Regional Oral History Office
The Bancroft Library

University of California
Berkeley, California

Oceanography, Population Resources and the World

Roger Randall Dougan Revelle

OBSERVATIONS ON THE OFFICE OF NAVAL RESEARCH AND INTERNATIONAL SCIENCE, 1945-1960

Interviews Conducted by
Sarah L. Sharp
in 1984

Underwritten by The Office of Naval Research

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Background of the Regional Oral History Office

Oral history is a modern research technique for preserving knowledge of significant events as recounted by participants. These tape recorded conversations are the vivid, irreplaceable view of a narrator who has been deeply involved in the events described, with the dynamic quality of the ancient oral tradition. Because it is primary material, oral history is not intended to present a verified, complete report. As a basic document itself, it is used to illuminate other, more conventional sources. These memoirs can also inform current leaders of the thinking and practices of their predecessors.

An oral history memoir is a recorded and transcribed series of interviews carefully designed to cover the major stages and events in the life and work of the selected individual to convey the uniqueness of his or her personality as well as contributions to important facets of California affairs. An oral history study is a set of interviews of varying length by a number of individuals who have observed the same aspect of human endeavor from varying viewpoints. Memoirists review their transcripts after editing by the interviewer. Transcripts are then retyped, indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library and other suitable locations. The memoirist receives a copy for his or her own use. The Bancroft Library safeguards and administers the use of the memoir, and other personal or business papers which may be donated, according to the narrator's wishes.

The Regional Oral History Office is almost unique in the field in maintaining a permanent staff of experienced interviewers, each knowledgeable about oral history techniques and also familiar with several aspects of the socio-economic, scientific, cultural, and governmental life of California. Each memoir is assigned to a specific staff person who follows through from initial research and planning of interviews with the narrator to the presentation of the completed volume.

There are at present six staff interviewers, with several others on call in more specialized fields. The Office is under the direction of Willa Klug Baum, herself an experienced interviewer-editor with a distinguished national reputation. Faculty members in departments concerned with given project fields are consulted and upon occasion undertake portions of interviewing. An interviewer normally works on only two or three projects at a time, in order to maintain close contact with memoirists. A full-length biographical memoir, or a set of shorter interviews on a single topic, requires on the order of two years to complete.

Although the University provides a modest budget for basic administration, the substantive work of the Office is funded by gifts and grants, which are tax-deductible. Funds are sought to undertake specific projects, developed by the staff and its faculty advisors, which are designed to broaden and enrich available materials on the significant factors in the life of the Bay Area, many of which have statewide and national implications. These projects germinate from The Bancroft Library's continuing list of outstanding individuals and organizations whose accomplishments should be documented for posterity.

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On behalf of future scholars, the Regional Oral History Office wishes to thank those who have generously donated funds to complete this oral history memoir project with Roger R. Revelle.

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Oceanography, Population Resources and the World

OBSERVATIONS ON THE OFFICE OF NAVAL RESEARCH AND INTERNATIONAL SCIENCE, 1945-1960

INTERVIEW HISTORY

Roger Revelle, among his numerous scientific and administrative achievements, was intstrumental in the establishment of the U. S. Navy's Office of Naval Research (ONR) in 1946, as it evolved from the Office of Research and Inventions. As his career took him back to Scripps Institution of Oceanography (SIO) after World War II, where he had been a graduate student and young professor, Revelle rose to the positions of assistant director and then director of this expanding scientific institution. Upon his return to Scripps, Revelle remained a civilian consultant to ONR and built a crucial relationship in oceanographic exploration and research between SIO and ONR especially in the 1950s when he was SIO's director.

Transcripts of two oral history sessions, conducted on 3 and 4 November 1984 at Revelle's home in La Jolla, California, are presented here, the first section available of a longer work. The first session documents Revelle's perspective on some of his work with the navy and ONR, mentioning the Bureau of Ships, the Office of Research and Inventions and ONR as its successor, various SIO-ONR expeditions and operations, his membership on the Naval Research Advisory Committee, as well as notes on his own family. The second interview covers Revelle's activities in international science between the 1930s and 1960, highlighting the expansion of UNESCO to include scientific endeavors, the origins of the Intergovernmental Oceanographic Commission and the Scientific Committee on Oceanic Research, and other aspects of oceanography in the post-World War II world.

The interviewer has tape-recorded six other oral history sessions with Dr. Revelle, covering his youth and education; additional navy experiences during World War II and his participation in Operation Crossroads; his directorship of SIO, 1951-1963; his directorship of the Center for Population Studies at Harvard, 1964-1976; and his further involvement in international science, 1960-1980. Transcripts of these sessions are currently being prepared by the Regional Oral History Office of The Bancroft Library at the University of California, Berkeley, and will be available for deposit in libraries and manuscript repositories as edited, indexed and bound volumes in the near future. The transcripts included here, which the interviewer and Dr. Revelle have both edited, comprise the first volume of a longer series of oral history interviews conducted with Dr. Revelle entitled Oceanography, Population Resources and the World. Kathryn Ringrose, of the University of California, San Diego (UCSD), has also interviewed Dr. Revelle, on the topic of his substantial efforts between 1954 and 1961 to establish this particular campus of the University of California. Ringrose's interviews were conducted as part of a larger oral history project organized to document UCSD's first twenty-five years.

In order to prepare sufficiently for these interviews, the interviewer conducted research on several levels: examination of the Roger Randall Dougan Revelle Papers* which have been collected at the SIO Archives in La Jolla; reading of secondary works which highlight the recent history of oceanography and other areas of Dr. Revelle's career and life; and, consultation with Dr. Revelle himself about critical episodes which he thought needed oral documentation.

The significant contributions to oceanography which Dr. Revelle has made came to ROHO's attention through Professor Harry N. Scheiber of the Law and Society Program at Boalt Hall School of Law at the University of California, Berkeley. Professor Scheiber was instrumental in the interviewer-editor's obtaining a seed grant from the UCSD Chancellor's Office to initiate preliminary research and interviewing on this oral history project. Thanks are due to Dr. Marvin K. Moss, technical director of the Office of Naval Research, who arranged for a grant from ONR to fund the following two interviews.

Sarah L. Sharp, Ph.D.
Project Director
Interviewer-Editor

May 1986
Regional Oral History Office
The Bancroft Library
University of California, Berkeley

*A guide to the Revelle papers and a lengthy introductory biography which accompanies it were prepared by Deborah Cozort Day, SIO archivist, and is available as SIO Reference Series 85-26 from SIO.

I TRANSITION FROM THE OFFICE OF RESEARCH AND INVENTIONS TO THE OFFICE OF NAVAL RESEARCH

[Date of Interview: 3 November, 1984]##

Section 940D in the Bureau of Ships

Sharp: I thought we would talk about your work at the Office of Naval Research,* the transition of your coming back to Scripps, what that meant for you and what that meant for Scripps, and then talk about ONR and Scripps and the different contracts and different work that Scripps did for ONR, primarily some of the operations at Bikini, Eniwetok and elsewhere.

I found quite a bit of material on the Capricorn expedition and Operation Ivy, which was the first part of that expedition. That's about it, but I have quite a few detailed questions, so that will probably take us a couple of hours if you have that much time.

Revelle: Oh, I have all the time in the world. As long as you can stand it I can stand it.

Sharp: Let me just ask you one question for Deborah Day at SIO** Archives. She wanted me to find out from you, if you remember, what might have happened to some of the files of your work in Washington, D.C., because there's not very much about it in the papers that you've given over to her so far.

##This symbol indicates that a tape or a segment of a tape has begun or ended. For a guide to the tapes see page 112.

*Revelle recalls these early days, and the origins of ONR, elsewhere also. See his article, "The Age of Innocence and War in Oceanography, Oceans Magazine, March 1969, p. 6. Additionally, readers may be interested to see, "Recent Developments on Oceanography at the United States Navy Hydrographic Office," R.O. Glover, Transactions, American Geophysical Union, 27: No. IV, August 1946, pp. 561-563.

**Scripps Institution of Oceanography.

Revelle: They probably got thrown away. I was in the Bureau of Ships during most of the war. I was in Section 940 of the Bureau of Ships, which was the sonar design section. I had a special sort of subsection of that called 940D, which was the oceanographic subsection of the sonar design section of the electronics design division. Rawson Bennett, who later became Chief of Naval Research, was head of that electronics design division. Jack Myers, Commander Jacob Myers, USN, was head of Section 940, sonar design.

Sharp: Those papers from that work, would they just have remained with the Bureau of Ships then probably?

Revelle: Or else they were just destroyed or thrown into the National Archives. I don't really know. Or, they would be in the Bureau of Ships archive, if there's a Bureau of Ships archive somewhere.

Sharp: Well, I'll give her that lead and see what she--.

Revelle: Section 940.

Sharp: That's fine.

Revelle: In 1946 I was appointed to Admiral Blandy's staff for the Crossroads operation. We pretty much ran my part of Crossroads out of the Bureau of Ships office, the Section 940D office, although I was assigned to Admiral Blandy's staff as his oceanographer. He was a wonderful man, by the way.

Sharp: You talked a lot about him during our last interview as being very helpful as well as--.

Revelle: Very bright.

Sharp: Very bright.

Revelle: I told you a little story. He was so impressively bright, it was very interesting to look up his class and find that the number one man in the class was W.H.P. Blandy.

Sharp: So he kept that going as leadership.

Revelle: I mean the leading grade point average in his academy class.

Sharp: So it really came through later on.

Revelle: He was called "Spike" Blandy because he had a big nose.

Revelle: In the summer of 1945 I was out in the Pacific, first in Hawaii--Pearl Harbor--for a while, I think there primarily looking at destroyers and their bathythermograph equipment. My friend Cesare Lombardi Barber was also there. He was the fleet smoke officer and he was always called Joe. They had found a way to use smoke very effectively, quite different than Joe and I and our colleague Jeffries Wyman had thought we could use it in our experiments in 1944, to protect the ships against the kamikazes just by making a blanket of smoke.

Sharp: I've seen pictures of that; it looks like it would certainly work very well.

Revelle: It was very hard on the poor kamikazes.

Then I went to Guam, where I was on Admiral Raymond Spruance's staff, introducing the system that had been developed by Mike [Morrough] O'Brien at Berkeley, Bill [Willard] Bascom, John Isaacs and Joe Johnson to tell the depth of the beach in shallow water from what happened to the waves as they came in onto the beach. If you look at these waves here [gestures to ocean outside window] it's a rough surface offshore, but as they come into the shore you can follow individual waves. They get steeper and steeper and closer and closer together. They "feel" the bottom, is the way people say. Finally, at a certain depth, depending upon their height, they break. If you could then see what the height of the waves was you could tell the depth of the water where they break.

Sharp: And the depth would be important for knowing how close--.

Revelle: It was essential for landing craft. The great tragedy of not knowing the depth was at Tarawa, where the landing craft got grounded on a reef a long ways offshore. The marines were just sitting ducks. As I remember it, a thousand marines were killed there trying to get in to the beach. They had to wade through the water.

Sharp: And they were under no cover, completely obvious.

Revelle: That's right. So that was the beginning of the attempt to measure in various ways the depth of the water. One way to do it, and one way that was used, was with frogmen called UDTs, the underwater demolition teams. But another way was by flying over the inshore zone and looking at the waves. That was in connection with the contemplated invasion of Japan.

Sharp: We talked about that.

Revelle: Operation Olympic and Operation Coronet. At the same time--I don't quite remember how this worked out bureaucratically--I had become head of the geophysics branch of ONR, but I never was there while I was on the Crossroads expedition. In fact I stayed at least part of the time in the Bureau of Ships until the summer of 1947, because I organized the Bikini resurvey from my office in the Bureau of Ships, as I remember it.

Post-War Arrangements for Naval Support of Pure and Applied Scientific Research

Sharp: I would like to ask you a few questions about the geophysics unit, because a lot of the reading that I have done about those early years of ONR talks about convincing universities to do some of the work that the geophysics branch and the rest of ONR had in mind. That was an issue that had to be solved.

Revelle: The problem was really not that. It was quite a different problem. That was to educate me particularly, and I guess a lot of other people too, that the way to support research was to support the research that researchers wanted to do instead of dreaming up projects for them to do. This idea had really evolved in the office of the Coordinator for Research and Development, an admiral named Furer and his staff, which was led by Commander (later Captain) Bob Conrad. He had a lot of bright young lieutenants on his staff. One of them was later Assistant Secretary of the Navy, Jim [James H., Jr.] Wakelin. Another one was John Burwell; two others I remember were Bruce Old and a man named Krause. There were altogether about a half a dozen of these young reserve lieutenants.*

Sharp: Are these the "Bird Dogs"?

Revelle: I've never heard that term used. Must be somebody else. But these guys didn't really have anything to do in the war, because the Coordinator for Research and Development, Admiral Furer, didn't have

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*This group of young navy reserve lieutenants has been referred to as the "Bird Dogs." See an account of the origins of ONR which they wrote in "The Evolution of the Office of Naval Research," Physics Today, XIV (August 1961), pp. 30-36, as reprinted in James L. Penick, et al., eds., The Politics of American Science, 1939 to the Present. For a view which emphasizes Admiral Bowen's role, see Harvey M. Sapolsky, "Academic Science and the Military: The Years Since the Second World War," as reprinted in Nathan Reingold, ed., The Sciences in the American Context: New Perspectives, pp. 379-399.

Revelle: anything to do really. So what they did was to spend their time thinking about after the war and the way to organize research in the navy on a post-war, peacetime basis.

They really invented the Office of Research and Inventions which became, later, the Office of Naval Research. They got Hal Bowen, Admiral [Harold G.] Bowen, who had been director of the Naval Research Laboratory, appointed as first chief of the Office of Research and Inventions. They recruited Alan Waterman from Vannevar Bush's organization. Bush had been head of something called the Office of Scientific Research and Development, OSRD. That was disbanded pretty much in the summer of 1944, after V-E Day, long before the victory over Japan and various things happened to the research that had been supported by OSRD.

Underwater sound research and development had been under the direction of Division 6 of something called the NDRC, National Defense Research Committee. There were two parts to the OSRD: the Committee on Medical Research which did medical research and the National Defense Research Committee, which did everything else. In addition, of course, OSRD had started the atomic bomb project. But that became a separate organization, the Manhattan District.

The part of underwater sound research that we wanted to preserve here in San Diego was the work that Carl Eckart and his group had been doing for the University of California Division of War Research.

Sharp: Right. UCDWR.

Revelle: UCDWR. The two people who worked on transferring that research to the navy were Lyman Spitzer and I. Lyman Spitzer is now one of the country's leading retired astronomers. We're all retired, more or less. He was a very, very bright man. I thought he had an IQ about forty points higher than mine. He was very quick and at the same time very personable. Wonderful man.

We talked to Carl Eckart at considerable length about what could be done. Carl was willing to stay out here. He was a theoretical physicist, who had originally been concerned with statistical mechanics and quantum mechanics. He had taken up particularly one aspect of that, called irreversible thermodynamics. Then he came out here during the early part of World War II, I believe in the summer or fall of 1941, to UCDWR. His interest pretty much then changed to thinking about underwater sound, the propagation of sound in the ocean, which is a fascinating subject.

Sharp: And a very basic one for the navy to be concerned with.

Revelle: From the point of view of the navy, a very basic one. What the physicists call classical physics as opposed to quantum physics or modern physics. It dealt with hydrodynamics, which is the motions of particles of fluid under various kinds of forces. Lyman and I wanted to continue that work, and Jack Myers wanted to continue it. We persuaded Carl to organize a post-war laboratory to do it.

But the question was to get support. The navy, before the war, had never supported any university research, nor had they, quite understandably, been willing to make long term commitments. I believe, in the fall of 1944, we wrote a letter for Admiral [Edward L.] Cochrane, the chief of the Bureau of Ships, to sign. He sat on that letter for months, worrying about it. We talked to him several times about it. Admiral Cochrane was another marvelous person. So many admirals, not all of them, but so many of them are very admirable people. He was one of them. He eventually signed this letter. The letter was a classical letter in some ways; it didn't really say that it represented a complete change of policy--but it <u>was</u> a complete change of policy. What it did say was that the Bureau of Ships would contract with the University of California to support the Marine Physical Laboratory for the indefinite future, without limit of time. In other words, the Marine Physical Laboratory had tenure. That was a great departure from government policy, let alone navy policy.

On the basis of that letter Carl agreed to take the job as director of the Marine Physical Laboratory. The university, on the other hand, agreed to appoint him to a professorship.

Sharp: Was that a matter of convincing the university also to make some changes?

Revelle: Both sides had to be convinced. I don't quite remember how it worked at this end. Carl and Harald Sverdrup, I think, worked that out together here with President [Robert Gordon] Sproul and with the academic senate. What Lyman and I did was basically to convince the navy.

The Marine Physical Laboratory was an independent laboratory on the grounds of what's now called Navy Systems Command, or something like that. It's the old U.S. Navy Radio and Sound Laboratory, later called the Naval Electronics Laboratory.

Sharp: If I could just stop you there, some of the writing that's been done about ONR recently has looked at it as this terribly innovative group because of the benefits that scientists would have. For example, some people have said it was very attractive to scientists because they didn't have to have competitive bidding; that wasn't required for the work to be done. The scientists could publish the results of the work that they did without any restrictions, unless of course it was a classified project. But the major switch, and you said this in the interview that you did for the people at Texas A&M, in that oral history, was that the navy accepted the idea that basic research was valid for its own sake.* That was the major departure.

Revelle: It was a major step in changing the government's relationships to science. The Office of Naval Research was really the precursor or the model for the National Science Foundation.

Sharp: But the same people that do this writing about ONR as being this bright light on the horizon in terms of support for scientists have said also that this didn't last very long, sort of a honeymoon period, that by four years later, say 1950, that things were not so easy for scientists; there were many, many more restrictions and basically that the push for applied research won out. After 1950 it was much less--.

Revelle: After when?

Sharp: After 1950, say.

Revelle: That's not so.

Sharp: How would you describe it then?

Revelle: In the first place, the National Science Foundation was established in 1950. They gradually took over more and more responsibility for basic research.

But at least until 1960 the Office of Naval Research still supported a good deal of basic research without any change in policy at all so far as I know. The budget, of course, which was the key

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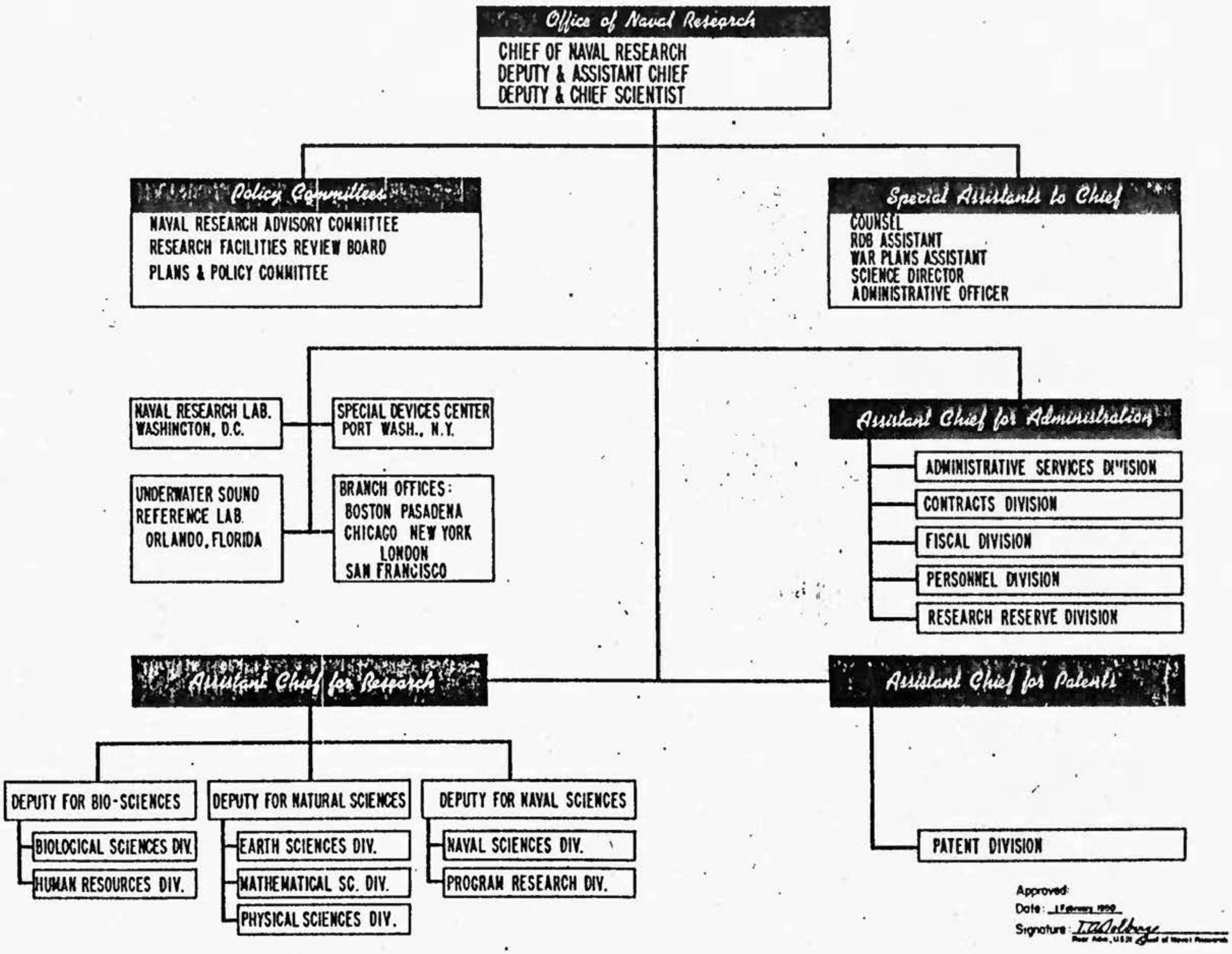
*This interview exists in rough transcript form and is located in Carton 1, Folder 33, listed as "Texas A&M, Oral History 1976," in the Revelle papers at SIO Archives. This interview was conducted by Robert Calvert, at Texas A&M, on the history of oceanography in the United States.



Organization of the Office of Naval Research in 1950, from "Annual Report of the Office of Naval Research--Fiscal Year 1950," p. iv.

Revelle: thing, the budget for applied research went up and the budget for basic research went down, not by any means to zero but down quite a bit.

For example, ONR was supporting Willie Fowler at Cal Tech* and a lot of other high energy physicists. The agreement with NSF was that the Atomic Energy Commission would support the big machines, the big accelerators, and the National Science Foundation would take over from ONR the support of the scientists who worked on those machines, who did not maintain them but came from other places to work on them. So it was support of high energy physics that NSF took over pretty much from ONR.

Sharp: Let me just ask you about something, though. In that same interview that you did with Texas A&M you said that the people at ONR, after a while, were unable to hold up against the bosses, who were committed to applied research. That intrigued me. I wasn't sure whom you meant really, and when exactly that might have happened.

Revelle: I don't remember the dates very well, but it is quite right that-- I'm not quite sure just how to say this. At first, the Office of Naval Research was directly responsible to the Secretary of the Navy. If you look at the charter, it says that. Sometime in the fifties, I think it was, maybe later, the navy got itself reorganized, and Bureau of Ships, all these bureaus, disappeared. They became various kinds of commands, as they call them.

I never quite understood that change in organization, but in the process the Office of Naval Research became responsible to the Chief of Naval Operations and not to the Secretary of the Navy.

The Chief of Naval Operations, his primary job is to maintain the readiness of the navy for war, for combat. Everything is aimed toward that end, not because there is a war on the horizon, but that's what the Chief of Naval Operations is for. He doesn't command the ships at sea or the fleets at sea; he's responsible for preparations and making sure that everything is ready.

The Chief of Naval Operations is also a member of the Joint Chiefs of Staff. The Joint Chiefs of Staff give instructions to the theater commanders, who are usually unified commands. For example, in the Pacific it's not Commander in Chief of the Pacific Fleet, it's Commander in Chief of the Pacific, period. In that case it's usually an admiral. In Europe it's usually a general, not always. But at the level of the fleet, the navy part of it, those guys are responsible not only to the theater commander but also the Chief of Naval Operations. It's a complicated business that I don't really understand too well.

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*California Institute of Technology.

Revelle: But in any case, from the standpoint of ONR, there was much more pressure for them to support applied research that had an obvious navy application.

Sharp: So the theme of preparedness was more strictly tied to the research.

Revelle: Yes, to a large part of it, but not by any means all of it. For example, the Scripps Institution of Oceanography still has a contract with ONR, a big contract, under which they do oceanographic research. Any kind of oceanographic research is regarded by ONR as important to the navy. The Scripps people do what they think they ought to do or want to do.

What happens is that once a year the Office of Naval Research sends out a big expedition here to La Jolla, about eight or nine or ten people. The Scripps people tell them what they're planning to do the next year. Then they get support for doing that.

Sharp: Do the ONR representatives make some suggestions about changes?

Revelle: Very little, very little. They never make any suggestions on the basis of what's most important to the navy. They make suggestions about whether you can save money by doing it this way rather than that, and that's quite natural and proper. But the Office of Naval Research still, in oceanography at least--oceanography broadly defined, including marine biology--is pretty fundamental. In fact their support, from my point of view, is much better than the NSF [National Science Foundation] support.

Sharp: Why is that?

Revelle: The NSF support is tied to projects, proposals made by individual scientists or small groups of scientists for a particular research project. At the end of each project, you've got to apply all over again. Each project proposed has to go through so-called peer reviews, and that's not by any means an unmixed blessing.

Sharp: Sometimes, it strikes you, that could be quite political.

Revelle: It is political in a funny sort of way. But more seriously there's no continuity to it. Particularly in oceanography you really do have to have continuing research. Problems never get solved within a certain time frame.

The nice thing about the Office of Naval Research was that we never bothered about peer review. The section heads and branch heads and division heads had the ultimate responsiblity and authority.

Revelle: They often asked people for advice, but they never--it's quite different than NSF, where the staff members there are really in some ways kind of clerks, not entirely, but to a considerable extent.

Much more so in NIH [National Institutes of Health]. In NIH they really are clerks, and all the decisions are made by the study sections.

It's kind of a spectrum, from NIH, where the study sections have the ultimate authority, to NSF, where the staff has joint authority with peer review groups, to the Office of Naval Research, where the staff has the authority. That last is by far the best way to do it in my opinion. At least in oceanography it is, because in oceanography they support institutions and not individuals.

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Revelle: The people in the Office of Naval Research in my time were quite a remarkable group of people. Fred Seitz was head of the physics section; actually he was a little bit later than me. When I was there it was a man named Urner Lidell.

But Fred Seitz, just about the time I left, became head of the physics section. Mina Rees was head of the mathematics section; she later became dean of Hunter College and president of the Graduate School and University Center of the City University of New York. Joe Weyl was the son of the great mathematician Herman Weyl; Randall Robertson later became one of the leading figures in the National Science Foundation; Manny Piore became chief scientist for ONR and then chief scientist for IBM.

In my branch there was Dan Rex, a meteorologist. I brought in Gordon Lill and Beauregard Perkins, and Johnny Knauss was there for a while.

Sharp: Another name I saw was Earl Droessler.

Revelle: And Earl Droessler was there. He's had a rather varied career. He was one of the latest presidents of the American Meterological Society.

From the standpoint of science administration, the ONR scientific staff was a talented group of people, remarkably so.

Its leader, the spiritual leader, was Bob Conrad, Captain Conrad. He died of cancer a very short time later. And Alan Waterman, who had been the chief scientist under Vannevar Bush in the Office of Scientific Research and Development.

Sharp: That's right. And Waterman then went to NSF.

Revelle: Later became the director of NSF. So we really had a wonderful group of people and a wonderful time. We used to have staff meetings every morning talking about how to develop the policy for scientific support.

Sharp: The experience of getting MPL established--Droessler or Conrad or some of these other people, did they have projects on the East Coast or elsewhere that went through similar kinds of--?

Revelle: No. No, they didn't. What Jack Myers and I did in the Bureau of Ships was to establish, on a post-war basis, the descendants of the wartime acoustic or sonar--underwater sound laboratories. There was one at New London, Connecticut, called USNUSL [pronounced "Usnussel"], U.S. Navy Underwater Sound Laboratory, which was run by the navy. The MPL was in some ways unique in that it was run by the University of California. Then there was the group at Harvard, the Underwater Sound Laboratory at Harvard, which disappeared. They didn't maintain it after the war. The Woods Hole Oceanographic Institution, however, was supported by the Bureau of Ships in just the same way that the MPL was.

Sharp: Right. This is putting us ahead a little bit, but I'm wondering if you remember what was called the Badger investigation in 1950. Vice Admiral Oscar Badger--do you remember the name at all?

Revelle: Not at all.

Sharp: Of course, this is after you were back at Scripps, but ONR came under considerable fire from Vice Admiral Badger. There were hearings held where each ONR contract was brought up before this vice admiral, and each one had to be justified. There was considerable criticism. Badger eventually decided that they were all fine, and no cuts were made. They were all justified by the staff at ONR.*

At about the same time, the general board of the navy came to look at ONR and the numbers of contracts and the money that the contracts represented. Again, no changes were made. At this point the Korean war was heating up, and attention was taken away from ONR for the moment. No cuts were ever made. There were two instances, then, of ONR's having to justify what it was doing. I wondered if you recall any feelings about that with some of the work that Scripps was beginning to do.

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*See Sapolsky, "Academic Science and the Military: The Years Since the Second World War," p. 387.

Revelle: No, not at all. The Scripps contract was actually started not by me but by a lieutenant in the Geophysics Branch of ONR, with $125,000, which I thought was a hell of a lot of money. That was when I was still working on the Crossroads operation. The contract number was ORI something or other.

Examples of ONR-Sponsored Projects

Sharp: I made this chronology for you of some of the ONR work that I had noted.* I took the time period '46 through '61, when you ended your major period as director at Scripps, and just gave you a selection of some of the ONR activities at Scripps, so you could see them. I wanted to ask you some questions about some of them. That first ONR contract that I saw, that was for research and surveys as well as training of some of the military personnel in different oceanographic--.

Revelle: As you probably remember, or probably know, these contracts were always stated quite broadly. That doesn't mean that you had to do everything that was in the contract. It probably is true that we did have--at first we had several navy and air force meteorological officers here, not when I was here, but during World War II; Walter Munk and Harald Sverdrup taught these meteorologists how to forecast waves and swell and surf on the beach. Then they went into different theaters of operation and did that. The navy and the air force--it wasn't the air force then; it was the army air corps--had trained a whole collection of young meteorologists, put them through sort of a crash course at college and then sent them out to the various theaters.

Joe Smagovinsky, who later built and organized the Geophysical Fluid Dynamics Laboratory at Princeton, was talking to me about this the other day. He was assigned to an aircraft training center in the middle of the United States as a meteorologist, a just-out-of-college meteorologist. He had to forecast whether, if they went on a mission, they could come back and land at that base or not. That depended on whether there was fog there or not. So he had to forecast fog. He still shudders to think about the responsibility he had and the lack of knowledge that he had to do it with.

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*See following pages for this chronology.

elected Activities relating to ONR at SIO, 1946-1961

1946 July 1. 1st ONR contract w/SIO began, for $120,000 for 1st year. for conduct of oceanic research and surveys; training of military and civilian personnel in certain areas of oceanography. (Shor, p. 36)

1948 Wm. G. Van Dorn devised a magnesium-rod release timer for a deep-current meter on a project for ONR. Van Dorn got his Ph.D. at SIO in 1953. (Shor, p.

1952 Operation Ivy was first thermonuclear test and actually was 1st part of the Capricorn Expedition. several of the SIO scientists who had participated in the atom bomb tests were also on the trip. C.N.G. Hendrix, CDR, USN was i the Project Office at ONR in Pasadena and facilitated arrangements for Caprico "The US Navy should exploit to the fullest every opportunity to obtain useful operational knowledge concerning nuclear-powered submarines in general and the effect of atomic explosions on submarines at periscope depth." (memo, from ONR, Research and Liaison Officer, SIO, to ONR, dated Oct. 1, 195 part of Ivy was to create a tsunami and measure effects.

In 1951, west coast development of the Visibility Lab. this lab was funded by ONR. research was conducted on penetration of daylight into oceans and lakes and on visual sighting of underwater objects by swimmers and aviators. new facility for the lab at Point Loma constructed in 1952 as part of SIO. one of the important people in this change was Seibe Quimby Duntley, as head of the lab at MIT. (Shor, p. 112)

1953 Operation Castle. a continuation of SIO's study of water waves produce bu thermonuclear explosions. "This work is, in effect, an additional study following several similar studies performed by Scripps in the past. The subject study, however, presents far more clearly controlled conditions, which fact, together with the fund of experience already gained promises to be unusually rewarding." (Proposal for Additional Task Under Contract 233 (020), dated May 27, 1953.) there were to be four shots. For example, "Shot I affords the first relatively clear-cut opportuni to study the generation of surface water waves from an explosion over deep water."

1955 Alfred B. Focke was scientific director of Operation Wigwam, a nuclear depth charge project conducted in mid-May 1955, soon after Focke assumed MPL directorship. "As had been the case w/Operation Crossroads, environmental studies were necessary beforehand. Scripps scientists helped make the selecti of a site near 29 degrees north latitude and 126 degrees west longitude, 'in a biological desert, some distance from any commercial fishing areas, where transportation of contaminated water is away from fishing grounds.'" "Areal surveys were carried out in the spring of 1954, and after the test in mid-May of 1955 additional field and laboratory tests were made to monitor the effects...." (Shor, p. 405)

1956 ONR helped to fund and to co-sponsor w/SIO an international symposium, "Perspectives in Marine Biology," held in La Jolla in March, under auspices of the Internatinnal Union of Biological Sciences. 170 scientists from 14 nations came Adriano A. Buzzati-Traverso organized the program. (Shor, p. 204)

1956? Marston C. Sargent, who had worked with Revelle in Section 949D of the Bureau of Ships, came to SIO as an oceanographer for ONR located at SIO. Responsible for all west coast oceanographic organizations that had contact w/ONR. (Shor, p. 232)

1958 Fred N. Spiess became director of MPL in 1958. A change in research emphasis in the Bureau of Ships led to shifting the major support for MPL to ONR about 1958. Among Spiess's first tasks was to separate the facilities of the laboratory from those of NEL then still funded by the Bureau of Ships. (Shor, p. 84)

Planning for the NAGA expedition began in 1958, an expedition to the South China Sea. Some funding from ONR and U.S. Public Health Service. Anton Bruun was scientific leader, Capt. James Faughn was project officer. (Shor, p. 415)

1960? Feasibility studies for Project Mohole carried out w/funds from ONR and NSF. Scientists were sponsored by the "AMGOC" committee of the Academy of Sciences to drill hole to the Mohorovicic discontinuity, i.e. the earth's mantle. (Shor, p. 303).

Revelle: That training of naval officers and air force meteorologists had pretty much stopped by the summer of 1945. There were still quite a few of these chaps who came back here and worked toward getting a Ph.D. as civilians. But the whole military establishment was in a shambles in 1945-46, being dismantled, reduced, people being demobilized, in very large numbers. So many things that got on paper had nothing to do with reality.

I remember in the Crossroads operation, most of the enlisted men there were on probation; they were people who had committed some offense, and instead of being sent to jail they were sent out to Bikini [laughs].

Some of these guys were very difficult to work with. Bikini atoll is about twenty miles long, and with landing craft it took about two hours to get from one end of the lagoon to the other. These people would get to the end of the lagoon and say, "Well, we think our discharge papers have arrived." So they'd go back to the ship, and by God their discharge papers <u>had</u> arrived! So we had to recruit another crew for the boat.

Sharp: Didn't make for very efficient working.

Revelle: It was very difficult. But although it says here, "training of military and civilian personnel," the emphasis was on civilian personnel. This was just really to justify, or to make kosher, our teaching activities as well as our research activities. These things are always stated as broadly and as loosely as you can.

Sharp: Let me ask you a kind of a general question regarding the idea of ONR supporting research the scientists wanted to do. In the post-war period things got turned around a little bit, so much so that in university research, some people were starting to make a case that what they were doing was in the national interest. Just about anybody could make a case for their research being done in the national interest. In the post-war period there was this blossoming of all this university research that was being done.

Revelle: Oh, yes, sure, of course. But one of our policies at ONR was that anybody who put in his proposal that he wanted to do this because it would help the navy--we pretty much automatically turned him down.

Sharp: I've heard that story, and why is it? Because you didn't believe them or didn't think that it made any difference?

Revelle: Helping the navy was not a good reason for doing research. The only good reason for doing research was that they wanted to do it in their bellies; they were driven by curiosity, the desire for discovery and the desire for fame, which is what drives scientists. Scientists in general, although they give lip service to it, for the most part this business about the national interest doesn't send them at all.

Sharp: But it was a reality that in this period there were a lot of university--.

Revelle: The reality was that a lot of university research got started and done, but it was not a reality that people tried to justify it as in the navy's interest, or the interest of the Department of Defense, as far as I know.

In fact, we had two or three different mottos in my part of the ONR. One was that any proposal for less than $5,000 we automatically funded. Another one was, as I say, that anybody who said he wanted to do this because it was good for the navy, we automatically turned him down, unless it was for less than $5,000.

Sharp: One of the other ideas that I've seen in some of your papers at Scripps is that it seems to me that you really like to support young scientists, too. I have seen that in a number of places. Some of the UNESCO committee work, often you were making the case that support should be given to young scientists as well as more established scientists.

Revelle: Of course.

Sharp: Did that start in this period when you were beginning to have the clout to give young scientists a break?

Revelle: Sure. This has both positive and negative aspects. What I think is most important in the support of science is continuity. I don't think that established, mature scientists who have demonstrated that they can do first-rate research should have to compete on a project basis, which is the way the peer review system works with everybody.

You can carry this idea about new blood too far. You can say, "Well, that guy's been getting support for several years, we'll drop him now and pick up somebody else." That's very bad business. It's part of the whole business of tenure in a university. But that's what often happens, particularly at NIH.

Sharp: And yet the reverse could work also; the people who are used to getting funds might think perhaps that they were sort of untouchable, that they could not be cut because they're so outstanding.

Revelle: That just doesn't work, of course. A man like Walter Munk, for example, is still just as productive at age sixty-seven as he was at age twenty-five; he's full of ideas all the time, good ideas. And he knows how to accomplish what he wants to do; he has the mathematical ability and the ability to recruit people and the judgment.

Sharp: When you were at ONR and approving or disapproving a lot of these contracts, did it seem to you that sometimes people were submitting projects to you because they knew you and they knew that you would okay them, that you would support them because you knew who they were?

Revelle: No, that in general was not true, because in the first place the geophysics branch was much broader than oceanography. We had people who were concerned about aeronomy, about meteorology. I didn't know any meteorologists. Earl Droessler did, of course, and Dan Rex did. Other people were very concerned about seismology and the interior of the earth, the use of big explosions--. Let me just describe three of our major projects.

One of the things we did was to support Irving Langmuir and his friend Schaefer in their studies of weather modification, cloud seeding.

Sharp: Is that Milner B. Schaefer?

Revelle: Vincent Schaefer. Bennie Schaefer was a quite different guy, a fishery biologist. But this man was Vincent Schaefer, and he was a kind of a genius. He and Langmuir had worked together on dropping silver iodide into clouds and precipitating water. We supported them with a lot of money, about $250,000 a year, which was a lot in those days. That was a project that Dan Rex and Earl Droessler had taken up before I really was on board very much, although I was ostensibly on board.

One thing I remember about this project was that Langmuir got the idea that he could steer a hurricane. He seeded a hurricane somewhere off the coast of Florida, and the hurricane promptly turned and headed right for the beach. [laughs] He said, "Look, I steered this hurricane." The navy backtracked very quickly on that. We had a lot of publicity about Langmuir, and the navy said, "Langmuir didn't have anything to do with changing that hurricane's direction at all." We could see millions of dollars of lawsuits.

Revelle: But he was typical, very egocentric--most scientists are quite egocentric, and Langmuir was certainly that way. Also a great man, a Nobel Prize winner.

Sharp: What were the other projects?

Revelle: Another one that we supported was an Antarctic expedition, headed by a man named Finn Ronne, and called the Ronne expedition. I was very reluctant about that; I didn't think that that was a very good way to spend our money. But Ronne was an adept and shrewd politician. He got congressional support for his expedition. In fact he persuaded a congressman down in Texas to get a bill passed that would give a big navy seagoing tug to his expedition.

Sharp: So you might have had some people from Congress who were lobbying with you?

Revelle: Not with us, but with much higher levels than we were involved with, such things as giving a guy a ship. What I remember specifically about that was that I tried to get the Chief of Naval Operations [CNO] to help out with this expedition. I told you all this before.

Sharp: No, not this part.

Revelle: The Antarctic man on the staff of the CNO was Rear Admiral Richard Evelyn Byrd, the famous polar explorer. He was dead set against the navy giving any support at all to Ronne. The reason was that Ronne had "betrayed" him. "How had Ronne betrayed you, Admiral?" I have a vivid picture of this in my mind still, because he literally was somewhat insane, Admiral Byrd, I think. His eyes flashed and his face got flushed and he said, "He arrived in New York a day before I did and gave a press conference!" Literally, the word he used was "betrayal." So the result was that we got nothing out of the CNO; we put some money into the expedition for equipment and for scientific so-called experiments or scientific observations.

But really, Ronne was one of these guys who just took money wherever it was. "Gold is where you find it," and he'd do what he'd want with it. One of the things he did was to take his wife on the expedition, as well as the wife of his pilot. The two women didn't get along. The result was that eventually the pilot and his wife left.

Then Ronne went down there and, I guess, did some exploration, particularly in the Weddell Sea area, on the eastern side of the Antarctic peninsula. He named a lot of features for various people, Revelle: including Mount Rex for Dan Rex, Mount Daniel Rex, and Revelle Bay for me. Revelle Bay may or may not exist; it's covered with ice. It does possibly exist. [laughs] Someday when the ice melts we'll know whether it exists or not.

A third research project we had was with Merle Tuve; he was the head of the Department of Terrestrial Magnetism of the Carnegie Institution in Washington.

Sharp: What was that project?

Revelle: It was basically a seismic exploration project using huge explosives, even as much as several hundred tons of explosives, to get a sound signal, an explosive signal, basically an artificial earthquake, into the ground and transmit it over very long distances in an attempt to understand the deeper structure of the earth's crust. With these huge explosions they could get a signal over about a thousand miles and get some idea of the depth of different layers in the crust.

Nowadays this is a big continent-wide project run by Jack Oliver of Cornell. But Merle was the man who pioneered it. He was a famous wartime scientist; he and his colleague from Carnegie, Larry Hafsted, had developed a proximity fuse at the applied physics laboratory at Johns Hopkins. Merle was later home secretary of the National Academy of Sciences, really a great man. Very difficult to work with, however. He wanted to do what he wanted to do when he wanted to do it. I remember arguing with him about what was the relevance of this project to the navy. (This was before I got converted.) He said he didn't give a damn whether it was relevant to the navy at all; it was a good scientific project.

Sharp: And he wanted you to fund it.

Revelle: Yes. Exactly. And we did. That was part of my education. Those are the three specific projects that I remember. But we supported a great many other things too, including Woods Hole and Scripps. We tried to get new laboratories started, new oceanographic laboratories. One that I was largely responsible for was the Chesapeake Bay Institute at Johns Hopkins University.

Sharp: I remember reading about that a little bit.

Revelle: Bert Walford of the Fish and Wildlife Service and I worked together on that. We persuaded President Isaiah Bowman of Johns Hopkins University that they should do this. And they did. Unfortunately

Revelle: that laboratory has not proven to be very viable; it's never amounted to very much. Don Pritchard, one of our bright young Ph.D.s at Scripps became head of it. He's now gone to Stony Brook--they have an oceanographic institute there, and he's assistant director of that.

[tape interruption]

Sharp: I just have one other question on that, and then I want us to talk about that transition period when you were coming back to Scripps. You said that you had to get converted to the idea that--at least I think this is the conversion--that--.

Revelle: The basic way I had to get converted was to accept what the scientists wanted to do because it was good science and not that it should be done because it would have some relevance to the navy's concerns.

Sharp: I didn't think that you would have to be converted about that.

Revelle: I had had five years of experience in the Bureau of Ships, where Lyman Spitzer and I used to dream up things for guys like Russell Raitt and Carl Eckart to do, and people at Woods Hole to do, most of which never worked out. We should've learned our lesson.

Sharp: Because they didn't want to do it?

Revelle: Well, they wanted to do it; they were quite willing to help the war effort any way they could. But it turns out that the only thing that scientists can really do is what they know how to do, not the things that you think of for them to do, but things that they think of for themselves to do because they know how to do them. I'm not sure I'm stating this very well. Very little good science gets done on the basis of one person thinking up the project and somebody else carrying it out.

Sharp: Is that because it's so ultimately personal? That the way the scientist thinks and creates projects leads from one to another?

Revelle: It's basically because he has certain tools at his command, certain technologies or techniques, which work in certain ways but not in others. Unless you're very familiar with his technology and his methods, it's awfully hard to design a project for him to do which he can really do. Does that make sense?

Sharp: Yes, it does.

Revelle: For example, take Merle Tuve's project, these deep seismic explosion things. He could measure certain kinds of waves and not other kinds of waves, even though maybe the other kinds of waves were more interesting from the navy's point of view.

Sharp: But not from his, and not from what he was working on.

Revelle: Not what he was able to do.

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Revelle: Russell Raitt and his colleagues at UCDWR would bravely try to go out and make the measurements that Lyman Spitzer and I thought of, but very little ever came of those measurements. What did come was a great many things they did in spite of us, understanding of reverberation, understanding of bottom noise, understanding of the focusing of sound at certain distances from the source and things like that. The reason I cite that example is that it might have been that I wasn't bright enough, but Lyman Spitzer was just one of the brightest guys who ever lived. And even so it didn't work.

II RETURN TO SCRIPPS

Relations With Harald Sverdrup

Sharp: I thought we might push on into this period that you were coming back to Scripps. Tell me first of all why you decided to come back. There is the letter that you wrote.*

Revelle: I think that letter is quite correct.

Sharp: Let me get it out for you here.

Revelle: I read it last night.

Sharp: I would think it might have brought back some stories about Sverdrup and some of your feelings about him as a person and as a scientific leader.

Revelle: I had the greatest admiration for him; he was a wonderful man. He was a perfect man in many ways. Very, very well organized. He had no hang-ups, so he could work very fast. He had theoretical limitations; he was not in the same league mathematically with Carl Eckart, for example. Carl not only knew this but didn't really think much of what Harald did theoretically, in spite of the fact that Harald's heuristic methods, essentially pragmatic methods, turned out to be of very basic importance in modern oceanography.

The so-called Sverdrup circulation, which is the circulation of the major ocean gyres, basically his idea, was later developed by Henry Stommel and Walter Munk. It's called the Sverdrup circulation.

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*See the following pages for this letter dated 6 January 1948, from Revelle to Sverdrup.

 17(1

San Diego, California
6 January 1948

Dr. Harald U. Sverdrup
Director, Scripps Institution of Oceanography
La Jolla, California

Dear Harald:

We talked so long and so late last night that it seems desirable to summarize on paper my position with regard to future work at Scripps:

(1) I want very much to return permanently to the Scripps Institution. Although I like my present position in Washington as Head of the Geophysics Branch of the Office of Naval Research and consider that it is an important and necessary job, I feel, after thinking about the matter a great deal, that in the long run I would be happier if I had more to do with the actual conduct of scientific research rather than with its over-all planning and coordination at the rather remote level of Washington.

(2) You have stated, and I agree, that if I return to La Jolla I will inevitably have to take a major share of the responsibility for the research and development work of the Scripps Institution in physical, chemical, and geological oceanography and for the integration of this work with the biological investigations.

(3) I do not feel that I can undertake these responsibilities without adequate authority in personnel, budgetary, and other administrative matters involved in the carrying out of research policy. In the establishment of such policy, all senior staff members of the Institution should participate on an equal basis, but the carrying out of policy cannot, in my experience, be effectively done by a committee.

(4) I believe that a merger of the Marine Physical Laboratory with the Scripps Institution would be desirable and fruitful. For many reasons the physical location of the Marine Physical Laboratory should remain at the Navy Electronics Laboratory, San Diego. This would insure that even in the event of a merger with Scripps, the Marine Physical Laboratory would retain its separate identity and autonomy.

(5) If such a merger between the two laboratories took place, I believe that Carl Eckart might be persuaded to assume the directorship of the combined organization. His great analytical ability, sound and objective scientific judgment, and modest, generous, yet firm character would eminently fit him for such a position. I have only one reservation in

Dr. Harald U. Sverdrup 6 January 1948

endorsing him: that he is perhaps somewhat overcautious and too conservative to seize and exploit the many opportunities which should arise during the next few years to develop and expand the science of Oceanography and the Scripps Institution.

(6) I would be very glad to serve as Associate Director under Dr. Eckart with the understanding that I would have, under his general direction, the responsibility and authority for administration of research and field work in oceanography at La Jolla. I believe, from discussions with him, that such an arrangement would be satisfactory to him. I also consider that because of our different characters, points of view, and experience, we would form an effective and productive team.

(7) I would also be happy to be Director of the Scripps Institution with Carl remaining as Director of the Marine Physical Laboratory, which would be an autonomous part of Scripps, and feel that teamwork between us would be equally effective on this basis. This arrangement would have the advantage that Carl could continue more freely and fully his own research, but it might be impossible at this time in view of the reported opposition within the Institution and University to my appointment as Director.

(8) Overriding all the above considerations, I am sure that in the long run I will personally achieve the greatest happiness and satisfaction if the Scripps Institution and the science of Oceanography continue to grow in importance and value to the country and to the world of science. I do not know how best to accomplish this objective which we both share, but I feel that without responsible and devoted leadership, the Scripps Institution might slip downhill very quickly in these unstable days. After being involved for so many years in the fostering of oceanographic research, I would have a sense of profound frustration if the Scripps Institution should disintegrate or fail to fullfil the potentialities which you have so ably furthered as Director.

You are at liberty to use all or any part of this letter in any way you see fit.

Sincerely yours,

Roger Revelle

RR/w

Revelle: And the unit of flow in the ocean, a million cubic meters a second, is called the Sverdrup. For example, the Gulf Stream carries about seventy Sverdrups, whereas all the rivers in the world put together carry less than one Sverdrup. These ocean currents are really large-scale phenomena.

I adored him. I thought he was a wonderful man. One of the things that was in some ways a mistake on my part, not in all ways, but in some ways, was that when he came here as director, in the summer of 1936, I had already arranged to go to Norway for a year to work with another Norwegian oceanographer, Björn Helland-Hansen. I would really have been much better off, in terms of science, if I'd stayed here in La Jolla and worked with Harald.

Sharp: You told me that when we met last time, and yet the period in Norway--.

Revelle: That was a great personal experience and a great living experience.

Sharp: So you traded one for the other, as it were.

Revelle: Yes. One of the interesting things about life is that whatever you've done, you think it was the right thing to have done.

Sharp: You come to some peace about it anyway.

In this letter to Sverdrup you set out your goal of coming back to Scripps to do research, and that that's really what you want to do, and on top of that, have a major role in how Scripps comes along. Were those ideas a surprise to Sverdrup or were those things that he certainly knew about you anyway?

But in the conversation that had preceded your writing this down, were those things that he already knew that you wanted to do?

Revelle: What I'm saying is that we'd already pretty much agreed to what was in this letter.

Sharp: Did you know him well enough that he sort of knew these things even before you told him anyway; he knew what you wanted to do?

Revelle: I think so. He was quite a small man, quite little, in some ways kind of a pixie of a man. He was very tough, just about as tough a person as you could find. He'd spent seven years in the Arctic.

Sharp: You get pretty tough there I would think.

Revelle: With four other people, four other Norwegians on the Maud, a ship that was especially built so that it wouldn't be crushed by the Arctic pack-ice. That really in some way--I'm not sure it transformed him--but it made him the man he was.

Sharp: You described that expedition to me a little bit before: that they took the ship in there and stopped when they couldn't go any further, essentially.

Revelle: Basically, what they were trying to do was drift across the North Pole, to do something that Fridtjof Nansen had tried to do thirty years before in a similar ship, Fram. The game was to get frozen in the ice and then let the ice carry them across the Polar Sea, but it never worked very well. The ship didn't move very far. It really was just not the way to get across the Polar Sea.

They tried it twice on Maud, first for three years and the second expedition with the Chukchi, the Siberian version of the Eskimos. He pretty much stayed in the Chukchi camp, so that the Chukchi language that he learned was the women's language. It turns out that the women and the men in Chukchiland speak a rather different language. The result was that when he was talking to the men they would laugh at him.

Self-Assessment of Scientific and Administrative Skills

Revelle: I'm really better as a scientific administrator or a scientific leader than I am as a scientist, I think. What I did during the war was essentially scientific administration. Maybe I had a subconscious feeling that I could do better as a leader of the Scripps Institution than I could as an individual scientist. I don't remember thinking that, but looking back on it now, I may very well have had that feeling.

One reason was that I never had enough mathematics in college or at any time. Particularly physical oceanography depends on quite a bit of mathematics. So do other kinds of geophysics, for that matter. So people like Teddy [Edward C.] Bullard and Harald Sverdrup and Walter Munk, who know mathematics, were all able to do many things that I couldn't do, not to mention Carl Eckart.

Sharp: And yet at Scripps--this is one of my questions for you much later down the road--you had a way of creating an atmosphere for cooperation among the scientists who had different gifts and had different training, Sharp: and not always peaceably perhaps, but blending together somehow and working together on individual projects to use whatever expertise they had in certain areas.

Revelle: I had some scientific ability which was of a rather peculiar kind. I had somehow an ability to get to the heart of the matter, what was the real question, not the apparent question, the obvious question, but the real question, and also the ability to see how you might do that, how you might answer the real question instead of the apparent question.

So I was a help to many people in that respect, even without the mathematics. I really don't know any mathematics except arithmetic, but it's amazing what you can do with arithmetic.

Sharp: Just the basics.

Revelle: Yes. So I was a scientific leader in a real sense in that I helped people think through what they wanted to do and what was important to do and to some extent how to do it.

Sharp: Having that ability, you thought then and you still think now that being director of Scripps as opposed to being at Scripps and not being director was a better way to go.

Revelle: Yes, that's right, very much so.

Sharp: Just sort of an estimation of what you did best and what you could live with?

Revelle: And what I did well. I was a very good director of Scripps, the best they ever had except Harald Sverdrup, in my opinion. I think most people would say that, too. Not that I was a good administrator. I told you before, I guess, I think administration is a vastly overrated subject. But what I was good at was finding people and inspiring people and getting them to work together.

Sharp: I am really interested to know how you decided who should come to Scripps, what kind of people you really wanted to have around, to be on the staff in terms of scientists and mathematicians and all of that, the different disciplines, what your criteria were for wanting somebody to come. You can think about that and we'll talk about it when we get to talk about your years as director, because to me that looks like a central decision that you had to make, frequently, and one that certainly made the composite character of what Scripps was when you were there.

Revelle: Well, I made some mistakes in selecting people. The wrong criterion I sometimes used was to find people who could do different jobs, I mean different kinds of jobs. For example, in submarine geology one of the important things is to use the organic remains in the sediments, the shells and organisms in the sediments, as a means of defining and describing the environmental conditions at that time and the mode of deposition and the nature of the oceanic environment. One of the very important kinds of organic remains to do that with are critters called foraminifera. These are little one-celled protozoa, but they have complicated shells. There are literally thousands of different kinds of foraminifera, and thousands of different kinds of other organisms called radiolaria, which have beautiful shells made of opal, and also skeletal fragments of a special kind of algae, called coccolithophoridae.

So I brought here Fred [B.] Phleger, from Amherst, to head a foraminifera laboratory. He brought with him a woman named Frances [L.] Parker. They did that for thirty years, looked at the foraminifera that were collected in the sediments, particularly in the long cores that penetrated beneath the sea floor. Another person who looked at a different kind of organism was Bill [William R.] Riedel, who looked at radiolaria. Still another one was Milton [N.] Bramlette, who looked at coccolithophoridae.

It turned out that some of these micropaleontologists, as they were called, did unique and good things. Others never really did; instead they did a fairly routine job. Walter Munk was skeptical at the time, about my criterion of finding people to do a particular job. He didn't think that this would necessarily bring people with first-rate minds.

Sharp: He was right?

Revelle: He was right, and I was wrong. I think my criterion was, or at least should have been and often was, to get people who were bright and let them do what they wanted to do. It was not to try to fill slots, except in that one case with the micropaleontologists, which was a mistake.

Sharp: It seems like it demanded quite a leap of faith on your part, that you trusted your sense, feeling, that the people were bright. It had to have been based on some assessment you made of what they'd done already.

Revelle: Had to be, of course, sure, or on recommendations. I think far more important than any personal judgment here was the system we had. Right from the beginning we used the University of California system

Revelle: of appointment. What really made the Scripps Institution a first-rate place was that we were part of the University of California, not in terms of money but in terms of standards.

At Woods Hole who was on the staff was largely a personal matter on the part of the director. Columbus Iselin's idea was that almost anybody could be an oceanographer. All they needed was sort of enthusiasm.

We never made an appointment without an ad hoc committee, mostly from the Scripps Institution but often people from UCLA, too. We emphasized the university's standards for appointment and promotion. We always treated our research staff as if they were faculty members.

Sharp: Yes. I had seen some explanations in letters, I guess, that you had written to different people coming on board and explained to them what their appointment would be, how it was equivalent to the assistant professor level or whatever. It was interesting to me because it seemed like you were creating, as Scripps grew in this period that you were director, you were adapting different staffs and academic structures to Scripps, always changing the structure a little bit to fit what Scripps wanted to do. I'm thinking particularly of--and now I don't recall his name; it's in my other notes--but someone who retired, I think from the Bureau of Ships, and you brought him out.

Revelle: You don't mean Charles Wheelock.

Sharp: Yes, it was Wheelock. That's right.

Revelle: I brought him out as associate director. Charles Wheelock was an admiral, a rear admiral. When he retired from the navy to take the position with us, he was Deputy Chief of the Bureau of Ships. He was a wonderful man.

Sharp: He certainly fit in with what Scripps needed.

Revelle: Oh, yes. He had a lot to do with starting UCSD. He was a completely loyal, completely ethical person, a wonderful guy, I thought. Everybody did in fact. He later became quite involved with the planning of the Santa Cruz campus, too, after he retired from here. Dean McHenry could tell you about him.

Sharp: I'd like to save the rest of these questions about your period as director, maybe, till the next time.

Revelle: I'm sorry we got ahead of ourselves.

Sharp: Oh, no, that's okay. I prompted it.

One of the other ideas in this letter to Sverdrup was, I guess, a little more difficult and serious, because you wrote about the idea that Scripps might slip downhill.* Tell me about that; what was that all about?

Revelle: I don't know. I noticed that in the letter. Before World War II, Scripps didn't amount to very much, as a matter of fact. It was a very small place and not a particularly good place. It was all right, but Sverdrup had made all the difference in the world by coming here. He was the world's leading oceanographer. The rest of the staff were not really up to him until nearly the end of World War II. By that time there were several good people: Martin Johnson, Carl Hubbs, Norris [W.] Rakestraw, Walter Munk, and lots of young students, many of whom later became staff members. Then we took on Russell Raitt and Leonard [N.] Liebermann.

But I don't really understand what I was saying there. I think what I was saying was that we had a great opportunity to become a--I didn't really visualize how much of an opportunity it was, as a matter of fact. Harald had started this with getting the Marine Life Research Program going here, and we obtained two ships, the seagoing tug, Horizon and an old mine sweeper, which we called Crest, from the navy. Harald had conned the state of California into supporting this Marine Life Research Program, which was to be in cooperation with the California Division of Fish and Game and later with the federal government. Even, before we obtained the two navy ships, during his directorship the program had started. He used our old schooner, E.W. Scripps, for preliminary cruises along the coast. But I don't really understand what I meant when I thought Scripps would disintegrate or go downhill, because the only way it could go was up. [laughs]

Sharp: Well, maybe that's from your perspective now. You didn't think that at the time, did you?

Revelle: Think what?

Sharp: That Scripps was that minor a place.

Revelle: No, what I thought was that we had not really done much in real oceanography. We didn't have the ships; we didn't have the people or the equipment. Woods Hole had done better at exploration in the

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*See p. 20b.

Revelle: deep sea with their <u>Atlantis</u>, because they had covered the North Atlantic, at least a large part of the western North Atlantic. They had the advantage of being on the East Coast where all the money was and all the people were.

Sharp: Yes. This was the provinces; the West Coast was still the hinterlands, quite the frontier.

Revelle: Very much so. As I say, I'm not quite sure I understand what I was saying in that letter.

III FAMILY NOTES: ELLEN CLARK REVELLE, CHILDREN AND GRANDCHILDREN

Sharp: This might be a good place to talk a bit about your family, Mrs. Revelle and your children, at this point, and what your coming back to La Jolla would have meant in terms of the family, what changes--.

Revelle: Well, Ellen has always been a very good soldier. Whatever turned out she was willing to do. She loved La Jolla and wanted to come back here. During most of the war we lived in Silver Spring, just outside of Washington.

It was really a terrible place. It was a lower middle class community for the most part. At least I thought it was. Eventually we bought a house on Fulton Street, just off Foxhall Road. That was a complete transformation as far as level of living was concerned.

Sharp: That was better?

Revelle: It was right in the best part of Washington, the high-class part of Washington. Ellen bought that house pretty much on her own. By that I mean that she found it and pretty much agreed to buy it before I had a chance to look at it. But I liked it a lot; it was a beautiful place.

As you know, we have four children. The first one was born in 1932; she's fifty-two today. We've been trying to find her today, as Ellen told you, to suggest that she telephone her son, the schizophrenic. Our second daughter was born in 1936, Mary Ellen Revelle, who has been for many years, ever since 1957, married to an Italian physician, Pier Franco-Paci. He's on the staff of the Peter Ben Brigham Hospital. He's a cytological pathologist, a first-rate person, a very nice man--very Italian, and just as nice as he can be. Very generous hearted, a kind and good person, and a marvelous cook.

Revelle: Our third daughter Carolyn was born in 1939. She's married to an economist and lawyer named Gary Hufbauer, who's a La Jolla boy. Gary and Carolyn went to school at the same time; she went to the Bishops School, and he went to La Jolla High School. When they were here they never really went with each other or cared particularly for each other. Later he was pretty much involved with Helen Raitt's daughter, Martha Raitt. Then he went to Harvard, and Carolyn went to Wellesley. They got to know each other back there.

Our son Bill was born in 1944 in Washington, while we were still living in Silver Spring. That was just the time I was working my tail off in the Bureau of Ships, working very hard and very late at night. I remember that Ellen would bring him into bed and nurse him in the middle of the night, and he was a very noisy nurser. He'd ruffle and snuffle. [laughs] Kept me awake, and I took a dim view of that.

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Revelle: Ellen and the children spent the summer of 1945, when I was in Guam, in Woods Hole, Massachusetts. That's when they learned to love Woods Hole. Ellen and our youngest daughter Carolyn had spent two weeks there in 1943 and 1944, while our two oldest daughters went to a summer camp in New Hampshire. They had sweated out the rest of these two summers in Washington. In 1946 they all came out here for the summer, when I was out at Bikini. Annie, our oldest daughter, by the time we came back here, was fourteen or fifteen years old. Fifteen, I guess, in 1947. She had gone to school in Silver Spring, as did Mary Ellen. The Montgomery County schools in Maryland were quite good. But we eventually put Annie into the Cransbrook School in Michigan, which was a school started by her great-uncle, George Booth.

You know that Ellen is a granddaughter of James Scripps, who was really the founder of the prominent Scripps newspapers. His newspaper was the <u>Detroit News</u>, and his half-brother, E.W. Scripps, and his full sister, Ellen Browning Scripps, worked on that, as did another brother, George Scripps. They all got rich.

The <u>Detroit News</u> was a success from the day it began. The reason it was a success was that James Scripps had three original ideas. He was the real newspaper genius of the family, but he's very much under-appreciated because of the flamboyant character of E.W. Scripps. James Scripps's first idea was that there was a new class of readers in the United States that had not existed in England, where he came from. These were the working people.

Revelle: The United States has always emphasized universal education, which the British never did. So all the American working people could read and write, unlike the British working class, who couldn't. So he decided that he'd start a paper for these people. The only way that they could afford to buy a newspaper was if he kept his cost down to virtually nothing. So he sold his papers for a penny, literally. They'd usually been sold for a quarter before that.

Sharp: The readership would have been pretty broad, I would think.

Revelle: In Detroit, yes, it was.

The third idea was that his new class of readers were mostly interested in local news and short stories, so he insisted on having 250 stories in every edition of the paper.

Our son Bill is also married to a La Jolla girl, Eleanor McNown. He met her at Pomona College, although he was a good friend of her brother's, Rob McNown, when they were in high school here together. Mary Ellen and Carolyn went to Bishops. Bill and Annie went to La Jolla High School.

Sharp: How did you decide to send two to one and two to the other?

Revelle: Well, that was up to them. Mary Ellen was very shy and not very tough, very easily hurt. I don't quite know why Carolyn went there; why she wanted to go to Bishops. We didn't send our children anywhere. They went where they wanted to go.

Mary Ellen went to Pomona for two years. Then she spent a year in Italy, first at the University for Foreigners (L'universitá pas Straniére), they called it, in Perugia, where she learned Italian, and then at the University of Florence, where she met Piero. He was a medical student at the University of Florence.

One sort of simple rule of life to follow is never to send your daughter to Italy if you don't want her to marry an Italian. Something perhaps you should remember, although in Piero and Mary's case I was enthusiastic about their marriage, after I had made a special trip to Italy to meet him and to see the two of them together.

Sharp: I'll try to remember that. I'll write myself a note in her baby book.

Did you find yourself having some ideas about what you thought you might want them to do when they grew up, or was that part of what you thought about for them?

Revelle: Obviously every parent has dreams about his children becoming presidents of the United States. None of my daughters were interested in science at all. Carolyn particularly has a sort of a phobia about mathematics; Mary has to some extent also. I got turned towards geology by a class I took at Pomona with a man named Alfred Woodford. Ellen, also a non-scientist, had reluctantly taken geology during her senior year at Scripps, and to her surprise enjoyed it and got a reasonably good grade. So she thought Mary, too, could manage it. I thought also that maybe an easy science for Mary Ellen would be geology at Pomona, but it turned out to be very difficult for her. She practically flunked the course. I tried to teach her some, and the chemistry was just completely wrong for doing that. She just couldn't learn it, or wouldn't learn it, had a mental block about it. She didn't have a particularly distinguished record. She didn't flunk out of Pomona, but she didn't have a very high grade point average for the most part. She's very slow in examinations particularl[y] She can't seem to do things very fast.

Sharp: That's a real handicap in college.

Revelle: Yes. But as time has gone on she's gotten better and better. Her intelligence, good judgment, and remarkable intuition now actually shine! I'd like to show you a letter that we just received about her.

Sharp: Oh, I'd like to see that.

I thought at some point we might talk about your involvement in cultural things in La Jolla and San Diego County. Did Mary Ellen get to participate in some of that and maybe sort of start her on the road to getting into art history because of what you and Mrs. Revelle were getting involved in?

Revelle: I don't think so, not so far as I can recall. She's always loved Italy. We went there for the first time in 1948 and stayed in Florence. Then we went back there in the summertime for several years. We went more or less every summer until 1954. Then she decided that she was going to spend a longer time in Italy, so she spent her college junior year there in '55-'56, first in a summer program, some kind of exchange program, and then on her own. She never really took credit for that at Pomona; it was not really a part of the orthodox junior year abroad program.

But she fell in love with Piero, so the next summer, '57, they were married in Florence. They lived for eighteen months in Italy while he was finishing his military service. Then they came back to Boston, where he was an intern at the Quincy hospital, then a resident at Beth-Israel Hospital, one of the Harvard hospitals. He had a fellowship with a man named Craig, who was a pathologist there.

Revelle: Their three children were all born in this country, but then they went back to Italy; he was at Bari for a while, in southern Italy, and then later in Rome at the Maria Elena Instituto per Tumoria--it's a cancer institute. Italy has a peculiar medical system; they don't really recognize anybody else's degrees any more than the United States recognizes Italian degrees.

Sharp: So he had some trouble when he went back?

Revelle: Yes, he did. He didn't have a real job, although they brought him on to reorganize this cancer institute, the pathology part of it.

Sharp: Sometimes they have to retake exams.

Revelle: Really do it all over again, in spite of his six or seven years at Harvard. So they had to come back to this country, and they did. He's been on the staff of two Harvard hospitals--first Beth-Israel and then the Brigham.

Sharp: Does he still have a family in Italy?

Revelle: Oh, yes, a lot of family. Mary and Piero and their children are visiting Italy right now. His parents still live in Santa Maria degli Angeli, which is at the foot of the hill just below Assisi.

Piero has three sisters, just as my son has. One of them has had a hard time lately, getting a divorce. The other ones seem to be in good shape. They're all quite intellectual, the sisters.

Their father is a physician, and their grandfather was also a physician. But the father is really a violinist manqué; he'd much prefer to be a professional musician than a physician.

Sharp: It's quite a family to have some contact with now.

Revelle: It's an interesting family. Mary has three children, all of whom are spectacularly successful. One of them, Christopher, is a student at the Stanford Law School, a freshman at the Stanford Law School. He graduated magna cum laude from Yale. Stefano, the second son graduated from Columbia in architecture; he's about to go to graduate school in architecture. Myra, the third child, is a freshman at Yale.

Annie has five children by her first husband. They are a much more varied lot. Loran, the oldest boy may or may not finish his doctoral thesis in French literature. He's been trying to for years, but he seems to have a mental block about doing it. Mark Roger, Revelle: the second son, is a schizophrenic; he's the one who lives in York, Pennsylvania. One daughter, Holly, is married to a Guatemalan Indian; they live in Oregon. Holly's a professional hypnotist, believe it or not. Annie has another daughter, Cindy, who is a graduate at Scripps in neurophysiology, a very bright, active, beautiful girl with hell of a lot of drive, quite egocentric. A fifth child, a son Eric, is about sixteen and goes to the Buckingham Brown and Nichols School in Cambridge.

Annie herself is a peacenik. She runs something called the Mobilization for Survival in Boston, which is an anti-Reagan organization, to put it mildly. They are in favor of a nuclear freeze and in favor of getting out of Nicaragua and out of El Salvador. Annie is divorced; she married at the end of her freshman year at Wellesley. Her husband was a poor choice, I think. He runs a little publishing company in York, Pennsylvania, although he has a Ph.D. in oceanography.

Sharp: I think I was reading a little bit about arrangements for that wedding, and he was going to come back out to Scripps to be a graduate student.

Revelle: Yes, that's right, and he did. Four of their children were born here; the fifth one was born in York. She went on to finish her college undergraduate work at Millersville State College in Pennsylvania. Then she got a National Merit scholarship, one of those Ford Foundation scholarships, to go on to graduate work at Bryn Mawr. Just about that time she got pregnant, and I think her husband--that it was his idea that she should not go on to graduate work but be a good housewife and have children. Very old-fashioned type guy.

She's had bad luck with men. She's had two people she was married to and one she lived with, and none of them have worked out very well. Her problem basically is that she can't believe that people like her, so anybody who does like her, she falls for him completely. I think she's a wonderful woman, but she's had a very hard time in life.

Our third daughter, Carolyn Hufbauer, has two children. Randall is a sophomore at Pomona. Ellen, the other one, goes to the National Cathedral School in Washington, D.C. She is president of her class and has just received early admission to Harvard. Carolyn's husband, Gary Hufbauer, is very bright. He was Deputy Assistant Secretary of the Treasury in the Carter administration. He found in the Treasury Department that economists were supposed to be on tap but not on top, that the Treasury Department was actually run by

Revelle: lawyers. So he took a law degree while he was in the government, at Georgetown University, at night. He's now of counsel to a big law firm in Washington: Chapman, Duff, and Paul. He's also on the staff of the Institute of International Economics, which is a German Marshall Fund funded institution. Recently he has been appointed to the Wallenburg Professorship of International Financial Diplomacy at Georgetown University.

Carolyn is a restless, independent-minded person, and she has had a series of jobs in Washington. For several years she ran a landscape architecture program for George Washington University and did very well at that. She's an energetic, hard-working, dedicated kind of a gal. Whatever she does she does as well as she possibly can. She's now in a little firm in Washington which is practicing landscape architecture. I don't know how stable their marriage is, but so far it has held together all right.

Our son is very happily married to a La Jolla girl, Eleanor McNown. She comes from a Christian Science family. Both her mother and her father are very shy, retiring, sort of inward people. She tends to be that way, but she has forced herself not to be. She has been president of the League of Women Voters of Evanston, Illinois, and on the board of the state league. I wouldn't be surprised if she eventually became national president of the League of Women Voters.

Sharp: Maybe we'll see her as part of the presidential campaign debates somewhere along down the years.

Revelle: Yes, that's right. She was born in Evanston, likes Evanston, and I'm afraid they're never going to come back to California.

Sharp: People who really like Evanston are quite committed to staying there.

Revelle: That's right. Funny business. I like Chicago; Chicago is a great city, but it's a miserable climate, of course.

Sharp: It is, yes, very bad.

Revelle: Do you come from there?

Sharp: I'm from Kansas City, but my sister and her husband live in Evanston, so I know it very well. Springs are okay, but winter--forget it.

Revelle: Bill teaches at Northwestern University. I told you, I guess, the other day he called me up and said, "Dad, I'm a professor." He got promoted to the professorship.

Sharp: That's quite a plum. The tenure system has changed so drastically, you really have to be extremely good.

Revelle: He is very good; he is very good. He's an experimental psychologist, a personality psychologist. None of this stuff about physiology but about, basically, statistical tests of people's behavior and attitudes. Lately he has gotten into cognition, how the short-term memory works and how human beings learn.

Sharp: And measuring that? Is that part of the quantitative--?

Revelle: Yes. He's a computer wizard, very good at computers. He went on a couple of expeditions when he was a boy at Scripps. One was our Naga Expedition to the South China Sea and the Gulf of Thailand. The other was an expedition to the Bering Sea. His first scientific recognition came as a result of this Bering Sea expedition, when he was about fifteen years old.

If you look at the great book of geophysics, called The Earth, by Harold Jeffreys, it says that one of the great mysteries is how tidal friction works in the ocean to slow down the earth's rotation. And Jeffreys says it must be due to the strong tidal currents in the Arctic Ocean. Well, Walter Munk took a quite skeptical view of this statement, so he asked Bill to measure the currents in the Arctic Ocean on this expedition that he went on.

The way Bill did that was by putting a little block of wood, a float, over the side and having a string attached to it and seeing how fast it paid out the string. That's sort of a standard way of measuring currents at the surface. It turned out the currents were negligible; there was hardly any current at all.

Sharp: So Bill had a real revolutionary role right out the door.

Revelle: That's right. So in Gordon MacDonald and Munk's book on the rotation of the earth they have a footnote about William Revelle and his measurements of the Arctic Ocean currents.

His name is William Roger Revelle, but he never uses the Roger. He's obviously establishing his own identity: William Revelle. He publishes a lot, not as much as he should, I guess, but what he does publish is of high quality.

Sharp: And enough to make him a professor.

Revelle: He's a sort of mainstay of the Psychology Department at Northwestern.

Sharp: As I understand it, psychology departments line up on one side or another in terms of experimental psychology and sort of everything else.

Revelle: That's right. Here at UCSD it's mainly phsyiological, whereas Bill's group is largely--whatever you want to call it--non-physiological.

Bill and Eleanor have two children, by the way, two boys, David and Daniel, who are too young to tell what's going to happen to them. One of them is about thirteen or so and the other's about ten. But the ten-year-old particularly I have great hopes for as a scientist. I think he might turn out to be a good scientist.

Sharp: Does he do a little experimenting now?

Revelle: He doesn't do any experimenting, but he thinks awfully quickly and learns very quickly. He might turn out to be anything, but it's possible at least that he might be a scientist. They're both quite bright; the older one is more interested in baseball than he is in science.

Sharp: Well, considering this year, he's got a lot to take his mind into sports, Chicago is doing so well. That's quite an achievement for the Cubs to be in the World Series, for sure. Forty-five years or something--. Pretty astonishing.

Revelle: That's right.

Sharp: My brother-in-law was very pleased.

Revelle: The one who lives in Evanston?

Sharp: Right. Quite proud that the Cubs had done so well to get into the series.

Revelle: Neither Northwestern nor Chicago, neither of the two universities, do very well in sports at all of course. I don't think Northwestern has ever won a football game in the Big Ten. And Chicago gave up altogether. Still, coming from UCSD, we can't complain.

Sharp: You don't expect a lot.

Revelle: No.

Sharp: When I was an undergraduate here that was always the standard joke, that sports at UCSD was a little water polo.

Revelle: Tennis, volleyball.

Sharp: Yes, a little tennis, a little volleyball. Water polo was probably the area of most achievement.

Revelle: Really?

Sharp: Yes. I understand their teams do pretty well.

IV ONR AND SCRIPPS IN THE POST-WAR ERA

Midpac, The Visibility Lab, Operation Ivy

Sharp: I thought I might get us to talk some about ONR and you at Scripps after you came back, and as the large contracts began to come to Scripps, mainly because it shows a lot about you. It also shows a lot how ONR was thinking, what it wanted to have done, and what Scripps was doing, and how it all meshed together, because they certainly--.

Revelle: I think that's very interesting. Did you see that letter from Rawson Bennett?

Sharp: Yes.

Revelle: That was a very interesting letter. Rawson and I had been associated ever since the late thirties, since about 1938 or '39. He was the test officer, the experimental officer, on the destroyer squadron that came out here to study the behavior of sonar gear. I guess it was a destroyer division, four destroyers. He was a great big man. He had feet about my size, and he was about as tall as I am, and much heavier, much bigger.

Sharp: That's a big presence then.

Revelle: Yes, he had a tremendous presence. As I told you, he was later head of the electronics division of the Bureau of Ships, and I worked for him there. I guess I told you how I got there, didn't I? I couldn't stand working for the Navy Radio and Sound Laboratory.

Sharp: I remember seeing that 1942 letter when you laid it out pretty clearly that you wanted to go to Washington.

Revelle: I thought we talked about that already.*

Sharp: We did.

Revelle: But anyhow Rawson got me a job in the Hydrographic Office. He was always very cautious and very conservative and didn't want to build up his staff. It was all right for me to work for him.

Sharp: But a little at a distance?

Revelle: Not at a distance, right there, but not on his payroll. The Hydrographi Office paid the bills, but I worked entirely for him and Jack Myers, never for the Hydrographic Office. I got Mary Sears, remember, to do my job in the Hydrographic Office. That turned out very well, so well in fact that what was then the Hydrographic Office is now called the Oceanographic Office of the Navy.

Sharp: She was quite productive and very dynamic.

Revelle: Fantastic. Small, stout woman. Never married. But a will of iron. Anyhow, after the war Rawson came out here as head of the Navy Electronics Laboratory when I came back from Washington. I had this idea that we should have a deep-sea expedition. We really should start exploring the ocean and not just the southern California coast line. So we dreamed up this two-ship expedition, which was called Midpac, the Mid-Pacific expedition.** One ship should be our tug Horizon, and the other ship we wanted was the Navy Electronics Laboratory PCE(R)-857.

I think Rawson was in favor of this expedition, but he didn't want to stick his neck out. So he wrote that letter saying all these things that would be held up if we assigned the ship to the expedition. On the other hand, it would be a benefit in some other way. It left it up to the avy department, which was fine with me, because I had lots of friends in the avy department. So in fact we did have the expedition.

One of the things that I remember most about it was that the officers and crew of the navy ship were very reluctant to go on this expedition. After we had been out at sea a few days their clutch broke down, so they said they wanted to go back to port. Unfortunately they didn't have enough fuel to go back. It could have taken maybe four or five days.

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*See previous interview for this discussion.

**The Mid-Pacific Expedition is covered more completely in Interview V.

Revelle: In any case, Horizon had a lot of fuel, and we used it very carefully and very penuriously to fuel this navy ship. Jim [James L.] Faughn was our captain, another gem of a man, very able and very dedicated. He never gave them enough fuel to get back to port. [laughs] Just enough to keep on with us. We went down as far as the equator. Our plan was to go from the equator up to Hawaii and then out to the Marshall Islands.

By the time they had gotten to the equator there was clearly no possibility of getting back to San Diego. They followed us all the way to the Hawaiian Islands. They went into the navy yard and sent for their families, thinking that it would be at least a three months job to get it repaired, or hoping that it would be a three months job. I went to the commandant of the navy yard and said, "This is a high priority ONR expedition; we've got to get that ship out of here." So he gave it number one priority in the yard, and they got out in a week.

In the meantime we had gone on to the Mid-Pacific Mountains west of Hawaii and did quite a bit of interesting work there, including several things that were quite revolutionary. The Midpac expedition was really a great expedition in terms of its results. One thing we found was that the Mid-Pacific Mountains, which extend west at about right angles to the Hawaiian chain, out as far as Wake Island, were a great underwater mountain range, and the mountains were flat on top. That meant that they were what Harry Hess had called guyots, named for the Frenchman for whom the geology building at Princeton is named.

Harry had thought that they must be as old as the ocean, two billion or three billion years old. They had just slowly sunk over that tremendously long period. We found, on the tops of these flat-topped seamounts, shallow water corals, which meant that they had been cut off by wave action at sea level, just as Harry had thought. But corals didn't even exist two billion years ago. They were actually Upper Cretaceous corals, about eighty million years old. So the seamounts had sunk, or the ocean had risen, during the last eighty million years, by six thousand feet. This was really one of the very first pieces of evidence about sea floor spreading, although we didn't realize it at the time. As the ocean bottom spreads out from the mid-ocean ridge, it gets deeper and deeper.

Sharp: And changes really the whole configuration of that part of the ocean?

Revelle: Well, no part of the ocean is more than 150 million years old, none of the sea floor. In that particular area it was only about eighty million years old. Russ Raitt at the same time had been doing seismic measurements, and he found that the sediments on the sea floor were only one or two hundred meters thick instead of thousands of meters thick, as they would have been if the ocean was old. A large part of the deep-sea floor had only a small amount of sediments; they just weren't there. The reason they weren't there was that they had never been deposited; the sea floor was quite young.

The other thing we did was to measure the heat flow through the sea floor. Quite contrary to expectation, it turned out to be at least as high and maybe higher than on land, which was really quite a surprising result. Everything turned out to be different than anybody had thought it would be before. Particularly those three results were quite memorable: the heat flow, the thinness of the sediments, and the young age of the guyots.

Sharp: The fact of ONR's participation in Midpac leads me to ask you how these particular discoveries benefited the navy or ONR.

Revelle: They didn't really benefit the navy directly. They were fundamental discoveries about the ocean, but they didn't tell you very much about how a submarine could behave.

Sharp: So they were still very much in the basic research category.

Revelle: Sure. Everything in the expedition that I remember was basic research. But there was clearly, from that letter of Rawson Bennett's, some underwater sound propagation work that was done too. I don't really remember that, and I don't think anybody on the expedition took it very seriously. Not that we shouldn't have, but we just didn't have people who were much concerned about it.

We finally got out to Bikini. That was the end point of the expedition. Russ Raitt made some seismic measurements out there which confirmed what had already been discovered on the Crossroads operation; that the coral reef was about five thousand feet thick, underlain by an old guyot that had somehow become a coral atoll instead of a guyot. In other words, the atoll was underlain by a flat-topped seamount. It was just about the same depth as the Mid-Pacific Mountain guyots, but with four or five thousand feet of coral on top of it.

Revelle: That was in 1950. I was still just acting director of the Scripps Institution at that time. That was the beginning of the Scripps program of worldwide exploration. We really just transformed the place by that expedition.

Sharp: Let me ask you to look again at this chronology that I drew up. We don't need to talk about the entries one by one, certainly, but maybe you could make some general comments about the direction of this work and what expectations ONR might have had about Scripps and Scripps's participation especially in the thermonuclear tests: Ivy and Castle, and the deep underwater test, Wigwam.

Revelle: Well, some things in here are not so.

Sharp: Okay, which ones?

Revelle: You say in 1951 Walter Munk started work with MIT's Visibility Lab. No, he didn't. He never worked with the Visibility Lab. The Visibility Lab was headed by a man named [Seibert] Quimby Duntley. In fact the Visibility Lab still exists. Duntley was basically interested in how you see things, the actual physical problem of seeing an airplane, for example, in the air or a submarine in the water from the air. The National Research Council had, maybe still has, a committee on visibility, and he was for many years the leading spirit of that committee. They were located down at NEL, not in the same place as the navy laboratory but in the NEL area, a different place.

It was really a very specialized operation. They had one man, John Tyler, who did quite a bit of work on the transmission of light in sea water. That was good fundamental research. The rest of it was fairly applied research which had quite a bit of mathematics in it. Duntley was a physicist. But not in the mainstream of oceanographic research. I think your description of it is correct, except I don't remember that we actually constructed a new facility. We just used the old buildings down there.

Sharp: I guess what I was getting at was the idea that the Visibility Lab had a west coast branch.

Revelle: No, this was it. MIT was the branch after Duntley moved out here.

Sharp: And Dr. Munk was not part of that?

Revelle: No, not at all.

Sharp: Was ONR?

Revelle: I think it was probably funded by ONR, yes.

Sharp: Did some Scripps people work with Duntley?

Revelle: No, he brought his own group with him.

Sharp: Entirely, then?

Revelle: They were never really on the faculty here, though eventually Quimby was. It was essentially a service organization from our point of view. Nothing wrong with that, but I mean it was really in some way an adjunct of Scripps rather than a real part of it. Duntley is now retired and a man named Roswell Austin is in charge of it.

Operation Ivy was the beginning of our Capricorn Expedition. Walter Munk and Bill Bascom were there, and John Isaacs was there. I think Russell Raitt was there too, but I think he and I and most of the rest of us arrived, like Gustaf Arrhenius, just at the end of the Ivy test. We were starting on our exploring expedition.

Sharp: Right. That's what I think, about maybe October. I have some very specific questions about Ivy if we could handle those now. The operation itself occurred in October 1952. From what I understood the idea was to see the effects of atomic explosions on submarines at periscope depth and the creation of a tsunami was the specific event that would--the effects of that were what you were going--is that right?

Revelle: More or less. The primary object of the test was to test a thermonuclear weapon, a thermonuclear device, I guess would be the best word, to see whether it worked or not. But as was typical of those tests out there in the Marshall Islands, as opposed to the later tests in Nevada, they tried to do as many things as they could, learn as much as they could about the effects, the phenomenology as it was called. We, as usual, were there to see what the waves would be like and what other underwater phenomena there would be. I don't believe the purpose of the test was to see what it would do to a submarine at periscope depth.

One of the things that we did get involved with was an idea that John Isaacs and I and Walter Munk had, which was that we might trigger a tsunami. We were worried enough about that so that we conned the navy into spending a hell of a lot of money to be prepared to evacuate all the Pacific islands that were liable to be affected by it, to make many of the observations from the air rather than from ships. It cost the navy about $50 million, our foolish idea. But it was not necessarily a foolish idea; it just turned out not to be so.

Sharp: Not if it had happened.

Revelle: Yes. The reason we thought it might happen was that it was clear that in the past there had been big slides from the atoll. The western side of the atoll was practically vertical for a thousand feet or so, in fact overhanging. The coral reefs built out near the surface more than they had underneath. We were afraid there would be a big landslide, and if so it would trigger a tsunami.

This is a very amusing story. I have mentioned it before. At the time of the test Walter Munk and Bill Bascom were sitting in two rubber rafts with recording devices which were supposed to record a sudden change in pressure on the bottom, which would indicate a landslide, at least the start of a tsunami. They sat there and nothing happened of course.

Just about that time the radioactive cloud started to move in their direction. The Horizon was standing by and so they decided to pick up Bill and Walter and to move out to avoid the radioactive cloud.

Later they came back to pick up the instruments. Walter's instruments had a huge signal on it, recorded after he had left. It was obviously due to some malfunction of the instrument. But in any case it was a very big signal.

He still wonders what he would have done if he'd still been there and seen that signal and sent the word to the Horizon, which would have sent the word to the fleet to start evacuating the islands. He said he probably would never have come back. [laughs] It's just one more example of how you've got to be lucky.

Sharp: Right. Very much so.

Revelle: One of the bad things about that particular operation was that the two ships, particularly the Horizon, got heavily contaminated with radioactivity. We went right on with the expedition. We didn't really realize at the time how low the level of radioactivity had to be to be tolerable.

Sharp: Medically?

Revelle: Yes. But more particularly, radioactive contamination made it impossible to do any work after that on the Horizon that involved radioactive material. The background was just too high.

Sharp: You say that people weren't very aware of what the radioactivity, even low levels of exposure--.

Revelle: Well, what happened was basically that the tolerance levels went down a lot after that. We were careful to stay within the tolerance level as it was thought to be at that time. But that was a lot higher than what you needed for experimental work, where you have to have essentially a natural cosmic-ray background. I suppose if some of the crew got cancer now they might think that that's how it happened.

Sharp: In fact there are some. I know, for example, about an oral history that was done with Dr. Stafford Warren at UCLA. That may be subpoenaed as evidence in a couple of cases of people who were with--I'm not sure if it was Operation Crossroads.*

Revelle: It must have been Crossroads because that was the only one he was on.

Sharp: They were using the oral history as evidence to support some people's claims that the cancer that they have now resulted from their exposure at Crossroads.

Revelle: I wouldn't be surprised.

Sharp: That's something that you have thought very much about personally?

Revelle: No. Why should I?

Sharp: That you might have too much exposure yourself over long periods of time because of your involvement in some of the operations?

Revelle: I've never really thought about it. Maybe I should but I haven't. Staff [Warren] was just as careful as he could be. It seemed to me he leaned over backwards to try to avoid hazards. That was a very dangerous operation, however, for many people because of having to try to clean up the ships afterwards. Not for us, but for the enlisted men primarily.

[Brief conversation with third person]

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*Stafford L. Warren was interviewed by Adelaide Tusler of the UCLA Oral History Office. See An Exceptional Man for Exceptional Challenges, UCLA, in 1966 and 1967. The oral history was not made available until 1983.

Sharp: You were telling me that you didn't consider the danger or the possibility of danger to yourself in the tests.

Revelle: We were just all in the same boat pretty much.

Sharp: Literally.

Revelle: Yes, that's right. I mentioned this story before also. I remember at the airborne test at Crossroads I was standing on the deck of Mt. McKinley, that was Admiral Blandy's flagship, along with Norris Bradbury, who was my classmate at Pomona [College] and director of the Los Alamos Laboratory. I remember him jumping up and down with excitement and glee. He said, "Those things always go off!" That was only the fourth one that had ever been exploded; one at Alamogordo [New Mexico], two in Japan, and this was the fourth. This was the airborne drop.

Sharp: That's right, which was Able.

Revelle: Able, yes, the Able test. I would say we were at least five or six miles from the atoll at that time. We didn't have a radioactive cloud flying over us.

Sharp: One of the people who played a part in Capricorn that I was surprised to see was--I wanted to show you this letter that I found--was President Robert Gordon Sproul. I thought we might talk a little bit about that. I want to show you some other things, too, that I found after I got that all ready. This is a letter that you sent to Sproul describing what the operation was to be. I don't know if you remember that letter or writing it to him, or about why you wrote it.* [lengthy pause while Revelle reads letter]

Revelle: I see Russ [Raitt] was out there during the test. All sounds very reasonable.

Sharp: Why would you have written him a letter like this?

Revelle: Because we were doing something that, I guess, the U.C. regents should have certainly been informed of.

---

*See following pages, pp. 46a-46b for this letter.

16 April 1952

[redacted]

Memorandum

To: President Robert G. Sproul

Subj: Proposed participation of Scripps Institution in Operation Ivy

We have been requested by the Office of Naval Research to undertake responsibility for two phases of tests of a large atomic weapon at Eniwetok during October and November, 1952. You are undoubtedly aware, from other sources, of the nature of these tests. Our responsibility would include: (a) a seismic refraction survey of Eniwetok atoll to be undertaken under the general direction of Professor Russell W. Raitt; (b) measurements of surface water waves generated outside the atoll under the general supervision of Mr. Willard Bascom, Assistant Research Engineer on our staff.

In order to accomplish these tasks, we are proposing that Contract Nonr-233(05) be increased in the amount of $210,000. This proposal is stated in rather general terms so as to avoid the necessity of classifying it.

Both of these investigations are of very considerable geophysical interest. As you are aware, Professor Raitt has recently completed working up the results obtained from his seismic refraction survey of the Bikini atoll carried out on the Mid-Pacific expedition in the summer of 1950. These results show that Bikini is very porbably a submerged volcano covered with a crust some 3,500 feet thick of the skeletons of corals and other marine organisms. This constitutes the first definite proof of Darwin's hypothesis, proposed over 100 years ago, that coral atolls are formed around a subsiding island. As part of the preliminary work for Operation Ivy, three deep holes are being drilled in Eniwetok atoll down to 5,000 feet. It is hoped that these will penetrate through the coral cover into the underlying volcanic rock. A seismic refraction survey of this atoll would clearly extend the significance of the drilling and, at the same time, furnish positive confirmation of the results on Bikini. The combined work on the two atolls should constitute a classic of the earth sciences.

From the military point of view, it is necessary to know the attenuation of seismic energy in the coral and in the underlying volcanic material, in order to interpret the earth shock resulting from the test explosion. This earth shock is expected to be sufficiently severe to demonstrate the value of the weapon against such military objectives as oil fields.

[redacted]

[redacted]

To: President Sproul -2- 16 April 1952

The measurement of the waves produced by the explosion is likewise of both scientific and military interest. For the first time we will have here a man-made event of geophysical magnitude corresponding in energy, although somewhat smaller, to the great explosion of Krakatoa, which produced devastating waves at distances of 30 miles and which produced measurable waves in both the atmosphere and the sea at distances of many thousands of miles. In other words, we may have here for the first time a man-made tsunami or seismic sea wave which can be measured accurately and observations of which may be valuable in interpreting natural tsunamis.

The schedule of the Operation will be somewhat as follows: HORIZON and SEAMOUNT (SPENCER F. BAIRD) leave San Diego approximately September 23rd, arrive at Kwajalein October 12; refuel and reprovision at Kwajaleinn October 13; refraction survey begins October 14 and ends October 23; buoys for wave measurement installed on seamounts north of Eniwetok October 24 to November 1; buoys recovered November 1 to November 10; refueling and reprovisioning November 11 and 12; the two ships sail on a southward course to Tonga November 12; return by way of the Marquesas and Tuamotu Islands, and the Albatross Plateau November 12 to January 15, 1953. The latter part of the expedition, subsequent to the departure from Kwajalein, will be supported by other ONR funds except for a nominal 20 days required for the return trip to the continental United States. The entire expedition will be geological and geophysical in character, in contrast to our Shellback operation, which leaves May 15, and which will deal primarily with the circulation and character of the ocean waters.

The question may logically be asked, why should Scripps vessels be used for this work at Eniwetok rather than U.S. navy ships? The answer is that a good deal of special gear has been built into our ships for seismic refraction work which is not available on Navy ships, and that for the wave measurements it will be absolutely essential to use a dredging winch such as we are attempting to obtain for the SEAMOUNT. The dredging winch on HORIZON, though not satisfactory for use at oceanic depth greater than 15,000 feet, will also be available for such wave measurements as we can make by lowering instruments to the tops of seamounts at depths of about 3,000 to 4,500 feet.

In addition to the two ships, we will also plan to station observers on five island stations; probably Wake, Midway, Guam, Ponape, and Bikini. To measure the waves at these distances, "tsunami" recorders somewhat like those developed by Dr. Munk and his collaborators will be used. We are cooperating with the Department of Engineering in Berkeley and with the Woods Hole Oceanographic Institution in the work. I hope to participate in at least part of the expedition.

Respectfully,

Roger Revelle
Director

[redacted]

RR:rb
cc: (blind) Mr. J. W. Smith, ONR
Mr. G. G. Lill, ONR

Sharp: That's what it looked like, but I wasn't sure what role besides an informational one Sproul might have had. I did find this other letter of yours asking Sproul to contact the U.S. State Department and to communicate with them, with foreign ministries of the islands involved, for the whole Capricorn Expedition.*

##

Revelle: We thought we might need help on the islands we planned to visit, first Fiji and then Tonga, then Samoa. (That was American Samoa.) Then Tahiti, then the Marquesas [Islands]. We also visited the Tuamotus [Islands] and the Cook Islands.

Side Note on Robert Gordon Sproul

Sharp: I was wondering how you might remember Sproul in this period, and what kind of involvement or contact other than this letter you might have had with him about the expedition itself, what role he might have played?

Revelle: Actually I don't remember his playing any role, but maybe he did. He was always enthusiastic about Scripps. He was always enthusiastic about everything, everything enterprising that the university did. We were kind of pets of his.

Sharp: That's clear; that's clear in some of the other documents I've seen. I was wondering what your personal relationship might have been with him.

Revelle: I think it was very good. It's awfully hard to say though. He was an opaque man, though he was so jolly and so sort of boisterous. But very difficult to know how he really felt about anything. At least that's the way it struck me. Being president of the University of California is probably second in importance to being president of the United States--well, not quite; Harvard may be even more so. But it's a very important job, and it requires a guy who's very much of a politician. And politicians are born to be opaque, not transparent.

Sharp: At the point of these expeditions you were in some respects <u>the</u> new person, obviously the new director at Scripps, and maybe in somewhat of a vulnerable position in the sense that these expeditions had to go well in terms of your own future and credibility and so on.

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*See p. 47a for this letter.

Director's Office

17 October 1952

PRESIDENT ROBERT SPROUL

Our two largest ships, the HORIZON and the BAIRD, will soon be sailing on our CAPRICORN Expedition. The second leg of the expedition, which is designed to reveal the basic structure of the South Pacific and its later geologic history, will depart from Kwajalein in the Marshalls in mid-November following the military phase. It will traverse a region of sparse European settlements. It is important that our visit to these settlements be conducted on a social and political level commensurate with the high scientific ideals of the expedition and the scientific attainments of its members.

From the standpoint of the best results for the expedition, and in the interest of good international relations, I believe it would be appropriate for the United States Department of State to communicate with foreign ministries of the countries concerned, informing them of the purposes of the expedition, the names of the participants and the approximate dates of our arrival in their possessions, and requesting their good offices in facilitating the scientific work.

Could you attempt to persuade the State Department to do this?

It is planned that visits be made to Suva in the Fijis, a British Crown Colony, about 25 November; Pago Pago in American Samoa about 10 December; Papeete in the Society Islands, a French Colony, and Nuku Hiva in the Marquesas, a French Colony, in late December. In addition we wish to visit Raratonga and Mangaia in the Cook Islands, administered by New Zealand, and perhaps Tongatabu in the Tonga group, a British protectorate.

Biographical sketches of some of the members of the expedition are enclosed. In addition to those mentioned, we are taking eight graduate students and ten technicians in the scientific party.

Respectfully,

Roger Revelle

Sharp: Sproul might have been in a position to really see and assess what the expedition was doing and all of that. I just thought you might have had more contact with him as the expedition went on.

Revelle: I don't remember whether you mentioned it or not, but the key thing that happened to my career, as far as becoming director was concerned, was a conference we had out here at La Jolla in the spring [March] of 1951, I guess. I can't quite remember. When did I become director, July '51? This conference was held after our Mid-Pacific expedition. We assembled a quite distinguished group of people. Detlev Bronk, the president of the National Academy of Sciences, came out, and Bob Sproul was here the whole time. It was a conference on the future of the Scripps Institution of Oceanography.* I think probably Rawson [Bennett] was here, although I'm not sure about that. This was basically Bert Walford's idea, and his idea was that if I was going to become director I had to put on a spectacular of some kind.

Sharp: The conference coincided with the Charter Day events.

Revelle: I don't remember just when it was, but one of the things we did do was to dedicate the aquarium museum. We have some lovely pictures of Bob [Sproul] and Bronk and me doing that. Bob really devoted two or three days, the whole time of this conference, to the conference. I realize now that what he was doing was making it possible for me to be director. He wanted me to be director, but like all other people in the University of California he wasn't a free agent. It had to be sort of a general-overwhelming-the-opposition sort of thing, and that's what it was.

Sharp: I thought we might talk more about this when we talk about your years as director. I mean the transition from associate to director was not smooth.

Revelle: The transition from assistant to acting director was smooth enough. The path to director was not smooth; that's quite right. No problem about being acting director, which is a very bad thing to be, by the way. Nobody should be acting director.

Sharp: Because of just the limitations that you have?

Revelle: Yes. You can't do anything. I didn't pay much attention to the limitations, but I should have.

---

*This conference was held 25-27 March 1951. See the following pages for the announcement for Charter Day 1951 and Revelle's letter to Dean L.M.K. Boelter at UCLA inviting him to the conference.

10 Direc
Box 1

12 February 1951

COPY

UNIVERSITY OF CALIFORNIA
SCRIPPS INSTITUTION OF OCEANOGRAPHY
CHARTER ANNIVERSARY CELEBRATION
EAST LAWN, MONDAY, MARCH 26, 3:00 P.M.
PRESIDENT ROBERT GORDON SPROUL, PRESIDING

Invocation: Reverend George Giffin Culbertson

Address of Welcome:

Dr. Roger Randall Revelle, Acting Director of the Scripps Institution of Oceanography

Address: "The Position of the Scripps Institution of Oceanography in the University, the State, and the Nation."

President Robert Gordon Sproul, University of California

Address: "Oceanography and the Nation's Welfare."

President Detlev Wulf Bronk, Johns Hopkins University

Dedication of Thomas Wayland Vaughan Aquarium Museum

Response from Dr. Thomas Wayland Vaughan, Director Emeritus, Scripps Institution of Oceanography

Benediction: Reverend George Giffin Culbertson

COPY

S'O dirtec
Box 1

Director's Office

24 February 1951

Dean L. M. K. Boelter
2066D Engineering Building
University of California
405 Hilgard Avenue
Los Angeles 24, California

Dear Dean Boelter:

Knowing of your interest in the Scripps Institution and in the science of oceanography, I am writing to invite you to a conference which we propose to hold here on the La Jolla Campus on 25, 26 and 27 March 1951. The purpose of this conference is to discuss the place and function of the Scripps Institution of Oceanography in the University, the State and the Nation. At this critical period in national and University affairs we believe it essential to take stock of the contributions which this Institution can make through research and education, and to consider carefully the possible future of the La Jolla Campus within the University of California. We hope at this conference to be able to size up our problems and our future in the light of our experience, and with the advice of a number of invited guests who can give us good counsel.

We want very much to have you attend if you can do so. President Sproul plans to be here part of the time at least, and we are inviting ten others from the northern and southern sections of the University. These include, from Los Angeles, yourself, Deans Dodd, Knudsen, and Professors Epling, Jahn, Kinsey, Hugh Miller and Slichter. An equal number of people from outside the University have been invited including President Bronk of Johns Hopkins University, G. E. Hutchinson and Daniel Merriman from Yale University, C. O'D. Iselin, Admiral Solberg and Admiral Nimitz, together with representatives of the petroleum industry and of fisheries organizations. The meeting is scheduled to begin at 2:00 P.M., Sunday 25 March on the La Jolla Campus and to continue until 27 March. I hasten to point out that the 25th is Easter Sunday and for this reason some of the invitees may be unable to be with us Sunday afternoon when we plan to go over the La Jolla Campus and the Point Loma Annex, including our four sea-going vessels. But we hope that all who can possibly do so will be present at dinner Sunday evening and be able to take part in the ensuing roundtable discussion.

We plan to make reservations for all our guests at one hotel in La Jolla or nearby so that the group will be able to meet together for inform[al] discussions which should be a most valuable supplement to the planned conference sessions. You will be informed later concerning details of arrangement[s].

Some funds are available to defray the travel expenses of those who cannot otherwise handle them. If you wish to have these costs defrayed, please indicate on the enclosed sheet. We will send you a travel advance together with required forms and instructions.

Very sincerely yours,

RR:mcw

Roger Revelle, Acting Direct[or]

Sharp: I thought we might talk more about Dr. Sproul later on when we talk about your years as director, especially his retirement and whatever changes that might have meant when Clark Kerr came in, because certainly there were some changes that would have occurred--.

Revelle: That has to do primarily with UCSD more than with Scripps. There was never any question about my being a fair-haired boy of the regents and the university administration as far as being director of Scripps was concerned. They wouldn't let me quit, in fact. I was still director of Scripps until 1964, even though I was University Dean of Research at the same time. That's another story, really, about Bob [Sproul] and Clark [Kerr].

In 1950 we had something called the loyalty oath controversy.

Sharp: Thought we'd talk about that too, maybe the next time we meet.

Revelle: He [President Sproul] was very much on the spot then. He never recovered from that. John Francis Neylan particularly just hounded him to death after that.

Sharp: Governor [Earl] Warren had quite a bit to do with the loyalty oath.

Revelle: Oh, he played a big role, a wonderful role in fact. I took a vow to vote for him no matter what he ran for from then on. There was a famous meeting at [U.C.] Davis which adopted what they called the "alumni compromise."

Sharp: When was that, now?

Revelle: That was in the spring of 1950, before the Midpac Expedition. Steve Bechtel and Donald McLaughlin and others had been appointed by the alumni association to try to resolve this issue. They proposed the "alumni compromise," which was adopted by the regents at their meeting at Davis. Warren was there presiding over the regents; he was the ex officio president of the regents. He had always been on the faculty side, but he showed it very much that day.

After the vote Mario Gianinni--the Gianinnis always were entitled to have a member on the board, apparently--said, "The flags will fly in the Kremlin tonight. I hereby resign from this board of regents. I'm going to spend the rest of my life organizing vigilantes to fight Communism." Those are almost his literal words. I know he said, "I hereby resign from this board of regents. The flag will fly in the Kremlin tonight." The rest of it, I'm a little bit confused about exactly what he said, but that's essentially what it was.

Sharp: What a divisive event.

Revelle: It was terrible. It was an awful time. What Governor Warren said was, "Mario, you don't want to do that. If you don't agree with us it's your duty to stay here on the board and try to persuade us that we're wrong." Mario didn't say anything, but he did resign. I thought that was so in accordance with the true spirit of democracy, what Warren said. I thought it was a wonderful statement. And we went then happily on the expedition, feeling that everything was under control. But it really wasn't.

Sharp: Not at all. It came up again and again later that year.

Revelle: But we were pretty much out of it. Russ and I, neither of us signed the oath originally. I guess maybe we did sign it before we went, as a result of the alumni compromise, before we went on the expedition.

I was acting director all this time, so my neck was way out, but I felt so strongly about it that I was willing to forget about the future and just stick to that issue.

What I was opposed to was the regents' violation of the principle of tenure, which I think is an absolutely fundamental principle for a university. It turned out that in the long run it got settled in such a way that tenure was preserved.

I'm sorry; this is taking us a little far afield from ONR.

Sharp: No, I think that's probably okay because it's all happening at the same time and it's hard to separate the topics. But we'll come back to ONR.

Scripps Assisting the Navy in Thermonuclear Testing

Revelle: What you keep coming back to is what good did all this do to ONR. I don't think that would ever occur to the ONR people. Their principle, as I said, was to support first-rate science. And this was certainly first-rate science. Not high energy physics, of course, not the fundamental constitution of the universe, but insofar as any kind of geophysics is first-rate science, this was.

Sharp: One of the things that struck me about the different operations in this period was something that you had said earlier about the cumulative nature of the atomic test operations, that people were

Sharp: learning more and more about the effects of the explosions and understanding better what happens to the waves, and all of the dynamics of what the testing caused. The scientists were building on what they had learned in the previous operation, and these were pretty closely following one right after another to learn specific new things and add to the body of information.

Revelle: You mean these nuclear tests?

Sharp: Specifically Scripps's role in measuring the effects of the waves and the waves themselves. They were learning, one operation after the next, more and more.

Revelle: I see. Is that in writing somewhere?

Sharp: No, it's just sort of a conclusion that I had come to from when you were first talking about ONR and the idea of supporting basic research, recognizing that a scientist builds on what he or she does cumulatively, that one experiment after another, one test after another, that that's really collectively what a lot of this testing is, is learning more.

Revelle: Maybe my attitude has changed since then. I at least don't think now, and maybe I didn't think then, that we were adding cumulatively very much in studying the phenomenology of the tests.

We did those jobs in Bikini and in Eniwetok and in Wigwam, I think, basically out of a sense of obligation to the navy, not thinking that we'd learn very much oceanography. I won't say it was a price we had to pay for being supported by ONR; it wasn't. There wasn't any price for that.

But nevertheless, I'd been in the navy for eight years, and all of us had been involved with World War II. We felt that we ought to do what we could to help the United States government. At least that's the way I feel about it now, and I think I felt that way then. I don't think we thought of it as very good science.

We got some science out of it. For example, in [Operation] Wigwam, one of the very interesting results was that the radioactivity seemed to be in very thin layers. You remember that was an underwater explosion at a depth of around two or three thousand feet. It did broach the surface, but there was not much of a mushroom cloud. I don't remember there was any mushroom cloud, just a big bump on the surface. Some radioactivity escaped into the air, but most of it went into the water. It went into the water in a series of sheet-like layers.

Sharp: Very spread out?

Revelle: Well, they weren't very continuous. We made one series of measurements the meter would jump like this [sketches meter reading on paper] every now and then and then jump back again as you kept your instrument going down. This was an instrument designed by Ted [Theodore R.] Folsom at Scripps. Then we'd lower it again, half an hour later, and we'd get these same jumps but at a different depth. So apparently you had lenses of radioactivity rather than a continuous sheet.

The way to describe the phenomenon is that you can think of the ocean as like a deck of cards with a series of sheets superimposed on each other, which don't exchange very rapidly. The water from one layer doesn't exchange with the water from another layer. They're each pretty much independently acting sheets of water.

I'm pretty well convinced that that's the way the ocean is, although there have been few opportunities to test it as well as we had had in Wigwam. Particularly in the thermocline it was true but also to some extent in the mixed layer.

So that was a real scientific result, but unfortunately hard to confirm. The trouble with the atomic bomb tests was you only did them once. If you could do them repeatedly--.

Sharp: Once was probably enough for a lot of people.

Revelle: Oh, God yes. I'm not complaining. But that's what kept it from being very good science.

Besides the ships out on the atoll, Bill [William G.] Van Dorn visited half a dozen islands and installed tsunami recorders on them, basically sea-level or tide gauge type recorders, in case we did have a small tsunami. That was a real scientific adventure to find out what sort of events did happen, low frequency events, in the Pacific islands. Bill found very few tsunamis. I'm not sure that he ever had a very good one.

But you know, there is now a tsunami warning system in the Pacific. It's maintained by the cooperation of several Pacific countries. It's basically an information network, a communications network based on tide gauges.

The tsunamis are quite interesting phenomena. They travel at a velocity that depends on the depth of the water. The formula is simply that the velocity in meters per second equals the square

Revelle: root of the density of the water times the acceleration of gravity times the average depth of the water in meters, a very simple formula. The velocity of the tsunami's wave turns out to be about eight hundred miles an hour across the deep Pacific.

Sharp: At that velocity, what sort of warning is possible?

Revelle: A couple of hours. That's a lot. There are certain places that are particularly at risk, one of them is the harbor of Hilo, in Hawaii. The harbor has such a configuration that it acts as a kind of a funnel and concentrates the wave.

V MEMBERSHIP ON NAVAL RESEARCH ADVISORY COMMITTEE

Sharp: I'm pretty much done with my questions, but I realize that we didn't talk about NRAC [Naval Research Advisory Committee]. I thought you might want to say just a little bit about it. This portion that I sent you is from a very elaborate report that NRAC had had done by Arthur D. Little [Inc.].*

Revelle: On basic research in the navy.

Sharp: Right. It's quite impressive because there's this bound volume with colored charts and diagrams of how basic research is necessary and useful. Also striking, besides just the physical presentation, is the conclusion that NRAC came to based on the study: that there should be two times the amount of money put into research at ONR.

Revelle: How much?

Sharp: Two times.

Revelle: Into basic research?

Sharp: Right. Which was a pretty hefty increase. I wonder if you remember being part of the committee, having the report come to you and making your suggestions about what changes you thought should be made.

Revelle: Was Harvey Brooks on the committee at that time? I don't think he was. He was either before or after this time.

Sharp: No, not at this point.

Revelle: He's another of my heroes, Harvey Brooks. Remarkable man. Again, one of these guys without any hang-ups, like Harald Sverdrup and my son-in-law Gary Hufbauer.

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*The following pages are excerpted from the NRAC report for 1959, as pp. 54a-54m.

Table of Contents

Members of The Naval Research Advisory Committee

r. R. F. Bacher, Chairman, Physics Dept., California Institute of Technology

r. C. C. Furnas, Chancellor, University of Buffalo

r. T. K. Glennan, Administrator, National Aeronautics and Space Administration

[r. E. H. Heinemann, Vice President, Douglas Aircraft Company, Inc.

r. R. A. Kern, Temple University Hospital

r. A. B. Kinzel, Vice President, Union Carbide Corp.

r. J. W. McRae, Vice President, American Telephone and Telegraph Company

[r. G. Norton, President, Institute for Defense Analysis

r. E. R. Piore, Director of Research, International Business Machines Corp.

r. I. I. Rabi, Department of Physics, Columbia University

r. R. Revelle, Director, Scripps Institution of Oceanography

r. F. Seitz, Chairman, Physics Dept., University of Illinois

r. C. G. Suits (Chairman, NRAC) Vice President and Director of Research, General Electric Company

r. F. E. Terman, Provost, Stanford University

r. E. A. Walker, President, Pennsylvania State University

DEPARTMENT OF THE NAVY
NAVAL RESEARCH ADVISORY COMMITTEE
WASHINGTON 25, D. C.

IN REPLY REFER TO:

ONR:103:jg
Ser N-152
24 Apr 1959

My dear Mr. Gates:

The report transmitted herewith for your consideration marks a beginning of research on research in the Navy. We are fully aware that without development, production and operational training, there can be no effective fighting force. However, the current thinking with respect to research, and especially basic research as a Naval requirement, is much less clear and the relationships in this area have not been fully developed. This report begins to lay the basis for a clear expression of the requirement, bearing in mind that the success of the Navy in accomplishing its mission in competition with other world powers depends largely on a continuous flow of new and better weapons and techniques. This in turn, requires the continuous development of new technologies which have their roots in the results of basic research.

The report strongly supports the Navy's need for basic research. Only by active participation in a program for which it assumes a direct responsibility can the Navy insure a rapid flow of the products of new science from the laboratories of the Nation into the uses of the Service.

The Naval Research Advisory Committee believes that this report makes an appreciable contribution to a development of the understanding of the relationship of basic research to the missions of the Navy. However, we are acutely aware of many unsolved problems and we hope this report will provide the basis for further study.

The Committee urges that the Navy implement the recommendations of the Naval Research Advisory Committee, herewith presented.

Very truly yours,

C. G. Suits

C. G. SUITS, Chairman
Naval Research Advisory Committee

Honorable Thomas S. Gates, Jr.
Secretary of the Navy
Washington 25, D. C.

Conclusions and Recommendations

of the
Naval Research Advisory Committee
concerning the report "Basic Research in the Navy"

This report sets forth the nature of basic research and its relationship to military end items. It establishes, by historical example and otherwise, the Navy's need for an increasing flow of basic research.

Basic research has played a tremendous role in the past, transfiguring the Navy by findings in such fields as radar, inertial guidance, missile propulsion, and atomic propulsion, and the accelerated pace of scientific progress in the last decade emphasizes its importance. The report points out that while the Navy can support only a small part of the total research of the world or the country, it must do enough in each area of interest to provide effective coupling and judgment for its own needs. It must also do that basic research essential to provide for its own direct needs in those areas of peculiar interest to the Navy which are not being adequately covered elsewhere.

In conducting basic research for either of these reasons, the investigators within the Navy Department must be constantly alert to recognize the impact of any findings on the needs of the Navy Department. These may not necessarily be related to the immediate objective of a given project but may well bear on the potential over-all position of the Navy. This is truly important. Time and time again, as brought out in the report, unexpected or even incidental findings have resulted in a major improvement in weaponry, communications, and the like. Said another way, only those engaged in basic research in a given area who, at the same time, have Navy interests at heart, are in a position to appreciate scientific findings of others and the significance of such findings to the Navy.

The report sets forth the judgment of those engaged in the direction and application of basic research in industry with respect to the level of basic research appropriate to the total Navy effort. Essentially this judgment is to the effect that the basic research effort in the Navy be approximately doubled in order to restore the former relationship of basic research to the total research and development effort. This would also bring the proportionate Navy basic research effort closer to that now current in those progressive industries operating in the areas of science and engineering.

The Committee concurs with the findings of the Arthur D. Little Study Group. It believes that this study lays the basis for detailed consideration of the basic research program required to fulfill the Navy's needs. However, it should be emphasized that this laying of the groundwork is but the first step in the process of rehabilitating the Navy's basic research program. In order to implement such rehabilitation a second step should be pursued forthwith.

The next step comprises the detailing of the program proper. Study of such detailing can be done well only by those who have a close working relationship in the Navy and with the scientific community, namely, the Office of Naval Research. It is recommended that this group prepare detailed programs in each of the fields of science related to the missions of the Navy as set forth on Page 49 of the report, plus such others as may be pertinent. In considering these fields it is obvious that certain items are the prime responsibility of the Navy; for example, oceanography. It is obvious that others are a major responsibility of the Navy; for example, meteorology, navigational phases of astronomy and astrophysics, marine phases of biology and biological sciences, the claustrophobic phase of psychology, and the like. Other areas are so broad that they are found wherever basic research is being done; for example, physics, material sciences, mechanics, electronics, mathematics, and the like. In these areas an effort sufficiently large to provide good coupling is needed. By setting forth specific programs pertinent and suitable to each of the areas in question and bearing in mind the foregoing, an over-all program can be prepared.

The approach just outlined is by no means novel, having been attempted more than once in the past. These attempts have not borne fruit because they consistently showed a requirement for total funds many times greater than contemplated at the time, and the principle of selection by areas was abandoned in favor of priority projects. To prevent this, after such a total program has been prepared by assembling detailed projects, a third step is in order. There must be another critical review still following the area distribution to bring the total cost within the augmented budget. If the budget augmentation is sufficient, i.e., double that of fiscal 1959, as herein recommended, the over-all program should approach the fulfillment of the needs herein set forth. Experience with the augmented program will show the success of the proposed approach and additional steps may be taken in future years, as necessary.

It is the Committee's recommendation that ONR proceed immediately with the studies outlined above and that a program corresponding to a doubled budget be prepared by the Office of Naval Research and be endorsed by the Secretary of the Navy.

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Summary and Findings

During World War II it became strikingly evident that scientific research is essential to the national security. The Scientific Research Board Report to the President in 1947 forcefully emphasized this point, stating:

> "The security of the United States depends today, as never before, upon the rapid extension of scientific knowledge. So important, in fact, has this extension become to our country that it may reasonably be said to be a major factor in national survival."

The Department of the Navy, fully cognizant of this trend, led the Federal Government in implementing changes in its organization and budget to reflect the requirements for expansion in scientific research. With the establishment in 1946 of the Office of Naval Research "to plan, foster, and encourage scientific research in recognition of its paramount importance as related to the maintanence of future naval power, and the preservation of national security," the Navy increased sharply the percent of its budget devoted to research.

Research in science and engineering is generally considered to consist of a continuous spectrum of activity having as its three major segments basic research, applied research, and development. Only by having a properly balanced and administered program at any given time in all segments can the rapid evolution of new weapons systems and techniques of warfare be reasonably assured. The most perplexing problem in achieving a properly balanced research program for the Navy is the establishment of an appropriate level of participation in basic research. There are two major reasons for this. First, there has been some lack of definitive understanding as to the nature of basic research and its role in the furtherance of the missions of the Navy. Second, substantial Government sponsorship of basic research is so recent a factor that policies are still in the formative stage. Therefore, at the recommendation of the Naval Research Advisory Committee, this study was undertaken to attempt to determine a basis for decision by the Department of the Navy in establishing proper levels of participation in basic research. Despite the obvious difficulty of this assignment, the potential usefulness of any quantitative findings in promoting future Navy effectiveness was thought to make the undertaking worthwhile.

For purposes of this study, the official Department of Defense definition of basic research was utilized. This definition, found to have broad acceptance by industry, university, and Government personnel, is as follows:

> "Basic research is that type of research which is directed toward increase of knowledge in science. It is research where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study.". (Ref. DOD 3210. 1 Nov. 12, 1957)

The key question at the outset of this project was whether a necessarily broad definition of this type was interpreted in a sufficiently rigorous manner to permit the nation-wide collection of comparable and valid data on basic research policies, budgets, and expenditures from Government, industry, and university sources. This is a problem which has bothered the Congress and the Bureau of the Budget in the past. Considerable effort was expended in studying this matter, and it is gratifying to be able to report real progress toward clarification of this issue.

the output of all meaningful basic research is almost invariably represented by publication in the form of papers appearing in recognized scientific journals

The output of all meaningful basic research is almost invariably represented by publication in the form of papers appearing in recognized scientific journals. The infrequent cases of secrecy in basic research cause a delay in, but do not prevent, publication. This being true, if there is widespread consistency in the interpretation of what constitutes basic research, a correlation should exist between the number of people claimed to be performing basic research in Government, industry, and university laboratories, and the number of papers originating from each of these sources appearing in selected scientific journals. In the investigation of this assumption, data collected by the National Science Foundation were used to calculate the number of basic research workers claimed by Government, industry, and university laboratories, and the number of papers originating from each source was obtained by inspection of a selected sample of thirteen recognized scientific journals. A sufficiently strong correlation was obtained, between numbers of research workers and numbers of papers, to permit the conclusion that policy with respect to basic research definition and freedom to publish, is remarkably consistent nation-wide. On the basis of this important knowledge, it then became possible to collect with more confidence data from a number of sources for comparison of basic research policies, budgets, and expenditures. Furthermore, it was possible to make simple, rough checks as to reasonable validity of the data.

In the course of this assignment to assist the Navy Department in basic research policy formulation, three lines of attack were pursued:

a. Orientation

It became evident at the outset of the study that a broader understanding of basic research is a necessary step in evolving improved basic research policies. Therefore, much effort was devoted to the development of a concise and novel presentation, as given in this report, of the dependence of the Navy on technology, the nature of basic research, and the relation of basic research to the missions of the Navy.

b. Judgment and Analysis

People skilled in the art of administration of research were sought out in order that their experience and judgment as it might apply to the assignment could be used to advantage. This involved discussions with leaders in industry, in Government, and in universities.

New and extensive data on research and research personnel were collected and analyzed.

c. Quantification

A unique approach was made toward the synthesis of a mathematical model of the relationships between segments of the research process, in an attempt to develop a method for predicting proper levels of effort in each segment of the process.

Principal Findings

participation by the Navy in basic research in many fields is essential to the furtherance of its missions

Careful study has shown that participation by the Navy in basic research in many fields of science is essential to the furtherance of its missions. In this period of accelerating technological advance and dynamic international competition, national survival is largely dependent upon speed of acquisition and application of new knowledge. The vital role of basic research in accelerating progress is clearly demonstrated by a study of actual case histories, presented herein in the form of schematic models, and by an analysis of the research practices of leading corporations similarly faced with the problem of survival in this age of technology.

A dominant requirement of the Navy today is that of leadership in the development of new weapons systems and techniques of warfare in this period when rapid technological advance and international competition combine to render obsolete many weapons even before the production stage can be initiated. Such leadership can be maintained *with participation in basic research, scientists remain constantly abreast of the expanding frontiers of world science*

only by means of an aggressive, wisely conceived, properly balanced, and skillfully managed research and development program involving many fields of science. Essential to the success of such a program is effective participation in basic research, the life blood of the entire system of technological innovation. The basic research segment of the program is responsible not only for developing new knowledge, but also for communicating with the frontiers of science on a world-wide basis, and transmitting such knowledge or understanding to closely coupled applied research and development segments in order to maximize its utility. This vital function can be performed efficiently only by scientists actually participating in basic research and familiar with the needs of the Navy. With participation in basic research, scientists remain constantly abreast of the expanding frontiers of world science, and maintain the conceptual ability necessary to assist in evolving rapidly those applications vital to enhanced Navy effectiveness. Without participation, communication slows, the life blood is drained, and the over-all research program quickly deteriorates.

During the decade 1947 to 1957 leading corporations in high technological obsolescence rate industries have been far more aggressive in their participation in basic research than has the Navy.

While the basic research requirements of the Navy cannot be exactly compared with those of any other organization, the best available possibilities for comparison are found in technically based industries. Industry represents the second largest source of basic research funds. Many corporations have endeavored to evolve sound policies with respect to the extent of their participation in basic research in order to achieve that balance in their research and development programs most likely to guarantee corporate growth in the face of stiff competition in a period of accelerating technological advance. Information on research and development expenditures was, therefore, gathered from a number of leading technically based corporations. Excluded from the figures were Government contracts and those engineering activities not normally included in the research and development budget.

In 1947 the Navy allocated 10 percent of its research and development expenditures to basic research. This compared very favorably with the policies of many leading industrial corporations. However, a distinct divergence of policy occurred over the next ten years. Data from two of the most successful corporations in each of five technically based industries (chemical, petroleum, communications-electronic, pharmaceuticals, materials) showed these ten corporations in 1957 devoted 10-20 percent of their own research and development expenditures to basic research. The average allocation of 16 percent is in marked contrast to the Navy which currently allocates only 6-8 percent of its research and development budget to basic research.

Dollar figures add further confirmation. Information supplied by fourteen top corporations in these same industries showed that between 1947 and 1957 they tripled their total research and development expenditures and increased the basic research portion by a factor of 4.5. In the same period the Navy doubled research and development expenditures but increased the basic research portion by a factor of only 1.5. This increase in basic research expenditures was essentially offset by reason of the fact that the total cost per scientist increased approximately 50 percent during this same period.

A group of industrial directors of research familiar with the problems of the Navy were unanimous in their judgment that the Navy should increase the percentage of its research and development budget devoted to basic research.

To take advantage of the experience gained by industry in establishing corporate research and development budgets, we sought the opinions of leading industrial directors of research on Navy participation in basic research. The thirty-three men approached for opinions administer almost one half of industry's basic research expenditures and are responsible for allocation of funds within their respective corporate research and development budgets. Sixteen of the thirty-three believed they had sufficient knowledge of the Navy and its missions to be willing to express a judgment. Given the task of constructing a research and development budget for the Navy considering its missions, size, technical complexity, strength of Soviet competition, and the severe consequences which would be faced for being second best in national defense at this stage in history, it was the judgment of the majority that the resulting budget should show basic research in the range of 15-20 percent of the total research and development effort. An aggressive approach to participation in basic research is demanded, since nowhere is success more important today than in military technological advance.

the greater the technological strength of the competition the greater should be the emphasis on basic research

In general, the greater the technological strength of the competition and the less immediate the probability of conflict, the greater should be the emphasis on basic research. Thus, under such conditions, the nature of weapons which might be used against this nation, and the countermeasures which might be employed, become less predictable, forcing a broadening of the basic research effort. Conversely, basic research plans can be more specifically drawn if conflict appears imminent.

Although there is legitimate widespread concern about a national shortage of scientific manpower, the Navy should find this no immediate obstacle should it decide to increase its basic research effort.

With any substantial increase in Navy participation in basic research, the problem of availability of competent scientific manpower will arise. At this moment it appears from a study of meritorious proposals turned down, or discouraged prior to submission, that sufficient manpower exists to expand the Department of Defense basic research effort in outside contracts by approximately 70 percent (omitting certain large capital equipment proposals). In addition, a rough approximation indicates an increase of about 10 percent is currently possible in the Navy in-house basic research effort. However, a serious manpower shortage may well develop in the near future as national research and development activities are currently expanding at the rate of 10 percent per year, whereas the number of scientists and engineers is increasing at the rate of 5 percent per year. At present approximately 25 percent of scientists and engineers are engaged in research and development activities, but only about 2 percent are engaged in basic research.

An expansion of the Navy basic research effort will place a premium on improved program planning and communications. The former might be achieved through greater use of scientists in a consulting capacity. The latter will require continuing study and emphasis since more than one half of the work performed will be outside of Navy laboratories and widely distributed geographically.

Because of the length of time required to evolve results, Federal budgeting for basic research presents special, and as yet not completely resolved, problems.

Budgeting for basic research is complicated by the necessity for planning on a long-term basis, while budgeting and operating on an annual basis. Planning basic research must take into account the time needed to form the research team, perform experiments and analyze and publish the results. The over-all time required for this process, as measured by the current average life of Office of Naval Research projects, is about 5 years.

Considerable progress in budgeting has been made through the availability of no-year money (available until expended) and advance financing of research projects. These tools are limited, however, by the amount of funds made available each year in the face of stiff competition offered by current fleet requirements particularly at times of expenditure curtailment or limitation. In order for the Navy to establish a more aggressive basic research program, methods must be found for budgeting and contracting on a basis which will tend to allow longer range planning and eliminate damaging annual variations. This is a problem of broad national interest, involving many agencies in addition to the Navy Department. The solution rests in large measure on bringing about a better understanding and appreciation of the role of basic research to provide the basis for coordinated budget planning by the Executive Branch and Congress.

It may be possible to develop a mathematical model of the relationship between segments of the research process that would aid in determining a proper level of Navy participation in basic research.

A program to develop a mathematical model of the relationship between the segments of the research process has shown enough promise to warrant consideration for further development. Results obtained by trying to fit a few actual case histories into the model as it now stands have been encouraging. However, more time is needed to substantiate the basic assumptions of the model, and the relation between what it predicts with respect to a proper level of basic research and what is observed in the real world.

Supplementary Observations

There exists within the Navy Department a general belief that the Office of Naval Research is the sole Navy office authorized to finance basic research. This misunderstanding stems largely from budget procedures, and has led to some confusion as to the extent of the Navy basic research effort. In addition, it has handicapped the administration of Navy laboratories in initiating basic research programs. Corrective steps and education are required.

Among Department of Defense laboratories, basic research contributions by the Navy laboratories are outstanding. This is especially true of the Naval Research Laboratory, which writes approximately 30 percent of all scientific papers originating in Department of Defense laboratories. The Naval Ordnance Laboratory, Naval Ordnance Test Station, Naval Electronics Laboratory, and others also make significant contributions. Knowledge generated in these basic research programs has contributed significantly to Navy effectiveness.

among Department of Defense laboratories, basic research contributions by the Navy laboratories are outstanding

This study is, so far as could be determined, the first of its type for the Government. In performing research on research, investigators are immediately confronted with the handicap of woefully inadequate data. With total research and development expenditures now amounting to approximately 6 percent of the Federal budget, more study of research is indicated. This is the path to improved national policies from which will emerge more effective utilization of our scientific resources. Some of the techniques developed or employed during the course of this study appear worthy of refinement and application by the Navy to such areas as:

a. *Research planning*

It should be possible to plan more effectively expenditures in basic research through detailed analysis of such factors as the so-called barrier problems within fields of interest to the Navy, and the relative worldwide research activity within such fields through literature investigations, coupled with study and evaluation of scientific manpower. Machine techniques and mathematical models may become useful in this regard.

b. Intelligence

Analysis of world-wide basic research activities by advanced techniques should offer excellent opportunities for progress in the field of intelligence.

Sharp: What was your participation on NRAC?

Revelle: I went to all the meetings. I think they met about once every three or four months. Quite often. We went on field trips to visit navy labs and various kinds of navy installations that did R and D, like Point Hueneme, like the Naval Ordinance Test Center at wherever it was in Pennsylvania. Many of the labs weren't very good.

Sharp: By not very good you mean underdeveloped?

Revelle: I mean the people were pretty routine people. They did testing; they had few original ideas. They just did tests. On the other hand, some laboratories were very good; the Naval Research Laboratory was absolutely first-rate. The Navy Electronics Laboratory had a lot of good people.

Sharp: What sort of influence do you think you had on the committee overall?

Revelle: I don't think I had a great deal of influence. Not as much as Ed Heinemann, for example, or Guy Suits or Gus Kinzel.

Sharp: Is that because they were in industry?

Revelle: They were involved with development, and 90 percent of the navy's expenditures are in development. Just a small part in research. Billions of dollars in development and millions of dollars in research. The research people on this were Manny Piore and I.I. Rabi and Fred Seitz and me and Bob Bacher. It was a very good committee; this list you have shown me is a remarkable group of people.

But after a while I got kind of bored with it. President Nixon finally fired me in about 1968. I'd been on NRAC for God knows how many years, at least ten years, and I think about fifteen.

Sharp: Did he want to replace you with somebody else in particular?

Revelle: No, he just thought that they ought to have some rotation. I think he was right. But I think NRAC turned out to be less and less effective. One of the reasons for that was that it didn't have to be effective. We had people in the office of the Assistant Secretary of the Navy for Research and Development, particularly Bob Frusch, who were absolutely first-rate.

Sharp: This was somewhat duplication of effort.

Revelle: How are you going to advise Bob Frusch? He knows everything already. He knows what needs to be done and how it should be done. All you can say is, hooray. He's now, by the way, as you know, chief scientist for General Motors. He was administrator of NASA for a while, as was Keith Glennan.

Sharp: To get back to what this report says, there should be twice as much money spent on research on ONR. That sounds like that might be some of your handiwork, that you would--.

Revelle: The whole committee agreed on that. I couldn't possibly have put that across by myself. It's the industrialists that really had that say, people like Kinzel and Heinemann and Suits. The reason was that they spent, in their companies, as I say, for development an order of magnitude more money than ONR spent for research. They felt that development really depended upon their basic research. I don't remember [J.W.] McRae, who is one of the people on this list. I remember everybody else. [R.A.] Kern was a physician. We had a physician and later a psychologist on the committee as well as these more physical types. Eric Walker was a powerful influence and later became chairman. He's still very active, although he has retired as president of Penn State [University]. Everybody on this list is either retired or dead. Bob Bacher is still extant. Keith Glennan is still alive. So is Heinemann. Gus Kinzel has had a very bad stroke recently. Heinemann had a stroke. Rabi is still going strong; Piore is still okay, so is Fred Seitz. [F.E.] Terman is dead. The man that seems to be most active in this kind of thing is Eric Walker, still.

It got to be in the long run a set of--we'd get briefed by naval officers. There is just nothing duller than a naval briefing, except two naval briefings.

Sharp: They weren't asking for your advice then. They were coming to you and giving you the state of the operation.

Revelle: That's right, yes, pretty much. One of the things about this committee--they never did any work themselves. They would appoint their deputies to do the work. That was fine if you were head of the development part of a big company, like Gus Kinzel.

But I could do something too, because there were some of our guys who were very much involved with navy R and D, particularly Fred Spiess. So he was my deputy, and he did the work, what work there was to do. You might even talk to him sometime about that, because I think he later became a member of NRAC. He was director

Revelle: of the Marine Physical Laboratory, and later for a year was director of Scripps.

Sharp: That was during the interim, right after you.

Revelle: Interregnum between me and Bill [William A.] Nierenberg. But he had been acting director during my absence when I was science advisor to Stewart Udall, in 1961-63.

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Revelle: I remember one serious problem before NRAC was the concern that the navy had about its danger of disabling missiles, particularly ICBMs [intercontinental ballistic missiles], or any kind of space missile. It looked as if you could send a directed beam toward a missile and disable it. So its missiles had to be hardened against such a beam. That was for several years a great worry for the military people. I think it has been overcome since then. But for a while it looked as if you could send a laser beam or an X-ray beam or something like that against a missile.

Sharp: Was it a matter of some scientific testing to show that that was impossible, that you couldn't do that?

Revelle: You could do it, no question about it. So what they had to do was to harden the things, to put them in a casing that was pretty much impenetrable to radiation, as I remember it. That was all very highly classified. Even the NRAC didn't really get too much into it, though we all had a top secret clearance. But this was almost "eyes-only" stuff, pretty much, it was so dangerous.

I guess basically by the time President Nixon decided to get rid of me I was ready to be gotten rid of, because I was bored with it. I just didn't like military things any more. For me there had been a transition from the [Operation] Ivy days to 1968-1970. Part of it was a revulsion against the Vietnam war.

At first, I remember an NRAC meeting at which Harvey Brooks and I argued that if we were going to win the war in Vietnam we had to take it more seriously. For example, we had patrol boats on the Mekong River, which were just sitting ducks for the Viet Cong. The very least we could do was put some armor on them to make them effective. In the early part of the Vietnam war I was a hawk, basically saying that we ought to really go at it. After a while I decided the whole thing was a mistake.

Sharp: What turned you around?

Revelle: I guess as much as anything what turned me around was the Tet offensive, in which there was so much destruction. It showed that North Vietnam had tremendous power. We really were fighting the wrong war in the wrong place at the wrong time, with the wrong people and the wrong equipment.

Sharp: We misunderstood what was going on and what we could do about it?

Revelle: That's right. And I guess also I felt the war was so destructive of people, not only our people but the Vietnamese people. I'd been there, you see, on the Scripps Naga Expedition in 1959 to '62, and they were very nice people, little, hardworking people. It was horrifying to think of the cruelty and the destruction and the hatred that was involved. I just didn't like it at all.

I remember I made a speech at a Revelle College commencement at UCSD about that time. That was about 1972, I think. I said I had changed my mind completely about it, that we ought to get out of there. I was ashamed of myself for having been in favor of it.

Sharp: It's one thing to change your mind privately, but it's another thing to change your mind publicly and lay it out in front of a big audience, your change of mind, especially about something that people came down on one side or the other pretty strongly. How did that feel?

Revelle: I felt I had to do it. We had some very bad experiences at Harvard at about that time. I told you that I spent a whole night one night trying to make sure my building didn't get burned down.

Sharp: In demonstrations, you mean?

Revelle: Yes. Every window in Harvard Square was broken that night. My friends there, some of the ones I respected, were very much against the war. That was one of the reasons I was glad to be out of NRAC. I just didn't like it any more, military things. And I don't like it today. I think we've become a kind of a garrison state. [President] Eisenhower, in 1960, said just that, that we must keep the military-industrial complex from dominating the country. And he was right; it is dominating the country. I worry about it a lot, and I'm ashamed of my country for being this way.

Sharp: Scripps has had a role in R and D, or the R part of R and D, for the military complex in the United States since 1951 at least. That has continued. That's an important part of some of the work that Scripps does.

Revelle: Well, I don't think that anything that Scripps does, as Scripps, has much military application. Some of the things at the Marine Physical Laboratory do. That's one of the reasons it's down at Point Loma. I think some of their work is classified; I'm not sure though. Fred [Spiess] could tell you better than I can. But you know that there's something called Jason, which is an organization basically of physicists that meets here in La Jolla for about six weeks each summer. Walter Munk is a member of it, Bill Nierenberg, Herb York, Freeman Dyson, Jack Ruina, quite a few very good people are members of it. Many of them are very dovish, particularly Ruina and Dyson. They do a lot of classified thinking for the armed services. They were supported, at least until recently, by the Mitre Corporation, which is a spin-off from MIT. It means MIT Research. Jason used to be supported by the Institute for Defense Analysis. That was what Garrison Norton was president of. He was, by the way, one of the assistant secretaries of the Navy for Research and Development.

I guess from my wartime days, when I never worked so hard in my life, except that I've worked harder since, to the present time, I've gone through a transformation about military things. World War II--this is before your time--this was kind of a holy cause on the part of many people in the United States. We thought that Nazism was an unmitigated evil that had to be gotten rid of. I don't think most people felt the same way about the Japanese. We just felt the Japanese were a bloody nuisance. But Germany was a different story altogether. Although it wasn't until later that we learned about the Holocaust, we were set deeply and fervently against the Nazis. An awful lot of people, certainly including all the people I knew, were dedicated to winning that war. Did everything they could to do so.

I remember at the time of Crossroads, we had a monopoly on the atomic bomb then. Some of us at Bikini suggested that maybe we ought to send some kind of an ultimatum to the Russians, to the Soviets, saying we'll use our weapons unless you promise not to develop an atomic bomb. But the Truman administration had the good sense not to say anything like that and they said we mustn't even talk about it. That was at Crossroads.

Sharp: Such early days those were.

Revelle: That was in 1946.

Sharp: Even throughout the fifties, through Wigwam for example, that still seems so early and elementary compared to the changes in the testing that have gone on in the past few years.

Revelle: Yes, it was a different order of magnitude, of course, a completely different situation, just by numbers; that's the difference. It's completely insane now, I think. The word MAD really means mad, although it's supposed to mean "mutually assured destruction."

You know the surprising thing is there are an awful lot of people like me. Herb York, for example, who was Undersecretary of Defense for Research and Development, is a real peacenik.

Sharp: And is doing quite a bit now in terms of education.

Revelle: Yes. Jerry Wiesner. He gave a speech on the National Academy of Sciences in which he said there are no experts. The American people should decide. A very moving speech. And Murph Goldberger at Cal Tech, [McGeorge] Bundy, now at New York University, Hans Bethe at Cornell. They're all peaceniks. Almost everybody who knows about it is. It's this bunch of ignoramuses in the present administration that think nuclear superiority is a great idea.

Sharp: And it looks like we're going to have them for a while longer.

Revelle: Four more years, yes.

Sharp: It's very depressing.

Revelle: It's terrible.

Sharp: I think I've probably come to the end of my questions for today, unless you have some other things you'd like to talk about.

Revelle: I don't think I really put across the idea that the early days of ONR were a very exciting time for me and one that had a lot to do with my intellectual and moral development. We all felt we were really pioneering. It was an important thing to be doing, to develop the governmental support for basic research in the United States on a liberal and farsighted basis. This business of supporting what the scientists wanted to do, that was the main thing, as opposed to supporting things that the bureaucrats or the government wanted to do, even though the government, of course, is just people.

VI CARL ECKART AS DIRECTOR OF SIO AND PROFESSOR

Sharp: There's that letter that we didn't really talk about that Carl Eckart wrote to you in 1948.* It says things are just falling apart.

Revelle: "You've got to come out quick."

Sharp: I wondered if you remembered what you thought about when you got this letter, how you felt about it.

Revelle: I don't remember. I didn't remember that letter until you showed it to me today. It was a cry for help. He was not really a very good director. A wonderful man, but a poor director. The reason he was a poor director was that he took everything so seriously; no matter how small or no matter how big, he paid attention to it.

Sharp: So he wasn't delegating some matters?

Revelle: He did delegate, but a lot of things he took very seriously and very personally. He took it too hard. He finally just quit. It was very hard on him. He had a personal problem for a long time too; his wife was an alcoholic. Her name was Edie. He bore that with Christian fortitude, but it was a terrible strain on him. I've forgotten what happened to her.

Carl was a perfectionist, and everything had to be done right. You can't really do that when you're director. You've got to depend on people, and if they don't do very well, maybe that's the best they can do. You can't very well fire people in an academic institution; you've just got to live with them. But that letter was sort of typical of his approach--the way I read that letter is that most of the things he was complaining about were the things that you couldn't do anything about and shouldn't worry about.

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*See the following pages for this letter.

CARL ECKART
BOX 992
LA JOLLA, CALIFORNIA

May 8 1948

Dear Roger:

I have been going over things in my own mind, and reached a conclusion that I don't like. That is, that unless the pace here slows down, I'll not be able to keep up with it. The job has kept me going seven days a week, and many evenings.

Consequently, it does not seem that I can justifiably consider the matter that we discussed briefly some time ago — your continuing as consultant for ONR. The present expansion of activities here is such that it will take both of us just about full time to run it.

A number of things are going wrong here at the moment, because the "Institution is not accustomed to running itself. Everything appears to need some personal contact each day. otherwise it goes wrong. I consider my job to be primarily that of the scientific policy and program, but get no time for it at all.

The thing that made me realize how much being overloaded with desk-work keeps me out of contact with things was an examination of Champion's shop-time for the past month. He devoted all of the time of all of his people for two weeks to writing requisitions that are probably useless now. I should have caught that before it was over.

The matter of the Museum building, etc., should have been studied carefully, but I have not been able to do so. Monday, McElvey & Hope will be here and want descisions.

The renewal of NOs-2074 lagged because I did not keep at it. It is now in the stage it should have been several weeks ago: a letter will be mailed to the Bureau on Monday. It will ask 10% overhead on MPL and 25% on S10. This is based on a document called "Explanation of Principles for the Determination of Costs under Government Research and Development Contracts with Educational Institutions," issued jointly by the War and Navy Departments in August 1947. This does not, in my mind, give any consideration to the existence of the NEL Problems D18.1 & 18.2 and the assistance that Scripps has, and will, receive from NEL. But my instructions from Taylor and Underhill give me no alternative but to request the overhead on that basis.

Very little has been done about the Horizon. They have removed some of the gear to the new buildings, but there seems to be little ~~activity~~. Clem got your outline, and we went over it yesterday. I told him to work with John on getting the ship on the ways for sand-blasting and painting; to hire a steno who can work for him and probably also for Leipper. He wants to have a man from Martinolich come down here for a few days, rather than sending a delegation up there. He feels that they will stand around much of the time if they go up; this sounds reasonable, so I told him to proceed on that basis.

Dana did a good job of selling his jobs, so that we shall have to move quickly, or find ourselves with the leavings. Inman will, I think probably go. He is the backbone of the sand-transport work, as you know. It may be that we can get Reid in return, but Pritchard is out of the question.

I saw the preliminary budget on Friday at UCLA. We got everything we need, except that Fran's salary must continue to come from the contract. He feels badly about the prospect of losing Inman. I ought to think that one through, but haven't found time, and probably won't until the middle of next week, from the looks of my calendar.

I sent you a copy of the letter about the ships purchase. On Monday, I'll phone Sproul and follow through on that. I don't see how it will be possible to complete the deal, however, even if he can be brought to authorise it. There just isn't time enough.

I haven't had a chance to do more than give your memo on the Marine Life program a single reading. On checking with John and Marston, I find them very vague as to the people that should be offered jobs. In a way this is not urgent until the ship is ready to operate, but we must make committments now, and get the paper work done. In addition, there is a lot to be done at Point Loma. I haven't ~~talked~~ with Leipper, but ought to. Is Burt going to take over the BT processing? He passed the 2d part of his exam, but not brilliantly.

The requisitions are stalled, of course, since no letters have come from the Bureau. Hammond is getting ready to take the E.W. Scripps out next week. Martin and Carl

Hubbs will presumably arrange something to benefit the Marine Life work.

In general, I think our most urgent task is to build an organisation, whose members know what their authority and responsibility is. We have one who uses his authority irresponsibly, but most of our people are pretty vague and aimless. Such an organisation can't be built on paper. It needs lots of personal contact and discussion. You and I must be in accord on the jobs each group is to do, and then we must talk with them frequently, clearing up misunderstandings and making decisions that the others cannot or are hesitant to make. We must also get their slant on things, for they usually will have information that we lack. This means that both of us must be here most of the time. I hope it doesn't mean that you can't go to Oslo, but we will have to make progress quickly, otherwise things will go to peices during that period.

This is a long and gloomy letter, but I think a realistic one. The Institution has at least three major functional components: Teaching, Research and Facilities (including both ships and other facilities). Thus far, I think, the instruction is moderately ok, except for the physical oceanography problem for the Fall. But the other two components certainly are not functioning yet, and it is up to us to get them going.

Sincerely

Carl

Revelle: In any case, we got along very well. We never had any quarrels. We did quarrel later, after I was director and he'd returned to being a professor, because he felt that I had not gotten him any graduate students, and that somehow I should have done something about that.

Sharp: Was that something that was in your control?

Revelle: I don't think it was. He was a great theoretical physicist, and great theoretical physics maybe doesn't belong in oceanography. The ocean is a very messy object, infinitely complicated, very difficult to study. Studying it on the basis of the absolutely most fundamental hydrodynamics is not very productive.

It takes the Sverdrups and the Rossbys to find things that other people can build on. Carl Eckart took a very dim view of Rossby [Carl Gustaf Rossby] because Rossby never really did things in a fundamental way. He was very intuitive and was quite satisfied with rather superficial solutions--superficial in the sense that they weren't based on fundamental hydrodynamics. They were mathematically difficult but not fundamental.

Still, the Rossby waves in the air, the jet stream, many things which are now the basis of modern meteorology and climatology were his developments. But Carl just thought they were not necessarily so, is what it amounted to, because Rossby hadn't proved them rigorously. Rossby just proved them intuitively, what the mathematicians call heuristically.

I felt and still feel a great debt to Carl Eckart. He was always my protector, defender, and champion. He was in love with Ellen [Revelle]. She was in love with him too, pretty much. But a very honorable man and wonderful man, but not very good at running things.

Sharp: There is certainly a desperate tone--more than tone, he just lays it out that things are a mess.

Revelle: He was vice chancellor here for a while; he wasn't very good at that job either. I'm not quite sure why not. I wasn't here then; I was at Harvard. Of course he had a series of heart attacks. Eventually he wasn't getting enough blood to his brain, toward the end of his life. He wrote a book which we're now publishing--Leonard Liebermann has worked on it a lot--called Mathematics: Our Modern Idol, or something like that. To some extent a lot of it isn't very coherent because of his illness.

Sharp: That was toward the end?

Revelle: Toward the end of his life, yes. He had some very interesting characteristics. He was a marvelous lecturer; like all good professors he was a ham. I don't mean by that that he was very funny, but he would make startling or sort of shocking statements every now and then in his lectures to wake people up and get their attention.

Sharp: Good idea.

Revelle: You'd always think when he was lecturing that you understood every word he said; it was so clear and so straightforward. Then afterwards you'd wonder what the hell you had heard. It was a very curious experience.

He wrote in a little notebook, never crossing anything out, every word a gem in the right place at the right time. Beautiful, logical, crystal clear thinking.

They say about Mozart that he composed in his head, so he worked very fast once he started writing things down on paper; he had it all written out in his head.

Maybe Carl was like that too. In any case what he wrote down in very precise, neat handwriting was beautiful. Never had to correct it.

I have to do things over and over again. I get new ideas, and a lot of the ideas turn out to be bad and so on. Carl was just about as different from me as two people could possibly be. But a loving, warm, wholly admirable human being, I thought, and a sad human being, because of his marriage and because he never had any children. He loved our children.

Sharp: Did he spend a lot of time--?

Revelle: Yes, he spent a lot of time with them. Bill [Revelle] loved him, particularly Bill. And that was reciprocated. We were his family for a long time. You ought to talk to Ellen about it sometime. After his first wife died he married Johnny Von Neuman's widow, Klary Von Neuman Eckart. They lived here in La Jolla.

The Von Neumans spent two Christmases with us here in this house before Johnny got sick. He took a long time dying, in Washington, of cancer. Hard on Clary. After she had been married to Carl for several years she drowned in the surf down here just

Revelle: west of where they lived. Many people thought she had committed suicide. I've never been convinced of that. She was a Hungarian, with all the sadness and craziness that Hungarians have. They say about Hungary that they have such a tragic history the only thing you can do is either weep about Hungary or laugh about Hungary. She sort of alternated between those two extremes.

Anyhow, that was the end of that marriage, and he never married again. He lived by himself for many years. I think that he and Klary were reasonably happy together. He was her third or fourth husband, I think. Her mother's last name was Dan. She was a much more comfortable person than Klary was, much more fat on her bones. Klary was always very thin.

VII RECAP ON ONR: "REALLY PIONEERING"##

Revelle: Let me tell you about one of the interesting experiences we had at ONR, which shows you in some way how we operated. Mina Rees and I decided that we should support astronomy, which was really neither geophysics nor mathematics but pretty closely related to both. So we each allocated $25,000 out of our budget to support astronomy.

I went around the country interviewing leading astronomers as to how we should spend this money. This was the first time that the federal government had ever supported astronomy.

They all told me the same thing. By leading astronomers I mean Ike Bowen at Cal Tech, Lyman Spitzer at Princeton, Harlow Shapley and Bart Bok at Harvard, Fred Whipple of the Smithsonian Astrophysical Observatory, and a man from Michigan, I believe his name was McMath. What they said we should do with the money was to set up fellowships for young astronomers to go and work at the big observatories. So we did set up twenty fellowships, $2,500 apiece, which was a lot in those days, for young astronomers to work at Mt. Wilson, Palomar, McDonald in Texas and Yerkes in Wisconsin, and Lick in California, which they did.

That turned out to be a wholly successful thing to do. A good many scientific papers resulted from it, and it was the precursor of the NSF [National Science Foundation] support of astronomy, which burgeoned twenty years later and became a very big enterprise. We were the very first bureaucrats to ever put any money into astronomy.

Sharp: How did you think of that idea? Why did you think that you wanted to do that?

Revelle: Because it was an important science that no provisions had been made for in ONR. It was part of our general notion that we ought to support basic research. We didn't think anything practical would

Revelle: ever come of it. I don't think anything ever did, but that was not the point. Of course astronomy is now one of the major things that the government does support, basically because of its great interest for the human mind and spirit.

We've all remained friends, the ones who are still alive: Mina [Rees] and Manny Piore and I all remain very good friends ever since those days. I never see Randall Robertson; I'm not sure he is still alive. Fred Seitz, of course, I was associated with him for many years afterward Some of the people I haven't seen, like B.W. Smith; I don't know what happened to him.

One of the things I had to do before I left ONR--I remember that's one of the reasons I didn't come out to Scripps very soon--was to find a successor for what I thought was an important job. I finally persuaded John Atkins of MIT to be my successor. Later he became chief scientist of ONR.

Sharp: Did he come in with pretty much the same ideas that you had?

Revelle: Oh sure, everybody did.

And then the geophysics branch--there were some wonderful people: Gordon Lill, Art Maxwell, Johnny Knauss, Earl Droessler, who held on there for many years. But many of them eventually went to NSF. I don't quite know why they did that except I guess, as you said yourself, ONR became quite restricted by the necessity to do navy applications work. NSF was a more lively place, they had more money, for one thing.

These guys were all very talented science administrators. This is sort of a special breed of people who work on the interface between the government and the scientists.

VIII WORLD WAR II AND THE CLIMATE OF COOPERATION

[Date of Interview: November 4, 1984]##

The Influence of Harald Sverdrup, George Deacon and Others

Sharp: In the post-World War II period there was quite a bit of writing done by scientists about international cooperation, some of it in fear of the secrecy issue, that nations would be so secretive in the development of their own science that cooperation would be eliminated, that each nation would just be too separate. Also, some of the scientists were reeling from the use of the atomic bomb; they were given a feeling of what had science come to that that was what we ended up with in terms of scientific achievement. So there were some scientists pushing for more cooperation, the idea that if we cooperate sufficiently with other nations that some way that could never happen again. We would come to know each other so well and depend on each other so much, that the spirit of cooperation would dominate.

So there were all these influences in the post-war period, when you read about it anyway.* That's where I thought we might start--talking about some of those attitudes and feelings that scientists had.

The reason I thought we'd begin with Sverdrup is because, of course, he was a European working in the United States. I'm wondering what sort of international influence Sverdrup might have meant for Scripps, and if international cooperation at Scripps was somewhat more taken for granted because you had people like Sverdrup already and were working with other Europeans also.

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*See for example, "The Place of Science in the Programs of UNESCO," Arthur H. Compton, Proceedings of the American Philosophical Society, Vol. 91, No. 4, October, 1947, pp. 303-306.

Revelle: Let me just finish this article.*

[tape interruption]

Revelle: It is true from that article and from all we know about him that Harald was a convinced internationalist. And for a variety of reasons--one was that he came from a very small country, Norway, which couldn't be much by itself; it had to be part of an international structure. The other reason was that the oceans are an integral object; you can't deal with one part of the ocean without dealing with other parts.

One way to put this is that in every teaspoonful of seawater you dip out of the ocean there are molecules that have been all over the world during the past few hundred years.

In fact it's generally true that international cooperation is most important and most essential in the field sciences, the sciences that deal with geography, geophysics in most of its aspects and ecology, of course, in the biological realm. International cooperation has long been essential in astronomy because different parts of the sky can be seen only from different parts of the earth. They can't all be seen from the same place.

The need for international cooperation is much less obvious in chemistry and physics and reductionist biology, that is its biology of single organisms or the organs and the cells and the molecules of those organisms. There's always a tension between the different kinds of science as to the value and particularly the amount of money that should be spent on international cooperation. We tend to be misled a little bit by such organizations as CERN, the European Center for Nuclear Research, which is an international organization with a large budget supported by many countries. It's centered around very big machines, the CERN particle accelerators. There cooperation is essential simply because so much money is involved. It's easier sharing the costs. To some extent that's true in oceanography too; no one country has been able to allocate the resources to oceanography which would enable it to make comprehensive observations in all oceans.

An outstanding example of cooperation in the field sciences is the Antarctic Treaty, which provides that the continent of Antarctica shall be reserved for scientific research, and no national claims to territory shall come into force during the lifetime of the treaty. Many countries have cooperated in Antarctic research--it's not

*"New International Aspects of Oceanography," Harald Ulrik Sverdrup, Proceedings of the American Philosophical Society, Vol. 91, No. 1, February 1947, pp. 75-78

Revelle: entirely altruistic--they may hope that eventually they'll be able to establish territorial claims to part of Antarctica, or at least some kind of claim down there.

I think that certainly having Sverdrup here made it seem natural and proper to develop international cooperation, particularly for the Scripps staff to be involved with international cooperation in oceanography. That, after the war, developed in various directions. I don't think, however, that he had much more influence on developing international cooperation in oceanography than native-born Americans, particularly on the East Coast, men like Columbus Iselin, Henry Bigelow, and Alfred Redfield, especially Iselin.

Harald was involved, when he went back to Norway, in helping to think through new international organizations in oceanography, which eventually led to the formation of the Scientific Committee on Oceanic Research and the Intergovernmental Oceanographic Commission at UNESCO. But those didn't really take shape until the late 1950s, after his death.

My first experience in international affairs was in 1936, when I was twenty-seven years old and had just gotten my Ph.D. and was the Scripps representative at the meeting of the International Association of Physical Oceanography in Edinburgh, part of the general assembly of the IUGG, the International Union of Geodesy and Geophysics. There I met Bjǒrn Helland Hansen; I'd met him already at Scripps. I met Joseph Proudman of England and various other Englishmen, George Wüst was there from Germany, Haakon Mosby was there from Norway, and a great many sort of old-fashioned oceanographers, descriptive oceanographers.

The next international meeting I attended was in 1939 in Washington [D.C.], again the International Association of Physical Oceanography. That was the last meeting until 1948. The war had begun already in September of '39 just before the meeting started in Washington.

Sharp: So as far as the Europeans were concerned that made a break.

Revelle: It sure did, as far as everybody was concerned, it made a break.

During World War II I developed quite close relationships with George Deacon, later Sir George Edward Raven Deacon, who was involved with studies of underwater sound which would help in understanding the performance of what the British called Asdic and we called Sonar. We found that George in England and we in the United States had pretty much come to the same conclusions about the performance of underwater

Revelle: acoustic submarine detecting equipment. Its performance varied enormously with the water conditions. If you had what's called a thermocline near the surface, that is a sharp break in temperature near the surface, the sonar gear would maybe give you an echo from a submarine about three hundred yards away, a very poor performance. If on the other hand there was a deep mixed layer, a submarine could be detected at maybe five or six thousand yards, an order of magnitude difference.

What we developed in this country more than the British did was use of the ocean conditions that affect underwater sound by submarines, whose job was not to echo range but to hide from echo ranging. It turned out that if the submarine could get below the mixed layer into the thermocline, it was very hard to detect. So some people at Woods Hole, chiefly Maurice Ewing, Allyn Vine and Alfred Redfield, developed a "submarine bathythermography," which was carried on the submarine. With it, the submarine could measure the vertical temperature distribution in the water in which it was and move accordingly.

Alfred Redfield pointed out that submarines could sit on the bottom of the mixed layer or in the thermocline and not run their engines at all. They could balance themselves in the water column because the density was increasing with depth in a sufficiently sharp thermocline faster than the submarine's own density increased by its being compressed. You had to take the submarine's compressibility into account. The submarine got heavier as it went deeper because it was squeezed together. But it didn't get heavy as fast as the water did when the water changed markedly in temperature with depth. We published something in conjunction with Mary Sears--by we I mean Section 940D of the Bureau of Ships--which we called "Sumbarine Supplements to the Sailing Directions," in which we showed the submariners what it would be like in different parts of the ocean, what they should expect it to be like, areas in which they could sit on a thermocline and areas in which it would be much more difficult.

Sharp: It was a matter of actually writing instructions for the people involved in that part of running a submarine?

Revelle: Yes. A lot of that was done here in San Diego at UCDWR, by Dick Fleming and George [Stewart]--the man who wrote Earth Abides, a professor of English at Berkeley. He wrote several books like that, one about the Donner Party [Ordeal By Hunger].

Revelle: We had him working with us to put these things into readable English. Anyhow, what I started to point out was that we worked quite closely with the British oceanographers, particularly with George Deacon and his crew. Then after World War II George became the first head of the new National Institution of Oceanography in England. We've been associated all our lives in international cooperation ever since that time. He's now Sir George.*

So we were quite prepared after the war to continue international cooperation in oceanography. One reason was the development of the bathythermograph, which is a gadget that measures temperature against depth in the upper layers of the ocean. You can use it from a moving ship, from any ship in fact. All that's needed is just a simple small winch with a thin wire on it, and you lower the instrument from that. In those days we used to recover the instrument and collect the record, which was on a little smoked slide, from it. Now they use what they call the expendable bathythermograph; they never recover it, but they do recover the record. One expendable bathythermograph costs about $50.

Sharp: That's a technological improvement.

Revelle: Tremendous improvement, yes. They're used now on many merchant ships. Many of these ships have devices that automatically record and transmit the message. You don't really have to have a skilled technician on board;just the radio operator can do it.

During World War II, one of my principal responsibilities in my Bureau of Ships job was procuring and distributing the surface ship bathythermographs and seeing that they were used.

Sharp: I think the last time we met, when we were talking about your World War II navy activities, I had shown you a couple of reports of your tracking down some of the bathythermographs, where they were used and _if_ they were used, which was sort of a major question, I gather.

Revelle: That's right. There were some scoundrels who never put the instrument into the water; instead, they scratched the slides with a pencil.

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*Sir George Deacon died in 1984. See "George Edward Raven Deacon, 1906-1984," by H. Chavneck in _Biographical Memoirs of the Royal Society_, Vol. 31, November 1985, pp. 113-142.

<u>The International Association of Physical Oceanography and the Idea for the International Geophysical Year</u>

Revelle: After World War II the first meeting of the International Association of Physical Oceanography was in 1948. As I remember it that was in Oslo [Norway]. Ellen and I and two of our children, Annie and Mary, went on our first post-war trip to Europe. Of course we had been in Great Britain and Norway in 1936, but this was our first trip after World War II and the first time we had been on the continent. We went to Italy and then to Austria where we joined Walter Munk and his first wife, Martha Chapin. Then we traveled across Germany by train to Hamburg. Walter and I then flew from Hamburg to Oslo, leaving our wives and children behind to come by train.

That train trip across Germany was terrible, not terrible in terms of difficulty but in terms of the suffering of the Germans and the hatred that the Germans had for us, for Americans and other Europeans too. It was a very bad time in Germany. Not anywhere near so bad in Austria, but even in Austria they had a severe inflation.

By 1952 or thereabouts Lloyd Berkner and some of his colleagues in geophysics in Washington, including Jimmy Van Allen, the man who later discovered the Van Allen belts around the world, had proposed a new International Geophysical Year, which would be fifty years after the second polar year, which was, I guess, 1907. It was also, I think, the seventy-fifth anniversary of the first polar year, which would have been 1882 since the IGY was to be in 1957.

Their idea was first presented at the next IUGG meeting, which was in Rome in 1952. We oceanographers didn't at first have much participation in the IGY, because we had no international cooperative mechanism for observations. The IGY was basically an observation enterprise.

Sharp: By mechanism do you mean organization?

Revelle: Organization, yes, worldwide organizations. The IAPO [International Association of Physical Oceanography] was essentially a device for exchanging information and ideas. They did do some technical things too, as Sverdrup points out in this article. They maintained a standard for measuring the salinity of seawater, so-called standard seawater, at a laboratory in Copenhagen. That was the laboratory of the International Council for the Exploration of the Sea, ICES, which was primarily a European organization for the study of the northeast Atlantic. They had standardized and maintained standards for measurement of salinity, the salt content of ocean water, which had

Revelle: to be measured with extreme accuracy. By extreme I mean at worst two parts in the fourth significant figure. The average salinity of the ocean is about thirty-five parts per thousand. We tried to measure it to two hundredths of a part per thousand.

Sharp: More finely tuning your idea of measuring what the salinity was. There must have been some discussion about how exactly to decide on--.

Revelle: That was done basically in the 1910s, quite early on, by a man in Copenhagen named Martin Knudsen. I met him at the IUGG meeting in 1936; he was one of the people there.

You had to measure salinity to that accuracy because it doesn't vary very much in the deep ocean. If you didn't measure it that well you might as well not measure it at all. Now they try to measure it to less than one part in the fourth significant figure, in other words less than a hundredth of a part per thousand, an accuracy which you need for studying the deep water. That's done now largely with a "CTD" or conductivity-temperature-depth recorder, which you lower from a ship on a long cable. It sends electrical signals back to the ship. Quite an expensive instrument, but by measuring both the temperature and the conductivity you can get the salinity to the required high degree of accuracy.

Cooperation with the Russians and the Japanese

Revelle: One of our problems in international cooperation with the Russians is that they don't measure the salinity with sufficient accuracy. You really can't rely on their salinity measurements at all. So this has seriously limited oceanographic cooperation with the Soviets, not because of any ideological difficulties but because of this technical difficulty.

Sharp: Why is it that they would not push the measuring in the same way as the Americans and other Europeans have?

Revelle: I really don't know. I think it's basically because they haven't devoted enough resources to it. They have some very good theoretical oceanographers. For example, the best book ever written on underwater acoustics was written by a man named Brekhovskikh. And they have some very good people in the study of turbulence, Professor Monin, who is the present director of the Institute of Oceanology of the Academy of Sciences of the USSR, is perhaps the outstanding theoretician in the study of turbulence.

Revelle: They do a great deal of modeling. I went to a meeting in Tallinn recently on monitoring of the ocean. Most of the papers were by young Russians, the sad reason being that this was just after the Korean airliner was shot down, and consequently it was awfully hard to get to the Soviet Union. Not many airlines were flying there. Since the Russians were paying for the meeting, they insisted you fly on Aeroflot. And Aeroflot was hard to get to. I had to fly to Mexico City to catch it.

As I have said, most of the papers at this meeting were by young Russian scientists, and most of them were presentations of very complex, long-drawn-out mathematical models, which were, for me at least, impossible to follow. I think they were impossible for anybody to follow in an oral presentation. But that seems to be their tradition. Brekhovskikh was there, this man who wrote the wonderful book on acoustics, and he was a tower of common sense. He'd say, "Well, what are you really driving at?" to these poor young fellows. That was quite discouraging to them because he's such a great man, a leading Academician.

Anyhow, cooperation in field work and work at sea with the Russians has had serious difficulties because of the poor quality of their salinity observations. That was not obvious in the early 1950s though, and we welcomed their cooperation in the IGY. The first time that the Russians really showed up at an international meeting was after the death of Stalin, in 1954, at an IGY meeting in Brussels.

Harald Sverdrup was there, and we were there with our daughter Mary and our son Bill. We sent Bill home, I remember, from Brussels. He was nearly ten years old at the time, traveling home by himself. He was very pleased with himself. He and Harald got along very well.

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Revelle: The Russians showed up at this meeting after the death of Stalin. It was the first contact we'd had with them since World War II. One of the people there was a man named Vladimir Kort, who was at that time the director of the Institute of Oceanology. He and I became very good friends and remained so as long as we had contact with each other. Many of the Russians who showed up at that meeting are still active in international cooperation; they come to the International Council of Scientific Unions meetings, for example.

Sharp: I was looking at some clippings from the scrapbooks in the Scripps Archives. There were a number of Russian scientists who came to visit Scripps a little bit later.

Revelle: They brought their ship into San Francisco, and we paid their way down here from San Francisco. We got a dispensation from the Navy Department to let them come. That was much later, though, I think.

Sharp: I think it was '58 or '59.

Revelle: As I remember it, there was also an IUGG meeting in Berkeley. That was in 1964. I think that was another time when we brought the Russians down, in '64. Much later. Before that they'd been in Hawaii, though. They'd brought their ship in to the Pacific Science Congress in 1962. I think it was the Lomanosov. We held the International Oceanographic Congress at the U.N. in New York in 1959 and again the Russian oceanographers brought one of their big ships. So international cooperation developed very fast after those beginning meetings on the IGY.

The first actual field operation I was involved with internationally was called the Norpac operation, which was an attempt to get a synoptic picture of the North Pacific. The Japanese and the Canadians and the Americans cooperated. I don't think the Russians were involved with that, just the Japanese and the Americans and the Canadians. But we all put in several ships, and between us we made a pretty good oceanographic map of the North Pacific ocean waters north of about 20° latitude. This is what oceanographers and meteorologists call a synoptic map, that is, observations taken at pretty much the same time over a big geographical area.

The man who was particularly involved with that was Joe [Joseph L.] Reid [Jr.], who is now a professor at Scripps. He was then a graduate research assistant. But he organized the thing pretty well. I made my first trip to Japan at that time, in the early 1950s. The Japanese, of course, were just coming out of World War II, and their facilities were quite bad, very primitive buildings, a grungy city, and frightening taxi drivers.

Sharp: Was one of these facilities what was called the Hydrographic Office?

Revelle: In Japan? That's right, the Japanese Hydrographic Office. It was headed by a funny little man named Suda.

Sharp: I think you had quite a bit of correspondence with him.

Revelle: Oh yes, that's right, a lot. He visited us at Scripps, stayed at our house in fact, at this very house here. He was funny in many ways. I remember he asked Ellen, "How often you go beauty house?"

Sharp: He'd heard that that was something American women went to?

Revelle: That's right, yes. Another man whom we got to know quite well in Japan was Koji Aidaka. He was a very theoretical oceanographer, much like the Russians, a long series of mathematical equations that you couldn't make head nor tail of.

Sharp: Sounds like some of the current economists, who are so theoretical, the modeling is the main thing.

Revelle: That's right. Very little contact with reality. That's the way most of our Economics Department at UCSD is, for example.

Another person involved with our early Japanese American cooperation was Kozo Yoshida; he was a student of Aidaka's who had become a professor at Tokyo University, taking Aidaka's place there. He died of cancer a few years ago. The Japanese oceanographic chemist, Yasuo Miyake and his wife and daughter came and stayed here in our guest house for nine months. We became very good friends. His wife's name was Susu; she was a painter, did beautiful watercolors. Their daughter was a musician, a pianist. They were extraordinarily proud of her. I think she actually became a concert pianist, in Western music, not Japanese music. One of the interesting things about Japan is how they have absorbed the Western artistic culture.

Sharp: Very much so, in terms of concert music and philharmonic type music.

Revelle: In fact, did you know the head of the Boston Symphony is a Japanese?

Sharp: Oh, that's right.

Revelle: Scientific cooperation with Japan developed continuously after Norpac. Eventually we organized a broad Japanese American cooperative scientific program.

Sharp: You had four or five folders in the papers at the archives having to do with that program.

Revelle: My part of it was basically in geophysics.

Sharp: Yes. Some early stages of the agreements about how exactly the work might be done and arranged. It was a small amount of papers; I didn't know if that indicated that was something that didn't develop fully, or you just didn't have very many papers.

Revelle: It developed fully, all right. I was the American joint chairman of the geophysics panel for that cooperative agreement. That was mostly in the early 1960s. It still goes on, but I'm no longer associated with it, haven't been for a long time. Not since I went to Harvard.

IX GETTING THE "S" INTO UNESCO

The Committee on UNESCO in the National Research Council

Revelle: About the same time as we were developing these cooperative arrangements with the Japanese and in the IGY with the Russians, as well as with many other countries, UNESCO had a committee called the International Advisory Committee on Marine Sciences [IACOMS], which had an international membership.

Sharp: I had some more specific questions about that. I thought we could discuss that as an example of some of your UNESCO work. Then on the other side of that, the UNESCO Committee of the National Research Council as well.

Revelle: I was involved with the UNESCO Committee of the National Research Council, I think, before I became a member of IACOMS. I was the American member of IACOMS.

Sharp: The date I have on your becoming a member of IACOMS is 1956. There's a letter that I saw that you wrote to a Professor Pierre Auger--.*

Revelle: He was the assistant director for science at UNESCO. I thought I was a member earlier than that.

Sharp: Well, it's a pretty formal letter; like you were indicating that you would accept this membership. You may have been working with them quite a bit before that. But '56 is the date.

Revelle: I thought it was earlier.

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*Revelle accepted membership in IACOMS in 1956, according to a letter he sent to Auger, of UNESCO, in Paris, dated 4 June 1956. In the letter, Revelle comments on his interest in oceanic surveys.

Sharp: Maybe we could spend a little time talking about that. I sent you a copy of two sets of minutes of the UNESCO Committee of the National Research Council.* In the 1961 minutes, that's where--.

Revelle: Are you sure it's as late as that?

Sharp: This is the set of the 1961 minutes.

Revelle: Oh, I remember. That Committee of the National Research Council went on for a long time, but the first one was when Bart Bok was chairman, and that was the early 1950s. So this is 1951. You see this was the ninth meeting in 1951.

Sharp: I thought we might just talk about how that committee changed and how your participation changed. Maybe we could just start by your telling me how you got involved in that particular committee, if you remember.

Revelle: I don't really remember how I got involved with it to begin with, but as you can see it started at quite an early time.

Sharp: I was surprised that it was so early; I didn't realize that it--.

Revelle: UNESCO itself was started in 1946. Bart Bok used to claim that he was the man who put the "S" in UNESCO. It started out as the United Nations Educational and Cultural Organization. Bok used to say that he persuaded the organizers to add science to it.

Sharp: Does that sound right to you?

Revelle: I don't really know if it's so or not. But that certainly should be evident from the early history of UNESCO. I know that they had not originally planned to have science involved. I don't remember whether Bok was a member of the National Academy of Sciences at that time, but I think he was. Most of us were not. But in this group here in this photograph, in 1951: Merle Tuve was a member.

Sharp: Of the Academy?

Revelle: Of the Academy. I'm pretty sure that Maurice Visscher was a member, and Ralph Cleland of Indiana University. Those were the only members of the Academy. Wally [Wallace] Atwood was the staff officer

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*National Research Council, Division of International Relations Committee on UNESCO, Minutes of the Ninth Meeting, October 8 and 9, 1951, Washington D.C.; National Academy of Sciences-National Research Council, Minutes of the Committee on Science in UNESCO, 18th meeting, February 27, 1961.

Revelle: for the International Relations Division of the Academy, which was listed as part of the National Research Council. Watson Davis was head of Science Service. Paul Gross was a scientific administrator, vice president of Duke at that time. Wally [J.W.] Joyce was about all there was of the science part of the Department of State. Helen Putnam worked for the UNESCO Relations Staff--that was in the State Department also. Gene Weltfish was a communist anthropologist. She was one of Franz Boas's students together with Ruth Benedict and Margaret Mead--the group that organized around Boas. Dael Wolfle was executive officer of the American Association for the Advancement of Science for many years. Maurice Visscher was a physiologist in Minnesota. Raymund Zwemer was another of the Washington scientific bureaucrat types.

I was brought into this after the committee was organized, as I remember it. I think it was in the early 1950s. [tape interruption] How I actually got involved with it, I think somebody must have asked me to be involved. That's usually the way with me. "I can't say no."

[tape interruption]

Revelle: There was another organization at about the same time, which was organized by ICSU [International Council of Scientific Unions], or by IUGG maybe, called the Commission on Oceanography. The chairman of that was an Englishman named John D.H. Wiseman. We had a meeting in Monaco. Harold Urey was there; first time I got to know him. Mary Sears was there and Anton Bruun, the Danish marine biologist who had been leader of the Galatea Expedition. Harald Sverdrup was there, I think, pretty sure he was.

What I remember about that meeting in Monaco was that Harold Urey said that the most important thing about scientific cooperation was publication, and he proposed establishing a journal that could be a medium of international communication. We all agreed that such a journal should be established. It became the Journal of Deep-Sea Research, which Mary Sears was the editor of for many years. That's the leading, or at least was for many years, the leading oceanographic journal.

Sharp: Is that, for example, where people might have published articles on the IIOE, the International Indian Ocean Expedition? Because that was, in terms of cooperative ventures, that was one of the--.

Revelle: Yes, but the Journal of Deep-Sea Research was just a scientific journal. It did not have especially to do with international cooperation, but it was an international journal published by Pergamon Press.

Sharp: I have a couple of other questions about this--.

Revelle: Coming back to the UNESCO committee in the National Academy of Sciences, I think I must have been asked to become a member of it, like you so often are. And I did become a member. It thought of itself as basically a committee to guide the scientific work of UNESCO, working with our National Commission on UNESCO. But it was independent of the National Commission. All it could do, of course, was to provide advice. That's all it did do. You get a pretty good flavor of it from these minutes.

Sharp: The contrast between the '51 minutes and the '61 is interesting, because the '51 is much more tentative about the kinds of scientific work that might be encouraged, taken on, supported. By '61 the projects are very clearly defined, and the evolution of support for these kinds of scientific projects--.

Revelle: The principal reason for that was that UNESCO appointed a very good Assistant Director General for Science. He was a Russian named Victor Kovda. He was a soil scientist. He realized quite clearly that what UNESCO could do was to encourage, stimulate, and support international cooperation in the field sciences, like for example soil surveys, studies of arid lands, geological correlation, all the things that in fact UNESCO has done over many years now, cooperation in oceanography and in other kinds of geophysics--but not meteorology, because that was handled by the World Meteorological Organization.

So he started UNESCO on a program that made sense, not some sort of vague, do-good scientific program. They did continue to support many other things, like for example the International Cell Research Organization [ICRO] and the International Brain Research Organization [IBRO]. Basically European cooperation in advanced biological science.

They started scientific field offices in Montevideo [Uruguay] and Djakarta [Indonesia]--or maybe it was Singapore, somewhere in the southeast Pacific--and in Cairo [Egypt]. Those were never very effective, but the idea was to bring in scientists from developing countries to develop science in developing countries. They also had a field office in Nairobi [Kenya].

UNESCO in National and International Politics

Revelle: The difficulty was that UNESCO never really got very good people for those field offices and never supported them adequately. UNESCO is far too centralized an organization. Ninety-five percent of the staff is in Paris.

Sharp: I wondered about that. Was that an issue as early as this period, the early fifties, or was the centralization an issue more towards the later period, towards the sixties?

Revelle: It became more and more obvious, one reason being that we were learning about the need for international scientific development of the so-called less developed countries as opposed to scientific cooperation among the rich, industrialized countries. In my opinion the best international aid organization is the U.S. Agency for International Development, believe it or not.

Sharp: Why is that?

Revelle: The reason for that is that half their staff is in the field. Many of their staff members spend years in a developing country and learn about its problems and try to do something about its problems. Particularly in some countries they've had an outstandingly good staff, in Pakistan, India, Egypt particularly, to some extent in other African countries, like Ghana and Kenya, and in smaller South Asian countries like Sri Lanka, Nepal. So there are more people in AID who really know about a developing country than any other organization, even the World Bank. The World Bank has offices in many places, but again, they keep most of their staff in Washington, whereas AID has a continual exchange between the field and headquarters.

Unfortunately, AID is a highly bureaucratic organization. That's primarily because it has been so constrained by Congress. Congress has really never accepted its existence very enthusiastically.

Sharp: Because it seems too independent to them?

Revelle: No, because there's so little support for international aid among the United States public. Most congressmen are quite sympathetic to it, but they have to think about their constituents, so they put all kinds of restrictions on it that make it appear better to their constituents. I think Congress on the whole has done the best they could, but they've tied AID up in red tape, accountability and things like that.

Revelle: This has not been true of UNESCO. They have not been tied up in that kind of red tape by the member countries. They've gotten themselves in trouble internally, by their own dynamics.

Sharp: And yet if you look at some of these reports and so on over time, you see how the budgets have increased for scientific projects, grants-in-aid for young scientists to be supported, to attend meetings as well as to carry on their own work. Those budgets have really gotten bigger.

Revelle: Yes. The difficulty with UNESCO, from the standpoint of the United States and the other advanced countries, is that, like all these international organizations, it's now run by the developing countries. From our point of view its basic problem is UNESCO's governance. Each country, no matter how big or how small, has one vote, so that forty African countries have more votes than all the developed countries put together, in spite of the fact that even in terms of population the developed countries have three times the population of Africa. But there aren't so many developed countries.

Sharp: So because of the one country, one vote system the clout is considerable for the developing--.

Revelle: It's overwhelming. The present directors of nearly all the U.N. organizations are people from developing countries. And then executive boards are dominated by the developing countries.

Sharp: You had a very interesting letter. It was an early one; it was 1950, and it was among this pack you sent to Maurice Visscher. I wanted to remind you about that letter because of some of the things that you said in it. Initially, in the first part of the letter, you talk about the grants-in-aid program in UNESCO for the international scientific unions and the reasons for continuing it and so on. Then you get into a very detailed argument about UNESCO being involved in science for world peace and for U.S. benefit and so on. I thought we might talk about that. Take a few minutes to read it over again if you want.* [tape interruption]

Revelle: As I say in the letter Alexander Hamilton pointed out that individuals are the only proper objects of government. It is only by development of the concept of individual citizenship in the world community that progress will be made toward effective world government. You just can't do it by the "concert of nations" business.

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*See following pages for letter to Visscher, dated 4 February 1950, Revelle papers 81-23, Box 1.

81-23
box 1

February 4, 1950

Dr. Maurice B. Visscher
Department of Physiology
The Medical School
University of Minnesota
Minneapolis 14, Minnesota

Dear Maurice:

I have been trying for some days to prepare "a clear statement in two or three pages concerning the scientist's point of view regarding the place of grants-in-aid in the UNESCO program". Since, according to the minutes of our committee meeting, this statement is to be sent to everyone on the committee's memorandum mail list and to all the members of the U. S. National Commission, it seemed to me that it should be in a form that could be published in some such journal as Science or the American Scientist, and should contain a summary of the development of relationships between UNESCO and the International Council of Scientific Unions, and the extent and character of previous grants-in-aid as well as arguments for continued UNESCO support of international scientific cooperation. This has proved to be a rather large order, particularly because so many international scientific organizations now in existence or in the embryonic stage are not members of the International Council of Scientific Unions.

I remember, however, that you were planning to go to Washington for the meeting of the Program Committee of the U. S. Commission for UNESCO, and that you wanted to have something to present at that meeting on this problem. I am therefore putting down my rather disconnected notes on the various reasons for continued UNESCO support of international scientific organizations, so that you may use them for whatever they are worth in your discussions in Washington. I hope to follow these up shortly with a more elegant and complete document.

1. The international scientific unions are in a sense laboratory models for the development of international democratic procedures. Their scientific activities, administrative decisions and interchange of ideas result from discussions, conflicts, compromises and agreements between active scientists at all levels of age and accomplishment, acting as individuals and not as representatives of a national interest. In his famous Federalist Paper No. 15, Alexander Hamilton pointed out that individuals are the only proper objects of government, and it is only by development of the concept of individual citizenship in the world community that progress will be made towards effective world government.

#2

2. The international scientific societies have long been and continue to be leaders in the development of a world point of view as opposed to national points of view. Through their experiences at meetings of such bodies, younger scientists will gain the understanding and impetus to become forceful advocates of international cooperation in their own communities. Thus international thought and action will evolve from below rather than be imposed from above.

3. The maintenance of peace and security must be dynamic rather than static in our rapidly changing world. Scientific discovery and the resulting technological advances are perhaps the most important cause of change in the world today. In order to play a role in the forefront of changing ideas UNESCO must keep abreast of advancing science. The international scientific unions are by far the best medium to serve UNESCO in this regard through advisory councils, committees of experts, symposia and documents. But they can only do so if they are strong and healthy.

4. Peace is indivisable. While areas of poverty and misery exist there can be no lasting security. Poverty and misery can be temporarily alleviated through material aid, but in the long run only scientific discovery and application can produce a remedy. In general this remedy can be applied only by the peoples themselves, but the peoples must have the necessary tools if they are to work out their own salvation. A broadening and deepening of international scientific cooperation is necessary for the distribution of some of these essential tools.

5. The work of the international scientific unions has demonstrab[ly] led in the past to steady progress and accomplishment. Such progress even though slow is more desirable than spectacular short term projects, which are liable to leave little lasting effect.

6. Support of international scientific unions, in order to be effective, must be on a long term basis, but the benefits and accomplishments resulting from such support are often immediate and new benefits continually arise. Thus the international unions form a framework in which short term projects can be most successfully developed.

7. Support of international scientific cooperation is very much in the enlightened self-interest of the United States. This is true not only in fields where research problems involve large geographical areas, such as meteorology and astronomy, but also in the many sciences where other countries lead in basic research while the United States has emphasized application.

Very sincerely yours,

RR-b

Roger Revelle

Sharp: Do you know why you had to write that letter? Do you remember? You're talking about the grants-in-aid programs specifically. Then you go into a much longer explanation of reasoning for U.S. involvement, and so on. I wondered just what had prompted that kind of general statement.

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Revelle: If I read between the lines here, this was in connection with the UNESCO subvention to ICSU [International Council of Scientific Unions]. ICSU has received, throughout the history of UNESCO, about $400,000 a year from UNESCO, which contributes a good deal to their budget, or it did contribute a good deal to their budget. There was always some question about whether it should be continued or not, whether this was a good way to spend UNESCO money. This is essentially a set of arguments why it's a good way for UNESCO to spend its money.

Sharp: I was interested in part of the argument; you're saying that one of the reasons the United States should keep its participation, that the U.S. benefits from its involvement in UNESCO because some of the other nations lead in certain areas of basic research, while the United States was putting emphasis on more applied research. You're bringing up some of the arguments we were talking about yesterday about what we can learn. There's a certain exchange of information that was really useful to the United States.

Revelle: Remember the date of this letter. It's 1950. That was before the tremendous burgeoning of American science. It was just after World War II, before America became so preeminent in basic science. ONR had just been going for four years; NIH [National Institutes of Health] started. NSF [National Science Foundation] had not started. So we had not really had this enormous flowering of basic science; it came later than 1950. Then also it was a little bit exaggerated even then, because the only countries that had done quite a bit of basic science were half a dozen European countries: Germany, England, Sweden, to a much less extent France, Italy. The Netherlands had done a lot of course. Belgium and Switzerland too. But there were only a very small number of countries where pure science had been done on a large scale.

I became a member of the U.S. National Commission for UNESCO some time in the 1950s, and I was a member for six years. I was vice chairman for three or four years. That was a frustrating experience, because the State Department essentially paid no attention to the commission. We were appointed by the Secretary of State, but with an inadequate budget for the activities of the commission, and a mediocre staff. The State Department would assign people it didn't

Revelle: know what to do with to the secretariat of the commission. The commission has become in more recent years a sort of defender of UNESCO within the United States government, but the Reagan administration cut out their budget entirely. They've been living entirely on private funds. They may not even exist any more for all I know, even though the commission is mandated by Congress. In the act in which we joined UNESCO, Congress set up this commission.

Sharp: In this period what was it that the National Commission was supposed to do?

Revelle: It was supposed to advise the State Department as to our policy in UNESCO.

Sharp: You would bring into that arguments and explanations for UNESCO's role in science internationally.

Revelle: That was its legislated function, its authorized congressional function. In addition it felt that it had a major responsibility to sell UNESCO to the American people, and they did that by publications and meetings and things like that. But they never succeeded very well, as you know; there's very little American support for UNESCO. I think most people think of it as an unnecessary organization because it deals with education, science and culture, which Americans somehow don't think is--.

Sharp: Tall on the totem pole. What about the role generally of American scientists in UNESCO?

Revelle: Let me modify this statement a little bit. Americans have always been enthusiastic about education. That part of UNESCO they should be willing to support and are willing to support, or have been. They're enthusiastic about science, and what they know about UNESCO science, they mostly are supportive of that too.

What gets them, basically, is the cultural part, particularly international culture, which they regard as a waste of time; most Americans do, I think. I don't think it is; it's terribly important, but it's vague and it's ambiguous, and there are no short-term benefits, except for tourists. Preserving the monuments that were submerged by the Aswan High Dam and were reassembled above the lake level at Abu Simbel in Egypt: I think many Americans think that's a good idea, because they can go look at them. But very few Americans actually will look at them or have a chance to look at them. I think the preservation of the great structure at Borabadur in Indonesia--again, people think in principle that's a good thing, but they're not willing to spend much money for it.

Revelle: That's a hangover, basically, from our early days. John Adams said that "my sons have to study engineering in order that their sons can study art and literature." Americans have never gotten over the idea that they should still be studying engineering and not be much concerned about cultural things.

Also UNESCO has aroused antagonism because, as an international organization, it has to give equal time to the Russians and the other Eastern bloc countries.

Sharp: Also, about the role that you mentioned of the developing countries, I think many Americans might not consider that--the idea that the developing nations should be in control of a certain kind of organization--.

Revelle: I don't think so either. Any man in his right mind in a developed country would think that on a short-term basis that's bad. On a long-term basis it may not be.

For example, in UNESCO right now the director general is a guy from Senegal, Ahmada M'tar M'bow. He is obtaining staff members from developing countries more or less out of proportion to their population, and certainly out of proportion to their competence.

So you get all kinds of highly paid, high level bureaucrats in UNESCO from African countries, for example, who don't know their ass from a hot rock, I mean about what they're doing. And they get paid several times what they would get in their own country, so they all love their jobs. But they're incompetent. Even a third or fourth level American bureaucrat is better than most of these guys. But the trouble is that the Americans for the most part have not tried to get jobs in UNESCO.

Sharp: They just don't see it as an important enough organization?

Revelle: One thing is the pay is not so awfully good. You don't get the best people. It's very good for a person from a developing country. It's pretty good even for an American but not outstanding.

More serious, it's a dead-end career. You take time out from your normal course of advancement in the United States to do it.

Sharp: That kind of service doesn't transfer back once you come out of the service in UNESCO into something usable?

Revelle: No, it doesn't. So we haven't had as many good Americans as we could have had. We've had some very good Americans: Sidney Passman and Jack Fobes are two that come to mind. Several others in education and the social sciences have been very competent people; many of them were later involved with the [U.S.] National Commission [for UNESCO], people who had served on the UNESCO staff.

But this committee in 1960, the NRC committee on UNESCO, had some first-rate people in it: Luna B. Leopold, Ted [T.C.] Byerly, Joe [Joseph B.] Platt, Ralph Cleland, Elmer Hutchison, [Conway] Zirkle. Every one of them was a pretty outstanding person. So a lot of Americans have had a lot of heartaches about UNESCO, a lot of good Americans.

Sharp: Let me go back to the question that I asked before about how you would assess the role of American scientists in UNESCO in this period, in the fifties. You described the involvement of several other scientists; how would you look at it generally?

Revelle: In the fifties, the part that I know best was the Division of Ocean Sciences. An American, Warren Wooster, was the first secretary of that division, the first head of that division. He was one of the best scientific bureaucrats who ever lived--is one of the best. He became a professor at Scripps afterwards, then became director of the Rosenstiel Marine Laboratory of the University of Miami and then director of the Institute of Marine Sciences, or something like that, at the University of Washington. Very, very good man. He did a lot, particularly later when he became secretary of the Intergovernmental Oceanographic Commission.

Sharp: You would judge his efforts at pushing the kinds of oceanographic work to be done by UNESCO--?

Revelle: As very important. The Intergovernmental Oceanographic Commission started out as the only game in town as far as governmental cooperation in oceanography is concerned. It pretty much still is. Again, however, it has become run by the developing countries, and they don't care much about it. They use it as a political tool. So it has gone downhill a lot.

Sharp: One of the comments that you made in one of the letters, the letter that you wrote to Minnich, it's one I saw later after I sent you all that stuff. In '61 he was executive secretary of the U.S. National Commission.

Revelle: And I was the vice chairman of that commission.

Sharp: One of the things you wrote about in the letter was the program commission of the UNESCO general conference. He must have been asking you for these comments, but I thought we might talk about that a little bit because of some of the changes it represents.

Revelle: In a UNESCO general conference, which is held every two years, what they do is to have a group of commissions that meet on the different aspects of the UNESCO program. One is the commission on the science program. I've been a member of several U.S. delegations to UNESCO, and I've always been on that commission, the last time in '79. By that time I'd gotten the status of an elder statesman, so people treated me with respect. (That was not so true in the early days.)

They still operate the same way; there's been no change. They take up a long series of resolutions one by one. The secretariat pays a minimum of attention to them, to what the commission says, although they take note of what the commission says, and they have minutes. All these U.N. organizations produce infinite quantities of paper.

Sharp: That's clear just in your own papers; the UNESCO body of papers is far greater than any of the others, probably combined. It's fearsome when you start to go through it because it's endless.

Revelle: I think in some way one reason for this is to obfuscate things. If you just produce enough paper you don't have to pay any attention to any of it.

But what I said here was, I think, quite right, that they should deal with a few broad issues of policy and not all these piecemeal resolutions. It is quite right that the developing countries, particularly, think of UNESCO as a grab bag of goodies, not as an organization for promoting international cooperation. They don't care much about international cooperation.

Sharp: It could be that that's their understanding of international cooperation, that monies are put into the UNESCO fund for them to help themselves to; that's their understanding of how it should be.

Revelle: That seems to be their understanding, yes. It's a very poor concept, however, particularly because the money doesn't amount to very much.

Sharp: Was that idea of the developing nations, did that come gradually through UNESCO as it grew in the fifties?

Revelle: Well, you see, the basic concept was flawed when they organized these international organizations. At the time that the United Nations was formed and the specialized agencies were formed, that is particularly FAO and UNESCO--the World Health Organization and the World Meteorological Organization really existed before the United Nations was formed with different names but basically the same kinds of organization--there weren't very many countries in the world. Colonial empires still existed. I think if you look at the original members of the U.N. or UNESCO there were only about thirty-five or forty of them, mostly developed countries. For ten years or so we were the dominant force in all those organizations, the United States with its western European allies. The Russians never had a chance, they were always voted down.

During the late 1950s and most of the 1960s, new countries were formed about once a week as the colonial empires broke up. Not that often, but these organizations now have more than 150 members, as opposed to about fifty or less than fifty when they started. It seemed like a perfectly reasonable idea to have one nation, one vote, when most of the nations were allies of the United States, and they all had the same basic ideas about what life was all about: emphasis on education, emphasis on economic growth, emphasis on democratic procedures, to some extent a commitment to eliminating poverty and eliminating class differences. You had a group of like-minded people in all the intergovernmental organizations, except for the Communist bloc, but the Communist bloc was always outvoted.

As time went on, the system of having each country be an equal member led to this utterly insane situation when forty African countries, most of them with less than ten million people, now control the organization--countries that have corrupt, weak, and inept governments, that have most of their peoople living in poverty and misery and ignorance, with no human rights, the status of women as low as it could possibly be. It's a different ball game which nevertheless existed in embryo when the organizations were formed, because of this one nation, one vote principle.

Sharp: From what you're describing, it contrasts sharply with the efforts at international scientific cooperation that resulted in some of the expeditions like the Indian Ocean expedition, using that as an example. That was fairly high-minded effort because it was basic research. UNESCO putting money into that contrasts with this kind of change in the organization itself.

Revelle: They didn't finance operations like the Indian Ocean expedition. The countries themselves financed them.

Sharp: But with some UNESCO--

Revelle: UNESCO provided coordination. A lot of coordination.

Sharp: Coordination and support in that sense.

Revelle: That's right, primarily exchange of information.

Sharp: But that sort of went on at the same time that UNESCO itself was changing a lot.

Revelle: The planning for the International Indian Ocean Expedition pretty much was over by the time these changes in U.N. agencies occurred. The developed countries still had a lot of influence and a lot of participation.

Origins of the Intergovernmental Oceanographic Commission and the Scientific Committee on Oceanic Research

Sharp: Are there some specific recollections you have about how scientific efforts within UNESCO have changed as a result of this shift in the developing nations' position of power?

Revelle: I can put it in a reverse way. When we organized the Intergovernmental Oceanographic Commission its rules were intended to be exclusive, that is, its charter says that membership in the commission will be open to those countries that wish to cooperate in international oceanographic research. To us, that meant having ships and doing oceanographic work in the high seas, big oceanography, big science. It was intended to be cooperation with the Soviets, with the Japanese, with the French, with the Germans, with the Canadians, hopefully with the Indians and the Australians and the South Africans.

Now the IOC has 120 members, something like that. Most of them are developing countries that don't even know what oceanography is, or know very little about what oceanography is. What they think of is studies of their estuaries, their inshore fisheries resources.

Sharp: Within the boundaries of their own nation.

Revelle: Yes, exactly. You could have seen the original concept for IOC if you had attended a general assembly of the IOC in 1962 or '63, when I was head of the U.S. delegation. There we were planning the International Indian Ocean Expedition. We were thinking about

Revelle: exchanges of information about programs and free exchange of observational data, all the things that you have to do internationally. The work has to be done by individual countries, but the value of the work is enhanced if all the data can be freely available to all the people who want to do theory and want to work out ideas about it, to make models. That was what the general assembly of IOC was in those days. Now it's a mishmash of high-sounding words and low-quality action.

I've been to several recent general assemblies of IOC, not as a delegate but as a representative of the Committee on Climate Changes in the Ocean. We have hopes that IOC's part of the World Climate Research Program will go pretty well, but the IOC is now run so politically and with so little attention to research that it is not very encouraging. It's run by a Portuguese named Mario Ruivo, who is very ambitious politically and who caters to the developing countries. Their organizational structure--the president is a Filipino, one of the vice presidents is a Frenchwoman; she's quite good. One of the vice presidents is from Eastern Europe, Claus Voigt from East Germany, and one is from South America. I'm not sure if there's one from Africa.

So, as a case in point, they won't let South Africans attend any meetings or take any part in it.

Sharp: Because of apartheid?

Revelle: Yes. Everybody's against apartheid, but it has very little to do with scientific work.

Sharp: Well, a lot of people wouldn't separate that.

Revelle: Obviously that's the case. But the South Africans do pretty good oceanography and they are in a very critical part of the world.

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Revelle: From the American point of view, we would like to have IOC concentrate on oceanographic science and keep the politics at as low a level as possible.

Sharp: If you look at the whole stretch of time, say, 1946-1947 through 1960 or '61, what is it that you see going on in terms of international cooperation?

Revelle: Everything was getting better all during that period, much, much better. The IGY was a great success; the IBP [the International Biological Program], that was in the 1960s, was a moderate success.

Revelle: The International Indian Ocean Expedition was pretty successful; the continued exchange of data worked pretty well. The cooperation with the Russians was getting better and better all the time. I think that was a period of great optimism. I was certainly optimistic. And of great hope.

Sharp: Certainly organizationally, with SCOR and the IOC and IACOMS* getting organized, the mechanisms were--.

Revelle: IACOMS was there all during the 1950s, but it pretty much has disappeared. Its place was taken by the IOC, a very much more powerful organization. And SCOR has turned out to be a marvelously successful organization. We organized it in 1957; there were seven of us: Anton Bruun, Gunter Bohnecke, Columbus Iselin, George Deacon, a man named Eyries from France, George Humphrey from Australia, though I guess he was a little bit later. There are now well over two hundred members of SCOR, and it has had something like seventy-five or eighty working groups. It's really now an international union of oceanography, although it's not recognized as such by ICSU. It's orthogonal to the other unions, which are disciplinary, in the sense that oceanography is not really a discipline. Rather, the ocean is an object of study, and many sciences are involved with it. SCOR organizes International Oceanographic Assemblies, as they call them, which bring many kinds of scientists together to talk about what they have learned about the ocean.

Sharp: In their particular discipline?

Revelle: But because the oceans are a unit it's quite logical for people to talk about their different kinds of discoveries; they all fit together. They all help understand the other fellow's problem. It's really a remarkable example, in fact unique in the world in that there's no other object of study where unity is so important, where coordination and cooperation and interchange between different disciplines plays a greater role. Not in astronomy, not in meteorology, not in solid earth physics or solid earth geological investigations. In no other part of the earth sciences or the biological sciences is mutual understanding among different disciplines more important than in the study of the ocean.

Sharp: In this 1957 article that you wrote on international cooperation in the marine sciences, it seems that you were holding up the astronomers as somewhat of a model, because they were doing a very

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*International Advisory Commission on Marine Sciences

Sharp: good job of cooperating with each other.* Did it seem like through the fifties and into the sixties that oceanographers, those who studied the ocean, were evolving into that also?

Revelle: Very much so. They've never had the exchange of data that the meteorologists have had or a synoptic picture of the ocean like the meteorologist's synoptic picture of the atmosphere. The reason has been that it's awfully hard to make such a picture. Oceanography, like most science, progresses because of improvements in technology and techniques. This is hard for scientists to admit, but it's true that the instrumentation is the real limitation.

Our limitation has been that our principal instrument has been a ship. Ships are awfully expensive, and there are bound not to be very many of them. It's impossible, from the number of ships we have available, to really get a synoptic picture of the ocean. Now for the first time it is going to be possible with satellites. Satellites are going to revolutionize oceanography.

Sharp: Because of the aerial possibilities?

Revelle: Because you can cover the whole world at once, in a limited way. You can only cover the surface of the ocean. But you can do a lot with the surface, because that's where the exchange with the atmosphere takes place.

Sharp: Especially if you can see a lot of it at once.

Revelle: You can see all of it every day essentially.

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*See "International Cooperation in Marine Sciences," Roger Revelle, Science, Vol. 126, 27 December 1957, No. 3287, pp. 1319-1323.

X SELECTED ISSUES IN INTERNATIONAL SCIENCE

The Tension Between Secrecy and Exchange

Sharp: There's another level of concern. What about the issues that you couldn't exchange information on? At the same time that you were participating in and pushing international cooperation, you were also working on some extremely classified projects. For example, once you came back to Scripps, the different thermonuclear testing went on.

Revelle: That was basically with the Atomic Energy Commission, although the contracts were with ONR.

Sharp: That's obviously some information that was not going to be shared.

Revelle: That's true, of course.

Sharp: What about the tension between secrecy and exchange?

Revelle: That was not serious as far as the atomic tests were concerned, because we got out of those fairly quickly, and moreover they really didn't involve much science.

What was much more serious was the navy's development of ballistic missile launching submarines, so-called Polaris submarines, because they felt that they would have to be concealed. The essence of the Polaris submarines is that you can't find them. That's why they're such a wonderful instrument to maintain the peace. A counterforce strategy doesn't work against Polaris submarines. As long as the submarines cannot be detected or tracked, they're a marvelously stabilizing influence in this terribly dangerous world we live in. But the key word is <u>not</u> detected and <u>not</u> tracked. The

Revelle: navy felt that one important aspect of that was to find shallow water spots in the oceans, seamounts, basically, where a submarine could sit on the bottom and not be detected.

They organized a program of surveying the ocean, those areas of the ocean where the Polaris submarines could operate, which basically had to be within about two thousand miles of the Soviet Union in those days, to determine the bottom topography, particularly the high points of the bottom topography.

Sharp: Places where the submarine could rest.

Revelle: Exactly, or sit. Secondly, as time went on, it turned out that the sonar picture completely changed. Until very recently, passive listening had superseded echo ranging, because you could detect submarines at very great distances by listening for them, particularly the Russian submarines. They have been very noisy. These changes had come about through developments in signal processing, basically integrating a good many signals and analyzing the frequency distribution of those signals. Much of the underlying research was done by Carl Eckart and his group.

By using the sofar principle, the propagation of low-frequency sound over great distances, because it's refracted up and down in a series of waves, detection of a submarine has been possible over a thousand miles or more. The navy has a series of underwater hydrophones at different parts of the world, which do just that: a completely secret system of underwater listening devices all connected together by radio. The propagation of sound to those instruments over the bottom is a very important aspect of the effectiveness of those things.

So the navy for many years classified bottom soundings. The result of that was that the only people who were taking bottom soundings were the navy, and the navy just was incapable of doing anywhere near enough. But the scientists quit doing it, quit surveying the bottom.

Sharp: Civilian scientists?

Revelle: Guys like Maurice Ewing and Bill [H. William] Menard and all the other American submarine geologists, because you couldn't publish the soundings, couldn't find out what the other guy was doing, couldn't integrate them together. I thought that was self-destructive.

Sharp: It was a matter of cooperation among American scientists.

Revelle: But cooperation was impossible if the soundings were classified. So eventually the navy decided, after a strong push from me and other people, that it was against their interests, so they declassified the soundings, and the soundings then just poured in. Now we know an awful lot more about the deep sea topography, along with everybody else. But I used to say, and I thoroughly believe, you mustn't classify anything that God has classified. The duty of the scientist is to break God's classification system. That's what it's all about. In order to do that it takes a lot of scientists working together. God has been pretty clever in classifying things. [laughs]

Sharp: The mysteries are considerable.

Revelle: Sure. I wouldn't argue that we shouldn't classify things that men do, like instruments and devices and techniques. It's undesirable, I think, to do it, but there are obviously some good reasons for doing it. The arguments against classification are that technology doesn't progress very fast if you do that. Unfortunately it progresses pretty fast even so.

So that was the real problem of classification; it was not the weapons tests problem but the problem of classifying fundamental information about the ocean.

The "Very Complicated Business" of the International Indian Ocean Expedition

Revelle: As far as my work with UNESCO was concerned, my principal objectives were to get the Intergovernmental Oceanographic Commission started, and the Scientific Committee for Oceanic Research [SCOR]. Part of that, of course, was the International Indian Ocean Expedition. Planning for that expedition was a very complicated business.

We had a man named Bob Snyder, who was our coordinator for that. He was not an oceanographer, but he had been a test officer with the navy for many years. He was an entrepreneur, a promoter, a very hard working and earnest sort of a guy. Not very bright.

But the scientific planning had to be done by the scientists themselves, and our main problem there was to bring in the younger scientists. The planning was organized by this committee of so-called senior statesmen, the Scientific Committee for Oceanic Research. The original idea was, I think, Henry Stommel's. Henry Stommel and Columbus Iselin. At our first meeting of SCOR at Woods Hole in 1957, Revelle: we tried to think of what needed to be done in oceanography and particularly what needed to be done by international cooperation. And Columbus said that the great unknown area of the ocean was the Indian Ocean. I believe Henry Stommel had put the idea in his head. Columbus said what we ought to do is think about finding out more about it. That was the origin of that expedition.

It wasn't an expedition in what you might think of as the normal sense. It wasn't a well-planned, completely integrated operation. It was a lot of ships from a lot of countries, everybody doing his own thing, telling each other what they were doing but not necessarily working together. A lot of independent entrepreneurs. This bothered the governments, I guess all the governments, but particularly the American government, because what they wanted was a plan, something definite, something concrete: so many lines of soundings in certain well-organized places with definite objectives. That's the way bureaucracies operate, at least in the engineering agencies of the American government.

Sharp: Well, understanding what it is that's going to be accomplished.

Revelle: But that was always kind of vague, and of course it should have been vague. How do you know what you're going to accomplish when you don't know what you're doing? You're setting out in an unknown area, you don't know what the problems are, you don't know what the difficulties are, you don't know what the results are going to be. That's the essence of science as opposed to engineering. We did it, I think, in the right way. We couldn't have done it any other way. But in any case I think it was the best way.

The International Indian Ocean Expedition [IIOE] went on for a long time. A lot of ships got there; we know more about the Indian Ocean now in some ways than any other ocean, particularly about the geology. We found all kinds of interesting things. The Indian Ocean is remarkable in many respects. For one thing it has the world's strongest ocean current, the Somali current, which flows off the east coast of Africa, about the size of the Gulf Stream.

Sharp: That wide?

Revelle: In terms of the amount of water transported. It's narrower than the Gulf Stream. But the interesting thing is it exists only for six months of the year. The other six months it turns around and goes in the opposite direction, because of the monsoon.

Revelle: It's the same way along the equator; you have an equatorial undercurrent like the Cromwell current in the Pacific, which exists only during the part of the year when they have the northeast monsoon.

You have remarkable phenomena like the "90-East" ridge, which is two thousand miles long, straight north and south along the 90th meridian. Nobody has any idea how it got there. Quite a remarkable topographic feature of the ocean.

You have quite complicated plate tectonics, because several plates join at a triangle in the southwestern part of the ocean. They slip past each other and they bump into each other and do all kinds of curious things.

You have the Indian subcontinent, which traveled four thousand miles from Antarctica to its present position, plowing right across the Indian Ocean.

You have the relationship with the monsoon.

You have some of the most fertile fisheries areas in the world.

Many, many strange and curious things about it, none of which were known before we had our expedition.

Sharp: And with that expedition there would have been these independent entrepreneur scientists working on bits of research attached to each of these topics.

Revelle: That's right, exactly. We had to produce some kind of a coordinated plan. I spent one of the most nightmarish months of my life doing that, Anton Bruun, George Deacon, and I, the three of us, in Copenhagen. We worked day after day at Charlottenburg, the castle that's the headquarters of the International Council of the Exploration of the Sea, trying to draw up this book, really, basically a justification and an outline of what could be done, more than a plan of who's going to do what, but all the things that needed to be done and all the ways that they could be accomplished.

We first had a meeting in Copenhagen, in the Royal Palace there, the government buildings, where we had about fifty people. Later in the summer George and Anton and I presented our plan at the meeting of the International Union of Geodesy and Geophysics in Helsinki. But between those two meetings we spent this nighmarish time at Charlottenburg, trying to work it out. It just seemed to go on forever; we just couldn't seem to get it finished.

Revelle: That was the year the Royal Society was celebrating its three hundredth anniversary, about 1962, I think it was. Maybe it was earlier than that. Maybe it was 1959 or '60. But George had to forego going to that; he wanted very much to go to it. He was, of course, a member of the Royal Society, instead of which he just sat there in Copenhagen working on this plan.

[tape interruption]

Revelle: After that initial planning stage was done the International Indian Ocean Expedition was taken over by Warren Wooster and his group in UNESCO as far as continued coordination and publication and things like that were concerned.

One of the big things that IOC and SCOR have been involved with has been publication of the general bathymetric chart of the oceans, which is a map of ocean bottom topography on a scale of one to ten million. Let's see, how much is that? That's one inch equals ten million inches or about one hundred and fifty miles, so twenty inches would be three thousand miles. That's about right, about one to ten million, something like that. The chart is a remarkable accomplishment. It's largely based on the work that's been done since World War II in all the oceans. Bob Fisher of the Scripps Institution was the principal compiler for the Indian Ocean.

Notes on the World Climate Research Program and the Law of the Sea Issues

Revelle: The IOC is now, in principle at least, playing an important role in the development of the World Climate Research program, the oceanographic part of it, particularly promoting installation of tide gauges and organizing bathythermograph programs, programs of lowering these expendable--so-called XBTs. That may work out as an important job for the IOC to do. Recently they sponsored a meeting in Paris for the Tropical Oceans and Global Atmosphere part of the World Climate Research program, the TOGA program, one of the two major oceanographic programs. I was the chairman of the organizating committee for that meeting. That was a fairly successful conference.

International oceanographic cooperation is, if anything, improving as a result of the World Climate Research program. A lot of people have real hopes for getting synoptic pictures of the ocean. There is another program called the World Ocean Circulation Experiment, which will be a very big three-dimensional program, including

Revelle: underwater observations as well as satellite and other surface observations of many kinds. Another major international effort is the study of oceanic aspects of the carbon dioxide problem; how does the carbon dioxide in the atmosphere increase and how does it interact with the ocean.

Sharp: Will the IOC play some role in that?

Revelle: It may, but we hope the governments will support the scientists. It may very well be that the ocean carbon dioxide program will be coordinated internationally by UNEP, which is the United Nations Environmental Program, plus IOC plus WMO. The Committee on Climate Changes in the Ocean is jointly sponsored by IOC and by SCOR.

I was pretty much out of the international cooperation in oceanography business from 1964 to 1978, when I was at Harvard. Forgot all about geophysics and talked a lot about population problems.

Sharp: When we talk about Harvard, one of the topics I hope we'll talk about is what--not now because we don't have the time--changes you made in terms of organizations and everything else so that the organizations you supported and belonged to matched the new interests that you were picking up at Harvard. What you dropped and what you kept in terms of organizations, and what new ones you got into. I thought we'd get into that.

Revelle: Well, I abandoned oceanography for those fifteen years pretty much entirely, one reason being that I never was convinced that the ocean's resources amounted to very much compared to the land resources. That's not entirely true, but it's certainly true right now. The total protein from the ocean that we get from eating fish and shellfish on a worldwide basis is only about 5 percent of our total protein consumption, or perhaps less than that, maybe 10 percent of the animal protein. That could change with the development of aquaculture, but it's certainly not going to change with the capture fisheries--the way in which we harvest the ocean's living resources now.

All during this time--I should make a caveat here--I was a member of the Committee on Ocean Policy of the National Research Council, and I was a member of the U.S. delegation to the Law of the Sea Conferences. So I did keep up to that extent, and that was quite a big extent.

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Revelle: I tried very hard to put across a position on oceanographic research for twenty-five years. That was the freedom for each nation to do research in other nation's continental shelves. This situation got worse and worse all during the Law of the Sea Conference. We had a proposed regime of what we called rights and obligations.

Sharp: I saw some material on that.

Revelle: What we meant by that was that any country could do research anywhere outside of territorial waters provided that they notified the coastal state, provided they allowed coastal state people to take part in the expedition, provided they shared all the data and all the samples with the coastal states. The coastal states never bought that; the United States was pretty much alone in that, the United States and West Germany and a couple of other loyal allies. The Netherlands was one of these. The British never bought it, the Canadians never bought it, the Australians never bought it, let alone the developing countries. So now we have the worst of both possible worlds. We have all these rights and obligations and at the same time we have to get the consent of the coastal state, and the coastal state can control publication. So it's a very bad outcome of the Law of the Sea Treaty.

That was the purpose of American oceanographers participating in the Law of the Sea Conference, trying to change this situation. We never were able to do it. There were quite a few people involved with this: Warren Wooster was involved, Johnny Knauss very much so, Paul Fye at Woods Hole, I from Scripps, Bill Nierenberg from Scripps, Tom Clingan from the University of Miami. They were the principal ones. And George Deacon, in the earlier days, from the United Kingdom.

One of the ways in which I tried to push this was through Elisabeth Mann Borgese's Pacem in Maribus conferences, where one of the principal actors was Lord Ritchie-Calder. She had at those conferences a lot of the diplomats from the Law of the Sea Conferences. Elliot Richardson was a tower of strength on our side, did everything he could, including organizing oceanographic trips for the delegates.

Sharp: But those didn't make too much difference?

Revelle: Nothing made a difference. We just lost consistently. This makes the IOC in principle much more important, because one of the provisions of the Law of the Sea Treaty is that in any agreed-upon

Revelle: international program, all the countries that agree to it have automatically given consent for the work to be done in their waters. How that's going to actually work in practice I don't know.

Sharp: But it pushes the IOC back into a position of power.

Revelle: Very much so, right in the center of the governments.

Sharp: I'd like for us to, if we talk again about international cooperation, push more into the present era than you have just done. You've given us a really good outline for the next time to get more detail on additional changes in international cooperation up to the present. We've already done quite a bit of that. I think I have a couple more questions about this--.

Revelle: I should just say that the TOGA [Tropical Oceans and Global Atmospheres] program, that part of the World Climate Research program, has elicited the cooperation of quite a few countries already: Japan, Australia, China, France, we hope India. The Indians are particularly stuffy about oceanographic work in their 200-mile exclusive economic zone. They just won't seem to allow it, period. They don't even seem to want to have tide gauges installed in important places in their coastal waters.

Sharp: Will things be different now?

Revelle: I don't think so. I'm saying that at the present time this is true, right now. We have a long way to go with the Indians, and yet they have a very important role in the monsoon area, which is one of the key climate areas of the world.

Sharp: I was particularly wondering about Mrs. Gandhi's passing and just what that will mean in terms of cooperation.

Revelle: She apparently got along pretty well with President [Ronald] Reagan, and they organized something called the Indo-U.S. Scientific Initiative. I'm involved with this to some extent as a member of the National Academy [of Sciences] committee which monitors the program. Six of us are members: William E. Gordon, Sheldon Siegal, Normal Borlaug, Franklin Long. It's a good group of people. We have an oversight responsibility to advise the two governments on how it's going.

One of the aspects of this Indo-U.S. Scientific Initiative, as it was called, is the study of the monsoon. You can't study the monsoon without studying the ocean; at least that's what we Americans think, and most monsoon people think so too. Not all of them, not the Indians; the Indian Meteorological Service thinks you can do

Revelle: everything by doing just meteorology. I might say I think they are very backward. But what they're trying to get out of us is a Cray computer to make better models of the monsoon, not a gift but just the right to buy one. That's a slight leverage we have over them. But they are very tough to do business with.

However, the monsoon program seems to have a lot of support in a lot of places. The reason is that there's a real hope of forecasting climate for one or two years in advance, that is, climatic variations from season to season and maybe from year to year, particularly the so-called El Niño phenomenon, which is a worldwide event. It's not just in the eastern tropical Pacific. It seems to be intimately related to the monsoon.

I guess I have some hope that through the World Climate Research program we'll have a real handle on making forward steps in good international scientific work on the currents and the motions in the ocean and the exchange with the atmosphere. Not with the biology very much. The biology comes into the carbon dioxide problem.

Sharp: Might that be addressed in this new carbon dioxide--?

Revelle: Yes, I hope so. One of the interesting outcomes of the IGY was the beginning of monitoring of atmospheric carbon dioxide, which was started as an IGY project essentially by the United States and mainly by me. It has been done ever since 1957 by Charles David Keeling at Scripps.

He has carried out one of the most beautiful and important sets of geochemical measurements ever made, a beautiful record from 1958 on for the last twenty-five years, which shows that atmospheric CO_2 has increased by about 8 percent during that time, about 25 parts per million, from 315 to 340 parts per million.

Now this has become an international program with all kinds of people all over the world talking about carbon dioxide. It was just an embryo program when we started here at Scripps. Everybody talks about carbon dioxide now, and a lot of people are doing something about it in terms of making measurements and writing papers.

Sharp: Looking over, I think I have my questions answered for the time being. Once I do a little more reading of some of the papers you still have at the archives in this later period that involved cooperation on some of these later international projects, I might

Sharp: ask a few more questions. I'd rather not have us go on any more until I get a chance to do some more reading, get some more background on some of this stuff that we've been talking about.

Revelle: We've covered several things today, again sort of in a stream of consciousness way, but maybe that's not bad.

Sharp: No, it isn't, with some direction and questions from me, I think. These different organizations are just hard to talk about because they lead from one to the next, and the topics lead from one to the next.

Enthusiasm for the International Oceanographic Congress, 1959

Revelle: One of the important things that I was involved with which we haven't discussed--we might just say a word about this before we stop--was the International Oceanographic Congress in 1959. That was originally thought of by Dael Wolfle of the American Association for the Advancement of Science. He organized an American committee with Mary Sears as chairman, who couldn't have been a better choice, and with Gustaf Arrhenius and me and several other people whose names I don't remember, as members of the committee. We enlisted the cooperation of SCOR and IACOMS and UNESCO and held it at the United Nations in New York. It wasn't the IOC; the IOC didn't even exist then. This was 1959. It was the cooperation of the Division of Marine Sciences, Warren Wooster's group in UNESCO. UNESCO managed to arrange for us to meet in the United Nations building. Our plenary sessions were held in the U.N. General Assembly Hall. We used most of the conference rooms for our various simultaneous sessions. We met for about a week there, and that was really quite an experience. I was the president of it and presided in the General Assembly Hall, which was a thrilling thing to do.

I remember that Vladimir Kort brought the _Academician Lomanosov_ to New York for this meeting. The Soviets gave a reception onboard the ship. Kort and I somehow got into a vodka drinking contest; we were drinking vodka in champagne glasses, big champagne glasses.

Sharp: How did you do?

Revelle: Well, we did all right. I stayed on my feet, and so did he. Several other people didn't. He was a great bear of a man, about as big around as he was tall, and he was not short. He was a huge guy. Very much of a seagoing oceanographer, not much of a theoretical oceanographer.

Revelle: The most interesting thing about the congress was that a thousand people turned up. We had no idea there were so many oceanographers in the world. If we'd had it twenty years before there would have been at most fifty people, all you could get in the world. It was an enormous difference. That was just pure science, just people giving papers. We had a series of commissioned papers by, among other people, Walter Munk, Milton Bramlette, and Anton Bruun.

Unfortunately, Anton wasn't able to give his paper. He was the first leader of our Naga Expedition, and he came back from that first summer very sick. When he arrived in New York he was quite sick, and they put him in the hospital. He stayed in the hospital in New York for a month or so. The American doctors thought he had cancer of the liver; they had never seen a case of amoebic dysentery. He picked it up in Thailand. It was amazing that the New York Hospital--you know, it's one of the world's leading hospitals--just completely misdiagnosed it.

But there were a whole bunch of other guys who did present commissioned papers, and very good ones. That was pretty much pure science--they addressed the questions of where did oceanography stand? What was interesting about it was it was such a revelation of how much had been accomplished in the previous ten years. Even so we had still not even thought about plate tectonics--hypotheses did not appear until the middle of the 1960s. But a lot of other things had been done. Mary [Sears] published a book about the congress, the papers of the International Oceanographic Congress.

Planning for the Intergovernmental Oceanographic Commission in 1960 and Its Relationship to UNESCO

Revelle: There was another week that Kort and I and John Lyman spent together, and George Deacon, in Paris in 1960. What we were trying to do was to plan the Intergovernmental Oceanographic Commission and how it would work and how it would function: the voting procedures, the organizational constitution, what it would do, what would be the rights and obligations of the different member states and so forth.*

Sharp: Did you all have pretty different ideas of what you wanted?

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*Interested readers may wish to see "The Intergovernmental Oceanographic Commission of UNESCO: Its Capacity to Implement the International Decade of Ocean Exploration," Margaret E. Galey, Ph.D. dissertation, University of Pennsylvania, 1970.

Revelle: Yes, we did have somewhat different ideas. I don't remember what the differences were. But the interesting thing about it was that we'd hammer all day on Kort, and we'd finally come to an agreement by, say, five o'clock or six o'clock in the afternoon. The next morning the agreement had come completely unstuck, and we had to start all over again.

Sharp: People had too much time to think about it.

Revelle: He didn't think about it, but he called Moscow every night, I guess. Every morning we were right back where we started, for a whole week.

Sharp: The main reason that I didn't include any questions on the IOC is that I had thought there had been so much written about it.

Revelle: But nobody has ever talked about this week that we spent together in Paris. There was just four of us, Deacon and I and John Lyman and Kort, plus, I guess, some of the secretariat from UNESCO. But I don't remember them. I don't even remember what we disagreed about, but I do remember that we had this both amusing and frustrating experience of having to start from scratch every morning; every day we'd have to do it all over again. [laughs]

Sharp: In terms of the IOC and the plans that were made for it at this meeting, what were some of the most important decisions that you remember being made about the direction of the IOC, or where you would go for support or what the range was?

Revelle: Let me just think a minute about that. As I said, I don't remember why we disagreed. The things that you would disagree about would be the composition of the secretariat, who would choose the secretariat. The second problem was how it would be supported; where would the money come from. The third would be its functions, how much data would be exchanged, how much would be revealed and publicized about different international oceanographic efforts, who would be members.

Sharp: That exclusiveness that you mentioned before.

Revelle: Who would vote or how the decisions would be made. Those questions were settled in the following way. As I said, all countries who wanted to do cooperative work in oceanography could be members. The members would be countries, not individual scientists. That was necessary because you needed a governmental mechanism to complement SCOR, the scientists' mechanism. I think that we decided that the members did not have to be members of UNESCO. I'm not quite

Revelle: sure about that; but I'm pretty sure that that was the case. Membership was independent of membership in UNESCO. Each country would have one vote; we had to follow the U.N. procedures there.

The secretary of the IOC would be selected by the General Assembly of the IOC and not by the Director General at UNESCO.

Sharp: Keeping it pretty independent.

Revelle: We tried to keep it as independent from UNESCO as possible and still get the money out of UNESCO, because the governments were not prepared to set up a separate international organization. I wish they had been, and it would have been much better if they had. But they just wouldn't go for it. We wanted a WOO like WMO, World Oceanographic Organization; we had to settle for this organization within UNESCO. One of my purposes for being a member of the U.S. delegation to UNESCO was to lay the groundwork for this organization at the previous general conferences of UNESCO. So, UNESCO called a conference in 1960 or '61, I think it was '61, in Copenhagen, to organize the IOC, the founding conference of IOC. This meeting in Paris between the four of us was a preparatory meeting for that organizing conference. The latter had been called by the previous general conference of UNESCO. I was a U.S. delegate for two or three general conferences working on this idea, getting UNESCO to agree to it.

So it had been agreed by the general conference that there should be an organizing conference for the proposed IUC. This was the preparatory meeting for the organizing conference. I think the actual establishment of the IUC had to be approved, after the proposals were made at Copenhagen, by the general conference of UNESCO. At Copenhagen, Jim Wakelin, our assistant secretary of the navy, was the principal American delegate. A man named Federov was the principal Russian delegate. We always called him Big Federov, even though he was only about five feet two inches tall. He was a very bulky man. There was a much smaller Constantine Federov who was an oceanographer, called Cookie Federov, who later became secretary of the IOC and head of the Oceanographic Division of UNESCO, a perfectly respectable oceanographer. He was quite effective in those jobs after Warren Wooster left.

So I remember the principal issue was the choosing of the secretary, independent of UNESCO, by the General Assembly of the IOC itself; the secretary could then choose the staff.

Revelle: The decision-making process, through a general assembly of all the members was one of the problems, as opposed to an executive board. In fact, however, there is an executive committee too, the EC as they call it.

Sharp: Was that decided on at this point or did that come later on?

Revelle: We decided all these things at that meeting in Paris, and then they were adopted, with a good deal of argument, at the meeting in Copenhagen, where people who were much higher in the hierarchy than scientists were the heads of delegations, like the assistant secretary of the navy.

Sharp: Wakelin?

Revelle: Yes. Who was, by the way, one of those guys on Admiral Furer's staff I told you about yesterday and a very good friend of mine. We agreed about everything. We had no problem.

Sharp: He was in a good position for you then.

Revelle: Oh, of course. Sure. Many things work in life through the "old boy" network. It doesn't seem that there should have been any very serious issue at this preliminary meeting in Paris that we had with Kort, but for some reason we argued about everything. And I don't remember what we argued about.

Sharp: What sticks in your mind is his own kind of decision making that had to do with what he was hearing from--.

Revelle: Well, he didn't make any decisions. That's the problem. They were all made for him in Moscow, unlike George Deacon and John Lyman and me, we were on our own. Nobody was instructing us. That's in some way the difference between Americans and Englishmen and Russians. We know so much about what's feasible in our own countries that we don't have to be instructed.

Sharp: But with the Russians--.

Revelle: They don't really know.

Sharp: It's not revealed.

Revelle: That's right.

Sharp: Did you ever have any sense that he minded working that way? I mean he could obviously see that you and Deacon were not working the same way. You weren't making long-distance phone calls to get some sort of approval of anything. Did you ever talk about that with him?

Revelle: We never did.

Sharp: That wasn't something that was possible.

Revelle: No, not really. In fact I'm not even sure he made phone calls; I just think he made phone calls, because as I say he was always right back where he started from the next morning.

Sharp: Did he speak to you in English or was an interpreter used?

Revelle: He spoke good English. No problem. He wrote an article in the Scientific American, I remember. At that time the Scientific American paid $1,000 for every article regardless of who wrote it or how good or how bad it was. Standard fee. We had an IUGG meeting in Berkeley in 1964.

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Revelle: Ellen and I met Kort and his companions at the airport. The first thing he said when he got off the plane was, "Where's my $1,000?" [laughs] (For his article for the Scientific American.) So I got in touch with Jerry Piel that afternoon and they had a check out at Berkeley the next day or the day after.

Sharp: Trying to keep relations--.

Revelle: They were obligated to pay it, and he needed the money. He couldn't take much money out of Russia. So that $1,000 meant a lot to him. He has pretty much disappeared from the hierarchy that I see nowadays in Russia. He got replaced by Monin as director of the Institute of Oceanology. I don't quite know what he's doing. I haven't gone out there to the institute lately. No reason I shouldn't, but I just haven't. I've been in Moscow several times since those days but never for the purpose of visiting the institute. He's no longer part of their oceanographic bureaucracy, though.

Sharp: So he might still be working as a scientist.

Revelle: I think he is, yes, leading expeditions I think.

Final Comments on Early Post-War International Cooperation and Interest in Oceanography

Revelle: I should have said something sooner about the International Oceanographic Congress. The main thing about the Oceanographic Congress from my perspective was the unexpectedly large attendance, the quality and diversity of the papers, and the number of countries that were involved.

Oceanography had been transformed between the end of World War II and 1959.

Sharp: When Sverdrup was writing that article, even in '47 he was saying how governments and independent scientific organizations were beginning to see how important oceanography was and were beginning to support it. He certainly was right.

Revelle: He was absolutely right about that.

Sharp: And ten years later, fifteen years later, the world oceanographic effort has doubled and tripled in size, in terms of money that is given to support it as well as organizational structures that were devoting themselves to exchanging information, coordinating projects.

Revelle: One simple measure is the oceanographic ships. Poor Harald, all during his directorship, had to get by with one converted schooner, the E.W. Scripps, which wasn't much of a ship. By the time I left we had twelve ships, none of them huge. Now we have four but they're much bigger, more total tonnage than we had before.

Sharp: But the Oceanographic Congress and the variety of papers and interests that you saw represented there, that was still a surprise to you that it had grown so much?

Revelle: Very much so, yes. It certainly was. It had a good feeling too, a feeling of optimism. Those were the days when everybody was gung-ho about science.

Sharp: And the money was going pretty well.

Revelle: Yes. And, of course, from the standpoint of the United States the Russian Sputnik had aroused tremendous interest in science here.

Sharp: When we talk further about national science policy there are quite a few of your papers that show that post-Sputnik influence in terms of pushing money for scientific education. You see a lot of graphs of numbers of Americans involved in scientific research and education as well as professionals in terms of industry involved in science. The same graph for the Soviets, their numbers are higher always. So there's this obvious competition after Sputnik as part of the cold war and the rest of that. So we'll talk quite a bit about that.

Revelle: The typical difference between the Russians and ourselves was in their ships. A Russian ship typically was a ten thousand ton usually converted passenger ship or freighter, passenger ship usually. I guess maybe they built some for oceanography, but they were built like passenger ships pretty much.

Sharp: Pretty bulky?

Revelle: Ten thousand tons is a big ship. Nothing like the modern oil tankers, of course, which displace several hundred thousand tons. The E.W. Scripps was about a hundred tons, for example. The Soviets have what they call complex expeditions. They have a dozen winches and a hundred scientists on each one of these expeditions of theirs. We had ships like the Horizon and the Spencer F. Baird, which were seagoing tugs, about five hundred to maybe fifteen hundred tons, very much smaller, with a smaller crew, and a much smaller number of scientists. All the scientists had plenty of opportunity to do what they wanted to do. You planned so that nobody was held up by anybody else.

In the Russian case, they're constantly fighting to get their wires over the side and keep them from getting tangled with the other wires that are over the side.

Sharp: Why is that there is that--?

Revelle: That's just the way they do things. They do everything in a big way, but not necessarily a very good way. Their nuclear weapons are bigger than ours, but they're not very accurate and so forth.

Sharp: It gets me back to the salinity measurements. That is a good way of generalizing, perhaps, about some of their approaches; they're not as accurate. Their standards and expectations are not the same as what Americans, at least, want to work with as a cooperative enterprise.

Revelle: That's right. That's in fact also true of the other major oceanographic nations. The French are coming up very fast. The Germans always have been precise and good. The Japanese are pretty good also; they do a tremendous amount of oceanographic work. And it's easy to cooperate with them, because you can trust the data if they give it to you.

Sharp: You'd think for cooperation that the level of trust, one scientist really trusting that they could use the other data--.

Revelle: Yes. It's not that it's secret, it's just because it isn't very good.

Sharp: You think we're done for today?

Revelle: I guess so. I'm sort of unwound. I haven't really much more to say that I can think of.

TAPE GUIDE - Roger R. Revelle

Sarah Lee Sharp

B.A., University of California, San Diego, 1971, with major in history.

M.A., University of California, San Diego, 1975, with major field in United States history; Teaching Assistant in Comparative Americas, 1972-1975.

Ph.D., University of California, San Diego, 1979, with major field in United States history; dissertation entitled, "Social Criticism in California During the Gilded Age."

Interviewer-Editor for Regional Oral History Office, 1978 - 1986, specializing in California political and legal history.