TEXAS A&M UNIVERSITY
ORAL HISTORY COLLECTION

INTERVIEWEE: Russell Watson Raitt

INTERVIEWER: Robert A. Calvert

PLACE: Scripps Institution of Oceanography, university of California

TIME: 9:00 AM

DATE: July 2, 1976

RC: Professor Raitt, would you first tell us when and how you first developed an interest in the sea?

RWR: Well, I think it began when I was doing part-time research at Point Loma, during World War II, when I was engaged in undersea warfare research, an attempt to do something about submarine warfare. Then it became clear that there was a very inadequate knowledge about the ocean and that some of my research ability could be useful.

RC: Now, you had earlier worked, had you not, in the engineering corps?

RWR: No, not in an engineering corps, if you mean in military.

RC: No, I mean as seismograph work.

RWR: I had worked for five years doing geophysical exploration, using seismic reflection technique.

RC: And from there, then, you went on to work in war research. What took you to war research?

RWR: I think it was the recognition that the U.S. would be involved in the war sooner or later, and it was necessary for people with technical knowledge to come to some help in dealing with the situation, particularly with the submarine. German submarines were sinking shipping across the Atlantic very rapidly. And, if it continued very long, England would have been completely isolated. They formed a laboratory down here at Point Loma, National Defense Research Committee. I heard about that through my

connections at Cal Tech. I joined them in the summer of 1941.

RC: What were you doing with Cal Tech?

RWR: I had some indirect association with them because my work in geophysics was working with a company in which the other members of the company were also graduates of Cal Tech; I had friends there. I had no direct association with Cal Tech, in the sense of being on the staff or anything like that.

RC: Did any of the work you did with the Geophysical Engineering Corps direct you toward the sea?

RWR: Yes, that was Geophysical Engineering Corporation. It was engaged entirely in doing exploratory seismic reflection work, looking for oil structures. It had nothing to do with work in the sea.

RC: Who organized the research after you arrived at Point Loma?

RWR: Well, the director at that time was Vern Newtson, professor of physics at UCLA.

RC: Did you propose the work you did with underwater sound and impulse waves yourself, or were they predetermined missions?

RWR: No. We were working on programs in the laboratory. The responsibility of the laboratory was to do whatever seemed most appropriate for our experience, and the problems most appropriate to learn as rapidly as possible those things that would be useful in undersea warfare. So I, particularly, was working on assigned projects, not things that I initiated myself. Of course, the ideas, how to do it, and so on, came from the people that were carrying out the work; but we generally worked on projects that were requested by the management.

RC: Did you notice a goodly number of people coming through this sort of work in the war to move on into the field of oceanography after the war was over?

RWR: Quite a few of them, like myself, whose interest in oceanography developed

through this work, came in from other fields but continued on in oceanography. Yes, I was not the only one.

RC: Do you feel that the connection between military and oceanography has been beneficial for oceanography?

RWR: I think so. The relationship with the military has been beneficial in a number of ways.

RC: Did you think, in your contact with the military, particularly later on, the military might have been too mission-oriented, vis-a-vis hard science, in terms of projects it funded?

RWR: That's possible about some of the work maybe. Some of the government R&D probably is too much mission-oriented. My contact is in two phases. One is that phase where I was doing work in wartime. The emphasis was, naturally, mission-oriented then; and it was only recognition of the deficiency of knowledge about the ocean that led me to continue, ~~later~~. Later, after the war, the work I've been doing is not mission-oriented at all. It's research, fundamental research. And there's quite a difference there between the wartime work and the post-war work.

RC: Would you comment on the discovery of the deep-scattering layer, speaking of pure research--how it came about, with whom you were associated?

RWR: Yes, I participated in that directly, and it came about because one of the first assignments I had was to learn about the scattering that came out of the sea, because this was directly interfering with the detection of submarines by echo ranging. And this is the scattering that comes through the organisms that are diffusely scattered throughout the sea, not just things like whales and fish and so on, that give discreet echoes, but that give a general scattering which sounds very much like the reverberation that you hear in an auditorium. When a sound is emitted in an auditorium, it's reflected back and forth and dies away. The sound that you get out

of the sea when you send a pulse into it sounds very much like that, so they call it "reverberation." So we were studying reverberation because it was interfering with the detection of submarines. When we directed an acrosonic projector into the ocean to study the volume reverberation, we directed it vertically downward in order to eliminate the reverberation that came out of the surface, which is separate from that to come out of volume. As soon as we did that, we found that the scattering was not uniformly distributed throughout the sea. Before that, everyone more or less assumed that there was something scattering that was uniformly distributed. As soon as we put it downward, we saw immediately that there were layers. That came very early in the study of reverberation out of the sea. So, that was, in a sense, a scientific discovery that was completely unexpected and a by-product of work which was directed toward the practical study of reverberation.

RC: You keep saying "we." With whom were you involved?

RWR: I was, as everybody else working there was, involved in some sort of group activity. The group I worked with was led by Dr. Iring. There were two Irings; I think his name was Carl Iring. He's a physicist from Utah, Brigham Young University. The other man was Ralph Christianson. Iring, Christianson, and myself were working together studying reverberation. So this was a joint activity; not any one person was responsible for it.

RC: Did you suspect at that time that it was caused by masses of fish and animals?

RWR: Yes, at that thime, there was a vast ignorance; and no one really knew for sure. Even then it wasn't certain what it came from: whether it was from organisms living in the sea or from some structure of the sea itself. However, I think it very quickly became apparent that it wasn't a structure

of the water itself,but it was the organisms. I think the fact of the deep-scattering layer was the strongest single thing showing that because the scattering layer existed fairly deep in the ocean, where the water structure is fairly uniform. And the scattering from areas where there's more layering, like the thermocline, was much smaller. So there are various things that indicated it. I think the strongest indication it was biological was that the scattering layer moved up and down diurnally. It came up at night and went down during the day, which is the well-known vertical migration of plankton and small fish and so on.

RC: Well, was vertical migration known at this time?

RWR: Yes, I think Martin Johnson was the one who was the most influential in pointing it out because he had studied the vertical migration of plankton by taking plankton net studies, and so on. So he was aware of the diurnal migration of the organisms of the sea. And he was also at the laboratory. He wasn't there quite as early as I was, but he came later on from Scripps Institution and joined the same laboratory. He's here now, by the way, if you want to talk to him.

RC: Did the discovery aid your individual research? I mean, did this discovery of scattering layers move you more firmly into oceanography?

RWR: Perhaps, yes. It was an exciting thing to happen; and certainly it makes the field more interesting when you find that, with fairly simple experiments, you can make fundamental discoveries. The job in scientific research is making discoveries. And so this is the kind of motivation that led me into oceanography. It was such a wide open field that one could go out almost any direction and discover new knowledge. So that was an interesting time to get into oceanography.

RC: It's been suggested that the 1950's and early '60's were, in effect, the "age of exploration," as far as oceanography was concerned. This might now

be closing off, that, quite frankly, more oceanography might be done away from vessels and on land, as oceanography becomes a more particular rather than broad field. Would you agree with that?

RWR: I think oceanography will continue a study of the oceans by ships going out into the ocean. The age of geophysical exploration of the ocean is still continuing, but it's past this initial phase. The thing that we got into, in a sense, is almost similar to the period of the 16th century exploration of the oceans that started with Europeans like Columbus and other people going out. But the explorations are still continuing, and new things are still being found. And I think it'll be a long time before you can study oceans by sitting at a desk.

RC: Some people have suggested that, to be a good oceanographer, it's necessary to "get your feet wet," to go out on vessels. Do you agree with that?

RWR: Maybe. It isn't necessary for every oceanographer to "get his feet wet," but some of them sure do. Oceanography is an experimental science. Everything that's been learned about the sea is learned by somebody going out into the ocean and observing it. And the people, however, that have worked with data, have made very fundamental and useful contributions to understanding the data that's taken by people that make observations.

RC: Why did you and four others form a Marine Physical Lab?

RWR: That's not an absolutely simple question to answer. There are aspects of that that involve the whole subject of why it was desirable to support research in oceanography. Marine Physical Laboratory was just one of a number of efforts to carry out this need for research in oceanography. And it came into being through Navy support and recognition by people in the Navy that more fundamental research was needed in oceanography. Marine Physical Laboratory was set up to emphasize those aspects of oceanography that are associated with a propagation of sound in the sea.

However, this was not oriented towards application of underwater sound, but rather to understand the fundamental properties of the ocean related to the propagation of underwater sound. The details of how it came into being is a fairly complex story, which I don't have all of the answers to, although I may have remembered it at one time in the past. But I think quite a bit of this is written up--the details of how the Marine Physical Laboratory came into being. The main person, of course, involved in that (it would have not come into being if he had not taken a very active and aggressive role) was Carl Eckert. And he was the important person in the Marine Physical Laboratory. I was one of the people that was fortunate enough to become associated with him in the formation of the laboratory.

RC: How'd you meet Carl Eckert?

RWR: I met him first in this wartime laboratory, which I mentioned at the beginning of the interview. I met him very shortly after I joined in the summer of 1941. He came from the University of Chicago.

RC: And then after.... Would you agree with this: As the war is winding down, it becomes apparent that more pure research has to be done in the sea. And, from this, then, the Marine Physical Lab evolves?

RWR: That was it. It was a strong feeling of a number of people, both in the academic world and in the Navy, that the research that really began during the war should be continued, particularly in those fields which had been opened up during the war but in which no work, really, could be done because the emphasis had, of necessity, to be placed during the war on the application of knowledge rather than on the acquisition of knowledge.

RC: The Marine Physical Laboratory--was it funded by the Office of Naval Research?

RWR: There was no Office of Naval Research at the time the Marine Physical Laboratory was formed, so the funds initially came from the Bureau of Ships.

RC: What types of information about the bottom were you able to gather as a result of techniques you developed with sound transmissions?

RWR: All of our knowledge of the ocean bottom is gained through that kind of technique, the use of underwater sound. All of the information comes by studying the reflection of sound, either from the sea bottom or from layers underneath the sea bottom, or by propagation through the sea bottom that we call refraction. So, basically, there are two fields of study here: one called the reflection of sound, the other refraction of sound. And this is what shows the association between the knowledge of the seafloor, structure of the seafloor, and the propagation of sound, which is that we learn a good deal about the nature of the sea bottom by studying the underwater sound. At the same time, one gains a good deal of knowledge about how sound is propagated by learning about what lies beneath the sea floor. It's a very important part of the whole problem.

RC: When did you come to Scripps?

RWR: Well, the transition to Scripps was a gradual one. I started at the end of the war in the Marine Physical Laboratory, which was a branch or a division or a laboratory of the University of California, separate from Scripps Institution. As time went on, the relation with Scripps Institution became stronger and stronger, and then--I can't remember the exact date--probably in '49, I got an appointment as associate professor at Scripps Institution. But my association with Scripps began before that. Although I really wasn't part of the academic staff or faculty of Scripps, I was still associated with them. I taught a course in underwater sound before I was on the faculty. So the transition was not a dramatic or abrupt one, but a gradual one.

RC: What happened to the Marine Physical Laboratory when you came to Scripps?

RWR: Well, it continued and still is in existence. And because of my residence

here on the campus, and other people's from the Marine Physical Laboratory, we have a branch of Marine Physical Laboratory here on the campus, as well as a continuing activity down at Point Loma.

RC: So you make a transition, then, from Marine Physical Laboratory to be, finally, full time at Scripps....

RWR: But I'm still part of the Marine Physical Laboratory,even though I'm a member of Scripps on the faculty. The research I've been conducting through the Marine Physical Laboratory, which is a research division,in a sense, of the Scripps Institution of Oceanography.

RC: Had you always been interested in teaching, or did the teaching at Scripps develop, let's say, in addition to research?

RWR: Whatever effectiveness I have in teaching is due maybe to graduate students learning how to do research from me. I'm not a teacher in the sense of lecturing to students, but rather in helping them with research problems.

RC: When you came to Scripps, how large was the faculty here? Do you remember?

RWR: That'd be hard for me to say. I think you'd better look that up. It would be much smaller than it is now.

RC: Well, it'll roughly quadruple while you're here. And as it quadruples, what happens in terms of sound transmission work in seismology? Do you seem to give direction to that part of the extension of the faculty?

RWR: The whole field has changed. My own involvement in what they call "seismic methods of study" came from the sound aspect. However, that's not generally true. Most people getting into this field do it the other way: from being directly interested in seismic work and then applying that to the ocean. So there is seismology involved here on the campus--more, you might say, of a classical seismology. Professor Bruun, for example, is a leading member of that field. He's been president of the Seismological

Society in America. And there are other people at the Institute of Geophysics and Planetary Physics (IGPP) who are prominent in this field of seismology, which is really dry land seismology, in a sense, but-- it evolved from the land--it's also getting more involved into the ocean. So there's approach from two different directions--the idea of sound propagation, on the one hand, and,on the other, you might call it continental seismology, earthquake seismology, and so on. Earthquake seismology is moving into the ocean. That's why the current development's very active. Some of the leading work is being done right here at Scripps Institution, up at the IGPP, in that they're putting instruments out on the bottom now, instead of just having them on dry land. They're putting seismographs on the bottom to study earthquakes.

RC: Usually you think of a scientist working out of a very traditional department, that is, department of geophysics, or what have you. But here there seems to be a mixture of scientists working in and around the ocean. Is that of particular value to a scientist doing research?

RWR: Sometimes, Yes. The value, I think, is from the sharing of information from various disciplines. So that a scientist at Scripps.... I think this is true in general of oceanographic institutions: the science is not as compartmentalized in oceanography as it might be in a more traditional university where you have departments of physics, chemistry, mathematics, in which a man might carry out his entire scientific career in one fairly narrow field. Oceanography doesn't have these sharp boundaries. One sees that right from the beginning, when you go to sea and share the use of a ship with many other disciplines. That, in a sense, is an advantage, a broadening experience, to work in an oceanographic institution.

RC: Now, were you the driving force behind the operation MIDPAC?

RWR: I wouldn't say I was the driving force. I think the most important person in MIDPAC was Roger Revelle. He was the leader of the expedition. I

was chairman of what they call the Joint Planning Committee. We wanted to have a two ship expedition. That was mainly my interest because I wanted to do seismic refraction work which requires two ships. Scripps Institution only had one ocean-going ship; that was the "Horizon." There was a similar interest in doing oceanographic exploration of the geophysical type at the Naval Electronics Lab. We got together largely through the efforts of Roger Revelle and formed a joint expedition of the two laboratories. My function there was to be chairman of a committee, where we met about once a week for several months before the cruise, to discuss what we were going to do.

RC: Was the MIDPAC Expedition related to the later NORPAC Expedition?

WRW: I don't believe so, except the name. NORPAC was an area describing an expedition studying the North Pacific. The MIDPAC meant studying the mid-Pacific. But I don't believe the two were directly related. I had nothing to do with NORPAC.

RC: What sort of information about the bottom was retrieved in the MIDPAC Expedition?

RWR: There was a variety of projects studying the morphology of the ocean bottom through echo sounding. We were doing studies of ocean bottom structures through seismic refraction methods. There was the first expedition to study heat flow, also. Of course, bottom coring was very important. The first time we used the piston core that was kind of a new device at that time developed by Hoolenberg, a Swedish oceanographer. So we had piston cores; we had gravity cores, taking samples of the seafloor, taking temperature probes, and a variety of things. It would be hard for me to, informally like this, really summarize the whole thing. In a way, it was a model for many expeditions to come later on--the Capricorn and other expeditions.

RC: That was exactly what I was interested in--the idea of it's being a model for later expeditions. That's how it's usually characterized. What did it do in your particular research? What direction did it take you after MIDPAC?

RWR: Oh, I think it demonstrated to us for the first time that we could go out and use seismic refraction methods to learn about the structure down through the upper mantle. So there was quite a lot of new information gained in MIDPAC, which certainly justified us continuing it to other parts of the ocean to see if what we learned on MIDPAC was typical of the ocean and what the differences and similarities were. So, in that sense, the success of MIDPAC did stimulate continuing work in my field, as well as in many other fields, like the study of the heat flow from the ocean bottom, for example. Initial results on MIDPAC were very important in encouraging further work in that field.

RC: Do you enjoy the actual expeditions of oceanography more than the theoretical and laboratory work?

RWR: Yes. I enjoy being out at sea more than interpreting the data.

RC: Now, what do you consider your most important accomplishment in oceanography?

RWR: I guess maybe what I've done in the general field of mobile hunting, or seismic refractions, studies of ocean structures, in terms of the length of time I've spent doing that and the quantity of data in the archives.

RC: Would you explain what "elastic anisotropy" is?

RWR: I guess you're referring to something that is really part of this study I'm mentioning of the ocean bottom structure. In that study, we measured the velocity of the waves under the ocean bottom. And the objective of this work has always been, mainly, to achieve penetration through the crustal rocks which form sort of a thin veneer over the bulk of the earth, to penetrate through that crust into the real earth material, the mantle.

The great bulk of the earth is called the mantle. That's this solid exterior of the earth that's about 3,000 to 4,000 kilometers thick and lies above the molten core. We found that the velocity of this mantle--the upper part of the mantle, right below the crust--depended on direction of travel. In other words, it was not isotropic. "Isotropic" means that the properties are the same regardless of what direction. It was anisotropic, and a good deal of our work here in the last two years had been studying the anisotropy -- attempting to measure it and, if possible, learn something about what causes it.

RC: Well, what did you hypothesize about the upper mantle of the ocean, in terms of exhibiting it? Was there any clue you had that the upper mantle might exhibit this?

RWR: The idea that it was anisotropic came not from our work, but from other people. The principal one here is Harry Hess who studied anisotropy of the rocks that are believed to be typical of the rocks that come from the upper mantle. He was a very broad-thinking geologist, but one of his specialties was studying these rocks in a microscope. These were samples of rocks that came from various places where it was believed that they were extruded by volcanic process from deep down in the earth and, therefore, represented the pure mantle. When you look at them in a microscope, you can see that the grains in a specimen--say, a small specimen the size of a few centimeters--are not oriented at random but lined up. This interested him many years ago when he studied this anisotropy in the little samples. So, he was aware of this anisotropy in small samples. The thing that was startling about this anisotropy was that it occurred over long distances--hundred of thousands of miles they are lined up. It wasn't surprising to anybody that you'd find this anisotropy in a small material, because the crystals that make up the type of material that forms the earth's crust are anisotropic crystals. They don't have a simple,

cubical structure, but have a more complex structure. So both the index of refraction (that's the speed of light to the crystal) and the speed of sound (speed of elastic waves) both depend on the direction of propagation through the crystal. So, if the crystals are lined up over a long distance, then you get an effect of velocity being in different directions, ~~then~~, similar to what we observe in our seismic experiment.

RC: Well, how is this related to seafloor spreading?

RWR: An experimental relation that we have found is that there is the direction of maximum velocity in the direction that the seafloor was spreading at the time the rock was formed. So, the obvious conclusion here is that the process of the rock being formed at the time the ocean bottom was spreading apart.... In the process of formation, the crystals are lined up. The details of how this happened are not known, but there is a clue to that in the observation that the direction of maximum velocity is in the direction of spreading. And we observed that first in the area right off California, where the sea bottom directly west of here is formed by east-west spreading, roughly (it's not exactly east-west). And we found that, when we first observed this, the maximum velocity was, roughly, east-west. And it turns out that,if we compare it in detail with the estimated direction of spreading, then it agrees almost exactly 90 degrees. By making this first statistical study of all of the stations within the area and comparing the velocity, its direction, with the direction that it was moving at the time it was formed..... So this is all fairly recent, too. It's all in the process of.... In fact, we have a paper in the press right now that's just being submitted to the Geophysical Research Letter giving the latest study of what I'm talking about here. It's a fairly complex study. It's going on now, and all the answers aren't in yet.

RC: But this has been, generally, the continuous direction of your work the

last several years?

RWR: Anisotropy is part of it. The study of velocity structure and how it's related to the geology of the area, is continuing also. Anisotropy is certainly part of it.

RC: How did you, as an American scientist, find yourself on the Russian vessels, and how well did you function there?

RWR: You seem to know quite a bit already. I have been invited to participate on some Russian oceanographic ships. I guess my acquaintance with Russian scientists, initially, is the reason I'm in it. Actually, it was associated with anisotropy because both times I went aboard a Russian ship, they were conducting experiments to measure anisotropy. So these were people that I knew previously, before I really got into this field of the study of anisotropy. The field that we're in, you see, is an international field in which many countries participate. And the Russian geophysicists, oceanographers, are very active in it, very well supported by the Soviet government.

RC: How did you meet the Russian scientists?

RWR: There were two occasions where we had good contact. One was a meeting which took place at UCLA in 1963 to discuss results of the International Geophysical Year. This meeting was held just before the international meeting of the IUGG (International Union of Geodesy and Geophysics), which meets periodically every three years. And, in 1963, it met at Berkeley. Before that, there was a meeting at UCLA to discuss the results of the International Geophysical Year. The Russians sent large delegations to both meetings. It was at a time when Russia was opening up, sending people abroad for the first time. And a number of people that I knew only through scientific reputation came to this meeting. Some of these people were people who were participating in this oceanographic

work that I participated in at sea with them. So my direct contact with them began here in 1963. Then, later they had an International Oceanographic Congress in Moscow, in 1966; and I went to that and made contacts again with some of the same people that came over here in the summer of 1963.

RC: And from that came the suggestion that you go on this vessel?

RWR: Well, then I was invited, actually, by Oudensev, an oceanographer at the Institute of Oceanology in Moscow, who was one of those that came over in 1963 that I met at UCLA. As a matter of fact, I invited three of them to come down with me to La Jolla, and they visited me in my home and got to see Scripps Institution of Oceanography. And then the three Russians, my wife, and I drove up to Berkeley to the meeting. It was not only a good scientific contact, but a friendly one as well; so we remained friends. And the principal one is a man, Gleb Oudensev, who has been a leader of quite a number of Russian oceanographic expeditions. And he has invited me to go on two of them. Well, he's invited me to go on more, but two of them I was able to.

RC: Did you have any problem ,by the way, receiving clearance to go on the oceanographic vessels, the Russian oceanographic vessels?

RWR: I suppose that there may have been a problem in, let's say, a local Russian problem in issuing me the invitation. I don't know. I was never aware of that. I usually received the invitation through some official letter through the Academy of Sciences or through the Director of the Institute of Oceanology, or something like that. But, previously, I always had sort of an informal invitation. Oudensev travels quite a bit; he visits. For example, he is the Russian who is essentially in charge of the Russian participation in the deep-sea drilling project. You're aware of deep-sea drilling project?

RC: Yes.

RWR: Russia is a participant in that; they contribute one million dollars a year, or something like that. They participate not only as scientists that go aboard the "Glomar Challenger" on these cruises, but they also contribute money to it. This man I'm talking about, that's my friend, is the principal Russian dealing with the Russian part of participation in that deep-sea drilling project. So he comes here fairly frequently, not only being on the committee, but because he has participated in the drilling legs of the deep-sea drilling project. So, some of these invitations I get come informally to me when he comes through. He says there's going to be an expedition; so-and so's going to be on it; would I be interest in joining it? Later on then, I may get a letter from...some official letter inviting me. There may be some problems with the Russians, but I was never aware of any problems.

RC: Do you speak Russian?

RWR: I don't speak Russian, no, so I do have difficulties aboard ship, however, not very serious, because the Russians.... It's really necessary for them to know English. In order for them to publish and have their work known abroad, which they're very anxious to do, they have to have some knowledge of English, not only to have their articles printed in foreign journals, but to be able to keep up with the literature in this field. It's obligatory for them to know English. So, I have no trouble communicating with the scientific party on the ship. My main problem was with the crew. Very few crew members could speak English, but with the scientific party there was no problem.

RC: Do you notice any particular difference in approach to science and oceanography, from the point of view of the Russians vis-a-vis ourselves?

RWR: Oh, I guess so. Every nationality has some difference in attitudes

toward things. Not basically, though. I think what we're doing, what the Russians are doing, is an international science. And I think we're technologically ahead of the Russians in many ways; we have much better computers than they do, for example. But basically it's the same; we're all working the same field. There's no real, fundamental differences. I go aboard their ship, and they're doing the same thing. They're towing long streamers behind the ship, They have air guns banging away, and they have magnetometers going, And they're shooting explosives through seismic refractory things and measuring heat flow in the ocean bottom--they're doing the same thing and with pretty impressive facilities, too. I mean, a lot of support, big ships, 70 people in the scientific party. There's a lot of similarity and differences, too. There are bound to be differences.

RC: It's been suggested to me that, possibly, the oceanography projects in the Soviet Union are better funded than ours. Would you agree with that?

RWR: Well, in some sense, I suppose, relative to the per capita gross national product, I'm sure it's better funded. They have these big ships that certainly cost a lot more money in dollars to operate than ours do that are much smaller, and they have a large scientific party, and so on. It's very hard to compare funding in the Soviet Union with funding in the U.S. in terms of dollars and rubles; but I think in terms of man hours expended in the field, relative to the per capita gross national product, it's more than we do. Yes, I don't think they get any better results, probably not quite as good.

RC: Why do you feel as if we get as good data from our vessels as the Soviet Union does in terms of funding of expedition, size of expedition? Is there any explanation for that, or have I inferred wrong?

RWR: I find it difficult to make this kind of comparison. I'm frequently asked to compare the Russian with the American. That's a question that

intrigues people much more than comparing the U.S. with England or France, and so on. I find that comparison difficult to make. That's the point. Some people find it easier than I do. People are interested in the level of technology, in the magnitude of the achievement, and the quality of the work, and so on. To me it's not an easy question to answer, not conspicuously one way or the other. It depends on the individual situation.

RC: Do you have any views on the role that sea conferences have played in bringing together international cooperation?

RWR: You mean international meetings?

RC: International meetings, scientific exchanges. . .

RWR: Very important. More so in oceanography, I think, than in many other fields because the study of the oceans is very much an international field in which many countries share in exchange of information dealing with the oceans.

RC: Do you see the pushing of nations' boundaries further into the oceans as a threat to your particular academic discipline?

RWR: Well, it makes it more difficult. Yes, oceanography is getting more involved in problems of the jurisdiction over the sea.

RC: Do you feel as if, perhaps, that these international conferences will aid in this sort of thing--we being allowed and they being allowed to work closer to the continental shelves or closer to boundaries?

RWR: Well, in principle, it could. At the moment, it isn't obvious that it is helpful.

RC: The next series of questions I should like to ask have to do with your opinions of education in oceanography and possibly the future of oceanography. Do you see any marked change,in terms of education of oceanographers,that has occurred in the last 15 or 20 years?

RWR: I think there's always a continuing change in education, in the sense

that there is no dogma, no routine. The whole oceanography is a developing field, and the education of oceanographers goes along with the development of knowledge. So, it's continually changing; that's basic. I'm not sure that answers your question.

RC: Well, do you see the rise of oceanography departments as specific departments in academic institutions as, essentially, a good thing that's occurred in the last several years?

RWR: Well, yes, in the sense that it's led to an increased knowledge of the ocean.

RC: I'm struck by the way that the original development of oceanography had to do with people drifting into oceanography--if I may use that metaphor--from other academic disciplines and sort of developing from geophysics, from work in seismology like Professor Shor next to you, and moving to a place like Scripps Institution and creating Oceanography. And with the impact of ONR, particularly, and NSF after the war, there have been developed specialized departments of oceanography.

RWR: Oh, yes. Along with this development of oceanography, there's a continuing flux of people from other fields--from physics, chemistry, mathematics, whatnot. There's a continuing flux of people from those fields into the oceanographic field. That hasn't stopped. In other words, people that are in oceanography don't come exclusively from these oceanographic departments. So ~~that~~ the existence of departments of oceanography and degrees being given in oceanography, to me, is not a fundamental change in the situation of education in oceanography; in part, because the people within the department of oceanography, the people that work within the departments of oceanography, themselves, are working in disciplines similar to others in non-oceanographic departments. They tend to be chemists, geologists, geophysicists, physicists, working in

oceanography and continue this kind of disciplinary studies right within the department. So the existence of departments of oceanography, to me, is not a fundamental departure or a fundamental change in the science of oceanography.

RC: Is it possible that there are too many departments of oceanography now?

RWR: I don't think so. The growth of any field is better in many ways, if growth by proliferation of institutions rather than just a few of them growing to colossal size. One of the problems, I think, in oceanography is that ships are very expensive, and small oceanographic institutions would have a greater difficulty in keeping a ship going. Even large institutions like the Scripps Institution (this is one of the largest, possibly the largest) has difficulty maintaining its fleet of ships. But there are some advantages, don't you see, in small institutions.

RC: Such as?

RWR: Well, there's the advantage of simpler relations between the people involved and a more intimate contact among your people. When things get large, they get problems just because they are large--problems of administration, for example.

RC: Was the establishment of the University of California at San Diego beneficial for Scripps?

RWR: I think so, yes. That has the contact with an expanded university has been beneficial to Scripps, I think.

RC: Then there seems to be some perfect size you're speaking of. In your opinion, is Scripps Institution now too large?

RWR: I'd hate to make an opinion about that. Some aspects of it are too large. I look with some nostalgia back in the period when I knew everybody here. Now I don't. I go to a gathering of people that they tell me are all from Scripps Institution, and I may know about ten per cent of them.

So I sometimes think of the good old days when I knew everybody.

RC: The ocean has been looked upon, fortunately or unfortunately, in the '60's , as a possible solution to man's problems in terms of food, mineral resources, and so forth. Are you optimistic about that sort of approach to the ocean?

RWR: I'd say you're getting into a subject that my opinion's not worth much. Yes, I think there's been, maybe, too much optimism about what some people almost think of as the inexhaustible resources of the sea. Some of those resources, I think, we're already pushing pretty close to the limit--some of the fishing in the sea, for example. Pretty clearly, many things that, at one time, looked like they were inexhaustible in the sea are now quite clearly not inexhaustible. No, I don't look for a real tremendous development where the sea could provide almost limitless possibilities. There are some resources of the sea not being utilized yet--for example, on the sea bottom. There are projects for mining the seafloor, the deep seafloor, for example. I think you'd better ask somebody else about that deal.

RC: What do you see as, may I call it, the future of oceanography? I mean, what direction do you see your particular discipline taking in the immediate future?

RWR: Oh, I don't see it. That bothers me a little bit. The future seemed very clear to me at the end of the war, say in 1946. The immediate future seemed very clear to me at that time. It doesn't seem so to me right now. Maybe that's because I'm old. Let younger people think about that. I don't really see it very clearly. There's always a hope, I think, that there'll be a breakthrough or some unforeseen development. All the exciting things that have happened to me in my oceanographic life couldn't have been foreseen, couldn't have been thought of, because

they were discovered in the process. So, you might say the things that seemed clear to me weren't that I predicted or imagined everything that was going to be discovered, but what I meant was "clear" was the way to go.

RC: And now it seems to you as if oceanography is spreading in many particular directions?

RWR: I think the reason it isn't clear is that it's much more complex, and we're utilizing facilities and ideas that are much more complex than the ones we worked on 25 to 30 years ago.

RC: Now what are your intentions in future research?

RWR: It's hard for me to know how to go. I'm retired, you know--about a year ago. I think, maybe, one of my major responsibilities right now is sort of cleaning house, in a way, sort of winding up some of the things I've been doing and trying to throw away a lot of the things that aren't needed to hang on to-- some of the things that we've acquired in the process(and I think that this is true in many fields of oceanography), in a sense, archives. The biologist may have acquired a whole lot of specimens, things that they put in bottles of formaldehyde; and geologists collect rocks off the seafloor and also cores of sediments. These are archives which are useful for people later on. The question comes up that can be solved by reference to these archives of material, cores that have been studied in a preliminary way, that contain information that no one dreamed, maybe, was important at all at the time they did it. And our work here has similar archives. We have records, seismic records, that are continuing to be useful. Many people come to examine details that we didn't have time for or weren't even aware that they might be important. So, I have some interest in cleaning up the archives so that the material that's useful will be retained, and the stuff that isn't, that's just in the way, will be thrown away. In addition to that, I'm still somewhat in demand to go to sea. I participate in continuing work on contract

proposals as an investigator. I continue to do that. In fact, I'm on two expeditions: one going out in September, and I'm also going out in January.

RC: I hope the one in January is to the south!

RWR: No, it's almost to the same place. It's going back. The ship will still be there, you see. It costs a good deal of money to get a ship out into that area, and the most interesting parts of the world to study are far from Scripps. That isn't entirely because we've studied everything close by and we have to go further. There's just a whole lot of interesting geology and geophysics in areas like Southeast Asia, which is a fascinating, very complex region--so complicated that it's only recently that people are getting into it because it's very hard to understand. The region that we're immediately adjacent to in California, the region in the ocean right off here, is a simple, comparatively simple area in terms of the field that we're doing, the geophysics of the ocean bottom. So we have our ship out there now. It's in Guam, coming into Guam very soon--July 5. And then it's going to be out there, oh, for another six months on various projects before coming back to San Diego.