, I believe. That was quite a long trip for the director to be out there, riding on this carrier. It was with Captain Griffin, who would soon become an Admiral.

Jay was just along for this ride, getting familiar with carrier life at sea. But that's the way you make good friends with people in the Navy. Get to know them while living with them, and putting up with each other.

Griffin got to be someone who Jay knew very well, and, as a result, I got to know him pretty well too. He later became a senior officer with a carrier division and other positions leading to OP03 in the Pentagon, where we got involved in doing some special studies for him—some top secret studies of carrier operations in a strategic role in the Pacific. How many carriers you should have in groups; how well could they make strikes; and how safe were they? We got into things like how well could they be tracked by DF, by the Chinese land-based DF systems.

To give you further flavor of being on that staff, another officer I met there was Admiral Felt. He had a carrier division and knew a number of senior OEG people pretty well. He was one of the senior naval officers who really read the OEG reports and wanted to know what was in them, and appreciated them.

One day he came in, and just appeared at my office door in Yokosuka. It was a narrow office with a real nice big leather chair. I don't know where that chair came from, whether it came from the Japanese or not—I'd love to know. But it was a very big leather chair that the Admiral settled in and asked for a quick summary of what new reports are available, and so on. Meanwhile, the people who had ushered him in the building, his escorts, and so on, were just going wild.

The people in the building were expecting him to see our Admiral up on the second deck, and Admiral Felt was lost in the building. [Laughter.] He wasn't delayed terribly long, but it was just enough to shake everybody up. They finally located him in my office and quickly took him back up to meet the admiral. That was just an interesting little event, of my first meeting with him. It was significant because he played a big role in what happened when I went to the 6th Fleet in '56. That was two or three years later, when he had become the commander of the 6th Fleet, and I was the OEG rep assigned to him. But that's jumping ahead a little bit.

But covering our relationships there at COMNAVFE, I was a civilian on the staff, but was treated like an officer on the staff. I attended all the regular meetings, much of which was really boring. It wasn't something that I had a great need to know about. But in order to not miss significant things, you had to put yourself in where you were participating in the full staff discussions of every problem they got.

A lot of it might be considered OR today because people are more interested in all kinds of management issues. While some of that was not of great of interest to me, my time in Japan was very interesting. Steinhardt made several visits while I was there, including a visit when I was the OEG Rep on the 7th Fleet. He liked

Japan. So I had to entertain him when he came to Japan, taking him sight-seeing, and so forth. He fully enjoyed it and was interested in photography, as I was too. I got a fair number of pictures that we took together.

MR. VISCO: How long was a normal tour.

DR. BEATTY: Normally, that was about a year and a half, but my assignment actually went from '53 to '55, including the time at 7th Fleet. My tour was unusual, because I was accompanied by my family for most of the period. When I first went out, I was by myself. When Steinhardt visited in December '53, he decided that I needed to be out there longer, and my family ought to go too.

At first, there was some resistance to doing that. As I said earlier, my wife was one of the few, initially, during the war, to be out on a field assignment with me. But that was quite different. This was the post-war period, when it was not a policy to have the wives on OEG overseas field assignments. Part of it had to do with where would you stay and could they get housing.

But he had seemed to have discovered that it would be possible to get housing on the base, and it was on those grounds that he got my wife all set up to go out there. She went out on MSTS, and joined me right after Christmas, in January. I'd gotten a Japanese house off the base by that time, out in Kamakura, near the big Buddha. That was interesting, to have my children along to experience living in Japan. My oldest child, Barbara, was born in '46, so she was old enough to go to school by train and bus to the base in Yokosuka.

As it turned out, however, we really couldn't live on the base. That turned out to be all wrong, when my family arrived. After getting over the initial problems of setting up to live off base, we adjusted well and ended up preferring to live in Kamakura. It was better, even though the traveling took more time. I loved traveling around Japan. There were many opportunities to get to Tokyo, where we had meetings—official and unofficial—with members of other Operations Research groups living in Japan.

There was a bar in Tokyo that I think the ORO people must have discovered first, but everybody else tended to visit there when they were in Japan. I got to know some of the Air Force people pretty well, including Bob Johnson, one of the senior people working with the Air Force. I got to know him quite well, but I lost touch with him some time ago. I'm trying to think of some others whose names I'm not sure about. I remember meeting Darwin Stolzenbach, who had a beautiful house in Tokyo.

MR. VISCO: ORO.

DR. BEATTY: Yes, with ORO. There were enough others to hold joint OR sessions, where we met on a monthly basis, rotating between the groups. These included dinners or lunches, where the host would be a senior person from that base, like the Admiral or General. These meetings were a good opportunity to exchange information.

We didn't really work that closely, that often, but there were opportunities, such as war games, for example, to get together on a working level. Some of that was kind of fun. We'd go up to Tokyo and participate with all the OR people being involved, and getting to know each other. I participated in at least one of those games. I also was the host whenever they had the meeting down at Yokosuka. I remember one of the meetings up in Tokyo where the host was the now famous black Air Force general—what's his name?

MR. SMYTH: Davis.

DR. BEATTY: Davis. Benjamin Davis. I got to know him. I had met him on one of my trips out there. Even though I was in Japan, I made a couple of trips back to the States, partly for consulting with personnel there, and reporting on all our projects. When I went back on one of those trips I was flying VIP class with the general, and we were both making a stopover in Hawaii. I was going through, and he was stopping over in Hawaii for a number of days. After we were scheduled to take off again, that evening, I was surprised when he showed up on the plane. He was so upset with his treatment in Hawaii. He said he wasn't going to stay there, that this was impossible. So he was suffering that kind of difficulty back in those days. He flew back to Japan on the same plane with me, and I met him again later when he attended one of our MORS-like dinners, where he was the host.

There was a fair amount of contact out there. A lot of it was almost unofficial, only partially official, sometimes joint work. We got to know quite a few people that way.

MR. SMYTH: If I could just clarify. You went over to Japan in mid '53?

DR. BEATTY: Mid '53.

MR. SMYTH: And you returned when?

DR. BEATTY: I returned in '55, after a tour at 7th Fleet. I'm trying to remember exactly when I shifted—some time in '54, I think—to the 7th Fleet assignment. That meant that my family was left back in Japan while I was off on the flagship. Normally the ship visited many interesting places, so you're away from base for long periods.

I joined 7th Fleet about the fall of '54—maybe it was early '55. I'm not quite sure, but it was just before the offshore island crisis with China. I know that we were scheduled to make a stop in Taiwan at the naval base (Keelung) near Taipei. That was during the period when there was a lot of excitement about the Quemoy-Matsu war with China. As a matter of fact, things got so tense that our visit to Hong Kong and Singapore was canceled. We were stuck in that area for a long time.

To provide better communications we had shifted from the cruiser to a command ship. We acted as the communications center for the Taiwan Defense Command, and assisted in setting up a shore-based command in downtown Taipei. We made a lot of trips back and forth from Keelung to Taipei.

That was a period when I got to visit some Taiwanese radar sites. One was on the north tip of Taiwan. A short helo flight along the coast took us to this radar site, where we observed, on radar, Chinese planes as they took off from airfields in China. It was fascinating to watch the big CIC (Combat Information Center) board plots of all these Chinese airplanes, all written with Chinese symbols.

We also made a trip down to Kaoshiung on the southern part of the island, where we visited the Taiwan Navy base and Officers Club. A brief stop was also made at a little offshore island (Pescadores) between Taiwan and the mainland, where there's another radar site. We went ashore in small boats to the beach for picnic and beer, followed by an official visit to the Command Center.

While these activities were interesting, there was a real concern with the possibility of a war starting, and what might happen. Admiral Pride had taken over as commander of the 7th Fleet. I was working with his staff, trying to be helpful in solving the Admiral's problems. There was great interest in the junk problem—whether there might be an invasion using large fleets of junks, and how to handle this threat.

The fact is people were worrying about this problem even when I was up in Japan. One of my predecessors there had gotten involved deeply in this to the extent of getting hold of a bunch of junks in order to make tests. It was like an OpTevFor (Operational T&E Force) assignment for this person—Henry Rugo—who we haven't heard from for a long time.

He had been working out of our base in Japan when Sid Shear was there. Sid and I had overlapped a little, while he got very interested in the problems of mine clearance and tests of mine-hunting sonar. Joe Engel, who followed me to the 7th Fleet, also got interested in the mine hunting problem, particularly the problems of false targets, clustering effects, and so on

I don't know when it got started but—

MR. VISCO: The routing sheets?

DR. BEATTY: The OEG routing sheets. When CNA opened its museum for the 50 year Anniversary in 1992 some of these historic routing sheets were displayed, showing the names of all these famous people like Jay Steinhardt, Joe Engel, and others. Since everybody went by initials, everyone knew a person's initials. The key thing was the extent to which people really used this to communicate—something like e-mail.

It's like a slow version of e-mail, but there were a lot of comments squeezed on these sheets, depending on the topic. Of course, everyone was always waiting to see what Jay was going to say, because he was the director at this point. He not only "ate those up," he also "spit them out." Wrote some real biting comments; he could shoot something down real fast with a pertinent comment.

I think I have in my possession a copy of one of the old reports, with a tattered sheet on the front, with some interesting names on it. I've got it around here somewhere. But I don't know how many are actually left. There are probably not too many extant, you know. But they're interesting from a historical—memorabilia point of view.

MR. VISCO: Let me add a comment on that. At ORO, we had a routing sheet that went on reports. It had blocks where you filled in the names of the people.

DR. BEATTY: Right. The order, how it was going and—

MR. VISCO: Right. And it also had a date when they received it, when they let it go. Well, I have a lot of those reports in my personal files. A couple of years ago I lent one of these reports to somebody in the Pentagon—somebody in

the Army—to look at it. After I retired, it finally came back to the office, and the secretary sent it to me. It had the date that the report was signed out to me. And of course it had never been signed back in again, which she pointed out. The library would be after me because I had this report for about 30 years. [Laughter.] You know, it's fun to go back and look at some of those old sheets. They get tattered and torn—but that was the way the library used to send stuff out.

DR. BEATTY: Better jump on to the 7th Fleet here. I haven't gotten to the 6th Fleet yet, so better finish off the end of the 7th Fleet tour. It was a key period with all those tensions with China, and worrying about the threats of the junks. I remember, even after many years, that the Navy made many tests of junk vulnerability to various weapons. They tried all kinds of things.

They were hard to sink. Napalm was one of the more effective things to use. At least it would wipe out everybody on board. They tried all kinds of weapons. It was like a field operational test and evaluation, but with realistic targets—the junks acquired from the Chinese.

Not only were they tough targets, there were a whole lot of them. It was an OR type of problem. How do you handle this? This was one of the things we were trying to analyze. But what kind of floored me was the attitude towards that of the 7th Fleet Commander, Admiral Pride, who had made it quite clear that if there was to be an invasion like that, the most effective weapon was the atomic weapon.

I don't know whether he had approval for taking such drastic action, but I think he might have requested permission to knock them out with atomic weapons. That was to me a little scary, and it dampened research on solving the problem by more conventional techniques. I wasn't in that assignment all that long, but I do remember worrying about the possible Chinese invasion of Taiwan and how to counter it.

I was still involved, to some extent, in following the operations of our Navy flights doing intelligence collection. I was one of only a few people in the organization who had the clearance to be involved in that.

Finishing up that assignment, I was relieved by Joe Engel, and came back to Washington in '55. [I might inject here, that getting back on MSTS required messages to Washington to approve my travel; my family was no problem, but I was only accepted because I had a Dr. title that was stretched to mean medical Dr.] When we came back, we moved to the Falls Church area rather than Fairlington, where we had lived before going to Japan. We were there less than a year before moving to France for an assignment to the 6th Fleet.

I had a tour in the Pentagon where I was a scientific analyst to the nuclear warfare desk. One of the main projects that I remember there was the Navy's attempt to develop a nuclear-powered seaplane. This was something they were serious about. This was the big P—Martin flying boat—big enough to actually carry a nuclear propulsion system. It was a very advanced system that was technologically too ambitious for its time. The other project I remember was development of umpire rules for war games. I may have mentioned this earlier, but it relates to the work at 6th Fleet.

When I got to 6th Fleet, the summer of '56, there was Admiral Felt. He was just ending his tour there. We had a couple of port visits together.

Home port was in Villefranche, a really wonderful place on the bay close to Nice. We had a villa that looked out on the bay toward Cap Ferrat. I relieved Joe Engel at 6th Fleet and moved in to the same villa that he had rented. That was another connection with Joe, who had relived me at 7th Fleet.

There is a picture on the third poster board showing the USS Salem in the bay of Villefranche. Somewhere in this picture is the villa where the Engels and the Beattys lived. As a matter of fact I showed that to Joe's wife one time, when she came to the annual OEG Chinese dinner party where I had this poster on display. She was really excited to see her villa there, even though it's hard to see.

MR. SMYTH: You've had, obviously, in the first half of the 1950's, a number of overseas assignments. Were these pretty much at your request, or how many people comprised OEG at this point, and why was it that you were continually heading overseas?

DR. BEATTY: Well, I liked to travel; I was available; and, partly, Steinhardt sent me places.

MR. SMYTH: There was mutual agreement.

DR. BEATTY: Oh, yes, yes. You've seen both Joe and I were in the field a lot in that period. In my case, part of it had to do with my having the clearances, and that Steinhardt

wanted me to be out where I could use them. I don't remember, exactly, what all was involved in my going to the 6th Fleet, but I was very fortunate to have gone at that time. I considered it the best assignment I ever had. Everything was ideal in terms of all the relationships, and the acceptance of new ideas.

Excuse me. (pointing to poster.) Here is the picture showing me in my office in Japan, in Yokosuka, where Admiral Felt came in and was lost by his escorts. Here I am at my desk in my regular chair. Beside this, was a much bigger leather chair where visitors, like Admiral Felt, could sit.

But this is mostly about the 6th Fleet. I should note here that I'm now pointing to one of these poster boards with 6th Fleet pictures. Otherwise, it's difficult when you're trying to figure out later what's going on here.

As I mentioned before, Admiral Felt was there for only a short time. We were just getting involved in starting up new concepts. Preceding me was a lieutenant on the staff—Lt. Denton. You've probably all heard me mention Denton's name very much—Jeremiah Denton—just a lieutenant at the time. He and I got involved in working closely together.

Denton was a flier. He had been involved in evaluating airborne electronic warfare systems, such as the APS 20 radar. He even flew blimps to evaluate the radars on the blimps. He had gotten to know a number of senior people through that evaluation period. In particular, he got to know Admiral Felt very well. Admiral Felt had great respect for Denton and believed everything he said. Denton had a long experience of looking at radar scopes and was full of this idea of making it more complicated for people to look at the fleet with their radars.

There's a big debate about who invented this concept of dispersal of the fleet in order to make it hard to find the carriers. Even within OEG there's debate. Some of my own people give more credit to Denton than they do to me, even though I was involved in developing this concept even before I got to the 6th Fleet. I might add that it was John Coyle who first got me thinking about this concept.

John Coyle had been involved in a strategic war game where the concept of many radar targets was introduced to confuse the Strategic Command bomber people. This led to the need for umpire rules to cover such situations in war games at the Naval War College. I noted only briefly before that I worked on this problem before going to the 6th Fleet. While John Coyle was a big pusher of this concept of lots of radar targets, he tended to stress putting radar reflectors on smaller ships to make them look bigger.

There were a lot of people, though, who thought carriers were too big; they would stand out in comparison to the smaller destroyers. They thought you couldn't solve this problem too well. Denton's experience with the APS 20 radar, which is a very powerful radar, indicated otherwise. Many ships gave relatively good signals. Variations with target aspect injected an element of variability.

If the ships were all lined up parallel to the coast you could then make a comparison and probably pick out which one is bigger than the others. But as soon as you start pulling them apart and changing their direction, then it gets much harder to pick out which one is the biggest. Classification by radar becomes even more difficult when you are faced by large numbers of ship targets, such as the merchant ships in the Mediterranean. They aren't as big as carriers, but they are pretty decent radar targets, and there are a lot of them—over 1,000 merchant ships there. Of course, an essential part of the idea was to get the fleet dispersed in the background of merchant shipping. Denton and I went to work developing, implementing, and testing this concept.

Prior to the 6th Fleet project we had worked on techniques for calculating how a fleet of aircraft could locate a target in a background of similar targets. It was like hunting for scud missiles. You'd have to go down and get close enough to identify each target.

I was already making some of the very first simple models of investigation theory to determine how quickly you could find the carrier targets. With enough planes in a wave you should be able to find the target fairly quickly, particularly if you're not worrying about being shot down. As soon as you put in the threat of being shot down, then it gets a bit trickier.

The whole business of making various kinds of attacks and investigations is almost like sweeping minefields. It's a similar problem. I got very interested in the mathematics of that and made a very simple model that could be used for war gaming.

Thus, it wasn't as though we hadn't thought about this problem. We were pushing the same ideas. Denton was very strong at it, and he was really good at writing papers, like white papers, that an admiral can appreciate.

Hit all the issues real well, explain why we need to do this, and so on.

Despite our different approaches, we collaborated well. We were a team; we worked together. While he was convincing the admiral and developing tactics, I was also involved in tactical planning for conducting exercises and data collection. One of the aspects I addressed was the ASW implications. When you disperse widely, you don't have your ASW screen anymore. Now you're vulnerable to submarines. Is that going to be much of a problem?

In those days, we were only thinking about survival for four hours—long enough to launch all our nuclear strike aircraft. That was the goal. Of course we wanted to survive a lot longer than that. Some people, such as the ASW officer, weren't too happy with the conclusion that you can actually do pretty well at sweeping along without a close screen of escorts, because the submarines are going to have a hard time getting to the carrier that quickly. I always was the one that followed up with the ASW analysis to support the basic concept.

MR. SMYTH: You got to 6th Fleet in '56?

DR. BEATTY: Yes, '56.

MR. SMYTH: If memory serves, that was on or about the time of the Suez crisis.

DR. BEATTY: It was.

MR. SMYTH: Yes. Any involvement?

DR. BEATTY: We were right in the middle of it. Matter of fact, that was one of the things that sold the Haystack concept to the new admiral, who was Admiral Brown, who relieved Felt.

Felt was only there for a short time while we got started, but I still remember his departing speech. He gathers all the commanding officers to give a big presentation to everybody—all his admirals, captains, commanders, and many others as well.

And what does he do? Right in the middle of this, he has Jerry and me stand up and be recognized, and he thanks us. "I want you guys to listen to these two. They've got something important to say. Pay attention."

That was the kind of a guy he was. He was really sold that we knew what we were talking about. Before answering the question about the Suez crisis involvement, let me describe the early stages of Haystack development.

After some initial planning we began to make some initial tests of simple versions of Haystack. First, I should explain that the word Haystack came from Denton—as in find the needle in the haystack. That's one reason Denton is given credit for inventing the Haystack concept. Despite that we remain good friends: I don't take it away from him, but I still feel somewhat unfairly treated in the matter of recognition.

As I said above, we collaborated closely. We were called "the gold dust twins," because we went everywhere together, giving briefings, and working with other staffs on tactical plans. We got involved in test planning. I got involved particularly in the data collection and analysis. My job was to determine how well the attacking planes had done in finding the carriers. The opposition came from bases primarily in Italy. Carrier task forces, including logistic forces, were dispersed widely in various areas of the Mediterranean.

In general, the concept worked rather well. The search and attack planes didn't do very well at finding us. It took them a long time to find us, even in our initial dispersed formations, while we were still in the learning phase.

We had run through all the details of conducting dispersed operations. How do you handle your communications when widely separated? How do you manage your planes that are trying to land while you're operating with hardly any radio transmissions. How do you avoid giving away your location while maneuvering dispersed units?

There are many of details that I still can't go into, but this poster presents some information from a news article. This came from the Washington—was this the Post? Yes. No. Where is it? Anyway, the title is—"Cat Brown's Kittens Have Claws." [Laughter.]

DR. BEATTY: Like we're the kittens and I've got claws. It's a long article—I think a "Saturday Evening Post" article. In fact it is the "Saturday Evening Post." There's a little excerpt here.

This reporter was on board watching what was going on. He writes it up in a way that provides a summary of the concept.

He says—"Traveling with the fleet, evaluating its operations, with the unbiased eye of a layman, is a thoughtful young MIT scientist, a PhD whose job it is to determine, by cold mathematics, the effectiveness of defense and attack procedures." I won't try to give you all the other details. He goes on to explain how this new era has arrived. The fleets go out and you don't see any ships out there. It's a little scary. A lot of people were not sure about it.

The ASW officer was scared. He thought this was a stupid idea—ASW-wise. We had opposition. As a matter of fact we had a visit around this time by Edward Teller. Professor Teller arrived, in company with Admiral Heyward. He's the Admiral Heyward that just passed away.

He had a couple of Nobel Prize winners with him. They came out to visit the fleet, in particular, Admiral Brown on his 6th Fleet flagship. We went over and met them on the carrier, flew back with them and had dinner with them. Then we gave them a briefing on the Haystack concept.

I don't know whether you got that one [poster board] before, or if you took the picture.

MR. VISCO: Yes, I did. I didn't get it printed yet.

DR. BEATTY: [discussing first two poster boards] Here was the real early one. You've covered that. And here was the one during World War II showing Shockley and Kimball. What I'm looking for is not on this one, but another set of charts which has a picture of Teller and the staff, when we were greeting him. I'd like you to get that picture, because it would add to the Haystack story. Can you stop for just a second, while I see if I can put my hands on that chart?

MR. VISCO: Where were we?

DR. BEATTY: We were at the 6th Fleet.

MR. SMYTH: I think we were with Dr. Teller and the 6th Fleet.

DR. BEATTY: Yes. I was explaining this picture of the development of the Haystack tactics. [The Teller picture shows him kneeling before Admiral Brown, who is "Knighting" Professor Teller with his sword while members of the staff and the Teller Panel look on. More on this later.]

We traveled all over the place making these Haystack tests. For example, I flew in with Denton to Naples to collect the data forms from the aggressor pilots in Naples, who had been out there to attack us during one of these exercises. They told us what they thought happened, and turned over the data forms I had prepared for them, along with their track charts, narratives, and radar photography. We flew back to the ship where I could start to analyze what they really had done when they said they had attacked certain targets.

That analysis was not easy. It was pretty hard to resolve a large amount of conflicting, and frequently inaccurate, data, such as track charts and radar photos of often poor quality. It was hard work correlating data from both the offensive and defensive forces. Many attacks were on questionable targets. For example, at least one attack was probably a cruise ship off the coast of Corsica. I was doing most of this reconstruction in my stateroom, where I had a desk, file cabinets, and a safe. Often, I was working late at night with pictures and data all over the place. It was a slow difficult task to pull that all together.

When the Suez crisis developed we were out there in the middle of it. We went into one of our Haystack formations while the French and the British carrier forces were so close that the admirals were sending messages back and forth about interfering with each other. They didn't like what we were doing, and we didn't like what they were doing.

Meanwhile, we were watching everything they were doing with airborne early warning radar. This was one of the things that Denton had stressed—getting our ships to appreciate the value of looking at radar pictures from AEW aircraft.

They had the consoles on the ship—our cruiser had them. You could downlink the video picture to the ship to see what that plane was seeing. That was called Bellhop. The reliability had been so poor that hardly anyone wanted to bother with it. It wasn't something they were trained to look at very much, and they didn't fully understand the potential benefits. It was a real struggle to get operators trained to use Bellhop.

Denton was pushing it, and I was also spending a lot of time in CIC observing and encouraging Bellhop use, particularly when we got some direct land-based AEW coverage from the Willy Victors, the WV-1. That's the big Constellation plane with a big APS 20 radar. Those are very powerful AEW planes that can see ships at long ranges. We could get a link with them quite frequently. When they came out we could not only have a good picture we could also be talking to them.

In fact there were times during the Suez crisis I got more deeply involved than I wanted. Normally only officers would talk on the net to the people on the WV-1 aircraft. But Denton was away, and in the crisis I got thrown in.

MR. VISCO: You filled in.

DR. BEATTY: Yes, I was filling in, talking to the Willy Victor about what they were doing. No one on the ship knew these people and their

operations the way Denton and I did. Denton had close personal contacts and had flown with some of them. Let me make this point by relating the Christmas story.

During the Christmas period of '56 the Dentons had planned a party at his house for the crew of one of the WV-1 planes that happened to be in Nice. But something went wrong with the Denton's furnace. They had soot all over the place.

There was nothing to do but transfer the whole party to our villa. This meant moving a lot of these people, who were staying at the Ruhl hotel over in Nice. I went down there with my blue-and-white Chevy that was like a convertible with not much passenger space. But you could put people on the front and the back. [Laughter.] I must have had a dozen of these people, that I picked up at the Ruhl and drove through the streets up to our villa in Villefranche, where we had quite a party.

Also at the party was "Doc" Abbott, who was the operations officer on the carrier division we had worked closely with on Haystack operations. He was another close friend of Jerry Denton. The two ended up having races with these little cars that you can control [laughing]. They were both quite talented and competitive.

Every time I have gotten into a little athletic competition with Denton, I have regretted it. He makes fun of me. [Laughter.]

My golf shots can be erratic at times, which seems to amuse him. I admit, I'm not much of an athlete, but I enjoy playing golf for the pleasure of being outdoors. But Denton likes to excel in all kinds of sports.

We had quite an interesting time that Christmas. Getting to know people, like George Washington, who ended up in charge of all the Willy Victor planes, was mostly through Denton's knowing these people. Being a friend of Denton helped a lot.

Another interesting event I mentioned earlier was the Teller visit. One of the reasons for the visit was that Teller had just submitted an article about why the Navy needs to put its fleet under the water. In other words, he was recommending nuclear-powered aircraft carriers that can submerge. He considered ships so vulnerable that you could only save them by putting them below the surface.

He was writing a big article on this that was going to appear in various places. He was doing this for the Air Force. It was a real blast at the Navy about this problem of ship vulnerability. So, it was important to feed him this new concept.

MR. VISCO: Do you remember the date of that visit that's in the photograph?

DR. BEATTY: I can't quite place it. It'll take me a little while to spot it. I'm trying to remember whether it was before or after we made our big trip to Washington, where we briefed a large gathering of admirals on the Haystack concept. I think this was in '57. Probably in the spring of '57; something like that. I can eventually put it together but I'm not quite sure now. It's in that time period, but I don't think we had everything in order to put before him. It had to come after we had collected our latest set of radar data of all the Mediterranean. What you see here is only a partial picture of what we had shown.

We had a montage of radar pictures we'd collected. I had collected all this radar scope photography, that included high altitude pictures, but some ship data gotten when we had radar ducting, and could see ships well beyond the radar horizon. The radar was painting ships at ranges over 100 miles—that was on one of our sweeps over to the Straits of Gibraltar, where the ship traffic is quite dense.

It's not everyone at the same time, but at different times. It gives you the sense where the merchant ships are, showing where the traffic streams are. At the same time you can see areas where the distribution is more diffuse. Down along the coast and streaming into Straits of Gibraltar it gets crowded. The picture we showed to Teller was bigger and more complete than what you see on this poster. This is just a sample reconstructed from memory, illustrating the idea of what we did.

On the left is the Bull's-eye formation with the carrier at the center of a circular screen of escorts. On the right is a dispersed formation hidden in a background of merchant ships. The Bull's-eye stands out very clearly, while you have to remove the merchant ships to see the dispersed ships in the Haystack formation. We did this using a clear overlay that we pulled off to reveal the Navy ships. Even then, the carrier positions were not immediately obvious. When the overlay was put back on the naval ships appeared to vanish. With the Bull's-eye you could still see it clearly when the merchant shipping was added.

This was a very simple-minded demonstration. But, it obviously made a big impression on Teller.

MR. VISCO: Yes. I'll bet.

DR. BEATTY: It really did. In fact, he said he's going back and rewrite his whole speech. His whole paper. He canceled it and revised it. The Air Force got back to him some months later to convince him to rethink his position, and get him back on their side. [Laughter.]

To me it was very significant and very interesting. Participation in that was one of the most interesting things, I think, I've ever done.

Returning again to the Suez crisis in late '56, I remember watching the French and British carriers up near Cyprus. We could see them clearly on our radar scopes; they were in Bull's-eye-like formations. There were also amphibious ship groups that were easy to spot. Those were very important for the big invasion down to Egypt. I could see them on Bellhop. I was making crude tracks while trying to get someone excited about looking at this interesting data. Elsewhere on the ship people were studying more conventional intelligence trying to figure out what was going on. What's going on? Where are these ships? What are they doing?

"Look—those must be those amphibious ships that you're worrying about. They're over there. Get someone working on this." Well, nobody's trained to do this. You need to be able to take this picture from Bellhop (it's on a regular PPI scope), get someone to make a copy on a clear overlay, and make target tracks. It gets real tricky when you're trying to track something complex like that, but significant groups would stand out. You should be able to track them easily if you do it right. It's something Navy personnel weren't trained to do—surface tracking using AEW radar pictures. It was so frustrating trying to push this. Denton was away for part of that period and I was on my own trying to handle it.

Besides, we were right in the middle of important air activity, with bombers flying from Cyprus, right over us. In fact I would go down to CIC and watch to see what they are seeing on their radar scopes. What's their picture look like down there? Kind of confusing. . .

Then I go up on deck and I look up—there are trails showing clearly that they're in a line-abreast sweep, coming directly overhead. You could count them easily—something like 20, 30 bombers in several waves. Attacking this problem optically, you get a really good picture of what was happening.

That's another defect, that they couldn't absorb that data. I'm sure lookouts on deck were looking up and observing the trails, but the information was not getting down to the CIC in a useful way. Even with radar, they weren't tracking all those bombers very well.

A different kind of excitement during the Suez crisis occurred one night when we were tasked to enter Alexandria on a rescue mission. We were making a dash there, while making plans to take a large number of refugees on board the flagship. At the last minute another ship was assigned to take over the rescue mission. I didn't get to Africa until many months later on a peaceful visit to Tripoli in Libya. I did get to see a number of other interesting ports on that tour, including Istanbul and several Greek ports.

Rhodes, in particular, was also exciting, in a different way. That's where Jerry Denton and I were ashore together, riding bicycles together. [Laughter.]

He was ahead of me, going down the street. I was right behind him. I hadn't ridden a bicycle for a long time. My bike had these handbrakes which I wasn't familiar with. I pulled the handbrakes suddenly, they jammed, and I went flying through the air on to the street. Jerry looked back and sees me lying on the street and comes back and rescues me. [Laughter.]

You know, he made some fun of that story. About Ralph Beatty ending up on the street. There are other fascinating stories that people love to construct that I will leave to some other interview in the distant future.

MR. VISCO: Do you want to finish off that anecdote about your Rhodes adventure?

DR. BEATTY: Yes. I should say I was not seriously injured. It was something that I suffered from for a while. I wasn't sure whether I might have broken some bones or something. I never did get patched up but I felt of sore for some time. I should have gone to the doctor and gone into sick bay, and probably would have recovered faster. I think I may have cracked a rib or something.

MR. VISCO: Oh, my.

DR. BEATTY: It was something I didn't do too often. I haven't ridden a bicycle much since then.

I remember other memorable visits during that tour to the 6th Fleet. Naples was a port we visited frequently. That was one place the wives could get to more easily than others.

Majorca was another nice place to visit. I never did get to Spain proper, though I did get

to Lisbon. Jerry and I were together in Lisbon, and we were also together in Majorca. Our wives joined us there.

Another time we were on Capri with the Dentons, and took the trip up to the highest point on the island. We rode an outdoor cable up to the top where the view was spectacular. We got some good pictures of the two couples.

You can see that my wife got along real well with Jane, and Jerry and I got along well also.

MR. VISCO: Yes. Those relationships were extremely important, not only socially but also professionally.

DR. BEATTY: After leaving the 6th Fleet we continued our close contact—through the Vietnam War period when Jerry was a POW, through his time as a Senator from Alabama, to his retirement in Mobile. We still remain in close contact with Jerry and Jane and their children on a continuing basis today.

MR. SMYTH: When did you get back from 6th Fleet?

DR. BEATTY: We got back in the fall of '57—in October when Sputnik went up. We were making our return from Nice, driving up through France into the mountains, where you felt like you're isolated from civilization.

Then there was this thing about Sputnik in the newspaper. You know, what's going on?

I remember that's when we were making our return trip. I also remember Jay Steinhardt had held me over there as long as he could, to try to finish up a report on Haystack. It was an embarrassment to not have a smooth final report. This was tough stuff. No one truly appreciates the analysis that's required for this revolutionary type of project.

I was doing this single-handedly, putting all this material together. He held me over a couple of extra days to make sure I had at least something partial—representing our contribution to this effort, plus getting a commitment to continue to work on this, and complete a final report, when I got back to Washington.

I finished the Haystack report in '58, I think. It was an unusual report issued jointly by OEG and the 6th Fleet. The report was based on the data obtained from Haystack exercises Alpha, Bravo, Charlie, and Delta. I gave a briefing on that over at CNA, a number of years ago, to get them thinking about the early beginnings of information warfare.

I don't think the Navy has fully wakened up to the significance of the initial Haystack concept, now over 40 years old. I suspect the Navy may have overreacted by relying so strongly on information warfare, as though it is some new discovery.

Looking at the history of Haystack-like concepts, it seems as though it goes through cycles of about five years, when a new group comes out with a new version of it, where they give a new name to it. They won't buy the old one. It's got to be something new. That's probably why the Haystack name doesn't get used much. But it was the initial version—that's over 40 years old. [Laughter.]

DR. BEATTY: There were many other people that got involved in similar ideas. The Pacific Fleet came up with what they called random-metric distributions—or something like that. It was not quite the same thing as Haystack.

We have debates about it now, but it's gone on over the years, where different admirals will come along with different carrier groups and develop similar concepts, rediscover some of the old ideas, but push different aspects of it.

There's a long history of these developments. It goes up and down, through phases and people, where they're trying to do various version of the concept. Now they want to use islands or exploit inlets on the shoreline. Some of this is a distortion of the original concept of being able to do this far from shore. I get very frustrated when I see what they're doing or not doing.

At the same time, I'm also intrigued with the extent to which many groups are really pushing the whole idea of information warfare, which is really what this is about the Haystack concept was the first development of information warfare. It needs to be brought to the attention of those people who are selling it now. You've got to think about this aspect of it. It's not just cyber war. That's part of it; you know.

In fact you should worry more in this technological age about the extent to which all these things work and don't work. I tried to address some of these issues when I worked on Haystack reports during the period, between the 6th Fleet assignment and my tour in Hawaii from '60 to '62 at the ASW Forces Pacific Command and CincPacFleet.

One was OEG Report 77. I mentioned earlier this was an unusual report. It presents both an analysis and a description of the Haystack concept. It also includes the detailed 6th Fleet instructions on the operations.

Then there was a technical analysis of the first dispersed formations, faced by waves of attacking planes. That was OEG Report 81. It was an air defense analysis with some similarities to a typical APL defense analysis. It differs by including the concept that the attackers don't know where all the targets are. The aircraft are not coming in with all the streams headed for a small region of capital ships. We had disagreements with APL over this different approach. How do you do air defense and include effects of target location uncertainties?

One of the things I got involved in during this period was a test led by the WSEG (Weapons Systems Evaluation Group) in about 1959 of electronic warfare.

I was very much involved in those tests as a planner. I'm trying to think what I was doing at that time. I have to compose my thoughts a little bit more.

MR. VISCO: You were thinking of the WSEG tests in '59?

DR. BEATTY: Yes, the WSEG tests. I was trying to put it in relation to some of the other things I was doing too. I was the OEG rep to the Planning Committee and I remember meeting with the Navy admiral who was in charge, trying to decide what didn't we like about these plans, and what do we think needs to be changed. We were saying we have to make sure this is not a set-up, where they can come in and cream the Navy with attacks focused on known target positions. We feared we we're going to look bad if we're not able to exploit one of our main capabilities of deception to complicate the targeting problem. That would be left out of these tests.

So we kept trying to put those ideas into the test design, while trying to do all the other conventional ECM tests at the same time—checking out your Aegis type systems and all your other missile radars and electronic systems. There were a lot of people and forces involved in this evaluation. We never did get it quite the way we wanted, but we did influence the selection of the kind of threat aircraft, and some of their tactics coming in, so it wouldn't always be a set-up.

It's hard to remember all the issues that came up, but we had some basic arguments with APL about the philosophy of doing air defense—whether it's active, or passive. In OEG, we were using the term total air defense to mean the combination of active and passive defense.

I probably should have used the words information warfare, so that we could lay claim to that. But that's what we were doing. [Laughter.]

As noted earlier, OEG Report 81 was my analysis of total air defense, similar to the Haystack concept. It was an analytical model of air defense, where you're doing offense and defense at the same time. Instead of converging on a small area, the attacking aircraft are coming in streams, or channels, and don't know where the principal targets are located.

The attackers have a choice of picking targets at random for standoff missile attacks, or approaching each target to close range to attempt identification. Meanwhile, they're being shot at and lost. When the counter-targeting and active defenses are both working effectively the offensive forces suffer high attrition rates. This type of model was later extended [Atlantis III in '74] to include submarine attacks and space surveillance.

MR. VISCO: Would you care to give any advice, or suggestions for the younger analysts coming along, or even some of the senior analysts that are still around?

DR. BEATTY: One of the things I would stress is looking to the past and not forgetting the past. Pay attention to what's happened before. There might be some useful ideas there.

Also, pay attention to the impact of technology on what you're doing. Make sure you master the tools for making good models. It's not necessarily the latest artificial intelligence program, but maybe it's a combination of some simpler things.

But I'm afraid that we're somehow not training people properly in some of the more historic ways of looking at things, stressing the significance of applying simple analytical models based on operational data before constructing complex computer models.

You know, I started out with very simple investigation models before I went to the 6th Fleet. These were pretty straightforward geometric models, looking for the optimal path width. A field of random targets was divided into channels. How much do searchers deviate to see everything? How wide do you make the channels? Make it too wide and you're going to go back and forth without advancing very fast. Make it too narrow and you don't cover much of a channel. There's an optimum there that can be approximated relatively simply.

This whole concept can be extended to mine sweeping. In fact there are some models that are very similar to what I was doing in '56. My simplification of Fahs's CNA model in '79 was presented at MORS around '92 or '93, or so. It was about the time I went out to Monterey, I think.

It was an analysis showing how you could model the investigation process. Coming after Desert Storm, I applied it to the case of hunting for Scud missiles in a field of random targets. There's a simple mathematical formula for it. If you're in a channel and advance from one target to the next one on your track, you can derive a fairly complicated integral that can be calculated on a computer.

Jim Fahs did that at CNA, and got a pretty neat model. As a CNA reviewer of Fahs's paper, I derived a simpler version of it, which matched his almost perfectly.

It turns out that Robert Arnold of OEG, I think, had done the same calculation in 1956. [I only learned about Arnold's paper recently.] After the MORS meeting in Monterey, I learned that two Englishmen solved the same problem in April 1979.

When I gave my paper in Monterey, Alan Washburn, who's one of the more talented theorists in search and investigation theory at the PG School, said he didn't quite understand what I was doing. I guess I didn't present it as well as I might have. It was just a bunch of slides. I think he had gotten it but wasn't quite sure where I was going with it. More importantly, he gave me a reference to something he had seen, that was very similar, by some guys in England [M.R. Bathe and P.J. Haysman, Royal Military College of Science, Operational Research Branch, Shrivenham, September 1979.]. Done almost the same thing as a matter of fact.

Bathe and Haysman had carried out a detailed calculation arriving at a closed analytical solution in the form of an Airy integral. Pretty fancy. [Laughter.]

When I got back home, I looked at it and discovered that my simplification turned out to be the first order approximation to his Airy integral. In other words, if you take this Airy integral and expand it first order you get what amounts to my simple formula—which is like taking a Pythagorean type approach to it. The best distance here is the hypotenuse, where the sides are the average along-path and cross-path errors. Combining those mean-square errors in that simple way gives an answer pretty close to the optimal answer.

That was something I did at CNA while reviewing Fahs's paper.

MR. VISCO: So sometimes simpler is good enough.

DR. BEATTY: Yes. It's simpler, but sometimes it takes a little longer to do. That's been my problem. I get on a problem and I think I've got to simplify it I sometimes take more time than I should, just to make sure I get a simple version. But there are times when you can apply that simple approach to get a quick answer.