

Professor Joseph L. Reid Marine Life Research Group

**Interviews conducted by
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Joseph L. Reid, 1975

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INTERVIEW ONE: 4 FEBRUARY 1999

Russell: ##¹ My name is Joellen Russell, and I'm sitting here with Joe Reid and we are going to try to do an oral history of his early life here at Scripps. Today is the fourth of February and it is about one o'clock in the afternoon.

Joe, what drew you to SIO? I understand you came in 1948 as opposed to Woods Hole Oceanographic Institution or the University of Washington.

Reid: I think at that time Scripps was the only one I had heard about, and I just wandered by to see what I should do. The reason I came was that during the war I was a Navy navigator in the western Pacific and I found some islands out of line and thought the charts might be improved. I did that part of it actually after VJ Day in the late months of '45. I was interested in the depth of the ocean and the positions of the islands and thought I might like to work on that. Therefore, after I got out of the Navy in early '46 I thought I was qualified to be a merchant marine officer. I sat for the exam and got a license as a third mate on ocean-going steam motor vessels or something like that. They tried to persuade me to go out on a merchant ship, and I wasn't really ready to go back to sea quite so quickly. Instead I wandered around and I got a job in Europe for a while and traveled there.

I came back in late '47, wandered around a little more. I had an old war-surplus Jeep that I drove over the country, and finally I heard from someone that there was a Scripps Institution of Oceanography that might be interesting. When I got the mate's license, I did write to the Coast Survey to see if they could use someone who was a qualified navigator and deck officer in some of their surveying work in the western Pacific, or southwestern Pacific. They wrote back saying that the Coast Survey wasn't doing that part of the world, they were doing the coastline. They forwarded my letter to the [U.S. Navy] Hydrographic Office and they wrote back, I believe, and offered me a commission to work, but they were at work at that time in the Philippines and Alaska. Somehow, I was only twenty-five years old, I don't know why I didn't want to work in the Philippines or Alaska, so I did this wandering in Europe. But I did keep an interest in this.

Finally in August or September of 1948, I showed up at the Scripps Institution, not knowing anything about it. I did have my bachelor's degree credits with me and I was admitted as a graduate student, in a rather cursory fashion. There weren't very high standards in those days or perhaps I wouldn't have got in. That's how I got here. I don't know how many students there were in all at that time, not an awful lot. There were half dozen or so in civilian physical oceanography. There were four naval

¹ The symbol ## indicates that a section of tape has begun or ended. For a guide to the tapes, see page 36.

officers and four or five Air Force officers. There still remained one or two or three of the Argentine naval officers who had been sent here. That constituted the physical oceanography student body. There were at least that many, or probably twice that many, other students at the time. I don't recall the numbers.

Russell: From your early experience in the Navy, you felt that you'd be suited to doing oceanographic work, you said. I'm curious. How important do you think experience at sea is to an oceanographer? I know we have people here who, actually, today who don't really go to sea.

Reid: It depends upon how they behave themselves, often how they take to it. I know something about the management of ships and whether they can stand and work in and what is feasible with them. Most of all, I know what the captain's responsibility is, and therefore I've never had any real fusses with the captains, except one. We won't talk about that.

Russell: What were your first impressions of SIO as an educational institution, and as a research institution?

Reid: The place was much smaller at that time. They had only six or seven faculty, I believe. We began by taking the core courses: physical, chemical, biological. I left something out, didn't I? Geological. That was the first semester for everyone, and then we got more closely into the branches that we were concerned with. There was one nice thing about it at that period. The faculty and the student body were small at that time. There was one seminar every Friday afternoon. All the researchers and students attended. Of course, this covered the whole range of subjects that Scripps people were working on, and we got to know everything that everybody was doing, whether it was in our field or not. There weren't enough people to fill it up every Friday during the year; we had a few speakers from outside. I wouldn't say it was a more convivial place. We didn't particularly get along better than other places, so far as I know, with each other, but we did know what the other was doing.

Russell: I understand that you didn't quite overlap with the director, Sverdrup². He was leaving just as you were arriving.

Reid: I think he left in June or July of '48, or thereabouts, so I never had a chance to talk to him.

Russell: You didn't meet him when you were first starting out?

Reid: No, I didn't. He came back for a visit, I think, a year or so later. I talked to him then. I never got a chance to work with him.

²Harald Ulrik Sverdrup (1888-1957), Norwegian oceanographer and polar explorer, director of Scripps Institution of Oceanography, 1936-1948.

Russell: What were your impressions about Revelle?³

Reid: Well, he was there. I've forgotten what he was doing. He was trying to help create the Office of Naval Research and carrying out the further extension of the Marine Life Research Program, that is, the CalCOFI program at sea.⁴ What he had been doing in the previous year was to find enough ships to carry this out. This was supposed to be a monthly survey that was to study the California Current from Baja California up to Washington, carried out monthly for an indefinite period. Scripps had only one vessel at that time, the *E.W. Scripps*, which was really too small. We had to find three ships, some of which were retired naval vessels. The Bureau of Commercial Fisheries had the *Black Douglas* so there were four ships put out to take the observations every month. I made one of the first trips on the *Horizon*. We had the *Horizon*, the *Crest*, the *Paolina-T*, and the *Black Douglas*.

Russell: Who were you working with when you first started your research after your classes?

Reid: I worked for a while with Walter Munk.⁵ He was working on a topic there that did eventually become important to me. Actually, I was just a graduate student making some calculations for him. He was working on his paper on the wind-driven ocean circulation, and it was a model, a pre-computer numerical model, of the circulation based upon the Sverdrup transport. He came out with calculations of what the strength of the Gulf Stream, and the Kuroshio should be, and he needed someone to make some kind of calculations by other methods on what the geostrophic flow should be there. It was mostly in the Atlantic, the south Atlantic, where the Meteor Expedition had got a lot of east-west lines that could be integrated. My job was to carry out the double integration—hours before sitting at a Marchand calculator. Something times something—whirr! It turned out that those calculations were nonsense.

Defant⁶ had done a paper, and he said if you refer all these calculations to some surface, you'll get the right speed, you'll get the right transport—it'll work. It didn't get the right transport. Clearly, he was quite mistaken. We

²Roger Randall Dougan Revelle (1909-1991), American physical oceanographer, assistant director of the Scripps Institution of Oceanography 1948-1950, director of the institution 1950-1961 and 1963-1964.

⁴Concerned about the disappearance of the sardine from California waters, representatives of the fishing industry, federal and state fisheries experts, and academics representing the research community met in 1946 and established the Marine Research Committee with support from the California legislature to investigate the cause. The investigation was named the California Cooperative Oceanic Fisheries Investigations (CalCOFI). Harald Sverdrup represented SIO, and in 1948 he called the SIO part of the project the Marine Life Research Program.

⁵Walter Heinrich Munk (b1917), physical oceanographer trained by Harald Sverdrup, founder of the La Jolla branch of the Institute of Geophysics and Planetary Physics.

⁶Albert Defant (1884-1974), distinguished Austrian geophysicist, professor of oceanography at the Institut für Meereskunde at the University of Berlin, participant in the Meteor Expedition of 1925-1927 and editor of the publication of its results.

would get twenty Sverdrups southward across, ten south, thirty Sverdrups northward across, twenty south. It wasn't working. After a while I parted from Walter, no great problems there, we just drifted apart, doing different things. I was then offered the job of—what did they call it those days— junior oceanographer employed by the university to work with the CalCOFI program (the Marine Life Research Program), helping to run the ships. I moved to the Point Loma office, the Scripps Field Annex it was called, and worked out there, helping to get the work done and processing data, and trying to understand the results. I suppose I began that about 1950.

Russell: So, once you'd started working in CalCOFI, you eventually got involved also with other expeditions that were going and doing deep-sea work in addition.

Reid: At that period, Roger was leading MidPac and other expeditions, in the South Pacific and over the north as well. It was the first time that Scripps ships had ever gotten out of the California Current or the Gulf of California. However, he was operating more as a geologist than geophysicist in that mode, and I didn't go along on those. I was working on the physical aspects of the California Current. I've forgotten what the name of the other one was; it's certainly in Betty Shor's book.⁷ I wasn't on that, in any case.

Russell: There were a whole bunch. There was Capricorn, there was MidPac. There was a whole series.

Reid: Capricorn, that's what I was thinking of. That's right.

Russell: There was another one called STEP-1.

Reid: Oh, that was much later. In the middle fifties, or early fifties, I think. That's one that Warren Wooster⁸ did.

Russell: Did you work much with Warren Wooster?

Reid: We collaborated on one encyclopedia article. He was originally a chemical oceanographer, though he moved more into physical and finally into fisheries oceanography.

Russell: Betty Shor says in her book that you predicted the south equatorial counter-current, the presence of it, and that Warren Wooster's experiments on STEP-1 Expedition actually proved it. They found it.

⁷Elizabeth Noble Shor, *Scripps Institution of Oceanography: Probing the Oceans 1936 to 1976* (San Diego: Tofua Press, 1978.)

⁸Warren Scriver Wooster (b1921), American oceanographer who trained at SIO under Revelle. He served on the faculty of several oceanographic institutions including SIO and the University of Washington. Like Reid, his interest was the descriptive oceanography of the Pacific.

- Reid: That's not quite the right sequence. I didn't predict it from any model. I just observed it in the density structure. Then he went off and took some more [measurements] and thought it was so. I did the same thing for the Atlantic. It was very simple. That's what I began to do: spread out data and look at them and all of a sudden this big sore thumb stuck out.
- Russell: John Isaacs⁹ was the head of the Marine Life Research Program at the time?
- Reid: He became the head in 1958.
- Russell: Oh, okay, much later.
- Reid: Since quite a large number of people had to be assembled to run this—not only the crews of the ships but the technicians to go to sea, and the people ashore to do the calculations—there was a head of that group. It was Admiral Wheelock,¹⁰ a retired naval officer. He died in the middle fifties and that's when John became the director. However, I think Roger was actually running it himself. Wheelock was engaged in other things at Scripps, and the program was running pretty much on its own, because there were a couple of very competent people on the clerical staff there.
- Russell: So, oceanographically, were you doing your own work, or were you collaborating with anyone in those early days in the Marine Life Research Program?
- Reid: I was the only one who wanted to do that kind of work. I was working on the California Current and the physical aspects, and not many other people were. One of the problems was that Sverdrup himself and Dick Fleming, who had been at Scripps at that time, had written a book called *The Waters off the Coast of Southern California*, and they did a grand job on a minimum data base.¹¹ When I did my major work on that, I remarked that they had done remarkably well with the small data set they had. I noticed that when Ron Lynn¹² wrote another one later on, he said that I had done more, that we all had gotten more, than one would have expected with the data base that was there. The data base kept growing, but the results weren't changing very much.

⁹John Dove Isaacs III (1913-1980), American oceanographer and inventor of instruments; joined SIO in 1948; director Marine Life Research Group 1958-1980; director Institute of Marine Resources 1971-1980.

¹⁰Charles DeLorma Wheelock (1897-1980), American rear admiral, served as SIO as associate director 1953-1961 and first director of the University of California Institute of Marine Resources 1954-1961. Wheelock played a major role in planning the UCSD campus with Revelle.

¹¹H.U. Sverdrup and R.H. Fleming, "The Waters off the Coast of Southern California, March to July 1937," *Bulletin of the Scripps Institution of Oceanography* 4(10): 261-378 (1941).

¹²John G. Wyllie and Ronald J. Lynn, *Distribution of Temperature and Salinity at 10 Meters, 1960-1969, and Mean Temperature, Salinity and Oxygen at 150 Meters, 1950-1968, in the California Current* (La Jolla: Data Collection and Processing Group, Scripps Institution of Oceanography, 1971).

Roger was really building Scripps at the time, and we got some more billets out of the state of California. More people were appointed, some under the Marine Life Research Program itself. Roger was trying to build the place up, and he was trying to get into the forefront of the various programs. Now you will hear more about this from the biologists, but he did get a Rockefeller grant to start a huge meeting here of biologists from all over the world on the more modern aspects of it. I don't think they were quite into DNA at that time, but that was the direction they were going.

Russell: What year was this?

Reid: It must have been late fifties, I think, or maybe early sixties. He was working on this for a long while. You don't get a Rockefeller grant in one year.

Roger was trying to build on the strengths that we had, and far and away the greatest strength we had in physical oceanography was Walter Munk at the time. The rest of us were graduate students who hadn't done much yet. And since Walter had written this wonderful article, which was a mathematical model, why there were people beginning to think, "Gee, Walter has done so well here, we're going to be able to do it all by calculations from now on and one doesn't have to go through this nonsense of going to sea."

Russell: They're still saying that! [laughter]

Reid: Therefore, Roger was not really paying much attention. Well, I thought he wasn't paying much attention to the sort of thing I was doing, because it was not in that vein. It was local and was not earth-shaking. Of course I was wrong, because I was too narrow. And that's how the Norpac¹³ thing came along. It was the first time I realized he had a broader view of the whole thing. You've got the thing on the Norpac. We were making monthly cruises off the coast of North America from Washington down through Baja California.

Russell: That's part of the Marine Life Research Program.

Reid: That's right.

Russell: What became CalCOFI.

Reid: Yes, it became CalCOFI. Well, let me see. I went to Canada for a month, not because I was invited, but because I wanted to go over because

¹³ Norpac Expedition, July-September 1955, included R/V *Horizon*, R/V *Hugh M. Smith*, R/V *Paolina-T* and R/V *Stranger*.

my wife¹⁴ was British and on a Fulbright fellowship and her visa was running out. That was during the nastier parts of the McCarthy period, and in fact the immigration service was so beat down. McCarthy was so hard on them they were afraid to open their mouths. Even a question like, "How can I get a visa for my wife?" they were reluctant to answer.

Finally, one old fellow took me over to a corner and said, "Look, you can't get a visa except being out of the country. Mexico is close, but there are thousands of people down there waiting. Why don't you go to Canada?" So I wrote a letter to John Hart,¹⁵ the only name I could find in an oceanographic institution up there in Nanaimo, Vancouver Island, and said, "I'm coming over. Do you have something for me to do?"

We drove up and she had to get all the papers. We got to Vancouver and she had to go on alone to talk to the consulate. She wasn't there long. They gave her a lecture: "That's not what Fulbright fellowships were for. But come back next week and you can have the visa."

Russell: Ah, marvelous.

Reid: So we had nothing to do then except wait for the week. We went over to Vancouver Island and met John Hart, the head of the Pacific Biological Station. He'd never heard of me, just got this letter, but he invited us to dinner with Jack Tully.¹⁶

It turned out that I didn't really have to have a job now. Jack and I talked for a bit and he said, "Why don't you stay here for a month? We've got a residence you can have free and work on some of the stuff we're doing." So I did. And that's when the business of the other paper by a fellow named Doe,¹⁷ who'd been working there, came up. He had some data off the west coast of British Columbia, good data, except at a place where the currents come in and split, and it was very messy—no pattern to it. I realized that when you stuck these two things together, why all of a sudden it all made a great deal of sense. That was fine. But then it struck me, or maybe it struck Jack, that if doing it on that scale was good, why not think bigger? So we decided it would be nice to speak to the people at the University of Washington who had a ship, and his Canadian vessel, and a Scripps CalCOFI, and Bureau of Commercial Fisheries.

Russell: This was between you and Jack Tully?

¹⁴ Freda Mary Hunt Reid (b1926), was trained as a botanist in England. She married Joe Reid in 1952 after completing graduate work on algology and ecology at Ohio State University. She worked largely on phytoplankton as a researcher at the Scripps Institution of Oceanography beginning in 1952.

¹⁵ John Lawson Hart (b1904), director, Biological Station, Fisheries Research Board of Canada, Nanaimo, 1950-1954.

¹⁶ John Patrick Tully (b1906), oceanographer, Fisheries Research Board of Canada.

¹⁷ Learmont Anstice Earlston Doe (b1916), physical oceanographer, Fisheries Research Board of Canada, 1949-1952.

Reid: Tully. John P. Tully.

Russell: This was during the month that you were up in —.

Reid: That must have been in 1953, I think. We thought if we got hold of another Bureau of Fisheries outfit in Hawaii, we could cover the whole northeastern part of the Pacific. So I came back—.

Russell: Would you like to stop for a minute and get a glass of water?

Reid: That's all right. I came back and talked to Roger about it, not knowing what he was going to do because he hadn't shown much interest in that sort of thing or particularly what I was doing at that time, except at the CalCOFI meetings. But he thought about it for a while and apparently decided we were thinking too small. Why don't we get the Japanese in this as well and cover everything north of twenty? They couldn't do it in 1954, the next year, but they could get it set up by 1955.

Roger went to Japan. Mind you this was less than ten years since the war was over, and Japan was not in the best of shape. They didn't trust Americans and were not trusted by Americans, so the matter of such collaborations as we had to do was something of a worry. But it all came off beautifully.

Russell: So Roger made the first step toward forming a collaboration?

Reid: Yes, it took someone in Roger's position to do that, to get the attention of these other people.

Russell: How many of their ships participated?

Reid: I think I've got the numbers over here. There must have been twelve or thirteen Japanese ships.

Russell: Really? How many total ships were there in the expedition?

Reid: Sixteen or seventeen, I think.

Russell: Is this the biggest expedition that Scripps had put up yet?

Reid: Oh, yes, the only time that any substantial number of Scripps ships had collaborated with each other.

Russell: Was for MidPac and Capricorn?

- Reid: Those were single-ship efforts.¹⁸
- Russell: Those were single-ship efforts? I had no idea. So that was the first multi-ship—.
- Reid: Yes, that's right.
- Russell: Multi-international effort that Scripps had put together. It wasn't just Scripps. It was between you, Roger Revelle, and Jack Tully.
- Reid: And that worked great. And in fact that was satisfactory. Another fellow, Townie Cromwell¹⁹, a graduate student at the time, had moved to the Inter-American Tropical Tuna Commission, which was on campus, and he was going to do a cruise in the eastern tropical Pacific the next summer. And someone, I don't think it was Townie, it must have been me, said, "Gee, we've got north of twenty north, and now he wants to do from twenty north to twenty south on the eastern end. Why don't we do twenty north to twenty south all the way across to the west? And we did that one, called it Equapac, and that was carried out, too.
- Russell: That was in the late fifties?
- Reid: That was in 1956.
- Russell: Okay. Right on the heels of the Norpac Expedition.
- Reid: Yes, that's right. Tying that CalCOFI data into the Canadian data, and then conceiving of doing the whole north Pacific and having it work out, and the pictures it presented were the thing that really got me started on the very large-scale operations.
- Russell: So what were the major challenges you came across in organizing such a large expedition?
- Reid: Oh, not much. The Japanese were extremely willing to do this. Tully was interested. The fisheries program was run by the local Bureau of Commercial Fisheries, and the people in Hawaii all wanted to do the same sort of thing. This was by and large, people doing the same sort of thing they wanted to do anyway, just on a different and perhaps larger scale. I don't know what Roger had to do in Japan to persuade them to do it. I think they were quite willing to do it as well, but it was not a matter of having to argue hard for it. It seemed a good idea at the time. It wasn't a matter of great diplomacy or anything like that. It all fit together.
- Russell: Right. The funding. Did each institution fund their own ships and their

¹⁸ MidPac and Capricorn expeditions both used two vessels.

¹⁹ Townsend Cromwell (1922-1958).

own personnel?

Reid: Yes. Now, I'm a little bit vague about this. Somebody else will have to tell you, but at that time the CalCOFI monies came from the state of California. The Marine Life Research Program was running this and hired the technicians. The other costs, I'm not sure that those costs would have covered the extensive work for that Norpac trip. The rest of the institutions, that is, Scripps and the University of Washington, received block grants from the Office of Naval Research at that time, and that helped to get it through. How the Canadians and the Japanese funded theirs, or the Bureau of Commercial Fisheries, I don't know. But they did manage to.

The issue of what the ocean looks like, that is, what had been called—nobody calls it anything anymore—the dynamic method of looking at the density field and getting the relative geostrophic flow, had not really been heavily implemented. It was suggested a long time ago, and bits of it done, but Albert Defant in Austria had taken the data and used these calculations to do the circulation of the Atlantic Ocean from fifty north to fifty south. The trouble is, he does not make it clear how he had done it. He was a mathematical physicist, and it sort of had to have a theoretical justification or it wouldn't be respectable. So he tried to convince people that when you start integrating, you've got the vertical shear and wherever the shear is zero, that means zero flow.

He selected a zero surface that way and integrating above that he would get the flow going the right direction, then he would get the transport as well. You see, I'd already found out, while working for Walter, that his surface wasn't any good. The trouble is nobody could duplicate his work, and nobody could believe any other thing, so they threw out the whole method. He published that in 1941. When we got together after Norpac cruise—we got together in Hawaii in the fall of 1955, and everybody spread out his data on geostrophic flows, zero over a thousand decibars. I believe that Roger and several other people there were not even sure that it would be a coherent picture. But it was. When I saw that, why, I got very interested. I did mention the picture that I'd been concerned with.

Russell: What were the major scientific results?

Reid: First, we were able to make a map of the north Pacific Ocean and later the equatorial Pacific which showed the flow of water above a thousand decibars which is pretty much the sense of flow everywhere. The value isn't quite right because there is more deep flow. The pattern of zooplankton over the whole Pacific was clearly shown, and the distribution of characteristics of phosphate and oxygen. We couldn't really do silica or nitrate in those days.

The principal effect was on me. I'm not sure how many other people cared

about this, come to think of it. I sent a copy of the atlas to Woods Hole, and they thought it was a gift. They didn't review it. They didn't think it was of a level to justify reviewing. [laughter] If they put it in the library, that's fine.

Russell: Did you have any close colleagues at Woods Hole?

Reid: Well, I knew the people there. We didn't collaborate. What I knew most were, of course, Fritz Fuglister²⁰ and Val Worthington.²¹ I began to be acquainted with them in the middle fifties. They didn't visit very much, but they were doing not exactly the same sort of work. They had the same sort of purposes but were going about it in different ways. We got on all right.

Russell: Did you ever interact with Margaret Robinson²²?

Reid: Oh, yes, Margaret and I were *very* good friends. She was working with the Bathythermograph Section when I first came. Have you ever gone to interview her? No, it's too late, I guess.

Russell: Yes.

Reid: She took the courses and wrote a number of papers and proved to be the most magnificent manager of several people working with BT²³ slides. No commercial outfit could do it with anything like the speed, efficiency or cost.

Russell: In Betty Shor's book she says that Margaret was involved in the first North Pacific Ocean Atlas.

Reid: That's not this one. It was a great disappointment to her that her later atlases didn't get finished, or printed.

Russell: I've got just one other person that I wanted to ask you about, which was Snodgrass.

Reid: Jim Snodgrass.²⁴ He was the electronics technician for the area.

²⁰ Frederick Charles Fuglister (b1909), physical oceanographer, Woods Hole Oceanographic Institution.

²¹ Lawrence Valentine Worthington (b1920), physical oceanographer, Woods Hole Oceanographic Institution.

²² Margaret King Robinson (b1906), got an M.S. degree in physical oceanography from UCLA for work undertaken at SIO. She worked at SIO beginning in 1956 as supervisor of the Bathythermograph Data Analysis and Processing unit. She was a consultant and eventually established her own firm in La Jolla, California called Compas Systems, Inc., Data Processing.

²³ The mechanical bathythermograph (BT) was invented by Athelstan F. Spilhuus. It recorded ocean temperature structure on smoked glass slides.

²⁴ James Marion Snodgrass (b1908), oceanographic instrumentation engineer, marine biologist, and head of Special Developments Division, SIO.

- Russell: Did you interact with him much? I understand he essentially invented XBTs²⁵.
- Reid: No. I don't know. He worked on them, but I don't know whether he had anything to do with the company that manufactured them or not.
- Russell: I was curious if you'd run across him. Betty Shor's book goes into detail on how they came up with an XBT and I was curious.
- Reid: Well, if she says so, she's probably right. I wasn't concerned with the XBT. You're sure it's XBT, not STD, you mean?
- Russell: No, thid specifically expendable, the one with wire on it.
- Reid: Okay, XBT.
- Russell: Actually, those were the major questions I wanted to ask you for this first interview. So, if it's OK with you, I'll end there. ##

²⁵Expendable bathythermograph (XBT), an improvement over the mechanical bathythermograph, was developed in 1960 and recorded results on paper instead of slides.

INTERVIEW TWO: 11 FEBRUARY 1999

Russell: We kind of already started into the second part of the interview, which was developing reputations, both yours and Scripps.

Reid: Heavens!

Russell: Heavens. They actually in some ways developed together. So, the first major expedition you organized was the Norpac Expedition, and we talked about that a little bit last time. I was hoping you could contrast the challenges of organizing a big expedition like that in the 1950s with what it takes to organize such an expedition like WOCE today, the World Ocean Circulation Experiment. What were some of the differences between organizing something like Norpac and organizing something like WOCE?

Reid: Well, first I'll give you another example. After Norpac in '55, the Inter-American Tropical Tuna Commission wanted to do some work in the equatorial eastern Pacific, both sides of the equator. They were going to go out to a thousand, two thousand miles offshore, and it seemed obvious to me that why not carry it all the way across? Because then we could tack that on to some of our twenty south. We suggested that, and Scripps put in another couple of ships and the Japanese cooperated again, so that we managed to get twenty north to twenty south all the way from South America to Asia. That again was done with more genuine interest, everyone doing his own self interest, than any great corralling or lobbying.

Russell: How was the data shared and reported?

Reid: Everybody gave everybody else his own data. This was never an issue in those days. One went on an expedition but the data were held in common. As a matter of fact, another thing about the Norpac, the thing that got me into anything like biology, was the fact that we did need to draw maps and had to choose among the people on Norpac. Somebody got the oxygen, somebody got the silica, somebody the temperature, somebody the phosphate, etc. And somebody was going to do the zooplankton.

The fellow who was doing the zooplankton threw up his hands. It was just too messy. I was the editor of the thing, so I thought we'd better try something on this, so I tried, and it was rather splotchy. There were big signals, but it was sort of empty in mid-ocean and high along the equator and high latitudes along the eastern boundary. Of course these were net hauls made from one hundred and forty meters to the surface, or something like that, and there was a great deal of variation in them. One of them was also the day/night variation, the night ones always got a lot more than the day ones. I don't know what I did, but I did try to reduce the nights to match the day or the day increased to match the night, and found

that it was to a first-order contour. That is, one cannot vouch for every point we got there, but the whole thing is reasonably so. Then it also occurred to me that it was very intimately related to the patterns of phosphate. That led me to look around and find more phosphate and more zooplankton data and map the whole Pacific. I did manage to find it. The correlation clearly does hold up. That was the only paper I ever had turned down.

Russell: Really! Where did you submit it?

Reid: I submitted it to *Deep-Sea Research*. The very nice and honorable lady, Mary Sears,²⁶ was editor at the time, incorruptible and fair. Mind you, this is a personal view, which was probably correct, but other people might object to. Mary was quartered at Woods Hole, she was in the biological group there. She said that whenever she got a manuscript, she sent one copy to someone she knew at Woods Hole and one copy to somebody else. Well, I think we were confused. The problem was that some of the biologists objected because this rather suggests that phosphate with its nutrients and zooplankton are directly related.

Russell: And this was a problem?

Reid: It was a problem, because all of the work had been done over time, you see. The big thing at the moment was the work that had been done in the North Sea and the English Channel, a sequential thing. In the winter time there is no growth of any kind. Nutrients accumulate, spring comes on, and tiny little mixed layer and the phytoplankton begin to grow like mad and graze down the zooplankton. By the end of summer the zooplankton have come alive and eaten all the phytoplankton, so now the nutrients are gone. So clearly, nutrients and zooplankton are inversely related. In other words, I didn't have a license to discuss zooplankton; they told me that wasn't fair.

Russell: Because you weren't a biologist.

Reid: And furthermore, I think the Woods Hole people did not like this large-scale approach at the time, so they turned it down. So I sent it to *Limnology and Oceanography*, and they published it without even changing a word. And the biologists seem to like that now. In fact, a year or so ago somebody called from ASLO²⁷ and asked my permission to put [the zooplankton map] on the issue of some volume they were putting out.

Russell: During this whole period of expeditions, you were still working with the Marine Life Research Program?

²⁶ Mary Sears (b1905), marine biologist, Woods Hole Oceanographic Institution.

²⁷ American Society of Limnology and Oceanography.

- Reid: Yes, that's right. I was also writing papers on the California Current until several years ago.
- Russell: So although you focused on the hydrography, you did have contact with people who were working on the biological aspects?
- Reid: Yes, in fact when I was director of the Marine Research Group, MLRG didn't always get along too well. We wrote one paper trying to put the other part of it together as the various species related to the distribution and circulation in general. This was a product of the Marine Life Research Program.²⁸ Ed Brinton²⁹ and Abe Fleminger³⁰ and Elizabeth Venrick³¹ and John McGowan³² had all been working on the biology. We expanded that into an account of the relation of the circulation of the nutrients, not just to the zooplankton volume, but to individual species. There are essentially three great divisions in the zooplankton space: empty spots in the centers of the gyres and the rich ones here and here and here [pointing to the area along the coast of Japan on a map] and here [pointing to the coast of California] and here [pointing to the area along the coast of Peru] and the rich ones along the equator.
- Russell: There are rich ones along the Kuroshio?
- Reid: Yes, axis of that gyre, the subarctic gyre.
- Russell: Right. And then the subarctic.
- Reid: It turned out there were some data that could do this in the Atlantic as well, and the same patterns emerged. But the thing that was most obvious was that each of these areas has particular niches. That's the kind that lives in the subarctic zone only, one other one and some that live only around here in that circulating system [pointing to a map].
- Russell: Around the Antarctic.
- Reid: And some that live only in these empty spots.
- Russell: Only in the actual gyre spaces.
- Reid: Yes, that's right. Some that live. . .
- Russell: Only on the equator?

²⁸ Joseph L. Reid, Edward Brinton, Abraham Fleminger, Elizabeth L. Venrick and John A. McGowan. Ocean circulation and marine life. In: *Advances in Oceanography*, edited by Henry Charnock and Sir George Deacon. New York, Plenum Press, 1978, 65-130.

²⁹ Edward Brinton (b1924), biological oceanographer, Marine Life Research Group, SIO.

³⁰ Abraham Fleminger (b1925), invertebrate zoologist, SIO.

³¹ Elizabeth Louise Venrick (b1941), research oceanographer, Marine Life Research Group, SIO.

³² John Arthur McGowan (b1924), biological oceanographer, SIO.

- Reid: Well, in this case these are the empty spots, the great desert domains, which have something and the equatorial types of various kinds. Some inhabit both the equatorial and the mid-latitude gyres. Some curious ones live almost everywhere, some cannot abide the low oxygen domains of the Eastern Tropical Pacific, and some that just plain love them.
- Russell: This was published when?
- Reid: This came out in '78. It was a symposium on ocean circulation of marine life, so we decided we would write a paper on ocean circulation of marine life.
- Russell: How did you divide your time between the expeditions that are blue-water hydrographic work and your Marine Life Research Program?
- Reid: I don't recall any abiding interest. Just worked on what was at hand. The writing of this the zooplankton species things had all been prepared by these other people. My role was just sticking it together and organizing them by the particular areas they were working on and relating them into the circulation, the rich productivity domains and lesser ones, plus a few odd-ball remarks about how it all works out.
- Russell: In the fifties and sixties, how did SIO, under Revelle, divide the resources between coastal work, like CalCOFI, and blue-water expeditions?
- Reid: By the time that came along, block funding, I think, was pretty much at an end, but it was done with individual proposals.
- Russell: When would you say that started?
- Reid: Oh, it began certainly in the early sixties, I think. The Marine Life Research Program, of course, had funds to do the CalCOFI work and other more general research as well. I was lucky to have a billet on one of those projects that were set up by the state of California for the University when the Marine Life Research Program was begun. So my income, my salary, was at least secure. I didn't really know it at the time. When they first hired me, back in the early fifties, I had to sign a new contract every year because they kept changing the titles and furthermore that was the period of loyalty oaths and the like. So every time I got a new contract extended I had to sign a new loyalty oath. And I signed them because I had a wife and two small children at the time. I certainly did not like them or the requirement of signing them.
- Russell: You'd already served in the navy.
- Reid: That didn't count, you know. There was no way out of it. A lot of people at Scripps and at the University in general refused to sign and

finally that requirement was taken away. But I wasn't among that group, I'm sorry to say.

Russell: Was it contentious among your colleagues?

Reid: Well, so far as I knew, everyone in the University and at Scripps was opposed to it, but it is rather demeaning. After all, anyone disloyal to the United States and planning to do it in would certainly sign that thing a dozen times a day with no qualms of conscience.

Russell: I can see that would have been difficult, at the minimum, annoying.

Reid: The McCarthy era was not quite over by that time.

Russell: So, did the way that you were able to spend time to do deep-water work and doing your CalCOFI work, does that ever change up until you became the director of MLRP, the Marine Life Research Program? Did you always find it easy to do the work that was at hand, or did you have to worry about [it]?

Reid: It was a wonderful place to work. Nobody ever told me what to do. Occasionally, John Isaacs,³³ when he was the director, would ask me to do something or to write up some more things on the California Current. I did. That was it. I was never told not to do anything, or that I had to do much of anything. It worked like a private institute for advanced studies, so far as I could tell. It was great. It was a wonderful place to work. I got some support from the University for a couple of engineering aides for a while, and then, when it became obvious that I could also get money out of NSF³⁴, I did that and did not use the IMR³⁵ money. From the time I was director of MLR, I never spent any money on my research. I could get it easily from NSF and ONR. MLR had money in those days, which were used for various other things, but not for my work.

Russell: Were you involved in helping mentor or teach at that time?

Reid: I don't know when I began to teach. It must have been the early sixties or something like that. Middle sixties, late sixties. All of a sudden! At that time I was a researcher.³⁶ Joe Curray³⁷ came and told me I was a

³³John Dove Isaacs (1913-1980), came to the Scripps Institution of Oceanography in 1948 as Associate Oceanographer and served as Assistant to the Director 1948-1958. He was Director of the Marine Life Research Program 1957 and served as Director of the Institute of Marine Resources 1961-1962 and 1971-1980.

³⁴National Science Foundation (NSF).

³⁵University of California Institute of Marine Resources (IMR).

³⁶SIO has two separate employment categories for scientists, Research Series and Faculty.

³⁷Joseph Ross Curray (b1927), oceanographer, got a Ph.D. for his work at SIO in 1959. He was chairman of the Graduate Department 1973-1975, chairman of the Geological Research Division 1976-1978, and chairman of the SIO Faculty, 1983-1986.

professor now. It didn't make any difference so far as I could tell. But Scripps was growing rapidly at that time. Biologists or somebody will talk about the Rockefeller grant and the great conference held here in early '60s. I had nothing to do with that. It was just of some general interest that it was taking place. Roger was still doing his best to get us moving along in the right directions.

Russell: What were the right directions?

Reid: Well, of course again, I still had a sort of worm's eye view of it. Obviously, the most important thing to do in the world was to continue to work on the general circulation. No question about that. But geochemistry came in and...

Russell: When was that? Was that just for GEOSECS³⁸?

Reid: When did Hans Suess³⁹ and Wally Broecker⁴⁰ begin to deal with this sort of thing? How did I get involved? It must have been in the middle sixties. There was a long cruises from Australia to Chile. I wrote a proposal. I wanted to do a north-south line out of the mid-South Pacific. Henry Stommel⁴¹ and Bruce Warren⁴² wrote a proposal to do some east-west lines. NSF decided they didn't want to do both of them, why didn't we get together. So I was out voted and we all took these east-west lines. And on the southern one at forty-three south, why Henry was along and it occurred to me that this was, at least, one of the longest legs ever done.

Russell: How many days out were you?

Reid: Oh, I've forgotten, fifty odd, maybe sixty. We did stop for four hours in New Zealand to let an observer off.

Russell: Four hours. That doesn't count.

Reid: We had the Aristotelian unities of time, space and personnel. The gear was the same, the CTD⁴³ was the same, the people were the same, and

³⁸Geochemical Ocean Sections Study (GEOSECS) was a multi-institutional international oceanograph program which began in 1971. This NSF sponsored International Decade of Ocean Research (IDOE) project made detailed measurements of the physical and chemical characteristics of ocean waters in the Atlantic, Pacific and Indian Oceans. SIO geochemist Harmon Craig was a member of the executive committee of the project.

³⁹ Hans Eduard Suess (b1909), chemist, UCSD.

⁴⁰ Wallace Broecker (b1931), geochemist, Columbia University.

⁴¹ Henry Melson Stommel (b1920), oceanographer, Woods Hole Oceanographic Institution, Harvard University, and Massachusetts Institute of Technology.

⁴² Bruce Alfred Warren (b1937), physical oceanographer, Woods Hole Oceanographic Institution.

⁴³ Conductivity, Temperature, Density (CTD), an electronic instrument dropped from an oceanographic vessel to record conductivity, temperature and density against depth. This information could be used to calculate salinity.

while the results might not be as perfectly accurate as we would like, they were certainly going to be consistent. Wally had been making arguments on one carbon-14 sample in the Atlantic and Suess from another on the Pacific. It occurred to us that maybe the geochemists ought to get some more data at sea on some such cruise as this, what Henry and I thought of was maybe a north-south cruise from the Aleutians down to as far as we could get toward the Antarctic. Then look at those results and see what they would find. We decided that Henry should discuss this with them, because his suggestions had a good deal of force. He did, and of course the geochemists grabbed the ball and ran with it and we got three oceans. They were a bunch of madmen. I dropped out of the GEOSECS organization; it was just too much for me. I thought they were going to quit rather than me. I was afraid that when the ship was supposed to leave for the first leg, two of the people, the geochemists, would have a great fight and they would both leave.

Russell: Were they quarrelsome?

Reid: They were. They were an awkward lot to get along with.

Russell: Who do you remember in this awkward lot? Who were the people?

Reid: Well, for example, Craig, whom you know here. He wasn't very different in those days, maybe a little worse, I'm not sure.

Russell: Hmm. Harmon Craig.⁴⁴ Wally Broecker?

Reid: And Wally Broecker, and Karl Turekian⁴⁵ on occasion did things of that nature. They would start fights with even moderate people.

Russell: Did they make good sailors?

Reid: Well, I never went to sea with any of them, so I don't know, but they got the work done.

Russell: We're still using the GEOSECS data today.

Reid: Yes, that's right.

Russell: So, how did SIO contribute? SIO or Roger Revelle? How did he help you get your work done? Or were you insulated from that by being involved with MLRP? Did he help bring in good students?

Reid: I never had interest in undertaking administration. Somewhere in

⁴⁴ Harmon Craig (b1926), geochemist, SIO.

⁴⁵ Kark Karekin Turekian (b1927), geochemist, Columbia University, Lamont Geological Observatory, and Yale University.

the late sixties, I think, we set out to do something like that. Fred Spiess⁴⁶ asked me to become chairman of the Ocean Research Division⁴⁷, which was a different thing in those times, and, knowing that I had no experience in this sort of office work, why, they gave me a Management Service Officer, Jim Faughn, who knew everything. He was the nicest fellow you'll ever meet. He was the ideal administrator. He had a talent. He could tell six months before it was going to happen that some trouble was going to happen, and he would take steps to fix it so that nobody ever even knew that there might have been such a fuss.

Russell: Amazing. We need more of those.

Reid: So, this was a matter of taking care of their academic promotions, whether it's sitting on the paperwork side, small budget to administer, really not much to do. They were an oddball group that didn't get along with anybody else at the time. Then after a few months they took Jim Faughn away from me. I felt betrayed. They sent Dave Wirth, who I didn't know very well. He'd been a marine technician, I knew him, but it turned out that he operated in a slightly different fashion but it was just as effective.

We had some terrible problems before us, such as so-and-so wants to hire so-and-so as a draftsman and somebody else who was clearly opposed wants so-and-so. When troubles of that kind came up, Dave would present the problem to me, and if I listened very carefully, I would realize that he was also very cautiously suggesting a solution. And when I got it quite clear in my mind, I would give him a direct command to do that.
[laughter]

In that division eventually the people made peace with the other divisions and went into them. No, somebody else did follow me, that's right. I guess maybe at the time I became professor I got out of the job. I don't think I was fired.

Russell: Oh, you became Marine Life Research Program director in 1974, so at least by then...

Reid: I think I was through with the Ocean Research Division by that time, I'm not sure. Those were all so unimportant to me that I have trouble finding the memory. Being chair of that division certainly was not a great bother. That's not to say that I carried it off perfectly well. I didn't really take it very seriously.

⁴⁶ Fred Noel Spiess (b1919), oceanographer, director, Marine Physical Laboratory, SIO.

⁴⁷ On February 1, 1962, SIO was administratively reorganized into departments, divisions, laboratories, organized research programs and groups. The Oceanic Research Division (later Ocean Research Division) was the largest research unit at SIO. It supported researchers in physical oceanography and researchers interested in the close interaction of biological and geological problems with the marine environment. Professor Reid chaired the Ocean Research Division 1968-1973.

Russell: Do you remember what the comparative reputations were in expertise between Woods Hole and Scripps?

Reid: In what sense?

Russell: Well, your work focused a lot on the Pacific and Woods Hole people had been focusing on the Atlantic. Then, again, there's sort of the age-old axiom that Scripps turns out experimentalists—people who go to sea—and Woods Hole turns out theorists.

Reid: That's not strictly true about Woods Hole. They did have the oldtimers like Fritz Fuglister, Val Worthington, and Metcalf⁴⁸, who was less well known, were certainly the experimentalists. I knew them very well and regarded them very highly. The theoreticians, well, they showed up from time to time, but until Fofonoff⁴⁹ came along and two or three others aside from Henry⁵⁰, of course, who was there most of the time but not all the time. That's the only part of Woods Hole that I knew about, the people who were doing experimental work.

Russell: Fofonoff is the one who did the original equation of state and sea water?

Reid: That's right. How different were we? The methods that we were using; Woods Hole was measuring temperatures and oxygen and sometimes phosphate, and so were we. Woods Hole, I think, did as well as anybody could do at that time. They were the first major users of the conductivity measurements. We got into that too. But Worthington went to sea. Once we were going toward the Ross Sea south of Australia, and he said he would like to go along and see how we did things. So [Worthington] came along and worked as a marine technician as they were doing it all. And Arnold Mantyla was along. He was very shocked to find that—these were the reversing thermometer days and we always put two protected and one unprotected on every bottle because these things don't work perfectly all the time—they usually either work very well or not at all. But if you've got two down there it's easy to tell which one it is, and Arnold found to his horror that Woods Hole used only one protected thermometer. So they had only one reading of temperature, and we had two. So I can't fault their salinities. On the other hand, up to that time they'd been measuring temperature and salinity, but their oxygens were not well measured. I don't think they took them seriously. They took their salinities and temperatures very seriously, and they were very well done. They just didn't have anybody there who cared about oxygen.

⁴⁸William Gerrish Metcalf (b1918), physical oceanographer at WHOI beginning in 1952.

⁴⁹Nicolas Paul Fofonoff (b1929) was an physical oceanographer at WHOI 1962-1969 who later became the Gordon McKay Professor of the Practice of Physical Oceanography at Harvard.

⁵⁰Physical oceanographer Henry Melson Stommel (1920-1992) was at WHOI 1944-1960, and returned to WHOI in 1978 after working at Harvard and MIT. Stommel was one of the most distinguished American oceanographers of his generation.

- Russell: Have they improved since then?
- Reid: Well, we've done our best. I'm not sure they use the Carpenter method yet.⁵¹
- Russell: I don't believe they do actually. That was kind of a sandbag question.
- Reid: Titrator or something like that.
- Russell: They use a similar automatic titrator to the one we use, I believe, but they use a different amperometric detection system. Ours, I believe, is better. But then maybe I'm biased.
- Reid: There aren't many people there concerned. McCartney⁵² uses oxygen now, so he'll keep an eye on it. If it looks as if I'm digging at Woods Hole, I don't mean to. Their interests are not quite like ours.
- Russell: That was what I was curious about. How do we differ from Woods Hole? Are we just simply looking at different oceans but in a similar way, or are there some fundamental differences in the way we go about oceanography?
- Reid: They're not terribly different, they're more a matter of an individual investigator rather than anything else. What McCartney does is very similar to what I do. We look at things in a slightly different way, but what we are after in the end is the same thing about the circulation. When he thinks I'm not quite right, I think he's not quite right. Bruce Warren is both an experimentalist and a theoretician.
- Russell: Well, I think that was pretty much what I had hoped to ask you for the second interview.
- Reid: Fine.
- Russell: The last piece would be looking at sort of the broad-scale view of what oceanography is and how well do we organize to accomplish our goals, but we can get to that maybe next time. Is that all right?
- Reid: Yes, fine. ##

⁵¹J.H. Carpenter, "The Chesapeake Bay Institute Technique for the Winkler Dissolved Oxygen Method," *Limnology and Oceanography* 10 (1): 141-143 (1965).

⁵²WHO'S THIS???

INTERVIEW THREE: 18 FEBRUARY 1999

Russell: I'm here with Joe Reid for the third of three interviews with him for the centennial oral history project. Today is the 18th of February. I wanted to talk today about your thoughts about the work that we're doing today at Scripps. I know you were very involved in the World Ocean Circulation Experiment including serving as a chief scientist on WOCE Hydrographic Program Leg P-17, the leg in the South Pacific. What did you think SIO's role was in WOCE? Were we significant participants?

Reid: We certainly were, I think. Scripps people were chief scientists on many of them, and, more importantly than that perhaps, the Scripps methods were used on most of the legs. The people who did the work were essentially the Scripps technical people, our Ocean Data Facility, because they've done a better job of this sort of thing than anybody else.

Russell: What sort of things? You mean the oxygen and nutrients?

Reid: The measurements of CTD, salinity, temperature concern. Woods Hole and others were up to par as well, but on the oxygen and nutrients, no other groups could quite match the accuracy that Scripps could. So far as planning the work is concerned, WOCE proved to be not exactly the well planned coverage of empty areas that one might have thought. It did something of that, but it was also a little bit of a hodgepodge of particular cruises that people wanted to take, whether it was of the general interest or not.

Russell: I know you managed to accomplish Norpac all in one season, all in one year.

Reid: Norpac was one cruise carried out by about sixteen ships, June through September or October of 1955. Equapac went on the next year and was about three months also. With the expeditions that followed, Norpac and Equapac covered everything down to twenty south. Of course one-ship operations such as GEOSECS and WOCE couldn't hope to do anything seasonal; in fact, both programs avoided winter in high latitudes. I pointed out in my oxygen argument that GEOSECS was never poleward of thirty degrees latitude except in high summer, so we don't know anything from those two programs about winter conditions in high latitudes.

Russell: Do you think that was significant, that WOCE was—the cruises were spread out over a period of years?

Reid: That was pushed by the economics. That is, we couldn't equip enough ships to work both summer and winter. That is, to have gotten winter data in high latitudes as well would have been at least a third more in cost, and it was expensive enough as it was. Whether we could have

found enough scientists who wanted to work in high latitudes in winter is another matter too.

Russell: You did take Norpac out.

Reid: Norpac was a summer cruise. I've been in high-latitude winters in the Bering Sea and Okhotsk Sea, and Greenland Sea, but Val Worthington had been up there as well in the Labrador Sea and along the coast of Greenland in winter, and I think in even worse conditions than I had.

Russell: How do you think a coordinated international program like WOCE compares with something that was single ship like GEOSECS?

Reid: Well, I think WOCE was simply a large-scale GEOSECS laid out with emphasis on physical oceanography as well as geochemistry. On GEOSECS the station pattern reflects the notions of the geochemists of where they wanted to sample, not any array of data which would allow circulation to be entered into directly.

Russell: The results from the two programs: are you confident that WOCE will produce as many papers? I mean I know that many of the geochemists still use GEOSECS data, even today. Do you think the WOCE data will be as successful in generating new ideas and new work?

Reid: Well, that depends really upon what people do with the WOCE data. We're at the first stages; we've got a fair number of small area studies, very neat studies here and there, and the real test will be the findings about large-scale circulation. This is of local interest. Other people will be more delighted or really delighted with some solution in a particular place. But this depends upon what physical and chemical oceanographers can do with the data explaining the circulation and what the modelers can do. Whether the modeling skills will be enough to make a proper use of these in the next few years is yet to be seen. Modeling is hard.

Russell: Not having done it.

Reid: I haven't done it.

Russell: Myself neither. The oceanographic community is now discussing the climate variability and predictability program, kind of a son-of-WOCE program called CLIVAR.⁵³ It's going to combine oceanographic measurements with modeling research. What do you think of the new integrated approach to ocean science?

⁵³Climate Variability and Predictability (CLIVAR) is an interdisciplinary research effort within the World Climate Research Program focusing on the variability and predictability of the slowly varying components of the climate system.

- Reid: I really don't know much about how they are going to go about this. Certainly, an integrated approach is appropriate since it's one fluid that we're dealing with but what the appropriate interactions should be to get them all together, well, it is highly desirable. I remember Henry Stommel once said that he didn't think much of the American Meteorological Society. He said all they wanted from us was the surface temperature. I told this to some climate scientist, and he said, "Yes, that may be true, but all you want from us is the surface winds." In other words, they're really split.
- Russell: How do you think SIO should be involved in this new program?
- Reid: Well, I think that they should. We have quite a bit of expertise here in this sort of thing and of course with the meteorological studies that are being taken care of here by the climate group, I think they should take a very important, perhaps a leading role. I don't know enough about the program to know exactly how they're going to carry this out. That is, simultaneous atmospheric and hydrographic measurements. I have to think what that means logistically.
- Russell: Should we be pushing to make more hydrographic measurements?
- Reid: Every physical oceanographer, most of us who work with hydrographic data, think there certainly are a few more places where we need to get good data. If this is done for the sake of CLIVAR, it may have a different framework of observations from my particular interests.
- Russell: I'm curious. Where in the ocean would you like to have more measurements?
- Reid: Well, I would like, from a simple point of view, two or three more good north-south lines across the equator in the Atlantic Ocean. When I did the Atlantic Ocean, I had to skip everything between seven degrees north and seven degrees south or thereabouts, because there simply were not enough good stations reaching to the bottom arrayed in a fashion to really talk sensibly about the circulation there. Tsuchiya⁵⁴ and others have done nice work with shallower data, a thousand and fifteen hundred meter stuff, but they weren't deep enough for my work. In fact, whenever I finish the Indian Ocean, which is unlikely, if there are some more Atlantic cruises I might try to go back and finish off the Atlantic properly, as well as tying them all together someday.
- Russell: Is there any other spot you think where we need attention?
- Reid: I would like to see some more good measurements in the

⁵⁴Mizuki Tsuchiya (b1929) is a physical oceanographer associated with the Scripps Institution of Oceanography since 1969.

southwest Indian Ocean. I would like to see quite a lot more in the north Indian Ocean, particularly in the Bay of Bengal, because WOCE had a limited array of data there. They need some seasonal coverage. And the north Indian Ocean would be wonderful. The data available to work with there is very limited. WOCE went up the east side once and part way up the west side once, but the circulation that takes place within the Arabian Sea and the Bay of Bengal can hardly be addressed by the WOCE materials. That would be really tough. It's a void of good data. The International Indian Ocean Expedition took quite a lot of data there, but most of it was shallow and very little of it was of the high quality we need because in the deeper waters of the north Indian Ocean, there isn't much of a shape as defined by the present data. After all, it's the turn around point of the waters coming in. By the time they've been mixing both horizontally and laterally for a long distance, and it doesn't have the kind of shape that one would need.

Russell: If you were director of the NSF⁵⁵ for a day, how would you put the oceanographic community to work?

Reid: I can't imagine sitting on such a position, or such a painful position.

Russell: Well, you could just be king for a day. What would you have us focusing our time and efforts on?

Reid: Getting some new ideas and getting rid of some of the old ones. One of the problems I've had here is noting, first of other people and that finally in myself, that we're overwhelmingly influenced by good papers written thirty years ago. When they've achieved the force of law, they're unchallenged. I think that these have been accepted too long—good papers, mind you, but not necessarily exactly right for all that. I think that such concepts as the Stommel and Arons hypothesis are wonderful, good physics, that explain quite a lot of what is going on in the ocean, but [it] was never intended by its authors to describe every part of the deep ocean in detail. But people have clung to that, partly because it's the only thing that's ever been done of any value at all on the deep circulation in terms of theory. Why don't they improve the theory, or get another one, or something like that? Henry [Stommel] himself at some meeting at Woods Hole said that he thought the statute of limitations had run out on the Stommel and Arons paper.⁵⁶ He wouldn't mind someone else doing it another way. And I'm sure that there are such things in modeling as well. But something that seemed to work to a certain extent twenty years ago may still be considered the starting point, rather than the backing-up point from the other work they're doing. It may be so in geochemistry as well. What do you think?

⁵⁵National Science Foundation.

⁵⁶Henry Stommel and A.B.Arons, "On the Abyssal Circulation of the World Ocean: An Idealized Model of the Circulation Pattern and Amplitude in Ocean Basins," *Deep Sea Research* 6: 217-233 (1960).

Russell: Oh, well, I personally think that's very true. I would like to see the conveyor belt bite the dust.

Reid: It's a charming notion. They die last. Ideas, I think, are what we need, but I really believe that someone, smarter than I, should be able to take the theory of ocean circulation and do more than just give us the upper two hundred meters and the Ekman drift, because it is obvious that these great gyres, which are formed by the wind-driven circulation, extend to great depths. They also change their shape and shift to the poleward somewhat at greater depths, but they are identifiable down until they get into the depth of the ridges where they're broken up and where you have gyres split in two below the Mid-Atlantic Ridge, but it's obviously the same sort of thing. This is common to all oceans and therefore this means there must be some notion one could derive in terms of the wind-driven ocean circulation which does allow the concept to extend to greater depths. The concept when I came into the field, and the concept many people have, is that the wind-driven ocean circulation has to be a hundred meters and the thermohaline. What's going on in between?

Russell: Certainly doesn't work in the southern ocean, does it?

Reid: Well, a curious thing happened when I was asked to write a chapter in the Stommel volume. I didn't know what to write about, and I decided to look into the mid-depth circulation, because very little work had been done on that since the attempts that Wüst and Defant made in the 1930s. I first asked a few other people what they think is going on at a thousand meters, anywhere you like. Nobody said anything, and they looked a little embarrassed. I rephrased the question, later on I asked some of them, "Say about the time you were taking your qualifying, if somebody asked you to say something about the two thousand meter flow anywhere in the world ocean, what would you have picked?" No answers. So the only useful answer I ever got was from Mindy Hall, who was at Woods Hole. She was working on the Gulf Stream. She knew that the Gulf Stream went down to two thousand meters. Nobody ever even said the Antarctic Circumpolar Current, we sort of think that's going east all the way around. It wasn't that they said anything wrong about the flow, it's just that nobody had ever thought about this. They didn't have any wrong ideas about it; they didn't have any ideas at all.

That still seems to be a problem the modelers have not addressed that seriously. They're trying very hard to do some things. I sometimes think they are trying to run before they can walk or even crawl. We're never going to get the heat budget right until we have the water, all the water, going the right direction.

Russell: Sounds very reasonable. The Joint Global Ocean Flux Study that they've just finished up here ended up being a collaboration between primarily

biological and chemical oceanographers—.

Reid: Yes.

Russell: —instead of the more traditional collaboration, like GEOSECS, between chemical and physical oceanographers. Do you think that this is a long-term change in the collaboration between ocean scientists, or was this just a one-shot kind of deal?

Reid: Well, I don't know. The data they wanted to take and the places they wanted to do it were selected on the basis of the particular things they wanted to find out. They couldn't do it for the whole ocean, couldn't do it seasonally very well in many places. It seems a more process-oriented study on the basis where they thought they could find out some solutions. The nature of the stations and the data they collected were of not much immediate use to physical oceanography except for the exchange between the ocean and the atmosphere, which certainly could be useful. That would be a localized case. I hope that between them the chemists and biologists got what they wanted out of that, I haven't followed the literature as much as I should have.

Russell: No, I think that's what I was curious about. In reading some of the literature, there's a lot of collaboration between chemical oceanographers who basically hitch rides with the physical oceanographers. I was curious what your opinion was in watching over the last forty years the change in the collaboration between perhaps physical and chemical oceanographers, maybe now being more focused between chemical and biological oceanographers.

Reid: I hope there is more of that. Well, I suppose what Ralph Keeling is trying to do now, and is obviously dealing with oxygen is not entirely free of such things as photosynthesis and respiration and, to look at it on that scale—.

Russell: I have another question if you would rather.

Reid: Sure.

Russell: Let's see. SIO has been innovating in oceanographic observing systems. For example, Munk's tomography or the satellite oceanography that Sarah Gille has been working on, or Dave Sandwell. Then there are Russ Davis's drifters. These are alternatives to, or supplements to, our traditional hydrographic measurements, to try and get a more synoptic view of ocean circulation. How do you think these new techniques have contributed or will contribute to our understanding of how the ocean moves?

Reid: Well, I think they are intended to tell us something about what

actually happens in the ocean. That will certainly help improve our understanding. That is, to make truly small-scale measurements about the flow, with current meters and the like, was not much use, as you know, dealing with how big an area this particular flow would cover. We know from some of the drifters that there is a great deal of random walk involved in most any circulation. The slower the general circulation, the wider the variations. There are some very interesting things. Of all of those floats that were put in the Labrador Sea in the last two or three years, hardly any have ever come out. And that's interesting because it's clear that the characteristics of the water which exist in the Labrador Sea do extend southward in the Labrador Current along the east coast of the United States for a substantial distance. Why does the water go there, how do those characteristics get there, without the floats going there? That's something you have to think about. Maybe the concept of a direct outflow of substantial magnitude is wrong. Maybe what we have is something that takes place, a water mass which is formed within the Labrador Sea but which is simply renewed and replaced in situ large-scale connection. I would find that very disappointing, but that's neither here nor there as to what's really going on.

Russell: I'd like to take a kind of broader view to finish things up with our oral history. How would you change SIO to better meet the sort of future challenges we have as oceanographers? How would you get us ready to tackle what's left to do in oceanography?

Reid: I wish I had an answer to that, but I don't. It seems to me that we've got a bunch of people here each doing his thing and whether the sum of their efforts is exactly the sum we should look for, I don't know. Things are being found out on all scales and all disciplines. Which of these will seem important ten or fifteen years from now? I'm sure they'll all be important, but which are most important? I'm in no position to state.

Perhaps we will get out of the climatological group here something really fundamental about understanding El Niño, really understanding it, not just saying when the trade winds drop we get an El Niño. They are most sophisticated in that already but which would give us some months or a year or so ahead prediction.

Whether Walter Munk's and Carl Wunsch's notion about dissipation of tidal energy will work out, I don't know. That's an extremely interesting one, because that's not just a question about how do you get rid of the tidal energy, but what does this tidal energy do? How much stirring in the ocean comes from that? One of our big problems is how does mixing really take place in the ocean? I don't see mixing in the work I do. I simply see that mixing has occurred and is probably still occurring. The mechanism of this mixing and the form in which it takes escape me. That's obviously a matter of mixing and there's critical mixing over here but just on what scale this is taking place is not obvious from the kinds of

observations I have.

Those are things the modelers have to take into account when they try to do their large-scale work. I know I'm very hard on them at times, but they have an immense job in front of them. We have to know far more to include their results. They have to put far more inputs into their models than I have to put into my work although, lord knows, I have put enough..

Russell: If you were starting out as a graduate student again today, would you choose to do the work you've been doing?

Reid: Well, let's put it this way. If I were a starting graduate student here now, I wouldn't know any more about it than I knew at the time. Lord knows what I would get into. When I came to Scripps, I had not really much of a notion of what I wanted to do here. I had to bump my shoulders against various things and my head against other things, and finally find a path I wanted to follow. Do any of the students come here with a firm notion of what they want to work on?

Russell: I certainly didn't. I came as a geochemist and ended up doing chemical oceanography and seem to be moving back into geochemistry. I don't know.

Reid: I think the first thing I had in mind when I came here was simply mapping the ocean's terrain. I'd been in the navy as navigator in the southwest Pacific and found that some of the islands were not where they should be. They were put on charts by German surveyors in their brief colonial period back in the 1880s or 1890s. The Germans are very good, but they didn't have much time to do it. I thought, wouldn't it be nice to go back and find out where those islands really are, because they are fascinating places. One was oriented northwest/southeast on the map, but it was really northeast/southwest, another was split. But somehow I didn't get into that kind of work. I got into ocean circulation, and I've never been able to escape.

Russell: My final question is: When do you think oceanography will be done? What do we have to do to finish?

Reid: Whose definition of done? That's the question. It is unlikely that we should understand everything about it for a long, long while. Whether we understand enough to make the heat budgets work, will we have the circulation of the ocean down so that we can make accurate and useful estimates on the transport of heat and the exchange with the atmosphere – in other words, understand how the ocean is being driven. What is it that makes the water dense enough to sink here? What makes it go where it goes from there and what happens to it along route? Those are the things that we'd like to get more than the arm-waving qualitative view that we have now which is probably wrong anyway.

Russell: It sounds as if we have a lot of work to do.

Reid: I wish I could get a big burst of information from something like the paleoceanographers. I have some notion of what the ocean is doing now, and it's a long chain of qualitative reasoning and even a few notions about why it's doing that, and that's even more wobbly chain. What I really need is for someone, perhaps a paleontologist, to indicate to me something about what the ocean was like at some other time, perhaps during the last glaciation. What was it really like in a way that I could get my teeth into? It would be like cutting a Gordian knot. It might shatter some of my most treasured notions. If they're going to be shattered, I would rather see them shattered right away. Perhaps what I've been thinking is important is not important at all. Something else is really the driving force that makes them operate. The paleoceanographers have not done much for me so far. But theirs is a hard job as well.

Russell: Not enough stations or cores taken?

Reid: Well, I wonder how much have they been striving for. I remember years ago when CLIMAP, you remember CLIMAP?

Russell: Yes.

Reid: Several graduate students or young scientists came to me saying, "This is what I find in the sedimentary record, deposition of these bugs and so forth, a long time ago. Could there ever have been a flow from here to here that would do that sort of thing?" I told them the absolute truth: I don't know about what it might have been a long time ago. I don't think anyone else knows either. Why don't you go ahead and publish your map here and somebody else will publish some others and by the time you get half a dozen of these together, why, some will be wrong but maybe they will converge upon some ultimate truth. It didn't work. It turned out that at that time, I was told, they could not possibly publish such a conjecture without the blessing of some physical oceanographer. That was absolute nonsense. That was information, and if it never got out to the world, good information even with a bad conjecture can be useful. They might be roundly damned in the literature for having suggesting such a thing, but still that bit of information would linger in the minds of the people who read it and would come to fruition some day.

Russell: Is there anything you'd like to add to the interviews?

Reid: I can't think of anything. I think Scripps is a great place to work. Believe it or not, in the old days I used to get job offers from other places from time to time. I've spent a little while away, but never severed my connection here. I think that's partly because I knew where things were at Scripps, knew who to talk to, knew how to get things done, and I didn't

want to have to go running somewhere else. I found it an ideal place to work. I couldn't imagine any place better.

Russell: That's a great place to stop.