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Interview with Walter Munk
at La Jolla, California

By Ron Doel
January 8, 1997

RON DOEL: This is Ron Doel, and this is an interview with Walter Heinrich Munk. We are recording this on the 8th of January, 1997, in La Jolla, California. I'm aware that there is another oral history interview that Finn Aaserud did with you on the 30th of June, 1986, concentrating particularly on your role in JASON; it is available at the Center for History of Physics of the American Institute of Physics. I will not duplicate what his interviews have already covered, but we will be touching on other aspects of your involvement in the JASONS as well.

I do want to begin by noting that you were born on the 19th October, 1917, in Vienna. You have written in a number of publications about your early life and family life¹. But I am wondering what you recall primarily about your parents. Who were they and what did they do?

MUNK: Well, my mother divorced by father. My mother was a quite remarkable and independent woman. She had four brothers; she was one girl in the family. Her father, my grandfather, Lucian Brunner, whose portrait you can see in our kitchen, was a successful banker and built faniculas.

DOEL: Trams?

MUNK: Yes, in Merano and Bolzano. Judy and I made it our fun to try and identify which one he built. And had a bank in Vienna it was called Lucian Brunner after his name. Later on he became sort of a socialist, and he changed the name of the bank from Lucian Brunner to Österreichische Volksbank, Austrian People's Bank, but maintained one hundred percent of the shares. My wife always says that that is the kind of Socialist that she would like to be.

DOEL: When did that occur?

MUNK: That was before I was born.

DOEL: And that was well back when he changed the bank's name.

MUNK: Yes. My grandfather died before I was born. I have, in fact, some very interesting documents about him. He ran for Mayor of Vienna against a man named Karl Lueger, who is well known in Vienna--one of the main streets of Vienna is called the Karl Lueger Lane--and I have his campaign speeches. That must have been about before World War I, you see. They are fascinating because they sound so much like some speeches today. My grandfather accused Karl Lueger of getting a pay back from the contractor who got the contract to build the street cars in Vienna. Now, my grandfather Lucian was Jewish and Karl Lueger was an Anti-Semite, and part of the campaign had to do with that. I think it took some courage by my grandfather to run for mayor. I don't think he had a chance; he was soundly defeated. He also was well known at the time for bringing a lawsuit against the Catholic church in Austria.

DOEL: That's very interesting.

MUNK: 95% of Austrians are Catholic, because he claimed that a church tax should not be levied against people who were not in the same religion, and that was not a very popular thing to do. And he carried it himself-- he appealed twice, lost twice, eventually won, and used his own-- he studied law to pursue that case, and I have some of the literature. My mother grew up in that atmosphere. Then when my grandfather married again (his first wife died) mother found she didn't like her stepmother, and she went off to Cambridge, England, and became a student there. She became a student at Newman College, which still exists, and was on the hockey team. Not so long ago when I was given an honorary degree at Cambridge University, Judy and I mentioned that my mother had been there. They looked up the files, which were very complete. It was quite wonderful about her activities, and she really became an Anglophile from her years at Cambridge. She probably felt more at home in England than she did in Austria after that.

DOEL: Do you think it was in part because of what the family was going through and what your grandfather was trying to do?

MUNK: I don't know, she just loved it in England. She was a good tennis player and she was in the tennis team and the hockey team and a very active woman. She always told us she really didn't want to marry my father, who was a neighbor in Vienna so they knew each other well. But she did, and married him just before World War I. He never worked. He had money and he is a very well educated man. He took a degree in law, but he never held a job; didn't have to. During the war he was an officer in the Austrian army, but he served largely as a chauffeur to Franz Joseph of Austria and he at the time owned the only Rolls Royce in Austria, and he made that available to the Emperor. We have a picture somewhere of him opening the door for the emperor. So it was sort of a comic existence. But then she divorced him when we were quite young. Mother sent us to Switzerland during the divorce case so we would be away from it, and we lived in Lake Geneva for a while, my sister and I.

DOEL: And you were only perhaps four or five somewhere around?

MUNK: I was about seven or eight I think. I could reconstruct that. **And then we came back.** But so I grew up into a very international family really. My mother's cousins and things by the name of Brunner are prominent people, especially in Italy and still are. Part the family in the late 19th century became Catholics, so I am from a very mixed family. **And we have a family tree going back to 1780, which is very unusual.**

DOEL: That is unusual.

MUNK: Because one of my forefathers, and I have it here, started a trust around 1800 which was called "Against the Impoverishment of the Family Brunner," and in fact it must have been one of the earliest trusts when some of the family had hard luck, **I remember even when I was very young a cousin of mine who was a professional dancer got some money from the trust to see her through some tough times, and we still see our Italian cousins, they're friends, they are very elegant people, they live in Cuomo, and they have a castle in Austria, a real elegant estate. We were there last summer, all of us all my grandchildren and everybody.**

DOEL: That must have been an interesting reunion.

MUNK: They live a very elegant life. And so we have always been really part of a very international family. My mother married again, a wonderful man, a Tyrolean Catholic family, who was a sub-Cabinet member in the Austrian Government, a civil servant.

DOEL: And this was in the 1920s, that government.

MUNK: It was just before, ten years before-- well, I don't know, that must have happened in 1939, in the late '20s.

DOEL: Yes, this was before Engelbert Dollfuss becomes the leader.

MUNK: Right. And then he became, when my mother married him he was head of the Austrian tobacco monopoly. The Austrians had a monopoly for making cigarettes, the state had, and he ran that. And then he became head of the Austrian salt mines. It's a very interesting job because it has such, it would interest you, it goes back throughout history, it used to be the private ownership of the Hapsburg families, and you know Salzburg means salt castle and heline [?] and all of that whole area is essentially connected to the salt mines, and they are still going, five of them or so, and I grew up with my stepfather taking me on inspection trips into the salt mines.

DOEL: That must have been interesting.

MUNK: Very interesting. And they had their own ancient tradition. For example, in our house that we had near Salzburg, which we had until quite recently, we had a clause that goes back, I don't know how long, that workers in the salt mines were permitted to go through our property because it was a short cut to the salt mines but nobody else could.

DOEL: Yes.

MUNK: And they would, go in the morning, the people would walk up to their jobs. And so it was a fascinating job. In fact we went back to the salt mines last year when we visited Austria with my grandchildren. I wanted them to see something of it. And there is a private chapel in honor of my stepfather and I was very happy to see that; it was named for him.

DOEL: That's interesting, yes. And he had been in that position then for quite a number of years?

MUNK: Yes, he was in that position, he was-- And then when the Nazis came in, as you know Dollfuss was murdered, Schuschvick [?] eventually fled, and then my stepfather and mother left the country too. I was in America at that time.

DOEL: You were already in America. How difficult was it for them to leave Austria at that point.

MUNK: Not comparable to people who suffered so much. I mean they had enough money, and it was obviously a nightmare but it was-- I should just say that my family was very fortunate as far as that was... And they weren't really a typically Jewish family, you know, and then mother just left, went to England where she had most of her friends, then came here, never was really impoverished to the extent that so many unfortunate people were.

DOEL: Indeed, but clearly for anyone who needed to leave Austria in those years under those circumstances, and with the Jewish roots of part of the family, it had to be somewhat tremendously difficult.

MUNK: Yes, and you know, we had a house we loved near Salzburg, I have some quite lovely pictures, because when we went back with my grandchildren the people who now own it, a Von Fürste from Munich, have loved the estate, it is a beautiful old 300 year house, and they have taken beautiful care of it and we have stayed in contact with them and they had to spend time there, and we will go back. But mother, who sold it to the Baron Fürste's father, we found-- he showed us the old correspondence and it was a very nice friendly correspondence really about their selling it.

DOEL: And that was before...?

MUNK: No, she sold it after the war.

DOEL: It was after the war?

MUNK: Yes, there was the usual thing of some local Nazis trying to change the entry in the books, yes it happened.

DOEL: That is what I was going to ask about I would have expected.

MUNK: Yes. it happened. and it was fully restored with a minor lawsuit, I mean it was just a thing, and then mother sold it. And we still own some property by the lake there, very valuable, which I was in charge of for many, many years, and we tried to sell it because absentee doesn't work very well at that distance. We eventually did sell it and it has become a public park in that village, it is lake property, very, very nice.

DOEL: Do you remember what year it was that your mother and stepfather left Austria?

MUNK: It must have been just '39 isn't it, that's when...?

DOEL: The Anschluss was March of 1938.

MUNK: It must have been late '38 or early '39; I would have to reconstruct that. And they went first to Switzerland and then mother went to England, and then my mother and stepfather had a problem for a while and then weren't together, and then finally my stepfather joined my mother

in ??? ???. But there was some difficulties.

DOEL: But they were separated from each other and often, there were detentions for some of the people.

MUNK: Yes, but they were separated for sort of personal family reasons, but then they made up and saw each other again. We were very, very fond of our stepfather, and certainly knew him much better than our own father, so my life really centered around that.

DOEL: I understand. You know, I had wanted to ask you earlier did your mother ever talk to you about the reasons that she was fond of England and why she decided to go to Cambridge?

MUNK: Well, I think she wanted to run away from her stepmother, and how she got interested in Cambridge particularly... oh, well, she had two brothers who were living in England, Marcoff and Otto - and Marcoff had started a branch, a private banking firm which was sort of connected to my grandfather's, in England, and that banking firm existed until very recently. And that of course comes very close to my life because then they started a branch in New York. And at that time the Austrian economic situation was poor, not as it is now, very good, and mother thought that the chance for me to have a career in banking was rather unique. It was difficult in Austria to have a career, and therefore I was sent to America for that very reason, so that came close to being my background.

DOEL: Indeed. I do want to ask you some more about that in just a moment. You mentioned the house that you were living in when you were growing up.

MUNK: We had a house in Vienna, and we had a country house near Salzburg, which we rather loved more. That it is called the Egelgut, and in fact there is a picture of it in here.

DOEL: That's right. I do remember seeing it.

MUNK: It is a lovely house with a big piece of land and forest brook. And we went there every summer and every winter, skiing and summering. The house in Vienna was a four story typical nice Vienna house, and Judy and I went there once when four families lived there when we first met. The first floor was occupied by the Cuban Ambassador, second floor was occupied by a very interesting man who wrote for the New Yorker in Vienna and his name was well known, Judy might remember it. There were stories in the New Yorker occasionally about news from Europe, ten-fifteen years ago, and his name was well known.

DOEL: Interesting. And he was living in the...

MUNK: In the second floor ,and we had a big house, quite elegant, but we didn't care for it, quite a formal house in a way. We loved our place near Salzburg and we plan to go back there next year and visit our people who bought it, who are very nice people.

DOEL: And I imagine that is where you gained your love of skiing and outdoor activities being

out there.

MUNK: Yes, my mother's oldest brother Felix was an excellent skier, so was my father. He was a good skier and after he and mother divorced he lived in Kitsbuhl [?], and he eventually married a number of women, all of whom were attractive. I think he had a total of four marriages, my mother was the first. But he was a good skier, and so was I at one time, more or less.

DOEL: I am curious what you remember reading as you were growing up. Did you have particular interests as you think back on it?

MUNK: I was very un-intellectual. I lived for playing tennis in the summer and occasionally would make it in some tournaments to the semi-finals, but never further. But I lived for it and I lived for skiing in the winter and I was very un-intellectual person.

DOEL: How did you find school in those years?

MUNK: I had a private tutor until I was about eight or nine, then I went to a gymnasium and had a very moderate record, nothing outstanding.

DOEL: Were there any subjects that you found more interesting than another back then?

MUNK: I can tell you which I found less interesting: Latin.

DOEL: It sounds like it was a classical curriculum.

MUNK: It was a real gymnasium, which was heavily centered on that. And so when I went to New York I had really no, not much of a background. Of course I was very young, I was fourteen. So mother suggested I should do that because she thought it was a unique opportunity, and I think it probably was.

DOEL: How did you feel about it at the time?

MUNK: I resented it, and I know unfairly so. I know that my mother must have found it difficult and did it because she thought it was the proper thing to do, and I always resented it.

DOEL: At age 14 that's normal.

MUNK: Yes but she-- I was sent to a small school on Lake George called Silver Bay, and it so happened that Judith's grandmother had a house on Lake George which we visited last summer, and we went looking, hunting for where Silver Bay has since closed but there is we discovered a Silver Bay YMCA where the school was, and we had a very interesting time looking through the files of the camp, and it said all about the school. It even had some pictures of the people. It went bankrupt the year after I went there.

DOEL: I remember you've written about it.

MUNK: But not because of me. But I think there were only about, I forgot, less than thirty students. And the reason it was chosen is that mother wanted me to have a transition. And the then Ambassador, American Ambassador to Austria, had his son go there and he just said it was so wonderful, so that mother applied and sent me there.

DOEL: So that was the connection that particularly centered you on the Lake George area of the United States?

MUNK: It was just an accident. She was trying to figure out where can I send him for a year before he starts working, and that was very good. And I enjoyed that a lot, it was a fun year.

DOEL: How much English had you spoken already by that time?

MUNK: I had English nannies so I probably spoke better English than I speak now. Never was a problem.

DOEL: Had you visited England already at that point?

MUNK: Yes, yes. Mother took me twice to England, and I don't remember the details. I know she took me to England once to pick up some gold that she had in deposit there, and I remember only that she didn't know decimal points. She said she had, I forgot, funny story, she said she had about \$10,000 of gold that she wanted to transfer, and we went there, that was at the same firm and it was \$100,000, and she said, "Oh, I made a mistake." I do remember that it is sort of an non-pertinent story.

DOEL: Had you done much traveling before you went to the United States?

MUNK: Not much. Not abroad. I had been to Italy on summer vacation. We always went to our place in Assay [?] every summer. I had been to England. Not very much really. It wasn't the kind of thing to do. You know, one went and spent summers at one's own place really. Mother had not been to the United States before I came.

DOEL: That I was going to ask you, whether any of the immediate family had in fact visited.

MUNK: No, but we had our Italian cousins which we often visited. And mother's cousin who was a partner in the bank here in New York, Julian Triar was his name, had come to Austria many times, and so he was my contact, and so was the man who was the head of the bank, Hugh Cassel. Hugh Cassel was a man who my grandfather had financed to start the bank in England, that his son Marco worked in, but his son Marco didn't know anything about banking, so he had a young man who had no money but knew something about it, and Marco start the firm in England. And that is probably why my mother went there because she had a brother living there.

DOEL: Yes, that makes sense. And, indeed, during those years Austria was financially very

impoverished.

MUNK: Very impoverished.

DOEL: You know, I was interested a moment ago when you had said that your grandfather had died before you were born.

MUNK: Yes, the year before I was born, must have been 1916.

DOEL: Because it is clear that he still had an influence over you.

MUNK: Enormous. I mean through my mother and even through his money that even had enough left to get us started on this house. I think we eventually-- mother gave us \$5,000 to start on this house, which was enormous help. We had nothing. So his influence was very deep. And my wife, who is really more interested in that than I am, really was fascinated with what a fine man he was. He really had some courage.

DOEL: Well that is what I was thinking about in particular that you were aware through the family of the sorts of things he had done. The Catholic church in Austria was particularly conservative in those years.

MUNK: Very. And to have a lawsuit against them at your own expense was not really a way to become popular.

DOEL: No.

MUNK: We used to be told that our maids would come home crying because they had been to a church service where they had spoken against my grandfather. And he got himself arrested when there was a student protest in Austria, and Judy always admired that. He, when they had a protest, he decided to appear and be arrested with the students. And he had his own way of doing things. And when we came back every summer, that is mother's story from the country place, by train to Vienna with huge numbers of suitcases and stuff and very complex, he would get off one station before the main station and come home in his own ??? and let everybody else worry about the luggage. And I always thought that was a great idea. I wish I could imitate that. No longer possible.

DOEL: That is interesting. When you had that conversation with your mother where she had wanted you to go to America, was that something you talked about with your stepfather as well? Did he support that idea?

MUNK: He certainly did not oppose that. But I found that transition very difficult, even though, you know, it was meant as pleasant as could be because I wasn't impoverished. I mean I spent my first night in New York on Park Avenue because Cassle had an apartment there, and they all picked me up when my ship arrived.

DOEL: What kind of ship was it that you took?

MUNK: Oh, it was a Belgian-American line I think, I forget. So my Uncle Julian, who was sort of taking care of me, had some very definite ideas about how one should grow up in America, and they were that you really had to earn everything yourself. He really sort of was in charge; my mother had put him in charge. And Silver Bay was fine for a year; I was very comfortable and got to know some people I've known for a long time. But then when I started working at the bank in New York I just hated it. I was entirely in the milieu that I didn't like. I was a runner, a runner means you go and deliver stocks and things, and the others were really tough and all, and I was totally lost. It was not what I was accustomed to. And so I went home to Austria the first summer afterwards without asking mother for a visit, and I hadn't done very well. I was uncomfortable, they didn't think I did a good job, my uncle wasn't...

DOEL: Your uncle Julian.

MUNK: ...wasn't very enthused about my...

DOEL: You know I was trying to think of how young you were when you did that. You were then fifteen when you finished up at Silver Bay.

MUNK: So I was sixteen, but still now I first worked at-- Let's see, I came to Caltech in '37 and so it must have been '34, '35 and I was born in '17, so I was very young. And I decided to live at International House in New York because it was a little bit more like what I was accustomed to, and they were very, very nice.

DOEL: I was going to ask you about the International House. That was on Riverside Drive, wasn't it?

MUNK: Riverside Drive. That Mr. Rockefeller had built. And the sign on the door was that Brotherhood May Prevail. And they were wonderful. It was very nice and I really shouldn't have lived there; I really wasn't a student, you see. But they permitted me to, and partly I enrolled at Columbia just to qualify to stay there.

DOEL: That's interesting; I hadn't realized that. You know before—I really want to talk to you in depth about all of that—I am just curious what you remember about the year you had spent in Silver Bay. What kind of courses were you exposed to there?

MUNK: Oh, I had a good course in descriptive geometry. I don't know, that is a very elementary thing, but it did in fact get me a little interested in mathematics. I thought the school was quite wonderful.

DOEL: How big-- it was very small, wasn't it?

MUNK: 30 plus boys. The man who had been Headmaster had come and seen us in Vienna to interview me actually, and there was some element of homosexuality involved in that school I

think that I vaguely remember. But I also remember that the boys were very nice. It was a funny group of people. It was not a cheap school. And I was certainly, I think, I was the best tennis player which made it nice, and I was the only one who could ski. You see the skiing hadn't started and I started to ski.

DOEL: That's true, that's very true.

MUNK: Cross country, you know. Lake George isn't very good for skiing.

DOEL: No, but cross country if you had a good winter you could...

MUNK: What Judith could tell you if she sat here is that mother had sent me to a boxing school in Vienna to prepare for my American adventure. She thought that was essential, so I had taken some lessons in boxing, and I was also playing cello and I took my cello with me, which I remember then she found so amusing is I had my boxing gloves that I had to some how or another move on the ship tied around the cello, and I think that saved me from when I arrived at Silver Bay with a cello, which would make me a sissy, but then boxing gloves tied up I think that probably...

[end of Tape 1, Side A]

[beginning of Tape 1, Side B]

MUNK: Totally, totally, totally removed, isn't it.

DOEL: But these are formative experiences, and I think in terms of the way in which you interact with others or deal with unusual situations may have tremendous influence on...

MUNK: And I loved Silver Bay and I really didn't-- the atmosphere on Wall Street was just terrible. I mean since then I love to go to New York, it is fun, but it was just very unhappy for me then and I did a lousy job at the bank, and I found it totally uninteresting.

DOEL: When you had finished that first year in Silver Bay...

MUNK: Yes, that got me the equivalent of a high school degree.

DOEL: That is what I was going to ask, but you earned that very young compared to most high school students here.

MUNK: I was young, but apparently did very well in that sense, but the school wasn't very critical. But I had the equivalent so I could go to Columbia. I remember asking Julian, my uncle, what courses I should take and he said I should take a course in ethics, so I took a course in ethics. I don't think I remember anything about it at all. But there was nothing in mind about scientifically oriented. He just thought maybe it was a very good idea in Wall Street at the time

that you should learn something about, or maybe even in Congress today.

DOEL: You mentioned that you had studied mathematics and geometry when you were at Silver Bay.

MUNK: Well mathematics, that is an undergraduate high school course, yes. But I remember it was the first time that I sort of found some interest and then when I went to Columbia I took a few courses. I really wish I could look up what I took. I took some courses in elementary freshman physics and some in mathematics. I mean calculus and so on, whatever you could take on an extension basis.

DOEL: Did you have like a favorite subject when you were at Silver Bay? Was it becoming mathematics do you think?

MUNK: My favorite subject was to play tennis.

DOEL: Yes, that's right.

MUNK: And they were very sports oriented, actually, that school.

DOEL: I can imagine. Do you remember any other science classes that you were enrolled in back then?

MUNK: No I don't. I really wasn't science oriented or anything else. I do remember getting a kick out of descriptive geometry, which, as you know, is very elementary. I don't even think you teach it-- it must be in high school at the second year or something now.

DOEL: Yes. But of course back in the 1930s one typically didn't even get calculus in the American system, so then things have changed in more recent years.

MUNK: Totally. Now, I must have taken a freshman calculus course in Columbia at the extension. I don't remember; it had that little influence on me. And then I went home twice, so this lasted two and a half years I think.

DOEL: When you were taking courses and when working at...

MUNK: Taking courses and working. And then Mother said that if I really didn't like it I could quit. And I enrolled-- I think I wrote the story here. I was really hopelessly naive I decided to go to Caltech because I liked the names of streets in Pasadena—they were so romantic.

DOEL: Yes, and I want to cover that in just a moment. Do you remember any other professors that you were taking those courses from at Columbia?

MUNK: At Columbia? No. And I doubt whether anybody of any consequence taught in extension.

DOEL: That is what I was wondering too, because these were the night classes exclusively. What were the other students like who were taking those courses?

MUNK: Oh, I have no memory of anybody. My entire non-Cassel involvement, Cassel is the name of a company, was at International House where I did make some friends that I stayed in touch with for a long time.

DOEL: I was curious about that. Who did-- these were clearly international--

MUNK: Well, there was a German man named Fruehlich, who later on became a book publisher and was very successful and I stayed in touch with him. And there was a young Austrian girl whom I enjoyed whose-- they owned a factory in Austria. I do remember that was still through my stepfather's thing that the Austrian Consul took us to lunch once. So I was not-- there was some contact even at that time through my stepfather with the Austrian people, even though that was after the Anschluss. I guess you know it isn't quite so simple, people could maintain some relationship.

DOEL: Although indeed it becomes more difficult. Were there other people that were particularly important for here during those years?

MUNK: Well, my Uncle Julian and his wife Charlene. Julian died and Charlene then was in California. She had married him very, very young, she was 16 or 17, and she went back and lived in Los Angeles. And maybe one of the reasons I choose Caltech was that she-- I would have a-- and I was very, very fond of her, a very wonderful very young woman. My Uncle Julian was a devout Catholic, like people who were Jewish three generations ago became 150% Catholic, so they grew up. And they lived quite an elegant life in New York they had a chauffeur and a car and so on. And I was kept at a little bit of a distance, but I was very fond of Charlene and then when I went to Los Angeles, certainly I saw her once or twice a month or so. So I wasn't totally alone.

DOEL: Right. Did you get much opportunity-- clearly your schedule was very busy at those years. You were working for the firm during the day and took night classes. Would you take one class at a time, would you ...?

MUNK: One or two or so. So I had a very limited extension.. I am really amazed that they let me get into Caltech. That was the luckiest thing that ever happened, and I never will forget how flexible they were. I don't know whether I wrote that up or not, but I really just bought a car, a DeSoto convertible, and hopped into it after mother gave me some money and said you do what you want.

DOEL: And your father agreed?

MUNK: My father was not involved.

DOEL: Your stepfather I mean, I'm sorry.

MUNK: I don't think he was really involved in that decision, she just said here. I had done my share. And she was very disappointed and she thought I should maintain some contact with the firm in case California failed, and I cut all things totally. I didn't want to have a, yes, you know there is-- it just reminds me there is a wonderful Norwegian named Fredjrief Nansen, probably means something to you.

DOEL: The Nansen bottles, yes.

MUNK: And when he said he would go across Greenland on foot, which had never been done. Have you heard the story?

DOEL: No, I haven't.

MUNK: Oh, it is a wonderful story. He was getting his degree in Biology, Physiology actually, this is a story from Harold Sverdrup, who knew him well. He had announced that he was making plans to cross Greenland and it was known to all Norwegians, you know they are very exploration-minded and it had not been done. So when he came for his final doctor's dissertation and the committee debated whether he passed or not, the majority of them decided that he should not be accepted, but then one or two people said, "Oh, the poor bastard is going to die anyhow when he crosses Greenland. Why not let him have this degree." So consequently what he did is after he landed in Greenland is he burned up his boat and everything so he had no possibility of going back, and I think I sort of treated the New York situation that way—there was no way I could get back. Though Hugh Cassel was a very nice man, he had been wonderful to my family. If he had not saved what grandfather had given to his son Marco to start a bank in England, which was about one-fifth of his property, and then when Marco died that one-fifth was divided after the war to the remaining children so my mother got essentially 1/25th I think. But Cassel, you know we were all enemy agents, but Cassel was a British citizen, saved it. He treated the family very well and we owe a lot to him, a wonderful little man, I liked him very much, very nice to me.

DOEL: Did you stay active in sports when you were in New York City?

MUNK: No. I went skiing out of New York occasionally at a time when there were no lifts and things and there were other people-- there were some people at Cassel's whom I enjoyed. You asked me a question. There was a man named Henry Wallich who became a member of the Federal Reserve Bank for many years. His father was a partner in a private bank in Berlin named Dreyfus. They sort of go back as far as the Rothschilds, and they were sort of a more successful bank than my grandfather's but sort of the same, and young Henry and I were the same age, sort of commiserated with each other a little bit because we had been sent away from our very comfortable things, and he and I used to go skiing together. And I maintained my friendship with him. He eventually went to Yale, became a professor there, and then eventually a member of the Federal Reserve Bank. So I remember him with great pleasure.

DOEL: Did you get to the museums or other cultural attractions?

MUNK: Not very much.

DOEL: I was curious just if you had an interest in that at all, you were growing up...

MUNK: Not really. I didn't really like the city. I know Henry and I once decided we wanted to see how long we could stay awake, and we had been to a party and so it had been about 40 hours and we decided to see whether we could spend another two nights without going to sleep. And it had fatal consequences. I think we both fell asleep in a subway and managed... So it wasn't very intellectual. And I learned flying at the time. I remember I went out and took some flying lessons on Long Island.

DOEL: Interesting. There were a few small airports out in the Island in those years weren't there?

MUNK: There were 12 airports. And I enjoyed being out of the city. I thought even that gave me a feeling that was a little bit more familiar to me. And my wife Judy took flying a little later so we both at one time had a pilot's license. I crashed and when I flied solo, but not seriously; just hit a fence and there was some damage, and that ate up all the money I had and my flying career ended.

DOEL: When was that?

MUNK: That was during those years I was at Cassel's. I would just go on weekends and take flying lessons.

DOEL: That is interesting. You really had a very interesting time.

MUNK: Very unintellectual things.

DOEL: But yet you weren't taking the night classes at Columbia, you knew that you wanted to be involved in...

MUNK: I think probably mostly because I wanted to stay at International House, and I had to have some university attachment.

DOEL: Some way to do that, yes.

MUNK: And I was not calculating for the future.

DOEL: I understand. I am curious what you felt about Columbia as a place?

MUNK: Didn't like it, certainly not extension. You know, in a big city an extension course

doesn't have any of the element of fun and excitement and people who go there are very ambitious, which I was not. I had no friends in the extension classes.

DOEL: And those were all offered in the Morningside Campus and the Upper West Side?

MUNK: Yes, so I could walk over from International House to 116th street, 125th to 116th.

DOEL: Right. You were mentioning that decision time came for you and you bought the DeSoto and...

MUNK: And drove to California without having made any plans. I would be furious if my children had been that irresponsible.

DOEL: What did you know about California?

MUNK: Charlene was there, had a house, and I had read catalogs and I remember thinking that the names were so beautiful: Altadena, Pasadena and I had seen some pictures. Of course Caltech is not the most beautiful campus in the world. But I wanted to get away from New York.

DOEL: But it clearly appealed, and you knew you had Charlene who was there.

MUNK: Yes, and in fact I stayed there when I arrived with her, and she had a lovely house.

DOEL: What part of Los Angeles was she in?

MUNK: In the Hollywood Hills, she had that for a long time. My uncle when he died, I mean she was well off and bought a wonderful house, a big house, eventually then bought a house north of Los Angeles on the beach. What are they called?

DOEL: I know it better south of Los Angeles.

MUNK: Oh I bet you do know, the area that always gets stopped when there are fires and floods or rainfalls or so.

DOEL: I know what you mean. We'll put this in the transcript for later, we will add it in there.

MUNK: I was very fond of her, and Judy was very fond of her, and we kept our contact with her until she died. They are both very good horseback riders, by the way. Julian, my uncle, rode formally and she'd wear an English saddle, they would ride in Central Park and have their horses.

DOEL: Did you ever do that yourself, did you go out with them?

MUNK: I never did. My daughter Edie is quite good at it.

DOEL: You mentioned a moment ago and I was interested in this, that you had begun to look at

college catalogs and in that time.

MUNK: And I must have known that the Cassel thing was not going to last, but I did depend on mother giving me some money, and I think I told the story here but I was very irresponsible. She gave me \$10,000, which was a lot of money at the time. And then when I was admitted to Caltech I sort of fell in love with a girl named Barbara Sage, and I also became interested in and I liked drinking Bourbon and I essentially spent the money in the first year, which I wasn't supposed to do, in a little bar in Colorado State called the Circle Bar, we would go there every night and we didn't drink, but we would have a good time and very irresponsible. And then I wrote back to mother and said could I have some more money, I've spent it all, and she said no.

DOEL: That must have been a bit of a shock for you.

MUNK: Bit of a shock and then I got a job, an assistantship. I'd done quite well and there was a man named -- oh gosh I should know -- who became president of Stanford and then president of the Huntington Library. He was teaching history at Caltech. A well known man, we can reconstruct.

DOEL: We will reconstruct it. Turbin [?] was the provost at Stanford but that is not the fellow that you...

MUNK: Here is Judy. Oh Judy you must meet Mr. Doel. And there was a technical problem. I had failed on some medical thing which was apparently trivial, I can't tell you the details, and it really didn't amount to anything, but, you know, it was not easy getting an American visa because so many people applied.

DOEL: Particularly in that period of time it was critical.

MUNK: And then mother wrote to Masserich.

DOEL: As you mentioned he was president of Czechoslovakia at the time.

MUNK: Yes, and somehow or other and then we got a call from the Ambassador saying that he had heard that there were some problems and it was immediately fixed—typical Austrian fashion.

DOEL: And that you had gotten it within a week, then, after your mother had contacted Castle. You also had mentioned it was very interesting a moment ago off tape, that your grandfather had also know Herzog who became ...

MUNK: Yes, Hansel was a poor student, and apparently my grandfather would have evenings where he invited students over, and that is how mother met my stepfather. She met him when she was a young girl and he was a young student at the University of Vienna, and then when they hadn't seen each other for a long time and they ran into each other after mother had been divorced a few years and remarried.

DOEL: As you say your grandfather's influence extended in...

MUNK: In many ways, isn't it. And I wondered that-- we wondered the other day, he was one of the founders of a Swiss insurance company called Helbatsia [?], and we owned founders shares, I remember even that were named for grandfather, and I don't really know what happened to them. I asked my cousin Marco, Felix's son, whether one should ask now that the Swiss are finally getting a little bit more open whether we still have some shares in our name. They were worth a fortune at one time—it was a very successful company, and we never really bothered to look into that.

DOEL: You were very, of course, to the revelations that were in November and December as I recall of last year.

MUNK: Well even now there was something in the paper this morning that the Swiss are being asked to open some of their books, and I wonder whether my father still had an account in Sangaein [?] where he used to be as a young man before he went to Austria.

DOEL: That is very interesting. Had you ever, and I am quite curious in what you said a few moments ago, did you ever have contact with any of that extended group of individuals that your grandfather had met in later years?

MUNK: No, I never met Masserich, never met Herzog, certainly. I think that I was too young for that. We had a very extensive Italian family that I did mention with whom we are in contact. They in fact maintain a very elegant presence in Austria. As I said they have a castle there where we go when we want to live elegantly. And many relatives all of whom are very prominent people. You know the Austrians had a presence in Trieste, and they were a leading family. They owned a shipping family, the Italian American line or so, and they still are a very prominent family. We go and see them whenever we go to Europe, the present generation. I like very much for my grandchildren to maintain some of that; that is why we took them. I don't know whether we will succeed or not, but it is so much more fun to have that to have some kind of relations.

DOEL: Now it was in 1937 that you took the trip out to-- and was it clear to you already at that point that you weren't going to be able to return to Austria?

MUNK: Yes, like Nansen I had burned my bridges.

DOEL: Yes, but to Austria too? Had you thought about going back to Europe do you recall, or did it seem...?

MUNK: Until that time, you know, I still really lived much better in Austria than I did in New York, I think it became of course irreversible in 1939.

DOEL: It did by then, but earlier I was wondering if you were still thinking about returning?

MUNK: I don't-- I am trying to think of the answer. I still enjoyed in the summer going home and it was very pleasant. I guess I really became irreversible in '39 and especially in '40 when I joined the army, when I enlisted, and I don't quite know whether there was a time when that became irreversible. I had applied for citizenship of course. I took my exam for citizenship in '38 I think, I have to find out when I was naturalized, and I flunked it. And that was a very curious thing. I had taken a course at Caltech by a man named Monroe, that may still be part of the legal requirements that everybody in an American college has to take a course on the American Constitution. Is that still a requirement?

DOEL: American history generally, but not necessarily, I think, constitution.

MUNK: This was the constitution and I remember saying oh what a bore this would be, and then enjoying it enormously and learning something about it, and Monroe was such a wonderful teacher. And so when I appeared at the examination for citizenship I really knew something about the constitution. And I appeared with my roommate there as a witness and I was asked one question. The question was "What is the constitution of the United States?" And oh, I loved that question, and I started talking and the man looks at me, interrupts me and says, "Come back next year when you have learned the answer," and apparently what you were supposed to do is learn the book by heart which had verbatim answers to any of about 30 questions, and the verbatim answer was quote, "The constitution of the United States is the Supreme Law of the land," end quote.

DOEL: So it was by rote that you needed the search link to...

MUNK: So I had to learn that by heart, and the man who examined me was a bit of a "well you college guys think you know all the answers." I really didn't feel that way; I really was interested but I must be one of the few people who flunked the exam and had to go back a year later.

DOEL: And it took a year before you were eligible to try again. Interesting. One thing I was real curious about a moment ago, you had mentioned you were beginning to look at college catalogs when you were...

MUNK: In New York.

DOEL: In New York. Were there other schools that had intrigued you as possible places to go?

MUNK: I think I concentrated on Caltech. You know, I have never been very good, even today at examining many options. I kind of hear something I like and just do it and the idea that you should really balance it, which is very wise, is not something that I do very well and I have sort of been that way for all of my career. Just find something I like. So I think probably I had no other options, I don't recall having made any other-- and even that option I hadn't done the wise thing of being in correspondence.

DOEL: But of course it works out very well for you in your first meetings.

MUNK: Luck.

DOEL: How much did you know about Caltech before?

MUNK: Nothing.

DOEL: Did you know their strengths?

MUNK: No. I don't think there was another option. I am trying to think about it. And I did at the time-- I had several times saying what am I going to do I certainly don't know anything about-- I think the only thing I did make up my mind was that I was going to try and work in a field where you could do some outside work, field work. I didn't want to be a theoretician and I didn't particularly think of wanting to spend my life in an indoor laboratory.

DOEL: Sounds like science maybe had something to do with it by that point?

MUNK: My Uncle Felix was sort of an amateur scientist, and probably quite a good one. He is my mother's oldest brother who taught me skiing.

DOEL: Right, we were just looking at his portrait in the kitchen..

MUNK: Right. And he was a frustrated non-scientist. And Lucian, his father, had brought him a little factory to keep him busy, at the same time as Marco started the bank. And that was a little factory in Vienna that the family owned for a long time even after the Nazis that made pills and things, and my uncle designed all the machines, he was apparently very good, but he always talked about physics, but sort of from the philosophical sort of view. It was kind of fashionable, you know, about the meaning of the uncertainty principle and things. But he was the only one in my family I think that had an interest; nobody else every did, certainly not my father, not my mother who took botany in Cambridge. That is what she read. So I can't really think.

DOEL: It's interesting that you recall hearing discussion about Heisenberg's Uncertainty Principle?

MUNK: From Felix.

DOEL: And course quantum mechanics had become a big issue by the 1930s.

MUNK: And then, as you probably know, that the then Dean whose name I should reproduce for you, was so amazed at my naiveté. I think I told the story, I came in there and he said let me open, let me get your file, and I said you don't have a file. He said well you know you have driven out, so if you can pass the entrance exam we will let you in.

DOEL: When did you actually take that?

MUNK: Well I had a month, and I rented a house on the corner of a lake in California and I studied, I had gotten some examples of what you are supposed to do, and I studied hard and I passed it. And probably I do remember that I had no difficulty with trigonometry and with the geometry and I passed, and I really was not-- I became a good student in my junior year that is when I met Gutenberg and I became genuinely interested. Until then I was even at Caltech just sort of farting around.

DOEL: Do you remember what else was on that entrance exam was it fairly extensive from humanities to the sciences or was it really focused on economics?

MUNK: I don't think it could have been or I wouldn't have passed it. But I did enjoy Caltech from time zero. I got a room in Blacker House, one of the houses, and I still think it is a wonderful place. And they were flexible enough to permit that. Millikan was president.

DOEL: Indeed, and we need to talk I think a little bit about him later in the interview. Had you traveled much in the US by the time the major cross countries...?

MUNK: No, no.

DOEL: So this was a new experience.

MUNK: No, I had been to Florida as well. Not uninteresting, my Uncle Marco, who had started the bank in England, had made very good friends with an American family named Ennis who had a house in Evanston and a house in Florida.

DOEL: Evanston, Illinois?

MUNK: In Evanston, Illinois. And they were awfully nice to... Oh, and my other uncle, Otto, these are all my mother's brothers, Philip, came...

[end of Tape 1, Side B]

[beginning of Tape 2, Side A]

DOEL: ...in America as a young man and stayed with the emphasis...

MUNK: And then died of cancer here. So we had a relation to this family, and I once went to Florida when I was at Cassel's over Christmas, they had invited me to come. And my Uncle Julian Tryer [?] and Charlene were very close to them. So they were wonderful people. I think he was a minister, I forgot.

DOEL: It was still a new experience for you to be going through the Midwest and into the Far West.

MUNK: Cassel said one very interesting man, an Irishman named Justin Conven, who had

become a partner as a very young man, 25 or so, very handsome, superb horseback rider, and he drove me down to Florida, I remember that episode, and we kept contact with him and Judith met him. So there were some interesting people I had almost forgotten Justin. He was great fun.

DOEL: What was it like for those first few years at Caltech what was it like just to be a full time student in a place like that?

MUNK: No problem. That was fun, that was nice from the very beginning. I was admitted and then I got myself a room at Blacker House and I had a roommate and joined some of the-- oh, and we started a ski club.

DOEL: You did.

MUNK: And I was president, and we did very well, we competed and beat UCLA and beat Stanford. And I remember we ran everything, there just three or four of us, and we ran the slalom and the downhill and everything, and I loved the California mountains. I mean it was so wonderful to drive them Friday after classes up into Bishop or something and to ski up there. Really, and I fell in love with California. I fell in love with California the night I entered across the border and spent the night camping out. I forgot how I came in from Arizona or something, and I remember camping out somewhere, and there were some other people camping and talking all night, and that was a wonderful evening in the sort of desert.

DOEL: Under that kind of a dome, it is a good point.

MUNK: Dome of stars. And then I always have been really in love with California. I think it is a wonderful state.

DOEL: Of course you mention you were in pretty close contact with your aunt.

MUNK: With Charlene, who was a California girl. Her family owned some schools, military academies for boys, they have something called Page Military Academy and that was -- her maiden name was Vaughan and they ran the schools and they were Californians. I think that must have had a lot to do with it.

DOEL: Was the curriculum at Caltech in those years set in the very beginning?

MUNK: Yes.

DOEL: You knew what you would have the first few years.

MUNK: No, I came at the day when I first interviewed, he said what would you like to take, and I had looked over the catalog and there was a course on relativity and I said I wanted to take that. He said you don't take that until you have taken a lot of others, I thought you just chose what you want. It was equally naive that you could just come and show up. And I picked up some

interesting sounding courses which you don't take until you know something. So then I chose Applied Physics. Mother thought even then that I should be careful to take something I could make a living on, and Applied Physics was sort of in between physics and electrical engineering, very similar to electrical engineering and not geophysics, I didn't even know about geophysics.

DOEL: Most people don't entering into college or university.

MUNK: Didn't know anything about it until we came to the La Jolla situation, which we haven't done yet.

DOEL: Right. But this was of course one of the major undergraduate programs, undergraduate majors that Caltech had in those years.

MUNK: In those years. But I took Applied Physics.

DOEL: Did you know anyone who was working in that area?

MUNK: No.

DOEL: It was worth seeing it from the catalog?

MUNK: It was a catalog type of thing, yes.

DOEL: What were the classes like in say the first few years when you were taking the required courses?

MUNK: Tough, like I remember Smythe's course in Electric Engineering. So then I passed, but I didn't do well until my junior year. And, but tough and interesting. I really liked it from the beginning. But then we were talking about that I didn't have any money in the second year. I became an assistant to-- there was a man named Kermit Roosevelt who taught history also. He is famous because he worked for the intelligence community once.

DOEL: Indeed he did, yes.

MUNK: And I think he is responsible for something that happened in Iran, Persia. And he was at Caltech as an instructor, and he gave me a job as his assistant, and I enjoyed him.

DOEL: What sort of a person was Kermit Roosevelt?

MUNK: Not like his father but his father, I mean the other Roosevelt...

DOEL: FDR.

MUNK: Yes. Kind of quiet and scholarly, yes scholarly would be the word. But he was nice to

me and then I meant to think of the man who became president of Stamford and Huntington Library because I also helped him. He was teaching history and we must remember his name.

DOEL: We will make sure that gets added to the transcript.

MUNK: Oh, and I have always enjoyed him and we kept even-- I saw him once or twice afterwards. I was one of a thousand students, but they were awfully, awfully nice to me, really, for no good reason.

DOEL: I was interested that you were in contact in those early years with two of the historians on the staff, and there weren't that many.

MUNK: Well, I think that was partly because they used my linguistics, I knew German and others to help, and I probably couldn't get a job being an assistant to one of the physics teachers because I didn't know enough physics. So it may have been part, not because I was interested in that because it was the only job I could get.

DOEL: Yes and so you were using your German then?

MUNK: I helped Kermit Roosevelt a little bit, there were some problems with translation but mainly I was just a TA, ordinary teaching assistant, you know, helping correct papers and doing that work, and it just barely kept me alive I think, although I don't remember ever being terribly short. But Mother I don't think she ever gave me any more money until Judy and I-- much later. She was absolutely right—I was totally irresponsible.

DOEL: But given the kind of environment in which you had grown up with it was a very different experience for you in learning how to...

MUNK: It was easy, like Silver Bay was easy. I enjoyed it. I had no problems after that I liked Caltech and I liked Pasadena.

DOEL: But that still must have been difficult when you realized you would have to work to stay at Caltech.

MUNK: It wasn't so hard. I don't remember ever really being troubled by it. And I don't know, maybe even, I may have even gotten some help then, I don't remember, from Mother after I performed. I don't even remember.

DOEL: Yes. Of course it was just around the time that your Mother would have been leaving with your stepfather to Switzerland.

MUNK: Yes. And then you know they came to Pasadena.

DOEL: Yes, when was it that they came out?

MUNK: It must have been just about in '39.

DOEL: So it wasn't a long stay over then for them in Switzerland.

MUNK: No. And England. They bought a house and my whole family came, including my Uncle Felix, and they bought a house. I stayed not at home but stayed at Caltech I had by then stayed for two or three years and I was embarrassed as Judith said by my Mother's Quaker activities.

DOEL: Was she already active at that point?

MUNK: Yes.

DOEL: When had she converted to the Quaker faith?

MUNK: She must have started in England. It must go way back. But she became really active, I think more active when she came to America.

DOEL: And you remember that as being something that started very early on from the time that she came to America, because we were discussing off tape briefly her involvement in the protests, the anti-war protests of the 1960s, but this had come much sooner.

MUNK: Yes, she was always a pacifist, whatever that means. I mean she had told me that she thought that World War I was-- I once remembered she seldom talked about it, but when she did she thought that even then, you know, when she was in Austria and my father was in uniform she thought it was a terrible mistake, and in fact I think she thought that the allies should win, even though she had been sent home from England. I think she would have preferred to live in England then.

DOEL: Was that a shared belief among others in the family or was she pretty much alone in voicing that?

MUNK: No, you know the rest all became super patriots in a way. My Italian—well that is World War II—my Italian cousins, very prominent cousins all became very prominent in the Mussolini thing and I really, I don't remember any of them protesting the fascist regime. They were doing too well. I don't think it is an issue I would care to raise when I visit them, as much as I love them.

DOEL: How often would you see your family then when you were living at Caltech?

MUNK: Oh, about two or three evenings, I think twice a week or so I would go and...

DOEL: It would be dinner and...

MUNK: But I kind of, it is really awful, I was kind of glad to be alone and I didn't really-- I would have preferred to in some ways to just be at Caltech alone.

DOEL: At that age that certainly is a common feeling that many people wanting to establish independence. You were clearly moving in new directions.

MUNK: And my Mother always thought I was sort of irresponsible, correctly so. I never knew exactly what I wanted and I hadn't planned well, and I didn't like to hear that I was irresponsible.

DOEL: That sort of thing apparently came up a few times when you would go home.

MUNK: Yes.

DOEL: Do you remember any other professors in those earlier years at Caltech who were influential for you?

MUNK: Pief.

DOEL: Before you met Gutenberg and others.

MUNK: Oh, well the man whose name we should have...

DOEL: We will make sure that that gets in.

MUNK: I think those... oh, Hausten [?], who wrote the book on physics, you must know, became president of Rice, I took physics under him. I thought he was terrific. Then Fowler, whom I stayed in contact with all my life, was there; he was a wonderful man.

DOEL: Right, Willy Fowler.

MUNK: Willy Fowler. I got to-- he was an instructor, Bateman was teaching mathematics, and I took a course from him on differential equations. Great man isn't he. Who else? I remember those people. I think that is about it.

DOEL: Those were clearly some of the critical people in the physical sciences side. Did you meet Millikan in those days, would you meet him?

MUNK: I met Millikan, first of all he gave a reception for freshmen or new people at his house, and you know he had a fantastic memory and he would see you in the hall and remember you by name; I don't know how he did it. And later on when the invasion of Austria started and I thought it was sort of the end of the world I wanted to join the ambulance corps and I went to Millikan and asked him whether he would help me, write a letter, and he saw me and he wrote such a letter, and that was, you know, when we were not at war yet. He wrote a letter to the state department, I remember that, I wish I had that—very nice—saying that he had met me and we were students and I wanted to be admitted to-- and I don't quite remember what happens but it

never did materialize. I wanted to drop out of school.

DOEL: And was this a feeling that, was it soon after the Anschluß or was it after?

MUNK: Yes after the Anschluß. I thought the Anschluß was a nightmare, partly because of my stepfather's feelings about that. And then you know, I forgot when did the United States go to war?

DOEL: It was late in 1941 after the Pearl Harbor attack.

MUNK: And then in '39 I first went down to Scripps, we can come to that, and then next year you know I joined the National Guard in Washington and I served for a while, and that is when I really became a good American citizen I think. It was good to serve in the army. I kind of believe that it would be nice to have some kind of program where everybody serves for a year, it doesn't have to be in the military.

DOEL: Right, but something similar to what European countries are often now doing, as Germany has had, that kind of mandatory service.

MUNK: Right. Then that comes back from those days really.

DOEL: Before we talk about Gutenberg I was curious if there were any others. You had mentioned...

MUNK: Buwalda was a geologist, Buwalda.

DOEL: I was going to ask if you had met him.

MUNK: Yes indeed, I took classes with him. And then there was a major earthquake in California, was it '38, with a big strike-slip displacement, and I had asked-- and I then, you know, I had decided to go into geophysics and I am having a little trouble to know when I had taken Buwalda's course in geology which was the real thing I learned how to go out and look at the. Buwalda was wonderful.

DOEL: What sort of person was Buwalda?

MUNK: Oh, he was great. I went on a field trip with him, and that was a real experience, and he would say what do you see and look at that and what do you think are the forces behind it. And that is really, I had a great influence, even though I don't think I was ever good geologically, but I enjoyed it.

DOEL: This was a summer field class or ...?

MUNK: No, that was a regular field trip at Caltech. And then I remember asking Buwalda that if there were an earthquake tomorrow could I go along and measure the strike-slips with them

and then there was an earthquake tomorrow and he had said yes, and I went with Gutenberg, Richter, and Buwalda in measuring strike-slip displacement. And now what, it was a major earthquake, it must have been about '38.

DOEL: Around '38 you think.

MUNK: Yes. And we went and looked at some places where there were displacements, I have some photographs of it.

DOEL: Interesting. Had you taken the photographs?

MUNK: That I took, not very good. And that was very enjoyable, I really loved that. And partly my interest that I have never been really 100% a wet oceanographer, in fact that we started IGPP is that I found a solid part of geophysics just as interesting as the fluid part.

DOEL: Clearly your influence at Caltech would have led you in that direction.

MUNK: In fact Caltech had no oceanography, although they do have a laboratory, but that had nothing to do with me.

DOEL: Right, but at that time Caltech's interests were limited into solid...

MUNK: Gutenberg, Richter, Bulvuldred, yes.

DOEL: And some meteorology, which I wanted to ask about.

MUNK: And some meteorology, which I had very little contact, yet Millikan's son and Krick [?] of course, I had no real contact.

DOEL: That was another area?

MUNK: The area of meteorology, I remember meeting Krick, who was kind of a character.

DOEL: What are you thinking about when you say he was a character?

MUNK: Oh, you know, there were two theories of how to do prediction, if you recall. One was to find the analog of a previous weather situation, and the other one was to understand the physics of it, and Krick believed in the former and tried to persuade General Arnold, then Chief of the Air Forces, that that was what we had to do. And it turns out that the degrees of freedom of the atmosphere exceed the historical record so that the probability of finding a situation close enough to be really helpful is actually small. You are better off working in parameter space and understanding the physics. It is not an obvious solution. I don't one would have known at the time.

DOEL: Well that was very common in meteorology in those days, that kind of synoptic meteorology, thinking back through earlier patterns. MIT was largely doing that.

MUNK: MIT, and Krick was.

DOEL: Indeed, things change a lot in the years after that point. We do need to cover that but that is interesting. This is a war type experience that you interact with Krick on these matters.

MUNK: Not closely, but I don't have a close memory of it but I do remember hearing from him. I don't think I took any courses in meteorology. I was kind of busy doing the geophysics.

DOEL: What was the first course that led you to think about the earth sciences?

MUNK: Trying to find a job for the summer so I could see my girlfriend, ??? Anderson, and that was really my first oceanography which led then into-- I must have taken Gutenberg's course before I went to Scripps, I am not sure I went to Scripps in -- now I have to get the dates right again, in '39, and then switched over I think to a major in geophysics, after that it was a major in applied physics. That was not a difficult switch you realize.

DOEL: Right, but it was the summer of '39 that you...

MUNK: Yes, and the only reason I came down is because it was the only job I could get down here. I mean La Jolla was a place of summer colleges and one business which was the Scripps Institution.

DOEL: And was this Barbara Sage that you mentioned? This was someone else?

MUNK: That, was a girl named Anderson from Texas. She was a student at Scripps College and I dated her and her grandfather was the Scripps family lawyer name was J. C. Harper, one of the people who helped E.W. Scripps start the Scripps Institute. And she would go and spend the summer with her grandparents there and I...

DOEL: How did you meet her by the way?

MUNK: Oh, I met her at some Caltech-Scripps college function. They still have kind of dances, and we...

DOEL: I didn't realize they had that kind of connection.

MUNK: Yes, totally impersonal, and I met her and she was a skier, and I kind of fell in love with her. It didn't last terribly long, but I was very fond of her. And then so when she spent the summer here I said I would go and find myself a job, and someone said, "Oh you know, there is this funny institution doing oceanography," and I wrote Harold Sverdrup, Director, and he offered me a, I know it was a \$50 a month job, and I don't think I ever lived better than that summer because you could get abalones almost off the pier, and we ate well and did a lot of

fishing, and no problem.

DOEL: La Jolla was a very different community.

MUNK: Very different community. And I stayed at a funny building called the Community House, which is the site of IGPP. Partly IGPP was put there for sentimental reasons. And I remember the first night I got there. I remember the smell, the smell of a marine biological station, and there were a few beds with no mattresses on it but some springs that were sagging and I put my sleeping bag down in one of the rooms, and it was just perfect.

DOEL: Yes, I recall, I think, that you had made a similar comment about the Bermuda station, that there was a kind of oceanographic smell to it.

MUNK: Yes, a nice smell. Yes, I remember the Bermuda smelling like that too. Scripps today does not, at least not-- probably not in any of the buildings. But it was the formaldehyde or some very moist, you know mildew was part of it, not exactly what you necessarily want.

DOEL: Do you think you'd had Buwalder's course before you...

MUNK: Yes.

DOEL: You knew you had that?

MUNK: Yes, I took his course and took his field trip. His field trip made a real impression on me. I remember how much I loved it.

DOEL: Where did you go for the trip, how far out?

MUNK: In Southern California entirely, going across into the desert looking at the San Andreas fault. And there were about something like twenty students, and it lasted about a week. And he was just wonderful. He would sit somewhere and he would give a talk about what you'd see, and he was also personally such a fine inspiring man.

DOEL: Did he talk to you about the research he was doing himself?

MUNK: I don't remember. He was an equalitive geologist, but I don't mean that in a negative way, gemophologist is that what you would call...

DOEL: I think that is a fair description of the work he did at the time, yes.

MUNK: And of course Gutenberg was there and encouraged me when I came back, and I remember we were talking about what should I work on a thesis on, and he pulled a piece of paper out of his right front drawer where he had written down potential theses' for students, one of them is how mountains are made.

DOEL: Not a small problem.

MUNK: Not a small problem. By then I decided I really wanted something with Scripps, and they were very nice to accommodate me, you know, so flexible.

DOEL: When you say thesis, are you referring to getting the Bachelor or the Masters at this point?

MUNK: I am referring now-- I got my Bachelor degree in 1940, and then a Masters degree at Caltech but on an oceanography thing that Harald Sverdrup suggested. Not a good Masters thesis, it was on the internal tides in the gulf of California, there were some measurements. And I remember Bateman helped me solve a problem, a mathematical problem.

DOEL: Right. And I do want to get to that, I have brought a copy of that paper with me about the Masters paper. I am real curious, though, you had already met Gutenberg before you came down to Scripps?

MUNK: Yes, but I don't remember when I formally switched from applied physics to geophysics, but I then had decided I would like to continue the oceanography but I wanted to finish my Masters degree at Caltech, and they were very accommodating, that is all I remember.

DOEL: Do you remember taking any courses directly from Gutenberg?

MUNK: Yes, and Richter.

DOEL: And Richter?

MUNK: Yes, both.

DOEL: Do you remember what they were teaching in particular? Was it survey?

MUNK: It was general geophysics, yes, survey, I still have his books. I remember Richter so well because you talked about some problems in seismology and he would say, "Oh, there was an earthquake which had that kind of a feature," and he would go into his archives and pull something out.

DOEL: That was how he thought about the problems, he could think in terms of discreet...

MUNK: Oh and I met the guy who built seismographs, Benioff.

DOEL: Oh yes, Hugo Benioff.

MUNK: Hugo Benioff, who also liked violins, music, and he had met one of my father's many wives so I got to know him a little bit, and he had built at the time was the best seismograph.

DOEL: He was quite an instrumentalist.

MUNK: Quite an instrumentalist.

DOEL: I remember he had begun to study astronomy at Harvard under Harlan Shapley before he moved into-- I wondered if he had ever spoken to you about that?

MUNK: I don't remember that. But he was entirely an instrumentalist, I don't think he one could look up and see he wrote any, and think his contributions were-- But it was a very interesting subject of how to build a good seismograph and when we started IGPP really, in a way it was the experimental part that was overshadowing, and I think it was something that I had brought along is can you build a zero frequency seismograph, by that I mean what is the total displacement that follows an earthquake, is it even in principle possible to do that, and we used to and you could call it either high frequency geodesy or low frequency seismology.

DOEL: Interesting way to put it, yes.

MUNK: And that was a central fault when we started IGPP, could we monitor changes in landscape in a dynamic way so you don't depend on fossil evidence entirely, but you would have-- and of course today with GPS and so on, this is the key way we are still working on trying to understand earthquakes and earthquake prediction.

DOEL: Indeed, but that was a very critical problem back at the founding of the IGPP.

MUNK: And it was very critical and foremost, and John Burger and others really who started building the equipment in Piñon Flat was pretty much oriented in that direction. I thought it was a good choice, and it was not obvious then because Scripps didn't really have any solid earth. And one of the very lucky things of course is that the vague idea, not carefully thought through, that it would pay to do some solid Earth geophysics at an oceanographic institution just happened at the time of the plate tectonic revolution where it turned out that the very key to the history of the Earth was in the oceans, and that was not something that I or anybody else realized at the time it was just plain, simple luck that we had started our solid earth at a good time.

DOEL: Right, and we are going to cover all that in detail. As you say the 1960s becomes a truly extraordinary time for the solid earth sciences. I am curious do you remember in particular the kind of issues that Gutenberg or Richter would bring up? Did Gutenberg bring up broad scale geophysical...

MUNK: I remember his pulling out that piece of paper, and one of them said the building of mountains, and I thought gee that is a pretty big topic.

DOEL: Where did you get that bit, was that something that come up orogeny or convection current and other ideas?

[end of Tape 2, Side A]

[beginning Tape 2, Side B]

DOEL: And I just want to make sure we have that on tape that you are responding particularly to having your girlfriend living in La Jolla, your interest down here.

MUNK: Yes, and then I just loved my summer at La Jolla, and I became interested in what Roger and Harold Sverdrup had said. I decided that was just what I would like to do. I enjoyed my trips on the boat, E.W. Scripps. And then when I went back and asked Gutenberg whether he would take me under those circumstances he immediately said yes of course I could write my thesis down here and he would sign up for it and he would help. He was very broadly interested, you know, his book on *Hunt Book de Geophysic* [title in German], like all German handbooks, is too heavy to be carried. It had some things about oceanography.

DOEL: Did he talk to you about German geophysics when you were working with him his recollections or impressions of...?

MUNK: He must have you know he lived very close to the campus and I had been over to his house many times.

DOEL: Was this when you were an undergraduate as well as a graduate student?

MUNK: Yes.

DOEL: Was that common?

MUNK: I don't know when he invited me. It may have been after I became a graduate student, but I do remember going to his house and it was always very-- he was very sweet and listened. And he told me the story about tsunamis, that is one thing I remember because he had written on tsunamis, and he told me that he was responsible for changing the name from tidal wave to tsunamis because he didn't want people to have a false impression that they were related to the tides, and that he was succeeded in having the people refer to it as tsunamis and then he told me that some years later he had a Japanese graduate student and he asked him what tsunamis meant in Japanese and it translates to tidal waves. He said by then he had succeeded.

DOEL: At least within the English language. How well did you get to know Richter?

MUNK: Not personally, but remembering how always helpful he was. He was sort of the ideal number two person. I mean Gutenberg could have not done his work without Richter, and certainly Richter would have never done it without Gutenberg, who I think had the ideas. But I remember him with great fondness. Always nice to see him and always thoughtful. I don't think I took a course from him. I took a course from Gutenberg. Can one get one's old records from Caltech going back that far?

DOEL: I believe that would be there that you could see your transcripts.

MUNK: If anybody can who would help us. I never kept any archives any of my old records.

DOEL: I have heard that as well unfortunately. That would be a valuable source of information about Caltech and earth sciences.

MUNK: It would have helped us in today's discussion.

DOEL: Did Gutenberg talk to you about his own research programs?

MUNK: Yes, yes. I forgot what he was doing at the time, I know he was very broadly interested, and I think that earth core was a hot topic then. Madame Layman, when did she write, when was the earth core, fluid core, discovered? The solid core within the fluid core?

DOEL: That had been earlier.

MUNK: Much earlier, wasn't it.

DOEL: But of course around 19-- and I would like to ask the Quean Rickman [?] ideas challenging the conventional core were coming out in the early 1940s but probably not in this country then. A clue that he was very interested in earth's core.

MUNK: Yeah, yeah. I don't really remember what the courses were other than they were always were very helpful.

DOEL: Did he talk about exploration geophysics or did it focus more on...

MUNK: Oh yes, he did, and we learned something about exploration geophysics. And I had a job, and that must have happened the year before I went to La Jolla, I had my first job which was as a potter for a company under a man named Soske, and it was called Geophysical something Incorporated, and I spent a summer.

DOEL: Was it Geophysical Services?

MUNK: No, that is DI.

DOEL: That was later indeed.

MUNK: That was a much, much smaller company by a man named Soske who had gotten his Ph.D. at Caltech.

DOEL: I have heard of him before.

MUNK: S-O-S-K-E. And he was working Coalinga [?] in the Peal [?] Valley, and I got a job as a potter. That was you would pick up the seismographs from a pick up truck, and I had that job for the whole summer, and it was hot and interesting, and I still had my DeSoto and I had a collision and totaled it—that is sort of what I remember of that summer, totaled it. Wasn't my fault, somebody drove on the wrong side of the street at night and hit me. But I didn't get hurt but my car was, my wonderful old DeSoto was totaled.

DOEL: I am sorry to hear that. Were there other students working on the crews or was it mostly...

MUNK: On the crews at Scripps you mean?.

DOEL: Oh I mean when you working as a potter that summer.

MUNK: On the crews, yes, yes. Yes, Russell Raitt by the way, he was a professor here was working for Soske, I met him then. He was working on the analysis of the records that we took.

DOEL: He was an older student of that?

MUNK: He was already an employee. He was a full time employee of Soske's. [answers phone call]

DOEL: You were saying that the Russell Raitt, you had met him...

MUNK: Yes, he was an employee then, and then later on he came to Scripps during the war effort, he worked with-- but he was working at Soske's. I think otherwise I was probably the only student there, but it was clearly a summer job. And I forgot how I got it, Soske had his relations, maybe Gutenberg got it for me.

DOEL: But you already had a pretty good idea of what was involved in using the sound trucks and everything. The recorders. We were talking about a moment ago after a brief interruption your experience that summer when you were working out in the field explorations, geophysics. Were there other memorable experiences from that summer which come to mind?

MUNK: I do remember learning how to put down pots without slowing down the pick up. You would hang over the side and you'd have to swing your

DOEL: Swing your arm right out.

MUNK: You know about it, yes, so that when you put it on the ground your arm swung backwards at the same speed at which the car was going, you know, you couldn't just place it down because it would fall over. I also remember meeting a librarian at Coalinga Library that summer and enjoying. Though I remember also being very hot. It was a good experience.

DOEL: I'm sure. Did it help you learn about instrumentation, was it the sort of work that gave

you some exposure to keeping an instrument in calibration?

MUNK: I had never been a good experimentalist, but I do have a firm belief that nearly all the good things that happened in geophysics and oceanography really followed rather than led the development of new methods of measurements. You know, the idea about somebody having a theory and then building a piece of gear to check it, and then finding out that it was right, sort of has never happened in my experience.

DOEL: Certainly not in geophysics.

MUNK: And certainly not in oceanography, and so I have my whole career whether it comes from that age or others has been to become excited about new methods of measuring things, and then trying to find a good excuse to have them built, and then eventually learning new things that are very exciting. I have always had very good experimental partners, and I appreciate them, and I like to go in the field with them, but I have not been the person who built the equipment.

DOEL: Indeed, one of the characteristics of geophysics in the 20th century was the accumulation of so much new data, and it had enormous pitfalls.

MUNK: But you know in meteorology at the time when I went to-- even afterwards, after I came out of-- even after the war was that the meteorologists did not take their own data. The typical meteorologist would use weather maps prepared by the Weather Bureau to, I mean in the Rossby-Holmboe-Bjerknes days you did not go and build some equipment to measure things in a new way. It was very much you used existing. And when Schafer and Langmuir started their work on taking their own measurements of ice crystals it was unheard of that anybody other than the Weather Bureau should take new measurements. In oceanography on the other hand, there was always the tradition that you could try and build new apparatus.

DOEL: That is a very interesting distinction. Of course Langmuir had gotten interested in those years admittedly after World War II and cloud seeding and related research efforts.

MUNK: But then now of course lots of people built the equipment.

DOEL: But that is a very interesting point, because meteorology had its transition perhaps, or rather the developments were occurring very much in the years after World War II.

MUNK: Right. And the whole era of Rossby I just read a wonderful summary of Rossby's career in the bulletin of the Meteorological Society, and I knew Rossby quite well, and that meant a lot to me. He was an adventurous guy. But he at the time I met him I mean the thing to do was you took existing weather maps and thought about them. You did not say gee, I really ought to measure things a little differently; that was not the way it was done.

DOEL: I do want to talk a lot about Rossby and the related work that you do, and also in the context of your visit to Oslo in 1948. I am curious on a few other things from the earlier Caltech. Please go ahead.

MUNK: No, I must be sure I keep my dates straight.

DOEL: Well I remember that you had your BS from Caltech in 1939, and then by 1940 you had already gotten the masters from...

MUNK: Yes, I got it in '39 and I was at Scripps in '39 summer wasn't it, and then I got my Masters in '40 on the Scripps internal waves, then I went back to Scripps for the summer, and then I think I joined the National Guard.

DOEL: Right, I think that is exactly it in sequence.

MUNK: I told Harold Sverdrup I really felt I had to go and do that. Of course he was very sympathetic.

DOEL: Given his own situation, I am sure he very much could appreciate that. One thing I was curious about from the earlier Caltech experience, did you have any classes in astronomy astrophysics that you recall?

MUNK: No. I wish I had because it is such a wonderful subject.

DOEL: But given Caltech's relations with Mount Wilson being near by...

MUNK: I don't believe so, and then later on I became very interested in astronomy in connection with rotation of the Earth and I did have to go and learn something about it.

DOEL: I was just curious if you had recalled any classes around at Caltech.

MUNK: I don't think so, not that I remember. Zwicky of course was there, and he was famous for giving a difficult course, and I remember getting to know him a little bit. But I don't think, no I did not take any formal astronomy courses.

DOEL: Of course Zwicky's interests were a bit far removed from rotation of the Earth and so forth, more classical areas that you became interested in.

MUNK: Yes, right, and it was sort of the kind of thing that most astronomers turn their noses up on, did then and still do of course; it is Victorian astronomy. By the way, going through a revolution right now as we speak and back to my old love on tides right now is what I am working on. Because of new measurements, namely the satellite altimetry.

DOEL: And I should say you were pointing a moment ago down this very long desk that is here in the room that we are doing the interview in, that your work is indeed right at the end of the...

MUNK: I work in here. When Judy first built the house after we got married she thought all husbands liked to have studies that are private and removed. I happen to like to work right in the

middle of things, and if I am holed up somewhere I don't like it, and that must come from liking to work in the middle of a ship or so. I don't need privacy. And so she built me a room down below which is occupied by Emmy, but I have never stayed there, I immediately moved into the hall and I have been here ever since.

DOEL: And you could say this hall is immediately-- it is in the center of things indeed.

MUNK: Your interview will suffer by it, but it is a very deliberate choice which has been...

DOEL: Was that how you felt too when you were at Caltech? Did you tend to do your studies in the middle of others?

MUNK: Yes.

DOEL: It has always been a tradition?

MUNK: Yes, I have never felt I closed a door and worked in a room. I think I like to be-- I can do that perfectly well and I feel very happy if I see things going on, as long as people don't try and protect me. As soon as somebody whispers then I am interrupted.

DOEL: That's interesting. You are able when you want to concentrate then you find that you can summon that kind of concentration, you have no difficulty?

MUNK: I like it best on a ship when you are in the middle of the laboratory and I get myself in a corner and I can work very well. I have written a lot of things under those circumstances.

DOEL: That is very interesting.

MUNK: How about you, do you have to have a private study to ...

DOEL: I have actually worked in both environments, but I don't necessarily require it.

MUNK: We ran into that problem when we built IGPP. I kind of objected and so did Judy in taking open big spaces and cubbyholing them—it is architecturally unattractive. But I have learned that there are people who just cannot work unless they are in a cubbyhole and we have really had a thirty year of a losing fight of keeping some rooms open and undivided because eventually the cubbyhole people win. You can't tell them that they must do otherwise. And once it is divided the divider seems to win and it is terrible.

DOEL: That is a very critical issue I think in many fields, but particularly geophysics where the interaction between people from different and different disciplines becomes critical for work on certain issues, and I do want to talk to you about that when we can talk more about Scripps. Were there any other people in geology or geophysics in your undergraduate years that you feel were important for you?

MUNK: I am trying to remember the person. I then moved into Mudd Hall it is one of the buildings in Caltech and I had an office there, very nice beautiful new building, and next to me was a man whom I should remember who became a well known geologist, but I wasn't terribly close, I met Pief at the time — he was teaching an electronics lab course that I took when I was a senior I think and we both stayed at the athenaeum for a while, that is after I became a graduate student I guess. I stayed upstairs in the upstairs athenaeum there is open space for sleeping. Pete Panofsky, who is such a wonderful man, you know about him, and Judy went to school with his wife at Bennington, so we have double connections there.

DOEL: I am curious in general if there are other experiences that you had in your undergraduate years at Caltech that we haven't covered so far.

MUNK: The field trips certainly that we mentioned with Buwalda were very meaningful and with...

DOEL: And it sounds as if he instructed you when you were out in the field, you weren't left to go looking or to reconstruct the landscape as much on your own?

MUNK: Well, you just listened to him when he looked at something and try to see the history behind it. You know, it is a fantastic talent of some geologists that they can see the dynamics of the last ten million years in their geomorphology. And I even had to produce a geologic map, the only time I ever did it, which I didn't do very well but it was a very good experience to do that.

DOEL: I can imagine. One reason I was asking you that was that I recall that when J. Harlen Bretz at Chicago taught the course around Lake Barabou in Wisconsin he would simply ask his students to try to recreate the geological history without giving a lecture or discussion.

MUNK: Lecture would be not quite a fair word for Buwalda. He shared with you how he reacted looking at something.

DOEL: Interesting. You see we are learning about how he thought about these problems.

MUNK: How he thought about it, and responded of course a lot to questions. They weren't lectures they were sort of sitting around somewhere and looking and trying to... he would show you how to open your eyes and look.

DOEL: When you look back on your undergraduate years at Caltech, did you feel that you were given up to date instruction in all the fields you were covering? Or did you feel that there were certain areas left uncovered?

MUNK: I felt that I've gotten a wonderful education, and from people very, very well informed and thoughtful people, and nothing but a positive reaction to it.

DOEL: Did you come to know, and this will overlap I am sure with your Masters year there, the

people at Berkley? Byerly and so on. Did you come to meet Byerly?

MUNK: No, I don't know when I met Byerly probably later, not closely no.

DOEL: Did you know, was there a colloquium or a lecture series that you were attending. Did you meet other people coming through this?

MUNK: Oh yes Caltech had colloquia, but I certainly don't remember any connection with UCLA or Berkley at the time.

DOEL: Do you remember any of the lecturers, any of the people who came through as visitors or speakers at Caltech?

MUNK: I think that probably happened more when I came back after the War. Then of course I became sort of a young person as part of that. I don't recall that during the undergraduate years. I became a good student when I was in my junior year. I enjoyed my skiing at Caltech. Generally a very positive experience.

DOEL: Were there other activities you were involved in, you mentioned presidency of the ski club.

MUNK: And... not very, I wasn't very, I don't remember anything else I was very active in. I had my girlfriend Barbara that kept me busy, and I met a kind of interesting character at the time named John Lillie who became well known with his experiments with dolphins.

DOEL: I was going to ask you if this was the same man.

MUNK: He was in our ski club. In fact he and I shared and he became a good friend. I sort of lost contact with him. I think also he went a little bit nuts on his idea of what lengthy submersion means to people. But he was an interesting man.

DOEL: And he was on the ski team you say.

MUNK: Yes he was, he and I shared the presidency in two years or so and we went together.

DOEL: And you mention a number of the factors that brought you down to La Jolla during that summer between your senior and your first graduate year at Caltech. You mentioned too the memory, the smells the first night. What other things come back to mind when you **think** about what it was like to come to know the research going on or the broad activities.

MUNK: Well I met Roger that year, that summer. I met him—of course he was a character even then—but I met him because they gave me a desk in the library, the old library that has been torn down, which is big. And then Harold Sverdrup suggested that I look at that data, and then I think I said this, I noticed there was another table that had books piled high and I asked someone whose books they were and well that was Roger Revelle. He piled them up and he never took

them back, I think the librarian objected to that, you know. And then one day he came to look at something and spent time talking to me, as he would and of course.

DOEL: What was he like in those days?

MUNK: Oh, he was a very romantic character even then. And then he was going to take a trip out to measure currents between here and Catalena Island and he took me along, that was my first sea trip. And you know, he is no better experimentalist than I am but he is very stubborn and I remember he was trying to get his Eckmann current meter to work, and it is a current meter that has a disk that is magnetically oriented, and for every ten turns of the propeller you drop a ball into one of eighteen deep compartments telling you which way it was oriented, and he couldn't get the balls to drop. I remember spending all night trying to see why after ten turns the ball didn't drop, and he got it to work. I don't know with sheer stubbornness, and I helped a little bit but not much and we took measurements then and then we went to Catalena and had a nice shore duty and all had a beer or something, and it was fun.

DOEL: Did he talk to you about the research that he was doing. He was analyzing the cores from the last *Carnegie* cruise wasn't he.

MUNK: From the *Carnegie*. Well actually he was then doing some also current work on the circulation. And I also met Frances P. Shepard at the time, in fact the night I came down to stay at the community house, Frances Shepard with two of his students Ken Emory and Robert Dietz, appeared from Illinois, they had driven to La Jolla, and they occupied the other two rooms at the Community House, so I met Frances P. Shepard then. He was a beach processes man, a pioneer. Do you know of his name?

DOEL: Yes indeed, and his work becomes rather interesting in the late 1940s regarding dailies and some of the issues that you found interesting in sea level change.

MUNK: Though I thought that Frances Shepard's work, I have always felt it was so not qualitative but not very decisive, not very interesting. I never thought of him as being a great scientist. He certainly wasn't in Roger's class. And now is the first time where we are coming into something where I need to be careful on and Harold Sverdrup shared that with me. He wasn't sure-- you see then Shepard was not a member of the faculty, he was a summer investigator there from Illinois being given space, and I know Harold had some doubts as to whether he would want him to-- And Shepard wanted to come permanently and Harold resisted it. In fact there were some people here that surprised me. There was a physical oceanographer named George McEwen and I couldn't figure out and it was my first real experience in research what George McEwen was doing, he did some forecasting, some long range forecasting which as far as I could see was useless, and Harold Sverdrup thought so too, but it appealed to the local community and they supported him with a small grant. And he had figured out a climate coefficient which was sort of defined as maximum mean monthly temperature minus minimum divided by rainfall in mercury units times solar days and something. It was a complicated definition, and it was normal to give 100 for San Diego and no other city in the United States

could reach 100, it was suitably defined. And I remember asking George and saying why did you multiply by the square of the sunshine instead of just that, I mean was there some physical reason, I never got any, and I was very disappointed. I didn't understand how that was exciting, they were sort of definitions without reason and I also found that Sverdrup was disturbed and wanted to eventually stop that activity because he thought that McEwen was doing things. So that was disturbing to me. I have since learned that probably McEwen was a better scientist than I thought.

DOEL: But at the time it impressed you that there wasn't really a strong physical correlation that you could see?

MUNK: I thought it was arbitrary number punching. That was not true certainly of talking to Roger, and certainly not of Harold Sverdrup. It was a totally different kind of era. Harold had inherited McEwen.

DOEL: That is one of the important issues I suspect that coming from Vaughan's era — Harold Sverdrup elevated the standards that Scripps considered.

MUNK: He was very disappointed he would say privately into what he inherited, and especially George McEwen he was hoping that he could get some physical oceanography started and he never found that George McEwen was the person to do it with. And so he was kind of glad to see me come. Of course he was working with Fleming and Roger whose interests always encompassed everything. But certainly Sverdrup was not able to make a closer contact with McEwen.

DOEL: And as you say and clearly Fran Shepard's work was considered very controversial through the latter 1940s.

MUNK: And Harold Sverdrup didn't think it was very good.

DOEL: Clearly Harold Sverdrup was a major...

[end of Tape 2, Side B]

[beginning of Tape 3, Side A]

DOEL: You were saying a moment ago that his influence on you was tremendous. One thing I am interested about among many things, how much interaction did you come to have with him in those earlier years? Clearly people who were coming from European schools may have had a different way of interacting with students than those who came out of Caltech.

MUNK: I think he was unusual for a European in not having a... He was not a person who would slap your shoulders or something like that at all, but he was there was no Herr Professor kind of thing. And you know he had gone through Leipzig first degree, in fact he liked the American informality very much. You may remember that when he eventually went back to

Norway he chaired an international commission, actually a Norwegian commission which tried to reorganize the Norwegian University away from the German...

DOEL: Towards the American model?

MUNK: Towards, yes. So I think there was something in that that was much deeper and besides it not being really the only physical oceanography student it wasn't a matter. And then Ann Gudron enjoyed playing tennis with me. She was a good tennis player. And whenever I would beat her, which would be usually, they would ask me for dinner, that was my great interest thing. So we would play tennis and then she would say have some Fiske pudding with us. I don't really like Fiske pudding, but it was a great honor to join them and eat. They had a daughter named Anna, a very good looking Norwegian girl, and it was good fun to have company with her. And so we became very... I enjoyed it very much and I think they enjoyed having me.

DOEL: And this was already starting the first summer that you were down here and...

MUNK: Yes, I was beginning to be tasked to be play tennis. And Fran Shepard's son Tim who is two years younger than I am and I became great friends and we have maintained that. He was really very close we often went skiing together and we dated together he now lives in Kauai. I recently spent an evening with him. He is writing a book about his father. When I am a little to say things about Fran it has more to do with Tim than anybody else.

DOEL: I understand.

MUNK: I really did disapprove very much of Fran later on when he opposed Roger Revelle's directorship, and we will come to that later. I thought they all behaved very badly.

DOEL: Yes, that is certainly a critical episode in the development of Scripps.

MUNK: Yes.

DOEL: One thing I was curious about just in thinking of things you've said recently, was athletics important to quite a few members of the Scripps community when you think back to those years?

MUNK: Athletics, no.

DOEL: It was more your interest in your...

MUNK: Gudron wanted to have a tennis partner and it worked out very well. No, no there was no athletics that I can think of at all except people went swimming and caught abilonos and went grunion hunting, and we had fun. I was never a good swimmer but I joined the rough water swim one year. I forgot what year that was, whether it was the first or second year, but then it was a long swim and we started on the north end of the pier and ended up at the cove. Much longer

than now. And there was a famous, there was an author living in La Jolla called Max Miller who wrote a book called *I Cover the Waterfront* which was once a bestseller, and he had traditionally come in last, he was very proud of that, and he had great difficulty with me because I was behind him and he had to wait until I passed him so he could maintain his record, he had a very difficult time doing that. And Revelle's children swam with it, and Tim Shepard did, so those were friends.

DOEL: I was simply curious, certain institutions informal sporting events sometimes are...

MUNK: Too small, and you know there was no student body at the time. I was really, really Roger had just gotten his degree and at the time there were no other students, I was the only student and there was not anything that resembled organized athletics.

DOEL: How much do you particularly remember learning from Harold Sverdrup in that early summer? What sort of things did you deal with most often?

MUNK: My chief learning was really the second year when he was working on the book *The Oceans*, and when he gave me a little office across the hall from his back door in what is now the old George Scripps building, and he would call me in. I have written that up in connection with a volume on him, and he would call me in to use me to talk about what he was doing, and that was a wonderful experience.

DOEL: You were reading some of the manuscripts that were coming in for the volume?

MUNK: Well I was listening to him before he would... He would have his desk full of books and things. You know he had several really great ideas about it, one of them of course was that there was a point in writing a book about all aspects of oceanography, biology, chemistry, geology, and physics, and it was probably the last book, it was the last book, and there was a wonderful review of it that came up. You may have seen that, I mean reviewing the oceans as if it were written yesterday, where did it last, where did it not. If you haven't seen it you should.

DOEL: I believe I have but I will...

MUNK: And I wrote a chapter on that. But I think that comes from Hal Sverdrup's Arctic days, when he really felt that a single combined work on looking at the oceans was the thing to do and he undertook this job.

DOEL: Did he talk to you much in those early years about the *Maude* and the *Nautilus* and his...

MUNK: Yes he would talk about the *Maude*, he would love to talk about the *Maude*, and he would at dinner time and he would love to tell stories, and many, many, many of them, right. Yes he would talk about that. He was perfectly willing. But at those other things he would have his books around and he would walk around and try and speak us to what he thought and he had this idea in physical oceanography which I think was new that you start with the Antarctic and

you think of the oceans as three fingers extending north. I don't think that had been done. I mean probably everybody else started with the North Atlantic.

DOEL: That was the familiar ocean, and of course the meteor had been studying that.

MUNK: So he did it that way and he tried that, and I really just listened. But I certainly remember those days. And then he would go and dictate to Ms. Kenter, Tilly Kenter, or was it Ruth Rayden, no it was-- those were the two secretaries, and do a pretty good job of dictating a chapter. But he had thought about it, he had a good memory and he would talk about it for a few days and reorganize it, and by the time he dictated it it was a good first approximation.

DOEL: It is very interesting that he would dictate a chapter rather than try to write a rough draft. Were there other scientists that you knew at that time who did similar practice?

MUNK: No, but I didn't know many people writing books. No, and I bet you that was pretty unique. And it was part of his great desire at synthesis — I mean he felt that things have to make sense in some simple way, and if they didn't he would talk about it, and he carried that into his life, you know, and things had to be reasonable, he believed in that. I think I have a story about him that I may or may not have told you or told elsewhere that one day he asked me to come to a meeting in San Francisco oceanography, it was the first time I ever went to an oceanography meeting, you know being invited by your professor to go. And then he said in the evening—oh, and he had been in San Francisco before when he got back from the *Maude* some decades earlier and he prided himself with an enormously good sense of direction. [Hi, sweetie, I am telling the story about Hal Sverdrup] And so he said let's go and have dinner at the Fisherman's Wharf and I said love to, and we took a cable car, and he said oh I remember yes from my previous visit, he said yes now we go two more blocks and then we turn right to get us to Fisherman's Wharf, and we went two more blocks but it turned left and he was stuck only for about ten seconds, then he said, "Ahh, very reasonable directions." But that was sort of part of his...

DOEL: I was going to say that sounds like what you remember of his kind of flexibility of dealing with situations.

MUNK: But always trying to find some simple reason behind it. He was certainly a great synthesizer.

DOEL: That actually leads to two interesting issues. Did he talk to you about the relationship, the interface between theory and experiment?

MUNK: Oh, he didn't talk about it, but it is very clear how he worked. He worked like Rossby did in looking at data, and then he pulled his reasonable but not very good ability in mathematics to quantize it. Not in any phony way, I mean there were people at that time who were trying to impress their colleagues because they had learned how to solve a differential equation. That was not him. He really then did try and use quantitative tools, but they always came second. And even for Rossby, though Rossby was a better mathematician his primary drive was some intuitive feeling and then came in his mathematics, and it would drive Carl Eckhart crazy.

DOEL: I could imagine that would.

MUNK: And not because Karl was wicked, but it just was a collision course. Karl's way of doing things was to make a very careful mathematical statement of assumptions and then let the mathematics tell you what it implies. And this idea of... he just used to hate Rossby's, not personally but the way he worked, and even Harold. That was just two different ways of doing it. Roger comes closer to the sphere of Rossby's school.

JUDITH: And you do a little bit and that used to drive Karl up the wall.

MUNK: Karl was very rough.

JUDITH: He would get very upset, emotionally upset.

DOEL: Well those are certainly important but very distinct ways of proceeding in science like oceanography or geophysics. Do you remember anything in particular, interactions between people like Rossby and Carl Eckhart over those sort of issues, discussions...

MUNK: But they clashed. But Rossby was here for a summer I don't quite remember was it ... it must have been '40, 1940. Could we see because that is when he did his great paper on planetary waves. And I got to know him and he would reason-- he needed an audience like Harold did only more so, and I was the picked audience and that was great fun, and he did his work on planet airwaves and that $f_{dy} = \beta$ and the fact that there are a whole class of oceanographic problems where it is not the Coriolis Force but its variation with latitude that give you the basic physics. That came out of that summer.

DOEL: Right. Did he interact with others in addition to yourself, Rossby in working through those problems, or did you feel you were particularly the one?

MUNK: I think it was probably with me there weren't many other, and with Harold himself, but I think probably more with me. And then Rossby asked me whether I would join him in Chicago.

DOEL: This was of course 1950.

MUNK: Much later, much later, after the War, and I declined, and he never forgave me. He had never been declined before, when Rossby called you said yes. "Tomorrow, sir?"

DOEL: That is interesting. And it is interesting too particularly since Rossby left the United States not very long after.

MUNK: After that to go back to Sweden.

DOEL: Had he already known that this would be his new pathway at the time that he called you?

Or did that come afterwards?

MUNK: I don't think he knew. He was at MIT before, but then he spent a lot of time at Woods Hall, was that-- we need to get his dates straight. And I visited him in Sweden after he went back. No there was no problem that I don't remember whether at the time-- no I am sure when he asked me to come to Chicago there was no thought of his leaving. He was building up a department.

DOEL: Right, and he wanted you to be a collaborator in this broad undertaking that he was doing. We do need to get back to that in particular. One of the other things I was very interested in is indeed that problem of maintaining interdisciplinary connections and what Harold Sverdrup was very...

MUNK: Yes, and we had a weekly seminar that where everybody spoke on all subjects, what was it, Thursday afternoon at Scripps hall, and of course that has never happened again. And people have tried interdisciplinary seminars and they always fail, and it fails because there are so many seminars that people don't have the time but that was wonderful you went to those, always.

DOEL: What caused that seminar series at that time to succeed? The one you're remembering from the pre-War era?

MUNK: I think it was the thing to do when you were at Scripps. I mean Harold Sverdrup just by example said that you need to know about what your colleagues, what different fields of oceanography, and have no question, that was the day you went to the seminar and you didn't not show up.

DOEL: Did Harold Sverdrup take a hand in summarizing papers and posing questions to the speakers, or did others share in the role of actually running the seminars?

MUNK: I don't have any memories of him taking sort of a leadership, or they didn't tell us or so on. It wasn't really his style. I know that he personally was disappointed in the quality of the research group at Scripps, other than I think he had certainly admired Roger and Dick Fleming and probably Martin Johnson, but other biologists, chemists certainly, George McEwen, he felt were not and there was nothing he could do.

DOEL: Of course Dick Fleming and Martin Johnson were the collaborators on the ocean...

MUNK: It interested me always why he didn't ask Roger with whom he was personally closer, and I think for a very good reason—Roger never got anything finished. He probably realized that the book wouldn't be written if Roger was-- whereas Dick Fleming who does not have Roger's vision, was well organized person who did a good job.

DOEL: That is a very interesting observation. Do you remember any particular colloquium out of those early ones that you were attending, any that left an impression?

MUNK: Not off the bat, and I think most of them really came when I returned after the war.

DOEL: After the war. And these were mostly in-house speakers in the sense that they were mostly coming from...

MUNK: Mostly in house of course visitors ...

DOEL: Rossby you say was there

MUNK: He was there. I don't even remember whether he gave a seminar that summer. My contact with him was largely his sitting on the beach and saying come and join me and listen to that, and he had just worked through the planetary waves. And then I remember he showed me, he was intrigued with some... he was intrigued with using elegant mathematical solutions, much more so than... but it still came afterwards and not...

DOEL: It was the physical intuition that proceeded in his thinking. That is very interesting.

MUNK: What is the name of the book Kuerrant [?] and Summerfeld, so he was using that as a text book and I remember he was so pleased in finding a dispersion relation that he had gotten out of his work with beta, with the beta effect, that helped him do the work. I think that was very good work, and it still hasn't come to the end of its usefulness. And of course Harold's very late and really very amazing work that led to the Sverdrup dynamics which came clearly out of observations, was really made the beta effect of the-- made the importance of that effect clear because you got things to happen at the equator that weren't singular were the Coriolis Force goes to zero. And Harold was very worried about it. He said how can something so obvious have escaped the literature, there must be something wrong I am doing but he has a very simple way of finding the dynamics of the oceans, and he spent, he waited for a year before it published because he felt it was so simple that it should have been clear. And of course he had done, actually Harold Sverdrup had written a paper on tides in the Arctic ocean which foreshadowed that particular kind of dynamics. That wasn't as well known, but it was probably the earlier part about writing about the way the equations that depend on the DFDY rather than the F.

DOEL: That is very interesting. One of the things I just really didn't want to lose a thread on, did Rossby talk to you about his sense of who the more interesting people were in meteorology or water...

MUNK: I think so. And you know, he was a very gregarious man and he was one of the earliest probably the first person I know who would pick up the phone and call anyone anywhere. You know, that was unheard of. I mean probably Harold Sverdrup wrote down a list of things he would discuss before he picked up a phone to call Los Angeles, but not Rossby. And so he was sort of in daily touch with the community, most of which were Norwegians, Bjerknes, Humboldt Petersson, Colonel Petersson and all these people, and he had an entourage of meteorologists and that is how he worked.

DOEL: And clearly by that point you had become familiar with the ??? polar front and ??? and so on.

MUNK: Right, so he operated that way, he was an operator, and he got new ideas and he never really quite-- he drove other people, then Carl Eckhart, crazy, but they were more loyal than he was humble. He was really kind of a dull man who was good at vector analysis, but was one of the well known people that had written a textbook that everybody knew. He objected to Rossby bringing him some new concept and then going on to something else before it was cleaned up and I have always thought if I were a good caricaturist I would have shown Rossby with his big smile and his fanny hanging out making a new pile, and hung over there with a broom saying cleaning up the last one you know and putting it in a sink, saying, "Good stuff, wait, wait! I am still cleaning up the last one." I thought that would have been a wonderful caricature of the time of how they worked.

DOEL: That is very interesting yes.

MUNK: If I could give you that picture I wish somebody could make such a picture.

DOEL: You did a good job of describing it right here, and I think that is real interesting given in your own career that you didn't want to be someone sweeping up the last parts but you were much more comfortable going into new fields. And it sounds as if you were getting an exposure to this sort of exposure early on in your own graduate training.

MUNK: Right, right, and it has its advantages and disadvantages. I find that almost nothing I have done hasn't been done better since, and of course that is the way it ought to be, isn't it. But it is nice sometimes to have done something that stays without being improved or corrected. But I certainly have had fun doing things before other people did it, and at the time it was kind of recognized, and then find that years later it has been done so much better.

DOEL: But on the other hand it is following earlier and pioneering work of those that have made the first step. It is a different sort of activity.

MUNK: It is a very interesting problem, and I guess you just do what you care to do. I am not a ???.

DOEL: Who else did Rossby feel was doing important work in meteorology at the time? Who did he particularly respect or whose issues did he...

MUNK: Oh the Bjerknes people and Harold Sverdrup. You know Rossby never got his doctor's degree, and therefore he expected who would get a degree under him to do more profound work than he had done, so it was very difficult to be his student. But who were the good people in Rossby's day? Well, Charney of course came up, Jules Charney and I got our PhD the same day at the same place, with the same committee exactly, and Charney, yes, I think he probably respected. But who were the good people at the time if you look at Rossby's geologic thing? I mean the Bjerknes was...

DOEL: It was largely the Bjerknæs school and some who were coming out from it. I was just curious if he had mentioned others in this country who had helped him. Wexler was just coming over at the same time but he was just finishing his own degree during the war period.

MUNK: And then Joe Smagorinsky, and of course Humboldt. I think that they weren't his equal, not that he kind of made that apparent, but I don't really recall of any other giants at the time.

DOEL: Now this gets interesting in connection with Van Neuman's [?] project at Princeton and of course in the post-War.

MUNK: Yes, and then Charney was close to that.

DOEL: Indeed. What sort of person was Charney? How well did you come to know him?

MUNK: Oh he was mainly just great fun, he had a bit of a magic of how he dealt with people and it was just really nice to be with him, and he was thoughtful and good with his students. I was very fond of him.

DOEL: He was caring as a mentor.

MUNK: Very much so, I was very fond of him. Judy, you liked him to didn't you Charney, Jules Charney? He had sort of a bum marriage and then he had a girlfriend that lived in Italy and we enjoyed her.

JUDITH: He just loved good food.

MUNK: Yes, he loved good food and beautiful women. And his son Nick started a magazine called *Psychology Today*. It was started in our little guest house up here. Nick Charney had just gotten a degree in Psychology at Chicago and he had this made idea of starting a magazine and he had no money, and we put him up here for a couple of months.

JUDITH: We wouldn't let him buy anything but he would go and buy something too expensive. We let him sleep here until he figured out what he wanted to do.

MUNK: Right.

JUDITH: You can have lunch any time here five minutes for now.

MUNK: You know what we are going to do it is 12:40. I am going to walk you around once we need to both stretch our legs.

DOEL: This is a good time to pause.

We are resuming our interview right now after a good lunch break. You were talking we were

talking a bit about Harold Sverdrup and the research that he was doing that you were becoming acquainted with during the first year, the first summer that you were down at Scripps. One thing that we haven't spoken about is how you became interested in the research you did for your Masters thesis. How did that come about?

MUNK: Harold Sverdrup suggested it. He had just taken the E.W. Scripps down-- actually I don't think he himself went to Baja, California. It was our first, you know the Scripps institution was really at that time very land-bound and...

DOEL: Just because of limited access to ships.

MUNK: Ships, exactly. And the trip into the Gulf was a major step forward and was one of I suppose our three major steps, first the Gulf of California, then the waters of California when we got the sardine money, and then the Revelle global exploration or Pacific exploration phase. But anyhow he had looked at some measurements indicating a variable temperature structure in the Gulf which he thought could be due to a standing internal wave, and he, Harold, suggested that I have a look. And so I just read something about internal wave dynamics and didn't make an attempt to analyze the data. I don't think it was a very meaningful research thing, but it was certainly an interesting way for me to start, and I chose it because it was suggested by Harold.

DOEL: And I should just say we are talking about internal waves in the Gulf of California, which appeared in the Journal of Marine Research, which itself was a new journal back in...

MUNK: Yes and I think Rossby had a lot to do with starting it.

DOEL: Interesting. Did he talk to you much about the journal or about publication access for people working in meteorology?

MUNK: I do remember him saying something about my paper when I met him, I think a little later. And I was surprised and I said have you read it and he said I read everything that appears in the Journal of Marine Research. Because it was an easier time to keep up with the literature. But I ran into some kind of a mathematical problem and I remember Bateman helping to find a way to solve it but many, many years later I was going to go back to internal waves in a quite different context. And in some work that did and is still used but very different from the Gulf one...

[end of Tape 3, Side A]

[beginning of Tape 3, Side B]

MUNK: And it was to be many years until the Doctors was to be done because the war came in between.

DOEL: The War, indeed, and we will turn to that. I think we had probably better talk first about the war years before we do get in and discuss the work that you did for the Ph.D. You mentioned

indeed in the acknowledgments of that paper that you were indebted to Bateman and also to Dr. Lek and a Mr. Brenner.

MUNK: Well Louis Lek was an oceanographer who had studied with Defant in Holland, and had come to the United States and had built a beautiful house up on the hills here, and he was a friend. And I had forgotten that I acknowledged to him. Did I acknowledge to Roger and Harold, I just forget?

DOEL: You did to Harold, but not to Roger.

MUNK: I don't think Roger had much to do with that actually.

DOEL: This was separate work that was being done at the time.

MUNK: Right.

DOEL: And what sort of person was Lek? What kind of training had he gotten under Defant?

MUNK: Well, Louis Lek was a wealthy young Dutchman, and his family were part owners of the de Beers' diamond business, and he had sort of studied this as a hobby he never had a job then he came to America and built this lovely house to sort of live there, and he is perhaps best known because he invited Defant to come over and work with him on translating Defant's two volume physical oceanography which I have somewhere here.

DOEL: And you are pointing to the shelves right now in the room that we are doing the interview in.

MUNK: Right, and I think one of these things I think the green one here is-- and Louis translated those two very extensive volumes. And he was a friend and we saw something of him. Judy got to know him afterwards. And he died some years ago, quite some years ago. He gave me some golden cufflinks which I still have.

DOEL: Do you remember roughly when it was that he had invited Defant to come over to?

MUNK: Can we look that up? It is one of those Defant's book the green ones on that second shelf? No that is the C's.

DOEL: Right here indeed volume one here is I am taking it down from the shelf right now.

MUNK: See whether in the introduction ... Oh it is not in very good shape is it.

DOEL: Well it is in fine shape except for the cover.

MUNK: See whether it must say something about...

DOEL: It does. 1961 was when it came out with Pergeman [?] Press.

MUNK: Yes, but when was he in La Jolla, would it say so in the introduction? There must be something by Louis, too.

DOEL: I see the introduction by Defant, and in scanning this quickly, no I don't see him mentioning being over at...

MUNK: He stayed at Louie Lek's house, so there is bound to be somewhere.

DOEL: He mentions where he stays in Stockholm with Rossby.

MUNK: But not-- there must be something somewhere.

DOEL: And there is a mention of Woods Hall in a slightly different context. I do see it here now, drafted years ago was translated by Dr. Louis Lek, La Jolla, California, the second volume, but it doesn't indicate what year, it doesn't say what year that had been. Was it after the war that that happened or do you think that might have been before?

MUNK: No, it must have been after the war. Defant was one of the great people in oceanography clearly came from Innsbruck.

DOEL: How many others were working in Holland in oceanography? How strong was the contingent, or do you feel that he was working largely alone?

MUNK: Gee, the Dutch, I have met some other oceanographers. I don't think Louis ever worked in it. It was a game to him and then he enjoyed translating. He never really did any significant research.

DOEL: And Defant himself?

MUNK: Oh, Defant did a lot of work.

DOEL: Indeed he did, and I wanted to be sure his research...

MUNK: He comes from the meteor period, you know, he participated in the meteor expedition. They did great, they did some very good work.

DOEL: What I was curious about particularly is his main institutional base as you remember during the times that you had known him.

MUNK: Oh, well, during the war I think he was in Innsbruck, of course the meteor work was by a man named Wüst.

DOEL: Georg Wüst.

MUNK: Roger, I remember never really wanted to see him he said Wüst was a Nazi, and Roger must have had some intelligence information. I remember that quite distinctly that Roger showed one of his very rare statements of not wanting to really see somebody. Defant worked closely with Wüst. I don't know. I went over to Germany soon after the war and met some of the people there, a man named Brunnacke who had been very prominent.

JUDITH: Well we went up the mountain to see Defant.

MUNK: We went up and saw Defant in Innsbruck years later. He was a sweet man. My memory was that I visited one afternoon went up to Louis Lek's house because Defant wanted to talk about something, and he had just made some plots for his book and there was the usual scattering diagram and he had plotted a beautiful smooth curve through it, and I really didn't see any obvious sign of correlation, and I asked him how he had drawn that curve, and he gave me a wonderful German answer. How is your German?

DOEL: Sie ist nicht gut.

MUNK: Sie ist nicht gut. He said mein mustoche und kosmission shrunck hahben [?]. And I wish you would remember that it was such a wonderful answer for an Austrian. Mein mustoche and kosmission shrunck hahben.

JUDITH: What does that mean?

MUNK: One has to have a cosmic swing to see how that relation exists. I have never forgotten that answer—it is a very good one. And if you knew enough what you wanted you could do that.

DOEL: That certainly wasn't Carl Eckhart's approach in such matters.

MUNK: Oh no! Karl would have been equally horrified.

DOEL: That was one of the things I wanted to talk to you about. When did you particularly-- you had taken classes under Carl Eckhart as I recall, had you not?

MUNK: You see then that really is after my army days. I came down in 1940 again and worked but not wholeheartedly because I was so upset about the Anschluss, and then as you know before the summer was over I actually drove up to see ??? without much plan and enlisted in the Washington National Guard. At that time I had asked Harold Sverdrup when I was still here saying could I become his Ph.D. student, and I wrote that up in my biography, because I never will forget it, he didn't answer me, and sort of fifteen seconds of silence is a long silence when you ask someone that and I didn't know whether he was preparing to make a polite no, and then he said, "You know I cannot think of a single job that will open up in oceanography in the next ten years.." And I in my usual non-think said, "Oh fine, I will take it." And that was the beginning and of course then came twenty or thirty years when there was never less than five good jobs waiting to be filled.

DOEL: But of course his own experience was one of waiting for positions or not being able to take a position because of his earlier commitment to the expeditions that he had, but his formative years were...

MUNK: And like in Norway you didn't get a job until somebody who had tenure at the University of Baden [?] died or retired and they were very limited, and of course the explosion of marine activities was yet to come. But anyway that was when I decided I wanted to become an oceanographer.

DOEL: Clearly that was an important moment to you because you have remembered that in your own writing.

MUNK: I remember it extremely well.

DOEL: Do you remember feeling concerned at all about the prospects for employment, or did you feel that it would work out for you?

MUNK: Well you have heard enough about my irresponsibility and the fact that I never really had a hard time, that I really didn't worry, I didn't think about it. I mean it wasn't a matter of being so smart, you know; there was going to be a revolution. And Roger Revelle who spoke at my 65th sort of used that as a theme he just said that I was lucky, and he said that people who review scientists vastly underestimate the importance of luck. And I think I really probably remained lucky until I ran into the environmental battle. No, I have had two very bad experiences, three bad experiences. One is losing my clearance, that was terrible. The second one was when Mohole failed, of which I had some responsibility, and the third one is when we ran into the environmental groups it was awful.

DOEL: And we will cover those appropriately with all the other things occurring at the time.

MUNK: I am just anticipating. But so I worked sort of half-heartedly, Harold said I could come and become his student, then I drove up and joined the army and I was gone for about 16 months, it is still before Pearl Harbor. And you must realize that it got to be an awful bore — I thought we were going to be in war in a week and I signed up because I thought that is what I ought to do. And then after spending a year and a half plus and learning about when to salute and when not to salute I was beginning to get bored, and I joined up as an enlisted man and never told anybody that I had had any degrees because I didn't want to go to an officers school or something like that. And then Roger and Carl Eckhart, especially Karl, and Harold Sverdrup and Dick Fleming and others started that activity at Point Loma. It was then the US Navy Radio and Sound Laboratory, and the activity was part of the University of California Division of War Research, and part of the NRDC that you were mentioning before.

DOEL: We had mentioned that of course off tape when we were talking over it at lunch. I wonder just before we talk about that, I was curious what led you to enlisting in Washington. Of course you end up in the army ski battalion and there are clear connections there.

MUNK: Well partly it was my girlfriend Barbara Anderson, she was living in Seattle then.

DOEL: I see.

MUNK: But it wasn't just that, and we weren't really that close any more. But I drove up and saw her and then I just decided one day to drive by and enlist and nobody asked any questions. You probably don't realize how disorganized it was. And the National Guard is really kind of a sad outfit. I think I was the only one who had gone to college in my whole battery, so it was a quite new experience, and I certainly wisely never told anybody because that would have been the end of my existence. I would have been teased forever. It just wasn't the thing to do.

DOEL: And I imagine most of the others who had enlisted were native born Americans.

MUNK: Yes, in Seattle, partly a few of them because the judge told them they could either go to jail or join the National Guard, there were some people like that in our division. And that was quite an experience and I am certainly not regretful of it. And then I did have a chance to get started on this ski group and that was very nice.

DOEL: What was it like on a day to day basis during those months that you were there?

MUNK: In the winter we would drive up and we would learn how to carry a heavy pack, how to ski with a pack, how to ski with guns. But of course the real American ski group started in Camp Vail, Colorado a year later. This was sort of an early and half-assed attempt by a few people up there who knew something about skiing and they let me join. But as a whole it was a bore because it was peace time, and the National Guard is not a very interesting outfit. It was probably a mistake. So when I got a letter from, probably from Harold saying that they had started a group of people working on war problems and I could I find a way to join because there were just no young people around, I got a discharge and it was peacetime and it was not all that hard, and I drove my car, a different car, down, I had taken it to Seattle, and arrived in La Jolla three days later to get-- and Pearl Harbor happened. And so I would never have been discharged of course and my outfit went into the Japanese period of war and was totally wiped out, enormous casualty rates. And I always felt a little bit guilty of having left them at the time when this was eventually happening, so I have never felt very good about this, but that is what happened. And then started working as a civilian on Navy problems. At point Loma.

DOEL: Right and as you say it was when Roger and Harold Sverdrup had recruited you.

MUNK: Yes and it was very interesting. I think my first acoustics happened at the time. I remember building a microstructure instrument which has since become-- or there was one of the few things when I helped build something. We didn't do very well but it was a good idea it has since become a field that really took many years to build good ones. And then starting to think about acoustic problems, which in a way I didn't really do until very recently until ten years ago but I did...

DOEL: Right, that field begins to develop much more in the recent times...

MUNK: For me.

DOEL: For you particularly, for then. Other applications as you will of course have written occurred earlier, but in terms of oceanic research. I am curious about a number of things that were occurring at the time. Did you know primarily just about the work that you were asked to do by Harold and Roger, or did you come to know the broad sweep of research that was being sponsored by the California War Research Division?

MUNK: No, Roger and Hal did not lay down, oh Martin Johnson was there too, and that is when the dynamigration problem came up when we learned that the acoustics was different in the day than at night and were immediately identified by Martin as being having to do with the movement of copper parts. But I, no, Roger or Harold I don't remember ever having them sort of suggest topics I think I did that pretty much on my own. I remember writing something on the ocean microstructure and its effect on sound which was not published it was part of the... and Roger spending a whole typically five hours at his house going over it with me with great care as he would anything he would do.

DOEL: So he would read for instance a manuscript of yours with you there and you would work through it as you...

MUNK: And fall asleep and wake up and...

DOEL: Did you really?

MUNK: Oh, he did, and I did too probably, yes, yes, yes. Roger would just spend all night treating it if necessary. It was wonderful and annoying too because he would keep on asking things that he thought you had covered, coming back to it. Very good critique. By the time you were through with him your manuscript had been thoroughly chewed up. Now I am trying to think exactly what happened, because I ran into even some clearance problems at that time, and some of them quite stupid, and I don't know whether you want to even take the time hearing it but I remember going dancing in the back country here at Lake Hodges, and all our vans were protected by soldiers, and being asked by a sergeant what I was doing there and I told him that I had come to blow up his dam, and being turned into the then FBI for having threatened to blow up a dam. And that's somewhere on my record, and then being chewed up by the captain who was the commander of the Navy Radio and Sound Laboratory saying I shouldn't, one doesn't say these things in war.

DOEL: One doesn't even joke about these things.

MUNK: One doesn't joke about it, yes he took a-- I remember his name was Captain Hammond and he said well you know it is ridiculous but you just don't do that, you don't, so. Oh yes, I was

out there dancing with one of the daughters of the Scripps family, and I didn't like the fact of being stopped and asked what I was doing, and so I gave suitable answer. And so, and then what happened I forgot, I should be more accurate, but I went and worked in Washington for the Air Forces.

DOEL: Washington DC?

MUNK: DC, in the Pentagon with a man named Seiwel after having been at Point Lomas you see, and Seiwel, that is I think must have been written up, Seiwel was an MIT graduate and had become a major in the air forces and ran something which was under, what was called the Weather Directorate. And he was very ambitious and he wanted to corner the market on meteorology and oceanography both under the Air Forces. And it was a pretty sorry performance because they would put out maps and charts that were often wrong, but he insisted that the time was such that one couldn't really take the time to do it properly.

DOEL: And this was synoptic meteorology that he is dealing with.

MUNK: Yes. And it was then that I learned that we were going to have an invasion of Northwest Africa. Now when did that invasion take place? That was our first allied move where we didn't react to the Nazis but took some initiative. I probably have that in my...

DOEL: We can add that to the transcript later.

MUNK: We can add that. But I learned that we were going to have an amphibious landing, and I think that is an interesting chapter and I really like to think back on it, and I asked something about the wave conditions and couldn't get any information by anybody, they didn't know, and then learned that they were practicing in some beaches I think North Carolina with ships called LCVPs, Landing Craft Vehicle and Personnel, and learned that if waves exceeded six feet the landing craft would broach and would swamp and people were hurt and they would call off exercises. And then from just looking at the literature said well that is an impossible situation because they are going to land in winter, I forgot what month, when the average breakers in that area are higher than six feet, and what is going to happen, it is going to be a catastrophe. And you wouldn't believe it, this had not been asked by the people who were planning it.

DOEL: This is very interesting because it is clear that as you were recalling it that this was a question you were raising and one that military...

MUNK: I raised that question and I'd written the first on it saying we must find a way to predict waves in advance so we could pick a few good days so we could get in without the casualties. And Seiwel who was the commander officer looked at that and said, "Well you know, I can't go to my superiors on that basis I was just nobody, but you get somebody else to come out and read it and see what he thinks?" especially the suggestion that we should be able to do predictions. And I called Harold and said we are doing something of some importance, would you come for a week, and he immediately came and we spent a week going over it, and he reported to Seiwel

that a) that it was a major problem, and b) that the suggestion that one should be able to predict was a good one. So that was an important thing. So we worked together then on, really together on one desk day by day by day, on the prediction problem

DOEL: And when you say we, you mean you and Harold.

MUNK: Harold and me. It was a great honor for me that he was willing to do it. But it was the right idea. And it had never been done for some reason. In fact our whole knowledge of waves at the time if you look back on it is utterly pitiful.

DOEL: It was very empirical to the degree it was being considered.

MUNK: Very empirical, and not even dimensionally wrong, I mean you don't relate height to period. You ought to have gh over u square where u is wind. So we have some dimension as parameters we have learned, and they were not complete and what we eventually wrote which became a publication by the hydrographic office H0601. Has become a bit of history. Other people have written it up not I. Klaus Hasselman has written on it and others.

DOEL: And that is also in the volume.

MUNK: In the volume, yes. So Harold and I worked on that, and it was a very exciting time. By today's standards we didn't even understand what a wave spectrum was, didn't understand-- you know, the concept of a continuum that you talk about variance per unit frequency band was not understood by any oceanographer of the day, and certainly not by me and not by Harold. We dealt with it as if waves could be considered single frequency phenomena whose frequency would change from day to day, and so walked about the period and the height.

DOEL: Which in some sense seems to be a translation from classical mechanics.

MUNK: But you see the funny thing is that at that time the acquisitions understood continuous spectra. Certainly optics people understood that you get the spectrum of a rainbow. And I think that in physics the interesting thing is not-- and the way the first wave spectra were done, which is by the British, is that they would record a wave record on a tape where it was half white and half black, and the boundary between the white and the black was the wave record, and then they would look at it with a slit, and you obviously get more reflectivity from light when you have more white than more black, and they would spin that around the wheel, the wheel was about four feet in diameter, and spin it so that you jack up the frequency and play through an RC filter with the resonance, and then let it slow down by friction, but know what rate at which to turn and so at any given moment you would see the energy density through the band with the red filter and so they were the first to understand that that's the way that you should consider it. But it was amazing, there was none until the British did that that understood that. Still, but if you read Klaus Hasselman's paper, which is very kind, in some sense we did a fairly good job. The empirical relations we used for prediction haven't really changed in all these years. They still are roughly the right numbers. But the step from a discrete to a continuous spectrum was a major

step and happened then subsequent to our work, Harold's and my work.

DOEL: Indeed, it was in the immediate post war years that you began to pursue that. I was very interested in why you feel those kinds of connections, either between the oceanographic and the optical or the acoustics community hadn't been cemented before. When you think back on it...

MUNK: I have often thought about this. I think you know in optics and in acoustics your eye does not see the wiggly p of t or b of t electromagnetic things. It hears and sees the spectrum. In ocean waves you do not see the spectrum, you see z of t against a pile. So in each field you are taking the way of describing it that was more natural to your senses, and especially since it is very hard to build very long time ??? and RC circuits that are resonant. Not until the British learned how to speed up time was it able to be done. And I claimed one step further. Not until John Tukey had translated the equivalent of a Band Pass Filter into a numerical program did the oceanographers and other low frequency people and suddenly the seismologists were worse. I want to be sure of saying that. Understand how you deal with noisy processors. And I am deeply indebted to John Tukey who happened to-- I met him at the time he came out and taught me, and if you recall at the time we did specter by forming the auto correlation and then taking the cosign transform. But that is all right. It was the beginnings of understanding how to deal with it. And it did not come about until the digital methods and the appropriate computing devices began available.

DOEL: I was going to ask about that, and of course that becomes critical in the immediate post war years as well. Was John Tukey at Princeton already when you first came too?

MUNK: Yes, and working at Bell Laboratories. So when we did this work you asked, because we were doing another moving ahead, there was no understanding of the spectra and we simply took the empirical relations and did the best we could. We tried to check this out, you may have read about it, it was kind of amusing. We thought we said we have to do some confirmation that what we are doing is useful. You know, it was a real emotional burden people are going to come in and land and drown if necessary, and furthermore the whole concept at the time was that the Nazis were winning. They were winning in Africa, they are winning here. This was our first move of taking the initiative. So it was a terrible load to worry about. Then we thought about going to Pan American Airways and getting records from the Azores because they had landing airplanes and had kept records and we thought we would practice with those on hind casting.

[end of Tape 3, Side B]

[beginning of Tape 4, Side A]

DOEL: ...of high waves that you completely missed.

MUNK: That we completely missed. And we were very discouraged and worried until we found almost by accident that they were sort of evenly spaced and we found out that they happened on Saturday nights. We made a policy decision that they were related to Portuguese wine and not to

geophysical phenomenology. And otherwise we were encouraged that we were doing a meaningful prediction. And then the invasion came, and as you know we probably picked two good days. I didn't actually pick those days, but the methods were ours. And then the lining came out-- I don't even remember what the causalities were, but very low. Very successful that day.

DOEL: How long did it take you before you knew how successful the prediction had been?

MUNK: Well, the trouble was is just that time that we lost our clearances again, Harold and I. It was utterly foolish because we were party to knowing that the invasion was happening, so to cut us off at that time was very foolish. And it was during Christmas time, I remember, and that was suddenly a low of my life when after having done that work, with no explanation, somebody hadn't said here is the reason—there was none forthcoming. Just one day went to the office and the Colonel said, "I'm very sorry. I have received orders that you no longer can have access. So I will let you know. Just stay home until I call you." And poor Harold. And we know the story now. There's the record down there.

DOEL: And you're pointing to the side of the bed where you're reclining right now.

MUNK: Right. And I have all that junk.

DOEL: We had spoken a little bit about this at lunch time through the Freedom of Information Act.

MUNK: Yes. And we have the records. I was traced, so was Harold. It turns out that they considered Harold and me as a package. And eventually a month and a half later I received a notice that my clearance had been restored with no explanation. And ever since then I have enjoyed a rather high level of clearance. But I never have gotten over that month. It was terrible.

DOEL: Was it limited to that month, or did it reoccur later on?

MUNK: It never reoccurred again. For Harold it did. He did not get totally cleared, and as I mentioned, I think it was partially his bitterness about it, which he never expressed to me, but which I knew—couldn't talk about it is why. It was one of the reasons why he eventually did not want to continue as Scripps director. It was very unfair to him.

DOEL: Given the kinds of commitment that he had made to stay in this country, given his desire early on to have returned to his homeland, clearly this was a major issue for him.

MUNK: Yes, yes. So I didn't know quite what happened. It has been written up in various places about the landings. Then of course eventually I came back because Scripps organized a program for Navy and Air Force officers to get courses in oceanography.

DOEL: I wonder if before we turn to that, you had mentioned that you had begun to read through the material that had come back from the foyer, if you have a better understanding of

what had occurred that had caused your clearance to drop during that month?

MUNK: Well, I think what happened must have been that somebody got a signal that I had been cleared into something and there was some suspicion, and I was I think it was about 40 days I was followed 24 hours a day, I did not realize that. Whether they did a good job or I was totally blind or both, but day by day I got up at so time, I took the bus to there and I went there and I had dinner there and I went out on a date and I didn't come home until 2 a.m., and this and that. And what did it say, Judy, it made some uncomplimentary statement that I was...?

JUDITH: The landlady thought you were messy.

MUNK: The landlady said I was very messy with my room. That's undoubtedly true. That was part of it.

DOEL: That appeared in the FBI report.

MUNK: That is part of it. What else was bad? It ended up by saying that there was no evidence that I was disloyal but that I might have very poor judgment. It didn't explain that either. In Harold's case it was worse. It quoted neighbors here saying that Gudron was making anti-American statements.

JUDITH: You haven't gone through your second file yet so you don't know.

MUNK: Judy is right. I didn't really want to read what it said in the second file and I guess I should some day.

DOEL: But you had mentioned that you had gotten those in the midst of the controversy over the acoustic experiments, you hadn't wanted to...

MUNK: Now that that is over I have no further excuses, and Judy has been pushing that we should go and read it. It is not very interesting reading. Lots of it is blacked out wherever there are names, and it says such things as "he left the house at 7:30 did this and did that," but it lasted day and night for it must have been for...

JUDITH: It says you stole a car.

MUNK: Did I really?

JUDITH: It says there was a man named Munk that was arrested in San Diego and you were him and...

MUNK: Yes, but there was something wrong with that, did they eventually clear me of that?

JUDITH: Eventually.

MUNK: Yes they eventually-- somebody else had stolen.

DOEL: Who actually had the last name of Munk or it was a greater confusion.

MUNK: Yes I guess so, I had kind of forgotten, but if you ever want to, which I hope you don't I will be glad to open those files for you. But it might be more fun to go over it with you than doing it alone or with Judy.

DOEL: Yes, we might want to consider that in a later part of the interview to review that.

MUNK: I thought that someday I ought to do something about Harold Sverdrup. There have been little biographical sketches of which I wrote one and I think they are totally misleading. That should be part of it and could be part of it if we wrote it today. But it would be bitter. Does one want to do that?

DOEL: As we were talking about off tape at lunch it seems that this is part of the broader historical experience, that there were difficult moments at the same time that other events and other kinds of involvement worked much better both for you and for your colleagues, and that indeed these are more difficult times, but they are worth remembering in the same ways that other developments are worth remembering.

MUNK: I think that you are right. And one of these days I would like to rewrite something about Harold.

DOEL: We can come back to that in a later interview. One thing I was curious about was whether these difficulties became public at the time. Were any accusations made that actually circulated in...?

MUNK: No, and I was told not to even discuss it, which didn't help any either.

DOEL: That must have made it even more painful.

MUNK: Made it even more painful. Roger and Ellen were very helpful. Roger knew about it, and I think he is probably one of the ones who helped bring it to an end. I have always been grateful. He said that he spent a day a month running down to Naval Intelligence saying why don't you guys do something about it, and never anything substantial.

DOEL: So you felt that it was a problem that could reoccur more or less through the latter years of World War Two that it hadn't fully been resolved.

MUNK: I have never ran into problems since and I have carried clearances until today and much higher ones than at the time this was.

DOEL: Was this a secret clearance?

MUNK: This was secret, which means more then that it does now. Secret then is probably top secret now, you know, and so on

DOEL: But Q clearance of course hadn't...

MUNK: Q is of course atomic energy, and since in the last two years I have been involved in some intelligence work I have had all sorts of clearances so it has never happened again, I have always mentioned it in filling out my forms, so I am curious that at least that has been ignored by the people who worry about my present clearances.

DOEL: Yes, but this is indeed part of the broader tapestry of the experience of quite a few Americans.

MUNK: Have you heard about this before in other cases, about people like going through this phase?

DOEL: There are others that clearly did, and it certainly comes to be a major problem during the McCarthy era involving quite a few scientists in a number of fields. And so it is not indeed an issue that ends in World War II. We may want to address that again as the McCarthy era and the way it affected University of California.

MUNK: Yes, loyalty of.

DOEL: Yes. Because I think that is another critical set of issues. I was curious if you recall any occasions where colleagues, that perhaps you found out subsequently, were asked about your credentials or your loyalty. Were people at Caltech for instance asked about...?

MUNK: I don't know that, but of course it is very standard. I probably not so much the last year but hardly have a week go by without somebody coming to check the clearances of people, even today. And so I am quite sure any kind of investigation involved asking your colleagues whether they have any reasons to believe that you are not loyal. I don't recall any names. I always would give the same references. Roger was one and Giff Ewing was one and Columbus Islin was one who I don't know. So I am sure that was very common.

DOEL: I am curious if people like Gutenberg or even Millican were references.

MUNK: I am not aware of that.

DOEL: I see.

MUNK: It was probably more likely to be my Scripps connections by that time.

DOEL: And you mentioned one individual who is critical for oceanography before...

MUNK: Seiwel.

DOEL: Well also Columbus.

MUNK: Oh Columbus Islin?

DOEL: When did you first come to meet him?

MUNK: Then, now let me think a little. I spent one winter in Woods Hall in the immediate post war years working on building a wave instrument, again related to the amphibious problems and that is when I got to know both Maurice Ewing and Columbus Islin.

DOEL: And this is after the war was ended?

MUNK: What year was I...? It was the year when the SOFAR Channel was discovered and again we could reap...

DOEL: The SOFAR Channel was actually a little bit before the war ends, so this may be about '44.

MUNK: Oh this must have been when I went, and spent an absolutely six to eight months winter in Woods Hall, which I have always felt very romantic about and it was terrific.

DOEL: I really want... I'm sorry. I was real curious about how you viewed the differences between Scripps which you had come to know and Woods Hall, which in some sense was it's closest...

MUNK: I liked... they are very, very different, they were more so different then than now. And I guess I was invited by Columbus to go out there and Harold Sverdrup. I must have by then returned here because we were teaching those classes to the officers of the Air Force and Navy, and then took some time off to go to Woods Hall.

DOEL: And these were classes in?

MUNK: Oh in oceanography with emphasis on marine predictions, particularly waves.

DOEL: After the empirical relations had been worked out you were teaching the methods to...

MUNK: That is correct.

DOEL: How large were the classes?

MUNK: Oh gosh, we must have trained 100 people before we got through. And many of the subsequent oceanographers came out of these classes. A whole list of them of people who became important. You could probably reproduce some of those through John Knauss and Art Maxwell and so on. They were-- you know there hadn't been any classes on any ocean aspects

any place until these were given. And Harold Sverdrup and Dick Fleming and I did the teaching, mostly Sverdrup and I. You see the little book about oceanography for meteorologist that is about fifth next to the C, go left, and isn't that...

DOEL: It is indeed.

MUNK: That was a short version of the big book which is here, which was written because of those classes.

DOEL: It is very interesting. We are holding, and it is by Sverdrup of course, Oceanography for Meteorologists, and I am holding a copy that is inscribed to you with best wishes from H.U. Sverdrup, and dated April 22, 1942. And indeed it was published in that same year, so the courses were beginning then very, very early on.

MUNK: Yes and it probably speaks about the courses.

DOEL: There is a preface in here which does mention them.

MUNK: You see he had written that book. The book itself was delayed for security reasons, and it is a completely maddening story because it said that the enemy would find enormous advantages in being able to read this book, which of course is a little bit hard to justify, but I don't know.

DOEL: So, it was actually kept classified during the time that the courses were offered in the...

MUNK: Yes, it was then classified. And it must have been, yeah, and I must reproduce the year, we can go over that. I went to Woods Hall for the winter and it was very different and it was a wonderful year, and I have never quite gotten over how wonderful it would have been to spend one's life at Woods Hall, just as it is wonderful to spend it at La Jolla.

DOEL: We will need to talk about that particularly as we get closer to the late 1950s when that offer had seen much closer to you than at other times.

MUNK: I had several offers to go, and at one time Judy and I even picked a house in Woods Hall.

DOEL: Is that right, I hadn't realized that.

MUNK: We go back now and look at the house and feel a lit bit sorry that we haven't been able to live two lives instead of one.

DOEL: Did you find that there was a difference also in the research style, the way that one did work at Woods Hall compared to at Scripps? Did you notice other differences at that time?

MUNK: It was very different. First of all there was Columbus, who was such a wonderful

character. He would go every day through all the labs, but he didn't want anybody to talk to him, and he would ask pointed questions and wanted a quick answer. He told me once that he had never sent a letter that he didn't write out in longhand, so whenever he did answer something it was almost a work of art. He considered it to be. It was a wonderful principle, isn't it, not to go and ever do anything unless you can do it to a high perfection. And I got to know him that winter. For some reason that I never understood but other people have spoken about he was very distant to most of his colleagues, very quiet, had no friends. He would take the boat in the evening to Martha's Vineyard where he lived. He probably found it more acceptable to invite someone who was not from Woods Hall, and so I spent two nights there, twice a night there he invited me to come, which I hear was very, very rare, and listen to him and his wife talk about their experiences. They both enjoyed drinking and it was always sort of wet, but very exciting evenings, and I never quite really forgot the beauty of those evenings. It was a great honor for me to be invited. And he was a wonderful man I thought. Very intuitive, and to me very warm, and I talk about that experience during that Woods Hall winter. It was a wonderful winter. We worked at Gay Head on Martha's Vineyard, put some instruments down. I worked with a man named George Clarke, who was a member of the Woods Hall staff, a Harvard biologist, and I got to know Maurice Ewing, who had in fact designed the basic wave instrument that we were using.

DOEL: What were your impressions of Ewing? What sort of a person did he seem to you?

MUNK: I never liked him. It may have been partly that he was going out with a girl that he later married named Midget Kidder, and I got to know Midget Kidder before she was married and took her out sometimes, and that may not have been very helpful. But he was a fantastic character. I remember enjoying, I saw him at his home a number of times and he always would--there was always something so dramatic happening. When you were next to him you felt that the latest development in the world was taking place that evening at his house.

DOEL: You are saying this is how he would project events that he had been involved in?

MUNK: In a sort of understated way. Now I remember at that time when I was in Woods Hall he was getting interested in underwater photography, and he always had some interesting young Navy officers around who snooped around to see whether something was happening, but that was part of the fun. And he had some difficulty getting money to build his underwater apparatus when he had that incredible luck once of taking a picture of an underwater wreck and catching the name of the boat, and identification of wrecks was a real problem, and so he said well he would pull that out of his pocket, say here is one picture that I happened to take, and it had the name of USS so and so on it. But he always had some very dramatic things happening, and of course the discovery of the SOFAR Channel was an enormous event, the fact that you could hear large distances.

DOEL: Do you remember talking to him about that work as it was going on?

MUNK: Yes.

DOEL: Do you recall any of those discussions in particular?

MUNK: Well, I know of course his literature well from then I have often-- though he did the obvious thing, though there is a minimum of sound speed should have been obvious to even the most simplistic oceanographer because you know that sound speed increases with temperature and sound speed increases with pressure, and we knew enough about the oceans so he calculated that. We also know that in any physical subject the minimum in phase velocity is a wave guide. Now there were some-- the subject is not without controversy. There was a man at Harvard who had apparently written on that, it could be reconstructed, and there was some attack that Ewing had taken his idea without credit, that we could go back to, and somehow or another that other man was involved with one of Maurice's previous wives.

DOEL: I am wondering if you are thinking of Eldon Leet [?] he was the seismologist at Harvard.

MUNK: That must be it. And didn't Leet in fact predict that there would have to be an underwater sound channel? I think there is a literature on that that I don't quite remember but there was something of that sort. But Maurice did the right thing. He took two boats and he had one of them drop dynamite and another one he lowered his sideophone [?], and he gradually moved away until they were 2,000 or something kilometers apart. A beautiful experiment, simple.

DOEL: Did he strike you as a good experimentalist?

MUNK: Oh, superb, except in a funny kind of way. Always very impromptu, you know he would not get his equipment ready beforehand, but then he would go aboard and he would work all night doing his own welding and this and that to make it work, and he very demanding on himself, never slept. And so his experiments always had that great drama in them because they weren't prepared. I have some sympathy for that, even though I think it is not the way to do it.

DOEL: I gather that was a different style from what was practiced generally at Woods Hall, and at Scripps for that matter.

MUNK: I don't know. Anyway the person, probably the most important person at Woods Hall. Columbus once told me that there was a little friction between Columbus and Maurice. Maurice really felt he was the most important contributor to things at Woods Hall and he probably was. I don't quite remember, but he was a difficult man too, and yet he had great loyalty of people who worked with him and that was before the Lamont days of course.

DOEL: Indeed. Joe Worzel would have been part of the group. Were there others that you remember meeting? Did you meet Worzel when you were there?

MUNK: Yes, and then there was what's his name that went to Washington, acquisition whom we know well who since has died worked with the Navy all his life. There was a whole group of Ewingites and they swore by him. He certainly evoked great loyalty in people who worked with

him. But he was selfish. I remember it must have been at about the time when the underwater gravamitist that had been built by Baning Miners [?] and who were unique at the time for being able to measure gravity in submarines.

DOEL: Yes, it was a critical development.

MUNK: Baning Miners decided that he would stop doing it partly because he was so tall and the submarines were so small, and he offered three of them to people best able to use it, and there was a meeting at Scripps here, and present were Louis Lecter, Revelle, and Maurice Ewing.

DOEL: And this is probably right after the War?

MUNK: And that is right after the War. I can't place that. And the subject was as to how they should be distributed. And I for some reason Roger had invited me to listen in, and Maurice opened the meeting by saying that it would be interested of the United States if all three of them went to him, because he had more experience in using them than any of the other people. It is true! And I remember him saying that when he finds that he had submarine time, that no matter what his plans were, whether he was doing this, no matter what he would pack up and go on a moment's notice. That was his first priority and on that basis he feels that the best, his commitment to it, and maybe he was right, you see. It was an unpleasant meeting. It ended up with Ewing getting two out of the three. I wish somebody for historical purposes could get this more accurate. I am a little afraid talking out of memory. But that was the general feeling.

DOEL: But clearly it is a strong memory with you what had gone on at that meeting. Do you remember any other discussion among the other principles who were there of the Baning Miners?

MUNK: That was the one discussion that I remember. And as I mentioned I do remember Ewing saying that he should have all three of them, and making a good case. He really was committed to underwater gravity work, and he was a very good man at getting data; he demonstrated that again and again.

DOEL: And as you say that is one of the areas that becomes a part of Lamont Geological Observatory in the 1950s. The gravity work did continue quite strongly there. Were there others that you met at Woods Hall that were quite...?

MUNK: Oh I met a whole bunch of very romantic people. You know, Columbus had gathered, he enjoyed asking people who had not gone through the usual academic mill. There was a whole bunch of people starting with the captain Lambert Knight and others working under very difficult circumstances in a very cold winter, and all I can say to you is that everybody was devoted. We would go to Falmouth for lunch and drink some Martinis. I can't do that today and work all afternoon. And there were some wonderful girls working at Woods Hall at the time I remember. It was just very, very exciting. The work at Gay Head went quite well. I pulled an almost unbelievable boner for which Columbus gave me hell. There was a man there in navy uniform named Otis Barton who at one time held the record of deep diving, hard diving, and we lost the

wave instrument off Gay Head somehow or another in a reasonable depth of water, like 100 feet or so, and he heard about it, and he said, "Ah, that is a job for a diver. I will find it for you and bring it up." And we said oh that is great and we had a DUKW, "duck" there to work with us, you know, that was an amphibious vehicle, and we noticed with pleasure that it had an air supply that was used to blow up tires. While you were driving you could change the pressure in the tires, it was one of the reasons-- because you wanted to have soft tires in the soft sand and so you could change it. And we said to Otis Barton, we being two or three of us, that is wonderful we don't have to get air we already have a compressor aboard the DUKW, and so we hooked him up to that, and he went down looking for it. And he came up very soon, he spoke of himself in the third person always.

[end of Tape 3, Side A]

[beginning of Tape 3, Side B]

MUNK: And he came up and said the diver doesn't feel very well. But then he said the diver will try again, he went down he came up much sooner he said the diver feels terrible. Well we came home and told Columbus that, and Columbus turned furious and said don't you know the compressor is taking in gasoline fumes, it is right next to, you have almost killed that poor bastard. We eventually grappled for it and found it and did some...

DOEL: So Otis Barton didn't try to dive another time?

MUNK: He didn't try to dive with us another time. He kept on diving actually. But it was a wonderful winter. We got some work done. We tested the effect of oil on waves, an ancient subject which certainly nobody understood then and don't necessarily understand now, and it was very exciting.

DOEL: Did you do a lot of travel during that time? Earlier you mentioned you had been to Washington but when you were at Woods Hall did you more or less stay?

MUNK: We more or less stayed. One thing that is interesting is that Columbus came by one day and said come to Boston with me to see, what was Bigelow's first name, Henry?

DOEL: Henry.

MUNK: To see Henry. Henry had been his teacher.

DOEL: Yes, that is right.

MUNK: He said I am trying to persuade Henry, who is depressed because he is useless in the war effort, I am trying to talk him to writing a book on waves, and why don't you come and help me by telling Henry that that would be a useful thing, and the problem was that Henry would say that he doesn't know anything about it, but he was such a good natural scientist and such a good

observer that Columbus thought, and I agreed, that it would be a useful book. And so we took the train up I remember and took a taxi over to Henry, whom I had not met, and talked about the book, went back to Woods Hall the same evening. And eventually Bigelow and Edmondson, Tom Edmondson wrote a book on waves, I think a good book which I think was a result of that at least a beginning during that meeting.

DOEL: That is very interesting. What were your impressions of Bigelow?

MUNK: Bigelow? I don't really remember other than I thought it was a wonderful visit. I admired him but I especially enjoyed being with Columbus Islin. I thought it was a great honor that he would take me along.

DOEL: That must have given you an extended chance to talk with him again on the trip up.

MUNK: I never think... I think I never saw Bigelow again. I got to know Alfred Redfield a little which later on turned into a real friendship. Judy and I became quite close to the Redfields. We once spent a summer living in their house, they have often been here, and we got to be good friends.

DOEL: I was curious in the time that you were at Woods Hall go to know any of the other geophysicists or scientists at MIT or at Harvard in those years?

MUNK: I met the people who were talking about... Of course Alfyn, Bill Von Arx, Fritz Fuglister. If you came up with names at the time I probably met most of them then and remain, and also what is the name of the biological acquisition from Harvard, who is such a wonderful man who started really the field of listening to marine mammals. And yes I met that group of people.

DOEL: What were your impressions particularly of Alfyn of someone whose career stayed...

MUNK: He and John Isaacs, oh they were the people who came up with mad ideas and always interesting and so I enjoyed seeing him a lot and we kept our relations with him going all the time. Bill Van Arx, sort of an inventor, a little bit less outrageous than Alfyn but almost equally.

DOEL: What sort of thing qualified a an outrageous suggestion or idea from Alfyn?

MUNK: Well of course eventually the *Alvyn*. But he always had a, oh gosh it is hard to give you examples, but he always looked at things in a very different way which is fine, and he did it in a funny kind of a dramatic way, oh well you know, "I just thought of this and that," you knew damn well he had been stewing about it, or it just occurred to me two minutes ago that... Bill Van Arx and I worked together a number of times of sequined. So it was an important winter, I formed almost as many friendships there. Henry Stommel was not there then I met him really, I probably had met him but he was not at Woods Hall I think that winter. When did he come to Woods Hall first? I think it may have been with Rossby. You see he was working with Rossby

in Chicago.

DOEL: He was part of the teaching effort I believe at the time. It is clear that you and Hank Stommel become much closer in the years immediately after the War professionally. I did want to ask you some things about the plans that you and he were working on in those years as we get to that. It seems to that part of the work that you were doing at Woods Hall involved simply doing more wave measurements.

MUNK: Yes, it was a wave oriented project. The idea of building an instrument that had what they called a slow leak, so you referred the pressure to some mean pressure outside had been Maurice Ewing's. He had done the original design. We used that to get records of pressure against time on the sea bottom, which is probably still the best way of doing some of the wave work. And learned a lot. I do not recall whether any of those records were subject to the beginnings of harmonic spectral analysis, but I think not yet; I think that came a little later.

DOEL: One of the things that I found interesting in looking at one of the papers that subsequently came out from that work, the 1946 paper that you had done with Harold Sverdrup and yourself, Theoretical and Empirical Relations in Forecasting Breakers and Surf, was this delayed in publication because of the...

MUNK: Yes, yes, yes.

DOEL: That is what I had thought. You made an interesting comment. Near the end that the assumptions underlying the classical equations are never completely fulfilled in nature and also mentioned that classical hydro dynamics, a field which seems to have fallen into disrepute during the last few decades.

MUNK: Where did that statement appear?

DOEL: In the 1946 volume of Theoretical and Empirical Relations in Forecasting. I was wondering if that was your line or Hall's, and whether that had particular significance to you.

MUNK: I don't remember that. It was incredible how poor the literature was of that subject.

DOEL: I can imagine. I just thought to show you here this is what I was... I was just commenting right here on the conclusion, which mentions that classical hydro dynamics had fallen into dispute in the last few decades.

MUNK: Well, you know the relations between sea floor pressure and surface height is of course classical. You know I had forgotten that statement. It is almost having to justify using hydro dynamics. I mean we used certainly the classical dispersion equation, which relates period length velocity.

DOEL: I thought it was interesting too, that it got that kind of emphasis that you felt you needed to say that.

MUNK: To say that, yes. Funny, I'd forgotten that. It must have been basically, you know, that you can calculate for linear waves knowing either the wavelength you can compute the period and the velocity.

DOEL: One of the other interesting things mentioned in the paper was that you were doing some experimental work, the wave tank as I recall, that was coming up. I am wondering how important that experimental technique seemed to you at the time?

MUNK: When you look at that paper for the prediction of height and period, you find that some of the data had been taken in tanks. What is was is to relate the wind speed, the length of fetch, and the duration of the wind to the height and period of the waves. This is the waves in the storm area, then comes the problem of how they decay in a region of relatively low winds, so called traveling a swell, and finally the transformation as it goes into shallow water, and we divided the subject in this way and tried to...

DOEL: I am sorry I didn't mean to...

MUNK: No I had nothing more to add. I am still thinking about that sentence. I guess it just meant that the people who are working on the waves at the time had stopped using the classic relations which are still an enormously powerful first approximation. And I am only surprised that we felt it needed to be said.

DOEL: It is interesting word "disrepute," as if there potentially a point at which almost consciously a decision had been made to not use the classical.

MUNK: But of course it is not a theory by today's standards. It is a set of using empirical relations guided by certain hydrodynamics, and a theory of waves and wave predication that really doesn't come until much, much, much later.

DOEL: Right. No, indeed and it is clear that these were steps that in part were greatly stimulated by the war time needs.

MUNK: Very much so.

DOEL: What I was also interested in though in thinking about that was that Rossby at Chicago was quite interested in using experimental techniques in order to study global circulation when he did the rotating dish experiment.

MUNK: It was Fultz.

DOEL: Dave Fultz was the one who actually followed through with that. I was wondering if that was something that you and Rossby had spoken about during that time that you and he were together at Scripps?

MUNK: I don't think so. He was then working on this wonderfully simple relation of planetary waves that has been the basis of all dynamics ever since that anybody could have done, but he had the foresight to do it.

DOEL: Had you met Dave Fultz?

MUNK: I have met Dave Fultz I don't know him well. And Von Arx was doing experiments too.

DOEL: I am curious in a general way whether Rossby had ever spoken to you about the value of using experimental techniques about them as opposed to simply making new observations, building networks to actually study in a broader way phenomena that were naturally occurring.

MUNK: Yes, I thought that by far his major effort was using the classical meteorologic observations as represented by weather maps to think about meteorological dynamics. Of course I do remember that the jet stream was discovered at about that time. Did that involve a radically new method of observation or just that people were able to go higher and higher?

DOEL: More, as I recall, more the latter.

MUNK: More the latter. And then he was very, very interested he was working with Palmaine [?] whom we met at the time, and of course most of the angle momentum of the atmosphere is associated with westerly jet and that is where I first became interested in Earth rotation is just that problem.

DOEL: That comes of course a little bit later in this period of time. I wonder if this is an appropriate time to address the influence that Carl Eckhart had on you. Clearly you were coming, when you would come back from your wartime service you would come to know him better.

MUNK: I got to know him quite well and we were friends. He was very difficult because he had a different standard. He was truly a theorist, and he did some fantastic work. I thought that as a whole, you know, he had the idea, he realized that the ocean community did not include physicists of the kind of depth and experience that he was. He wouldn't say, that that is very immodest, but he was in a different class of certainly theoretical physics than anybody, certainly including Rossby or Sverdrup or any of those people. And he really thought that a person of his background should be able to revolutionize the subject, because the people in the subject didn't know nearly as much physics as he did. And I think he failed in revolutionizing oceanography. He wrote a book that you may be aware of, the book on Ocean Hydrodynamics, I have it some place. He was very elegant; in his lectures were unbelievably elegant. His book was elegant. In writing his book he decided that the standard way that had been followed ever since the Beackmas [?] day, physikalisches hydrodynamik, with the independent variables being u , v , w , ω , p or whatever. That that wasn't the best way of doing it and that you could get more simple looking equations otherwise. So he invented his own notation in the book, probably a better one

than has been standard, but the result was that very few people took the effort of learning a new language. It is like having to learn French to read something. And the book had as a goal combining two effects which are not part of classical hydrodynamics, as you find it in land. And the two things are rotation and stratification. You know they are the way that land hydrodynamics differs from Beackmas physikalisches hydrodynamik. It is rotation and stratification. Land is almost entirely homogenous non stratified water, and almost entirely neglects the fact that it is in a rotating effort. And now Harold wanted to give a systematic treatment of that, not so very different from what Beackmus thought, and he probably knew more than other people. And he allowed for compressibility. And it became-- I liked the book, I used it, but that is because I knew him, but I don't think it had a profound influence.

DOEL: It part as you mentioned because...

MUNK: Partly because of the rotation problem.

DOEL: The rotation. I was wondering had he discussed that with others before he actually employed it? Was there a discussion about his using that?

MUNK: He used lectures, and then the lectures became the basis of the book.

DOEL: Did he use that notation in his lectures?

MUNK: Yes.

DOEL: So all of you in essence learned it as students.

MUNK: Yes, yes, and he was inspiringly elegant.

DOEL: Was he one who would think through his lectures well before he presented them?

MUNK: Yes, yes, including where you write an equation on the blackboard so you don't have to wipe it out. Yes, yes, there was thoroughly-- He had beautiful handwriting, and I claim that he is the only one I know where a subsequent publication made the papers less readable than it was in his original handwritten note. Because his handwritten notes, the placement of the equations and everything, was all part of his way of presenting a subject.

DOEL: You could see the structure of the logic.

MUNK: Right. And I do have some of his handwritten lecture notes at the office. I should be careful that somebody gets those, they are a work of art.

DOEL: Yes, historically they are very valuable.

MUNK: I should ask Debra about that. And I did go to his lectures. I learned a lot, but I feel

closer to Rossby's standard than I do to Eckhart as far as my own style, way of thinking is concerned.

DOEL: I was wondering whether you felt that Eckhart had that kind of physical intuition that you said you found interesting and helpful in someone like Rossby.

MUNK: I think he had very little intuition. He had enough intuition to choose interesting subjects. I mean he has written a few things he came very close to getting the Nobel Prize. Did you know that he on the way, the equivalence thing he'd order paper that was published a week later than another paper that.. so he was really a first rate physicist. But we would define, we would go to very great care of defining his quantities, and then he would manipulate them correctly to see what they inferred, and he did great work with that. But it is a different way; it was not guided by data or by some kind of an intuition it was a very deductive and deliberate approach.

DOEL: Come very clear. Did he take part actively in the colloquious series?

MUNK: I think so and I was always scared of him when I spoke because he was very critical. I did once talk in his presence, gave a talk on the rotation of the Earth, and there is a question of how you describe the earth because you can't just call it rigid in this kind of problem, it is deformable, it flows plastically, and I used a Calvin solid, one that behaves like a solid at high frequency and like a fluid at very low frequency. And he challenged that and said that it was in the lecture thermodynamically unsound. Which I am sure it is, and I said gee everybody from Lord Calvin on who has talked about this subject has used this as a basis, and I remember Carl saying that is no excuse at all.

DOEL: But that would strike terror into the heart of a Ph.D. candidate I would think.

MUNK: Yes.

DOEL: It reminds me of some of the other work being done at roughly that same period of time, the question of how to treat the outer regions of the solid Earth. M. King Hubbert was working on that question of deformity.

MUNK: Yes, well he was more interstitial fluid oriented and I think Carl actually had great regard for King Hubbert, I seem to remember, but I am sure he would have, Carl Eckhart would have taken a dim view of Lord Calvin because Calvin really was very intuitive and did fantastic things.

DOEL: That is interesting, that is very interesting that intuition could be problematic, that it might steer one from deductions from first ...

MUNK: But you need all sort of people, isn't it, and I am sure I have learned an awful lot from him even if it wasn't style, and his lectures were a pure joy of elegance, and certainly his quantum mechanical way of discussing wave guides and things, which are now really accepted

by everybody in geophysics, I learned much earlier than I would have otherwise of how you find a way of presenting these dispersion relations and what they infer.

DOEL: That is a very interesting point. When you were learning about Harold Jeffries and others who were involved in the structure of the Earth, was this something that came principally from Gutenberg or was it also from Carl Eckhart's records.

MUNK: No, Jeffries was Harold Jeffries. Harold Jeffries is an elegant writer, and I'd read his book on the Earth. I met him very soon after our first trip to England. He in fact, believe it or not, Judy will tell you about it, had been a botanist, you probably don't know that. He was originally a reader in botany and my Mother was a student of his.

DOEL: Is that right, very interesting.

MUNK: And when we went to England the first time and Mother came along and we asked Harold for tea, he came by and he looked at my Mother and he called her by her maiden name, he had remembered that. So she had read botany under him. So remarkable that a botanist would learn so much physics and mathematics that he could do...

DOEL: And a remarkable personal memory as well.

MUNK: That was a remarkable personal memory, wasn't it. And we got to know him quite well and his wife, Lady Bertha, Lady Jeffries I mean, and I gave the Harold Jeffries lecture at the Royal Tsunamis Society some years ago, and Bertha was very concerned that only the very best be said about her former husband. I got letters from her briefing me as to what to say starting six months before the talk.

DOEL: Interesting. Was it difficult to follow her?

MUNK: I don't know it was no problem, she is a very nice woman. We dedicated, Gordon McDonald and I dedicated our book to Harold Jeffries.

DOEL: Dedicated your book to Harold Jeffries, and we will almost certainly get to that today, but I very much want to talk to that as we get to them. I was just trying to make sure I had the chronology right. Wasn't it 1955 or so that you had gone to Cambridge for the first...?

MUNK: I'll have to look that up; I have been there three times.

DOEL: I don't think you had gone though prior to the time-- it was after Oslo and after the Guggenheim.

MUNK: Yes, I went to Oslo for the first time, I was married to Martha, and our marriage fell apart and then after Judy and I married, which is now 43 years ago so that is '97... '55 isn't it. Judy, when did we marry? You never remember either.

DOEL: 1953 I believe.

MUNK: I got that wrong. Judy we have been married 44 years.

[end of Tape 4, Side B]

[beginning of Tape 5, Side A]

DOEL: You will know better. I simply seem to remember that date from something that I have done.

MUNK: You have to transcribe all that stuff?

DOEL: Actually someone on staff does the transcription on the interviews.

MUNK: I am sorry for them. What is perhaps worthy of saying after coming back to Scripps after the War and working on the waves and then some other things happened, is that I was too lazy to get a Ph.D. thesis written, and Harold Sverdrup kept on saying you had better get your thesis written, and then one day he read me the riot act. He said unless you come up with it in a certain time you will have to leave here.

DOEL: This was in 1947 that he had said that, so about two years...

MUNK: Something like that. And then I wrote, finished a thesis it is the shortest thesis of Scripps records, I think it is 17 pages, and I wrote in probably less than a month and it shows, but for some reason or another the Committee accepted it. And it has a fatal flaw in it also which people didn't realize, and I finally got my degree, but it took some real nudging by Harold to get me to do that.

DOEL: It wasn't uncommon for geophysicists in that time, particularly in the hurly-burly of the years after the War, to not get their dissertations done quickly: there would be exhibitions were ongoing, the military projects were expanding. Do you feel that it was that you didn't want to do a thesis or were there so many other possibilities that you had for research?

MUNK: It isn't the greatest fun is it writing theses, is it. It is more fun to do something new and it takes a degree of self discipline.

DOEL: Was there a particular style that Harold Sverdrup wanted you as a Ph.D. student to follow when actually doing a Ph.D. product?

MUNK: Well, it was understood that unless you were as bright as Rossby, which nobody was, that you have to get your union cards if you wanted to stay, and he did the same things as I do with our students.

DOEL: Surely. I just meant in terms of the kind of problem that he felt was suitable for a

dissertation versus an area of research?

MUNK: Oh, he would have accepted any number of things I had already done for a thesis, and I just didn't want to prepackage something; I could have done that. What is interesting about working with him which I haven't mentioned is that although he was a Norwegian he was an excellent editor, and I remember the first few times when I wrote a paper he would really have me go back and rewrite them sentence by sentence, and he did it extremely well, you know, you would have a sentence and he would say what are you really trying to say in that sentence? And then you'd say, and he'd say well why don't you say so. And you must know that so well. I mean attempting to do that with students. But he really taught me how to write scientific papers, he taught me how to think about it, and he was very patient about it.

DOEL: I gather this was something that all those around him at Scripps were in some sense encouraged to do. Roger Revelle, did he get the same kind of influence?

MUNK: I don't know, he was not Roger's-- in my case he was head of the thesis committee, I think Roger's head of the thesis committee must have been T. Whalen Warn [?].

DOEL: I believe that is correct.

MUNK: So, I think Harold Sverdrup had less to do with Roger's thesis than he did with my work.

DOEL: You had mentioned in one publication that you credit Harold Sverdrup with two things, one being careful attention to writing, and the other being very careful and responsible use of recording data. I was curious what you were thinking about when you were writing, and you also make a very interesting observation that the kind of scientific practice that you were involved with at that time regarding data presentation has been lost in computer analysis and plotting.

MUNK: Yes.

DOEL: And I am very interested in what you feel has been lost in more recent times.

MUNK: I have a book by Friedrief Van Yansen [?] here somewhere about his work in the Norwegian Sea where I remember there is two pages is spent on a single point in a plot where Yansen makes an argument that for this and that reason we should ignore that point, and they are good reasons. And that of course is lost nowadays. But in general I used to work with Harold. I helped him with plotting by the way when he wrote his book. You know, one person would read and one would plot and we would take turns. And so each individual point as you made a plot you were hoping in some sense that it would continue some kind of pattern that you had seen, and if it fell on that pattern you had some delight, and if it did not you generally were hoping there was some good reason for it. Well, that of course is lost. You generally now get a plotting and the time it takes to plot it is negligible if it is a computer plot. And we were just, the pleasure of taking some data and then seeing how it develops when you plot it is almost gone,

and I was talking about that. And I suppose it is irreversible, and you take three orders of magnitude more data it is probably more important than the terrible care you take with each point. There are still people who practice the old fashioned care. Joe Reed at Scripps is one of those. But I do very much recall making plots when we didn't know how they would come out, and when the excitement of seeing what kind of relations you get would be sort of spread over half an afternoon.

DOEL: And you saw it literally unfolding as you continued the work.

MUNK: Unfolding, exactly.

DOEL: When do you see the transition occurring between that style of research and the more modern, when did it strike you as being quite apparent?

MUNK: Gosh, it would certainly have to be applied to in connection with technology, isn't it. At what time did we start taking readings and plotting them individually on graph paper? I don't know, I would have to...

DOEL: We can visit that later.

MUNK: I think we could probably go back and look at that in terms of-- I could look at my own papers and things. I can't answer you that off the cuff. But I hardly ever now take a graph paper and plot things up. A little bit, but very seldom.

DOEL: It has clearly influenced your own research as well, the availability of the new techniques.

MUNK: Oh yes. Though I am not a pioneer in computer use, it certainly totally changed the way you do things.

DOEL: There are points we need to cover simply in terms of computers and recording instruments and the way in which data were available for use during the post war years.

MUNK: Yes, I mean I grew up in the pen and ink stage, eslo and angus [?] which had an arm so that it would have a curvilinear kind of response.

DOEL: Right, you were holding your arm as the stiff instrument of the pivot, yes indeed. I think given that we are almost to the end of the two hours that we had promised to give ourselves for continuing this afternoon, we may want to talk a little about the origins of the work you had done at Bikini Atoll, or we may if there are other things that you feel that you want to cover that we haven't so far in your earlier career, this would be a good opportunity to.

MUNK: Well at the moment at least I'd rather respond to questions you have, and I don't have a feeling of anything that we have... I have sort of interrupted you when something came up anyhow.

DOEL: Perhaps what we should do and I am realizing this at the moment buried under a few pages where I am sitting here. The thesis that you did, it actually turned out to be published in two discreet articles as I understand. The one in '47 and the one in 1948, the latter one, the one that appeared in the bulletin of the Seismological Society of America, the first one Increase in the Period of Waves Traveling over Large Distances in the Transactions of the Aging...

MUNK: Well I really should clarify the confession that was made, and probably not made very well, the first one really confused two entirely separate phenomenologies. You can get an increase in the period of waves simply because the high frequencies are more quickly attenuated than the low frequencies. That is you have a broad noisy broad band things. You can also get an increase in the period of individual identified crests because of dispersion. I confused the two, they are two separate phenomenologies in my thesis, and discussed them both together. They are very, very separate. The person who actually criticized it was Maurice Ewing. And I must, I would be interested in whether in my second paper I give credit to Maurice, I hope I did.

DOEL: A note of period increase of waves. You point to another paper. I don't yet see in looking at actual citations.

MUNK: Does it mention? No citation of Maurice's. I know he was the one who pointed out that those are two separate problems, and if I didn't do that it wasn't correct.

DOEL: You did use the... What you mention here, and I am not sure whether I am not following it, you mention that you wanted to use the opportunity to point out an error in the previous paper, and the previous paper as I believe it is cited is the first one that you refer to, although at that moment in the paper you don't go on to address where it was that that insight had come from. And you feel now when you look back on it that it was Maurice who had...?

MUNK: Yes, I do remember his pointing that out, and he understood that problem very well.

DOEL: What was the forum that he had pointed that out to you in? Was it an individual meeting do you recall?

MUNK: I think probably in a personal meeting or something. I do not remember how he pointed that out. There was no-- I don't think there was a written letter to the editor or something like that. I am sure there wasn't. It was rather nice. And I didn't understand the problem, and I wrote my thesis in such a damn hurry because I wanted to get it out of the way that it wasn't properly viewed. And my committee consisting of Bjercknes, Sverdrup, Holmbul [?], the same as Charny had.

DOEL: You had mentioned that. That is rather interesting, yes.

MUNK: None of them knew enough about it either so I got away with it. And I should tell you many years later I received a call from the UCLA graduate office that they wanted me to call them, and I said oh-oh, they are going to withdraw my degree because they found out that there

was an error. Fortunately there is no known technique for a university to withdraw your degree once granted.

DOEL: Had you really worried at the time that you heard that they were calling back that...?

MUNK: No, it is more of a joke, and instead they elected me as an honored alumnus of UCLA.

DOEL: Indeed. I remember the occasion that you had written about. But, you had mentioned and it was one of the other aspects of the work here that what you were working with is phenomena from three different realms of geophysics, the tsunamis from the elution earthquake, and the forerunner of the swells recorded in England, and then the Montana earthquakes and other shocks. And one of the points that I sensed you were making through the paper was of finding ways of generalizing treatments that would range between the solid and the fluid portions of the Earth.

MUNK: You see, the tsunamis fluid, but in a tsunamis the time interval between the first and the second crest, and you can— it is a classical problem. It is discussed in Lamb under the wording ??? ??? problem. That time interval increases with distance as x to the one-third power in a shallow water environment. That is very different from having generated a broad band signal which in some statistical way has lots of zero crossings originally, but as you attenuate all the high frequencies becomes more narrow band and more low frequency. Those are unrelated, and they can both be in water, or both be... Examples for those exist, both of these things happen in the solid earth, both of them happen in the fluid earth. SO it is not a solid versus fluid earth distinction; it is a distinction between two phenomenologies.

DOEL: Yes, and that puts it quite well. I was wondering how influential Gutenberg was in any other conceptual work that you did for physicists?

MUNK: I am not sure he understood that problem. You see, and Maurice Ewing did, he was just getting interested in dispersion that is before he wrote the book with Frank Press on... Then he became very interested in dispersion. I remember I once had dinner with him and he told me that he had just discovered some work by either Stokes or Love in which these dispersive relations were explicit. I don't think the seismic community understood that. I mean I wasn't quite as bad as I put it out. I think it was not generally understood. But it is certainly understood by all students of geophysics today that these are two separate phenomena.

DOEL: Right, and the critical point though is that at that period of time it was a new area and there was not a lot of work.

MUNK: You see, in ocean waves swell is a combination of both, you get-- it is a combination of differential attenuation plus dispersion, and still you have to be very careful. But the fact is that storms are shorter period in the storm area than they are when you get 2,000 kilometers away.

DOEL: Right. It was interesting when you mentioned that you had dinner with Ewing. By that period of time were you traveling to the East Coast, was that a meeting that occurred....?

MUNK: I know I once had dinner with him, and I remember him saying you would be interested in this, I found this interesting paper in the Royal Society, some old paper that seems to be related to what he, Ewing, was doing with dispersive waves. I mean he was discovering that. I had been to Ewing's house a number of times. We certainly did not have a close relation, and it eventually ended up rather badly. I was on the list of people for the Lamont directorship.

DOEL: In the year after he had left to go to Texas?

MUNK: He was still there, but he had decided he would leave. And I was asked to go and I had not pushed for that job. In fact Judy and I had both pretty much decided that that is not what we wanted but we were invited to go there and we had dinner with Judy, we spent the night at Maurice's didn't we at the director's house in Lamont? Yes. It became a little unpleasant and as I don't know, it is not important and by then I think we decided that that really isn't what we wanted and we were not trying to oust him as a job. And I mean Maurice was a great man, and I would have ... I don't know, he was bitter of course.

DOEL: It was difficult at that time because of his relations with Columbia University.

MUNK: With McGill, who had been chancellor here, and I think that was a shame. After all he is a great man and there was an argument about who would keep overheads and it seems to me thoroughly unworthy of the whole thing that should have become a critical issue, and he should have been permitted to stay at Lamont as long as he wished. It was unfortunate that I said when I was asked would I permit my name to be in it I said yes, and then they asked me to go and visit him.

DOEL: You felt uncomfortable by the time you went out then in terms of...

MUNK: Yes, they asked us to stay the night, and I remember it became a little strange.

DOEL: Strange in a...

MUNK: Well I don't know. It wasn't a very comfortable evening.

DOEL: I am curious, because this is critical for understanding the future direction of another major research institution, Lamont, and these become critical issues. Were you aware of much of what was going on at Lamont in terms of research and other...

MUNK: Not closely, but I mean they are one of the great institutions. They probably had more data than anybody, else did which served as the basis for the plate tectonic revolution. I think it was their data gathering abilities in Maurice's insistence on appropriate storage of the data, it was really their great contribution to that plate tectonic revolution. I think Maurice himself was skeptical for some time about basic issues. And so I have really great admiration for what he did there. And I think I would have made a lousy director; that is not my kind of institution. I would

have at one time liked to have been director of Woods Hall, and that is about the only job of that sort other than what happened here that I would have liked.

DOEL: I am sorry I didn't mean to interrupt you in that moment. Were you thinking back to an earlier time then as well in the late 50s rather than...?

MUNK: Well I was once offered the deanship which I did not want, and then once offered something much better that was they had one endowed chair at the time and it was called the Doherty chair it has since been split but there was at one time one coherent endowed chair, and that is the one that Judy and I came awfully close in thinking we should accept, awfully close. We picked a house, I don't quite know what happened eventually but those things are so difficult. It would have been wonderful, just as it has just been a wonderful life here.

DOEL: I do want to talk to you about that again once we get a little closer to that period of time.

MUNK: I am sorry I keep on going ahead.

DOEL: That is OK because it is appropriate in the way that unfolded. The one thing I was just curious about in what you were telling, I do sense that even by the time you had arrived in Lamont for that overnight stay you were already uncomfortable with the thought of the directorship and then what you were learning in the course of that evening and the interaction just made the matter more difficult still.

MUNK: Yes and I think, Judy — oh she is on the phone. I think we thought at the time that we should have not gone we weren't serious and we felt that it was unfair that Maurice had been put into that position, and us appearing as possible successors. I am not really qualified for the Lamont job, it is basically a solid earth job and I don't know enough geology. It was probably all wrong.

DOEL: As you say, Lamont is a unique institution and that it already by that point it was covering a number of different areas and it was branching into other areas.

MUNK: But so was Woods Hall. But I mean the emphasis there was strongly geologic and...

DOEL: Geophysical, and of course not as much as later on physical oceanography, which is one of the areas in which Lamont had not taken.

MUNK: And in fact Maurice Ewing was not really terribly interested in what is normally called physical oceanography. He was interested in marine geophysics and marine physics, but he certainly he did not think about the oceans in the stomal [?] sense, you know conservation of waticity [?] and that sort of thing it is a different field.

DOEL: Indeed, some of the long term members of the Lamont community remember him feeling the ocean was simply the impediment that kept him from making even more direct observations of the ocean floor. That changed in time, but indeed it was present them. We will

return. Those are important issues for later, on that question of directorship of Woods Hall and those career choices that you faced in the latter years.

MUNK: Yes, well that was kind of funny the whole Woods Hall directorship. Paul Fye was director and he had brought up the subject of the deanship. Well I would be delighted to meet with you whenever you wish, I hope you don't stretch it for too many years, just so we have a fairly coherent. So I don't say things that are totally different from what I said today.

DOEL: Well I hope we will have the transcript prepared by the next time that we meet. But simply let me say right on tape at this moment in addition to thanking you for this very long session that we will, and this should be on the tape, not to make the tape available to anyone or its transcript without your express knowledge and approval as stated in the permission form that you will be getting from the Center for History of Physics at MIT. But let me thank you very much again.

CCR

Interview with Walter Munk
at La Jolla, California

By Ron Doel
September 16, 1997

RON DOEL: This is Ron Doel and this is a continuing interview, a second session, with Walter Munk. Today's date is the 16th of September, 1997. We're making this recording in Walter Munk's home in La Jolla, California.

One of the things I didn't get a chance to ask you about in the first interview was the formation of the Institute of Geophysics at UCLA. When did you first become aware of discussions about creating the Institute?

WALTER MUNK: I did not participate in the creation of the Institute. But Harald Sverdrup was involved and Jakob Bjerknes and--what is the name of the geochemist who really started it all? Oh gosh, he was the key person who tried to persuade Robert Gordon Sproul, the president, oh, what is his name? He only died recently.

Doel: This isn't Joe Kaplan that you're thinking of?

MUNK: Joe Kaplan was really the principal figure. At the time, really, the idea of having a geophysical institute was probably a new idea, and Joe was the key behind it. I remember going to the annual meetings at UCLA, which in a way were the key meetings, the kind of thing that is now sponsored by the American Geophysical Union.

Doel: Things like the Chapman Conference?

MUNK: No, there was a meeting once a year at UCLA. I remember one time going when Teddy Bullard spoke for the first time about the generation of the magnetic field by the fluid motion in the core, and somebody else up there had done some work on the fluid core and said my heavens, these two things really fit together. Those were very good meetings. Joe Kaplan—there was some in-fighting, and the history of the Institute, the statewide institute, has been written up since then by Anderson...

Doel: Orson Anderson.

MUNK: Orson Anderson, and the present director is Minster, [J.] Bernard Minster, a French geophysicist who is now sitting here at La Jolla at the IGPP. He's now the statewide director. So there's a lot of work that has been done in reconstructing it. It turned out to be a useful way to get me started. I'm not quite sure how, but my original appointment was as an assistant professor in the statewide Institute of Geophysics.

Doel: This is where part of your funding was coming from, of the FTE.

MUNK: Where the FTE came from. And then when I-- I think-- I'm not sure we have covered that. At one time when I wanted, when I thought I would leave here, and I had been offered two jobs, one at MIT and one at Harvard-- has that been covered?

Doel: We haven't. I want to make sure that we get to that since it's in the late 1950s. But it's clear, or at least it seems to me, that your experience in the Institute helped you think about what it was that you wanted to create here.

MUNK: At that time.

Doel: I'm wondering, you mentioned a moment ago the experience of going to the meetings and finding them very stimulating.

MUNK: Very stimulating.

Doel: What do you think, or what did you hear was the reason that people like Kaplan and Holmboe and Sverdrup had in mind in creating the Institute?

MUNK: I think it was Joe Kaplan who had in mind creating the Institute. I don't think Holmboe or Bjerknes or anybody would have thought about it. And then Joe Kaplan was somewhat controversial — you need to look at the history. I think these other people came in because president Sproul had accepted the idea of an institute but...

Doel: Did not accept Kaplan as director.

MUNK: But had not accepted Kaplan as director. Harald Sverdrup didn't ever initiate, saying we need a geophysical institute.

Doel: Do you remember talking with him about it? What his views towards the institute were?

MUNK: Not much, not much. In fact, I was surprised in seeing Orson Anderson's and other accounts to hear how much involved Harald appeared to have been.

Doel: That's interesting. You didn't see it from here.

MUNK: No.

Doel: How often would you yourself get to UCLA for those meetings?

MUNK: Before we started La Jolla?

Doel: Yes.

MUNK: Oh, once or twice a year. Then when I was offered the jobs at MIT and Harvard simultaneously—and we should talk a little about this.

Doel: We have to. It's a major event in your career.

MUNK: Roger came and said, why do you want to leave? What is it that you want to do that we can't do here and do better here? I told him I wanted to pursue more general geophysics than fluid earth oceanography, only. He then arranged to start an institute here by arranging for a certain number of FTEs, Full-time Equivalents, a little bit of money. Roger was very persuasive, and I stayed. But it led to some difficulties between Louis Slichter and Roger Revelle. Roger's idea was that what started the Institute was largely part of the La Jolla campus, Scripps. Louis Slichter's idea was that we would be run out of his Los Angeles headquarters.

Doel: Out of the statewide operation?

MUNK: Out of the statewide. Gordon MacDonald fell in between. He had just become a member.

Doel: Of course he was your collaborator in the earth rotation work.

MUNK: Yes. I remember a meeting at Roger's house. Louie and Gordon had come down. Louie wasn't very healthy. They'd really gotten angry at each other. Roger said if it's here, it's got to be part of Scripps, basically, and Louie had said no.

Doel: This is about 1960? This is very soon after the Institute has begun?

MUNK: Yes. It was really quite awkward, because I had really two boxes. Roger, I think, was right. I don't think you want an activity on a campus that's not locally controlled.

Doel: What was it that Louie Slichter wanted particularly to direct? Was he worried about duplication of research efforts, or resources?

MUNK: Well, I think he really wanted the power of being the czar of geophysics at the University of California, period. His successor, who was an atomic energy commissioner and a Nobel laureate, who's that?

Doel: Libby.

MUNK: Libby was worse in that respect. He came down once. We sat in this house, and his idea was that Scripps should be part of the statewide Institute of Geophysics. He essentially ordered me to go in a direction so that geophysics was the umbrella and Scripps would become a

part of the statewide Institute of Geophysics . I remember telling him that I thought he had to get himself another boy, that I wasn't interested in moving in that direction.

Doel: I imagine you were astounded to hear Libby.

MUNK: I was astounded, and he gave up on that. He had in mind at one time having a very powerful statewide structure run out of headquarters. But Louis had that to some extent, too.

Doel: That's interesting. And this was the early 1960s. When Harold Urey was concerned about developing space facilities, space laboratories, that were all intended to be connected, as I understand it, to the Institute as well.

MUNK: And of course Roger also was a bit of an empire builder. It was an inevitable clash. I was personally so much closer to Roger than to the others that I always sort of took that side. And if somebody wants to follow it up they should look at Orson Anderson's and Minster's recent histories of the institute on the Scripps website. ¹

Doel: We will make sure there are citations to those in the transcript. Did you sense how much support there was elsewhere in the California system for Libby's idea of integrating Scripps in to the statewide system?

MUNK: I don't think there was much support. I think Libby was an empire builder, and at that time it went through an era of having grandiose plans. But I must say after I said I wouldn't do that-- first of all, I think it would have been a failure, that-- no, I can't do that. He gave up on that. After I'd said no he never came back to it. He was a difficult person.

Doel: What sort of person was he?

MUNK: I thought he was a bit unpleasant, and I had a very bad reaction once from the adjacent point of view. He was then at Atomic Energy Commission, a very powerful man. He asked a physicist-- see, I'm having trouble with names, of Greek extraction, who had this fantastic idea of space experiment whereby you would release some radioactive material at great height and see what sort of artificial aurora you would produce.

Doel: This was part of the Argus experiment, the atomic explosion?

MUNK: Yes, yes. Who was that?

Doel: I'm trying to remember myself.

MUNK: Nick [Nicholas C.] Christofilos. Once in my presence at a cocktail party in Boulder, I think Libby was professor in Boulder at some time--he asked Nick to do something. Nick

refused, or Nick said he didn't think he could do that, and Judy and I were there. And Libby said to him, "Well, you know you have a clearance now which We're in a position to remove." Nick was working at Los Alamos and depended on that clearance, and I guess Judy and I never quite forgave Libby for hearing him threaten someone that way. We also knew his first wife, Libby's first wife, but he married again, and it was not very pleasant. I was not very fond of him. I thought he was a powerful and pushy person.

Doel: Do you know who made the decision to bring in Libby as the director after Slichter?

MUNK: After Slichter? Well, it was probably getting a Nobel Laureate as a director seemed like to that committee a pretty wonderful thing to do, and Libby had brought into the geophysical interests in addition to his chemistry.

Doel: Do you remember others who were being considered at the time?

MUNK: No, I was not involved in that choice. I certainly was not on the committee. I don't know, and again, I think one might find some information on that. We all thought it was wonderful to get him in. I mean, he was a man of great prestige. I subsequently thought he was a very difficult man.

Doel: Did it become apparent fairly soon that he had these broad ambitions, or did it take a while before you began to feel uncomfortable with the way the Institute was going?

MUNK: I don't remember. But I remember only that he came down from Los Angeles to try and work on a policy whereby Scripps would become subservient to the statewide institute.

Doel: Was Roger Revelle still here at the time that he came down?

MUNK: When did Roger leave?

Doel: It was about 1964.

MUNK: He must have still been here, yes.

Doel: How well did you know Slichter?

MUNK: Quite well, and his wife, Martha. Quite well. They were actually close friends of Vern Knudsen, who was chancellor of UCLA, and Judy's parents were close friends to Vern Knudsen. So there was a social connection. The Slichters were very, very nice people. Very loyal to UCLA and loyal to the university. Very nice people. Louie fought hard for his institute.

Doel: Knudsen was quite involved in making the Institute happen, as I recall.

MUNK: Louis was.

Doel: But Vern Knudsen was also working on it.

MUNK: Vern Knudsen, I believe so. But the role of Joe Kaplan is an interesting one. Joe Kaplan was sort of a fast talker, big mouth. Minster, the present statewide director, told me once that he thought that his role had been underestimated in that, he was the man who thought it up and didn't quite succeed in running it after it was started.

Doel: What sort of reasons do you think kept him from becoming director?

MUNK: Well, I think he talked too much. He bragged a lot about his own accomplishments, and people sort of found that a problem. I think there were some personal reasons like that. You really ought to look at the history. I found him OK, if you could just overlook the fact that he would say "I did this," and "I did that," and "I'm responsible for that." Other than that he was really a very sweet man. But it did get on people's nerves.

Doel: I'm wondering what his scientific reputation seemed to be. At that time he was working on upper atmosphere problems.

MUNK: Yes, there is a line, a spectral line, isn't it, that's known by him and some other person, the Kaplan-something line?

Doel: I believe that's right, yes.

MUNK: He spoke about that line very much. That was his line.

Doel: That's a good way to put it. How was Slichter regarded as a scientist?

MUNK: He was a combination of, apparently, a good experimentalist, and at the same time, competent theoretically. He had done some applied geophysics. He was interested in the application of geophysics to industry. He came from Wisconsin, didn't he?

Doel: Yes.

MUNK: And his family was a known family in the scientific world. Charlie Slichter, is that his father?

Doel: I believe that's right, yes.

MUNK: There are all sorts of Slichters around, and they were very prominent at that university. It is well known to people other than me that they've been presidents or something. He had done

some work using an enormous cable to produce an electric field for seismic, for mineral exploration. So he had done some ambitious experimental things. And I remember seeing the paper he wrote that was I think published in the Royal Society about interpreting the experiment in terms of the distribution of minerals within the Earth. It was a very professional paper. But he is not one of the great geophysicists of our era in the sense that Maurice Ewing was.

Doel: Slichter didn't publish a great deal during the time that he was director of the institute. But were his papers, those that he did produce, well regarded as you recall?

MUNK: I think so, I think so. I think he was known probably best for his electrostatic work or whatever it's called. Then he became interested in free modes of the Earth. Probably the best person to speak to about that would be Leon Knopoff. I thought he was a good, solid scientist, Louis. Not, as I said, a leading person, and when he was statewide director, he certainly was very helpful to anybody who worked in the field, to try and help them. He was very helpful to me. Other than that collision with Roger one day, it was really very pleasant to work with him.

Doel: When you say he was helpful, I'm wondering what comes back to mind? How did he help you?

MUNK: Getting money, for example, for La Jolla, that kind of thing. When we started the institute we often needed some help. The university bureaucracy is not negligible. It took someone like Louis and his close relation to his chancellor and the president to get permits for various things to get started.

Doel: In 1959, he allocated a thousand dollars in general unrestricted funds to help start the institute, then \$2,500, as I recall, for FTE allocations.

MUNK: Yes, that kind of thing. That was more important than the size of the number would indicate to you.

Doel: It's absolutely critical for getting something new started. Was he a good administrator, do you think?

MUNK: Yes, I think probably. Not in some restricted sense, but he cared terribly about the Institute, and he supported the people in it. And that's what is important, I think. He was very loyal to UCLA. That's probably part of the problem with getting started here, that he saw it more as an appendix to his own UCLA activity. But otherwise he was just loyal to people who were working in the Institute. Note Slichter bought a lot in the S.E.A. subdivision adjacent to S.I. Loyal, and always helpful.

Doel: Do you remember discussions on whether there were new fields that ought to be brought into the Institute's purview that weren't done in the 1950s? Or did you feel that pretty much you were covering those areas you felt needed to be addressed?

MUNK: I think the normal modes of the Earth had not been done. In fact I missed on that. I had become very interested in power spectral analysis at the time when very few people could do that. And I wrote a paper that I should reconstruct with Louie and others using some gravity records to look for the normal modes, and I made a basic mistake. I felt one should use a record that happened between big earthquakes and not during an earthquake, because I felt during an earthquake you would get some kind of transient phenomena that would make interpretation difficult. We couldn't find anything. Then a little bit later, especially I think Freeman Gilbert and others, did a very similar analysis on a record which covered an earthquake, and discovered the normal modes. It was a big thing at the time. I just picked the wrong records. It was stupid in retrospect.

Doel: Things are often clearer in hindsight.

MUNK: Yes. It was a kind of funny story in that Louis, I know, was very much interested in that. He helped establish a station on the South Pole, which was uniquely located because some normal mode oscillations which are to some extent effected by the Earth's rotation, have some unique properties at the polar rotation. That was the purpose for doing it. And that was very well done.

Doel: Was that part of the IGY? The South Pole station that was established?

MUNK: I don't remember. It could well have been.

Doel: Yes. And this was a seismograph station?

MUNK: Gravity. As you know, gravity and seismology blend when you get to very much lower frequencies. It's not obvious. You think of an acceleration, but of course, G is an acceleration. So when you go from measuring gravity to measuring acceleration of the ground, well, the answer is clear. When you have frequencies lower than the normal modes, you are more in the gravity regime, and when frequencies are higher you are more in the acceleration regime. But it's the same instrumentation, the same theory.

Doel: Right. Was it a particular problem to develop instruments that would withstand the conditions?

MUNK: That would be stable enough at low frequencies?

Doel: Yes.

MUNK: Very much so. That was the challenge—those instruments did not exist. IGPP in La Jolla had this as a major consideration when we started in getting very stable, low frequency instruments built. The key was to really go to digital, rather than analog, so that the filtering could be done digitally. Julian LaCoste, a Texan, had built the best instrument at the time. It was built around a very intriguing principal called the zero length spring. And it was Cecil Green who purchased us these original instruments which got us started here. And people under Freeman Gilbert's leadership, Jon Berger and others, we really then became sort of the normal oscillation group here at IGPP.

Doel: I should ask, is this in the late 1960s that we're talking about?

MUNK: Yes. And now these instruments span everything from cycles per second to cycles per week, and with great accuracy.

Doel: But indeed through the 1950s this was a major bottleneck?

MUNK: Yes, it was a major bottleneck.

Doel: We've been talking about the development of the Institute, and this might be a good time to try to cover that in greater detail. I wanted to hear first about the two offers that you got from Harvard, and also Woods Hole [Oceanographic Institution].

MUNK: Not Woods Hole, MIT.

Doel: It was MIT, thank you. That's right.

MUNK: It was a man named Bob Shrock at MIT, who was then Department Chairman. At Harvard the offer came from a well known and wonderful man, whose name at the moment I can't recall, who was head of Geophysics at Harvard for many years. A high pressure man.

Doel: Was this Francis Birch?

MUNK: Francis Birch. Thank you. They both had me come to talk to them, and I was very much inclined to go. Especially Francis Birch, I thought was a wonderful man.

Doel: What sort of person was he, as a scientist, and as a person?

MUNK: Just excellent.

Doel: You're smiling as you say that, in recalling him.

MUNK: Yes, I enjoyed, so much, meeting him.

Doel: Had you not known him before?

MUNK: I forgot when I met him. He was one of the people who reviewed Gordon MacDonald's and my book *Rotation of the Earth*, and he did it so helpfully and constructively.

Doel: As a manuscript?

MUNK: As a manuscript. We had sent it to him. Not the Cambridge Press. He was very nice. I remember going to his house, at the time, at Harvard. I also remember that the person who runs Harvard is the Dean. The Dean is a very important position at Harvard. The Dean at the time was the man who served under President Kennedy.

Doel: Was it McGeorge Bundy?

MUNK: We went to see McGeorge Bundy. Francis Birch took me over to talk to him. We talked for a while and then McGeorge Bundy said to Francis Birch, "Now you leave the room so I can hear what Walter thinks about your Department." I was amazed. I remember that, because I didn't think you addressed famous scientists that way. But if you were a Dean at Harvard, that's how you did it. No matter what your level was. But it was not embarrassing because I felt Francis Birch had a great Department.

Doel: I'm curious, what you did think of it? Harvard in some ways was a little slower than some other Departments in developing geophysics.

MUNK: Very much so. And MIT and Harvard couldn't have been more different. I got letters from both of them, politely attacking the other one, when offering me a job. And I think I was intrigued by Harvard, but partly because of the Harvard name. Certainly from a professional point of view, MIT offered more opportunities at the time.

Doel: You were saying that you did like what Harvard had to offer.

MUNK: I think it was partly still the romantic influence of Columbus Iselin that I had in mind. And meeting Bigelow once, that I mentioned already.

Doel: Yes. You're referring to the first interview that we did.

MUNK: And Frank Press was... No, was that much later?

Doel: I believe he was still at Cal Tech at the time.

MUNK: There was a subsequent time when I was being talked to when Frank Press was involved. But at the time it was Shrock. The thing that Judy and I remember was that we were

having one of our daughters, and I was taking Judy to the hospital. We were ready to go and the phone rang, and it was Shrock offering me the job. I hadn't been offered it yet. And I said, "Really, I don't think this is a good time for me to think very well about it. We're about to go to the hospital." I hung up, and we started walking out the door when the phone rang again, and Judy said, "That must be Harvard." And it was Francis Birch. And so we've always remembered that. I took it very seriously. I was greatly honored by it. Then Roger made his counteroffer. And for reasons I don't think were any more than emotional we eventually decided to stay.

Doel: I wanted to hear more about both your reactions to Birch, and the MIT offer. Birch and Gordon MacDonald were working reasonably closely at that period of time.

MUNK: Yes. Well, Gordon came out of Harvard of course. You know, I'm in quite a different field than high pressure chemistry, which was Gordon's field, and Francis Birch's. But it was partly a romantic thing that I felt about Iselin, and others.

Doel: Were you interested in developing oceanography at Harvard? Were you given a sense that this was what you would be able to do?

MUNK: No, I think really I was more interested to work as a geophysicist. It was not a question of asking me to come there for purposes of developing an ocean component, although I'm sure at Harvard you could do whatever you wished, and at MIT too. Well, the ocean component at Harvard was biological. And at MIT at the time, I think that Rossby's MIT attachment was quite separate. A quite separate group from the geophysical group that Shrock was heading.

Doel: Was the '59 offer from MIT right around the time that Cecil Green made his big gift to the Department?

MUNK: To MIT, yes. Yes, I think that came at about that time. There's an interesting story that Cecil told me about Maurice Ewing that we haven't talked about; it just comes to mind. Maurice wanted Cecil to endow the new Lamont laboratory, and it was at the time that Cecil had made a major gift at MIT, though I don't remember the chronology in detail. Cecil told me that he told Maurice, "Well, come and see me." Maurice went up to see him—I don't know where "up" was. It could have been in Dallas. And they spent the whole night talking, Maurice trying to excite him about his ideas. Cecil decided not to do it. It was a major decision in his career, in Cecil's life, that he decided that he did not want to become a major donor in Maurice Ewing's dreams. Later, as you know, he did support Maurice. He supported him after he left Columbia to go to Texas and established him. I think, endowed the chair that paid Ewing. So.

Doel: And provided the research vessel?

MUNK: And the research vessel. But at the time I remember Cecil telling me that Maurice had come up and that they talked all night, and that Maurice had sort of suggested that the best place

to put one's money was with him. That would be Maurice's line. And that Cecil was very loyal to Bob Shrock. He decided he couldn't do both.

Doel: He felt that it would be setting up competition, or undermining MIT?

MUNK: I don't know that. All I remember was that he said to Maurice, "Come up and talk to me." They talked all night, and that Cecil then decided not to become a major donor.

Doel: Yes. That was an important moment in the development of Lamont. Do you recall Cecil giving a reason why he felt that it wasn't appropriate to do it?

MUNK: No, I don't remember why. But I think that he probably did not want to spread himself too thin. It's not much more than that, I don't think it was an anti-Ewing move. After all, he did listen to him.

Doel: That's very interesting. The MIT offer to you, I deemed to come at the time that the major gift from Cecil had been announced.

MUNK: I'm not clear on the chronology. I have a book of Shrock's about Cecil [Green] where we could probably reconstruct that.²

Doel: I'm pretty certain it was just about that same period of time. The grant was intended to create a semi-autonomist group that would link-- the physicists, chemists, others interested in earth problems, together, in an interdisciplinary way.

MUNK: I think so, yes.

Doel: Was that appealing to you, that kind of organization?

MUNK: Yes. Oh, very much so. And after all, when we started IGPP here it was broad. We would talk about that when you get to it, what were the basic ideas behind it.

Doel: We could, if you want, cover that now, because We've really launched into that. One of the things that I found very interesting was that one of the offers from Bob Shrock came at the very end of March, in 1959, and already by the middle of April you were working out the plans for the Institute--the draft that you had written with Dick Vacquier.

MUNK: Vacquier. Well, I had become very interested in solid earth geophysics, partly because of the rotation of the Earth problem, partly the normal mode problem. And I guess I felt that oceanographic institutes should really deal to some extent with the solid Earth problems. I think we gloat in here that it was very lucky that it turned out that the subsequent plate tectonics

revolution really showed that the key to the history of the Earth was on the ocean floor and not on land.

Doel: Indeed.

MUNK: We certainly didn't know that. So that was lucky. There were a few things when we started, and one of them, really, I thought was a good one. It was that the construction of the Earth history was based on paleo-data, and we thought that instrumentation had developed to a point which could measure the d/dt , the changes in real time. And by that I mean how, to what extent, continents are moving, and the Gulf of California is splitting, and mountains are rising.

Doel: So it was a critical advance in precision.

MUNK: And that had not been done. I think that was an original idea, in other words. I remember using the phrase at the time that you could figure it as low frequency seismology or high frequency geodesy. Or DC seismology, or high frequency geodesy. To make that connection.

Doel: Did that catch on quickly? Did you find that you had support, or did it not seem right?

MUNK: No. I didn't. But I remember Frank Press once gave a little talk when I got some kind of a medal, and he mentioned that he was fascinated by this idea of DC seismology. In fact, we had a doctor's thesis at IGPP of a man named Bill Farrell on trying to build a displacement seismometer — how would you go about building an instrument that would tell you when an earthquake was all over how much you had moved by essentially twice integrating acceleration.

Doel: Interesting, sure.

MUNK: Acceleration is d^2/dt^2 . Is this in principle possible? Yes it is; it's very difficult to do. We do this nowadays routinely with GPS, by integrating accelerometers. But that was an idea. And that led to the Piñon Flat Observatory, about laser strain meters, measuring the strain of the Earth in real time. And many things of this kind. And it's still a key component to IGPP.

Doel: In the late 1950s, early 60s, as the debate over continental drift was beginning to rise, Birch, and Gordon MacDonald, were among those who didn't see reasons to accept it.

MUNK: And Jeffreys. And Maurice Ewing. And Keith Runcorn.

Doel: Keith Runcorn was leaning towards polar wandering.

MUNK: He was leaning into that direction, but he made a switch. Gordon did. Jeffreys never did.

Doel: Right. Did that come up during your interviews? Did you discuss broader Earth problems when you went to Harvard and MIT?

MUNK: I don't remember. I would like to remember. And of course, I played no role in the plate tectonic revolution. I've always been sorry that Scripps, which had made some very significant measurements really, did not. The magnetic stripes were first done by an Englishman named Mason and Victor Vacquier played a significant role. And the heat flow measurements by Bullard were done from Scripps ships. I was among them. And Russell Raitt's measurements indicated that the sediments were only 100 meters thick instead of 2,000. All should have told us at the time what's going on, and we didn't see it. The big appreciation came from other people. Nor from Lamont. I mean, Maurice Ewing was busy collecting good data, and had the good sense of keeping it available, but the coordination into this hypotheses was done in England.

MUNK: We weren't very bright.

Doel: One could make quite alternate interpretations. This was such an extraordinary development.

MUNK: Yes, it is extraordinary. Bill Menard, you know, had written about it.³ As you know, Bob [Robert S.] Dietz, much underrated, who died recently, had some of these ideas.

[Begin Tape 1, Side B]

Doel: How well did you know Dietz?

MUNK: Quite well. When I first came down to Scripps at the very beginning, my first visit, and I think we discussed this I spent the night at the Community House, now the place where IGPP is located. A day later Fran Shepard arrived with Ken Emery and Bob Dietz. They occupied the bunks next to mine. They were good lieutenants to Fran Shepard. We would see a lot of them, yes.

Doel: In developing the [IGPP] Institute of Geophysics in La Jolla, did you sense that because of the IGY there was now an opportunity to attract a different kind of student to geophysics that hadn't been possible earlier? Or had that not been a concern for you, as geophysics developed?

MUNK: I think we were more research-oriented. There had of course been students from time zero, at IGPP, but I think the basic idea was to combine geophysical and fluid measurements, and that there was a lot to be done by this kind of modern measurements. Very measurement-oriented, even though I myself, as you know, am not a good instrumentalist--it was, in fact, very much in my forte. But actually the earlier appointments were not particularly experimentally oriented. I really-- we need to look at the record for George Backus, or even Gilbert, [who] came early. Then, other appointments less powerful. Hugh Bradner, Dick Haubrich did not work out

so well. But, then getting started on those measurements, low frequency, came about very quickly, and Freeman Gilbert was an important factor.

Doel: I was also struck by-- and I'm wondering if this was your vision, to try to keep the Institute reasonably small, to just 12 people, as I recall. What led you to that idea?

MUNK: In general, kind of an Austrian love of intimacy. It worked very well. You know, we were at a size where I could write a letter of support of promotion without having to pull out any files. At a certain given size you know what's going on, and the bureaucracy takes on an entirely different view if you can, without having to review anything. And we were that size.

Doel: Was there a model that you had in mind as you worked to create the Institute here?

MUNK: I don't think so. I think it was the idea that it would be small enough so that the director could continue being a researcher, and that did turn out to be the case. I mean I really never stopped working on my own. We always had an associate director of the associate director. That's a problem, by the way. The associate director of the La Jolla Laboratory was an associate director of the Institute of Geophysics Statewide, and when I came in, Louis Slichter ran his Institute with three associate directors. There was one associate director for space, that was Gordon MacDonald. There was one associate director for the atmosphere, and then there was the associate director for La Jolla. So it was kind of funny, three responsibilities. But we always had, then, someone else. Originally Freeman Gilbert was willing to be associate to the associate director, and then he succeeded me. When he started he got John Orcutt to help him, who is now director, etc. So that made it very nice. We really always had at IGPP, I think, a very compatible group of people. We've gotten, of course, much bigger.

Doel: Right. Was personality important as well as scientific credentials when you considered candidates for the Institute?

MUNK: Not particularly. But I made some mistakes, I think. I think some of the appointments weren't, in retrospect, very good. But some turned out to be very lucky. I mean, the Backus-Gilbert thing was fantastic. John Miles wanted to always come, and certainly he's an outstanding man. So, I don't think it was a matter of having friends come. I really didn't know any of these people when they came. We were sort of shopping around in the Revelle sense, trying to see what we wanted to do and who were the imaginative people.

Doel: Something I found in one set of papers was a very interesting statement you made at that time about Scripps: "The informal intimacy of the early days is gone. We have professional administrators and parking problems. I have found these developments very disturbing, yet have become convinced that a modern scientific institution must have a broader base than that of the Scripps Institution during Sverdrup's days."

MUNK: This was comment about Scripps, other than IGPP.

Doel: Yes, but it was your thought about part of the need?

MUNK: I think so, and We're having a problem now that's closely related to that. We do not have a director, as we speak. We are suffering very much of a lack of leadership. And to some extent, the Institution is not pulling together. There are different groups that have different interests. And it has to do with this size problem. And it's very bothersome. But I think the other option of having very limited goals doesn't work. It works even less well.

Doel: Yes. One other thing I was very interested in in the early development of the IGPP was the appeal that you made to the Fleischmann Foundation. How did that come about?

MUNK: Oh, I knew-- what's his name? I knew a man who was in part of the Fleischmann Foundation. Walter Orr Roberts. Does that mean anything to you?

Doel: It sure does. He was in Colorado, at the High Altitude Solar Observatory.

MUNK: Well, the building of the IGPP building, of course, meant a lot to us because of Judith. And I don't think We've spoken about that. It has meant a lot to me. Both the old IGPP and the new IGPP. We put an awful lot of effort into trying to do this well. I think, it's a good story.

Doel: I want to hear about that. Are you talking about the architectural component?

MUNK: Yes, an important part of making the institute work is to provide an appropriate house. We had decided that, "we" being Judith and I, long ago that we wanted to build something less formal than Ritter Hall and so on, and that we wanted something like a wooden building. The site, which was where I had spent my first night at Scripps, meant something to me romantically, but also was one that nobody else wanted. It was too far from 'Scripps'. People thought it would be too remote. We're now right in the middle of things.

Doel: As things have grown.

MUNK: And from the very beginning, we played a very unusual role. Professors don't usually participate to the extent to which we did.

Doel: In the design, you mean?

MUNK: In every aspect. Choosing the architect. Judy chose Lloyd Rivocco, essentially. We had enough backing from the administration here that could get away with that. And really Judy and I meeting with the campus architect made all the decisions. We worked in great detail on that.

Doel: In contrast to Ritter Hall, what was it that you wanted in the Institute, architecturally, to make it work?

MUNK: We thought it should be more like a home than as a hall. More flexible. And the choice of redwood, of course, is connected to that. The choice of the site. We thought Scripps had done a poor job architecturally. I think you will find broad agreement. I think it's fair to say that the two geophysics buildings have really changed the whole Scripps campus. And much of what's gone on since we first started is, an expression of that. That's a compliment to Judy. She really has changed the way the Scripps campus has developed.

Doel: I think I know what you mean, but I want to be sure. When you say that "it's changed the campus," what are you thinking about?

MUNK: Well, the buildings were not little cement blocks, but much more home like. Much more informal. There were several very major decisions on the old IGPP. One was to have laboratories where you could drive trucks in and out, so that the experimental work could be done. In the old Ritter Hall type buildings you had to move all your gear up and down in twenty-eight boxes by elevator. And when you got out to sea you found that you had forgotten box twenty-five. Whereas what we did at IGPP is to assemble things in the portable laboratory adjoining the permanent lab, and then drive the truck in and pick up the assembled portable lab, all tested and put together. We pioneered--"we" being Frank Snodgrass who worked with me then--the idea of building these portable labs that you use in your permanent labs, and then they were picked up with a forklift, put in a truck, put aboard the ship. Standard procedure nowadays. We were the first to do it. Even our ships now have screw holes in the deck, four on center, and all the labs are built to fit into those.

Doel: That's very interesting.

MUNK: It was an evolution.

Doel: Was it a result of particular frustrations here, or had you been thinking about how to expedite it, make it more efficient?

MUNK: No, just wanted to do a better job of building a building, that's all. It hadn't been done very well. None of the previous oceanographers or directors had participated in any significant extent to building the laboratories, and we thought it showed. And it was done very deliberately, as I said. Remember, there were very interesting problems. First of all, we had to raise some money. I think you've heard this story, probably. We built the original building for less than one million dollars. The university committed \$486,000, which was half of that, if we would find the other half.

Doel: This was the basis of the appeal to the Fleischmann Foundation?

MUNK: Fleischmann was just one. We had just started. It was a different time in research. There is, and was, an Air Force Office of Scientific Research. They were very interested in, at the time, in the nuclear test ban problem, and the building of sea-bottom seismometers and things of this sort. One day the head of the Office of Scientific Research, the Air Force Office, came walking down these steps, just outside here, with two of his officers, saying he was very pleased with what's happening at IGPP, and how could he give us a hand? I said, "Help us get a building." And they gave a major contribution to the building. It happened really like that. With ONR at the time, it was most remarkable, when I had made a proposal for my own work, and when the money came I noticed that \$10,000 had been added, that was not requested. I asked the then Project Officer, "Was it a mistake?" He said, "No, we decided to add that as a contingency fund for the Director so that you would have an easier time getting the Institute started." Amazing. Quite extraordinary.

Doel: It is extraordinary, particularly in the current climate.

MUNK: Yes. And that was part of it. The ONR thing lasted for about ten years.

Doel: So this was in the 1960s, the first year?

MUNK: Unsolicited, saying that they wanted to contribute towards the Institute. The funds were usable by the Director, with very little need for detailed accounting.

Doel: That was somewhat more characteristic of science generally in the US in that period of time. But it still is significant for your development of the IGPP.

MUNK: Yes. I also went and asked for some money from NSF. The director at the time was Waterman. The first director. And I remember not getting a response to our letter saying, "Are you considering it?" And then when I was serving on some kind of a White House Committee, and going into the john at the executive office building next to the White House, there was Waterman taking a leak. I said, "Well, I finally have you where I want you. How about our building money?" He became quite distraught. But we did get it two weeks later.

Doel: Now that was fortunate.

MUNK: That was a fortunate thing. And what happened--this is sort of a funny story. We had our \$500 K, then we got \$250 K, was it from the Air Force? You have to check the numbers. They said, "All right, We'll give you half of what you need remaining, if you can raise the other half." And then the Air Force came out and we said, "Well, we need another \$250 K." And they said, "You can have \$125 K if you can find the other \$125 K." Then the Fleischmann Foundation, which we applied to said, "Well, you can have half of that, if you can find the other half." It was like a so-called geometric series. It was a half, then a quarter, plus an eighth, plus a sixteenth-- Which has the characteristic that when summed over an infinite number of terms does

reach one. We were within \$20,000 of what had been said we had to have when we applied to the US Steel Foundation. The then President of the US Steel Foundation, who was, I think, Bob Knox's father; Bob Knox, was at Scripps. He came by, very, very politely, saying that he wanted to tell us in person that they couldn't do that, that it wasn't their style to do bricks and mortar. Instead of writing this he came all the way to tell us. We had lunch down at the snack bar. I was very disappointed, because I thought we were finished with that phase. He had come here with the idea that it couldn't possibly make any difference whether they gave us \$20,000 or not. We told him the story, like I told you now, and he took the train up to Los Angeles and decided to change his mind, and he'd give us the \$20,000. Within two weeks we were set to go. It really ended that long road.

Doel: That's interesting.

MUNK: Then we had this very interesting phase of doing the design, with three of us, really, at the time: the head architect and Judy and I meeting once a week. When all the decisions were made, the Regents, of course, appointed an architect, but we had chosen one. We chose Lloyd Ruocco. Lloyd Ruocco was a local architect. He'd never done a laboratory building. We thought that was a great asset. We didn't want to design around plumbing works, which is the usual way you build a laboratory, by starting with plumbing works. We had a consultant on how to put in the plumbing works and the wiring and all the other services. The third time we met, we were three weeks into the game, the service consultant appeared at our conference with a set of drawings that nobody had asked him to do.

Doel: This is Ruocco?

MUNK: No, this is the consultant for services. He had taken it on himself to design a building. There was no design yet. And it was a cement box built around the wires, and the plumbing works and the others. The head university architect, with whom we got along very well, turned to him and said, "Who asked you to do that?" He said, "Well, we all know that that's the way you build a laboratory." And I said, "Well, we'd like to do a little better." The man got very annoyed, and he said, "Well, who makes the decisions around here?" Then our head architect said, very nicely, "Well, in the final analysis, Judith does." The man got very angry; almost walked out. But, as you can see, we eventually had our way.

Doel: Indeed. I was wondering, too, were you consciously thinking about, how to facilitate communication among individuals in terms of architecture? How to get different disciplines integrated?

MUNK: Yes.

Doel: What things seemed most important?

MUNK: To be a place where the people enjoy being. So it wasn't a matter that you went from a nice home to a cold office. Getting carpets, for example, at the time was unheard of. We mentioned that we wanted carpets in our offices, and we were told that only the President of the university could rate a carpet in his office, that it was against the rules. So we called it "acoustic floor covering" and got away with it. We had a wonderful double entry table of sources and sinks for the money. What did we need? Where did it come from? The Fleischmann Foundation money was the freest of all. They didn't give a hang. So we put the 'acoustic floor' covering onto the Fleischmann column. Etc., etc. We were very concerned about laboratories where you could assemble things, as we had said before. That was a key issue. The seminar room was intended to be really attractive. You know our seminar room that's overhanging the ocean?

Doel: Yes.

MUNK: It still is really a wonderful place to have things. But we thought about such details as a blackboard that wraps around the room, instead of having sharp corners. I had just come back from Russia where I heard some lectures where people had a small board that wiggled. Every five minutes you'd have to wipe it out and start again, and nobody remembered what had been done before. I said, "Well, let's have a board long enough so you could develop a whole lecture, so you could come back and show them that this is where you had made that decision. It's so easy to do. Why not make it possible? The additional cost of a good board is not a major consideration. The curved solution was a Ruocco invention.

Doel: This was right after your 1962 trip to the Soviet Union?

MUNK: Yes. And even having enough space for chalks to be able to lie there instead of falling on the ground, and making a mess. So we built our own chalk tray that was four inches and had a decent lip. There was a lot of thought, and a lot of details. And yet it was a very inexpensive building. You realize that it was by far the cheapest building on this campus. It was built for \$20 an assignable square foot, at a time when the Salk Institute, built at the same time, was \$120 plus.

Doel: That's quite a difference.

MUNK: Quite a difference. No unnecessary fancy things, but very good design. One key element was that Lloyd Ruocco, who had never built a laboratory before, he originally turned us down when offered the architectural contract, because he was afraid of the university bureaucracy. He said, "I'm not able to stand up to that. I've never worked with big groups." We said, "Well, we will run protection for you. You don't have to worry about that." Then he came in with some design, and he said, "That's not a very good way, but the university will never accept it if we do it in a better way." We said, "What is a better way?" And he would talk about it. But he said, "It's impossible for the university to accept that." So Judy said to him, "Well,

would you promise us the first time through to do things the way you think is best, regardless of whether you think it has a chance to be acceptable to the university? Then we can back-track if necessary." Apparently it's part of the ethics of an architect that he's willing to go through more than one set of drawings, that he is supposed to be willing to make one set after the other. So he agreed. Then we would meet again, and he did something that didn't seem quite right, and we said, "Why did you do that?" "Well, the university wouldn't accept the right way of doing it." We said, "Lloyd, you have agreed, first time through with no limits, you do what you think is best." We finished that way. And the bid came in on the money. There was never anything that had to be given up. A wonderful story. You shouldn't give up on what you want to do because you think it would be too expensive, that there would be people objecting to it.

Doel: Were there other things that he didn't think possible?

MUNK: Oh, many things. And every time we had to remind him, "You do it how you would do it in the ideal world, and forget about the fact that it's not an ideal world."

Doel: What else was it that he didn't think possible?

MUNK: I'm going to have a little problem. We should ask Judy. There were a number of things on that score.

Doel: Things like the redwood?

MUNK: And we designed our own furniture. You know, the furniture in each office was home-designed. The round table that had the same curvature as the blackboard in the seminar room, was home-designed by Lloyd. There were all sorts of things that would seem off-hand to be too expensive, but were not. I had only one problem. When it was almost finished and we were ready to move in we had gotten along with Lloyd very well. He had in fact given us a little fireplace for the seminar room as a gift because he had such a good time. I asked that we put in some curtains made out of tapa cloth that Hugh Bradner had picked up in the Tonga Islands, for \$20 I think. Big, huge, tapa cloth. Very unusual. Lloyd said he didn't like it, he didn't want to have it. I said, "Why are you against that?" And he said, "It will make it look like a South Pacific whorehouse." I said, "Ah, that was exactly the effect we were trying to achieve." And it turned out everybody enjoyed the South Pacific whorehouse atmosphere.

Doel: I suspect you're thinking now of coming back from Samoa in '63?

MUNK: Yes. It was a real adventure, anyhow. Building that building is part of my life, certainly. And building the new Revelle laboratories which we should give a little time to. That was, how many, thirty years later? And I think equally successful, but different.

Doel: We do need to cover that when we get to the later periods. Wasn't there debate whether the IGPP should be located at Scripps or on the upper campus, that Vice President Wellman had thought it needed to be elsewhere?

MUNK: Yes. He wanted a more interesting place than down below. There was never really a great deal of opposition. As I mentioned to you, nobody really wanted the area that we picked. They thought we were extremely foolish. First of all, it was a difficult site—very steep, which they thought would add too much to the cost. And secondly, too remote from the rest of Scripps. Which it was. But, it is no longer.

Doel: Was it a question of locating it somewhere else at Scripps, or putting it up into the main campus?

MUNK: I don't recall that being a serious problem. But we chose the site. And the administration at La Jolla, Roger Revelle and others backed us. They said do what you want. The idea of building it out of wood was turned down, by the campus architects, they wanted us to build a much more standard building. That did come up. At one time we actually said we would return the money; we didn't want to be associated with it. It was a bit of a showdown. It was meant, it was not a bluff. And I guess the universities are not very good at turning money down, and we won that. But there was a time when it was said that we must not do that. That came up again on the new Revelle laboratory, and you must remind me to tell you about the second time, when they said we couldn't do that for various reasons.

Doel: I just made a note to make sure that we do cover that. How did you come to know Robert Orr Roberts, when you first made the approach to Fleischmann? How long had you known him?

MUNK: Gosh, I knew him, and I thought everybody knew him. But he was generally a person who was just helpful, to people who wanted to do things. And well, wasn't he at the time working at Boulder?

Doel: With the creation of NCAR [National Center for Atmospheric Research]?

MUNK: NCAR—wasn't that his creation? I had met him through that, I think. And you know the Fleischmann Foundation was a very curious foundation, with just a few people meeting in some upstairs little room, in Nevada, making their own decisions. And they kind of liked people who tried to do something different.

Doel: How long had Walter Orr Roberts been on their Board? Had this been a long association?

MUNK: I don't know.

Doel: Did you know him well? Did he talk about what he was doing in the 1950s, his relationship with Harvard, and some of the political difficulties that he had?

MUNK: I think so. I only remember knowing him, and having fun talking to him, and then coming to him and saying to him is there a chance we can get some help with building a laboratory.

Doel: Yes.

MUNK: No very close association, but a very pleasant one.

Doel: Right. As you were developing the IGPP, were people like Urey and Jim Arnold interested? Did they want to have a hand in thinking about how the Institute would look?

MUNK: Oh, the architecture?

Doel: Or the kinds of problems that would be addressed.

MUNK: I think yes. But Jim and Harold Urey had their own group, a very effective group. At one time having joint appointments with departments at UCSD was a key issue, a very good idea. And we had joint appointments with many of the people. You see, I have not thought about this so much lately. Urey was always a help. He was a very positive man. He was on some kind of a state board for us. Jim Arnold was always very helpful. But not really part of the basic development. When we had lots of space initially we housed a few of the upper campus astronomers. Margaret and Geoffrey Burbidge stayed at the IGPP for some years, until we had to throw them out because we were getting full. And they never-- Geoff never forgave me for that.

Doel: That was part of his personality, though, wasn't it?

MUNK: Yes. We had lots of visitors from abroad. Our relation, especially to the Cambridge Group, was always close. Through Teddy Bullard, and Bob Parker, who came at an early time, and is a product of Cambridge. The relation between what is now the Bullard Laboratory and our Institute has been almost that of two sister institutions.

Doel: I sense that the question of bringing in visitors was always important for the way that you could see the Institute operating.

MUNK: Well, Cecil Green made a gift, to make that easy. And we should go over that. You need to establish the year of that gift. We had just gotten to know him. He had retired to La Jolla. He had asked me once to give some lectures at TI [Texas Instruments] to some future employees. He did that with many people, including at one time Maurice Ewing. Ewing and I shared a week of talking to their summer students who came there. Later on many of them

became employees of Texas Instruments. He, Cecil, came here and he helped us build our house. The drainage of our patio was dug by him and me with picks and shovels. He would come by and he would enjoy doing that.

Doel: This is very interesting. You're referring to the area just outside the doorway, towards the patio maybe thirty feet from where we are sitting?

MUNK: Yes. Yes. Very helpful. That goes back. Then when we had finished IGPP he and Ida came by, and said he had just finished the Green Building at MIT, which is 19 stories high. We were 19 rooms horizontal. Same dimension, but different directions.

Doel: Did you feel that you wanted to try to keep things reasonably on one floor as possible?

MUNK: Yes. We thought that a skyscraper at MIT, architecturally, has always been a hindrance rather than a help.

Doel: Communication was limited by the floors?

MUNK: By the floors. It was sort of a two-floor mixing measure, because there are alternative male and female johns at even and odd floors. But otherwise, the elevators are inadequate, and I don't think it was a very friendly design. Cecil came by one day when we were nearly finished and said he liked it.

Doel: Liked "it" being--?

MUNK: Us, IGPP.

Doel: IGPP.

MUNK: And could he give us a gift? Judy, from the very earliest time, had planned to have a sculpture by Donal Hord called "Spring Stirring," a big diorite, to go outside of the seminar room. So Judy said, "Yes, give us 'Spring Stirring.'" It was located in the garden of the sculptor who lived in Pacific Beach. It was late evening, and the sculptor, Donal Hord, had gone to sleep. We drove down, and using a flashlight, we climbed across his fence so Cecil and Ida could look at "Spring Stirring." Judy was very close to Donal Hord. He did the two carved panels on the walls behind you.

Doel: Right in the room where we're talking? Yes.

MUNK: And Cecil said, "Yes, I like it. I'll give that to you." The cost was \$16,000, for the diorite. It has no connection to geophysics whatsoever. People who come and visit always try and figure out why did we get that statue. The only reason was because Judy liked it.

Doel: But this is an interesting picture, too, all of you climbing over the fence to see what it looked like.

MUNK: Have you seen it? Do you recall the statue at all?

Doel: I don't think I have.

MUNK: Let's look at it sometime. Then we had a big debate of how to base it. We thought putting it on the ground was too informal. Putting it on a pile of stone made it look too much like a graveyard. So we got a stone out of the same San Diego County Quarry from which this diorite came, and made a bowl which was filled with water, in which the statue still sits, so it's sort of separated from the ground in an interesting way. That statue is quite well known in San Diego. People come out to see it. I mean, it's in certain catalogs. It's one of the known statues of San Diego. And Donal Hord died three years later, and in his will had willed the statue to Judy, so we would have had it anyhow. Whether we would have been sufficiently unselfish to give it to IGPP, I don't know. But, it would have-- Anyway, that's been sort of our theme mark. And when we first opened IGPP we filled that bowl with champagne. And I remember Harold Urey getting quite high.

Doel: He sampled the water, so to speak.

MUNK: He sampled the water.

Doel: That's very interesting.

MUNK: So, that's part of the history of our Institute.

Doel: You mentioned that Cecil Green also endowed the visiting opportunities?

MUNK: Yes. A few months later, I was in my office at IGPP. He came by and said, "Do you have time to see me?" I said, "Of course." He said, "Would you like it if I made an endowment to the Institute?" "I have in mind," said Cecil, "that it should be for visitors who come for no less than two months and no more than two years." They should not be a substitute for the university supporting permanent staff." That was the given. He had done some similar things elsewhere. And as you can imagine, we were happily willing to accept that gift. Then he started what is now the Green Foundation, with a \$600,000 gift. But it almost wasn't accepted. He had looked into the performance of the University Endowment Fund, and had decided they hadn't done very well. And he said, "The condition of the gift is that you start your own-- that you have your own foundation, which will administer the funds." The then Treasurer of the University of California at Berkeley, statewide, Mr. Hammond, advised against accepting it, because he viewed it as a criticism of the endowment fund, which of course it was. We had a fatal—fatal isn't

perhaps the right word—a meeting to which the Chairman of the Regents, who happened to be a San Diegan, deWitt-Higgs, came, and Cecil, and Cecil's lawyer, and Bill McGill.

Doel: McGill, then Chancellor?

MUNK: Then Chancellor.

Doel: So this is clearly then 1968, 1969?

MUNK: 1969, I guess that's when McGill was Chancellor—in which the terms of the gift were stated by Cecil's lawyer. Hammond formally suggested that we should not accept it because some other people might think that they should give money that would not be administered by the UC Foundation. To which Higgs said, "Well, that may not be such a bad idea." And that determined that we started that foundation. It was really hanging on a thread.

Doel: That must have been quite a day.

MUNK: Yes. Then Ida gave some more money. We now have about \$2 million. It's been a God-send. We use some of that money to bring people in. People have alternated between being world-famous scientists and people who just got their degree. We deliberately make that mix. We think it's a good mix. And almost anybody who is anybody in geophysics in the world has at one time been a green scholar. There must be now thirty or forty of them. You will see their names when you go to our building. It made it possible, in some ways, to be a small institute without being insular.

Doel: That was a critical part of the design from the beginning, wasn't it? If you kept it at twelve people or so, you kept the group intellectually vigorous by bringing in a constant stream of visitors?

MUNK: Yes. We've had people like Sir Edward Bullard and Tuzo Wilson on one hand, and new graduate students on the other hand. And We've been very, very careful not to use the money for giving parties and things. It's been very carefully administered.

[Tape 2, Side A]

Doel: One thing I was curious about: how did you first meet Cecil Green?

MUNK: He would invite people to come to Dallas to talk to the summer students. I don't know if I had met him before then, but somebody had said something about me or something. I mentioned I was together with Maurice Ewing there one year. And then by accident he and Ida decided to spend some time here and bought a house in South La Jolla. I think he was a little bored. He'd come up and help Judy and me build this house.

Doel: This was soon after his retirement then, from active participation in Texas Instruments?

MUNK: Yes.

Doel: You came to know him fairly well. Did you come to know him quickly or did that grow over time?

MUNK: It grew over time. I'm sorry about his present deafness; it has made it very difficult to be close. It's really a terrible impairment.

Doel: As We've mentioned off tape, he's now 96.

MUNK: Yes. We know him well, and love him dearly.

Doel: Has there been other individual philanthropy in the Earth sciences, that has had the influence that Cecil and Ida Green have had?

MUNK: In a very different way, David Packard. I'm on the Board of the Monterey Bay Aquarium Research Institute, MBARI, that David started. A year before it started, for twelve years now. I got to know David Packard quite well, he died a year and a half ago. He's very different from Cecil, but has done a great deal with his very extensive fortune. Both he and Cecil have certain things in common that I found surprising. One is how very much concerned they are about details. You wouldn't think that if Cecil gave a luncheon that he would worry as to exactly what would be served and how the table would be set, but he was. He is. And David wants to make sure that things are the way he thinks they should be. They both are very, very thoughtful of people. They never have too little time to be concerned with people who are associated with them. Cecil and Ida were just wonderful. Their friends could always count on them. When my daughter Edie first became interested in television, he arranged a job for her in Dallas. It was typical.

Doel: That's very interesting. We'll cover clearly more of Cecil's influence, and Packard's influence, later on in the interview. I want to make sure that we cover issues in the 1940s that we haven't yet had a chance to address. One matter we haven't spoken about was the series of grants from the California, and the US Fish and Wildlife Service to Scripps, soon after the War ended for development of the biology program. How important was that for the development of Scripps?

MUNK: Very. That still happened under Sverdrup. I remember Harald Sverdrup telling me about that offer. As you know, it was closely tied-in to the disappearance of the sardines. [Robert] Sproul offered him the two ships, and Harald Sverdrup then decided that the one person on the Scripps staff who knew something about ships, namely Roger Revelle, should be in charge

of that investigation. Roger was still in Norway I think, on a post-doc. Oh no. What was the year of that?

Doel: This came in 1946 or '47.

MUNK: No. Roger was in the Navy then.

Doel: When he was leading the ONR program?

MUNK: Yes. Harald asked him to come home and run the program. It was very important. It was a change--It was sort of step three. Step one had been when we had virtually no ships at all, except some little thing they could work in San Diego Bay. And then step two was the *E.W. Scripps*, the schooner, that made it possible to go into Baja, California, and into the Gulf of California, and things like that. And then step three was the *Horizon* and the *Baird*, which was the beginning of doing Pacific Ocean work.

Doel: Were there any individuals at Scripps who felt this was an inappropriate development for the Institution?

MUNK: I'm sure, because Scripps was always very divisive, and almost anything that was suggested by the Director had its detractors. It's never been an Institute that pulled together very well, I think. And I'm very sensitive to it now.

Doel: Did there always seem to be a collection of different fiefdoms?

MUNK: Yes, that's a good word. The people who worked here were laboratory people, not oceanographers, and Harald Sverdrup was trying very hard to make it an oceanographic institute. The possibility of getting two boats was central to that effort. But most of the people who were here were not very interested in doing sea-going work. So it was not met with jubilation.

Doel: Was Sverdrup regarded as an administrator?

MUNK: I don't think so. He had an open-door every I think Thursday afternoon, when people could come see him. That's about all the time that he really spent sort of being the Director, and then he wasn't... Otherwise he did a lot of his own work, and was very efficient. But it was so small then, you see, that they were colleagues. No, I don't think anyone thought of him as an administrator.

Doel: That's interesting. One thing I meant to ask you before, did you and he always speak in English when you were here, or were there times when German was an easier language to use?

MUNK: No. No. For neither him nor me. I always spoke English. I was brought up by an English nanny. I never had a problem. No, the idea-- no.

Doel: I'm also thinking of your travels in Russia for instance, when occasionally German seemed to be an easier language.

MUNK: Yes, I remember one occasion when we went to a party in Soviet Albania, and a old man at the party had been a prisoner of war in Germany. He and I had one word in common, which was adequate for our conversation all evening, and the word was *scheiss*.

Doel: Yes, I know it.

MUNK: A very descriptive word.

Doel: But it conveys.

MUNK: It conveys.

Doel: One important development that we really haven't covered yet, is your involvement of the Bikini nuclear tests, and Revelle's role in that effort. What's the first recollection you have of discussions about taking part?

MUNK: I remember distinctly Roger calling me into his office saying he wanted me to meet Captain so-and-so, a Navy Captain, who was talking about an event that was forthcoming that would lead to a great shock to some coral reefs, a physical shock. I think it was really more dramatic. I asked him what was the probability that the Island of Bikini would be subjected to some experiment, and he said, "One." I remember that probability. That was the first I knew about the tests.

Doel: Of course, it was classified at that time of the discussion, wasn't it?

MUNK: That was classified, but of course everybody had known-- I mean, the existence of an atomic bomb was known then. The test of doing it under water was, I think secret. No, I think the whole thing at the time was classified. I don't quite recall to what extent Bikini was classified. The fact that the test took place, I think, was not classified.

Doel: That's correct, it was public.

MUNK: But the details of the nuclear devices... There was a Bikini ABLE and Bikini BAKER. Bikini ABEL was shallow, Bikini BAKER was sea-floor, which if I remember right, was 140 feet or so. Sea-floor meaning the floor of the lagoon bottom. Bill von Arx and I were assigned the job of measuring the circulation in and out of Bikini Lagoon, and estimating how soon it

would be after the explosion that one could get inside there without danger, and what was the flushing rate. We had ten days to do it, or something incredibly small, because it hadn't been planned adequately ahead of time. We decided that Bikini Lagoon was about, oh I don't know, 20 miles on the side. It's not big, but it's not small. And you certainly can't walk around it. It has many entrances and exits. So we managed to get a hold of a little airplane with a pilot, a Navy plane, and we would drop dye markers into the openings and closings of the Lagoon, and photograph them for a tidal cycle, to try to estimate how much net mass of water was coming in and out of the openings. There's something like a dozen openings, of which the biggest one is called Enyu channel, which we left to the last. And so we would fly and work about two or three of them on each flight. I was the bombardier. We used very intensive dye markers that were given to people in life rafts, so that you could find them when they were downed at sea. And we built our own little bomb sight, and learned how to drop them into the middle of the openings. It was fun.

Doel: You had to learn that quickly if you only had ten days to get this work done.

MUNK: Very quickly. It was an art. We then could see how it would float and integrate over the tidal cycle. The thing I remember about it is that we had done eleven out of twelve openings, and they all integrated to a net in-flow, and we were getting worried, because of the conservation of mass principle. We were hoping that on the last flight of Enyu we would get a net out-flow. Otherwise we didn't know what we would do. We couldn't propose that our measurements show that there's in-flow every place. And on the last day we did Enyu and it turned out to be an opt flow. Things balanced.

Doel: Were you worried about the measurements not being accurate? How much mapping had been done of this region?

MUNK: The maximum, but nothing on the flow. Nothing about the circulation. So we did eventually get a picture of an out-flow, and I think it's the southeast corner and in-flow every place else. But I don't know what we would have done if we had measured an in-flow every place. We couldn't have contended that that was correct. It was a very, very exciting time. I remember that in spite of all of our attempts to handle that green dye—do you know what it's called? Very intensive dye? Even with great care, it got all over everything.

Doel: You had to be very careful in using it, as I recall.

MUNK: Very careful, it got all over our pants. And I would come home and put my-- clothes into the ship's laundry. The ship's name was *Sumner*. It was a survey ship. And a few days after we started, we were invited to cocktails by the Captain, whose name I recall, Ciano, who wore dresswhites. As we came in to have a cocktail with him he looked at his dresswhites and said, "I don't know what's wrong with the ship's laundry, but my dresswhites are coming back with a greenish color." I didn't have the courage to tell him that I had put in my clothes--

sometimes, with a little bit of greenish dye, which apparently had spread. All the uniforms on the ship had acquired a greenish color.

Doel: That's interesting.

MUNK: So, we did that.

Doel: Did you feel you were getting all the logistical support that you needed to do those investigations?

MUNK: No. I remember we tried very hard to get some help, and Captain Ciano said that his full compliment was 120 people and he only had 118. So he couldn't spare two people to help us. We finally commandeered a little motor boat with one sailor on it, and we always had one person to help us. Other than two out of 120. So, the answer is no, but we managed to get our job done.

Doel: Part of being successful, it seems, in those kinds of operations is figuring out how to make do despite such impediments.

MUNK: Very important. I think the key to oceanography at that time, maybe less so now, was that you learn to improvise. Always. Nothing ever quite worked out as planned. Improvisation is a way of life. Being able to manage under unexpected circumstances was part of the challenge.

Doel: It's clear that those tests involved biological as well as geophysical investigations. Did you feel that there was a growing integration of results that the tests helped to inspire?

MUNK: Yes. Roger saw to that. He was in charge of the geophysical and biological work. His interest in radiological effects comes out of this era. You know, he wrote about that, and became very knowledgeable. He was always an integrator of different fields. There were many adventures. We don't have the time to talk about them all.

Doel: What stands out in your mind, though, as a particular adventure? Or at least a really good example of what you're thinking of?

MUNK: Well, I gave you one, and that was getting the circulation of Bikini Lagoon in twelve days. After the explosion-- oh, that is a different story. That is a different nuclear explosion. That was MIKE shot.

Doel: In 1952?

MUNK: In 1952, when our ships got rained upon by radioactive rain. Have we talked about that?

Doel: We haven't, this might be a good time to do that.

MUNK: That was a big operation. There were many Navy ships there. We were the only civilian ship. An interesting story is that after the test they were all sent one direction and we were sent the other. I still don't know what was behind it. But a few hours after the operation we were rained upon. This was on the *Horizon*. And she never again could be used subsequently for research work that required low levels of background activity. We had an Air Force Captain along whose job it was to be radiological safety officer.

Doel: This was on board the *Horizon*?

MUNK: On board the *Horizon*. After we pulled out, he would come by every fifteen minutes and stick his geiger counters into our guts and say, "Pardon me, but that's my job. It reads zero." Then it rained, and he came by the bridge. I was in the bridge and he stuck it in, and he said, "My God, that thing isn't working." Because it was going.

Doel: It was making too many clicks?

MUNK: It was making too many clicks. It turned out that we had been rained upon radioactively. We all took our clothes off and threw them overboard, and washed down. I am not sure whether we were damaged at the time. Certainly the ship was damaged badly in the way of radioactive contamination. I have always wondered whether it had a health affect on the rest of us. I had a thyroid operation about two years ago, and Judy felt it might go back to that stage.

Doel: This was for a melanoma, or something like that?

MUNK: Well, I had my thyroid removed. But we all decided that that was not something to sue over. We probably could have sued for damages, but didn't do it.

Doel: Were there others who died of cancers who were a part of that crew?

MUNK: I don't know. But there was a discussion as to whether we had had some-- whether this had some mal affect. It was a serious contamination.

Doel: Was this something you were concerned with at that time? Did you recognize how significant that was when it happened, or was it much later?

MUNK: No. No, no. It was in line with my usual way of not worrying too much about it. No, not at all. That was the other story that I should tell you about. It was the subsequent test. We were worried about setting off a tidal wave, a tsunami, by the H-Bomb. That was a very dramatic few months of our lives, and it's worth telling. I think I told a short version of it in my reminiscence.

Doel: You have a version in the reminiscences. The comment you just made about how intense this period was in your life: what came back to mind as you said that?

MUNK: Well, going to a South Pacific Island and trying to figure something out, and the connection with nuclear work. All that is exciting. I found very exciting. And people worked well together. I mean, that was an aspect of it, that made it enjoyable.

Doel: We're being joined by Judy Munk. The comradery was--?

MUNK: It was fine. But there was friction sometimes between people in uniform and outside. It is not easy to work on a Navy ship as a civilian. And if you had an executive officer who was not interested in making it work, he could make your life very unpleasant.

Doel: As in the Bikini test that you're thinking about?

MUNK: Well, yes. But it's always been that way. You have to make an effort to get along. And some people never quite succeed. And I felt generally I've managed pretty well to have a successful time, and make friends rather than enemies.

Doel: What made for a successful operation of that sort? What had to happen for you to succeed?

MUNK: To get the data, and if it makes sense. In that case, we published the flushing time of the Bikini's Lagoon, I forget, in about two weeks or so, and made some estimates as to what would happen to the contamination. It was useful, and it was probably approximately correct.

Doel: When you look back on it, were there other things that you felt you needed to do in order to make that interface between science and the Navy-run ships work?

MUNK: Just get along. There are stories. I've always felt that Scripps scientists get along very well with the members of the crew, and that was one of the nice things about our life. Then, unfortunately, a sociologist made a study of that phenomenon, and it was very upsetting. He told me that I was all wet. He would ask some specific questions like, "You have two sittings for dinner. One at five and one at five forty-five, on Scripps ships. Can you tell me whether the crew and the scientists mix well?" And the answer is no. The crew sits 90% at the early sitting, and the scientists on the (still-too-early) next sitting. And he said, "Well, when you all got into Tahiti," and there was this wonderful bar right next to the docks—there were two bars—he said, "Did you all come and sit together?" Well, it turned out that scientists went mostly to one bar and the crew members were at the other bar. I remember another question was, "You said you enjoyed the crew so much. How many of them have come to your house for dinner after you got home?" So he's written a paper saying that that's an illusion by scientists on oceanographic

vessels. In fact, according to his numbers, which you cannot dispute, it doesn't work out so well. But I still think it worked out well.

Doel: How universal was that experience? I'm thinking of certain ships, which also kept the crew eating at different times and their schedules separate from those of the scientists as well as those of the officers. It would seem that that was more common than not among oceanographic vessels. Was that your experience?

MUNK: Well, I know it does not work on Soviet ships, where they had actually different messes for the upper level scientists, the technicians, the sailors. Really separate messes, like on some of our Navy ships. I was amazed that in a communist country the separation was so enormous. One time when we had the head Chinese oceanographers, T.K. Tseng, and the head Russian oceanographer (I've forgotten who it was, maybe [Andre] Monin) Roger and I had a discussion at lunch here. As an exercise in sociology we asked them to estimate the ratio of his salary as Director to the janitor in his institution, and where were the ratios highest. And you can imagine the answer. The ratios were high for China and Russia, and very low for the United States. Four to one for us. Thirty to one for the others. An amazing social situation.

Doel: That must have lead to an interesting conversation.

MUNK: Yes.

Doel: I'm struck by your observation about the kind of segregation that was occurring even among the scientific crew in the Soviet ships.

MUNK: Isn't it amazing. Judy and I once joined a ship somewhere, and the Chief Scientist had his own mess, and invited us. That was imposed. Someone who studies this kind of sociology should talk about that.

Doel: Did you have a sense of who made these decisions? Was it the scientific community, or was it imposed from outside?

MUNK: Gosh, I have a feeling that this maybe an ancient tradition. I don't know. I'd like to learn about it.

Doel: One could imagine very different dynamics occurring when scientists can interact with the engineers and the technicians on board. I would imagine that on the Scripps voyages that was important.

MUNK: I always thought it was pretty good, but I thought I had to tell you about the only sociological study I know of, which found otherwise.

Doel: An interesting observation. Did Roger Revelle talk to you much during the time that he was director of science research at ONR about what he particularly wanted to see happen? The goals he had in mind?

MUNK: I saw a lot of him. I don't remember having directed talks about these things. He certainly was anxious to get back and get Scripps out to sea. One of the references I gave you about the Revelle years talks a little about that. I knew him well, and you know, I would stay at his house. We'd be together. I don't recall some directed conversations to which way should we go.

[Tape 2, Side B]

Doel: What about particular examples of developments that he saw as very positive that he wanted to support?

MUNK: Well, he wanted to do better work with biology at Scripps. He felt that our biologists were not participating in the, even then, major developments of instrumentation, of measurements theory. He tried to solve the problem by getting some money. He got a million dollars from the Rockefeller Foundation, which was a lot of money then.

Doel: That certainly is. This is back around 1953?

MUNK: You know about that. He tried to use that to extend in a thrust sort of way to our biological work. And failed. I think he came to the conclusion that one shouldn't try and upgrade an existing group; it's better to start a new one.

Doel: That's very interesting. Why did it fail, do you think?

MUNK: Because people can spend any amount of money. No matter how much lack of imagination they have, they seem to be able to do that. The group of biologists we had then really hadn't kept up with other developments, and my theory is that they didn't want to do anything new. They wanted to do more of what they were already doing. Scripps wasn't on a very high level of science. Eventually, his idea, I think, became that you start new groups with new exciting young people, and let the other guys do what they want to do.

Doel: Did the biologists have visitors coming in?

MUNK: Oh, I'm sure.

Doel: I was wondering if they had a similar arrangement as what you were trying to do on the geophysical side.

MUNK: I don't think so, no. I think that the Green Foundation gift was the first one that was identified. But Roger asked an Italian geneticist, Adriano Buzzati, to come at the time, with the idea that that would perhaps get some modern genetics principles going. But it was a failure. Adriano left thinking that it just didn't work.

Doel: That he couldn't stimulate the kind of work he wanted to see done here?

MUNK: I think one of the reasons for the enormous opposition to Roger-- you know with ten out of twelve faculty members writing a letter to the President, blackballing Roger--had to do with the fact that they were worried about their own position. They must have sensed, perhaps without admitting it to themselves, that the field had developed and that they had not participated. And they were afraid, rightfully so, that Roger would try to do something about it.

Doel: Did Roger talk to you about the Rockefeller Foundation's own reaction to the development? People like Warren Weaver were clearly quite interested in how these grants worked. I'm wondering if you recall hearing about the Foundation's reaction?

MUNK: I don't remember anything specific. I got to know Buzzati quite well. I knew Roger well, and I know about his hopes. But I can't tell you anything that I remember about the Foundation gift itself.

Doel: But as you say the one million dollar grant for marine biology, at that period of time, was major--?

MUNK: Enormous.

Doel: Were there any efforts made to try to link the Scripps group with other university groups, or was it kept fairly insular?

MUNK: Well, you remember that Roger said, at one time, that unless an oceanographic institute was closely associated with a nearby university, it would not be a good institute. In some ways, the whole starting of UCSD was related to that feeling. Of course then, the attempt was made to have our Scripps departments have close relations with the corresponding upper campus departments. And IGPP, in some ways, was probably more successful than others in achieving that. I think that that was less successful for the biologists.

Doel: In part because of the kind of work that was being done?

MUNK: I think so.

Doel: ...and personalities, perhaps? Zobel, Fox, others? Did you know any of them particularly well?

MUNK: I knew them all. I knew Zobel, but had little to do with his work. I knew Sumner, he was on my thesis committee, who taught me something about diurnal migration of copepods, which became acoustically important many years later. I knew Carl Hubbs, who was nice to be around. He took me on one of his field trips into Baja, California. And Dennis Fox, but not professionally really. That's sort of the group. Am I missing someone?

Doel: Those are key players. And John Isaacs.

MUNK: And John Isaacs. Well, John, I was much closer to. But John was in all fields. John is worthy of a separate discussion.

Doel: What sort of person was he, as you look back?

MUNK: He and Alan Vine were the two people who always came up with mad ideas.

Doel: What was the maddest idea that comes to mind?

MUNK: Oh, that cars driving on the right side of the street create cyclonic circulation in the atmosphere. But John is responsible for the main out-fall from our sardine work.

Doel: I'd like to hear how that came out.

MUNK: Are you aware of that? The main idea we pursued for many years, namely, why had the sardines disappeared, was the wrong question. The right question was why had they been plentiful for twenty years? Because when you looked into the paleo records, you found that there were very few time periods when the sardines were plentiful, and we had asked the wrong question. That was his work. I think that's an important conclusion. He, for example, invented the space hook, do you remember that? If you have a satellite, which rotates at the same rate as the Earth spins, if you then force it to be a little further away, it would go slower. If you keep it connected to the Earth and force it to go at a higher rate of rotation than it would normally do then it will pull upwards. That idea has come back in the last few years in connection with how to build larger antennas on satellites. So, he was a genius, John Isaacs. I wouldn't regard him as being a biologist because he was everything. You have heard about him?

Doel: I've heard some, and I was particularly interested here in your impressions, and how important people with these kinds of diverse backgrounds actually were for Scripps in this period.

MUNK: Very important if you don't have too many of them. I don't think you want an institution with twelve John Isaacs. But one is great. He kept on throwing everybody off by asking questions, and always starting with saying, "Of course you know all about this, and it's

probably a very naive question.” Then he'd follow with something incredibly deep and profound, which you didn't know how to answer.

Doel: Anything particular come to mind when you think of that, that he would ask?

MUNK: Oh gosh, it happened all the time.

Doel: I'm sure it did.

MUNK: I need to think about that.

Doel: Sure. I believe I had heard one proposal, I think attributed to John Isaacs, of towing icebergs down from the Arctic to supply water to human populations, such as Los Angeles. That was his idea?

MUNK: You mean Antarctic. That was his idea. It may have happened-- it may have had a little bit of literature. I seem to remember someone finding eventually that it had been-- it was his idea. It happened in an interesting way. John Isaacs started out thinking about running water through a pipe, from regions of much water fall to the other, and he was thinking of a pipe that's under the oceans. Then he asked himself from an economic point of view what should be the diameter of the pipe? And he found the bigger the better. And eventually he wound up pursuing this idea of capturing an iceberg and floating it in. And as you know, it was actually later on supported by the Saudi Arabians. But it also turned out very much later to be physically wrong. The melting mechanism of an iceberg floating in sea water is subject to some very interesting physics called double diffusive mixing—a field that didn't exist then, and it's very different from anything that we at the time had known. So I think it's probably impossible to do that well.

Doel: The role that Isaacs was playing was a spark plug with ideas?

MUNK: Oh. and more than that. You know, Roger had great affection and confidence in him. Roger asked Isaacs to be his principal assistant at one time, at Scripps. It was Roger's attempt to get Scripps more experimentally-oriented. And he tried to get John to do it. He appointed him in charge of “Special Projects.” That was supposed to help people develop the instrumentation. John worked with a man named Jim Snodgrass, there being several Snodgrasses, in doing that. And he and Jim wrote a paper on whistlers. Whistlers are whenever you have lightning in the Amazon Valley you get a radio signal. So, you can see how broadly he was interested. His widow lives a few houses down from here. Mary Carol Isaacs.

Doel: What was Isaacs' training?

MUNK: Engineering. He was a product of Dean O'Brien. M.P. O'Brien, at Berkeley. A man with whom Roger had very close connections. A very influential engineer during World War II.

And in some ways, the father of beach processes, erosion. John came out of that school, together with other people like a man named Folsom. Also Putnam, Johnson.

Doel: You had worked with Putnam early in your career, as I recall?

MUNK: Yes. I knew the whole group. But when I started the internal wave work, we had a relationship. M.P. O'Brien was sort of the leader of it all. A wonderful man. And as I said, close to Roger. A great supporter of the Scripps Institution. Isaacs came out of that group.

Doel: Interesting. Were there others who came out from that group who had a similar career to John Isaacs?

MUNK: Bascom.

Doel: Bill Bascom?

MUNK: Bill Bascom, yes.

Doel: When you mentioned biologists, I was thinking about a 1952 joint paper that you did with Gordon Riley on how plants get their nutrition. How did that come about?

MUNK: Well, Gordon was visiting here. Gordon was one of the first biologists to try to set up mathematical models, if you wish, of how things worked. And I became intrigued with the idea that there must be some optimum size of diatoms. If they're too big, then the renewal of nutrients when they sink or rise, is less than --when the volume per unit mass is smaller, and if they are too small, they don't sink. We tried to put that into some language, and we wrote this paper together which for some reason actually endured for a while. I think those efforts endured only because there were so few in that field who could even do very simple hydrodynamics. There's nothing fancy about it. In the last thirty years, twenty-five years, when the whole generation of people in that field know how to handle that, it's gone enormously beyond that.

Doel: But you were doing that work a quarter century earlier.

MUNK: Right, and it was somewhat...it has somehow endured. I don't know whether it is today considered to be correct, but it was an attempt to quantify things that people had to quantify. And I enjoyed Gordon Riley a lot.

Doel: What sort of person was he?

MUNK: Oh, very fun to work with. And I saw him again when he had gone up to Canada to become head of the oceanographic department at Dalhousie in Nova Scotia. Judy and I visited his house, which was in the middle of nowhere, on some waterfront, very hard to get to. I think

that was not a good experience for him at Dalhousie. He challenged it and it didn't work out very well. But he was certainly... And there was a man at Scripps named Marston Sargent that I should have mentioned. He was in the Navy when we went to Bikini. He was a Lieutenant Commander, and he was supposed to be the liaison officer. When I mentioned that there were difficulties, I remember Marston Sargent losing his temper on the ship because we hadn't been given any support by the executive officer. He had an unpleasant shouting match with this man, because he was trying to help us. I think we need to help Judy. why don't we stop for a little while.

[Lunch break]

Doel: We are resuming after a very good lunch break. You had begun to talk about Marston Sargent. I wanted to make sure that you had completed that thought.

MUNK: He was in uniform at the time, and assigned as a Project Officer on the Bikini thing. He had been at Scripps, and came back to Scripps as a-- I've forgotten what his job was here. He was a biologist. And I just mentioned him when you asked the question about how our relations with the Navy were. I do remember a collision aboard the ship. Some things like this were not totally easy, the two cultures. They have to each make an effort to get along with the other.

Doel: By this point, by 1948, were the security clearance issues more or less over for you, or did you still have echoes of the wartime troubles?

MUNK: They were over by that time, for me. And they really never came back. As I think I mentioned I had now complete intelligence clearance, which was much more difficult to obtain, and no further problems. Q clearance. No, did not come again.

Doel: Did your clearance increase gradually in the 1940s, as you took on more assignments? Or, once things were clear, did you then get a high clearance?

MUNK: No, it depends on need-to-know at a certain level. You know, after you have your normal clearances, if you are involved, there is a need to know. My clearances then came in through JASON, and in recent times through a group called MEDIA, which you may or may not know about.

Doel: MEDIA is new to me.

MUNK: MEDIA is a group that was called Environmental Task Force, that Vice President Gore started, about four years ago, five years ago, with the question as to whether our intelligence agencies were collecting data that would be useful for social purposes. He had about fifty civilian scientists cleared to learn what was available. Mostly overhead assets, sometimes. So one year was spent learning what is available, and then about another year to decide which would

be useful, and more recently actually what has been done. It was a very interesting experiment as to how two cultures, very, very separate cultures, can work together. The intelligence community, took a rather dim view of the university community, and the university community, held equal feelings about the intelligence community. And I think it's been a success. They work together now. Each has learned to appreciate the other, and there's some results.

Doel: How did you become involved in that?

MUNK: I was asked. I was one of the fifty who was asked. I have no idea how my name came up. Actually, from Scripps there are three of us. Ed Frieman, John Orcutt, and me.

Doel: The three people who have been in directorship capacities?

MUNK: Not me.

Doel: Well, no, not at Scripps. And we have to talk about that.

MUNK: And Charlie Kennel, whom we spoke about.

Doel: Yes, at lunch, off tape.

MUNK: It was a group that worked in a very high level. And we saw Mr. Gore a number of times, and still do.

Doel: Do you have a chance to talk to him directly about environmental issues? What sort of things have come up, when you've spoken with him?

MUNK: It's in connection with MEDIA. And I've met twice with him, with about ten people present. I mean not one-on-one, but not a huge group. In each case I walked away being I think almost inspired by him. I think he has a good deal of insight, and he also has a sense of humor, which apparently does not come across to the public.

Doel: Did he seem different to you, than the public persona seems to suggest?

MUNK: Yes, yes. I thought he was very enjoyable.

Doel: That's very interesting. We need to cover some of these very recent developments. But we best make sure that we cover some of the earlier issues. What was the experience was like for you here at Scripps once Sverdrup did leave in 1948? Did you consider seriously going elsewhere? Did the climate at Scripps seem quite different to you, after he departed?

MUNK: No, I think I was sufficiently fond of Roger that that problem did not come up. It really never has come up. I thought a number of times of leaving, but not because of the dissatisfaction with Scripps' leadership, but because one just considers different things as they come up.

Doel: Yes, we'll talk about that, particularly during Bill Nierenberg's directorship. I'm curious what view you had towards Eckart as director?

MUNK: Oh, Carl, really, almost from the very start, gave the leadership over to Roger. He may even have, himself, interpreted his job as a stewardship for passing it over to Roger. So it's not really an independent directorship, under his leadership. He was in a way miscast as a director. He's a very private person. He liked it best when he was acting as a teacher or working on his own research. I think he was never happier than when he gave up the job, and he had three or four ideas that he wanted to work on, all of which were really very good. I think when he had the chance to do that he was happiest.

Doel: You noticed a change in his outlook and disposition?

MUNK: Yes. I remember once him saying that he felt like a pregnant woman, and he was waiting to give birth.

Doel: Did he talk to you about his views about the state of physics on the whole? His views towards quantum mechanics, and the direction of physics in the mid-twentieth century?

MUNK: Well, I probably didn't know enough physics to be a good audience to him. I wish I had known more. I'm not a physicist. But he did speak about his ideas, and about his science. And we have some stuff in the first interview we did about what a wonderful lecturer he was. He was just-- the word "elegant" has to just apply to it. It was just beautiful to listen to him present his things, and I learned a lot. I learned about dispersion theory, and wave theory, and how to present it, from listening to him.

Doel: You mentioned in the last interview that his notes later became very helpful for you.

MUNK: Yes. I think I may have turned some over to Deborah [Day], but probably still have some. They're a work of art.

Doel: It seems that he felt that he was much more comfortable in classical physics than in the work that he had done prior to the War.

MUNK: Yes, although he's probably best known for his other work. Yes, he was very comfortable. He did a very significant amount of work on time-series analysis. Similar to that of Wiener, Tukey, and other work. In fact, he developed much of that on his own, and later on realized that an almost independent and parallel development was taking place at the MIT

Radiation Laboratory, under the leadership of Wiener and others. The whole subject of time-series analysis was one in which he made some very original contributions. I think he did not publish, but he developed many of the concepts. It became so important in later times about analysis of geophysical and other stochastic processes.

Doel: Indeed. One of the things I'm thinking about, too, with regard to Carl Eckart, is his views towards the organization of science, what size of institute he thought was optimum. Did he talk to you about what later became known as Big Science?

MUNK: He was, as you know, first in the University of California Division of War Research, under Knudsen. Earlier we mentioned Knudsen, and he was the head of that during the War. Then Carl became the Director of NPL, a position really created by the Navy to keep him here. I probably give the wrong impression. His chief contributions during the War were in acoustics. And he wrote the so-called "red papers," which for a while, were the bible of acoustics, in military acoustics. Then became Director of Scripps, which I think was not very enjoyable to him, and then he, in fact, became a Vice Chancellor for Academic Affairs at UCSD for a brief period of time. That was not a happy time for him. I remember that he became very annoyed because he had made some requests, and they were not being followed. And his orderly mind translated the way in which things were done in physics into how they are done in an organization, and the fact that somebody didn't do what he said should be done was very upsetting to him.

Doel: Did you stay fairly close to him during the time that he was up there?

MUNK: Yes.

Doel: So you and he would talk about these kinds of frustrations?

MUNK: Yes. We often had him for dinner. He was sort of a lonely man. By that time he was separated, or divorced from his wife. His first wife had a drinking problem. Then he married Clara von Neumann, John von Neumann's widow. And that, unfortunately, didn't work out very well either. And she eventually, you know, drowned.

Doel: That was a difficult period.

MUNK: For him, yes. But, I think he enjoyed, as a whole, the experience at Scripps, as much as anything he had done.

Doel: And I do sense, that the kind of work that he was doing in time-series geophysical analysis had an influence in your own thinking.

MUNK: Very much so. Yes, he taught us an awful lot.

[Tape 3, Side A]

Doel: You mentioned, just before we went off tape, teaching at Scripps. Were there parts of the curriculum that you felt needed improvement for those who were entering into the graduate program at Scripps, or did you feel that the curriculum then was fairly well in balance?

MUNK: Oh, the curriculum was an accident, people talking about subjects they were interested in. It was optimized with regard to their interests rather than with regard to the students. There were very few of us. At some time or another, I taught most of the courses, in particular oceanography, sort of changing around as my own research changed. I think students never were given enough emphasis here, but it's gotten better.

Doel: Was it a frustration for you, when you were here in the earlier years?

MUNK: Oh no. But I think I said what I wanted to say. We talked about what we enjoyed talking about.

Doel: It makes it clear. How important was teaching for you in the earlier years? Did you find that it helped to stimulate your thoughts about research, or did they seem to be fairly different activities?

MUNK: No, I think the chief thing about teaching was that you got to know some people who later on worked with you as partners. Scripps always has been more of a student-teacher partnership, than a teacher to student thing. And that certainly was true in my case. Like Chip Cox, whose wife you saw this morning, he got his degree on interpreting the glitter of the sun on the water as a way of measuring surface roughness. By the way, that was an idea that Carl Eckart had mentioned in one of his lectures.

Doel: Is that right? I was curious, because this was work that you did in the early 1950s.

MUNK: Yes. Carl had written some notes about glitter, and then I became interested. Then Chip thought that would be an interesting thing, and then we got a hold of a B-17 airplane from the Air Force, and flew to Maui eventually, and photographed glitter on the water. It was a good experiment, and in fact, still is being quoted. I think it was a success. Chip Cox was an excellent experimentalist, and made things work.

Doel: This was known as Operation Ripple, if I recall that correctly?

MUNK: I didn't remember that name. But we translated glitter photographs into slope statistics, and that became more important as people became more concerned with scattering of radar and other electromagnetic waves from the rough surface, and even in connection with acoustic scattering.

Doel: Of course, some of that work was already developing out of World War II and earlier in the mid-1940s.

MUNK: Yes.

Doel: How closely connected were you by the 1950's with the other groups that were concerned with questions of scattering? I'm just curious how quickly those interrelations came about.

MUNK: I've always been interested in air sea boundaries scattering. In fact, Bill Nierenberg and I wrote a paper on the radar cross-section of a rough surface some years later.¹ And JASON was concerned with the scattering problem. Carl Eckart understood that very well—it's a physics problem. Bragg's scattering relations were well known in physics literature.

Doel: Indeed, that would have been a major topic of classical physics.

MUNK: A major topic of classical physics.

Doel: You also were also, during that series, putting slicks out on the water, from the airplane?

MUNK: That was done when I was in Woods Hole immediately following the War. There we did some work that was on an idea of Maurice Ewing's of how to build a wave instrument. It's described again here. And as part of the study we once spread oil on the water. Oh, and during the glitter work, we did in fact put some oil on the water as part of that experiment, and found that the mean-square slopes were reduced by a factor of three. So yes, that continued.

Doel: You had also gotten an offer from Haurwitz at NYU around 1948. I think it was about the time that Spilhaus was leaving to go to Minnesota.

MUNK: Uh huh. Gosh, I had gotten an offer from New York twice. One was New York University and one was the City College of New York, many years later.

Doel: I think this was the NYU offer in 1948.

MUNK: At the time there was so few oceanographers around that that was not-- I mean, it was probably easy to get an offer. I think at one time we went to Yale together, Judy and I, and I was supposed to talk to Kingman Brewster. Was that his name?

Doel: I believe that's right, yes.

MUNK: Kingman Brewster, because he wanted me to become Chairman at Yale, and Kingman Brewster was very interested in architecture. When we arrived he and Judy spoke incessantly, they paid no attention to me, and we met without that problem ever really coming up.

JUDY: Well, they knew that if I wanted it-- they offered me-- well, no-- who was head of the architecture department there?

MUNK: Oh, a famous man.

JUDY: Yes, the man who did the Beverly Hills City Hall. What's his name? But they knew that if I wanted to come, they assumed that Walter would go.

Doel: They were very smart about that.

JUDY: Yes. But I had a wonderful time. And Kingman Brewster, they had opened up sometime before, the Sarinaan Dormitories, which are fascinating pieces of architecture. And Kingman essentially 'slept' with the architects, worrying about the plans, unlike what has happened here. That's why Yale was really doing something really magnificent, with the library, the second library, and all that stuff.

Doel: When did that offer come about?

MUNK: Oh gosh, when did we go to Yale? I'm not good at relating to dates. I wish we had before us some kind of a timeline I could relate to.

Doel: I have something of a timeline, but this wasn't something I had known about. Was it after the time that you had founded the IGPP?

JUDY: It was after our first trip to Italy, because Kahn, you know, he had done the Arts Building there, and it turned out to be such a disaster. The sculptor studio, you couldn't get into it. You couldn't get the big stuff in.

MUNK: I need a timeline. When were we in Venice?

JUDY: The campus library there at Yale is so elegant.

Doel: Is it likely to be the 1970s?

JUDY: Well it must have been before IGPP. Because you never thought of leaving IGPP.

MUNK: Yes, that's correct.

Doel: So that would make it earlier then?

MUNK: Yes, a bit earlier.

Doel: Since Judy's in the room now, let me ask both of you, how did you two meet?

MUNK: Judy had gotten polio when she was 21. She was a student at Harvard at the time, in the design school. And then she was in an iron lung for a while, went through a difficult time, and then came here. She was doing a little better, and stayed with her grandmother. I met her then, actually through Helen Raitt, I think. I was married. And my marriage sort of went to pot. When I came back from Operation Capricorn, I decided to divorce, and then soon thereafter married Judy.

Doel: Had your first wife Martha had training in the sciences?

MUNK: No. She worked in movies. She was a movie writer. And had no scientific knowledge. Nor did Judith. Nor do my two daughters have any particular interest in science. We should find out when I went to Venice. But that was later I believe. [In 1972] All right. I really do need a, as you call it, a timeline at some time, to reconstruct things.

Doel: On many things I have dates; that was just one item that I didn't. One interesting note that I saw in correspondence from the time when Carl Eckart was Director was that one task you had been given at Scripps was, as you called it, "the awkward one of reorganizing the shop." I'm wondering what memories that brings back?

MUNK: Did I get that responsibility?

Doel: Apparently so.

MUNK: I had forgotten that. I would not be well qualified to do that.

Doel: Clearly you didn't feel very comfortable in that realm.

MUNK: I think there's a mistake somewhere. I don't recall that. Certainly I'm not a person who helped with experimental facilities. John Isaacs did, and Jim, and I spent, as I said before, my life was really working with very good experimentalists. But always as being their partners.

Doel: This was in one letter, as I recall, written at the time.

MUNK: Oh really? I don't think that was an important situation.

Doel: I was also curious about was the way that committees were structured at Scripps. Physical oceanography had been divided, and you were in charge of what was called Section Two. George McEwen director of Section One, and Robert Reed of the Third Section. How did that work in practice?

MUNK: I don't think that that's an essential thing. I mean, I had forgotten that too. George McEwen was, of course, we talked about him at Scripps. I don't remember that, and it's not an important part of our life.

Doel: That's fine. You had gone in the late 1940s to Point Barrow, Alaska, to the Arctic Research Laboratory, as a guest of the Navy. How did that come about?

MUNK: Oh, oceanographers would be invited to participate in journeys. I had gotten to know, and again I must remember a name, a man here, at the Navy laboratory, who was sort of Mr. Arctic Oceanography. He was along when we first had a submarine go to the Pole, a nuclear submarine. I should remember his name. I knew him. Waldo Lyon. Yes. He asked once whether I would like to go. John Knauss went on that trip. If I remember correctly, we went on a submarine mother ship called the *Nereus*. I remember, it was a pleasure. The one to Point Barrow was when I was-- no, I think those were two different trips. One was a trip into the Arctic on the *Nereus*, and one was a flight up to Point Barrow to visit the Arctic Research Laboratory, which was being maintained by ONR. We were asked to make some sort of a committee study about the future of the Arctic Research Laboratory.

Doel: Of course, that had just been established a few years earlier.

MUNK: It had just been established. And my memory of that is, again, of a meaningless, but funny story, of getting up there and it was all housed in Quonset huts. And I remember after having landed there, the next morning going into the wash house, which was a Quonset hut, or the house where you had to do your important functions, and I noticed that everybody was sitting next to each other on one side...

JUDY: Oh, you mean the shithouse?

MUNK: A shithouse. And I thought, oh, these people must have gotten queer after being up here for a while. They like to sit close to each other. Most of us like to be left alone. I sat on the other side, and I noticed snickering going on. I knew something was wrong. Then a wind blast came, and I found out why you sat on the upwind side instead the downwind side. I jumped up and everybody roared with laughter.

Doel: You were getting initiated.

JUDY: You get the most wonderful picture, isn't it? I can just see that.

MUNK: I think I was at the time also on a review committee, and it was decided then to do some work of drilling through the ice and maintaining a station, an Arctic station at the time. There was some work of that sort going on.

Doel: This was a one-time review, it wasn't a lasting association of any kind?

MUNK: No.

Doel: When you were doing some of the initial work on ocean currents in the 1940s, was there a plan to actually write a book with Henry Stommel?

MUNK: No. It's a part of a history that is a bit complicated. I really someday would like to get that straight in my own mind. Henry Stommel had written what is a classic paper, it's called "Western Intensification of Ocean Currents."⁵ One of the really important ones. He was here for a lengthy visit at the time. Sverdrup was working on what since has become known as Sverdrup Dynamics. One of the really important contributions of Sverdrup's career. I wrote a paper which in some way combined the Stommel results and the Sverdrup results.

Doel: And I think you're referring to on the wind-driven ocean circulation, what became the classic 1950 paper.⁶

MUNK: Yes, but the real important contribution was Henry Stommel's. That solution I made had, as special cases, the Sverdrup solution in the open ocean, and the Stommel solution at the boundary. I believe I was the first one to get a numerical number for the total mass transport as a function of the wind curl. The wind stress over the oceans. Which was an interesting number. And I worked on that, in part, on a sabbatical in Oslo.

Doel: This is the one that was funded through the Guggenheim Foundation?

MUNK: Yes. The first of my three Guggenheims. I had two others later on. So that, yes, was the time when I was interested in ocean circulation.

Doel: What was the reaction to the paper, once it appeared? How did Rossby, for instance, react to the argument that you were working on?

MUNK: I don't know. The people who were interested in it, and I remember, was first Columbus Iselin, whom I asked to comment on it. Then a man named Ray Montgomery.

Doel: Who was also then at Woods Hole, if I remember.

MUNK: At Woods Hole, who was editor of the *Journal of Meteorology*, where it was published. Who I remember made some very interesting comments on the paper, and suggested some changes that were good. That is about the story. Henry [Stommel] speaks about it in his biography, which has been published.

Doel: Uh hm. Indeed you cited reactions from Rossby and Eckart, and of course, Harald Sverdrup.

MUNK: Yes. I don't recall-- did I mention previously some reactions by Rossby? I didn't know. I didn't remember that.

Doel: Simply in the acknowledgments that I have here.

MUNK: Okay. Oh yes, I do remember that I talked to Rossby about it beforehand, and it was closely related to the beta effect. That's correct.

Doel: One of the issues at the time was to try to account for the fine structure that was beginning to emerge from studies of the upper Gulf Stream, wasn't it?

MUNK: Well, it was later that the meanders were discovered.

Doel: The big gyres?

MUNK: The big gyres. Actually, you know, the meanders forming and breaking off into Gulf Stream rings, was not known at the time.

Doel: That's right.

MUNK: That came later on. And there was a wonderful Gulf Stream expedition in which I participated under the general leadership of Fritz Fuglister of Woods Hole. It was called Cabot.

Doel: What kind of background did Fuglister have?

MUNK: Well, he was one of Columbus Iselin's disciples, Columbus thought that the way that you make an oceanographer is to take someone with no knowledge of science whatsoever, and make him a disciple. I mean, that was his idea. He had lots of people at Woods Hole. That was really a demonstration that, I suppose, oceanographers should be wealthy yacht owners or people with no science-- and Fuglister was an artist. His father was a cook in New York. Fritz became a great observer of the Gulf Stream. There were other people like Woodcock, who later on married Rossby's widow, Harriet. That was one of the reasons Woods Hole was so much fun is it had this assortment of characters, who had not gone the normal route to have a scientific career.

Doel: In that sense, did it differ significantly from Scripps, do you think?

MUNK: Yes, yes. We did not have the Iselin-- that Iselin touch.

Doel: You felt that was really coming from Iselin's directorship of Woods Hole?

MUNK: I think he chose those people himself, yes.

Doel: That's interesting.

MUNK: They were people like Woodcock who had been a sailor on the *Atlantis*, and Columbus thought, "This is really a good man, and a good observer, why don't I make a scientist out of him?" He became a superb scientist. He was a deluxe observer, and became recognized by everybody. Harald Sverdrup did a little bit of that on the *Maud* expedition. One of the men in the *Maud* expedition was a man named Dahl, and he and Sverdrup built a current meter together. Well, Dahl, without even a high school education if I remember right, became head of the Norwegian Atomic Energy Commission.

Doel: Is that right? In a general way, as you look back, were there elements of what was going on at Woods Hole that you wanted to see done more at Scripps?

MUNK: I've always, and up to and including today, look forward to any visits at Woods Hole. One of the things that's so nice is that the places are different geographically, as well as intellectually. Though I suppose they were more different then than they are now, intellectually.

Doel: But there's been a kind of convergence that's come over time?

MUNK: Yes, I think so.

Doel: Over the last decade, or has it been longer?

MUNK: Well, there's been so much exchange of people, from one to the other. Our students going there, their students coming here.

Doel: My impression is that Lamont was always more insular. There weren't as many exchanges that occurred between Scripps say and Lamont.

MUNK: But it came much later, of course. Did Lamont come earlier than Miami? Yes.

Doel: Yes, it did.

MUNK: And Rhode Island?

Doel: It preceded all of them. It began its operations by late 1940s.

MUNK: Right. And at that time Lamont was highly geophysical, and not really in competition with Scripps and Woods Hole, which were not mainly geophysical.

Doel: Right. That's well put. One of the other developments we haven't had a chance to address, at least in detail, was some of the instrument work that you were doing in the late 1940s, and I have a paper here, the one that you had written with Hector Iglesias and T.R. Folsom.⁷

MUNK: Thank you. It's one of the few times when I built an instrument. The idea was to build an instrument that was responsive to frequencies between those of the tides and the waves, namely tsunamis, if you wish. And you can do that by plumbing. You can leak off the tides, and you can do that very elegantly with appropriate diameter tubes and things. We built that and installed it at the end of Scripps pier. The idea being that one should be able to use that for tsunamis warning. Tsunami warning is a difficult problem. That would give you a warning only by a few minutes, when the waves arrive that you have a better chance to detect them in the presence of a swell than you would otherwise. But to get a real warning, the one you get now, is that you immediately analyze Earthquake records and see whether an earthquake was tsunamogenic (is the magic word). But still, as far as I know, you have to then depend on actual tide gage records, to confirm that that earthquake had been the source of the tsunamis. And I spent a little bit of my life on tsunamis problems. I was interested at one time. And I still am, in a way. It's an interesting set of problems.

Doel: In the analysis, you proposed some very interesting future research challenges as well.

MUNK: Yes, and I tried to do some work this year on that. There is a new chapter, and I don't know whether we'll ever get to that, but it was going on in 1996 and 1997. It involves an ancient problem, tidal friction, the dissipation of tidal energy, that we worked on when we wrote *The Rotation of the Earth*.

Doel: You and Gordon MacDonald?

MUNK: With Gordon MacDonald. We know very precisely how much energy is dissipated by the tides, and we know that because it affects the orbital system of Earth, moon, and sun. It can now be measured with great accuracy by measuring the rate at which the moon is moving away from the Earth, which is about an inch a year. This has been measured with great precision, ever since 1970, since we put a laser-reflector on the moon.

Doel: That's part of the Apollo Program, and the laser measurements have since been conducted regularly?

MUNK: Yes. But we don't quite know how it's dissipated. We only know the total. It's a zero-sum game.

Doel: Because you also have atmospheric wind-driven affects as well?

MUNK: Yes. In that particular connection, solid Earth dissipation is very small. It's an ocean gain. And ever since Harold Jeffreys and G.I. Taylor, has been thought that that dissipation is largely in shallow seas. And I worked on that at the time. The last two years we had the benefit of some satellite altimetry, which gave a new look at the problem. And this year I have two papers out on tidal friction. Ever since the environmental problem diffused and we could do a little work again, I got interested in that again.

Doel: And you're referring to the acoustic tomography controversy, which we will cover later.

MUNK: Which we must cover. There's a new chapter in what's happening on the dissipation taking place right now. I just have finished a paper on that subject. It's a very interesting problem.

Doel: How did you come to have Hector Iglesias and T.R. Folsom as your collaborators in that first effort?

MUNK: If you are talking about tsunamis, Folsom because he knew how to build things. Hector because he was one of three Argentinean students who were with us three or four years and were looking for things to do. And he, Folsom, was interested in that problem. We just made a decision-- I don't know how. People work together sort of like people get married. It's an accident as to what's happening, and it always has been.

[Tape 3, Side B]

Doel: I wonder if one reason why there were a number of Argentineans who were interested in the oceans was that the Argentinean Navy seemed to be fairly active among the Latin American nations.

MUNK: They were all Navy officers. The senior man became an Admiral. I remember [Hector] Iglesias, Etcheberry, and one who became an Admiral in the Argentinean Navy. They came here, and they were paid rather well. They were the envy of all the other students because they all bought fancy cars and took out all the girls. And they could all dance all night. We had a good time.

Doel: They'd still show up in the morning and work?

MUNK: Yes. I took one of them with us when we went skiing in Sun Valley once. We had a great time. So they were very much part of Scripps. Harald Sverdrup got along with them. And I have a beautiful silver tray that Harald Sverdrup gave me which is a gift from the Argentinean Navy officers to Harald Sverdrup, that we use. I should show you that some time. It comes out of that period.

Doel: I would like to see that.

MUNK: I would like to say something about the Argentinean students, something I suddenly remembered.

Doel: Please. Yes, I'd like to hear it.

MUNK: They questioned us about ONR. "They" being Navy officers. And they were very good students. Absolutely excellent students. They thought that there must be some secret Navy reason why the Navy was supporting oceanography in the Scripps Institution. ONR at the time, remember, was even more free than it is today, or considerably more. Really going under the assumption that helping to have a healthy ocean community in the United States was a good thing for the Navy, in general. And I remember talking to them about it, and they just couldn't believe that something as deep and as profound and as common sense as that could really be the basis of the ONR support. I remember them saying, "Well, what is the reason, and why are they doing it, and what do you have to do for them?" They know they have taken this broad view that a healthy ocean community was good for the navies of the country. Wouldn't it be wonderful if Argentina could take a similar view? I just happened to remember that.

Doel: Yes, that's telling. And I would imagine that you at times heard similar comments from some of the visiting Soviet scientists.

MUNK: Yes. They probably never believed it.

Doel: During the window, when Soviets were able to travel freely to the United States right after World War II, did any come to visit at Scripps?

MUNK: Oh, yes. This always was a problem. We always had to have special permission from the State Department, which was usually interpreted very bureaucratically. Like you can have them, but they mustn't be able to look at the beaches, or some stupid thing like that. And you know, Judy and I went to Russia four times, at about the same time, and generally had an easier time going there than they had coming here. Our State Department was the real problem, not the Navy. Very narrow principles on which they would decide whether somebody could come.

Doel: The State Department by the late 1950s had restricted zones, which Soviets could not visit.

MUNK: Yes, and San Diego was a restricted zone.

Doel: I was thinking that that was likely the case.

MUNK: That was probably a balance to Sebastapol, because if we wanted to go to Sebastapol, it was a closed city to us. The way we finally got to the Caucasus, was that we had a car. We were put into our own back seat, and the Russian driver would have to take us closer. We couldn't decide where we would drive. An odd situation.

Doel: Was the first trip to Russia the one that you made in 1957? I saw in one of the round-robin letters among oceanographers, that you had written a question of whether you would travel in 1957.

MUNK: When was the Cuban Missile Crisis?

Doel: 1962.

MUNK: Well, that was the first time-- I should tell you that story, but that was earlier. We had a meeting in Copenhagen that Roger Revelle was very prominent in. Judy and I went. No, it was in Brussels, sorry.

Doel: Was this IGY related?

MUNK: We met for the first time with Russian oceanographers. The head of the Russian Institute of Oceanography was a man named Vladimir Kort.

Doel: What sort of person was he?

MUNK: Oh, big, gruff man who loved to go to sea and drop Nansen bottles. Nothing complicated about him. He loved taking classical Nansen sections, and he would have been happy in his life if he had done nothing else but that.

Doel: Did that already seem to be outdated by the time that you met him?

MUNK: Yes, but most of Russian oceanography was in Brussels. There was an attempt made to plan something in the future. We should reconstruct that. Roger and Kort carried out a conversation using Russian translators, who at the time were always KGB paid. We had a whole day's meeting, and Judy and I sat away from the main table and listened. Revelle and Kort were getting absolutely no place. Kort was saying, "nyet, nyet, nyet" to any kind of suggestion. Roger tried to probe. The second day a Finish oceanographer named Hela, who was a regular representative from Finland, after a while suddenly said he wanted to make a statement. He said he hadn't mentioned that he spoke fluent Russian, and he had listened for the last day to the conversation, and he wanted to announce that the Russian translators were deliberately and continuously mistranslating everything that was being said by the Americans, and miss-translating everything that the Russian's said in reply. And that he thought the meeting was useless under those circumstances.

Doel: Did he give an example of what he meant?

MUNK: I wish I could remember what the theme of that meeting was. But that was what he stated with great consternation.

Doel: Very important.

MUNK: The translators, of course, turned red and furious. But the meeting broke up. Then when they got together, Revelle and Kort both asked that Hela be the translator. The translators

were just cut out. And the meeting went splendidly. Kort and Roger were both big, tall guys who loved to go to sea, and became very close friends. At one time at the New York Congress, where they both attended, they had a very wet party and both got quite drunk. One of the pimply Soviet translators came by and tried to influence their discussion. And almost as if planned, Roger and Kort picked him up by his elbows and carried him out the door and closed the door. Roger turned to Kort—Kort, by that time, had learned some English, and said, “Are you going to get in trouble over that?” And Kort said, “Oh, I don't think so. I will just tell them I was drunk.” So that's a good story, but it's a true story. And the Hela thing was very much in my mind. I have since heard of similar stories. Teddy Bullard once told me that when he was head of the National Physics Laboratory, in the very early days, a Russian delegation came over and they had a similar experience. But, they had some people from British Intelligence listening in the background. And at one time, Teddy announced that he had been informed that the translation was totally and deliberately misleading, and he would suggest they come to an end of the discussion, and that they decide among the Russian delegation what they wanted to do about it. From then on they continued, I believe, with some other translators. So that was apparently par for the course, that the KGB sent in translators who would attempt to direct the discussion, instead of acting as translators.

Doel: Uh hm. It was over political issues, or at least anti-international cooperation?

MUNK: Yes. And then Kort was a wonderful guy. When we went to Russia the first time, we were sort of his guests, Judy and I. I went four times, three times under the Academy.

Doel: The 1962 trip when Judy helped prepare diary entries for, did occur exactly at the time of the Cuban Missile Crisis.

MUNK: That was when we took a car, a Land Rover. We lived in England. I was on a sabbatical at Cambridge. It is an interesting story, and I'd like to tell you that. I'm not much of a big city boy anyhow, and the idea of being in Moscow for two weeks didn't sound very nice. So I schemed, even before I left, to get an excuse to go and see something of the country. They had a field station. “They” is the now Chekhov Institute in Moscow (it wasn't called that then) had a field station on the Black Sea, in a place called Galinzig. I had the Scripps librarian look up papers from somebody from Galinzig, and they found some dealing with near-shore currents, which by some stretch of the imagination was a subject on which I had worked. Very vaguely. Because waves breaking on the beach induced those. So I wrote back to their Academy saying I'd love to come, but I was particularly interested in the work by this man at Galinzig and I needed to have conferences with him. And it was very difficult for them to say this was not true. So, they agreed that I should be able to go down and meet him. Then we were in England and we decided we really would like to drive. And we bought a Land Rover with that in mind, thinking that it was up to the Russian winter. This was October. And it was around my birthday, because October 19 was a birthday for the Soviet Union as well as for me. I forgot what happened. Some sailors rebelled in Saint Petersburg on October 19. And when I was invited to come, we wrote back and said we would like to come by car, and the Academy Foreign Secretary wrote back and

said, "I would advise against it. The weather begins to get bad in October." But never said, "No." So we put our Land Rover on a ship and went to Helsinki, not the usual Leningrad way, and decided to drive across the border on a Sunday morning, early, when anyone of any consequence wouldn't be awake, in the hopes that they wouldn't turn us back. And we had a stack of Academy letters. Somewhat irrelevant, but nonetheless important looking. And we arrived at the border station, which at the time was almost inactive. (I don't know whether it is now.) We went on a one-lane road through the forest to get to that station. At 6:00 in the morning, on a Sunday.

Doel: You were planning this very carefully?

MUNK: Very carefully. After peeling off letter after letter, they eventually didn't know what to do. And we told the man there-- we threatened that-- "the Academy is an important organization in Russia", and we were getting very impatient, so they let us through. And we drove all the way down to Moscow in two days. We stopped at Novgorod and Leningrad. An interesting trip. They, the Academy people, were furious with us when we got there. They knew what we had done. They said, "We never gave you permission." No visitor had ever come in his own car. I made some rude remarks saying that surely they were sufficiently flexible to adjust to some new situations. Anyway, they could have told us to just get out again, and didn't. I did get permission to continue the trip. And they assigned an oceanographer, a young man who has become a well-known physical oceanographer, Vladimir Komenkovich, to go with us, which was wonderful. He was an oceanography student. And we drove. While we were in Moscow the Cuban crisis occurred and we really didn't know what was going to happen.

Doel: You weren't getting much news I imagine either.

MUNK: No news. We tried to get into the American Embassy, and failed. And then Teddy Bullard got word to us through the British Embassy that we should be concerned or something. Judith's mother and my daughters were in Cambridge, England. But anyway, we left. And I must say it was a one month trip. I mean, we were on the road for a month. We drove 5,000 miles. Nobody was ever rude to us on that trip. It was not difficult to move. People were interested and helpful.

Doel: And you were meeting oceanographers that you had already met in other contexts? Was Gleb Udintsev one of the people that you met?

MUNK: He's a geologist?

Doel: I seem to recall your having met him.

MUNK: Yes, Udintsev. I pronounced it a little differently. Yes, yes. And there were people-- there was one man who had written on the wind driven ocean circulation, **very similar to what I had done.**

Doel: Your 1950 paper.

MUNK: And I remember his accosting me and saying that he knew all the papers we had written, but we had paid no reference to his work, which was true. Though I've always been a poor reader, and I didn't deliberately leave him out, but I've always been accused by the Russians of not paying attention to their literature. Whereas, in fact, they were reading ours. It was a reasonable objection.

Doel: Was it largely, do you think, the availability of translations, or did most of the Russians at the time need English in order to keep current?

MUNK: I think they had better facilities, and maybe more time, to read our literature. I think there has always been an asymmetry. It seems to me they have a more scholarly and proper point of view that you pay attention to literature. And I am particularly bad about that. I've always been bad about searching literature. But anyway, Kort was still-- was it--? No, by that time it was Andre Monin who was Director. Kort had been Director the first time we went, and we saw him. He was happy to have gone back to taking Nansen casts of the oceans and going out on the ships (*Note from Munk: I have mixed up the 1962 and 1966 trips. Kort was still Director*). That was his thing. And we drove down. It was an adventurous trip, and got to Galinzik on the 19th of October, my birthday, when they had a big party. I thought that Galinzik, a field station, smelled a little bit like Scripps had smelled when I first got there.

Doel: An early memory that you had.

MUNK: An early memory. And I liked it. And we came in, and the Galinzik people all knew we were coming. We were the first non-Russians who had visited there. So, they were very-- that was an event for them. And everybody was well on his way in the party, and it was a great party. And we never went to bed. The next morning they had arranged for drivers to take us out through Sebastapol, down to where we were going to catch the ship to the Black Sea at Odessa. But they wouldn't let us drive, which was a good idea, because we had been up all night. And I recall that about an hour out of Galinzik I suddenly turned to Judy and said, "My God, we never met that man. That was the reason why we got permission to go to Galinzik." She said, "Oh my heavens!" We were so busy. And I told that story to Andre Monin years later. He once was a house guest here. He said, "I know, but he was promoted anyhow."

Doel: This fellow?

MUNK: This fellow who had been our excuse for going to the Galinzik station. It was a very exciting time of our life, that trip. Judy did nearly all of the driving. She's a good driver. The roads were terrible. We were suspicious about why they told us exactly where we must travel, so a few times we said let's go and take some side roads to see what's really going on. Each case, we almost got stuck in the snow, and the mud. So, that was good reason for keeping us on track, not necessarily a major security consideration.

Doel: Yes. I'm curious what felt about the relationship between the party apparatus, the politics and the oceanographic community and the Soviet Union, in the early '60s?

MUNK: I thought they were happy when they could not see too much of the political apparatus. The scientific community really didn't have very high regards for the politicians. But I think they must have been scared to show too much independence. And the story I told you about Roger and Kort, it was typical of what they really liked to do but couldn't do. We always found it very enjoyable to visit there, when you got to be alone with people. Of course, they didn't invite you to their houses. It's not part of the culture of either Russia or China to do that. You met in restaurants. And it was rather formal in the sense that people constantly toasted each other and so on. But all my trips to Russia were always very, very rewarding.

Doel: 1962 then was the first trip that you made, and then '68?

MUNK: '66 was the second.

Doel: You did go then in '57? One note you wrote at the time indicated you were interested in going-- including to Moscow for the astronomical meeting. I think that might have been the International Astronomical Union meeting.

MUNK: I don't know. But the one that I mentioned, when we drove was our second trip. Monin was Director then. The first trip was when Kort was Director.

Doel: Let me ask did you attend the Rancho Santa Fe Conference?

MUNK: Yes. That was mostly a John Isaacs product. That was the beginning of attempting to do ocean climate.

Doel: I'm thinking of a 1950 meeting that Louis Slichter had organized.

MUNK: That's another one. I don't remember now. Can you tell me what happened at that meeting?

Doel: That was a broad interdisciplinary, multi-disciplinary meeting generally about solid Earth geophysics.

MUNK: I don't remember. I was thinking of another conference that John Isaacs called much later, which involved Bjerknes and became the sort of beginning of ocean climatology, showing another side of John Isaac's broad interests.

Doel: That does occur later on. I also wanted to make sure that you had a chance to talk a little bit about the Midpac Expedition.

MUNK: I didn't go. I went on Capricorn.

Doel: Yes, which was two years later.

MUNK: Two years later.

Doel: How important was Midpac for Scripps?

MUNK: Very, very. It was the beginning of going from a-- when I said there were sort of three steps, the *EW Scripps*, *Gulf of California*, the *Horizon Baird*, the *California Coastal*, and then the Midpac-Capricorn, etc., which was the total Pacific. So, yes, very, it couldn't be more important. And Roger personally led it, and personally led Capricorn.

Doel: Did you regret not being able to go on Midpac?

MUNK: Yes. Afterwards. I've forgotten what I did at the time. I'm sure they would have let me go. And then on Capricorn I wanted to go. I didn't have my own particular research project, but I learned a lot. I remember who my shipmates were there. Martin Johnson went. Russ Raitt was doing seismology. We were towing a magnetometer. Gustav Arrhenius had asked me to take some air gas samples, which I did do. And I had just learned to dive, and I was an active participant in diving with the newly developed aqualung.

Doel: This was all very new at that time.

MUNK: It was totally new, and there was no system to it. But I think we dove at almost every stop in the Pacific. I learned that you don't dive without sharks around. If they're not there when you go in, they're there ten minutes later. And we had been told that it was Navy policy that sharks would not attack you unless you were at the surface or unless you were bleeding, and so that was our arrangement. We wouldn't let anybody dive if they had a wound. And we would get to the surface quickly at the end of the dive. And then when we came home eight months later we learned that Navy policy had changed. But for some reason it had not reached us, which was a good thing.

Doel: What was the new Navy policy?

MUNK: Oh, that sharks could attack you under other circumstances as well.

Doel: I suppose it was good that you hadn't known.

MUNK: Yes, there's even a theory about it. The sharks have a very keen sense of smell, and if you're afraid, you emit some things that they can smell and they're more likely to attack you. And since we weren't afraid, we didn't emit that particular odor.

Doel: That's interesting. There's a number of things about the expeditions I wanted to make sure we spoke about. There was a tradition that quickly emerged at Scripps to name expeditions, that one had Midpac, one had Capricorn. Other places like Lamont simply had ships going out at different times. Do you think the naming of an expedition affected the way that it was carried out, or conceived?

MUNK: I think that must have been maybe Bill Menard or Bob Fisher. Really you should ask them. Or maybe Roger himself who thought of the romantic names. I don't know the story behind it, but, it would have been nice. That's a good thing. Not just named by the ships name, like the *Baird* Expedition, or the *Challenger* Expedition. And now we name some of our streets for these expeditions. We have various streets at Scripps, you know, roads named for that.

Doel: That's right. Scripps has expanded now. Another big issue that comes out from your early scuba diving work that was done in Capricorn: Did you see a clear role for humans in oceanographic study, as opposed to the more traditional instruments that were in use?

MUNK: Well, I believe that there are very few things. There are a few things, but very few things. You do better by having people go down than an instrument go down. You accept such an enormous safety margin that you end up spending 90% of your efforts in maintaining safety. That's more true of diving submarines than it is of shallow diving. I mean most of the diving was less than 100 feet. But still. I think we went diving for the adventure. And we never learned anything enormous. But it was useful. At one of the expeditions in Bikini Lagoon I remember calibrating my wave instrument for measuring tsunamis by raising it from the sea floor to my head and keeping it there for fifteen minutes, and then putting it down again. That is sort of a heavy site function input, which gives you a good calibration plot. And I remember that circumstance, because that's in my 65th thing⁸. Bascom took some pictures of me when I wasn't aware that there was a shark behind me. I have a beautiful picture of a shark behind me, with my holding up the instrument, calibrating it. And I accused him of wanting to take footage of "Shark Attacks Oceanographer." But no, diving-- I don't want to say that people haven't done very important work. But as a whole I feel that instruments are vastly superior to people.

[Tape 4, Side A]

MUNK: And I rather suspect that in the next ten years the AUVs, automatic underwater vehicles, can start taking over much of the sampling which is now being done from ships. AUV's will have automatic, built-in tasks to perform.

Doel: The kinds of sampling that had been routinely done on the Capricorn or the other expeditions?

MUNK: Or that Kort was doing.

Doel: Uh hmm. I was very curious, a moment ago when you said that the Soviet program, as a whole seemed to be lagging behind what was being done in the West.

MUNK: We always thought it was a little factory-like. They had these big ships-- it's a tradition to have these *Akademician* so-and-so, and *Akademician* so-and-so. I mentioned to you that they had four messes on these ships. They would actually have three or four observations going on simultaneously, so you had to organize it so even the angle of the instruments that would go down on were compatible, so things wouldn't get snarled.

Doel: So the cables wouldn't get messed up?

MUNK: Yes. And they even had, I remember, printing presses aboard, so papers by the time they came home could be published.

Doel: Is that right?

MUNK: Yes. Very impressive for publication. But not the kind of atmosphere which induces new ideas and new experiments trying to do things that had not been done before. So their whole system seemed to me, was organized into rather dull, factory-like kinds of surveying, rather than having a person go out with an idea and being able to try and stop, and try and stop.

Doel: Things were routinized and methodized --it was not easy to be innovative.

MUNK: That was my impression. Yes. I think they made a mistake in putting so much emphasis on big ships.

Doel: Did you talk to people like Kort, about that? About the research philosophy?

MUNK: Yes.

Doel: What did they say?

MUNK: Oh, that I was probably right.

Doel: Had they been architects of that program?

MUNK: I think those were political decisions. There was somebody in the government who thought that big ships would add more prestige. Those guys were good scientists, and would have preferred to operate in the American mode. Yes, we did talk about that.

Doel: That's very interesting. I imagine that people like Kort were actually writing while out at sea.

MUNK: They did collect data and analyze and publish it, before they got back.

Doel: But if they are publishing it on board the ship, that might suggest that peer review, in the way that you're familiar with, was not occurring.

MUNK: These were not published journals, but in government things. But at least the data was worked up and neatly represented, in columns and summations.

Doel: And the printing press implies that it was also prepared first to circulate at that stage?

MUNK: Yes. There was, as you know, great competition between different government bodies in the Soviet Union. There was the Academy, which was the elite organization, and then there was the State Bureau of Oceanography, and there was this and that. They competed for funds. People who know lots more than I do about this have written on that subject.

Doel: Were there any research programs where you felt that the Russians were doing a generally better job than in the West?

MUNK: Oh, turbulence theory. Turbulence theory. Kolmogoroff, etc., and Monin, which included measurements. They were very good. The good ones are very good, and very scholarly. But even today when we have all sorts of Russian scientists in America in various institutions, their emphasis seems to be largely, not entirely, on theory. They've done a good job.

Doel: That kind of mathematical, physical work has long been a hallmark of the Soviet achievement.

MUNK: Yes. They have very good people.

Doel: We have been talking again now for several hours, and we probably should not try to push too hard today. We will pick up tomorrow. We've covered a number of developments in the early 1960s, but there are a number of gaps yet in the 1950s that I want to try to close. You had, in the *Compendium of Meteorology*, the famous volume that came out in '51, Ocean Waves as a Meteorological Tool. I'm wondering how that paper came about?

MUNK: That was an outcome of my World War II work. I had mentioned to you that Harald Sverdrup and I, at least, did not when we did our wave prediction understand wave spectra. Nor did anybody else.

Doel: That was very-- that was a wide field, a frontier.

MUNK: The British, really, men named Barber and Oursell, and others, started learning how to analyze wave methods into different frequency bands. That became very interesting to me as I learned the technique, partly under the guidance of John Tukey.

Doel: How did you come to know Tukey?

MUNK: I've forgotten how, but we became good friends. We met somewhere, and Tukey had a series of disciples in various fields. He was a statistician, but he wanted to apply what he had learned. And so he had a following.

Doel: He was at Princeton at that time, wasn't he?

MUNK: Yes, at Princeton. And also a consultant at Bell Laboratories. And I met him and he often came back to La Jolla and he taught me how to do spectra analysis. We talked a little bit about--

Doel: We talked a little bit about that.

MUNK: And also about spectra. Anyway, if you have an impulsive-like storm somewhere, you first get low frequency waves because they go faster, and later on high frequencies because they go slower. And the change of frequency with time tells you how far the source was and when the impulse-like generation happened. That was the key to our later experiments in Waves Across the Pacific, where we did that.

Doel: Yes, the question of wave attenuation.

MUNK: Yes. But very nice frequency time plots. You see these ridges of those plots of frequency increasing with time, and it turns out to be linear from simple theory, and the slope tells you how far away it is. If it is a nearby storm, then the low and high frequencies follow each other rapidly. If it's far away, so it has plenty of chance to disperse, it may take a week to go from the early 20 second waves to the late 12 second waves. And that obviously was a way of locating storms, by waves, and that's what this was about. And it works. You can locate storms, but not predict them. It's an after-the-fact situation. And it became essentially the method that was used in our Waves Across the Pacific, which we haven't discussed yet, or have we?

Doel: We haven't.

MUNK: That was one of my great adventures. And we should discuss that.

Doel: We will.

MUNK: But that was what this was about.

Doel: One of your footnotes in the paper from Palmén talks about the lack of information on the southern oceans. I was wondering if that was one of the stimuli for what becomes the 1963 program?

MUNK: Yes. It turned out that there was virtually no observing system in the southern seas. And what we learned from swell coming up, all the way from the Indian Ocean and the South Pacific, had significant information then about storm systems. I have an interesting sequel. On this expedition we would observe these wave systems coming up. Beautiful records, as you could see how the low frequencies were followed by high frequencies, and then we built a directional system off San Clemente Island so we could actually locate not just how far they were, but where they came from, by getting the direction. And we would see these fantastic wave systems come in from almost antipodal points. Fascinating that you should be able to get waves from that far away. They weren't very high when they came here; they were not surf-boarding waves. But they were detectable. We could pick up a millimeter swell because of their spectra signature in the base of eight foot wind waves. No problem doing that. I think that's when John Tukey got interested in it. And many, many years later, in my Heard Island adventure, because I hadn't taken my old paper, I didn't realize that the best storm we've ever had, we had located on the chart very close to Heard Island. Only we had experienced that storm at a safe distance of 20,000 kilometers. And that time, when we were at the same place, we had the storm. We lost all our instruments. It damaged them all and we lost one. I hadn't realized that we finally had gotten to real contact with the storms that we had observed at a safe distance many, many years earlier. The same storms.

Doel: That's very interesting.

MUNK: And since then, the satellite people have been able to measure wave intensity on a global basis by using altimeter reflections, and I have seen some plots of the intensity of waves on the global scale, and the highest intensity sort of peaked around Heard Island. It was not a very wise choice, I think.

Doel: We do need to cover *Waves Across the Pacific*. I was also wondering, to try to keep things in chronological order, whether we might want to talk about early work that you began the early 1950s on sea level and the rotation of the Earth. How did you first begin to think about this problem?

MUNK: How did that-- was that simultaneously with the wave work?

Doel: Virtually: 1952 is your publication with Roger Revelle.

MUNK: Oh, but the book was published in 1960. So most of the work came after '52. Then, Gordon MacDonald and I-- and then when did we do *Waves Across the Pacific* [1965]? We thought we should find out where they came from.

[break]

Doel: We're back on tape after a short interruption. The Guadalupe Island study, number 57 on your c.v.,⁹ in fact published in 1957, is what you were talking about, where you mentioned you found evidence even after 20,000 kilometers.

MUNK: Yes. Oh, and then in '63 we did the directional recording at San Clemente Island to show where the waves really came from. Waves Across the Pacific came in '65. And the expedition to study-- but that wasn't the main paper. The main paper was Number 96 in '66¹⁰.

MUNK: That was published in *Phil Trans*. And I had already started working on tides then, so it was considerably later than *The Rotation of the Earth*.

Doel: What we ought do today is talk a little bit about how *The Rotation of the Earth* came about.

MUNK: We had an Air Force student here by the name of Miller. I'll tell you exactly how that came about. It must have been in-- oh, number 92,¹¹ but it started even earlier. Yes, it started in 1950. What happened, I think that's kind of interesting, is that I had noticed that some astronomers in Paris, whose name was Stoyko, a husband and wife team, had noticed that there was a change in the length of day between winter and summer using pendulums for time keeping—a tour de force. Ah, no, it was the other way around. I had read a paper that came out of Rossby's group, of one of Rossby's people, who wrote a sort of a review paper saying that there was no law of God and nature that the total angular momentum of the atmosphere has to be constant. Because God and nature say that the angular momentum of the planet Earth, atmosphere plus solid Earth, has to be constant. But if the winds go faster and the Earth rotates slower, there's no problem there. So he said that-- oh, back again a step. What had been discovered was the high altitude jets.

Doel: During World War II?

MUNK: During World War II. We didn't know. It became obvious, this was Palmén and Rossby, we go back. It's much more interesting, that most of the angular momentum of the atmosphere was associated with the high altitude jets in the northern and southern hemisphere. That was the discovery. Then it was stated that those jets are different in winter and in summer. And in this review paper I referred to, it was pointed out that there's no law of nature that they have to be constant, but that they should have to be associated with, and I remember the words, "imperceptible variations" in the spin of the Earth. Imperceptible was the word. And then in our book on the rotation of the Earth I talk about this. That word imperceptible is what got me going.

Doel: Right. You wanted to see if this might be perceptible.

MUNK: And I found by chance that the astronomers had apparently measured something of this sort. Miller was looking for something to do. He was an Air Force student here. And I said, "Let's try to calculate how big it should be and compare it." And it was an easy matter of taking

numbers and integrating it, and we came out with, I don't know, a few milliseconds difference in the length of day between winter and summer, which happened to agree with what the astronomers had measured. And we published it. Then what happened is very odd. The astronomers learned a better way of doing it, and they got numbers that were considerably smaller. At the same time, it was found that the longitude at which the winds had been measured was the one that had the greatest changes, and when you averaged it around the whole world it was much less. Each of those activities reduced the affect by factor of three. But the conclusion remains the same, by strange luck. So, we published a second paper showing that. And that is one of the few things that I've done which was a real discovery. Because nobody had suggested previously that there should be a seasonal change in length of day. The effect is very big by today's standards of accuracy, and everybody corrects for it. And the seasonal effect is still the biggest effect. And as a whole, the cumulative effect is that you slow as much as three quarters of a second at some times of the year as compared to what it would be if there would be a constant spin, so it's a large difference.

Doel: Indeed it is.

MUNK: I think we were the first to write on it. But we never-- nobody's really given us credit for that. Not that it matters. But, it's surprising, I've had credit for things where I've done very little. In this case we were the discoverer of that effect. That got me started, and then I ran into Gordon MacDonald who found that interesting, and then we formed a partnership. Then we couldn't stop learning one thing after the other, and the tidal friction. And one thing we did was to combine two subjects that were totally separate. The spin of the Earth and the wobble of the Earth. We considered them together, and we combined that. And today that's a very respectable subject. There are hundreds of people working on it. They can measure the wobble to a millimeter. They can measure the change in the center mass of the Earth to a millimeter. And it's fantastically accurate. Lots of people are working on it.

Doel: But indeed, as you say at that time, this was a new undertaking.

MUNK: Astronomers were interested in latitude change for a long time. They had never made much of a geophysical interpretation.

Doel: Yes. Did you come into contact with any astronomers in the United States who were working on these classical problems?

MUNK: Well, so we really became very fascinated. It was very hard to let it go. It was such an interesting subject, and an interesting subject now.

Doel: Clearly it had many ramifications. But did you come into contact with people like Dirk Brouwer at Yale?

MUNK: Yes, he was one of the eight men. Our nation's timekeeper then—that's an official position of the Navy—was a man name Markowitz. He rather objected to seeing a dirty geophysicist mess with his astronomic measurements. But the fact is they had screwed up on interpreting their measurements. They did not understand... in fact the position of the astronomers were worse than the oceanographers. Because you see, they have very definite periods, to 8 significant figures, and they had very funny theories as to what happens. It started with Lord Kelvin, who is a great hero of mine. And so we came in and we had a meeting that was called the previous year to actually celebrate the subject. It was called by JPL people in Pasadena, and I was asked to speak historically about *Rotation of the Earth*. There were quite a few astronomers there, and they really never paid any attention to us. They resented the geophysical intrusion upon a clean subject. Which we dirtied up for them. But those things have been very useful. I kind of think that it has done useful things. It's a way of measuring things on a globally integrated basis. And it's still being pursued.

Doel: I want to cover that work a bit further on. I'm looking now at the 1952 paper that you wrote with Roger Revelle. There were other issues coming into play, including Reginald Daly's work on the ancient beach lines, the question of ocean level, these kinds of broad-scale changes that could also relate to that. I'm wondering how involved you became in these other lines of research? Fran [Francis] Shepard clearly was concerned with the question of ocean beaches.

MUNK: Well, not with that aspect so much. If you melt an ice cap and you raise sea level everywhere, you change both the length of day and the position of the axis of rotation. That's what Roger and I did at the time.

Doel: Indeed.

MUNK: And if you put certain limits on how much melting you could have from the measured amounts. And that subject, by the way, is active today. We've got some new numbers, and it's fascinating. That's all I can say.

Doel: Do you remember the reaction to this paper when it came out?

MUNK: Probably not.

Doel: I'm not surprised. I'm just curious if you did remember it stimulating any discussion?

MUNK: People were used to hearing about it, and I gave some talks. But it really became popular after we got out of it.

Doel: I realize the generations are quite different, but I'm curious if you met Reginald Daly, or had a chance to talk to him?

MUNK: Roger knew him. And he was certainly a major figure on that. We referred to his work, didn't we, in our paper?

Doel: Yes, you did.

MUNK: And it's interesting people have now been studying former lake levels from the point of view of what was the glaciation like in past times. It's being done very well today.

Doel: Right. And Wallace Broecker at Lamont addressed that in his thesis.

MUNK: Oh really?

Doel: He was concerned with related questions.

MUNK: Really. And of course, he's done other things since, mostly. He's more of a geochemist.

Doel: We probably ought to bring this particular session to a close. We will be resuming, of course, tomorrow. One of the major developments for Scripps in the late 1940s came when, as did you say, Bullard was a visitor, and helped to introduce heat flow at Scripps. I wonder if you remember discussions about what kind of program could be done?

MUNK: Yes. Very much so. Roger became fascinated with the problem. Teddy was a good machinist in addition to being a superb physicist, and I remember how I envied him that he would go down to the machine shop and turn these lathes and do all this instrument building. It's in the best tradition of Cambridge physicists. John Isaacs had a secretary who was-- no, Art Maxwell worked on that, and his first wife had a very interesting figure. Teddy said he shaped the instrument after her behind. You see, the things I seem to remember are terrible. And he built the thing. It had not been done, measuring the temperature in the sediments. Then Richard van Herzen took his degree, and Art Maxwell, didn't he, on that subject. They were really the only students of Roger's for which he was responsible. That work came into prominence also in connection with early work on plate tectonics, because heat flow was an important measure.

Doel: It was clearly critical, and it had not been done Pacific prior to that.

MUNK: There's one thing I'd like to mention on that, which I think to me is important. I had gone through the time when all the geophysical methods that had been successful on land were transferred to use at sea. Seismology, which Bullard, but Ewing particularly, worked on the sea floor, heat flow measurements, gravity, magnetics--all these things had not been done at sea when I started out, and were done at sea subsequently. And the interesting thing is they were done better at sea, eventually, than on land. The deep ocean environment is very benign, and offers a better condition for doing, say, heat flow measurements, than you ever have on land, with all the meteorologic disturbances.

Doel: Because of the stability of the water column?

MUNK: Because of the stability of the water column. I think the magnetometers towed at sea were more effective. The seismic work at sea has been more effective in a way than on land. So I had gone through a period when all the techniques which once were considered too difficult for sea use became superior to use at sea. Teddy Bullard played a significant role in almost all of these developments. Heat flow is one of them.

Doel: And were heat flow measurements then done routinely in the subsequent expeditions? On Capricorn, for example?

MUNK: Heat flow was done very actively on Capricorn. [Conversation with Judy about young lady, daughter Edie. Judy invited to join discussion]

Doel: We had been speaking a moment ago about heat flow work, and the contributions that Teddy Bullard had made in inspiring new forms of instrumental development here.

MUNK: Yes. In fact, that's one thing we did better than Maurice Ewing. He came later. Most things he had done earlier.

Doel: Heat flow was one of the sticking points for Lamont's development. In fact, I seem to recall that Lamont had borrowed one of the instruments from here, but it hadn't worked out very well for them.

MUNK: He never admitted it, I think, but we had done that well and better. In our competition with Lamont that was one of the nicer developments.

Doel: Do you remember if there was much sharing of instruments between the major oceanographic centers by the 1950s, as these instruments were being developed?

MUNK: Oh, I think so. Actually. I do have a story here about [Felix A.] Vening-Meinesz's gravimeter that we don't need to repeat. That's not good sharing. I think so. I think until recently, the competition, at least between the major institutions, was constructive. Positive. I'm now worried, because we are probably shorter of money than we've been in any time in my career. Not shorter of total money, but shorter in money per investigator. And I think probably more time is being spent on making proposals by investigators now than ever. Because it has to be spread.

Doel: It's certainly becoming a significant fraction of people's time.

MUNK: Someone told me yesterday that the average investigator submits nine proposals a year.

[Tape 4, Side B]

MUNK: What I wanted to say, since you don't mind me rambling on: I think the worst thing about the present situation is not that We're short of money. It'd be perfectly right to be poor and be very careful of one's things. But then it has made our competition less constructive. More so than I remember it at any time in my career.

Doel: In part because as the resources become scarcer, it inspires behaviors that conserve what funds the donor has left?

MUNK: Well, people worried about keeping alive become less cooperative.

JUDY: I think it's uncomfortably reminding you of the Italian situation.

MUNK: Correct. That is correct. I was once in Trieste in someone's home, when they talked on the phone about a proposal by someone at another university. The person said, "Oh, it's not good. And this is wrong, and that is wrong." Then when they hung up I said, "Gee, We've just been talking about it, and you told me you thought it was good work." He said, "Ah, but he's competing for the same money." I was appalled. Because in the United States, people who competed, never do this. When somebody did good work, even if he competed, you'd say so. That seemed totally foreign to me, as it was, in Trieste. I'm afraid We're coming closer to this-- as Judy was saying. That's the bad part of being poor. Not having less money.

JUDY: A better example of that type of thing would be the student problem we ran into in Italy.

MUNK: Yes. Students in different universities were not permitted to tell about their work to each other, because each professor who had a real fiefdom didn't want the other professor to know what he was doing. So the students would get together and say, "Well, what are you doing?" And it was like, "Well, I really can't tell you."

Doel: That must have been a very different experience, after coming to here.

JUDY: You had a seminar, remember.

MUNK: I was asked to arrange a seminar on predicting storm tides, in the Adriatic and the Venice Lagoon. I was asked by the Academy, the Italian Academy of Sciences, as it's called, it's the oldest academy in the world. We arranged that, and I had asked the people that we had heard about to work on it. They all said, "Well, we'd love to come, but We're not allowed to talk about our work." But for some reason they started talking, and it was clear that they had suffered under the fact that they were being separated instead of talking to each other. I want to say again, I think our present situation is a bit dangerous, that it will be coming, to some small extent, Italianized in that sense, which is terribly bad. Much worse than being poor.

Doel: Uh hmm. Have you noticed that in other countries as well?

MUNK: In other countries, in Europe at least, the science money, as a whole, in the last ten or fifteen years has increased. It's only in the United States where it's been so very well off, that we have a diminution.

Doel: That puts it well.

MUNK: And how do we stop that? It's awful. I mean I don't mind that we have less money. I just don't want to be stuck having people becoming destructively competitive instead of constructively competitive.

JUDY: I think you would do lots more to exchange students, if you do that. Every student has to spend two years here and trade. Then nobody could keep a secret.

Doel: We've almost gotten out from the 1950s; we have a few topics to return to. But we have spoken now for over four hours today on tape, and I think this is probably a very good point to call this segment to a close. Let me thank you again very, very much for the long segment that we've done today.

[End Tape 4]

ENDNOTES

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CCR

Interview with Walter Munk
at his home in La Jolla, California

By Ron Doel
September 17, 1997

DOEL: This is a continuing interview with Walter Munk, and Judy Munk is here in the room with us. We're making this recording on the 17th of September, 1997, in La Jolla, California. I did want to ask you about an issue that we only touched on quickly in the last session, why you were particularly interested in getting to that meeting of the IGY Advisory Group in Brussels? This was the one that you had attended, I believe, with Roger Revelle.

JUDY MUNK: It was the meeting with the Russians, wasn't it?

WALTER MUNK: Yes. I think our interest was that we had had no contact at all, and the opportunity to see what they were thinking was an interest, a curiosity, if you wish. It was not so much science-oriented. It was oriented towards seeing the Russians, and seeing what they had done.

DOEL: But isn't it also in the broader sense of understanding, what another major community in the field is doing, after a period in which you've had difficult contacts?

MUNK: I think our interest in the Russians was more social — they were the enemy, and what in the world were they doing? Not to try and learn some new oceanography. That was secondary.

DOEL: Interesting. You had mentioned that in 1962, when you and Judy traveled through the Soviet Union, your impression was that their work was generally unimaginative, or routinized—not of the cutting edge?

MUNK: More so than ours. It was more like what we would expect from a government agency. I mean, we kid ourselves, perhaps, that there is more imaginative work being done at the university institutions than by the civil servants. Theirs was more the civil-servant type. Even within the Academy of the Soviet Union.

JUDY: But Walter, you hadn't heard of Brekhovskih's work.

MUNK: That's right. There were many people there.

JUDY: It was so classified, you never knew.

MUNK: We didn't know about that-- but, no to your previous question, I would like to try and answer a little bit more explicitly. I think there is a streak of adventurism, in me at least, and Judy. And there's a certain pleasure, a certain excitement in meeting with your enemies, whether they were the students who were trying to haul you down, or whether it was the Russians, or whether it was China, where it was forbidden. I remember reacting to the statement that it was

impossible to go to Albania, at about that time. I would say, "How ridiculous—nothing is impossible. "I tried to arrange a vacation to go to Albania. And I went about this with some care. I got the President of the Academy, the National Academy, to write a letter to the Albanian Academy of Sciences. You may not believe it, but there is such a letter, saying that it was very important for me to study Albanian tide gauges, which is obviously a fictitious comment, and requested permission for me to go to Albania. We were living in Italy then. But the fact was, we never heard from them. There was no response. But, I would like to just admit, honestly, that I seemed to have been driven when I was younger, to some extent, by things that were supposed to not be possible, or difficult. Like driving a car through Russia—that was not permissible, so we had to do it.

DOEL: Indeed, you did that with similar care, by selecting 6 am on a Sunday morning as your time to go across the Finnish border.

MUNK: And I don't want to pretend it was oceanographic curiosity which governed all these moves.

DOEL: No, indeed, and I understand. I fully appreciate that.

JUDY: You had both your mother and mother-in-law playing children's duty in England while he did this, you must realize.

DOEL: Roughly when was that when you made the effort to get to Albania? Was that in the '60s or '70s?

MUNK: It must be when we lived in Italy. I was in Venice, and we need to get a timeline established. [1972]

JUDY: But, that was later.

MUNK: That was much later. It was difficult to get into Albania.

DOEL: That remained so through the 1980s, indeed. I recall a reference in the SIO Archives about another kind of adventure, in the sense of your being able to drive down the hill into Scripps at one point without setting foot on the brakes?

MUNK: Oh gosh. I was even younger, and more foolish then. That was when I was a student. Tim Shepard, who was my friend, claims this, but I don't quite remember it. That's what he said. And in the book he has recently written about his father, he recalls that incident.

DOEL: This is Francis Shepard that he's writing about?

MUNK: Yes.

JUDY: You know, the Brussels incident was very interesting.

MUNK: We spoke of the Hela thing yesterday, about the poor translations, Judy. It was very interesting.

DOEL: And very important. One thing I am curious about appreciating that you had a range of social reasons for wanting to get into the Soviet Union. Did you find that kind of reaction also influencing the kind of scientific work that you did? When something seemed to be a particular challenge, you were more attracted to it than other kinds of problems?

MUNK: Yes. I told you that my rotation of the earth work started with the word 'imperceptible' being used by one of Rossby's people. I'm afraid that kind of set me going.

DOEL: Are there other instances, when you look back, other problems that influenced you similarly?

MUNK: I wouldn't be surprised. I'd have to think about it. You know, there is such a thing as being a contrarian. I'm afraid there is a streak of that. I don't know, I did not go into the major acoustics effort with that in mind. It wasn't of that sort. Oh, tides, you know, I spent ten years in tides. I mean, I was told that tides went to bed with the Victorians. That sort of bothered me.

DOEL: It was Darwin's analysis.

MUNK: Yes. And we mentioned it again this year. Things that go to bed with ancient history seem to have a way of occasionally coming back in an interesting way.

DOEL: Indeed. You were speaking a moment ago about classified research that you began to find out about in the Soviet Union. Did you get a sense of what was being done?

MUNK: Oh, I didn't find out any classified information.

DOEL: You had mentioned the work of Brekhovskih?

MUNK: I didn't learn until much later what he did. But Brekhovskih had, independently of [W.] Maurice Ewing, discovered the sound channel. When I wrote a book-- we wrote a book called *Ocean Acoustic Thermometry*, published last year. I asked Brekhovskih for an account. He sent me a letter, and it's included in the book.¹

DOEL: This is good to know.

MUNK: Everyone has found it very interesting. I had occasion to mention it to [Rear] Admiral [Paul G.] Gaffney [II], present Chief of Naval Research. It was not generally known that Brekhovskih had independently discovered it. There was no question he did; he also published

on it. But it was something that seemed to have slipped through people's recognition.

DOEL: Was there pressure in the late '40s, early '50s, to keep some of your research classified?

MUNK: Now let's see, that was after the war. But when did I join JASON?

DOEL: That would have been in the 1960s.

MUNK: Yes, '60s.

DOEL: The JASON was classified. But I'm thinking of your other oceanographic work.

MUNK: Not, before I joined JASON. After World War II ended, there was a period when I was not involved in any classified research. Oh, Bikini, of course. There were classified aspects of Bikini. Oh, and then I attended various nuclear explosions, and they were classified they did require a clearance. So I'm not correct in what I'm saying. But, other than those particular expeditions, the work I did at the time was not in any way classified.

DOEL: I'm wondering if any of the publications that you wrote were actually kept classified, for a period of time. Or does your CV really reflect the work that you were putting out in that period?

MUNK: No, I think I had just a few-- well, I had written JASON reports which were not available. Quite a few. But they come a little later.

JUDY: But you can't do classified work on Scripps property.

MUNK: We cannot do it-- we have that rule. I approve of that rule. You cannot mix foreign graduate students with classified work. It leads to continuing embarrassments, and I fully approve of the principle that we do not do anything classified at Scripps. If we want to, we can go down to MPL. They have a facility. Ever since I was with JASON, there is a so-called-- they call it TANK. A very high level of classification over at General Atomics, which is available to me. So at this time, I had every opportunity to go to other places, but it is not part of my Scripps existence.

DOEL: How long has that policy been in place?

MUNK: Oh, for a long time. And one of the problems during the student uprising was a statement by the students that it should not be permissible for members of the Scripps faculty to know what MPL was doing, that the marme physical lab part of Scripps does have at Point Loma--a classified facility. But they went further, if I remember correctly. They said that a professor at the University should not be permitted, even as a consultant, to be engaged in classified work. When I was Chairman of the faculty (I'm now getting ahead of myself-- it's your

fault) when I had the first meeting as Chairman. I did the usual thing of giving a brief introduction, and I stated that I wanted it known that I was doing classified work, that I was a member of JASON, which was considered almost as bad as having an entrance to hell—private entrance to the devil—and that I had no plans to discontinue that. I made it very explicit-- it was one of the few very sensible things I've ever done. It stopped them from being able to attack me on that basis. You know, the principle is very simple. I do believe that the United States, including the students who protested, are better off with a university connection to our military than without it. It's very difficult to challenge that principle, and I believe in it.

DOEL: Since we are now talking about this issue: when did the conflict begin to start taking place on campus? When were you first aware of it?

MUNK: Well, we have to get the years when I was Chairman, but it had started then. And as you know, it was Vietnam-related.

DOEL: You certainly were Chair during the time that William McGill was the Chancellor at Columbia. Those are the years from 1968 through '69.

MUNK: I was Chair from when McGill was chancellor at VCSD. I was Chairman for two years, I think. It's the usual term— maybe even three years. It's the usual thing, and I need to get dates straight. But, that's when I was Chairman.

DOEL: How did you come to be Chair?

MUNK: Oh, I was invited.

JUDY: Everybody took a turn.

MUNK: Well, not everybody.

JUDY: Well, I know, but...

MUNK: I thought it was my duty and privilege to accept it. There's a nominating committee of the faculty, and I had been asked to do that. I wanted to try that experience, and see if I could do it. We must talk about it. It was an interesting time. Some people got very badly bloodied. Judy played a very significant role. Judy has more guts than brains with things of this sort. But we came out well. I mean, we were a very active Chairman. We had lots of the situations here at our house. We invited the key people, like Marcuse and, what's her name?

JUDY: Angela Davis.

DOEL: Angela Davis?

MUNK: Angela Davis. And the man who wrote--

JUDY: *Soul On Ice*.²

DOEL: Eldridge Cleaver?

MUNK: Eldridge Cleaver came to our house. We played a very active role in this, and we came out all right. Some people got badly, badly hurt.

DOEL: Who are you thinking about when you say that?

MUNK: Oh, gosh. I thought that our faculty behaved badly. They would come to a decision one day, and then at the next meeting the students sent in thirty people to shout at them, and threaten them, and they would back off from what they had previously decided.

DOEL: So they seemed weak-willed?

MUNK: That's a very weak way of putting it.

DOEL: Interesting.

MUNK: I was particularly amazed that some people, who are now faculty, went out of their way to circumvent themselves for what they had done, saying, "Gee, I should have never worked with the Defense Department on this, but I didn't know any better." And, "Thank you, students, for telling me that I was doing something bad." These were people who we had sort of admired. Roger Revelle's favorite saying was, "Professors are a sorry lot." We must keep that quote. "Professors are a sorry lot." And we walked away saying that in general that is true. Now, I don't think that UCSD was particularly worse than others, or maybe it was the other groups in the world.

DOEL: These were common reactions in this period of time. It was clearly an extraordinary period. Are you thinking of colleagues who were at Scripps, particularly?

MUNK: No. In our Physics Department. Yes, I'm thinking of a few people who I think I prefer not to name, whom I knew as physicists, and who had done interesting work with the Department of Energy and others, who were then accused by the students at a meeting saying, "Why did you ever work for that wicked government group?" They came back saying, "Yes, it was a terrible mistake. I will never do it again." And the funny thing was that the students, in attacking people, went after those who tried to accommodate the students, and left me alone. I had not, at the time, attempted to say, "You know, I did something terrible." Now, Marcuse, who was the focus of the UCSD troubles, and UCSD, by the way, was probably mild compared to Berkeley and Harvard and others. But Marcuse, I had very little respect for him. Not because of his political position, but because I thought he was untruthful, and he would get other people into

trouble, and then he himself would back out and let others pick up the pieces. We can discuss that in some detail. I wouldn't mind going on record.

DOEL: Yes, I would like to hear more about that.

MUNK: I thought he behaved very badly.

DOEL: Had you known him before?

MUNK: No.

DOEL: He really came to your attention through--?

MUNK: Through the controversy. And the renewal of his contract. You know, he was not a UCSD product; he had come as a visiting professor. The renewal of his contract came during my term. The Regents were not very anxious to renew it, as you can imagine. The faculty voted very strongly that he should be renewed, and it became my job to speak for it to the Board of Regents. I went up to a Regents' meeting to request that he be re-appointed. But it was a difficult thing to do, because I had no respect for him.

DOEL: Um hmm. How did you handle that?

MUNK: I asked for him to be re-appointed. I didn't talk about my own personal views. And he did get re-appointed.

DOEL: Yes, he did.

MUNK: I thought we had done very well. And I remember coming home at night, and on the way home from the airport, I stopped at the Marcuses' house--

DOEL: It was about 11:30 or 12:00 o'clock?

MUNK: Oh, late. Thinking he would, at least, be very pleased. And I said, "I just came back from the Regents' meeting and I want you to know that your appointment has been approved." And Mrs. Marcuse said, "Why, of course. How else could it be?" And was really quite rude that I even should have bothered to think that there was anything to tell them about. I was very disappointed.

DOEL: I can imagine.

MUNK: He once went to give a talk to Scripps' students. It was very interesting. Judy attended. And he gave a talk. You know, our student body at Scripps was more conservative. But still, there was a lot of sympathy with anti-Vietnam, as there was with all of us. Marcuse said, "If you

do any work, you students, here at Scripps, which could possibly be used by the US Navy to their advantage, you are doing something terrible, and you should stop.” The students who listened to that tried to translate it. There were questions saying, “Well, what about this, what about that?” Of course, it develops that there’s nothing you can do in improving your understanding of the oceans that isn’t in some ways of benefit to the US Navy, whose job it is to operate in the ocean. Marcuse made a complete ass of himself. I mean, his statement about “there’s nothing” was just not operationally meaningful.

JUDY: And the students just went out shaking their heads.

MUNK: He left after that talk.

JUDY: And never appeared at Scripps Campus again.

MUNK: Never appeared again.

DOEL: Never came back to Scripps again?

MUNK: No. A complete failure. It was a stupid talk.

JUDY: Nor did the ‘students’ come again -- and you said that Scripps was “unarmed,” unquote. You know, they weren't putting guards on the buildings and there were windows without bars on them, things like that. And they never bothered Scripps.

MUNK: They never bothered Scripps at all.

DOEL: That’s very interesting. And there were, by this point, bars on windows on the upper campus?

MUNK: I don't know that.

JUDY: Well, there were-- it was figurative there were security measures.

DOEL: You felt the security presence much more in the upper campus than you did down at Scripps?

JUDY: Oh yeah. There were nightwatchmen, and there were all kinds of things.

MUNK: Yes. So, my chief annoyance with Marcuse was, and Angela Davis, and even, oh gosh, what is-- the two-time Nobel Prize winner chemist?

DOEL: Linus Pauling?

MUNK: And Linus Pauling.

DOEL: Was he down here also?

MUNK: He was down here at the time. They would induce the students to take some position, and then they would back off.

JUDY: And leave.

MUNK: And leave with Linus Pauling, there was a question as to whether the student body should strike—that means not attend classes—because of the Cambodian invasion. I do think that the Cambodian invasion was a wicked thing, done improperly. None of us really knew how improper it was until years later. Anyhow...

DOEL: But that was your reaction at the time?

MUNK: I didn't think that not going to classes is a very good way of preventing a Cambodian invasion. We had sort of an emergency meeting in the upper campus, and I spoke against the strike, and Linus Pauling spoke for it. Linus Pauling appeared at that meeting and gave an impassioned speech that we must not go to classes the next few days, and we must do this, and then he left town.

JUDY: He picked up his suitcase as he went out the door, and then he left town.

MUNK: And left town.

JUDY: All the students were trying to invade the buildings, and things like that.

MUNK: He had known he was going out of town. In fact, he had gotten an official leave of absence.

JUDY: He said, "For me, I'm not going to--"

MUNK: "I'm not going to classes. "He said, "For the next two days, I'm not going to teach my classes. "The fact was he had already asked for a leave of absence months ago. His statement was true, but totally misleading.

DOEL: It seemed duplicitous?

MUNK: Yes. Angela Davis, for the same meeting, had dinner the night before with us ahead of the meeting on this same issue. And I talked to her about it. She was right in this room at dinner. She agreed to some kind of a course of action. I would have to reconstruct it. And next at the meeting, she did something just opposite of what she agreed to. I was just amazed, and I

asked her afterwards, I said, "I thought we had a discussion of this, and you said you would do this and that." She said, "You have such an old-fashioned idea about..." or was it? "...an old fashioned idea about truthfulness." The idea behind it was being consistent in telling the truth is not a significant factor when you consider social ills. The means justify the ends. She was amazed that I was even bothered by the fact that she had said one thing the day before, and did something else the next day. Those things were really an interesting lesson.

DOEL: Did all this gradually develop through the mid-1960s? Or did these changes really come very, very quickly?

MUNK: I think they came quickly, and starting with the Berkeley situation. I don't recall. The only previous thing of this sort I went through, and I believe we have discussed that, was the loyalty oath controversy. Have we--?

DOEL: We haven't actually covered the loyalty oath period. Only your own security issues and Sverdrup's problems.

MUNK: Oh, the loyalty oath requires a few words, because it was one of my first contacts with Revelle and Eckart. At some suitable time we should have a few minutes on that.

DOEL: We, indeed, should. And one of the things that's come clear is that you had ambivalence about the US being in Vietnam at the time. How did your own views towards the Vietnam conflict, evolve during the late '60s and '70s.

MUNK: Well, I feel like [Robert S.] McNamara, former Secretary of Defense.

DOEL: You're thinking of the book he has written about his own revelations.³

MUNK: It was a mistake. I was at JASON involved not in a leading way, but in a secondary way, of designing what was called the McNamara Fence. And do you know what I'm talking about?

DOEL: Yes, the electronic fence.

MUNK: Yes. Generally, as you know, I think that having a good Department of Defense is a very good idea, and having a good Navy was really helping all of us. So I was never part of the liberal faculty. But I thought Vietnam was very unfortunate. If anything, if you take the norm of the faculty, I'm probably on the side of being very pleased to being able to participate in the defense problems.

DOEL: I'm interested in the way that you presented yourself by saying that you were involved in activities like JASON to the student body. Do you remember any discussions with student leaders? How did the leaders of the campus unrest react to you personally?

MUNK: I came out quite well, by having made that statement. It really made me think that that's the right way to behave. I should tell you, since you're going to have to put this in some sort of sequence, that same year we went to the JASON summer study in Boulder. That story needs to be told. We learned that the students were going to invade the JASON meeting place. We had a classified activity in a space that we had rented from the University of Colorado. And the SDS students...

DOEL: Students for a Democratic Society.

MUNK: ...announced openly that they were going to have a meeting to prepare for a protest that the University would permit JASON to occupy University premises. And Judy and I learned about this. We saw the posters that said, "There's going to be a meeting at 5:00 o'clock. Please come." And we decided to go. And there was a big table and--

JUDY: In a little house that people were [inaudible].

DOEL: Where the protest was being planned?

[Tape 1, Side B]

MUNK: Yes. We sat around in a big circle. There must have been about 30 people there.

JUDY: It wasn't a table. We sat around in a circle of chairs, in a little bungalow, over where the students lived-- the student housing area.

MUNK: Anyway, the man who was running the meeting--

JUDY: Walter and I had gotten there a little early. You know, we're not exactly inconspicuous, with the wheelchair, so we don't fade into the woodwork very well. Anyway, we walked around the little room, and there was a big copy of *Das Kapital* up on the bookshelves. Walter ran his finger along the top, and it was very dusty.

MUNK: They hadn't read the book in some years.

JUDY: Then we sat back down.

MUNK: And the man from the student body, who was running it, started asking every person in the room to identify himself and what his interests were. And it so happened we were the last. And they all said, "Well, I'm so-and-so and I think this terrible thing is happening, and we've got to protest about that wicked group of professors being here." And so I came last, and they said, "Why are you here?" I said, "I'm a member of JASON, and I wanted to know what you were up to." It later all turned out.

JUDY: The discussion ended up there the last.

MUNK: And then, the next day, they broke in. We had some guards. They broke past them. They picked up a few typewriters and threw them to the floor at noon, at lunch time.

JUDY: The next day, we had heard that they were going to break into the building-- and you and another man equally vicious looking--

MUNK: Marshall Rosenbluth. Not very vicious looking. We were there, and tried to stop them from coming in.

JUDY: Physically. And all the big scientists, you know, big machos, disappeared.

MUNK: Disappeared quickly. We stood there for about five minutes arguing, but then they brushed aside us.

JUDY: They finally pushed you aside. Physically.

MUNK: Physically, and came in.

DOEL: Were these some of the same people who had been at the meeting the previous day?

MUNK: Yes. Then there were some arrests. We stayed. What was amusing is, the man who had run the meeting wanted to become a graduate student at Scripps, so he came to see me and asked for some help to get into Scripps. I don't quite remember how it came out.

DOEL: Did he come, eventually?

MUNK: I don't remember.

JUDY: I think he applied. There were never any hard feelings. I mean, he was a charming man.

MUNK: But, you should recall that there were some people who really were greatly troubled. People would give talks. I remember Roger Dashen—no longer with us, a wonderful scientist—giving a talk in some other country, and being actually howled out of the room. Couldn't give his lecture. He had been invited.

JUDY: The meeting had turned mean, really. The reception had turned mean. Not just howling.

MUNK: Ugly. So, it's not just that people were actually physically threatened, when they appeared at many of the JASON groups particularly. The reaction of the JASON leadership was to keep the names of the JASON group secret, which I thought was a mistake. Just from my experience. I thought the way to handle it was to be up front.

DOEL: That's a very interesting point. Was there a significant discussion about keeping JASON's name closed, or were there others who supported your argument?

MUNK: We were in the minority. And I think the general view was that anybody who didn't want it would have priority, that nobody's name should be released. Even if only one person was against it, that person's name should be protected.

DOEL: Gordon McDonald was already involved in JASON at that time. How did he feel about those issues, as you recall?

JUDY: About like you, I think.

MUNK: About like me.

DOEL: I'm very curious about what finally happened at that protest planning meeting that you had attended.

MUNK: Oh, they were making strategies as to how to break in.

DOEL: Did they engage you about JASON?

MUNK: No, they thought it was appalling that I was there, and they didn't know it. Nobody had bothered to check. They thought I was a sympathetic faculty member from the University of Colorado who was going to join them.

DOEL: So once it was clear, it didn't lead to discussions, as had happened here on the campus?

MUNK: Oh, there were many discussions, and some very interesting ones about whether one should do what some of us were doing. Yes, it was a real...

JUDY: At one time, I remember there was a young man here that got himself put-- did he get put in jail, or something like that? He was just a youngster who was mixed up. He was behaving very badly. And you found out that his father was the Naval Officer down at...

MUNK: His father was an Admiral.

JUDY: He was in command, I don't think he was an Admiral.

MUNK: Yes, we called him; he was a retired Admiral. The student had written a particularly nasty letter to me, as Chairman, with four letter words and so on. He had a funny name. And I found out, one way or another, that his father was a retired Admiral, and I called his father and asked him about that. His father just was very apologetic about it. He obviously wasn't very

happy. But then he said, "Well, why aren't you guys more strict with your student body? Why are you permitting all this to go on?" I think, Judy, we're going too far away from interesting things.

DOEL: These are significant moments in the history in the 20th century, and you were witnesses to these broad developments. I should note that there is an oral history, the one that Finn Aaserud did with you that covers certain parts of JASON, although I'm not sure that these particular developments are mentioned there. I don't believe that they are. But, we'll make a footnote to that interview.⁴

JUDY: The interesting thing about his father's discussion was he looked upon going to college as though you were in high school.

MUNK: That we should be fathers, not professors. What is it? There's a Latin word the faculty is supposed to be--?

DOEL: *In loco parentis*.

MUNK: *In loco parentis*—that was his reaction. But it was a very interesting time, and as I said, my overriding beliefs are that the work of the faculty participating with the military is the strength of the United States, and even people who are anti-military should accept the fact that having university participation was a good thing. And maybe because I'm an immigrant, and rather appreciated in my later career, that I became involved in a significant way perhaps, made me more-- may be partly responsible for the view that I do have.

JUDY: There was a very serious set of conversations that you used to have with your mother.

MUNK: My mother, who was against--

JUDY: Marched in Washington against the War. And she was an older woman at the time.

MUNK: And she was very sincere. The Quakers had their view. And yes, they were against war in any circumstances. I didn't think one had a choice after Pearl Harbor.

DOEL: Did that become a difficulty in your relationship with your mother?

MUNK: Not really. We agreed to disagree on that. But at the same time, you see, we wanted to learn about the Russian-- the Soviet situation. To me doesn't make much sense, but that's how it is.

DOEL: How did these controversies in the late 1960s affect Scripps?

MUNK: Well, Scripps certainly was, within the University, the part that had the least

controversy.

JUDY: It was a graduate school; it wasn't emotionally unstable,

MUNK: But Roger, of course, who had hoped to be the first Chancellor, and was not, has always wondered about how he would have conducted himself as Chancellor under those difficult circumstances.

DOEL: What did he say about that, that you recall?

MUNK: Oh, his views were similar to ours. I remember his coming as a visitor to the one of the faculty meetings that I chaired, and saying afterwards that he was... I think what he said was that he was proud that he had gotten into Scripps or something. I think he felt that way. He always, you know, has been very liberal. He's always taking his own--But liberal isn't the right word. He's always been taking his own position, on everything. So, with that in mind, we do need to speak about his role in the loyalty oath controversy.

JUDY: What about the undergraduates' position? I used to go to the meetings.

DOEL: The faculty meetings?

JUDY: The student-faculty meetings. And the students were in attendance, they were open meetings. There was a very interesting discussion about the plans for the University, because these seats were always full, there was standing room only outside, and on and on. They were thinking of replanning the rooms where the faculty would meet, because there was never enough room. Well, Walter always said, "Well at least I don't speak to an empty house." Then four years later, at the faculty meetings, there's nobody there.

MUNK: If you have your choice between being Chairman when you had great difficulty getting a quorum, and being Chairman when you're the head of hostile stand-only crowd. I'll take the latter.

JUDY: But the crowds were very uncomfortable, especially for somebody like me, and I was on crutches at the time instead of in a wheelchair. They were very threatening. And I can remember going and sitting down at about the third row from the back, something like that, in the room where they had these things. I noticed there were two youngsters that came in that had some paint cans. And they put them under the seat, and I watched that. Then I took my cane and tapped them on the shoulder, and in a loud voice, when it was relatively quiet before the meeting began, I asked them to pass me those cans of paint. And there were enough people sitting on all sides, that turned around and looked at these kids, and they had big cans of paint that they were going to heave at somebody. And I asked them to pass them back to me, and they did.

MUNK: They were so shocked that this gray-haired lady would tap them on the shoulder, they

didn't know what to do.

JUDY: But I mean, still, the things that could have gone on...

MUNK: That should have happened.

JUDY: I always thought that the attitude that McGill had towards this kind of emotional stress was sort of like Reagan's, it pushed it over the edge.

DOEL: I'm curious what you mean by that. I want to know what you felt about McGill's work as Chancellor.

MUNK: I think McGill—I have to say this very carefully—got a big charge out of being in a dangerous situation. Almost sort of looked forward to a show-down, I think.

DOEL: Do you feel that attitude exacerbated the difficulties?

JUDY: I never was able to really be positive about it, but, I was very uneasy about it. And of course, there had been... who was the President of the other university that had just caved in? That was the worst disaster.

MUNK: Yes, McGill never did cave in, but I thought he almost tried--

JUDY: It was sort of like the Berkeley situation where Reagan sort of--

MUNK: Judy's comparison to Reagan is a good one.

DOEL: That's very interesting.

MUNK: As Governor at the time, Reagan regarded it as a personal challenge. Because when Eldridge Cleaver came here that one night, we should discuss that a little; it was very interesting. Cleaver was scheduled to give a talk, and his talk lasted for 45 minutes. It consisted mostly of two words.

JUDY: Well, we had dinner with him before.

MUNK: He came for dinner before.

DOEL: What was he like, when he came for dinner?

MUNK: Very nice at dinner.

JUDY: Very nice. Attractive. He spoke beautifully.

MUNK: And then, after his speech--

JUDY: We walked over.

DOEL: From here, to the campus?

MUNK: Yes. Then he gave his speech, which consisted mostly of two words, that were, "Fuck Reagan." Which he repeated a thousand times, to thunderous applause. Then he came back here. We hadn't invited him, but he had showed up, and said, "Can I come by for a nightcap, or so?" He was sweaty and tired. And was very, you know, beautifully spoken. There was nothing wrong with his language. He knew more than those two words. It was almost-- well, it was an act. I mean, he was the revolutionary. At dinner he was interesting and spoke well, and we had a good time with him.

JUDY: It was kind of interesting because he came with his goons.

MUNK: He came with his goons. He had set up six people to stop anyone else from coming down our stairs.

JUDY: And our children, who had been having supper with my neighbor down the street, they couldn't even come down the stairs.

MUNK: Until they identified themselves.

JUDY: Until they identified themselves. And then, the man who was on the school board--

MUNK: Oh, I had a friend-- Ken Rearwin who worked for-- what's the biggest brokerage company in America?

JUDY: Paine-Webber?

MUNK: Shearson something [Shearson-Lehman]. He was head of the School Board, and he wanted to come down to talk to me about that awful meeting that had just been held. And the goons wouldn't let him come down. He said, "I'm a friend of Dr. Munk's." They said, "We don't care, you can't come down."

DOEL: Were you aware of this at the time?

MUNK: No, he called me furiously the next day. He's never forgiven me for that. He still lives here in La Jolla.

DOEL: How much time did all this take?

MUNK: I didn't get any work done. That's the only other time when I had sort of a crisis time. You operated a very different way. There's no end of things that you can do to tell your point. There's no saying, "I should do two or three things and that's it." You should talk to n people, and n is infinity, trying to get to your point. And much later that came up again when we had our problem with the environmental groups. At that stage of an operation, I find that it must be--politicians must know that well. You should talk to so many people and try to persuade them that they should help you, but there is no end in sight, and you have no time for anything else. You always do less than you should do.

DOEL: You were learning the lesson of how to operate in this new environment?

MUNK: Yes, it's a very different environment, which is totally incompatible with sitting and writing a paper.

DOEL: Yes. I was wondering if any of that experience did, in fact, age you, or change the way that you operated in the scientific environment?

MUNK: Well, it did. I think the one thing that applies is that when we ran into problems at ATOC, it meant that I didn't want to give up. The great similarities—I never thought of it—between the meetings with the environmental groups, where we were called, "Whale Killers," and the meetings at the time when we were called down for helping the military.

DOEL: You didn't see those kinds of connections?

MUNK: I never pictured it that way, but the meetings were very similar. Very angry, very noisy, lasted forever, and had no content. It was a circus show in both cases. But I suppose students' reaction would tell you that you've got to give people a chance to do that. Whether it accomplishes anything intellectual or not, people have to be given the chance to say what they want to do. To say what they think.

DOEL: Judy, I didn't mean to cut you off.

JUDY: What I was going to say was one of the examples to illustrate that point was that when we went to those environmental hearings, we stayed until the very last. Several times somebody in the audience, approaching midnight at times, would say things and they'd say, "Well, they're not even here to listen." And you and I were always there.

MUNK: Always there. That is correct. And just stayed.

JUDY: We'd put up our hands and say, "We're right here. We haven't left." They'd think the other side was all gone, and they could say anything they wanted to. Or, accuse you of not caring, because you weren't going to sit out the hearings.

MUNK: Though we were very active non-activists. And participated fully.

DOEL: We will cover this in depth. I'm glad we did establish the connection here in the moment. Thinking back to the Vietnam era, and the unrest, were there political discussions going on at Scripps that you remember? Did people like Bill Nierenberg, or Bill Menard, or others take part them?

MUNK: Bill Nierenberg, of course, was on one extreme of the political spectrum. Bill Menard, no, he was not. I think the answer to your question is: remarkably little. I'm sure there was some—there was certainly some. But, it would give the wrong impression to emphasize it. I think Scripps, as a whole, had surprisingly, and maybe disappointingly little, discussion along that line.

DOEL: From what you could tell, did that seem shared among other oceanographic institutions in the country?

MUNK: I think so. I wish you would ask that question to other people as well. I think so. Well, it's partly because we had been accustomed to working with the Navy. And all oceanographic institutions. We were aware of the fact that the Navy is not totally staffed with wicked people. We also were aware of the fact that most high ranking Navy professional officers really took a very dim view of weapons of mass destruction, because it's not a very honorable way of conducting yourself, and that's where that we had some meaningful understanding with them. If you understand what I mean.

DOEL: I do. Did any of your graduate students get actively involved in protesting the war?

MUNK: Remarkably few. Again, maybe yes. But, the important answer is, hardly at all.

JUDY: And after they heard Marcuse's arguments--?

MUNK: He defeated his views rather successfully when he gave a talk.

JUDY: I mean, it really was interesting to see everybody just walk out.

MUNK: They walked out because he didn't make any sense. There were many people in the audience who were sympathetic to the fact that the Vietnam War was a terrible war. But his argument was totally irrelevant. It was impossible to carry it out.

DOEL: Yes. They were cognizant that virtually all areas of study could have broad implications, or could benefit the Navy. That was what defeated his argument?

JUDY: He was suggesting that they shouldn't work on anything that--

MUNK: We've talked about that, Judy. But you know, I must try-- I do have to fit that in, to give you the experience we had during the loyalty oath controversy.

DOEL: This is a good time, because you're thinking about it now, and I do want to cover that period.

MUNK: It was the second year. I think it was either '39 or '40, you have to reconstruct that-- '39 was the first time I was down at Scripps, in the Summer, and '40 was the second. So, we have to reconstruct that. Roger led the campus here, which, of course, was only Scripps. In opposition to the loyalty oath.

DOEL: Just to be clear, this isn't the post-war period?

JUDY: I was married to you, so that was later than that.

DOEL: I think we're talking about the loyalty of the issues in the 1950s, when the Regents, early in the Cold War, were applying standards throughout the California university system.

MUNK: Yes, yes. It was much later. I'm sorry. Anyway, whatever year the loyalty came up. It was after we were married. And that, of course, would have been after 1953. Roger came to me one day and said, "I think you ought to learn how one handles these things. I'm going to be giving a talk down at Balboa Park to respond and tell them about the university feeling, and why don't you come along?" And he, Roger, Carl Eckart, and I, drove down. It was in one of the rooms in Balboa Park, there was to be a meeting about loyalty oath. Roger gave a talk, very convincing, about why it was a bad thing to ask people to swear that they were not communists. That first of all, it was not effective, because communists would swear that they weren't, and they were. Secondly, it was not very honorable. Afterwards he sat down. He was quite pleased with the talk he had given. Then one man in the audience with these short sleeves, got up and said, "Dr. Revelle, are you a Communist?" And Roger said, "No." "Then, why the hell don't you say so?" And the audience applauded wildly. Roger had completely failed to make his point. It was my first lesson in American politics. I remember that very well.

DOEL: How did you feel about the loyalty oath matter when you heard about it?

MUNK: I felt it was stupid, and unnecessary, and undignified. But I wouldn't have, been very active in that, except for Roger's leadership. It didn't bother me terribly to sign a piece of paper saying I'm not a communist. Which of course, is a poor reflection on me. But Roger felt strongly about it.

DOEL: Did Roger feel more strongly than others, at Scripps, at the time? Was he the one you felt was the leader of the opposition?

MUNK: He led the opposition. It's typically Roger. When some cause came up, then he

became the natural leader. Not always very effective, but I have to tell you another story, if you want amusing stories in the eventual thing you're going to produce. We went to a cocktail party somewhere, and there was a retired Marine General named "Howling Mad Smith." That was what he was known as; he was General Smith. He had been called that, a famous man, and nobody would question Howling Mad Smith's loyalty to the United States. Roger had met him somewhere. He lived in La Jolla. And asked him if he would help the university, in understanding the position as a friend in court. And Howling Mad Smith hadn't responded. Then the problem came up at a private party, and there was a discussion of that point. It was a very open discussion. There were two ladies at the meeting, and they turned to General Smith and said, "General Smith, don't you think it's terrible that these professors aren't willing to swear that they're not Communists?" And Howling Mad Smith's fist came down—we didn't know what he was going to say—and he said, "Lady, would you be willing to swear that you're not a prostitute?"

DOEL: That must have generated some reaction.

JUDY: It was hysterical.

DOEL: Were you there too, Judy?

JUDY: No, I missed that one. But the next one I was there.

[Tape 2, Side A]

MUNK: Now going back to when I was faculty chairman. There are two things that I'd like to add to this if that's all right?

DOEL: That's fine.

MUNK: One was, that there was a man named Hunt from Texas who had just built the high-rise that's known as 939. One high building in La Jolla. At the time it was built, there was a great deal of opposition about building a high-rise. It changed the whole spirit of town. A great deal of opposition. But it sneaked through the City, and it was built. Hunt had written a note saying to me as Chairman of the Faculty, why wasn't I doing what the vast majority of the residents in La Jolla wanted, telling the students to go and keep quiet? Judy and I went to his apartment at 939, went to talk to him. We had a very strained discussion about why wasn't I telling the students to shut-up and do what they were supposed to do. When we knew that nearly all-- that 90% of the La Jolla population was against the terrible behavior at the university. And I said, "Mr. Hunt, I understand that 90% of the population was against the building of 939; why didn't you stop building it?" He turned absolutely white with anger. And we left.

JUDY: We had been invited to a party, at 939, his ADT. Where he was going to discuss this, in this room of people.

DOEL: So, this was a broader discussion going on?

JUDY: Right, right.

MUNK: And I guess the other story that is true, that we used to go to meetings that had been announced, where the University position was to be attacked. We thought it was our job, as Chairman of the Faculty, to be represented at this. And there was an announcement of a meeting in Pacific Beach about discussion of the university's position. There was a man who is back at the university now named T. George Harris. He used to run a magazine called *Psychology Today*. He's a member of the university community. He had dinner with us, and we saw that in the paper, an announcement of a meeting. I said, "Let's go and find out what they say." We went to that meeting, and it was a meeting of people like the retired chief petty officers of Pacific Beach.

JUDY: There were about 150 people there.

MUNK: And almost unanimously against the University position, of not being willing to swear that they weren't communists. It was a little bit similar to the thing that I mentioned to you at JASON. We were there. We didn't identify ourselves—nobody asked us to. At the end of the talk, I got up and said, "I'm the Chairman of the U.C. Faculty and I'd like to say a few words." And, all hell broke loose. Everybody howled, "Get him out of here." One person ran up and said, "We have ways of handling people like you, and it's called tar and feathers."

JUDY: They were angry. A lot of angry people.

DOEL: Did you feel in physical danger at any point during these conflicts?

MUNK: Yes, it was threatening. And then a man--

JUDY: In the very back row--

MUNK: In the back row got up, a tall, nice guy with a lumber jacket, and said, "Don't you think you guys ought to give Dr. Munk a chance to say what he wants to say?" I'm sure he wasn't sympathetic, but he made it possible to say a few words. I'm sure they weren't very effective. Not like Roger's words. But I got out alive. And T. George Harris will tell you, it was an interesting evening. So we had our little contact with opposition.

DOEL: In both periods of time, the loyalty oath issues in the 1950s and the campus unrest in the '60s, do you feel that either of them had a significant effect on the kind of work that was being done at Scripps, or did it undermine the spirit of the facility?

MUNK: No, I don't think so. You'd like me to say that, but I don't think so.

DOEL: No, I'm not asking for any particular kind of reaction. I certainly don't want to lead you. When did the controversy over Vietnam and military participation finally seem to be over?

MUNK: By the time I was succeeded, in the Faculty Senate, it was already no longer threatening. Who succeeded McGill as Chancellor? I think, by that time, he and I had similar terms.

DOEL: McGill left for Columbia in very early 1970, as I recall.

MUNK: Oh, it was over long before that. I can't tell you how it petered out, but it petered out by then. The man who was Chairman of the UCSD student body, who had the equivalent position that I did, was a man named Tom Shepard. We made it a point to get to know him, and worked rather well together. And that turned out to be very effective. He was a-- I don't mean sympathetic to what was going on, but he was a reasonable man. He was a good chairman of the student body. We see him occasionally still, after all these years. We sort of formed a friendship at the time. I thought that was one of the best parts of those two years.

DOEL: That's interesting. How did you do that in practice?

MUNK: Oh, we called him up and asked him to have dinner with us.

DOEL: Extending, in a social way, what you were doing with other visitors?

JUDY: We had an interesting experience when we had the Regents Conference for dinner.

MUNK: Oh, during that time, we asked the Regents for dinner. There was a Regents meeting here. That was the time when the Regents had actually had some physical altercations with students on one of the other campuses. I don't know whether you remember that. And especially Mrs. Hearst.

DOEL: She was on the Board.

MUNK: Board of Regents. And of course, Dutch Higgs, a San Diego lawyer (Dutch is his nickname) was Chairman of the Regents, I think; I'm not sure. They had a meeting here in San Diego, and we thought it would be interesting to have them come here for dinner and meet some of the faculty. There was really very little good contact between faculty and Regents. We invited some key people and had a big dinner.

DOEL: There must have been quite a few people. Wasn't there over 20 on the Board of Regents, at the time?

MUNK: We probably had 40 people for dinner.

JUDY: Oh, we had more than that. I think there were about 80.

MUNK: Oh, no.

JUDY: Walter, the whole living room was filled with tables. This area was filled with tables, and we had two tables in the bedroom.

MUNK: Anyway, we had the Regents, a selected group of faculty and the Chancellor for dinner. I don't think it did much good, in the way of getting people together. But, it was a way that Judy and I wanted to try. As long as we were doing the job, we thought that we should be in contact with as many people as we could, to attempt to get some meaningful discussion going. Mrs. Hearst was rather rude, I thought.

JUDY: She was very angry. And this was before Patty was kidnapped.

MUNK: Before her daughter--you know.

JUDY: She made some remarks that I thought, that if she had been my mother-- I wouldn't have recognized her. Only my mother never would have acted like that.

MUNK: Well, she was hostile, at the dinner.

DOEL: Highly inflammatory?

JUDY: Yes, she was. I mean, her remarks were very inflammatory to any kind of situation. Anybody would have reacted in an unattractive way. We felt so badly about that, because she just really fell apart.

MUNK: That may have been why. She may have sensed that there was some disagreement within her own family. I don't know. But she was a poor guest. She was rude, and very aggressive.

JUDY: This was her personality, somebody like that would crack.

MUNK: That's not-- Judy-- that's enough on that.

DOEL: This was clearly an important period.

MUNK: In forming one's life, yes. And I wouldn't have missed it for anything. I'm glad for the chance of participating at an exciting time in the formation of this campus.

DOEL: After your time in the faculty senate and all of these activities, did you have any difficulty getting back to scientific research, or did it just come very naturally once you had more

time again?

MUNK: I really need to look at your timeline, as to what I published following then. You say that that happened in 1968?

DOEL: In '68 - '69, clearly you had fewer publications in those years. You had four that actually appeared in '68. They were likely written in '67 or so.

MUNK: Some output, yes. Notice there in '69, I had a paper that was-- this is number 115, do you have that?

DOEL: I have that.

MUNK: Called "Some Superficial Effect for Moving Sources in a Stratified Fluid."

DOEL: Yes, published in the *Journal of Defense Research*.⁵

MUNK: What do you think that is about? It's a funny title, isn't it?

DOEL: Classified, we should note.

MUNK: Oh, that was the only classified—as I mentioned here-- well, it's obviously what a submarine does, that you can see from above. *Journal of Defense Research*—yes. I think that was my only classified paper in that list. I hadn't even realized I mentioned it. But, the title was acceptable, as classified.

DOEL: For JASON research, clearly even the title would have been classified, so it would not appear in your bibliography.

MUNK: You see in *Symposium*, and *Naval Hydrodynamics*. I did not stop my JASON activities--

JUDY: Do you have a listing of the hot water problem?⁶

MUNK: That's a different era.

DOEL: Your adventures in plumbing.

MUNK: Anyway, I did not stop working in oceanography.

DOEL: That's an interesting point, because clearly you had very little time for research.

MUNK: For a while. Yes. But it came in bursts, so there would be two weeks where you could

do nothing else. So, there may be two weeks, whatever.

DOEL: Did the Mansfield Amendment have a significant influence on funding for Scripps, or elsewhere in the university?

MUNK: We certainly thought of that as being something that was very unhelpful. It didn't have a direct effect on support for my work. I should say here that I never have been held back, until perhaps very recently. But never in my main career held back by lack of money. There are many things were held back during the work that should have been done, but it wasn't lack of support.

DOEL: We spoke yesterday about the kinds of funding available to the oceanographic community through ONR and then NSF. There are some issues of that sort that I think we need address from the 1950s. I'm curious if you had discussions with Roger Revelle or others, were there concerns about the Navy being the primary patron for oceanographic research through the '50s? Was there a feeling that other branches of the government ought to become more involved?

MUNK: Well, the formation of the National Science Foundation, was welcomed as a positive thing. And the first director of the National Science Foundation was Alan Waterman, who came out of ONR. I think Roger himself took the view that by having been one of the people who set the tone for ONR, he helped bring about the National Science Foundation. So, it was a positive thing. But I don't remember any concerns. The reason there weren't concerns is that ONR took a splendid, broad view of support. I think we had some examples yesterday we mentioned.

In fact, I think ONR has been generally much broader in support than the National Science Foundation. So I don't have that feeling at all.

DOEL: Were there times when you were frustrated with policies that developed within NSF toward the Earth Sciences?

MUNK: I have been bothered that a few proposals to NSF in recent times, not mine actually but others, having to do with ocean acoustic tomography, were declined with words. You know, usually there is a review of your proposal. There were statements that I thought were very unfriendly. I thought... Acoustic tomography was, and probably still is, somewhat unpopular with the ocean community.

DOEL: Are you thinking of the biological part of the ocean community, in particular?

MUNK: No, I think that was independent of the marine mammal issue-- not marine mammals; that's an entirely separate chapter.

DOEL: This is a broader disquiet that you feel?

MUNK: It was at the time when money was beginning to be scarce, and tomography was not cheap, and there was a general feeling that you were using money that others should have gotten. And then when we got a lot of money from DARPA for ATOC, a very large sum, I think, many people felt that we were getting too much money. We're way ahead of ourselves now.

DOEL: We will get back to that. How important was the IGY for Scripps? Did it really make a difference?

MUNK: Not to me, very much. And I participated in the IGY in a very peripheral way. I think it was important more that it represented some sort of a step function of support for the field in general. I believe after IGY ended it never really went back to the previous level. In some ways IGY was used by the key people at the time, as a way of trying to improve the support structure for oceanography. I think we may be seeing something of this kind now — the year of the oceans that Admiral Watkins and others are trying to do. I'm all for it. The hope is that that will, in some ways, create a better support climate than we've had for the last five years.

DOEL: When you think of the key people who were attempting to use IGY to build a broader base, in particular who are you thinking about?

MUNK: Well, the IGY leader was--

DOEL: Joe Kaplan was the chair of the committee. But I'm wondering who you thought of as central figures?

MUNK: Lloyd Berkner. He and Revelle, and Ewing participated. Ewing was, in some sense, because of his geophysical interests, a little bit away, from the key of IGY-- no, I don't know.

DOEL: Yet IGY was conceived broadly.

MUNK: Lloyd Berkner was, in my opinion, sort of the key person who defended it before Congress, who was very aggressive. I remember thinking that he had an incredible amount of chutzpa. I've forgotten the number of millions of dollars that were asked for, but let's just for the sake of discussion say it was \$47 million. After he had talked to Congress, there was some exchange published somewhere, saying, "Dr. Berkner, would you be willing to go with \$45 million?" And he said, "No, \$47 million." As you know, these extrapolations of planning are not very precise.

DOEL: Indeed, and IGY brought in tremendous logistical support. It clearly is a major event in the history of mid-twentieth century science. How well did you know Lloyd Berkner?

MUNK: Not well, but well enough that we knew each other. I served on a committee which I think he chaired. I really want to be sure, and I've almost forgotten it. That was a very interesting committee. It had to do with the nuclear test ban. After America became worried that

the Soviet Union was cheating. It was necessary to establish a set of seismic stations to try and monitor nuclear explosions. A proposal was made by an American seismologist at the time--it's very interesting really. It goes back to our discussion yesterday. It was at a time when the community did not understand the concept of noise spectra. The proposal was based on the idea that if you build instruments sufficiently sensitive, you could do a job of picking up smaller things. Rather than that you had to deal with a signal-to-noise problem, and the noise was not instrument sensitivity, it was noise like microseismic noise, noise from other geophysical events.

DOEL: That's very interesting. You felt the seismological community didn't fully appreciate that?

MUNK: Did not understand it. Then we proposed to the Soviet Union the so-called Geneva network that we would accept as the basis for joint monitoring of each other's tests, which turned out to be physically wrong. The basic idea of being sensitivity-limited rather than noise-limited had not been understood. If I remember correctly, and it needs to be checked, after we had agreed with the Russians that we would accept certain limitations on that basis, General [President] Eisenhower had to go and say that although we had proposed and accepted, we had made a mistake, and that network was not capable of monitoring on the required basis.

DOEL: Later Edward Teller and others began to raise the question of muffling large atomic tests.

MUNK: Bethe and Teller were arguing about blowing things up in cavities. But it was a bit of a natural disaster that the American scientific community had proposed a system that couldn't work, because of not understanding the physics of signal-to-noise at the time. Then, a new committee was established, under I believe Lloyd Berkner's chairmanship, on which I served. The reason I was asked to serve is that's during the days I had been doing some work on seismic noise. I had by then learned something about spectra representation, and I served on that committee. Tukey was involved. And naturally, he was a member of the committee. We ought to go back to that. VELA Uniform is the name of that broader project. The efforts of this scientific research was involved. The story I told you yesterday, when they offered some money to the building here, is related to this. I wish my memory were better to give you a very explicit thing. But I did serve on that committee. Texas Instruments was very much involved, at least GSI, Geophysical Services, Incorporated. The exploration community was much ahead of this kind of thing.

DOEL: In terms of the instrument development?

MUNK: And understanding signal-to-noise, which is different.

DOEL: This is a very interesting point. There were a few people who raised the concern that there wasn't sufficient representation of exploration geophysicists on the original Berkner panel.

MUNK: I heard that someone from TI, was on that committee, who was intellectually the most

knowledgeable. Therefore I didn't have that feeling, because I remember this man was very active in the committee. So I served on that. I've forgotten whose committee it was. I think it was quite a high level committee. I believe that's when my contact with Lloyd Berkner started.

DOEL: What sort of person did he impress you as being?

MUNK: Oh, a little bit like Joe Kaplan.

DOEL: How's that?

MUNK: Well, he was immodest. And he was pulling no punches. I told you the story about the \$47 million. I think that describes him. I couldn't imagine having the guts to tell that to a Congressional committee-- if they had said less, I would have said, "Oh, thank you very much; that's great." And walked off. Most of my colleagues would have done likewise.

JUDY: Is that when [Hans] Bethe was coming in, or out?

MUNK: I think the Bethe-Teller controversy is a little bit different from the controversy I mentioned, that was understanding noise limitations to detection, as opposed to instrument sensitivity.

DOEL: I think that's correct. That other issue came earlier in the Eisenhower administration, and then indeed, recognition of the detection problem unfolded in the 1960s.

MUNK: The beginning of IGPP was closely timed with that, and that's why we got some of our money, and our support. Our early work had a lot to do with measuring noise spectra in the Earth. We were quite good at that.

JUDY: Is that George Backus?

MUNK: Not George, but his brother Milo, who was at Texas Instruments. Milo Backus was a significant actor and may have been on that committee.

DOEL: That's very interesting. How often did you go to meetings of the committee?

MUNK: I don't remember. It was a very active committee, because after all it was a terrible thing for the President of the United States to have to say we agreed to something, and now we're going to change our mind. We all felt that we had let him down. We, the scientific community of the United States.

DOEL: Clearly, the spectral analysis work you were developing, was coming out of your wave research. But how well did you come to know the seismological community through this work?

MUNK: Quite well.

DOEL: Who in particular did you come to know?

MUNK: Milo Backus is a good example. George Backus's brother. And Dick Haubrich, who I mentioned. He was a really great expert in the field. We started taking microseismic spectra—you'll see a few papers.

DOEL: I was just noticing one in 1963, "The Comparative Spectra of Microseisms and Swell."⁷

MUNK: Right. That was on this very issue, which had not been previously well done. So, that's where the two fields joined. Microseisms, you see, are in part due to ocean waves. And that was, I thought, a paper where some progress was made. It's very much along that line. Now, Freeman Gilbert and George Backus were in the center of doing the normal mode measurements. It's a different subject. The microseismic noise is different. But they, of course, became awfully good at applying spectral methods. But the emphasis is different. Here we were talking about continuous broadband noise. The normal modes of the Earth are very narrow lines. Although they're not infinitely narrow and discreet, one needs to appreciate that they have a bandwidth, it's on the other side they are remarkably narrow.

DOEL: But this was also a critical problem in the early 1960s, as I recall? Particularly in looking at large earthquake records.

MUNK: Yes. I mentioned to you yesterday that I personally missed the boat. I was so continuum noise-oriented that I thought the right things to look at were the time between earthquakes, but it was just the wrong thing.

DOEL: Then the community began to find it in the Peruvian records and the Alaskan records, in the mid-1960s.

MUNK: It then led to the introduction of the Backus-Gilbert method of inverse theory. It started with knowing the normal mode frequencies. Nowadays, you get thousands of normal modes at the big earthquake, but not an analysis. And the inversion meant how did you translate from the frequencies to a knowledge of the distribution of density and the elastic constants as a function of depth of the Earth.

[Tape 2, Side B]

DOEL: How much did this involve you in instrumentation development in that period? Or did the two problems really remain somewhat separate, as you did this work?

MUNK: That instruments became essential. We talked lately about LaCoste-Romberg.

DOEL: We did, yesterday.

MUNK: And Cecil Green gave us some money, he was a friend of LaCoste's.

DOEL: Did you know him as well?

MUNK: Yes. I had met LaCoste through Gilbert and others, and Burger. I was director then, and Cecil's gift was essential in getting us started. The only contribution I made was towards an attempt, stillborn, to build a DC-sized monitor, to measure seismic offsets. And even then my contribution was mainly working with a student, who tried to build it.

DOEL: This was your doctoral student, whom you mentioned yesterday?

MUNK: Yes, Bill Farrell.

DOEL: Who did you regard as leaders in attempts to understand microseisms?

MUNK: Well, I think Haubrich, who otherwise hadn't had a brilliant career. He knew as much about it as anybody at the time, and had done the best job of analysis and instrumentation.

DOEL: Frank Press, had done work in coupling atmospheric surface waves with microseisms.

MUNK: I think those are different. The main microseismic bandwidth centered at something like 1/7 Hertz. And that's ocean wave induced, rather than atmospherically induced. I think Frank's work-- well, I'm not sure but I think was some different from what I thought was the center of microseismic work. But it's true that there's somebody at Lamont, whose name you must know, who became the leader of microbarometric pressure that's in the atmosphere—who was that? One of the key old timers at Lamont. They worked on promulgation of very low frequency pressure waves in the atmosphere. Frank was also involved in that. And I knew that person, but I can't at the moment remember his name. I think it is Donn.

DOEL: We'll make sure it gets on the transcript. Did that effort come to involve people in other branches of the IGPP, or did any of the geophysical community at Berkeley become interested in these problems?

MUNK: Well, we knew [Perry] Byerly at Berkeley. There has been a long effort to get Byerly's group to be part of IGPP, which they resisted. They antedated us. They had a very good seismic program. There was no reason in the world why they should have joined. We certainly knew them and had fine relations. But, the basic answer is no. We did this locally. Nor was UCLA involved—the Leon Knopoff effort was in a different direction.

DOEL: How active did you become in any of the major national geophysical organizations: the AGU, other groups?

MUNK: I had a very undistinguished career. I had never been the chairman or vice chairman of the oceanography part of AGU. It would have probably been not difficult to play a role in that.

DOEL: Did you seek not to play a role?

MUNK: Almost. If I want to talk about being petty, I remember being annoyed at the AGU at one time, having a little battle with them about the spelling of the word "spectra," the plural of the word spectrum. If you really want to hear how petty I can be, I wrote a paper in which I discussed certain spectra, and they changed it all to "spectrums."

DOEL: Is that right?

MUNK: I threatened to resign from the American Geophysical Union if they didn't yield. And they did yield, but with a letter that said that it was not a policy change, it was an exception made. And for some reason, I really never after that felt I wanted to be active in them.

JUDY: I think he must have been having problems somewhere else along the way.

MUNK: I find "spectrums" unattractive, it bothers me. It's not a good plural.

DOEL: I wouldn't have expected that either.

MUNK: They had a policy for a while that the plural of spectrum was spectrums. Well, the answer is no. I was somewhat active in some Academy committees. I should say that more gently. I have not been active as an officer in some of the American groups. I was a member in many of them. And I have not been active on the international scene, where you can have a career all of your own. There are people who have had careers in the various international organizations, and spent a good deal, maybe a majority of their time on that. It's very time-consuming. Roger, of course, managed somehow to do a lot internationally. But I think if you do that, you really have to decide that you'll spend a very significant part of your time, or you shouldn't do it at all. And I didn't do it at all. I still have problems remembering all the international organizations, which I should know blindly, and know what they do. I have to sort of look it up.

DOEL: Did Roger talk to you about balancing involvement in those kinds of international activities with his own research?

MUNK: Yes. He expressed his disappointment, more than once, that I wasn't willing to do what he had done, which is to follow-up scientific work with the related social implications.

DOEL: What sort of thing did he have in mind?

MUNK: Oh, well, his famous example is that when he went to Pakistan with John Isaacs, to find

out what was wrong, why their tree roots were rotting, and found that the water table had gotten too high. And you see, he was willing to follow-up, but then they decided that they needed to build pumps too-- build a drainage system. Then they found that the education system in Pakistan didn't produce enough engineers to build pumps, and he followed up on the educational system. He was willing to always undertake these things which are so important.

DOEL: With the broader policy issues?

MUNK: The broader policy issues. And at one time we have to talk about the Academy's oceanography committee, because I really acted against his wishes. At the time there were two Academy committees, one on the Board of Ocean Sciences and one on Ocean Policy. We have to get the name straight. We are looking ahead again—it's up to you to eventually put it into place. But Frank Press had decided--

DOEL: As President of the Academy?

MUNK: Yes. There were a few other fields where they had double-fields, sociological fields, scientific fields. Frank had decided that they should be a single field. And I totally agreed with him. He decided to connect the two oceanographic ones into a single one. Roger objected. Eventually as a last resort, because I was known not to be a very good committee man, Frank asked me --and he said it was a last resort--whether I would Chair a combined Committee. And I did. We took some people from each. But I've always felt that the sociological element had been overblown. Roger thought I was wrong on that I had not paid sufficient attention to the sociological element.

JUDY: In hindsight, what do you feel now?

MUNK: I felt that some of the work they did was sort of busy work. I remember somebody made a study in the Board on Ocean Policy as to how many ship-days were lost by unnecessary international regulations. Of course, the answer to that is too many, no matter what you find out. I didn't think that the world gained very much by putting money into deciding how many times people couldn't do their work because-- I'm using that as a model. And I remember even from much earlier days, what I thought was a committee of this sort, during World War II. A committee was asked to make a study as to how you decide whether to send soldiers into Arctic or tropical theaters of war. Obviously this was an important issue—we were fighting both high and low latitude. And that committee, I thought, came up with the ideal conclusion. It was a one sentence conclusion. It said, "If you want to know whether you should deploy a soldier in an Arctic or tropic theater of war, ask him." I thought some of the sociological studies that the Academy had done were the non-sensical kind. I was really unsympathetic. Roger, who was probably right, thought that I was more-- and Warren Wooster and John Knauss, and the majority of people. And it's important to say I really not done their job world.

DOEL: We're talking now about the late '70s, '80s when Press was President of the Academy?

MUNK: And I was incoming Chairman of the Ocean Studies, of the new combined Ocean Studies Board, which does still exist today. One of the things I wrote, which I didn't give you in my list, was a talk I was asked to give when there was a celebration of twenty years of Ocean Studies Board, and I wrote something up.

DOEL: That would be very interesting to see.

MUNK: You can look it up; it's in my reprints.⁸ I thought we did a pretty good job at the end. I was really kind of pleased. But we did not do too much sociological work.

DOEL: I'm wondering if there were particular policy issues that Revelle felt needed to be addressed, that did come into your differences with him?

MUNK: Yes. I can't think at the moment. But he felt that it was our job as scientists, to follow-up on the sociological implications of our work, and I agreed with him. This, as you know, is a very important element in today's thinking.

DOEL: In your view, was it that there were appropriate policy issues to be addressed, but that the means by which they were being addressed by committee members didn't seem to you to be particularly fruitful?

MUNK: Maybe so. Maybe that's the key. I thought as a whole that people who were professional sociologists did not do as good a job as the scientists. That's a terrible thing to say. I hope nobody reads it. They brought up studies which you could do, but like the ones about losing ship time, they weren't really the essential ones. I felt that having two committees was a mistake, that no scientist who was willing to accept a membership in the Academy Committee and the Ocean Studies Board was a scientist who would accept that if he was not interested in broad national problems. You don't go to Washington and become a member of an Academy committee to do science, but only if you would like to participate in national policy. So to have two committees, one that was supposed to be a science committee and one that was supposed to be a social committee, didn't make any sense.

DOEL: Policy needs to be integrated into the scientific committee as well?

MUNK: Yes. But Roger felt that the professional sociologists really were a very important element of this. I wasn't as convinced of that as he was.

JUDY: May I say something about this?

DOEL: Yes.

JUDY: When you got to know Roger, his interests were so broad. I think a perfect example of his kind of thinking was when he did go off to Pakistan to do something about the water

problems, and then the water problems turned out to be an engineering problem, and then the engineering problem turned out to be a cow problem, and then the cow problem turned out to be an education problem, and then something else turned out to be the population problem. Then, when he got all down to whether you could have abortion or not have abortions, or whatever it is, he finally decided the solution was that you educated women.

MUNK: Yes. That was his basic conclusion.

JUDY: The reasoning that went from one end to the other was really what fascinated him. He withdrew from secret work, as you remember. He didn't belong to JASON.

MUNK: Oh, he was never asked to be.

JUDY: Oh no, but he got the secret clearances, remember?

MUNK: I didn't know that. I don't remember that. You better be sure if you say that. I don't think that's quite right.

JUDY: No, I think I remember him saying that he had--

MUNK: I don't think so, but I don't know for sure.

JUDY: It would be interesting to know, because I understood that that's what he told me.

DOEL: When did he make that decision, to at least pull back--?

JUDY: Well, I'm a little confused with what I think I remember. But I know that he looked at life in a different way.

MUNK: Roger welcomed, when you worked with him, a spreading of the problem into other disciplines. I did it reluctantly. I preferred to remain concentrated on one. I mean, both had their advantages and disadvantages. When you are too defused you do things poorly. But when I worked with him on the rotation of the Earth problem, and we found that glaciology and other things become important it gave him a great deal of pleasure. I was always somewhat reluctant to undertake some new and broadening parts. There is sort of a mental difference there.

DOEL: At Scripps, were more of the leading scientists following your viewpoint, compared to Roger's? How did that work out in practice at Scripps?

MUNK: John Isaacs was the Roger type. He enjoyed this very much, so was Warren [S.] Wooster more than others. I think there is Roger's differences in people, isn't it? I was certainly not the person who would dare to go into new social things, which Roger just happily accepted, and became extremely good at.

DOEL: It seems that there's a few issues that play here. On the one hand leading from the natural sciences into the social sciences and policy arena, and the broader interdisciplinary research programs as well.

MUNK: You see, he really was good. He knew a lot of biology. He knew a great deal of chemistry. He certainly knew some geology. He knew all the essentials of physical oceanography. He was the broadest representative of the ocean sciences during his lifetime.

JUDY: A lot of other people weren't that good.

MUNK: Among other things, it requires a better memory than most of us have. He had a great memory.

DOEL: It's very interesting that you say that.

JUDY: He could think things out, for instance. He would think about something, and then bring out something that he had--

MUNK: And he knew his numbers well. He remembered his numbers. He would always come with a little piece of paper, figure out how many cows per Indian inhabitant. One way or another.

JUDY: He also knew his money. He kept track of the money.

DOEL: That's interesting, that he could move from one currency to another, fairly easily?

JUDY: No, that he could move from how much something cost to how much something else would cost.

MUNK: He was a good farmer. You know, he owned some land in the Imperial Valley, and maybe we should go on record with that. And he kindly invited us whether we would participate, and we did participate with him in a number of land ventures, including a big piece of land off to the north of San Diego, and a ranch in Imperial Valley, which we had for a long period of time. Even today we are in litigation with the City of San Diego on a piece of land that Revelle bought, and my mother was a small participant (3%), which is now in litigation. It's the land where the Delmar Fair is being held. It's a big piece of land. Judy is on the Board of Directors. Roger was really quite wonderful, those problems take a very broad interest in economic, agricultural, and other problems. And he understood that, and he was fascinated by it. It was part of the same make-up. He was a good maker of money for that reason.

DOEL: His investments were successful, those that at he made in California?

MUNK: Very. I think so. He originally said he wanted to make enough money so that his first daughter, Ann, who had become an heiress from the Scripps family—she had gotten a trust of

some dollars. He said he thought it was his job to create an equivalent trust for all his other children. And he did.

DOEL: That's interesting. How often would he be in the house here?

MUNK: Oh, often.

JUDY: I still miss him. I keep looking around. If we have a good party, I look around for him to come ambling in.

MUNK: You know, he was famous for how effective he was as a recruiter. We perhaps talked about this. He was the world's best recruiter. The reason for it was—and it's in one of the references I gave you about the Revelle years—is that he became genuinely interested in what people were thinking. He just listened, and wanted to know what fascinated Harold Urey, and Jim Arnold, and there's nothing more pleasing to any of us but to be taken seriously. So when he eventually understood what people wanted to do, he created the background to make it possible for them to do so, and that's why they came here. Then he would take them all across the campus and share his dreams as to how it would be developed. At the final stop of his usual recruiting day, he would take them to our house and say, "Here's a typical faculty house."

DOEL: How did you feel about that?

JUDY: Oh, we were all torn up. We had no floors, and half the windows were out, half the roof was out—we had no roof over us.

MUNK: Even then, it was probably not even a typical faculty house.

DOEL: This is an extraordinary place.

JUDY: You should have seen it—we didn't have a roof on the house.

MUNK: I mean, saying this more or less as a joke, but that's what happened. And of course it is nice, because Judy knows how to build.

DOEL: We were talking a bit earlier about your involvement in both the national and international organizations that involve the Earth sciences. In say the 1950s, or early 1960s, as you were developing the IGPP, which centers seemed to be competitors and on the par with Scripps? Which were the major international centers that mattered?

MUNK: The place where you go to learn something?

DOEL: Yes. You mentioned England of course.

MUNK: Cambridge certainly was a key, and it always has been. The relations between IGPP and Bullard Laboratory and its predecessors were extremely close. Other places you went to. But Cambridge one is the number one that comes to mind.

DOEL: Others?

MUNK: Norway.

DOEL: Norway?

MUNK: Well, Norway very early in my career.

DOEL: Oslo was critical?

MUNK: But not after that. The Norwegian predominance disappeared.

DOEL: After the deaths of Rossby and Sverdrup?

MUNK: And didn't come back until a wonderfully active young man named Olar Johannessen started the Nanson Institute for Remote Sensing, which has been active. Please let me think for a moment about what other places I would mention. Hamburg under Klaus Hasselmann, on climate-related issues, became leading, absolutely leading. Better than any of us.

DOEL: Is this during the 1960s?

MUNK: Oh no, no, much later. When Klaus Hasselmann went and started this institute, and the institute in Hamburg. I need to find out the dates of that. You are thinking about the 1960s?

DOEL: Soon after the IGY?

MUNK: Not American places that you went to, other than England. Maybe, you know, some people like Hank Stommel always had very close French connections. For some reason Judy and I never had as close a French connection. Our connections were more Italian, but you did not go there for scientific leadership. We didn't, anyhow. Gosh, I don't come up with anything but my English example at that time.

DOEL: That's interesting. If you think in more recent decades, say the '70s or '80s, does the list increase tremendously?

MUNK: Yes, then the German thing, and Kiel, yes, became very good. And of course, we talked about Russia before. But I still don't think that that's what you want, where we, at the time, would choose to go for that reason. I don't really have much of an answer. In at least the kind of work that we were doing, we had the Cambridge group and some other American groups.

DOEL: You mentioned Hank Stommel had French connections. Was it particular to him, or was it Woods Hole more broadly?

MUNK: I think that was him. He had some very nice French students. He always has been a little bit of a Frankophile, like we are Italianophiles. He was elected to the French Academy. I don't think anybody else I know of was.

DOEL: That wasn't very common among the US oceanographic community?

MUNK: I don't know of anybody else. But I don't know.

DOEL: Thinking generally about WHOI [Woods Hole Oceanographic Institution], was there a noticeable change in the leadership with the program, once Columbus Iselin was asked to leave as director, in the 1950s? [*Note from Munk: He was never asked to leave.*]

MUNK: Then came Admiral Smith—wasn't he the next--?

DOEL: Smith was, and then Iselin came back for a time.

MUNK: And then he came back for a while, because he was horrified at what Admiral-- he was not very happy with Admiral Smith, and he thought he had to get back to rescue it. And then Paul Fye came in. There was a lot of internal trouble associated with Paul Fye's directorship. Are you aware of that? You know some people left, including Hank Stommel and others. And it took-- it was not until our friend from England became Director, John Steele, that they came back. We ourselves got along very well with Paul.

DOEL: What do you feel that the tensions that emerged at WHOI were about?

MUNK: Well, Paul must have made a mistake. He appointed someone to become Director of Research. He probably, from his war years, had a more directed view of research than people at Woods Hole, and oceanographers like their freedom. We're a bunch of prima donnas. At one time he tried to organize it more into a sort of a coordinated system, and Hank and many other people just got very annoyed and left.

JUDY: I think a funny paper example of this kind of thing would be that there was never a table of organization at Scripps.

MUNK: It came actually to Roger, and not to--

JUDY: Yes. There was no 'table' of organization. You couldn't find out the difference between a professor or a technician or a student, or whether they were a carpenter or something like that. The telephone book was all the same, and when Admiral Wheelock came to help Roger and needed to know about something, he made a secret table of organization, and locked it in his

bottom drawer, because if anybody had discovered it...

[Tape 3, Side A]

JUDY: ...they would have thrown him out.

MUNK: You will find a very good discussion of the Woods Hole problem in Hank Stommel's memoirs. Which, by the way, I gave you. Did you bring it back?

DOEL: Yes, it's here.

MUNK: Okay. Some day you should read that. Hank talks about it very movingly.

JUDY: When we would go to Woods Hole, I can remember when you first saw Paul Fye's little book about the table of organization.

MUNK: I was amazed.

JUDY: Everything was categorized. You just hooted, and said, "Scripps will never do this." And of course, they had to eventually.

DOEL: When did that happen, though? When did the transition occur here at Scripps, when its size really made that necessary?

MUNK: Well, I don't think we've ever been-- I think that Scripps, as a whole, is still very poorly coordinated.

JUDY: No, but you can tell, from the telephone book, what people do. And you didn't used to be able to.

MUNK: I think that gives the wrong point. I think Scripps maintains a high degree of chaos, with all the advantages and disadvantages attendant to that lack of organization. And neither Nierenberg nor Ed Frieman really ever attempted to go along the Paul Fye route, which was to have a meaningful table. I'd emphasize the other side.

DOEL: Was it apparent to members of the community here that WHOI was really being reorganized in a different fashion? Was this generally shared knowledge?

MUNK: It wasn't quite as deliberate, as you put it. I think Paul made a mistake. He probably had some reasons why he wanted to see this-- he wasn't being wicked—he's was not a wicked man. Then there was a showdown one evening. I think what Hank said is that people dug in on their position, Paul got mad, and it just went badly. I don't think one can read broad philosophical basis into it.

DOEL: Thinking just about Scripps, was there a point at which you felt that the structure, the size of Scripps, had shifted in a significant way, that things felt more like a kind of big science than they had before?

MUNK: Well, there are two parts to this. I think it happened sort of in a way that there was never one breaking point. I personally am a small place man, and yet I've contributed as much to having Scripps grow as anybody, so it's very inconsistent. I would love to see Scripps be a small place, and IGPP doing just what it did. The two aren't compatible. I don't know how to solve that problem.

JUDY: And you told Mother, when you married me, that all you needed was a bread board.

MUNK: That's correct. That's part of the inconsistencies.

JUDY: That bread board is pretty big.

MUNK: That's a good quote. I did really believe that.

DOEL: Is that right?

MUNK: Yes.

JUDY: It's been a family joke for years. Also that with Walter you have to multiply...

MUNK: I'm poor at making financial estimates. I think I mentioned that whenever I make an estimate, they multiply it by four before they start thinking about it.

DOEL: I hadn't recalled that.

MUNK: Mel Driscoll, who works out NOAA [*now ONR*], with whom I worked in the early ATOC [Acoustic Thermometry of Ocean Climate] days, once told me that when he asked for something like cost, he then secretly and quietly multiplied it by four, and then he started planning. My other partner, Peter Worcester, probably does the same. Partly because I'm sort of always hopeful, optimistic-- but you should be very realistic in financial planning. So I was always hoping things would go well.

DOEL: When you first began developing what became IGPP on the campus, did you find that the resources were sufficient for what you wanted to do?

MUNK: Well, it turned out that way, didn't it. There was a little dreaming involved. I had no idea where we'd get the money for the building, and other things. Roger started us with a certain number of FTEs and a certain budget. It was very small, and as you mentioned Louis Slichter gave a thousand dollars here and there.

DOEL: Was it Roger that had done the negotiating, or had you been directly involved in that?

MUNK: No, I think he really came up and said, "This is what I can do," and I thought it was just fine. You really need to look at the history of Anderson and Minster to get some numbers. I had always been so optimistic in saying that if one does good work, the support will come. But not really. If one had taken all the difficulties, one probably wouldn't have started it.

DOEL: That applies certainly to many ventures. But were there particular individuals you wanted to recruit to La Jolla, that you weren't able to?

MUNK: Did we have any major failures in recruitment?

JUDY: Um hmm. Hasselmann.

MUNK: Hasselmann—but I did recruit him, and he might be the most successful physical oceanographer alive today. It's a funny story. He's a very-- he's a brilliant man. And he rather naively had set himself to solve "the turbulence problem". That's a cruel way of going about it because I don't believe there is a turbulence problem. He was a very young man then.

DOEL: This is defined in terms of its application to ocean phenomenon?

MUNK: Well, no, more general.

DOEL: Even more generally?

MUNK: More generally. Then, he sort of got stuck. And he had never been to America, and he heard there was to be a meeting on ocean waves in Maryland. You can find where that was.

DOEL: One that was sponsored by the National Academy, when you were the Chair of the discussions?

MUNK: Oh, some of it, yes. I think that's it. We can locate that, among things. Klaus Hasselmann decided that he wanted to come, so he wanted to give a paper. Should we find it first?

DOEL: It's in the 1960s, I remember.

MUNK: There was a problem being widely discussed then of non-linear coupling—resonance coupling, non-linear coupling of ocean waves. Various people had tried to do that. Owen Phillips and others. A difficult mathematical problem. And Hasselmann decided it was so much simpler than the turbulence problem--and it is--that he would do that and then give a paper here. When he gave the paper in Maryland, he sounded like he was off his rocker, but he didn't know about that. He said, "Well, you know, I wanted to work with something easy for a while, so I

thought I'd solve the non-linear ocean wave resonant coupling." The room was filled with people who had tried to do that and failed. They didn't exactly like that statement. But knowing him, it was not intended as it sounded.

DOEL: But did others know him as well, to know how to interpret it?

JUDY: No, they'd never seen him before.

MUNK: He was an unknown. I was so impressed with him, and the University was so much simpler then than now, that I offered him an assistant professorship. You know, you could do that then. You can't do that now. He accepted, and he came here for a few years. We became very good friends. Eventually, his wife decided that she wanted her children brought up in a more orderly society than California, and they went back. He also spent some time at Woods Hole. He's become one of the really influential people. He started a Max Planck Institute in Hamburg, on which I served on the Advisory Board for a while. That is a wonderful place. That's the story. I failed to keep him here.

JUDY: Oh, you tried to get Hank Stommel to come here, too.

MUNK: We tried to get Hank Stommel here, and he didn't want to stay. He's really a New Englander. Then I tried to recruit—oh, I must tell you that; it should be in my record--- Chris Garrett, with whom I had worked on internal waves. Chris said, "No, I couldn't think of working in a country (he's in Canada now) where a man like Nixon can become President." Then the day that Nixon abdicated, I phoned him up and said that I had gone to some trouble to meet his condition. Would he come then?"

JUDY: And he did this at 7:00 o'clock in the morning. He could hardly wait after he heard the news.

DOEL: And what happened?

MUNK: He stayed in Canada. He's in Victoria, and he's very happy. But we're very good friends. Otherwise not too many failures. I would have liked for Chris and Klaus to be here.

DOEL: In addition to individuals, were there research lines that you had wanted to see, or problems addressed, that for different reasons you found it difficult to do?

MUNK: Well, I did have a bit of a principle in mind. Two principles. One was the one we discussed yesterday, observing the Earth in real time. The other was that since I was really basically a sailor at heart, I thought students should learn something about both the solid and the fluid parts. While I was Director, students really went to seminars in both disciplines; they no longer do.

DOEL: When did that become separate? When did people stop going to both?

MUNK: Oh, it's hard. There are so many seminars that you can't go to them all. And there's always a tendency that you go to your own field. I think that after Freeman Gilbert succeeded me, others didn't think it would be a good idea; it was just a matter of time.

DOEL: These things do tend to evolve. Wasn't there once a point in which there was a central colloquia that eventually everyone had gone to?

MUNK: I'm talking now about the early Sverdrup phase. During Sverdrup's time, it was on Thursday afternoon, or whatever, seminar by all fields, and you went to that. It was wonderful. You didn't have to make any decisions as to where to go and where not to go. I learned some marine biology and I learned some geology, and it was wonderful. It just can't be done today.

DOEL: And in Sverdrup's time, were these largely led by guests or by individuals at Scripps?

MUNK: I think mostly individuals at Scripps, but some guests. I'd say 2/3 and 1/3.

JUDY: And one place where that's been essentially kept up with is at DAMTP they do that.

MUNK: Yes, the Division of Applied Mathematics and Theoretical Physics at Cambridge, called DAMTP, they have a seminar to which everybody goes. They also go to tea every afternoon and to coffee every morning. That's an interesting story. After being at Cambridge and enjoying that, I tried to get us all to go to at least a coffee break in the morning at IGPP and another coffee break in the afternoon. I tried very hard. And at one time Judy and I offered to pay for all the deficit, because we didn't want people to have to pay—they could just put in a quarter. There was always a deficit, and we said we'd take care of it. We never succeeded for people to start doing that and coming in. And at Cambridge, you just do that, coffee time, tea time.

DOEL: That was an important moment where people could hear what others were doing.

JUDY: And we never were able to do that.

MUNK: We never succeeded.

JUDY: I don't think anybody else does either. How does TGIF go?

MUNK: I don't know if other parts of Scripps have been more successful than I have. But we failed. At one time it was going well, but the deficit became so large that it became a real financial burden that Judy and I had to pick up, like \$50 a month or \$100 a month. I thought the way we could solve it without being petty was to go away and see what happened. Instead of saying, "I now refuse to." So we went to Cambridge, and by the time we came back there was a

coffee break, and somehow or another the financial situation had been solved.

DOEL: So, it did succeed then?

MUNK: Well, for a while. In the early days it did lots better.

DOEL: Was this the '62 sabbatical that you're thinking about?

MUNK: The sabbatical at Cambridge. Was it '62, the first time I went to Cambridge?

DOEL: 1955 was the first time, but that would have preceded IGPP.

MUNK: It must have been the '62. And I loved those breaks at Cambridge. I always go to them.

JUDY: But you don't go-- I mean, the latter part of you being Director, you didn't go. Other than your own, you didn't go there.

MUNK: Yes, it just didn't work here. I probably should have worked harder at making it go.

DOEL: Was there another venue where people did get to talk to one another, at lunch for instance?

MUNK: Well, for a while. I probably am the wrong person to speak. There are groups which meet every day, at lunch down at the Scripps snack bar. And there is a TGIF (Thank God It's Friday). For some years now I have not been a significant part of that.

DOEL: In the early days of the IGPP, as you were developing it, what were the ways in which people got to talk to one another?

MUNK: We used to have everybody here for spaghetti dinner once a month. Everybody. And it meant *everybody*: the secretary, the people in the shop. When it became so big that you had to make a choice of whom to invite and whom not to invite, it became unpleasant, because you hurt peoples' feelings.

DOEL: When did that happen, roughly? How long after the founding?

MUNK: I would say something like year five after IGPP was started, when the number became larger than 30. You know, we could give spaghetti to everybody when there were 30. There is just no really good way to make distinctions of whom to invite and whom not to invite, without hurting peoples' feelings.

DOEL: That's a very important point. It was difficult, I suspect, to find an alternative as the

institute continued to grow?

MUNK: Yes. But we were closer then, in a way. But it must be said for Freeman Gilbert and John Orcutt that they I think have succeeded, surprisingly, to keep a good spirit going at the IGPP, even today. We are more than 100 people—I don't know how many we are.

DOEL: What in particular are you thinking about, the activities that they have done, that you feel have been successful?

MUNK: To get good people to come here. It's really the only thing I took very seriously. I didn't bother about money or things very much, but I tried to work hard at getting good people, and taking them seriously. Freeman has a wonderful taste for people, and John also a great taste—that tradition.

JUDY: Cecil Green made that possible.

MUNK: Cecil helped a lot because we could invite people to be here for a few months, and get to look them over and see whether they were the right people to ask. And for them to look us over.

DOEL: As you noted yesterday, this was because of the funds for short term visitors--?

MUNK: He formed the Green Foundation.

JUDY: And that's for all ages. It doesn't just mean--

MUNK: We mentioned that we alternated between more famous and others. But of course recruiting, hopefully it was mostly on the younger side. Though Teddy Bullard became a card-carrying member of IGPP for many years. Very effective member of IGPP, after he retired from Cambridge.

JUDY: And then Frank was here a couple of times.

MUNK: Which Frank?

JUDY: Last name Frank, from Bristol. Charles Frank.

MUNK: Oh, he came on here. He eventually got a Nobel Prize.

JUDY: Two of them. He was here for two.

MUNK: I think not. Anyway, we had some very distinguished visitors; that's the key point.

DOEL: In addition to Teddy Bullard, whose influence was certainly great, who else among the senior visitors made a very large impression?

MUNK: Oh, we must go down to IGPP and look at that list. It's a list of Who's Who.

DOEL: Yes. I was just wondering who you recall, as you think back?

MUNK: Oh, about 40 people it would take such a small effort to go through that list.

DOEL: Okay. But [J.] Tuzo Wilson's visit, it seems, was important?

MUNK: Oh, he's a symbol of successful people we had. But more interesting, we had people who were not that famous and became famous.

DOEL: Who are you thinking of now, when you say that?

MUNK: Almost everybody. Our first, Herbert Hubbart, who runs a group at Cambridge now. I would do you a disservice...we should look at that list.

DOEL: That's fine. We'll take a look at that before the next segment.

MUNK: It's an international list, and it's been very good.

DOEL: Who made the selections of people to invite, in the early years, under your directorship?

MUNK: Oh, the Director.

DOEL: You.

MUNK: In the way that people would come by and say, "You know, so and so has done interesting work; wouldn't it be fun to have him here?" I'd say, "Oh gosh, yes, let's go write him a letter." Now there is a committee of three, consisting of Gilbert, Orcutt, and Munk, who respond to peoples' requests, including their own. So it has never become very democratized. But Orcutt makes the key decisions.

DOEL: Of the graduate students that you've trained, who was the first?

MUNK: Yes, Chip Cox. He's certainly been successful.

DOEL: I have a list, in fact, of some of your graduate students. Gordon Groves, Earl Gossard, Charles Cox, whom you just mentioned, and your first female graduate student, June Patulo.

MUNK: But it goes on. This is just the beginning, isn't it?

DOEL: Indeed. It goes on to include Mohammed Hassan, Gaylord Miller, Mark Wimbush, Jim Irish, Jim Cairns, Gordon Williams, and Peter Worcester.

MUNK: Not all of those have been... I wouldn't say that that list was one I thought about as being a particularly successful group of people. Many of those sort of left. I think you would find that if you looked over Henry Stommel's list of students that they'd done better.

DOEL: Is there a set of factors that you feel helps to account for that? If, indeed, there is that kind of distinction?

MUNK: I don't think that, certainly in the last ten years or twenty years, that I was the one that people who came here wanted mostly to work with. And I, unlike other people, also thought that at the very most I could manage was three people. There are some professors who have larger numbers of students, and who somehow manage to handle that. I cannot. At least, I would not do so.

DOEL: You preferred to keep it a small group, at any one time. Three was, you felt, the maximum?

MUNK: But I have not been a terribly successful teacher. I would not say that that has been a great strength. Chip Cox really became successful because he's so good. Some of the people on that list became successful. Listening to them, I was hoping there would be some other names of people.

DOEL: This may be a somewhat dated list.

MUNK: But, it doesn't seem to me to be that I'm that big of a success as a teacher.

DOEL: Which students were particularly important to you, as you think back?

MUNK: Oh, they all were close friends. Gaylord Miller, who died of cancer, meant something to me.

JUDY: And George died also.

MUNK: He was not my student, Judy.

JUDY: I thought he was.

MUNK: No. Cox is probably the most successful scientist of my students. But there are others who probably have bigger careers. I worked well with post-docs, I mean, I think I had something to do with Chris Garrett's career. I saved him from doing a mathematical job when he came to Scripps that he wanted to do and would have been less interesting than the work we did. And

Klaus Hasselmann: I had some effect on his career, but he already had a degree in physics.

DOEL: As I recall, June Patulo also died fairly young.

MUNK: She got a reasonably good job, but she was not a brilliant innovator of oceanography, as it happened. There must be some other names. I really need to think.

DOEL: We'll make sure as we go through the transcript.

JUDY: How about our professor at MIT—our Italian friend?

MUNK: Oh, Paolo Rizzoli. Yes, I was on her committee-- I wouldn't call her my student, but I helped that she is a successful professor at MIT. I was on many, many committees. You know, I think that you looked at my students too narrowly. There usually is one professor who is influential --I've been on many, many committees. There, I think, you have a much broader list of people. But I can't say, Judy, that I've been a very successful teacher. When I warmed up to giving lectures I usually did a fairly good job. I sort of stopped not wanting to do it, and then doing it, and then finding it great fun to talk to people. And sometimes doing a good job.

DOEL: How big were those seminars that you would give? How large were the seminars when you were teaching?

MUNK: At Scripps they were not large.

DOEL: Would it be half a dozen students?

MUNK: Maybe a dozen. Probably more like a dozen. I mean, that's seminars, but I mean, I gave courses. I used to teach almost all the courses at Scripps, because there were so few of us. I taught waves, dynamic oceanography, and others. And many of the people who took those courses are all over the country.

DOEL: Did you tend to lecture more, or did you ask the students to give presentations on different areas of research? I'm just wondering what your style tended to be.

MUNK: The way Scripps runs is by making your students your partners. And I did do just that. You will find that I published with many of the people whose names you have. They became real partners. That's still true today even. I mean, throughout my career, I became close friends and worked with them, rather than the lecturing part—it was not as important.

DOEL: Virtually from the start of your publishing career you've published more papers with co-authors than you have as a single author. That wasn't the trend generally in the Earth sciences, until more recent decades.

MUNK: I always enjoyed working with people, and did more cooperation with others than was the case. I can say that unlike some people in the field, I never permitted myself to be a co-author, unless I felt I had done a very significant part of the work. There are some laboratory directors who have been named on all papers, or many papers. I really believe that I have not done that. And I don't do it today, when people offer.

DOEL: Um hmm.

MUNK: Yes, Judy? I know I've done that, because I've turned down being co-author many, many, many times, when I've thought that I'd been just a small part of the research.

MUNK: Yes, the one person you're thinking of that had his name on so many papers, was [W.] Maurice Ewing.

DOEL: I was wondering about that.

[Tape 3, Side B]

JUDY: Most people would agree that that's the case. What you just said.

MUNK: Oh, that what I said was true?

JUDY: Yes.

MUNK: Spiesberger is the one who thinks that claimed credit--but I never heard him say that. We had some differences of opinion. A former student of mine, his name is [John L.] Spiesberger. We ran into some problems recently because he thought maybe that his own... he was my student. That the idea of using acoustics for climate should have gone to him. And he made... But I think what I said is correct.

DOEL: That's interesting. And I'm curious about that perception of [W.] Maurice Ewing, that your feeling was that he had attached his name to many more papers than he had directly participated in.

MUNK: Perhaps-- you know, he was such a powerful man. I really don't know. I really hate to say that. There must be people in Lamont [Lamont-Doherty Earth Observatory] who can make that judgment.

DOEL: The one thing I'm curious about is just how people in the broader oceanographic community regarded the practice at Lamont only in first occurrence?

MUNK: I thought that what I just said was felt by others as well. But, I mean, that's something that one can be objective and careful on, and I've never made a study of that—let's put it that

way.

DOEL: As you say, there's a lot of gray area about what constitutes a significant contribution to a particular paper.

MUNK: Yes.

DOEL: I was also wondering as you think through the Sverdrup years and then the Revelle era, and later other directors at Scripps, what did it take to be a successful director, at Scripps?

MUNK: Well, Revelle was clear. He exercised leadership, took us to sea, brought some support, had a broader interpretation of what marine biology means. Was a real leader. He had the secret of leadership. Certainly it was not because he was a good bookkeeper or good administrator, whatever that means. But I think he was an excellent administrator, in my definition of the word. Nierenberg, I thought, had a very good first ten years when he came. He pioneered the deep sea drilling, he took a very active part. Roger had not been successful in getting better laboratory work going at Scripps. He, Roger, was not much of an analytical laboratory man. Bill Nierenberg appreciated good instruments from his earlier career, and did a lot to improve the instrumentation. And, of course, Bill was a natural in recognizing the emerging importance of computer work, and again, supported that.

DOEL: That was also something that you were interested in, within IGPP.

MUNK: Yes. But Bill really, as Director, made the transition. Probably earlier-- maybe earlier than any other institute. And then I sort of fell out with Bill for the last half of his directorship, because I thought it was difficult to disagree with him graciously. Those words are sort of important; I'd like to put it that way. It was difficult to disagree with him graciously. He was a young English friend, who has gone another way. There were times when I disagreed and it became unpleasant to disagree with him.

DOEL: Because of the way in which he would react?

MUNK: Yes, the disagreement. I think I'd like to put it that way.

DOEL: I'm curious what sort of issues were at the heart of those disagreements, when you had them with him?

MUNK: Ummm. It's sort of a difficult thing. There were times when he could be very rude to people. I was on the Executive Committee, and Bob Fisher was in charge of our ship operations, and Bill would shout at Bob and be very rude to him. He'd get very mad with him, and I didn't interfere. I am sorry that I didn't try and stop that. I found it so embarrassing that I kept quiet about it.

DOEL: I'm sure that was very difficult after the style of leadership that you felt was needed.

MUNK: Bob, for example, never forgave me for sitting there and letting Bill be rude to him, and Judy thinks I did the wrong thing. But that's not your question. I'm getting off.

DOEL: It is, indeed, a part of it.

MUNK: There became a problem of how to build another building, which became the Nierenberg Hall. Judy thought about it a little and made a suggestion, which I'm awfully glad didn't happen. But that's where we now have Revelle, IGPP Revelle Laboratories across from our first building. I'm so glad that... But Nierenberg had set up his mind to build across the street, with a building which I think is somewhat of a monstrosity. He was quite rude about it, when we suggested that maybe that wasn't the best way of doing it. He made up his mind on something, and I found it very unpleasant. We sort of saw less of each other.

DOEL: He was autocratic, you feel, in the way he made decisions, and then in the way in which he carried those out?

MUNK: Well, I don't know. He had a habit of sometimes speaking loudly to people, which I found difficult. And easier to avoid than to do anything about. At the same time, I must say first of all, that he was always a great supporter. He was very loyal. People were worried that a non-oceanographer physicist became Director of Scripps. They shouldn't have been worried. There was no question ever of his loyalty to the Institution, in anything he did.

DOEL: He was coming out from his NATO assignment, wasn't he? At the time that he came?

MUNK: Yes. And personally, when I had my 65th birthday, it was Bill who organized it, very thoughtfully. When I broke my hip at Sun Valley, he tried to find a plane to pick me up, because I was sort of in bad shape. He was very, very thoughtful in a way about people, but combined with being difficult under many other circumstances. I think I said that rather well, didn't I? The way I feel about it. I'm sorry that we sort of fell apart.

DOEL: I'm sure that was difficult for you.

JUDY: I think one of the problems was that Bill was incredibly jealous of Roger Revelle.

MUNK: He was incredibly jealous.

JUDY: And it never went away.

MUNK: And this, should this be in the--?

DOEL: Don't forget that any part of the interview that you would like to have closed can be

closed.

MUNK: I would like to follow Roger's criteria. I think he didn't close anything, did he?⁹

DOEL: I'm honestly not sure whether he did.

MUNK: We must find out. But we had statements about the Chancellor, and Roger did, and others, that were sort of that sort, and he just said if I'm going to say so. I think we ought to play the same game. But Bill will probably see the necessity of that. Bill has been very interested in the history of Scripps, and he's writing a biography.

JUDY: Extensive.

MUNK: Extensive one, Bill is, and he told me that he would not permit it to--

JUDY: He's writing a biography of Roger also.

MUNK: No, I don't think so. Do you know that Judy? I don't know that. I don't know that. But, Judy's statement about being jealous of Roger, which he would deny, is certainly true. And I can understand that, too. Roger had the great love of people, so many people.

JUDY: And it became apocryphal after awhile. You know, every time somebody didn't like something that Bill did, everybody had an easy way out by saying, "Well, Roger wouldn't do that." And I think that must have gotten to him somehow.

MUNK: Right.

DOEL: How well had he known Revelle?

MUNK: Not at all well. And they never were friends. I mean, they got along. Roger made it a point. But Roger was not a friend of Bill Nierenberg's. Then Roger came back when Ed Frieman became Director. He returned to Scripps after that. Ed and Roger got along very well.

DOEL: And there was the brief interim period where Fred [N.] Spiess was actually director?

MUNK: That was before Nierenberg, not before Ed. And I chaired the committee for both the appointment of Nierenberg and Frieman. And I would like to say, it was very hard on Fred Spiess, who was Director, and in some ways perhaps expected to be made the permanent Director. He was not Acting Director, he was made Director, but on a very limited term, until the committee came in. And my committee—it wasn't just me, it was a committee—decided to go to an outsider, but I was a very much part of that. I thought it was better to get someone with different kinds of experience from outside the Scripps family. I'm not sure, in retrospect, whether that was the right decision.

DOEL: How big was the committee that you put together?

MUNK: For Nierenberg it was about twelve, for Frieman it was an impossible twenty.

DOEL: That is quite a large number.

MUNK: In fact, Nierenberg was a member of it.

JUDY: He insisted that he should be a member.

MUNK: And I would like to say that Fred Spiess was hurt; he has never quite forgiven me. He's been sort of guarded ever since. I'm not so sure that he is not right. I think maybe it would have been the right thing to do-- to make him Director. He's a very good marine experimentalist. He had a very successful tenure, as NPL Director.

DOEL: He had been instrumental in building the FLIP ship, hadn't he?

MUNK: Right. And much more than that. But, he has loyalty of people at NPL, just like John Orcutt has the loyalty of people at IGPP. And he's a remarkable man.

DOEL: Was there significant debate among the committee members when Nierenberg was appointed?

MUNK: You mean at the earlier committee?

DOEL: The earlier committee, Yes. I'm curious about both committees, but first that one.

MUNK: We had more than one candidate, as always. And Nierenberg was not the first candidate. There were two others before him. I guess that seems to be par as to what's happened ever since. Galbraith was Chancellor. I think we did a rather good job by persuading Galbraith that after the committee had made a list of a few people, very few people, instead of letting it drag out, that we made appointments with all the people—none of them were local then—and he and I decided we'd spend a half of week visiting one after the other and making a decision at the end of the week, instead of the usual thing. I think that was interesting. And the first man refused because his wife didn't like America.

DOEL: He was a foreigner as well?

MUNK: He was a Canadian. And the second candidate, I don't see why it has be confidential, was Frank Press, who would have been a splendid Director. He decided that he really wanted to be at MIT. I don't remember at that time if he was already the department Chairman at MIT. You could easily check that.

DOEL: I'm not certain either. But it certainly would have been right around that time, and he would have just have arrived at MIT.

MUNK: Then Bill was the candidate. And we went to see him at Berkeley. An interesting thing about Bill is he had been head of the Hudson Laboratory, part of Columbia, and learned something about ocean acoustics at least. He also had experience at NATO as the Chief Scientist—a quite high position. Had done a good job. He was a very interesting man, and seemed to us a very good choice. He was interested in doing something broader than his physics, and accepted the job. As I said, I thought he did a very good job on the deep sea drilling beginnings.

DOEL: That's something I want to make sure that we cover, because clearly you were well aware and involved throughout...

MUNK: Not a large actor in deep sea drilling.

DOEL: But I'm thinking of its predecessor as well.

MUNK: Mohole, which in some sense preceded deep sea drilling. I was very much involved in that.

DOEL: Yes, I want to make sure that we cover that.

MUNK: But in deep sea drilling, and later, I was not a major player.

DOEL: You're thinking particularly, what becomes the JOIDES program?

MUNK: Yes. So that's what I'd like to say about Bill.

DOEL: You've mentioned, of course, the more difficult personal relations. How did you feel about the kind of direction that he gave to Scripps?

MUNK: I thought it was good. And especially the first ten years were good. Then, as I said, my objection was not so much any decisions he made, but it was difficult to not agree with him, and it made the whole running of Scripps more difficult. Others may feel differently about this.

DOEL: I was wondering if you felt tempted, at that point, to look elsewhere, or whether you felt it had reached a point at which you weren't certain whether you could remain at Scripps?

MUNK: A problem did come up by the Chancellor. I don't know when it was, whether it would be a good idea--

JUDY: No, he's asking the question whether you thought you should go elsewhere?

MUNK: Oh, because of Nierenberg? No, no. No, I never did.

JUDY: It hadn't occurred to him.

MUNK: It hadn't really earlier-- no, it never occurred to me that it was a situation that made it good to think about leaving. No. And Bill didn't--he supported me. You know, I became a Navy Chair, for example. Which was a wonderful appointment for me. Bill put me up as a candidate for that. He said I was the right person for that. He was not a petty man. Even at the time when we particularly weren't close, he did the right thing. I want to say that.

DOEL: Yes. What did the Navy Chair give you?

MUNK: \$200,000 a year, without having to account in detail how it was spent. It was tremendously free money. Not totally so, but much more so than other funds that were being distributed. I could go abroad without getting permits ahead of time. I could do almost anything that was legal and reasonable. I never felt the restrictions as they remained were even the slightest burden.

JUDY: But I think more important that that was, you felt that you weren't in competition with younger men.

MUNK: Yes. It gave me a chance and then it was renewed, to keep on working without thinking that I occupied a chair that really should go to a younger person. My university chair eventually was split among three people. And I would have been embarrassed, after a certain age, to retain the chair, when there were so many people who wanted academic appointments. But the Navy Chair made that unnecessary to worry about. So, that was wonderful.

DOEL: And we can make sure we have that accurately on the record, but we're talking about the 1980s, that the Navy Chair became yours?

MUNK: My only requirement for having the Navy Chair was to write a letter once a year to the Chief of Naval Research. I collected the letters here from my second appointment. The first one must be with Deborah [Day], because it meant that I would write a letter once a year in which I said what I thought was important. And I brought it home thinking that it would help me when we talk about the more recent things. We ought to get that from Deborah for my first appointment. Here, for example, was my letter to Paul Gaffney of October, '96.

DOEL: This is quite recent.

MUNK: I wrote him a letter in October '97, and he wrote me a very nice letter back, that I kept.

DOEL: [looking through the letters] So, this is 8th of November, 1996?

MUNK: Yes. And now the next one will be due very soon. But you see, the requirement of writing a letter, and you see, here's what I've done. It's the only obligation. It's, of course, wonderful.

DOEL: This concerns the re-appointment?

MUNK: This was renewed then, for another four years. I received it 1985 to 1990, then I had an extension from '90 until '95. I very carefully did not spend it all, and I have some balance left, and I simply ask year-by-year for a cost extension. So I have essentially occupied this since 1985. It's now in the twelfth year.

DOEL: Yes.

JUDY: The result of that, Walter, is that you no longer vote on the academic appointments?

MUNK: Well, I fell right in between the time where there was an age limit of where you have to retire as the Scripps professor. After you retired, then each department can separately decide its policy. I was appointed as RTAD, Recalled to Active Duty. The decision as to whether you can vote on a new appointment in your department is made by each department, if you are an RTAD. My department voted not to have anybody with an RTAD appointment vote on new appointments. And that kind of bothered me.

JUDY: It's not IGPP. This was Scripps.

MUNK: This was the Scripps department.

DOEL: Scripps-wide. How many people were on that status, at that time?

MUNK: I think I was one of the very few, and that's one of the reasons I was bothered by it. I think, in fact, the only other person on it eventually was Chip Cox, who thought it was a good idea to keep older people out from deciding whom to bring in. I must say, I was bothered by it, and decided I didn't want to go to any department meetings as a non-voting member, and haven't gone at all. Judy thought that was very bad. Right, Judy?

JUDY: That's one of the times I thought you were being small-minded. Yes. You let your ego get in the way, which you don't usually ever do.

MUNK: Well, yes.

DOEL: We have a number of topics from the 1950's that I do want to address. You had mentioned Cambridge when you were thinking about the more influential places where people would go for a sabbatical, or where you would think to send your students. What in particular, if you think back say to your first Cambridge sabbatical in '55, was particularly stimulating? I

believe you were attending meetings of Batchelor's group, at that time?

MUNK: Yes, and Bullard's. We had many geographically different places. Maddingly Rise is what is today the Bullard Laboratory. In the middle of the Cambridge thing, on Silver Street, in a building belonging to the Cambridge University Press, is the Department of Applied Mathematics and Theoretical Physics. The names mean nothing. Batchelor ran that department with an iron hand. Bullard, during my lifetime, ran the department, the geophysical department, up there. The first time I was headquartered out at Bullard's. The second and third time I was headquartered on the other one. It wasn't a very important thing, because you go back and forth, and I sort of always was between the two.

DOEL: Were they very different in their organization, in their style?

MUNK: The difference was the people were different. I sort of had interests in both. The first sabbatical out at Bullard's lab I wrote actually a draft of the book that was published in 1960, *The Rotation of the Earth*. I wrote quite a few chapters then. But then it was to be two or three years until it was finished, and then another two years.

DOEL: I have a copy of some of the table of contents, from that book. Some of the early chapters involved procession, nutation wobble and dynamics, formation.

MUNK: Yes, you see, the book is here of course. [pointing to a shelf]

DOEL: Yes.

MUNK: Yes. We had a lot to learn. It was really going into a new field. And the Bullard Laboratory was a good place. Cambridge was a wonderful place. That was Harold Jeffreys.

DOEL: How did you come to know Jeffreys during that time?

MUNK: Oh, he was a very quiet man; it was hard to know him well. But he was very cordial, and we saw each other occasionally. He hardly ever said a word.

JUDY: We didn't really know him.

MUNK: He became almost talkative in his very late days.

DOEL: Is that right?

JUDY: It was almost-- maybe because of Lady Jeffreys.

DOEL: Bertha Jeffreys?

MUNK: Bertha, yes.

[Tape 4, Side A]

DOEL: Were there others that you recall as being particularly influential that you met and interacted with them?

MUNK: Yes. There was a wonderful group of people with Teddy Bullard. Maurice [N.] Hill, outstanding. But many other people with whom we formed friendships with. Really a wonderful group of people.

JUDY: Michael Longet-Higgins.

MUNK: Michael Longet-Higgins comes before then. He came as soon as he got his degree, and spent the first year after he had gotten his degree here at Scripps. And spent it really with me. It was called a Commonwealth Fellowship. He, of course, has become a very senior figure. He is in San Diego now, though he is associated with a different institute, not Scripps. But still, one sees a lot of him.

DOEL: Which institute is that?

MUNK: It's non-linear mechanics.

DOEL: Is that the one-- that's on the campus?

MUNK: Henry Abarbanel is Director. There was some difficulty in getting Michael a faculty appointment at Scripps, and he was so senior that nothing else would do.

DOEL: During that time when you were in England, did you come to meet-- other geophysicists such as [P. M. S.] Blackett and Keith Runcorn?

MUNK: I got to see something-- a lot of Keith Runcorn. Blackett I met and he once came and had lunch with us, and actually walked down the beach with us. P.M.S. Blackett. We met a lot of people then. The first time I went I was associated with Trinity. And George Batchelor did that. And the next two times it was Churchill College.

JUDY: And then one of your heroes, of course, was there.

MUNK: G.I. Taylor was alive. He's one of my heros. And he's one of the people that I really admire.

DOEL: What made him one of your heros?

MUNK: Oh, he was just incredibly imaginative, in a simple way. And I define "heros" as people who do things that I couldn't possibly imagine to duplicate. Even if I had incredible luck and infinite time. And Sverdrup, Revelle, and G.I. Taylor fill that definition.

DOEL: Did you keep in contact with Runcorn as the book developed? Clearly there were areas of interest there.

MUNK: Yes. He has followed us all our lives. Judy has a joke that no matter where we visited, and we always almost ran him down with the car, because suddenly there he was. Whether it was Japan, or China, or Russia, certainly we almost knocked him down. I hate to joke now, because he came to such an incredibly tragic end, in San Diego.

JUDY: He was very sweet to me. He was interested in architecture, and he used to take me on all kinds of architecture journeys.

MUNK: In Newcastle. Yes, he went to great lengths to take you to see interesting buildings. And he introduced us to Lord and Lady Jones, who were very active in building the old Shakespeare Theater on the Thames River.

JUDY: It opened this year.

MUNK: It opened this year, and we saw a little bit of him then. He was always very thoughtful.

JUDY: I was interested in housing problems long ago, and he took me to see some of the new English experimental housing. It was fascinating. We talked about churches. We talked a lot about architecture.

DOEL: He did have quite a broad range of interests, didn't he?

MUNK: Very much so. He started out with Blackett on thinking that maybe magnetism is a property of rotation. You knew that. Which turned out to be wrong. Blackett, of course, was a fantastic character. The Americans considered him unreliable because he was very far to the left politically. It was in one of the sordid chapters that there was some difficulty in getting him a visa.

DOEL: In fact, it was denied on a number of occasions.

MUNK: Yes. Incredible, for a kind man, who didn't misuse his political friends at all.

DOEL: Did he talk about his politics with you at all?

MUNK: You knew that. I didn't know it was widely known. I don't remember. When he came

to visit, I just was embarrassed that he had troubles getting his visitor visa. But then, was it after he had come the first time that they denied it to him? Because he came here.

DOEL: Do you remember when it was, that he came here first?

MUNK: No. No.

DOEL: In the 1950s, he was having difficulties under the McCarran-Walters Immigration Act.

MUNK: I see.

JUDY: When G.I. came, it was really great fun.

MUNK: Yes, G.I. Taylor, and George Batchelor were here. That may have been during-- way back there was a meeting called Project Sorrento. There's a Sorrento Valley here, of course it's not the original Sorrento in Italy... it was the earliest attempt to ask whether one can detect enemy submarines by other than acoustic means. It's a field in which I have spent a great deal of time. Non-acoustic; it's called Unsound A.S.W. by the detractors. I think in some ways it's a field which I started within JASON, and had some very interesting history, which we probably cannot discuss. But G.I. [Taylor] and George Batchelor, and Tommy Gold, and Leonard Lieberman, were here. And there was a meeting, that I was not a member of, which was the earliest discussion of what are possible ways of detecting submarines.

JUDY: Leonard Lieberman lives next door to us.

MUNK: He chaired that meeting. And I think Ewing was involved. And that's when we first met G.I. Taylor, I think.

DOEL: Interesting.

MUNK: G.I., you know, had done the original theory on the nuclear explosions, on how the cloud goes up.

DOEL: The physics of the mushroom cloud.

JUDY: Another thing that's kind of interesting around that time was when Roger got the money from General Atomics, when they first came. That had a great influence on the beginning of the campus.

MUNK: General Atomics gave us a million dollars to help start the campus. Seriously, it was such a small sum, if you think of it. But it was an offer to the Regents that if you started here next to General Atomics, here's a million dollars. Probably had an influence in making-- in the decision they made in having--

DOEL: This was part of that broader debate involving trustee, Ed Pauly, on whether it would be in this vicinity, or--?

MUNK: Or in Balboa Park? Correct.

JUDY: Yes. But also it was interesting because it brought a lot of another kind of people to the area, like de Hoffman--

MUNK: What's-his-name, who died? He was assistant at Salk?

JUDY: Freddy de Hoffman.

MUNK: No. No. I'm thinking of-- not of Freddy de Hoffman. The woman who is very active in the theater, whose husband was a biologist?

JUDY: Oh, you mean *Civilizations*? [[Nb — is *The Ascent of Man* the actual work meant here?]

MUNK: Yes. Who did the television show--

JUDY: Right, who wrote the book *Civilizations*.

DOEL: When you said that Judy, it went out--

JUDY: You know who I mean.

MUNK: Brought a lot of interesting people here.

DOEL: Was it Bronowski?

JUDY: Yes, Rita and Jacob.

MUNK: Rita and Jacob Bronowski, yes. You are right. I'm glad you have a good memory, Judy.

DOEL: Runcorn was then doing a fairly new kind of analysis of the remnant magnetism. How did Scripps regard that work as he was developing it?

MUNK: I think he always was a little bit wild. Which other people are too, and that made it fun. I think Scripps enjoyed having him come here, and he had a following among some people, like Harmon Craig, and Ed Goldberger, and Christopher Raynes, and us. By the way, one thing we had not thought about was [John] von Neumann's visits here.

DOEL: I want to hear about that.

MUNK: Things come up as you talk. Roger tried to recruit him.

JUDY: He was scheduled to come the following--

MUNK: Well, apparently other people thought he was scheduled to come there also. But he was wonderful company. When he first built his computer at Princeton, he then talked to various people about what could we do. And the only person that really helped him a lot was Jules Charney. I remember making suggestions, which were in retrospect the wrong ones, because they were just doing something we were doing anyhow a little faster, instead of thinking of it as a revolution in the kinds of problems you can do. But Roger had a place out in Julian, where we'd go for weekends, where he would ask some interesting people. Those were wonderful weekends. Everybody sat around in the living room and talked.

JUDY: And drank.

MUNK: And drank.

DOEL: This was mostly scientific community members?

MUNK: Oh Yes. But wonderful people. And Judy and I had the privilege of seeing them many times. Von Neumann really made a great impression on me, but for reasons that are very different. He was so simple. Like many great people -- like G.I. Taylor, that's what made me think of it. There was no difficulty in talking science with von Neumann. His concepts were simple. I remember thinking about doing tidal work. I thought that maybe computers could be used to compute ocean tides, allowing for the fact that the ocean basins have a very complicated geometry, obviously, and maybe you could do a very realistic volume problem.

DOEL: The sort of thing you could not do without access to a computer?

MUNK: Exactly. That was a better idea. He liked the idea, and it has since become the way of doing it. And I remember the first time we talked about it he said, "What are you going to do about boundary dissipation?" That is still the problem today.

DOEL: He was identifying it?

MUNK: Yes. Just like that. I had forgotten about that.

DOEL: Von Neumann was also instrumental in the effort in 1956 to simulate the discussion at NSF about whether the country needed a federally-funded center for theoretical geophysics.

MUNK: Oh yes. Yes, I had forgotten that. He thought it did. And it did not come about.

DOEL: Do you remember how you felt about it, at that time?

MUNK: Oh, I could have felt anything that von Neumann wanted was good. Probably Roger was very much in favor, though my own idea would not be to start a theoretical institute, because the fun we have had at our institute, with interplay of data and theory. I actually believe that nearly all great progress followed new observations, and not new theory. Yes, the inverse problem is a wonderful example of a really significant theoretical contribution. That was probably one of the best. Still it followed people measuring the normal modes. That came first. It could have been the other way around, but it wasn't. That's the best example I can think of, where a really significant theoretical advance was made. But even then, it followed rather than preceded the measurements. In oceanography, I don't know of any really major step forward that didn't follow rather than precede new kinds of measurements.

DOEL: That's a very interesting point. And, as you say, the plan for an institute of theoretical geophysics didn't take root. Some were concerned that it would steal from existing geophysical centers.

MUNK: Roger thought really that the Institute for Advanced Study [IAS] was a bad idea.

DOEL: Is that right?

MUNK: He thought, you know, the idea that you make it so ideal for people by having them live in the most wonderful houses with plenty of money and beautiful laboratories, and no obligations... teaching obligations. Roger thought that was a menu for mediocrity, eventually.

JUDY: Antisepticolcy was not the way--

MUNK: Antisepticolcy was not the way to go. Even for people as brilliant as Einstein, it was not the way to go.

DOEL: Von Neumann was also at the IAS.

MUNK: He was there. But you know, von Neumann's mathematical colleagues were not supportive with his going towards computers. Heaven forbid, build a piece of gear that had never been-- an excellent point of just this case. And so, for von Neumann, although he stayed there--

JUDY: He just did his own thing.

MUNK: He just did his own thing. A wonderfully interesting example of that. I really agree totally with Roger that people do their best work when they have obligations, certainly an interaction with the young students. And if one doesn't want to do it, at least the Institute of Advanced Study is not the way to go. I think even they have realized that. I mean, they have taught students, and others. We've been there a number of times. That's where we met J. Robert Oppenheimer, who was still Director then. My only memory of J. Robert Oppenheimer is that

his wife said, "Oh, you come from Australia." And I said, "No, from Austria." She said, "Oh, how could I have made that mistake?" And Oppenheimer said, "Well, most Australians you know come from Austria."

DOEL: That sounds like he had quite a sense of humor.

MUNK: Oh yes. Sometimes a little biting.

JUDY: We went to a big party there, and didn't he go around closing all the windows, and she went around opening them?

MUNK: That's correct.

JUDY: Or, he opened them, and she closed them.

MUNK: No, he opened them and she closed them.

JUDY: That's right.

DOEL: This is at the Institute for Advanced Study?

MUNK: Yes. I visited there. Von Neumann asked me.

JUDY: And I met Einstein.

MUNK: Yes, Judy met Einstein under some odd circumstances.

DOEL: Is that right?

JUDY: With Eleanor, [Jule Charney's] first wife. A marvelous, humorous blond woman. We were driving around in her old car. And it stopped, the car broke down, right in the middle of the campus. On, you know, the oval lawn. So Eleanor got out and she raised up the top of the car, opened the hood, we were just looking, and suddenly I saw this silhouette coming across the lawn. Of course, I looked down at his feet to be sure that he didn't have any socks on.

MUNK: And he said, "Can I help you?"

JUDY: Eleanor and I looked at each other and said, "Oh, we don't think so." And he was just charming.

DOEL: Was he what you expected?

JUDY: Yes, yes. And you know the story about his not wearing socks came from the discussion

of his wife mending socks. And sure enough, he didn't have any socks on. It was cold, too.

DOEL: It certainly can get cold there. Thinking of that area Princeton was where Harry Hess was working. How well did you know him?

MUNK: He was at the University and not the Institute.

DOEL: That's right.

MUNK: Oh, I knew him quite well. When Roger got me into the Cosmos Club--

DOEL: Roughly, when was that?

MUNK: Oh, I ought to know that. I think that was a little earlier-- just after I became 30 or so [after 1947]. Quite young. And at that time, the Cosmos Club was really the place where you ran into the people who made decisions in Washington. It was very heady. Roger and Harry used to talk all night about geology. Occasionally I was permitted to listen.

DOEL: What were those discussions like?

MUNK: Oh, wonderful.

JUDY: Lots of martinis.

MUNK: Lots of martinis.

DOEL: Do you remember any particular issues that came up during those times that you were present?

MUNK: Harry Hess was one of the--

JUDY: Didn't the Mohole discussion start--?

MUNK: The Mohole was of course involved. But it happened I think even before Mohole. And Harry Hess and Roger and I were very much involved in the Mohole.

DOEL: I want to make sure we cover that.

MUNK: Did we cover that before?

DOEL: We haven't spoken about that yet.

MUNK: Oh, I'm going backwards. Anyway, Harry had invited us once to his house. I was a

great admirer of his. What was odd about him, he was an extremely careful laboratory worker. Very, very responsible about every detail. Yet, when he spoke about geology, he was completely a romantic and imaginative person who was throwing out mad ideas a dime a dozen. It came to me his laboratory work was totally inconsistent with his field speculation. He eventually became an Admiral in the Navy Reserve. And he discovered the guyots, the sea mounds.

DOEL: Of course, he was at Guyot Hall, at Princeton.

MUNK: I see. That's why he called them guyots.

DOEL: He was aware of the legacy at Princeton.

MUNK: I see, that's where that word comes from. An elegant man. Very nice to be with.

DOEL: Did you come to know other people in the department, or was Hess the one that you particularly knew?

MUNK: Who else was in the department? Later on, of course, I got to know the man who tried to save Mohole, who became the Chairman.

DOEL: Bill [Willard] Bascom?

MUNK: No. What is his name? What is the name of the man-- I can look that up. He was in Hess's department. A wonderful geologist. Tried to make some sense out of the mess, but didn't succeed. No, I knew all the people at Princeton. Murph Goldberger was in physics then. He was the early JASON Chairman. When I joined JASON, Murph was Chairman. He was a wonderful Chairman.

JUDY: Marshal Rosenbluth—was it Marshal then?

MUNK: Marshal was-- Marshal, of course, came to the Institute for Advanced Study, once here, once at Texas. Then he came back here. He was not at the University, I believe. I'm not sure. Ed Frieman was of course at the University. But I didn't know him then. Oh, Lyman Spitzer we knew, and he was at Princeton.

DOEL: How did you come to know him?

MUNK: Through Roger. They were good friends. And that was really the early Navy days. You know, when we first started.

DOEL: When the three of them were in charge within ONR?

MUNK: Yes. Lyman occupied a high position during the war, on National Defense Council--

NDC, or whatever it was. When I started-- you know, we talked earlier about work at NORC, Knudson was Director, but Lyman Spitzer was sitting somewhere at headquarters, probably New York, wasn't it? I forget.

DOEL: I'm not certain.

MUNK: But I really will be very happy if you succeed in making some kind of a meaningful interview transcript, because this does remind me of so many things that have happened that have been so interesting.

DOEL: This happens. Our memories are not linear in the way in which we recall experiences. When you said that this recalls particular things, I'm wondering what came into your mind?

MUNK: Oh, I've forgotten now. What were we talking about?

DOEL: You were talking about Harry Hess, the Cosmos Club. How important was Cosmos Club in your professional career?

MUNK: It was in another location. They were near the White House then. Oh, for a young person to see all the greats, and be able to listen to them, was pretty fine.

JUDY: And besides, it was cheap.

MUNK: And it was much cheaper. No longer is it, Judy, especially when I take you.

JUDY: I thought you didn't have to pay an initiation fee?

MUNK: Yes, I was young enough to be one of the few people who didn't-- after a certain age, you pay what is now, it's up and up and up, what is called an initiation fee. I did not pay one because I had gotten in sooner.

DOEL: Who else do you remember meeting? Were there others who became important in your later career that you came to meet through the Cosmos Club?

MUNK: Gosh, we met almost everybody there, who was anybody. I don't know. I don't remember. It would be kind of interesting to look at that.

JUDY: Was Bill Menard a member?

MUNK: No, I don't think so. And he was not terribly centered in my career. I don't remember. I just know we often went then, to dinner. Roger would circulate from one table to the other, because he had friends at each table. He knew everybody. Oh, O'Brien was a member. Dean O'Brien. But it really was an adventure to be there for dinner, and see people that were making

important decisions. I think the Cosmos Club has never quite been in the same arena as it was at that time.

DOEL: Because it was more central at that point. [*break*] We are resuming, after an interruption. We will be bringing this part of the interview to a close. Let me just thank you again very, very much for the long session that We've had today.

MUNK: Thank you. It turned out to be really kind of fun.

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2. Soul on Ice [1967].
3. Robert S. McNamara, *In Retrospect: The Tragedy and Lessons of Vietnam*. New York: Vintage, 1996.
4. [Finn Asserud, Oral history interview with Walter H. Munk, 1986 June 30. 1 session, audiotape, 2 cassettes, 2.0 hrs. Transcript, 38 pp., Niels Bohr Library, American Institute of Physics, College Park, MD].
5. W.H. Munk, C.G. Callan, R. Dashen, J.B. Hartle, J. Miles, W.A. Nierenberg, C. Wright, and F. Zachariasen, "On Some Superficial Effects from Moving Sources in a Stratified Fluid," *J. Defense Research*, Series B: Tactical Warfare 1, 2(1969), classified.
6. "The Delayed Hot-Water Problem," *J. Appl. Mech.*, 21, 2 (1954): 193.
7. R.A. Haubrich, W.H. Munk, F.E. Snodgrass, "Comparative Spectra of Microseisms and Swell," *Bull. Seism. Soc. Am.*, 53, 1 (1963): 27-37.
8. Walter H. Munk, "On the Ten Year Anniversary of the Ocean Studies Board", NAS Ocean Studies Board 10th Anniversary at Woods Hole, MA, July 1995.
9. The Revelle interview transcript does contain closed segments.

MMD

Interview with Walter H. Munk
in La Jolla, California
OHI

by Ron E. Doel
February 4, 1998

Ron Doel: This is Ron Doel, and this is a continuing interview with Walter Munk. Judy Munk is joining us this morning, and today's date is the 4th of February, 1998. We are making this recording in La Jolla, California.

One matter that we touched on only briefly in the first interview was the difficulties that Roger Revelle had first at Scripps and then at UCSD, the nearly missed appointment as director at Scripps and then his failure to become the chancellor at UCSD in the very early 1960s. I wanted to hear your recollections of the controversy that emerged when Revelle was being considered by [Carl] Eckart and others as the candidate to lead SIO in the late 1940s.

Munk: Yes. I have written on this in something called *The Sverdrup Years*.¹

Doel: Yes, indeed, which was only published now about six years ago.

Munk: Six years, yes. The historical community is slow to publish things. In some sense I gave this some thought at the time. What is principally in my mind is that, well, it really started with Harald Sverdrup deciding that he wanted a active seagoing activity at Scripps, which had not been the case. This had become possible because, curiously, of the disappearance of the sardine population, which meant the State of California gave us two ships, the *Horizon* and the *Baird*. Roger [Revelle] was still in the Navy and Harald Sverdrup wanted him to come home and run the seagoing operation, which Roger wanted very much to do so. But there was a lot of objection to Roger by the then faculty at Scripps. There were about 12 people, I don't know the number, and I think nine or ten of them really objected to it for what turned out on retrospect many years later to be really in some ways trivial reasons. They had to do with Roger's non-punctuality, letting people wait, being poor at answering letters. All of these things were associated with the fact that he would engross himself with whatever he is doing at the moment, which is also his most endearing quality, and perhaps you cannot have both in this world. Anyhow, there was concerted opposition.

Doel: This was by Carl Hubbs and—

Munk: Carl Hubbs and Dennis Fox, Claude [E.] ZoBell, Francis Shepard, very much so. The two people who did not join was Fred [B.] Phleger and myself. I was very young then and didn't carry any weight, but I would not sign several documents which are in our archives, letters to the president, [Robert G.] Sproul, blackballing him. Kind of funny letters in retrospect.

Doel: These spoke about him not being a good administrator?

Munk: Correct.

Doel: And were those letters were vague about these points? Did you feel it was the question of his punctuality, of his absorption in the work at hand, that people had in mind?

Munk: Yes. Of course Roger [Revelle] was much younger than they were, and they may have objected to seeing him become director. But it really was almost amusing at the time when that opposition was very active and Roger was very much aware of it. There was a faculty meeting with those people, and you might have thought that he would go out of his way to be on time because of this controversy brewing. But he was speaking to one of the Scripps sailors I think and forgot about time and came 45 minutes late, at a time when this was a key issue. And so that's what happened.

Doel: And you were at that meeting?

Munk: I was at several meetings where he didn't come when he should have. I think the decision was made by Harald Sverdrup to call in Carl Eckart, who didn't want to be Scripps' director. He was director of MPL. He surely didn't want to be Scripps' director. He was also a surprising candidate in a way. He didn't really know much about the oceans. He certainly was not an ocean-going man, which is what was needed at the time. On the other hand, he was a very prestigious, nationally and internationally known scientist. Undoubtedly the best from the point of view of prestigious scientist, the highest rank we've ever had at Scripps.

Doel: But as you say, he was in classical physics.

Munk: He was a classical physicist, who had of course during the war worked on anti-submarine warfare. I mean that was his contact. And Carl [Eckart] really regarded himself as a placeholder who would do the job for a few years and then for Roger [Revelle] to take over. And he immediately appointed Roger, I forgot the title, associate director. So, and then let Roger pretty well make the important decisions. And that lasted for a while. We knew Carl very well also. Perhaps not quite as well as Roger, but we did know his situation, and eventually of course Roger did become director.

Doel: But the opposition continued during this time. There is a letter in the archives saying as a reminder to [Robert G.] Sproul, "We haven't changed our minds."

Munk: Yes. That's a wonderful letter, and I've quoted that in a recent 6-year-old publication that you mentioned. It started out by, "Just because you haven't heard from us for two years doesn't mean we've dropped our opposition."

Doel: I was wondering if you remember having conversations with any of them? Did you try to change their minds about Revelle's directorship?

Munk: My chief memory is with Fran Shepard, whom I knew the best of the opposition. An interesting thing is that Fran Shepard's son, Tim Shepard, with whom I was very close, with

whom I'm still close, was one of my best friends. He has written a book about his father, and he has a whole chapter about this problem. If someone were to ever study that, one should read the book that Tim has written that may not see the light of day. We do not know.

Doel: It's not yet published?

Munk: No. It's written, and he sent it to Judy and me to proofread. It's called *The Beachmaster*, and it's an amusing and very extensive book. We spent two months every evening for half an hour or an hour reading it. And the relation between Tim's father Francis [Shepard] and Roger is a very essential part. Tim himself was very fond of Roger Revelle. He courted one of his daughters, and he thought his father was dead wrong. The interesting thing is that his father ten years later said he was, he had been wrong, that Roger had been a wonderful director and—

Judy Munk: Well, I don't know how truthful that is, Walter.

Munk: That's what Tim says.

Judy Munk: I know that's what Tim says. But it's always bothered him. He loved both you and Roger and his father, and he wanted to have his father not turn out to be so blinded.

Munk: Yes. He himself thought his father was wrong, and the book infers that his father eventually realized that. There were some letters that Fran wrote to Roger later in life which did indicate something of this kind— somebody has to take that seriously. But I think on retrospect the opposition of course was wrong and trivial, trivially wrong. There were no basic issues, ever—basic issues of the philosophy of how you run an institute. They were always objections on trivialities.

Doel: I'm wondering about your own role at the time -- you were junior at the SIO.

Munk: I did not join. I was one of two people who refused to write that letter. We once went to see Vern Knudsen, then chancellor at UCLA. He was a friend of Judy's parents as it turned out, but I didn't know Judy then.

Doel: Yes.

Munk: And especially went up to tell Vern that we thought Roger was a splendid choice for directorship; you know, UCLA at that time played a very dominant role in San Diego affairs, it wasn't just a neighbor. Vern [Knudsen] had just recovered from a heart operation. He received us in his house for about two hours, and then we had a very good discussion. I think Vern knew Roger because Vern had been playing a very significant role during the war on anti-submarine warfare problems. He himself was one of the world's experts in acoustics. And I think agreed with us, and may have been part of what eventually— I don't know. That was the only active interference I remember that I played.

Doel: One thing I'm curious about: at the time, were there any discussions about what kind of research ought be done at Scripps? Say, any general discussions about whether the kinds of research made possible by military funds ought be pursued?

Munk: I don't remember a discussion of anyone objecting to a military patronage. Those problems really came out 20 years later.

Doel: No indeed. I meant, were there questions whether to expand SIO research into the kind of lines that those new funding sources would make possible?

Munk: I don't remember discussions of that kind that would have been crucial. I would make a very damning statement, which is that the people who were at Scripps then were not very good scientists, they were not leading people, and that they were afraid of the kind of people that Roger wanted to bring in. Not necessarily the students of people who were here already, but people who had some major thoughts on science as a whole. So I think to some extent it was the known situation of a mediocre department objecting to somebody who wanted to really try and reexamine the whole basis.

Judy Munk: Was there just a little bit left over of the gentleman science problem?

Munk: You mean Roger being an gent—? No.

Judy Munk: No, no. I meant that you did oceanography as a gentleman.

Munk: Oh, that. Thank you, Judy. You know, it used to be that oceanography was done by wealthy yachtsmen who wanted to have fun during their vacation. Of course to some extent that still existed. Fran Shepard came from a wealthy Bostonian family—

Judy Munk: There were no Jews.

Munk: And there were no Jews, that's correct. But on the other hand Hubbs and Fox and ZoBell do not represent gentlemen oceanographers. I think that they really objected to someone who obviously was going to examine the very basis of which things were done. They felt very nicely comfortable in our little environment where people weren't questioning them.

Judy Munk: Sort of a naturalist situation instead of a physics situation.

Munk: Now Carl Eckart of course was an internationally known scientist. Carl was the wrong man for the directorship except in a holding operation. He later on was made vice chancellor in fact, and that worked out very badly. It wasn't something he should have done. It made him personally unhappy. He had an innate sense of neatness that is very apparent from people reading his papers. I mean he would, with great accuracy, state his assumptions and do his algebra—the very opposite of Roger's mathematics, if you can call it that.

Doel: Yes.

Munk: It wasn't much mathematics.

Doel: And you mentioned in the earlier interviews, very different from [C.-G.] Rossby's style of operation as well.

Munk: Right. And of course Rossby would drive Carl insane. That kind of neatness is very hard to exercise when you run an institute. It's more of a model situation, and it was very difficult for Carl to adjust that you couldn't do things that way. By the way, I do want to go on record. Together we have just finished the National Academy biography for Roger [Revelle]. It went in last week. It's by Ed Goldberg and me and Tom [Thomas F.] Malone. That will now be printed. We did spend an incredible time on it, and it does say what we want it to say about Roger. We thought about it quite hard, and I would like to refer to that too.

Doel: I'm glad to hear about that, and I would like to see that. And I do want to ask in a moment about the controversy regarding UCSD. But I'm intrigued by the point that Judy raised: did you sense a different group of people coming to SIO after the war? Clearly the transition between wealthy or almost amateur class also happened at Woods Hole.

Munk: Yes, the wartime transition was enormous. Scripps changed from one place to another; it changed from a land-based biological station essentially, which it still was when Roger came in, to a seagoing institute. I think that the people who had been at Scripps, like McKuen and ZoBell, Fox, did not really participate in that evolution. The people who did work with the Division of War Research and then with our work on the nuclear explosions and all, I guess I participated. John Isaacs did, who came at the time, and Carl to some extent did. But it was not that people at Scripps who were there already changed their way of doing it. That may be part of the opposition. A new group of people come in.

Doel: It's the addition of people with different training and a broader outlook?

Judy Munk: Yes. Gustaf Arrhenius came.

Munk: Gustaf Arrhenius came, and Ed Goldberg came, and Harmon Craig came. Then of course the beginnings of UCSD, and that changed the atmosphere totally. And Roger was always very interested in new things. You know, he was generally accused properly that he was better starting things than running things.

Doel: Or finishing things?

Munk: Or finishing things. Very true. And so he certainly participated, but probably led that revolution.

Judy Munk: I'm not sure how true that is, because he never really had a chance to finish anything.

Doel: Can you tell me what you're thinking about, Judy, when you say that?

Judy Munk: Well, he never had a chance to really be director of Scripps until after he had already done all the work.

Munk: I don't think so. He was very happy to start things that, no, I think that, like many people he wasn't terribly good at—

Judy Munk: He wasn't able to finish being chancellor here.

Munk: He never started.

Judy Munk: But he did all the university start-up work before he went. And every time they replaced him, it took five men to do it. So I mean—

Munk: Correct. But still I think—

Judy Munk: So there is something unbalanced about saying that he couldn't finish anything. It just means that he didn't, because he was pushed on to something else, because he never was made head of the first thing. It's a funny kind of a progression.

Munk: Well, he left lots of things unfinished, because he got interested in something else.

Doel: Did that happen in writing the joint papers with you?

Munk: Oh yes, yes. You had to go and sit at his doorsteps all night to get them done, and it was great fun doing that. No, the incentive for finishing papers with him had to come from his co-authors. But then once he started, he was totally absorbed. He would never take it lightly. But I don't think I ever remember his coming saying, "We ought to finish this job. It's been overdue." That's not so very important.

Doel: You've written also about the 1960-'61 controversy over his candidacy to be UCSD chancellor. There are a number of factors that people often point to—Ed Pauley's opposition—

Munk: Ed Pauley, chairman of the board of regents then.

Doel: I'm wondering what additional factors you feel played a role.

Munk: I think Ed Pauley is the central point, and that could be combined with the fact that President [Clark] Kerr was not willing to put up a fight. He said afterwards—he has just written

a book that you should refer to, I don't know whether it's come out.² Kerr wrote a book about his life as president of the university. He came down for a day to talk to us, which is interesting. I think he has had a conscience about the Revelle thing all his life since it happened. It's very clear he's very defensive on it. He says now that he could have put up a fight, but he would have lost it.

Doel: Why did he feel that he would have lost it?

Munk: It's his judgement that the regents would not have bought it. Certainly Pauley, who was very powerful, would have opposed it. I can judge the statement. I would have preferred people who put up a fight even though they think they might lose it. But it came as a complete shock to Roger when he was asked by Kerr to take a trip to Washington to try to talk Herb [Herbert F.] York into accepting the job. It was sort of like asking somebody to woo a woman with whom the person himself was secretly in love.

Judy Munk: Not so secretly in love.

Munk: Captain Standish. Herb [York] says— I have no doubt about this, I'm sure it's correct—that when he was asked to become chancellor and when Roger [Revelle] talked to him, he did not have an idea about the Revelle situation here and that everybody had expected that. He was not aware of it. I think he told me once that if he had known that he might not have accepted it.

Doel: That's very interesting.

Munk: Roger and Herb of course became very good friends and they admired each other a great deal. There was a clear understanding. But I was chairman of the faculty at that time, it was a very small faculty. I had that job twice, both times during crisis situations. I had the job of writing a letter to Herb saying that we are looking forward to seeing him, and I was present when Herb for the first time appeared at the campus here to meet people. Keith Brueckner was then a very powerful person in our new faculty. There was a very strained meeting, but it came as a surprise to Roger. We thought for a moment about, I don't know, going on strike or something. Roger himself said, "No, you can't do that. That's the perfect right of the regents to appoint who is going to be chancellor, and I don't want you to go and put up a fight." He discouraged it.

Doel: But you were considering taking action?

Munk: I was considering it. I don't know. I remembered hearing him say that that's not the way to do it.

Doel: Deborah Day mentioned a conversation that she had had with Clark Kerr recently about this period of time. She said that Kerr had mentioned two additional problems concerning Roger's candidacy for the chancellorship. One was the land that he was purchasing, including

here in California, and the other were the affairs he had heard about. I wanted to ask your preception about whether these matters also played a role.

Munk: Roger had bought some land. He of course was vital in buying the land where we're now sitting. It would have not happened probably if at the key time Roger said, "Well, I'll advance the money," and we just go pay for it, "then you can pay me back." Then there were some other land speculations, and you should know that my mother and we were partners to some of them, so I'm not neutral in that. There was a story in the paper once that suggested that Roger was trying to build UCSD because he had owned some land and that existed—

Doel: So that sort of charge was circulating?

Munk: Roger wrote back a very angry letter which probably was published in "Letters to the Editor." I do remember that, where he said it was totally wrong -- maybe even, what you do when you bring a lawsuit? It certainly was libelous. No one who knew the picture, ever seriously accused Roger of trying to make money out of his ideas. In fact he did very badly in that sense. That was not in his mind. I don't think that that was a serious problem like what Kerr says.

Doel: This simply wasn't a political factor, in your view?

Munk: Although it had come up and there is some record of such statements, in my opinion it was not a first-order problem. The other problem that Kerr mentioned I had not heard about. Roger's private life was interesting, and I had not heard and I don't know what to say. A little bit like Bill Clinton. I don't think that—I had not heard that that had been a factor in the chancellor decision, and if Kerr said so I—

Judy Munk: Well, I can shed a little remark that it was ripe for such kinds of things in the neighborhood, because of his arguments with the newspaper. The Copley [newspaper] group, if you want to call it that, was willing to latch onto any kind of a rumor or any kind of gossip or any kind of bit of any information that would be derogatory to Roger. The climate was right for all kinds of stuff.

Doel: For feeding this kind of information back to the regents?

Munk: But it did not... I don't think it happened, and I didn't know that Kerr even had any knowledge, until you just said so. It certainly didn't become common knowledge. When Roger had all these interviews with *San Diego Magazine* with Mary Harrington Hall, which were very detailed—there certainly was no mention of his private life in those interviews.

Judy Munk: I know. But I'm just saying that he had stepped on enough toes so that if there had been any remarks, they would have been very happy to spread them, just like the Fran Shepard letter that went up about him.

Munk: Well, you're saying it could have happened, but I'm not aware at the time of any opposition that had anything to do with Roger's private life. That's all I can say. Judy is certainly right that somebody could have brought up something.

Doel: Yes. It is important to understand why Kerr did or did not choose to fight.

BEGIN CLOSED SECTION

Judy Munk: If that's so, that makes Kerr even less attractive to me.

Munk: I would suggest that Kerr is looking for reasons after the fact.

Judy Munk: Right.

Munk: That makes it seem better that he didn't put up a fight.

Doel: Very interesting.

Munk: And that is a comment I would rather not have published while Kerr is still around and me.

Doel: I understand.

END CLOSED SECTION

Munk: But that would be my interpretation. I don't think those were the factors. You know, there were other battles. [Jonas] Salk had an argument with Roger. I think we talked about it. And at the time, if you opposed Salk, you opposed womanhood—

Judy Munk: And apple pie. It was motherhood, Walter, not womanhood.

Munk: Motherhood. Sorry, motherhood, yes.

Doel: And this is not to diminish Pauley's opposition, which was tremendous.

Judy Munk: It was just adding on. Muddied the waters.

Munk: Right.

Doel: That must have been extraordinarily difficult, not only for Roger, but for you, in having to write invitations to Herb York.

Munk: But we all got to like Herb York very much. He was a wonderful choice.

Judy Munk: It was really kind of amazing, because you know he had to retire after that. Remember, he was chancellor twice, the third one.

Munk: He was chancellor twice, yes.

Doel: We were talking just before the interruption about your getting to know Herb York, and feeling comfortable with him and his directorship.

Munk: Yes. His chancellorship.

Doel: Yes. Chancellorship is the word I was reaching for. Was it clear to you that it was likely to be impossible for Roger to stay on here once the chancellorship for him was lost?

Munk: I think so. He said that it would make it very difficult for Herb York to be chancellor if here remained here— there were many people of course by that time who had taken it for granted and felt offended. And I think he did a right decision. You know he was offered some sort of a job that didn't work out, as vice dean of research for UC. I think he had to go.

Doel: Were there other offers before the Harvard opportunity?

Munk: There was an offer at St. Louis by Washington. Is it Washington University?

Doel: Washington University, yes.

Munk: I think so. That he considered.

Doel: Barry Commoner was at Washington University at the time if I recall.

Munk: Yes. And course he had the job with the Department of Interior with [Stewart L.] Udall, with whom he became rather good friends, and then the Harvard thing came up. Those things are so much better available out of the archives and from Deborah [Day] than my trying to recollect them. I do remember that he wrote that statement that there was nothing wrong with Washington University that a hundred million dollars wouldn't fix, or something like that. And he probably had many offers. I don't recall now.

Doel: I was very curious about the sorts of things you talked about with him as he made choices of this sort.

Munk: Yes. It was sort of a tragedy, wasn't it, but it ended well. Ellen says today that their lives at Harvard were the happiest of their lives.

Doel: Did you have that impression from Roger too?

Munk: He enjoyed it a great deal, and when he finally left they gave him a wonderful party there, really people who appreciated him. He did a good job.

Doel: Was it possible for you to continue collaborating with him when he was at Harvard, or did that become fairly difficult?

Munk: We drifted apart at the time. It so happened that Judy and I took a sabbatical at Cambridge while he was there. Judy got a Radcliffe Institute Fellowship, and I went slumming at MIT, so I had someplace to sit. Even though we were in Cambridge he had made his own friends in Cambridge and we really didn't see very much of him during that year. We saw more when they finally came back here. He had made a new milieu for himself, and didn't seem to me he was terribly trying to hold onto his old milieu; he was enjoying his new one.

Doel: By that time he was much more into policy issues and environmental issues.

Munk: Yes. Population and food.

Doel: Yes. Had you read Rachel Carson's *Silent Spring* when it came out?

Munk: Yes, yes. It was a very influential book indeed. Indeed. I wish that people who represent this view today were as thoughtful and thorough that she was.

Doel: You say you recall reading *Silent Spring* soon after it came out. Were others at SIO and IGPP also were reading works of that sort, or did you feel somewhat alone?

Munk: No. And you know, Roger [Revelle] was never an environmentalist with a capital E.

Doel: That's clear.

Munk: Certainly not. He really originally got a kick out of the greenhouse problem because he said oh what a wonderful experiment. It will be awfully expensive to do it if you had to do it from scratch, and there we have a injection of attention by people that we can study and learn something about how things go. He looked upon it really from that point of view rather than something awful happening to the world. I think he held that kind of view. He never was overly interested in keeping people out preserving things. He thought the world was dynamic, and you just try to take advantage of that.

Doel: He had faith in the resiliency of natural systems?

Munk: Right. And then the one time we were at Annapolis to give a talk to the Navy, I don't know whether some of this we've said before. If you remember it, you stop me.

Doel: I'll let you know.

Munk: But we were invited to give a talk to, and the then Chief of Naval Operations gave the first talk. [Jacques] Cousteau was there and Roger [Revelle] was there, and a man named John Craven was there, and there were quite a few of us, and Cousteau was giving a talk about that the North Atlantic was dead and been killed by pollution. Roger told him in no uncertain terms he didn't believe in that at all, thought it was much more robust and that those were scare words, and if sort of one could get those talks out of Anapolis it would be interesting. But Cousteau really at the time was representing an extreme view for which there apparently was no great evidence.

Doel: Did you feel closer to Roger's point of view on that?

Munk: Much more so, yes.

Doel: I'm curious how you felt about environmental issues of that kind?

Munk: We didn't have this kind of thing about environmental goals fighting at the time; it was really much earlier. I mean, he was aware of the fact that there were important things going on. It was not really a major part of our relation. We were interested in science more than— Of course Roger later became very interested in sociology. He often told me that he thought it was disappointing that I paid so little attention to the sociological implications of our work, and he was right. When I took over the chairmanship of the ocean studies board there were two boards then. One was devoted to ocean policy and one to ocean science. Frank Press had decided that that should be combined to a single board, and Frank was quite correct. Roger opposed it. He thought that one should keep them separate. I disagreed with Roger. I felt that combining was the right thing to do. Roger was of the socially oriented board. I became chairman of the new combined board.

Doel: And that was in 1985?

Munk: In 1985, and it was the first Ocean Studies Board, the new name. There was a lot of opposition and Roger really felt that that was making a mistake. And I don't want to say anymore than just that there was a disagreement. Well, the reason I disagreed is I that-- well, we had about a third of our board was socially-oriented when we started, and about two-thirds were science-oriented. We took over quite a few of the people from the socially-oriented board. But I thought that any scientist who comes to Washington to belong to an Academy board doesn't do it for the sake of science, he does it because he would like to participate in some way in the implications. You don't need a separate board that's interested in the social orientation. I mean the only reason for anybody to take academy jobs like that, which are often a bore, is because you think you are going to do something for society. On the other hand, Roger felt, and undoubtedly correctly, that when amateurs try and do sociological work they make a mess of it, because they don't know enough about it. It has its own rules and its own discipline and most scientists are not humble enough to know that they have to learn those techniques; they think they know all the answers. He felt that way about us.

Doel: It really does come down to the problem of multi-disciplinary or interdisciplinary activities?

Munk: Yes.

Doel: And how one effectively integrates people with different methodological bases.

Munk: Yes. You can get a great deal of information on that from John Knauss, who lives here who was very much on Roger's side in this connection.

Doel: I want to make sure we cover that tomorrow, when we turn to the 1980s and ATOC [Acoustic Ocean Tomography].

Munk: To finish that Annapolis thing, it was very odd. We were waiting for the Chief of Naval Operations to arrive to start our speeches, and somebody came by and gave each of us an envelope. I opened my envelope and there was a check for a thousand dollars. It was a lot of money at the time, especially we didn't expect that. John Craven, who was one of the speakers who was there, opened the envelope, looked at that, and said, "Sorry, I've got to go to my room and work more on my talk."

Doel: A check can do that. It was also around this period of time, in the early '60s, that Roger Revelle was called on by the Academy to lead a review of natural resources in the United States. How much were you aware of this?

Munk: I was not involved.

Doel: Did Roger talk much to you about it?

Munk: No. I'm not a good resource for that purpose. What year are we talking about?

Doel: We're talking about '62-'63 in general, and of course you were traveling during 1962.

Munk: No, Roger, felt that he himself one should spend more fraction of one's time on socially-oriented problems. I hadn't fulfilled that condition and he didn't criticize me for that, but he also didn't try and waste his time talking about it. He had other people to talk to who were more interested. I was more science-oriented. And I also knew much less. You know, he had much more economic feelings, he knew about agriculture, he knew about a much broader range of problems. I was and am not very effective because, even though some people say I've been a Jack-of-all-trades, they've been all physical trades, not really trades in sociology.

Doel: That's clear. But I'm thinking of Revelle's departure to Harvard and the gap in the leadership that was here at the SIO in the 1960s.

Munk: Yes.

Doel: Clearly the question of the next director was being discussed?

Munk: Yes, yes.

Doel: There were quite a few people who wanted you to become the next director, [Harold C.] Urey and [James R.] Arnold and many other people. I'm wondering how you felt about that?

Munk: I was asked by Kerr whether I would take the job, and it's somewhat related to what we've just discussed. I said I really would like another ten years of science and declined. When did [William A.] Nierenberg come there?

Doel: In 1965.

Munk: Yes. Then I was chairing a committee... As you know, Fred [N.] Spiess became, not acting director-- he was full director, but it was a temporary appointment but without the acting word. In some sense he was probably the most likely candidate for the directorship. I chaired the committee and we came up with Spiess, with three candidates. We haven't talked about this before, have we?

Doel: We spoke only briefly about this.

Munk: Frank Press --

Doel: Press was the second candidate?

Munk: The first one was Bob Stuart from Canada, who had a French wife who didn't like America.

Doel: And you weren't able to persuade her that this was an acceptable place to live?

Munk: No. And Bill Nierenberg was the third. Like any committee like that, the chairman doesn't have a great deal of power. He has some power. I thought it would be better to have a outsider, and Spiess really has never forgiven me. He considers that I'm responsible for the fact that he didn't become permanent director. And I think Carl Eckart would like to have had Spiess in that position.

Doel: That's interesting.

Munk: I don't know on retrospect whether Spiess wouldn't have been a better director. It might have been a mistake. Nierenberg did very well the first ten years, especially when our deep-sea drilling program started. He was enormously loyal to Scripps. We have never had a problem on

that, no matter where people came from -- once they were Scripps directors they played that fully, and Bill [Nierenberg] did certainly. But I thought in his last half of his directorship he did not do well. I don't know whether we want to talk about that. Anyhow, Fred Spiess would have been a good candidate. He is a splendid man. He was a Navy commander of a -- I've forgotten, was he skipper of a submarine or something? He was good with people, people at MPL swore by him and succeeded Carl Eckart. He was a good leader, well organized man, and he certainly expected to become director and didn't.

Doel: And you say it was difficult relations with him thereafter?

Munk: It has been ever since. Yes.

Doel: You mentioned some issues that had come up during Bill Nierenberg's directorship. I sense that you find his directorship was in two parts, a good first ten years, then a latter period that was not as successful. What didn't go as well? What was missing?

Munk: Well, I think Bill stopped listening to people but himself. He became self-assured and that he knew the answers, and he had the habit of talking instead of listening. It became very difficult to have a gracious disagreement with him.

Doel: And you found that really important in your own style of running IGPP?

Munk: Yes, yes, we disagreed. Back when what is now Nierenberg Hall was built, we, Judy and I, had some feelings. As you know, we had been very active on what kind of building should go at Scripps. We tried to suggest something, and he wouldn't listen. And I remember once being present when Bill [H.W.] Menard tried to talk to Bill [Nierenberg]. Bill Menard is one of the architects of the plate tectonics revolution and wrote a book about it. Very good book; he really did know something about it. Bill told him what had happened, and Menard said, "Bill, I don't think it was that way," but Nierenberg interrupted him and wouldn't listen. And there was a man whom he really should have listened to instead of lecturing him on plate tectonics, of which Menard knew all and Nierenberg knew some. I felt, at the time, that he was not as good a director as he had been. He was not a good director.

Doel: Because his mind was becoming closed?

Munk: He was not willing to listen. On the other hand, you know, he did many things extremely well. He certainly brought a revolution in equipment at Scripps. I think we may have had that. And in computing technology and space technology. The kind of things that Roger [Revelle] had not been interested in, even though it was an earlier time. He certainly never fought hard for having good analytical equipment at Scripps. That was not his interest. He was interested in ships, but not laboratories. Bill Nierenberg did understand, as an experimental physicist, the importance of having the best equipment. So you will find people like Ed Goldberg really think very well of him, but they have to speak for themselves.

Doel: Did you find in that last ten years of Bill Nierenberg's directorship that he also wasn't listening to you on the direction in which the SIO should go? Or to others?

Munk: Not listening, period. I didn't occupy that kind of a position with him that he should listen to me, but I thought the place lost something in the way of coherence, partly because the leadership was not willing to listen. He probably will say differently, but that was my impression. I found it very hard to have a good relation with the director's office.

Doel: Roger Revelle was occupied by a myriad of things in the early 1960s, but I'm wondering how interested you felt he was in NASA, which had become a primary patron for large, new technologies. Did he see the potential for satellite studies of the oceans? Was that something you remember talking with him about?

Munk: I do not remember his being particularly active in satellite oceanography. Have I forgotten something? Did he ever write any papers that had to do with it?

Doel: I didn't recall seeing any. I was just curious if you had recalled any conversations with him about that.

Munk: I don't recall any conversation. Giff [Gifford] Ewing, who was here, was one of the early enthusiasts. There was a wonderful meeting at Woods Hole called "Oceanography from Space." That was in a way the beginning of what has been an extraordinarily successful development. I can't answer that. It may be that I've forgotten. But nothing comes to mind that says that Roger took a strong leadership position on some particular development in space. You need to refresh me if there are some connections. I think of course just knowing Roger, that he must have been a great supporter. He liked new ways of observing, but I have no particular memory of that.

Doel: Yes. Thinking about the difficulties that emerged in Bill Nierenberg's leadership, has there been a real change that you've noticed in the style that scientists use in dealing with one another? You put a lot of stock into the ability among scientific leaders to communicate openly, to be able to disagree, but remain cordial.

Munk: Well he certainly was not secretive. I mean, you knew what he was thinking.

Doel: You knew where he stood?

Munk: But he didn't listen to the other points. So I'd never accuse him for being secretive.

Doel: I didn't mean to imply that. I was just curious if you felt, looking at broad changes in the way that American science has developed, whether this has been a trend, or not?

Munk: I don't know. I think the thing to say about Bill [Nierenberg] is that maybe after ten years in a job like that you should quit anyhow; that's it's possibly nothing in detail that matters, but that there should be an upper limit on tenure as director.

[end of Tape 1, Side B]

[beginning of Tape 2, Side A]

Munk: You take on a job with a great deal of enthusiasm about what you want to do, and after ten years you are bound to not quite have the same enthusiasm. We very much made that part of our deliberations. I then chaired the appointment committee that appointed Ed Frieman, and we decided among ourselves, those on the search committee, that we would try and write some kind of limitations into the tenure. There was a 10-year period that was mentioned. There is something more specific, that the job ought to be reviewed after six years and not extend, under normal circumstances, after ten. Ed happily accepted that as being a good idea, and in fact quit after ten years, to the consternation of some people, because I thought he did very well. I think partly it was that Bill had had the job too long.

Doel: That's very interesting. Did you sense any doubt in Bill Nierenberg's mind about whether he was there too long, or was that something that just wasn't communicated?

Munk: No, I did not sense any doubt in Bill's mind. But I do know that the chancellor asked a few people, including me, whether we thought that tenure had been long enough.

Doel: And that was in the 1980s that you recall?

Munk: Oh, a maybe a year or two before a new Director was appointed. I certainly said that I didn't think a further extension was a good idea.

Doel: That's very interesting.

Munk: But you see I was director at IGPP for much too long. I think we probably spoke about this before.

Doel: Not so directly. You haven't mentioned to me before your own feelings about your longevity in the position.

Munk: I forgot. I think I was director... I must really look for how many years, but very long, much longer than ten years?

Doel: Yes.

Munk: How long was it?

Doel: It was over 20.

Munk: Twenty-four years?

Doel: I think so.

Munk: I remember being in bed reading the paper with Judy every morning, and seeing a quote of—who's the man who became president of the University of California? He was a UCLA physicist who became the next president. [Did this name come to mind?]

Doel: I should know it.

Munk: Doesn't matter. He announced one day that he was going to step down and ask for a search committee. The reporter said, "What made you think about stepping down, President Blah-Blah?" President Blah-Blah said, "Well, I thought I'd better think of it before others think of it." Judy looked at me and said, "Well what about that? Doesn't that apply to you? I think really it does." I like to think we had a very good relation at IGPP, but I then said, "I think you're absolutely right," and we asked for a new search committee to come in. I remember asking for a meeting with all the IGPP staff at lunchtime, and I said I'm going to be the only speaker: I wanted you to know I'm going to step down, and we've asked a search committee to be done. And the way I arranged it is that I was going out to sea that afternoon, so we had lunch and I walked out, was gone for a month. Deliberately. I didn't want to have to bother to see people who would have to say, "Well gee, I hope you stay a little longer" or something. By the time I came back it was so well accepted; there was no problem. So I think I did a good job of terminating it. And of course Freeman Gilbert had been the associate and became director and did a splendid job. He has been succeeded by John Orcutt, who did an equally splendid job. So we've had a very good time at IGPP.

Doel: Was there ever any doubt in your mind that Freeman Gilbert was going to be your successor?

Munk: I don't think so, because he had all the attributes: wonderful person, superb scientist, had been essentially in the job of working with me for four or five years. And he was willing to do it.

Doel: Yes. Did he talk with you about whether taking on that position would essentially mean stepping out of a research appointment, or did he feel that he would be able to manage both responsibilities?

Munk: No, but we always had managed. If you look into my career, I think I kept on.

Doel: You were able to keep doing research, you mean.

Munk: So did Freeman, and so did John Orcutt really. We have always made that position one where it wasn't supposed to be a job that could keep you away from doing research and teaching. It never has been. Of course we're smaller than the Scripps Institution by one order of magnitude.

Doel: Indeed.

Munk: So that's been the way it has been. And IGPP has played a very vital part in the development of Scripps as a whole. I mean Freeman became department chairman, John Orcutt was really the principle person working with Ed Frieman. Bob Knox, who was working with John Orcutt, took over our ships. We had done our part in contributing to Scripps on the whole, and UCSD. Freeman and I had both been chairman of the department, of the faculty, so we had been I think good citizens of Scripps and good citizens of UCSD.

Doel: I've been curious about is how certain individuals at Scripps became deeply integrated into UCSD while others did not. I'm thinking for instance of Ham [Harmon] Craig, who stayed at Scripps.

Munk: Who do you think was integrated well?

Doel: I wanted to hear your point of view on how well that integration worked. You were chairman of the faculty during critical times. You clearly represented someone who did bridge the campuses.

Munk: We've done that, but as a whole I think if you wanted to say what is really important, it is that Scripps insisted in maintaining its own identity. There were undoubtedly lots and lots and lots of examples, but the number one thing is I think again that we try to maintain our own identity. There was a time when for example they wanted to incorporate the Scripps land more closely into the development of the regular campus. At one time a chancellor Bagely McElroy thought of building his house near our tennis courts here, and there were problems of removing it. We put up a fight saying that land was given by E.W. [Scripps] for purposes of oceanographic work, and Bill was very powerful in that battle. He wouldn't put up with any of that. See, that's to his great credit. I think we sort of like having UCSD there, but we'd like to be considered to be not just a part but a rather unique part. We started it all.

Doel: Are there ways in which you wish greater integration existed, or on the whole do you feel that the distinctness that Scripps has maintained has been largely to your advantage?

Munk: Well, for the sake of the field we should play a bigger role in teaching undergraduates. It's important for the country. I have not been very effective, but I've done a few things, very, very few. I think it's a bit of selfishness. I like Scripps having maintained its little bit of its identity, and every director of Scripps has held onto that little bit. If the art department of UCSD tells us what kind of artistic things we can do on the lower campus, we certainly get angry. There

is a foundation called the Stuart Foundation, and they told us we musn't do this or we musn't do that. We musn't have a whale at the aquarium; we don't like that. We can do better ourselves.

Doel: These were all recent developments?

Munk: There's always a Stuart problem.

Doel: The tensions have long been there.

Munk: With building. I mean, there is a quite large organization for buildings and grounds, and they naturally and rightly think that, they should be responsible for all the buildings. We think we should retain some degree of control, and when we built old IGPP we very, very much tried to retain our identity—and did that successfully, but worked very hard on it.

Doel: Yes. You mentioned how difficult that was to maintain the design that you and Judy wanted.

Munk: Yes.

Doel: I found in the archives a 1955 report that you had prepared. It was during the time that you were on sabbatical in England, and you contributed to ONR London an overview of the development of oceanography in Northern Europe. I'm wondering how that came about? I have a copy of this report here.

Munk: I haven't seen that of course for some years. Yes. Well, I need to really put it this way. I do remember doing that.

Doel: It's a comprehensive overview of developments. It's a very helpful document for understanding oceanography in the 1950's, from your point of view and broadly.

Munk: I would have to say that I wanted very much to use my sabbatical to visit European institutes, particularly in the north, and it was very welcome to get some financial help from ONR to do these travels. We didn't have very much money. And I ought to say for the sake of honesty that that was undoubtedly a part of why we accepted that. But on the other hand it also was very interesting to visit people, very rewarding, and I remember some these visits, now that I look at your piece of paper. Visiting Rossby, visiting Albert Derfant, such a wonderful guy, visiting Peterson in Sweden. Judy and I did those jointly and had a very good time doing it. But I think I was more driven by my own interest in seeing people and finding out what's happening and finding a way to afford to do that than anything else. I had forgotten about this report. It's not something I would have remembered.

Doel: It clearly represents a substantial amount of work that you did in pulling information together and laying out systematically the strengths and weaknesses of these various institutions. As you say, funding was much tighter immediately after World War II for travel of that sort.

Munk: Yes, yes.

Doel: Those funding sources became more generous by the '60s.

Munk: We're now supported very well. Besides, you know, I really love having things to do with ONR, and that was very welcome.

Doel: In certain nations, Germany and France in particular, there were occasionally tensions when U.S. scientists with military agency connections visited; some felt national security issues interfered as opposed to more purely scientific concerns. Did you run into any doubts of that sort when you were visiting Northern Europe?

Munk: I don't think so. I think I had my union cards as an oceanographer by then, and people regarded me mainly as a Scripps oceanographer and not as an ONR functionary. And I remember being received very well, always. All of these things. Even right after the war in Germany seeing Mr. Brunache, who had been head of the Deutsche Institut für Geophysik [unclear on tape] or something, and being very courteous. So I don't have that memory. I have only a memory of wonderful times with these people and enjoying getting to know them.

Doel: Yes. That's very helpful to know, because it clearly wasn't necessarily so easy in the post-war period, for other eminent scientists.

Munk: Yes, it was very easy. Oceanographers really have a wonderful relation to one another I think, at least at that time. It was a wonderful experience to visit.

Doel: Before we talk about MOHOLE, you had mentioned briefly work that you were doing with Gordon [J.F.] MacDonald that led to the 1960 book *Rotation of the Earth*. But I was struck yesterday in reading the last chapter of the book, where you discussed the secular changes, and very recent work: [S.] Keith Runcorn's work suggesting polar wandering, new evidence regarding magnetic reversals. These involve major questions coming before the geophysics community.

Munk: Yes.

Doel: Were there points of difference that you had with Gordon MacDonald over how to treat these kinds of evidence that were becoming available, or did you feel that the two of you were pretty much of the same mind?

Munk: We had no essential points of difference. We would argue about sentences and paragraphs and how it should be said, but not about the basic content. That book I think was a joy. Gordon, you know, who has had a remarkable career nationally and internationally says he thinks it's the best thing he's done.

Doel: Is that right? That's interesting.

Munk: Yes . And of course it's totally outdated, because the experimental situation has improved by orders of magnitude. I think it was a good book. We did some things that hadn't been done before. We combined two different fields—the people who observed length of day, the spin of the earth, were very different from the community that observed latitude variations. They never talked to each other. And in general the papers that are trying to interpret the astronomic data had a very naive view about the geophysics of the earth. That's where we came in. We called it a geophysical interpretation. And we organized it by frequency. We started with high frequency, that means changes in one year is high frequency, and then ended up with, as you were just mentioning, geologic changes, geologic time scale. I think that was the general organization. There was another book written 20 years later which followed the same organization. Kurt Lambeck in Australia wrote a book with almost the same title.

Doel: *The Earth's Variable Rotation* was his title.

Munk: Right. And he followed the same basic structure and used the same publisher. So I think we did a good job. I still occasionally go back and have recently really become interested in some of the problems that started us working on the book. Carl Wunsch and I now have a paper in press, and there is one being reviewed, on tidal friction, which came out of that era. So we have done some new work on this subject. It's an interesting subject.

Doel: Thinking of the original book, how well did you come to know people like Dirk Brouwer and other astronomers?

Munk: You know, I became interested really just because I had read someplace that the French had discovered that there apparently was a seasonal variation in the length of day in using pendulum clocks, which is incredible, because they are so inaccurate really. Then I had read a Rossby paper arguing essentially that the angular momentum of the atmosphere was bigger in December than it was in July because the westerlies blow much harder in the Northern Hemisphere, and that is not quite canceled in the Southern. There was a paper by Victor Starr, who said that these changes in atmospheric momentum introduce imperceptible—and the word was *imperceptible*—changes in the length of day, and I wondered how big was perceptible. That's what started it all. We found out not only was it not imperceptible, it accounted for some measurements that had existed. It became so interesting, we just couldn't quite get away from it.

Doel: Yes . You and Roger Revelle were contributing to parts of it when you began looking at the role of the core.

Munk: Right.

Doel: And it was clear that you were very excited by [E.H.] Vestine's work that had been announced in the early 1950s indicating the westward shift of the magnetic field.

Munk: Yes.

Munk: And it's still an interesting problem. At this time the precision is so enormous you can tell millimeters instead of 30 centimeters of changes, and there are a whole slew of people are working in that field. In that whole slew probably 50 people in the world. At the time we wrote there was only, there were only two. And it was a great deal of fun. I think the idea that looking at a field from a new point of view and trying to combine observations which had been independent of one another is a good idea. And chiefly learning a language. We have to learn the language of the latitude people, we have to learn the language of the time measuring community, and it really also introduced us into every aspect of geophysics, because every geophysical change--sea level, glaciation, motion in the core, motion in the mantle--has an effect on the rotational vector of the earth.

Doel: It does. It makes for very interesting reading, seeing you interact with these different communities.

Munk: Right. So it was a wonderful way of learning geophysics, because you had to really think of it in some bulk terms. And it still is.

Doel: Yes . When you addressed reverse magnetism in rocks you cite for example [John W.] Graham's work at the Carnegie Institution of Washington. By the 1950's he published his doubts that the observed magnetic variations in rocks were caused by the field, believing they were caused by pressure. Did you get your views primarily from reading the literature, or then did you pick up the phone or write? I'm curious generally, when you were going into fields that had not been primarily your own, how you came to understand the work.

Woman: *[interruption by door bell]*

Munk: Oh, can you get it please? Oh, thank you, thank you. I never as you know was a good reader, though of course one does have to read some things I think— Oh hi Mary. Let's interrupt for a moment.

Doel: *[tape off, then back on...]* We are resuming after a nice lunch break. You were mentioning just around the time that we stopped the challenge of learning a new "language," such as what astronomers meant by certain terms, in putting together the book. Did that come easily, or was that part of the more difficult tasks?

Munk: No, I think the difficult task is to understand what kind of measurements they made and translate it into something you could use geophysically to limit events on the earth that must be so large that they produce impossible wobbles that haven't been observed. And it was very odd. . . , I think we talked about it once before; learning how to deal with noisy processes is something that happened in my lifetime, the [Norbert] Wiener-[John W.] Tukey thing, the so-called power spectral analysis. Astronomers of course who have very nice regular motions stuck even longer than oceanographers to old-fashioned Fourier series kind of things, and we tried to translate some of the things we've learned about noisy processes to the observations that they've made. There was a great deal of resentment on the part of I remember our nation's astronomer, Mr. [W.] Markowitz.

Doel: At the Naval Observatory?

Munk: At the Naval Observatory. Didn't like that. The passion to fit a few sine waves to a noisy process is very, very much ingrained in their thinking. We tried to interpret some of the data as noisy data, which I think has turned out to be the right interpretation. And then it was not without objection from the astronomic community. Now they had a meeting two years ago celebrating something about earth rotation. I was invited to give a talk in connection with that book, and I was the only non-astronomer there.

Doel: This was at the American Astronomical Society?

Munk: It was given in Los Angeles, and it was the American Astronomical Society. I don't know, I have a paper I gave about Gordon's and my book. I should look it up. They really didn't like us outsiders coming into interpret their data, and the astronomers are really a very ingrained group, like we are, and have their own prejudices.

Doel: Wouldn't you say that was a small community within the American astronomical community, those who had stuck with classical celestial mechanics?

Munk: And the real astrophysicists don't think much of positional astronomy. I mean that's ancient stuff.

Doel: Different methods, different instruments.

Munk: Even though it's been very successful lately because of the very large accuracy of navigational methods. They have learned all sorts of new things by these methods, and JPL is leading in that field.

Doel: Right. But that's come out from space and planetary exploration. Which again was separate from astrophysical cosmological traditions.

Munk: Right. And you know we put a retro reflector on the Moon in 1960 as part of the effort then, and we've been measuring—we, not us, but American scientists—the rate at which the Moon moves away from the Earth, which is about two to three inches a year. Now that is new data which is now very precise data. In fact it is the best data there is to estimate total tidal friction. And my attempt this last year with Carl [Wunsch] to do a paper on tides came out of those measurements. They are really very provocative measurements.

Doel: It's an order of magnitude above what had been available at the time you and Gordon [J.F.] MacDonald were first working on that.

Munk: Right.

Doel: Well you mentioned a moment ago the book's last chapter on geologic knowledge. Was that written primarily by Gordon MacDonald, do you recall, or did both of you take a hand in writing that?

Munk: No, we really both wrote every chapter. We did not split the book. It's written I think almost every sentence has had some contribution from both of us.

Judy Munk: One hot summer pacing up and down the halls of UCLA.

Munk: Yes . And arguing furiously. So it really, and I think it shows. It's not a book, you know, which falls apart into five people writing a book and each writing a chapter. No, we wrote that book together.

Doel: It is seamless. I was just wondering how, you dealt particularly in that chapter with new information that was just becoming available the debate over continental drift, Keith Runcorn's paleomagnetic data was helping to re-ignite?

Munk: Right.

Doel: Certain sentences referred to what some regarded at the time as the poor fit of the paleontological data used to support a united Gondwanaland. Had you read the works of Du Toit and others at that time, do you recall?

Munk: No. I think—Maybe Gordon knew more about this—we were aware of it. No, I personally had not. By the way, Runcorn of course was once a big opposer and then became a big supporter.

Doel: Indeed he was.

Munk: Now you know we dedicated the book to Sir Harold Jeffreys, who of course never accepted any of the plate tectonics ideas, and it was really a bit of a tragedy. His book *The Earth*

was once s a leading document for people who wanted to become geophysicists. Successive editions, of which there were many, became increasingly out of tune with what was learned. But he refused to adapt to the new evidence, and so his book became successively more out- of- date.

Doel: Yes. That were five or six editions at least before his death.

Munk: His wife still writes to us.

Doel: Bertha Jeffreys.

Munk: Lady Jeffreys. I was asked to give a talk at his 100th birthday or something. She was very defensive, and wrote me all year before as to what I should say and what I shouldn't say.

[end of Tape 2, Side A]

[beginning of Tape 2, Side B]

Doel: Another issue I wanted to make certain we covered, was your part in what became known as Project MOHOLE. You've mentioned earlier, that you regarded that as one the leading disappointments of your professional career. I'm really curious to have your recollections of your first involvement either in the American Miscellaneous Society or to what became MOHOLE.

Munk: I was on the NSF review committee. So was Harry [H.] Hess. We both expressed our disappointment at one time that there weren't any really new ideas that we had reviewed. And so someone said, "Well, what would you consider really to be some really basic thing?" I forget now; I think it's been written up. One of us said well, we could go and get a piece of mantle, and see what it's made of. Of course in retrospect that isn't anymore a very good statement, because there are plenty of places in the world where mantle material comes to the surface. At the time it seemed significant.

Doel: But at the time that wasn't known and so—

Judy Munk: Everybody was talking about Mr. Mohorovicic or whatever his name was.

Munk: Yes , and so the question was, well, how would you do it. The obvious thing is that the crust is thin under the oceans and thick on the continent, so you do it under oceans. Well then drilling in the deep sea had not been done. And someone there made the correct judgement that it should not be difficult to hold a drilling ship in place without mooring it by using acoustic methods of placing yourself with reflectors, with transponders. It ought to be possible to do it. Then the story of course has been written up. We had the American Miscellaneous Society (AMSOC), which is giving the Albatross Award this coming Tuesday for the, I don't know, 20th time. I don't know how many people have had it. We decided that was a project to our liking, and we had a breakfast on our patio. We had a great fun time to be very non-bureaucratic and

having society sponsorship. NSF of course was totally incapable of dealing with a group that had no administrative standing, and sent us back to become appropriately organized.

Doel: Who were you dealing with at NSF at the time? Was it Bill [Willard] Bascom?

Munk: No, no, no, Bill was at the Academy.

Doel: That's right.

Munk: The organization was more for those people who then became the men who did the work. Judy's brother, Ed, worked with Bill on the first drill string.

Doel: That's right.

Munk: And you know we organized as a committee of the Academy.

Judy Munk: It turned out the same people were on it were on AMSOC.

Munk: We have not spoken about this in detail before, have we?

Doel: No, we haven't.

Munk: I mean it was kind of fun, because the same people who were having fun as members of AMSOC were also members of the Academy, and that included [Roger] Revelle and [Harry H.] Hess and [W. Maurice] Ewing, all sorts of people who really had a standing. The Academy accepted that, we got a million dollars, and started working towards drilling off Mexico, Guadalupe Island. That was a huge success. The whole thing went extremely well. It was in deep water, 10,000-12,000 feet of water, and the position was held. I selected the site for being sheltered by Guadalupe Island. I have a feeling we spoke about that.

Doel: No, we haven't.

Munk: I figured that they were safely in the lee of Guadalupe Island. I have since learned that an island can do two things: can either provide a lee or it can focus wave energy on the other side. The latter turned out to be a much more accurate description. When we left by helicopter after being there a week, and I looked down. The sea was calm except in one spot, which is where the rig was anchored. And whereas we had said we wanted to be in a place where waves wouldn't exceed six feet, they were never less than eight in two weeks, so it was a total goof. I remember that because I made the mistake several times. I've learned since that you have to be careful how islands affect waves.

Doel: But that also speaks to the value of having aerial or satellite data on oceanographic phenomena.

Munk: Yes, yes. John Steinbeck was part of the crew.

Doel: How did that come about?

Munk: Bill asked him. That's typical Willard, said who is the best man to write about this? Well John Steinbeck. So let's call him.

Judy Munk: Fritz Goro did the pictures.

Munk: Fritz Goro was the scientific photographer for *Life* magazine, who took the pictures.

Judy Munk: Those two didn't get along.

Munk: They didn't get along.

Doel: Steinbeck you mean?

Judy Munk: He was a bully.

Doel: I'm curious what your impressions were of him.

Munk: Of Steinbeck? I was very impressed with him. He went aboard and, unlike other people, went aboard two days before we left. He talked to every member of the crew in a relaxed and friendly way and trying to learn something. He didn't run off every evening to some cocktail party. And he very much was part of the adventure of that drilling thing.

Doel: What sort of things did he ask you about? I didn't mean to interrupt you.

Munk: I don't remember. I do remember, he was a great shipmate.

Judy Munk: He really kept his eyes open.

Munk: And kept his eyes open. The boat was enormously noisy, and you couldn't speak unless you shouted. You had to make up your mind whether you would lose your voice or not contribute to the conversation, and John Steinbeck and I think nearly all of my friends were in the former class; we all lost our voices. Steinbeck's voice was kind of hoarse to start with, and it became entirely so. After we came home, by the way, we took John Steinbeck to lunch here on his way home to New York.

Judy Munk: On the way home we had a little sun beam talbot car and somebody had—

Munk: Put sugar in the gas tank.

Judy Munk: The car coughed and just stopped on the way home on the freeway, and these men were just in the back exhausted. They hadn't slept for three days and three nights or something like that.

Munk: John Steinbeck was absolutely exhausted. He said, "Would you mind if I put my feet up for a moment before lunch?" and he never woke up until it was time to catch his plane to go to New York. He wrote us a note afterwards which I wish I had kept saying, "Sorry about going to sleep as soon as I arrived at your house." He said, "There's not many guests do that, but perhaps more of them should." So it was something of an insult. It was a wonderful note. Anyway, it was clearly remarkably successful.

Judy Munk: You should read those *Life* magazine articles. Deborah [Day] has them.

Munk: They were wonderful. By Steinbeck, with pictures by Goro.

Doel: Isn't this very close to the time that Steinbeck received his writing award, the Nobel?

Munk: I don't know.

Judy Munk: [*In unison*] I don't know.

Munk: What stands out in my mind after many years is that we couldn't of dreamt when we got back, with all the success, that the next step wouldn't be easy towards doing the real thing. What we did not realize was that the very success of that mission spelled the end of our group running the show.

Judy Munk: Well, but do you know that's really very symbolic of what's happened to you before.

Munk: Well, let me just say that it was sort of a funny thing. Groups that hadn't been interested, big companies, suddenly did decide it would be interesting to bid on it. You know the story about Brown & Root, I don't need to repeat it. They had originally been placed as ten out of ten among the people who proposed to become the major contractor, and then for some reason that is not clear—and there has been a lot written about it—they got the contract. They were totally incompetent. They'd had no experience. But our success, totally unimaginable to us when we left the ship, became the beginning of the downfall. I don't know what Judy will say, I didn't mean to interrupt, but in a way I thought that it was similar with the Heard Island experiment. It was successful. In some way we'd gotten so much publicity that it had caught the attention of the environmental groups which it would have never otherwise, and it spelled the beginning of the end of our easy success. My own reaction was that I could see another MOHOLE coming, and I didn't want to have this happen twice in my career. I certainly wouldn't do what I did at MOHOLE, where we would go to Washington maybe once every two months and attend a 2-day conference and then make all the decisions and go home again. That just didn't work. That

meant that we played almost no role in the decision making process of who would be the prime contractors.

Doel: You feel that signaled lax oversight, and opened the door to the kind of abuses that followed?

Munk: That's correct. And that if you want to do something of this sort, it has to have two ingredients: one is that it's a good idea worth doing, the other one is that you are willing to give it all the time it takes, yourself, because nobody else cares as much as you do. So when things went wrong on the ATOC issue, we did not repeat that mistake; we stayed in the middle of it. It would have I think collapsed very easily if we hadn't decided to fight it through. So that was a big lesson in my life that I didn't want to make that mistake twice.

Judy Munk: But what's kind of curious is that both of these things were really sort of exotic inspirations coming out of old problems. I mean, it wasn't something new that you thought up, it was what you and a group of people thought: the time is right, and the tools were available.

Munk: Right.

Judy Munk: And that's what happened on both ATOC and the MOHOLE.

Munk: Yes.

Judy Munk: But these were two exotic solutions to an old problem, and it wasn't as though you went around advertising it. But I can remember going with you, you were so excited about being able to hear this last discussion, the ATOC business. When you talked about it in New York, I mean everybody couldn't help but get excited about it.

Munk: But it was the beginning of the downfall.

Judy Munk: But as soon as that kind of gossip gets on the network or whatever it is, it seems to breed its own downfall. Does that mean that you don't talk about what you've discovered?

Doel: It's interesting too how different the broad political environments were, the late '80s and the early 1960s.

Munk: No, they were very different things that went wrong.

Judy Munk: Very different.

Munk: I mean Brown & Root was not Greenpeace.

Judy Munk: No, but, in that case it became a big fight with big contractors.

Munk: Yes .

Judy Munk: In this case it did not. Because you took care of it, it didn't.

Munk: But it became a big fight.

Judy Munk: But it became a big fight— In a political way.

Munk: You know, it bears out the Revelle lesson, which of course in a way he missed too because he was one of the people who could have stopped the MOHOLE if he had done it earlier. It is that, if you have an idea that you think is worth doing, technical or scientific, you need to also be willing to follow through in the political and social end of it. You can't expect to just sit there and have an idea and say, "You do it." That doesn't work. That's the lesson. Roger certainly lived by that. When he had some things he wanted to do, like when he was in Pakistan because he had been asked to find out what to do about all the water rising. He found out that the problem was they didn't have good pumps and they didn't have good schools where people learned how to run a pump. He followed through on those next steps. He wasn't afraid to say, "Well, we ought to do something about the Pakistani educational system."

Doel: He went to root causes.

Munk: Yes , yes. On the other hand, where do you end? I mean, you can't— I know.

Doel: There is a question of balance in terms of commitment.

Munk: Yes.

Doel: I'm curious. As you think back on how AMSOC developed, was there a model of any sort in your mind, or were you inventing it?

Munk: Well, AMSOC was the invention of John Knauss.

Judy Munk: Well you're talking about the society.

Munk: Well, AMSOC was a convenient and amusing way of reorganizing. It was just an accident that Hess and I and Revelle were involved.

Judy Munk: Like the Three Musketeers. I think it was that way.

Munk: Yes . And so we said let's go drill a hole to the mantle. Well who's going to run it? Well, let AMSOC run it, you know. That was boys having fun, and NSF was totally incapable of seeing something done in a way like this. They need to have their legal guarantees. I mean, AMSOC had no understanding.

Judy Munk: There is the little discussion about the Albatross Award.

Munk: Well it's been covered.

Judy Munk: But there's a history of that in AMSOC.

Munk: Yes , I mean a few people wish to take that lesson seriously; there are books and there is a discussion, yes, especially since we've giving another Albatross Award on Tuesday.

Doel: Is that right?

Munk: Yes . Here, in San Diego at the Oceansides meeting. And I think in some sense you know it wasn't a failure. We ended up with \$20 million spent that didn't lead anyplace. That's a big amount of money. But four years later, whatever, the deep-sea drilling project started, probably with a significant connection to that previous effort. Certainly there was no question in anyone's mind that you could keep ships in place to do that, and you might give AMSOC some credit for that.

Doel: When did you begin to sense that the undertaking was going awry?

Munk: No, we had no idea. When we came home, everybody feeling glorious saying we've shown it, now on to the next step, which was to plan not just a two week's drilling but to go for a much longer time. It was just inconceivable. I remember Bill [Willard] Bascom sending a telegram to President Detlev Bronk, then president of the Academy, saying here is what happened and we are ready to go, sir, for the Academy. It just seemed inconceivable. That's what stands in my mind, how utterly shattered we were when it started going down from that initial success.

Doel: Yes .

Judy Munk: I remember going on an airplane trip with you, with Gus Kinzel, who was head of the committee, and we lived up on the hill here. The Union Carbide man.

Munk: Oh. That was much later. Yes. I forgot who was head of the National Science Foundation then. The head of the academy was, what's-his-name, a Nobel laureate physicist, and who succeeded Detlev Bronk. He did not back us. You know that we asked him really to say he should fight with National Science Foundation, that this was not a suitable firm to undertake that work.

Doel: Phil Hadler came—

Munk: No, it was not Phil Hadler. Before. [*phone rings*]

Doel: We'll make sure that's on the transcript.

Munk: A very well known man. [Frederick Seitz] He said he would not introduce any friction between the academy and the foundation over that. He wouldn't do that.

Munk: Anyway, I gave you my principle reactions to that. I think scientists very often like to start new things and are not willing to really put their life on the line. But some are. It's a mistake not to.

Doel: On the publicity, on that really extraordinary effort that came about with getting the *Life* photographer, John Steinbeck, others involved, who was the person who initiated this?

Munk: Willard Bascom.

Doel: It was his decision?

Munk: Yes . I remember asking him, "Why did you ask Steinbeck? Did you know him?" He said, "No, I wanted to think who would be the best person to write it up, and it seemed like Steinbeck, so I gave him a call." That's Willard at his best.

Doel: Yes . Did all of you feel comfortable with that level of publicity?

Munk: At the time, yes. Well, there was no publicity in the modern sense. I mean, it was decided somebody ought to write it up and who would be a good person. Willard did that.

Judy Munk: It was no company PR job. It was like you were all going out on an expedition, and who should—nobody would have, you know, time to write it up, and who would be good and would know how to do it and would think it was fun. It was taking another member of the journey. It was doing a PR job.

Munk: Yes . It was not a PR in the sense of an organized group who is good at getting, knows how to call these people. It was very much in the old-fashioned way.

Judy Munk: Everybody had read him, so everybody was looking forward to seeing him, you know.

Doel: Sure. But in many scientific disciplines in that period of time, that kind of publicity wouldn't necessarily be welcomed for major new undertakings.

Munk: Willard, perhaps unlike most scientists, welcomed that.

Doel: Did all of you feel similarly positive towards this?

Munk: I think we were pleased to see John Steinbeck as a shipmate.

Judy Munk: Yes . If it had been— I mean, all of you liked John?

Munk: Yes . And he did a good job. You really ought to get back to the old *Life* magazine and read that story. It's well worth reading it.

Doel: Yes. An extraordinary amount of money was going into NASA and space exploration at virtually the same time. As you think back, did that affect the way in which you conceived or thought that the discussion about a broad new undertaking of this sort needed to be done?

Munk: Yes. Your question is a good one. I remember we talked about inner space. That was our answer to everybody getting excited about outer space; we were going to do something about inner space.

Judy Munk: Roger [Revelle] used to say people know more about the face of the Moon than they do about the bottom of the sea.

Munk: I don't think that was his original quote, but that's been—

Judy Munk: No, but he used it all the time.

Doel: He used it. It might have been Athelstan Spilhaus [Sr.] who first made use of that, but indeed it was widely used.

Munk: But you are quite right. It was during NASA publicity, and here we came up with the excitement of inner space.

Doel: How well did you come to know Detlev Bronk during this time?

Munk: Oh, quite well. You know he was a wonderful character. The annual meetings of the National Academy still took place in that small room that is now called something else.

Judy Munk: The library?

Munk: It was a very intimate affair, and I thought he was great. And he and Roger loved each other. They were one of a kind.

Doel: They certainly shared the international interests, didn't they?

Munk: Right. And Detlev had a wonderful memory, he was always full of positive enthusiasm for interesting ideas, and he was a real leader. One day after I had attended a meeting at the Academy, somebody said, "We are going to go to The Bronx for dinner," and I wondered why in

the world with all the good restaurants in Manhattan you would go to The Bronx for dinner, but it was not The Bronx but Detlev's house for dinner. *[laughs]* I was told since in fact by Bill Nierenberg that it wasn't so independent, because the Bronx was named for one of Detlev's grandparents in some sense, and the name was related.

Judy Munk: The 'k' was changed to an x.

Munk: We went to dinner at Detlev's house, who had the only California bungalow in Manhattan. It was on the East River and it was a one level house and looked more like a California bungalow than a Manhattan townhouse.

Doel: That's very interesting.

Judy Munk: No, it wasn't a bungalow. It was rancho style.

Munk: Yes . Right.

Doel: But still distinct?

Judy Munk: Oh, it was.

Munk: And he gave his own— He must have been president for a long time, and he really was a leader. He and Roger really had a particularly close relation to each other. I remember he came at Roger's request to either inaugurate our new aquarium museum, or something, like that. He would do that.

Judy Munk: He was a good party man, too. Wonderful party. Both he and Roger, they would walk into a room, and you just felt like having a party.

Munk: Though they were very different in stature and so on.

Doel: It's clear from your smile, Judy, you are remembering some of those parties where they were present.

Munk: Right.

Doel: Did things change when Bronk was no longer a leader of the Academy? Was this a transition that really was marked for the Academy?

Munk: It became bigger, and different, just like Scripps became bigger and different, but the people who succeeded him , except that one man whose name I am blocking.

Munk: Phil Handler is a wonderful man, and we certainly enjoyed him.

Doel: Did you mean Fred [Frederick] Seitz?

Munk: Fred Seitz! And Fred Seitz is very much alive.

Doel: Yes he is.

Munk: I really think he should have had some stamina. He felt it would be difficult to back us up on MOHOLE at that time. That was against Brown & Root, they'd been appointed, and the Administration was friendly to them, and so he wouldn't put up any help. Sort of like Kerr and Revelle.

Doel: Interesting analogy.

Judy Munk: That's not a bad analogy.

Munk: I really have objected to that ever since. But I thought Handler was great, certainly Frank Press, gets all my admiration, and the present president does a good job. No, I think there have been very good people. But the thing got bigger and I got older. Who knows whether these ideas of changes are because you get older and because times change, but they were very special times, and I fully enjoyed it.

Doel: How accessible was Fred Seitz to you when you felt the need to talk to him?

Munk: I was not really the major person. I mean Ewing, Revelle, Hesse, Menard, there were others, but I was part of it. And I don't think it was a question of accessibility. He was accessible. It was really like with Kerr. He didn't feel that MOHOLE was worth getting into a tangle with NSF. I can understand that.

Judy Munk: And with President Johnson.

Munk: And President Johnson, who you know the rumors had it, not confirmed to this day, that Johnson was the one who wanted his friend Herman Brown to be involved. And Brown had been a supporter of the Johnson Presidency. Who knows what [Alan T.] Waterman did. I wish Waterman would have spoken about that, because he was head of NSF. He would have been the one who ultimately made that decision.

Judy Munk: But it went down into the funny cracks. Walter, who was the man who was the chair who was the head of the company?

Munk: Oh, he was a friend of Seitz's - Gus Kinzel. He was head of Union Carbide then. He lived here in La Jolla.

Judy Munk: He did.

Munk: And he was appointed to us by Seitz to look into that mess at the time when we were falling apart. He also made a wishy-washy thing saying it's not worth making any changes.

Munk: Seitz was the man who, if he had chosen, probably could have reversed it and made it a success and chose not to do it. It's very much like Kerr. It's easy for me to sit on the sideline and say he should have offered to put his job on the line. He didn't. He chose not to do it, and maybe from some greater point of view it was reasonable not to do it.

Doel: How close did you feel that the JOIDES program came to what you had hoped to achieve?

Munk: Well, it was much better of course. But you know the basic fallout is very much involved. We felt as a group that MOHOLE should not be a one hole task-- that you not put in \$10 million just to bring up one pound of sample material. We felt rightly that the earth is more complicated than that, that a single sample of anything had not ever solved anything, that there are differences in the mantle and differences from one place to the other. I think that has turned out to be the correct view. People who believe in single samples solving a problem, like the physicists sometimes do, it's not the way it goes. I remember during the war there was some question about absorption of light in the ocean, how far could you see a submarine, and the physicists at Point Loma said, well, we'll go make an experiment and measure *the* absorption of light by water. Well, it's a silly way of putting it. It will differ by orders of magnitude between the Sargasso Sea and being offshore here, and this idea of getting *the* number, like getting the atomic weight.

Doel: But as you say, that's very much in the thinking and the training of physicists.

Munk: And that became the key issue: it was getting *the* sample of mantle that was going to solve the problem.

[end of Tape 2, Side B]

[beginning of Tape 3, Side A]

Munk: Brown & Root looked upon it as a one-hole problem: you get *the* sample of the mantle, everybody would end up with a Nobel Prize for learning all about the history of the earth, and I think the committee correctly said that that doesn't solve earth problems. First of all, on the way down to a deep hole one should learn all one could from sedimentary samples. Now you see that is a side issue, but it's scientifically the correct one. You should gradually move towards deeper and deeper samples. It was on that issue that the thing eventually came apart. And there were some Congressional hearings, and one scientific group at the time, I wasn't involved, actually said that they thought that it was a waste of money to proceed on that one-hole basis. The thing was canceled after quite a bit of hardware had been built, and so on.

Doel: Indeed.

Judy Munk: But it was canceled by the scientific group.

Munk: We said that the thing had gone haywire and it wasn't worthy of further public funds. What I wanted to say is that, JOIDES certainly rejected the view that you go for a one sample business; you go and look at the different terrains. In retrospect it was absolutely the correct decision.

Doel: Yes . Were there any geophysicists connected with MOHOLE who thought that one sample might be a proper method.

Munk: No one I remember. The people at NSF who had chosen Brown & Root were defensive.

Judy Munk: Their answer was that if they couldn't lick the problems they'd hire an acre of engineers.

Munk: Oh, the Brown & Root problem. Yes . What Judy referred to is that one time it was pointed out to Mr. Brown that they no previous experience in this, and he said, "I can always hire an acre of engineers." I have never forgotten that sentence. It seems the epitome of what we don't like. When you have no knowledge in a subject.

Doel: At that point did you have any input into the way in which technical decisions were being made at Brown & Root?

Munk: Well, we fought it, and we went to numerable meetings. You know, the Academy doesn't make it difficult to have meetings. But they just didn't get anywhere. I remember trying to be involved in an article in *Fortune* which I have mentioned, because it was the best writeup I think that anybody has made, and I have the reference to it. I had a date with a person who was writing it. *Fortune* magazine at that time had some very long, detailed, meaningful, carefully researched articles. Maybe they still do. I don't know about it.

Doel: They certainly did at that time.

Munk: And I flew East, especially for an interview with this man. I remember instead of landing in New York at 9:00 p.m. when I was going to meet him, there was some problems with planes and we landed at 2:00 a.m. in Philadelphia, and I took a taxi all the way with some people to New York—and finally got to his house around 5:00 a.m. and woke him up, and we talked all morning. I mean we were committed at that time, but it was a losing battle. That was a very good story. Anybody who wants to do that, should go back to that *Fortune* story.

Doel: That speaks to your own commitment.

Munk: Too late. I should have been committed when they picked Brown & Root. Or somebody else should have been committed. Don't run it by committee. I mean, the trouble is there were five of us, and none of us felt it was our job to make sure that this wouldn't happen.

Judy Munk: And I don't think Bascom ever understood why he wouldn't be picked instead of the committee.

Munk: Yes, he thought it was so obvious. Of course Brown & Root eventually offered to give him sort of advisory position, which was just a way of avoiding.... It's a sorry part of the history.

Doel: But as you say, your level of commitment to other projects during that period of time was extraordinarily high. Was there any discussion with Harry [Hess] and Roger [Ewing] about bringing in someone more junior as a way to address these problems?

Munk: Well, Bascom was brought in as the project director. He's not a particularly good scientist, but he certainly had a good track record of getting things done, and he lived up to that expectation. He's a good engineer.

Judy Munk: But they wouldn't have it.

Munk: But Brown & Root didn't want him in a leadership position.

Doel: And you say by this point, the decision had been made in such a way that your input was quite limited?

Munk: Mm-hmm [*affirmative*].

Doel: Were there any lessons that you took from that as JOIDES developed?

Munk: I had no part with JOIDES, but as I mentioned, it was anti- the one hole approach. And it was very well done. They brought the oceanographic institutes together. Scripps was the leadership institute. Bill Nierenberg did a very good job—this was early in his directorship—of really taking his responsibility as Scripps director seriously. There was a little of jealousy between him and [W.] Maurice Ewing, who wanted to really make all the decisions, and Bill didn't like that. But I thought Bill was at his best in getting that started.

Doel: Did he talk to you about difficulties in dealing with a strong personality like Ewing?

Munk: Yes he did. You know there was this funny...it was sort of an unwritten law that everyone who was a marine geologist agreed to, which is you never find oil beyond the continental shelf. It was the number one law like you mustn't steal or something. And of course the first hole they drilled in the Gulf of Mexico, when they were offshore, they found oil and gas, it was very, very funny. Really that destroyed a long-standing idea. Somebody who knows more

geology than I do should put that in better words. But I think Bill [Nierenberg] found it difficult to deal with [W.] Maurice Ewing, but it wasn't easy to deal with Maurice Ewing. He really wanted to make all the decisions. But I thought, as I said, that you know I've said some negative remarks about Nierenberg, but not when it came to getting JOIDES started.

Doel: And as you say, you were not deeply involved?

Munk: I was not. I was not in any significant way involved in JOIDES. And that was a great success story, wasn't it? I mean, I mean some people say that it's one of the important events of our generation. It certainly did end up years later, or still going on today, with having a theory about the history of the earth that is well substantiated and many of the problems have fallen into place. And that is entirely different from what it was before it started.

Doel: As a technique and an organization it was a major development in the earth sciences.

Munk: Yes.

Doel: When you necessarily became more involved in science policy issues, as MOHOLE unfolded, did you come into contact with people like Harvey Brooks and Don [K.] Price about that?

Munk: Yes. Well Harvey Brooks through Roger. They were close friends. I don't remember being involved with Harvey. When was MOHOLE by the way, in 1960 or so, I was involved with Harvey Brooks when I was asked to go to Woods Hole. He was one of the people who spoke to me about this.

Doel: What were your impressions of Brooks and the way that he viewed science policy?

Munk: Oh, he was one of the important people, and he was very good. There are many, many committees to look at ONR and how they are functioning. Only three years ago I was on a committee that was chaired by [Richard L.] Garwin]. Somebody gave us a document of four pages about what ONR should do and has done,. I read it and I had no idea that it was dated 30 years earlier, and Harvey Brooks had written it. Somebody just said you might like to read that before we meet tomorrow morning. I said, "Gee, that's great. Who wrote that? That's what we should send in." Well, that was written by Harvey Brooks 30 years ago. And only reason we should have known is that it was some paper didn't quite look like modern paper. So that's my answer to you about Harvey Brooks.

Doel: What was he like as a person?

Munk: Oh, he was warm and fun. I didn't know him well, but well enough, yes.

Doel: You became involved in JASON at virtually the same time that MOHOLE was coming about.

Munk: Yes.

Doel: Did your experience within the defense community, influence what you did within MOHOLE, or did the two just seem to be very different?

Munk: Not MOHOLE. It certainly influenced my work. Not the Waves Across The Pacific, later on when I became interested in acoustic problems.

Doel: Yes . I want to make sure we cover that as we get into ATOC and the controversy in detail tomorrow.

Munk: Uh-huh [*affirmative*]. This is very helpful. I thank you for doing that.

Doel: You're pointing to a time line that we have here, that we're using in the interview.

Munk: Yes,

Doel: By the time that Waves Across The Pacific came about, you had been working with Frank Snodgrass for almost a decade.

Munk: Yes.

Doel: How had that collaboration first come about? Did you have a role in bringing him to Scripps?

Munk: Yes, I brought him here. I was interested in waves, really going back to World War II. We talked about this before, and wanted to do some experimental work. And I went to a meeting where Frank gave a talk on remote sensing. He had put in a wave recorder, and he brought it in over the telephone, and he was going to show a wave record at a San Francisco meeting that was taken a hundred miles away. I mean, ridiculous by today's standards. I only remember when everybody in the room watched this enormous undertaking, and there was unfortunately a human being in-between. He had to call up some corporal at some marine station to say, "Throw the switch," and so when we were all waiting breathlessly and he took the telephone and said, "Throw that switch," and at the other side there was a voice saying, "Switch? Huh?" It turned out that corporal had gone to lunch, and nobody knew about it. It was a total failure. [*laughs*] Nothing happened. But I asked Frank then and there whether he'd come and work with us. And he did. He was an extraordinary undertaker. He was an engineer who really very much was involved in the fundamental scientific problems. Not so much to solve them, but they drove him to do his work. He was really motivated by the interesting scientific problems, so he did not himself do much in the way of writing. That was a great partnership. I eventually got him an

honorary Ph.D. degree in Australia which he deserved. He had been co-author on many papers. Certainly the work wouldn't have happened without him. It was the essential thing.

Doel: Indeed, he was working on digital recording.

Munk: At a very early time. When the first hand-held computers came out, he took one apart. He used the logic to be part of a computing [??]. It was way ahead of the time. He was always fascinated with digital problems, and we worked very well together for many, many, many years.

Doel: What was it about him that made that work out so well?

Munk: Well, he loved the sea, he was very good in the ocean, he was very dedicated to his work, people liked him.

Judy Munk: Sweet man.

Munk: And he was a sweet man.

Doel: I imagine he was often here at the house?

Munk: Oh yes. We were close friends.

Judy Munk: No, no, oh, I was thinking about his—

Munk: Your silence was deafening.

Judy Munk: No no. He went through a kind of a complicated—

Munk: Divorce history.

Judy Munk: Yes .

Munk: Yes, and we are still in touch with his widow. We were good friends.

Doel: Did the divorce come during the time that he was here at Scripps?

Munk: Yes.

Doel: Before he retired?

Munk: Yes.

Judy Munk: Oh yes.

Munk: But he was a great partner. I really should say, it's been probably said before, that I'm awfully dumb on instruments, not good at making them work, so I depended very strongly on that partnership. Yet I like to work on problems that have to do with data. So this was a very essential, and it was a real partnership. I don't mean to say he was an engineer who worked *for* me; we were partners. We decided what to do together. No place was that more true than in the Waves Across The Pacific where we made that decision jointly to try and do that experiment.

Judy Munk: He didn't make you feel guilty about not managing instruments well either.

Munk: No.

Doel: But others did?

Munk: No, no.

Judy Munk: No, no, no, it's a huge joke.

Munk: It's been a huge joke. They keep me doing something that keeps me busy so I don't go and throw any switches, and that's all right with me. There are people better than I am who have been in that position. [*laughs*]

Doel: You certainly made that clear in your writings, that this was not the role you wanted to play on board ships.

Judy Munk: I remember Frank tried to teach you something when you were building your first music machine. Don't you remember, when you were building the hi-fi set here?

Munk: Oh, I tried build the—

Judy Munk: The Heathkit, right. Frank just despaired, because you kept looking at the wiring and thinking, "Well, this is a much better way to do it."

Munk: And then 27 steps later I found out that I had gotten into a corner.

Judy Munk: Then Frank would have to come up and start you all over again.

Munk: Right, right. These were these kits for building your own radios during the [unclear].

Doel: Of course the Heathkit made one of the first kits for computers as well.

Judy Munk: Right. I know. That was what was so funny.

Munk: But Frank was really quite a very interesting— for example, he was interested in Boolean algebra.

Doel: Is that right?

Munk: He was a person interested in basic digital theory. he was a deep person.

Doel: It sounds as if he became very interested in the sorts of problems that you felt were important.

Munk: Yes. He had worked with waves before he came here, and then when he came, obviously I think he thought it was a good marriage. He worked for the engineering group there, and we had much more interesting problems, really. He certainly participated in the excitement of, first of all when we measured the early swell measurements at Guadalupe Island, then off San Clemente here. We got these very long dispersive signatures, which indicated a range bigger than the Pacific Ocean was wide. We had that excitement together. So either there was something wrong with basic physics—because it was very easy to measure that from the rate at which frequency changed from day to day-or it was something else, we said the only that is possible is it's coming out of the Indian Ocean, and there is a great circle window into it. We said well let's find out whether that's really so.

Doel: That became the motivation for doing Waves Across The Pacific?

Munk: It first became the motivation of doing a triangular array of San Clemente Island for the purpose of measuring direction. We inferred from those signatures that it had to come from a narrow sector either pointing towards the Tasman Sea between New Zealand and Samoa, or to the left looking down from here of New Zealand, between New Zealand and Antarctica. But a narrow range anyhow. We said let's go and measure the direction of that long swell and see whether it agrees with our assumption. And that's where it came from. That was years before, and we set up a triangular array off Clemente Island. I had the pleasure of learning the language of arrays that the astronomers had developed.

Doel: Radio astronomy was just coming into existence.

Munk: And they had learned how to use arrays of pointed receivers by appropriate—what's today totally standard—techniques to get directions. Obviously you get directions by getting the phase at which a given wave comes across your two receivers, and you need three to remove ambiguity. And we did what, I thought at that time was very ahead of the time: using e modern cross-spectral techniques, we established the directions very, very clearly of incoming swell. We found that they were consistent with the proposed window. That was the first thing.

Then years later, having established that, we thought it would be fun to measure along a great circle route--we could then understand and see what happens to a swell system as it moves away

from the storm area at very large distances. And I had the wrong idea. I thought attenuation would be associated with the long period swell coursing the trade wind sea and interacting nonlinearly with another wave system. And accordingly we placed our stations on both sides of the trades rather closely. That turned out not to be the case. There was no anomalous attenuation in that strip of latitude.

Doel: But it was only after you had done the experiment that you began to find that you might have wanted a different distribution of stations?

Munk: Correct. That's when I tried very hard to persuade the people who made the film that they shouldn't be afraid to say that the experiment didn't come out as we had planned, because that's the way it's supposed to be. If you knew what happens, you wouldn't have the experiment. It's been my experience all my life that any of the successful experiments have really not come out the way we thought. I think that's part of the fun.

Doel: Some experiments that we most well remember are those that didn't confirm what was expected. I'm very curious how you came to learn about what the radio astronomers were doing with their arrays.

Munk: Well, I had become interested in the modern methods of power spectral analysis I think at a very early time. The people who did it earlier than me in oceanography were the British. A man named Norman Barber, from New Zealand, worked in England during the war and did wave analysis. My contact with John [W.] Tukey was about the same time.

Judy Munk: Well, you had Norman come here to stay for awhile.

Munk: Yes . And Barber came here for a year, the analysis of the triangular array of San Clemente Island which was published in the *Phil Trans* [*Philosophical Transactions of the Royal Society of London*]. Barber and I and Snodgrass are co-authors.

Doel: Right.

Munk: So we learned together, learned a lot from him, and it wasn't all that difficult. The radio astronomers had done something very obvious, I think the key lesson was you could do almost as well with a few point observers than with a single enormous dish, provided you obey proper sampling theorems that you are not too far apart. And one can define what too far means very precisely. I did learn that, probably not by reading, because as I told you I don't read well, but I learned that probably by hearing about it and then trying to figure it out myself, and from Norman Barber.

Doel: Had you met anyone in the radio astronomy community to talk to them directly about problems of this sort?

Munk: Well, I met Tommy [Thomas] Gold. He was not one of the chief ones we met. The man who first measured a radio star and whom I became friends with and we later visited in Tasmania, the early people, yes I did meet some of those people.

Doel: Did you meet Otto Struve, who had been at Berkeley?

Munk: No, not Struve.

Judy Munk: He was a very odd personality, a really quite amazing man.

Munk: Yes. I found the interferometry in radio astronomy just fascinating, and we used the same principles, interferometry, in our wave work. Yes, and I did meet many of them. It was a small community and very easy to do.

It was mutually beneficial. They got a kick out of hearing our applications, and I certainly got a kick out of their pioneering work. There was Hambury Brown and [Twiss?] in Australia who did the pioneering work. Yes, and so I got enormous benefit from learning about these techniques and being one of the first to use them in the oceans. Yes.

Doel: What also struck me was that Waves Across The Pacific was, in its small scale, with a few observers, and a few locations, in similar relation as the IGPP was to SIO, and as Waves Across The Pacific, was say to the International Geophysical Year, that you fashioned this as a small scale undertaking.

Munk: Yes, yes, as we did the Heard Island experiment. It was done by a few friends getting together, no organization—there certainly was no organization, no coordinator, no administrator. It was a very romantic thing to set it up. There you sit with a map out and say, well, here's a good great circle route to the islands nearby, all right, you take this and our only graduate student was going to go to Yakutat, Alaska—

Doel: Alaska, yes.

Munk: We persuaded Flip to go where there were no islands in the North Pacific. Klaus Hasselmann, who is probably the leading oceanographer of my era, offered to take Hawaii, and remembers that with great fondness. I took Samoa, and then a man named Peterson [Powers?] took New Zealand. Frank was the traveler who was going to go back and forth and make it all happen. I mean he was the key person who did all the instrumentation. And there was a big step from sitting here and saying alright, here is an island, and we really want to go to that side of the island so we are exposed to the southern swell, and here is a little village according to my charts... here we looked at some Pacific island maps, Yaloitoi, Samoa, it looked alright, and we wrote a letter to the village. That this is between that kind of planning and actually getting there is one of those great, enormous, romantic things about oceanography which is so much fun. We never got an answer from that village. We wrote twice. Eventually our Samoa station was a

success, because right outside that village there was a little rock corner which was ten feet across where we could lay the cable so it wouldn't be pounded to death by the surf. Of course we had no idea. How can you know that without being there and actually taking a dive, a snorkeling dive? But, so it's a funny kind of thing about planning these things.

[end of Tape 3, Side A]

[beginning of Tape 3, Side B]

Doel: You were saying, ---I didn't want to interrupt you.

Munk: Yes , well, that's all. That was it. The enormous step between saying let's do it and Frank saying all right, we'll use digital paper tape. That's what we used at the time, very successfully. And each of us saying all right, we'll go there, and poor Gaylord Miller going to Yakutat Alaska and finding he had a great difficulty getting a cable ashore and had to shoot it in, and us in Samoa arriving and not knowing that anybody would put us up in that village, with my two daughters and my wife. [check publication]

Doel: So you arrived without knowing?

Munk: Without knowing. The chief, this fellow showed up next morning, and I said, "God, I'm glad to see you. I didn't know whether we could come to your village. He said, "Oh, we built you a fale. We're looking forward to seeing you." I said, "Why didn't you write me?" "Oh," he said, "We Samoans are known for our hospitality. We don't have to write you back and say. You should have known that you would be welcome." [laughs]

Doel: I have a xerox here from your popular presentation on Waves Across The Pacific, showing not only a map of the experiment we have been discussing, but also a photograph of you and Judy in front of the fale.

Judy Munk: Yes , Deborah [Day] at the SIO archives has my diary of that period.

Munk: And we had great difficulty with Flip. They ran out of beer or cigarettes or something.

Judy Munk: Cigarettes.

Munk: The Flip captain decided to go home. And that really would have been awful. I sent them a telegram saying if they went home I wanted a statement by the captain in writing that it was a matter of danger to the ship to stay in position longer, and that I would— And they never left. Such a statement was not forthcoming. And you know we had a station on Palmyra Island on the Equator which Gordon took. That became a very dramatic thing, because the radio man who went with him—

Judy Munk: Well, but previously, before you sent the man out there, you consulted your friend John Isaacs.

Munk: Oh, yes. I consulted my friend John Isaacs, who said, "Never be"—

Judy Munk: I hear the kids and we were all drinking bourbon and then he gave you this advice.

Munk: He said, "Never be under the control of your radio man. You must have a code so you can send information out that he doesn't understand." I said to John that I thought he was a hopeless romantic, but maybe he was right. And we have a code that if there was an emergency on the island—because it was uninhabited with only those two people there—Gordon should send a message saying something absolutely idiotic, and the idiotic thing was that the "Fourier integrals didn't converge." You know, I'd kind of forgotten about that, and I was living in Samoa with Judy, and suddenly a message came via amateur radio to Samoa that somebody there...an amateur radio operator came, took his bicycle down and said, "I've got a message to give you," and it said "the Fourier integrals didn't converge." Now imagine sitting there on a tropic island remembering that you had that, and what do you do about it?

Doel: Yes. What did you do about it?

Munk: I flew to Hawaii, we chartered a plane, and we went down to Palmyra Island. They had almost killed each other. We took the radio man off, whom Judy when we interviewed him had told me rightfully that he was not well balanced. I had ignored her superior judgement. We took him off, and brought another person in.

Judy Munk: It would have been murder.

Munk: Finished the job. [*laughs*] It's sort of an interesting story.

Judy Munk: Yes, but finding that island and finding him.

Munk: Oh, we had a terrible time getting to the island.

Doel: You had chartered a plane?.

Munk: It didn't make it. It couldn't find the island, because they were some World War II pilots who hadn't flown and hadn't navigated for some years. They had forgotten how to do it, and we had to fly back to Honolulu with two drops of gasoline left. Then next day flew again, and this time found it.

Judy Munk: They couldn't see the islands, you know, the few little clouds and you miss one of those little islands, you never know you missed it.

Munk: Yes . You may think it's easy to see an island, but when you have lots of little clouds, it looks just like another little cloud. So it was very dramatic.

Judy Munk: You were sitting behind him, when the sweat was coming out of his collar.

Munk: Right. The men couldn't find the island, and their gasoline was low. We had to make a decision as to at what point to turn around. There were no gasoline drums on the island, so you know it was a difficult decision to make. But the thing to me that was so interesting is that you sit here and you get a message like that. You are having a wonderful time and what in the world do you do? Do you really take off? I guess there is no choice. You just take off and live up to your commitments. But it seems awfully weird at the moment to do that.

Doel: I would imagine the feeling of being yanked out from that world that you had created with that kind of notice must have been really disconcerting.

Munk: But you would never forgive yourself if you had not done it. There have been a few times of my life, I don't know whether we discussed the H-bomb test that Isaacs and Revelle and I, yes, did --think we did--where we insisted that they should have a scheme for evacuation if there be a tidal wave.

Doel: Yes.

Munk: That was the same thing. I mean, we said there is a real concern, now do we do something about it? And there you sit. You say, and do we write a letter to the head of the Atomic Energy Commission saying he has got to do something? And you do. You hopefully do.

Doel: Yes . How long were you actually in Samoa?

Munk: Three months. Except for that side trip to Palmyra.

Doel: Yes. That was the one trip that you made during that time.

Munk: That was the one time. Right.

Doel: I'm aware, Judy, you took the 4:00 a.m. shift of measurements, for at least part of that time?

Judy Munk: Oh, [unclear]

Munk: Yes, she did. Yes, she did.

Judy Munk: Did you read our notes at all, my notes?

Doel: Those I didn't see. Not from that trip.

Judy Munk: Well, they're down there somewhere.

Munk: Judy wrote some good notes.

Judy Munk: Well you see I suffer under this reality, what do you call it, arrival. Walter had told me that in the South Pacific. . . .you know, we had these two little children. The children in Samoa and the South Pacific... Walter said if they were crying you didn't need to discipline them: the children, they could just run loose and they could all just be accepted by everybody. When we got down there, it really turned out to be quite different. When you live in a place with no walls, there are all kinds of walls, mental walls. You are not allowed to. . .you can't shine your light right or left. Everybody's territory is known ,even though you can't see where a wall is or where a fence is or where anything is.

Doel: You had to learn lots of rules?

Judy Munk: You have to learn lots of rules. And our children, you know, they didn't know these rules. I was so innocent, I just let them loose. They'd come back with burning embers on their legs because they had wandered into somebody's kitchen area, which you couldn't tell. Kendall, my youngest daughter, was a very little brown-skinned girl. She was accepted immediately. She had nice dark brown eyes and was a barefoot child; she looked like a Samoan for all intents and purposes, and so she was immediately accepted by all the children. Edie is fair-skinned, and she was a little older, she was, what, six, six-and-a-half, something like that, and she was looked on as the white bread. She was the "palagé."

Munk: "Hey palagé!"

Judy Munk: "Hey, palagé!" All the children would tease her, and of course she became very aware of this. Then we were givensilao, so that I could go have a little more freedom, since I was on canes. I wasn't in a wheelchair so much at that time. Then they assigned the chief's daughter, one of the chief's daughters, to us to help manage my little open house, which had no walls. It was right where everybody could see us. And Walter had sent out a kerosene refrigerator, because we were changing the children's diets.

Munk: There was no electric power.

Judy Munk: No electric power.

Munk: No telephone.

Judy Munk: No telephone. We didn't care about the telephone, but the chief's sister appropriated the refrigerator immediately for her chickens, and the chief appropriated the other half of the refrigerator for his beer.

Munk: Yes . We didn't know there was a law that the chief has a right to everything in the village. So when the kerosene thing came, he said, "Ah, I've been waiting for years to keep my beer cold," and started filling it. [*laughs*]

Judy Munk: But that was really kind of wonderful, because that meant that he came to see us every afternoon and had a beer. Walter and all of us would sit there on this platform, which was like a stage looking out at the village, and we would hear all of the gossip.

Munk: He spoke good English.

Judy Munk: He spoke good English. And he had great fun with Walter, because Walter was under his thumb. There must be nothing that Walter could do without his permission. So Walter had to get dressed up in a lava-lava every week and sit with the chiefs to decide the problems of the village, for hours, while they negotiated what was going to happen. And he looked sillier than anything. He had beads around his neck, and he had to take off his shirt, and he had to sit in a position which cramped you up. And we were made to go to church every Sunday. We had to get all dressed up and wear hats and everything. The children had to sit in the front row. It was just like the Somerset Maughan stories, you know, with the old man with a fly swatter.

Munk: If children whispered—

Judy Munk: Whispered, he snapped them on knees.

Doel: He would snap them?

Judy Munk: Yes . The parishoners weren't allowed to leave. They'd lock the church doors. You couldn't leave without leaving your offering. And the clerk would read out everybody's contribution.

Doel: And the amounts?

Judy Munk: For the Missionary Society. Yes.

Munk: The chief would sit there as you left. Everybody had to give some money. He would say "That's not enough!" [*laughs*]

Judy Munk: The children were not ever allowed to interrupt their parents. They would stand on the sidelines when all the grownups were together. The children would go, "Psssst!" "Psssst!" carries a long way, you see, and the parents would ignore this until they got ready. Then they'd

go over to the side and then the children could whisper all these rules, you see. And our poor little girls, they really had a terrible disadvantage. The chief's daughter was supposed to be the, well, she lived in Hawaii during most of the year but when she came home she was to become the Village Maiden. The Virgin.

Munk: The Virgin.

Judy Munk: They are the ones that held the kava bowl at their ceremonies. Well, you weren't supposed to go shacking up in the bushes when you were serving your time as the "virgin" daughter of the chief, so her brothers and her father beat the hell out of her when she was caught in the bushes. Her face was absolutely smushed. She couldn't go out. So she stayed home with us. She couldn't move, because she was so beaten up by her father and brothers. So here was all this freedom that Walter had told me that these women were allowed, [*laughs*]. We arrived at a very interesting time. The old chief was dying. While he was dying, they had moved him to the ceremonial fale right next to the sea. The whole village took turns singing to him and bringing him special little things to eat, not ever leaving him alone. They would play the guitar all night and change the lights and keep the flies off of him until he died. Then they had three days of a funeral. Our little fale was right next to that, so we could see the whole thing. It was just like something out of Somerset Maugham. It was an unbelievable introduction into a village that had no, as far as we were concerned, no connection to the rest of the island during that time.

All the Margaret Mead anthropology that had been written, this turned out to be a joke.

Doel: How so?

Munk: Well, Judy, you better be careful in your professional assessment of Margaret Mead. I think there way a problem, you don't want to ask someone a question that suggests an answer, because politeness says you say "yes." So you say, "Isn't it so that you--?", that's the wrong way to ask a question. They say yes. I think you have to be a little careful. I don't know enough about her work.

Judy Munk: Right. But anyway it was a different way than suggested in "Growing up in Samoa."

Munk: Anyway, we had a wonderful time.

Doel: How much had you known of the culture before you got there? Had you been working to understand it before you arrived?

Judy Munk: You know, we went remarkably equipped as far as our own personal needs were concerned. I think that had to do with Ritchie [?].

Munk: Yes.

Judy Munk: I had a woman who helped me for many years with our eldest little daughter' who was not very well. Not these two, but another daughter. She was part-Cherokee but she had lived in the Philippines for a long time. She made me, she made me muu-muus that were long and could be worn to church. She really sort of equipped me with the things the children needed and warnings about how I would have to behave. And she turned out to be very right.

Munk: She did not go with us, but she helped us get there.

Judy Munk: It was very obvious that, after we got there, we turned out to be the chiefs "tourist attraction." We were on the route of the bus all around the whole island, and the chief used to show us off as the captive guinea pigs. Buses would come with their lights on us at night—

Munk: And shine in our fale, like a theatre.

Judy Munk: And shine on our bed. "We'd be sitting up in bed like this, and so I realized that I had to do something about my "stage set". So with the chief's wife, I bought several bolts of wonderful Hawaiian material. We made bedspreads and things so that we didn't look like shanty town. We looked pretty theatrical. And we had the instruments come right in on the back of the fale next to the refrigerator.

Munk: Yes . Right into the fale.

Doel: You were there 100 days, outside of the interruption. Did you try to continue other scientific research? How did you spend the time?

Munk: Oh, taking that much data and reading it and managing it. No, I don't think I did anything else then.

Judy Munk: Well, you had to keep the instruments going.

Munk: I had to keep the instruments going.

Doel: How much time did that take each day—if there was such a thing as a typical day?

Munk: Well, I think that's all we did. We had some problems occasionally with batteries. Charging our batteries became the chief problem, and the generator that we took along wouldn't work. You know how dumb I am at that. I had to learn how to fix the generator and charge the batteries. Very healthy experience.

Judy Munk: That gave us a chance to go over and meet the Catholic minister, because he had a generator.

Munk: Oh, he had a generator. It was that sort of thing that is infinitely time-consuming. Yes, it's a day's job to try to get a piece of bolt. Then we saw the local doctors who were former Navy people, Navy enlisted men, who became the doctors in these little villages, and were very good. You know, the Navy did an extraordinary job in helping the whole South Pacific culture—

Judy Munk: Not become like Hawaii.

Munk: Not become like Hawaii. Samoa had the ruling that the property could not be sold to people who were less than—and I may be wrong—eleven-twelfths Samoan. So it did not pass into outside hands. And that was a Navy, law that they had suggested, and the governor of Samoa, who was an American, and kept the property from falling into non native hands. I do not know what happened since, but it was a very interesting existence. Then there was the challenge of getting the cable out to the instrument, which was in a hundred meters of water, quite a ways off. I mentioned that it depended in a key way upon one single rock ledge where the cable could be led into. The Samoans were very good divers, so we did that. And then I do remember we had quite a bit of slack left, like a thousand feet, and we figure-eighted it outside of our fale on an open piece of land. The location was beautiful. There was the ocean, there was our fale. It was a magnificent thing. Then Frank Snodgrass reported to us somehow through amateur radio that we should be careful because in Hawaii the cable had deteriorated, and he didn't quite know why. He wondered whether we installed the appropriate kind of cable that should withstand immersion for a long time. So we were all very worried.

It turned out later that the reason his cable had deteriorated was that some fishermen had built a fire on top of it where it came ashore. It was not a deterioration. I was worried about that, and I went to our chief and said, "I think we better put a sign up where we put the figure eight that people should stay away and not build a fire there." "Oh!" he said, "That's not necessary. This is the land where my great-grandfather, the old Chief Satelle was buried, and his son, my grandfather and my father." He said, "No one would dare to make a fire on the land where the Satelles are buried." Then he waited a second and he said, "Better make a sign." So, you know, you are kept busy on things that you would ordinarily not spend any time on. It's part of the satisfaction of that existence. I mean, the most important thing in the world is that you find a little screw of a certain size which is necessary to keep your generator going.

Doel: Mm-hmm [*affirmative*]. You used the word romantic before. It seems that that was important for you?

Munk: Oh yes.

Doel: There was also clearly an attempt to communicate the kind of romantic quality of the experiment. It reminds me of the efforts with John Steinbeck to get the story out with MOHOLE. Was this something that you felt ought to be done with the story of this experiment?

Munk: Well, actually, the National Science Foundation had hired someone. They were quite anxious to let their support be known to a broader public and they hired an Englishman to make a movie. Have you seen *Waves Across The Pacific*?

Doel: I am aware of it. I haven't actually seen it.

Munk: Oh, you might want to see it. There are copies. This man came to our island, Adam was a wonderful man, and we enjoyed working with him. We have seen him since, but many years ago. So the *Waves Across The Pacific* movie was done at NSF initiative, but we liked so very much the man who was involved. I don't know whether we were seeking—I don't think we were seeking publicity, but we went along with the NSF plan. I certainly felt the whole thing was wonderfully romantic and would enjoy talking about it to friends and to give a talk.

Doel: Did you feel ready to come home after the hundred days?

Judy Munk: Oh yes, to go back to school.

Munk: If you mean were we anxious to get back, no. No. Judy and I have a habit that wherever we are, we'd like to continue doing it. That means when we're home we don't want to go away, and when we're away we don't want to go home. Some people are the opposite way. So I don't think we were just waiting—

Judy Munk: We were very concerned about worms, with children. They come up through your feet, so I was trying to keep shoes on the children. I could keep sandals on Edie, but I couldn't keep sandals on Kendall. Also we were worried about head lice.

Munk: And they did get head lice.

Judy Munk: I was very careful. There was a cold shower near where the toilet was, about 50 feet away. I had the children put their feet in special kind of water supposedly when they came in and out from outside. I tried to keep their head lice down. I thought I had succeeded. I got home and we had a series of very, very hot days, and suddenly the children were sent home with head lice. Sure enough I looked in their heads—Edie, my eldest daughter, had hair down to her knees, and sure enough the head lice had all hatched. I hadn't realized that the eggs were way down next to the roots almost of their head. I remembered my grandmother's recipe about head lice.

Munk: Honey, we're going too far away from the subject.

Judy Munk: Anyway, it turned out that Kendall had several kinds of worms, and nobody knew what kind of worms she had until we finally took her down to the clinic, and somebody left over from the war who had been out in the South Pacific recognized the worms. And we couldn't get medicine for her, because it wasn't allowed in this country.

Munk: We had to smuggle some in from England.

Judy Munk: We had to smuggle it in from England. Uncle Teddy Bullard smuggled the worm medicine in.

Doel: Very interesting. Social factors certainly influence how work gets done.

Munk: Absolutely. Yes.

Doel: I do think it's an important part of the story that you're both telling.

Munk: Yes. I think we wouldn't have done the experiment if it had been totally devoid of any excitement and romance. Any of the experiments I've done. I mean they've always had this element.

Doel: I think that's an important point that you're raising about the broader design. While you were in the Pacific, did you think about other kinds of problems that were coming into range of new instruments? It was just a year or so later that Frank Snodgrass developed equipment that could be free-dropped from a ship.

Munk: Yes. That was in our mind then, that we had been working with cable-connected systems. For ocean waves that's all right, because when you get down to a hundred meters offshore, you are offshore essentially. For tides that is not all right. We had the pattern of going to lower and lower frequencies. We had started out with surface waves, sea, swell and surf. Then eventually tides, and that's when we made the policy decision. I don't remember whether there was ever a moment when we said that we'd like to learn how to drop things to the sea floor. That was a very important decision.

[end of Tape 3, Side B]
[beginning of Tape 4, Side A]

Munk: That's a hard decision, because you like to keep a security leash on things. Frank really railed against that for a year. We would make tests for the boat but we'd keep a cable on. One day the cable got severed by accident, and it was really autonomous and everything worked fine. All the troubles we'd had for a year had to do with the leash instead of the cabling.

Doel: Interesting.

Munk: We went that way. And do you know, talking about things more generally, when we did our acoustics initially we worked with autonomous sources and receivers. Then, when we started getting into climate, we made the policy decision that we have to get to a cable connected source, because you need to make long-term observations. That may have been the basic problem that has hounded us since. It may have been better to accept the difficulties of autonomous systems,

batteries and things, and not worry about cables. Maybe even our problems with the environmental groups wouldn't have happened if we had autonomous rather than cable-connected systems. So maybe I have a lesson that I hadn't thought of all my life: don't tie things to shore, just drop them and leave them alone.

Doel: It's very interesting. But at the time this was a utilitarian and operational decision.

Munk: Yes. The tide gauges were very successful. We eventually ended up being able to measure a 2 millimeter amplitude in 4000 meters of water, and getting very high quality records. In some sense it turned out to be an easy thing to do. We had a little window really of 20 or 30 years before people were in to start measuring open sea tides from satellites using satellite altimetry. During that window, the few of us in this world who cared occupied probably about 50-60 deep sea stations. Today it's unlikely to continue because of the satellite measurements being so good.

Doel: And the coverage is sufficiently globally comprehensive at high as well as at low latitudes?

Munk: Right.

Doel: How did you arrive at these policy decisions, say the critical one of developing the free-fall instrument? You mentioned that Frank Snodgrass felt more conservative--that he really wanted the cable.

Munk: No. That was very much of a policy decision. I can't tell you know just how it was made, but we agreed that that's the way we should go. We were kind of excited about it, because there hadn't been any free falls. Now I mean it's enormously common, but it seemed like the thing to do. But once having gone on that way and building instruments and learning how to work, it was psychologically very difficult when you had it on a leash to say, "All right, let's drop it," even though we had the acoustical recall worked out. It wasn't until fate made it necessary to do that that we really dared to really disconnect ourselves.

Doel: Yes . I understand that. How actually did the acoustic recall work?

Munk: Oh, you give a signal to something on the bottom that opens. When it gets the acoustic signal, it opens up something that permits the instrument to come back by its existing buoyancy. In other words, it's always buoyant, but it's being held in place by some mechanical device that responds to an acoustic signal and opens up.

Doel: And that is what is triggered?

Munk: Yes. That of course is now commercially available and very routine and very reliable. But we were the first to build that.

Doel: What were the greatest challenges in the building of those instruments? You mentioned the power source as being one problem.

Munk: Well, lasting for a couple of months that's no problem. For acoustic source transmitting it's a very major problem, because your power needs are so much bigger. I think our principal problem was that it's difficult to build pressure gauges that don't respond a lot to temperature changes, and it's somewhat difficult to separate the two signals. That was the first thing. The second thing was instrumental drift. [*phone rings*] Judy? I don't want to answer that. The other one was that the instrument drifts away just by things that we do understand. Those were the two problems that never really had been solved, but we understand them well enough that we can live with it. The recall was remarkably simple. Of course the navigation was not so good, and we depended on finding our place by actually interrogating a bottom transponder. Nowadays you can do so well with GPS [Global Position System] that that is trivial.

Doel: Right.

Munk: This was not trivial at the time, when you have something in the middle of the ocean and come back two months later, and there is nothing around but water: where the hell is it? Is it still there? Just sending out an acoustic signal and getting the answer back was a fantastic experience. You know, you haven't been there two months and you send the signal, then you have to wait because it takes four seconds to get to the bottom. Then you have this interminable eight seconds, and suddenly you could hear "boop-boop-boop-boop." Oh my God, still there, still ready to be recalled. It's a fantastic experience. Everybody howls with delight. [*laughs*]

Doel: And by the late '60s you were working on a variety of issues relating to tides and long periods. The internal wave work was also beginning at that time?

Munk: It was beginning. I think I was largely working on tides. The internal wave work was not experimental at the time. I think it was Chris [H.] Garrett and I deciding to make a model spectrum which became known as the G-M spectrum, which seems to survive in some form even today. Yes. But the tides was the main experimental effort. I don't quite know. I need a better time line. The tide work was successful. There had been no previous deep-sea measurements of tides. We measured them, we measured them very well, and the problem of dissipation of tides in shallow water remains a first order oceanographic problem today. But of course the Tropics Poseidon satellite altimetry has given it an entirely different face. As I mentioned to you, the reflector from the Apollo on the Moon also has come in.

Doel: Indeed. In those early days when the first satellite systems were being developed, were you in touch with researchers at NASA with the intent of aiding development of systems that could be directed towards oceanographic problems?

Munk: Yes. The man who I most remember is John [R.] Apel, who was at that time placing SEASAT. Now we need a date for SEASAT.

Doel: SEASAT occurs in 1978.

Munk: Well past IGY, in the '70s. So that was much later then, than what we are speaking about, isn't it?

Doel: Yes.

Munk: But there were some plans then, and I remember his coming to Scripps to try and get some interest and not getting any. I did really from the very beginning feel that it was a very exciting thing and backed it as well as possible. I asked him the other day for his memory, because I kind of feel that as a whole the ocean community has never been very helpful in new ideas. They are conservative.

Doel: Do you feel they've been more reactive than proactive?

Munk: To that. But I mean I love ship-going work, but I think it shouldn't be combined with objecting to other methods of doing things, which it often has. Certainly the satellite work of oceanography was not spearheaded by the standard oceanographic community. It happened almost in spite of them. He mentioned that he had come to Scripps and talked to a leading oceanographer—that's not me, certainly, and I don't remember who it is—saying "well we will be able to measure the surface height to eventually maybe 10 centimeters." Of course they've done much better. And the answer he got, he told me that was, "We wouldn't know what to do with that data if we got it." Which is so totally wrong, because the satellite optometry has revolutionized physical oceanography. So I think the ocean community generally has not been at the forefront of new methods of observing.

Doel: In this particular instance, why do you think that was so? Why did others at Scripps react negatively?

Munk: Others at Scripps? Well, I wasn't the only one who backed it. But why did the community as a whole? I think that people have the false idea that it is in competition with the work that they are doing and think of it as a zero-sum game. So that if somebody gets some support, somebody else will have to do without support. In case of the satellite development, it certainly did not turn out to be the case. The fact that some new measurements were done and new things were learned had a positive feedback on the support of oceanography. And the basic idea of a zero-sum game, so that if you support Able [then] Baker is going to lose, has at least in my lifetime been wrong.

Judy Munk: Well, you know other people on the side now, when they say that they are going to have to give up some of the ships at Scripps, might say well—otherwise.

Munk: Okay.

Doel: In the current funding environment, I think that's been an important factor.

Judy Munk: Right. They won't put it in a proper context. [*interruption...*]

Munk: And maybe every community, I don't know how many communities are really open minded to basic change. Is that the rule?

Doel: There are times when a scientific community will very quickly incorporate a new tool or a new technology, but in this instance, when there is extraordinarily different kind of technology already in the hands of very different groups, it can be dislocating or lead to worries of one's own methods and techniques and ways of doing science being undermined. I was wondering if you felt that was the sense of this situation here.

Munk: Yes. There was a meeting in Melbourne last year by the Commission on Physical Oceanography. There is a, I forget the acronym. It's an ancient group that has existed for 30 to 40 years. Roger [Revelle] was president once and gave a talk which had as a theme all the new developments that were taking place like the satellite stuff and others. I gave the presidential talk, as it was called, in Melbourne last year. I quoted from Roger and really used as a theme the idea that the community has not as a whole welcomed new developments but they eventually sort of grudgingly accepted that. That talk was published. It's very much that idea. And I think that there is some truth to that.

Doel: As you look back in this period, I'm wondering whether you sense that in significant ways oceanography was becoming big science, and if so, what are some of the benchmarks where the transitions occurred?

Munk: Well, there was the collected work when mesoscale eddies were discovered, the MODE expedition, Mid-Ocean Dynamic Experiment, which was sort of run by Henry Stommel and Allen [C.] Robinson. And that was the beginning.... That has sort of a very interesting background. Oceanographers had really visualized their ocean as one of steady flow so that if you once learned that the current does this and that in that area, you can apply it to the future. There had been a few disturbing experiments indicating that there were enormous changes from one time to the next [or perhaps it should be 'from time to time']. And oceanographers were so short of observations that you didn't generally repeat observations. You said, "I've done this area. I don't want to do it again."

Doel: Right.

Munk: When that was, it was found that there were enormous changes. I mean currents were reversed and go in different directions. John Swallow, with his mid-ocean floats, really did the measurements that kind of revolutionized and— [*knocking on door*] Excuse me. [*interruption*]

Munk: Then that led to a set of experiments called the MODE experiments, which [interruption] Hi, Brad. That was a major effort that involved all of us, the whole community, and more than many ships. That was really the beginning of a sort of a big experiment with international scope, and taught us that the very ability of the ocean was much, much larger than people had thought; that most of the energy, kinetic energy is in these variable eddies and not in a steady circulation. That was a revolution.

Doel: It was. That was critical.

Munk: Mesoscale resolution. And I think that was the beginning of, in some ways, big oceanography.

Doel: One thing I noticed in looking carefully through your own publications is that by the mid-1970s not only are the multi-authored pieces much more frequent, but by 1976, '77, '78, papers with three or more authors becomes much more common. And after that, papers with even more authors.

Munk: Mm-hmm [affirmative]. That's part of that same thing. And now of course with our ATOC it's terrible. I mean there are so many people who have really contributed. You have a group of ten people or more, and you really do need to mention their names. Yes.

Doel: Yes. Is it a different feel for doing science now, or did you find it much more of a gradual evolution from smaller to larger scales?

Munk: Well, we had a group of people when we did the Waves Across The Pacific, there were seven or eight of us. The tomography thing has become bigger. There is that change, and of course there have been these large expeditions which I think were necessary. They were beyond the scope of a single investigator.

Doel: Let me ask one last question for this session. We will pick up on lots of issues when we resume tomorrow. In the late 1970s you had received the start of a number of major awards from the Navy and from other groups. And I think particularly in this case of the Conrad presentation that you had gotten in 1978.

Munk: When did I get my Navy chair? In '85.

Doel: That was '85, so that was a number of years later. There was a sentence that stood out from me when I read the citation, particularly recalling what you mentioned earlier in the interview. It says that during the war since you were foreign born, you didn't have a security clearance, but this in no way hindered your work, which you carried out with a great diligence. Of course the actual situation was much more complicated with the security clearance matters. And I'm wondering if that moment gave you a chance to reflect back on that really long association that you had with the military, and whether you felt by the late '70s that the needs of the military had

changed. Were there different kinds of demands that one had to take in mind to defend the United States by the late '70s compared to the 1940's —and the kind of service that you could provide?

Munk: Well, JASON played a major role. I had a fairly complete set of clearances, which I still have. And the Cold War did present some very high level problems that I was aware of and worked on, partially with some success. And that certainly is a major part of my life. I mean there were things that happened that really were of concern all the way up to the President that had to be solved, of things that they were doing which we didn't understand and on which we worked.

Doel: These are matters that remained classified to the present?

Munk: Some.

Doel: I'm not asking you to talk about classified work.

Munk: In some sense we were more afraid of the Russians than we should have been. We gave them credit for doing incredible things which we couldn't do and therefore it was amazing, and then turned out that they couldn't either. I think we over estimated. But that's what you probably have to do in a time like that; you have to be overcautious. But I was very interested in that, and in some sense qualified because of the work we'd done to think about those things. I really appreciate that, in spite of that early clearance problem. I eventually had a chance to really be helpful and spend a lot of time doing this.

Doel: In some ways the broad concerns of military and related agencies have shifted from the Cold War adversaries like Communist China and the Soviet Union towards less developed countries, to other sorts of problems.

Munk: Yes.

Doel: Has that affected your relationship to the military?

Munk: Yes, in a way it's less exciting. Having a technically able enemy who does things that are very curious is very exciting, and it gives you some real problems. But certainly part of my life was under that shadow, and when I would go to Russia with, knowing I had some clearances, it added some concern about what to do. And I played a somewhat meaningful role—I mean, it was known that I had certain things that I was associated with that we wanted to not spread, and it added a lot to the excitement of life I think.

Doel: I can imagine. When you say that, I was wondering if at any point you were worried about the possibility of a kidnapping when you were in the Soviet Union?

Munk: Well, I was always amazed. I've been to Russia four times, and I always sort of expected, and had been warned, like all of us, to beware of beautiful women who try and come into your bedroom and some things like that. I was always a little disappointed that nobody tried to pump me and no beautiful women every came into my bedroom. I once asked somebody in the Naval Intelligence business why that was so— [*phone rings*] And well they gave a nice answer. They said, "It was probably known that you wouldn't respond," or something like that. But I still have to say that. [*laughs*] So I never went to a point when somebody tried to pump me that I was aware of. And they must have known, I'm sure they did know, that I was associated. There is a man in Russia who is well known, and I was just looking at his book when you came.

Doel: [Leonid Maksimovich] Brekhovskikh, who had done sound channel work in the Soviet Union

Munk: Yes, who is sort of the acoustics man in the Russian Navy. I sometimes felt I had a little bit of a parallel here, and knowing him made it even more interesting. Did we tell you the story about the Greenland Sea experiment?

Doel: We are going to cover that tomorrow. I very much want to hear it.

Munk: Yes . When I had to go to visit him in Russia for fear that there would be interference. I must tell you that. That's an interesting story.

Doel: I very much want to cover that time.

Munk: He recently got an award, the U.S. Navy and the Oceanographic Society was kind enough to establish. It's a medal that carries my name, and he's the fourth recipient, this man. He gave a talk when he received it, which is kind of fun for me, in which he said that he was so interested because he and I had somewhat overlapping careers on opposite ends of the Iron Curtain. He as an acoustician who got into oceanography by force; I was an oceanographer who got into acoustics in some sense by force. He says that in his talk. It's a delightful talk, and I have a copy of it.

Doel: That would be very interesting to see.

Munk: Yes. So my Navy existence was mostly, but not entirely, JASON. And of course before that there had been those Bikini tests which really put me in touch for the first time. And I have great respect for the professional people who do the work. There is a tendency in my community to sort of downplay the accomplishments of people who belong to the government, and I don't participate in that.

Doel: I'm thinking of a recent book chapter that Ray [Raymond] Siever published. The book was edited by Noam Chomsky and it was on the university in the Cold War era.³ One point that

Siever tried to make about postwar geophysics generally was that research priorities had been set, perhaps distorted, by cold war funding opportunities.

Munk: I don't feel that. That charge is made by a lot of my colleagues at Scripps, you know, that ONR was giving money to, throwing money at people like us because they had particular needs. I don't feel that the Navy ever really in a way interfered with any kind of decisions of what work we wanted to do, and I really totally disagree with that. Now there is of course a set of interests. I think that the opportunities for choosing subjects are so vast that— *[phone rings]* I'm sorry. That I have no objection to having them somewhat focused by national needs. In some sense the challenge when there is a need is more satisfying to me than if there is not.

Doel: That's a very interesting way to put it. We have plenty of questions that I still want to cover with you certainly tomorrow, but this might be a very good place to bring today's session to an end.

Munk: Okay.

Doel: Let me thank you very, very much again for this long session. And we will bear in mind that certain sections of it are closed.

Munk: You ought to check with Deborah [Day] whether there is a cutoff point, because I guess tomorrow's session will be a little different.

[end of tape; end of February 4th interview]

ENDNOTES

1. The S I O: The Revelle Years. University of California, San Diego, Scripps Institution of Oceanography, Annual Report 1991.
2. [The Library of Congress catalog doesn't list a clear candidate-- you don't mean Higher Education Cannot Escape History (1994) ?]
3. The Cold War & the University: Toward an Intellectual History of the Postwar Years. New York: New Press. Distributed by W.W. Norton & Co., 1997.

MMD

Interview with Walter Munk (and Judy Munk)
in La Jolla, California
Session #5

by Ronald E. Doel
February 5, 1998

Ronald E. Doel: This is Ron Doel, and this is a continuing interview with Walter Munk. We are making this recording on the 5th of February, 1998 in La Jolla, California. Just about half an hour ago this morning you heard that your chair from the Navy will be extended to the year 2000. Congratulations. It raises an interesting point about changes in patronage for oceanography and other sciences. Funding has shifted increasingly from block grants to institutions and individuals face greatly increased competition in grant proposals. What are your thoughts about that trend?

Walter Munk: I'm prejudiced to think that there is a place to give some support to people who have a good track record and not to depend so much on the ability of writing proposals, which are expensive both from the point of view of the proposal writer's time and from the agency's need to review it. The old ONR of course was very much attuned to the track record of the people and those who had made contributions and had also done things useful to the Navy would have their proposals almost automatically renewed. I think the record, if somebody were to study it, was pretty good of ONR and the country getting its money's worth. I'm very, very lucky in having been given a Navy chair which permitted one to work almost without any bureaucracy. There was no need to tell ahead of time exactly what you would do, nor where you would travel, nor this and that.

I was the first person to get one of these chairs in 1985, and one way or another with some changes this has been extended. I'm now 80 years old and I still wanted to go on, because I have some things to work on. On my last trip to Washington I stopped to see Lou Goodman, who runs Physical Oceanography, and Jeff Simmons, who runs Ocean Acoustics, and asked whether they would let me keep my balance and add to it for another two years. They said I should write a letter and this morning I had a call that they will do so. That leaves me in the wonderful position of having had generous support for about 15 years, which I think has worked out well for

everybody concerned. And I think, as I said, there is a place in the present competitive arena for some support based on this kind of a consideration rather than on specific proposals.

DOEL: Has it seemed to you a gradual shift from the block proposals or the individual support to the competitive peer review, or is there a period of time that you look back on where that seemed to change quickly?

MUNK: I think that's been a gradual shift, and it has something to do with the supply and demand problem when there are lots more people and less money. It became increasingly scrutinized and then impersonal. I think that's happened not just with government grants. I once was on the Guggenheim Foundation Review Board, and I used to get the proposals in geophysics.

And there generally were let's say three times the number of applicants than the number of fellowships and I felt that the reviews could be quite effective. I would read all the proposals and try and learn something and say here are the outstanding ones and try and select one out of three. When that number changed eventually to one out of twenty, I thought it became a useless exercise. It was almost like throwing dice. And it's the same sort of thing that we're talking about.

DOEL: Yes. And this was also a gradual change, from the 1:3 to the 1:20 ratio? When did you first start doing the reviews for the Guggenheim Foundation? Was it in the 1960s?

MUNK: The one-in-twenty was just out of the hat, but it was one in many. I stopped doing it when it became one in many, because I was feeling so uncomfortable. I was playing God, throwing dice and not really making any informed decisions. Your decisions might have been wrong in the first place, but at least there was some substance. I thought that substance became very small when the ratio became too big. And I had no idea what you can do about it.

DOEL: Do you find that reviewers at NSF and the other large government agencies are voicing similar concerns?

MUNK: I hope so. I think so, yes. And the peer system of course is supposed to handle that. I think that I am not a great admirer of the peer system, but I don't know any substitute. The peer system inevitably works towards the conclusion that if you have a radical idea, something that's really unusual, you are not going to get the 100 percent support from the review committee that you need nowadays to get funded. So it is an inevitable outcome that safe science, rather than daring science, is the one that wins out. And there have been examples of that we could speak about, and I think that's not good, not good for the country.

DOEL: You're thinking of examples in oceanography?

MUNK: Yes.

DOEL: Which ones come to mind?

MUNK: Oh, some things now underway in connection with measuring tidal dissipation in the area around the Hawaiian Islands, a sort of a radical proposal, and obviously some people said that's not the way the oceans are. And you know it's been said that Einstein would have not gotten support for relativity and Newton couldn't have gotten support. One can ponder that. In some ways the Navy chairs that you mentioned are a response to not going entirely in the direction of these peer reviews, to try to have some balance. But I'm not well informed as to what the right solutions are.

DOEL: Within the IGPP you liked to use your judgement on the character of people for the short term visitors, for instance.

MUNK: Oh, yes, we do. Well, as you know, I have not been director for many years, but we have for example a fellowship program generously endowed by Cecil [H.] Green. We have Green Fellows, not by color but by name, and we have two or three a year that come from all over the world. Now that the program is 20 years old, I think you will find that nearly all the people who have been successful have at one time or another been Green Fellows. It's a wonderful program.

And we alternate between the old and famous and the new and unknown very deliberately. And it's the second class that's the interesting one. You don't go wrong in giving Sir Edward Bullard a Green Fellowship, which we did, and other people of that kind, but you may not do so well in getting in a postdoc here. And there we act very much on intuition, "we" being Freeman Gilbert, John Orcutt and Mark Zumberge and I. We meet once a month, and Freeman will say, "Well this guy really did a nice job when I heard him talk," and somebody else will say that, and we make quick decisions. As a whole I think they have been very good.

DOEL: Did you notice a shift, a change in the kind of funding, that the SIO was receiving generally from federal patrons in the 1970s?

MUNK: Oh yes. I mean, there was originally too great dependence on institutional grants, and it meant that the Scripps director and his administration could decide where to put the money. In some ways I welcome that the great decision-making power of the Scripps director would be a little diluted, so that the decision who should get financial support would come from an entirely different group, more national group, than the director. Otherwise if the director was mad at somebody, not only could he stop him from being promoted but he could also starve him to death. This way there was a bit of balance.

DOEL: This was during Bill [William A.] Nierenberg's time as director when these changes occurred?

MUNK: But it happened nationwide.

DOEL: Yes.

MUNK: At the same time, there is a movement towards these national programs like MODE which I think was inevitable. I'm not the one who has thought very much about those, and it's easy to express your dissatisfaction, very hard to find a solution.

DOEL: You're raising good points. When you were director of IGPP, what kinds of nonconventional funding sources did you try for? Or during that time did you find that you were able to get sufficient support from the major agencies?

MUNK: Well, I would divide that question in two. There was the problem of building the building, which I think we've discussed. Then there was the question of running the institute.

And did I tell you, Ron, about when I first got some money from ONR on my own research fellowship there was ten thousand dollars more than I'd asked for? I called back and they said, "Well, that's to help you start the institute." That was wonderful; I like to think it was money well spent in retrospect. It's almost impossible today. But maybe not quite. And we started talking this morning about the fact that I'm going to a meeting where we attempt to consider to what extent the new era of deep sea oil drilling in the open ocean has to do with what we know about the ocean, physical ocean dynamics. I would think there ought to be a source of support coming from that direction. There is some, but it's totally negligible.

DOEL: And you mentioned off tape this is a meeting occurring tomorrow in Irvine, California.

MUNK: Yes. Sponsored by the Heinz Foundation. And I'm coming in under the disguise of a industrial speaker, because my brother in law Ed Horton builds deep sea drilling rigs, and he and I had talked so much about it, he's asked me to give half his talk. So I'm coming in a very unusual disguise. I hope they let me talk. [laughs]

DOEL: That's interesting. I'm curious if you feel the oil companies generally have given the kind of support for oceanographic research that you felt was appropriate?

MUNK: Well, of course not. And I think they've been shortsighted, and they run into problems and then try and find fixes. They respond, but they have the general feeling that it's not up to them to really understand what happens, and I think that not understanding things can be very expensive, but it's interesting to me and what we've learned is that it's a new ball game. They're worried about mid-ocean shear, which gets into all the internal wave pictures. Entirely different kind of field, which we ought to take very seriously, because apparently it can be limiting to doing drilling work in the deep ocean. They found that the shedding of eddies in the loop current in the Gulf of Mexico-- this shedding of eddies by the current started a vibration that was so severe that they couldn't do their work. And now at the expense of a quarter of a million dollars a day for drilling, that's not a very nice thing to happen. And what do we know about it?

DOEL: Was there an attempt at Scripps to develop a kind of industrial associates program that would bring in support from oil companies?

MUNK: Yes. Under Bill Nierenberg a very definite attempt was formalized. I think Joe Curray was in charge, they would meet twice a year, the industry representatives would get the enormous asset in turn for contributing \$10,000 a year of hearing what people had done a little sooner than it is published. It did not survive. I mean it was basically a good idea, and maybe in some form it should resurface. It was mostly marine geology. But eventually -- I don't think the oil companies got their money's worth, and most oceanographers thought it was a nuisance and took time away from their field, and it was not very successful. Yet, I think somehow or another a way should be found to make that possible.

DOEL: I'm curious about something else that occurred when Bill Nierenberg was director: his

role in establishing the Climate Research Group in 1965. Jerome Namias and Richard Somerville were involved, as I recall.

MUNK: Yes.

DOEL: How did that come about?

MUNK: Oh, it goes back a long ways. John Isaacs was the man who had gotten Namias here, and even under Revelle John organized a meeting in Rancho Santa Fe that was really sort of a real event you ought to find out when it was in which the idea of first asking how does one predict variably on a climate scale came up. Jac Bjerknes was the key figure. He had been very interested in that. Since then of course this has become a very active field. I think Bill Nierenberg did just the right thing in starting such an activity. There was some reluctance by existing oceanographers to think about those problems. In some ways I think it's a key problem of oceanography today, what happens on a year-to-year scale. As you know, there has been progress with El Niño, but very little progress in other things. When we come to speak about ATOC I would like to tell you about the few things that we learned in the last few years, none of them had been predicted or even understood by the climate models.

DOEL: It's an important point. I do want to cover that in detail.

MUNK: Yes.

DOEL: I'm very curious about your recollection of the resistance to thinking about these sorts of problems.

MUNK: It's not less resistance, but you are in the middle of a job that you want to finish, and you have ship time for the next year, and you are not going to turn around and start something new. So it's a little bit like we talked about the new observation methods and satellites. The existing people, with some success, tend to have a great deal of inertia, and so really the right thing to do is to start new groups, which is what Bill did. Of course Tim [P.] Barnett was here, but he did that with the people you mentioned, and it's very similar to what Roger [Revelle] tried to do when he felt that our marine biology was old-fashioned and not keeping up with the time. He tried with some Rockefeller money to change the old group, and failed. And maybe that's the lesson. You want to do something else, you start bringing in some new people in a new unit.

DOEL: Roger's interest in the Rockefeller funding: was that at all related to climate, or was that entirely biological oceanography?

MUNK: Oh no, that was entirely biological. Modern genetics and new methods of observations had nothing to do with climate. Climate didn't really come up until after Roger and Ed and Dave Keeling got the CO₂ work and got everybody's great interest. Of course our time series of California currents started with the sardine work. We mentioned yesterday that the State of

California gave us two ships. That is one of the longest time series ever taken anywhere. We now have 30 years of it. And there are very significant changes off California. Again I don't remember whether we spoke of it, but the water has gotten three-quarters of a degree warmer on the average with very good statistics, and the volume of zooplankton has diminished I think by 80 percent from what it was 30 years ago. Yet we have no idea whether these changes are part of a larger picture. They certainly are not part of any climate prediction model. We have no idea whether it extends outside of the continental shelf and slope which is the area covered by those cruises, and I think it's that kind of problem which needs attention. And satellite oceanography is very helpful in considering the global aspect of these changes.

DOEL: How involved did you become in that work? Was that something that you had been involved in once the climate group was started?

MUNK: No, no. In some ways we would have liked to, because our ATOC work is definitely climate-oriented, but there is lack of interest if not resistance by the ocean community, including people here at Scripps, to the new method that we developed of ocean acoustic tomography as a tool. We have not been popular with the community, and we still today have not established ourselves as being one of the really important tools for doing that, and maybe we don't deserve to. We have to demonstrate that we can do some things better. I'm prejudiced to think that we can do some things better, but the battle is not over, and it was sort of diverted by our biological battle with the environmental groups which really took our attention off the main issue, which is to what extent can we measure climatic changes better, and in what context, than other methods. That is the real issue.

DOEL: But as you say, there has been debate within the physical oceanography community over its efficacy.

MUNK: Oh, very much, yes. And we're not very popular. There have been some statements that have made us quite unhappy, because we thought they were ill-founded. But the main thing is not to worry about that but to try and do the best work you can and see whether you survive on that basis.

DOEL: Mm-hmm [affirmative]. I was wondering when you mentioned those criticisms if you were thinking of Russ [E.] Davis here at Scripps.

MUNK: Yes. He is a close friend whom I sort of love, but he certainly has not been helpful in that work. He doesn't think it's the way to go. He thinks that his deep current meters his deep current drifters, excuse me, drifters which he pioneered, is the way to go. I think they are wonderful, but I think that we have a contribution to make. We could speak about that. It's very interesting. I think we should talk about that when we talk about ATOC.

DOEL: I very much want to. When you think back on that early climate group that Nierenberg established, was it well regarded within Scripps and IGPP?

MUNK: I think it was not well regarded in the same sense as I just spoke. I don't think that the Russ [E.] Davis group, which is very powerful in this institution, thought that this was an altogether good development. That had to do partly with personalities.

DOEL: How effective did you feel that early group was?

MUNK: I think it was a good thing to do, and there is always a problem. You know the key new element of climate work is Klaus Hasselmann's contribution. Have we discussed that?

DOEL: No, we haven't.

MUNK: Well I mean Klaus really defined the climate problem. It had been defined incorrectly in the days when I got into oceanography. At that time people had certain models of climate change that were very concrete and very non-statistical. They were something of this sort: if the water level rises in the Arctic you establish a new circulation into the Arctic Ocean. Then it gets warmer and the ice melts and you change the albedo and that has a back reaction, and you would get oscillatory solutions in a sort of a step-by-step manner.

DOEL: A kind of feedback loop.

MUNK: Feedback, deductive, totally deterministic. That's the word.

DOEL: Are you thinking for instance of the [W. Maurice] Ewing-[William] Donn model?

MUNK: Exactly. That's the prime example. All right, very good. Now, against that was the [Klaus] Hasselmann viewpoint that you have a random walk, that weather -- the short period changes -- give random pulses to the large scale circulation, and those random pulses, like any random walk, can produce large and long period variations. It's an entirely different view. It doesn't have that dramatic deterministic content, and it has definitely turned out to be the key issue in climate. And Klaus defined that, and is responsible for that. He was then responsible for founding an institute in Hamburg, which had been at the lead for the last 20 years.

DOEL: I wanted to ask you about that. That was the Max Planck Institute that he had set up?

MUNK: Yes.

DOEL: And you were on the board of advisors?

MUNK: Yes.

DOEL: For that? What was that experience like?

MUNK: Oh, wonderful. We'd go once a year to Hamburg I think I must have gone three times.

Klaus is a close friend of mine. I met him many years ago when he was working on ocean waves, and we may have discussed this: in older days, the director of IGPP could say, "Come and we'll give you an appointment as an assistant professor," which I did, without talking to anybody, and then later had it confirmed. You don't do that nowadays. And he came and he was here for years, three or four years, and did some wonderful work.

DOEL: This was back in the 1960s?

MUNK: This is in the 1960s. His wife didn't think that this was the best climate for her children to grow up in. She wanted better schools and less freedom for well, that's not the right word, but more disciplined upbringing, and so they went back to Germany.

Yes. We have to look at the time line for when Klaus [Hasselmann] was here. But I think he and Hank [Henry M.] Stommel are the two outstanding figures in oceanography in my lifetime, and Klaus is a brilliant man, and at the same time a very nice person, a great deal of fun, quite informal. He has made that major contribution to climate work that we are speaking about.

DOEL: How much of that work began here?

MUNK: Oh, that happened after he was here. That happened after he went back to Germany.

DOEL: That was done in the Max Planck-Institut?

MUNK: Yes. He did some work here, and we worked on a few papers together, which was very much fun. We were I think the first ones to use bispectral analysis, the analysis of non-linear processes by that method, and he was a co-author of "Waves Across the Pacific," which we discussed yesterday. He volunteered for the Hawaii assignment, and ever since he has had a warm spot in his heart for Hawaii. They went back last year to work there. for three years.

DOEL: That's interesting.

MUNK: And Klaus has a mad scheme of answering all questions of modern physics in a very innovative and totally untraditional way. He thinks that quantum mechanics may be based on a false premise. He has held that view now for 25 years, and in his free time he is working on his method of revolutionizing physics. I have heard him talk about it; he has written and published on it now. I tried to get some interest going in our physics department here, and they paid absolutely no attention to it. They said it was totally irregular and didn't make any sense. I wonder whether he will win. If he wins on that one, he will be one of the celebrated personalities of our century. And he is still working on it, and he thinks he has something very, very important.

DOEL: What particularly does he argue?

MUNK: He thinks that instead of strange particles you should deal with soliton light wave solutions. Otherwise I am not the person to explain what he does.

DOEL: But it's more than a formalism?

MUNK: I don't know. But he's been at it for many years.

DOEL: That's very interesting. I'm also curious about the lectures that you delivered in 1967 as part of the Mobil Corporation Colloquia.

MUNK: Oh, that was a minor thing. I don't think we should spend time on that. I think Cecil [H.] Green had something with it. They wanted to have some contact with oceanography, the kind of thing we just spoke about.

DOEL: That's what I was curious about.

MUNK: I never thought of it that way. I went there and tried to have the fun of speaking to some young people about what was going on in oceanography.

DOEL: And you had given a series of lectures beginning with history and then moving into ways of treating problems.

MUNK: Yes. I've forgotten all about it.

DOEL: I was just wondering what insight that gave you into the kind of knowledge that Mobil scientists had of these issues.

MUNK: Gosh. I think I was trying to please Cecil, who asked me to do something. And they also probably paid me quite well, and we were building a house so that was useful. [laughs]

DOEL: Do you recall how well prepared the audience was for what you had to tell them?

MUNK: I don't remember.

DOEL: Okay.

MUNK: Or the lecture, for that matter. [laughs]

DOEL: It was published, of course, as part of the Mobil series.

MUNK: Yes. And they did a nice thing. In some ways when we spoke about the relation between the oil industry and the ocean community, you reminded me just now that there was an attempt to do something maybe we ought to try to revitalize this kind of interaction. I don't think

convincing the President of an oil company is very helpful, although it may be necessary. But the really important thing is to work with people at the appropriate level who might be able to use it, and that was really an attempt to do that.

[end of Tape 1, Side A]

[beginning of Tape 1, Side B]

DOEL: You had also returned to the Soviet Union in 1968. We have of course spoken at length about your 1962 visit that coincided with the Cuban missile crisis -- when you had crossed the border coming in from Finland. What do you recall about the '68 trip and how that came about?

MUNK: Is that when we went down to Georgia and Armenia by plane?

DOEL: I believe that's the case.

MUNK: Yes. I remember chiefly as the adventure of going to Georgia and Armenia and having a great battle letting me take the train instead of the plane. We had a real battle on that.

DOEL: Reminiscent of what you had faced in getting across the border in 1962?

MUNK: Yes. I don't think there was any, I don't have much of a memory. I would have to look back at my own notes to see what happened.

DOEL: Did you notice any remarkable changes in the Soviet scientific community by that time?

MUNK: You need to tell me who was director then. I think it was [Andrei S.] Monin at the time.

DOEL: I believe that's right.

MUNK: Previously it was [V.G.] Kort. And we need to know, because those were the personalities that sort of set the visit. Monin of course is a very sophisticated oceanographer/theoretician/mathematician, and I think it must have been built around him. He is a great lover of modern art, which at the time in Russia was almost unheard of. And if I remember correctly, the really most notable thing that happened during that time was that he took me to an illegal exhibit of modern art in Moscow in somebody's attic by an artist who was doing that. I need to really look at notes before I can answer your questions. But it didn't seem to be a very important part of my life, and I have been there four times, and I have a little problem remembering.

DOEL: I understand. Given that there were significant changes in the international climate between '62 and the end of the 1960s, did you notice particular differences in their work?

MUNK: No. And unlike my visits to China, where the differences from one time to the other were unbelievable, I didn't have that feeling in my Russian visits. I've always felt that they were worrisome and you were glad to get out eventually. At one time I went there and Roger [Revelle] got sick. Maybe that was '68. It's possible. Roger had a lifelong fight with blood circulation in his legs. He became infected and had to be in bed, and the last thing he wanted was to be treated in a Moscow hospital. I mean nothing could be more dreary. I thought I ought to help get him out. He couldn't walk well. I took him to some friends in Denmark, where he then went to the hospital for treatment. I think they were called the Wolfs, the oceanographers in Denmark.

I flew out with him, and my most memorable memory was that he wanted to go on SAS [Scandinavian Airlines] to Copenhagen. I had a ticket on Scandinavian Airlines to Stockholm, and I appeared after an hour and a half's trip to the airport in Moscow with my ticket. They said they couldn't change it, I would have to go back into the city to a travel bureau and get a permit to do that, even though the flights were not full. And that was impossible, because the flight to Copenhagen was leaving soon. And I did an incredibly stupid thing. I went into the men's room, crossed out Stockholm, putting in Copenhagen, with my own pencil, and then appeared at another window and gave them the ticket and they accepted it. It's a wonder I didn't go into a Soviet jail at the time. But we did get to Copenhagen, and turned Roger over to his friends and I went home. [laughs]

DOEL: It's clear you did understand how the system worked.

MUNK: It was impossible! You know, an hour and a half back, and then probably standing in 2-hour line, meantime all the flights were gone, and I didn't know what else to do. [laughs]

DOEL: It was a good solution. When you look back on the 1960s, were there major events either in the IGPP, or Scripps broadly, or in oceanography that we haven't touched on that come to your mind?

MUNK: No, I don't think so. I am looking at your time line, and I don't think so. In a way I wish I had that time line dispersed with what I published. I could get my papers out, my publications. That would make it a little easier for me.

DOEL: I have it right here.

MUNK: Oh, good. Let me look at that. [tape off, then back on...]

DOEL: You just mentioned your deep sea tide work, and Frank Snodgrass's interpretation of it.

MUNK: Yes. And Judy got a fellowship to go to MIT, to Harvard at the time. The Radcliffe Institute work.

DOEL: And you went with her to MIT in the late 1960s?

MUNK: I worked at MIT.

DOEL: You mentioned that your contact with Roger Revelle was limited when you were back in Massachusetts?

MUNK: That is correct. He had adjusted I think to the happenings here in La Jolla by really creating a new set of friends at Harvard. I do remember being a bit disappointed that when we spent our year there we didn't quite pick up from what it was before, and we didn't see him very often. But I can certainly understand that.

DOEL: Good morning. I should note Judy Munk is joining us here at the table.

MUNK: At 10 o'clock. [laughs]

Judy Munk: Yes. For not too long.

DOEL: Who did you regard as the most interesting collaborators at MIT?

Judy Munk: He was slumming at MIT.

MUNK: I sat in Jule Charney's office. Jule is one of the interesting and fine people, but he was gone, and I worked with Norman [A.] Phillips and with [Dennis] Moore and wrote two papers on ocean dynamics that I thought were interesting. Then, at the end of that decade I really started working hard on tides. And in some sense one of the papers from that era was being used when we met in London last year on a symposium in honor of Dave Cartwright. The literature was reviewed by various people. We developed a new method of prediction which we called the "response method". It was one of the few contributions that I have made in my career. It was a different way of looking at it. It was a very traditional field, because people had worked on it since the days of Lord Kelvin without much change, not taking into account what had been learned about stochastic processes and things of this sort. I think we injected a new note into this field.

DOEL: What was at the heart of the response method that you developed?

MUNK: That you make better use of the astronomic ephemerides. Instead of analyzing the records in parts just by themselves, you use the full information about the ephemerides of the earth, moon and sun, and you predict as if they are playing through a black box and creating the local tides, instead of just making a kind of blind Fourier analysis of tide records.

DOEL: That's very interesting.

MUNK: And that was what we did. The method works, but it's one of the cases of improving something that works very well already. It wasn't much of a markage for the 2 percent improvement we got, but it was an intellectually interesting exercise.

I also started doing a little work then with a student, Mark Winbush, on thinking about currents in the benthic boundary layer, the boundary layer above the bottom, about which there had been very few measurements. It was a natural outgrowth of being able to drop instruments to the sea floor, that you should start asking what are the time series of currents and temperatures when you are half a meter above the sea floor in 4 kilometers of water -- what kind of regime is it? And it's very, very different from the boundary layer between air and water.

DOEL: Yes. As you say, this was largely a new field, apart from sea bottom photography and a few limited forms of measurements?

MUNK: That is correct. And when we go to the early '70s, that was overshadowed from my work by the work on internal waves with Chris [Christopher] Garrett. Chris was one of the many gifted Cambridge mathematicians and he came to work here on that sort of problem. I got him interested in working on internal waves. I would like to eventually come back to that, because it's the beginning of my interest in ocean acoustics.

DOEL: Yes. We very much need to cover that. But when you spent the year at MIT, did you find any techniques being used there that you wanted to bring back to Scripps or IGPP?

MUNK: No. No, you just sat next to other people. I sat next to Norman Phillips, a wonderful guy. Frank Press was chairman. I once remember saying, "Frank, how sad we didn't work together at that time." You know, he was an active researcher while he was chairman. Well, he said, you know it's all an accident. If you had sat on another floor probably we would have spent the year doing something together. But I was on the floor where Norman Phillips was, -- we ran into each other going to the head.

DOEL: The mixing ratio was not terribly good in the Green Building.

MUNK: No. The Green Building was very poor. And if it weren't for the fact that men's and women's room were on alternate floors there would have been almost no mixing at all. But this imposed a certain degree of circulation on the building.

DOEL: Yes. What were your impressions of the department at the time?

MUNK: Oh, we had a good time there. It's one of the good departments. I had been on the visiting committee for MIT with Cecil Green for many years, so it wasn't going into a strange world. Judy's activities at Harvard were the key reason for going there, and then Judy broke her leg. I was in Washington when it happened and you know that meant that that made it sort of difficult. But we managed.

DOEL: Did you get over to Harvard during that time?

MUNK: Yes. I didn't work there, but I went to Harvard every day to pick up Judy after the day's work. Judy worked on a very interesting project having to do with building one of the UCSD colleges. She felt they ought to be built into a canyon. We have these wonderfully steep canyons, and she made a detailed plan which I think was extremely good but has never been materialized.

DOEL: Why did that not come about? Had it come close at any point?

MUNK: No, no, it was a radically different way of doing things. [laughs] Like all of Judy's ideas. They don't always work. I'm amazed how many of them have worked.

DOEL: This is interesting. But she was able to spend the year developing a comprehensive plan?

MUNK: Yes. She had architectural drawings. You know the Radcliffe Institute Fellowship actually has requirements of reporting on the work. It's a structured fellowship, and it was wonderful. I think that is a superb idea. You know, the idea behind it is that women who were creative are given another chance after their children get a little older and their husband becomes more liberal or something. This was her chance. I don't know whether we spoke about it, but it was a wonderful story because she went back to the same building that she had been admitted to. Did we speak about that?

DOEL: I believe we spoke about it off tape.

MUNK: I really think I'd like to record that.

DOEL: Please.

MUNK: Judy had been admitted to the School of Design at Harvard, and she came down with polio actually when driving up from New York to Boston to be admitted, when she was 21 years old. She reached the apartment she had rented in Cambridge and didn't wake up for 40 hours. She was very lucky they found her. Then she was in an iron lung and really out of commission for quite some time. Later we married and years passed, and then she applied for this fellowship and was accepted. We went there to find out what they had in mind, and they said oh yes, we have a place for you in Robinson Hall. Judy said, "Oh, that's great. That's the building I was going to work in." Then they said, "Well, let's take you down. You're in the basement floor. It's not quite so good." She said, "Oh, that's fun! That's where I was before." And then we rolled in and they said, "And here's your desk," and Judy said, "My heavens, that was the desk I had been assigned to before I came down with polio." And as far as Judy could see the place hadn't changed, hadn't been swept for 20 years. It was 20 years. She was 40. And I think there is some kind of a lesson that I find very moving, that if you really want to do something you might have

an interruption but you get another chance.

DOEL: That's a very good story, and a very optimistic one. I'm glad we do have that on the record. Did you get up to Woods Hole during that time too?

MUNK: Yes, oh yes. We have always over my entire career -- less so now really -- gotten to Woods Hole two or three times a year. We love going there. There were two occasions when I was asked to come to Woods Hole. We spoke about that. And we always felt a very romantic attachment. We go to Woods Hole and we look at the house that we almost bought, and say, "What would life have been if we had gone there?" It probably would have been just as good as here, but you can't do everything. Unfortunately.

DOEL: Indeed. I'm just curious how much time you had during that sabbatical year that you recall out at Woods Hole? You were primarily at MIT.

MUNK: I primarily was at MIT, yes. And the children were with us, and we weren't quite free to go. And we had a rather dreary apartment. We are so spoiled in our La Jolla life. But it was a very nice year.

DOEL: You had another major trip around that same time, when you went to Venice in 1972.

MUNK: Yes. In 1972. That was a romantic year. Our life in Venice, after the tourists went home in September was really wonderful. We had an apartment in Trieste where my aunt, Hilda Brunner, lived. She's from a prominent Triestini family, and they have been there for generations, our cousins. My mother's maiden name was Brunner, and they made it very nice to be there. My Aunt Hilda had a house in downtown Trieste of five acres, very much hidden away and unknown with some hothouses for roses. The house is still in the family. She maintained three gardeners at the time.

DOEL: Is that right?

MUNK: I worked at the Institute of Theoretical Physics that is in the suburbs of Trieste which was under [Abdus] Salam, the director, who is a Pakistani and a Nobel Prize-winner. We lived out in that part of town. And then Judy and I would take off every Tuesday morning and spend Tuesday, Wednesday, Thursday, Friday in Venice, come back Friday evening and spend the weekend in Trieste. We stayed at a little pension in Venice, very, very fourth rate, one you don't even find it in the tourist guides. And had a superb time. We had been asked, or at least I was invited, by the Venetians, to work on the so-called aqua-alta problem, involving the storm tides on the Adriatic. As we got there and we found that there were, I don't know how many, a dozen organizations in Italy that were making their living working on predicting the storm tides. All of them work pretty well; it's an easy prediction. The natural period of the Adriatic is 20 hours, and no matter how bad your physics is you get the right prediction. [laughs] I felt that that wasn't the thing to do, and then what happened is Judy had become interested in cleaning statues with

lasers. We ended up really working on that problem, not on oceanography. I think we talked about this before too.

DOEL: Yes, we did.

MUNK: It was a very, very different climate. I learned something about restoration. Not much, but enough to work with Judy. It was her sabbatical really. As far as the storm tides are concerned, we ran one meeting for the Accademia dei Lincei where we invited people from each of the institutes to come, and they all said they couldn't speak because their professors didn't want them to.

DOEL: You had mentioned that a few sessions ago, and that you were also very troubled by the kind of peer review that you saw was going on.

MUNK: That's correct. And that was one of the reasons that took away the fun of working on that. But there has been an after-story, and again you will have to help me. I got the idea then, which I think had not come up before, that if gates to the lagoon were built these are the gates at the three openings into Venice Lagoon: Porto de Lido to the north, Malamako [?] in the middle, and Chioggia in the south. The plan was to have movable gates to close those openings at times of storm tides. Once you had those, you could solve the second most important problem of Venice, namely the pollution of Venice Lagoon. Everybody knows that the lagoon is very badly polluted. And the way it is flushed now is what an oceanographer would call by diffusion. The water comes in and out each of the openings, and in the process of going in and out it removes some of the polluted water, but not very effectively. I claim to have seen a particularly ugly piece of refuse on three successive tide cycles coming in and out.

I suggested that once you had the gates, you could program them so that you would close the two southern gates during rising tide so the water would have to come in on the northern side through Lido, Porto de Lido. Then you would close Lido and open Chioggio on the falling tides, and so you would force a circulation. Water would be coming in to the north and going out in the south. In mathematical language, you would rectify the tidal flow to produce a steady flow. That would be much more effective in flushing out the water than the diffusive cleaning that we now have. Years later, I persuaded the authorities of Venice that they should investigate this. And they did. They put that idea on to the computer model that has been built to study Venetian problems, and it was very effective. I remember in five tidal cycles, two days, you were down to 5 percent of the pollution. It seems to me it's a perfectly good idea. But since the gates have never been built, it has never been used. But it meant that we went back to Venice three times as guests of the City of Venice, and these were three noteworthy visits. I loved going there as a guest of the City of Venice, and we had a good time, and I think there was a considerable amount of work done on this. I am convinced that it would be a good thing to do if the gates were ever built, and that you could make a significant improvement in the climate of Venice pollution.

DOEL: Why haven't the gates been built thus far? Is it a political problem, or are there other

technical issues that are still unresolved?

MUNK: No, it's a political problem and not a technical issue. The technical design has been reviewed to death internationally, nationally. Very good companies have been asked to review it, including the Battelle group in America. They backed it. The cost, if I remember right, in the last few years had gone above a billion dollars. The economists found it cost effective to spend that money. There seems to have never been the will to really do it. During this same era the British decided that they had to do something about the potential flooding of London. London is a low lying area, and potentially subject to flooding from storm tides, just like Venice, storm tides in the North Sea. They built some gates at the Thames River [River Thames] near Greenwich. Those are a fantastic engineering solution. There is a big diameter cylindrical pipe that can be rotated to form a barrier. We were invited to go to the opening, so I have seen it operate. And they did all that while Venice is still talking. They started later and ended sooner, and there were times, I think it was three years ago, when there were three high storm tides, and they effectively stopped part of London from being flooded, the underground being filled with water.

DOEL: That's very interesting.

MUNK: So I think it's a matter of political will that seems to be lacking.

DOEL: Yes. Did you get a chance to travel much through Europe in '72 during the time that you were based in Venice?

MUNK: Yes. That was the time when we were elected to a organization called Leopoldina. Did we talk about that before?

DOEL: I don't believe so.

MUNK: Leopoldina is a natural science society that was based in East Germany in a place called Halle [former East Germany], and they claimed to be older than the Royal Society. In other words, very old. But the way they substantiated the claim is that two people had lunch and spoke about this some 300 years ago, and then nothing happened for 30 years, but by dating back the society to that luncheon they were able to beat the Royal Society. Anyway, they invited me to join. I never had heard of them. I asked my friend Gustaf Arrhenius who knows everything whether that was a reasonable thing to do. He said yes, by all means, they are a respectable society. We accepted.

DOEL: Was he a member as well?

MUNK: No, I don't think so. We were living in then Italy, and they invited us to come up and give a talk at their expense. So when we left Venice we sent the children skiing somewhere, and we drove up to Germany through Checkpoint Charlie into Halle. It was a politically fantastic

experience like what we spoke before, sort of like going into Russia, only I think worse. The Germans at the time combined their Communism with German efficiency, and it's a deadly combination, and the police there are impossible. But we drove there, spent three days in Halle, remember being really very taken with those people's attempts to keep up some kind of open-minded science going. Of course since then they have become incorporated into Western Germany.

DOEL: Right. But the DDR [Deutschen Demokratischen Republik, former East Germany] science groups were among the strongest that existed within the Communist Block.

MUNK: Yes, but still not comparable to the Max Planck-Institut.

DOEL: No, they weren't.

MUNK: You know about them then, a little bit, yes. They were very nice to us, but we were horrified at the political situation that existed. We may have told you: a small example that I remember vividly is that they weren't supposed to listen to Western television, only to East German television. And the East German television, when it went from one program to the other, showed one kind of clock ticking away, and the West German television had a different kind of clock. They asked the children in school to draw a picture of the clock between hours, and on that basis could decide whether their parents illegally were watching the wrong kind of television. Then they were punished for that. And I've always thought that that was about the worst possible political reaction that I could imagine for any country to take.

DOEL: That really is. That's a very powerful example.

[end of Tape 1, Side B]

[beginning of Tape 2, Side A]

DOEL: How long were you there?

MUNK: Oh, three days. And then we drove out and joined the children.

DOEL: Did you have a chance to see any laboratory facilities in East Germany?

MUNK: Yes, we visited Warnemünde, which has an East German oceanographic station. I had been somewhat interested because it was the oldest tide station in the world. They had taken records for well over a century. And it was huge, like all things in Eastern Germany or Russia, there are always lots more people than we have. So it ought to have been a leading institution, and yet it looked awfully unproductive, bureaucratic. I thought this idea of throwing lots of people into a field to become dominant, whether it was in East Germany or Russia or China for that matter, where they had more people working in oceanography than anyplace, never worked. It's the few people with freedom to go out and make up their mind that really seems to count.

DOEL: That's very interesting. What factors besides large numbers of people influenced the quality of the work?

MUNK: I think it's related to what we spoke about earlier today. It's about making proposals and supporting people who could predict what they would later learn. As you know, there was a story in Russia: the way you survived as a scientist is you reported last year's work so you could predict what you would find. You would always be a year behind. And this lack of ability to adjust to new ideas I think produces mediocre science. It produces surveys rather than new ideas. These big ships needed to have a program for each day months ahead of time because they had three cables over the side they wanted to be sure they didn't become entangled meant that if you learned something new you couldn't adjust and say, "Well, I want to change and do a different kind of set of measurements tomorrow." No, you went on for another six weeks, and then the chances were gone. And our idea of working from small ships, at great discomfort, but small ships where the skipper, the captain, would do what you asked, being able to change your mind at a moment's notice and not be punished by it I think is a key to successful new research. Surveying can be done very well, and the Russians did a good job, but not science. They knew that, and they were terribly jealous of our ability to try and do unexpected things and to live up to that. I think that's a key thing. It's a little bit like our proposal writing, and having to have broad peer review.

DOEL: That's very interesting. Did the East Germans have any ships of their own?

MUNK: Yes.

DOEL: Were those large as well?

MUNK: I think they worked with the same massive, large ships. I remember that one of the ships even had the printing press -- so they could publish their papers before they reached port.

DOEL: You mentioned that occurred on the Soviet ship, or perhaps that was East German?

MUNK: I don't remember.

DOEL: But it was certainly from the Communist Block.

MUNK: Yes.

DOEL: That's very interesting. Were the West Germans using smaller ships at the time?

MUNK: Yes. The Hasselmann group, the climate group, by the way, did not have ships. There is the institute in Bremerhaven [Germany] that's very good, which concentrates on Arctic, and they had the new Meteor, which is a very elegant ship. And it certainly was used in the sense that I'm speaking about, giving people a chance to try new things. There was a world of difference

between West and East Germany then.

DOEL: Did you get much into Germany or France during that time?

MUNK: I've never spent much time in France. We've been Anglophiles and Italiophiles. I'd gotten started that way, and repeated our visits to those countries where we had friends and felt very comfortable. I never really spent much time in Germany except with Klaus, and of course we visited Austria because my relatives are still there.

DOEL: Right. Were there other memorable developments that came from the Italian trip that come to mind?

MUNK: Well, I think it's the only administrative success in my life. I talked the Italian government out of I think \$30,000 to do the experiments with restoration, and that was a matter of pride. I didn't think we should ask Americans to pay for fixing up Venetian statues, and to my amazement we got the money from the Italian government. That was a very successful and kind of romantic life. You know, we set up a man named John Asmus, a great laser expert. I think we may have talked about that. It happened in an old church in Venice, a 12th century church that had been deconsecrated. Did I tell you that story?

DOEL: If you did, I believe it was off tape.

MUNK: That's a wonderful story really and it ought to be on tape. There is a 12th century church called San Gregorio, which had been deconsecrated and became the restoration laboratory. As you can imagine, it's not exactly the latest modern industrial background for doing a laser experiment wires were hanging down the walls and so on. But it was a beautiful, wonderful place to work. Then John Asmus came with his lasers. The idea was to apply laser pulses to the statues and burn away and evaporate the black encrustation which are formed by the sulfides and chlorides in the bad environment. The classical method is sandblasting. No matter what you call it, it's abrasive; you have particles impact and cause damage to the surface. The laser method would evaporate the black encrustation, and it had the nice property that when the black encrustation was removed, and the white marble appeared, it has a much stronger reflectivity, and therefore it's a self-limiting process. So once you had the black layer removed you didn't damage the surface. The idea has prevailed since that. It was Judy's idea. Of course it became the basic idea for the national defense of the United States against missile attack -- you send laser pulses out and you damage the incoming missiles. The same method, but somewhat larger intensities.

DOEL: But you don't think Edward Teller got the idea from Judy.

MUNK: No, I don't think so. Anyhow, we set up, and John Asmus made the wire connections. We needed to cool the lasers, and in the daytime we would get the ice from a neighboring nunnery, who had an icebox machine. At night we would get it from a next-door pub that had an

ice machine. And Asmus set that up in a week, with wires hanging all over the place. Then he turned it on and it worked. He stated that the only conclusion he could make was that the church had never been properly deconsecrated. [laughs]

DOEL: [laughs] That's interesting.

MUNK: And it worked.

DOEL: Did you find any other applications for that technique once you came back to Scripps?

MUNK: No. We did continue some work in our basement at IGPP. I'm almost sure I told you the story about Nierenberg -- he was attempting to find more space because everybody was complaining about space, so he personally went around and looked at each laboratory and saw where he could find some space. He saw us applying laser pulses go on a statue somebody had loaned us. You know, Revelle had the idea, when he was asked what is oceanography, that "Anything that anyone at Scripps does is by definition oceanography." Bill said, "What are you doing here?" We said, "We're doing oceanography." He didn't want to be more narrow-minded than Roger, so he didn't tell us to stop. [laughs]

DOEL: That's really interesting. Was space a genuinely major concern at SIO during this time?

MUNK: Always. No matter what you build, it always is. It's an unsolvable problem.

DOEL: Work usually expands quickly to fill available space.

MUNK: All space. When we built our new IGPP laboratory a couple to three years ago, and we said well now we'll have no problem for a few years. It was amazing how quickly it was filled.

DOEL: Yes. You are speaking now of the new Revelle

MUNK: The new Revelle Laboratory.

DOEL: I want to get to that. You mentioned that there are certain architectural issues you particularly wanted to treat at that period of time. Speaking of Roger Revelle, he came back to Scripps...

MUNK: Oh, but that's much later.

DOEL: I believe that was in 1975.

MUNK: Oh, was that when he came back?

DOEL: Just a few years after your return from Venice.

MUNK: I see. Yes.

DOEL: Did you play a role in creating the opportunity?

MUNK: Yes. Roger is a proud man, and he didn't want to come back unless he was asked. Actually, the connection was more with the upper campus than with Scripps initially. He gave a course to undergraduates on population and food. And although he was a wealthy man, he wanted to be invited and paid. I went to Paul Salzman who was then vice chancellor, and said, "Please, you've got to arrange for Roger to be appointed and paid or he won't come." There was some objection, and I think I fought that through.

DOEL: Was it old personal animosities, or the feeling that funds weren't available?

MUNK: I think the latter. Just, "Why should we? We're always short of money." But it was a definite issue once. I can say I played a major role in his coming back. There could have been a bottleneck, and I became aware of it. And then of course -- we'll get to that at the very end -- when he got back to Scripps. Ed [Edward A.] Frieman gave him an office down near the director's office, and the round trip completed.

DOEL: Yes. I remember within a year of his return to La Jolla, you and he were publishing together again: the energy and climate study, which addressed global warming. It was already in print by 1977.

MUNK: Uh-huh [affirmative].

DOEL: I have a copy of that. That was a National Academy of Sciences publication.

MUNK: Yes. That was fun. I happened to fly back with Roger. You have to appreciate how much these things are matters of accident. I sat next to him in an airplane, and he was trying to write the basic budget consideration of how much CO₂ goes out, how much goes back into plants and so on, the sort of dynamics where you could predict under certain scenarios how CO₂ would fare. You had to put in diffusion in the ocean and of course Roger's mathematics was largely nonexistent. Mine isn't very good, but it's better. And so I said, "Roger, can I try to set that down as a series of coupled equations? It really is sort of rate equations." He said that was a good idea, and I wrote those out. I think in some sense that was probably helpful. And then I set this up on a computer and we published this paper.

DOEL: This is "Carbon Dioxide Cycle in the Biosphere," yes?

MUNK: Right, right. And that was nice for me, because it got us back together and doing things together.

DOEL: Yes. How well received was that paper?

MUNK: I don't know. I thought that people paid a little attention to it, because it hadn't been done quite that way before. It predicted how things would level off by 2050. I think in some sense it could be used but probably someone else has done it much better. I don't know the literature.

DOEL: But it was an early attempt to quantify these kinds of changes?

MUNK: To quantify. Yes. That was very interesting, I thought: the equations aren't important, but they forced you to ask what do you need to specify in order to predict at all, what are the important factors. So you can close the loop and have a deductive system.

DOEL: Yes. Was that something you worked on almost entirely with Roger or, to develop the equations, did you find that you needed to speak with other people?

MUNK: It wasn't that much of a problem really. But if you talk of that period from my point of view, we should talk a little about the internal wave work.

DOEL: This is probably a good time to talk about that.

MUNK: Because it really was a key point. And that goes back to my Oslo visit. I've been amazed how many papers there were on internal waves going right back to the Norwegians, Otto Pettersson, [Alfred] Defant, each reporting some experiment, but making no reference to each other. For example, the most common one is that you measure temperature against time at a given depth and you get internal wave signatures. Others have towed temperature senses through the water and got variation in space. Other people had dropped instruments and found that there were changes. There were these different views, but no connection was made. Chris Garrett and I sat down and said, "We ought to try and see whether we can write a proposed spectrum in space and time" -- for example vertical space and time -- "and see whether different cuts along different axes could be consistent with the observations that had already been taken." In order to make a comparison of an x-y-z-t phenomenon, you have to have a model and then ask yourself how would it look if you only varied time, using mixed x and y and z. So we played around, sitting at the table where you and I are sitting now, every morning for a year. We tried different realizations of such a spectrum and asked whether it would in fact obey the data. I think there is something really kind of important in that. [W.F.] Ekman, who had in fact thought about this problem, built some current meters known as Ekman Current Meters. They go way back.

DOEL: Yes. They became almost standard instruments, didn't they?

MUNK: They became almost standard. But when he took current meter readings on a vertical string only say 200 meters apart, he found that the measurements he made were sometimes almost entirely different. He said, "How could it be if you have something that has periods of hours, and you measure them at two points 200 meters apart, how can they be so independent?" He doubted his instruments. When I met him as an old man, he told me that he had never

published for 20 years because he couldn't understand how readings separated by such small vertical distances could be so different. And I kind of convinced him, just before his death, that he should publish, and he did. But it was published 20 years after he'd taken the readings. Now, there is a simple concept to anyone who studied stochastic noisy processes that if you have a band width df you are incoherent at times $1 / df$ apart. You see, they were not accustomed to broad band processes; they were accustomed to discrete spectra with spectral lines.

It's part of the same process. In working with Norman Phillips at MIT, we tried to really specify that in space, when you are more than $1 / dk$ apart, you should have incoherent measurements. And all that Ekman had found was that the internal waves are broad wave number band processes, and therefore could be expected to be incoherent at a few hundred meters separation. And that was the key. But Chris Garrett -- who understands that very well -- and I found that we could propose a broad band spectrum that became properly incoherent at 200 meters vertical separation. That was our key contribution, but it I think was an important one.

DOEL: It was an important one. That work wasn't being done at other centers at that time.

MUNK: No, it wasn't done. Yes. So we set up a spectrum which became known as the G-M spectrum that has survived 'til today which it shouldn't have.

DOEL: Why is that?

MUNK: Oh, because what we wrote in our first paper is that that was a what do you call it, something to shoot down?

DOEL: A straw-man.

MUNK: It was a straw-man. We thought it would be better to present a straw-man so that when somebody organized an experiment he could say, "How far should I have my instruments apart? What should be my sampling strategy?" It's better to have a straw-man to shoot down than to have nothing.

We very explicitly said it was a straw-man. Well, it has survived until today. You will see people who say, "We made measurements and we get 3 G-M," or "we get $\frac{1}{2}$ G-M," then "isn't G-M awful." What we found is that the internal wave intensity is remarkably universal: under many different circumstances, different times, different places, you are within a factor of two of a proposed spectral density, which is remarkable. Nobody expected that. And that's become again an issue. The very stability of that spectrum is a key issue in certain problems. But our model became sort of a shoot-down model and never was really quite shot down, and it's what got me into the acoustic game. And we should draw some sort of a line, but that was definitely the way that it happened.

DOEL: I was very curious how much your work with JASON and in particular anti-submarine

warfare played into your interest in internal waves.

MUNK: Later Chris [Garrett] and published that paper, and then a few subsequent papers. It was a spectrum which was complete but maybe wrong. Yet it was $x y z t$, and you could ask yourself what would a given experiment do, you see?

I was then very much aware of the acoustic problem through JASON. Not through my Scripps work. You see, variability in transmission is really the key surprise about acoustic transmission in the ocean. Why is it so variable? Why do you have fade-outs when 100 was the intensity that you had gotten five minutes earlier. And we learned that we had all suffered from Ma Bell, the Bell Telephone Company people, who were actually having a monopoly in underwater acoustics: not only the cables they laid, but also the theory of interpretation. They had tried to interpret variability in terms of model that was mathematically manageable, a homogeneous isotropic turbulence, obeying a Kolmogoroff spectrum. There is everything wrong with it, the ocean variability is not homogeneous, it's not isotropic, and it doesn't obey Kolmogoroff. But you could integrate the equations. And it's incredible to me how long that point of view lasted.

So when we got the internal wave spectrum, we said we ought to have another go at the acoustic variability. Here's something which is at least based on some observations: can we compute how sound would travel through an ocean that is disturbed by a G-M internal wave spectrum? Then in JASON, Fred Zachariassen and I did the first calculation, and we came out well. We found that the variance, the simplest statistical measure, came within a factor of two of what was observed. And that was exciting, because the internal waves was based on one kind of measurements that oceanographers had done over the period of years, the acoustic variability on an entirely independent set of measurements. Combining the two had not been done. It turned out to be successful, and then became the essence of work that JASON did for many years resulting in a book called *Transmission of Sound through a Variable Ocean*, by [Stanley M.] Flatté and Roger [F.] Dashen -- who was the most brilliant person at JASON as far as I'm concerned -- and Ken Watson, [Fred] Zachariassen and myself. I think that did have a significant impact on how we viewed ocean acoustics. That's what got me into it.

DOEL: One point that you have emphasized in your writings is how unmixed those two communities were the acoustic community just did not have an understanding of ocean dynamics, which oceanographers knew.

MUNK: Yes. It's back to the same situation with the positional astronomers and the geophysicists not bothering to learn their language. The acoustic community had invented their own ocean on the basis of mathematical manageability instead of on the basis of observational evidence. They haven't bothered to really work together. Now, some people will challenge this and say but so-and-so did this and that, and it is true, but I am largely still correct. There was no reasonable, no significant contact.

I remember the joy that I have only had a few times in life, of combining two very different

things and finding that they were of the right order of magnitude -- hoping you didn't make a mistake by a factor of ten getting there. One such moment came when Fred and I got our first variance from the G-M spectrum that we could compare to the measured acoustic variability. The other was when I worked on the rotation of the earth and we asked how much change in the length of the day winds would produce. It was the same kind of thing. There were some numbers by different communities, and you went to the effort to learn the language and see what they would imply. The fact that they came out well is a tremendous satisfaction.

DOEL: Clearly you found satisfaction working at the boundaries of these different communities.

MUNK: Yes.

DOEL: When you addressed these problems, did JASON seem the kind of organization that allowed you to easily integrate results from different groups?

MUNK: Oh, of course. JASON is a group of people, and very free, and beautifully run, and it was also reasonable from the National Defense point of view. I mean, thinking back now about the acoustics -- in fact, internal waves are the essential limit to using ocean acoustics. Whether you are looking for submarines or looking for climate changes, it is not the only, but the essential limitation. And there has been a significant fallout on literally hundreds of applications.

DOEL: Yes.

MUNK: Much of JASON is very classified, but they permitted us to publish in the open literature. They permitted us to write the book. And although the book writing was done on our own time, the basic research had been done on JASON time. Then there were many classified results, and there still are some classified aspects to this kind of application.

DOEL: Yes. During these years, say particularly in the 1970s, how many of your publications remained classified, compared to those you can cite on your c.v.?

MUNK: About one third were in the classified column. But JASON was not my major pastime.

DOEL: Yes.

MUNK: It was a month and a half each summer, and it remained essentially that. But it was very much worthwhile. If I hadn't been at JASON I wonder whether I would have been so interested in this problem. The interplay of knowing something about the acoustic classified literature and that work is very stimulating.

DOEL: Yes.

[end of Tape 2, Side A]

[beginning of Tape 2, Side B]

DOEL: I was thinking about that too. Did your experience in JASON broaden your understanding of the techniques and the tools potentially available for this work?

MUNK: Yes. But there were also national needs that required attention. The one statement that I can make with a little bit of care is that there were some problems at the time I mentioned it to you before which really worried people all the way up to the Office of the President. It involved what the Russians were doing in the way of anti-submarine warfare. It turned out that the previous work that ONR had supported on internal waves became a key issue, a key resource. And intelligence problems: I've always felt that when ONR tries to justify its existence in Congress, say by talking about that 6-1-6-2-6-3 sequence going from fundamental research to more applied research to applied research to systems -- the way in which they justify is not really the way I saw it. The most important applications were not towards developing a weapon, but having an informed analysis of some potential Intelligence data. What was needed there was to do good work in a general field that was of some applicability. Much broader than trying to set a direction in which that research would lead to an application.

DOEL: Yes. Scientific intelligence was a fundamental issue during the Cold War years. John von Neumann for instance contributed a lot of time to it after the late 1940s.

MUNK: Yes. During the missile crisis, and the missile gap.

DOEL: And related issues. Did you play a role in thinking about intelligence-gathering as one component of scientific work?

MUNK: Well, JASON was involved in some intelligence issues, just as I discussed. The Soviets had certain things that they were doing that we didn't understand and that worried us very much. So I must have spent a total of a year of my time when I add up ten years, on a certain issue which is still classified, and which involved that kind of consideration.

DOEL: I imagine the Walker spy case was particularly disconcerting, knowing how much the Soviet military community then understood of what had been achieved here.

MUNK: So stupid, that they would sell that information for a few hundred thousand dollars is incredible to me, that somehow or another that came to pass. In the meantime of course the Russians were working on some aspects of this problem, and [Leonid Maksimovich] Brekhovskikh became their internal wave man. It would still be great fun to write a paper on what they knew and what we knew. Maybe it would be permissible today, if one had the time to sit down and do that, but it hasn't been done.

DOEL: Yes. And you've met him, as you mentioned, on a few occasions.

MUNK: Yes. He's been to our house, he had dinner with us where you had lunch with me.

DOEL: We're speaking of a room just across from us in this house.

MUNK: He appeared with another man. He appeared in Washington one day and telephoned us and said, "Can I come for dinner?"

Judy Munk: Tomorrow night.

MUNK: I said, "Do you know it's not next door, but we'd be delighted." Oh, he said, I have a plane ticket and I'm going to come with Mr. So-and-So who is head of a laboratory of chemistry of some sort. We said oh, we'd be delighted to have you. Then I called up the people in the Naval Intelligence with whom I had contact, said, "Should I do that?" They said, "yes, you must go ahead and do that, but we will advise you that you get someone else to sit in to protect yourself." We asked Ken Watson to come, and his wife.

Judy Munk: And Aunt Hilda was here.

MUNK: And my Aunt Hilda from Trieste was here. So they came, and we were told that the man who came with [Leonid Maksimovich] Brekhovskikh was a known KGB man.

Judy Munk: KGB. Charming. Oozed.

MUNK: Charming man who drooled over Judy's and Aunt Hilda's hands and kissed them and so on. Rather revolting character, and who did most of the talking. Then they left and said goodbye, and I had no idea why they came. We never discussed anything of any consequence whatsoever.

DOEL: Did Brekhovskikh seem guarded in what he said or could say to you?

MUNK: I think that somehow or another he must have asked me I don't know what. No, I present it as it was. I had no idea why he flew across the country and had dinner with us from anything that happened at that dinner party, but maybe he didn't expect to be as limited as he was by the presence of the other guy. Then of course when we come to the Greenland Sea. I think I may have told you that story.

DOEL: You simply mentioned that you wished to speak about the role that he came to play in it.

MUNK: Yes.

DOEL: When did this dinner take place, roughly?

Judy Munk: We never asked him why he came to see us. We wanted, Walter wanted him

MUNK: No, that was independent, but I do have to go on record with the Greenland Sea story.

DOEL: And I want to make sure we get to that. But are we talking about the 1970s here, the visit from Brekhovskikh?

MUNK: Oh gosh. I would think so, because I had already worked on I would think so, yes.

DOEL: What sort of a person is he?

MUNK: Oh, he has a sense of humor, and he's a superb scientist. When you talk about acoustics he is the best. Like Klaus is the best in climate. A superb scientist. And he has a very pleasurable taste of writing short books, and you always find what you want, unlike those German monographs where you have to go through 500 pages to find something, because everything has to be treated. He has learned the key of what not to talk about, which is so important, and so elegant.

DOEL: That's very interesting. You held in your hand a moment ago his book *Fundamentals of Ocean Acoustics*.

MUNK: He has written a number of classics. He is known for his books globally.

DOEL: This particular book appeared in 1982. Which are the other books that you are thinking of?

MUNK: Yes. Oh, one is *Propagation of Waves in Layered Media*, which is known by everybody. But then, as I mentioned to you yesterday, he started as an acoustician and later became an oceanographer.

DOEL: Yes.

MUNK: When we had our MODE expedition -- that was during the days of studying mesoscale resolution -- we learned that the ocean was extremely variable on the 100 kilometer, hundred day scale. He ran a set of experiments from Russia which he called... what were they called? Polygon, I think. And did the same basic thing. They hung a set of current meters they made current meter moorings, like we did, to try and learn about that variability. It was a real revolution. The trouble with the Russian current meters is they didn't work very well. Nor did ours. But ours were better. Their current meters had such a large failure rate that the Polygon experiment essentially failed, though it was ahead of the MODE experiments. We should be very clear that that important experiment was first done in Russia. Later on there was a combined effort which was suitably named POLYMODE, a combination of the two.

DOEL: When you mentioned instrument problems, was it the design of the instrument or was it their manufacture that caused the problem?

MUNK: Probably the manufacture. But you know we didn't do so well. I remember in the early MODE days that somebody by accident placed one mooring next to each other, where one mooring was moored from the bottom up and the next one, only a kilometer away, was hung from the top down. They current meters were at the same depth. But they found the one that was hung from the top down had seven times the spectral intensity than the one from the bottom up, yet they were close together.

DOEL: Yes.

MUNK: The result was that most of that variance came from the strumming of the mooring line came from the top down. And that's terrible! I mean, it showed that the whole experimentation was wrong and that we were completely overwhelmed by a noise that had not been properly taken into account. And I remember when that was discovered the first time, and they said, "My God, what is NSF going to do? Are they going to cancel us?" We decided wisely to immediately tell them that everybody goofed, and they have to learn how to deal with strumming. I am just trying to say that there were no white angels. We made goofs. Theirs had a terrible failure rate. But eventually we learned about mesoscale dynamics, which was one of the great developments of that era.

Judy Munk: Just shows you don't have to be very good, you just have to be the best.

MUNK: Right, right.

DOEL: [laughs] That's another way of defining priority.

MUNK: That was an important ocean issue.

DOEL: Yes. I just want to spend just a moment here, while Judy is in the room with us, to mention that we discussed both the Radcliffe Fellowship that you had when you were at Harvard in 1968 and your time in Venice in '72, when you did the restoration work. Walter has done a very good job in presenting the basic thrust of what you wanted to do there. As you think back to those two particular times, what kind of memories stand out?

Judy Munk: I guess I've always been disappointed at when I think I do a good job on trying to figure out a problem, I run into problems politically when I try and have them applied.

DOEL: Are you thinking particularly in this case of the campus design?

Judy Munk: The campus design and the design that I did for the square down in La Jolla and the design.

MUNK: But we mentioned Judy's design for a canyon college, which I still think would have been a real thing.

Judy Munk: I mean, it would have changed the face of how they're managing the northeast portion of the campus.

MUNK: To the better.

Judy Munk: I think would have been much better.

MUNK: And even cheaper.

Judy Munk: [in unison] And even less expensive.

MUNK: But, and even though we knew all the chancellors that was too radical.

Judy Munk: Right. Nobody would

MUNK: We should try again.

Judy Munk: Too late.

MUNK: It's too late now. The canyons are all filled. But on the other hand, your work in Venice did have an effect on people. That technology has been used. We're not up date what's being done now, but it had an effect. That was not a total failure, the laser method of cleaning statues.

Judy Munk: Well, the plaza downtown that I tried to do was a complete disaster.

MUNK: Oh, you are talking about another problem.

Judy Munk: Right.

MUNK: I think you should stick to those two.

Judy Munk: My little theater solution to those concerts was a simple idea. My interference in this stupid convention center solution that we've got downtown.

MUNK: Judy is talking about other than those two events, where she had what she thought were worthwhile ideas. I think they certainly are, and didn't manage to get people to support them.

But I think your success rate is surprisingly high instead of surprisingly low. I mean, you have gotten things done. We do have a fantastic laboratory at IGPP, you have changed the way Scripps is building, you had an effect on the cleaning of statues. You must... it's a question of whether the glass is half full. In your case it's a quarter-full or three-quarters empty. [laughs]

Judy Munk: Well, it's very obvious that the things that have worked, or at least been shifted in a direction that was better has been when I work on something that touches you enough so that you put your oar in. And you and I work... You work on my problems more successfully. No, that's not quite true. When you work on my problems, they turn out better, and vice versa. When I work on your problems, in some ways they turn out better.

MUNK: I need to add for our discussion that Judy and I wrote a paper together in Venice. It was called "Venice Holographs."

Judy Munk: Terrible paper.

MUNK: We presented it. I was elected to the American Philosophical Society and you are supposed to give an inauguration talk and we used it. It almost ruined our otherwise very good marriage. You know, scientists are accustomed to try to say things in as few words as possible. Judy would write a long paragraph about something and I would go through and cross out 80 percent of it. Judy would say, "You are ruining all the romance. You are taking out all the important things." It was a great challenge to our marriage.

DOEL: As I recall, that was still a fairly comprehensive publication that appeared.

MUNK: Well.

Judy Munk: Well, not very good.

MUNK: It had everything and the kitchen sink.

Judy Munk: Walter and I are really so undereducated when it comes to such things.

MUNK: You see we tried to bring in some Venetian history, and we are not historians.

Judy Munk: People who were really historians just laughed at us, you know.

MUNK: There were some people at the audience who said have you read this and that. We did not really study the history of Venice well enough.

Judy Munk: We knew a couple of things that nobody knew like building the Suez

MUNK: The Suez Canal.

Judy Munk: The Suez Canal. Which was an interesting lesson.

MUNK: Did you know that the Venetians once thought of building the equivalent of the Suez Canal right after Vasco da Gama discovered the way to India?

Judy Munk: Because it was going to ruin Venice?

MUNK: They knew it would ruin Venice trade, and they thought about it, and they sent a survey team out. The survey team made a mistake. The numbers were wrong -- on the basis of which, it was decided that it would be impossible to dig the Suez Canal.

Judy Munk: Then they sent a second team, but they never got a chance to correct those numbers, because they got mixed up with the Arabs or somebody in a battle.

MUNK: It was some other thing. But it would have been possible that the Venetians, who were very good hydraulics people, could have built one. Judy got interested in that, and we had some friends in Venice who went to the library with us, the wonderful old library near San Marco, where you could look at the old Venetian manuscripts.

Judy Munk: In old red boxes.

DOEL: Is that right?

MUNK: Manuscripts that were, you know, in red tapes that were sent back to the Serenissima. They talked about this era.

Judy Munk: In boxes, not tapes. The English did the red tapes.

MUNK: Oh.

Judy Munk: The red boxes were the [? unclear].

MUNK: Red boxes. And there is a wonderful story there that is not generally known, that might have kept Venice a powerful nation for another two hundred years. You know, they stopped being really a leader in 1500, partly because of that.

DOEL: That's very interesting.

MUNK: So, that's kind of a fun story.

DOEL: Yes.

MUNK: But we are not historians, and we can't do everything. We were interested in the historical background.

DOEL: You had a particular issue that touched on history that you brought in, as I recall, from that time.

Judy Munk: Yes. But if you read this little paper

MUNK: Don't read it. I'm not going to give it to you.

Judy Munk: I mean it's silly.

MUNK: But the Magistrata de Aqua [?], which was the second most powerful man in Venice after the Doge really did a good job of managing the water problems of the city at a very early time. It's really astounding how well they did then, unlike the recent history of building those gates.

DOEL: The gates, yes.

MUNK: Yes. Unlike. Totally unlike.

Judy Munk: Of not-building the gates.

DOEL: This was something that Walter and I discussed earlier today. Speaking of trips, it's around this period of time that Walter made a trip to China in 1977. And I wanted to make sure that we had a chance to talk about your experiences in China while Judy is here with us.

MUNK: She did not go with us until a much later trip to China.

DOEL: When was that that Judy went?

MUNK: I did four trips to China, and Judy went on the last one. And I need to get those dates straight. But my first trip to China was part of an Academy committee with Phil [Philip] Handler as chairman, then president. It was a group that had been started to try and reestablish contacts with the People's Republic of China. I went not as an oceanographer, but as a member of the Academy. But I did manage to get a trip down to Qingdao, which was the oceanographic capitol of China at the time.

Judy Munk: Everybody goes now, but at the time

MUNK: Everybody goes now.

DOEL: But at the time you were one of the first Westerners to go?

MUNK: I was I think probably the first who went to Qingdao. Then I was asked to chair a committee that went back to China. That was an oceanographic committee, and we have written a report on that. Then I went a third time. And then Judy and I went on one of these more personal visits where you can then travel around China with one person to help you, just the three of us, which was by far the most interesting one.

Judy Munk: That was sort of the same format we used when we first went to the USSR.

MUNK: When we went to Russia we managed to get somebody, a young person

Judy Munk: A young postgrad who went with us. And they have since become very well known. So it wasn't.

MUNK: Yes. In both cases.

Judy Munk: So it wasn't a party member.

DOEL: I wonder what that was like to be the first Westerner to see Qingdao?

MUNK: I enjoyed being at Qindao. We knew the man who was the head of the laboratory they had four laboratories of oceanography in Qindao. The most prestigious is the Academy one what's the Chinese academy?

DOEL: Academica Sinica [?].

MUNK: Yes. The man who was director was a man named Sheng Kui Tseng, and he had been a student in America. He studied in Michigan, and then he came to Scripps, and I vaguely knew him from his visit at Scripps. He was our host. He went through a very difficult time during the Chinese Cultural Revolution. They made him a janitor. From director to janitor. He had to clean toilets.

Judy Munk: They shipped his family far away, so he was alone. He was a janitor in isolation.

MUNK: Yes. Terrible. He spoke about it reluctantly. We kind of pushed him for that. Judy sort of pushed him to say well who became director after they threw you out, and he said, "Do you really want to know?" Judy said yes. He said, "Do you see that bus over there, the man who is driving it? He was the one who was director for three years and who gave me the janitorial job." We said, "Now what have you done?" And he says, "I ignore it. I've never talked to him since."

DOEL: That's an extraordinary description. In many respects we know far less about those developments than we do say of what had happened to Soviet scientists under Stalin.

Judy Munk: It was interesting, because when we went to these places I always tried to figure how we could have luncheon with the staff not with the staff, but with the members of the

MUNK: The junior workers, the technicians. And we did. It was a horrible lunch, wasn't it, at the same time?

Judy Munk: Well, it was difficult to manage, because nobody wanted to let us do that. They said oh no, we're going to be served lunch in the office or whatever.

MUNK: Some kind of dining room.

Judy Munk: And finally you and I just got up and walked where we saw everybody walking to, which was around back.

MUNK: Yes, with a tray.

Judy Munk: With a tray, and we followed them.

MUNK: The cost of lunch was 4¢.

Judy Munk: Yes. There were two big windows in a wooden shack. We looked in, and there were about ten people in there with great big soup bowls -- you know, like they had in the Army.

DOEL: And they ladled out the soup?

MUNK: And they would ladle it out into a pot, in a tin pot that you'd carry, like the old railroad workers used to carry. And it was 4¢, but it was some kind of smooch. It wasn't bad.

Judy Munk: We wanted to see that.

Judy MUNK: Yes.

DOEL: Was this in the 1980s or '90s that you made that trip?

Judy MUNK: Oh, the '80s.

MUNK: Late '80s.

Judy MUNK: Five years ago. [laughs]

MUNK: We can perhaps straighten this out when we get a more complete time line.

DOEL: Yes. We'll make sure of that in the transcript.

MUNK: Okay.

DOEL: What impressions did you have of the Chinese Academy facilities during that first trip that you made in '77?

MUNK: A bit like the Russian. A huge number of people, and-- I think I told you this story -- very, very specialized. A man who worked as a student on storm tides was working on storm tides when he retired 40 years later. And I think I told you the story that when I went there and somebody said, "How interesting that your father was an oceanographer." Did I tell you this story? He said, "Well, you're working on acoustics. Your father worked on tides?"

DOEL: That's very interesting. It startled them to imagine someone being that broad and eclectic?

MUNK: A jack of all trades. Yes. They wouldn't. It's not the Chinese fashion. Not the Russian fashion.

DOEL: And they were clearly aware of your work. How much did you see of Western literature there? How much seemed to be getting to the libraries?

MUNK: Oh, always. You should ask that first in the Russian context. They were so much better at knowing our literature than we were in knowing theirs. They were better at knowing our literature than I am in knowing our literature. They said, "Have you seen that paper by your colleagues at Scripps?" and I said, "My gosh, no, I'm not aware that he did this." And "I have to come home and look at that." And the same is true of the Chinese. Very hard working in trying to understand the other literature.

DOEL: I recall hearing some of the Russians complain about citation practices, that their work had not been, in their view, properly cited.

MUNK: Very bitter. I remember once when I first went to Russia, the first trip, after I had worked on the wind-driven ocean circulation. There was a man there whose name is certainly known, who had done some similar work, and he said, "You never refer to my work." He was absolutely right. All I could say is I'm guilty. I haven't studied the Russian literature.

DOEL: Why do you suppose generally that was so?

MUNK: It's my limitation.

DOEL: But you weren't alone. There were a great many Americans who didn't.

Judy MUNK: Oh, because the Americans were all talking to themselves. Walter, I am going to have to go. Emmie and I are meeting. Your lunch is all ready for you.

MUNK: Oh, good. Okay.

DOEL: Thank you so much.

MUNK: Okay.

Judy MUNK: And we'll be back.

MUNK: All right.

Judy MUNK: I took some money, so my checks don't bounce.

MUNK: Okay. You have some money for tomorrow?

Judy MUNK: Yes.

MUNK: Okay, fine. Okay, bye-bye.

Judy MUNK: Bye-bye. Your lunch is on the table for you.

MUNK: Well you know, I've always felt this idea, that you have to decide what life is for. People say we need better ways of communicating, better, more efficient ways of passing information along so we know what everybody is doing. That sort of robs you of all the fun. The question I would ask is, "How would you feel if we finally discover life on Mars, and they are way ahead of us and they have solved all the oceanographic problems that we are worrying about, and all you need to do now is to go into their web page and see why there are bottom-intensified currents?" I would feel enormously cheated. Because the fun is in learning and uncovering and not reading someone else's solution. A little bit of that is true in Russia. When we finally came and found they had worked on similar problems, they of course correctly accused us of not know what they have done. But the fun really is in learning and not in knowing. Especially if you are not a very good reader.

DOEL: In those instances where you really did want to know what a colleague in the Soviet Union or China had done, did you find that there were sufficient translation facilities available, or was that a problem?

MUNK: If you knew about a paper, there were always ways of getting it translated. And in the rare instances when I asked, there have been translators. I find someone. There was always some student here who knew Russian who needed a little money that you could hire people to translate the papers. And of course there is a translation service, there was one, to a limited extent. Abstracts would be translated.

DOEL: Right.

MUNK: So I think it was available. You must separate me from other American oceanographers. I have been a poor reader, because I have too much fun in doing something else.

DOEL: Yes, but I did mean it in the broader sense: was this a common issue that many Russians perceived?

MUNK: But I think for people who tried, it was possible to translate.

DOEL: I didn't mean to cut you off on China, if there were other important recollections that come back to mind about your early visits. [interruption]

MUNK: Well I always felt if I compare my four Russian trips to my four China trips, that the Chinese were more enjoyable. I remember that the day we left after a visit to Russia, no matter how exciting, that we breathed a sigh of relief when we were aboard an American aircraft, leaving. I never had that feeling in China nor at any time that there was a threat. Maybe there was an underlying threat that one sensed, I don't know. But that was very real.

DOEL: Certainly in the Soviet case, as we discussed yesterday, there were important national security issues, particularly what the KGB and others knew that you knew.

MUNK: Yes. I have always had this underlying feeling of being threatened. And people would be very rude, which they were not in China. I mean you wait to check in a hotel, and the girl who is supposed to check you in is talking to somebody, to her girlfriend, letting you wait ten minutes, and then if you expressed any kind of discontent they got furious with you, who are you to tell them off? Always, there was always that small official rudeness that I did not think one generally sensed in China. The person who looked at your passport, you know.

DOEL: Yes. And you were gesturing a moment ago to show the kind of disdain that you had felt in the Soviet Union.

MUNK: Yes. But I feel privileged at having participated in those two developments for two countries. It's added an awful lot.

DOEL: You felt they were fruitful at times.

MUNK: Oh yes, yes.

DOEL: Another activity that you were involved with in 1977 was the geophysics section of the National Academy, when you had served as the leader of that section. I'm wondering as you think back what you felt were the particular challenges that faced this section at that time, and what you wanted to get done?

MUNK: No, the NAS Ocean Studies Board is 1985, much, much later.

DOEL: That's much later. But you were heading the geophysics section of the Academy.

MUNK: Yes. Oh, oh. It's not worthy of much discussion with you. That section was preoccupied, as all sections, with the election of new members. You go through and you are judged on the basis of how successful you are in getting new people in. I was successful in getting new people in. We used a technique of saying I'm going to be in the job for three years, and there are different institutions and there are different subjects of geophysics. Let's make a 3-year plan that is fair in not pushing any one particular branch of geophysics or any particular institute. And we agreed in advance, we made a little subdivision of our section, and we agreed after much fighting as to who should be the nine people, and we got all of them elected, which was unheard of. So it was quite successful on that basis, but I think the whole effort of those sections where you really worry about election rather than solving problems is not worthy of much more discussion.

DOEL: Mm-hmm [affirmative]. Were you frustrated that the Academy wasn't doing more?

MUNK: The section heads are not supposed to do anything but worry about elections. I guess everybody has to pay his dues, if he is a member, at some time to do that. And again, I know of no substitute of how you go into govern it, except it's a shame that it means so much that people really care about being elected. I was once in Russia asked, right after I was elected... in Russia is of course this is more important, because your salary doubles.

DOEL: Yes. And the academy system in the Soviet Union was so much stronger.

MUNK: Is more so. I don't know how it is today, but it was extremely important. I remember being asked by someone there what happened to me after I got elected to the Academy, and I think the question was money-wise. I said my earnings went down \$50 a year. "What do you mean?" I said, "Well, I had to go and subscribe to the *Proceedings*. [laughs] Unlike there, where they double their earnings. I think that part of the Academy which has to do with who gets the honor of being elected is kind of nice, but it's not worthy of I wish there were other ways of doing that.

DOEL: Yes. We also spoke briefly yesterday about some of the awards that you began receiving in the late 1970s, particularly the Conrad Award.

MUNK: From the Navy.

DOEL: You also were the Senior Queen's Fellow in Australia?

MUNK: Yes.

DOEL: How did that come about?

MUNK: Oh gosh, I don't know. I mean, those things happen all the time. You get invited and if you think you we had never been to Australia, and what a chance. We spent the time in

Canberra. I have some people there I admire, and it was very pleasant to be there. Of course I have been back since, especially the Tasmanian one was more recent. It's not terribly important as far as my career is concerned. It is part of the very pleasurable things that happen when you are somewhat successful, that people ask you to come to places you would like to visit. And it's a good way to visit.

DOEL: Yes. In the 1970s, were there any other visits you made that left a strong impression?

MUNK: I don't remember. We have to go and look through the record.

DOEL: That's fine. I was simply wondering whether something comes very quickly to mind.

MUNK: No. I am very bad at remembering when I went.

DOEL: Understood.

MUNK: Some people are splendid about that. They'll say oh yes, in February I did this

DOEL: I'm not asking you to try to do that. I'm just thinking back to this general broad period of time, the 1970s and '80s.

MUNK: I don't think it's a terribly important issue. I remember going to Australia and working on a paper for the Stommel volume on *Internal Waves and Small Scale Processes* and having an interesting time talking to Stuart Turner, a well-known Australian hydrodynamicist.

DOEL: There was one other matter during this period, clearly not pivotal to your career, but I think illustrates part of your character: when you arranged with Teddy Bullard to write his own citation, rather than you.

MUNK: Oh, you know that story.

DOEL: You had noted it, and I since read it. How did you work that out with him?

MUNK: Oh, I think I've mentioned that I had written two outsiders about him, and I got awfully bored. So when a third invitation came up I said, "Teddy, I'm not going to accept it. Would you like to write your own biography?" Because people generally don't have a chance to say what they think is important, and that's why I think our discussion is such an interesting one, because you do give me a chance to say what I think. And Teddy said, "That's a great idea." As you know, he wrote it very critically on himself, as one hoped he would.

DOEL: It was very frank.

MUNK: Very frank.

DOEL: As I recall, he mentioned that he was advised take a job, any job, that came about, and having to clean the floors after the war.

MUNK: Right, yes. Then you know the story. It came back to me all redlined by somebody who had reviewed the paper, the editorial. I said, "Why in the world did you change it?" and they said, "We couldn't possibly publish it as it was, because Sir Edward would be gravely offended." [laughs]

Teddy and I tried to do a few things like that. One was... the National Academy had a limit of how many words you could have in a paper, but papers by fellows were not reviewed at the time, so at least you could be sure to get it in -- but it couldn't be more than two thousand words. He was a foreign member, so we were going to write two papers, the first under the names of Munk and Bullard; the second under Bullard and Munk. We were going to end in the middle of a sentence in the first paper, and start in the middle of the sentence in the other, and thought we could get away with it because nobody could review us. And then another time, we'd been through England and we saw a piece of machinery on a farm that was totally un-understandable. It was sort of a Rube Goldberg thing, and we photographed it because we tried to figure out what it was. We thought we would publish it in our paper in the Academy as Figure 1, and never refer to it in the text. People would see that picture and they would start reading our paper looking for it, and would never find it. [laughs]

DOEL: [laughs] Now did that actually come to pass?

MUNK: Unfortunately no.

DOEL: And the Munk and Bullard, Bullard and Munk papers that cut off in mid-sentence? That was never written either?

MUNK: It was never written. No. This was things we talked about. That would have been a good idea, wouldn't it?

DOEL: It would have been in line with a number of things the two of you did. It's clear that you were very fond of Teddy Bullard.

MUNK: Yes. Then in the early '80s we did the work on the internal waves, and the beginnings of tomography were really based on them. Well, internal waves led to the acoustic problem of acoustic scintillations, if you wish. Then Carl [Wunsch] and I in 1982 wrote our paper called "Ocean Acoustic Tomography: Large Scale Climate Measurements in Modeling." No, no, that was not the first paper.

DOEL: there were a few earlier.

MUNK: The earlier ones which started that work going was "Ocean Acoustic Tomography: A

Scheme for Large Scale Monitoring," which we published in *Deep Sea Research*. That was certainly the beginning, and we felt really that that was necessary because the mesoscale revolution had made us aware that the sampling requirements in a variable changing ocean were much more severe than in a steady-state ocean, where a single ship going to different places at different times made much more sense than when you have an ocean which varies both in space and time.

DOEL: Yes. That did affect thinking about instrumentation in a very profound way, realizing the extent of the gyres and the energy distributions within them.

MUNK: Yes. But the sampling requirement was critical, rather than what kind of instruments you used. So we had started work on acoustics, and as you know the ocean is a good propagator of sound and a very poor propagator of electromagnetic energy. In that sense it pushed us towards an acoustic scheme. And we're still caught by that 15 years later.

DOEL: When you published in *Deep Sea Research*, did you feel that this publication would reach all the relevant members of the community that you wanted, say, as opposed to publishing in the *Proceedings* of the National Academy, where it would come to the attention of climate researchers and others?

MUNK: We were more interested in reaching the ocean community. You are quite right, if you want to reach the more broadly communities. But I think our interest, you have to make a decision, and our decision was that. [phone rings] Could you excuse me?

DOEL: [tape off, then back on...] We're back on after a brief telephone interruption. You were mentioning that you really wanted to reach the ocean sciences community, oceanographers particularly.

MUNK: Yes. I mean people nowadays, when they have very new ideas, do publish in *Science* or *Nature*. We hope to do a paper in *Science* on ATOC now, usually a short sort of summary paper, and usually in parallel with a more detailed paper in one of the oceanographic publications.

DOEL: There's two ways that we could proceed: one is to go directly into the ATOC issue and the controversy, but perhaps we should consider the work you did on the Ocean Studies Board first.

MUNK: On the Ocean Studies Board? Let's dispose of that.

DOEL: You mentioned in one of the previous sessions that, in accord with what Frank Press wanted, you had combined the policy and the scientific components.

MUNK: Yes. And that was not just in geophysics, but also in other fields. The Academy had parallel committees in many fields, one socially oriented and one scientifically oriented. It had

gotten so elaborate that these groups would compete for government money which the Academy should never allow. They should be in a position to reluctantly accept assignments if they consider them to be of national interest, and if they consider themselves qualified to do it.

DOEL: Right. But not to be recruiting.

MUNK: Not to be recruiting. And Frank decided very correctly he had to change that, and one of the ways was to reduce the complexity of the organization. He tried, and in other fields it worked quite well. In oceanography, there was so much opposition that he didn't get anywhere. When he asked me to do that it was really the last resort, he said that if that didn't work he would just stop all committees on oceanography for a few years and then try again. And I was given that job, and we did manage. We were able then to appoint a new committee. We had a new name. It wasn't called the Ocean Studies Board before, and I was given a pretty free hand. I then asked people with whom I enjoyed working, like Carl [Wunsch] for example, and so on. We should look at the names. And also the key people from the other two committees. Roger [Revelle] was on it. As I mentioned to you, he really did not agree with having the social problems so closely coordinated with the others. But he, as always, he helped. He was positively helpful and the committee was a pleasure to work from.

DOEL: Had the committee been very active prior to your becoming chair?

MUNK: In different ways. There were different committees. I mean, there are some very famous things. Actually the mine warfare committee in some ways had a importance all of its own in the history of American academy and Navy relations. And the committee on oceanography, when Harrison [S.] Brown chaired it, made a study that was certainly of national interest.

DOEL: That was the major study that he pioneered back in the late '50s, early 1960s.

MUNK: Yes, yes.

DOEL: How well did you know Harrison Brown?

MUNK: I knew him not very well, but well enough. I would have liked to have been on that committee. I was just beginning, and I was not asked. At that time it was a great honor to be asked. [laughs]

DOEL: Yes. That was a critical committee to be on.

MUNK: Yes. And it did a good job.

DOEL: And who represented Scripps on that?

MUNK: Oh, Roger [Revelle].

DOEL: It was Roger primarily?

MUNK: So we started the new one. I chaired it for three years. Mary Hope Katsuras [?] was our secretary. She is a real character; I'm going to see her at the conference tomorrow, as she is now working for the Heinz Foundation. We managed under the circumstances. We had a policy of trying to get away from recruiting money. We stopped. Said that's not worthy of the Academy, and that we would look at every request from three points of view. One was, was it of national interest? First order. Two, why didn't the people who asked for it do it themselves? Was there a good reason to go to the Academy? Three, were we qualified, and was there somebody in our section interested in chairing it? We thought with those three criteria our workload would go way down. In fact it didn't.

DOEL: Is that right?

MUNK: Very little. I remember that Mary Hope pointed out, she said, "You know you thought you'd get to doing very few things, and in fact it hasn't been that way at all." There was a report that had been started called "Oceanography 2000," I believe, or "Oceanography 90." And I stopped that. People had started to bullshit around what would happen and didn't know it. But in some ways it never was stopped. I mean the "Oceanography 2000" came up in various things. But some people were very upset, because they had done some work on it and we actually never published an "Oceanography 2000" report.

DOEL: One thing I was curious about: when Ronald Reagan was President, there was considerable pressure to reduce federal funding available across the board for science.

MUNK: Yes.

DOEL: Did you have much contact in that period with members of the government addressing these broader policy issues, the role of federal funding in science?

MUNK: Well, the Ocean Studies Board did one thing you remind me of to pass time. The Academy had very good connections to the major agencies like Navy, Interior, DOE, and so on. These had gradually disappeared. The Ocean Studies Board reestablished that. We would get the Commanding Officer, the Admiral in ONR to come to our meetings, we would get the head of NOAA [National Atmospheric and Oceanographic Administration] to come, we managed to get the five or six key people, somebody from the State Department. So we reestablished our connections to the government on a reasonably high level. That was really one of my major concerns at the time, that I thought that an Academy committee that did not have the decision making people involved would not succeed.

DOEL: Yes.

MUNK: Now there were problems at that time. There were some members of our group who were very anti-government, and whom we had to try and keep from having the work fall apart. What's his name, the geochemist from Harvard, who was a very welcome dissenter? Emmons? [sp? Could not locate in available records.]

DOEL: the name is familiar. It might well be.

MUNK: He would complain about things.

DOEL: This is a reverberation of Vietnam War tensions?

MUNK: Right. And I was chairman of the faculty at UCSD during this time.

DOEL: Yes, that first came in 1968 to '69.

MUNK: Oh, that was very, very different. It certainly was an important part of my life. And I think we talked about it.

DOEL: We did indeed.

MUNK: Yes. Anyway, there was a little bit of that involved, and we had to try and be constructive. And I remember that the then Admiral of ONR, whose name is "Smoke" Wilson -- he is retired here, and I still see him occasionally -- being very skeptical of those pinkos who were coming to those meetings. But eventually we ended up on good terms with him.

DOEL: How did you manage to reconcile those views?

MUNK: By listening to them.

DOEL: Just by keeping the communications open?

MUNK: Yes. Yes, using Roger's ways of taking people seriously, taking the time to hear what they wanted to do. No, no other things. They started from the premise that we weren't really interested in that, and ended up knowing that we did pay attention. So that was quite satisfactory really, but it was very time-consuming.

DOEL: How much time did you end up spending on this in any of those years?

MUNK: Going to Washington monthly and having lots of interruptions phone calls. There were a few crises during the time. I made a suggestion that got everybody very mad, and I had to go call everybody on our committee and say, "All right. I will back out. And would you support this and that?" What was that? I did something very stupid. I made some decision without asking people that got everybody very mad. I then asked whether I should resign, and decided

not to anyway. Very time consuming.

DOEL: I can imagine.

MUNK: But it ended up pretty well. If you were interested, you should talk to Mary Hope.

DOEL: And you were replaced by John Sclater?

MUNK: John Sclater from Scripps. And then Bill Martin [?], and today Ken Brink from Woods Hole, who is going to be at the meeting tomorrow. So the new structure has survived.

DOEL: How much contact did you have after 1988? Were you able to pull out pretty much?

MUNK: I generally have tried to disengage pretty completely, like not trying to tell IGPP directors what to do.

DOEL: Yes.

MUNK: No, I pulled out to a very large extent. I mean, you maintain some contact, but it was not trying to influence things.

DOEL: And it was during this period of time that Ed Frieman became director?

MUNK: Yes. I chaired the committee that nominated him at Scripps. That was a time-consuming thing. It was an enormous committee, 20 members, much too big. Bill Nierenberg was on it, and Roger Revelle was on it; lots of people, and too big. And very, very difficult to make progress. Nobody really thought in terms of Ed when we started. But the committee worked well together. It's difficult, and we did not have fights. Fred Spiess was on the committee. He had not really forgiven me for the previous Director's Search Committee (which I also chaired) for selecting Nierenberg. But we ended up well.

Now the odd thing that happened -- I remember so very well -- is that the new director of Scripps of course has to be a member of the academic department, but the mechanism for electing a member of the department is very different from choosing a director of Scripps. The department has to vote into admitting him. And we had selected Ed after many, many, many, many months of discussion, and the committee was supportive. There was no strong opposition. The department of course, which knew very little about what we had done, said, "Ah, you're getting another physicist like Nierenberg. You're getting another Navy man like Nierenberg, and you have the same chairman who selected both." We had a meeting at the department where some people at Scripps said that we were selling out to Washington, and to the Navy. We had a very difficult meeting, and I thought for a moment that they would vote Ed Frieman down as a member of the department, which would have gotten us into an absolutely impossible position. I

was the person who spoke for the search committee at that department committee, and it was very unfriendly. I was being accused, you know, of being a pawn of the Navy.

DOEL: I imagine that was difficult.

MUNK: Very difficult. Then we have several members of the department who were Quakers, who thought, and with some reason, that we were not considering scholars and people. Ed [Frieman] of course was and is an active man in Washington and he's a good person. He doesn't do bad things. He has his own conscience. But when it came to a vote, we didn't know whether he is going to survive. And there was a one-third vote against him, which is enormous, and I don't think Ed knows to this day. It was one of the most difficult meetings in my life. It was annoying that people questioned whether that committee of 20, which God knows was big enough, had been fair in considering candidates.

DOEL: And fair and rigorous in evaluating?

MUNK: Yes, yes.

DOEL: How much of that had to do with [Bill] Nierenberg's personality, versus the kind of constituencies that he represented?

MUNK: Yes. Well it obviously is related. That was after Nierenberg had gotten some opposition, and so there was another physicist, both of them were members of JASON, and there was me as a member of JASON. It looked bad? Yet Ed and Bill are two people who are about as different as can be.

[end of Tape 3, Side A]

[beginning of Tape 3, Side B]

DOEL: I want to make sure that this is on tape since it just ran out: you mentioned that there was a one-third vote against Ed Frieman in the department, and you weren't sure he knew.

MUNK: We kept it from him, for fear that he would not accept the job if he knew that. Maybe that wasn't right. But then the people who objected got to know him and like him. I mean, nobody is totally popular, but the objection that had been raised against him as a sort of a military man were totally unfounded. He is not that kind of a person. So eventually, if the vote had taken place ten years later, it would have been different.

DOEL: Yes. But as you say, this was a particularly important issue right at that time, with broad implications.

MUNK: Yes.

DOEL: I should also mention, just in case this wasn't recorded, that you also noted that Frieman, like Nierenberg and like yourself, was a member of JASON.

MUNK: It looked like a conspiracy, we're trying to sell Scripps to the military. And as I mentioned before, it may not be on tape, the two people are just opposites, they are so very different.

DOEL: Yes. Was that something you worried about as you proposed his name as director, or did this only come clear when the faculty opposition arose?

MUNK: Well, again, you know he was not the only candidate. We had reviewed other people. When we finally agreed on him and he accepted, we had lived with that. After talking to him and others so long that it came as a bit of a surprise that there was such bitter animosity. It came as a surprise to me, I think to most of us.

DOEL: When you look back on it, does it seem that that animosity had its roots in the 1960s links between science and the military, or were there other dimensions that were also important for understanding that vote?

MUNK: I think those are the two things.

DOEL: Yes.

MUNK: The other one was that Bill [Nierenberg] represented a view that had gotten some degree of opposition. They thought we were just perpetuating the same kind of point of view at Scripps.

DOEL: When you became aware of the problem, did you speak informally with members who were going to be voting, outside formal committee meetings?

MUNK: The way search committees work, you have to pretty well know ahead of time what happens.

DOEL: I meant when the vote on Frieman coming up within the department.

MUNK: Oh, that department meeting. That came as a surprise. I mean, we had of course contacts. There was a person in our search committee representing the department all along. I forgot who that was, but whoever it was didn't help much at that meeting. I mean, yes, different parts of Scripps, including the department, were of course represented at the search committee. I'm glad that's over. That was very unpleasant. [phone interruption]

DOEL: We're resuming after a quick telephone conversation. We're likely to be interrupted again in a few moments, and I wonder if this might be a good time to ask you about your

receiving the National Medal of Science in 1985. I wanted to ask generally, when you think of the various medals and awards you have received, which were the most meaningful?

MUNK: Number one is the National Medal of Science. Probably number two being elected to the Royal Society as a foreign member. Number three probably getting an Honorary Doctorship from Cambridge in England. I think those are the three that I am most pleased about. I guess having the Navy and the Oceanographic Society name a medal for me has been a mixed blessing. Usually that's done after you are no longer around. But that has been very nice, because they have gotten such good people to get the medals, including the last one, Brekhovskikh. Brekhovskikh is living under very difficult financial circumstances in Russia. He was, I was told I was not there at the ceremony very disappointed that there wasn't a stipend that goes with it. Judy and I are thinking we should take some of our savings and endow a stipend for the future. I think we are going to do that.

DOEL: That's very interesting.

MUNK: I think it would give us great pleasure to do that.

DOEL: This might be a very good moment to pause since what we need to cover very soon will be the ATOC controversy, and we might want to cover as one large block.

MUNK: Let me do the Greenland Sea for a moment, because it has to do with the Brekhovskikh story.

DOEL: This is appropriate then. Let's go to that. I was curious generally how you first got involved in the Greenland project? This became a large, international undertaking, did it not? It takes place in the 1980s.

MUNK: No, that didn't seem. We wanted to find out under what conditions acoustic tomography could give unique measurements. And the uniqueness there was that we could place some moorings in the summer in the Greenland Sea when there was no ice. Then the area is frozen over. The moorings come up to within 500 meters of the surface. You come back next year and pick them up and have the ability of getting some measurements when the area is frozen, covered by ice, and not easily accessible. That was the basic idea. We did put in some moorings; we used the ocean acoustic tomography methods of transmitting between moorings. I think the experiment was successful. It's been well written up and published. We learned something about how the water profile changes as you go towards freezing and convective overturning, the formation of plumes and all that. It was a good example of the use of ocean acoustic tomography. The story I was to tell is a story of going back to the Russian issue.

DOEL: I particularly want to hear that.

MUNK: I have a friend in Norway named Ola Johannesson who has an institute in Bergen called the Fridtjof Nansen Institute. One member on his board was visiting San Diego. It was a man named Anderson [?]. We had lunch with him right here, and we talked about the Greenland Sea. Johannesson was participating in that. Anderson said, "Well you know, I must tell you a story about what happened to a Norwegian Weather Bureau weather mooring." Have we talked about that before?

DOEL: No, we haven't.

MUNK: He said the Norwegians had a weather buoy offshore which they needed for their daily predictions. It automatically transmitted, six times a day, temperature, wind speed, ind direction, humidity, temperature in the water, and all that. But it also transmitted latitude and longitude, which of course were always the same. And one day the young lady who was transcribing the data came back excitedly saying, "Something's wrong with our latitude and longitude channel. It's changed." The next day it changed again. And when they plotted it up, it had gone to Murmansk. The Russians picked it up, and took it to Murmansk. Incidentally, they had removed a MODE mooring. Even the ship that removed it was seen.

DOEL: Is that right?

MUNK: Yes, the buoy was stolen.

DOEL: I hadn't been aware of that.

MUNK: Yes. We knew they do that. I mean they were trying to see what we were doing.

DOEL: Were these military ships or oceanographic?

MUNK: Oh, those were spy ships.

DOEL: I'm curious what you mean by that. Others have written about the mutual benefits that came from keeping certain kinds of surveillance technology on parity between the superpowers during the Cold War.

MUNK: Well, anyhow, one doesn't take somebody else's moorings in polite society.

DOEL: No. Indeed. It's a violation of scientific practice and national interest.

MUNK: But it's happened. To go back to Anderson's story, the Norwegians sent via the Norwegian Ambassador a protest, how dare they do that, and the Russians of course said, "How dare you suggest that we would do such a thing?" But about a month later the buoy was on the air again, and the Norwegians plotted it and it was going back. The Norwegians sent out a boat to quietly receive the buoy, and it was reinstalled. And Anderson said, "Now that you heard this

story, how could you possibly believe that you could set up moorings in the most strategic part of the submarine operations of the Russian Navy?" It's where they have to pass. "And this is an acoustic mooring, much closer to military devices, and you are using a coded signal," (which we do). "How can you possibly believe that those things will be left undisturbed?"

Judy and I thought that a totally convincing argument. What do we do? So we thought about it for awhile and we said, "Ah, Brekhovskikh." I called up my friends in the Navy, said, "Would you mind if I go to Russia and ask and plead for them to leave our moorings alone?" The answer I got was that yes, you can go, but be careful. We don't think you are going to get anything out of it. If you want to go, go, but it's not going to do you any good. So I sent e-mail to Brekhovskikh, or whatever it was, and said, "Can we come and see you and talk to you about the Greenland Sea expedition which is coming up?" And we got a message back overnight, "We'd be delighted to see you. You will be guests of the Academy. I have programmed you to give a speech on ocean acoustic tomography on the day you arrive," and something like that. Judy said she insisted on going with me because she was afraid they'd do something terrible, we've always been worried a little. She wouldn't let me go alone.

I gave a talk on tomography and learned to my surprise from questions that the Russian audience knew a lot about the subject. And the next day, we had a private meeting with Brekhovskikh, which we had requested, and appeared at his office. He said, "Before you say anything, let me tell you the reason of your visit. You came here to ask me to help so that your moorings wouldn't be interfered with." And I said, "Yes." And he said, "Fine. I will do my best, but I should tell you that I do not command the Soviet Navy. But I will do my best."

So, we went, and we got into the Greenland Sea on the boat a year later.

DOEL: And this was now '86 or '87?

MUNK: Something like that. The American Navy had had an exercise up there, unfortunately just before we came. We didn't know this would happen. It looked awfully bad. In fact the Navy used the same so-called m-code which has some unique mathematical properties. Anyhow, the U.S. Navy effort had been tailed by two Russian Intelligence ships at all times, which they have every right to do -- never out of sight. The Intelligence ships disappeared on the last day of the U.S. Navy operation and never showed up during the month when we were up there doing our work. No moorings were interfered with.

DOEL: You haven't had a chance to talk to Brekhovskikh to find out since then?

MUNK: I have not asked him.

DOEL: That's a very interesting story. It's interesting also because of that time relations between the U.S. and Soviet Union, under the Reagan Administration, remained somewhat tense.

MUNK: Of course we had sent them the expedition plan. We sent them data when we came home. We tried to do that. But still, I think it's quite conceivable that visit was important to make the experiment go.

DOEL: Did you feel in greater danger in the Soviet Union at that time compared to the earlier visits?

MUNK: No. We felt about the same. We stayed at the same horrible hotel that the Academy owns. And ate at the horrible restaurant called the Budapest. The menu hadn't changed and the quality had not improved. We also stayed in a room that I'd stayed in before, and the toilet was still plugged up. [laughs]

DOEL: That is interesting. What I had in mind before was that the GISP [Greenland Ice-Sea Project] group, the larger contingent of the Greenland Sea project, did involve people from the Institut für Meerskunde from Hamburg, as well as a number of other foreign agencies. It was large in terms of numbers of people -- larger than the kinds of organizations you had worked with previously, such as Waves Across the Pacific.

MUNK: But like MODE. We had participated in MODE, which was a large thing. And we had our own experiment, which of course depended on and tried to use other people's data, but it was relatively autonomous. It didn't mean that we would go down if some other data was not taken. Furthermore we installed some instruments for other people on our moorings. So it was a mutual give-and-take.

DOEL: Was it more difficult to work in this larger environment than the kinds of projects that you had done as smaller-scale Scripps operations?

MUNK: The planning becomes bureaucratic. There are more meetings, more committees, and more time spent. The actual operation then is not so very different.

DOEL: Primarily planning the overall design?

MUNK: Yes. I mean these hierarchy of committees that plan is a bore. On the other hand, what has changed in planning these things, which I very much approve of, is that when we made a proposal we used to not estimate our error limits. The question is, are we going to sample adequately and have precise enough data to answer the questions that were asked? It was not the case before. People went out and just said we'll do our best. It is now standard practice that when you go out and propose to do something, you are asked: "Is your sampling adequate? Is your precision adequate?" And I think that challenge is a very healthy one, and has come about in my lifetime. It's very interesting. I hadn't thought of it, but I hadn't heard it said among people who talk about how things have changed. And it's a major change.

DOEL: Yes. I think you are quite right about that. I was thinking about a paper that you wrote

and published in 1988 together with David Farmer on bringing physical and biological observations into harmony. I'm wondering how that paper came about.

MUNK: Do you wish me to tell you how that came about honestly?

DOEL: Yes.

MUNK: I belong to a club called the Thursday Club, here in San Diego. The club consists of a few people who eat sumptuous dinners the first Thursday of every second month. It's a terrible thing, because we eat too much and drink too much. Roger [Revelle] is a member, and other well known people are members. There are around 15 members. It's sort of an English kind of thing.

DOEL: And it's not just for the university community?

MUNK: No, it contains some people like the heads of some companies and so on. I'm going tonight. So it's a timely question. And I told Roger one evening coming home from one of these meetings, that for my taste, the discussion was too dominated by the biological people, the people who had the little biotech companies here.

DOEL: The biotech firms, companies like that?

MUNK: Biotech people. And I said, "Roger, I think I am going to drop out, because there is so much biological talk and I don't know enough biology." Roger does, I don't. And Roger said, "Well, I suppose it's too late for you to learn." That really got me. And I had been invited to go to a meeting in France on this biology-related topic. It was held in a chateau in France, Chateau de Brenner [?], and I had declined. I came home from that and wrote them an e-mail, said, "I've changed my mind. I'm going." You see the topic was bringing biological and physical science together and that's why I went.

DOEL: Interesting.

MUNK: [laughs] Judy went with me, and it was a great meeting. I got to know David Farmer, who has been a close friend since then -- when we talk about really good people like Hasselmann and others, he belongs into that set. He's a wonderful experimenter. We've been close friends ever since, so it was a good meeting. So that's the story. You mustn't try to find deep scientific reasons behind my actions.

DOEL: Historians are well aware that many interesting developments come from just such contingencies. Do you feel that there has been enough discussion about the kind of links that you proposed here?

MUNK: Yes. I think that part of oceanography has gone very well. During the Sverdrup days, I

was brought up that as an oceanographer you should know something about plankton, you have to know something about chemistry, because you learn from one another. I never saw that work well. It was always sort of a ideal, but— And the reason I think it didn't work well was that the ocean sampling by the physical oceanographers was so poor, and the lack of understanding for example of mesoscale variability was so poor, that the products that we came out with were useless to the biologist. And if they claimed that they were useful, which they sometimes did, being polite, they were lying. They were so poor that now you can demonstrate that what we put out couldn't have possibly been enough accurate to be a basis of biological understanding. That has changed, and the relation to understanding changes in productivity and the oceanographic data is now meaningful. And again, I'm glad you asked. That's one of the real changes, good changes that's happened in my lifetime. Satellite monitoring is of course very much a part of that.

DOEL: Do you feel that existing institutional structures -- or those being developed -- are sufficient to allow members of these communities to interact freely enough to address these sort of problems? Or are there still bottlenecks that keep interdisciplinary work from going on?

MUNK: They do it much better than they used to, but not well enough.

DOEL: Is it difficult to know the solution, or are there some things you would like to see be done?

MUNK: Well, it depends so much on a few people. Some biologists have really learned their oceanography and have become very effective people. It's not an organizational problem. But the fact that that has become more popular and more interesting and more successful has meant that quite a few people, good people have learned enough to bridge those fields. I think we're better off on that than we'd been 20 years ago. Much better. [knocking] Come in, Susie! [tape off, then back on...]

DOEL: We are resuming after another good lunch break. There was one question I wanted to ask you before we stopped for lunch. We were talking about integration of the biological and physical sciences. How important was the growth of the environmental sciences as an increasingly distinct field -- as well as growing popular concern for the environment -- in shaping your views about links between these fields?

MUNK: Mm-hmm [affirmative]. That did help. Then people became more aware of it. And it happened at the time when the physical observing of the oceans became so much better, and so really could play a part in the biology. I think that helped, mm-hmm.

DOEL: One other question I meant to ask you: when you spoke about the amount of time consumed by the Ocean Sciences Board and the administrative activities you had when you were here in La Jolla, how did you try to divide your day? Did you set aside a distinct period for writing and a distinct time for committee business and other work?

MUNK: I do now, and have for many years, work home in the mornings.

DOEL: What time do you start?

MUNK: I start early. I used to start around 5:30. I'm a morning person. And by the time we read the daily newspaper, I really, I sort of thought whatever work I did in the first two hours was the important part. I leave a message on my phone, or my secretary did, that I work home in the mornings, and if it's urgent they can call me at home and give my number, and if not I will respond in the afternoon, which I do, and it works remarkably well. People generally don't call me unless they really have to, and I do make it a point to call back in the afternoon. That way I have my three or four hours at least alone. Then I feel very happy in the afternoon to work on other people's problems and with other people. I've done that, I do it now.

DOEL: And this has been pretty much a lifelong practice?

MUNK: Yes. It's not just the number of hours. I find that when you are in the middle of something that interruptions stop you, you get tired after you put something aside three times, you are suddenly bored with it. So I think for me, at least having a time to stew without interruption is very important. God knows I don't know how you get ideas, but I do know I wake up in the morning and I say, "Gee, the obvious thing is to do this," and then you want to go do it.

DOEL: Do you keep a notepad by the bedside? Do write down ideas that come at night?

MUNK: No, I haven't. That would be a idea, because sometimes you forget about it. But I do wake up in the morning saying, "My gosh, there is an obvious thing to do which I haven't done."

DOEL: That's interesting. One area that I want to make sure we cover this afternoon is the emergence of the ATOC program, which we have begun to talk about, and your role in organizing the feasibility test. Was there ever a point where you felt concerned you wouldn't be able to raise the funds to do this? Or there would be some other problem that made you hesitate about committing the amount of time that obviously it ultimately took to begin this project?

MUNK: Well, it happened that the idea of giving up came later. Not early. We had been doing ocean acoustic tomography on a smaller scale for many years by then. Started out with a 20-kilometer scale, that was Pete [Peter F.] Worcester's Ph.D. thesis. Then we spent some years around 200-300 kilometer scale mostly devoted to learning about mesoscale. And then we did a few thousand kilometer experiments.

DOEL: these are in the 1980s now?

MUNK: In the 1980s, when we then decided to do Heard Island on a 20,000-kilometer scale. It came up very, very definitely at a fixed time. I gave a talk at ONR. They have a tradition of a seminar a few times a year, and I was asked to give one. I talked about ocean acoustic

tomography, probably about Greenland and so on, and someone said, "Could you use it to do global climate change?" It had never occurred to me that that was in some ways an obvious application of that method, because it has the right scale. And I remember just saying to the question, "I don't know. We've never thought about it." I came home, and we had a visitor from Australia, Andrew Forbes. I said, "Andrew, do you want to work on that problem and see whether we can do an experiment? What would be the numbers, what would be the intensities?" Andrew said he'd love to do that, and we sat down and spent six months and wrote a paper called... what was it the paper that led to the Heard Island experiment? We tried to consider that quite carefully. What I mentioned before about thinking of error bars and sampling limits before you do an experiment was an essential part. It had a good title, the one with Forbes. Oh yes. "Global Ocean Warming: An Acoustic Measure?" [phone interruption]

DOEL: Okay. [right at end of tape]

[end of Tape 3, Side B]

[beginning of Tape 4, Side A]

DOEL: A moment ago, just before we were interrupted, you were talking about the first paper you published that led to ATOC.

MUNK: Munk and Forbes. We really pretty well tried to think through what we wanted to do.

DOEL: When you began doing that early work, were you concerned that the baseline would be long enough to overcome the problem of local distortions from the eddies?

MUNK: That was the problem. The key issue is that the mesoscale noise is so much bigger than the climate signal. If you go out and lower an instrument to a thousand meters and make measurements once a month, you would find variations of the order of a degree. Climate signatures are on the order of, at that depth, 5 millidegrees per year. So that's the problem. You measure say over 10,000 kilometers. Since the typical mesoscale is a hundred kilometers, you measure a hundred mesoscale diameters, so you reduce the variance by a factor of 100. And that was the basic idea. That's why long-range acoustics is a good way of measuring climate changes.

DOEL: Yes. Russ [E.] Davis of course continued to argue that even that baseline would not be sufficient.

MUNK: Well, you still have to worry about it. It still introduces the noise, but it's a hell of a lot better than having to have a hundred independent measurements.

DOEL: Right. I was just wondering how generally the community reacted -- whether you felt there was significant concern in the community about the measurement, or whether you felt that most were convinced by your arguments?

MUNK: I think that in some sense that was accepted as a pretty good idea. The objections to ocean acoustic tomography which carried over to the climate application were that it is an expensive method. And it's true that the sources are elaborate, we have to keep time to a millisecond, we have to have many different receivers to get many different paths.

DOEL: And Heard Island, because of its geographic convenience and special location?

MUNK: Well, they didn't object to that. I mean, the idea of Heard Island was I guess my problem of being a bit on the romantic side, just like *Waves Across the Pacific*: wouldn't it be exciting to have a source somewhere where we could hear the sound come in simultaneously on the American east and west coasts? I think that's a great idea. It really isn't very important from the point of view of climate, but I felt that was exciting and so did lots of other people. My view now is that it was a little too exciting. It's what really got the newspapers interested and what got other people interested and what eventually got to the environmental groups. That it was such a simple idea, and it had this thing that appealed to people. And it's such a small step from what is a good idea to what is a bum idea. You don't usually attack something you don't understand much as being a bum idea. It had to have the same simplicity to be a victim as it did as an asset.

DOEL: That's an interesting point.

MUNK: [laughs] As Judy was saying, we enjoyed talking about it a little bit too much, and so the very simplicity of the idea became the reason for its difficulty. Anyhow, as far as your question about Russ [E.] Davis is concerned, I think that's not the problem. When you look at maps of climate variability, as you know it isn't that the oceans, or the world, is warming at the same rate. The term "global warming" is a misnomer. There are some regions which even cool according to the models. Some warm a lot, some warm little. So you do yourself a disservice by taken averages which are too large. And if you look at the maps that people have taken of computer modeling, you find that 5000 kilometers is a good number. Ten thousand kilometers already starts mixing in regions which are definitely different. Five thousand is still the right scale for climate work. So in that sense Heard Island was an overkill. But the question that we raised was very deliberately, "Can you use man-made sources to go distances of the order of the radius of the earth," which is 6000 kilometers, "and still receive signals with a precision that makes it possible to measure small temperature changes?" Very simple. And the answer to this was not known. It there ever was an experiment where different estimates differed from easy to impossible, this was it. And we used various theories of internal wave absorption, this and this, with all our wonderful partners which I should say something about in connection with tomography, because we had the same group work together again and again and again. But when we evaluated that, there were differences between impossible and easy. So it was an experiment where we did not know to the last minute whether it would succeed or not. And that made it tremendously exciting.

DOEL: You've mentioned the names of several people -- I brought a list that comes from your

co-authored 1995 book.

MUNK: Yes. That's it. You know, when we started out, it was a bit of a shotgun marriage. We started our proposal here at Scripps. Carl Wunsch of course was always a member in connection with the satellite work, and, oh gosh, the man who used to be at ONR and then went to Florida, Hugo Bezdek, who was then at ONR: Bezdek said, "I'll support you, but you and the Woods Holes who have some similar ideas have to work together." So it was a shotgun wedding. We started working with [Robert] Spindel at Woods Hole. It was a most successful shotgun wedding, because we still are working together, and it formed a group of people who really kind of enjoy working together.

DOEL: This is the group that you mentioned?

MUNK: This is the group. See, that's the group that did all the tomography together. I do want to go back for a moment. You asked me to interrupt you.

DOEL: Please.

MUNK: Carl Wunsch and I, after we had written our original paper on ocean acoustic tomography*, not ATOC, spent a sabbatical in Cambridge together. Out of that came four papers or five papers. On that very subject. We really had a chance to think about various problems that we had left largely open in our original paper.

*Munk, W. and C. Wunsch: 1979. Our Acoustic Tomography; a scheme for large scale monitoring. Deep-sea Research 2b: 123-161.

DOEL: This is the 1982 sabbatical that you had in Cambridge?

MUNK: That's the '82 sabbatical. Carl was invited and then asked me to join him to write a paper for the Royal Society discussion, and it was called "How to Observe the Oceans in the Nineties." It was published. And that was a successful paper. It's been quoted since. The suggestion was made in that paper that a very successful use of methods is to use altimetry, satellite altimetry and acoustic tomography jointly, because the satellite altimetry has good horizontal resolution, and we do not. The satellite altimetry has virtually no depth resolution, and we do. We wrote that paper without really thinking anybody would pay any attention, and it's been widely quoted. In fact it is the basis of the present ATOC work, the joint use of satellite altimetry for remote spatial sensing and the ocean acoustic tomography for the interior sensing in the ocean.

DOEL: Was it a paper whose timing just seemed right?

MUNK: Yes. And it was a nice publication. You know, the Phil Trans [Philosophical Transactions of the Royal Society] does a beautiful job, and the Royal Society has its own

prestige. That worked very well. And Carl and I have a long record of working together and fighting together, but having a good time doing it. We still do. We did two papers together this year that are non-acoustics. Back to Heard Island. We needed to get the sources. The Navy had some sources on a special ship called the Cory Chouest, and with a big center well where they can lower sources. Admiral Pittenger, Fred Soalfeld and other Navy people worked hard in getting us the sources. They enjoyed the idea also.

DOEL: How long had that ship been in operation, the Cory Chouest?

MUNK: A few years, not very long. And so they let Bob [Robert Spindel] ride the ship to Yokohama. Bob Spindel installed the sources, and then I flew to Perth, Australia and joined the ship there. It was I think the prime adventure of my life.

DOEL: Is that right?

MUNK: And as you by now know, I had quite a few adventures. But the adventure of whether this would work or not was sort of more tantalizing than anything else.

DOEL: Was it primarily the intellectual challenge behind it, or were there other factors you think made that so?

MUNK: Well, I think the global nature is fun, as it was for Waves Across the Pacific. But mainly the fact that we really didn't know whether it would work. We honestly didn't know. In most other cases we had a pretty good idea.

DOEL: You have a hunch at least.

MUNK: We were perfectly prepared to come back and say man-made sources can't be used much beyond what we've done, and are no good for climate work. And the Navy, who supported our experiment, and other people, were perfectly well prepared for that conclusion. Then, as you may have read, the way the answer to those questions came about was most curious.

DOEL: Because of the early transmission?

MUNK: Because we were asked... Yes. I have lived that first day through my mind for a dozen times, because we knew we would start at midnight Greenwich, on "Australia Day", the first of February, 1991. And we knew the signal would take 3½ hours until it would come to Bermuda, and 3½ hours until it came to California. I wouldn't be going to bed for a while. I was going to bed the night before when somebody said that they usually do a 5-minute check prior to any operation. I said oh, sure, go ahead. And I went to sleep and then I was just dozing off when I got the answer back from Bermuda saying, "What's going on? We received you". I thought, oh my God, fantastic, went back to sleep, and then 10 minutes later the same message came from California. So we knew! I felt like when Mount Everest was climbed for the first time by [Sir

Edmund] Hilary. He had been living for that moment, and he was dead tired and he was sort of trudging, going step after step, and his realization that he had gotten on top of it came in the most anti-dramatic way. I don't know whether you remember the sentence. He said, "I suddenly realized that I was no longer going uphill." [laughs] I realized then, the morning before, that it was going to be possible. And that was a fantastic time.

Of course we had the problems that began, the biological problems began before Heard Island. It had gotten to the attention of the National Whaling Commission somehow, and they decided to challenge it.

DOEL: This was before 1991?

MUNK: Before '91. We were saved by John Knauss, who was head of NOAA then -- not by ruling in favor of letting us do it, but by insisting that a process of permits that usually took years would be done in a few months. It was the only chance we had. We had made all the arrangements with ten different countries. There was no way of postponing it. When it came up that they said you have to have a NOAA permit for that, and we had only two months to leave, we went to John and said, "Can that be done on a fast track?"

DOEL: Was this when the strategy was worked out that, rather than judging whether the experiment would be harmful, the study would be used to assess its effects on the marine mammal community?

MUNK: Well, yes, and the condition was then made that we had to have another ship whose only job it was to do the biological observations. Fred Saalfeld of ONR immediately agreed that he would support the second ship. The second ship was the same size as ours and had seven biologists aboard full time, very devoted people.

DOEL: This was the Amy Chouest?

MUNK: That was the Amy Chouest, including three Australians that did observe. There was a lot of criticism when ATOC started that the biological part of Heard Island had not been properly prepared, that it was not a good biological experiment. Which of course it wasn't. It was an add-on. But it was so arranged that if there would be any dramatic indications of hurting whales, that we were to be stopped. There was no question about that. Amy was there, and if any whales had breached, we would have stopped. And that was really its function. And the statement that it wasn't a well planned biological experiment was correct. It wasn't.

Don't forget also that the Heard Island source was 14 d13 louder than ATOC afterwards. It used bigger sources, and it used one transmission hour out of two, 50% duty cycle. We are now quiescent 98% of the time. Further, the source was much nearer the surface.

DOEL: This was the initial, 209 decibel source?

MUNK: Yes.

DOEL: Two hundred and fifty watts, if I recall correctly?

MUNK: Oh no. We are 250 watts now.

DOEL: So it was much more?

MUNK: Much more. Much more. Like 10,000 watts. The biological component was really quite wonderful. We all ended up going up with the biologists to the bridge and looking for whales and this added a great deal to the excitement of the experiment. But I do want to say now that it wasn't a biological experiment, and to attack ATOC because Heard Island was not a biological experiment didn't seem to me was quite fair.

DOEL: And until the Heard Island test, you had never been asked by any government agency to prepare a permit?

MUNK: That's correct. We had done acoustic work for almost 20 years. It never occurred to any of us that we would have to get permits, and of course many people who do acoustic work today using louder sources have never needed permits. If we can properly keep this thing from being publicized, NOAA has had sound sources at sea louder than ours during all this time without permits.

DOEL: Is that right? We can keep this part of the interview closed, if you like.

MUNK: Well, only those few sentences.

DOEL: Surely. But I'm curious what sort of experiments these are?

MUNK: NOAA has a source now in the Pacific that they do for various acoustic works which is louder, which never went through the permit process. This is what I want to keep quiet: we made the policy decision not to try and succeed by saying "Oh, someone else is louder than we are." Obviously all geophysical prospecting is louder than we are. The Navy is louder. We tried to fight our problem on the basis of its own merit. But it is an important answer when you asked, were we unaware. I mean, the seismic community had been using louder sources forever.

DOEL: Yes. In addition to the other sources of acoustic pollution, there is noise pollution from passing ships.

MUNK: Yes.

DOEL: Even though permitting had not been required up until that point, do you recall anyone

in the acoustics group mentioning that this could be become an issue or concern? Or was it one that really took everyone by surprise?

MUNK: Well, the pre-Heard Island challenge came as a complete surprise.

DOEL: That's what I was thinking of particularly.

MUNK: The post-Heard Island challenge to ATOC was not a surprise. But I had thought that our Heard Island had put that to sleep. Judy said she'd be surprised if it didn't come back. And so I think that's the answer. I really don't think, I don't remember any colleague saying we shouldn't do that or anybody prior to the Heard Island experiment.

DOEL: That's interesting. That's important. Did you have direct contact with members of any of the groups that became concerned with whales, around the time of the first Heard Island test?

MUNK: Yes, oh yes.

DOEL: Which ones in particular?

MUNK: Well, there is the National Whaling Commission. God I should you know my memory for names is poor. The man who runs the Whaling Commission -- we certainly had personal contact with them.

DOEL: John Twiss [Jr.]

MUNK: John Twiss. Good for you.

DOEL: What sort of person is he?

MUNK: Oh, he's a perfectly nice gentleman. I think he sort of I don't know what his motivation was. He probably thought he was saving the whale population. And he knew, yes, I think his is a very political job, the Whaling Commission. I thought he was a bit unreasonable and it came at a late time. We certainly never tried to hide Heard Island. To the contrary, we talked about it very widely. And I've seen him since and he feels he did a service to the community, and he took me to lunch the other day when I went to Washington. I mean we do know each other. And the NOAA group of course mostly was very helpful. There were some people -- there are several people at Woods Hole and others. Oh, talking about people in our community, yes. Yes, there are some people that really started objecting, but later not, they were probably not thinking about it before Heard Island, but maybe they had. Peter [Tyack], at Woods Hole. Then there is another man at Woods Hole, who is a well known whale man.

DOEL: William Watkins?

MUNK: Yes, Bill Watkins. Now you know I knew those people somewhat. My Italian friend, Guiseppe di Sciara, who got his degree on marine mammals and who lived with us here, knew all these people, and so we met them. Certainly Watkins was very much against the experiment before Heard Island, but I do not remember whether we'd heard from him beforehand. We really need to ask him that. That might be an important point. But I don't recall that somebody said "you mustn't do that."

DOEL: How did you know these people at Woods Hole? Was it from simply visiting at Woods Hole over the years?

MUNK: Well, I knew Watkins partly through Guiseppe di Sciara. And during the war when I was in Woods Hole, I met the early people who worked on marine noises. There was a wonderful man, still alive, who was the father of all work on recording the noises of marine life. We must get his name. [?] (Bill Scheville).

DOEL: We'll make sure of that names get on the transcript.

MUNK: I mentioned him in some talks. And I remember being in Woods Hole and just being fascinated by his stories about how they recorded those various noises. I mean, there was a community, we all knew each other, and I think enjoyed each other. It was not an antagonistic situation. After all, the discovery of the sound channel by [W.] Maurice Ewing, which in a way is the central discovery, happened just before I came to Woods Hole, and I remember the excitement of having discovered the SOFAR channel and being amazed that you could hear an explosive charge at a thousand kilometers. It was just fantastic. And so in a way I saw the beginnings of that excitement, and always felt very intrigued by the acoustic possibilities of doing oceanography.

DOEL: I was thinking about that, because it had occurred very early in your career.

MUNK: Yes, yes.

DOEL: What I imagine came clear after the Heard Island experiment was that you might need new permits for the succeeding experiments. Was there anyone who helped guide you through that process? Did you have meetings with those who knew Washington after that initial regulatory concern, or did you try to do that more from what you knew here?

MUNK: Well, when we came home from Heard Island, we thought we'd have an easy time of getting ATOC. We didn't. It took a couple of years until we got the money.

DOEL: Did this involve going back and getting funds from ONR?

MUNK: It was actually from ARPA [Advanced Research Projects Agency]. ARPA became interested and gave us some money for ATOC. Then this new group called SERDIP was the one

who really financed us. Victor Reis, who was then head of ARPA and then became the head of the DDR&E, took an interest.

MUNK: He was personally interested, and that managed to get us through. He was intrigued with the whole idea. And I did not at the time try and do much more on regulatory things. I thought the fact that we were 30 db's quieter, had a 2% duty cycle, working a thousand meters instead of 150 meters -- that those three things would be enough to keep us from any further trouble considering that there were no dramatic manifestations at Heard Island. I'm an eternal optimist. Judy thought otherwise, and was right. But when the problem did break with the Los Angeles Times article that we were going to kill 600,000 whales, then the university in fact asked us to take legal counsel. We did that very reluctantly, but ended up being very much taken with the man who helped us throughout, because he took the problem seriously, learned a lot about it, and became a real collaborator in our attempt to go on. It was absolutely essential.

DOEL: And this person was?

MUNK: Alan Waltner.

DOEL: We'll make sure that all these names are added.

MUNK: Pete knows all those names. Isn't that awful? I haven't talked to him now for a year, but I used to talk to him almost daily.

DOEL: I have similar problems. We'll make sure that these go in.

MUNK: Yes. But I do want to say that, with all the nasty things said about lawyers, this man took the problem so seriously that he could have given a scientific talk, or he could give one now, on the whole issue. He was a real pleasure, and he really helped us. He got so annoyed at some of his colleagues whom he had known in school who were representing the environmental groups that in fact he gave a talk at one of the environmental meetings attacking his own colleagues and friends for having made some very unreasonable requirements.

DOEL: The period we're talking about now is early 1994, when the LA Times piece appeared?

MUNK: Yes.

DOEL: Was there an undercurrent that you detected in 1993, or prior to then, that indicated a problem was brewing?

MUNK: Yes. There was a student from Cornell, Lindy Weilgart, who had written about us, started the thing on e-mail. She became sort of the epitome of our problem. We often mention her. She was a student in marine mammals in Cornell. She started an e-mail attacking us, and she got the units wrong, water and air units. She was the one who was interviewed

in the Los Angeles Times. She called them and talked them into that interview that took place without anyone talking to us.

DOEL: Yes. Was that when you were traveling to Washington for the hearing?

MUNK: Yes, we were. It was in fact Martin Luther King Day, and we were going to a hearing to ask for the permits. Lindy had organized for interview with a Los Angeles Times man, whose name I do remember, Richard Paddock.

DOEL: Later she was the one who worked with Hal Whitehead?

MUNK: She married Whitehead.

DOEL: Is that right?

MUNK: She became his wife, and then he and she were you might say the core of the opposition.

DOEL: Yes.

MUNK: And it's never stopped. And I've seen her a few times, and she is angry.

[end of Tape 4, Side A]

[beginning of Tape 4, Side B]

DOEL: Just as the tape ran out, you mentioned that she is so angry that you still can't talk with her.

MUNK: Yes. One day I went to talk to her, and said you made a mistake on that one thing. She mentioned the total intensity noise. And there's a difference when you talk about watts per Hertz or watts over the whole frequency band, again a unit problem. I said you were using the wrong unit, and she started shaking, she was so mad. I decided that we shouldn't be able to talk.

DOEL: Had the university itself sensed that this was a problem? Did they recommend doing anything?

MUNK: Not until after the Los Angeles Times piece. And then of course things really broke loose. It appeared in papers all over the country, we were telephoned, our public relations office got swamped. We have an organization called Friends of Scripps Institution, and they started calling individual members, saying, "You mustn't give money to Scripps." That's when Scripps became worried. And as I have often said, Ed Frieman was wonderful. I asked him at the time whether he wanted us to stand down, we were hurting Scripps, and he said no, he wouldn't have

that. He wouldn't be able to live with himself if he backed down.

DOEL: That's a critical point. He did support you.

MUNK: Yes. And I gave a talk here, they asked me to meet in the main building with the Scripps staff, with everybody who wanted to come. It was a full room in Sumner Hall, and I tried to say what's happening. It became severe. We were threatened that people would burn up buildings, we had to hire extra police. Everybody from the director on down did nothing else but trying to live through a fantastic amount of pressure. It was very uncomfortable. Judy and I would get phone calls all through the night saying, "Oh, you whale killers, you, and you should..." and on and on.

DOEL: Yes. And I want to get to each of those points in detail. I'm wondering what it was like for you to read the LA Times story. Did you read that before you got to this coast?

MUNK: I forgot. I think I read it in the morning because we take the Los Angeles Times. I remember well, it was in the left column.

DOEL: So you had arrived back here?

MUNK: I had come back from Washington, not knowing that there was such an article. And I then tried to reach the Times. I was furious. I knew Jerry Warren, the editor of the San Diego Union, and I called him up. I had known him because he lives here in La Jolla, and said, "Is there any way I can protest an article which got their numbers totally wrong and never checked with us?" Jerry called up the editor of the Times, then I got a call from Richard Paddock, who had written the article, to tell me he had been commanded to meet with me. I was going up to Monterey [California] for a meeting of the MBPRI Board of Directors. So I suggested we meet in Monterey for dinner. Then he came to the "Sardine Factory" where we had dinner. Do you know Monterey?

DOEL: I don't.

MUNK: There's a wonderful restaurant called the Sardine Factory. He said, "I will be host tonight," and I said, "You know there are two things I have decided. One is that I didn't want to take you for dinner, and the other one is that I didn't want you to take me for dinner. We had a rather good dinner. You know, he was not unreasonable, and he used an excuse for not having talked to us. He said he did make a call. Well he made one call apparently on a university holiday when nobody answered. I don't think he had a leg to stand on. And then he did write a letter, another first-page article a month later in which he got the facts straight, but it didn't help any. By then all the environmental groups had started building their campaign to stop us, and that second article didn't really enter the equation.

DOEL: Did you sense that he had particular sympathies towards environmental issues?

MUNK: He is not a scientist. I have seen articles of his on political issues. I think he just thought it would be provoking interest and get his name known, or whatever newspaper writers do.

DOEL: Had George Alexander already left the post of science writer at the L.A. Times?

MUNK: We had met George Alexander. He must have left. The science writer told us that once somebody has written an article about a subject, it belongs to him by some ancient newspaper tradition. I have a enormous amount of material I kept the files, and for somebody who wishes, we have all the information. There were some people who backed us, it wasn't all negative. But the negative ones carried the day.

DOEL: The hearing in Washington came just before the hearing in Honolulu, and by late March or early April the e-mail campaign reached its peak. I recall seeing "Pseudo Science Marches On" on an e-mail distribution list. Clearly that message reached globally and broadly, and was a polemic.

MUNK: NOAA folded up. They had given us a permit, it was done all publicly. But Jim Baker, under great pressure because some Senators threatened to cut the NOAA budget, said, "All right, you better go back and get a full environmental impact statement," which was two years and \$350,000 later. And the two years is what really hurt. And I don't blame Jim. He was also helpful then, but he really had his hands tied. There was no good reason why there shouldn't be an environmental impact statement. But we had gotten permits, we were actually at sea at Kauai [Hawaii] to place our source, and I was in the boat when the news came that the permit had been withdrawn.

DOEL: What were the hearings in Honolulu like?

MUNK: Oh, that was a disaster. I had a cold and lost my voice, as if it was a stroke of God.

DOEL: You and Judy were both together at these hearings.

MUNK: Yes, we went together. Awful meetings, you know, very hostile and unpleasant.

DOEL: How did it compare to the anti-JASON meeting that you attended years earlier?

MUNK: Oh, the anti-JASON meetings were a picnic. They only threw a few typewriters on the floor. But we had some kind of sympathy with the students who did it. It was morbid in a kind of a sympathetic way. Nothing like with the people who really hated us. It was just plain downright hate that we encountered there, and no discussion. I mean, these meetings gave everybody a right to say how terrible we were. And now you know we talked before about that. We were attacked on a number of bases. One, that it would gravely hurt the whales; two, that is was a stupid experiment because it could never give any information on climate; and three, that

we were secret serfs to the military I personally was doing this secretly, I didn't give a damn about climate, it was doing a Navy job for them. Those were the three major counts. And the second one was particularly annoying because it was so stupid. They said, "How can you ever measure anything when you are down in the sound channel a thousand meters deep when it would take a million years for global warming to have an effect there?" Well, that was stated again and again; somebody had started that argument and they thought it was a good one. It's wrong on two counts. First of all, even when you place a receiver in the sound channel, acoustic rays span the whole water column and you measure the temperature change in the whole water column. Secondly, the time it takes for the signal to get to the sound channel is 20 years and not a million years. The millions years are the right number if there was nothing happening in the ocean except molecular diffusivity.

DOEL: If there were no other ways of mixing.

MUNK: But we know even from the CFC's and from various other tracers that you get a signature down to the sound channel in less than 20 years. Yet we heard that argument again and again, how we have no excuse for doing it, it will take a million years to get down there. We said no, that's wrong on two counts, and then nobody paid any attention. Next talk same thing. You have no way of responding. Then the way those meetings are usually held, you are not permitted to respond. You were permitted to make a statement in the beginning, which was widely ignored, and then listen for six hours to people saying why you were doing awful things.

DOEL: That must have been an extraordinary experience.

MUNK: Yes. My wonderful wife went with me on all 11 hearings. I came close a few times to saying well what am I doing here, at midnight in Kauai listened to these things. But I really didn't want to go through a failure like Mohole once again.

DOEL: What kept you going?

MUNK: Not wanting to end my career on that note. And we'd spent a lot of money, public money, on what we thought was a good idea. We thought we had a contribution to make. You must realize I'm no longer PI [Principal Investigator]; Spindel and Worcester are. I am on their executive committee, I do hear what's going on, but they are running it. And they do a very good job. I waited for resigning from being PI until at least we were no longer under attack.

DOEL: Yes. You made clear yesterday that you did come to realize that to make the projects like this succeed, you had to invest very deeply into seeing them work. You also mentioned that you were being attacked in the House of Representatives members and by [U.S. Senator] Barbara Boxer and [U.S. Senator] Dianne Feinstein, as I recall—

MUNK: Yes. Yet they had both backed it. We had a letter from Barbara Boxer, or was it Feinstein, saying ATOC was a good thing. But they had forgotten that they had written that

letter. When they got thousands of letters from their constituents they then wrote and said we must stop that wicked thing. The worst person of all is the Congresswoman from Hawaii.

DOEL: [U.S. Representative] Patsy Minsk?

MUNK: Patsy Minsk accused us of trying to hide and put something over, when we had actually written her office. We had those letters. And she gave a talk saying that we were trying to put something over. And I went to see her in Washington, and said, "Really there is lots of things you can say against us, but here are the letters to your office a month ahead of time of what we are doing, so the idea that we tried to put something over on you and others is just not correct." She looked at those letters and said, "I will look into this," and walked out.

DOEL: You sensed, I'm sure, that this was part of larger agendas that were being played out?

MUNK: I suddenly remember something I should mention to you. After we came back from Heard Island, I gave a talk at the International Union of Geophysics and Geodesy in Vienna they meet every four years. It was about ATOC, about Heard Island, and the day before a German magazine called Der Spiegel had an article about us, very negative, about "the Noise in the Deep" it it was called in German. It proposed that we would hurt mammals. I thought that the meeting would be very critical but it was well attended and nobody protested.

DOEL: Your talk was at the IUGG?

MUNK: IUGG in Vienna. I thought at that time that there might be loud protests but there wasn't.

DOEL: Had the Der Spiegel piece come out already?

MUNK: Yes. There was also an article in Science by a science writer named [Jon] Cohen, who curiously enough I have seen the last few months. He is now in San Diego. The Cohen article says, "It is said by some that" instead of quoting the people who had made the observations. Roger [Revelle] was head of the AAAS then, or had just been. In one of the last things he wrote, Roger wrote a letter to the editor which they couldn't ignore under the title "Shame On You".

DOEL: It was my impression that Jon Cohen was sympathetic towards the protesters.

MUNK: I thought it was very clear, and it made a more exciting article than saying everything is fine. But the funny thing-- my daughter Edie is a friend of his wife, and said, "Come and we'll visit the Cohens." He said no, he didn't want to see me, he was worried about this article years ago. Then eventually we got together and I went to his house and had a drink. After the meeting he asked whether he could write a set of articles on the ATOC problem, and he wants to do it from a new point of view. He's written some very good things since, and I thought that would be great.

Anyway, the Der Spiegel piece came afterwards, I think, and Der Spiegel said the same kind of thing. But the meeting in Vienna went well enough, and at the same time, either before or afterwards, I went to Cambridge, and I was asked to give a talk at the [Harold] Jeffreys Memorial. It was a nice meeting, the Jeffreys Memorial, and I gave the Jeffreys lecture, whatever it was called. While there I asked the chief scientist of Greenpeace, who had opposed us, whether he could come from London to Cambridge and listen to the talk and have a chance to meet with him. His name I don't recall either, but he's written a book on global warming, and he's quit Greenpeace now, and he's rather well known, a good man. He said yes he'd like to come, and he will see me after the talk. He came with two other people, a young woman who was the head biologist of Greenpeace, and the third man whom I didn't know. Too bad Judy isn't here. She would tell you that better. I said, "I wanted to meet you because we are going to continue this work, and I wonder whether we could work together with Greenpeace and not against it and see whether we would do our future work in a way that was not objectionable to you." The chief scientist of Greenpeace said, "That's a good idea. Let's stay in touch, and we'll work that out."

But the third man present, whom I hadn't identified then, said, "No, we can't do that." It turned out he was the leading administrator of Greenpeace. He was a lawyer in London. And we said why not? He said, "It's not the Greenpeace function to work with people. Greenpeace has the function to stop wicked things from going on," a negative role or prohibitive role. Which is probably true. That's how they work. But I've never quite forgotten that. It casts Greenpeace in a role as a policeman, and not in a role to really try and do things properly or well. It would have been ever so much better for everyone, including the environment, if one could have worked with those groups. But I've since found that they generally are not interested in doing that.

DOEL: When you look back on that intense phase of the controversy in '94 and later, which group seemed to be in the forefront of the opposition? Was Greenpeace one of them?

MUNK: Yes. The NRDC [Natural Resources Defense Council] was the number one, which is curious because IGPP had worked closely with them in a positive way on problems of nuclear testing. This was not me -- this was Freeman Gilbert and John Berger and other people at IGPP. This was when the problem of whether the Russians were cheating came up. IGPP maintained some of the seismic stations in the Soviet Union, which were a key factor. NRDC were number one. The number two in America was the Sierra Club Legal Defense Fund, which is a separate organization from the Sierra Club, and which has since gotten a new name. They are now called the Environmental Legal Defense Fund. And number three was Greenpeace.

I thought the Sierra Club Legal Defense Fund was the nastiest of them. They had people who were downright nasty. They made their living by suing on behalf of other people, not by trying to make things work. Eventually we reached some sort of an agreement with NRDC. We also had discussions with the head of NRDC in New York. We bitterly objected to a NRDC fund raiser in which all the wrong numbers were used, apparently a successful fund raiser. The head of NRDC first talked about some Navy work: the Navy was testing depth bombs at a very high intensity,

and NRDC objected to that. And then in the same fund raiser they said another enormous experiment of this sort is ATOC even though our intensities differed by a factor of a million or ten million or so. It was like comparing and 1m anthill to a 10 km Mount Everest, literally speaking. And bringing them into the same article without mentioning that you are talking about an anthill and Mt. Everest. We challenged NRDC on that, and I know some people in Washington who had been on the board of NRDC who took our part. The NRDC head wrote to him, and he wrote a sort of a meaningless letter back that was nice to me personally, that he knew about my reputation and that I was a blah-blah-blah, but not ever answering, ever, the problem of an entirely misleading pamphlet. And Joel privately said, "You are being a bit naive. We at NRDC are very separate from the people who are the money raisers. We sort of smile at them, they are the ones who keep us going, but you mustn't confuse the money raising, fund raising personnel of NRDC with us. We are trying to do something about environmental issues." And that's probably true. They probably have professional fund raisers who don't care much about what's happening, but they want to have issues that raise money.

DOEL: Yes. But that does raise important questions. As you look back, was there a fundamental distrust among those who were willing to accept those incorrect figures? In other words, were they unpersuaded by the evidence that you were able to present indicating where the actual audio range was, or was it a problem of just not understanding the science? They seem to be two different sorts of commitments.

MUNK: Certainly a feeling of distrust, that we were dishonest, yes. Total distrust. Which I guess is in some ways related to not listening to the numbers, saying, "We know you are not to be trusted, we know you are a secret agent for the Navy. The experiment doesn't make any sense because you couldn't possibly get a number, because so-and-so told us that," which he did. So-and-so's a former professor of the University of California at Davis who published saying that you couldn't possibly measure anything, and he is a co-professor of yours. The fact that he didn't know anything about diffusion and convection didn't matter. That was the issue as to how long it would take for a signal to go down to the sound channel.

DOEL: That's very interesting. Someone had actually published this in a scientific journal?

MUNK: Oh, not published in a scientific paper, but given that to newspaper accounts. No, nobody published that. Because it's wrong, and I don't think he would get away with it.

DOEL: One couldn't imagine that getting through peer review.

MUNK: No. But I met that man, and twice at meetings I said, "Don't you understand there is a difference between diffusion and convection, and him saying, "No comment." Distrust. Absolutely.

DOEL: Did it remind you of any other event, development in science or outside of it that you had gone through, or did it seem a completely new experience?

MUNK: It was a completely new experience to me. I always thought in some ways I was part of the good guys. I have since met people like the people in the medical profession who work with animals, and they said, "Welcome to our group." I mean, they've known this kind of thing long before we did, and I imagine the nuclear profession knows that, and lots of other people. For me it was a new experience.

DOEL: But in your case too it involves members of a related profession, or at least some in the same scientific discipline.

MUNK: Oh. There was the Peter Tyack group, and when I went to Woods Hole I'd see them. I mean, they are colleagues. This man from if I said UC Davis I think that may or may not be right, but he was a former professor. He was beholden to environmental groups. He didn't know his ass from a hole in the ground in that field. But of course he had a union card, which was perfectly respectable, and they said, "Well, so-and-so has said that it takes a million years to go down," and he is a professor and a doctor.

DOEL: But you felt the majority of critics were outside the profession?

MUNK: Oh, oh, vast, vast majority.

DOEL: Does this represent a fundamental schism in the way that science relates to the broader public? Is this something new in American politics, in your view?

MUNK: I hope not. I mean, there is certainly a need for active concern about environmental problems. It's terribly important, and if it weren't groups worrying about it we would certainly do bad things to the earth. You would just like to see that done by people who are responsible and not antagonistic and emotional. There are groups of course which do that very well, and there are some groups, I think Greenpeace included, who I really wonder whether they are more interested in their publicity campaign than in really saving the earth. So I'm trying not to be bitter in the sense that I think it's very important that there are groups that have been fighting against interests who would do unnecessary harm to the world. I just wish it would be done in a more constructive manner.

DOEL: Yes. Thinking back to the phone calls that kept coming in the middle of the night: how long did that last?

MUNK: Oh, about a month. And you know we were going to transmit 20 minutes every three hours instead of one hour out of two at Heard Island, well 20 minutes every four hours on every fourth day. So they'd call us every 20 minutes every four hours, wake us up, and we eventually took the phone off the hook of course. But we didn't know... How do you know what people are going to do?

DOEL: This happened when we were off-tape at lunchtime. When you mentioned that extra

security guards were hired at Scripps, did you feel that this house was itself in danger? Did any protesters come directly to the house?

MUNK: No. But people have done stupid things like dropping bombs in clinics and stuff like that.

DOEL: Did the university develop a strategy of how to respond to the challenge, or was it more or less up to you?

[end of Tape 4, Side B]

[beginning of Tape 5, Side A]

MUNK: Waltner insisted that we do that, and insisted that when we went to Hawaii, we had to go to a public relations firm or whatever they call them to represent us. That went very much against our grain, because we always thought that that's such a bad way of doing things. I remember going to Hawaii and having to appear in one of those firms with many names on it, and they briefed us on how we should answer. It was a funny experience. We obviously are not good at this sort of thing. Chris Clark, a marine mammal man from Cornell who knows more about noise and mammals than any person in the country, maybe in the world, was working with us to try and organize a research program. When he re-appeared at that public relations firm prior to a hearing, he said, "I'm not going to listen to you. I want to talk for myself. And I don't like to be told what to say by a

DOEL: A handler?

MUNK: I felt the same way. They said, "Well, would you mind if we try and go through a test?" So I was set down, and this was a television interview practice. They asked me a question that I had never thought about. I became completely flustered, then said: "let me try again". They said, "See! This is a question you might be asked this afternoon." And so I kind of had to admit that maybe there was some merit in learning these things, much against my will. Then we would appear on television programs, and I'm very bad at that. I really don't like it. I remember having an interview and thinking, "Well that went relatively well." It lasted 15 minutes and it was not unfriendly, and then that same night they played one sentence out of the 15 minutes which gave a completely false impression of how the rest of the interview had gone, and I felt like I had just been had. From then on, and now that I'm out of it in a way, when somebody says, "Will you appear on a television?" I say no, I do such a poor job and I hate it so much, I'm happy to talk to you but I don't want to appear in a television program.

DOEL: Did you come to feel that in dealing with kinds of hearings that this kind of preparation was necessary?

MUNK: I don't know. We met in Hawaii a number of times with groups, invited everybody to come privately. An interesting experiment. We met some people who were really very worthy of talking to who cared and listened. We met people who had their mind closed, and didn't know

much. I remember having a discussion with a group in Honolulu which we thought went very well. It was an interesting discussion, and then one man who had been sitting through the whole thing at the very end made a statement that was totally unrelated to any discussion that had gone on, saying, "Well, we know that you are just a Navy agent," or something like that. It had the advantage that some of the people who opposed us really became aware of the fact that sometimes the opposition wasn't rational. And people who defended us, said, "I think you all should listen to what they say. We want to find out what's really happening." So it was an interesting human experience. I don't know what I would say if you asked now whether we'd go through the whole thing again. I probably would say no, rather not. But it certainly has been an experience.

DOEL: Of that I have no doubt. Were there people in Congress or in the congressional staffs, the major science committees, who were sympathetic?

MUNK: Yes.

DOEL: I'm curious which ones they were, and how effective you feel they became in the controversy?

MUNK: We saw a lot of them. It was arranged through our Washington office. We have Kathleen Ritzman here who helps with this, and she arranged seeing people. For example Congressman [George E.] Brown [Jr.], who is a very able man, knows a lot. We saw him, and that's when we tried to see Patsy Minsk. But we saw most of our Senators and Congressmen and their congressionals. And I think generally found them supportive and knowledgeable. My experience, except for Patsy Minsk, has been favorable. They wouldn't necessarily go out on a limb to protect us, but it was an entirely different feeling than with some of the people at those public meetings.

DOEL: Did you sense that any of the federal agencies that had been patrons or affiliated with the work, like NOAA or NSF, really came close to losing parts of their own funding because of the controversy?

MUNK: Well, Jim Baker was threatened with a cut by one of the people who was on the Appropriations Committee. I don't think it came very close to that. Of course some people in those groups really protected us. One of the curious things is that we had lots of opportunities to talk to groups and people, and in general even among groups at universities where I would have thought they would be antagonistic, people generally shared our concerns. The environment is important, and people who belong to environmental groups generally do it with the best of intention, but they are often poorly informed, and the people who lead them are often very much more interested in getting publicity than facts. I think there was a general sharing of that. I don't think in America as a whole, from what I have seen, the respect for the environmental groups is necessarily very high. We didn't hit a stonewall of people saying, "No, those are the good guys and you are the bad guys." But generally people were against us to start with. I remember I had

about four round-trip trips to Hawaii that year, and so I sat next to eight people going there and coming back. In each case when we started talking to each other they had heard about ATOC, and in each case they thought it was a bad thing, and in each case by the time we landed they didn't think we were so bad. And I said, "Oh, only a million more trips and I would have the majority on our side." [laughs]

DOEL: Did the controversy play out differently in other countries? You mentioned the Der Spiegel piece, which made me think about Germany.

MUNK: I don't know of a great deal of interest. The Australians of course were concerned about Heard Island, but were in the whole much more supportive than the Americans were, even though it was their island. They didn't seem to be as much concerned. Of course as you know there was an Arctic transmission, a loud one, a very good experiment and they had no, objection to it whatsoever.

DOEL: And the Arctic 8 nations were well aware of that work?

MUNK: Yes. I don't remember much international concern. I should try and think about this more carefully. There may have been some concern.

DOEL: But your perception is that, by and large, this really remained a U.S. issue?

MUNK: Greenpeace of course is very decentralized. They have Greenpeace in different places. Greenpeace Hawaii had its own rules, which were very different from Greenpeace California for example.

DOEL: That's interesting. What were the differences?

MUNK: Well, when Greenpeace California came to an agreement with us. That's when we finally got our permit. But Greenpeace Hawaii wasn't interested. They had their own plan. And when we finally got the Hawaiian permits from the Hawaiian government -- which generally is very environmentally friendly and started out being very negative on us, but eventually agreed that it was all right -- Greenpeace Hawaii came up with an article saying that if we ever started to put in the source they would raise a flotilla of boats and stop us. They didn't. By the time we eventually did employ the source it was no longer interesting news, so there was no flotilla of boats.

DOEL: When you were in Hawaii, did you meet with members of the State Government?

MUNK: Yes.

DOEL: The Governor, for instance?

MUNK: Well, not the Governor. We met with a man in the Governor's Office, yes indeed, and they were very interested in having Hawaii become a center of scientific environmental activity. So they were not unfriendly to us having a major activity in Hawaii. Then we met the Environmental Minister. There is an Environmental Minister in Hawaii, a young man, very well educated, I forgot his name. He was very kind in meeting us, he had a busy schedule, we met him at the airport in Kauai, he especially came early, had a very good talk with him. He eventually had to make the decision. So we had a good talk with him. He was careful, but he certainly knew what we were doing. And so the answer is yes, we met with lots of people. They were generally afraid of the environmental constituency. Patsy Minsk stands out as being in a class of her own on being negative without trying to find out what really happened.

DOEL: You felt that particularly after the meeting with her in the Washington office?

MUNK: Yes. She twice I had three dates with her. I really followed that up. The first time she didn't show up, the second time she canceled at the last minute, and the third time she came in and said, "Yes? What do you want?" I said what I just told you before, and she walked out, saying, "I'll look into it."

DOEL: And that was it?

MUNK: That was it.

DOEL: Barry Raleigh was director of the Institute of Geophysics at the University of Hawaii at the time. Were you in touch with him?

MUNK: Yes. And he was not interested. He certainly didn't oppose us, but he preferred not to be involved. We did ask him whether he would help us. There were some people at the University of Hawaii who did help us. There is one marine mammal man there, a very good man, who generally opposed us, but in a very sensible way. It wasn't one of these emotional things. And again, we could identify him. He's the one really expert man. Hi! [to Judy MUNK.]

Judy MUNK: I spent all your money. And we're going to give you a delicious piece of apple pie.

MUNK: Oh, oh. So, a mixed bag. [Returns to interview.] We have friends in Hawaii who really helped us. I mentioned John Craven to you before.

DOEL: Yes you did.

MUNK: He ran for Governor of Hawaii once, and he got all of 80 votes I think. But he is also a lawyer and an engineer and a scientist and a politician, and for a while he represented us as a lawyer in Hawaii, and knows something about it. He was in charge of this method of using the difference in temperature between the deep cold ocean and the warm upper ocean to generate

power. That was his project. So he had a lot of experience. And he tried to help us. It became very complicated. As I said, I hope someone like Cohen will write the story, because it's an interesting story, and I have all the materials. I kept all the newspaper clippings, and huge amounts of notes and writeups.

DOEL: It's very helpful, because clearly this is an important late 20th century story on the interrelation of science and society over environmental issues. And the Hawaiian incidents seem to stand out in your mind.

MUNK: It was the low point. The meeting in Honolulu, when the excitement was high and there were lots of people and I lost my voice and we did very poorly, it was the low point.

DOEL: You mentioned Judy was with you during those times. Did she take on any of the role of speaking for you when you had laryngitis?

MUNK: Well, she wouldn't do that in a public meeting, but when we would be with people standing around and shouting at each other she is quite prepared to speak her mind. [laughs]

DOEL: The Santa Monica City Council also passed a resolution condemning ATOC..

MUNK: Yes. And what is the name of the community north of Santa Monica where all the, on the sea, on the coast? Malibu. There is a man who lives in both Malibu and Kauai (Ray Chuan) who was one of the persons who appeared at all the meetings and opposed us. Judy, what's his name? He has a Chinese name. And he was one of, like Lindy Weilgart, he was always there, always opposed it. He got the Malibu City Council to pass a vote, again without talking to anyone but him, said he was a scientist and he understood it, and it was a bad thing to do. And I wrote back. I know some people who live in Malibu, and I said I'd like to appear before their Council and tell them what we are doing. The Mayor of Malibu actually telephoned me, and I said, "Could I appear?" They said yes, and we will let you know, and nothing ever happened. Do you think it's worthwhile? That's something that I would ask you as a citizen. But I was so annoyed about their passing a motion without any, obviously any insight. [? name of this individual? Is this Ray Chuan?]

DOEL: When you think back on it now, did the controversy affect your relations with SIO, with the university? Were there any lasting changes that came out from ATOC?

MUNK: I hope not. I'm not aware of it. Certainly not with the director's office. They were supportive always. I am not aware, it may have been that some Scripps people might have, there may have been two reasons. One is that I was not doing Scripps any good, and the other one is that they may have really had their doubts about whether we were doing harm to whales. I haven't heard that, and as a whole I think people stood behind us. Whether they thought it was a good experiment or not, I am sure they have divided opinion.

DOEL: Is there a consensus emerging in the marine biological community over what effects the experiment has had on either whales or sea lions or any of the potentially affected species?

MUNK: Well, what happened is that the agreement was that there would be an independent committee for which we would pay, but otherwise we would have no influence. It's a very good committee under a man named Bill Richardson. He's written a book on the effect of sound on marine life. Very, very good man. And they have taken a huge amount of data. That's why we are almost bankrupt. We had to fly for every transmission day with a plane to assess the number of whales in the area before and afterwards. That report is about to come out. I want to be careful not to talk ahead of them, but I have been to meetings where they gave their preliminary results -- this is Chris Clark from Cornell and Dan Costa from UC-Santa Cruz, who are the Principal Investigators. There is a great deal of data. It really was a major effort. And no indication of any significant effect by the sound. Of course it's always possible that there are subtle effects, long-term effects which you don't know from a year's data. There is one person in NOAA who says that there is no data that could be taken that would convince him that there may not be bad long-term effects. And we have no response to that. We have no long-term data. But as far as the short-term effects are concerned, did the whales leave the area as a result of it? Did any of them change their swimming? Did their heartbeat rate change? Were there some physiological changes? No such indication that I have heard of in listening to these accounts. So I think we came out that on our level of transmission it's not a problem. And in fact there were meetings because of the Navy's new program of considering whether they could work on a very, very much higher intensity, and it seems to me they got the permission. On that basis, we are really in the noise level.

DOEL: That's why I wanted to ask: does their report also consider other anthropogenic sources of ocean noise, and put ATOC into context?

MUNK: Into context? I don't know that. They know perfectly well that there are other sources. Indeed I don't know whether every part of NOAA knows what every other part is doing. In fact I know they don't. It's a big organization of different things that came together, and you know there was the Coast and Geodetic Survey, and the Weather Bureau.

DOEL: It's a major conglomeration.

MUNK: Yes, a conglomeration, and obviously you are not naive enough to think that's a well-coordinated group. It's not.

DOEL: Even in small organizations people don't necessarily know what happens in other branches.

MUNK: Right.

DOEL: One of the other issues you just raised about the project is that it is nearly bankrupt, or at

least quite low on funds because of the biological efforts.

MUNK: And the delay.

DOEL: Yes. What resources are there to compensate for all this?

MUNK: None. We are trying to find a new financial niche for the future now. As far as we're concerned we're out of business at the end of the year. We have data; it's interesting data. We have learned something about climate change that we could talk about in closing if you wish.

DOEL: I would like that.

MUNK: But as far as we're concerned, unless we find some new sources of support, the acoustic thermometry of ocean climate essentially discontinues next year.

DOEL: Of course it hasn't run long enough to evaluate the Greenhouse effect.

MUNK: We were more ambitious five years ago, but now we consider our function to test climate models on changes starting with seasonal changes, which we have done. We will be able to do year-to-year changes, with the idea that if a climate model doesn't predict the seasonal and year-to-year changes it's unlikely to predict the Greenhouse effects. So our goal is not to, at this time, to detect Greenhouse changes, but to help climate models to become good. I don't think they are good; they have not been adequately tested. Models obviously play a major role in any national policy because you have to depend on predictions which are done by models which have not been tested properly. So our role in that sense is more limited. It's working towards testing and improving climate models.

DOEL: When you think about the principle climate models that have been developed, the places where this work is being done, which groups stand out in your mind?

MUNK: Oh, I don't know. I don't think I know enough about it. We are working with the MIT model, which is one of the good ones. Various people have used some other models to compare our results with. I'm not the person to answer this. There are differences in resolution, number of layers, and this and that. Indeed lots and lots of differences.

DOEL: Yes.

MUNK: Our work has basically been to consider three sources of material: Topex / Poseidon altimetry, ATOC acoustic thermometry, and climate models for the same acoustic path. How do they compare? What do we learn by looking at those three sets of data?

DOEL: In thinking about your original intent with ATOC, and the current problem of funding, is it a question of ATOC as a technique, or is it the political controversy that you feel might prevent

there being sufficient funds for the experiment to run?

MUNK: Well, we'd like to think the political issue is gone. Biological tests which, as I said, have not been published but should be and will be, I think will show that it's not a problem. Except if you take an extreme view by saying that those things you could not observe may be harmful. We cannot talk about a 5-year effect because we don't know anything about 5-year effects. But otherwise, I think it came out well for suggesting that there is no significant effect and certainly no significant harmful effect. I think the matter of money is that we spent a lot of money, we got the results, and it's up to us now to find new sources of support, and we're going to try. I think we have a chance. And I think that what we've learned indicates that it shouldn't be the only way of doing it, but it is a powerful way of measuring large scale climate scale changes in ocean -- especially heat content, which is after all the primary climate parameter. It's the ocean heat content which governs the amount of changes in temperature you will get in the atmosphere. We measure that very well.

DOEL: I should note on tape that Judy MUNK has joined us again. We have been talking for the past few hours about the ATOC controversy. Clearly you lived through it. When you think back on the ATOC period, I'm wondering what images or feelings, recollections come primarily to your mind?

Judy MUNK: Well, I think it was absolutely ludicrous that somebody would botch the numbers. I sat through some hearings where Lindy [Weilgart] was there, and to hear her say that she couldn't have made a mistake because her husband did the arithmetic for her really ticked me off.

MUNK: Yes. Her husband. She said, "My husband is a Cambridge mathematician and he said my numbers are correct." Well, a Cambridge mathematician doesn't stop you from not knowing how to correct a mistake. By the way, the man's name is Ray Chuan, C-h-u-a-n, whose residence is in Kauai and in Malibu [? Nb -- is this the person also living in Malibu, mentioned earlier?]

Judy MUNK: He ought to be put into a mental institute.

MUNK: [laughs]

Judy MUNK: The other thing that made me kind of really upset is how these hearing processes go. You have 20 minutes to 25 minutes to state what you think the problem might have been, because you don't really know what it was, but might have been. Then you listen to people for 3 hours. Your main body of audience that you are trying to persuade is exhausted, they leave. And the kooks are left at 11 o'clock, and then they scream that well we've left. We're sitting right there in the front row, and they say well, you know, they don't care, they've left.

MUNK: We never left until the last person

Judy MUNK: We never left until the last person was gone. So there's no reasoning when there's

no format where you can answer a question when the people are still there that are really trying to figure it out.

MUNK: There are lots of people with great courage. There was at Harvard a young, very small woman who has studied the physiology of hearing in whales. This is her field, and she knows something about it. She stood up among jeering and said in no uncertain terms that that was a lot of nonsense. She had the courage to say that.

Judy MUNK: She had a little heavy bag that she brought up and put it on the podium. It held an eardrum of a whale. And she said, "In case you are all wondering what an eardrum of a whale does," and she described hearing with whale ears. And when she left, it was as though no one had heard her at all. So a man convinced against his will is of the same opinion still.

[end of Tape 5, Side A]

[beginning of Tape 5, Side B]

DOEL: Were there differences in the way that the politics played out in the late eighties and in the 1990s compared to the 1960s?

Judy MUNK: I have never been through those hearings before, so I don't know what hearings are like.

DOEL: Surely. I'm just thinking of the times that you were in other major political controversies.

MUNK: The Vietnam situation. There were some similarities.

Judy MUNK: Well, I listened to Walter when he was chairman of the faculty. There was a marvelously amusing discussion with the architecture part of the campus, because there was standing room only in these faculty meetings. Usually they're empty. Walter said well at least he's got a full house. I mean as long as you're going to do a job you might as well have a full house. There were some students that I was sitting in the back area. Somebody came in with a couple of cans of paint, and put them underneath the seat.

DOEL: I remember you mentioning that you had taken your cane and loudly noted

Judy MUNK: I took my cane and they had to give them up. So I just don't know how you manage things like that, except Walter was amazing because he'd get up and he'd say, "I'm a member of JASON. I worked for the Navy." He put it right on the table.

MUNK: Yes. That was what saved me in my inauguration talk as the chairman of the faculty. With everybody there, I had enough sense or luck to say this is what I'm doing and that's where I am. They couldn't say, "Gee, we have just learned that he is working for the Navy," or something

like that. It was a matter of record. I think that's the way to do it. I mean, it's not only the right way to do it, it's also the smart way to do it.

Judy MUNK: The other instance where something like that happened was in Boulder, when we had the problems with JASON. you and I went to a meeting. I said, well why don't we go see what they're talking about? So we read the newspaper, and it said they were having an underground meeting. So Walter and I went.

MUNK: I think we talked about that last time.

DOEL: Yes.

Judy MUNK: And luck was with us -- listen, we were the last people to be asked who we were. If we had been the first, we would have disappeared. But we were fortunate enough to be the last.

MUNK: Well, I think it's part of life, isn't it? Probably you should say that it was an enriching experience to once go through a public controversy, especially if you are on the negative side.

Judy MUNK: What I found so foolish about the men was you have such trouble describing that noise -- the noise that you were putting in the water. Everybody insisted you could put your ear in the water and hear it, and you never were successful in pointing out that it's like listening to the stars.

MUNK: To the satellites.

Judy MUNK: To the satellites. You have to tune in

MUNK: Yes. You know, you couldn't hear it against background noise. You needed to know the code to use it.

Judy MUNK: And you never got that across.

MUNK: No.

Judy MUNK: The other part that I thought was so foolish was that she did get the decibel units wrong, and you were never successful in pointing that out. Now I don't know whether you could have gotten that screaming memie that they brought in and turned on, and told them, "Well, that might be what that sounds like in the air," then done a demonstration like what's-his-name did with the O-ring.

DOEL: Richard Feynman, at the NASA Challenger hearings?

Judy MUNK: Richard Feynman. Right. See, that was something so simple. In hindsight I

DOEL: These kinds of images are very powerful, because they simplify stories down to critical elements. So this sort of technique was being used by the opponents? They played sounds at certain decibel ranges in the air?

Judy MUNK: Yes.

MUNK: Yes. At the one meeting we had

Judy MUNK: Two meetings.

MUNK: Two meetings. The man who is a diver brought in a record which had nothing to do with us, but which he played right in front of the meeting halls.

Judy MUNK: Full blast.

MUNK: So nobody could hear anyone speak.

Judy MUNK: It shook the whole building.

MUNK: Telling everybody that's what the whales had to put up with.

Judy MUNK: And the first time he did it, it was at the City Hall, the Convention Hall. And he had it out in the hall where everybody came in, and he played the thing almost 40 minutes into the hearings before somebody made him turn it off.

MUNK: Until somebody finally asked him to turn it off.

Judy MUNK: The second time he came into a hotel. People had heard it, and the judge -- well, the panel had heard it before.

MUNK: This is the California Coastal Commission hearing.

Judy MUNK: And they finally turned it off. But

MUNK: As far as newspaper reporters were concerned, that was the great fun, you know: there was the man showing what it sounded like to the poor whales. Whether it had any relation to that was not the issue. This was an illustration. I think we don't want to beat the thing to death, do we?

Judy MUNK: No, you just asked, that's all.

DOEL: Actually I wanted to simply ask you at this point, were there other parts of the ATOC experience that we haven't spoken about?

MUNK: We covered it very well. I'm just hoping that we have a opportunity to demonstrate what can be learned about climate change by long range acoustic methods. I think they have a unique opportunity to do that.

Judy MUNK: Let me ask you a question this way: If you'd had your, oh, four listening stations scattered around your

MUNK: Four?

Judy MUNK: Instead of just one. If you'd had the one in New Zealand and they had all been working, everything had been working, what do you think you would have discovered about the current El Niño?

MUNK: Oh, lots. Lots.

Judy MUNK: What?

MUNK: Oh, we would have observed it much earlier, we would have observed it with more precision much earlier. But that's not the fault of our position. Our source cable was cut, and just at the wrong time, just when El Niño was starting.

Judy MUNK: But that was already three years afterwards.

MUNK: Ah, that's much later.

Judy MUNK: What I am asking is, if you had been able to have it started when you thought it would start, and you had everybody in place, everybody was ready to go, and you would have heard through this whole sequence of this last weather pattern

MUNK: I think that would have been at least very helpful when we now try and raise support for continuing. I think we would get a very good El Niño signal. We haven't talked about it. Our Monterey source had the cable cut, possibly by a trawler. Trawlers are in the area, and cables are cut all the time as Bell Telephone will tell you. It took a while until we could get it fixed.

DOEL: When was it cut?

MUNK: Just when El Niño was starting last fall [1997]. It's now working. Then we had a failure, then we put the Kauai cable in. First we had a problem. We installed the Kauai source. There was an error made in installing it and it sank to the bottom. It had to grappled for, picked up, repaired, then it was working. Then about a month ago the cable failed ten miles away from

the source. That's now been fixed. But that took the remaining resources. So at the moment at least we're operational.

Judy MUNK: My question was quite different: if you'd had everything going three years ago, four years ago, would you have had a good ocean interior map, and had been able to work on the results long enough so that you really could have made some forward guesses based on that?

MUNK: We would have done very well on El Niño, I think so.

DOEL: Judy's question is a very good and important one.

Judy MUNK: Yes. That kind of hurts when I think

MUNK: It hurts this may

Judy MUNK: That's what hurts.

MUNK: We can't blame that on anybody.

Judy MUNK: No, no. Luck was not on our side. Whatever that word is. Just seemed to me we just hit that just wrong. Once things start to go wrong they seem to not correct themselves.

DOEL: One more question I wanted to ask in this session. When you think back, has there been any one principle that has been extremely important throughout your life, either religious or some other kind?

MUNK: Not religious. I'm not philosophically inclined, but I'm afraid that when somebody reads that long interview of yesterday and today it really sounds like a lot of preaching. Because there are some interesting things that really taught me a lesson. But I can't claim a guidance, something that my mother said or my father said or somebody else said that helped me throughout life. I think what did help me is that I have a good marriage.

DOEL: That's important, and clearly your optimism comes through in these interview sessions as well.

MUNK: And a wife who supports me. Not financially, but otherwise.

Judy MUNK: Oh no! You failed as an Austrian. No self-respecting Austrian would ever marry a poor girl.

MUNK: I told everybody I married an American heiress, and everybody believes it of course. It isn't really true. [laughs]

DOEL: Well let me thank you once again for this very long session that we've had today. You will be getting the transcript back from both Scripps and the American Institute of Physics, and we recognize that there are closed sections of this interview. You will see that before anyone else does. Let me thank you again, very, very much.