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OCEANOGRAPHY PROJECT

JOE REID

Project Coordinator: Robert A. Calvert
July 1976

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Joe Reid describes his interest in oceanography as having started during World War II. As a navigator of a destroyer escort, he noticed that the charts the Navy was using were obsolete. Consequently, after the war he set out to revise and update the charts, an idea which never really materialized.

Actually, though, Reid's formal study of oceanography did not begin until 1948 at Scripps Institution of Oceanography. Having completed a B.A. at the University of Texas in mathematics in 1944 and having seen Scripps during his travels, Reid came to the Institution as a graduate student in 1948, when Scripps' policy was that everyone went to sea.

Reid then became associated with the Marine Life Research Program, a state-funded program to study the marine resources off the coast of California. With this as a springboard, and presently serving as its director, in the last 20 years he has expanded his studies of the distribution of properties in circulation and current into the whole Pacific and North Atlantic.

As a descriptive oceanographer, Reid has organized, planned, and/or led several expeditions. One of these was in the Northwest Pacific. Here he studied the circulation of the ocean during the winter with respect to the wind-driven parts, as well as those driven by thermohaline processes. There was also the NORPAC mission in 1955 that was conducted as an international oceanographic effort.

Professor Reid also has an opinion on the trend in physical oceanography. He contends that the decline in this area stems from the attempt of the movement in the early 1950's to encourage theoretical work as opposed to observational endeavors. However, the sole hope for descriptive oceanography lies with the geochemists, who are working with isotopes, according to him.

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HISTORY OF OCEANOGRAPHY

INTERVIEWEE: Professor Joe Reid
INTERVIEWER: Robert A. Calvert
DATE: July 16, 1976
TIME: 3:00 PM
PLACE: Scripps Institution of Oceanography, La Jolla,
California

RC: Professor Reid, my first question is how do you explain your developing interest in oceanography?

JR: I suppose this came from World War II. I was in the Navy then. I don't at the moment recall just why I was in the Navy rather than some other arm of the service, but I was. Among the positions I held from time to time was a navigator of a destroyer escort; and we worked in the Mediterranean, the North Atlantic, and western Pacific for a while. And in the western Pacific, I noted we were navigating from charts prepared by the Germans, in 1895 to 1912, when they had a colonial empire there, which they lost, of course, in World War I. And these charts were not complete. The Germans usually do a very good job of this. But their colonial empire didn't last very long, and they had by no means completed the job. And when the war was over and I decided not to become a lawyer, as my father was, I thought it would be nice to take part in recharting those waters for bottom topography, reefs, positions of islands, and the like. And I wrote a letter to the Coast Survey asking for information on this, and they wrote back saying that they didn't do that part of the world. They were limited to the coasts of the U.S., but they forwarded my letter to the Hydrographic Office, who did offer me a job. But they were not doing the southwestern Pacific; they were doing the Philippine islands and the Aleutians at the time so I didn't get into it. But a couple of years later, after knocking around the world for a while, I heard about the Scripps Institution of Oceanography. I came and looked at the place and decided I would like to enter a formal study of oceanography. And I became a graduate student in 1948 and have been at Scripps most of the time ever since.

RC: Okay, I have several questions here. You took a B.A. at the University of Texas in 1942.

JR: That's right.

RC: What was that in?

JR: Mathematics.

- RC: Math. That's what I thought. It was in mathematics. When you become interested in oceanography, you don't really have any training in physical oceanography, do you?
- JR: No. The usual way at Scripps and at Woods Hole and at most universities is that you get an education in one of the disciplines of mathematics, physics, biology, geology, chemistry, as an undergraduate degree and then don't take up the study of the ocean until graduate work.
- RC: Okay now, you're at UCLA in 1948 as a student.
- JR: I spent a summer session there polishing some geology and some mathematics.
- RC: Well, now, was that preparatory for entering school here?
- JR: That's right. I entered Scripps in the fall of '48.
- RC: Was it fairly usual for Scripps Institution to ask their students to pick up extra courses at UCLA before they came?
- JR: I don't think there was a usual procedure. We had so few students then that I don't think one can generalize. I had just never had anything in the way of geology at all and thought...well, I guess I was still thinking of bathymetric studies at the time and thought I ought to know a little geology. And since the school didn't open until September, I spent my time in the summer there.
- RC: How many students were at Scripps when you enrolled?
- JR: There were about, at all stages, perhaps 15 to 20 at the most.
- RC: I take it then the contact with the students or contacts among the students would be pretty close at this time?
- JR: Oh, yes, that's right. It must have been more than that because we had a half dozen naval officers and a half dozen Air Force officers as students there, short-time students, as well as a civilian student body which must have been equal to that. We must have had 30 or 40 students, I suppose.
- RC: Were you encouraged almost at once to go to sea?
- JR: Yes, I believe that was a policy that Scripps had at that time--nearly everybody did. I don't know whether it was so much encouragement in all cases as simply something one had to do to collect the data he needed for his degree. That's changed considerably. Not all oceanographers go to sea now, of course; and the requirements are not there, nor is the encouragement there in all cases.
- RC: Why, do you suppose? Would you blame this on computers?

JR: No, it was long before that. This comes down to the sort of break between the pre-World War II oceanography and the post-World War II oceanography. And you will undoubtedly hear several versions of this, none of which is entirely wrong and none of which is entirely right. But there wasn't much oceanography at all in the United States prior to World War II. Woods Hole had a very small establishment, and Scripps existed, and there was some work being done at the University of Washington. But the total number of students was very small. The number of oceanographic research vessels was very small. Woods Hole wasn't giving degrees of any kind at that time; as a matter of fact, only Scripps and the University of Washington were, I believe. But the work being done was not well financed. There were a few ships and not much money to use them. And it did not seem a flourishing field for students to go into. The number of job openings would have been very small. In fact, a great part of the monies spent in oceanography in those days was from endowments or gifts of one kind or another. The federal government was putting very, very little, if any, money into it. I think the first federal money Scripps got was probably as part of the WPA projects in 1936-37, and that was more a general employment thing than a wish to encourage oceanography. They were probably giving this sort of money out to practically every institution that could use some. But before World War II, the emphasis was more on biology, that is, the collection of specimens, the examination of them, some studies in physical oceanography. But the emphasis was observational; that is, the ocean is quite unknown in so many respects we must get measurements of temperature, of salinity, of currents where we can, of the dissolved oxygen concentration, of the plant nutrients, the phosphate and nitrate. And then, mind you, I had nothing to do with oceanography during the Navy. I don't know exactly what happened there, but I suppose it was something like this: the problem of underwater acoustics was tremendously important to the Navy because of their anti-submarine warfare programs, and the problem of wave prediction for the amphibious landings was tremendously important. So a very great emphasis was put upon this, and a number of people from outside the field were brought in--first-rate physicists, mathematicians, people of this kind. And they concentrated, naturally, upon what they could do from first principles and from theory, and they did a tremendous amount. And rightly or wrongly, the impression immediately after World War II was that the era of observation, or what they call rather contemptuously "survey", was over. "It's going to be done now on the basis of what we've done already and our ability to work from first principles with the proper laws of physics." And that attitude has been held continuously by some people and less continuously by others and not at all by others. But I think this explains why going to sea is not obviously expected of an incoming student. That is, they could, even before computers came in, do good mathematical work, good work in physics, good work in theory of ocean circulation, without actually going to sea. And many of them chose to go that way.

RC: Roger Revelle said that if someone were asked to write an epitaph

for him, he should like to say he took Scripps to sea. Now, I take it he means by that that he took Scripps from an institution which largely surveyed the California coast and worked around local waters to sea: the MIDPAC and these sorts of expeditions.

JR: Well, that is true and Roger certainly deserves credit for that. Exactly how the kind of expansion that allowed him to do that took place, I don't know. That is, who were the senior effective statesmen of oceanography of that period? Certainly Roger was one of them, but he took Scripps to sea by the process of making the federal and the state governments provide the money to do it. And it is certainly true that he did. Woods Hole, likewise, began to expand at the same time; and the University of Washington which actually, I think, had done more long-range oceanography at that time than either Woods Hole or Scripps, began to expand.

RC: Well, there's a tremendous amount of infusion of monies, I suppose, largely because of the activity of ONR, which develops out of World War II.

JR: Yes. ONR first, and then second the National Science Foundation, and later AEC and such things.

RC: When this begins to come to the campus when you're here in '48 and '49, are you aware of these new increased opportunities in terms of money?

JR: Well, as a student I was never particularly on top of that sort of situation any more than as a professor I'm on top of it now, you know. You know the things you bump shoulders against, but I don't look very much farther than that. The first thing that happened in '48 was that a program, the Marine Life Research Program, a state program which had begun before Roger's time, or at least plans for which had begun before Roger's time, was just getting off the ground. And I became associated with that and have been ever since. Now, such monies as I dealt with for the first two or three years came from the state. Such ONR monies as there were--I don't know how much--I don't believe I had any direct contact with, at that time. They began to be obvious in the early '50's; that is, I believe the MIDPAC expedition was an ONR-supported one. That probably was the first long-range expedition Scripps ever mounted. And I believe that the funding for it was Office of Naval Research.

RC: Exactly what is Marine Life Research Program?

JR: This is a program that was proposed by Harald Sverdrup when he was director of Scripps Institution just before and during World War II, and by Elton Sette of the U.S. Fish and Wildlife Service, and by two or three people in the California Department of Fish and Game. Its object was to study the marine resources, the fisheries, of the coastal waters of California. The particular impetus, and the thing that made it possible to get it going, was

the decline at that time of the sardine fishery, which had begun in World War I and became enormous--nearly a million tons were taken in some years in the '30's. But the population was clearly declining, and the fishery was going down very strongly in the late '40's or mid'40's. And whereas the program of observing the resources would have been a useful thing to propose at any time, it was probably that particular crisis which led the state to provide the funds for it. And they--that is, the fishery's funds--have been provided by the state ever since.

RC: And you're presently serving as director of Marine Life Research.

JR: The Marine Life Research Program. That's right.

RC: And you began in it during your graduate career. Is that correct?

JR: That's right. The first research assistantship I had, I believe, was paid out of those funds.

RC: And exactly what was your first research assistantship in terms of what you did?

JR: Well, I was essentially working for that program. I went to sea on the very first of the cruises they arranged and helped to work up the data and have been involved more or less with those studies ever since. On the other hand, I have, for the last 20 years, not put most of my time into that but into larger area studies: first the whole North Pacific, then the whole Pacific, and now I'm creeping up into the North Atlantic as well.

RC: I should like to ask you something here about circulation and distribution of properties in Pacific, Atlantic, and Indian Ocean.

JR: Yes. Well, the Indian I haven't really got into yet because Klaus Wyrski just made an excellent atlas; and I think that's in such a shape that when I really want to learn about the Indian Ocean, I can go to that. The North Atlantic is still a jungle. It's had more people working on it than, I think, any other ocean; but they have worked on little bits and pieces of it in an incoherent fashion. It's something like the prayer one can forgive them for those things they have done, but it's hard to forgive them for those things they have left undone. This is a very opinionated and narrow view I'm expressing, but it is authentic.

RC: Okay, now what I'm curious about is how does someone whose principal work is supposed to be Marine Life Research Program end up being best known for circulation and distribution of properties in Pacific, Atlantic, and Indian Oceans?

JR: Well, the sequence was that we carried out and are still carrying out observations and studies off the coast of California, and I wrote two or three papers on this. But it appeared to me that a

good part of the problem could only be understood by consideration of, well, not only the California current, but all its tributaries. And if one defines tributaries correctly, we can get to the Antarctic and the Norwegian Sea if we wish. And the Marine Life Research Program is really quite broadly based. We do have the mission to the state of providing information and research upon the California current. There is nothing in the ground rules that says that we cannot work upon other things. Of course, if we decided to spend large amounts of state money in the East China Sea, why, the state would probably be upset. But the funding arrangement, you see, is such that we don't spend Marine Life Research monies on studying such areas as the East China Sea. It is also possible for people who are in MLR to obtain research funds from the Office of Naval Research and from the National Science Foundation for those studies which are not directly related to the California problem.

RC: Okay now. Let me make sure I understand this. May I begin with tuna, for example?

JR: All right.

RC: Okay. When you talk about distribution of properties in circulation and current, are you beginning, for example, to try to trace, let's say, tuna migrations--where they come and where they go--in terms of feeding depths and feeding levels, and such?

JR: No, as a physical oceanographer that's not the way I begin. You see, the Marine Life Research Program has counterparts within the National Marine Fisheries Service and the California Department of Fish and Game. These three form a cooperative group--it's called California Cooperative Fisheries Investigations. Scripps does the physical oceanography, the chemistry, and the plankton. The National Marine Fisheries Service does the intense surveys of egg and larval distributions for the fishes--not only the sardine, but the anchovy, the jack-mackerel, the Pacific mackerel, hake, etc.--which are resources to the people of California. The purpose is to assess what is there and what might be taken. The California Department of Fish and Game doesn't work with the egg and larval distributions, but works with direct acoustic monitoring of the populations. They send their ship around, acoustically searching for schools of fish and identifying and counting them. Now, tuna has not been a part of what the CalCOFI group has worked with--tuna, by and large, are open-ocean fish. We have the albacore coming in here as quite a major fishery in the summertime, but it's a relatively small part of the action. The biggest potential fishery we have now is the anchovy, and we're presently taking about 150,000 tons per year of anchovies. The limitation on that is imposed by the state regulatory agencies. We believe that substantially more than that could be taken with no danger to the fishery. There is no real pressure to do this, however, because the commercial fishermen are not quite sure what the price is going to be within the next few

years. If the Peruvian anchovetta fishery can't supply enough of the world's needs for fish meal, or if they have a failure, or if their prices are too high, then the California fishermen will be quite anxious to take more. In order to take more, they do have to do some recapitalization; that is, they don't have enough processing plants at the moment to handle much more than this. And they want to take a good hard look at this and make sure there is going to be a demand at a good price for the fishery before they really expand heavily. One of the curious problems is that the sports fishermen of California, who are far more numerous, of course, than the commercial fishermen, have been led to believe, quite wrongly, that it is the anchovy population that the sport fishes feed upon, and that there is a great danger that, if the anchovies are fished commercially in large quantities, then the sports fishing will decline. And since there are six million sports fishermen and a few thousand commercial fishermen, the votes would certainly go against the commercial fishermen at the moment. Still, if the price becomes right, I believe a good case can be made and the limits can be raised. One of the problems, of course, is that Mexico is about to enter massively into the anchovy fishery, and that can certainly change the political context considerably.

RC: Now, are these the sorts of problems you're called upon to deal with as the director of MLR? Is your problem one of coordination of an institution separate, but attached to, Scripps?

JR: The problem is one of cooperation with these other agencies, that is, the California Department of Fish and Game and the National Marine Fisheries Service, and, on an informal basis, with the recently developed fisheries services within Mexico, particularly the state of Baja California, which is the place that will be the basis for those anchovy fisheries. We have no formal arrangements with them whatsoever, but we have invited them aboard our research vessels to go to sea, and we've sent some of our people on theirs and are maintaining excellent relations with them. It is a common problem. We sometimes worry a little what will happen when our state departments begin to get in on this, because, I believe, with goodwill and not too much bureaucracy, this thing will work out very nicely for all parties concerned. But with 200-mile line limits being imposed by the Congress, and then by Mexico, and a new president in Mexico, and a new administration in the U.S., new complaints from the New England fishermen who have an entirely different kind of problem, well, as I say, we hope things can go smoothly and calmly. But I don't spend perhaps as much of my time on that sort of problem as I ought. I've been to sea in the western Pacific and the South Atlantic and the North Atlantic in pursuit of this, while still being a member of the Marine Life Research Program.

RC: Okay now, while we're right here, I want you to talk some about the expeditions--evaluate them, explain your role, the first expedition you went on.

JR: Well, the first expedition that I went on was simply one of the systematic studies off the coast of California, measuring the characteristics here. The first one that I planned and led and arranged the money for went into the Northwest Pacific in wintertime. In studying the circulation of the ocean, we must consider not only the wind-driven parts, but also those that are driven by thermohaline processes, such as the cooling of waters in high latitudes that makes them become denser and sink. Then they move from that part of the ocean, say, north of 50 degrees down to beneath the equator, perhaps all the way to the Antarctic or wherever they go. Data were lacking as to how intense this process is, that is, how deep the overturn in the Northwest Pacific goes in wintertime when the storms are violent and the temperature approaches freezing. There were no winter data from that area, and it was important to get some in order to understand the heat budget of the Pacific, and hence the whole world ocean, with which it connects. What is the circulation there? How deep are the effects of winter cooling felt in the Northwest Pacific? The answer proved to be that they're not felt very deep, that the very low salinity at the surface layers of the Northwest Pacific makes those waters very much less dense; and, although they can be stirred violently and cooled to the freezing point, they never become quite dense enough to penetrate very deep. In some other parts of the world ocean, such as the North Atlantic, much deeper overturn takes place and in the Antarctic another sort of overturn takes place. I believe that a coherent and internally consistent description of all three of these oceans and their connections through the Antarctic and the Arctic Ocean is possible in terms of the concepts and information we have now. Once it's written down clearly and coherently, then the numerical modelers and the theoreticians will be able to do their work much more straightforwardly.

RC: What about the NORPAC mission? Fourteen organizations, three countries, 19 ships--is that correct?

JR: Oh, yes, that was something else again. I'd skipped that one. That was one I had a part in organizing. It took place in 1955. That was such a long time ago. At that time, I had been working for about five or six years with the Marine Life Research group here in studying the waters off California. I spent a month in Canada with their research group to find out what they were up to. They had had an expedition at sea off the coast of British Columbia. That is an area where the west wind drift comes in against the coast and turns north and south--it splits there. And it was very difficult to understand what was going on because they'd measured an area that was too small to reveal the major pattern of flow. It occurred to me that we had had an expedition just south of that area at about the same time, and by combining the data, the whole pattern made sense. And the next step with their director, Jack Tully, was to think, "Gee, if two countries can do that, how about a few more?" This was a peculiar period of history then. The Canadians had just got their civilian-type

research going after World War II and so had we. And the Japanese were getting started again. And my interests and John Tully's, who was in charge in the Canadian group, and those of many of the research agencies in Japan were similar; that is, the notion of getting a large-area quasi-synoptic picture of the distribution of temperature, salinity, and density over an area as large as this was very tempting. We thought at first of doing this with the Canadian and the U.S. vessels, getting the University of Washington to help. At that time the Fish and Wildlife Service in Honolulu also had a research vessel, and we thought it would be nice to propose it to them. I don't know who had the wit to propose that perhaps the Japanese could be interested in it, but someone did, and they were willing. We tried to get this going in '54. It couldn't be done at that time, but it was proposed and carried out in 1955. It all came off remarkably well.

- RC: Obviously the expedition was productive in terms of scientific development and to your career. But overall how would you evaluate international cooperation?
- JR: When in a field such as oceanography, which covers such immense parts of the ocean, it's absolutely necessary. In fact, oceanographers have always exchanged their data since the very first expeditions in the nineteenth century. During World War II they didn't, of course, because they classified so much of their material. But since that time, their data have been exchanged. If someone goes to the trouble of collecting a set of data, he certainly has first use of it. But having processed it and written his paper, he does put the data into the public domain through national oceanographic data centers and international data centers so that everyone has access to the data. In fact, although I've gone to sea a lot, I'm sure that I've used many more stations taken by other people than those that I've used by myself. I can't cover the whole ocean; I just cover the parts other people have missed and use their data and then places where they've done the work.
- RC: Now, you said you were involved in cruise planning of regular cruises, as well as NORPAC and other expeditions, and in supervision and collecting and processing of data.
- JR: Yes, that's true.
- RC: How do you start?
- JR: Well, you first have an idea of something you want to do. Well, for example, let's not talk about a cooperative one, but an individual one now. The one from which I've just returned was a collection of data with which I will study the circulation of the deeper waters of the Philippine Sea. First you ask yourself whether there is some need to collect more data from that area. You look at what is available and decide what is needed to work more usefully on the problem, and then decide how much ship time you

would need. You have to find the time in the scheduling of the Scripps ships when the ship can be in that area and then request use of the Thomas Washington for six weeks at some specified time, and then try to raise the money for it. And you have to do this, of course, about a year ahead. And many changes can happen in the interval. You write a proposal either to the National Science Foundation, as I did this time, or to the Office of Naval Research, as I have on occasion, and explain what you want to do and what the costs are and ask that the money be granted.

RC: And then do you move out and ask other oceanographers from Scripps Institution and elsewhere to join you?

JR: Sometimes, yes, and sometimes, no. Sometimes it's something that only I want to do. Sometimes there are other people who have an interest in that problem or an analogous problem that could be handled at the same time on the same ship. We collect data not only for my purposes--that is, temperature, salinity, oxygen, nutrients--but we collect biological samples and information as well. We can make closer-spaced observations of temperature structure for the fine-structure people. And we keep records of magnetics as we go along; that is, we have a magnetometer. The magnetics people get a great part of their data from these expeditions. We do bathymetry as well; that is, we keep meticulous records of the depth of the waters over which we're crossing. On most of the expeditions that I have organized, there has been a visitor aboard from a nearby country, from Japan or from Brazil or from Canada, or when I'm working in the West Pacific, people from the French oceanographic station in Noumea, New Caledonia. But the number of people who are doing the sort of work I'm doing is not large.

RC: How do you explain that?

JR: Well, science has fashions just like anything else. I hope I don't sound critical of the people who are doing other kinds of work, but they are the fashionable ones at the moment. That is, there's been such a lot of progress in, oh, study of internal waves, for example, and micro-structure and meso-scale structure and of variability, that many of the younger people see those as the things that are most commonly written up and discussed and become interested in them. The field that I'm working in, which is generally called Descriptive Oceanography, some people think has been fully exploited and that there is no point in working on it any more. My feeling, on the other hand, is that we've just begun to get into it, that, for the first time now, we have enough data to do the work properly and enough concepts with which to exploit the data. This sort of work might be considered tedious or toilsome by some, but it is exactly the sort of thing I like to do.

RC: One of the things that's been suggested to me, and one of the reasons I'm talking to you, is that it's been suggested that such things as mapping and working with currents and so forth is not

only on the wane now, but shall be in the immediate future, as such things as computers and these kinds of calculations become more important. Would you agree with that?

JR: I didn't quite catch that. Did you say that mapping is on the way out or on the way in?

RC: No, is on the wane, is on the way out.

JR: There are certainly many people who will tell you that is so, but my version is that the maps that have been made are, in many cases, seriously in error, and a lot of those that need to be made have not yet been made at all. And the interpretation of these maps, of course, can't be done unless the maps are made. And we can learn a great deal from looking at the ocean itself. Maybe you can begin a priori as people did in the days when there were no data. It sounds as if I'm being hard on them; I don't mean to be. But the first theoretical work was done on homogeneous, flat-bottomed oceans on non-rotating earths, because they had to begin somewhere. Well, they've come a tremendous distance from that. The computers make a great number of numerical solutions possible nowadays which would have taken centuries to do before. So people are certainly much better at that. They have not got to the stage yet where they can really accept proper inputs of information from the real ocean. That's the problem. My notion is that the numerical model people and I will eventually come together when I understand enough about what I think is going on to give them a clear picture. Then, I have a feeling that somewhere along the line they're going to get to a point where they can say, "Ah, if that's what it is, then I can do such." and some beautiful numerical model of real significance will come out. But, you see, they're working so much in the dark with an ocean with which they're not very familiar. The ocean to them, inevitably, if they work on it time and time again this way, is something that comes out of a computer. And again I don't mean to knock them. If I were working with computers, my eye would be on the output just the same way, and I think I would spend more time looking at that than looking at the real ocean.

RC: Would you compare the level of the work you're doing here with, let's say, the level of similar scientists in Japan, in Canada, and in the Soviet Union?

JR: Well, that's hard. Comparisons are odious, as someone once said; but I do believe that the number of competent descriptive oceanographers is not nearly as large as it ought to be. This schism--and it's a very unfortunate schism--between the numerical modelers and the descriptive types (of course, there are many in between, each with its own specialty) is something that pervades the entire field of physical oceanography. And I think it pervades biology as well. You know the DNA types and people who look at fish. They say terrible things about each other, and some of them are true. That sort of schism holds in this field as well.

You'll have to ask someone else about relative merits of the theoreticians in the various countries. My impression is that the U.S. and Britain are leading in this, although there are some Soviets who are highly regarded. As far as descriptive work is concerned, there's not an awful lot of it of first-rate quality coming out. The Canadians are doing some, and the Japanese and Soviets. The burgeoning countries, those that are becoming developed now, are trying some work in this; but it's very hard getting started, and some of their work is rather primitive. There are only a few individuals who have done, or are doing, first-rate work in this. One is Klaus Wyrski, for example, at the University of Hawaii. He has done excellent work in this field and is continuing to do so. He and I have parted interests in the sense that he's now working on the very upper ocean, and I'm working on the deeper part of the ocean. That doesn't mean that we have anything to fight about. It's just that I can no longer get any really useful discussions with him or he with me because we have different focuses now. Fritz Fuglister, Val Worthington, and Bruce Warren are the descriptive types at Woods Hole. I worry about Bruce Warren because he's the youngest of us and he may be the last descriptive oceanographer left in this country. "Who's he going to talk to 20 years from now?" is what I keep telling him. He'd better bring somebody up to help. A good deal of the Soviet descriptive work is mediocre and unimaginative, but I can't cover all of the literature, of course. I cover only the translated journals and the abstracts of those that are not fully translated, I don't sound very modest in this, but then perhaps I'm not.

RC: Well, the reason I'm here is to talk about what appears to be the decline of physical oceanography. That's the sort of thing I want to know about and the explanation for it. And I had obviously worked the explanation out incorrectly. I had worked it out because of computers and technical advancement.

JR: Oh, no. You see, I use computers in processing and handling my data. For example, if I make a map that has 6,000 points on it, I don't plot those by hand. I get this stuff all on tape to make the appropriate calculations and they come out of the computer plotted for me, and then I look at the field. That is, I do use computers, but not to do anything that I couldn't do myself, just to do it in a much more economical and expeditious fashion. I couldn't operate nowadays without computers. I think that if there is any explanation for the trend, that it simply comes from the attempts that people made in the early '50's to encourage theoretical work as opposed to observational work. I've attended meetings, particularly one about 1962, at which the consensus of the meeting was that we don't really need to spend our time going to sea anymore, that we have enough information now, and that from now on the theoreticians will be able to carry it forward for us. Well, I've heard this several times, you see, and it used to worry me very much because it would certainly put me out of business. But, in fact, the going to sea has not

slowed down at all. In fact, the one surprising hope for descriptive oceanography comes through the new blood in geochemistry; that is, the people who are working with isotopes, things that allow one to compute the time elapsed since a water sample collected at great depth was last at the sea surface. The geochemists are a very brilliant and hard-working and aggressive group. The trouble is they did not have much familiarity with the ocean to begin with and were a very combative and contentious lot that didn't get along together very well. And their observations are expensive to make; that is, they need large quantities of water taken from great depths in the ocean. And then once they've got this stuff back, the things they have to do to measure the various concentrations are very expensive. For years they went on arguing about one sample collected from the bottom of the North Pacific, trying to explain all of ocean circulation on the basis of those few samples. And, of course, the samples had been taken by different people and processed different ways, anyway, so they might not really have been talking about comparable quantities. About ten years ago, Henry Stommel and Bruce Warren and I were at sea on an expedition that ran from Australia to South America, all along one latitude. I think our data collection system was very good. In any case it had one advantage: that is, even if some of our gear had been slightly out of calibration, at least this was one set of gear, one set of reagents, one set of techniques, one set of people, operating all the way across. Those data should be internally consistent; that is, we did preserve the Aristotelean unities of time, place, and personnel or something. I asked Henry, "Why don't the geochemists do something like this?" We talked it over and thought that, well, it would be nice for them to take one long line of stations, say from the Bering Sea to the Antarctic in the Pacific, in which they use one set of bottles, one set of reagents, equipments, techniques. And we decided that the best way to get this going would be for Henry, who is the world's most prestigious physical oceanographer, simply to write a few letters to the leading geochemists and to the funding agencies asking if they would like to consider such a thing. In fact, they would. With his influence and advice as the focus, they did get together. They did not do just the North and South Atlantic. They did the North and South Pacific and a couple of east-west lines. And next year they're going to do the Indian Ocean. They do think very large. They were after the isotopes, the Carbon-14 and the radium and the radon and tritium and so forth. The most important of these quantities is the Carbon-14. It is also the one which takes longest for them to process. And, therefore, not all of the numbers from the Carbon-14 are available at this time. But being aggressive types, they have gone to work on the data which have come forward first, the ones which are sort of standard quantities that people have been measuring well for years--the things I work with: temperature, salinity, oxygen, and nutrients. And they have concentrated very hard upon those, although they have a limited data base--just one line of stations isn't much. They have not hesitated to leap into it. And not being educated in

physical oceanography and the circulation of the ocean, they've made a number of outstanding mistakes; but it doesn't bother them at all, you see. They pick themselves up immediately when they're caught on this and keep working. And they do not hesitate to try any field; whether they've got a doctorate in it doesn't matter to them, or whether they've ever heard of the subject before. They have this feeling that they're smart. They can learn anything in two weeks. Well, as I said, it's a very contentious and sometimes unpleasant group of people, but a very effective group. The thing that bothers me is that, when I've talked to some young physical oceanographers and asked what they are working on and, "Well, why don't you use some of these radon data that the GEOSECS people have collected? They take samples just above the bottom in deep water, and perhaps you can use them in calculating vertical mixing rates. You certainly should know how to do this sort of thing better than they do." Their answer is, "But we don't know anything about radon." Of course, that's absurd. They could probably learn enough about radon in a week to do this work. You don't have to be that kind of chemist; you just have to know what radon is and what its half-life is and its solubility, and things of that sort. The geochemists know enough about radon, in fact more than is necessary with this, and know nothing about mixing, but leap in at once and do it wrong the first time, wrong the second time. But every time they do it wrong, they learn something. The thing that frightens me is the possibility that the next major finding about the deep circulation of the ocean might come from some geochemists instead of a physical oceanographer--wouldn't that be awful?--simply because they're not afraid to try. But yet in the process they, whether they will admit it or not, are becoming a sort of descriptive oceanographer. That's a new source of blood to the field. And where it comes from doesn't matter.

RC: And there is where you think the future of....

JR: It is one interesting possibility. Whether it is the best possibility at the moment, I don't know. You see, this field, that is, descriptive oceanography, unlike mathematics, is not something in which someone's competence can be evaluated at once. That is, in such things as mathematics a man makes his name and demonstrates his competence before he's 25. This seems to be almost always the case. On the other hand, this sort of thing, like some other fields, involves the accumulation of information in your memory bank and other concepts that take some years to acquire. And this again may be one of the reasons the students choose not to go into this. They think that with the computer and a good theoretical background, they can get their degree in three years, where, if they do it the other route, it would probably take five, maybe six. And what do they want to do? They want to get their degrees and get out and go to work. So there may be some young people around that, three or four or five years from now, I will think are excellent, that I simply can't evaluate now, or it may be that nearly all of the best

young people are leaping into non-going-to-sea work, or being led one way or another into things which are the fashion of the moment and will never get into this other part of the field. It is something that does give me some worry.

RC: What about your future?

JR: Well, as one of the young people around here said to me earlier this week, "Joe, you've got it made", by which I think he means I am a professor with tenure at a salary which, though I don't know how it compares with the rest of the world, is very handsome compared to what I ever expected to make out of this field. And I'm directing something and therefore have influence, and I'm associate director of something else. He must think that's pretty high. Well, if it is pretty high, I consider.... If I'm on top of a high hill, this high hill is all full of cactus. The other things you have to do in conjunction with directing this and being on advisory councils and so forth is such that it's not an unmixed blessing. I had about five good years at Scripps when I had one of those little cottages on the hillside and two people working for me. And since the cottage was isolated, most people at Scripps didn't even know I was here. I served on very few committees. Nobody thought of appointing me, for example. They didn't see me on campus down below every day. And I got more research done, more papers written, in that period than I will ever hope to again.

RC: But I assume you are going to continue to work on the North Atlantic.

JR: Well, I'm going to work up toward it. I've got the South Atlantic in reasonable shape. What I spend a great deal of time doing is trying to clear the desk of the correspondence, the appointments, the service on committees, so I can really get to work on it, you know. But the in-basket fills up as fast as the out-basket, and this goes by fits and starts. I never really get everything else cleared away, when finally there comes a day when I just forget everything else, no matter how pressing it is, and push forward on the sort of thing I want to do. An impetus can go on for several weeks and something gets done. Meanwhile, devastating things are happening because of my negligence in other things. But it really isn't all that devastating at the end of it. Not all these things are really as important as you think they are to begin with. There are very efficient people around here. You've talked to Walter Munk. Walter is some kind of paragon. I was his graduate student for a while, and I knew he was brilliant. A few years later, realizing the kind of problems he was selecting to work on, I realized he was clever. And then another few years later, when he was president of the academic senate during some of the turmoil, I also realized he's full of common sense. Now that's a very formidable combination. And I'm sure that he does more important administrative work than I do, yet he does it in some way that doesn't really interfere very much with the rest of his activities. He's somehow got the

thing into shape so that he handles only the important things, whereas I get hung up from time to time with things which, in the long run, I could have very well put off.

RC: What took you to A&M, by the way, in '61?

JR: Well, there are several reasons. One, I wanted to see what was going on down there. The second, my wife was restless; she likes to travel. We'd been at Johns Hopkins for six months once, and we'd gone to Canada again for a visit, and I wanted to see what was going on down at Texas A&M. There was a particular problem I wanted to work on with Bob Reid. This didn't particularly mean that I had to go there. I could have talked to him on the phone a few times, written a few letters; but it seemed better to go down there, so I did. They were kind enough to pay me a salary for two or three months or whatever it was. I enjoyed the experience. I haven't been very many places since then, come to think of it, for extended visits.

RC: Maybe that's what happens when you become an administrator.

JR: Well, that could be it.

RC: Do you have any more voyages planned in the near future?

JR: I don't have anything specific in mind at the moment, as to date. I think I would like to work the Northwest Pacific again and this time focus on the deep water. I **can do this in the summertime**. And I think that may come up in late '77 or early '78. You have to be clever in this sort of thing. You can't get money to run a ship 5,000 miles to a place to begin your work. You have to fit your work in a reasonable sequence with other areas. That means frequently trying to persuade somebody to go to sea in a certain month instead of some other month, or to do his work there rather than somewhere else. You really have to look around and find out who wants to do what and build a sequence of these things that makes sense.

RC: I should also, then, like to ask you a series of other questions about oceanography, if I may, that have to do with, if you'd like to say, the future of oceanography. In terms of Marine Life Research programs, are you as optimistic about the ocean becoming a source of protein as some of the popular magazines and press may happen to be?

JR: Well, I am optimistic that we can get more than we are now getting. I don't think there's any question of that now. How much more, I'm not certain. That is, we could certainly catch another 200,000 tons of anchovy; that's a drop in the bucket compared to the total mass of anchovies. The major exploratory fishing country, of course, is the Soviet Union now. The Soviets perhaps have a better notion of the maximum possible yield than anyone else. I don't know whether they're telling people about it or

not. But, of course, the form in which we get it is important, too. Out of oysters and salmon and swordfish, we can get only a limited amount; and that is sort of luxury food nowadays. What we can do with anchovies, instead of feeding them to chickens, has yet to be worked out; that is, whether they can be made into human food directly, which would be much more efficient, I don't know. John Ryther has done more work on this than anyone else. I believe his numbers were that we might possibly double what we were getting five years ago, but not much more than that. I don't know whether he considered direct harvesting of plankton, as the Soviets have proposed, or not. Clearly, if we could get it at that stage and could make it into human food directly, we would get much more food out of the ocean than we are now. I suppose what I feel most strongly is that we should do our best to make these estimates and learn as much as we can so that, when the crunch really comes, we can operate effectively in catching the right amount, that is, the optimum yield.

RC: What about mariculture?

JR: I don't really know anything about that. I've been told wonderful things about catfish mariculture--I mean, that's lab culture, isn't it? They're doing that rather successfully. I'm not sure mariculture in the ocean has ever been successful for anything except for those exotic oysters they moved from the Mediterranean to the Atlantic and back again, I think, before harvesting them, or the pearl oysters the Japanese raise. I don't have any direct information as to whether that's as far as they can go.

RC: What sort of threat do you think that twentieth-century America poses towards the oceans in terms of the environment and ecology?

JR: So much has been said about that, which I consider extreme and not believable, that I don't quite know how to answer. I think someone called me up four or five years ago during some of the greater excitement on this subject and asked me what I thought of someone's statement about the effects of this in the ocean. And he said it was going to be terribly devastating--the oceans would be dead within a few more months or years or something. The only kind of answer that I could make was I did not think he had enough information to justify saying that. On the other hand, I did not have enough information to say he was entirely wrong. There remains at least the possibility that it's even worse than this fellow said. I think we can certainly get ourselves organized to prevent this kind of problem; that is, it's a foolish thing just saying that, my, we're putting too much phosphate from detergents into the ocean. That, for example, is absurd. There is so much phosphate in the ocean already that the trickles that you get out of rivers might cause trouble in lakes, in rivers or in estuaries. But the ocean can take that stuff up all right. In fact, you could drop all the human fecal matter into the ocean in one place in the bottom of the North Pacific and wait a month, and you'd have a terrible time finding

out that it had ever been put there. On the other hand, some of the artificially created chemicals may be entirely different. I was just reading in "Science" this morning about more DDT in the Channel islands that's supposed to have wiped out the brown pelicans, except the brown pelicans are back again; and the controversy is over whether it's because they've stopped using DDT or whether they died of something else and some new population has come in. No one really knows, at least I don't really know. Cousteau, of course, is one of the great publicizers of the ocean, and we must be grateful to him for the amount of public support that he has brought to the field. I would hate to have to defend all of his statements. On the other hand, I hope I never have to attack them either, in any public contest. I believe the ocean can absorb any of the normal biological products. They can, of course, be concentrated in a way to cause troubles in various places, and there are certainly factory effluents that have been shown to cause harm. I think we ought to be able to stop putting those in the ocean; that is, industry, I'm sure, if people press hard enough, will collect its own pollutants and handle them properly. So far as oil on the ocean is concerned or petroleum waste, that's very bad; and I hope that that gets controlled, simply as the price of petroleum goes up enough nobody can afford to waste any. Well, there was another minor spill in Cook Inlet reported in "Science". An estuary, which is what Cook Inlet is, is particularly susceptible to that sort of thing. On the other hand, I'm not sure anything special needs to be done other than to see that the laws are correct and that they are really enforced. Petroleum, that is, raw petroleum, is a natural product. Too much of it in the wrong place will cause a lot of trouble. I have not experienced in the Pacific these things that have been claimed for the North Atlantic, that every net haul brings up lots of tar balls. That simply doesn't happen, perhaps because oil spills are fewer in the North Pacific and the South Pacific than they are in the North Atlantic. The great problem, of course--one that frightens me--is the disposal of nuclear wastes, because these apparently have accumulated in tremendous quantities. Something's got to be done with them. Apparently, even if we stopped using any nuclear processes today, we would still have the problem of disposing of what's been created. There has been a great deal of loose talk about depositing those at the bottom of the ocean. Well, again we can't say exactly what the dissipation rates are down there, but we have reason to think that they're certainly rapid enough that those that are going to be lethal for 250,000 years will certainly cause us trouble. I have no other solution, mind you, but what do we have to do? Pray for fusion to come in so that they can be worked backwards?

RC: In terms of funding in oceanography, funds have seemed to drop for oceanographic projects as overhead takes a greater percentage and as inflation rises. Do you see this also in the immediate future? Are you afraid that....

JR: Well, you see, I have been extremely fortunate in the work I've done. Maybe it's because there aren't many people doing the kind of work I do, but I have never been kept from doing something I really wanted to do by lack of money. Now, on the other hand, I've also patterned what I wanted to do to what I thought was reasonable funding; but I have not myself sustained any cuts, although many people have. And because I have not had that kind of difficulty, I have not kept my ear to the ground for the larger picture either, you know. Wait till my ox is gored before I start worrying about the goring devices. Well, one can be callous about this and say that a weeding out from time to time is a good thing. Perhaps every ten years we should drop out 50 per cent and start over again. When money is ample, Parkinson's Law applies: costs rise to meet income.

RC: Finally, was the location of the University of California at San Diego, here in the proximity of Scripps Institution, was that essentially a good idea?

JR: I can't really have an opinion one way or the other on that. I've had little contact with the upper-campus people. I really should be ashamed of that; I should have made a point of having more contact with them. I've served on a few committees with them from time to time. I go to the academic senate meetings. I go to some of their functions. But I certainly cannot say that the increased size of the upper campus in any way caused any trouble to me personally, or to anything else. I think our biggest trouble at Scripps at the moment is the size of Scripps itself-- it has simply grown larger than is really manageable. My solution would be to put a big bowl of black and white beans in the library patio where everybody walks by and takes a bean. And those who get the black beans, which will be 50 per cent, go off and form a comparable institution somewhere else. And then, when it gets too big, it splits again. It's not from any poor management necessarily, nor from any ill will, but things above a certain size just get harder to manage. As the number of people goes up, the percentage of your time that you have to spend just keeping the wheels turning, much less progressing, goes up. The amount of coordination that's required is just too large. But I shouldn't complain. I had those wonderful years when Scripps was run somewhat like my private institute for advanced study; and now, after these years, they asked me to do some of those things for the other people that were done for me. And, of course, it might be considered only justice, but most people have the wit to realize, "We'd rather have mercy than justice, wouldn't we?"

RC: In terms of preparations for an advanced degree in oceanography, in your opinion should they follow the policy they follow at Scripps Institution, which is to take a hard scientist and bring him into oceanography for advanced degree as you came in, rather than an undergraduate degree in oceanography?

JR: The question could be thought of in two forms. How do you get the

best oceanographer in an ideal world, or how do you get the best oceanographer under the present system? In the present world, I think that nearly every major institution that grants degrees requires the hard science bachelor's degree first. The University of Washington doesn't, for example. But I believe there is a danger in that case that, instead of finding out which of the hard sciences he wants to focus on, the student simply never gets deep enough in any of them. And this varies with the individuals, of course. Henry Stommel doesn't have a doctorate, for example. In fact, I think he began as a theology student and switched to astronomy and never studied oceanography at all. He just became the world's greatest oceanographer. But, you see, that was in a more loosely organized time. We are so stereotyped now; it is like the civil service or something like that. And the opportunity of getting research funds to do something with depends on your having passed through the private-corporal-sergeant stage. That is, there is no reason in the world that we could not get an educated physical oceanographer out of just a man sitting in a room reading a lot of books and papers, but it's never going to happen that way. And the training is getting narrower and narrower to each field; that is, at Scripps we have the constant problem of whether a beginning student in physical oceanography should really take one-quarter courses in biological oceanography, chemistry, and geology, as well as in physical oceanography, or is that a waste of his time. Well, I was at a site visit at Woods Hole earlier this year, and they make even less of an attempt than we do to get any breadth into their students. On their qualifying exam, which is written, there are ten questions, but the students have to answer only seven of them. And if there are any questions that deal in any way with geology or biology or chemistry, there are no more than three of them, and those are the three that are left out by the physical oceanographers. They simply choose not to answer those. And, of course, if the student knows that's going to be the case, why should he waste one minute on something he's not going to be examined on. I told them that they were taking great care never to create another Henry Stommel. I don't know whether it had any effect or not, but Henry's done some biological work as well. I mean, he doesn't pretend to be a biologist; he learned enough of it to apply it to the particular problem and write a couple of classic papers. But, typically, the students going through the degree-generating mechanism do excellent work in a narrow field and come up with an excellent thesis, but are--unless they are unusual people--condemned to that particular narrow strip for the rest of their career.

RC: And you think this sort of technical training is the direction education in oceanography is going to take?

JR: It seems to be going that way at Scripps and at Woods Hole, but I don't know about Texas A&M or the University of Washington because I don't have that much contact with their students. But Scripps and Woods Hole are the longest established and most

prestigious, and there is always some danger that the others will copy them whether they should or not.