Archives, Scripps Institution of Oceanography
University of California, San Diego
La Jolla, CA 92093

University of California
Berkeley, California
Regional Oral History Office

Oceanography, Population Resources and the World

Roger Randall Dougan Revelle
DIRECTOR OF SCRIPPS INSTITUTION OF OCEANOGRAPHY,
1951-1964

Interviews Conducted by
Sarah L. Sharp
in 1985

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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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INTERVIEW HISTORY

INTERVIEW HISTORY

When Roger Revelle returned to Scripps Institution of Oceanography (SIO) in La Jolla, California in 1948 as a civilian, after seven years in the navy stationed in San Diego, as well as at the Bureau of Ships and the Office of Naval Research in Washington, D.C., he did so with a renewed commitment to basic scientific research and a new desire to see SIO expand as a fully oceanographic institution. He was promoted to professor of oceanography and then to the position of SIO's associate director very soon after his return. Within three years, in 1951, he became the institution's director, remaining in this leadership role until 1964 when he left to head Harvard University's new Center for Population Studies.

Transcripts of two oral history interviews, conducted on 13 and 14 August 1985 with Dr. Revelle, and edited both by the interviewer and interviewee, are included in this volume. Revelle candidly relates his role in opposition to the University of California's loyalty oath, as this issue erupted among the university's professors later in 1948, only months after his return from Washington, D.C. As this storm passed, SIO's triumphant Midpac expedition followed in 1950, helping to ensure Revelle's appointment as permanent director. As the interview continues, Revelle discusses the course of study which developed at SIO and the training of graduate students. The interviewee also comments on the establishment of the Institute for Marine Resources which the interviewer selected as an example of Revelle's efforts to see SIO expand the scope of its activities. The interviewee's appointment as science advisor to the United States Department of the Interior in 1961 forms another chapter in this scientist's efforts at domestic and international scientific cooperation. Finally, Revelle comments on his role in university-wide administration at the University of California in the early 1960s, and the evolution of his scientific interests which partially led to his appointment at Harvard.

In order to prepare sufficiently for these sessions, the interviewer-editor conducted research on several levels: examination of the Roger Randall Dougan Revelle Papers which have been collected at the SIO Archives in La Jolla, California; 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 and comment.

The significant contributions to oceanography which Dr. Revelle has made came to the attention of the Regional Oral History Office through Professor Harry N. Scheiber of the Law and Society Program 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.

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

Fall 1988
Department of History
Bowling Green State University
Bowling Green, Ohio 43403

I "AN IMPORTANT TURNING POINT": CHALLENGING THE UNIVERSITY OF CALIFORNIA'S LOYALTY OATH, 1948-1950

[Date of Interview: 13 August 1985]##

Sharp: I think a good opening might be for you to just talk a little bit about some of the work that you were doing when you first came back to Scripps. The last time we met we talked about your reasons for wanting to come back and your work with the navy ending in 1948.

Revelle: Oh, I did talk about the last years with the navy, then?

Sharp: Yes, you did.

Revelle: Well, when I first came back I was associate director. Carl Eckart had been appointed director. I guess I told you the reason for that was that there was a lot of opposition to my being director. There was sort of an arrangement that Harald Sverdrup had made with Carl to be an interim director, hoping that the antagonism to me would die down. He had no intention of taking it for more than a minimum period, although it wasn't quite clear what that period was.

It turned out to be a difficult job for him. He was so precise and so painstaking and thorough that he was really unhappy on the job, quite

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##This symbol indicates that a tape or a segment of a tape has begun or ended. For a guide to the tapes see page 123.

Revelle: unhappy. You found a paper that he had written on all the problems of the Scripps Institution, which shows you the way his mind worked, very carefully and very thoroughly, but it's probably not the way to run the Scripps Institution of Oceanography. [laughing] It wasn't a very businesslike organization or a very formalized organization.

I remember one thing in particular. He was very unhappy with the condition of the buildings and grounds. He thought they were dirty. And they were. He practically had a tantrum trying to get the buildings and grounds people to clean it up. He lost his temper. Well, it was a good idea to clean it up, but it wasn't an overwhelmingly important operation.

Sharp: With your coming back as associate director, were there some of those areas that you were expected to help him with then?

Revelle: Oh, yes. I was supposed to be responsible for the work at sea. That was my primary job. At that time the work at sea was mainly the Marine Life Research program. It wasn't until later that we started our great exploring expeditions. I guess I was also responsible for relationships with Washington, D.C.

We had two Washington sources of funds. One was the Bureau of Ships and the other was the Office of Naval Research. The Office of Naval Research had made the kind of grant which I've always thought was exactly appropriate; namely, an institutional grant of $125,000 a year or something like that, which was really under the control of the people here in La Jolla.

Revelle: The Bureau of Ships project had two or three parts to it. One was support of the Marine Physical Laboratory. I'm not sure whether or not I told you about the Marine Physical Laboratory?

Sharp: Not really, and I thought we might talk about that.

Revelle: That was an idea Carl and I had while I was still in Washington, still in the Bureau of Ships. It was a proposal to continue after the war the kind of work that had been done at the Navy Radio and Sound Laboratory by the University of California Division of War Research, and particularly the fundamental research in underwater acoustics.

Revelle: The navy had never supported on a permanent basis any non-navy activity, so this was quite an innovative program. Lyman Spitzer and I drew up a letter, Lyman Spitzer being the famous astronomer at Princeton, but he and I worked together at that time. [brief tape interruption after phone rings] Lyman was with Division 6 of the NDRC, the National Defense Research Committee and I was in the Underwater Sound Design section of the Bureau of Ships.

Our boss was a man named Jack Meyers, Commander Jacob Christian Meyers, a very nice, shy man. His boss was Captain Rawson Bennett, who later became an admiral and chief of Naval Research. His boss was a man named Admiral Ned Cochrane, who was the chief of the Bureau of Ships. He was practically the same as God to laboratories and activities all over the country and the world.

Lyman and I wrote a letter for Admiral Cochrane's signature saying that the Bureau of Ships would support the University of California, if they established the Marine Physical Laboratory, on an indefinite basis, not just for a year or six months, but forever. Nobody thought, of course, that the Bureau of Ships would itself go out of business after a while when the navy was reorganized. Admiral Cochrane sat on this letter for about four months, trying to make up his mind whether to sign it or not, but he finally did sign it, with a little pressure from Carl, who threatened to go back to the University of Chicago if it didn't get signed pretty quickly!

So the Bureau then made a contract with the University of California to establish a Marine Physical Laboratory at Pt. Loma on the grounds of the Navy Electronics Laboratory in San Diego. Carl arranged the appointment of Russell Raitt and Leonard Liberman as Associate Professors. Leonard Lieberman came from Woods Hole, but he had been a doctoral student under Carl in the University of Chicago. Russell Rait had been here at UCDWR [University of California Division of War Research]. He also appointed Finn Outler, who was an ex-warrant officer and a great expediter, and several other people. He gradually built up a staff. That was in 1946, so by the time I came back here the Marine Physical Laboratory was going very well.

Revelle: The Bureau of Ships also supported some work on bathythermograph processing and various other things here in La Jolla. I've forgotten exactly what they were. I think one of their projects was on waves and currents.

I notice that in the budget that we had a couple of years later, the Bureau of Ships put in $420,000 and ONR [Office of Naval Research] put in $223,000. That was in 1950-51, by the time I'd become acting director. So the navy must have increased its support quite significantly, to $643,000.

Sharp: You were also doing some work on a sardine project when you came back, weren't you?

Revelle: That was the principal work at sea. We had three ships, Horizon, Crest, and E.W. Scripps, as I remember it, all working on the MLR [Marine Life Research] project.

Anyhow, after two agonizing years, Carl quit. He decided he just couldn't take it anymore. So I became acting director. That was sometime in early 1950.

Sharp: I think it was January or February.

Revelle: It was maybe January of 1950.

Sharp: It was pretty early, yes.

That was just about the time that the loyalty oath matter was heating up.

Revelle: The loyalty oath appeared in February, as I remember it, February or March.

By that time I was acting like a director, although I really wasn't. I took the lead here in La Jolla in opposition to the loyalty oath and urged our faculty not to sign it, and I didn't sign it. Some of the faculty signed it, but most of them didn't.

Sharp: Let me ask you, to begin with, why did you decide to get involved in the loyalty oath matter?

Revelle: Well, because I thought it was a very serious violation of the principle of academic tenure, which I felt was at the heart of a university, still do, that without genuine tenure the

Revelle: university becomes just a business. The academic senate at both UCLA and Berkeley took essentially the same position.

Sharp: I think, though, the statement that they had worked out, the one--

Revelle: Well, that was the statement several months before the loyalty oath came up, the statement that you're thinking about.

Sharp: Right, okay, yes.

Revelle: That was in 1949, I think.

Sharp: Right, yes. Both the southern section and the northern section of the senate were very much in agreement about the issue of the loyalty oath.

Revelle: Let me just take a took at that because the key to it is the last sentence. [reading from 1949 statement] "The members of the Senate request the privilege of affirming their loyalty to the principles of free constitutional government by subscribing voluntarily to the oath of loyalty sworn by officers of public trust in the State of California."*

Apparently even at that time, several months before, there was a movement to have a special negative oath. I don't quite remember how that worked, but by February the regents had actually imposed as a requirement that all faculty members should take an oath saying they were not Communists or else they'd be fired. The "or else" part was what I objected to, very much.

Sharp: It looked like there was quite a bit of work done on the La Jolla campus, different committees were set up. I found this list of committee assignments after I mailed these materials to you, so I didn't get a chance to send this one down.** Let me give it to you now. It was interesting that Ellen Revelle had worked on it, and some of the other wives had worked on it.

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*See statement dated 23 September 1949 on pages 5a and 5b.

**See memorandum dated 10 March 1950 on pages 5c and 5d.

23 September 1949

RESOLUTION OF THE ACADEMIC SENATE, SOUTHERN SECTION
September 22, 1949

The members of the Academic Senate, Southern Section, here assembled, concur wholeheartedly in the proposition of the Board of Regents that the University of California is dedicated to the search for truth and its full exposition. They firmly support the intention of the Regents to stand steadfastly for that freedom of the human mind and spirit on which this dedication rests.

This freedom, which is the epitome of scholarship and the cornerstone of the University, is safeguarded by the State and Federal Constitutions. The faculty of the University has given assurance that it supports these constitutions and the principles they guarantee by swearing the oath of allegiance prescribed by the State Constitution. Each member has already affirmed by this act his knowledge of and faith in the traditions of our society and ideals of our culture.

These ideals are inseparable from the essence of scholarship. A scholar who accepts an authoritarian doctorine, who clings to dogma in contempt of evidence and the free inquiring mind, destroys his competence in that field of inquiry and impairs his probity as a scholar.

The faculty of the University is deeply concerned with the integrity of its scholarship. It believes that the best judge of the competence and probity of its members is the community of scholars itself. The record of integrity of the University for four generations and its place of leadership among scholars refutes any charge that it has failed in its judgment.

The Southern Section of the Academic Senate takes the view that the affirmation of the oath prescribed by the State Constitution and the vigilance of the faculty to preserve and enhance the reputation of the University is a more effective safeguard of academic freedom and objective teaching than the proscription of any particular dogma.

The faculties assembled in the Southern Section of the Senate concur in the policy set forth in University Regulation No. 5. They also believe that the University should prohibit employment of any person whose commitments or obligations, Communist or other, demonstrably prevent objective teaching and the free pursuit of truth.

The members of the Senate request the privilege of affirming their loyalty to the principles of free constitutional government by subscribing voluntarily to the oath of loyalty sworn by officers of public trust in the State of California.

23 September 1949

RESOLUTION OF THE ACADEMIC SENATE, NORTHERN SECTION
September 19, 1949

The faculties assembled in the Senate, Northern Section, wholeheartedly concur in the policy, stated in University Regulation No. 5, which prohibits the employment of persons whose commitments or obligations to any organization, Communist or other, prejudice impartial scholarship and the free pursuit of truth.

The members of the Senate request the privilege of affirming their loyalty to the principles of free constitutional government by subscribing voluntarily to the oath of loyalty sworn by officers of public trust in the State of California.

10 March 1950

MEMORANDUM OF CONFERENCE ON LA JOLLA CAMPUS ACTION TO PROTEST THE FEBRUARY 24, 1950, DECISION OF THE REGENTS

In accordance with a resolution passed by the assembled faculties of the Southern Campuses of Riverside, Santa Barbara, Los Angeles and La Jolla, at their meeting on March 8, 1950, a tentative action committee to work parallel with the Los Angeles Faculty Committee was organized on Thursday evening, March 9. The following tentative decisions and subcommittees assignments were made.

Name of Committee: Scripps Institution Committee for Academic Freedom.

2. Organizations Contfact Committee: G. C. Ewing, James Rusk, Ellen Revelle, George F. McEwen, Robert S. Dietz, Russell W. Raitt. Will arrange speaking dates and urge action by La Jolla and San Diego organizations and their individual members.

3. School and Parent Teachers Association Committee: Denis L. Fox, Kitty LaFond, Mrs. J. Frautschy, Frank Coleman, James Rusk. Will urge individual and group action by Teachers and Parent Teachers Association.

4. State College Committee: D. L. Inman, E. F. Corcoran, W. McBlair. Will urge individual and group action by Faculty, Students and Alumni of San Diego State College.

5. California Alumni Committee: Claude E. ZoBell (not yet informed). Will urge individual and group action by California and UCLA Alumni in the San Diego area.

6. Claremont College Committee: W. S. Butcher, Jean Sullivan, Mary Ann Lodge, Dana Johnson. Will urge action by Pomona, Scripps, Claremont Mens College and Claremont College Faculty, Students and Alumni.

7. Publicity and Radio Committee: Helen Raitt, Preston Justice, Martha Munk, W. S. Butcher, David Poole, Bob Arthur, Ellen Revelle. Will arrange for press conferences, publicty releases, and radio discussions.

8. Committee on preparation of communications and other materials: Warren Wooster, Ed Goldberg, Martha Munk, John Cochrane. Will prepare material to be incorporated in resolutions and letters by other organizations and individuals.

9. Committee on Contracts with other Universities: Norris Rakestraw, Ed Goldberg. Will attempt to get faculty members and other persons from universities outside California to write President Sproul and Governor Warren.

10. Membership and Finance Committee: Melvin Wattenberg, D. L. Inman, Warren Wooster.

11. Policy and Coordinating Committee: Norris Rakestraw, Marston Sargent, Warren Wooster.

Persons willing to accept the assignment as speakers before organizations: G. C. Ewing, Marston Sargent, Walter Munk, Norris Rakestraw, Roger Revelle.

Action to be requested of all friends of the faculty's position:

a. Telegraph or write to Governor Warren, endorsing his public statement supporting the faculty's position.

b. Write to President Sproul ~~urging him to take a public stand in favor of the Faculty's position~~ stet.

c. Write to the Secretary of the Regents, University of California, Berkeley 4, protesting the Regents' ultimatum.

d. Two hundred mimeographed copies of statements of the Academic Senate, the Regents' statement to the press, and all available materials including letters already prepared by citizens of La Jolla, will be prepared and distributed by the Policy and Coordinating Committee.

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THE CHRISTIAN SCIENCE MONITOR, BOSTON, THURSDAY, MARCH 23, 1950

University of California Seeks Loyalty Compromise

By Robert R. Brunn

Staff Correspondent of The Christian Science Monitor

Berkeley, Calif.

A ban on employment of Communists on the faculty—but not through special loyalty oaths—is the University of California faculty's recommended solution to the nationwide academic freedom controversy.

And if the faculty suggestion settles the issue here, it may set the pattern for similar compromises on the country's college campuses.

The University of California faculty voted 1,154 to 136 against the employment of any "person whose commitments or obligations to any organization, Communist or otherwise, prejudice impartial scholarship and the free pursuit of truth."

Reject Loyalty Edict

At the same time, the faculty approved another resolution, by a 1,025 to 268 tally, to reject a special non-Communist oath ordered by the Board of Regents on a basis of sign by April 30 or quit.

The second resolution, however, backed the traditional constitutional oath, plus an agreement to sign letters of contract specifying the understanding that the university policy is against employing Communists.

Edward A. Dickson, chairman of the Board of Regents, indicated that a compromise is impending in a statement issued after the faculty decision. He said:

"It is the first time that the faculty of any great university in the country has gone formally on record as supporting a policy of outlawing subversive teachers and influences."

The board previously voted 12 to 6 for the special oath. Dr. Robert Gordon Sproul, university president, and Gov. Earl Warren were among the oath opponents.

Resist Political Tinge

The vote, taken by mail, was preceded by a stimulating discussion of the fundamentals of academic freedom.

Beneath the surface of the compromise is a significant faculty division. This division does not affect a united stand against the Board of Regent's ultimatum, but nevertheless provides a cross section of thinking in an American university at midcentury.

It involves the men and women who teach in the broadly social field—political science, sociology, economics, history, and those in the know-how subjects such as engineering, the natural sciences generally, and agriculture.

Actually there is a community interest between the businessmen-farmer-banker Board of Regents and the faculty members who do the research and teaching that is mechanizing California agriculture, destroying insects, controlling smog over cities, and refining cost accounting procedures.

When the oath issue first arose a year ago before the ultimatum, many professors in the California ivory tower were dismayed to discover that it had been invaded long before the Regents oath decision. This was invasion by mechanistic subjects and natural sciences, and many oath nonsigners believe it is these men, generally, who "don't care" about the finer points of academic freedom, who signed the oath most willingly.

The professors on northern and southern California campuses have now pushed aside this division in the face of the larger challenge to them, of the Regent's "sign or else" ultimatum. For the ultimatum they have substituted their compromise vote acknowledgment that Communists cannot be objective teachers. This amounts to acceptance of the Regents anti-Communist policy as an employment condition.

From the time the oath was asked a year ago until the oath ultimatum last month, about 86 per cent of university employees had signed. But the flat-out demand made of the nonsigners brought an electrifying faculty revolt in defense of the nonsigning minority.

To teachers of engineering or economics, making mandatory a special oath over and above that required of all state employees was discriminatory, a completely ineffectual means of combating communism, a violation of established fundamentals of academic tenure and privilege.

Divided Opinion

On this compulsory level, an overwhelming majority of the faculty was against the oath. Before that as well, the academic senate had voted against the oath, mandatory or not, as a matter of principle—but individuals were willing to sign.

So, the dramatic faculty rejection of the oath (which featured press conferences, repudiation of the ultimatum by almost all deans and department heads, and wholesale threats of resignation) came only with the regents' ultimatum to a remaining 14 per cent of the university employees.

Some professors who did not sign the oath, and who object strenuously to these compromise resolutions, voted for them to keep the university from further harm. Others who support the special anti-Communist oath voted against the compromise.

Most nonsigners of the special oath object to acceptance of "guilt by association" implications and political tests for teachers.

It is held by the faculty purists that the voted compromise amounts to a defeat. They see guilt by association and political tests for teachers explicitly condoned in the provision that all future faculty letters of job acceptance acknowledge university policy against employing Communists.

See Surrender of Rights

Even though vehemently anti-Communist as ideologists, they also see this compromise a surrender of Constitutional rights in regard to political association—as long as the Communist Party is legal in the United States.

Objectors would have any individual case of biased and partial teaching judged on its own merits as in the past, and dealt with accordingly. Singling out the Communist Party, they say, ignores the fact that any commitments—religious, political, social—can prejudice impartial scholarship.

Brushing aside all this careful academic thinking, the outright ideological backers of the compromise say it gets down to the most pressing question of our time. They argue that the Communist educational system has been known to warp philosophy, history, aesthetics, literary criticism, economics, and even biology.

Revelle: Oh, sure. Many of our people did.

Sharp: But you divided yourselves up there on the different committes to do letter writing and all kinds of public information, letting people know what was going on.

Revelle: [reading] "Organizations Committee. Gifford Ewing, Jim Rusk" -- I don't remember him -- "Ellen Revelle, George McEwen, Robert S. Dietz, Russell W. Raitt. Will arrange speaking dates and urge action by La Jolla and San Diego organizations and their individual members." "School and Parent Teachers Association Committee. Denis L. Fox, Kitty LaFond, Mrs. Jeffrey Frautschy, Frank Coleman, James Rusk. Will urge individual and group action by Teachers and Parent Teachers Associations." "State College Committee. D.L. Inman, E.F. Corcoran, D. McBlair." "California Alumni Association. Claud E. Zobell (not yet informed)."

Sharp: He was going to be told what to do? He was going to be told what his assignment was?

Revelle: Yes, but he was unwilling to play any part in it. [reading] "Claremont Colleges Committee."

Sharp: So you were organizing some pressure to be put on the regents by some colleges that were not UC colleges? San Diego State, for example, and Claremont.

Revelle: That's right. San Diego State and the Claremont Colleges. Ellen was a trustee of Scripps College and a member of the first class of Scripps College. I went to Pomona.

[reading] "Committee on Contacts with other Universities. For work with universities outside California." "Committee on preparation of communications and other materials. Warren Wooster, Ed Goldberg, Martha Munk, John Cochrane. "Membership and Finance Committee." Melvin Wattenberg was our accountant at that time. "Policy and Coordinating Committee." "G.C. Ewing, Marston Sargent, Walter Munk, Norris Rakestraw, Roger Revelle" are "willing to accept assignments as speakers before organizations."

Sharp: And you did that, didn't you? Speaking to schools and--.

Revelle: I did a lot of it.

Sharp: How did you present the issue?

Revelle: Well, essentially just as I tell it to you now. We actually said several things, and it was more or less in line with that statement there, that it was a completely ineffective thing to begin with because Communists wouldn't mind signing it, that all it would do would be to drive good, loyal people away from the university, and it did, in fact. Indeed, that was the time of the great exodus to Harvard. Henry Rosovsky and Tom Schelling and Bob Dorfman and other people, all of whom became famous leaders of the Harvard faculty, another was David Landes.

Sharp: The people that you were talking to, like in the La Jolla Women's Club and Kiwanis, did they accept those arguments?

Revelle: Some of them did and some of them didn't. One of our converts--our most important La Jolla convert--was General "Howling Mad" [Holland M.] Smith.*

Sharp: He seems an unlikely convert.

Revelle: Well, he wasn't at all unlikely, if you knew him. He was a wonderful man. He was trained as a lawyer, he actually had a law degree from the University of Alabama before he got into the marines, so he had a very legally trained type mind. He thought that the Regents were just way off base. I remember one famous time that he told us about. He said, "A couple of the good ladies of La Jolla were talking to me at a cocktail party, and said, 'We don't understand why those professors aren't willing to say they aren't Communists.'" And General Smith said, "Madam, if somebody asked you to take an oath that you were not a prostitute, what would you do?" He said, "They never spoke to me again!" [laughing[

Sharp: Oh, I bet not.

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*See news article on following page, p. 7a San Diego Journal, March 30, 1950.

2—SAN DIEGO JOURNAL—Thursday, Mar. 30, 1950

Gen. Smith 'Howls' Over Loyalty Oath

Gen. Holland M. Smith, USMC (ret.) is "howling mad" again today—this time about the impending non-Communist loyalty oath demanded of California faculty by the regents.

In a fire-eating letter to Gov. Earl Warren, who also has opposed the oath, Smith adds his views to a group of 51 other prominent La Jollans who have protested by letter the regents' action of Feb. 24.

The regents ruled that the entire faculty—including the 15 members at La Jolla's Scripps Institution of Oceanography—sign the special oath by April 30 or consider themselves fired.

"The proof of the pudding of loyalty is in the eating, not in words or special oaths," Smith wrote. "The part of the University of California which I know most intimately is the Scripps Institution of Oceanography. The staff there is engaged in important and often hazardous war research.

"To disrupt their morale and destroy their self-confidence by an action such as that taken by the regents, may strike a serious blow at the security of the country.

"I am convinced that [illegible] faculty and [illegible] united in their opposition to Communism

See SMITH BLASTS ... Page 1

Smith Blasts Loyalty Oath

(Continued from Page 1)

but it is essential that any action in these matters should be agreed to by both groups. Otherwise the faculty is left without a voice in choosing and dismissing its own members."

Such a situation, he added, "—would be considered intolerable by physicians, lawyers and other professional men, and seems to me to violate the most elementary principles of fair play."

Smith endorsed a letter by La Jolla citizens to Dr. Robert Gordon Sproul, president of the university, which states in part that the special oath "—insults the intelligence and reflects upon the loyalty and integrity of faculty members, and cripples the freedom of academic thought which is essential to education in a free society."

According to Dr. Roger Revelle, acting director of Scripps, some regents have indicated a willingness to compromise on the oath. They meet tomorrow in Santa [illegible]

Revelle: He had a very gruff sort of voice. [imitates the voice]

Sharp: Yes. But he was pretty effective.

Revelle: He was, of course, a great war hero, one of the greatest. He was in contact with Governor [Earl] Warren and people in the California legislature. He really pushed it very hard, at the highest levels he could reach.

Sharp: I've seen one of the letters where you had thought he would make a good regent.

Revelle: Yes, he would have.

We had one navy regent already. That was Admiral Nimitz. He was wonderful. He became a great friend of the La Jolla campus, and we subsequently named our marine facility after him, the Nimitz Marine Facility. He was always very enthusiastic about Scripps, and he did everything that he could to push it.

There were several high points of the Loyalty Oath experience. One was, of course, I got to know the faculty at UCLA quite well, particularly David Saxon, who was an assistant professor at that time and really risking his job because he didn't have tenure; he was risking his job more than most people. Another one was Carl Epling, who was chairman of the faculty.

Sharp: Right, yes. I saw his name on quite a few letters you had been sending.

Revelle: We had this committee of the southern section, which I was on; I went to lots of meetings. One of the members of the committee of the northern section was Mike [Morrough] O'Brien, the dean of the College of Engineering. He was an old wartime friend of mine. He didn't quite understand what the problem was, but he thought he ought to stand up for the faculty and the faculty's position, and he did.

Another one was Frank Kidner, who later became vice president for relations with schools, or something like that.

They were pretty much the faculty leaders--both present and past chairmen of the senate and guys that had paid a lot of attention to the

Revelle: senate during their careers. Then there were people like Mike O'Brien who just got in on principle. So that part of it was a very happy experience. What was unhappy was I was jeopardizing my job because I really didn't have a job. I was just an acting director, although I did have tenure as a professor.

Sharp: There had not been a lot of support for you when you came back to be director in the first place, and I wondered, this wasn't going to make it any easier?

Revelle: No. [laughing] That's right.

Sharp: But that didn't matter?

Revelle: Well, I have some principles, and that is one of them, the principle of academic tenure. I was willing to go pretty far for that. Two of my friends, my two best friends, Karl Rodi and Rollin Eckis, came down to spend an evening with me, urging me not to play such an active part, saying I was jeopardizing my whole future, but I was sort of in a crusading mood by that time. [laughing] So I said I appreciated their advice, but I wasn't going to follow it.

Then the alumni organized a so-called alumni committee that Steve Bechtel was a member of. I think he was the chairman of the famous big contracting company, The Bechtel Company. Don McLaughlin, president of the Homestake Mining Company, was also a member; he was not a regent at that time. I was nominated by the faculty committee to testify before that committee, I thought because of my Navy experience when I had cheerfully taken a variety of oaths--probably also because I was an innocent scientist and at the same time big and probably impressive-looking. I've forgotten who else were members of that alumni committee, but they prepared something which was called the alumni compromise.

Sharp: I tried to find out something about that, but I didn't really understand.

Revelle: Well, you ought to look at a book by George Stewart called The Year of the Oath.

Sharp: When we met before you mentioned the alumni compromise just fleetingly. You thought that it

NORTH SHORES SENTINEL | THURSDAY, MARCH 16, 1950

Scripps Official Protests California Communist Ban

"Academic freedom, which has been an established principle for a thousand years but has had to be fought for at intervals, and tenure of office are the two major issues in the dispute between the regents of the University of California and the faculty," Dr. Roger Revelle, acting director of Scripps Oceanography, explained this week to the La Jolla unit of the League of Women Voters.

"Communism is not an issue in this dispute, since the regents in 1940 established the policy of permitting no Communists on the faculty and the Academic Senate, which is the faculty governing body, raised no objections," Dr. Revelle said. Nor do the professors object to the long honored, all inclusive constitutional oath taken by the President of the United States, all government office holders, civil servants, and state officials which also applies to the university faculty under the category of "government trust." The faculty members have already taken this oath and have volunteered to take it over again now.

"However, the Academic Senate, representing over 1600 faculty members, with only 10 dissenting votes in the southern section and complete unanimity in the northern section, considers being singled out for a special oath an indignity, the first step toward destroying academic freedom, and jeopardy to the 30-year established system of tenure which has contributed so much toward making the University of California one of the best and most democratic institutions of higher education in the world," he contended.

Sharp: was a very worthy compromise, but I wasn't sure why.

Revelle: The compromise basically was that all faculty members who refused to sign the oath would appear before a committee of the academic senate and explain why, and if the committee of the senate thought that they had a good reason, that would be the end of it.

Sharp: And their tenure would be safe?

Revelle: Yes, exactly.

If they didn't have a good reason then they might very well be dismissed, not on the basis that they wouldn't sign the oath but on the basis that they were suspected of being or maybe actually were Communists.

There were people on the faculty, and I must confess I had a lot of sympathy for them, but I didn't agree with them, who said that it didn't matter what a person's politics were as long as it didn't interfere with his fair-minded teaching and his research, that no political test should be applied. After all, the Communist party was a legal party.

But I felt that it was impossible for a Communist really to be a fair-minded teacher, at least in those days, and maybe still, because it's kind of a religion, it <u>is</u> a religion, and religion in general I think that people sacrifice their principles for.

Sharp: I can understand if you were teaching something like philosophy or even literature or certainly history--that would be kind of obvious--but if you were in the sciences I'm not sure what difference it would make.

Revelle: Probably not very much, except that scientists have ideas too about all sorts of things, they're usually very bright people; only a small number of them are so narrow-minded that they think about nothing except their object of research.

Anyhow, that was the compromise, and it was approved at a famous regents' meeting at UC Davis. That was another high point of this experience. Governor Warren presided at that meeting, and the vote was in favor of the alumni

Revelle: compromise. I remember Mario Giannini, the son of the founder of the Bank of America--there was always a Giannini on the board; it was sort of by right of inheritance--he said, "The flags will fly in the Kremlin tonight. I hereby resign from this Board of Regents, and I'm going to spend the rest of my life organizing vigilante groups to fight communism." I think those were almost his exact words.

Governor Warren said something I've never forgotten. He said, "Mario, you don't want to do that. This is a democracy, and it's your duty to stay here and persuade us that we're wrong, not to resign." Giannini didn't answer, but he did resign.

I took an oath of my own that I would vote for Warren, no matter what he ran for, from then on! But I never had a chance because before I knew it he'd become chief justice of the U.S. Supreme Court. He never ran again for office.

Sharp: Why do you think the regents accepted that compromise?

Revelle: Well, they were looking for a way out. They were way out on a limb. The problem was, from the internal politics of it, that it was really President Sproul's idea to have the oath in the first place. So far as I could make out, it was President Sproul's idea to prescribe this anti-communist oath, the so-called "loyalty oath."

Sharp: A special oath for university employees?

Revelle: That's right. He thought it was a way to get the heat off the university, and he couldn't have been more wrong. He quickly changed his mind and became a leader of the faculty side, but by that time he'd lost the confidence of the regents, and he never regained their confidence.

Sharp: Let me back you up. Why was there heat on the university to begin with?

Revelle: Because, you remember, those were the McCarthy days, and he was persecuting <u>every</u> group that wasn't strictly conformist, and of course the university is not strictly conformist.

Revelle: I had a later experience when we were organizing UCSD.* I made a little speech at a public meeting on the Scripps campus in which I said that "the university is a place where you have to have all kinds of ideas and all kinds of people." Jim Archer, the leading alumnus down here in San Diego, got up and said, "This university is not going to have any radicals on it at all. I disagree completely with Dr. Revelle. We've got to keep it pure", I'm not quoting him, but essentially he said, keep it safe for Republicans. [laughter]##

Revelle: I was in really quite hot water about that. The San Diego Union reported that I walked out on Archer. I did not walk out on him. I'd said before the meeting began I could only stay a few minutes because I had another engagement, so I had to get up for my other engagement, but the Union thought it was more dramatic to say I'd walked out. They never apologized for that.

The Union was owned by a man named Jim Copley, who was very right wing. As I remember it, they had an editorial approving Archer's speech.

Sharp: Yes, that would be in line with the paper's point of view at that time.

Revelle: So that was one of the main difficulties I got into when we were organizing UCSD. [brief tape interruption after phone rings]

Well, anyhow, that was the end of it, as far as I was concerned.

Sharp: A couple of other questions about the loyalty oath, if I could ask those.

You had mentioned in one of our earlier interviews about John Francis Neylan and his relationship with President Sproul.

Revelle: It was terribly bad.

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*University of California, San Diego

Sharp: You said that he hounded President Sproul, and that the president never really recovered or bounced back from that.

Revelle: That's what I said just now too.

Neylan was the leader of the anti-Sproul forces on the Board of Regents, and he was a very forceful man. He was a big, handsome, big-voiced man who shouted most of the time, and he just treated Present Sproul unmercifully during the regents' meetings. We had an awful regents' meeting here in the Scripps Institution Library in which he denounced everything that Bob Sproul wanted to do.

Sharp: That regents' meeting was during the loyalty oath?

Revelle: No, it was after that. It was a year or so later. He kept right on it afterwards, until finally he died, I think, or maybe he even resigned, I'm not quite sure. He was the Hearst corporation attorney.

Sharp: I'd forgotten that.

Revelle: He was a very bad man, as far as I was concerned.

Sharp: He had a lot of enemies, that's for sure.

Revelle: Yes.

Sharp: I've run across his name quite a few times, especially when we were doing the Earl Warren oral history project, and people either really liked John Neylan or didn't like him at all; people tended to take sides.

Revelle: I can well imagine that he'd have aroused very strong feelings, one way or the other.

Sharp: Yes, he did.

Revelle: One other thing I remember about this loyalty oath business was I spent practically a whole night with Jim Archer. He sat in our living room, where you were a few minutes ago, drinking my whiskey and getting drunker and drunker and more and more unreasonable, from my point of view, about the loyalty oath.

Sharp: This was during the time it was going on?

Revelle: Oh yes, during the time of the conflict.

Sharp: He just came over to talk to you about it?

Revelle: Yes, after dinner. Between us, we drank about a fifth of bourbon, and he drank most of it. After that, he and I were pretty much enemies. Not so much because he drank my bourbon, but because we just didn't agree at all!

Sharp: He continued to oppose some of the ideas about how the campus would be organized and the kinds of people that would come?

Revelle: Sure.

Sharp: It seems like when people talk about colleges and universities, it strikes a chord about human nature, that people end up having very strong feelings about how universities should operate and what should be taught.

Revelle: Sure. And how it should be taught.

Sharp: And how it should be taught. Whether or not ideas should be free-flowing or they should be very strictly limited.

Revelle: There's always conflict between the town and the gown, as they say.

Sharp: It's particularly noticeable in La Jolla, I think.

Revelle: It certainly was. It still is. You have a better feeling for that than I have.

Sharp: Well, I know when I was an undergraduate here in the sixties some of us organized into little committees and went out to meet with residents of La Jolla, to show ourselves as being just regular college students and to try to diffuse a lot of the animosity that was building up about the Vietnam protest. That was put on top of other feelings, even when the campus was just organized, that people were against the idea of having a university around.

Revelle: Sure. One very powerful reason was that it would bring a lot of Jews here. And it did, of course.

Sharp: Sure, it did. I know there were some housing issues.

Revelle: Oh yes. The Real Estate Brokers Association, REBA as they call it, had an anti-Semitic covenant. They wouldn't show a house or property to a Jew even for rental, let alone for sale. I made a speech to them, a more or less famous speech, in which I said, "You've got to make up your mind. You're either going to have a university or you're going to have an anti-Semitic covenant. You can't have both." And they decided to pretty much abandon their covenant. But I think the real reason they abandoned it was that the U.S. Supreme Court said it was illegal about that time.

Sharp: You probably covered most of that with Kathryn Ringrose, do you think?*

Revelle: I don't quite remember what I covered, but I certainly covered this anti-Semitic business, yes.

We never had much trouble with the local citizens, the Scripps Institution didn't as long as we were named Scripps, after the loyalty oath problem, until we tried to buy some land, and then we aroused quite a bit of antagonism.

Sharp: Right. I sent you that. Do you remember the memo?

Revelle: Well, that was about city land. This was the land just south of Scripps, on the beach, which belonged to Will Kellogg, the trustee of the Frederick W. Kellogg estate.

We proposed to buy and did buy about two or three acres there, where I was proposing to build a marine biology laboratory. But there was so much opposition to our getting it at all, that the regents eventually decided we'd make it a purely landscaped entrance to the campus and not put any buildings on it.

Sharp: This was while you were director?

Revelle: Yes. That was one of the several things which really queered my pitch for being chancellor, the conflict I got into about this land, not anywhere

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*Revelle interviewed with Kathryn Ringrose of UCSD in 1984-85 on a project documenting the establishment of the university.

Revelle: anywhere near as serious as the [Jonas] Salk controversy, but the same kind of thing. The Salk problem was much later, however. There is a parking lot on the front part of the land south of Scripps, but it's all concealed by beautiful landscaping.

Anyhow, after the alumni compromise was agreed to by the regents, we pretty much said that everything was okay here in La Jolla, and most of our guys did sign the oath then.

Sharp: You had mentioned you couldn't remember whether you signed it or not, that you thought you'd signed it before you went on Midpac--

Revelle: Yes, that's right.

Sharp: --and I wasn't sure where to look.

Revelle: That was in April, you see, the alumni compromise. It all happened very rapidly. Just about two months.

Sharp: It did. When I was looking through all the letters, it was such a condensed period of time, the last part of February and then all of March very heavily fought, and then it dissipated.

Revelle: In April.

Sharp: In April, yes.

There's one letter that you sent to President Walter Hepner at San Diego State, and I was wondering about him and what his role was.*

Revelle: Well, he refused to take a public stand, for very good reasons, I thought, although I would have appreciated it if he had.

Sharp: He was an ally, then, of yours?

Revelle: Oh sure. He's a very nice man. I mean, I'm not trying to put people into categories of whether they were allies or not, but he was a very decent citizen.

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*See letter from Revelle, dated 29 April 1950, in Revelle collection at SIO Archives, 81-16, Box 11.

Sharp: He didn't think that a special oath was needed either?

Revelle: Oh no, of course not. But he never took a public stand about it because of his position.

Then from there on I devoted most of my time to getting the mid-Pacific expedition underway.

Sharp: I thought we would talk about that next. Are there some other comments or reflections about the loyalty oath that you think we should include at this point or we could go on and talk about Midpac?

Revelle: Well, I don't really think so. I think I've said pretty much everything I can think of, particularly, as I say, the close relationship I developed with a lot of the faculty at UCLA, and Berkeley too.

In other words, I think if you look at it historically, that was the time when I was fully accepted by the faculty, by the academic senate, as a real person because of the prominent part I played. I testified before the alumni committee, for example. I was one of the people chosen to represent the faculty, and the reason was that everybody thought Scripps was so pure, so obviously pure. [laughing] No radicals on the campus, and yet we still felt the same way as all those goddamn sociologists and psychologists and political scientists and historians!

Sharp: Well, if you were accepted as a member of the faculty, then, through this sort of trial by fire, what was it like before?

Revelle: Well, we were pretty much an isolated little place down here. I remember people like Paul Dodd, who used to be dean of Letters and Science at UCLA. He was always saying, "We want to help you out in any way we can." It always reminded me of the old joke about "I want to help you out; which way did you come in?" [laughter] What they really wanted to do was to have us as completely under their thumb as possible, although I'm sure they had good, idealistic motives. But that was the way it seemed to me at least. We were constantly fighting for autonomy of one kind of another--autonomy from Jim Corley and autonomy from the academic senate at UCLA, autonomy from the contracts officer up there, and so forth.

Sharp: Now, that is not undocumented at all because in the papers that I have seen, especially after you'd become director, that fight for autonomy is very clear. I thought we might talk about it some, but it's not hidden by any means.

Revelle: No, it isn't. [laughing] That's right.

Sharp: It's definitely there.

Revelle: I feel, in any case, that that was an important turning point in my career at the University of California, that loyalty oath business.

Sharp: You were coming from the navy--

Revelle: Oh course, I'd started here at Scripps.

Sharp: You'd started here, but then you were in the navy long enough that you were almost not an academic anymore and then you had to become one again.

Revelle: Yes.

Sharp: This issue, did it make you sort of confirm some of your own ideas about wanting to be where you were?

Revelle: Of course. Very much so. They used to say in the navy, "it goes like this all the time," you don't think anything of it. But that's different than being in the university. Very different. That's a good analysis of yours, that I'd become pretty navy-minded.

Sharp: Well, you'd been there a long time, and, from what I could tell, very busy, very consumed by navy issues--

Revelle: That's right.

Sharp: --and in your doing some rather innovative kinds of organizing within the navy, it would seem in a way to further your allegiance to the navy and help you think of yourself as a real part of what the navy was doing.

Revelle: Yes, but it also helped me grow up too because I had a lot of responsibility at a relatively early age.

II NOTES ON EARLY EXPEDITIONS

Midpac, 1950

The Ships

Sharp: One of the things you mentioned about your work late in the navy, in '46, '47, was you said you learned how to disburse funds. I would think that those sorts of skills, once you became acting director and then on to director, would have stood you in good stead.

Revelle: Sure.

I took a long time growing up. [interviewer laughs] I was not in any way a born administrator, just the opposite, in fact. I had to learn by doing and by making lots of mistakes.

Sharp: I thought we might start talking about Midpac.

Revelle: Well, that's a perfectly reasonable thing to talk about. That was, as far as Scripps was concerned, the beginning of our great age of exploration, which I think was one of the greatest periods of exploration of the earth, not just for Scripps but for oceanographers in general, Maurice Ewing at Lamont and the people at Woods Hole and to some extent scientists elsewhere in the world, particularly in the United Kingdom, the Soviet Union, and somewhat later, France. This was an age of exploration that compares in many ways with the exploration of the fifteenth and sixteenth

Revelle: centuries. Instead of discovering the land surface of the earth, we were discovering what was beneath the ocean. Everytime you went to sea you made unexpected discoveries.

Sharp: And with Midpac it was quite dramatic.

Revelle: Yes.

Sharp: Finding, literally, a mountain range.

Revelle: Oh, all kinds of things.

Sharp: A thousand miles long and a hundred miles wide.

Revelle: Yes.

Sharp: And it gave quite a bit of good press coverage for Scripps.

Revelle: Yes, that's right.

There were a couple of geophysical things that were more important. But the Mid Pacific Mountains were certainly the most dramatic.

Sharp: Let me just start at the very beginning and then work through.

Revelle: Well, I've forgotten who had the original idea to have the expedition. It could very well be that I had it, but Bob Dietz certainly had a part in it too. He was working at the U.S. Navy Electronics laboratory on Point Loma.

Sharp: One statement I have seen, in 1949 you officially proposed it in a report to the University of California.*

Revelle: I did?

Sharp: You did. Let me see if I can find it. [brief tape interruption]

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*See report dated 10 October 1949 on pp. 20a-g. SIO Subject Files, Box 6, Folder 35, SIO Archives.

UNIVERSITY OF CALIFORNIA
SCRIPPS INSTITUTION OF OCEANOGRAPHY
LA JOLLA, CALIFORNIA

10 October 1949

PROPOSED UNIVERSITY OF CALIFORNIA MID-PACIFIC EXPEDITION

Introduction

During and since the war great advances have taken place in geophysical and geological techniques for exploration of the sea bottom and what lies underneath it. These advances are now being exploited by oil companies in their search for new oil fields underneath the continental shelf. From the scientific point of view, however, the greatest opportunities lie further from shore, in the deep sea. In parts of this area, sediments have accumulated extremely slowly over millions of years. With modern coring methods, it is possible to obtain an undisturbed sample of the sedimentary layers laid down in the deep sea during the past five to ten million years. Changes in the type of sediments with depths in the core reflect changes in ocean conditions and world climate during the time of deposition. Using seismic methods, it is possible to determine the total thickness of sediments and the nature of the underlying rock, and thus to gain information concerning the history of the ocean basins over a very long period.

Knowledge of the rate of heat flow from the earth's interior is essential to an understanding of the processes by which great mountain ranges are formed, yet over the three-fourths of the earth's surface covered by the oceans, we have no information concerning temperature gradients in the earth's crust which determine the flow of heat.

Most geological text books state that the deep sea floor is a flat and featureless plain, but wherever modern echo-sounding methods have been used, the bottom has proved to have relief comparable to that of the Rocky Mountains or the Sierra Nevada. No data exist, however, for most of the oceans except along established trade routes.

Next to nothing is known about the currents in the deep sea or even whether appreciable tides and currents exist in these abyssal waters. Does the wave of the tide "peak up" on the continental shelf, with a consequent intensification of the inshore tidal currents? Are transient pressure gradients near the sea surface transmitted downward into deep water to give rise to oscillating currents? These and related questions can be finally answered only by direct measurement in the deep ocean.

Until recently, oceanographers have believed that life in the sea was concentrated in nearshore areas and in waters near the surface. Underwater sound studies during and since the war, together with the investigations of the Scripps Marine Life program, have shown that many species of fish and other animals live at mid-depths several hundred miles from shore in far greater numbers than has hitherto been suspected. These species may occur throughout the ocean basins in numbers comparable to those of the sardines in California coastal waters. If so, they constitute a natural resource of great potential value to a protein-hungry world. To make known the existence and extent of this resource would be an important service to mankind and this can only be done by deep sea exploration with modern methods.

One of the chief gaps in our understanding of the general circulation of the atmosphere is the paucity of data on the vertical structure of air masses in low latitudes over the ocean. Such data are almost entirely lacking from the Eastern North Pacific.

The University of California's Scripps Institution of Oceanography has become the largest oceanographic laboratory in the world, yet up until now its investigations have been primarily confined to a narrow strip of ocean along the west coast of North America. This was necessary in the past because its single ocean-going research vessel was not suited for sustained operations on the high seas, far from shore. It has now acquired three additional ships, two of which are admirably suited for deep sea exploration. One of these, the research vessel HORIZON, is capable of cruising 10,000 miles without refueling. Her history as a naval vessel demonstrated that she can keep the sea in any weather, and she has now been fitted with hauling gear that enables her to handle weights of many tons at depths up to 20,000 feet.

There are no boundaries within the ocean; it is a unit and no part of it can be thoroughly understood without knowledge of the whole. This fact has long been recognized by our sister laboratory, the Woods Hole Oceanographic Institution, who have taken the entire North Atlantic and the Mediterranean as their province.

By contrast to the Atlantic, the central and eastern Pacific is almost completely unexplored from a modern scientific point of view, but sufficient is known to indicate the existence of problems, the solution of which would be of the greatest importance in understanding the history of the earth. For example: there occur in the mid-Pacific many flat-topped banks or sea mounts, almost surely planed off by wave erosion in shallow water, but now at depths of 3,000 to 6,000 feet. It has been suggested that these may have been cut by waves during pre-Cambrian times more than five hundred million years ago, and that their apparent submergence was caused by the slow filling up of the ocean basins with sediments, or the slow addition of water to the sea by volcanic activity. Others believe that the bottom of the ocean has subsided over large areas in relatively late geologic times, and a third school have proposed that the flat tops of the sea mounts were planed off during the glacial period when the sea level may have been lowered by many thousands of feet because of the locking up of water in the great ice caps.

Proposed Expedition

In order to make a start on some of these problems, it is proposed that an expedition be undertaken by the research vessel HORIZON during the summer of 1950, across the North Pacific from San Diego to the Marshall Islands over a total distance of some 10,000 miles. About three months at sea would be required. The primary objective of the expedition would be to make geophysical and geological studies of the floor of the Pacific, but several other lines of investigation could logically be included; particularly radio-sonde observations of the upper atmosphere in the eastern Pacific, underwater sound transmission studies, measurements of internal waves and deep currents whenever the vessel stops for a sufficient period, and assays of the fish and invertebrate animal populations.

It is thought that the outward voyage would require 22 days actual sailing time. A total of 450 hours or nineteen days should be available for the proposed scientific observations at fixed ocean stations, and two days would be spent at a port in the Hawaiian Islands for refueling and resupply, making a total of 43 days elapsed time for the voyage.

The following outline of planned observations is given to indicate the magnitude and type of work to be done. The actual program would be sufficiently flexible to include suggestions by interested scientists prior to the voyage and to allow for shipboard changes in plans depending upon results obtained.

Thirty-mile long seismic refraction profiles of the sea bottom would be made at 15 localities. By this means it should be possible to determine the thickness of sediments and the seismic wave velocities of the underlying rocks down to a maximum depth of five to ten kilometers beneath the sediments. Techniques for this work have been developed to a high state of practicability by Dr. Russell W. Raitt of the Marine Physical Laboratory of the Scripps Institution.

A coring apparatus, similar to that developed by the Swedish scientist Kullenberg, would be used to collect long cores of the sea bottom at approximately 25 localities. Swedish experience has shown that cores up to 30 meters in length may be taken with this equipment. In collaboration with the U. S. Navy Electronics Laboratory, the Scripps Institution now has available an instrument of this type and our geologists are gaining experience in its use.

Measurements of temperatures at different depths within the bottom sediments would be made at each locality where coring is done, using the equipment developed last summer by Dr. E. C. Bullard and Mr. A. E. Maxwell during Dr. Bullard's stay in La Jolla. Deep current measurements relatively near the bottom would be undertaken at 15 localities with equipment being developed by Mr. William Van Dorn, Associate in Oceanography on the staff of the Scripps Institution of Oceanography. These would be accompanied by observations of internal waves in the thermocline made with the deep bathythermograph.

Net hauls for small fish and invertebrates at different depths above and within the deep scattering layer would be made whenever the vessel is underway, using the new high-speed collectors developed by Mr. John D. Isaacs, Associate Oceanographer on the Scripps Institution staff, and his collaborators. These instruments are now in quantity production and have proven most successful in all tests made to date. Measurements of the depth and structure of the deep scattering layer and the intensity of sound scattering would be made for comparion with the net hauls. The recording fathometer would be operated continuously while the vessel is underway on both the outward and return voyage; the proposed track of the ship has been laid out in an area where there are now virtually no soundings of any kind, let alone a continuous profile of the sea bottom. Approximately 80 meteorological radio-sonde observations would also be taken during the expedition, from the surface up to 30,000 feet or more.

In order to make the seismic refraction profiles, two ships would be required; preliminary discussions have already been held with representatives of the Earth Sciences Division of the Office of Naval Research concerning the possibility that the Navy can furnish a vessel for this purpose. The echo-sounding lines would also be much more valuable if two ships were operated in parallel at a distance of about a mile, so as to give some indication of the horizontal trend of the bottom topography.

Navy participation and support would be amply justified by the results on propagation of low frequency sound (from 3 to 2,500 cycles) over long distances which would be a necessary by-product of the seismic refraction work. These propagation studies would include determinations of ambient noise background at low frequencies as a function of sea state and wind force; measurements of scattering and reflection from the sea bottom as a function of bottom topography and sediment characteristics; and studies of focussing, dispersion and interference patterns resulting from the distribution of temperature and density in the sea water.

The Office of Naval Research has undertaken to furnish radio-sonde transmitters, balloons and other equipment for the meteorological program as part of the cooperative research project between the Department of Meteorology at UCLA and the Scripps Institution.

The second part of the expedition would center around Bikini Atoll in the Marshall Islands. More is known about this atoll than about any other, because of work there during the summer of 1946 and 1947 by the Oceanographic Section of OPERATION CROSSROADS. The results of these investigations have shown the great desirability of drilling a deep hole through the coral sediments of the atoll down to the underlying bedrock. The Geological Society of America has set aside $50,000.00 for this drilling project, and has appointed a committee composed of Harry S. Ladd, Chairman; James Gilluly; Roger Revelle and A. I. Leverson, to organize the work. The total cost of the drilling project will probably be in excess of $200,000.00. Attempts to raise these additional funds have been postponed until more is known about the structure of the atoll from seismic refraction studies. It is therefore proposed that a seismic refraction survey of the northeast corner of the atoll surrounding Bikini Island, and

of the slope of the bedrock outside the atoll, be made.

Some question has been raised concerning the generality of the deductions drawn from the Bikini studies, and it would be hoped that a reconnaissance seismic refraction section could be made across one or two other atolls. A thorough investigation of the flat-topped sea mounts in the Bikini area would be carried out by rock dredging and by seismic methods in an attempt to elucidate the character of the rock of which these strange features are composed.

It is contemplated that the work in the Marshall Islands area would require about a month, after which the HORIZON would return to San Diego. No stops would be attempted on the return voyage, which would last 20 days, but undersea plankton collections, echo-soundings, bathythermograph lowering and radio-sonde observations, would be taken.

Personnel

In addition to the crew of the Navy ship and the 17 members of HORIZON's crew, there would be a scientific party of 16, as follows:

3 geophysicists on HORIZON to take seismic refraction measurements

1 explosive expert on the Navy ship, to shoot charges for the seismic refraction studies

1 electronics specialist on the Navy ship to maintain the fathometer and assist with soundings

2 research assistants (geologists) on the Navy ship to take soundings

1 geophysicist to measure bottom temperature gradients

2 meteorologists to make radio-sonde observations

2 marine technicians for plankton hauls and bathythermograph measurements and general assistance

3 geologists to collect cores and dredgings

1 physical oceanographer for deep current measurements

Senior scientists of the Scripps Institution who would participate include Messrs. J. Frautschy, R. Raitt, R. Revelle and F. Shepard. Dr. Harry Ladd of the U. S. Geological Survey has been very much interested in the plans for the expedition and has agreed to send two Geological Survey geophysicists on the ships. The two meteorologists would be representatives of the Department of Meteorology, UCLA.

Budget

It is estimated that the expedition will cost about $60,000.00, including $45,000.00 for three months operation of HORIZON and $15,000.00 for equipment and other expenses. The Earth Sciences Division of the Office of Naval Research has given verbal assurance that it will furnish $15,000.00 of these funds.

In 1949 the Institute of Geophysics requested the President of the University to make a grant of $30,000.00 in fiscal 1949-50 for geophysical exploration of the deep sea. If this money could be made available during the present fiscal year it would go far towards defraying the costs of the expedition.

Information is requested as to whether the University can make available the $30,000.00 requested last year plus, if possible, an additional $15,000.00. If this can be done, the Scripps Institution would make a formal proposal to the Office of Naval Research for the remaining $15,000.00 required to undertake the expedition.

It should be added that the Chief of Naval Research has informed the Chief of Naval Operations and the Commander-in-Chief, U. S. Pacific Fleet concerning the possibility of HORIZON spending a month in Bikini during the summer of 1950; it is anticipated that these authorities will cooperate in every way to facilitate the work. Entry into the Marshall Islands area will not be possible during the summer of 1951, however, because of certain operations planned there by government agencies.

Roger Revelle
Associate Director

Revelle: [looking at materials] I wrote this when I was associate director. Carl must have been relatively enthusiastic about it too.

Sharp: Well, I wondered about his involvement and exactly what his role would have been. Would he have given you the freedom then to pursue this?

Revelle: Oh sure, oh sure.

Sharp: This came under the category of work at sea?

Revelle: Yes.

He was a theoretical physicist. He never really understand the kind of sloppy way that oceanography and geophysics are done. He was very, very precise and always worked from basic principles, deductively.

Sharp: And very theoretically?

Revelle: Very theoretically. Whereas oceanography, at that time at least, was entirely an inductive science. You just found out things, without thinking too much about how they happened!

So we must have been talking about it throughout 1949. I remember this document. This is quite a polished document. It's not something that you just dash off over a Sunday afternoon.

Sharp: Right. So you must have been trying to be very persuasive and complete and asking for UC support. The last item is level of support.

Revelle: Yes.

Sharp: Would President Sproul have been helpful in certain ways?

Revelle: Oh sure. He was always in favor of anything good. Sometimes he wouldn't help very much, but he'd always be enthusiastic about it!

Sharp: We'll talk about this more later but perhaps it fits here too because President Sproul was often in favor of things that you were doing.

Revelle: Yes.

Sharp: Even with the difficulties of the loyalty oath, of getting an expedition like this under way, the hard feelings about one issue didn't carry over into an expedition like this.

Revelle: Hard feelings about what?

Sharp: Hard feelings about the loyalty oath, all the difficult tensions that produced. Getting something like this expedition going was more hopeful, and much more pleasant to consider.

Revelle: Oh sure.

Well, I was kind of a pet of the regents. I'm not sure at this particular time that I was, but within the next few years, we couldn't do anything wrong at La Jolla! [laughing]

Sharp: From the papers, it looks like the period from mid-1950, when Midpac really gets under way, through your appointment and that first year of your appointment there are some very good events and some pretty difficult ones, all in that period together. I thought we might talk about some of them.

Midpac, from all that I've seen, looked like a pretty pleasant time.

Revelle: It was wonderful. It was a great experience, a tremendous experience, and really set the pattern for the next ten years.

The Scripps Institution's longest expedition up until that time had been to the Gulf of California--we'd had two expeditions before World War II to the Gulf of California on E.W. Scripps. One was sort of a general oceanographic expedition which Harald Sverdrup led, and the other was a geological expedition that Fran Shepard and Charlie Anderson and I organized. Tha was in 1939. So this was a complete change of mode of operation, the Midpac expedition.

I must have felt, although I don't see it in the record, that we had the opportunity to do something that we'd never done before; that is, to explore the whole Pacific Ocean, very much as Woods Hole had explored the North Atlantic from a physical-oceanographic point of view because of their ship Atlantis. And Maurice Ewing was starting to do the same thing with his ship, Vema.

Sharp: Was Woods Hole a model for what Scripps might become in their ability to get out into the ocean, the deep sea, and really do some very basic kinds of exploration?

Revelle: Well, they never really did that at Woods Hole. Maurice Ewing did. When Maurice was working at Woods Hole, then they were pretty much like Scripps, but then they split up and the geological-geophysical part went to Lamont, and physical oceanography and biology stayed at Woods Hole.

Columbus Iselin was the director at Woods Hole at the time, and he and I were sort of friends; not really, because he was very jealous of Scripps. He felt that any money that Scripps got from the government was money that wouldn't go to Woods Hole, whereas I felt just the reverse. I felt that any money that Woods Hole got meant that we got more money at Scripps, and vice versa. I was right, of course. By building up together, we walked over them.

So I wouldn't say that they were a model, except that they were an example of the fact that you should study a large part of the ocean, not just the coastal strip, as most American institutions had done, up until that time.

Sharp: One of the issues for Midpac, which is part of every expedition that you're going to try to carry out, was money, was the funding for it.

Revelle: Sure.

Sharp: And in the papers you can see this writing of letters that you were doing, trying to get some money from this group and patch it in with more money from another group and put it all together until there was enough.

Revelle: Sure.

Sharp: There's one letter that you sent to Allyn Vine and Beauregard Perkins--.*

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*See letter to Vine and Perkins, dated 9 January 1950, SIO Subject Files, Box 6, Folder 36, SIO Archives.

Revelle: Right, asking them for more navy money. And they came across.

Sharp: They did?

Revelle: As far as I know.

The main problem there was to get the second ship. For Russ Raitt's seismic refraction work, you needed a shooting ship as well as a receiving ship. Rawson Bennett, at that time, was director of the Navy Electronics Laboratory at San Diego. He was my old friend and my old boss in the Bureau of Ships, but he played it pretty cool. I'm sure he was thoroughly on our side, but he pretended not to be, pretended that it would divert his Navy Electronics Laboratory from its bureau-assigned responsibilities. [laughing] But Allyn Vine in the Bureau of Ships, of course, and Beau Perkins in ONR were enthusiastic supporters. Rawson really was too, but he wrote a letter to the Bureau in which he said if they assigned the ship, [EPCE (R)] 857, to our expedition that it would interfere with various other projects, and he wanted authority to do that. Of course they gave it. No problem.

So we had the 857 as well as the Horizon. One of the, not exactly the prices we paid, but one of the lures, the bait for the navy to take part, was that they could study underwater sound in a variety of environments--reverberation; bottom scattering.

Sharp: And this was really part of their justification for taking part?

Revelle: Yes. That was one of their primary jobs, to understand underwater sound propagation at different frequencies, and particularly at low frequencies. We dropped sofar bombs all the way just to see the transmission through the sound channel. We took a lot of meteorological observations.

So that EPCER 857 participation could be justified on a strictly navy program, although it did a lot of other things which weren't part of the navy's program at all! They had Bob Dietz and Ken Emery on board who were submarine geologists.

Revelle: Horizon was a seagoing tug with the consequence that she had an enormous fuel capacity and could stay at sea for months on end. EPCER 857 had a minimum fuel capacity, so it was planned that we would refuel them about every three or four days.

Once they got away from San Diego, the navy ship started to break down, and they wanted to go back to San Diego. I didn't want them to go back to San Diego. They were really all right. They had something wrong with their clutch, so they could only use one engine. So we would give them enough fuel to proceed, but not enough to get back to San Diego! [laughing]

Sharp: One of the reports you wrote was to Dundas Tucker.*

Revelle: Yes, that was after Rawson [Bennett] left the directorship at NEL [Navy Electronics Laboratory], and Dundas Tucker succeeded Rawson. I've forgotten what job Rawson had next, but within a rather short time he became the chief of Naval Research.##

Sharp: In the report you tell him that the 857 had gone in for repairs, and you were doubling everybody up on the Horizon, trying to get the work done and not slow down.

Revelle: That was when we discovered the Mid-Pacific Mountains.

That was quite interesting because EPCER 857 limped down to the equator with us, and then limped back to Hawaii, us keeping them in short fuel rations all the while! Jim Faughn was captain of Horizon, and he was a master of navy procedures. He was a retired warrant officer, and warrant officers are the backbone of the navy, even more so than chief petty officers.

Sharp: He was a good choice to be captain, then?

Revelle: He was wonderful, absolutely superb. He later ran our NAGA expedition in 1961-63.

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*See letter to Tucker, dated 26 August 1950, on following pages, 25a-c. SIO Subject Files, Box 6, Folder 38, SIO Archives.

Honolulu, T. H.
August 26, 1950

Captain Dundas F. Tucker
Commanding Officer and Director
U. S. Navy Electronics Laboratory
San Diego (52) California

Dear Dundas:

The HORIZON is just about to shove off on the second leg of the "Operation Mid-Pac", but before we leave I want to give you a summary of our work todate and of our plans for the future.

Both ships arrived here before noon on August 23rd, some twenty-eight days after leaving San Diego. The total steaming distance for the HORIZON was approximately 3750 miles and somewhat greater for the 857. Our speed when the two ships were in company and both proceeding on one engine was about nine knots. This was somewhat less than anticipated and resulted in a slight lengthing of the time required for getting from one station to another. We were nevertheless able to make observations of reverberation as a function of depth below the projector and of frequency over a twenty-four hour period at six stations and for twelve hours at one station. In addition there were half a dozen or so shorter stops where these studies were carried out. Low frequency sound propagation studies were made at distances from 60 to 100 miles at five stations and at shorter ranges at three additional localities. Both the sound propagations and the scattering studies were made over a wide variety of conditions of temperature gradient in the upper water layers, of bottom types and depth, and of concentration of organisms. Studies of the variability of ocean temperature in the upper water layers were made at all the scattering stops and the hydrographics of the water, the character of bottom sediments, and the distribution of organisms were determined as frequently as possible. Continuous echo sounding lines were run by both ships over nearly the entire distance except for a short period when the MEC equipment on the 857 was inoperative. Radio Sonde observations were taken twice a day throughout the cruise. Several balloon soundings went up over one hundred thousand feet and give a good picture of stratospheric temperatures and pressures. SOFAR Bombs were dropped at hourly intervals whenever the 857 was underway. I understand that many of the explosive charges dropped for the sound propagation runs were also heard at the SOFAR Stations.

We used the newly developed "jog log" to measure surface currents at two hour intervals throughout the cruise. This seems to be a fine and reliable gadget and may prove to have considerable usefulness, particularly for submerged submarine navigation. Bathythermograph observations were taken every two hours by both ships while underway and at half hour intervals whenever the ships were hove to near each other. These may give valuable information about the variability of Sonar Conditions over short distances.

On the northern side of the equatorial counter current there was a very pronunced thermocline at a depth of about thirty feet while at 4°north latitude the thermocline was more than 450 feet deep.

Thanks to your prompt action, the Navy Yard has mended our big winch in record time, and we will be ready for sea about midnight tonight.

We have taken aboard all the NEL Scientific party except Cosby, Young and Gibson and plan to do as much as we can with a single ship until the 857 is again in operating condition. This will mean primarily Bob Dietz's studies of sea mounts, coring, hydrographic and biological studies. In addition we will make a determined effort to measure temperature gradients in the bottom muds. These measurements are of great scientific interest because knowledge of the differences in temperature gradients under the seasand under the land is basic to any geological theories of the origin of mountain ranges, and, indeed, of the continents themselves.

It is now expected that the 857 will be out of dry dock on September 4 or 5, and will be ready for sea on September 6 or 7. We are hoping she will be able to join us some 500 miles west of here. We will then transfer her scientific party and as much of their equipment as possible and continue to Kwajalein and the Marshall Islands area. In order to accomplish the planned program in the Marshalls it will be necessary to reduce the stops enroute, after the two ships meet. But by taking advantage of the HORIZONS low fuel consumption with two engines we will be able to play a sort of leap frog to make all the planned sound propagation and seismic runs.

The sound scattering and sea mounts studies will necessarily be reduced pretty much in proportion to any delay in the departure of the 857 after the scheduled date of September 6; but in order to accomplish the planned Sofar program, it will probably be necessary to delay the return of the 857 to San Diego, by an amount of time roughly equivalent to any delay here in the yard. Fortunately Admiral Cowdrey (who is the present commander of the shipyard) is convinced of the importance of the work of the expedition and has given both the 857 and the HORIZON priority I in the yard.

25c

This means work on a three shift basis and a maximum of pressure to get the ships to sea as early as possible. We have also received all possible support and backing from ComServPac.

We have had excellent cooperation at all times from both the naval and civilian personnel of EPCE(R) 857. I wish particularly to commend Messrs. Batzler, Black and Roque for their hard and effective work in carrying out the scientific program. According to what I have heard from other members of the party, Black and Roque apparently never slept during the entire twenty-eight days. They accomplished a great deal and were always most willing to take any extra job which I dreamed up, even though they had always worked continuously for twenty-four hours or more.

Commander MacMillan has also been most helpful in every way and is constantly trying to maximize the scientific results; particularly those which might be of direct value to the Navy.

If all goes well and according to plan on this second leg of the expedition, I would feel that my presence here was no longer of great importance and would plan to fly home from Kwajalein so as to avoid too great a piling up of work at Scripps. If, however, as is perhaps more likely in problems and any work at sea of the new types we are undertaking, we continue to have our difficulties, I will plan to remain on the trip. If I do return from Kwajalein I would propose that Russell Raitt assume responsibility as expedition scientist.

We are loaded to the gunwales tonight with Bob Dietz's rather heterogenous gear. I hope only that it is stowed well enough so as not to be washed overboard.

With best personal regards I remain.

Very Sincerely Yours,

Roger Revelle by DM

Roger Revelle
Acting Director

RR/pm

cc: Dr. N. W. Rakestraw
Scripps Institution of Oceanography
La Jolla, California

Sharp: I've seen a reference in a letter to Teddy, and I think it's Teddy Bullard, but I wasn't sure.*

Revelle: Yes, it is. It must be Teddy Bullard.

Sharp: That you wrote in October, and you were talking about an acoustic signaling device that could tell when you hit bottom--a very basic innovation.

Revelle: Yes. This was one of John Isaac's inventions.

Sharp: That's right, yes. And it made me think about innovations in instrumentation and how important they were to the progress of the expeditions.

Revelle: Absolutely.

Oceanography has always been driven by technology, by new technology. We had a half a dozen new tools as a result of the wartime experience, which we were exploiting, on this expedition.

One of them was precise recording echo sounding. Another, it didn't come directly out of the war, but it came out of the development of electronics during the war, it was the heat flow device, the heat probe, as we called it. Another one was the towed magnetometer. A fourth was the seismic refraction method of measuring sound velocity at different depths beneath the bottom surface, so that you got an idea of the different layers of rock down there. A fifth was, again, not directly out of the war, but out of the development of electronics, the current measuring device, the so-called GEEK; the geomagnetic electrokinetograph or something like that was its actual title. It was invented by Bill Von Arx at Woods Hole.

Then we had some things that we had inherited from pre-war time, like the rock dredge which we used to pull off chunks of the bottom. And there was a new kind of sediment sampling device, the Kullenberg corer which came out of the post-war Swedish Albatross expedition.

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*See letter to Bullard, dated 15 October 1950, SIO Subject Files, Box 6, Folder 40, SIO Archives.

Revelle: All of these things represented new technology; they made it possible to find out a lot that we could not possibly have found out before World War II. Even today, oceanography, the present prospects for oceanography, depend on new technologies, primarily satellite technologies. The sort of in-between slump in oceanography in the late 1960's and 1970's was because there weren't any new technologies.

But now we have "acoustic tomography" and the various kinds of measurements that can be made from satellites, plus the CTD, the conductivity temperature depth recorder, plus the expendable bathythermograph. All these things are new technologies which are transforming oceanography again in several different ways.

Sharp: You had done a lot of adaptation of the ships too, to make them usable.

Revelle: Oh sure. Of course. The main thing was the winches. We had a big dredging winch, then some so-called hydrographic winches for taking water samples and plankton samples, and then a bathythermograph winch for taking bathythermograms.

Then, of course, we had a recording echo sounder, plus a fathometer, plus a radar. But the radar and the fathometer were primarily navigational devices and they didn't work very well very far from shore.

Of course, we had to have big laboratories too because a lot of instruments had to be kept protected. We had several recording instruments in the laboratory, as I remember it.

Sharp: You know, one of the things that I thought we might just touch on briefly, is the press of the research, using all of this equipment to the best and quickest advantage, just to get as much work done as quickly as you could once the expedition was under way.

Revelle: The way we did that was to work twenty-four hours a day, seven days a week. I don't mean that everybody stayed up twenty-four hours.

Sharp: But you divided it up.

Revelle: Yes. Both the crew and the scientists had regular watches. One of the interesting things I found out about myself was that I tended to stay up beyond my watch, so I would gradually go around the clock, sleeping at later and later times, until finally I started all over again, because I was so interested in what was happening, what we were finding.

Anyhow, we started out from San Diego and went down to the equator and then back to Hawaii, and then out to the Marshall Islands where we were going to do some seismic refraction shooting under Russ Raitt's direction and also some work on coral reefs and sampling of the seamounts.

We got down to the equator and back to Hawaii, and EPCER 857 went into the shipyard. They sent for their wives and families, thinking that they would be there all summer, the rest of the summer. Like all sailors, they weren't very much interested in going to sea!

Sharp: No real dedication there.

Revelle: Well, sailors are like that. Whenever they're ashore, they want to be at sea. Whenever they're at sea, they want to be ashore.

I went to the commandant of the shipyard, who was an admiral, and said this was a high priority navy project, and showed him my correspondence with ONR. So he gave the two ships number-one priority in the shipyard, which was quite impressive. The result was that Horizon got out in about three days, maybe less, and EPCER 857 got out in about ten days, much to the chagrin of the officers and crew of the vessel who, as I say, had sent for their families.

Sharp: Were the families there by then?

Revelle: I never heard what happened, but they must have told the families not to come when they saw what was going to happen to the ship. But I never heard a word about that afterwards. All this stuff that's in the correspondence about this great cooperation we got from the officers and crew of the EPCER was mainly window dressing.

Sharp: Well, I wondered about that.

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University of California
Scripps Institution of Oceanography

Director's Office

21 September 1950

20 September 1950
Action to: Dr. Norris Rakestraw

Last Saturday the 857 joined us, and a happier and more welcome sight you cannot imagine. She came along side about noon with mail and other good things which were also very welcome. On Sunday we occupied a station just about on the international date line. As we lay to, we oscillated back and forth between Sunday and Monday most of the day. Very confusing. We have now left the area of the Midpac Mountains and are steaming on a southwesterly course toward Bikini Atoll, about 400 miles away. Both ships will arrive there on September 23 (September 22 your time) in the afternoon. We are planning a beach party for all hands that evening. From September 24 to 29, the ships will core and dredge and take bottom photographs outside the atoll, especially on Sylvania Seamount to the northwest. During this period we will leave a group of six people camped on the beach to make chemical and geological studies of underground water and of the solution basins between high and low tide level which are such a remarkable feature of the island. We leave for Kwajalein in the morning of September 29, arriving there on September 30, when more than half the scientific party will disembark to return home by air. The two ships will then return to Bikini for about ten days to conduct a seismic survey of the atoll and the surrounding sea floor. With luck, the HORIZON will be home before the end of October and the 857 a few days later. Scientific highlights during the last ten days include the dredging of about 200 pounds of silicified limestone containing numerous shallow water clam and snail shells from a depth of six thousand feet on top of a seamount at the southwest summit of the Midpac Mountain range. Further southwest we obtained a Kullenberg core in a flat basin at thirteen thousand five hundred feet, consisting of a few inches of red clay above nearly 12 feet of fine grained calcareous ooze. An equally long core north of the mountains at a depth of fifteen thousand feet was more orthodox, with globigerina ooze over red clay. We have now made seven measurements of temperature gradients in the bottom sediments of the deep Pacific. At least five of these show a temperature increase of about seven degrees Fahrenheit per hundred feet, over five times the average gradient found on land. Evidently a lot of heat is being conducted through the sea bottom from the earths interior. Best regards to all.

Signed: Dr. Roger Revelle
SOUTH PACIFIC RESEARCH
EXPEDITION, M/V HORIZON

Revelle: Just to build them up with their bosses.

Sharp: Sure, yes.

Revelle: We had a lot of the scientific party on EPCER 857, and they all climbed aboard Horizon and went out to explore the Mid-Pacific Mountains.

Sharp: Some of the letters showed that you were trying to get spares for all the equipment, so that you had two of everything, in case something broke down or was irretrievable, and you had the sense of all these people and all this equipment just wall to wall.

Revelle: Yes, that's right.

We didn't have two of everything. Mostly we had one of everything. Particularly that was true of the heat probe. We had two kinds of heat probes, one that John Isaacs had developed, which was entirely non-electronic, entirely mechanical, and one that Art Maxwell and Teddy Bullard had developed, which had a lot of electronics in it.

We lost the mechanical one. There was a guy by the name of Bill Thompson who was in change of the mechanical one, and he lost it the very first time he lowered it over the side. So all we had left was Art Maxwell's probe, and with that we managed to get seven measurements. It was very difficult because you had to stick the thing in the bottom and not pull on it and let it sit there for about half an hour and how could you do that? Well, we did it essentially by being sure that we had a completely slack line from the surface, but if we had too slack a line the damn thing would kink on the bottom.

Sharp: So you had to keep it just exactly right.

Revelle: Well, it was impossible to keep it exactly right, but we did pretty well and didn't get much wiggling of it, judging from the record at least. The record consisted of a time trace of the temperature difference between two points in this spear that we stuck into the bottom, sort of like a rectal thermometer, except there were two thermometers a meter apart. What we were trying to measure was the difference in temperature between those two thermometers.

Sharp: And that difference, in knowing the distance between the two thermometers, you were able to measure the temperature gradiant.

Revelle: We knew the distance, of course.

We had to measure the temperature difference to about a hundredth of a degree, which we did. The gradient turned out to be about six hundredths of a degree centigrade per meter. So that would mean that in a kilometer there would be a 60 degrees centigrade difference in temperature.

The reason there was such an enormous temperature difference, which made it possible to make the measurement, was that the thermal conductivity of the mud, which was mostly water, was very low. In other words, the resistance to heat transport was very high. The heat conductivity was almost as low as that of water. So you could calculate, then, that at greater depths with less water and more sediment per unit depth of mud the sediments would have a greater heat conductivity, and the temperature gradient would be about twenty degrees per kilometer, which is still a hell of a lot. In other words, in about five or six miles below the surface the temperature would be of the order of 200 degrees centigrade, twice the boiling point of water at the earth's surface.

This very high temperature gradient, was completely unexpected, because the accepted doctrine was that the heat came from radioactive decay, and there was a lot of radioactivity in the continents where the unanium had concentrated but very little in the ocean, because you had basalts in the ocean, basic rock which were low in radioactive materials.

Sharp: Did you wonder if your technology was working right?

Revelle: Oh, we thought it was working right, yes. You could tell that by the record, by the trace.

This was a most surprising and interesting result. Later, when we worked up the data, it was quite clear that the heat flow from the bottom of the sea was at least equal to the heat flow from the continents, that the two parts of the earth had the same heat flow. But this meant that in the ocean you had to have deep-seated convection.

Revelle: The only way to get the heat out from the rocks a thousand kilometers or so below the surface was a slow overturning of the rocks.

Later, Art Maxwell ran another series of measurements from the East Pacific Rise to the Middle American Trench and found a continuous decrease in heat flow from the ridge to the trench, which was just sheer luck. He could have gotten measurements which would have shown the opposite, but this was what this particular set of measurements showed. We concluded from these measurements that there was a huge convection current going from the mid-ocean ridge to the trench, underneath the ocean. This is really the basic notion of sea floor spreading, of plate tectonics.

Results and Discoveries; Plate Tectonics

Sharp: So the research that you were doing on Midpac and making these measurements of these sub-bottom temperatures was built upon by succeeding expeditions at other places.

Revelle: Oh yes, sure.

Sharp: It takes a long time to be able to put all of this information together.

Revelle: Well, there've been thousands of heat-flow measurements now, and they show something quite different than we thought at that time. I guess I've got a blackboard out here. We might just look at it. [brief tape interruption]##

After thousands of heat-flow measurements had been made, a very complicated picture was found because of the hydrothermal circulation in the neighborhood of the mid-ocean ridges. Hydrothermal is a geological word which means that water circulation is due to difference in temperature driving cold water down and driving hot water up, in little cells of convection of water. The rock is badly cracked, and the cracks go down for two or three kilometers below the surface.

In fact, it has now been calculated that the whole ocean goes through the bottom rocks near the

Revelle: mid-ocean ridges in about ten million years and that really determines the composition of sea water, the exchange of salts between the rocks and the water. A very interesting business. We didn't expect that at all.

Our second great discovery on Midpac was that the sediments were very thin, only 100 or 200 meters thick. It had been calculated that the sea floor is a permanent feature of the earth's surface and that sediments had been accumulating there for literally billions of years, so you could get a very thick layer of sediments, and if you could drill through it you'd find sort of a record, a diary of the whole history of the earth. What we found instead was very thin sediments.

Because of this process I just showed you we now understand it. There is sea floor spreading, rock being formed at the mid-ocean ridges, moving across the ocean and sinking (being subducted) in the trenches. The sediments that accumulate on top of this rock are literally moving just as if they were on a moving belt, an assembly belt, from the ridge to the trench, and it takes them about 150 million years to do that. So nothing anywhere in the ocean is more than 150 to 200 million years old. The sea floor is very young. It's not permanent at all. A completely different concept from the ideas that were previously held. And that really came out of Russ Raitt's discovery of these very thin sediments.

Then, of course, there's been a lot of work lately, particularly the deep-sea drilling project, literally hundreds and hundreds of cores into the bottom, and they never find anything older than late Jurassic, early Cretaceous, about 150 million years ago.

Art Maxwell went on a famous drilling cruise, one of the cruises of the deep-sea drilling project in the South Atlantic, and he found that as you get away from the ridge you get older and older sediments, just above the rocky crust of the sea floor. The oldest sediments are right near the continents, the South American and the African continents, and they're about 150 million years old. Near the ridge they're only a few million years old, very recent.

Well, that was an elegant proof of sea floor spreading and the rate at which it happens. There are three or four different ways of measuring the

Revelle: rate of sea floor spreading. This was one of them.

Our third great discovery was about the flat topped seamounts in the Mid Pacific Mountains. There we found corals, shallow-water corals and shallow-water mollusk shells, which were late Cretaceous, about sixty-five million or seventy million years old.

Harry Hess, who had originally discovered these flat-topped seamounts, called them guyots, [spells out] after a geologist who had been an early professor at Princeton. There's a Guyot Hall at Princeton. Harry thought they might be early pre-Cambrian, that they might have been there on the bottom of the sea for two or three billion years. In other words, they were the original sea floor, peaks on the original sea floor which had been submerged by the gradual accretion of sea water, as more and more sea water came out of the interior of the earth.

Ed Hamilton, one of our scientific party on Midpac, showed that the corals were, as I say, about seventy million years old, and that the sea mounts had sunk during this relatively short time, geologically speaking, from the surface. These particular corals could only grow in shallow water, within, say, the top twenty-five meters. The same way with the clams. They were all shallow-water clams. So the tops of these seamounts had sunk 6000 feet, 2000 meters, in sixty-five or seventy million years. They were very young, not old at all, in geological terms.

The whole Mid-Pacific Mountains, for example, were formed during the last eye flick of geological time, compared to the rocks on the continents. That's true of everything in the ocean. There's nothing very old anywhere on the deep sea floor.

Sharp: Not so much now because people are used to the idea of all this heat and sedimentary exchange in the ocean, it's not such a new idea, though it keeps getting refined, but when you were first describing it in 1950 it would have seemed quite startling--the existence of a mountain range and all those things.

Revelle: It sure would. It was revolutionary. Nothing that we expected was true. Everything we <u>didn't</u>

Revelle: expect was true. The sea floor was young, not old. The sediments were thin, not thick. The heat flow was high, not low. The bottom was rough, not smooth, and so forth.

Sharp: And the fact of the continuous change. People like myself, who know very little oceanography, think of the ocean as having a permanent floor.

Revelle: Yes. Whereas it's constantly being replaced, renewed by interchange with the earth's mantle.

The concept of plate tectonics turns out to be of the very greatest importance for the history of life because if we didn't have the renewal of the sea floor, the earth continually renewing itself, pretty soon all the nutrients would be buried in the deep sea sediments, and living things couldn't find anything to eat. Plants wouldn't have any phosphate or any potash or any minor nutrients; because the nutrients would all be buried with organic matter on the deep sea floor. So life just couldn't persist. The persistence and continuing development of life really depends on the existence of plate tectonics on the earth.*

Plus the fact that carbon dioxide is continually being ejected from the mid ocean ridges and volcanoes near the subduction zones. Carbonate sediments are being swallowed up in the subduction areas, and then carbon dioxide is released again in volcanoes along the mid-ocean vents and along the volcanoes that come up on the island arcs, on the landward side of the deep-sea trenches.

So the whole concept of plate tectonics is a marvelous paradigm for the way the earth is, and the way life is.

Sharp: I've done just a bit of reading about plate tectonics, as sort of an overall system, describing what happens underneath the oceans and the relationship between the continents. It's not easy to understand, certainly, but it's plausible.

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*See John F. Dewey, "Plate Tectonics" Scientific American, 226 (May 1972, No. 5), pp. 56-68.

Revelle: Yes, that's right!

One of the great problems is how the subduction actually takes place. What the geophysicists think is that as the lithosphere gets cold, it gets heavy and actually sinks because it's heavier than the underlying rock, so it just goes down. And that starts the convective process.

Sharp: Are there other comments about Midpac that you should make?

I was thinking that we might contrast this early, important expedition with one that happened later on in your years as director to get an idea of what some of the differences were.

But if there're other things about Midpac that you want to discuss, we should do that first, and then move on.

Revelle: Well, we did a lot of other things. Bill Menard and Jeff Frautschy were on the ship too.

You see where the Hawaiian Islands are? [referring to world geologic map] This is not a topographic chart but it shows you the ages of the rocks at different places. Here in the west Pacific, you get early Cretaceous rocks, and here, on the east side of the Hawaiian Islands, you get much later, so-called Tertiary rocks. Here are the Marshall Islands, somewhere around here. What Bill and Jeff did was to come home along the Mendocino escarpment and trace that famous scarp. They now call it a transform fault. We didn't know what it was then.

It turned out later that Ronald Mason, who first appeared on our doorstep during the Capricorn expedition in 1952-53, managed to tow a magnetometer in a large area off the California coast, and he found a series of magnetic stripes on the sea floor, and that's what those different colors are, the stripes, although it's very much simplified. These stripes turn out to be rocks of different ages which were extruded during times when the magnetic pole was in the north hemisphere and other times when it was in the southern hemisphere. So you get what are called magnetic reversals about every 700,000 years, less than a million years. And in the Mendocino escarpment

THE MID-PACIFIC EXPEDITION OF THE UNIVERSITY OF CALIFORNIA AND THE U. S. NAVY ELECTRONICS LABORATORY

Two vessels participated in the expedition to the Marshall Islands, the Scripps Institution vessel HORIZON and USNEL's EPCE(R)-857. A total of over 25,000 miles of continuously recorded soundings was made during the expedition. Soundings in the deepest water were obtained even under storm conditions. The submarine topography was found to be irregular over about 80 per cent of the area covered, the remaining 20 per cent being very flat. Indications are that the topography over much of this area could be of use to navigation provided adequate charts were available. Several large seamounts were discovered. Well-rounded rock fragments were found on flat-topped seamounts (guyots) between the Hawaiian Islands and the Marshalls. A series of detailed sounding lines were made across a NE-SW trending ridge which lies west and south of the Hawaiian chain. This ridge, tentatively called the Mid-Pacific Mountains, has peaks which rise to within 450 fathoms of the surface, and has a maximum relief of over 2,300 fathoms. Some of the seamounts had flat tops; dredgings yielding reef corals and shallow-water shells prove that these flat-topped areas were once in shallow water.

Seventy-five cores up to 24 feet long were obtained during the trip. Most of them were in deep water, the deepest being 3,200 fathoms. Many of the cores showed alternating layers of different types of sediment. Near the seamounts several of the cores contained sand and even gravel layers. In a majority of the deep-sea cores one or more volcanic ash layers occurred. In a number of places cores penetrated only a small amount of sediment before encountering rock. Some of this rock, which was badly weathered, was found in the bottom of the cores. Indications are that many centers of vulcanism have existed in the Pacific in relatively recent time and have produced lava and volcanic ash which cover the sea floor in many places.

Dredging and coring around Bikini and the adjacent Sylvania Seamount showed a great deal of hard bottom. Even at 2,500 fathoms on flat bottom east of Bikini, the corer brought up fragments of coral. Volcanic rock coated with more than 4 inches of manganese was found in all dredgings on the edges of Sylvania Seamount. Several large fragments of reef coral were obtained on the saddle near the base of Bikini atoll. In a core at 1,500 fathoms on the slope of Bikini volcanic rock was found below 5 inches of globigerina ooze. A photograph indicated ripple marks on Sylvania Seamount at a depth of 750 fathoms. These are the deepest ripples yet discovered.

Evidence that the sea floor is a zone of weathering was obtained not only by the discovery of the weathered rock, but also by chemical studies of the interstitial water in the sediments. These contained up to one hundred times as much SiO_2 as the overlying water, plus considerable free

ferric hydroxide, and had a much lower pH and higher oxidation potential than the overlying water. All of these findings indicate solution of the bottom materials.

Geophysical investigations, made by refraction shooting using both vessels, show that the total thickness of sediments east of Hawaii and the mainland varies between about 1,500 and 3,000 feet. The sediments showed an increase in thickness towards the equator. Two major discontinuities are found in sound velocity, one at the base of the sediment layer where the velocity increases to 6½ kilometers per second, and another at a depth of 4 to 6 kilometers where the velocity is close to 8 kilometers per second.

Investigations of the surface currents during the expedition showed that, contrary to previous ideas, the equatorial currents have considerable north-south components. Using the geomagnetic electrokinetograph (jog-log), velocities up to 2 knots were measured in the equatorial counter current.

Investigation of the deep scattering layer showed that there are actually a series of deep layers which vary in thickness, depth, and character of organisms producing the scattering. Also it was found that while some of the organisms producing the layer move to the surface in a diurnal cycle, others stay at great depths. During the daytime schools of organisms often appeared as "blurbs" on the echo sounding records at depths of 10 to 40 fathoms.

Revelle: there are enormous displacements of these magnetic stripes, which are the transform faults. [brief tape interruption]

The main thing I would say about Midpac, to end up with it, is that because of its success we had a series of expeditions at least once a year from then on, for the next ten or fifteen years.

In 1951 there was something called Northern Holiday, which was led by Warren Wooster. In 1952 there was Shellback. I don't remember who led that. And then the Capricorn expedition, 1952-53. That was really the last time I was involved with an expedition as a sailor. After that I became a backstage type, getting other people to go on expeditions.

Capricorn

Revelle: Capricorn was a marvelous experience also. There we had our own two ships, Spencer F. Baird and Horizon.##

We first went out to Bikini with Horizon, and Horizon took part in the first tests of the hydrogen bomb in 1952. John Isaacs was out there and Bill Bascom, Walter Munk, and several other people but they are the ones I remember. Then we were joined in Bikini by Spencer F. Baird, and on Baird were Gustaf Arrenhius and Bill Menard and Bob Livingston, as a physician. And Bill Reidel. Most all of our geophysicists and geologists were on that Capricorn expedition. Russ Raitt was out there at Bikini and later on the entire expedition.

As I mentioned before, one of the things that John Isaacs and Walter Munk were concerned about was whether the big bomb would trigger a landslide; they conned the navy into spending a lot of money to prepare to move people off the Mid-Pacific islands if there should be a huge landslide and a consequent tsunami. They made quite elaborate preparations, at a cost of tens of millions of dollars. I was worried about it too. We all were because there evidently had been landslides on the atolls.

Revelle: Walter and Bill Bascom sat out in two small boats with wave measuring devices hanging a good ways below the surface. They didn't get any signal at all, and pretty soon it started to rain radioactive water, so they were taken off the boats and put on Horizon.

Anyhow, we started from the Marshall Islands and went down to the Fiji Islands and then to the Tonga Islands and then north to Samoa and east across the South Pacific through the Cook Islands to the Tuamotus, and then north from the Tuamotus to the Society Islands, to Tahiti, then northeast from there to the Marquesas, and then home across the central eastern Pacific, doing all the things we did on Midpac, essentially a repeat of the Mid-Pacific expedition but in a different area.

We had a new big winch on the Baird; this was our principal technical change. The winch held a tapered wire rope forty thousand feet long. It also had a spooling device that enabled us to spool different sizes of wire. What we had was a series of pieces of wire of different sizes all spliced together. The reason for that was that at that time it was impossible to build such a long wire rope of constant diameter that would support its own weight.

Sharp: It would have been just too heavy?

Revelle: If it were hanging down for 35,000 feet or so, it would have weighed twenty or thirty tons. A rope that was small enough to have that much wire and not weigh too much wouldn't support itself, so what we had was a big cable at the inner end and a small cable at the outer end, and then it would support itself. But the outer end had to be big enough to hold the weights that we were going to lower, the dredge and things like that. So we had at the outer end, as I remember it, about a 3/8 inch diameter cable and at the inner end about a 3/4 inch cable.

This winch didn't work very well.

Sharp: It would kink at the places where--.

Revelle: Not at the places where it was spliced, but it would overrun like a fishing reel, and then we'd get a snarl, a huge snarl, right there near the reel, near the winch itself.

Revelle: I think about the worst twenty-four hours I ever spent in my life was when we had something like 35,000 feet of wire out and the wire kinked; it snarled right near the drum. So we had to cut the wire and splice it. That meant you had to cut the wire and hang onto about ten tons of weight by wire clamps. We had enough unkinked wire inside the wire clamps so we could splice it. This took about twenty-four hours, and it was a nightmare because we were always afraid the damn wire clamps would slip.

Sharp: You know, you think of oceanography as quite elegant--

Revelle: Yes! [laughing]

Sharp: --with masterful discoveries, brilliant ideas, not of brute strength and having just the simplest problems.

Revelle: Oh yes, particularly when you're dealing with very deep waters. We were right over the Tonga Trench, which is one of the four deepest places in the world.

There are four trenches that are all within a few hundred feet of each other. The Marianas Trench east of Guam, the Mindanao Trench east of the Philippines, the trench east of the Kuril Island and Japan, called the Kuril-Japan Trench, and the Tonga Trench. They're all around 35,500 feet deep, within five or six hundred feet. The officially deepest one is now the Marianas Trench, but it could be that somebody will find an even deeper hole in one of the others by maybe fifty to one hundred to two hundred feet; the difference doesn't amount to very much.

We were trying to get some samples from the bottom of the Tonga Trench when this accident happened. We did bring up a sample of rock. The trench was empty of sediment. It was just bare rock down there. Probably there were some sediments, but very little. On the side of the trench Bob Fisher found a huge flat-topped seamount, which had actually been subducted into the trench, so it was part of the way down the trench with a sloping top. It was because of its having been subducted that its top was well below the surface. He did a lot of work sounding and surveying that trench and exploring what its bottom was like.

Revelle: You think of trenches as huge gashes in the sea floor, but of course they're rather flat. They're about twenty miles wide, let's say, and about seven miles deep, something like that.

Sharp: So they're fairly predictable in what they are going to be like?

Revelle: More or less, yes. Pretty much, in fact.

Some of them are flat on the bottom, because sediments have been accumulating in the center of the trench, but the Tonga Trench was virtually free of sediment, so it was V-shaped.

Sharp: And there's a reason why it didn't have any sediment?

Revelle: Probably there was no major source for sediments. The one that has a lot of sediments in it is the Acapulco Trench off Central America, which may even be a decaying trench, one that is not being actively formed by subduction at present. The reason that you don't have much of any sediment in trenches is that sediment is constantly being sucked down into the bowels of the earth, as the saying goes, by the subduction process.

Sharp: So they're constantly in the process of disappearing?

Revelle: That's right, yes. The sediments are.

Sharp: They sound a lot like marshes, in some ways, because marshes are always in the process of not being there anymore.

Revelle: That's right.

Well, the trenches are there, and they presumably stay there for a very long time, like hundreds of millions of years, but there's a continual renewal of the material in them.

Scientifically on Capricorn, I don't think we made any new discoveries of the same magnitude as Midpac, just a lot more of them, a lot more understanding.

The most interesting things were the more human things, like, for example, spending Christmas in the Tonga Islands and having a feast

Revelle: with Prince Tungi. He was the son of the reigning monarch, Queen Salote, but Queen Salote was in England watching Queen Elizabeth get crowned. So he was the head man, the de facto ruler while we were there, and he gave a big feast for us on his island of Tonga Tabu, a luau I guess they would call it. That was a very pleasant occasion with beautiful Tongan girls, one Tonga girl per man, kneeling by you and feeding you from banana leaves used as plates.

Sharp: An experience you wouldn't have gotten in America.

Revelle: That's right!

Another thing I remember was the Marquesas Islands. There was a book I read when I was a boy called White Shadows in the South Seas by a man named Frederick O'Brien which pointed out that the Marquesans were a dying race, they were disappearing very fast. But by the time we got there, there were quite a few of them, but they were all relatively small people. I don't mean four feet high, but ordinary-size people instead of being the large Polynesians that one would expect.

There was a man there named Rob McClintock. Helen Raitt tells about it in her book Exploring the Deep Pacific. He'd been there for fifty years. He sold shoes to the Polynesians, and he said that during his tenure in the Marquesas the average shoe size had gone down from about twelve to eight. It was quite clear what had happened, it was that there had been a seminal immigration. A few Chinese had emigrated to the Marquesas and married native women and they had had some descendants, and these descendants had survived and multiplied, whereas the ones that didn't have some Chinese blood in them died. And, of course, the Chinese, at least from South China, have relatively small feet; they're relatively small people. It was a very interesting anthropological phenomenon. The population was growing pretty fast with this new race of part-Chinese people.

Sharp: That's the kind of anthropological evidence that isn't particularly scientific, but it's so obvious--.

Revelle: Yes. I've never seen it written up, but I think Helen Raitt tells about it in her book. That's a wonderful book.

Revelle: I was fascinated by the Polynesians and how they lived, particularly in Tonga and particularly in an island called Vavau, which is one of the northern Tonga Islands.

Another thing I remember was coming up to the most northerly of the Tonga Islands, separated from the rest of them, and we were approached by a small boat. They had a sick baby on board, a sick child on board, very sick, and we had a doctor, Bob Livingston. The baby was dehydrated, had diarrhea, I guess, maybe even typhoid. Bob took it ashore, and, we never knew, but he may very well have saved its life. Bob Livingson is now on our faculty here at UCSD.

Sharp: In the medical school?

Revelle: Yes, in the medical school.

Sharp: Why had they come to your boat? Did they come knowing there was a doctor aboard?

Revelle: I think they just saw it was a lot of white men and they thought the white men might be able to do something. I think it was as simple as that.

Then, one of the other experiences I remember was at a place called Palmerston Atoll. Did I tell you about this?

Sharp: I don't believe so.

Revelle: Palmerston Atoll is a very isolated place in the South Pacific, one of the easternmost of the Cook Islands. The Cook Islands are scattered over millions of square miles. Palmerston is a place that had been settled by a white man named Ben Masters. When we hove to off the atoll, three boats put out from the lagoon and came alongside one at a time. When the first boat came alongside, a little brown man got out and said, "Hello, I'm Ben. I'm out of the <u>first</u> wife." After a while another little boat came alongside, another little brown man got abroad and said, "Hello, I'm Will. I'm out of the second wife." And a little while later Charlie, out of the third wife, arrived in the third boat.

This man, Ben Masters, had settled on Palmerston Atoll with three Polynesian women around 1860. Now there were four hundred

Revelle: descendants, but there were only seventy-five of them on the island, on the atoll. The reason for that was that about every twenty years or so they were wiped out by a hurricane, and their coconut trees were knocked down. The only thing they had to eat was seafood from the lagoon and the surrounding reef and it would only support seventy-five people, that was the carrying capacity of the atoll. So anybody in excess of that they'd ship off to one of the other islands, including New Zealand and all over the South Pacific. That's where the other 325 descendants of Ben Masters were, but there were seventy-five of them there on the atoll.

We had a lot of fun there because we decided to make the first survey of the place since Captain Cook. The only document we had was a chart, which said it was based on an Admiralty Survey in 1769. The only Admiralty Survey of 1769 was of course Captain Cook's. His ship, Endeavor, was the only British ship in the Pacific at that time. He had claimed that there were very strong currents around the atoll, two-knot currents. Then there was a later report, one hundred years later, in 1870, from a ship that had come there, and they said they couldn't find any currents at all. We didn't find any currents either, so we decided to make a survey, and we did. We found out that Captain Cook had been wrong in his navigation by about three miles. It was quite obvious why he was wrong, he was using a sextant to sight on coconut trees, and as you go around the atoll, you think you're sighting on the same tree, but in fact you're taking a fix on quite a different one.

Sharp: So all your measurements would be off, then.

Revelle: Yes, that's right.

We could make a much more precise survey because we had recording echo sounders that enabled us to chart our way along the bottom.

Sharp: So you were doing a little map making along the way.

Revelle: That's right. We made a map of the atoll, and we had the great satisfaction of out-navigating Captain Cook! [laughing]

Revelle: He was a wonderful man, a great explorer, a very humane and intelligent man.

This has nothing to do with my story, but he took a very dim view of scientists after his first voyage, and the reason he did was that he had a young minor nobleman named Sir Joseph Banks on his first voyage (Banks later became president of the Royal Society and was president for forty years.) When Cook was about to start on his second expedition, Banks wanted to go again, but he wanted to bring his mistress along also. He decided that in order to make her comfortable, he needed to build a big house on the stern of Endeavor. They did that, but when they took her off the ways into the harbor, she darned near turned over, she was so top heavy.

So Captain Cook, I don't know what he said, but he must have said something like, "Get that goddamn thing off of there, and make it fast!" They did take it off, and Banks didn't go on the voyage. Ever after, Cook took a very dim view of having scientists as part of his party!

Sharp: Well, Banks hadn't made a very good impression on him.

Revelle: Not after his proposal to build this house. Captain Cook probably didn't like it to begin with, and when the boat just about turned over in a slight seaway in the harbor, that was the end of it.

There were a couple of other interesting things about Capricorn. One was a place called Falcon Island, which was a disappearing volcano. Sometimes it's above the surface, and sometimes it isn't there. It's a typical guyot, really, only one that hasn't submerged yet. After an explosion it's covered with volcanic ash which at first is maybe three or four hundred feet high above sea level. Waves come along and wash away the ash, so it's then maybe fifteen fathoms below sea level. Less than that. About ten fathoms.

Walter Munk and our other scuba divers were diving on this Falcon Bank. By that time it was called a bank. When it's below sea level, it's called Falcon Bank, when it's above sea level, it's called Falcon Island. Walter was measuring the temperature of the rock there, which was presumably quite hot because it was a young

Revelle: volcano. He looked over his shoulder and there was a shark watching him, looking over his shoulder, reading the thermometer! [laughing] So the only thing to do was to keep on reading the thermometer and tell the shark to go away, and it did, so he got back all right.

Sharp: Walter had some pretty interesting experiences, between that and being in the small boat and missing the signal on the recorder.

Revelle: That's right.

Sharp: Things really happened to him.

Revelle: That's right.

One of the crosses I had to bear on that trip was Bill Bascom. He had had an operation a few months before, and they'd sewn him up again. He had an inoperable cancer, a huge cancer on his hip. We all expected him to die at any time, so we were very considerate of him, but he was his usual ornery self! I made him chief scientist of Baird--we had a chief scientist on each ship--and he was prepared to die, so he was very reckless and took all sorts of terrible chances, not only with himself but with other people too, particularly with the heavy wire cables; if they break, they cut you right in two when they slip across the deck. He would work with them over the fantail and things like that.

I remember one time I said or somebody said, "Bill, you're awful hard to get along with," and he said, "If you think I'm hard to get along with, what do you think about me? I have a hell of a time getting along with myself." He later mellowed quite a bit, and did many wonderful things, including inventing the method for deep-sea drilling.

Sharp: Having someone like that on a ship like that would certainly change the whole atmosphere on the ship, I would think, because of the kinds of chances he was taking.

Revelle: Yes, it was not easy.

But we also had Helen Raitt on board, and she was a civilizing influence, as many other women would be. It's a tremendous difference what just one woman makes, but in her case there was also a

Revelle: loving influence. She was a very warm-hearted woman.

Sharp: The book that she wrote is written with a great deal of warmth and liveliness.

Revelle: That's right. She was like that. She couldn't write very stylish English, but she had such wonderful stories to tell. The book was very nice.

One other place we dived was called Alexa Bank, north of the Fiji Islands. That was very interesting. It was a miscarriage, an aborted atoll, an atoll that never made it. It was about ten or fifteen fathoms below the surface. The water was so clear you could see right down to it. There were special kinds of corals that lived at those depths, but they couldn't grow up to the surface, the bank had sunk too fast. Bill Bascom and Walter Munk and our other divers dived on that bank and had a very nice time.

Sharp: Did you ever do any diving?

Revelle: Never did.

Sharp: Was there a good reason for that?

Revelle: No reason, except I'm not a very good swimmer, and you really have to take Scuba lessons to dive. In fact, one of the things we did here at Scripps for many years, and probably still do, is to teach Scuba divers from all parts of the University of California how to use a Scuba.

Sharp: That started while you were director, didn't it? That effort?

Revelle: Sure. Scuba started at that time, and that's why the teaching started! Our first diving instructor was a man by the name of Connie [Conrad] Limbaugh, whom you may have met, if you were here.

Sharp: No, I didn't.

Revelle: Maybe not, because he was killed in the early sixties.

Sharp: In a diving accident?

Revelle: Yes. He went over to France to visit Jacques Cousteau and his group, and they were diving in a cave somewhere on the Mediterranean coast of France. Connie was one of those guys who had no fear at all, and he went into the cave and got lost and went the wrong direction and ran out of air, and they finally found him up against the top of the cave, with no air.

Bill Menard was there, looking for him, and our other divers were too, I was in Paris at that time and I spent a week, talking with Jacques Cousteau about three times a day, about what we were going to do next, looking for Connie. Bill Menard said he was scared to death going into that cave because there were many different passages, and you didn't know which was the one that led out, you couldn't see.

Sharp: How very frightening.

Revelle: Oh, it was. It was terrible.##

III APPOINTMENT AS SIO DIRECTOR, 1951

Putting SIO on the Map World-wide

Revelle: The appointment, as you could tell from some of the correspondence, was vigorously opposed by several faculty members here, and I think for good reason.* At least I think they were quite sincere about it. As I said, I was very poor at keeping or breaking appointments, and I was pretty headstrong. I wanted to have my own way about many things. I hadn't really learned how to manipulate a committee, which I did learn later; I got very good at it, in fact.

They kept on writing letters to President Sproul about how almost anybody would be a better director than me, and particularly a guy named Harry Ladd, who was one of the principal geologists in the [United States] Geological Survey, and a good friend of mine, a very good friend of mine. I didn't know about this, any of this; it was all done without my knowledge. So I didn't know about Harry Ladd.

Sharp: Was Harry Ladd actually interviewed?

Revelle: I don't think so. I don't really know though, but I don't think so.

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*See letter to Sproul, dated 12 May 1950, on pp. 47a-b, Robert Gordon Sproul Collection, The Bancroft Library.

UNIVERSITY OF CALIFORNIA

THE SCRIPPS INSTITUTION OF OCEANOGRAPHY
LA JOLLA, CALIFORNIA

PERSONAL

May 12, 1950

Mr. Robert G. Sproul
President's Office
University of California
Berkeley 4, California

Dear Mr. Sproul:

We note that the Scripps staff has been asked to sign a statement indicating approval of Dr. Revelle's administration and urging his appointment to the directorship. Some of the signatures probably represent a very real and definite endorsement. The lack of our signatures means of course that we have not changed the opinion, already expressed, that his appointment would not be in the best interest of the Institution. We hold him in high regard as friend and as colleague. We have cooperated heartily with him as Associate Director and as Acting Director and would make every effort to continue such cooperation should he be appointed Director.

We have avoided detailed expressions of opinions regarding Dr. Revelle's candidacy, but feel that it is now desirable to mention some of the reasons why we have not favored his appointment as Director.

(1) The active and obvious campaigning has displeased us.

(2) Offsetting favorable qualifications that the Scripps staff has already noted, are some glaring administrative faults, including marked inconsiderateness of the time and convenience of others. In fact, he seems to have almost no conception of time, either in meeting appointments or in closing meetings. He is often inconsistent, as in making sudden changes of assignments, sometimes with hardly any notification to those previously given the responsibilities. He often organizes meetings poorly. Out of a generous heart he added to the staff a psychopathic misfit who had been dropped from NEL (other appointments, however, have been wise). By his own admission Dr. Revelle is not a very efficient administrator, a fault which has been recognized by both Dr. Sverdrup and Dr. Eckart.

(3) Dr. Revelle's occupations, including extramural interests, take him away from the Institution a large proportion of the time and keep him so rushed when he is in his office that appointments often prove difficult, particularly during ordinary hours. We fear that with the tightening international situation this difficulty will be accentuated.

(4) Whereas full consultation is often held with parties concerned, some actions have been taken that seem arbitrary to us. Similarly, though the committees that he set up have often been accorded appropriate assignments, at times, as in the last submission of the budgets, the committees have not been consulted, or even apprised of action taken.

(5) It is felt that Dr. Revelle's actions have resulted in the loss of some major subsidies for important scientific work at the Institution.

(6) Dr. Revelle's impetuous enthusiasm and crusading spirit command respect, but throughout his career have led him repeatedly to drop a major project in mid-stream when a more appealing enterprise caught his imaginative attention. We feel that he lacks perseverance. This failing is largely responsible, we think, for his limited scientific output, and also affects his administrative projects.

(7) There have come to our attention indications that Dr. Revelle would not live up to our ideal of a director who will lead the Institution forward on a broad and well-balanced line.

At your convenience, we should appreciate an opportunity to discuss with you this and other matters important to the future development of Scripps Institution.

Sincerely,

Denis L. Fox, Professor of Biochemistry

Carl L. Hubbs, Professor of Biology

Francis P. Shepard, Professor of Marine Geology

Claude E. ZoBell, Professor of Microbiology

Revelle: They did offer the job to Teddy Bullard--by "they" I mean the university--and Teddy turned them down and strongly recommended me, according to the correspondence. I haven't seen the correspondence, but Deborah Day has it.*

This hung on, this impasse or this conflict hung on even after the loyalty oath controversy and after the Mid-Pacific expedition, and it was not at all clear that I would be appointed director. This was quite an important job in oceanographic circles or in marine biological circles, so there were a lot of rumors flying around the country about it. I think. Although I didn't ever hear very much about it.

Finally my friend Lionel Walford, Burt Walford, who was the chief biologist for the Fish and Wildlife Service or the head of the research part of the Fish and Wildlife Service, said, "What you've got to do is to have a conference about the future of the Scripps Institution, and if you really put across that conference successfully that may cause the opposition to evaporate."

So I did just that, and you found several records of my asking President Sproul for money for the conference and writing him about it and getting him down here, and he did come, and sat through the whole conference.

Sharp: And that was part of the whole Charter Day celebration--.

Revelle: Yes, that was in March of '51.

One of the things we did was to dedicate the aquarium museum, the Thomas Wayland Vaughan Aquarium Museum. I remember one faux pas I made there. Dr. Vaughan was unable to come, but he did make a recording of a speech, and I completely forgot to play his recording! I made a speech and President Sproul made a speech, and we had ministers who gave invocations and things like that.##

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*See Bullard's review of Betty Shor's book on the history of the Scripps Institution "Varnished Tale of Scripps," E.C. Bullard, Nature.

Revelle: I got Det [Detlev] Bronk to come to the conference; I was kind of a pet of Det Bronk's, who was the president of the National Academy of Sciences.

Evelyn Hutchinson and Columbus Iselin came, and Giff [Gifford C.] Ewing and a lot of people from the university, but the most important was President Sproul.* It was a very successful conference. We had it at the [La Jolla] Beach and Tennis Club.

Sharp: Some of the other people who were there were representatives of petroleum and fisheries organizations.**

Revelle: Right. API. [American Petroleum Institute] For one, I guess Burt Walford was there for the Fish and Wildlife Service.

Sharp: And Evelyn Hutchinson and Daniel Merryman, both from Yale, and Admiral Solberg from the Bureau of Ships.

Revelle: Did he come?

Sharp: Well, he was invited. I was not sure he came.

Revelle: He was invited, both Solberg and Nimitz were invited, but I'm not sure they came. I don't remember that. You couldn't find any record of the conference, then?

Sharp: No, I didn't. What I found was a letter that you were sending, inviting people.

Revelle: It was a letter to Sproul telling him whom I was inviting.

Sharp: Right. Then there were a couple of the actual letters that you sent to different people. Let me see if I can find those for you. [leafs through papers] Oh, Dean Boelter.

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*See letter to Sproul, dated 19 June 1950, SIO Revelle collection, 81-23, Box 1, SIO Archives, on p. 49a-b.

**See letter to Sproul, dated 7 February 1951, SIO Director collection, Box 1, SIO Archives, on p. 49c.

June 19, 1950

President Robert Gordon Sproul
250 Administration Building
University of California
Berkeley 4, California

Dear President Sproul:

This letter is written with a two-fold purpose: first to suggest that in making up the schedule of Regents' meetings for 1950-51 you might be able to plan one meeting during the spring of 1951 on the La Jolla campus; second, to invite you to be the principal participant in the dedication ceremonies for our new museum-aquarium building during charter week of 1951.

In support of the first suggestion I should point out that the Regents may not be aware of the great expansion of this institution during the last ten years, and we would very much enjoy the opportunity of giving them a firsthand demonstration. It is no longer even approximately true that the Scripps Institution "consists mostly of a sea wall and a pier, both in need of repair", as one Regent is rumored to have said a few years ago.

Since 1940 our budget has increased more than ten-fold and our staff has grown from 35 to nearly 250 members. Before the war we operated one ship on a part time basis; we now have four relatively large vessels and several smaller craft. Although the number of students here is only a miniscule fraction of the total university enrollment, this number has also increased nearly tenfold since the days before the war. Actually we are now operating two "campuses", because almost half our staff is housed in temporary buildings on the grounds of the Navy Electronics Laboratory in San Diego. These include the supporting facilities and administrative headquarters for our fleet, several of the scientific divisions of the Marine Life Research Program, the cooperative laboratory of the United States Fish and Wildlife Service, the Marine Physics Laboratory and a good half of our division of Submarine Geology.

A Regents' meeting here might also have value from the standpoint of the University's public relations. According to the latest census, the population of San Diego County is

600,000 and to many of these people the Scripps Institution is a visible symbol of the University.

As you know, La Jolla is a pleasant place, and many of the Regents might enjoy the opportunity to spend a few days in its atmosphere of beauty and tranquility. I believe our two major hotels, the Casa de Manana and the Valencia, could easily accommodate them. Our library-reading room could serve as a meeting place of adequate size and privacy.

In a letter of March 7, 1950 to Professor Garff B. Wilson, I have already briefly discussed the possibility of a dedication ceremony for our new Museum-Aquarium during charter week of 1951. A public dedication is very much in order for this building since its exhibitery features are primarily for the use of the public in the San Diego area, and it should represent for many years to come a major attraction for visitors to this part of the State. We are planning that the museum shall portray all aspects of the sciences of the sea, and in this respect it will be unique in the Western hemisphere. The only other true oceanographic museum in the world is at Monoco, and it has fallen pretty much into decay since its founding nearly fifty years ago.

Even our present small, dark aquarium and dusty little museum attract some 50,000 visitors each year; this number may well be tripled when the new building is opened. Your presence at the dedication ceremony would add lustre and dignity to the launching of the Museum and Aquarium, and would emphasize to the people of this area the basic unities underlying the diversity of the University.

Respectfully,

Roger Revelle,
Acting Director

RR-b

CC: (Blind)
Professor Hubbs
(Chairman, Public Relations Committee)
Professor Johnson
(chairman, Building, Space and Facilities Committee)
Mr. Hilton

7 February 1951

President Robert G. Sproul
250 Administration Building
University of California
Berkeley 4, California

Dear President Sproul:

Regarding the conference of which I spoke, it is our feeling at Scripps Institution that the present national and international situation requires a careful evaluation of the position of this Institution and of our aims and viewpoint. We have therefore planned a conference of carefully considered advisors, each a man of high attainments in his special field and of pervasive mind. We have invited ten men from the University of California and ten from outside. We consider this program of inestimable importance, and thus it is highly desirable that neither we nor the recipients of our invitations feel any impediment to attendance. It is therefore important to pay their transportation and their living expenses during the conference. For this purpose it would be greatly appreciated if $2,500.00 could be made available from the President's Emergency Fund. This sum probably would be adequate for the anticipated expenses with some balance which would be returned to the fund.

Sincerely yours,

Roger Revelle
Acting Director

JDI:ch

Revelle: And Mike O'Brian, probably. I remember this letter to Dean Boelter. You gave me that too.

Sharp: I did? Okay.

Revelle: And Paul Dodd, and Cal Epling, all these characters from UCLA were there, including Louis Slichter.

Sharp: But I didn't actually find anything that told me what occurred during the conference and just what was being said and, indeed, what you concluded the role of Scripps might be.

Revelle: Well, I must confess that I'm making this up without having any record, and it may be wrong, but I guess that we would have concluded the following things: that we should strengthen marine biology (and I'll tell you a little bit about that later) a lot; that there were great opportunities in marine biology; and that we should take the whole Pacific Ocean as our oyster. We later took the world ocean as our oyster, but for the moment it was the Pacific, which is half the world, literally half the world. We probably felt that we should continue our deep-sea exploring and that there was a considerable need for more oceanographers.

It was about this time that the second National Academy [of Sciences] committee on oceanography reported. On that committee were Eckart, Revelle, Iselin, and Slichter. I've got a list of all the people at home in my briefcase. There was a first National Academy committee in 1927 to 1932 or '33. This committee was headed by the president of the National Academy of Sciences, Frank Lilly. Dr. Vaughan was on it and Henry Bigelow was on it and various other elder statesmen.

They recommended the establishment of the Woods Hole Oceanographic Institution, and as a sort of sop to the vulgar, they also recommended a new building at Scripps and a new building at the University of Washington. This resulted in a $3 million endowment for Woods Hole and a $40,000 grant to Scripps and about the same amount to the University of Washington.

The basic outcome of this first committee was a book written by Henry Bigelow on the status of oceanography. It's a very good book, a very enthusiastic book. Woods Hole was established

Revelle: then in 1930 or '31, with Bigelow as the first director.

The second Academy committee was after World War II. They published a report in 1951, I think called "Oceanography 1951." Most everything in that report was pertinent to the Scripps Institution, including the need for more faculty, support for expeditions, need for private donors, a broadening of the base of oceanographic support, doing a lot of both basic and applied research, particularly trying to build up biological and chemical oceanography.

But the primary thing that the Academy committee said in the 1951 report was that we needed more oceanographers, and the problem was to train them properly and how are we going to do that? There were two ways to do it. One was by training them at oceanographic institutions, like we were doing at Scripps. The other was to train them in the basic sciences and bring them in as professionals after they had their Ph.D. in physics or chemistry or mathematics. Columbus Iselin was in favor of the latter, and I was in favor of the former, so in fact we compromised and said we ought to do both, and that was right, I think.

At that time and ever since we've done both. Woods Hole now teaches a lot of oceanographers, and we teach a lot of oceanographers, and not so many come in from other professions as used to. In Columbus's day most people at Woods Hole had gotten educated in something else.

Revelle: I think we must have spent a lot of time at this conference in La Jolla talking about how we could produce more oceanographers. This was a big thing, of course, for the University of California, in any case.

Sharp: Having participants in the conference who represented different industrial interests--fisheries, for example, or different parts of the oil industry--they would then have their own specific interests.

Revelle: Yes, although in general they all said we just have to know more about the ocean. That was the fashionable thing to say, probably. [brief tape interruption]

Revelle: California was pretty much a province of the East. It was thought of by the East as an outback sort of place. It's hard to believe that today. When I was a boy, there were nothing but wheat fields from the west of Figueroa Street to Santa Monica.

Sharp: The way Los Angeles is now, that is really hard to believe. You might wish for those wheat fields back again, the way some of Los Angeles has developed.

Working Through the Opposition

Revelle: So that having all this attention from all these easterners was mighty impressive to local boys, and there was never any question, as far as I know, any question after that about my being appointed director. But I really don't know; I never looked into all the correspondence. All I know is what Deborah Day has put in that biography of me.*

Sharp: I have looked through President Sproul's papers at The Bancroft Library and a lot of the behind-the-scenes work about your appointment isn't there.

Revelle: Do you suppose it exists anywhere or he kept it all in his head?

Sharp: Some of it he probably kept in his head and some of it may be in the papers of the regents, or it remains with the individuals involved.

Revelle: Miss Robb is still alive, Agnes Robb, isn't she?

Sharp: I don't know.

Revelle: It was always very interesting to go to President Sproul's office because he was a clean-desk man.

Sharp: Well, Miss Robb really did it, then.

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*Readers may see "A Guide to The Roger Randall Dougan Revelle Papers, 1928-1979 in the Archives of the Scripps Institution of Oceanography," prepared by Deborah Day, Archivist, SIO Reference Series 85-26 1985.

Revelle: You'd go into her office next door and the whole goddamn floor was just covered with papers. Every square inch of the floor had a pile of papers on it. It was fantastic! [laughing]

Sharp: She was really something.

Revelle: She sure was.

Sharp: Right now, to those of us who look at the papers much later on, she really organized them very well. She kept records of phone calls.

Revelle: Well, she was absolutely invaluable to Bob, and maybe to Ide Sproul too.

Sharp: Yes, and over a long period of time.

Revelle: Oh yes.

Sharp: That consistency is really important.

Revelle: But, as I say, I don't really know much about it, what Bob did, about my appointment.

What did you find out?

Sharp: Well, there was some opposition to you which was very obvious, and there was support for you which was also very obvious, and maybe we could just start with that. For example, Vern Knudsen, who headed the special committee that was appointed to find the successor to Sverdrup. Eckart was seen as temporary from the very beginning. So from the period of the beginning of Eckart's tenure there was this off-again, on-again search to find a permanent successor.

Now, in February of 1950, President Sproul presented Eckart's resignation to the regents and your name as acting director at that point.

Revelle: It was February of 1950?

Sharp: Right. February 24th, actually, of 1950.

Revelle: Yes, that was the same day that they had this loyalty oath business.

Sharp: Yes, it was a pretty heavy-packed week!

Sharp: But I don't know how it was actually settled on that you would be acting director. I mean was it totally obvious? How were you asked, or were you asked to become acting director?

Revelle: Yes, I must have been asked. Or probably told!

Sharp: By President Sproul?

Revelle: Yes, I guess so. I don't think that he actually called me up or anything. I don't remember being called by him.

Sharp: You got a letter?

Revelle: God knows how it happened! It was probably just in the minutes of the regents or something like that.

Sharp: Since there was this period of time when there was an active opposition to you, for several years before, you know, since the point where Sverdrup says, "I'm going back to Norway and you better figure out what to do," do you remember what the reaction was when you were named acting director among the people at Scripps? Some people would have thought that was great, some people would have thought that was a very bad, bad thing to have occurred.

Revelle: I don't think so. I think they all expected me to be acting director, just because I was associate director, and who else would do it? I don't think there was any problem about that. The problem was about a permanent appointment.

Sharp: Was there any consideration of Carl Hubbs?

Revelle: No, not that I know of. There was no consideration, as far as I know, of anybody here at Scripps except me, but again I may be wrong.

Sharp: And Dr. Sverdrup had supported you to be permanent director?

Revelle: Very much so, yes. He wanted me to be director.

Sharp: That's what shows up in that one letter, that very--

Revelle: Nasty one.

Sharp: Yes.

And Dr. Eckart, did he have--

Revelle: He wanted me to be director. They both did. Walter Munk and Carl and Harald all wanted me to be.

Sharp: Dr. Munk was one of the people who went to visit Vern Knudsen when Knudsen was recovering from a heart attack. They went to visit Knudsen to support your--

Revelle: Yes, who were they? Do you remember who they were?

sharp: I do know. [looks through papers]

Revelle: Deborah has it in the biography.*

Sharp: Yes, it is there.

Revelle: Vern was also in favor of my being appointed, as I remember it.

Sharp: [finds reference in papers] Oh, Dr. Fred Phleger.

Revelle: Phleger, yes.

Sharp: They were the two who visited.

Revelle: Just two? Just Munk and Phleger?

Sharp: I think so.

Revelle: Not Raitt?

Sharp: Perhaps so, but I don't know for certain.

So there was this division, then, within the faculty at Scripps?

Revelle: Oh yes, sure. The people who were against me were Fox, ZoBell, Shepard, and Hubbs. The only one of those four who was more than mediocre was Hubbs. The rest were rather poor scientists. They'd all been here when I was a graduate

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*See Revelle Biography in Day guide to Revelle papers, pp. 15-16.

Revelle: student. I think it was at least partly just, you know, the hometown boy never amounts to very much! They could see all my faults, which I have plenty of, certainly.

Sharp: Is there one particular reason that you think they were opposed to you, or a lot of different reasons? You mentioned your role as an administrator had some weaknesses. Was that it?

Revelle: I don't think so.

I don't know, this is sheer guesswork and, perhaps second thoughts sort of thing, but I was not very tactful, and never really thought very much of ZoBell and Fox, let along Shepard, as scientists.

Sharp: And they knew it?

Revelle: And I must have let them know it, yes, in various ways. They never contributed very much to the institution. Shepard contributed more than the others. Particularly the Mid-Pacific expedition; was such a different magnitude of discovery than anything <u>those</u> guys had done. Fox and ZoBell never went to sea, or very rarely went to sea. Shepard went to sea a lot. He was primarily a descriptive geologist. You ought to talk to Walter Munk about that, if you have a chance.

Sharp: I'd like to.

Revelle: He knows a little bit more about it than I do, quite a bit, and I'm sure he'd give you a more objective account.

But, you know, I'd been in the navy and I'd run these big operations, like the Crossroads Operation and the Crossroads resurvey, and I'd been developing the program of the geophysics branch of ONR, and I did a lot of things during the war where I got a national reputation which they probably didn't think I deserved.

Sharp: Were they jealous?

Revelle: I don't know, but I wouldn't be surprised. You know, it was really sort of a typical conflict between, to put it very crudely, between sort of a national figure and a bunch of very local figures. I was very well thought of in Washington.

Sharp: Why did you want to do that? Why did you want to be director?

Revelle: Why did I want to be director? Because I thought I had all these wonderful ideas about what Scripps could do. You know, I thought we could make a tremendous place out of it, and we did. It's amazing how it developed.

Sharp: Was there some point where you thought the opposition might work? That the opposition would be so persuasive that you wouldn't get it?

Revelle: Oh yes, I was often depressed about it, and I feel very lucky that President Sproul finally decided to have me. He made it up all by himself, so far as I can make out. There was never any committee that I know of. He didn't follow any formal procedure.

Sharp: Well, Vern Knudsen's committee was--

Revelle: Still active?

Sharp: Yes.

Revelle: Oh, were they?

Sharp: Oh yes. What they were doing I don't really know. I know Vern Knudsen was head of it.

Revelle: I didn't realize they were active after they'd selected Carl [Eckart].

Sharp: Well, I <u>thought</u> they were, but again I don't really know.

Revelle: I don't either.

Sharp: I can't seem to nail it down anywhere.

Revelle: In any case, it seems to me that everybody, not everybody, but Knudsen and Sproul were completely convinced by this [1951] conference.

Sharp: When President Sproul was here, it would seem that there might have been an occasion for you to talk with him privately about your being director.

Revelle: He never said a word, and I never said a word.

Sharp: Did you want to and refrained purposely, or did you think that it wouldn't be appropriate?

Revelle: It never occurred to me. I just didn't think it was kosher.

One of the things that I am a little bit puzzled about in that letter was "the obvious campaigning."* What obvious campaigning?

Sharp: Well, I wondered about that. There wasn't anything you did that seemed obvious campaigning to you?

Revelle: Not that I can think of, except I was pushing pretty hard on the loyalty oath, but I didn't think of that as campaigning, just the reverse.

Sharp: And the idea of having President Sproul come to be part of the dedication?

Revelle: Oh, that was certainly campaigning, no question about that, but that was long after the letter was written.

Sharp: Right, yes, so I wasn't sure what was being referred to as the campaigning.

Revelle: I never lifted a finger to suggest that I should be appointed director, to anybody in the university--although I did talk about it to some people like Burt Walford. I wanted to be very much, and I obviously did some things like the expedition [Midpac] and the loyalty oath and the [1951] conference.

There was one other thing, which I'm not sure about the timing of, and I notice you have no record of this at all in your papers. We organized a real estate subdivision called SEA. Did you ever hear of that? Scripps Estates Associates. I'm not sure that that happened before or after I was appointed director. I could find out though.

The faculty had been trying for years to get a piece of land where they could build their own houses. I was very much in favor of this because most of them lived in these little cottages on the grounds of the Scripps Institution, and they were thought of by people in La Jolla as sort of nutty.

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*See letter to Sproul, dated 12 May 1950, on pp. 47a-b.

Revelle: You know, certainly not real citizens of the city or the village; interlopers from another planet in some way, queer people. And, in fact, it used to be called the Bug House, this place, because of the biological stuff.

Sharp: Right, but inferring that it was really a Nut House, perhaps.

Revelle: Yes. And Mrs. Vaughan tried very hard to overcome this feeling of inferiority on the part of the Scripps staff. She'd have a tea at least once a month. She was a proper New England lady named Dorothy Quincy. She was very much interested in getting the Scripps faculty to be accepted as members of the community by these teas, but it never worked very well.##

Sharp: She would invite people from the community to come, and the idea was to get together?

Revelle: Once a month, yes, exactly. Twenty or thirty people from the community. It was sort of pathetic, in fact, how hard she tried and how little effect she had. But the principal reason, I thought, was because our people lived in these goddamn cottages, which had very cheap rental, and were very cheap places.

Ellen and I lived in Cottage 24, for example, when we were first married. Dick [Richard H.] and Alice Fleming lived next door to us. Horace Byers and his wife, Frances, all the the graduate students lived in them, and most of the faculty. Martin Johnson and Claude ZoBell and George McEwen. Norris Rakestraw later.

Scripps Estates Associates

Revelle: I felt what we had to do was to get our staff members to be real property owners and taxpayers and involved citizens of the community. So I felt strongly that we had to have them build their own houses. They had tried two or three times before this to buy some land and had not succeeded.

Russ Raitt and I drove up to see a man named John Poole in Corona del Mar, who had laid out a subdivision called the Poole Street Subdivision just east of La Jolla Shores Drive. All those

Revelle: small houses you see on the east side of La Jolla Shores Drive are the Poole Street Subdivision. We said that we'd like to buy the land that he owned west of La Jolla Shores Drive, as much as we could for $50,000, which was all we had.

So he sold us forty acres of it, counting Sumner Canyon, and kept the front three acres for himself, the front part of the property where that awful man Mr. Fargo has now been building those terrible houses. This was a great triumph, $50,000 cash for that land, essentially $1000 an acre. It's now worth about $200,000 an acre, at least.

We got the land, and then we consulted Bill Wurster about the subdivision plan. He was the dean of the School of Architecture at Berkeley, a famous American architect, one of the great American architects. He said it should be kind of an amphitheater facing the sea as much as we could, and he described the kind of houses we should have, one-story, earth-colored houses with dark roofs and careful architectural control. He drew up a kind of a tentative plot plan for us, all free.

Then we had that drawn up precisely and laid in streets and sewers and water and power and telephone lines. Unfortunately, the power lines were all overhead; we weren't able to afford underground lines. The gas and electric company took a very dim view of underground lines, and they were unwilling to do it for less than $1000 a lot, and that was quite a bit of money in those days.

Then everybody built his own house, but several of them joined together to get a more or less standard design. Several of the houses have basically the same design. Most of them are wooden houses.

Sharp: This was all taking place in 1950?

Revelle: No, it happened in the summer of 1951 after I was appointed director.

That was, anyhow, one of the most important things I think I ever did, to push through this staff subdivision.

Revelle: We had all kinds of rules that John Isaacs took great pleasure in devising, primarily to prevent people from speculating. What we did was to subdivide the land into forty lots. Then we had a lottery to choose priority numbers, one through forty, and each person, according to his number, could choose one of the lots.

Sharp: The person that had number one could choose first?

Revelle: Yes, that's right.

The lots had different prices. The front lots cost about $5000 and the rear lots cost about $2000. So if you had choice number one and you didn't have much money, you could take a back lot; if you had more money you could pick a front lot. ##

[Date of Interview: 14 August 1985]##

Sharp: Yesterday we talked about your appointment, and I think we discussed all of the issues surrounding it, unless there's something that you've thought of since we left off yesterday.

Revelle: I don't think so.

One thing I did mention was our Scripps Estates Subdivision. John Poole kept the front three acres, the part going down to the beach, which he finally sold I guess for quite a large price. Those are those awful wooden houses that are on the slope in front of SEA.

Sharp: The building of houses in SEA, then, got under way after the land was acquired?

Revelle: Yes. As I said, Bill Wurster drew up a preliminary plan for it, and then we had a man by the name of Burl Phelps, who was a surveyor and planner, do the final plot plan. Then we put in roads, water, electric utilities, and sewer lines, and all the things that it takes to what they call "manufacture" a subdivision.

So far as I can make out, it all cost about twice as much as we paid for the land, about $100,000. So the total cost to individual lot owners, all of them members of SEA, (Scripps Estates Associates), when the land was subdivided into forty lots, was the average price per lot, about $3000.

Sharp: That sounds like a real bargain.

Revelle: It was. Even in those days.

What was satisfying about it was that all the professional developers were quite skeptical that we could do it. You know, a bunch of professors carrying through a subdivision. The reason it was possible was that Jeff [Jeffery D.] Frautschy, who was my assistant, took a major responsibility for carrying it through. He was basically a geological engineer and he could handle the engineering supervision of it.

Sharp: In looking through the reports of the director's office, there really isn't much mention of this going on. I'm not sure quite why. It doesn't show up, at least in most of the records that I saw.

Revelle: Well, Deborah [Day] missed it completely, as far as I can make out.

One of the amusing aspects of it is that, according to George and Betty Shor, who are sort of the keepers of the [SIO] flame, my business manager, John Kirby, wrote a letter to President Sproul deploring this whole exercise! I've not seen that letter, but Betty was going to look it up, and maybe she can find it for us in the archives.

Sharp: Now that I have a date, I can look in the Sproul papers and see if I can find that letter because there is a lot of correspondence going back and forth between John [C.] Kirby and Jim [James H.] Corley, and Corley and Sproul talking about Scripps.* Generally, it concerned things that were not quite up to what Mr. Corley thought they should be. So this SEA incident may fall into that category.

Revelle: Yes, it probably would.

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*Corley was the vice-president for business affairs at The University of California.

IV THE DIRECTORSHIP IN FULL SWING

Transition

Sharp: I was going to ask you what it was like taking up the full responsibilities of being director, but you had mentioned yesterday that you were acting like a director even when you were acting director.

Revelle: Oh yes, sure.

Sharp: So maybe that's a question that doesn't even need to be asked, since you were getting going.

Revelle: I don't think so. I was learning how to do it. But that was true even when I was associate director, I pretty much took a lot of responsibility.

Sharp: And that was all right with Eckart?

Revelle: Oh yes, sure. He wanted me to do it. He wanted to get out of the job as soon as he could.

Sharp: And everything that you took on as a responsibility was less that he had to deal with, in a way.

Revelle: That's right.

Sharp: There is the matter of the opposition. Once you became director and people were getting used to the idea of your being director, how did you deal with the opposition?

Revelle: Well, there wasn't any, really.

Fran Shepard had a project with the American Petroleum Institute [API] to study the Mississippi Delta and the coastal region down there. Claude ZoBell had another project with them on the microbiology of either the formation or the removal of petroleum, the decomposition, and probably both, oxidation as well as formation of petroleum. I was helpful in maintaining relations with the API committees for those two projects. I took a strong supporting role with Fran Shepard in pushing the results of his work with the API. So I don't think there was any problem. In fact, I tried to help everybody on the staff as much as I could.

Sharp: Maybe we could talk a little bit more about this, just how you saw your role as director.

Revelle: Well, I saw it in several different ways.

One important way was pushing the ocean sciences on the national and international scene, through the National Academy of Sciences and through the Office of Naval Research and various government committees. I was willing to serve on almost any committee that people asked me to serve on. Being director of Scripps was a powerful lever for that kind of job.

I was always interested in international cooperation, first with the Japanese and then later elsewhere in the world.

I thought it was also important that I should try to do some science, actual research, and I did that particularly with Walter Munk, then with Art [Arthur E.] Maxwell. Walter and I published a series of papers on the rotation of the earth, and Art and I wrote several papers, either the two of us or with Teddy [Edward C.] Bullard, on the heat flow problem.

Later, in 1957, I got very much interested in the carbon dioxide question. I was active in the development of the International Geophysical Year, which originally didn't have any oceanography in it at all, but eventually it did have quite a bit. That's where the carbon dioxide program started. It has now become quite a cottage industry, but at that time it was just David Keeling all by himself.

Revelle: The other thing I tried to do was to encourage our staff to do their best job of research. I used to walk around the place and talk to people about what they were doing.##

I was also concerned, of course, with building the institution, getting more people here and getting more activities, getting more ships, getting more money.

Sharp: I thought as we would go along we would talk more about that in detail. One way of getting into the specific activities that you were involved in as director is backing up a bit and talking just for a little while longer about the period when you were acting director.

President Sproul asked you to serve on different kinds of committees when you were acting director, and I'll give you a few names of those. I was interested in picking up on something you said yesterday when you were talking about the loyalty oath, that you began to feel much more accepted by the faculty and much more really a part of the University of California. There were other things that you were doing, some things that President Sproul asked you to do that maybe would have helped you feel more a part of the university. For example, in February of '51, President Sproul asked you to serve as a delegate to the all-university faculty conference.

Revelle: Was that at Davis?

Sharp: I'm not sure where it was exactly, but it occurred on February second.

The month before, Sproul had asked you to go to a meeting of the Marine Research Committee and to talk to them about funding for the Marine Life Research program.

So you were not only representing Scripps at some of these affairs, but pushing forward programs and finding your way around the university on a state-wide basis. If you look at the papers of what it looks like you were doing, you were learning a great deal about how to be a director, and also learning your way around the administration of different funding issues, and talking to the legislature.

Revelle: Yes.

Sharp: You just kept on doing it maybe in a little more high-powered way once you actually got the appointment?

Revelle: I don't think there was any difference actually, not that I remember. I acted just as high-powered as I could all the time, but I got better at it as time went on.

Sharp: But in the beginning did you feel as though you were doing it rather awkwardly?

Revelle: Well, my problem was that I was very poor at answering correspondence, and slow at making decisions. I felt I had no problems making decisions if I knew what to decide! But if I didn't know what should be done, I didn't do anything, and that was probably bad. I always tried to do too much, and I spent a lot of time in Washington, which I thought was good for the institution, but people here, of course, would have liked it better if I was just here all the time, like a spider in its web.

Permanent Status for The Marine Physical Laboratory

Sharp: After you became director formally, there were a couple of big issues that came up right away, and I thought we might talk about them a bit.

One of them was the future of MPL, the Marine Physical Laboratory. There's a letter that you sent to Vern Knudsen in July 1952.* Maybe we could just get it out and refresh our memories.

Revelle: I remember reading it. What I said was that we had two problems there. One was to complete our development of an acoustic device for the navy, and the other was to have Eckart and [Leonard N.] Libermann do some publishable research that would keep them in the scientific public's eye. That was a real problem. We were unable to recruit

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*See memorandum to Knudsen, dated 3 July 1952, on pp. 66a-b, Series 81-23, Box 2.

July 3, 1952

Memorandum

To: Dean Vern O. Knudsen

Subject: Status of the Marine Physical Laboratory during the Academic Year 1952-53

As you know, we have been in a very serious dilemma about the continuation of the work at the Marine Physical Laboratory on development of a low frequency listening system for submarines. Representatives of the Navy, including such clear-headed and realistic officers as Admiral C. B. Mommsen, Commander, Submarines Pacific Fleet, and Captain John McCain, Head of the Undersea Warfare Division in the Office of the Chief of Naval Operations, have repeatedly confirmed Carl Eckart's and my belief that the work we have done to date should lead to a very great increase in the capabilities of the U. S. Navy to deal with possible enemy submarines. At the present stage of development, however, all past experience indicates that the project could not simply be transferred to a Navy Laboratory. We are thus heavily obligated to continue this work up to the period where such a transfer could be made. We estimate that this will take at least another year.

On the other hand, both Professors Eckart and Liebermann feel that they have pretty much gone stale and do not feel themselves able to continue this development with the required vigor. As a result, we have been searching with ever increasing urgency during the last few months for competent replacements for them during the coming year.

The virtual failure of this search has made us realize that there is a basic weakness in our position: namely, that by having our two ablest physicists work in an area where their research results cannot become widely known, we have failed to attract younger men to work with them. In the long run, therefore, it may be that we have actually done a disservice to the Navy even though the short-range development is of great value. Consequently, I am completely conviced that these two faculty members should withdraw from sole responsibility for the classified work and should devote the major part of their energy and ability to work with our Physical Oceanographic group in La Jolla.

In partial justification of the position in which we find ourselves, it should be pointed out that our original intention in establishing the Marine Physical Laboratory was to foster basic research in the physics of the ocean and related fields but that the emergency created by the Korean War and the weakness of our National Defense against submarines led us to change the character of that laboratory. Even so it was never the intention that Eckart and Liebermann should carry the whole burden, and this came about through our inability to obtain the services of other physicists.

In order to solve the long-range problem, we must encourage Eckart and Liebermann to rebuild their interests and activities in basic research as rapidly as possible. We hope to do this by giving Eckart a sabbatical leave for the coming year to work at the Institute for Advanced Study, and by giving Liebermann leave to work at Yale with Kirkwood and Onsager. Liebermann has received a Guggenheim Fellowship which will partially defray his expenses at New Haven, and we propose to keep him on part time salary as Associate Research Physicist under our Bureau of Ships contract to pay the balance of these expenses. The work he will do at Yale is continuation of part of the research we have been carrying on for several years under this contract, and the propsal to continue his employment on the contract has received the verbal approval of the cognizant officers of the Bureau of Ships. A local precedent exists in the case of Associate Professor Walter Munk who received compensation from our ONR contract while on a Guggenheim Fellowship in Norway.

For the short range problem, we must somehow continue the development work at the Marine Physical Laboratory. If this development fulfills our confident expectations, every United States submarine now afloat could within a few months be equipped as a highly effective anti-submarine vessel. We have been negotiating with several younger physicists, and I hope to submit in the near future appointment forms for two of these men. Because of the urgent need and the fact that the work is classified, we have found it necessary to offer them considerably higher salaries than they might normally receive in academic ranks. There is also the possibility that we may be able to use the inducement of a visiting associate professorship (available while Liebermann is on leave, provided the President and the Regents approve) to obtain a good man. Sir Charles Wright, formerly Director of Scientific Research for the British Admiralty, has agreed to join us for a year as Research Engineer and head of the Marine Physical Laboratory.

Necessary papers to effect these changes will be forwarded separately, but I am sending you this general memorandum at this time in order to give you an over-all picture of the situation.

Roger Revelle,
Director

RR-b
cc: President Robert Gordon Sproul

Revelle: people, as I remember the letter said, for MPL because nobody knew where it was as long as it was classified. At least that was our interpretation of the facts.

Sharp: How was that resolved?

Revelle: Well, we got Sir Charles Wright to be director of the lab. He was an English underwater sound man, had been involved with the English development of ASDIC. Then later, Al [Alfred B.] Focke became director of MPL for a while. Then finally, Noel Spiess.

Another problem was in some way to integrate MPL fully with the Scripps Institution, not to have it as a separate university activity down here at Point Loma. Carl and I both wanted to do that right from the beginning.

Sharp: Yes, I've seen that. Why was it important to bring MPL more cohesively into Scripps? Why couldn't it exist more on its own?

Revelle: Well, it meant that neither institution was as strong as it would be if they were together. Fragmenting things like that means that neither institution is as good as it could be. And, of course, we had a lot of purely managerial problems. We would have to have a separate managerial staff for MPL if it was a separate institution. So it was much better to have SIO and MPL be integrated. In this way, the MPL people felt much less isolated. They were a little bit hesitant at first because they were physicists and they didn't think much of oceanographers, but eventually they became full members of the team.

Sharp: If you look at the history of Scripps during the period that you were director, one of the things that is clear is the expansion that you were generating and that you were helping other people to generate, but also trying to keep it all together so that it wasn't fragmented. MPL is kind of an interesting example of one that didn't get away!

Revelle: Yes.

Sharp: You know, that was brought in. Later on, I thought we'd talk about the development of IMR, the Institute of Marine Resources, because that's

Sharp: a really good example of something you went out to get and brought in, and it continued to grow.

Revelle: This was a period of great transition. Before World War II Scripps was a very small place. There were five faculty members and five graduate students, or maybe six, and a very small budget.

The director's secretary was a woman named Tillie Genter. She was a native La Jollan, like my wife Ellen, but about twenty years older, maybe thirty years older. She was our purchasing agent, she was our personnel officer, and the accounting officer, we had a superintendent of buildings and grounds, but she really ran that part too! [laughing] She did everything.

Now they have two or three hundred people doing the job that Tillie did, a huge administrative infrastructure. This was, I guess, an example of Parkinson's Law, that bureaucracies tend to fill up all the available space.

Sharp: And probably do things less well the more--.

Revelle: Well, not necessarily less well, but at least there's more complexity to it--people supervising each other and things like that.

At the same time the institution's budget grew. By 1950 it was about $1-1/2 million, as I told you yesterday. By the time I left, it was about $12 million. Now it's about $50 million. A lot of that is inflation, of course.

And it was higher when they had the deep-sea drilling project, which they lost in the last two years.

In the early days we had a weekly seminar or weekly staff meeting where one of us got up and talked about his research. That was one of the things that I thought was quite important, to have everybody know what each other was doing.

Sharp: To help that cohesion?

Revelle: Yes.

We eventually had to break up into divisions or groups. By the time I left we had something like eighteen divisions of the institution, which were more or less semi-independent.

Revelle: One example was the Institute of Geophysics and Planetary Physics. Walter Munk was unhappy about the growth of the Scripps Institution. He preferred a smaller, more intimate place, and if it hadn't been for his wife's physical disability, he probably would have gone to Woods Hole, although they were a big place too.

So, to make him happy, we started a branch of the university-wide Institute of Geophysics and Planetary Physics here. He recruited his own group there, people like Freeman Gilbert and George Backus and Bob Parker and Jim Bruun. Hugh Bradner was there too.

Another group was the Visibility Laboratory, which moved here from MIT.

Another group was the Marine Life Research Program, which John Isaacs ran.

Another was called the Applied Oceanography Group or AOG. It was a group that was studying the wakes of submarines and the wakes of other ships to see if you could detect submarines by some property of their wakes. This was a group headed by, or at least started by, Giff Ewing.

Then we had the Physiological Research Laboratory that Pete [Per F.] Scholander started, and the Shore Processors Study Group that Doug [Douglas L.] Inman ran, and still runs, for that matter.

Then, as you pointed out, by the late 1950s we had the Institute of Marine Resources.

In the mid or late 1960s, Bill Nierenberg started the Climate Research Group with Jerry [Jerome] Namias and the new man, Richard Somerville, they have now.

One of the big impulses to expansion in my time was a grant of $1 million for marine biology we got from the Rockefeller Foundation. This was Warren Weaver's idea. He was the soul of the scientific part of the Rockefeller Foundation.

Sharp: That was 1954?

Revelle: I guess so.

Sharp: Yes, that's what I wrote down.

Revelle: There was an Italian geneticist named Adriano [A.] Buzzati-Traverso who came down here for a visit one summer. I was quite impressed by him, and we asked him to stay. He tried to run this Rockefeller Foundation marine biology program. One of the parts of it was that we promised to appoint four faculty members. Buzzati was one of them, Per Scholander was another, Ben [Benjamin F.] Volcani, and Ralph [A.] Lewin were the other two.

Adriano organized a large conference of world figures in marine biology, including a lot of Europeans.* We had about a week-long conference at a hotel here in La Jolla, which has since disappeared. There're a lot of apartments there now. This was a very impressive affair. We all learned a lot, and also, of course, it did a lot for the international status of the institution.

Then Buzzati left after a few years and went to Italy and started his own institute in the Bay of Naples. Italian science, at least large parts of it, is rather corrupt, and he eventually was forced out of the directorship of that institute. Later, he became assistant director general for science at UNESCO.

One of the things Adriano did was to write popular articles on science for Italian newspapers. His brother was Dino Buzzati, the great Italian novelist. For a while Adriano was back here part-time and part of the time in Italy, but eventually he left altogether.

Sharp: After the initial $1 million grant from the Rockefeller Foundation to start the marine biology program, once that got started, with his efforts and other people's, that then became one of the major efforts?

Revelle: Yes, that's right. One physical outcome of it is the Marine Physiology Research Laboratory that Fred White is the director of. During Bill [William A.] Neirenberg's time they built that big

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*"Perspectives in Marine Biology," held in the La Jollan Hotel, March 1956, under the auspices of The International Union of Biological Sciences.

Revelle: marine biology building, which is full of biologists, and we have some very good biologists now.

Another area that I thought was important to develop was geochemistry, and we brought Hans [E.] Suess and Harmon Craig here. They worked together for a while, but they're both strong personalities and for some reason they didn't get along. They separated and ran quite separate laboratories.

Hans developed a Carbon 14 laboratory up on top of Mt. Soledad, which for a long time made many Carbon 14 analyses of all kinds of things, like archaeological remnants along the coast. He was a very original and good scientist who made many discoveries, including something called the "Suess wiggles" which show the marked variations of C14 production, probably because of variations and the abundance of cosmic rays, over a rather short period, like a few hundred years. Those have to be taken into account in Carbon 14 dating, but nobody did at the time. He also showed that the C14/C12 ratio in the atmosphere had varied over longer periods in the past.

Later Devendra Lal and I wrote a paper on this, in which we showed that these longer period fluctuations were probably due to variations in the atmospheric concentration of carbon dioxide. After we persuaded Harmon and Hans to join our staff, they helped bring Jim [James R.] Arnold here and then Harold Urey and Devendra Lal. Devendra and a group of people working with him, discovered Berylliun 10 and several other radioactive isotoopes that are formed naturally in the atmosphere and end up in the ocean.

Teaching and Training in Oceanography

Revelle: Perhaps geochemistry is one area where Scripps is still preeminent. We have people like Ray [F.] Weiss and Harmon Craig and Ed [Edward D.] Goldberg and Gustaf Arrhenius, and people that were here, like Ralph Cicerone, who is now at NIMH.

Sharp: I was looking back over some copies of the SIO staff council minutes, and in the minutes for May 7, 1959, there was a plan about teaching oceanography. There was also a reference to an

Sharp: article that had been published in Science magazine in about 1950 and you were one of the authors* together with Vern Knudsen of UCLA, [Alfred C.] Redfield of Woods Hole and Robert [R.] Shrock of MIT.

The minutes said that people should refer to that article and use it as a plan for what should be taught. There were four different areas: marine geophysics, marine geochemistry, marine geology, and marine biology. I was wondering what the criteria were for deciding that those really were to be the major areas that should be taught at Scripps, how you might have put together that plan, and how you thought it really could work at Scripps?

Revelle: Well, I'm not sure there's anything very special about that plan. I don't remember that particular paper, but oceanography has traditionally been divided into four sub-disciplines: marine geology, marine chemistry, marine biology, and physical oceanography.

Physical oceanography deals with what Harald Sverdrup used to call "the motions in the oceans," namely, waves and currents and turbulence, mixing in the ocean, the motions of the waters in all their variety.

Marine chemistry deals with the chemical constituents of sea water, and has expanded now to include atmospheric chemistry. It's in some way the central field of oceanography, the interactions between the sea bottom, the shore, the atmosphere, the life in the sea, and the water. As I told you yesterday, you have a hydrothermal circulation in which the entire ocean apparently circulates through the bottom crust in about ten million years, and exchanges chemical constituents with the rocks.

Marine geology and geophysics deals with what Fran Shepard and I used to call "the earth beneath

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*See "Education and Training for Oceanographers," Science III (June 23, 1950), pp. 700-703. Originally appeared as contribution No. 519 from The Woods Hole Oceanographic Institution and contribution from The Scripps Institution of Oceanography, New Series, No. 463.

Revelle: the sea." That's the sediments and the rocks and the interior of the earth, the geophysics. So it's really in some way more geophysics than geology, although a lot of people still work on the sediments and the fossils that are in them and what they show about the past. Geology basically is a historical science. It deals with the history of the earth. Geophysics is a tool to understand the history and the structure and the behavior of the earth.

Marine biology deals with the critters in the sea, plants and animals and bacteria, traditional studies of marine phytoplankton and zooplankton and larger organisms, particularly fish and marine mammals. It deals a lot with the biochemistry of marine organisms, how they work internally. Not very much with genetics, in the sense of molecular, biological genetics. Quite a bit with physiology, the physiology, for example, of porpoise brains and of salt metabolism.

My granddaughter is a graduate student here in the laboratory of a man named [Walter F.] Heiligenberg, meaning "holy mountain." He studies the nervous systems of fish, particularly electric fish, and she's trying to map the electric field in the brain of these fish, how the fish has a map in its brain of the electric field in the water. This is most difficult work. I'm not sure she's going to be able to do it, because she must deal with individual brain cells. The total brain of one of these fish is about half the size of my little fingernail.

Sharp: Very minute.

Revelle: Yes, the fish themselves are only about so big, about 4 or 5 inches long. [gesturing] They make signals that you can hear with a transducer in the water. It sounds like a loud pulse, bang, bang, bang, like that. And how do they do it and why do they do it? She actually has gone down to Panama to collect these fish, in the jungles of Panama.

Sharp: And then bring them back?

Revelle: Bringing them back alive, most of them, she hopes!

Then they anesthetize a fish and put it in a holding contraption of some kind. She actually goes in to its brain with a micro-pipette, which she makes herself. The tips of the pipette are so

Revelle: small you can't even see them with the naked eye; they're literally just a few microns in diameter. With these pipettes she penetrates individual cells in the critter's brain and sees what the electric signal is. Unbelievable.

One of the remarkable marine biological discoveries of the last few years has been the discovery of the faunas around the deep sea vents along the mid ocean ridges or spreading craters. This is, I think, an incredible phenomenon. Presumably the vents don't last very long--only perhaps a few hundred years at most, but while they are active, hot gases and lava come out of them, mostly gases. These gases contain a lot of hydrogen sulfide and metallic sulfides, and they produce tall chimneys, several feet tall, which are built-up deposits of sulfide.

Around these things there's a fauna of worms and clams and fish, a whole collection of critters. The nearest vents to the ones we are looking at are maybe two thousand miles away. Yet all the vents have more or less the same faunas. How do the organisms get from one set of vents to the next one? They must have larva of some kind which float in the water and somehow a few of them get scattered several thousand miles. It doesn't make sense, but there it is.

The most interesting thing about these vents is that the animals there live entirely on bacteria which oxidize hydrogen sulfide. Hydrogen sulfide is a deadly poison for human beings, but not for these bacteria. They love it, and they get their energy from the oxidation of the sulfide into sulfate, then they use this energy to make organic matter. The organic matter is transferred to the worms and to the mollusks in which the bacteria live, as their food. That's their food supply, basically their energy supply.

Sharp: So it's a complete unit.

Revelle: Yes. No photosynthesis at all. It's a quite different basis for life. Instead of plant life, it's bacteria life, living off its energy source from oxidation of hydrogen sulfide.

The water in these vents, coming out of the vents has temperatures up to four hundred degrees centigrade. Very high temperature. Three hundred degrees above the boiling point of water at

Revelle: atmospheric pressure. Yet it's liquid, and the reason it's liquid is because it's under such high pressure, three or four thousand meters deep. The Alvyn, the Woods Hole submarine, goes down and looks at these things. Instead of diving in Alvyn, Noel Spiess and his group use remote-controlled cameras to look at the vents, and instruments that they tow along the bottom. This may be the place where life began, and may be still beginning. Some of these bacteria apparently live at temperatures well above one hundred degrees centigrade; not above the boiling point of water because the water isn't boiling under that high pressure.

With temperatures like that, chemical reactions could happen very rapidly. It may very well be that some complicated organic chemical compounds are being formed there in the vents spontaneously, and that the chain of events from inorganic matter to living matter may be happening there, or maybe happened in the past there.

One of our biologists named George Somers has done a lot of work on this, and quite a few of the people in the department of oceanography at Oregon State have too.

Sharp: That's quite a different kind of research that is going on now, that is very exciting--

Revelle: This is very different, yes.

Sharp: --very different from when you were director.

Revelle: Yes, that's right.

Sharp: I thought we might talk a little bit more about this plan for teaching simply because it does show up in the staff council minutes, and discuss if people just automatically accepted that this was the way Scripps should be developing in the area of teaching, that these were areas that were obvious ones.

Revelle: Well, I don't really understand what you're saying. How does it differ from what we did before?

Sharp: Well, I'm not sure how it's different. Let me just see if I can find it here. [looks through notes] Here's the 1950 article that was in Science magazine. [hands article to Revelle]

Revelle: [reads excerpts from article aloud] "Although no individual method or principle of oceanography is unique, it is believed that the combination of principles and methods just described forms a distinct discipline which requires special training." "What is required . . . is the establishment of professional standards for oceanographic training." "Depending on his interests and aptitudes, a student must specialize in the physical, biological, geological, or chemical aspects of oceanography." That was what I said a few minutes ago. "In a field with limited outlets for employment it is important that basic training be broad enough to equip students for jobs in related branches of geophysics, geology, or biology."* That, I think, is a very important statement.

I guess the outstanding example of the problem of training too many people in too narrow a field is astronomy; during the last ten or fifteen years, because of the huge outpouring of money from NASA. There are a lot more astronomers now than there are jobs for astronomers.

This paper, of course, applies basically to the Scripps Institution. It clings to our traditional idea that there should be four basic courses in biological, chemical, geological, and physical oceanography, which the graduate students take during their first year. It adds to that this course on marine environments and regional aspects of oceanography, which I was not aware we had, but we probably do have. If you look at the catalogue, we have lots of courses.

Then it says that a student needs at least nine graduate units in one of the specialties. For example, in biological oceanography, physiology of marine organisms, population dynamics, biochemical problems in the sea, methods in biological oceanography, evolution of the marine world, and so forth.

Then in physical oceanography there are dynamical and physical oceanography, wave and tidal hydrodynamics, methods in physical oceanography, and others.

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*All quotes from "Education and Training for Oceanographers," p. 702.

Revelle: In geology there are beach processes and marine sediments, problems in submarine geology, petrography of sediments, and so forth.

So that really the course work can take a year and a half to two years, maybe even more, before the student is qualified to go on to a doctoral degree. We try to avoid giving the master's degree here. At Scripps, the master's degree is basically a consolation prize for people who it is felt, are not capable of getting a doctor's degree. They're essentially released, or discharged, or thrown out in the great world, after two years or so.

The other interesting thing about this article is that it emphasizes taking graduate courses or upper division advanced courses in one of the basic sciences as we used to call them in those days.

This problem of advanced courses in basic science was the reason why we started here what we called the Graduate School of Science and Engineering. It turned out to be an imaginary school. It never really existed, because it got transformed into the general campus before it got under way.

The idea for the school of science and engineering came about in the following way. When our graduate students were examined, their doctoral committees consisted of faculty members from UCLA and La Jolla. The UCLA faculty members didn't know anything about oceanography, but they knew a lot about physics or chemistry or biology. Our students didn't do very well with those people. They had learned a lot of oceanography, but that was quite specialized, and they were really pretty weak in the fundamental sciences.

I felt that we somehow had to give more education in the basic sciences here. The way to do that was to start a sort of publicly supported Cal Tech [California Institute of Technology] in the basic sciences, the sciences of physics, chemistry, mathematics, and biology.

Sharp: And then put the more specialized oceanography courses sort of on top of that?

Revelle: Well, not necessarily on top of it, but along with it.

For example, my granddaughter is taking courses in neurophysiology in the [UCSD] medical school.

Sharp: To help her with her brain research?

Revelle: Yes. She probably will end up not as an oceanographer but as a neurophysiologist, which is one of the things that Redfield and company said, that we wanted our students to have a breadth of opportunities for employment afterwards.

So, as far as I can make out, the only real difference between the proposal in this paper and what we traditionally did was to emphasize that graduate students need to take graduate or upper division courses in the basic sciences.

The paper by Knudsen, Redfield, et al., is also directed towards undergraduate preparation of people for oceanography. Traditionally, at Scripps we have avoided taking students who have majored in oceanography as undergraduates. We want them to have a strong background in the basic sciences.

Nowadays, they're very stuffy about admissions here. They admit only about one out of five or six applicants. They have about 180 students, and that's approximately all they can take. It takes forever to get a degree at Scripps, on the average about six years. That means they admit about thirty a year, something like that.

We had a graduate student named Johnny [John A.] Knauss who later became vice president and dean of marine sciences at the University of Rhode Island, and greatly expanded and deepened the marine activities there. Rhode Island calls itself "the Ocean State." He wrote a musical comedy, which was put on by our staff, called "Endless Holiday." (We had one expedition called Northern Holiday and another expedition called Southern Holiday, so Johnny's title was "Endless Holiday.")

The basic idea of the plot was that we started out on an expedition on Horizon and Spencer F. Baird, and I had forgotten to get permission from the regents. So we left anyhow,

Revelle: and the regents refused permission for us to go after we'd gotten under way and had been out a couple of months. We just had to stay out there, we couldn't come back in!

Sharp: So it was "endless" because you couldn't afford to come home?

Revelle: [laughing] That's right.##

The term "Endless Holiday" was very appropriate for some of our graduate students, these forty-year-old graduate students who have been here for fifteen or twenty years, who had never been able to pass their German examinations. That held a lot of them up. They could do everything, they were fully matured researchers, but they'd just never been able to read German! So they stayed on as graduate students for years and years.

Sharp: The matter of how the teaching would be distributed, what kinds of courses and so on at Scripps, when you were director and really getting the teaching of graduate students fully under way, was it a topic of big discussion among the faculty? Did everyone have their own ideas about how it should be done, and which courses students should be taking and all of that?

Revelle: Well, everything was okay in physics and biology, for reasons that I don't quite under-stand. There was never any argument among those people about what should be taught, but the chemists and the geologists argued like cats and dogs. We tried to start a department of geochem-
istry and geology, and it just never worked because these guys just couldn't get along with each other, and they eventually got so they would hardly even speak to each other, several of them. We have a lot of the world's best geochemists, but they all run their own little empires. I've never understood that.

We have a geologist--Albert Engel--[brief tape interruption]--he's extremely argumentative, and Harmon Craig is extremely argumentative, and Ed Goldberg has strong opinions. The result is that they all work in separate little boxes of their own.

Sharp: Their students must have a difficult time--

Revelle: Oh, they do, sure.

Sharp: --sort of easing their way through the program, having to deal with so many different personalities.

Revelle: Sure. But the faculty has now expanded to about one hundred people, counting adjunct professors and research workers who are also lecturers, about seventy with actual professorial titles, but enough for everybody. The problem is that it's hard for a lot of these guys even to find a course to teach, let alone worry about whether they have too much to teach.

Sharp: There are two topics that come out of this discussion about what kinds of teaching to do at Scripps. One topic is what you end up with, the actual training of people at Scripps. There were some references to the training of naval officers in oceanography, in the different aspects of oceanography, and the teaching of oceanography to oil geologists and fisherman, for example; and to the just plain graduate students who were coming through fairly straightforward academic programs.

How did you accommodate all those different kinds of students?

Revelle: Well, we concentrated on the normal graduate students almost entirely. We did, during World War II and for a short while after, have some short courses for naval officers and air force officers, particularly in wave and swell forecasting. A lot of those people then became oceanographers, people like Bob Arthur, for example. But after a few years we gave graduate work only for genuine graduate students.

That paper you're referring to was something that I used to do a lot of talking about, but we never did very much about it, teaching fishermen and naval officers and oil company geologists and people like that.

I was always quite concerned about the students, and always did some teaching. That was really, as I said, why we started the Graduate School of Science and Engineering, which became the University of California at San Diego before we knew it.

Sharp: The other matter, then, is how you would decide whom to bring into Scripps, which has a lot of different elements to it. One element is the

Sharp: search for temporary visitors who might want to come and do a certain kind of research for a period of a couple of months or something like that, that was one kind of visitor who would come.

Revelle: Traditionally, that was a very important part of the institution. It became less and less important as we got more and more crowded. We really just didn't have any room for many visitors, and they had to be asked by individual faculty members who would share their facilities with them, and a lot of them did.

But a guy couldn't come to me and say, "I'd like to spend a couple of months at Scripps in the summer time," because I wouldn't have any space for him.

Sharp: That was handled sort of on an ad hoc basis?

Revelle: Yes, people would essentially make their own arrangements with one of their friends on the faculty or in the research staff.

Sharp: And then the larger issue of faculty whom you would bring into Scripps was a much more complicated one.

Revelle: Yes, it is. It's complicated in two ways. One was, there were hardly any faculty positions. We had to fight for every faculty position. One of the great things that Bill Nierenberg has done is to increase the number of faculty positions to about seventy. When I left, it was about twenty-five or thirty, something like that. The other problem, of course, because there were so few positions, was getting agreement on appointments. There would be lots of faculty members who wanted their people appointed.

What we did for many years, and this was, I think, one of our very important inventions, although not completely original, was to appoint people to the research staff as oceanographers or associate oceanographers or assistant oceanographers. We rejected the term "junior oceanographer." We had another term for it, like "associate in oceanography," or something like that, which was a lower-ranking kind of job. But my policy and our faculty's policy (they were all convinced of it, I think), was to try to treat these people with research titles in every way that we could as if they had faculty positions.

Revelle: I used to tell these research staff members, and I think our faculty backed me up on it, that we would retain them in their positions as long as we possible could. They had a de facto tenure in the sense that we'd fire everybody else before we fired them. We really didn't think oceanography was going to collapse. They effectively had tenure on a local level, but they weren't members of the academic senate and they didn't have the prestige of being members of the senate. In fact, the staff council was an invention to treat everybody more or less alike. It's not an academic senate; it's the research staff as well as the professional staff.

My own feeling in the matter was that a professor is a special kind of a guy, a special kind of scientist or scholar who should have broad interests and also interest in teaching. No matter how good he is in a narrow field, he shouldn't be a faculty member unless he's really interested in producing graduate students and has enough breadth of interest to be able to do that, not just to guide students in his own narrow speciality.

Sharp: And also contribute to the profession in the sense of publishing.

Revelle: Well, everybody published. The research staff published just as much as the faculty members. There was no difference in quality; there was a difference in attitude and interests, as far as I was concerned. Now, I think Bill Nierenberg has forgotten this. He appoints faculty members whenever he can out of research staff members.

[A short portion of this transcript has been sealed until 2002.]

[This portion of the transcript has been sealed until 2002.]

An example which you can certainly put in your record but you probably shouldn't publish is Charles David Keeling. He wants, in his belly, to measure carbon dioxide, to measure it in every possible way, and to understand everything there is to know about carbon dioxide, but that's all he's interested in. He's never been interested in anything else all his life. He's never had any graduate students, or only one or something like that, a Master's student. I would never have appointed him as a professor. I would have given him all the honors and prestige possible as a marine chemist, with the rank of a marine chemist or marine geochemist. But now he's a professor, and we had a faculty committee the other day to consider his promotion in salary.

I don't know whether you understand this, but in the professorial ranks, all professors are not equal. Some of them get twice as much or more than twice as much as others. There's a series of grades of professor: 1, 2, 3, 4, 5, 6. He was up for an increase to Professor 5 or something like that, and our committee unanimously turned it down. It was a committee of the staff council. Ray Weiss was on it, who was not at that time a professor. Bill Fenical was on it, who is some kind of a half-time professor. And another chap was an actual member of the academic senate. We turned the increase down because Dave Keeling is so uninterested in teaching. He doesn't give a damn about teaching, and he doesn't do a very good job of it, in spite of the fact that he's a superb scientist in his very narrow field.

[Transcript resumes]

Revelle: The main way we recruited faculty and research staff members was just by taking advantage of targets of opportunity, finding good people anywhere who wanted to come here, or whom we could persuade to come here. We were growing so fast that we could always make room for a good man. It's much more difficult now.

We lost a lot of good people. Part of that was the result of Scripps Institution policy. First-rate marine scientists who are now all over the country got their PhDs at the Scripps Institution: people who are now at the Woods Hole Oceanographic Institution, the Lamont Geological Observatory, the Universities of Washington, Rhode Island, Miami, everywhere, they got their Ph.D.s here. They get kicked out of here because we don't appoint our own PhDs.

Sharp: Yes, that's a fairly common problem I think for a lot of departments.

Revelle: I think it's too bad, myself, because we've lost a lot of absolutely marvelous people.

Sharp: With good training?

Revelle: With good training, that's right. But anyhow we've always had that policy, and probably it's a good thing, although I've never been convinced of it.

One of the things we sort of specialized in was producing directors of oceanographic institutions. Several institutions are headed by our graduates. Of course, they can't all be directors of Scripps, so that's probably a good thing!

Sharp: They have to go <u>somewhere</u>.

Revelle: They have to go somewhere, yes.

Administrative Assistance from Charles Wheelock

Sharp: When Scripps was doing all of this expanding, there were a couple of groups that met with you to, hopefully, facilitate the expansion, the organization, and the communications regarding all that we've been talking about. One of them was the Director's Advisory Council and the other was the Staff Council. I thought we could spend just a little time with your recalling how you saw these two groups, what their roles were. [brief tape interruption]

Revelle: One thing you should remember about those committees is that in 1954 I managed to get a wonderful man as associate director. His name was Charles Delorma Wheelock. He had been Deputy chief of the Bureau of Ships. He was a rear admiral in the Naval Constructor Corps. One of the characteristics of naval constructors is they never go to sea, and the reason why most of them don't go to sea is they get deathly and permanently seasick. That was true of Charles. He could walk out to the end of the pier and get seasick practically by looking at the waves. Except for that physiological weakness, he was a marvelous man. He was a graduate of the Naval Academy and had taken graduate work at MIT, and had a master's degree in naval architecture and naval engineering.

When he came here, he came in a non-faculty position, as a research enginner, but, in fact, he leaned over backwards to be as academic as he could possibly be to make as complete a contrast to the navy as he could make. All decisions had to be made by committees [laughing] faculty committees. He would organize them, call the meetings, prepare the minutes, organize the whole affair.

Sharp: There's a lot of evidence of that in your director's papers.*

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*See examples on following pages: "Committees of the Staff Council, SIO 1958-1959," SIO Archives, Series 81-16, Box 16, F 53; "Minutes of Joint Meetings of SIO Staff Council and UCLJ Faculty," 3 August 1959, SIO Archives, Series 81-16, Box 16, F 54.

Comm; Hees, 1957-1962

COMMITTEES OF THE STAFF COUNCIL

SCRIPPS INSTITUTION OF OCEANOGRAPHY, 1958-1959

The first named is Chairman of the Committee or Panel. Those whose names are followed by an asterisk also serve on parallel committees of the Faculty at La Jolla.

ACADEMIC COMMITTEES

BUDGET & INTERDEPARTMENTAL RELATIONS: M.W. Johnson*, M.N. Bramlette*, W.H. Munk*, J.M. Snodgrass, F.N. Spiess, W.S. Wooster.

COMMITTEE ON COMMITTEES: C.E. ZoBell*, J.D. Isaacs*, R.W. Raitt*, F.N. Spiess, V. Vacquier, Tj. H. van Andel, R. Revelle* (ex officio).

EDUCATIONAL POLICY: F. B Phleger*, J. Arnold, S.Q. Duntley, W.M. Elsasser*, D.L. Fox*, Tj. H. van Andel, R. Revelle* (ex officio).

ADMINISTRATIVE COMMITTEES

BUILDINGS AND CAMPUS DEVELOPMENT: C.D. Wheelock, S.Q. Duntley, C. Eckart, J.D. Frautschy, J.C. Kirby, L.N. Liebermann, W.H. Munk.

Grounds Panel: Duties to be assumed by parent Committee.

Planning Panel: Members to be nominated on an ad hoc basis to deal with special problems as they arise.

Radioactivity Panel: L.N. Liebermann, E.W. Fager, T.R. Folsom, J. Arnold, W.H. Thomas.

Space Assignment Panel: Parent Committee will be responsible for recommendations to the Director concerning space policy and assignment of space to divisions or departments, which will then assign space to individuals.

Stockroom Panel: E.D. Goldberg, W.J. North, F. Snodgrass.

Water Supply Panel: W.J. North to be responsible for sea water and E.D. Goldberg to be responsible for fresh and distilled water. They may call on others to serve as consultants as indicated by the nature of the problem.

DIRECTOR'S ADMINISTRATIVE ADVISORY COUNCIL: R. Revelle, G.O.S. Arrhenius, S.Q. Duntley, W.M. Elsasser, J.D. Frautschy, Ruth Handley (Secretary), J.D. Isaacs, H.D. Johnson, J.C. Kirby, T.A. Manar, F.N. Spiess, J.W. Tippetts, C.D. Wheelock (Vice-Chairman), C.E. ZoBell. (All members of the DAAC are ex officio).

LIBRARY COMMITTEE: C.L. Hubbs, K.A. Clendenning, H.C. Craig, H.C. Craig, C. Eckart, W.M. Elsasser, G.C. Ewing.

MARINE OPERATIONS COMMITTEE: H.W. Menard, Jr., E.H. Ahlstrom, E.W. Fager, J.L. Faughn, J.D. Frautschy, R.W. Raitt, M.B. Schaefer, W.S. Wooster.

Pier Panel: J.D. Frautschy and other members of parent committee.

Ship-scheduling Panel: W.S. Wooster and other members of parent committee.

Marine Preserves Panel: E.W. Fager and other members of parent committee.

PERSONNEL APPEALS COMMITTEE: N.W. Rakestraw, W.L. Belser, J.R. Moriarity, F.E. Snodgrass, Ruth Young.

SELECTIVE SERVICE COMMITTEE: L.N. Liebermann, M.W. Johnson, J.E. Tyler.

PUBLIC CEREMONIES, INFORMATION & LECTURES: D.L. Fox, S.Q. Duntley, S.D. Hinton, L.N. Liebermann, T.A. Manar, C.D. Wheelock, W.S. Wooster.

(Members of this Committee will assume the responsibilities of the Colloquia, Aquarium-Museum, and Movies Panels).

PUBLICATIONS COMMITTEE: G.O.S. Arrhenius, F.T. Haxo, W.R. Holleman (ex officio), L.N. Liebermann, T.A. Manar (ex officio), P.Rudnick, F.P. Shepard, W.S. Wooster.

SAFETY & DISASTER PREPAREDNESS: J.L. Faughn, T.R. Folsom, R.W. Haworth, J.D. Isaacs, G.E. Jones, J.C. Kirby, C. Limbaugh, F.N. Spiess, J.M. Snodgrass.

Diving Certificate Board: C. Limbaugh, E.W. Fager, R.J. Ghelardi, E.A. Murray, W.J. North, F.E. Snodgrass.

Explosives Safety Panel: G.G. Shor, Jr., J.L. Faughn, E.E. Peterson.

(Civil Defense organization and functions to be reviewed by parent committee).

RESEARCH ADVISORY COUNCIL: R.S. Arthur, C.L. Hubbs, J.D. Isaacs, F.P. Shepard, F.N. Spiess, Tj. H. van Andel, H.C. Urey, C.D. Wheelock, C.E. ZoBell.

Gulf of California-API Advisory Panel: F. B Phleger, R.S. Arthur, M.N. Bramlette, F.P. Shepard, G.G. Shor, Jr., Tj. H. van Andel.

IGY Advisory Panel: H.W. Menard, Jr., R.S. Arthur, T.R. Folsom, J.D. Frautschy, J.A. Knauss, W.H. Munk, June Pattullo, R.W. Raitt, N.W. Rakestraw, M.B. Schaefer, W.G. van Dorn.

Kelp Advisory Panel: C.L. Hubbs, F.T. Haxo, W.J. North, J.E. Tyler, C.D. Wheelock, C.E. ZoBell.

Military Research Panel: F.N. Spiess, S.Q. Duntley (J.E. Tyler, alternate), J.D. Isaacs, J.A. Knauss, W.G. van Dorn, C.D. Wheelock.

MLR Advisory Panel: J.D. Isaacs, E.H. Ahlstrom, R.S. Arthur, F.T. Haxo, C.L. Hubbs, M.W. Johnson, J.C. Marr, J.L. Reid, M.B. Schaefer.

Tuna Oceanography Advisory Panel: M. Blackburn, R.S. Arthur, C.L. Hubbs, J.D. Isaacs, J.C. Marr, J.L. Reid, M.B. Schaefer, C.D. Wheelock, W.S. Wooster.

Southeast Asia Program Panel: W.S. Wooster, J.L. Faughn, J.C. Marr, M.B. Schaefer, Tj. H. van Andel.

MINUTES OF JOINT MEETING OF SIO STAFF COUNCIL AND UCLJ FACULTY

Library Auditorium, 3 August 1959; 16 faculty and 17 staff members present.

The meeting was called to order at 15:17 by Mr. Wheelock, Vice-Chairman of the Staff Council.

A motion was made, seconded and passed to accept the minutes of the last meeting of the Staff Council (11 December 1958) which had been distributed.

Mr. Wheelock then called on Mr. Isaacs to present the report of the Committee on Committees. Mr. Isaacs stated that this committee had been able within the last three years to reduce the number of panels and committees from 46 to 28. He distributed a revised Report of the Committee which differed from their original Report, submitted 17 July 1959, in the addition of four members to the Executive Committee. A motion to approve the revised Report was made, seconded and passed by voice vote.

Mr. Wheelock then asked Mr. Revelle to present a report on progress and plans for the new campus at La Jolla. Mr. Revelle said that the building program for the next five years was in preparation for submission to the State Legislature. This year's budget contains $172,000 for purchase of land south of the present SIO campus and $450,000 for the preparation of plans and working drawings for the School of Science and Engineering. Laboratory Building #4 which will be on the land south of the SIO campus will probably not be built for at least five years because of the lower priority given by the University to facilities for organized research as opposed to facilities for instruction.

The tentative schedule of building for the UCLJ campus (SSE through 1962-63) is as follows:

1960-61 - $5,000,000 for first construction on top of hill.
1961-62 - $5,000,000 for additional construction on top of hill plus $1,800,000 for equipment for the buildings.
1962-63 - $9,000,000 for laboratories, library, classroom and administration buildings, $3,500,000 for equipment for about 80 faculty, $1,500,000 for a corporation yard, $1,500,000 for utilities and site development and $1,500,000 for apartments for about 150 students.
1963 - About $20,000,000 for expansion beyond SSE.

This year $400,000 has been allotted for the operation of SSE and $1,000,000 has been requested for next year by President Kerr. If this request is favorably received by the Legislature, there will be provision for 38 new faculty by the Fall of 1960. Three departments will be established by then; Chemistry, Physics and Earth Sciences. In the following two years, departments of Biological Sciences and Mathematics will be established. The Physics Department will have a faculty which is strongest in solid state physics, plasma physics, and nuclear and high energy physics; the Chemistry Department will emphasize geochemistry, experimental and theoretical physical chemistry and biochemistry; the Earth Sciences Department will be concerned with the study of earth fluids, theoretical and experimental geophysics and geology; the Biological Sciences Department will concentrate on three fields - ecology, neurophysiology and molecular biology

interrelations of biochemistry and genetics). Besides these departments of instruction, there will be various research institutes whose members will come from several departments.

Engineering and applied science are also to be developed here. Mr. Revelle suggested that the following might represent the important engineering problems of the future: atomic power, material development, climatic and weather control, thinking machines and space travel. It is not clear what sort of engineering department would be prepared to handle these problems - perhaps, a combined applied and theoretical physics department would be best.

The Scripps Institution of Oceanography will not lose its identity, integrity or importance. There is little problem of support of research for more funds will certainly be made available for oceanography but there is a serious problem of housing. Until recently, federal funds have not been available to provide laboratories, etc. but there seems to be a recent tendency for granting agencies to consider these. The present fleet will almost certainly be replaced by larger more expensive ships by the federal government within the next 10 years and it will be necessary to train marine scientists to man them. There is a group at SIO which feels that a special curriculum in oceanography is desirable and practical and can attract first-rate graduate students. There is, however, not a consensus of opinion among the faculty in regard to this question at the present time. The Educational Policy Committee is going to consider the problem and, presumably, will report its opinion to the Faculty.

One of the desiderata mentioned by many of the newly-appointed and prospective faculty members is the development of strong departments in the humanities and social sciences. The theater which is to be built on top of the hill will partly serve this function. In addition, there should be departments of English, history, economics and anthropology by 1964. By 1970, UCLJ should be a full-scale university which can provide a sound undergraduate liberal education.

If the necessary basic sciences are developed at UCLJ, a medical school is likely to be established because the new County Hospital of 1200 beds will be within easy reach of the campus and could provide the clinical training required.

The increasing costs of land and housing in La Jolla present a serious problem for both faculty and students. The City of San Diego has a plan for the development of a community around the campus on an area of about 11,000 acres and with about 95,000 inhabitants but noone has yet presented a plan for keeping land and property values at a level which academic staff and faculty can afford.

Land for the new campus amounts to about 1300 acres: of this, the University now owns about 160 acres, the voters of San Diego have authorized the City to give the University about 525 acres and a bill has been introduced in Congress to transfer Camp Matthews to the University when the Marine Corps leaves it within a year or two. There are some possible hitches in these plans but it seems likely that the land will be available and will be accepted by the Regents.

Mr. Revelle then outlined the predicted student enrollment in the University and State Colleges for 1970:

UCB	27,500
UCLA	27,500
Davis	10,000
Riverside	5,000
Santa Barbara	10,000
UCLJ	5,000
San Francisco	5,000
Orange County	6,000
South Central	6,000
State Colleges	160,000

Of the 102,000 predicted for the University, about 2/3 would be undergraduates.

These enrollments (State Colleges and University) will require about 41,000 additional faculty members if the present faculty-student ratio is kept. The University will produce about 10,000 Ph.D.s in this period and only about 30% of these will go into teaching; leaving a deficit of about 38,000 faculty with the Ph.D. degree. As most other states will be facing the same problem, it is difficult to see where they will come from.

Following Mr. Revelle's report on progress and plans, there was a discussion of the position of the University in regard to the State Colleges. It seems evident that in order that the State Colleges may attract good faculties, they will have to make provision for research. Experience in other states has, however, shown that this soon leads to a desire on the part of the State Colleges to grant advanced degrees and thus become universities. There is now a joint committee including representatives of the Regents and the State Board of Education which has been directed to draw up a master plan for higher education in California and have it available by February 1960. Their plan will, no doubt, be a reasonable compromise but there is some sentiment in the State Legislature for presenting a constitutional amendment to the voters which would put all state institutions of higher education under one, probably elected, board of governors. This would greatly change the characteristics of the University. Two ways of partially solving this problem might be to have the University emphasize graduate and professional training and turn more of the undergraduate training over to the State Colleges and to have the University provide research facilities for State College faculty members; both of these suggestions, however, present many practical difficulties.

Mr. Wheelock then announced that he would shortly call a meeting of the Executive Committee to go over the Charter and By-Laws of the Staff Council. The results of their examination would be presented to the Staff Council for approval or amendment.

Mr. Wheelock then adjourned the meeting of the Staff Council and turned the chair over to Mr. Eckart, Vice-Chairman of the Faculty. Mr. Eckart pointed out that the Faculty has certain legal responsibilities to the Academic Senate and that one of these involves the members of the Senate who are members of the UCLJ academic committees (denoted by * on the Report of the Committee on Committees, 3 August 1959). A motion was made, seconded and passed by voice vote of the faculty members present to accept the Report of the Committee on Committees as it applies to these members of the committees.

The meeting was adjourned at 16:40.

E.W. Fager, Secretary
UCLJ Faculty
SIO Staff Council

Revelle: So we had more committees than we really needed because of Charles.

In my experience, you need faculty committees for two primary purposes. One purpose is to make decisions that the director doesn't want to make, unpopular decisions. You can always hide behind a committee. Of course, there the director has to play a strong role in getting the decisions that he wants but he doesn't want to take responsibility for. I finally got pretty good at that by sort of, if you want to call it "manipulating" the committee, being very modest, pretending it was somebody else's idea; you know, all the tricks of hiding behind other people.

Another purpose of a committee is illustrated by a library committee, where you really need a faculty committee because they're the ones who are going to use the books, and they therefore should decide how to spend available money for periodicals and books.

Then there is a third kind of committee that you want to avoid like the plague, but often can't. For example, a space allocation committee. Committees can never make up their mind about allocating space because it always hurts somebody and helps somebody else. That's a job the director has to do on his own and take the flak for. It would be nice if you could hide, but you can't because a space committee can never make up its mind, even with someone like Charles Wheelock running it.

The Advisory Council and the Staff Council were at least in part Charles Wheelock's inventions to get as much democracy as possible into the institution. But they turned out to be very satisfactory, particularly the Staff Council. I don't think the Advisory Council exists anymore. But the Staff Council is a very potent and useful body in bringing the institution together and making it work, and they have lots of committees.

Sharp: Within the council itself?

Revelle: Just like the academic senate in the university as a whole. The nice thing about the Staff Council is it doesn't matter whether you're a member of the academic senate or not; everybody's equal in the Staff Council.

Sharp: There were a couple of other issues that came up relating purely to the management of Scripps.

Revelle: One of them, of course, was who ran the ships. That was an interesting problem.

Sharp: And how these ships got paid for.

Revelle: Of course, that was a serious problem. It still is. It never has turned out to be completely solvable, particularly since the ships have now gotten so they eat their heads off in costs. Our big ships now cost $10,000 to $12,000 a day, for every day they are at sea. As long as you're at sea, $12,000 goes down the drain every day that you're there.

But who ran the ships was an interesting and important problem. I felt, and I think I was right, everybody would agree now that I was right, that they should be run by the scientific staff, not by the business office.

In those days the business manager at Scripps was a man named John Kirby, who was not responsible to me; he was responsible to Jim Corley, the U.C. vice-president for Business Affairs. He had an essentially completely independent organization. He ran the purchasing agent, the storeroom, the garage, the buildings and grounds, and I think even the non-academic personnel office--though I'm not quite sure about that. There was a girl named Ruth Handley who ran that--also inventory control and accounting. Accounting was independent too, I guess.

We had a series of difficult accountants, and finally we acquired a wonderful one named Herman Johnson. Unlike most accountants who say, "You can't do this, there's a rule against it," Herman Johnson would say, "We can do it; we'll find a way to do it," and he always did.

Sharp: That's certainly a lot more helpful attitude.

Revelle: As a result of his can-do attitude, he later became vice-chancellor of UCSD [University of California, San Diego] for administration. He became essentially the John Kirby of the whole campus, but a wonderful one, not an obstructionist like Kirby.

One of the very important things about this campus is, of course, the degree of autonomy it

has; the chancellor really runs the place. It isn't run by the U.C. administration in Berkeley, although there are rules and regulations that University Hall establishes. But the farther you can stay away from University Hall, the better life is for everybody. [laughing] And with a man like Herman Johnson, of course, who had everybody's confidence, that works pretty well.

Revelle: Anyhow, Kirby felt that the ships should be run by the business office, and he amassed a lot of evidence about how badly we were running them: my marine superintendent was "no damn good."

Sharp: Right, and that evidence is present in your papers, and in President Sproul's papers too.

Revelle: They would write to President Sproul about how I never made decisions and how everything would be fine if it was just run by the business office! But eventually, Jim Corley, said he was tired of arguing and he decided it was okay for us to run the ships. [laughing] And we did.

The ships have now become part of a national organization called the University Organization for Laboratory Ships, UNOLS, and they're supported by the National Science Foundation [NSF] mostly, I think almost entirely; the ships in some way only nominally belong to the local laboratory. We have the Melville and the Thomas Washington, which are under this UNOLS system. I'm not quite sure how it works financially. The NSF provides the operating funds, but they have to be reimbursed out of projects, individual projects, which contain ship budgets.

This is very bad because the ships are not very efficiently utilized. Say a bacteriologist gets a grant from NSF to do some work in the Philippine Sea. We should send a whole team of scientists to the Philippine Sea, but the ship is being supported out of his grant, so it's really impossible to do it, and often these biologists only work eight hours a day and lie to the other sixteen hours.

Sharp: Even though the ship is there anyway.

Revelle: Yes, and it could be doing a lot.

Sharp: Yes, particularly with the expeditions, we always had a combination of research activities.

Revelle: We always tried to have enough things going so we worked around the clock every day, and never stopped, except when we were in port, and being in port, as far as we were concerned, was a happy experience but a waste of time.

We have an elaborate marine facilities organization, as you probably know. It's called the Nimitz Marine Facility. It has its own shops, its own purchasing and storeroom service, and so forth. That is under the administration of the director's office at Scripps.

In fact, we don't have a business office anymore; everything is done under the director's office. It was not really very satisfactory to have a business manager responsible to and responsive to the vice-president for business affairs at Berkeley and not responsible to me.

Sharp: I guess at one point--this was early, like in 1952--Kirby was trying to deal with what he thought was a problem with your being away some of the time. So there was something called the Administrative Committee established in October and--.

Revelle: That was particularly when I went on these long expeditions.

Sharp: Yes.

Kirby then decided that he would supervise the marine facilities, as part of the role of the business manager's office, rather than you or anybody really attached to Scripps.

Revelle: Which would mean running them through the director's office.

Sharp: The director's office.

There were all those memos going back and forth from Corley to you and Corley to Sproul, Corley to James Miller* especially, regarding the chartering of the vessels as opposed to buying them or some other arrangement, and very elaborate ways of figuring out the costs, and which would be cheaper, and so on. It looks like there was a lot

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*Miller was assistant to the vice president for business affairs, James Corley.

Sharp: of tension about the ships, just what to do with the ships.*

Revelle: That's right. I mean, after all, it was not an orthodox university function. [laughing] We were regarded as a very queer organization.

Sharp: Yet for the work to go on, handling the ships, dealing with them and getting them paid for and everything, was obviously central to how Scripps was going to continue its expansion into more ocean exploration.##

Revelle: What I wanted to do was to allocate the charges by the year and not by the month. This offended the business people, but it seemed to me it was a sensible thing to do because we never knew how the different budgets were going to be used month by month and we would end up, if we made these monthly charges, with a surplus in one budget and a deficit in another, whereas if we waited until the end of the year, we had a lot of flexibility in which budgets could be charged because the work was so overlapping. But you couldn't do the charging in advance, and so I wanted them, as I say, to post-allocate for the whole previous year and to carry over university funds to the next year if necessary. Maybe eventually we did that, I don't really remember whether we did or not.

Sharp: When did the management of the ships come into the director's office as--

Revelle: It never came out of the director's office; it was always there. It's just that Kirby was always objecting to this.

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*See items in President Sproul's papers, including letter to James H. Corley, 10 September 1952, from John C. Kirby; memorandum to James H. Corley and Controller Lundberg, dated 18 November 1952, from Roger Revelle, subject: Request for changes in procedures for charging ship costs; and, memorandum to James H. Corley, dated 17 April 1956, from John C. Kirby. All from Box 414, University Archives, The Bancroft Library, University of California, Berkeley.

Advocacy for the Institute for Marine Resources, 1950

Sharp: I thought we might talk about how the Institute of Marine Resources got started, got under way because it's an important early effort of yours and, as I mentioned, it's a good example of how you helped the expansion of Scripps, even before you were director.

Revelle: Did it start when I was acting director?

Sharp: Yes, it did. It started really in January of 1950.

Revelle: Oh, when Carl [Eckart] was still director?

Sharp: Yes.

Revelle: I see.

Sharp: There's a letter that you wrote to Knowles A. Ryerson in January of 1950, right after New Year's Day, and what you called it was "an all University fishery research program."* Then there were a lot of letters to President Sproul all through 1950.

Revelle: It just shows how things don't get done very fast at a university.

Sharp: No. Well, especially with IMR, things got--.

Revelle: Well, I was thinking about something else. [U.C.] Davis is just now starting a fisheries program. [laughing] Thirty-five years later. And they're appointing a professor of fishery science. I hope it's going to be Jim Joseph, who's the head of the Inter-American Tropical Tuna Commission, but I don't know whether he will be or not. He's a very good man. But the university moves slowly in things like that.

Sharp: Especially when it looks like it's going to cost them some money.

Revelle: That's right.

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*Ryerson was assistant dean, College of Agriculture, at The University of California, Davis. See following pages for copy of letter. From Revelle papers at SIO Archives.

January 2, 1950

RR
Pac. Sci. Bd

Professor Knowles A. Ryerson
Assistant Dean
College of Agriculture
University of California
Davis, California

Dear Knowles:

A visit from you here in La Jolla would be more than welcome and I certainly hope that you can get down this way in February, as you suggest.

One of the things I would like to talk to you about is the possible establishment of an all University fishery research program. I recently went to the United Nations Conference on the Conservation and Utilization of natural resources and was very much impressed with the need for research on fish marketing and processing, which must be carried out if the oceans are to contribute their full fair share to the protein and fat requirements of the world. This kind of research does not lie within the scope of the Scripps Institution of Oceanography, but it certainly lies within the field of the University's interests, and it seems to me, perhaps naively, that it may be closely akin to some of the work now being done in the College of Agriculture.

One of the problems of fish processing research, for example, is the separation of the various components of fish tissue and their transformation into food substances of different type. An example is the Norwegian utilization of fish oils for margarine, "whipped cream" and similar purposes; another is the Chinese production of fish "curds" from the breakdown products of fish protein.

As you know, more than half of the world's population have a protein-deficient diet. This deficiency is very marked in India, and yet the Indian coastal waters contain an abundance of fish. The difficulty is the lack of inexpensive means of preservation of the fish so that they can be transported inland without spoiling.

As far as Scripps Institution of Oceanography is concerned I conceive our function in fishery research to be primarily:

1. Making known the character and extent of marine fishery resources, and

2. Improving the possibilities of their exploitation.

#2

It should not be forgotten, however, that at La Jolla the University has a campus as well as a research institution, and other departments than oceanography might very well be established here, if desired.

Best wishes for a Happy New Year, I remain,

Sincerely yours,

Roger Revelle,
Associate Director and
Professor of Oceanography.

RR-b

CC: Professor Carl Hubbs.

Sharp: Then they move real slowly.

Anyway, there are the letters to Sproul that you were writing, really explaining in great detail what your ideas for the institute would be and the difference between the Marine Life Research program and what the institute would be, which wasn't always that clear, especially to somebody sitting way up there in Berkeley. I wonder if you remember very much about President Sproul's role and whether or not he was really helping to get it going?

Revelle: Oh yes, he was very helpful, always helpful. Everything I wanted to do he thought was a good idea.

Sharp: That's, especially in the president's office, a very nice person to have in your corner.

Revelle: It sure is. That's right. Agnes Robb, of course, was never in my corner, but she was in nobody's corner except Bob's. She was very loyal to him and tried to save him from all those bastards that wanted to get something out of him. [laughing]

Sharp: We haven't talked too much about President Sproul, except about the loyalty oath, and I thought we might, especially in connection with IMR.

Revelle: Well, what he did was to appoint a committee, of course, which was his standard operating procedure, a committee headed, as it says in the record, by Baldwin Woods, who was vice president. But the principal member of that committee, as far as I was concerned, was Clark Kerr. He and I were both directors at that time. He was director of the Institute for Industrial Relations. Clark thought it was a great idea to have this Institute of Marine Resources and was strongly in favor of it. In fact, the whole committee was. The idea was basically to involve other campuses of the university, not just Scripps, in marine affairs.

The one thing that worked out really well for a while was the Food Technology part of it, where we had a man named Harold Olcott. I think he was at Berkeley or at the experiment station near Berkeley. He worked on the problems of fish spoilage, particularly fish spoilage in the can; not botulism, but oxidation and rancidity of the fish oil.

Revelle: As quickly as possible we made Charles Wheelock director of this institute, and he, in the process, was made a professor. He'd started, as I say, as a research engineer, but everybody down here loved him and it was really no problem to get him appointed to a professorship. But he took years to overcome the navy stigma, of course.

Sharp: There were other candidates for the directorship. Now, he's appointed acting director in July of '53.

But there were other people who were considered, and they were scientists.

Revelle: Oh, were there? I'd forgotten that. I was looking at the data and I didn't see any. In the things you sent me, I didn't see anything.

Sharp: This was for the permanent director.

Revelle: Yes, sure.

Sharp: And now I'm not sure when this was. [refers to materials]

Revelle: That was probably after Charles retired. Let me take a look at the list.

Sharp: Those were the people who were considered for permanent director. I'm not sure you can read my writing.

Revelle: [reads materials] Well, this was probably at the time that we were first getting a director.

Sharp: You mean acting director?

Revelle: No, when Charles was acting director he may not have wanted to be director. I don't remember that. He may have felt that it would be better if he wasn't. He was a very modest, gentle man, and he may have thought it would be better to have a well-known scientist.

Of this group here [Rolf Ling] Bolin was at Monterey, at the Hopkins Marine Station, Anton Bruun was the leader of the Danish Galatea expedition, John Kask was a famous fisheries scientist, Kenneth Rae later became head of the marine laboratory at the University of Alaska, [J.] Larry McHugh was one of our graduate students who became head of the Virginia Fisheries Research Institute, Dan Merryman was head of the Bingham

Revelle: Oceanographic Foundation at Yale, Athelstan Spilhaus was at this time dean of engineering at Minnesota. He was the inventor of the bathythermograph and of the Sea Grant system. Allyn Vine was a great friend of John Isaacs and a very inventive oceanographer, particularly for developing instruments. Lionel Walford was the man I told you about who was with the Fish and Wildlife Service. This is a good group of people, but for reasons that I don't remember we actually made Charles the director.

The problem was, at least as I look back on it now, the problem was to think of things for this institute to do to enlist the enthusiasm and the activity of people on other campuses without much money. Money is the most important thing in a university.

Sharp: Not ideas?

Revelle: Money, I'm sorry to say. [laughing] It runs everything else. Ideas are fine, but without money nothing works in a university.

Sharp: To be blunt, the budget that was originally given to IMR was very modest, like $22,000 in the first year.

Revelle: That's right. But we were supposed to build it up to about $100,000, I guess [brief tape interruption after phone rings]

Revelle: As you know, we had an Advisory Council and also an Executive Committee.

Sharp: Both of which I thought we might talk about, but maybe back up just a little bit now to the period in 1953 when you were meeting with a lot of different people. At Cal Poly, for example, you met with the Engineering Advisory Council of UC. I believe it's with UC.

Revelle: Oh yes, Mike O'Brien.

Sharp: And George Tenney.

Revelle: Lou Boelter, I remember that. One of the papers was on the Engineering Advisory Council.* [brief tape interruption]

Sharp: I thought we might just go over how you put together your ideas about IMR, what you thought IMR should be in the beginning.

Revelle: Well, as I said, what I thought it should be was a university-wide institute, with activities on different campuses, much like the Institute of Geophysics, which has branches at Berkeley and UCLA and La Jolla, and maybe at Davis also. On each campus the Institute would support the research of faculty and staff on marine-related problems but not really oceanographic problems.

There were three particular activities that I remember. One was the research on fish preservation technology that Harold Olcott conducted. Another was the possibility of deep-sea mining for manganese nodules. It became quite clear after the 1957 International Geophysical Year that the ocean floor was just covered with these things, billions of tons of them, and it looked like a promising marine resource to explore for copper and nickel and cobalt, not so much for manganese.

There was a young man whom Charles [Wheelock] interested in this problem. His name was John Mero. He became very much involved with mining the deep-sea nodules and in dreaming up ways of doing it. He organized a company later called Deep Sea Ventures, Inc., and in fact half a dozen companies got into this particular act eventually--Kennecott and Lockheed, I remember those two, and a Dutch consortium and various groups all over the world. It was pretty largely based on the work of this young graduate student who was supported by Charles and the Institute of Marine Resources. He was at [U.C.] Berkeley, not down here.

The third thing that they started to do somewhat later was to run the state Sea Grant program, which has its headquarters here but is

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*These were minutes of the Engineering Advisory Council, dated 8 May 1953, from the President's files, Robert Gordon Sproul, University Archives, The Bancroft Library, University of California, Berkeley.

Revelle: part of the university-wide Institute of Marine Resources. The guy that oculd tell you a lot more about this than I is Noel Spiess. He's the director of the Institute of Marine Resources. He said you talked to him this morning.

Sharp: I did talk with him this morning, and because he was doing so much work at MPL [Marine Physical Laboratory] when IMR was being started he didn't think that he really could say too much about the early days of IMR, and when it was really getting going, and what your ideas were for IMR and how they evolved.

Revelle: Well, I don't think they evolved very much. I think we had a basic notion and stuck pretty much to it. As I remember it, there was not much learning experience in it, but I may be wrong about that.

Sharp: I was looking at the testimony that [Wilbert] Chapman presented before the state Assembly Committee on Fish and Game.* What he wanted for IMR or an institute like IMR was a lot narrower. He wanted help specifically for the fisheries, and what you presented was a much broader concept.

Revelle: Yes.

Sharp: It looks like the broader concept was really what happened, what evolved.

Revelle: That's quite right.

Sharp: But there were some other people who were sort of ancillary to all of this, and I really wondered how they were involved. One of the people that I came across was Karl [F.] Meyer.

Revelle: Well, I don't know how that turned out, but he had a biological laboratory which, as I remember it, was in San Francisco and related to the [UC] medical school. He, for years, had conducted tests for the state of California on the sanitary

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*Presented on 1 October 1952, at San Diego. At that time Chapman was director of research, American Tunaboat Association. President's Files, Robert G. Sproul, University Archives, The Bancroft Library, University of California, Berkeley, Box 630.

Revelle: aspects of fisheries' products, keeping the bacterial count down and making sure that the fish were edible and the products were edible and safe. There were people in the university who felt that this was really not a proper university function, that this should be done by the state of California.

I felt that way too. I didn't think it was an academic thing to do. It was purely routine testing and it should have been done by a routine testing laboratory that was run by the state. So we never took that up as part of IMR. I'm pretty sure that Charles Wheelock felt the same way about it.

Sharp: In the early period, and perhaps throughout IMR's early history, it seemed like there was a lot of discussion about what was appropriate for IMR to take up and what was inappropriate for IMR. This is a theme that keeps running through all of this early material.

Revelle: Yes. The only thing I remember that was inappropriate was Meyer's laboratory. Everything else seemed to be appropriate enough.

There is one thing that, thank God, we did not do, and to some extent, or maybe entirely, that reflects the Scripps philosophy. Wib Chapman wanted us to develop an artificial bait for tuna boats, pointing out that for every ton of tuna they had to catch 200 pounds of bait and that half the crew was involved in catching bait and the other half was using the bait to catch the tuna. So it would have been a great economy if they didn't have to catch the bait first.

This was such a completely engineering, developmental thing that it didn't seem to me it was really appropriate for the university. Thank God we didn't do it because within a year the power sheave was invented and the whole tuna-fishing industry changed to purse seining with a power sheave. [spells it] It enabled them to haul these giant purse seine nets aboard the ship, to purse them up and to pull them in. It couldn't have been done by brute human strength, but with the power sheave it was possible.

Sharp: And fishing was not done with lines at all anymore?

Revelle: Fishing rods and bait and barbless hooks over the stern that they had developed here completely disappeared within about a year or two years.

Sharp: That really changed the industry?

Revelle: Yes, and we would have been on this wild goose chase with artificial bait for something that didn't have any use at all.

We were willing to look into the problem of how best to catch the tuna, what was the fundamental problem, not the problem of developing an artificial bait, which was one possible way of catching tuna. We were really outflanked or outinvented by the man who invented the power sheave, who was, as far as I know, a fishing engineer. I don't know who did it.

Sharp: How did it sit with people at Scripps, that you were doing something that was at least partially practical?

Revelle: They didn't object to that. Scientists, in general, like to do things that are of some use to somebody, provided they don't interfere with what the scientists want to do!

Sharp: I guess some people have looked at IMR as a gesture in the direction of practical science, and I wondered what you thought about that?

Revelle: That was certainly not my intention. I mean, I thought of it as a real thing, and not a phony, not a cover-up.

One of the things I remember we did--I'm not sure it was done by IMR, but I think it was--was to study the effects of explosions used to make different kinds of noises in seismic exploration for oil.

The oil companies were using dynamite or TNT, and this killed a lot of fish, or so it was said. The problem was to invent something that would be just as effective for getting the sub-bottom reflections and refractions but wouldn't kill so many fish.

Various devices were developed for doing this, basically releasing compressed air and things like that. That made a softer sound and it was less of an abrupt shock for the fish. If you could just

Revelle: extend the period of propagation of the sound from a fraction of a millisecond to a few milliseconds, apparently it doesn't bother the fish anywhere near as much. It was the sudden shock wave that bothered them.

I think that was done under IMR, although I'm not quite sure of it. It certainly was done here. Carl Hubbs was involved with it.

The basic problem with IMR was to interest people in the university in these marine problems, and that was a tough job. It's gotten more and more difficult for IMR.

It may be that IMR is not a very good idea because what has happened is that [U.C.] Santa Barbara, for example, and [U.C.] Santa Cruz and [U.C.] Berkeley have all gotten little marine laboratories of their own, which they run, and they're involved with marine policy programs and various others things about the marine environment, but they do it on a campus basis, not part of the Institute of Marine Resources, as far as I can make out.

That's in general in line with the movement toward autonomy of the campuses.

Sharp: Is it that IMR got a lot of interest going in the fifties, and now it's sort of outlived its usefulness?

Revelle: You ought to talk to Noel about that. He runs the place; he ought to have some ideas about what it's good for and what it's done. I don't really know. It was a good idea in the fifties, but maybe it's an idea whose time has not only come but passed.

One of the interesting things is that we have people in Santa Barbara who are interested in ocean policy, for example, like Biliana Cicin Sain and her husband Bob Knecht. And we have a couple of people at Santa Cruz who were interested in ocean policy too.

There're lots of interesting problems. The question is how to get the university people interested in them. The problem of the sociology of fishing communities, for example. Not so much in the United States, although even here there are some social problems, but in many developing countries there are people called artisanal

Revelle: fishermen. [spells it] Artisans, in other words. They're people who barely make a subsistence living; for the most part, fishing from small boats, bringing their catch into shore, distributing it on the beach, selling it on the beach.

I remember one fishing community in India, the west coast of India, whose primary way of making a living was smuggling; fishing was just sort of a cover-up. Another one was in Lagos, near the University of Lagos in Nigeria, whose primary activity was collecting sand for building construction, instead of fishing. These fishing communities just make a living any way they can, and they're usually the poorest and most backward communities in the country. In Bangladesh there're lots of them. They're Hindus, Hindu outcastes. They didn't see any point in migrating to India because they'd be outcastes in India, so they might as well stay in Bangladesh, but they're brutally exploited by the bosses in the villages or neighboring villages.

So in these artisanal communities, the problem is to improve their lot if you can by getting them better boats and better navigating equipment and better processing for their catch. The preservation of the catch is the most important single thing to do.

In Lake Chad there's another fishing community where about half the catch is lost by the time it gets to the city of Maiduguri, one hundred kilometers away, where the people are. They smoke the fish, but they smoke them in such a crude way that they're not very well preserved.

Another problem is that of coral reef communities, communities that use coral reefs as fishing grounds. They use the fish around coral reefs as their basis of subsistence. The coral reefs are being killed in many areas by sedimentation from the land. As agriculture develops and erosion takes place the mud washes out over the coral and kills it.##

So on a worldwide basis there're lots of very practical problems with fisheries that are not really oceanography at all, but are involved with economics and sociology and the anthropology of different communities.

Sharp: Sounds a little bit like the water logging and salinity issue, in that there's a specific problem having to do with water flow and all the rest of it, but the problem exists within a society, and all the other issues have to be addressed as well as the obvious physical problem.

Revelle: That's right, exactly.

Sharp: What I want to do before we end this today is to talk about really the end of the period that you were director, and the decision to leave and just what all of that implied.

Revelle: I don't know how you plan to do this, but we might talk first for awhile about international activities in oceanography, which I was involved with when I was director of Scripps.

One of the great events in oceanography in 1959 was the international Oceanographic Congress.

Sharp: We have talked about that.

Revelle: Oh, we have already?

Sharp: Yes. We talked about the congress and the commission, the Intergovernmental Oceanographic Commission.

Revelle: And about SCOR?

V ADVISING THE DEPARTMENT OF THE INTERIOR

The Appointment, 1961

Sharp: Yes. And the IGY.

Revelle: Yes. Norpac operation probably too.

Sharp: Yes.

Revelle: I see, we've done that already?

Sharp: We have.

Revelle: Okay.

Sharp: Yes, I wasn't missing it. When we had that first session on international cooperation, the IGY, and all of that that we handled through the fifties and the early part of the sixties. So I think we've done that pretty well.

Revelle: Okay, fine.

The other thing which you've sort of stayed away from because that is Kathryn Ringrose's job, but which you can't stay away from entirely was the beginning of the university here. Beginning around 1956, '57, most of my attention was given to that and I kind of neglected the internal problems of Scripps. I didn't neglect the external problems so much.

Revelle: Science in general got a tremendous boost from the Sputnik which the Russians launched at the beginning of the International Geophysical Year.

Sharp: That was '57?

Revelle: '57, yes.

The National Science Foundation and other government agencies in Washington all of a sudden started pouring money into research, and particularly oceanographic research. We started building new ships.

One of the things that was important at this time was the report of the Third National Academy Committee on Oceanography, which was headed by Harrison Brown. That was at the time when George Kistiakowsky was science advisor to President Eisenhower, and George seized on this report of ours, it's called "Oceanography 1960-1970," as a vehicle for inter-governmental coordination. Harrison Brown was very much interested in the politics as well as in the science, and he was determined that this report would have a real impact, unlike the 1951 National Academy report, which had some internal impact, but not much effect on federal policy.

So Harrison went to Congress, and the Congress organized subcommittees on oceanography, one in the Senate and one in the House, and a lot of legislative investigation and activity took place, and the federal budget in oceanography just went through the roof. Not so much because of expansion and support of Scripps and Woods Hole, but because the navy decided that practically everything they did was oceanography! So we had million-dollar budgets in oceanography, most of which we would not have recognized as really oceanography. But nevertheless there was a big expansion of real money too for oceanographic institutions. That was basically because of Sputnik on the one hand and Harrison Brown's committee on the other.

This was a committee, of course, of all the very familiar faces. Maurice Ewing and Columbus Iselin and me and Benny Schaefer and Athelstan Spilhaus and Fritz Koczy and I guess Gordon Riley and Dixie Lee Ray were members of it at one time too. Dixie Lee Ray later became governor of

Revelle: Washington. She was a biologist. I always liked her, but a lot of people didn't. She was a queer woman. She lived in a trailer with her two dogs, by herself pretty much.

In any case, these were going on at the same time, these national and international activities, which I was very much involved with, and at the same time we were trying to start the university.

Then, in 1961, the university was pretty well under way and they appointed Herb [Herbert F.] York as chancellor. So I decided it was a good idea to leave for a while. Jerry Wiesner was science advisor to President Kennedy, and I told him, "I need a job in Washington." He and I were very good friends. He was trying to get science advisors in every department, so he offered me the job of science advisor to the Department of the Interior.*

Sharp: Can I interrupt you for just a minute? You say it so matter-of-factly, that you told Jerry Wiesner that you needed a job in Washington. Could you have decided that way? I mean, you had invested a lot of time and effort at Scripps, and there was no way that you could see you could stay and not take a leave of absence or not leave permanently?

Revelle: Well, I just took a leave of absence. I didn't permanently leave. I didn't intend to. This was a political job really, and no political job has any tenure, so it was obviously a temporary job, and it was intended to be a temporary job.

Sharp: But there were other things that you could have done. I mean, you could have not been director and just done oceanography as a professor of oceanography. Was that a real option for you?

Revelle: By that time I probably didn't think it was. You know, you don't do very much science when you're a director. You do some, but not a hell of a lot. But in any case, of course I could have stayed director for the rest of my life. There was no problem about that. The regents thought I was a fair-haired boy, as far as Scripps was concerned,

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*See following pages for Department of Interior press release announcing this appointment, dated 12 July 1961. Revelle Papers, SIO Archives, Box 1.

Revelle, Biographical. box 1
Personal Data (Miscellaneous), 1958-1963

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DEPARTMENT OF THE INTERIOR

INFORMATION SERVICE

OFFICE OF THE SECRETARY

For Release JULY 12, 1961

ROGER REVELLE APPOINTED SCIENCE ADVISOR TO UDALL

Dr. Roger Revelle, of La Jolla, California, has been appointed Science Advisor to the Secretary of the Interior, Secretary Stewart L. Udall announced today.

Dr. Revelle, who is on a leave of absence from his post as director of the University of California's Scripps Institution of Oceanography, will serve as the principal science advisor to the Secretary and will coordinate the several scientific programs of the Department.

In announcing the appointment, Secretary Udall said: "Doctor Revelle will be the first science advisor in the history of the Department of the Interior. We are very pleased that a scientist of his broad experience and distinguished position in the scientific community has accepted this appointment.

"I am anxious that the Office of the Science Advisor shall exert a maximum influence on the policies and programs of the Department. In addition to acting for me in all scientific matters within Interior, he will represent this Department on the Federal Council for Science and Technology, and in other interdepartmental agencies for coordinating the scientific activities of the Government."

Born in Seattle, Washington, on March 7, 1909, Dr. Revelle received his A.B. degree in geology from Pomona College in 1929 and his Ph.D. in oceanography from the University of California in 1936. He has been professor of oceanography at the University's Scripps Institution of Oceanography since 1948 and its director since 1950. For the past several years, he has also been director of the La Jolla campus and dean of the University's School of Science and Engineering at La Jolla.

During World War II, he served as a Commander in the U. S. Navy. Immediately after the war, he was head of the Geophysics Branch, Office of Naval Research. He also served as a staff member of Operations Crossroads, the 1946 atomic test at Bikini.

One of the country's leading geophysicists, Dr. Revelle has led several oceanographic exploring expeditions into the south and west Pacific, and is one of the authors of modern theories of the structure of the earth underneath the oceans.

He is a member of the National Academy of Sciences, the American Philosophical Society, and the International Council of Scientific Unions. He has served the Government as a member of the U. S. National Commission for UNESCO, the Naval Research Advisory Committee, the Advisory Council of the Peace Corps, and panels of the President's Science Advisory Committee, and as a U. S. delegate to various international conferences.

Dr. and Mrs. Revelle are the parents of four children and have five grandchildren.

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Revelle: but not as far as the new campus was concerned. But they were very enthusiastic about Scripps, and they wouldn't let me quit. Even when I became University Dean of Research, I still had to be director of Scripps. So I spent a week down here and a week at Berkeley, after we came back from Washington, D.C.

Projects in the American West and Pakistan

Revelle: I got very much involved with one project in the Department of the Interior, that was the Pakistan project, and I was probably not an ideal science advisor because I didn't spend enough time covering all the divisions of the Department of the Interior. I did quite a bit though.

I was chairman of the Water Resources Committee of the Federal Council for Science and Technology, and we put across a big water resources research program in the Department of the Interior. That was a very tough thing to do. There were something like fifteen or twenty agencies all doing water resources research and they didn't want the Department of Interior muscling in on their act.

Another project was desalination using atomic energy. Atomic energy was riding high in those days and this was a possible use for it. So we examined the problem of how you do it. [brief tape interruption]

One of my jobs in Washington as Stewart Udall's science advisor was to be the Department of Interior representative on the Federal Council on Science and Technology, where each department had a representative.

Some of these representatives were more equal than others. Particularly I remember Harold Brown, who was Assistant Secretary of Defense I guess, or maybe Assistant Secretary of the Air Force. He didn't pay any attention to the council at all. He came to the meetings and said, "This is what we're going to do," essentially.

In the long run you had to get the secretaries involved for such a thing as this Water Resources Research program. Stewart Udall and Oville

Revelle: Freeman, the Secretary of Agriculture, came to one of the meetings, and they agreed, of course, with what I proposed, more or less over the dead bodies of the other departments, but because these were the two principal departments involved, they could put it across with the president, President Kennedy.

Sharp: The Water Resources Research program was then--.

Revelle: Was organized in the Department of the Interior.

Sharp: So it was working with different projects around the United States?

Revelle: That's right. It was largely supporting work in the states. That was one of the Congressional conditions, in fact, that it should be largely related to the state water agencies. Water is a very highly political subject in the United States.

Sharp: It certainly is in California.

Revelle: Yes, it is all over the West, and the states are very jealous of each other.

One of the projects I was involved with in the department was the Welton Mohawk Irrigation Project in the Gila River Valley. This was an alluvial basin in the Gila River where there was a lot of underground water, but the water was salty and very high in sodium. It was terrible water. We were obligated to give the Mexicans two million acre-feet of water each year in accordance with the U.S. -Mexican Colorado River Treaty, and these Welton Mohawk farmers were pumping out this awful water and dumping it into the Gila, which is a tributary of the Colorado, and it was going down to Mexico and ruining the Mexican farm lands. It was terrible.

President Kennedy was very anxious to have good relations with Mexico, and the only real way to solve the problem was to build a separate aqueduct for this water and dump it into the Gulf of California, not to put it in the Colorado at all. That was quite expensive. It would cost about $175 million.

My problem was to try to get the western states to agree to this. This was a hopeless job. The governor of Utah wouldn't even speak to the governor of New Mexico on the telephone about water. They insisted on having witnesses in the

Revelle: room when they talked about water! I was just a babe in the woods in this political jungle, without really realizing it.

Arizona had a senator named Carl Hayden who was about ninety-five years old at that time, or at least ninety, and he could only stay awake about ten minutes out of each hour. But during those ten minutes he was, as far as I was concerned, very destructive.

He got a resolution passed by the Senate which prohibited spending any money whatever, even on surveys of this proposed bypass pipeline, basically because the Bureau of Reclamation had negotiated this treaty and they were very proud of themselves because there was no mention of quality of water in the treaty. So they thought it was quite kosher to dump this destructive water, this poisonous water into the Colorado.

That problem finally got solved, but long after I left the depatment. After Steward Udall left, for that matter.

I've forgotten other things that I did. One I remember was that I fought constantly with the director of the Geological Survey, a guy named Tom Nolan, because I wanted the survey to support university research with part of their funds. They had a policy of hiring university faculty members on what they called a WAE [when actually employed] basis to work for the survey in Washington, D.C., or in the field, but in no way did they support university research directly.

I think they finally changed their policy. Bill Menard of our faculty later became director of the Geological Survey, and I think they loosened up a good deal. But at least at that time they had this rigid policy.

The Geological Survey, in general, felt they were really not part of the Department of the Interior, and they weren't going to take any shit from a science advisor, even though he was a famous scientist. They would go to meetings and they would never say, "Department of Interior," they'd always say, "U.S. Geological Survey" as their attribution.

Sharp: I was thinking later on, you and I might get a chance to discuss your work in the formulation of

Sharp: state and national science policy, on a big scale.

But hearing about the survey and the Water Resources Research program and the Gila River issue really helps me to get a sense of what your ideas were about how exactly the federal government should assist science, and what kinds of projects the government should fund. Maybe we've gotten a head start on that just by your mentioning briefly some of this work.

Revelle: Yes, that's an issue of course which evolved over the years, the federal science policy. The basis of the modern policy was invented during World War II by a bunch of young reserve officers in Admiral [R.A.] Furer's office. He was called the Coordinator of Research and Development. There was one of them who was not a reserve officer, Bob Conrad. He later became the uniformed head of the Office of Naval Research; not the chief of Naval Research but the principal idea man in a uniform. The others on Admiral Furer's staff were reserve officers: John Burwell, Jim Wakelin, a man named Krause, and various others whose names I've forgotten. I remember those three particularly. Manny Piore and Randall Robertson were not in Furer's office, but they joined in very quickly with the others to form the Office of Naval Research, together with Alan Waterman, who'd been in the Office of Scientific Research and Development as chief scientist.

They all joined together to form the Office of Naval Research. It was originally called the Office of Research and Inventions, but by the end of the war, when they all got demobilized, it became the Office of Naval Research.

They had the wonderful and very unorthodox notion that the federal government should support basic research. Even more important they defined basic research as that which researchers wanted to do. So the researchers would make proposals for what they wanted to do and ONR would support them, which is really the basis of ntional science policy even today, insofar as research is concerned.

That was not at all what I was used to in the Bureau of Ships. The Bureau of Ships told the scientists what we thought they ought to do, and they would more or less do it.

Sharp: As an assignment?

Revelle: Not exactly as an assignment. We'd say this was our problem, "Please work on it," and they would pretty much try to do that.

It took me a while to learn this completely new system where it didn't matter at all whether a project applied to naval problems; all it had to be was good science. In fact, in my section, the geophysics branch, we said that we would automatically turn down any proposal in which the guy said, "We want to do this because it will help the navy."

Sharp: I remember your telling me that, when we talked about your work with ONR. It sounded as though you'd learned the lesson pretty well if you were able to take on pure science projects.

Revelle: Oh yes, sure.

VI TIME FOR CHANGE

University Dean of Research, University of California, 1962-1963

Revelle: Coming back to 1960 and after, while I was on leave of absence, Noel Spiess was acting director of the institution. When I came back I was director and also University Dean of Research.

Sharp: Yes, we need to talk about that period.

Revelle: The University Dean of Research part of it was really a non-job because no damn dean, particularly no university dean, is going to have anything to do with my research or a professor's research!

Sharp: And yet, from the correspondence that I found, I was really trying to figure out what you were doing.

Revelle: [laughing] That's what I tried to do.

The one outfit that I managed to pester to the point where they desperately joined the [U.C.] Santa Cruz campus was the Lick Observatory at Mount Hamilton.* I suggested that they should join the

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*See Revelle's letter to Vice-president Wellman, dated 30 October 1963, regarding the Lick Observatory, on the following pages. Revelle Papers, SIO Archives, Box 20, F 15.

October 3, 1963

VICE PRESIDENT WELLMAN:

I had a long conversation with Albert Whitford yesterday about some of the problems of the Lick Observatory, both short and long range, and about the future of optical astronomy in the University of California.

We discussed three more or less short range problems, as follows:

1. Various kinds of auxiliary equipment for the telescopes are needed at Mt. Hamilton, together with the technicians to build, adapt, maintain, and/or operate them. Laboratories, offices, and especially houses are needed for these technicians. Additional housing to accommodate visiting investigators is also required, together with dormitory space and dining facilities for graduate students and unmarried employees. Federal research funds can probably be obtained to pay for the equipment and to hire the technicians, and perhaps to build additional laboratory and office space. But where can the money be found to build living and eating accommodations? According to Director Whitford, it is very difficult to make these entirely self-liquidating. This is especially true for the graduate student accommodations. Director Whitford proposes that everyone living at Mt. Hamilton be charged rent, and that at least a portion of the rental funds be used to pay back a loan from the Regents which would be used for prompt construction of additional living facilities. From experience with our campus housing at La Jolla, I am skeptical as to whether the rental money would be sufficient for this purpose. Moreover, it would be difficult to begin charging rent to the present staff at Mt. Hamilton without a compensatory increase in their salaries. If we did this, we would, in effect, be obtaining the money for new houses from the State indirectly. It seems to me it would be better to ask straight out for the money for new living accommodations, with the justification that these are a necessary adjunct to the research facilities.

2. A small computer is needed at Mt. Hamilton. If a contract can be entered into before March 15, 1964, an IBM 1620 could be rented for about $15,000 a year for the next five years. After the first year, the rental could undoubtedly be paid out of project funds, but it will be difficult to get a project started before the March 15 deadline. After this date, the IBM rental costs will be considerably higher. Probably a more important consideration is that if the University rents the computer initially with its own funds, it will be easier to obtain future extra-mural support. I urged Director Whitford to write to Chancellor Strong requesting allocation from some source of enough money to pay a year's rent on the computer, with the understanding that future rentals will be funded from research grants and contracts.

3. Director Whitford asked me for suggestions about possible members from the southern campuses of the new Statewide Committee on Astronomy. I suggested Dean Earl Griggs from Santa Barbara, Dean Nisbet or Professor Lawson from Riverside, Professors Libby, MacMillan, or Saxon from UCLA, and Professors Gilbert, Arnold, or Rosenbluth from San Diego.

Two of the long range questions about optical astronomy in the University of California are:

1. How many optical astronomers should be on the faculty of the University of California? My answer would be the smaller of two numbers: The maximum number that can use the 120-inch telescope most effectively (This is somewhere between 12 and 18); or the maximum number of first rate astronomers that can be induced to join our faculty. These numbers might be more or less than that derived from any formula about the actual or potential ratio of faculty members to graduate and undergraduate students. The situation will change if and when another large telescope becomes available for our faculty. To use the 120-inch telescope most effectively, our faculty must not use it all the time. Some observing opportunities must be reserved for senior visitors and promising post-doctoral fellows.

2. How should optical astronomers be distributed in the University? At least four considerations should enter into the answer to this question:

A. The need for a "critical mass" at Mt. Hamilton;

B. The need for undergraduate and graduate teaching in optical astronomy;

C. The need to operate the 120-inch telescope in optimum fashion;

D. The requirement that the faculty of the University of California should contain only first rate people.

It seems to me that many problems might be solved if all provisions for faculty appointments in optical astronomy were to be made joint between Lick and the Departments on the different campuses. That is, each FTE provision would be shared between Lick and a campus Department. Such an arrangement would be similar to that which now exists and is working well in the Institute of Geophysics and Planetary Physics. It could easily be established for new appointments, and I see no very good reason why it could not be applied to present faculty members.

I believe we should think seriously about the possibility of moving the headquarters of the Lick Observatory staff to one of the existing campuses. The Mt. Wilson and Palomar Observatories, which are acknowledged by all concerned to be the leading group in optical astronomy in the world, have always operated with their headquarters in Pasadena. The astronomers spend time on the mountain only when they are actually using the telescopes or doing something with the auxiliary equipment. There are obviously many arguments on both sides of this question, but a strong argument in favor of a move is that it would bring the astronomers into closer contact with physicists, chemists, mathematicians, and engineers. In spite of its ancient tradition, astronomy is not a unified or universal science like physics or chemistry. In the modern world it is probably better described as the application of the basic sciences to an object of study, namely, the universe. The more intimate and varied are the relationships between

astronomers and physicists and the other basic sciences, the more rapidly can we expect to advance our understanding of the stars and galaxies.

Roger Revelle

cc: Director Whitford

RR:pw

Revelle: [U.C.] Berkeley department of astronomy, they should be part of it, of the Berkeley campus. They were scared to death of that because Berkeley was so high-powered. These poor guys lived up on a mountain all by themselves, year after year, and they were very good astronomers but they'd lost contact with modern physics and modern chemistry, so they just didn't want to try to play in the same league with the Berkeley astrophysicists and theoretical people.

Yet it seemed to me that, from a teaching point of view, it was very bad business to have this isolated observatory on Mount Hamilton with practically no graduate students and no access from other campuses to the big telescope.

What I was particularly concerned about was that Margaret Burbidge here in La Jolla, one of the world's leading observational astronomers, couldn't look through the big Mount Hamilton telescope. A, she was a woman, and B, she was down here in San Diego, and they didn't let people from other campuses use the telescope. I thought that was an outrage.

Sharp: So there were aspects of that position that enabled you to do some good?

Revelle: Well, at least stir things up. So they eventually became a department of the Santa Cruz campus.

Sharp: And were brought fully into the university?

Revelle: Yes, more or less, insofar as you can be fully in the University of California at Santa Cruz.

Sharp: It looks as though you spent a little time putting out fires that were erupting, having to do with some research. I was wondering particularly about NASA and the university's relations with NASA.

Revelle: Yes, you had several papers on that, which I read. But that was, I think, a non-problem, as most of these University Dean of Research problems were. President Kerr apparently, or NASA, or somebody was anxious that we should get social science involved with the NASA program, and it was not clear why or how you do this. But we were supposed to report on

Revelle: what we were doing; I remember it from these papers that you showed me.*

Sharp: NASA was feeling that they weren't being totally informed, for example, of what the individual campus contracts were doing.

Revelle: Yes, I guess so.

Sharp: This whole period when you were having this twin responsibility of continuing as director of SIO, after you came back from the leave of absence, and then doing this University Dean of Research job, how did that work? You mentioned you came down to La Jolla a week and were there in Berkeley a week.

Revelle: Yes, that was true.

Sharp: That was it?

Revelle: Yes. It didn't work very well, but I had two good secretaries, which helped a lot, Priscilla Duffield here in La Jolla and Pauline Wyckoff in Berkeley. They more or less ran the show when I was not here.

By this time Charles Wheelock had retired. He had prostate cancer, had had an operation, and never was really himself after that, although he lived for a long time afterward. He became an advisor to Dean McHenry, the chancellor at Santa Cruz, and helped a lot with the physical development of the Santa Cruz campus.

His wife was a rather bothersome person. She was an artist, and she sort of led Charles around by the nose. I would not have wanted to be married to her. [laughing] She wasn't very popular here in La Jolla.

Pauline Wyckoff went up to Berkeley. She'd been my secretary before, here, and then went with me to Washington, then she settled in Berkeley. Priscilla Duffield was down here. She had been Bob Oppenheimer's secretary at Los Alamos during the

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*See memorandum from Revelle to Chancellors Franklin Murphy, Edward Strong, and Herbert York, dated 16 September 1963, and several other memoranda, Revelle Papers, SIO Archives, Box 20, Folders 15 and 16.

Revelle: Manhattan Project of World War II. She was the wife of Bob Duffield, a first-class chemist who was involved with General Atomics. She was a very strong-minded woman who really pretty much told me what to do and when to do it, and that was fine with me!

One of the nice things about it was that Ellen and I lived in a wonderful house in Berkeley. What's the name of that very fancy part of the Berkeley hills where many faculty members live? Is it Amalfi Drive? There was an English professor, a great expert on Mark Twain, Mark Schorer his name was. He was away on a sabbatical leave, and we lived in his house. That was an absolutely marvelous place to live, right in the heart of the Berkeley faculty in the Berkeley hills.

Sharp: Did you have a sense of really being a real participant in the Berkeley campus at all?

Revelle: No, not really. Well, I wasn't really involved with the Berkeley campus. I was involved with University Hall. I used the Faculty Club a good deal. I loved the Faculty Club. When Ellen stayed down here, I lived in the Faculty Club.

Sharp: That's a very beautiful spot.

Revelle: It's a marvelous place. So everything was nice about it except the job. It was essentially a non-job, but it was a job I dreamed up. Clark asked me what it would take to bring me back, and I suggested what I wanted to be was the University Dean of Research.##

Sharp: Well, what did you think it was going to be? Or what was your idea for it to be?

Revelle: I thought that I would have the same kind of a job that I'd had at Scripps, developing research programs and developing the university's research activities. I didn't realize what a low status deans have on the Berkeley campus. [laughing] Nor did I realize how the average faculty member--unlike the Scripps faculty--was uninterested in any cooperative ventures of any kind. They were rugged individualists, pretty much.

One of the things we tried to do was to set up a policy research institute. I think that eventually one did get set up, with headquarters at the [U.C.] Irvine campus. There was a big, beautiful

Revelle: woman named Catherine something who really wanted to do this. I worked with her quite a bit and with Seymour Martin Lipset, who was at that time at [U.C.] Berkeley, and was running the Institute of International Affairs, or something like that at Berkeley. They had one of those International Institutes at UCLA also, and they got support from the Rockefeller and Ford Foundations. I had something to do with their program, getting money for it, I guess, or getting support for it or getting people involved with it.

But it was not on the whole a satisfactory job, and it wasn't very satisfactory trying to run Scripps every other week either.

Sharp: Especially as large as it had become.

Revelle: That's right.

Sharp: I mean, it's very large now, but it was very large then too.

Revelle: Yes.

It's a somewhat different fate that has been suffered by Bill Nierenberg. Bill has been director too long, and he's gotten bored with the job. Although he was quite effective the first ten or twelve years, he hasn't been very effective for the last seven or eight yeas. He just doesn't have the push, the drive to do it. So Scripps has gone on, as Scripps went on when I was here every other week. It's hard to kill an institution. [laughing]

Sharp: Well, especially one that's turned out to be very successful.

Revelle: Yes. I mean, even if I hadn't been here at all, it would have run pretty well for years, just on momentum.

Further Notes on the Pakistan Project and Its Politics: Punjab and Sind Relations

Sharp: I thought tomorrow we would talk about the move over to Harvard and that summer of '64 and what all you were doing at the time, and then get you into

Sharp: what it was like to be there and the different projects that were all going on.

Revelle: I think the most important thing we ought to talk about is the work in Pakistan.

Sharp: With that I think we would wait till Friday because it fits in with other work in international cooperation.

Revelle: Well, the reason I thought of doing it first is that that's really what determined my going to Harvard.

Sharp: Yes, that's what I thought, but I think we could sort of start at the end of that, start at the end of your work in Pakistan tomorrow and then see how that fits in with the reasons for going to Harvard.

Revelle: Yes, okay.

This work on Pakistan was done '61 to '63, while I was in Washington largely, although we didn't actually get the report published I think until January of '64.

Sharp: I couldn't find the date. I saw copies of the different drafts of the report but they were undated, and I wasn't sure about the actual date of the final report.

Revelle: I have the report. I think it was January of '64, by that time President Johnson had succeeded President Kennedy. I've got a copy of the report out in the office, but they're very rare documents now.

Sharp: There isn't even one in your papers. At least I couldn't find it. All I found were labeled "draft," and I wasn't sure what the differences were or the supporting material even, that was added in its final form.

Revelle: Well, it was modified quite significantly to make it acceptable to the Pakistan government. There was one guy named Zulfikar Ali Bhutto, who became prime minister of Pakistan and after that was hanged. He was the minister of Irrigation and Power in Ayab Khan's government, and he was a Sindi. He was actually an immigrant to Sind from somewhere in India. His family had managed to get hold of tens of thousands of acres of land. They were the biggest land owners in the Sind, in spite

Revelle: of the fact that nobody was supposed to own more than 360 acres. Somehow or other it didn't apply to him or to other large landowners in the Sind.

Sharp: His role as minister of Irrigation and Power would have certainly helped him along in trying to get that land under cultivation.

Revelle: Well, it was under cultivation. It was in an area called Lakanal. I'm remembering all sorts of funny words, like Lakanal, in the Sind.

In the initial draft of our report we had said the Pakistans and the various aid agencies ought to concentrate their investments in the Punjab because it was clearly economically advantageous to do so, and that was where the most opportunities were for agricultural development. The Sind was much more difficult and much more expensive.

I spent perhaps one of the most unpleasant mornings I've ever spent in my life in Bhutto's house in Karachi being bawled out with my friends Harold Thomas and Bob Burden because we had neglected to emphasize what should be done in the Sind. He pointed out that there was a treaty between Punjab and Sind, an old treaty, still in force, which provided 40 percent of the Indus River water to the Sind, 60 percent to the Punjab and to other provinces.

So we had to abandon this policy of saying emphasize the Punjab, and tell all the good things we could think up about the Sind, which weren't very many.

Sharp: So the final report says--.

Revelle: Says both. It's much more even-handed between Punjab and Sind. However, if you study it carefully you'll see that we didn't really change our position.

Transition to Harvard University's Center for Population Studies, 1964

Sharp: When I was looking at the period of January through March of 1963, there was so much going on that it's very hard to sort out. You were very, very involved in Pakistan panel work, and then assuming

Sharp: this role as University Dean of Research, and then trying to be director of Scripps also. It's very hard for me to ask questions about it because I'm not sure where one leaves off and the other beings; it's rather circular. So all of the Pakistan activity I lumped under international cooperation, but one of the things I thought we might start with tomorrow is just why you went to Harvard, to begin with. The role of Harold Thomas, I thought, might have had something to do with it, but I really wasn't sure.

Revelle: Oh, very much so.

Sharp: The papers don't show anything at all. Maybe there's good reason why they don't show anything.

Revelle: I don't know what they are.

Sharp: Well, there's very good evidence for how the Harvard University Center for Population Studies grew, but the reason for your going and what other options there were, maybe you can just think about that.

Revelle: Well, the reason I went was primarily because Herb York had resigned and I still was not going to be appointed chancellor. That was quite clear, not from what Clark [Kerr] said but from what he didn't say. So I gave Clark an ultimatum. I had to decide by a particular time whether to take the job or not. This was one of the low points in our relationship. I remember very well calling him up and getting Kay on the phone and she said, "He's out poisoning plants and he can't talk now." [laughing]

Sharp: Did it seem an apt thing for him to be doing at the time?

Revelle: [laughing] That's right.

So I told Jack Snyder I'd take the job at Harvard, and that was that. Clark has since told me, we've become completely reconciled, and he's since told me it was impossible because of Ed Pauley on the Board of Regents.

Sharp: What was the problem with Ed Pauley? I mean, why was he so venomous?

Revelle: Because of the fact that the campus was put here where it is over his dead body. I told you about

Revelle: this meeting at Davis, or maybe I didn't tell you that. Not the meeting about the loyalty oath, the meeting when they decided to put the campus here.

Sharp: I think you did tell me about that.

Revelle: I told either you or Kathryn Ringrose.

Well, the outcome was that after Clark Kerr read this letter from Charles Luckman about the Scripps Memorial Hospital not having any problems with airplane noise, Pauley said, "Who wrote that letter?" Luckman said he did. Somebody else said, "Those people in La Jolla have been waiting long enough. I move we decide to establish a campus in La Jolla." [laughing] The vote was twenty-one or twenty-two to one to do that.

Sharp: And Ed Pauley was the one?

Revelle: Yes.

That was a pyrrhic victory, from my point of view, but still a very important victory. It was far more important to put the campus here than it was to make me chancellor.

It has its disadvantages certainly, the present location. The principal disadvantage is faculty housing. So we don't have much collegiality because people live so far from the campus. But still, it wouldn't have been much of a place anywhere else I don't think, and it's a great place here.

Sharp: Have you thought very much about how your life would have been different if you had become chancellor either the first time or the second time around?

Revelle: Yes. I would have lost my job during the time of troubles, when the students were pretty much in revolt, when Bill McGill was chancellor. He was ideal for the job at that time. He is a Scotch Irish man and has an Irish political sense of what's possible and what isn't. So he managed to sit it out pretty well and cool it off. I never would have been able to do that; I was too inflexible and too insensitive, I think.

Sharp: Insensitive to which group? To the students or to the administration?

Revelle: Both probably!

A lot of people lost their jobs then. One of them was my friend the President of Harvard, Nathan Pusey. He never really recovered from the '68 troubles at Harvard. He just didn't understand what was going on.

Sharp: He was president when you went to Harvard?

Revelle: Yes. A wonderful guy, but a pretty old-fashioned guy.

Sharp: If you'd become chancellor, would you have become more flexible? Isn't that one of the skills you would have picked up?

Revelle: I don't think so. It came too fast. Just all of a sudden out of the blue the whole thing blew up. [snaps fingers]

When I was at Harvard I spent one night I remember. The headquarters of our Center was in a little wooden house. They called it 9 Bow Street, between the bicycle shop and Adams House. It was built in 1810 and is still the headquarters of the center; I spent one night there afraid the students would burn it up. My student Andy Abbott and I stayed up all night to try to protect it. Every window in Harvard Square was broken that night, every window. It was just terrible. I just don't think I would have been able to do the right things. You couldn't even tell what was right to do. I mean, to a considerable extent it was a matter of luck.

Sharp: I never have really thought about it from the administrator's perspective or the college president's perspective because I was one of the students--

Revelle: Sure!

Sharp: --and I thought about it only from my perspective.

Revelle: What years were you here?

Sharp: I was here '69 through '71.

Revelle: That was a little bit later.

Sharp: It was a little bit later, but it was probably the roughest part for U.C. San Diego because of the burning. I remember a history graduate student who killed himself.

Revelle: He burned himself to death, didn't he?

Sharp: Yes.

Revelle: Immolated himself.

Sharp: I remember seeing his body in the ambulance as it went by. I was living on campus, and my mom called up. She was crying; she was really very, very upset, afraid that something would happen to me.

Revelle: You were a graduate student then or undergraduate?

Sharp: Undergraduate.

I remember in a weird kind of way I thought it was very exciting.

Revelle: Which college were you in?

Sharp: Muir.

Revelle: Because I gave a commencement address at Revelle College about that time in which, if I remember it, I talked about patriotism. I said, I remember very well, that I had originally been in favor of the Vietnam war and I'd finally changed my mind completely and thought it was a terrible mistake. That was around 1970, '71.

Sharp: I guess if you'd been chancellor, you couldn't have said anything like that.

Revell: Why not?

Sharp: Because it would have meant you were taking sides, and you probably couldn't have done that if you'd been chancellor.

Revelle: I probably would have, though! That's the way I am.

Sharp: Then you probably would have lost your job!

Revelle: [laughing] Yes, that's right. That's the way I am, unfortunately.

Revelle: I remember about that time I was on the short list for the presidency of the University of Colorado, and I queered my chances quite obviously by saying that the university's got to be run by the faculty; you can't let the students run the university. At that particular time there were a lot of faculty members who thought the students should have a real voice at running the university.

Sharp: In curriculum development and administration--?

Revelle: Yes, everything.

Sharp: --and also throughout.

Revelle: Yes.

I was quite rigid about it. A university has to be run not by the administration, not by the students, not by the politicians, but by the faculty. I still feel that way, even though the faculty doesn't do a particularly good job either.

Sharp: And they don't agree on what should be done.

Revelle: That's right.

Sharp: When was this at Colorado?

Revelle: Sometime in the early 1970s, late 1960s, about 1970, I guess. Maybe before that, '68 or '69. Earlier, I almost became chancellor of Washington University in St. Louis. I spent the whole summer deciding not to do that.

Sharp: When was that?

Revelle: 1961. They offered me the job and they pushed me rather hard. They came out here several times to see me, and I went back there several times. This was a very tough decision to make because the then-chancellor was a man named Ethan Allen Shepley who was a marvelous man. He was a lawyer who was completely convinced about academic freedom and about all the right things in a university in this very conservative community of St. Louis. I stayed at Shepley's house and became good friends with him and with Tom Eliot and his wife. Eliot was the dean of the college.

I finally decided that I just couldn't take the job, and there were two reasons for it. One of them was that they had a very famous and powerful

Revelle: medical school which wasn't really run by the chancellor. So half the place was not under his control, and this was the place that got all the money. It still does pretty much.

The other reason, I didn't realize for a long time that it was a city university; it was not really a university in the Harvard or UC Berkeley sense. It had no "Telegraph and Bancroft." Most of the students were commuters, whereas the essence of Berkeley for the students is that area in the neighborhood of Telegraph and Bancroft outside the campus. Certainly the essence of Harvard for the students is Harvard Square, not the Yard, although the Yard's important too.

So I decided that I didn't want to get involved with that kind of a university. Then I went to Washington instead.

You asked me how I managed to get the job in Washington. Well, it was really no problem. The only problem was I had to be introduced to Stewart Udall. Udall was told, "Here's your science advisor." [laughing] But we got along very well. He was a wonderful guy.

Sharp: I gather that just from what I have read about him, but as far as science was concerned--.

Revelle: He didn't understand science at all; he was a great conservationist, a great environmentalist.

Sharp: Yes, that was really his strength, and especially for the West at that point that was an important attitude.

Revelle: Yes, that's right. He was a Mormon, a jack Mormon. Do you know what a jack Mormon is?

Sharp: No.

Revelle: It's a Mormon who drinks and doesn't pay much attention to the church. His father had been chief justice of Arizona. He came from a very distinguished family in Arizona, a Mormon family, and his brother, of course, is the congressman, Mo Udal. So they were very much involved in Arizona politics for two or three generations.

He amazed me. I admired him tremendously. One of the reasons I admired him was that he had to make political judgments all the time, walk a very

Revelle: narrow tightrope because everybody in the Congress tended to regard the Department of the Interior as a grab bag of goodies.

Sharp: Yes, with all the funding that was available.##

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.

Assistant Professor, Department of History, Bowling Green State University, Bowling Green, Ohio, 1986 - present.