ORAL HISTORY PROJECT
of
The H. John Heinz III Center for Science, Economics and the Environment

In conjunction with the Colloquia Series

OCEANOGRAPHY: THE MAKING OF A SCIENCE
People, Institutions and Discovery

Transcript of the Videotape-Recorded Interview with
WALTER MUNK

Conducted at
Scripps Institution of Oceanography
The University of California San Diego
La Jolla, California
February 16, 2000

Interviewers: Naomi Oreskes and Ron Rainger

Funded by the Office of Naval Research

Video and Audio Recordings by George Washington University Television
Transcribed by TechniType Transcripts, Davis, CA

Transcription edited by Holly Given and Stephanie Mumma, March 2019

WALTER MUNK

February 16, 2000

Naomi Oreskes and Ron Rainger,
interviewers

[Note: The volume level for the interviewer's microphone is extremely low. Every effort was made to provide an accurate transcript. Where this was not possible, [unclear] is noted in the transcript.]

Naomi Oreskes: So today is February 16, 2000, and this is an interview with Professor Walter Munk at the Scripps Institution of Oceanography. Walter, tell us a bit about your background. You began working on oceanography during the Second World War. What was the first major project that you worked on?

Munk: The first major project I worked on was definitely wave prediction and was definitely war-related. And probably one of the most dramatic things that happened in my life came at a very early time. I was actually sitting in the Pentagon for a while, in connection with some meteorologic work. I had gotten out of the Army about six months earlier, started working as a civilian for the Navy and then some work for the Air Forces, and learned about the forthcoming invasion in Northwest Africa. I don't know whether you want me to give more details, but it was an amphibious landing, and it was at a time when the Allies had been losing. Time after time again, the Axis powers had the initiative. It was really the first development in the war where the

Allies were to take a step at their own initiative. When I learned about the landing, I tried to find out something about the amphibious, the wave conditions of the beaches.

During the winter at those coastlines, the wave conditions are very bad. There's a severe heavy swell coming in from winter storms in the north Atlantic. I asked to be able to learn something about the amphibious craft they were going to use, called LCVPs, landing craft vehicles personnel, and practice landings were being made in the Carolinas where the amphibious forces had a practice base, and learned that whenever waves there exceeded six feet, landing craft would broach, people would get hurt, and they would call it off. At the same time, the wave conditions in Northwest Africa during the winter are almost always worse than six feet, and it seemed like it was a catastrophe in the making, and that the only thing you could do is to attempt to predict and choose a time when there were one or two good days, and started to work on that problem.

NO: [unclear]. How did you come to be sitting in the Pentagon?

Munk: Well, I had worked for a while here at Point Loma for the Navy, and then some other problems came up and I spent about six months or more, I've forgotten how long, working for the Air Forces. So at the time I was working in the Air Force Office of Scientific Research, I think it was called. I forgot the precise name.

NO: Part of the Weather Directory?

Munk: Part of the Weather Directory, correct.

NO: How did that work? Did they come to you and say, "We have landings [unclear]" or [unclear]?

Munk: No. As far as I remember, no one was worried about waves. There hadn't been any amphibious landings previous to that. I believe the concern came up when I was briefed about ocean-oriented future operations, and this just seemed like a case that one ought to really do some work on. Of course, I had no degree. I had gotten a master's degree. I sat down for a while and wrote some kind of preliminary document in which I thought it should be possible, using weather maps, to make a prediction that would be useful.

I realized that I didn't carry enough weight to have that taken seriously, and I suggested at the time that Harald Sverdrup should be asked to come and stay with me for a few days, look at what I'd done, and then express his own idea as to whether he thought it was a feasible thing to do. It had never been done. As far as I know, and I am almost sure, there had never been an attempt to predict waves in this sense.

So Harald Sverdrup came and he spent a few days and then gave his opinion that it was indeed possible to do so and that it was important to do so. Then he and I worked together and really prepared a document that became the basis for wave prediction.

NO: [unclear]?

Munk: I was an American citizen then.

NO: [unclear]?

Munk: I don't think so. I don't think that was an essential point. The essential point was to do a good job. We were very concerned, of course, as to whether we would do a good job. It's horrible to be working on something when you know that a failure would mean loss of lives. So it's more worrisome than any other reaction.

NO: [unclear].

Munk: Yes, very difficult.

NO: [unclear]?

Ronald Rainger: There was, I think, primarily within the Navy, something that was called the Steere Code [phonetic] for a particular naval officer. But it was empirical. It wasn't very good in prediction, you know.

Munk: I have a vague memory of hearing that. Not having heard about it in a long time, could you tell me a little more about it?

RR: Well, it was code for attempting to understand surf and swell primarily, but it was strictly empirical.

Munk: Was it related to a prediction scheme or just a description?

RR: It was primarily descriptive. They tried to turn it into a predictive effort, but it was not very successful. Richard Siewell and--

Munk: Well, Richard Siewell was the man who was the commanding officer, who was the officer to whom I reported.

RR: Right. Well, he, at least at first, relied on the Steere Code and then it didn't turn out. There turned out to be a lot of discrepancies with the Steere Code. And then I gather that your work and Harald Sverdrup's work was very different and replaced that.

Munk: Yes. I don't remember the Steere Code, but when you mentioned it, it brought up some kind of a memory.

RR: It's somewhat peripheral.

NO: Let's talk about after the war. After the war, you were at Operation Crossroads.

Munk: Oh, yes.

NO: Tell us about that. What was that like?

Munk: Oh, that was very exciting. That was the bomb tests in the Bikini area. Roger Revelle was very much involved, responsible for all of the geophysical work. There were two tests: Bikini Able and Bikini Baker. Able was a surface blast. Baker was at a depth of 120 feet, I think, in the lagoon. Both were about twenty kilotons. The whole operation was amazing, being

the first underwater water shot, the whole idea about participating in an atomic explosion. The setting at Bikini was truly remarkable.

I got there very, very soon after the Bikinians had been taken away, evacuated, I mean simply removed from the habitat. I think I got there a day after the Navy transport ship had taken the entire population away. And we worked on that. We had different jobs to do.

My chief job at the time was to make an estimate of how soon after the explosions the lagoon would be radiologically safe. Bill von Arx, of Woods Hole [Oceanographic Institution], and I took that job as a joint responsibility, and it was a very amusing story, really. We had only two weeks to provide an answer, and although Bikini Lagoon is small by Pacific standards, it is not that small, some twenty miles or so, I would think. We can check on that, in diameter, and then there are many inlets and outlets. And in order to make any check, we would have to know something about the transport of water in and out of the various channels. There was no way to get around except by boat, which was very slow, and each channel had to be monitored for at least a tidal period to get any estimate of the net in-and-out flow.

We decided to do it by air. We got a hold of a little aircraft and pilot, and I became the bombardier. We set up a little bomb site, and we would drop the very, very intense greenish material that was available for life rafts so that they could be seen in case they were lost. I forgot what the chemical was. But they were little bombs that were furnished to people in the war so that if you were lost at sea, you would color the water so intensely that it could be seen from the air.

I rigged up a little bomb site that would try and hit the middle of these channels typically half a kilometer wide, and then we would come back again and again and photograph the location and follow it, hopefully, for a tidal cycle. I got to be a good bomber. We'd hit the middle of these channels.

My chief memory of that is that Bill and I were invited by the captain to come and have cocktails with him, and no matter how careful we had been with that very intense color, some of it slipped into our pants pockets and things. I had put my pants and shirt into the ship's laundry, and when we were received by the captain, we were received in dress whites, beautifully dressed, his first words were he didn't know what's wrong with the laundry, but he noticed all his whites had a greenish tint on it. And I didn't have the courage to tell him that I was probably the person who was responsible for it. [Laughter]

NO: That's great. So tell me about the science that you were doing. [unclear] but what did you learn scientifically?

Munk: We did learn what the flushing time of the lagoon is, sort of the time that if you ask how much net inflow there is and outflow to all the channels, compare that to the total volume, Bikini water, you would get some idea of how long it would take to renew the waters in the lagoon. And as it turned out, it was really quite a short time. If my memory is correct, people even were able to eat fish caught in the lagoon, provided they skinned it, at a very early time.

Bill and I spent a week sort of doing our bombing missions, and we had sort of given ourselves three inlets a day, and we had a week to do that. We left the largest, which I think was called Enyu in the southeastern corner to the last. And to our horrors, every night when we would add up our data, we would find that there had been a net inflow. We were very concerned that if we got a net inflow in the last day, we would be violating the most major law of oceanography, which is conservation of mass, and what would we do? We wouldn't dare to suggest that water was always flowing into the lagoon. To our relief, our last day showed a major outflow through a new channel.

NO: [unclear]?

Munk: That is correct, which was totally surprising. The other thing I remember we learned--and we wrote a paper on that--is that the flow into the channel was not wind-induced or tidally even induced, but wave-induced. The waves breaking on the windward side would come up. There were little surge channels in the coral reef, and the wave traveling up that surge channel would actually rise to something like one or two, half the wave height, and cause a water level rise by radiation stress. And the water would then flow downhill into the lagoon, being a wave-induced rather than a wind-induced transport, which had not previously been discussed. Not terribly important, I would say, but still something new. And we published on that.

NO: When you say you published that, did you publish it in a scientific journal?

Munk: Yes, yes. Later on.

NO: So if you had a scientific result like that, that you publish later on, would you have to wait a while before you got approval to publish it, or how did that work?

Munk: I don't remember. We published--I'd have to look at the publication. We published one about the circulation in the Bikini Lagoon, and whether that paper contained the wave-induced transport, I don't remember. But we were permitted to publish things that did not relate to direct atomic nuclear problems.

NO: Was there some kind of process you had to go through to get permission [unclear]?

Munk: Probably. I mean, the security was tight, and we all had to be cleared to work with these papers.

NO: What about your family back home? Were you able to tell people back home what you were doing, or was this all a big secret?

Munk: Well, just like it has always been, you can certainly say some things. No, there was no overall secrets saying, "I can't tell you where I'm going, but I'm going to disappear." No, no.

NO: It wasn't that bad?

Munk: It wasn't that bad.

NO: Did you say it was a bomb test? Were you married to Judy at this time?

Munk: No, no. I was not married to Judy at the time.

NO: If you had been married, could you tell your wife it was a bomb tests or [unclear]?

Munk: I don't remember that. I don't remember what the restrictions were.

NO: What about the relationships [unclear]?

Munk: Oh, with Captain Seanno [phonetic]. I think that was his name. Well, I think there was some strained relation at the time. I remember that we had great difficulty getting even very minor help with getting some boats to be assigned to us to do our work, and we had to go and

fight for it. Marston Sargent, I believe, was there. You need to check on that. He was a Scripps man. I think he was a lieutenant commander. He was helping Roger, who was the commander. And I do remember Marston blowing his stack one day when some very reasonable request from our part were not fulfilled. But, you know, you work on a Navy ship in a way that the Navy is not accustomed, and there are always problems of making it come out, and generally both sides attempt to do it, it works all right. My overall expectation is that these things can be done, but one needs to be thoughtful and considerate on both sides.

NO: [unclear]?

Munk: I remember that the captain said something about, "I'm entitled to a complement of 119 people, and I only have 112. I'm dreadfully short, and I can't spare anybody for separate duty." And then we got a little skiff, which had a crew of one, and a boat that we had 100 percent volunteer time. You know, very often small units are more likely to give you some assistance. I don't think that's a very important consideration.

NO: [unclear]?

Munk: Oh, many, yes. Of course, Capricorn was my major one where we tested an H-bomb of enormously larger proportions. It has been written up in my--1 wrote something about it in my

sixty-fifth and Roger Revelle has also said something about it. But that was a very dramatic episode in my life. John Isaacs, Roger Revelle, and I expressed our concern to the AEC [Atomic Energy Commission], the predecessor of the present Energy Department, that a fifteen-megaton explosion, which corresponds to a large-magnitude earthquake, would be held in a region that is aseismic, that is not accustomed to having any seismic shock, so it would be a major shock in this area.

When you examined mathematic features of where this test was to be held, you saw that there were aprons of sediments as if there had been slides of sediments on previous times. And so one's concern could be that a shock which was unique in magnitude would set up an underwater landslide and lead to a sloughing of sediments, and such a landslide would be more likely to set off a tsunami tidal wave than the shock itself. The shock itself was above water and would not be a good generator of a tsunami.

So we, in rather forceful terms, said that there was a danger that this particular test--Ivy, I believe. Was it the Ivy shot? Would set up an underwater landslide which could generate a destructive tsunami that would be destructive at some neighboring island places where people were living at a low level, and tsunamis could be disastrous.

We persuaded the AEC to change the strategy of this test so that the island itself where it was held, at Eniwetok, was evacuated. I'm not sure now the names. But anyhow, the whole test was held remotely, and an arrangement was made for a warning system so that if a tsunami would in fact be generated, that there was a way of taking nearby settlements and giving them a warning,

taking advantage of the fact that a tsunami is relatively slow and you would have ten, fifteen, twenty minutes, not very much, but enough to give word if you were prepared.

And what happened is that Willard Bascom and John Isaacs and others here then built small moorings, got small moorings, to be attached to nearby seamounts away from the energy, but not far away from the bomb site and have pressure recorders fastened to these moorings that would show if a tsunami in fact was generated. Tsunami would mean that the pressure would suddenly go up. Two of those were built at two neighboring sites, and Bascom occupied one and I occupied the other. We essentially stood in a two-by-two-foot raft with a pen and ink recorder in front of us, and I believe that we were as close as any other human being to this particular test, which is the biggest test ever held by the United States. And it's an experience that I do remember now, and how many years later is it now?

RR: Forty-eight.

Munk: Forty-eight, fifty. See, that's half. I remember very vividly as the bomb went off, you could feel the blast of heat immediately on your face. We had to wear--

NO: How far away were you from the blast?

Munk: A few miles. Very close. And then the mushroom cloud formed and powered over us, so it was rather as if the test wasn't at a remote distance, but you were being covered by it. Frightening experience. I observed the pen and ink recorder and it did not show any signatures. And then the way it was arranged, I will go back, is that each of us had a flag, and if we'd see something, we would give a signal. People on the horizon, who were standing by, were watching us with field glasses, and they had an open circuit to the commander of the task force, who had an open circuit to the various islands that were being considered so that there would be an immediate response, and they had a plan as to what to do if they should get that signal. Nothing happened. The *Horizon* came over and evacuated us.

What did, in fact, happen is that we had to steam away because there was a concern that there would be some radioactive fallout. We steamed away, and it rained, and it turned out the rain was highly radioactive. We had to go through the prearranged plan to take all our clothes and throw them in the ocean and take a shower and change into new clothes. It was totally unexpected, actually. It was just an emergency plan.

There was a radioactive safety officer aboard the *Horizon* whose job it had been to look out for just that possibility. I was up on the bridge when it started raining. He had been assigned to go around and stick a Geiger counter in everybody's tummy every fifteen minutes. And there he said, just like you did, there I am again [unclear], and up at the bridge we are standing and the counter went haywire. And he said, "Oh my god, it's not working." And he shook it and banged

it against the deck. But it wasn't haywire; it had started raining and we were all being rained upon.

NO: So were you worried?

Munk: Well, we did what we were told. The ship itself became so radioactive that it could never subsequently be used for any low-level work. We thought for a while that we might sue the AEC, which we could have, for the deterioration of the usefulness of the ship, but we never did. And I've often wondered, as have others, whether this had some kind of a health problem associated with it. There have been stories that people in that who did part of the test did, in fact, have some effect in life, but we never followed it up.

Then we went back a day later to where we had left the rafts, and I went aboard the raft and unrolled the paper. It was during the days when you had paper and ink recorder. I had properly, like we learned in school, timed it every five minutes, partly to have something to do so that we wouldn't get too nervous. And when I unrolled the paper, I found that very two minutes after I had left and applied the last time mark, there was an enormous signature.

NO: I've read that [unclear].

Munk: You've read that, so you know the signature was equivalent to a totally unbelievable wave, at an unbelievable time. But, of course, I never knew what I would have done. On one hand, you probably would have told yourself that that doesn't make any sense, and do nothing. But on the other hand, would you have taken a chance to say nothing if something unexplained happened that could have endangered people's lives? I don't know. I told Roger, and I believe I was thoroughly convinced that if that had happened, I would have never come back to the United States. I would have spent my life in the South Pacific islands, and I wouldn't be sitting here talking to you today.

NO: [unclear]?

Munk: Oh, probably what happened is that the instrument, which was clamped onto the cable, might have come loose and slipped down ten feet or something like that. We've never understood what happened.

NO: [unclear].

Munk: But you are concentrating on a very, very early 2 percent.

NO: [unclear]. [Interruption.]

Munk: And kind of a funny story also.

NO: [unclear]?

Munk: Sure. Now I'll get a little coffee.

NO: Oh, sure. [Interruption.]

So let me take you back to the moment of the bomb blast. I'm just curious about what you thought and what you felt when you saw the blast.

Munk: Scared. I was scared because it went so high and it went over us, or at least gave the impression. It wasn't like watching something from a distance. I think there must be fewer and fewer people who've seen H-bomb explosions in the atmosphere. I'd be interested in how many of us are left with that indelible impression.

NO: There was another question I had about when you did the calculations about what the pressure wave might be, you must have known something about the size of the blast. So that information was given to you, but that was obviously highly classified.

Munk: Yes. I think the yield of the blast was probably highly classified. Of course, the landslide, submarine landslide, isn't-- I mean, it was such a speculative thing. You didn't relate that if for so many megatons you would get that big a landslide. It was just if you had loose sediments that you subject to a unique shock, there should be a chance for an avalanche, isn't it? But the magnitude, we did try and do some calculations. I wish I'd kept my notes and records. I've been given hell by our archivist many times. Because we did make these estimates and we did ask how--the work is tsunamogenic such a landslide would be, and certainly we weren't expecting anything of the magnitude that occurred when that false signal happened. But as I said, in times like this, you don't really want to check your decimal points.

NO: Right. I had one other question about the tsunami [unclear] so obviously when the tsunami didn't happen, you must have felt relieved.

Munk: Yes.

NO: Did you worry at all that since it didn't happen, [unclear]?

Munk: We cost them a lot of money. I think I've cost the United States a number of times in my life some tens of billions of dollars, and this was the first. It was in the tens of billions of dollars, and I remember going back to--what was the appropriate BOQ? Was it possibly

Eniwetok? It doesn't matter. I went it, we were all--the thing was over. And I remember a young lieutenant jaygee coming over--they all knew that we had made these changes--and saying in a very annoyed way, "I hope you guys are now satisfied." Really very angry. A bunch of scientists not knowing what they're doing, had made a recommendation which was very expensive and turned out to be unnecessary. I think I was really not bothered by that. I said, "I would have rather lived with myself having made a mistaken prediction than having not had the courage of saying that there was a potential problem."

NO: Okay. Let's talk about your diving. You also did some dives. Let's talk about that.

Munk: Yes. We all became enamored, and Capricorn, we dove almost every day. Bob Livingstone, still a doctor here, was our doctor in charge. Bascom was a very active diver and photographer. Willard Bascom had just gone through a major illness and had had some very severe radiation treatment for cancer and was not expected to live, and he went on this expedition as being sort of his last adventure. As it turned out, he is healthy and doing well now. But it was in the early days of cancer treatments of this kind. He was very ill.

I remember his telling us that when he was diving under water with his camera, for us to stay out of his way. He wanted to photograph beautiful things like fish and coral. He didn't want any ugly human beings waving their legs in his camera. And that was against the accepted method of diving that was the buddy system. You should always be with someone else so you

could share the air and do other things. Bascom said he didn't want anything to do with the buddy system.

So one day I was down below doing something, and I noticed Bascom swimming over and photographing me at great length. I was doing something with an instrument, and I couldn't figure out what he was doing when he told us to stay away. And then I had a terrible premonition and turned around. There was a big shark behind me. I feel to this day that he wanted to get some unique footage.

NO: Sounds like it.

Munk: Shark eats oceanographer. But I have the picture. I can now document a six-foot shark six feet behind me. And I've often accused him of that. He proclaims that he was there to protect me. [Laughter]

NO: A likely story. So what was the purpose of these dives scientifically?

Munk: Oh, that was an occasion--we also had wave recorders at the bottom of Bikini Lagoon. It may have been--I'm not sure of the timing. We did make some measurements of an air-blast effect on creating waves, and I was in charge of putting wave recorders in the bottom of the lagoon to measure the dimensions ·of those waves. That's where the photography took place,

because I tried to calibrate a wave recorder by picking it up from the bottom and raising it by a certain distance over my head and holding it there, what mathematicians would call a heavy-side function calibration, where you had to stand still for a certain amount of time. And that's when this happened.

But diving was a wonderful thing. When we left, it was during the early days of diving, and we were aware of a Navy criterion of safety about diving. The Navy policy was that sharks would attack people under only two circumstances: one is if they were bleeding, some kind of a wound, and the other was when they were at the surface. So we were very, very careful to avoid this problem. If somebody had any kind of a wound [unclear], we wouldn't let them dive. And at the end and beginning of a dive, we wouldn't hang around near the skiff. We'd get started getting under water, getting through the boundary in a hurry, making sure there were no whales around.

Nine months later, it was a long expedition, when we came back we found that the Navy policy had changed and they now knew that there were cases beyond those two cases. I'm glad we didn't know that. My suspicion is that when you're afraid, you give off some smell. And they are extremely good, they might have known. [Laughter] But we swam, we dove, and the sharks would come.

My memory was, no matter where we'd go within five or ten minutes, the sharks would be there, and you could watch them and you could try and make sudden motions. But you either dove with sharks or you didn't dive. That was all over the Pacific, from the Bikini Islands all the

way to the Marquesas. It left me with an impression of how enormous the population of sharks really must be, because no matter where we were, they were there.

NO: Either that, or they really liked you. [Laughter]

Munk: No, that was true of all of us.

NO: You said an interesting thing about Capricorn in one of your autobiographical writings. You said that looking back on Capricorn, that all of the information was there to write a substantial paper on plate tectonics.

Munk: Yes, I think, on retrospect, at that time, including some of the Capricorn information which was that Russell Raitt's results, seismic results, that sediments were only a hundred meters thick and not a thousand, which was consistent with the age of the basin of a hundred million years and not a few billion, and some work on heat flow and some work on early magnetics, that on retrospect--and you know that subject better than almost anyone--one could have said that the basins were temporary and made some guesses.

I felt, as I said during that talk, that in some ways we missed the boat and that the people in England had more intuition perhaps in pattern recognition, I mean, so that they were the ones, and other people, too, but they were, I think, in some ways the most substantial people to put

data together and not the ones who had collected the data, of which Maurice Ewing was probably the most energetic, but lots of other people as well.

NO: [unclear], would he talk about that?

Munk: Oh, yes.

NO: And what did people think, do you remember?

Munk: Well, isn't it-- [Interruption]

[Note: Remainder of Tape 1, Side 1 is inaudible, about 4 minutes.]

[Begin Tape 1, Side 2]

Munk: First of all, quite a bit of it was handled through the institution, but I have no memory of ever having proposal problems. It didn't seem like the major thing. I thought that in some ways a system of highly qualified project officers with good scientific background, judging whether people were doing good work or not is a very good way of doing it, rather than depending on how well a proposal is written.

NO: Because they're different skills.

Munk: They're different skills, and proposal-writing is not necessarily a skill that's 100 percent correlated to having good scientific achievements, is it. It's a different thing. So that was not part of it. I thought, in fact, that the Navy bought two things. One is obviously learning something about the oceans in many, many different ways. The other was that they had a small group of loyal and informed people available. And those are two different words. Loyal to the Navy is one criterion, informed is another. So that if those people would go on committees, like they did, they were motivated to try and do a good job and they also knew something to do a good job. It doesn't do you any good, no matter how loyal you are, to be on a committee and act on a subject of which you have no knowledge.

NO: Are you speaking of people in the Navy?

Munk: No, no, I'm talking about the civilian scientists like myself who were supported and were loyal and were informed, and therefore could help in cases where they wanted our advice. So many committees in the world are staffed by people who don't really know enough about the subject to be useful. By having supported a group of people in our field, they had available a backlog of people who were loyal and informed. That alone, I think, did some good in the

history of the Navy of that time for many, many years and including times of emergency when you needed a quick reaction like when our bomb was lost or the submarines were lost and you needed some immediate reaction of people that could help, and that was always available. Within a day, you know, things could be done.

NO: So were you involved of either of those, the *Thresher* search or the [unclear]?

Munk: In a peripheral way. I was not one of the chief actors, but knew about it and certainly knew something about the *Thresher* search. There were other people who done the good job on that.

NO: But the point is they had the team of people they could call on.

Munk: That's correct. Loyal and informed. And I want to emphasize that those are two separate, but both necessary, in order to get something, get some help.

NO: One thing I found last summer was a notebook of Allyn Vine, a diary, and there's one entry of a 7 a.m. phone call from Washington, *Thresher* gone down. And I guess he took the train [unclear].

Munk: Right. Well, that's a wonderful case in point of what I'm talking about.

NO: So in the post-Capricorn period when ONR began to fund oceanographic work in a big way, what was the most interesting or important thing that you worked on?

Munk: For me at the time, I really wish I would look at my publications at the time. I worked for a while--well, I did some work on longer period waves, tsunamis, not yet tides. I did some work on ocean circulation, which was really not of any particular interest to the Navy, but I believe they supported me.

NO: So how did you decide whether ocean circulation or how do you decide what to do next?

Munk: Because Henry Stommel had written a very interesting paper on western intensification, and I was going on a sabbatical to Norway, and was concerned, had thought about this problem. This is the same time that Harald Sverdrup did his work on what's now called Sverdrup dynamics, which is the same general principle that leads to western intensification, only what it means in the open sea, away from boundaries. I guess what I wanted to do is to put the two together into one field, one kind of a circulation model that we'd use to the Sverdrup dynamics away from boundaries into the Sverdrup dynamics at the boundaries.

NO: So how did you understand the Navy's interest in that? Nowadays people talk about basic or applied research [unclear], and many people have said one thing that was good about ONR was that it supported basic research, yet at the same time it is obviously mission-driven that this is going on as part of the Navy. So how did you understand the balance between basic science and mission [unclear]?

Munk: Well, it's a gray boundary, obviously. There's certain things which ONR should and did not support. Plate tectonics as a goal was certainly as far from the Navy's mission as you could get. But still, the techniques that eventually solved the plate tectonics problem, the magnetics, the seismics, the gravity, and the heat flow--no, not the heat flow, but the first three became other tools that are used in a hundred different ways in Navy-related things, so that even for something chosen to be as remote from Navy mission as plate tectonics, some degree of support of the kind of work that was done eventually paid off.

NO: So it was kind of a coincidence?

Munk: That was a coincidence. The wind and ocean circulation was certainly not of interest to the Navy.

NO: Why not?

Munk: Well, it's again a subject maybe not quite as remote as plate tectonics, but as formulated at that time, is not an obviously Navy-oriented mission.

NO: Did it seem too general or too abstract?

Munk: Too general and too abstract. Yet what happened later, understanding mesoscale features, their effects on sound transmission, that means that it was part of the general background of ocean understanding which the Navy did and should have had an interest in.

NO: So what are the things that you can recall not being able to get funding for that you wanted funding for?

Munk: I do not recall ever being turned down.

NO: That's nice. [Laughter] Not too many scientists can say that.

Munk: Either by the Navy for getting money, nor by a publication for publishing until I had a paper turned down that I wrote with Roger Dashon. It was probably one of the best things we've done. [Laughter]

NO: What was it about?

Munk: Well, the paper by Roger Dashon and I was on acoustics, very relevant, about how acoustics behaves on a wedge in shallow water. It was turned down, and we were too bored to go back and rewrite it, so it was never published. That's not an important issue.

NO: Just one more thing about ONR, because you were saying that you didn't have to write proposals. But over time ONR did become more formalized and bureaucratic.

Munk: Yes. Well, yes, it did. But then at about fifteen years ago, I received the Navy chair, Secretary of Navy chair. Do you know about what I'm speaking about?

NO: Kind of. I know the title, but why don't you tell us a little more about it.

Munk: Well, under Secretary [John] Lehman, it was decided to appoint three people to a chair. The chair paid $200,000 a year, the money not to be used except in fraction for salaries but in research. That essentially meant that I have not had to write a proposal since that time. The chair has been renewed. The institution is going on. They are now five chairs, very well chosen, I think, and it's been wonderful. It freed me of the need to write proposals until we came to the

ATOC [Acoustic Tomography of Ocean Climate] problem. I have to make a separate situation here. ATOC is different. And it also meant that when I went past the normal University of California retirement age, I could pay me a certain amount of permissible salary without having to use up a university FTE that would ordinarily go to younger people. And that's how I've managed ever since.

NO: Since you mentioned ATOC, let's talk about that a little bit. That obviously was one of the more [unclear].

Munk: That's one of the cases when I cost the government tens of millions of dollars.

NO: [Laughter] [unclear].

Munk: The third one is Mohole.

NO: Oh, okay. We'll get to Mohole. We'll skip you around chronologically, though, okay? So, ATOC, Acoustic Tomography of Ocean Climate, okay. So that was part of a Pentagon initiative to take military technology and apply it to climate control.

Munk: Yes, correct. It's called Sverdrup Environmental Research. It came out of the Pentagon and just for what you've said, to apply known technologies to environmental problems. We did receive a very large amount of money that was certainly not ONR and came into a very difficult time because of opposition from environmental groups.

NO: Did that surprise you? I mean, were you taken aback by it?

Munk: Totally. We had done some acoustic work in tomography for fifteen years by then, and it never occurred to us that there would be a problem. The problem came up totally unexpectedly and might have never happened if it weren't for one of two accidents of some people becoming interested in it and then leading to a *Los Angeles Times* first-page article which proposed that we were going to kill 750,000 whales. I think without that article it would have never happened.

NO: So how do you understand that now? Do you think [unclear]? Do you think people, because the technology was unfamiliar, they didn't understand it and were distrustful because it had to do with the military? What sense do you have?

Munk: Well, that was all part of it. Well, I think we're to blame for two reasons. One is, we should have never used decibel units, dB units, which are used widely, because these are relative

units and they were misunderstood. The loudness of our work here, 195 decibels in the appropriate unit, is 250 watts. In air, 195 dB would be 250 million watts; 250 million watts would be destructive; 250 watts is not. The *Los Angeles Times* article did not know the difference, did not check with us, and made that mistake of a million to one.

Later, when they came out properly and said that it had been a mistake, by that time a few environmental groups who had decided that was a good issue for them, decided it was bad anyhow. Whether it was 250 million watts or 250 watts was not an essential problem. And I've never gotten over this. I thought that the group that opposed us, that most of them were looking for an issue and were not concerned about the quantitative aspects, the one I've just mentioned. It did not seem to think that the difference of a million to one was an essential one.

And even today there are publications--there had been one recently--about noise in the sea, that one of the environmental groups has decided to continue making an issue. There was a piece of paper with three columns. On the left, here's what the Navy uses for submarine detection. In the middle, this is the noise emitted by global shipping. And on the right is ATOC. And I don't want to put numbers in. I'm not against shipping. I'm certainly not against Navy. But the differences in audio magnitudes are of the same kind that we have just discussed, and to put these side by side is like saying I don't like to climb mountains, but I don't like an anthill either. [Laughter]

NO: That's a good analogy. So what happened to ATOC [unclear]?

Munk: Well, we were held up for about years, and it cost us over a million dollars in legal fees. But most of important of all, we lost our momentum. Thinking back now, at the time when we started the idea of using long-range transmissions to measure the inherent heat content of an ocean basin was very exciting to us, but to most other people, and that's why we got our support. After three years of fighting and not being able to do our work, the excitement passed to some other ideas.

So my interpretation is that we eventually demonstrated that it is not harmful to marine life. I don't think any people question that now. And we won the battle, but we lost the war because by now other techniques have developed and it was no longer exciting and we'd lost our momentum. I think the technique is not going to die. It's being used by others today in the arctic very successfully. In Germany, the Japanese are doing some work. The French are. But I think that our own group, which were the initial discoverers of this method, did not keep that. We lost that place. [Interruption]

NO: [unclear]?

Munk: Well, I'm not objective about this, because we had a difficult time, but I think part of the reason that there is significant opposition in America to environmental proposals must be similar to my reaction, the feeling that in some instances these oppositions were not really based on

good facts and good knowledge and had something to do with groups trying to find issues which would be popular and which would give them some money. I mean, I have seen lawyers in these groups who did not understand the problem, but who wanted to get paid.

NO: So you said the other place you cost the U.S. Government a lot of money was Mohole.

Munk: Oh yes.

NO: So tell us about Mohole. Tell us how Mohole got started.

Munk: Well, Mohole got started when I sat on a review committee of the National Science Foundation with Harry Hess, and we had reviewed a particularly dull set of proposals and said, "Now, why don't we see something exciting? What would be the most exciting thing to do at the time?" And I don't quite know who said what, but we said, "Well, we ought to get a piece of mantle and see what it's really made of." We had this ridiculous group that still exists today, called the AMSOC group, just founded by ONR people in jest, just to have fun.

NO: And that's the American Miscellaneous Society.

Munk: That's the American Miscellaneous Society. And we said, "Well, this is a good project for the American Miscellaneous Society." And so we got together, I think it was on our patio, so it is said, at a breakfast and made a proposal to NSF on behalf of the American Miscellaneous Society. And although we were a society of no standing whatsoever, it had most of the people in the country that--members of the Academy, for example, like Revelle and Ewing and Hess and others. The NSF declined considering a proposal by an organization like AMSOC, but it was easy just to retype the proposal on Academy stationery, and that was accepted.

NO: Excuse me for a second. Why did you go to NSF with that and not ONR?

Munk: ONR? That is an example of a topic, like plate tectonics, which was really not suitable for ONR support.

NO: So you retyped it on Academy stationery?

Munk: Yes, and were funded for something like a million dollars for a feasibility test, and did what I really think that first two years was under Willard Bascom's leadership, did a good job. For the first time it was learned how to station-keep without dropping an anchor, using acoustic transponders placed on the sea floor, which will reflect back from a ship's source and give the instantaneous location of the vessel with all six degrees of freedom, up and down, right and left,

forward and back, and the three rotational modes that were automatically fed into three outboard motors located along the ship's hull, which could maintain constancy in orientation, position, and that worked very well and that has become so standard. But the idea that you could do that as a prerequisite to drilling had not been considered by any of the oil companies. They still were working on land. So that was a very radical achievement.

Anyway, we did have an experimental drilling exercise off Guadalupe Island off Mexico in 14,000 feet of water, I think. Drilled successfully through the sediments into the hard rock at the time, and it worked exceedingly well. John Steinbeck was our raconteur and wrote this up in a wonderful article in *Life* magazine that is still worth reading. Roger, Gustaf Arrhenius, and Willard Bascom, and the same group, and myself, were very much involved. It was a fantastic adventure.

We came home convinced that we had world by the--what's the right word?

RR: By the tail.

Munk: By the tail, and that we had demonstrated it could be done and it was a good experiment. We did not realize that the very success carried in it all the seeds of failure, because the success had indicated to some large companies that maybe it was worthwhile for them to bid on becoming the general contractor. The company that did bid-- [Interruption]

As everyone knows, Brown and Root, nobody quite knows how they were selected. They were ten out of ten.

NO: But we know now. [Laughter]

Munk: We know now. Turned out to do a poor job. We eventually actually favored stopping the project. In a way I think it is a repetition of my experience with ATOC many, many years later when we came back from the Heard Island experiment which was our preliminary Guadalupe. Can you do it? Can you propagate over long distances? And we got resounding yeses. I thought we would have the world by the tail, and we didn't realize that the success of that expedition and the publicity that went with it had aroused the interest of some groups who then decided to oppose us. So I feel as though I have gone through this twice in my life.

NO: So do you think the message is that you should be quiet about your successes?

Munk: Yes. [Laughter] I do now wish we had been quiet. We had so much fun, that we enjoyed talking about it and for both cases. And in some ways there was a similarity between the two experiences.

NO: Last week when we were talking to Bill Nierenberg, he said that he thought the Mohole project was ill-conceived from the start because the [unclear].

Munk: Yes, he is correct. By today's standards, getting a sample of the mantle is a very naive statement. And on the other hand, the group that wanted to do this really did want to do a gradual approach, realizing that you do not study the earth with a single measurement. The Earth is basically homogeneous. You do not learn about the oceans by a single measurement, and you don't learn about the Earth. But the very question, what does the mantle consist of, what is the Moho, is not a good question by today's standard. Bill is right. On the other hand, the test might have been very much worthwhile in leading to the same kind of remarkable results that the Deep Sea Drilling Program program later yielded.

NO: You would have found out something for the future.

Munk: Oh, yes.

NO: [unclear]?

Munk: You really ought to talk to [unclear] other people about it. I think the sharpness of that boundary was exaggerated for seismic reasons. I don't feel I really know enough about it, but I

think what Bill says now is the general interpretation, that one would not today start a major effort to drill a hole to the mantle. On the other hand, the Japanese are doing just that. They now have a project, and they will drill to Moho depths, and maybe something will be learned that we don't know now.

NO: [unclear]?

Munk: Yes. Yes.

NO: Great. I want to ask you also about JASON. That was a summer study group that was supported by the Department of Defense that you participated in.

Munk: Oh, I participated in JASON every year of my life for thirty years. It's not just a summer study.

NO: What was that? Tell us about that.

Munk: Well, JASON was founded after the war as a result of people who thought that the collaboration between the civilian scientific community and the military had been a successful thing during the war, both sides, and it certainly was, and should be continued. So a group of

very prominent people got together and organized a group of people who would work on the military problems.

JASON is unusual in its continuity and what it's done. It has some very good people on it, full of Nobel laureates and other people whose names everybody knows. They would work for JASON in a very personal way doing their own algebra, their own data. It wasn't a matter of working as a high-level person and having people on committees under you do the work. JASON does its own work and is composed of people who enjoy doing their own thinking and not working through other people. JASON is cleared well at a very high level and has had access to people in the American military and government at a very high level, and it's maintained itself in that sense and has been an unusual factor in American military history.

NO: What are some examples of things that are unclassified now that you can describe that you worked on?

Munk: Well, I was particularly involved in Navy problems. We participated in some significant problems, some of whom are still classified. I have to be careful. But generally, the problem on the detection of submarines, JASON has made a significant contribution.

NO: [unclear]?

Munk: By listening to the Navy and other people about problems they had and then deciding what we thought we could do. Very informal, very much like, in a way, the ONR days when no one ever successfully told JASON what to do. They suggested that here's a puzzle, here's a problem. They are people who are loyal and informed--the same two words--and generally would like to use that as a means of helping to solve problems, and has had a very significant influence on what happened.

NO: So if it's not a secret, can I ask how many people we're talking about?

Munk: Oh, fifty. All cleared. All having very high clearances, yes, yes. I'm afraid that it's something that although JASON has been successfully getting younger people, it's one of the things it's done well. I've really been very happy with how they arranged it. One thing JASON did is that everybody got the same amount of money during the summer whether they were a well-renowned Nobel laureate or somebody who has just come out of graduate school and had been asked to join.

NO: Let me ask you a couple of questions about clearances. Much of the work that ONR supported was classified.

Munk: I don't think much. I think the majority was not. Some was.

NO: [unclear]?

Munk: You can't. Then you do a certain amount of work which will not benefit you in your promotion or your career and you do it because you think you can help. But I think my memory of ONR as a whole is a very small part of what I was involved in. [unclear] was always a small part, was classified, and so certain things you've done you just didn't get credit for it.

The difficult part in having a life where you are sitting a little bit on both sides of the fence is how do you not use something that you know. [unclear] concern to people who worry about clearances, they say could some intelligent foreigner figures out that you must know something because of some decision you made, even if you did not pass on any classified information.

NO: Do people talk about their problems or--

Munk: Well, it's something that everyone has to really worry about himself. You don't want to publish something where you say that some third party who would read it could infer something that is classified. I think we've all been successful. I know of no problems that's really remarkable that JASON ran into. I think people have been responsible.

NO: [unclear]?

Munk: Oh, well, within JASON, of course, there was free discussion because we had a classified facility. So there's no problem in talking about it. I think you generally in your ordinary work don't mix the two. As you know, Scripps does not permit classified material. University of California does not. I think it's the only possible decision. I would find it incredible to have a file in my cabinet and if a foreign student came in, you'd have to lock it. It just wouldn't be possible. So it's not as much of a problem as you, I think, think. I haven't found it that much of a problem.

NO: [unclear]?

Munk: Yes, yes, of course. Any classified work that makes recommendations is reviewed and reviewed and reviewed, but by a very different group. I do not agree with you about your high regards for the peer system. [Laughter]

NO: [unclear]. [Laughter]

Munk: Yes.

NO: [unclear]?

Munk: No, no. But I think that obviously it's like what Churchill said about democracy: it doesn't work very well, but there's no other better system around. And I think that is just the right view of the peer review system. But I think it's very important that a certain degree, a certain fraction of the support for people comes through channels which are not dependent on a peer review system. It's been my experience that when people really have radical and novel ideas, they are unlikely to get the near unanimous support which is required for our days for funding. Take a place like MBARI - Monterey Bay Aquarium Research Institute, which has its own income and doesn't depend on a peer review, it has an enormous advantage in being able to commit themselves to a high-risk ten-year development which one virtually cannot do with NSF support.

NO: [unclear]?

Munk: Did I use the words "stand alone"?

NO: Yes, that was a quote from one of your--but that's okay, you can change it if you don't agree with it anymore. [Laughter]

Munk: I prefer to stand with people I like.

NO: Actually, I think it was the 1976 interview you did with Texas A&M.

Munk: I see.

NO: But you can change it. That was a long time ago.

Munk: No, I don't think so. I think that I generally worked on subjects that were not the fashionable subjects at the time, and that's the key thing. I mean, I don't know of anybody who worked with waves when we started working on waves. I started working on tides when the subject was considered to have been laid to rest by Victorian mathematicians, nothing more to be done. I think I've generally chosen subjects that--as you may know, I had to write some kind of review about myself, and I said that I think that the reason for doing so is that I don't like to read. And so I like to work with subjects without a literature so you don't have to spend the first few months reading all sorts of trash. And then if you're successful and people come in and it becomes a subject of increasing literature, then go and do something else.

NO: Yes, that sounds good.

Munk: And that's not the kind of thing that blossoms under a peer review system.

NO: [unclear].

Munk: And you're not going to get unanimous support for doing something that probably isn't even well defined. If you do something new, you can't really anticipate what it will do as the peer review system would like you to do, the probability of success. But now I have to be careful. NSF has done some support for some things that are far out. I'm exaggerating here. But I am not a lover of the peer review system.

NO: Let me ask you about the alternative to that. I know in the generous gift you've given to Scripps from your Kyoto Prize, one of the things you said of that is you wanted to support daring ideas at the discretion of the director. I thought that was interesting because when NSF was first set up, many people saw that as kind of a healthy antidote to overview directors or like in the T. Wayland Vaughan years, [unclear]. Is it just the pendulum shifts back and forth?

Munk: I think so. And we added a sentence--I hope I get it right--saying that Scripps' tradition is to have directors who are adventurers and if some day we have one who is not, God help us. But I mean, yes.

NO: [unclear]?

Munk: I can't give you a specific example. I think you're absolutely right in asking that. Because I was well supported, well--1 really have to think about giving you an example, but I think you've gotten me into a corner here. [Laughter]

NO: [Laughter] Well, if you think of it, we can come back to it. Okay. Let me ask you something else. Let's talk about scientific leadership. In your talk last week, you talked about the greats. I think you included Nierenberg. You talked about the three greats in oceanography doing research--Ewing, Iselin, and Revelle.

Munk: Yes.

NO: You also quoted Nierenberg as saying that he thought the next generation of leadership were not as daring or adventurous.

Munk: Yes.

NO: So what do you think is behind that? What made these people greater than the next generation and not as adventurous?

Munk: And Bill meant that I should have done some of that. I mean, I certainly haven't been in the Ewing, Revelle, and Iselin position. And that might be selfishness. I really loved working on scientific problems, and I did not have as much fun as Roger did in starting new organizations. It takes an enormous effort and it's very difficult to do both, really. Ewing did do good scientific work; it was mostly taking good data. Iselin and Revelle, when they became involved in public problems--although Revelle did some very interesting things. But it suddenly competed with his scientific ability. I thought I preferred not to do that.

I was offered once the Scripps directorship by President Kem, and I said I thought I wasn't ready for it; I'd like to continue working on science. Selfish, selfish thing. And Hank Stommel, the great oceanographer at Woods Hole, was totally against occupying any kind of an administrative position. So we just didn't have anyone of the stature of these three men. And Bill is absolutely right, until perhaps Jim [James] Watkins, who became the spokesman for oceanography in Washington five, six years ago, a four-star admiral, former Secretary of Energy, a wonderful man, full of ideas, who had some of this magic of leadership, at least from a Washington point of view.

NO: Didn't he do something special about the post-war period that made it possible for people like you and Revelle to step up to the plate [unclear]?

Munk: Probably it was easier when you came out of war and doing new things. I mean, getting the government to support an order of magnitude increase in funding, which they achieved, wasn't very much money. It would be impossible to think of an order of magnitude increase now of having oceanography go from 200 million to 2 billion. That's not conceivable. So maybe the leadership potential is better determined at times when you come out of special situation than it is later on.

NO: What about Sverdrup? You didn't mention Sverdrup in your big three, but obviously he was a man of great--

Munk: Because we were talking about ONR. Hal Sverdrup certainly was a leader, but in a very modest and scientific way. Hal Sverdrup would have not been effective in the way Roger was. He was too modest and he wouldn't have gone into meetings and said, "We need this and we need that." It wasn't his way. He would have if he had been a generation later. He would have not filled that position, I think.

NO: [unclear]?

Munk: Yes, and very modest. I mean, he could have been more like Columbus Iselin, who also didn't push himself, but he was a very different person from Roger. A wonderful man, but I couldn't picture him in a national leadership situation.

NO: [unclear]?

Munk: Well, I think his life was greatly colored by the Maud Expedition, seven years in the arctic where he was scientific leader under Amundsen, with very catholic tastes of what to do with it, very well-organized, hard-working person. His book, *The Oceans*, with [Martin] Johnson and [Richard] Fleming, was the last single description of oceanography as a field as a whole that has been written and will be written, and then became really the focus of the educational initiative at Scripps, which led to modern oceanography. It was a set of courses that came out of the book, as was said the other day, and led to the development of oceanography as a discipline. So in that sense, he was a great leader. Oceanography as a discipline. I did not include him because I was thinking of the ONR-dominated era. I think the world of him, and I have three heroes: one is Sverdrup, one is Revelle, and one is G.I. Taylor, an English hydrodynamicist. Those are the people who have shaped my life.

NO: Before, you mentioned the Maud expedition. [Interruption]

[Begin Tape 2, Side 1]

NO: [unclear]?

Munk: I think both. I think both. That was his first major experience where he had a job. He was chief scientist and cook, assistant cook, and he came out of that as a mature man and scientist. A wonderful choice for the Scripps directorship. I'm amazed they were wise enough to choose him.

NO: [unclear]?

Munk: June Pattullo.

NO: [unclear]?

Munk: I don't know how she came to working with me. She was a student. I didn't realize it was the first woman oceanographer. She was just another student, and we had fun working with her. In fact, she and Roger and I wrote a paper together that has kind of survived. And then she went on. So I really had no great feelings. I am so pleased about all the energetic and attractive women who are in the field today. It certainly made it a better field. I've gone through this in

connection with the Cosmos Club, which kept women out for many, many years. It's a much better place now that women can go.

NO: [unclear]?

Munk: I don't remember how she got here. Wasn't she in uniform before she came and was an Air Force officer? I don't recall. We could look that up.

NO: [unclear]?

Munk: Well, going to sea is a problem. You have to have certain arrangements made which were not really made in our early ship time. So that is a consideration. I don't know anything beyond that, and I didn't know we had such a terrible record.

NO: [unclear]?

Munk: In keeping women out? No. No. I don't think so. Mary Sears, by the way, was very prominent and I remember her well. I'm not aware of any sort of injustices that she had to face, but there may well have been some. I've never had any trouble. I like women, and the more the better. [Laughter]

NO: [Laughter] [unclear].

Munk: And it certainly helped a lot when they came in. But in marine biology, haven't there always been women?

NO: [unclear].

Munk: We've had certainly not a very good time getting any black people to become oceanographers, and our record is still terrible. And I don't believe it's because of any effort by anybody to keep them out. I think we would welcome having some black students here.

NO: [unclear]?

Munk: Oh, more civilized. I get awfully tired of the deterioration shipboard after the first two weeks. Jokes use shorter and shorter words, and it's somewhat better--it's better when women are aboard.

NO: [unclear]?

Munk: Well, Chip Cox is from a Quaker family and has had a successful career. There have been people who objected to support coming from the military. Here at IGPP, within a few doors of where we're speaking, Jim Brune felt that it was not suitable. He's a Quaker. And I'm sure there are other examples. I haven't felt that way, and I never had the problem. I do remember the Jim Brune situation. But it's not been part of my life.

NO: So what would happen to a person like that [unclear]?

Munk: He'd have to get money from other sources. He certainly would be, if he's a good scientist, would be a proud member of this institute. I wouldn't think there'd be anything but support, and I think maybe it is odd that we should have such a large proportion of DOD [Department of Defense] support. People must realize that ONR is a very special situation. And there may be others you've interviewed who've been more aware of that than I have. I've always been known as being very happy with the ONR situation.

NO: [unclear]?

Munk: Mostly the JASON collection, and there we've had interactive work which on some occasions have been important. I mean, important, they went up to the highest level of government, decisions had to be made. And I think we could be helpful.

NO: This is our last tape, so I want to ask you the question I ask everyone. What should I have asked you that I haven't asked you about?

Munk: Oh, about fun. I mean, we've been so lucky of having the excitement and the fun of being in this field at this time. I think we've just been incredibly lucky. And for an Austrian immigrant to have participated in this field and also to be permitted to participate with high-level military people is just incredibly lucky. I'm very grateful.

NO: Great.

[End of interview]