OCEANOGRAPHY PROJECT

WALTER H. MUNK

Project Coordinator: Robert A. Calvert
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While Walter Munk was working on his Bachelor of Science degree at Columbia University, he transferred to the California Institute of Technology for the simple reason that he did not care for New York. On the West Coast, he then continued his studies in seismology, a vague version of physics that placed him in the field. During his junior year, he met a girl from La Jolla, an acquaintance which brought him to Scripps Institution of Oceanography as a summer research assistant under Harald Sverdrup.

After a Master's in 1940 from the California Institute of Technology, Munk joined the Army in 1941. Shortly thereafter, upon the request of Sverdrup and Revelle, Munk left the Army to go to the Navy's Division of War Research at Port Loma. Here the three men worked on anti-submarine warfare, in particular the problems of the intensity variation of underwater sound. Out of this research group came a high-resolution thermography.

However, Munk lost his Navy clearance in '42. He then went to the Pentagon as a meteorologist to become the Directorate of Weather. It was this job which sparked his interest in wave problems, for, as the Directorate, his main concern was to predict proper weather for amphibious landings.

In 1944, his Navy clearance re-established, Walter Munk returned to Scripps with a research professorship. Then in '46, "Operation Crossroads" took him to Bikini to make a circulation study in the lagoon. Upon receipt of a Ph.D. from the University of California in 1947, Munk returned once again to Scripps with his interest in circulation problems, especially wind-driven circulation. He even went on sabbatical to Norway and later published a paper on wind and ocean circulation. This study rekindled his concern for waves, which eventually became the study of tides.

The age of exploration was initiated then with the directorship of Scripps falling into the hands of Roger Revelle. It was during this time of the major expeditions that Munk developed his interest in geophysics, the study of the planet Earth. Only to return later to oceanography, Munk is now concerned with acoustics.

Walter Munk feels that the common trait of a great scientist is very simple: that great scientists are formidable people. They have the courage to stand up for their own belief or idea, even if it's not popular at the time. He does concede, too, that curiosity plays a major part in all scientific endeavors.

Lucille Gates, 1976

TEXAS A&M UNIVERSITY
ORAL HISTORY COLLECTION
HISTORY OF OCEANOGRAPHY

INTERVIEWEE: Walter H. Munk

INTERVIEWER: Robert A. Calvert

DATE: July 8, 1976

PLACE: Scripps Institution of Oceanography

RC: First of all, what brought you to Cal Tech from Columbia and was your BS related to the marine sciences in any way?

WM: No, I left Columbia because I didn't like New York, and California seemed far enough away. And my BS was not connected to the marine sciences. I don't think I'd ever heard of oceanography.

RC: And you were doing what at Cal Tech?

WM: I had made up my mind I wanted to be in some branch of physics where you spent a lot of time out in the field; and I sort of chose seismology, in a vague sort of way. I was going to go on and study earthquakes and took my Master's, actually, in geophysics.

RC: But it says you were working as a potter.

WM: Oh, I worked as a potter one summer in the San Joaquin Valley, which is, you know, the practical aspects of doing geophysical prospecting.

RC: Does it seem to you that a sizable number of people move into the marine sciences and oceanography from seismology?

WM: Not at the time. I got acquainted with oceanography because I was a junior at Cal Tech, and I'd met a girl whom I liked who was spending her summers in La Jolla. And I came here because it was the only way I could spend the summer here and be able to afford it--the only job you could get. So, I asked Harald Sverdrup, the director, whether he would give me a summer job; and he did. And here I am, some years later.

RC: That, by the way, is what Revelle said, too...that he thought you came into oceanography for love of a woman.

WM: For love of a woman, yes.

RC: With whom did you work as an assistant here?

WM: Well, I came; and Harald Sverdrup, who was director, took me under his wing. There was a total of, I think, 14 employees including the gardener. And that summer and the subsequent summer and the year after that, when I really came down full-time, I was <u>the</u> only student; I was the student body of Scripps. I would be invited to parties so that I could be introduced as "Here's our student." singular; and it was great. And Roger Revelle, whom you spoke to, was an assistant professor. He was also very kind and thoughtful and helped me when I first tried to learn what it was all about.

RC: Did you have any intention then of concentrating on a particular interest in oceanography?

WM: Well, not for the first summer; I didn't even know what it was all about. Harald Sverdrup suggested I look at some new data that he had taken in the Gulf of California, which indicated some curious phenomenon called internal waves, which I seem still to be interested in, though in another context, many years later. And I looked at the data and learned a little about the theory. That was the first summer and second summer I helped Roger Revelle on some current meter readings and things like that. And so, it sort of grew out of that. But both Harald Sverdrup, Roger Revelle, and others--Dick Fleming--were very kind and helped me; and we didn't have the present problem of many students seeking recognition. There was no competition.

RC: Now we're talking about, roughly 1940, are we not?

WM: Yes. Well, I think my first summer year was '39, yes.

RC: And by 1940, you are, in effect, a research associate on the faculty here?

WM: Not on the faculty. I mean, I came the first summer as some sort of a research assistant at $50.00 a month, and I don't remember what my title was. And then we sort of very quickly went into the war years, which changed things a lot; but I didn't really have a very solid job here till after the end of the war.

RC: Were there any other oceanographic institutions you were aware of in 1940?

WM: Well, yes. As a result of working here, I became aware of Seattle, The University of Washington, and, of course, the existence of Woods Hole. I think that's about it. And then I joined the Army in '41 and served for about two years. And I forgot...when was Pearl Harbor? December....

RC: December '41.

WM: Well, I joined in '40, and I served until very close to Pearl Harbor. In a way, I entered the Army because I thought we would all be fighting very quickly; I got a little bored doing nothing. I was not aware of Pearl Harbor being in the works before it happened, I guess, as was anybody else. And at the time, Harald Sverdrup and Roger had decided to start some oceanographic work with the U.S. Navy and asked me to join them. There were very few people, you see, who had had any interest and background; and I requested that the Army release me to work on this because, by then, I was getting bored doing close-order drill. And I think three days after I left my company, Pearl Harbor broke out. I would, of course, have never left had it been at some later time.

RC: Okay, I have several questions here now. You're speaking of, really, leaving the Army to go to the Division of War Research at Port Loma, right?

WM: Yes.

RC: Okay. Now, I have your occupation listed in the Army as ski troops. Is that correct?

WM: Oh, I spent one winter with the ski troops on Mount Rainier in Seattle. I love to ski, and they were beginning to think about ski troops. This was before Mount Hale started, and I asked for permission to join and spent two rather interesting months with them. And then, however, when we went back and nothing was happening, I really got awfully bored with learning how to salute whom and when and thought I'd like to go and work on something a little more important. And so, when Sverdrup offered to have a special request made so I could join this oceanographic group, I was rather glad to do it.

RC: How many people were at the Division of War Research at Port Loma?

WM: At the time, I would think about 30 or 40.

RC: That large. Were you primarily....

WM: ...including other things in oceanography. I mean, we were a group. Roger Revelle was a lieutenant J.G. in the service then. And we started learning something about underwater sound and effects of ocean conditions on underwater sound. It was very interesting, indeed.

RC: That was primarily anti-submarine warfare?

WM: It was.

RC: And this was where the work from the deep scattering layer first appears?

WM: Yes. Martin Johnson, whom you might want to visit here, essentially at that time solved the problem of the deep scattering layer. It was found that there was a phantom bottom which was deep at daytime and shallow at night, and he immediately related that behavior to what he had studied--so-called diurnal migration of copepods--saying it must have something to do with diurnal migration of marine organisms.

RC: Were you primarily involved with waves?

WM: Not at the time. I was learning something about underwater sound; I was working on problems of intensity variations. I became interested in something which is now a fashionable subject, microstructure. I was wondering whether there were smaller scale structures other than the newly invented bathythermography by Spilhaus could resolve; and we did build a better instrument and found that there were smaller structures. And I worried a little about what it meant. But that subject was not to become fashionable until 1970.

RC: But you did make advancements on the BT there during this period?

WM: We did build a high-resolution thermography, and I started thinking about the effect of this kind of structure on sound propagation.

RC: Rather quickly though, you switched to a job of a meterologist in Washington.

WM: Well, I ran into a curious problem, which I have never understood. I lost my Navy clearance, as did Harald Sverdrup; and there are some people who know why. And it was a sort of a difficult experience; I've no reason why. And I was told that my difficulties would not extend to another service, so I applied for that job. I was given a job in the Pentagon and worked there for a while, and then the same problem happened once again. Eventually it was resolved, I was given clearance, and I don't understand what happened during that interval. But, it was the loss of Navy clearance which made me go and take this Pentagon job. Then I became interested in wave problems for the first time, really, when I was working at the Air Force office of Directorate of Weather, of the U.S. Air Forces.

RC: Did even then you seem to see some kind of connection between meteorology and oceanography?

WM: Well, that's how it came about. I learned about the forthcoming invasion of Northwest Africa; plans were being made. I also learned that the conditions under which the landing craft, the so-called LCVP, could come in without brooching was...I mean, these landing craft were so poor, in a way, and the conditions during the landing period so bad that it appeared that in two out of three days they wouldn't make it to shore; and you'd lose half your people by drowning before they even made it to shore. So, it became absolutely essential to pick a good day, or we would lose the invasion before it even started. And I think I'm responsible, then, for suggesting that it might be worthwhile to attempt to find whether you could pick the good day and so make a landing during favorable wave and surf conditions, and started working on it both theoretically and experimentally. We did work out a theory, and I started to look at some data in the Azores as sort of a verification procedure. Pan American had landed flying boats in the Azores and kept track of wave conditions, so I thought I'd use that as a sort of a test of the method--picking up the weather maps, picking up the storm fetches, predicting the sea and swell, and seeing how well did we do in predicting waves of the Azores as a test to whether the method had any validity for their use on the Northwest African beaches. There was sort of an amusing aspect to it. I remember doing quite well, except, once in a while, there was a huge spike in the wave energy in the Azores, which we did not predict; it was very puzzling. And I later noticed that they happened invariably on Saturday nights, so I talked myself into discounting those waves, those peaks. Then, however, when the landing situation came closer, someone from high up expressed some concern whether the work that I had done in attempting to do a wave prediction was all right. I don't blame them; I mean, so much depended on it. One could have possibly lost the whole landing, and I was a very junior man. They said they couldn't possibly build on this unless it was confirmed by more experienced people, and I requested that Harald Sverdrup be asked to come and give an evaluation. He flew in and spent two or three weeks working over what I had done, adding to it a lot and then saying that he thought it was substantially all right. Then the method was really accepted as a method for the prediction of sea swell and surf; and it eventually became the basis of predictions for all our amphibious landings in the Pacific, Africa, and even across the channel in European theatre of war; I think it worked well.

RC: Now, you didn't stay, though, with meteorology all during the war.

WM: Eventually, then, my clearance was reestablished; and I decided to come back to La Jolla and work back on a Navy project here at Scripps, which was closely connected to the wave problem. And then the war ended, and I received a research position here. I was terribly lazy on my Ph.D.; I wasn't really very interested in it because, by that time, I had really done a few things that were more interesting than getting a degree. Eventually, when people read me the riot act, I wrote a thesis. I think I wrote the shortest and quickest thesis in the history of Scripps. It had 12 pages, and I wrote it in one week when somebody said I just had to do something. It was accepted; I got my degree. It turns out the thesis was wrong, but nobody has any mechanism for removing a degree once it is received. And so, that's what happened.

RC: And thereafter, then, when you return to Scripps... We're now dealing in '44, are we not?

WM: Yes.

RC: Okay, you returned to Scripps in '44, and you became a research professor, but what about "Operation Crossroads"?

WM: Oh! Well, then in '46...I think it was '46, wasn't it, Crossroads?

RC: Right.

WM: Somebody came by and said they were going to make a bang in Bikini, and they wished to evaluate what the oceanographic consequences of a nuclear explosion would be--very exciting, I thought, and I volunteered to go. Bill Von Arx of Woods Hole and I sort of took over the circulation. As always, it happened too late. We were asked to go two weeks before the explosion, which is absurd. We flew out and attempted to make a circulation study in two weeks, the purpose being to evaluate how quickly the lagoon waters would be renewed following an explosion, how dangerous it would be to the native fish, and things like that. We decided we couldn't possibly measure the in- and outflow into the lagoon by normal means in two weeks. We got hold of a seaplane, a PPN, and we decided we would use aerial means of measuring currents; I don't think it's been done before. We would drop little dye marker bombs, little bombs that had this very intense green-red dye, out of the plane into the channels leading into Bikini Lagoon and photographed it from the air repeatedly with a little bit of land in sight so you could see which way the dye marker was drifting in and out of the lagoon. We would do that over a period of at least half a tidal cycle, so we could see whether there was a net flow in and out of the lagoon. And there were 18 channels leading in and out of the lagoon. We would work three a day and decided in a week that we should have some idea about the circulation in and out. I remember, to my great horror, that on the sixth day, we had measured all but one channel, the big one called Enu Channel; and, on each of them, the net flow of water was into the lagoon. And I'd learned some basic lessons, which are called mass conservation, that when water flows into some place, it has to flow out again; and in every one of the ones we had observed, the water flowed in and not out. We said, "So, what are we going to do if Enu also has an inflow?" which violated all principles of physics. So, the last day we worked Enu, and Enu had an outflow which balanced all the others. So, things worked out all right; we made some sort of a prediction. It was a very exciting time, and I think we probably did all right.

RC: Now we're making predictions on the....

WM: This was Bikini Baker, second shot.

RC: You were making predictions on the size of waves, were you not?

WM: And we also...that's another job. I also decided that most of the circulation in and out of Bikini was not induced by storms but by waves breaking onto the reef, building up a head of a water which caused the flow of water in and out of the lagoon, so that the entire northeasterly side.... The real pressure bringing water in that flushed the lagoon was by waves, not by wind--very curious. Eventually, I published a paper on that.

RC: And the predictions of the waves and wave size came out about what you thought they would?

WM: I don't know. We did get a time that it took to flush the lagoon. Gif Ewing from Woods Hole and I worked on that. I forgot what the time was to flush out the lagoon we came out with, but we came out with a definite number. I've been back, since, three times; it's kind of a wonderful place. My chief memory, first memory, of landing in Bikini was that there were so many flies; nobody could sleep. Then the Navy sent some planes over that DDT-ed the island--this was just when DDT was coming in--and two days later they were entirely gone. I thought...wasn't that wonderful! Now I understand DDT is no longer very effective because the flies have become accustomed to it, and we're going to have troubles having another Bikini test.

RC: Is it fair to say, then, that "Operation Crossroads" is what turned you into investigating wave theory so carefully?

WM: Well, no, my wave interest came through the invasion of Northwest Africa, entirely, and that's how I became interested in waves; the wave work at Bikini was a curiosity and not a very deep thing. No, it was entirely the landing and the operation of wave prediction which got me interested in that subject--the previous WWII efforts in wave prediction.

RC: Is it your impression that WWII is what began oceanography into the stage of science?

WM: Well, it was a huge jump. Well, you know, you shouldn't say that. There's lots of interesting and good work that was done earlier, but it was an enormous change. And the oceanography following WWII was very different from the oceanography before WWII. It certainly started what you might call Modern Oceanography.

RC: It has been suggested, in an interview previously, that possibly the innovations that came from WWII,plus the computer, have now moved oceanography to the point that maybe the age of exploration in oceanography is over.

WM: Well, it depends on what you mean. If you mean by "exploration" exploration in a geographic sense--going to new places where you haven't been and seeing what happens--then, I suppose that's the case. If you mean by "exploration" learning what the basic facts of life are, I don't think so. I think it turns out that so much of the stuff that people believed in is still nonsense, and some really very fundamental new principles are being uncovered. It was only at Mode three years ago that we learned that most of the energy of ocean circulation is in eddies whose size are 100 kilometers and whose period is two months, and this wasn't known. You can't think of anything more fundamental in describing the oceans than that.

RC: Now, after "Operation Crossroads," what happens to Scripps?

WM: I went back; and eventually...I forgot when I got my faculty position; it must have been later.... I became interested in circulation problems and worked for a while on wind-driven circulation. I went to Norway on a sabbatical and wrote a paper, that had some success, on the wind and ocean circulation, though it was not really a very significant improvement over what Hank Stommell had done (whom you hopefully have on your list of people to see). And then I think I went back to working on waves and became interested in successively lower and lower frequencies into wind-waves, and then to swell, and then to seiches, and then to tidal waves, and then into tides. In some sense, I followed the road downhill into lower and lower frequencies over many years. These are quite different subjects, you know; they might sound the same to you, to others, but they are very different. So it was, really, learning new things about new subjects.

RC: Now the faculty at Scripps Institution takes off geometrically, if I may use that word, after....

WM: It does. After Harald Sverdrup left and Roger Revelle became director, we had our great age of exploration; I mean, it sort of went from studying San Diego Bay to studying the coastal waters in the Gulf--under Sverdrup and Fleming--to studying the Pacific under Roger. That's quite a jump. (He used to say the Pacific was our oyster!) And we had our major expeditions. I became quite interested in geophysics rather than sort of traditional oceanography; and, really, for about ten years, I think, I did more work in the description of the planet Earth, in the broad sense, than I did in oceanography. I really returned to the womb only about seven or eight years ago, when I decided I really wanted to do oceanography again.

RC: Okay, now, was it the beginning of such expeditions as MIDPAC and so forth that turned you into describing the earth?

WM: Yes, I think so.

RC: That was what broadened....

WM: Yes. And yet in some sense, we missed some of our best bets. You may have heard about plate tectonics, the principal discovery of our generation. I looked over the expedition report of Capricorn the other day. It was a major expedition where we were gone for seven months--not like today, where you fly people out and fly them back two weeks later--but we all were on two small ships for seven months. And I, being a little bit of a jack-of-all-trades, kept the expedition report and wrote up everybody's work. If you really look back to it, we had enough information that we should have been able to write a substantial paper on plate tectonics, and we didn't.

RC: It was really Woods Hole, was it not, who....

WM: Who did the major work? I wouldn't say that. If you give major credit to who's done it, it would be...no, it would be Bullard in Cambridge and Lamont in the United States. In a sense, if you now read the Capricorn report, we should have known better. We had the heat flow values; we measured the sediments and found they weren't thicker than the 100 meters--something had to go and renew the bottom all the time. Roger even used the words: "There must be some fantastic mechanism that flushes them down all the time." We had enough, but we weren't bright enough. Then Vocquier took some magnetic readings. In fact, we took a magnetometer on those expeditions; but he took some magnetic readings from California and got these very strange offsets, which should have told the story. On the other hand, California's a lousy place to study, because it's complicated; it's not as easy as the Mid-Atlantic Ridge. So, we missed a little on that one.

RC: I have a couple of questions that you may wish to avoid here, but I hope not. Would you evaluate Roger Revelle as the Director of Scripps Institution?

WM: Oh, he was wonderful! He's a real leader, a man of great imagination, tremendous interest in people and why they do things; and he had all the courage and self-assurance to organize this expedition era. That takes a little bit of doing, I hope you realize. He lacked certain attributes that some people think are important, like having a polished desk and a prompt telephone record. No one who has any real depth in his feelings for what makes a man great in the subject would take these negative attributes too seriously. They can be annoying and they can be negative, but what good does it do to have an efficient man who doesn't know which way he's going?

RC: Roger Revelle says one time.... I'm going to ask you a two-part question, if I may. Roger Revelle says one time that he would like for his contribution to be known that he put Scripps at sea.

WM: He did.

RC: That he, in effect, moved Scripps out. Was there a feeling at Scripps, when he put Scripps at sea, that you were on to something imaginative and exciting and espirit de corps and these sorts of things?

WM: Not by two-thirds of the people who were there. You see, they had now survived for some years without a boat. They had been accustomed to the local fauna and flora, and they regarded this as an intrusion into their existence. And, I think, largely the group of people who went with Roger, at the time, were new people who came here. It's awfully hard to change old people in their way of life. I think one makes progress in science not by convincing anyone but by having those who believe otherwise retire.

RC: Do you feel a sort of nostalgia for the fact that oceanographers may not go to sea anymore?

WM: Oh, but I don't understand that.

RC: Well, now you fly out; you're there two weeks; you fly back. There's not the "getting your feet wet" sort of approach.

WM: I think it was exciting when people lived together in small bunks for seven months, and we'd have all sorts of human problems that were severe but wonderful. I think all of us who went through that era will think about it always. On the other hand, you can't fake it. And at a time when it's cheaper and better to fly people in and out to do a job than to keep them bottled up, you can't fake it. So, time's over; there's no reason for doing it now.

RC: The second part of this question is: you described Roger Revelle in 1968 as "the last of the great naturalists."

WM: Oh, I wouldn't do that.

RC: That was what I took from the notes from the article. Is that a mistake?

WM: Did I say Roger was "the last of the great naturalists"? Well, I don't know; I forgot now what article that is. It may be all right; I mean Roger's interests are catholic. In the meaning of the word "catholic," you know...broad. Maybe that's the right word.

RC: That's certainly the context.

WM: What article was that?

RC: It was, in effect, a salute to Roger Revelle in '68 as he's leaving to do other things. And I don't mean to imply naturalist a la Jacques Cousteau sort of approach; I mean naturalist in terms of broad training.

WM: He was the opposite of a specialist--a universalist. His basis was geology; but his interest was always in how that does relate to the biology and to the physics and that...and so, he's a universalist. He isn't a naturalist in the sense of a Darwin or a Sumner or a Redfield at Woods Hole, but he's a universalist--I don't know what words I used--the opposite of a specialist. Very broadly interested, he looks at nature, I think, in a sort of Conan Doyle kind of thing, as a puzzle--infinite number of pieces of various sorts which have to be put together. He's not very good at solving differential equations; but, who wants to solve differential equations? Once you understand the subject, you always find the mathematicians who will pick up the pieces. So, he is a broad person ideally suited to geology, which is sort of a field which had this kind of broad viewpoint. I didn't know I used the word "naturalist". "Naturalist" has a heavier connotation in biology than I would think today would be natural, but he was the opposite of a specialist.

RC: Do you think it is possible, or is it exceedingly rare, let's say, in the present education system in science to turn out these sorts of men anymore? Were they products of pre-WWII?

WM: The person you will talk to about this is John Isaacs. This is his number one interest. He will tell you yes. He says that we have forgotten to educate people to be broadly based. Of course, we oceanographers have always been somewhat better at this than, say, the meteorologists. At least most oceanographers can recognize a fish when they see one, and most meteorologists don't recognize a bird when they see one. I mean, we have always had a little bit more broad interest. But certainly the days of Alfred Redfield--my ideal of a naturalist--a Bigelow, these people who really had the broad physical, chemical, biological, geological basis of the oceans at heart, are terribly rare. Roger was one of them; John Isaacs is one of them; they're not many.

RC: Can a Walter Munk appear again that can sort of--I don't mean to use the word "leisurely" incorrectly, but rather leisurely--follow his scientific interest from spot to spot, as he acquires prestige and national stature, anymore?

WM: Yes.

RC: You think so. You don't think that the pressures of publishing and surviving in the academic world are such that he's shut off?

WM: No.

RC: Now, you wrote eulogies on the following men, which I read. What I would like to know is is there any common characteristic that makes these men themselves great oceanographers: Harald Sverdrup, Columbus Iselin,and Maurice Ewing?

WM: Yes, well, they're very different people. I mean, you write eulogies on people because you're asked to, you have a respect for them, and it's the tradition. They're very, very different people. I hope you don't ask me to compare them because I wrote eulogies on the three. That's an accident, I mean. They were more different than they were alike, and I never thought of it in the terms in which you asked the question.

RC: Okay, well, what I was hunting for is--and maybe the answer's simply no--a common characteristic that creates a great scientist. Is there anything except curiosity?

WM: That's a good way of asking it. I think curiosity is number one. Let me think of these three men. You're asking me about Sverdrup and Ewing and Iselin. I think I'd rather discuss that problem without regard to these three names; I mean, it's an accident that all of us do the job of writing eulogies. I think the main thing that makes scientists good scientists is because they are formidable people. I kind of think that no man who would have been a failure in other fields and a failure as a person could be a success as a scientist. I have met people, whom I obviously will not name, who were good scientists and had all the normal outside signs of success--like being elected to the Academy and having people make proper speeches about them--but who were petty people. And, I think they never made it to the real top in science because they lacked the courage when they had an idea which wasn't popular; and it's that kind of thing that is more a description of a man's character,than as to whether they are good scientists or not: Are you willing to stand alone? Are you willing to go in a direction that isn't popular at the moment, drop out of a field? Those are human attributes. You could ask that of a man who builds shoes: Does he have the courage to go into a new style, does he want to follow everybody else in the field? I think that's more important than anything.

RC: The next series of questions really refer to, as a matter of fact, some things that aren't popular. Did you ever feel uneasy about a close association with the military, as an oceanographer?

WM: No. It's unfashionable at the moment. I thought the U.S. Navy was a quite wonderful organization, and I've had some association with them all my life. And, I guess, one's feeling is that as long as we put out the money for a Navy, which is considerable, it's our job, all of us, to help make it a good one; and oceanographers are in a position to interact positively. I've always enjoyed it; I mean, one has had little battles with things, but I do it voluntarily and happily.

RC: Secondly, I want to ask you about the problem of radioactive wastes. This is.... I'd like your attitude and opinion on these, if I might.

WM: I'm a newcomer at that, and you shouldn't take my answer seriously. I don't know where you got the information; but Bruce Murray, who is now the Director of JPL (Jet Propulsion Laboratory), has just completed a sabbatical here in La Jolla, at our Institute, and used it to finish a few things before he starts running an organization of 10,000 people, or whatever it is, which is the end of any individual work. He suggested that we look into--and it's no more than that--the possibility of jointly, Scripps and JPL, to see whether one can do a better job of disposing radioactive wastes. And, as you know, there are different possibilities--on land, in salt mines, and other relatively inactive geological regions, on the sea bottom, in certain regions which are not biologically or physically active, and, possibly, in space. We thought that this is something worth doing; but we will spend the next six months, beginning now, to decide if there is really a problem one can profitably work on. So, it's quite vague, but in the next six months we will look into it. My own feeling is that disposal of radioactive wastes can be done properly. I also am totally convinced that most of the people who have worked on it so far were out to prove a solution they had to start with. I was dissatisfied with the arguments that were given. But I'm sort of a positive; I think that the disposal of the wastes can be done safely and well. But I haven't seen anything that I've read that seems satisfactory to show what these things really will do. I thought we could do a better job.

RC: Do you think, in terms of issues like nuclear plants, disposal of nuclear waste, or other issues involving now a combination of ecology and science,there is too much public opinion input into this decision?

WM: No, no. That's a good idea, to have as much public opinion as possible. I don't like the discussion of the subject that's been given in the last six months, from either side. There was, on one hand, the discussion by the people who initiated Proposition 15 and the conservationists, who'd obviously made up their mind it was wicked to do so and then pulled in facts from all sides to prove it. There was, on the other side, the lobby that was supported by the utility industry and others, which had obviously made up their mind the other way; and they were pulling people and facts to prove it. I thought both were equally irresponsible; and, I guess, if there's any interest on my part and some others, it's that we think that we have no particular reason to go one way or the other and that one shouldn't go into a subject to prove a point that you decided upon before you start.

RC: I now have a short series of specialized questions.

WM: All right.

RC: An article in 1952 concerned the absorption of nutrients by aquatic plants. It sort of appears from nowhere in your career--I don't understand--and is never followed by anything afterwards.

WM: It's been a successful paper, which it doesn't deserve to be. I think it was successful because most biologists didn't do the kind of simple calculations that were involved. Gordon Riley...was the coauthor?

RC: Right.

WM: And we simply wondered why most diatoms are 100 microns instead of 1000 microns, or 10 microns. The basic idea was: if you're too big, you have too much mass per area to absorb the nutrients; and if you're too small, you sink so slowly that you don't ventilate. Would there be a size in the middle where you have a relatively large area-to-volume ratio and yet still sink through the water at a rate so you can ventilate? We tried to formulate that problem. We said, "It's easy; we should be able to do that tomorrow." And you find yourself six months later still trying to formulate it. And we all learned something; but I think it will be done, or has been done, better since.

RC: Is your present direction of research carrying you into the utilization and harnessing of waves for energy?

WM: No.

RC: The things you were doing seem to hint around this issue.

WM: I don't think so.

RC: Well, I probably misconstrued it.

WM: John Isaacs is doing that; you ought to talk to him. I'm not an enthusiast for power from waves, and I don't agree with people who are. It takes 20 kilometers of coastline to get enough energy equivalent to a major power plant. I think it would waste our beaches, and I think it's not a good way of getting power. It might be a good way of getting power under special circumstances, like a sea-based instrument of a remote island.

RC: All right. Do we know enough about the ocean movements to carry out what Spilhaus calls "seaward expansion"?

WM: Like cities under the sea?

RC: Like cities under the sea.

WM: Isn't that a social problem rather than a technological problem?

RC: Yes.

WM: We've heard him, and we've heard others speak about it. But my feeling is that it's 90% social and 10% oceanographic. The question is: How do you move people? Do they like to live that way? I mean, I hope people do it just because it would be fun to see how well it works. I don't think it's limited by oceanography; I think it's limited by what we know about city structure and city government. That's closer to Judith's field. I mean, I know technologically you could do it.

RC: Okay, what about aquaculture of various kinds?

WM: I don't know enough about it.

RC: Has enough work been done in, let's say, in control of wave energy and breakers and so forth, so that this could be....

WM: I would think so.

RC: Again, I really only have a couple of more questions. Now, in terms of scientific accomplishments, if you had to name one single greatest scientific accomplishment, what would you name for yourself?

WM: I think the work on the rotation of the earth, which was not oceanography.

RC: No?

WM: It's hard to do. You know, I've been a jack-of-all-trades, worked on a lot of things. Wave prediction for a long time had a value, although it's done vastly differently and better now. I was pleased about the work on following swell around across the Pacific, because it was so romantic rather than good science. It was just so much good fun. I have a bad memory, and I always think the thing that's going on now is the most exciting. I find the acoustic work we're doing now as exciting as anything that I've ever had anything to do with.

RC: What about the future?

WM: Well, I hope we can do a good job on building some acoustic arrays and using it to monitor the oceans, which is our present effort. I think that's a good way of doing it, and it hasn't been done. I don't see why it shouldn't work. I'll make a statement of principle, if you have the time...

RC: Oh, yes.

WM: ...in which I have been different than most people. There's a saying that you teach to all your students: that you should know what the problem is and then you build your gear to measure it; you don't do it the other way around. I've always done it the other way around. People call it a "solution looking for a problem." Then I heard of some new way of measuring things that sounded kind of fun and wasn't trivial and measured something that wasn't trivial. And I said, "Well, let's go and use it and see what happens." instead of saying, "Here's a problem I just have to solve, and how am I going to go about solving it?" And I find it very successful to find a new technique which has something to do with a non-trivial problem and simply see where it leads you without having very specific questions ahead of time, which is what you're supposed to do. So, this is doing it ass-backwards.

RC: Ah, a different sort of scientific approach.

WM: Yes, the way you're not supposed to do it.