

ORAL HISTORY DEPARTMENT
TEXAS A&M UNIVERSITY

INTERVIEWEE: Roger Revelle

INTERVIEWER: Robert A. Calvert

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La Jolla San Diego, California

RC: I notice that you say in ^{one of your articles} ~~your autobiography~~, Professor Revelle, that you grew up close to the sea and always were attached to and had the mystery of the sea about you. Is that your explanation of how you ended up in oceanography?

RR: Not exactly. ~~I was a graduate student,~~ I majored in geology at Pomona College. Afterwards I spent a year as a teaching assistant in the Geology Department at Pomona. Then I went to Berkeley as a graduate student in geology and a teaching assistant there. And the then-director of the Scripps Institution, a man named Thomas ^{Wayland Vaughan} ~~Waylon Vaughn~~, came up to Berkeley looking for a geologist, a graduate student in geology, to spend a year at La Jolla looking at some mud [^] mud that had been collected from the deep-sea floor by the nonmagnetic yacht "Carnegie."

"Carnegie" had a tragic end. It blew up in the harbor of ^{Apia} ~~Apia~~ in Samoa. The captain and cabin boy were killed and the ship was destroyed, but they had the good sense to send all their samples off at every port they stopped at. And they sent all their bottom samples--as we used to call them--samples of the bottom sediments, back to Washington. A man named John A. Fleming was then Director of the Department of Terrestrial Magnetism ^{of the} ~~at~~ Carnegie Institution, which was the operator of this nonmagnetic yacht.

^{Fleming asked Vaughan}
~~I asked Vaughan~~ if he could, if Scripps Institution could, examine these... ^{* A find} ~~send~~ somebody ^{at} ~~from~~ Scripps ^{who} ~~that~~ could examine these sediments

Vaughan

and later report on them. And ~~Vaughn~~ came to Berkeley looking for somebody to do this. There were two of us who applied for the job, a man named Bill Rand and I. Interestingly enough, both of us ended up involved with the ocean--Bill Rand is the ^{inventor and operator} ~~operator~~ of the various kinds of equipment for the oil companies for underwater oil ~~excursions~~ ^{exploration and drilling}.

~~I got the job~~ and The reason I applied for the job was, basically, because I was about to get married to a ^{young} woman named Ellen Clark, who was a Scripps College student and a niece of Ellen Browning Scripps (more or less the patron saint of La Jolla). ~~She had~~ This house right here ^{where we are talking} ~~which~~ was her mother's ^{(7348 Vista Del Mar).} It didn't look much like this then, but it was in this location and the central part was there. We'd come down here in the summer-time a good deal. It was during our courting period, ~~starting~~ starting in 1927. She was just graduating from Scripps, and we were about to get married, and we thought it would be a good way to spend the first year of our married life here in La Jolla where she was born, where she'd spent her summers. So, I applied for the job and got it.

At that time, Scripps Institution was a very small place; there were only five graduate students. ~~And~~ I believe there were only five faculty members and a total staff (counting the students and the faculty) of about 30 people. ~~And~~ I started looking at ^e these muds and learning how you look at mud. Two of the other graduate students were a man named ~~Richard Fleming~~, Richard Howel Fleming, who has ^{most of} ~~mostly~~ spent the last 25 years, ~~guess~~, at the oceanographic laboratories of the University of Washington, and a man named Maynard Harding. I don't know what became of him. They were both chemists.

One afternoon Dick Fleming came into ~~my lab where I was~~ the lab I was sitting in and said, "Well, you're the new boy here and we're supposed to go to sea. ~~We'll see you tomorrow~~ and take an oceanographic

station." And I said, "Okay." ~~and we~~^{we} got up about two o'clock in the morning. We were all living out there at Scripps Institution; we had a group of little cottages around the laboratory building, which ~~was~~^{were} built in 1916. Dick Fleming lived in one of these and I lived, Ellen and I ^{lived} ~~lived~~²¹⁰⁰ in another; he wasn't married then. He came and picked me up about two, ^{2:30} two-thirty in the morning. We drove down to San Diego to Point Loma where the Scripps Institution ship was tied up. The ship was an ex-purse-seiner called ~~the~~⁶⁵ "Scripps" about sixty-five feet long. It had one professional crew member, a guy named ~~Murdy Ross~~, or Murdock Ross, a Scottish railroad engineer. He'd come down to San Diego from British Columbia. I always used to say his idea of keeping a ship in good shape ~~is~~^{was} to keep it covered with grease like a locomotive. That wasn't quite true, but he was primarily a railroad engineer, not in any way a sailor. But he ran the engine, and ~~then~~ we three were the crew--Harding, Fleming, and I. We started out about ^{3:30} three-thirty in the morning and got off Point Loma about 15 miles to a deep area known as the San Diego Trough. And we started to take a station. Scripps had a program, started by one of the faculty members named Eric Moberg, a chemist, to make weekly observations of the nutrients in the water--the phosphate and the silica and the nitrate⁻ and the oxygen content of the waterⁱⁿ and the temperature and the salinity of the water and to collect plankton samples. And we did this by stopping in one place and taking a series of ^{vertical} ~~vertical~~ samples ^{at} ~~of~~ different depths-- ^a ~~just one~~ sample ^{at} ~~of~~ each of about 15 depths. This used to be called--probably still is called--an oceanographic station.

About 11 o'clock in the morning, the other guys said, "Well, you're the new boy, so it's your job to cook dinner." Well, I went down to the galley, and I...Scripps has always had a policy of feeding very well. We had steak and boiled potatoes, lettuce and tomato salad; ¹ ~~It was~~^a very good, ~~a~~ simple lunch. And the other guys came down and ate this dinner

in absolute silence, not saying a word. They didn't say whether it was good or bad; they just ate it. That's the way ~~that~~ sailors are: they hardly ever say much when they eat. And after about ten minutes, after they'd finished wolfing it down, they got up and said, "Well, it makes us seasick down here, and we'd better go up topside again. You'd better do the dishes." I didn't get seasick so I stayed and did the dishes and cleaned up and went back up, and we finished the station. ^{Then we} ~~we~~ came ~~in~~ back to San Diego; as I remember, we got back just around dusk. It must have been about 7:30, 8:00 in the evening. And this was an absolutely marvelous experience. I'd never really been on a ship before in my life. I'd just gone to the beach in the summertime. ^{real mad, working} ~~We~~ lived in Pasadena, ^(Before that) ~~and~~ when I was a little boy, we lived in ^{Hermosa} ~~Rodando Beach~~) And we'd go down to Balboa in the summertime. [^] But I'd never really been on a ship before, except the old "Congress" that was a passenger steamer that sailed up and down the west coast. This was such a wonderful experience that I decided right then and there I wanted to be an oceanographer. You had this marvelous business of doing science and being a sailor at the same time. So after that, we went out quite regularly on the "Scripps." The "Scripps" was an old purse-seiner--that's ^a ~~an~~ old fishing boat that's used off this coast with a special kind of a net called a purse-seine. We never went very far, but we used to organize cruises to sail in ~~these~~ southern California waters--about 100 miles offshore, about 150 miles north of here, and a little ways to the south--studying the ocean, essentially the water conditions off the California coast. ^P After a couple of years--~~this was 1931~~ about 1933-34, I'd been at sea so much that I actually became the skipper of this little vessel. I took ^{the examination for} a small boat operator's license up at San Pedro, [^] and so I was in charge. By that time, we had a cook as well as an engineer. I was the assistant engineer at first. When we were on these cruises that lasted a week or more, ~~why~~ I would spell the engineer, Murdy Ross, in the engine room. And later we had this cook, Frank...I've forgotten his last name. And I used to insist that we keep

the ship clean ~~the~~ I'd boss the other graduate students, research assistants, around--some were not actually graduate students. ~~And~~ ^{and} they didn't like it very well. I was fairly nautical, fairly much like a skipper should be, I guess. But, anyhow, we all got along. We went on several of these small cruises lasting a week to two weeks, coming into port at San Clemente Island and in Santa Monica, Santa Cruz Island, San ~~Nicas~~ ^{Nicolas Island,} and Santa Rosa Island, Santa Barbara Island, and Catalina, too. We used to anchor at the isthmus off Catalina. These were marvelous things to do. Finally, in 1936, after five years of this, I finally got through looking at the goddamn mud and got my Doctor's degree at Berkeley. Scripps Institution was ~~then~~ ^{an} part of the graduate school at Berkeley. ~~the~~ outlying field stations

In the meantime, I'd done quite a bit of work with Moberg and Fleming on the chemistry of seawater; particularly, Fleming and I did some work on the solubility of calcium carbonates in seawater, one of the earliest papers published on that. There were four of us--Moberg, Greenberg, Revelle, and Allen--who published a paper on the buffer mechanisms of seawater. My contribution to this was to show that boric acid was part of the buffer system of seawater. Not only calcium carbonate, the carbonates and bicarbonates and the cations, like calcium and sodium, but also there was some effect of boric acid; and that changed the relationship between pH and the amount of carbonated water.

And, about 1933, Dr. ~~Vaughan~~ ^{Vaughan} went on a year's sabbatical. He was chairman of a committee of the National Academy of Sciences to look at oceanographic institutions all over the world. I was really lagging way behind with this mud; I was doing all these other things on ocean chemistry. He wrote a letter back to Dr. Moberg, who was ^{Acting} ~~acting~~ director of Scripps then, "We've just got to get this fellow Revelle back on the job of looking

at this mud. Otherwise, maybe we'll have to fire him." This was right in the middle of the Depression, '31-'36. Moberg was very much against that because he thought I was doing okay, even though I wasn't looking at the mud. ~~I was doing okay.~~ At one time during this period, the University of California was very short of money, and they were thinking that they would have to fire all the research assistants down there. In order to keep the research assistants on, I believe the faculty actually took a voluntary reduction in salary. Another couple and Ellen and I were trying to prepare ourselves in case we did get laid off. Her mother had a lemon grove up near Riverside, and we went out to look at that and see if we could live there and grow lemons in that lemon grove. It was not a very promising prospect. Anyhow, they didn't lay us off. So we stayed on, and I finally got my thesis written on the marine bottom samples collected by the yacht "Carnegie." I got my Ph.D. in the spring of 1936 at Berkeley. That was when the Scripps Institution was part of the graduate division of the University of California at Berkeley. UCLA didn't have a graduate division then, just undergraduate. It was clear that I was quite determined to stay on, so they promoted me to instructor. My salary went from \$1200 a year to \$1800 a year, and I was an instructor until the war came along. I went on active duty in the Navy.

RC: That's in 1941, though. Between '36 and '41, what sort of projects did you work on at Scripps?

RR: Well, the first year after... '36-'37, our whole family went to Norway where I had what would now be called a post-doctoral fellowship, although we paid for it, at the Geophysical Institute in Bergen. Dr. ^{Vaughan} ~~Vaughan~~ retired in 1936, the summer that I got my Ph.D. His place was taken by ^{Harold} ~~Harold~~ Sverdrup, who was a great Norwegian oceanographer, probably the best oceanographer of his time, ~~physical oceanographer~~. Norwegians were leaders in physical oceanography for about 50 years--40 years, at least--because

physical

Villem Bjerknes

of the influence of a man named ~~Wilhelm Bjerknes~~ (S), a theoretical physicist whose specialty was hydrodynamics. He invented--~~well~~, he really applied hydrodynamics to ~~the~~ planetary circulations of both the ocean and the atmosphere. And there was a whole group of Scandinavians at that time: ~~Walfried Ekman~~, who was the inventor of the so-called ~~Ekman~~ Ekman Spiral, which is the theoretical way that a wind-driven current turns to the right in the Northern Hemisphere as you go deeper in a spiral; ~~Bjorn Hella~~ and ~~Hansen~~, who was essentially a descriptive physical oceanographer. He was particularly concerned with the accuracy and variability of observations. He'd been a colleague of ~~Frijoff~~ Nansen, the explorer and world citizen who invented what we call the Nansen bottle, a bottle for sampling the water at different depths, ~~an~~ ingenious device for closing the bottle by a weight that you dropped on the cable when you wanted to close it. ~~Hansen~~ ~~Hella~~ used these Nansen bottles ~~in~~ reversing thermometers to make very accurate measurements of temperature, and ~~the~~ water samples that they collected...they could make very accurate measurements of salinity. From these, it was possible to compute the surfaces of equal density in the ocean and the surfaces of equal pressure, or the pressure differential.

AP And those people in Scandinavia, particularly Bjerknes (S) and his followers, developed the theory of pressure-driven circulation in the ocean in which the pressure gradient was balanced against the Coriolis force that...not really a force, but because of the earth's rotation, currents turn parallel to the pressure gradient instead of at right angles to it, or nearly parallel. The real balancing force, of course, is the force of friction, but that's--the only way you can get that to balance is to have the current running very close to parallel to the ~~surfaces~~ contours of equal pressure--running along the hill instead of down the hill. This theory of pressure-driven currents was an equilibrium theory; it didn't allow for what we call convergence or divergence. You had to have continuity at every

plane, every horizontal plane, where what actually happens is that in some places the water converges and sinks ~~or rises~~ ^{diverges and}. In other places, you have a divergence. ~~Actually~~. If you have convergence, the water sinks; if you have divergence, the water rises and spreads out. And this theory didn't account for that. Ekman's theory, on the other hand, was based on a balance between Coriolis force and the stress of wind, the friction of the wind on the sea surface. And, again, essentially it was a second-order effect--the viscous friction in the water, so-called turbulent friction. ^{that gave the same balance of forces}

And there was a man named Otto Peterson in Sweden who ~~had done~~ ^{made} a great ~~deal of~~ ^{many} oceanographic observation. A great Dane, ~~a man named Martin Knudsen~~ ^{Knudsen}, worked out the method of measuring salinity very accurately and the tables, the so-called ~~Knudsen's~~ ^{Knudsen's} Table, that enable you to translate temperature and salinity into density. Ekman was involved in that, too. And at the same time, there were a lot of great meteorologists there at Bergen:

Jack Bjerkness, ^{Villem} ~~Wilhelm~~ Bjerkness' son; and ~~a man named~~ ^{Sverre} ~~Sverre~~ Peterson; Carl Gustav Rossby, who was a Swede who had come to the United States but was really part of this Scandinavian school, although he spent quite a bit of time at MIT. So, during the early part of this century and up until after World War II, the principal leaders in this kind of physical oceanography, the study of the currents, or what Sverdrup used to call "the motions of the oceans," were ~~all~~ ^{mostly} Scandinavians, ~~mostly~~ ^{mainly}

Norwegians, but some Swedes and some Danes, ~~and some Germans~~. And there was also a great Austrian named Albert Defant, a German named ~~Merits~~ ^{Mertz}, and another one named ~~Geuse~~ ^{Wüst} ~~Geuse~~. ^{Wüst} Geuse was primarily a descriptive oceanographer.

All these people attended a meeting of the International Union of Geodesy and Geophysics ~~in~~ ⁱⁿ Edinburgh in 1936. I met them for the first time there, except for Sverdrup, who ~~would~~ ^{had already} come out here with his wife and daughter. I met him out here just briefly before we left. And it turned out, actually, I would have done better just to stay here and work with

him, in terms of research and learning.

But we had a wonderful time in Norway, the first time either of us had ever been outside the United States. We had our two small daughters with us. One of them was ~~about four years old, and one was~~, oh, about three and one-half, and one was six months, when we went there. And, of course, a year later when we came back, our oldest daughter ~~got so~~ ^{had gotten so} she talked English with a strong Norwegian accent. Our youngest daughter was quite ill, ^{when we first started} had diarrrea, and she just got thinner and thinner and punier and punier, and we.... I remember, one night we were very worried about her, and I got on a streetcar. We lived at a place called Hope just outside of Bergen, next to another little suburb called Paradise. I got on the streetcar and went into town to see a woman doctor named Johanna Scram Anderson. I was scared to death that Mary was really sick. And she gave me some fruit juice called "saft," and she said, "Give the child some of this several times a day." We did, and she got well. Those were before the days of antibiotics, or even Sulfa drugs, I guess; I'm not quite sure about ~~it~~ ^{the latter.}

Living in Bergen

~~One of the people we became very good friends with was~~ One of the couples we became very good friends with were ^{Jack} ~~Vak~~ and Hedvig ^{Bjerknes} ~~Certness~~. ^{Jack Bjerknes} ~~Vak Certness~~ was the inventor of the theory of air mass analysis. During World War I, he had had the job of running what they call the ^{Vaer Varsling} ~~Vaer Barsing~~ ^{på Vestlandet - the} ~~and Paul Bestline~~ ^{service} weather forecasting for western Norway. Of course, there were very few ship reports. The radios were all silent. He had, somehow, to make forecasts for the fishing vessels, and he developed ^{the} ~~this~~ theory of fronts between air masses. Air masses ^{have different} physical characteristics... ~~one air~~ mass slightly ^{is one} cold front, in which ^a the cold air ^{slides} slide under the warm air, and ^{is one} the warm front, in which ^a the warm air ^{slides} slide up over the cold air. And I learned something quite fundamental from him; and that was that a good scientist really lets ⁿ Nature take the lead; ^{Jack} ~~that is,~~ he would never try to force the weather, never try to have a theory about it or a predilection

about it. He'd just lie back and let Nature tell him what was going to happen. And this was remarkable. It's said to rain 32 days out of every month in Bergen. It does rain ^{almost} every day. ^{He} You could look at the weather map and say, "It's going to stop raining at 12:15, and we can go for a picnic. We have to be back by 3:15, ^{because} ~~cause~~ the rain's going to start again." and he was always right within 15 minutes, both ends--really just incredible.

Another man was ~~a man named Svera~~ ^{Sverre} Peterson, who later came to this country and became a professor of meteorology at MIT and ^President of the American Meteorological Society. He was a ~~good~~ first-rate meteorologist but ~~not really out of~~ ^{didn't have} the great intuition that ~~Certness~~ ^{Bjerknes} had. Another man was an engineer named ^{odd} ~~og~~ Dahl. He'd been with Sverdrup on the ^{This expedition} "Maud" Expedition. ~~The "Maud"~~ was the thing that really shaped Sverdrup's life. The Norwegians, ever since the 1890's, when Nansen ~~tried to~~, succeeded in ~~freezing~~ his ship, the "Fram," in the polar ice, had the idea that you could drift across the North Pole if you got in the right place in the Arctic ^{Ice Pack} Ocean in a ship or something to live on. The other great Norwegian explorer, ~~Amundson~~ ^{Roual} Amundson, had a ship built called the "Maud"; and they tried this stunt again in ^{1918 or 1919,} ~~the 1920's~~, just about the end of World War I, going up to about 1923 or '24. They actually tried this twice, and all together Sverdrup spent seven years frozen in the Arctic. This was a profound experience. There were only about five or six of them on the "Maud," a very, very small crew ^{X A,} and being Norwegians, they managed to get along together for this ungodly long time--what seems to me to be a terrible, ~~terrible~~ situation. They did this by ^{being} ~~having been~~ very rigorously disciplined. Sverdrup went off one winter and spent ^{most of a year} ~~a winter~~ with the Chukchi, the Eskimo-like people who lived in Siberia, and wrote a book about it called ^{4 Hos} ~~Host~~ ^{Folk or "Witk"} ~~Chukehi~~ ^u ~~with the Chukchi People,~~ ~~Most of the~~ ^{when he was on the ship,} ~~time he was~~ he had a very full program of observations; all kinds of geophysical phenomena. He spent years and years writing up the results. He

was ~~all~~ over this by the time he came to Scripps, but he was then working on the results of the "Carnegie," ^{the expedition on the} ~~of this~~ nonmagnetic yacht that I was telling you about. And he really brought science, physical science, to the Scripps Institution. ^{P. Vaughan} ~~Vaughan~~, his predecessor, was a paleontologist, interest ^{ed} in corals. He was, I guess, the leading authority on taxonomy of fossil corals and also ^{on} in the taxonomy of the larger foraminifera, these big things like the ^{Murchisonites} ~~moonlights~~ that you find in ^{Egypt} ~~the~~ rocks of the Egyptian pyramids. And the way he got into oceanography was by deciding that, in order to understand dead corals, he had to understand living corals, the ecology of living corals. He made a famous series of experiments in which he took little pieces of coral and stuck them to ~~something like~~ a board or something like that and hung them in the water, tropical water, and watched how fast they grew. ~~I've heard~~ They grow surprisingly fast, about one centimeter a year, or something like that. And he always talked about the interrelations between marine organisms and their environment. That's what he thought ^{was} one of the jobs that oceanographers had: to find and study those things; that's something we now call "ecology." He wasn't very interested in physiology or biochemistry ^{X#} he didn't understand biochemistry, and he didn't really think of marine organisms as most biologists do--as simply instruments for studying biology. He thought that oceanography should be concerned with, as I've said, the ecology of marine organisms. In this respect, he was very much like his predecessor, William E. Ritter, the first director of the Scripps Institution.

^{Ritter} ~~Ridder~~ was a professor of zoology at Berkeley, and he founded something here in La Jolla called the Scripps Institution for Biological Research. And if you go down, that ^{grade} ~~grave~~ down by the Scripps Institution is still called the biological ^{grade} ~~grave~~. ^{At first, I} ~~I~~ have often wondered why it was called that, but ^X then ~~I~~ ^X of course, found much later that it was simply because that's

where the Biological was. And Ritter was...had exactly the same notion
 that ^{Vaughan} ~~Vaughan~~ had--that you ^{have} ~~had~~ to study whole organisms. He wrote a famous
 book called The California Woodpecker and I which argued his point of view.
 At that time, biology was going in just the opposite direction; it was going
 down from the whole organism to organs and to cells and to molecules.
 And that's been the history of biology, really, for the last ⁶⁰ ~~70~~ years or so,
~~60~~ years, since about ^{50/1915} ~~1915~~. And this, of course, led to a revolution ⁱⁿ ~~of~~
 biology. But we're just now coming back to Ritter's idea that we have to,
 that one of the important things that we need to learn about (very much
 more difficult subject than the study of organs or cells or molecules) is
 the study of the whole creature, the whole animal or the whole plant, ~~you~~
~~know~~, in relation to ^{the} other plants and other animals, which ^{are} ~~is~~ a major part
 of ^{its} ~~their~~ environment. Ecology is a very important subject which is
 worked on by a completely inadequate group of people. ^{There are not} ~~Perhaps there's not~~
 enough competent people in ^{the} ~~that~~ field. It was because of Ritter's disgust,
 really, or his vigorous opposition to the reductionists, that the Scripps
 Institution changed its name in 1924 from Scripps Institution for Biolo-
 gical Research to the Scripps Institution for Oceanography. Well, ~~it became~~
~~part of it.~~ ^{Ritter} ~~He~~ started ^{in the 1890's} ~~it~~ ~~by~~ every summer for about seven or eight years,
^{he} ~~establishing~~ ^{ed} a biologist's ¹ summer camp somewhere along the California coast.
 In those days, every university and every college tried to have a marine
 biological camp or station because there's such a wonderful variety and
 diversity among ^{marine} ~~the~~ organisms. And he had a summer camp in various places.
 He ended up in Coronado and interested ~~in~~ a man named Fred Baker here in
 San Diego ~~and~~ ⁱⁿ his work. And Baker persuaded him to stay and settle here
 and have a permanent station, or, at least, to come here year after year in
 the summer time. And the last one they had, the last one of these summer
 stations they had, was in the cove just north of the Valencia Hotel here in
 La Jolla. ^P And then Ritter became a very good friend of E. W. Scripps, ^{the}

newspaperman. Scripps was a monologist; he talked all the time. And Ritter was one of the few people ^{who} ~~that~~ could talk back to him, and Scripps was fascinated by this. He said, "I'm not endowing an institution. I want to endow a man named Ritter." Scripps managed to con the city of San Diego into giving, or ~~to~~ selling, him a pueblo lot. San Diego's an old Spanish pueblo, and it has a great deal of land which ^{is called} ~~we call~~ the pueblo lands of the city ^{of San Diego.} And they divided this up into lots of about 180 acres. La Jolla's on ~~several~~, two or three ~~x~~ of these lots; and there were 180 acres where the Scripps Institution is now which Scripps was able to buy from the city for \$1,000. ^{It was much} ~~It's~~ worth more than \$1,000 even then, of course, but he got it very cheaply in order to establish and build a permanent ^{place} ~~building~~ there for the Scripps Institution. The first building ~~there~~ was the so-called Scripps Building, which is still there, built in the early 1900's. ^{In} ~~Since~~ 1912, the Institution became part of the University of California as the Scripps Institution for Biological Research. Scripps and his sister, Ellen Browning Scripps, all together gave them an endowment worth about \$500,000, in those days, of stock. Most of the stock was in ^{or more properly the company which} ~~The Detroit News, stock in what was then~~ The Detroit News, called Association ^{of} ~~The Evening News~~. And they still have that stock. And ^{the dividends} ~~that~~ provided, or used to provide, the salary of the director but, of course, a very small part of the total budget now. ^{Then in 1924} ~~So~~ the Scripps Institution became the Scripps Institution of Oceanography. The name was changed basically because of Ritter's ← feeling that biologists were going in just the wrong direction. And it became ~~then~~ the first oceanographic institution in this country, one of the first in the world--not the first in the world, but one of the early ones.

~~The~~ Woods Hole Oceanographic Institution, which was the next major institution in this country, was started about 1931, about seven years after Scripps became ^{an} ~~the~~ oceanographic institution. Although Woods Hole ⁱⁿ ~~had had~~ ^{there had been} ~~had~~, of course, for many years, ^{at} ~~something called~~ Marine Biological ^{Laboratory} ~~Laboratories~~, it was a summer place for biologists. ^{It's} ~~That's~~ still an independent entity ⁱⁿ ~~of~~ Woods Hole. And they had also had the Bureau of Fisheries Laboratory at

owns and publishes

Woods Hole. So, they have a very long tradition of studying the ocean.

AP We had a man here, ~~named~~ Francis B. Sumner, who was brought here by Ritter because he had done some very interesting early work on the color ^{adaptations} ~~adaptions~~ of fish to their environment: fish ~~that change color when their~~ like the halibut and other flat fish that change color or change the patterns ^{on} and their scales ^{to match} for different environments. And Sumner became very much interested in the ^{Lamarckian} ~~Lamarckian~~ idea ^{of} proving or ^{disproving} ~~disapproving~~ the ^{Lamarckian} ~~Lamarckian~~ idea, that acquired characteristics are inherited. ^H We tested this out with mice; ^{he} ~~we~~ had thousands of mice. ^{And} There was a mouse house up on top of the hill just overlooking the present campus. Then, ^{Vaughan} when ~~Vaughan~~ came, he made him... ^{give up his mice.} Sumner always described it that the institution went wet. ^{And} ~~And~~ ^{Vaughan} ~~Vaughan~~ thought this work on mice was not really appropriate for an oceanographic institution, so he made Sumner ~~really~~ stop it after a few years. And Sumner went back to his fish and continued to do very interesting things ^{the adaptation} on ~~adaptions~~ of fish to temperature and other environmental factors--really, just the kind of ecology that ^{Vaughan} ~~Vaughan~~ thought ought to be done. ^{But he was} ~~He's~~ always quite bitter about having to give up ^{his} ~~these~~ experiments on mice. He was a contemporary of Morgan, the ~~great~~ Drosophila man. Morgan won the Nobel Prize basically because he chose the right experimental animal, the Drosophila, which reproduce every few days or every few weeks, I guess it is, where the mice take months. So, ^{Morgan} ~~Morgan~~ could get a great many more generations within an experiment than Sumner could. It's an interesting example of how much of a role luck plays in things of this kind, or, perhaps, insight or intuition. Sumner became a member of the National Academy of Sciences, but long after Morgan won his Nobel Prize. Well, I'm sorry I digressed to the early part of the institution.

RC: That's important.

RR: But coming back to what I did, or what we did.... After I became an

instructor and after I came back from Norway, a man named Francis ~~Shepherd~~^{He} appeared here in 1937. I ~~guess~~^{guess} he'd actually been here before that, even while I was a graduate student. He was very much interested in submarine canyons, and he ^{had gotten} got a grant from the Geological Society of America for \$12,000, I think it was, ^{to study} and studied the marine geology off the southern California coast. By that time, we had a new ship. The old "Scripps," the one I told you about, had blown up in a harbor in San Diego. (This seems to happen to oceanographic ships.) It had an explosion which killed the cook, ^{Frank,} Phillip Franco, and very seriously injured Murdy Ross--he never really recovered. This was while Ellen and I were in Norway. And so, ^{Harold} Harold Sverdrup managed to persuade Robert P. Scripps, the son of ^E P.W., who was then the editor and publisher of the Scripps-Howard papers, to buy him a yacht. And he bought Louis Stone's yacht, called the "Serrano," and we renamed it ^E P.W. Scripps. This was a famous early oceanographic boat. It was ^{originally} a topsail schooner, a Gloucester schooner. ^{We} They cut off the top masts but retained the other, the regular masts. And for years, we sailed this ship, as well as drove it underpower. Gradually the masts got shorter and the engines got bigger; and, after World War II, we gave up sailing altogether--it was just really a power boat. This is in contrast to Woods Hole, which had a ^{ketch} ~~yawl~~ named the "Atlantis," ^{of} built about 1932, ~~in~~ which Columbus ^{Iselin} ~~Iselin~~, the famous director of Woods Hole, was the first captain. She remained a sailing vessel all during her career, but we gradually got so that we used ^a the sails only to steady the ship, ^a the trisail to steady the ship. By that time, we had a professional crew; we had a captain and several members of the crew, in addition to the graduate students, who still stood regular sailors' watches ^{in the wheel house and on deck.} ~~in the engine room.~~ ~~And~~ We had a couple of ^{engineers} ~~officers~~ on the ship ^{and a} cook, all the people like that were ~~all~~ professional sailors. ^{Shepard} ~~Shepherd~~ had brought out with him two graduate students, Bob ^{Dietz} ~~Beets~~

Bob Dietz

and Kenneth Emery, who is now a member of the National Academy. ~~The other~~
 is a famous oceanographer and marine geologist with NOAA, the National Ocean
 and Atmospheric Administration. He's one of the primary authors of The Theory
of Seafloor Spreading, ~~Bob Beets~~. And ~~then~~ he also brought with him two
 guys from UCLA [↑] whose names I can't remember [↑] who had an idea about building
 a coring machine, a device for taking cores. (~~I'll think of their names,~~
^{Varney and Redwine} ~~but I can't think of them now.~~) They were basically engineers, with very
 expensive tastes ^X they spent a lot of money on this coring machine. The
 theory of it was that you would open a valve, and the water would rush into
 a chamber. In the process, it.... I'm sorry, that's not quite right. You
 plunged ~~it~~, ^{a core barrel into the bottom. There} ~~this~~ ^{etc.} ~~was~~ ^{barrel} a cylinder on top of a core ~~drill~~, and there was a
 valve between the core barrel and the cylinder. You ^{opened} ~~open~~ that valve, and
 then the barrel would sink into the mud because of the suction created by
 this empty cylinder. Essentially, mud would be sucked up to fill the
 vacuum. The trouble with this device was that the forces were too great.
 Something always broke. We spent thousands of dollars on it; it never really
 worked very well. It was about that time that ~~Glunberg~~ ^{Kullenberg} (?) was developing
 his corer which really operated on a very similar principle, and also when
 Charles Piggot was developing his gun, the Piggot gun. The Piggot gun, in
 fact, really worked pretty well. ^{And} ~~of course~~, the ^{Kullenberg} ~~Glunberg~~ corer worked
 very well, indeed. ^{Kullenberg} ~~Glunberg~~ corer really came in after World War II.
 Anyhow, one of the major things we did was to study the submarine geology
 off this coast and the sediments and their distribution. ^P Then, in 1938,
 '39, I guess, we had an expedition to the Gulf of California ^X that was the
 longest, the farthest away from home ^a ~~the~~ Scripps Institution vessel had
 ever been. And we did all sorts of things in the Gulf. This was a--what the
 Russians now call... ~~what do they call it now?~~ ^{a complex} ~~essentially,~~ a mixed oceanographic
 expedition doing biology and geology ~~and~~ physical oceanography, and

chemical oceanography, all at the same time.

RC: Was that very unusual? The early oceanographic ^{expeditions} ~~institutions~~ didn't do that?

RR: Yes, they did, ~~because~~ that was the way ~~that~~ all these big expeditions had operated. ~~the~~ "Meteor" Expedition, the "Challenger" Expedition, and the... well, there were half a dozen others in the very early days. The ^{Schmies} ~~Schmies~~ Expedition... all of these big expeditions did everything at once, basically because the ships were so expensive. The "Carnegie" did the same thing: chemistry and physics and biology and geology. That was what we did on our first expedition to the Gulf of California. This was a remarkable ^{area.} ~~place.~~ In the first place, it was very wild, very few people there. One ^{island,} ~~island,~~ ^{Tiberon} ~~Tiberon~~ Island, still had Indians on it living pretty much as they had lived thousands of years ago. ^A very, very primitive Indians. There were a few settlements, ^a ~~the~~ settlement at La Paz, ^a ~~the~~ settlement at Loreto, half a dozen settlements altogether. ^A small relics of the missions, or, in one case, a famous ~~mining camp,~~ copper mining camp. But the most interesting thing about the ^g Gulf was the water. While the shore was a desert, the water ^{seemed} ~~just seemed~~ with life. And this was because of the very peculiar circulation. It had been called by Cortez the Mare ^{Vermeo} ~~Vermill~~, the Vermillion Sea. The reason ^{he} ~~it~~ was called the Vermillion Sea was that it looked like tomato soup in the spring time. It had an ^{enormous} ~~enormous~~ plankton ^{bloom} ~~boom~~ every spring. ^{XA} And literally, if you went out in a small boat at night and dipped your oars in the water, the whole sea would light up for hundred of yards around the ship, light from luminescent organisms. The reason for this was that the surface water of the ^g Gulf was blown out of the ^a Gulf by northerly winds, and deep water came in to take its place. The ^{incoming} ~~deep~~ water was rich in nutrients, and the ^{outgoing} ~~surface~~ water was poor in nutrients. So, the nutrient concentration was extremely high. It came right up to the surface where the plankton organisms lived. The bottom deposits, as a result, were literally almost

nothing but diatoms, diatoms and ~~other~~ skeletons of other plankton organisms. And these were layered in bars--layer after layer on the bottom. ~~In other words,~~ It was anaerobic: there was so much organic matter settling to the bottom that ^{the} oxygen was all used up. So, you got beautiful, layered sediments. ~~And,~~ [^] of course, the chemistry of the water was fascinating. ~~The~~ [^] geology was even more fascinating because--we didn't know it then, but--this is the ^{boundary between} ~~order of the~~ two tectonic plates. And you got ~~these~~ very complicated trenches and ridges and troughs and other zones of extreme tectonic activity because it's right on the edge ^{between} of the Pacific plate and the North American plate. It's a continuation of the ~~South Pacific,~~ the Easter Island Rise ^{in the South Pacific} and the San Andreas Rift in California itself. It's a place very much like ^{it extends into} the rift valley in Africa and the Red Sea, where the earth is pulling apart. We didn't, of course, know it then; but we were fascinated by the very complicated basin and range structure of the place. The basins each had ~~these~~ anaerobic conditions in them. And so we went back again. We got a ~~another~~ grant from the Geological Society of America, ~~another grant,~~ and went back again to the ^gulf. And that time ^{it} was a purely geological expedition. Ken Emery was on it, ~~and~~ Charlie Anderson ^{of the U.S.} Geological Survey, ^{Fran Shepard,} Frank ~~Shepherd,~~ I, and several younger people. Quite a few publications came out of this on the geology of, ~~the~~ not only the marine, but the land geology of ~~lower Cali-~~ ^{the gulf of California,} ~~fornia,~~ The whole thing was really exploration of a very interesting kind. ^{PP} About that time, the war started in Europe ~~in~~ 1939. ~~It started the summer~~ ^{In} of the fall of ^{that} the year that ⁽¹⁹⁾ 1939, there was another meeting of the International Union of Geodesy and Geophysics in Washington. That was a very sad meeting because of the war, because the war had started. And all these people from Europe that I told you about before were there at that meeting, ~~too.~~ ^{(Many of them we} never saw again.) And we here at Scripps were getting interested in underwater sound. There was a Naval officer

names ^d Rawson Bennett ~~z~~ who was the operations officer, or technical officer, of a division of destroyers ~~here~~, which were testing sonar here. The skipper of this division was a man named Burhans, who later became an admiral--they both became admirals. They were ~~really testing~~, trying to understand what happened in the ocean to affect the performance of sonar gear. We used to call it underwater sound gear in those days. Rawson Bennett came out to Scripps and talked to Sverdrup and Dick Fleming and me. And we developed a theory of the propagation of sound in seawater, the ray theory [^] a very simple theory, essentially ray optics. At the same time, Ewing and ^{Iselin} Iselin were doing ~~it~~ ^{the same thing} at Woods Hole, which was quite unknown to us. Later, ⁱⁿ the summer of 1941, I went into the Navy.

RC: Did the Navy specifically contact you about trying to discover what happened to sound in the ocean?

RR: Yes, Rawson Bennett came out to Scripps, talked to Sverdrup and Fleming and me.

RC: And how long ^{dy} did you work on that?

RR: Oh, quite a while.

RC: Did that carry on to when you began to work in the Navy [^] in '41?

RR: Yes. We didn't start until about 1940.

RC: You said one time that, "The 1950's and '60's will be recorded as one of the great ages of exploration." for an exact quote. But it seems to me ~~^~~ from your discussion here, the '30's must have been a great age of exploration. What seems to you to be the difference in the two?

RR: Why, the scale.

RC: The scale. What major changes in techniques would you suggest occurred between the '50's and '60's, and the '30's?

RR: The introduction of electronics into oceanography. We used to have a saying in the 1930's ^{and} early 1940's, ^{that} we wanted less than one vacuum tube per instrument. The great difference was really the development of instrumentation in World War II. In the 1950's and '60's, we were exploiting those great

advances, not so much in actual instruments as in how to make instruments.

RC: And you think that the development of instrumentation came out of the work in the military services?

RR: Pretty directly, yes.

RC: I'm familiar with the work that came out of the Air Force School of Meteorology that developed between '42 and '45 and produced a good number of well-known oceanographers. What about the U.S. Navy? What would you suggest to be their major developments between '41 and '45?

RR: In what?

RC: In terms of instrumentation.

RR: Well, the main thing was the development of underwater sound of all kinds, both echo sounders and horizontal ranging devices, and the great interest that developed in the Navy in the propagation of sound in the ocean. But there were also developments in what we call magnetometers, that is, methods of measuring various magnetic phenomena on the bottom and in the water by the so-called towed-magnetometer, which really came out of the work on magnetic mines.

RC: Weren't you in the Radio and Sound Lab in San Diego in '41 and '42?

RR: Yes.

RC: What, exactly, was your work there?

RR: I was in a "sailor suit." I forgot to tell you that, in 1934, I went on a cruise on the USS Bushnell, which ^{was} ~~is~~ the flagship of the submarine force. There was just one submarine force ^{for both} ~~in~~ the Atlantic and the Pacific. And the Bushnell went up to Alaska, about half way out on the Aleutian Islands, and then south to Hawaii. I ran a line of oceanographic stations between the Aleutian Islands and the Hawaiian Islands, one of the first sections across that part of the...really, the first section across that part of the Pacific. This was primarily physical oceanography, as we used to call it, and ~~it~~

chemical oceanography--that is, measurements of the silica, the oxygen and nitrate, the phosphate, the temperature and salinity of the water. And it was those data I went to Norway with in 1936-37.

And as a result of this experience on the "Bushnell". The captain of the ship was a man named ^{Abel T.} ~~Abel P.~~ Bidwell--I remember his name. I also remember the name of the admiral, Cyrus W. Cole, ^{and the Flag Lieutenant, Leo Bachman.} This was a great experience in my life, ^{the} ~~this~~ summer on this Navy Ship. There were various other people onboard; ~~one was named Leo Bachman~~ ^{whom} I remember quite well. ^{But} I don't remember ^{their} the names, ~~of the others.~~ ^{But,} anyhow, Bidwell persuaded me to try for a Naval Reserve commission, which I did; and I got that in 1936. So, in 1941, when ^{the} ~~this~~ Navy Radio and Sound Lab got started here in San Diego and the University of California Division of War Research was established there, I went on active duty and became a project officer for various kinds of things, whereas Sverdrup and Fleming and Munk and the others ^{remained as} ~~were just~~ civilians in the University of California Division of War Research. We had a skipper of the Navy Radio and Sound Laboratory, ~~who was~~ a martinet named Red Ruble. He was a real "sundowner." This means various things in the Navy; but, in his case, it means he was a complete son-of-a-bitch.

RC: That's what it meant in the Army, too.

RR: He was an impossible man, and I was on his staff. He would send me out with a group of chief petty officers and one or two scientists on various kinds of field expeditions. For example, in San Francisco Bay we looked at the oceanographic conditions ⁱⁿ ~~of~~ the mouth of the Golden Gate, and then we did the same thing up at ^{Neah} ~~Nea~~ Bay ⁱⁿ ~~at~~ the Straits of ^{Juan de Fuca.} ~~San Juan de Fuca.~~ Two of the scientists on those expeditions were Waldo ^{Lyon} ~~Ryan~~--who has since become ^a ~~the~~ great expert on Arctic oceanography; ^h He was a physicist, young physicist from UCLA--and a man named ^{Jeffrey} ~~Geoffrey~~ Holt er, ^{and is} ~~an~~ idea man. He was a graduate student in physics, ^{at that time.} ~~too.~~ Idea men are a very peculiar breed, as

you probably know. They have lots of ideas, and most of them are not very good, but some of them are very good. He was constantly interested in original experiments of various kinds. Anyhow, we worked together on these surveys. Every time I came back ^{from} with a survey, I found I had a different desk and a different job. I got involved with radar propagation. I started a radar operator's school at the ~~the~~ ^{R S L} radio and sound laboratory, which later became the CIC School, which still exists down here in San Diego--a great big establishment now. But it started with six scared, stupid enlisted men who were sent by their skippers because they didn't know what else to do with them. They were just awful. We decided that we just couldn't teach guys like this to operate radar. We'd have to have standards. So, we insisted after that that we would only take people who could pass a certain score on the Navy's intelligence test, which I've forgotten the name of. It wasn't called an intelligence test, but that's really what it was. After that, they got better, and this thing really developed. After a while, it developed into a great big establishment. But this was, of course, a very dull job--teaching radar operators. You teach the same thing over and over again.

The one saving grace of it was that we would range on San Clemente Island, about ^{seventy} ~~seven~~ miles west of San Diego ~~and~~ ^S. Sometimes you could see it, and sometimes you couldn't. Why was this? Well, it turned out, ~~of course~~, that the reason was that the radar beam was refracted by the structure of the atmosphere. Sometimes it was bent down, and sometimes it wasn't bent ^{down}. There were two good physicists ^{who} ~~that~~ got involved with this. One was ~~named~~ Frank Abbott, and ^{the other was John Smythe} ~~I can't think of the other one. (I know him very well.)~~

Of course, once they got into it, I had to fade away because I just didn't know enough ^{about electro} ~~like the~~ magnetic theory. That was one of the problems of being a geologist: you never really knew much about anything. But the first ^{was} paper on radar propagation written out here...I was one of the authors of it.

And this, of course, has now become a science all by itself. I ~~really~~ got out of the sound propagation study by ~~getting~~ ^{underwater} involved with ^{also I became} this radar school. ^{and with radar propagation} ~~this~~ ^{etc}

My friend Rawson Bennett, by this time, had gone to Washington as the head of the design branch of the electronics division of the Bureau of Ships. I got in touch with him and said, "I just gotta get out of here. This is impossible." So he arranged for me to come to Washington to be assigned to the Hydrographic Office. This was in the winter of 1942. I went out to the Hydrographic Office. The hydrographer was a nice old admiral named Bryan but ^{he} didn't ~~really~~ have any idea about World War II or what in the world an oceanographer could do ^{in the war.} He put me to work looking at a ^{submarine} bank that had been found somewhere off the coast of Panama. This bank was very interesting because it didn't exist. Some ships had picked it up on their fathometers, on their echo sounders; ^o other ships passing over the same spot couldn't find it. Clearly ^{the people} what it was, was the deep scattering ^{layer} ~~there~~, but ^{guys} out here were just finding out about the deep scattering layer at the time.

RC: Yes, they were working out here in the Office of Naval Research on it.

RR: No, that was the University of California Division of War Research, Russell Raitt and Martin Johnson, particularly. They did two great things: one was to find the deep scattering layer; the other was for Martin Johnson to solve the problem of crackle, the background noise that got into the sonar gear, which affected also acoustic mines. ~~It turned out the way Martin did this was to ask...well,~~ In fact, Martin was responsible for both these discoveries. In the case of the deep scattering layer, he asked the following question, "What happens to it at night?" and it turned out that at night it didn't exist, that the scattering was just below the surface, not at depths of 100 and 200 meters. ^{Martin thought} It must be due to organisms because that's the way marine organisms behave--they go down in the daytime and up at night. That, of course,

~~is what~~ turned out to be right. In the case of the crackle, he said, "Where does it exist?" It didn't exist everywhere; it only existed over certain kinds of bottoms. He guessed that ~~these~~ ^{the noise due to} must be an organism called a snapping shrimp which lives in rocky bottoms ^{and} sandy bottoms, not in muddy bottoms. He got some of these shrimp...he got one shrimp and put it in a little jar and put a microphone in the jar--called a hydrophone ^{because it} ~~and~~ works underwater--and the darn thing was going tchk, tchk. You put a hundred of them in ~~it~~ ^{There was a} and ~~that's a~~ continuous popping noise like bacon frying. And, clearly, that was what ~~it~~ ^{the crackle} was. So then, these were two biological discoveries of very considerable interest. But, anyhow, ~~that was~~ what Bryan wanted me to do was look at this non-existent bank. I didn't think that was a very military thing to do. So, I went back to Rawson Bennett, and he said, "Well, we'll give you additional duty in the Bureau of Ships—in the underwater sound section of the Bureau of Ships."—(the underwater sound section of the design branch, of the electronics division, of the Bureau of Ships.) And I became Code 940C or 940D, I guess it was. My boss was a man named Jacob Meyers, a wonderful guy about my age. He was a commander, and I started out as a lieutenant ^{and} actually became a commander myself. We got along very well, and I became the Bureau of Ships' oceanographer. I worked with all kinds of things that went on. I ~~really~~ became ^P Project Officer for work that was being done at Woods Hole and at Scripps and the oceanographic work in ^{the} Radio and ^{Laboratory # by} Sound ^{at} the University of California Division of War Research and the related work that was being done at ^{New} London--the underwater ^{Laboratory} ~~place~~ that was called USNUSL, U.S. Navy Underwater Sound Laboratory.

RC: Was Sir ^{Edward} Bullard involved in that?

RR: No. He was in England.

RC: I mean at the one in England, in London.

RR: He was involved with many things, but ~~the two~~ ^{one of the} principal things that he did

during the war were ^{to} ~~the~~ work on magnetic mines and then on acoustic mines and finally on the so-called oyster mines. The oyster mine was a diabolical device, and still is diabolical. It depends upon the negative pressure wave made by the passage of the ship. You can't sweep it. The only thing that will sweep it, that will set it off, is a ship. So, you can sweep it by dragging a ship ^{over} on it, all over the bottom; but that's not exactly what you're looking for. ~~And the~~ ^{An} other thing he was involved with was ~~the operation and~~ ^{operational operations} Research, ~~business~~ ^{He was} during the war. ~~He's~~ ^{he} a first-rate physicist and had done a lot of geophysical work even before the war--measurements of gravity, measurements of ~~magnitue~~ ^{magnetism}. I would say that he is ^{now} probably the greatest living geophysicist. But, anyhow, I never met him during the war at all.

^{My group} ~~We~~ did come in contact with George ~~Beacon~~ ^{Deacon}, ~~my group~~. I worked very closely with a man named Lyman Spitzer, who is now professor of astronomy at Princeton and ~~after~~ the war, went into the thermonuclear fusion business. He's a very, very able man ~~with~~ ^{with} an IQ about 40 points higher than mine. No problem was too difficult for him; he was fantastic. And he had something called the sonar analysis group, which ^{was} ~~is~~ part of the NDRC ~~and~~ ^{and} I was his opposite number in the Bureau of Ships. So, we worked very closely together for many years, all during the war and after the war. Out here at Scripps, or at UCDCWR rather, I worked with Carl Eckhart, who was also a first-rate physicist. He was a theoretical physicist; and, like so many theoretical physicists in the 1920's, he ~~xxxxxxxxxxxxxx1920xx~~ came very close to winning a Nobel Prize but didn't because these guys were all in competition with each other in Germany. Somebody else came up with a ^{great theoretical discovery} a few weeks before he did. ~~But, up~~ until 1941, he was professor of Theoretical physics at the University of Chicago. ^{one of his} ~~his~~ students was Leonard Liebermann ^{who's} ~~he's~~ still on the faculty at Scripps. Carl Eckhart himself died about ^{three} ~~two~~ years ago. ~~three~~ ^{He} years ago. ~~And Eckhart~~ really revolutionized the underwater sound business

by treating it statistically rather than by ~~this~~^{the} ray theory or....

RC: I don't think I understand that. What's the difference between a ray theory and a statistic^{al} treatment?

RR: Well, in the case of the ray theory, we essentially assumed that the sound waves follow a path, a single path. The ray is simply at right angles to the wave^{sound} so you can trace the path of the wave by drawing a ray. Where you get refraction, you may get a divergence, and the rays will split, ~~like that~~. That means that the energy will be very weak there. ^{In} Other places, where the rays come together, the energy is concentrated. Well, that gives you some useful insights. ^{of} Statistical Theory was more powerful because, in fact, the ocean is so complicated. The problems of reverberation, for example, the problems of background noise, the problems of scattering, and the absorption of sound are all related to the microstructure of the ocean, as well as to the macrostructure. The best way to study these things is to look at the statistics, that is, what is the frequency of different events? The theory of reverberation⁺ what's the pattern of reverberation, the statistical pattern with time? One of the great advantages of this is that you can integrate over time^{on} as well as over space. ^{Carl had} He's written a famous paper^{on} what he called "Irreversible Thermodynamics," in which he dealt with phenomena of this general kind, hydrodynamic and thermodynamic phenomena. This was written, actually, before the war. And his interest really shifted then from nuclear physics and atomic physics to ~~this~~^{the} fields of hydrodynamics and thermodynamics; and he made great contributions ^{to} into the theory of it and, to some extent, to the practice, too. He wrote a classical series of volumes^{right} after the war on the propagation of underwater sound, ~~right after the war~~. It's still probably the most important book ever written on the subject, ^{of a} series of books written for the ~~NDRC~~, National Defense Research Committee.

It was one

UCDWR

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There were a lot of people down there at the ~~UCDWR~~ who were developing instrumentation and had ideas of various kinds about the various kinds of energy in the ocean. One of the amusing stories was that, when we were first there--^{the laboratory was} full of hot shots from the physics department at Berkely--led by Ernest Lawrence, Ed McMillan, and a whole group of people, Lawrence had the idea that these damned oceanographers were stupid, which they were, and that the thing to do, instead of fooling around with sound as we had been doing, ^{underwater} was just to get a big light underwater and see a submarine. His idea was that ~~what we had been doing was~~ fooling around with things that were too small, too inexpensive. If you just got a big enough light, you'd be able to see a long way. So he started ~~an~~ whole optical group down at UCDWR under his direction, or at least his inspiration, headed actually by ~~a man named Bowen,~~ Ike Bowen of Cal Tech. He was later ^a director of the Mt. Wilson Palomar ^{and} ~~Observatory~~ ^{Observatory} very nice, competent man. And they built this huge searchlight with millions of candlepower. And they had a black sock, 20 feet across and a couple of ^{hundred} feet long, to be a sort of imitation submarine. The problem was to test out the searchlight. Well, you could see this sock about 20 feet. ~~But~~ Lawrence decided maybe this wasn't such a good idea after all. The problem, of course, was ^{that} ~~this~~ scattering of light underwater created so much background that you didn't get any contrast. But they made a lot of studies of the propagation of light ~~and the transmission of light~~ in the ocean. They had several problems which we oceanographers thought we could help them with, and we could. One of the problems was that ~~the~~ ^{These} ~~damned~~ instruments leaked. Of course, they'd never ~~really~~ done any work under ~~it's~~ ^{the} really ~~every~~ difficult conditions of salt water and pressure, high pressure and highly corrosive water. And so they'd lower ^{a delicate electronic instrument and} ~~the thing~~ in the water and ~~ruin~~ everything. ^{new instruments} they'd have to take it out and start all over again. Finally, they got ~~them~~ so they wouldn't leak, and they went out and made a series of measurements about 50 miles offshore ^{from here} ~~here~~ and measured the transmission at

what actor would

^

different depths. ^{physicist who it} The ~~guy~~ that did it was a man named Malcolm Henderson. He said, "Okay, we've measured the transmission of light in seawater." And Sverdrup and I said, "Why don't you go over here and try again?" because we knew we were on the inside of a front, ^{between two water masses.} So, he finally did, and, of course, he got completely different results--just no relationship at all between the two series of measurements. And so the next conclusion was, "You can't measure the transmission of light in seawater; it's too variable." And so we said, "Well, now, you go over here, and you'll find it's like this, like it was the first time. You go there and you'll find it like it was the second time." which, of course, was true, more or less. But, anyhow, they kind of lost interest in it because of that damned sock. It didn't work very well. Then, ~~anyhow~~, all these hotshots disappeared all of a sudden. We had heard that they went to a place called Shangri-la. This was, of course, Los Alamos, as we found out after the war. ^P But, there were still quite a few physicists left, and quite a few engineers ~~left~~ ^{and} one of the big things they did was work on a mine detecting sonar, although it didn't start out that way. It started out with what they called a variable frequency sonar, essentially a frequency-modulated sonar. It turned out that this might be useful in detecting mines, and the submarine force had a real problem ^{with floating mines of} they couldn't get into the Japan seas because the Japanese had planted floating mines in the ^{submerged} Tsushima ~~Shuohima~~ Straits and the other strait between Hokaido and Hons^{hu}. All during the war, until the summer of 1945, none of our submarines ever went into the Japanese ^{Sea,} ~~seas~~. But, finally, we in the Bureau of Ships, in this section I'm telling you about, the underwater sound section, managed to get 12 submarines equipped with these sonars developed here at San Diego, at the UCDCW, and they did go into the Japan ^{Sea.} The irony of it ^{was that} by that time, the Japan ^{our submarine squadron sank only} Sea was essentially empty. They ~~sank~~ ^{lost} 25,000 tons of shipping and lost a submarine ^{in the process,} What had happened was that the oyster mines had been planted in

all the Japanese harbors, and ^{these mines} ~~it~~ essentially ~~just~~ stopped all traffic back and forth from Manchuria. These were planted by the 29th Bomb Com, the 29th Bomber Command, flying up from ^{the Marianas} ~~Marianas~~. They'd drop these mines in the mouths of the harbors, and the Japanese just couldn't get through them. That summer I was out in the Pacific with the Bureau of Ships. I was out there on Guam in the ^{Marianas and on Tinian} ~~Marianas~~, Saipan and ~~Tinnium (?)~~. I talked to Ellis Johnson, the man from the Bureau of Ordinance who'd brought these mines out and organized their being sowed in the harbors of the Japanese coast. ^{PP} By that time, I was involved with quite a few different kinds of oceanographic problems. The principal ^{one} ~~is~~ was the problem of surf forecasting. There were two aspects of this: one was, if you looked at the waves coming into shallow water, the waves would be bent and ^{crowded or} ~~crowded~~ and spread apart, just like the sonar; ^{we used} ~~you use~~ the same ray theory. This was because of the variation of the bottom topography. You could tell quite a bit about the bottom topography from the aerial photographs of the waves. The other thing was that ^{Harold} ~~Harold~~ Sverdrup and ^{Walter} ~~Walter~~ Munk, in this country, ~~Harold Sverdrup and Walter Munk~~, and people in England had developed a method for forecasting surf. Knowing where a storm was and how strong ^{it} ~~a storm~~ was and over what area it spread, you could tell where the waves would go and how high they would be on a beach and ~~what the other characteristics were and~~ how far apart they would be--things like that. This was ~~a~~ useful ~~thing~~ for amphibious landings. They were planning two amphibious landings in Japan. One was called Operation Olympic and the other was called Operation Coronet. I was on Admiral ^{Spruance's} ~~Scrunse's (?)~~ staff; ^{which} ~~his staff~~ was planning these landings. ^{Spruance} ~~Scrunse~~ was very much against it--not against me being there, but against the landings--because he thought it would be deadly, particularly for the Japanese. Many ^{men} ~~of~~ the Japanese would die, and a lot of our ^{also} ~~guys~~ would die. He just didn't like it at all. I was ^{on Guam} ~~there~~ at the time when the "Indianapolis" never arrived.

and other people said

I remember this very well. Somebody said, "Where's the 'Indianapolis'?" I ~~don't know~~ ^{don't know}. We had not gotten an arrival message from Manila. It turned out that she had left about ten days before, from Saipan for Manila, and never sent an arrival message. The person who should have been watching for this missed it. She'd been sunk, torpedoed, by a Japanese submarine. These poor ~~men~~ ^{men} were in the water five or six days; most of them were lost. We heard about this as it came in, first not knowing where she was and then looking for her and finally finding ~~her~~ ^{ing} her--not finding her, but finding her survivors.

She was the ship that had brought out the two atomic bombs to Saipan. There were only two; that's all there were. I didn't know that at all; all I knew was the "Indianapolis" was lost on the way to Manila. So then ~~Serume~~ ^{Admiral Spruance}

sent me from Guam to Manila to be on Admiral Turner's staff in their intelligence group. I was just climbing the ladder of the "Eldorado", which was Turner's command ship, when word came over the loud speaker that a bomb

had been

dropped on Hiroshima. That was the end of the war, right there. So, thank God, neither of these operations, Coronet or Olympic, came off. All the work that we'd done on intelligence, wave forecasting, and the bottom topographic mapping was ~~again~~ ^{previously} lost. But, ~~thank God~~, it was actually used.

Both wave forecasting and surf forecasting and the estimations of bottom depths were used in several amphibious operations. **P**This was really the earliest big thing that Walter Munk was involved with--developing ~~this~~ ^{the} surf forecasting method. He and Sverdrup were constantly having trouble getting cleared because they were both born abroad. In fact, Walter was not allowed to continue at the Radio and Sound Lab because they wouldn't clear him.

He enlisted in the Army for a couple of years and finally was sprung by an ~~oceanographer~~ ^{Seiwell} named ~~Cywell~~ ^{had} Cywell, who ~~was to~~ become a major in the Air Force and ~~who~~ had organized an oceanographic unit in the Air Force. ~~Cywell~~ ^{Seiwell} had also captured Sverdrup by this same process of being sure he wasn't cleared and then getting

him ~~cleared~~ not cleared, but kind of captured--by his group. And they developed the surf forecasting method and the wave forecasting method while they were working in this group under ~~Cowell~~ ^{Serwell}. Then, later ~~they~~ they started a wave forecasting school here at Scripps. Lots of both Navy aerologists and Air Force meteorologists studied ~~this~~ ^{here} and learned the method, learned the technique, and applied it. It was applied ~~particularly~~ in Sicily. It was applied in a very interesting way in Normandy. ^{He} Two men who did that were Charlie Bates and ~~a man named Crowe~~ ^{John Crowell}. ^{(Crowell} ~~Crowe~~ now teaches at UCLA, and I think Bates works for NOAA.) What they did was to say that the waves are going to be pretty ^g high, but not impossibly high, so that Eisenhower decided to go ahead. The Germans didn't believe it, didn't believe that they would go ahead with it, "Nobody would be out on a night like this." But they were ~~and~~ and that was one of the elements of surprise that made the landing a success. They were an awfully bloody business even so.

RC: How did you choose Mary Sears as the head of the oceanographic unit in the Hydrographic Office?

RR: I didn't choose her; she chose me. She appeared in my office in the Bureau of Ships, and I needed somebody. By that time, we needed somebody out at the Hydrographic Office to do various kinds of mapping and descriptive jobs.

She'd gone through a WAVE indoctrination course ^{VA} and I very well remember her coming into my office. There was this fat little figure, and she stood up ^{her} at a full height of about 5ft. ^{five feet three inches} 3in.; rigidly at attention--she was just scared to death. And this was a shock, to put it mildly. I asked her to sit down. And I got her assigned out there to the Hydrographic Office.

She went to town. She built up this huge ^A not huge but big ^A oceanographic unit which wrote intelligence, not exactly papers, but intelligence chapters ^{of} ~~of~~ descriptive oceanographic intelligence: descriptive oceanography, which was important to submarines and to all kinds of ships--destroyers, ~~VA~~ particularly--

dealing with the propagation of underwater sound and with such things as the density layers that the submarines could sit on, ~~and~~ wave conditions that would be related to amphibious operations, and things like that. Also, air-sea rescue. There were a lot of applications of oceanography around the edges of the naval operations. One of the things that Sverdrup did was to make current charts, which we ^{then} turned into handkerchief charts, that we gave to aviators. And these current charts supposedly came down in a raft: you looked which way you wanted to go, which was the best way for the current; both the current and the wind had about an equal effect on a raft. If the current and the wind were in the same direction, you'd go much faster than if the wind were in the opposite direction; and which was stronger--the current or the wind--determined which way you'd go. And this was, of course, a probabilistic thing. The currents never went in just one direction. Most of the time they ^{id} go in a certain quadrant, during a certain season of the year. And this was used also by air-sea rescue people to estimate where a ^{downed} ~~gallant~~ aviator would be.

Another thing I got involved with in the Bureau of Ships was smoke forecasting. This was ~~again~~ essentially a turbulence phenomenon. A man named Jeffreys Wyman of Woods Hole was the leader of this group. And the problem was which way would a smoke screen go and how long would it stay close to the ground or close to the surface of the sea, when would it rise, how much would it spread, how long, how much smoke would you have to put in it to make it useful, things like that. This work was never really used. The Navy really never did like the smoke screens. But what was used was something that came out of this, and this was done by a man named Joe Barber, ^{his real name is Cesare Lombardi} ~~Chasery, Lombardy~~ Barber, who is now a professor of English at the University of California at Santa Cruz and who was a professor of English ^{then} ~~then~~ at least a young assistant professor of English. He got into a sailor's suit, and he

was assigned to me. And he was a project officer for this smoke work. He got tired of ~~this~~ ^{the} research part and decided that he wanted to be more active. So he went out to the Pacific with some smoke generators, and it turned out that ~~and~~ ^{at} these big operations like Okinawa and Iwo Jima and some of the others big landing operations, there were lots and lots of ships lying offshore. And these were just sitting ducks for the Kamikaze. (The Kamikaze were more than a nuisance ~~here~~; they were quite a serious business.) So what they did was to spread a blanket of smoke over the fleet whenever there was a kamikaze signal or alarm. They'd turn on these smoke generators and just blanket the whole area with smoke. And the poor ~~damn~~ kamikaze couldn't tell where they were or where the ships were. So this worked very well. This was really Joe Barber's very special job.

RC: Well, now, did the Hydrographic Office result in creating the ONR?

Is that where the ONR came from?

RR: No, no, not at all. There was ~~an~~ ^{an office} called the Coordinator of Research and Development in the Navy department, headed by an admiral, ~~I'll be damned if~~ ^{Admiral Furer -} I can remember his name, but he had on his staff a whole collection of very bright young guys--one regular naval officer named Bob Conrad and half a dozen young ^{Reserve} lieutenants, one of whom was Jim ^{Wakelin} Wakler who was more recently Assistant Secretary of the Navy, a man named John ^{Burwell} Birdwell, a man named Bruce Old (who's now with Arthur B. Little and was, until recently, Foreign Secretary of the National Academy of Engineering), a man named ^{Kraw, and} ~~Crouse~~ ^{(this several others,} ~~admiral's name began with "F")~~. And they sat around and talked about what was needed for research in the Navy. They ~~really~~ drew up a plan for what ^{now} has become ONR. First of all, it was the Office of Inventions and Research, OIR, or ORI, Office of Research and Inventions. And the first head of it was ~~a~~ ^{an Admiral} man named ~~Bowin~~ ^{Bowen}, Harold Bowen, who had been Director of the Naval Research Laboratory. The Naval Research Laboratory became part of ORI, as

did Admiral DeFlore's group, the group that developed simulators of all kinds--training simulators. And then they had a headquarters organization that supported basic research in a variety of fields ~~and applied naval research,~~ ^{and also} ~~or~~ tried to support applied research. This became, ~~then,~~ ^{the basic idea} the Office of Naval Research, after a while. ~~And the whole notion was--~~ it was really a revolutionary notion--that the government should support basic research, which would be that research that scientists wanted to do, not the research that somebody in the Navy wanted ~~him~~ ^{them} to do. ~~Allen~~ ^{Allen} Waterman, who had been in the NDRC, became the civilian head of it. And then there was a very bright staff: I was one of them, although I was in a sailor's suit part of the time; Manny ~~Blot~~ ^{Pione} ~~Blot~~ ^{Blot} later became ~~the~~ chief scientist for IBM; Fred ~~Signtz~~ ^{Scitz} ~~Signtz~~ ^{Scitz} who is now ~~President~~ ^P President of Rockefeller University; ~~Mona Reese~~ ^{Mona Reese} ~~later~~ ^{Mona Reese} became the graduate ~~Dean~~ ^{Dean} of City University of New York; Joe ~~Wild~~ ^{Wild} ~~has~~ ^{weil} ~~been~~ ^{who} Dean of Hunter College; Randall Robertson ^{who} later became chief scientist of NSF--these people were just as gifted a group of scientific administrators as you can possibly imagine, all headed by Bob Conrad. He was never chief; he was only a captain, but he was the inspiration of the group. And they really invented the notion of support of scientists in their discipline doing the things they wanted to do. I was head of the geophysics branch. ~~And we~~ had a rule that we would automatically turn down a project which the guy said was going to have all these naval applications. We also automatically approved a project which was for less than \$5,000. These were our two fundamental rules of operation. ~~But we basically,~~ ^{we} ~~in~~ those days, didn't have advisory committees or ~~as pure a view~~ ^{"peer reviews"} which everybody talks so much about now. We thought that was for the birds. We decided if this ~~guy~~ ^{applicant} was good and this ~~guy~~ ^{one} wasn't, that was that. ~~One of the things we did....~~ ~~And~~ ~~in~~ my geophysics branch, there were three of us: one was a ~~man~~ ^{meteorologist} named Dan Rex, who was also in a sailor suit as I was; and a man named Earl ~~Presley~~ ^{Druessler} ~~Presley~~ ^{also} a meteorologist. ~~These other two people were meteorologists.~~ And we decided that oceanography was

somewhat different than, say, physics or chemistry or most kinds of geophysics, because ~~these~~ ^{The} ships were such expensive instruments. ~~But~~ ^{Consequently,} as far as oceanography was concerned, we tried to support a broad institutional program rather than specific individual projects. And that's still done pretty much, although what they do now is ~~primarily~~ ^{do} support the ships ~~in~~ ^{from} a separate fund and then support individuals or groups in their research proposals.

RC: Alright now, I'm jumping ahead of my story for a second, but do you think the ONR is still as hard-science oriented as it was then? Is it not more research oriented now? I mean, in terms of special mission research-oriented?

RR: You mean mission-oriented?

RC: Mission-oriented, right.

RR: It was research-oriented. It's now mission-oriented.

RC: Okay, right.

RR: Well, I think that's true. The quality of the staff of ONR has progressively declined over the past 25 years, 30 years, just about 30 years now.

RC: It is 30 years now, yes.

RR: The staff in the early days had some...well, as I say, ~~these guys~~ ^{they} all became great men, whereas now, it's very unlikely that anybody will hear of any of them except that they're devoted public servants. The people in oceanography, at least, still have very high regard for them. Walter Munk, for example, ~~is~~ ^{thinks they are} very great. The people at Woods Hole think so, too. ~~I think so, too.~~ But, they're not able ~~really~~ ^{committed to} to hold up against the overbearing weight of the bosses who are ~~now to this~~ idea of directed, or not so much directed as applied, research. It doesn't affect oceanography quite as much as it does other fields. The Navy, in general, still thinks that anything you find out about the ocean may be useful to them sometime. That was one of the great things we put across during the war. And, as you probably know, the Hydrographic Office, which regarded Mary Sears and her group as essentially an

unnecessary pimple, has now become the Oceanographic Office ~~XA~~ and the Hydrographer is now the Oceanographer, showing the ability of the Navy to absorb things.

RC: What happened to Mary Sears, by the way?

RR: She went back to Woods Hole, and she continued research on plankton, particularly zoo plankton. ~~More particularly,~~ ^{And} she became editor of The Journal of Deep Sea Research. She's a marvelous editor, and she really built up that journal. It's a great scientific journal. She was also the clerk of the Woods Hole Oceanographic Institution Corporation and ^{of} the Board of Trustees-- really a very important person in the Woods Hole hierarchy ^{because she is such} as a sane, gentle, good person. She also became very much interested in politics; and she, for many years, was chairman of the school committee, ~~founded the school committee~~. That was really her principal ^{Falmouth} interest, ~~this~~ ^{at} school committee and Republican politics on Cape Cod. She never did much in ^{research} ~~science~~, either before or after the war, ~~but~~ I guess the most important single thing ^{she} ~~ever~~ did was that she was ~~the~~ chairman of the committee that organized the ^f first International Oceanographic Congress which was held at the United Nations in 1959. A job like that she ^{just} ~~just~~ was superb at, ^{she is} so strong-minded and gentle ^{at the same time} behaved.

RC: What was the ONR Joint Task Force Number One in 1946?

RR: That wasn't ONR. Joint Task Force One was the Bikini atomic bomb ~~explosion~~ ^{test}.

RC: Okay, and that doesn't come with ONR? You don't go to that from ONR then?

RR: Actually, I transferred from the Bureau of Ships to ONR right in the middle of that operation. But I was on ~~task~~ ^{detached} duty on Admiral ~~Blandy's~~ ^{Blandy's} staff all during the time of the Bikini tests. Actually, I was on Ralph Sawyer's-- Ralph Sawyer was the chief scientist--ship, the ^{Whiting} ~~Kenneth White~~, but we were both on Admiral ~~Blandy's~~ ^{Blandy's} staff. I was in charge of all aspects of the oceanographic and marine biological work.

My first contact with Walter Munk was in 1937, when he was an undergraduate

at Cal Tech. He came down here in the summer time. I think he ~~basically~~ came ~~down~~ here because he was interested in a girl named Bumps Anderson, the granddaughter of ^{Eller} Allan Browning Scripps' lawyer, Jacob ~~Hartford~~. Anyhow, ^{Walter} he was here and he worked at Scripps. We spent several days together sitting in a rowboat about half a mile off the end of our pier, measuring currents. We had a contraption that you could lower to the bottom ~~It~~ ^{XI} would sit on the bottom and would measure currents there, and you could also measure them in the water coming up to the surface. ^{We} You did this by hand, over and over and over again, for several days. And he's been ^{here pretty much} ~~coming back~~ ever since. I think he was one of our first summer fellows at Scripps. He turned out so successfully that they've been running this Summer Fellowship Program ^{ever since, for the} ~~the~~ last 40 years. I don't think they've ever found anybody quite his equal. In fact, I don't know of anybody who ever became an oceanographer that way besides Walter. But ^y maybe once in 40 years is enough.

RC: Well, exactly how did Operation Crossroads evolve?

RR: The first I heard of it was when two ^R Naval captains (I was a commander in the Bureau of Ships then), ~~two Naval captains~~, one of whom was Horatio ^{River} ~~River~~... you may have heard of him; ^h he was later Deputy Chief of Naval Operations, Commander of NATO forces in southern Europe, later Ambassador to Spain, a little man from Puerto Rico, about five feet, two inches high, one of the brightest men I ever saw in my life. He and another captain named Ashworth, Fred Ashworth, who was an aviator, called me into their office and asked me to help them pick out a site for an atomic bomb test, a test of atomic bombs against naval ships. Mary Sears and I got together. They were thinking about Bikini already, and ~~was~~ the Marshall Islands in general. We recommended Bikini. But ^r they'd already been thinking of making the test. You may not remember, you're too young; but, in those days, there was a great deal of question about whether, with the existence of an atomic bomb, naval ships were of any use any more. So the problem of just how

vulnerable ships were to them was an important subject. ^{The United States,} ~~We~~ of course, had lots of ships, ~~we had~~ the greatest Navy that the world had ever dreamed of, let alone seen--many more ships than we could ever possibly use again. So they appointed Admiral W. H. P. Blandy (R) as commander of ^{the} ~~this~~ task force and Deke ^{Parsons} ~~Persons~~ as the technical officer in charge. Both these officers were members of what the Navy ~~has~~ called ^{the "club"} ~~gun (?)~~ or ordinance officers, Although Blandy had actually had command of a task force. They were both ~~surface officers,~~ ^{sufface} ship officers. ~~But,~~ I guess Parsons was actually a specialist, ~~but~~ not ~~actually~~ in line of command. He was a specialist in ordinance. He had been involved right from the beginning at Los Alamos and elsewhere in the Manhattan Project, and he was the bombardier on one of the two atomic bomb drops in Japan. And they assembled a staff, a big staff, and worked very closely with Los Alamos, who were making the ^M bomb, the ~~atomic~~ ^{atomic} devices--~~they were really devices.~~ One of them was a ["]device["]; the other was an actual weapon. One of the things that we thought had to be known was what would be the effect of the explosion and the radio ^{activity} on the atoll. So we decided that we would make an ecological base study--find out about as much as we could about what was there, what things were like before any radioactivity had been released, before there was any explosion. So we sent out an old ship, the "Bowditch," with a whole group of scientists, various kinds of scientists--geologists, botanists, fish people, people to study all the different kinds of living things that were there. One of the people was Harry Ladd ^{from} ~~with~~ the Geological Survey ~~Another~~ was a man named Taylor from the University of Michigan, William Randolph Taylor, who was a great taxonomic botanist. Jack ~~Mar~~ was there for the fish part of it. There were quite a few fish people. We had experimental fishermen as well as ^{*} fish taxonomists, and they spent about six months making a profile of what Bikini Atoll was like and the surrounding waters. Ken Emery was there

and did a lot of work on the bottom ^{hydrography}. I ~~guess~~ Bill Van Arx was there, as
 were two or three ^{other} people from Woods Hole, looking at the water and looking
 at the diffusion of the water. There was a ~~group~~ ^{properties of it} of people from a lot of
 different institutions. One of them ^{Marston Sargent} was Marston Sargent from Scripps,
 who was my assistant in the Bureau of Ships ^{and}. He was made the projects
 officer. ~~Now~~, This old hydrographic survey ship, the "Bowditch," was sent out
 there and spent about six months there, long in advance of the arrival of
 the task force, finding out what the biology was like. Then we had other
 oceanographic responsibilities, ~~that were:~~ ^{was} one, to measure the waves; that
 was perhaps the most important. It was thought there might be some very big
 waves, particularly from the underwater test. We also had a group of ~~ships~~
~~destroyers~~ to measure the radioactivity in the water outside the atoll, to
 try to follow ~~the~~ the fallout. ~~I'm not sure they were destroyers;~~ I think
 they were EPCER's--smaller than destroyers--about six of them. John Lyman
 was in charge of that part of it. Then we had various...I guess ^{most things were} ~~everything~~
~~was really running~~ ^{related} to the waves, one way or the other, but we did a lot of
 different things. We put up photographic towers 100 feet high with cameras
 on them that could be started automatically--still cameras, essentially,
 that would take very good pictures of what happened, the sequence of events.
 We had bottom-mounted wave meters called Turtles ^{designed by Allyn Vane} ~~by Alan Bynes~~ at Woods
 Hole. Jeff Holder ^t had a very simple system. He put up poles on the beach
 with a series of tin cans nailed ^{at} to different heights above the beach.
 Those cans ~~that were~~ ^{would} filled with water to show how high the wave was.
 Another man, who was not part of our group but who was the same ingenious
 kind of person ^{as Jeff Holder,} was Bill ^{Penney} ~~Penning~~, who later became Lord ^{Penney} ~~Penning~~ in charge of
 the British atomic weapons establishment. He had tin cans that were closed,
 and you could measure the blast by seeing how much these were bent or dented,
 how much they were squeezed together--almost as ingenious as Jeff's idea of

these open tin cans that would show the wave height. We had a whole ^e ship
~~that we~~ devoted to these oceanographic measurements. John Isaacs was very
 much involved with the photography. Gifford, Ewing, and Walter Munk were
 involved with the measurements of the dispersion ^f of diffusion of the radio-
 activity inside the atoll. We had a lot of ways of measuring radioactivity
 inside the atoll. I think Ted Folsom was involved with this, too; I'm not
 quite sure whether he was involved or not, but I think so. John Isaacs can
 tell you a lot about ~~that~~. ^P We had all sorts of funny experiences. We were
 putting up these wave-measuring devices and dispersion-measuring devices
 all over the atoll, which was about 20 miles long. They had a series of
 landing craft to carry them around and to move them from one end of the atoll
 to the other. These were not ^{L C B M S} ~~LCBMs~~. They were one size ^{larger} ~~large, essentially~~
^{over} 50 feet long, and quite beamy--sort of barges. Those were the days when the
 Navy was ^{at its absolute nadir.} ~~its absolute~~ Navy. Most of the enlisted men there were guys who
 had been parolled [/] or sentenced to the Navy's equivalent of jail for quite
 a long time. But, ^{if} they'd go out to Bikini, they'd be parolled [/], put on
 probation. And it was typical that ^{the} crews of these damned landing craft would
 get up to the far end of the lagoon and say, "Well, we think our discharge
 papers have arrived. We'd better go back." And the only guys who were there
 with them were civilian scientists. ^{the} Sailors paid very little attention
 to them. They'd go back, and, by God, [^] their discharge papers had arrived.
 And you had to get another crew and start all over again. But, we had
 also quite a bit of experimental work in advance of--particularly the under-
 water test because the underwater test was.... There had been a theory
 developed in the last part of the war ^{which} which was given a good deal of credence
 by Winston Churchill. He thought it was true that you could make a huge
 wave if you made a big enough explosion ^S at the right depth underwater,
⁺ The theory more or less bore this out ^{and} and ^{at} that this wave would travel a
 very long way, would be a kind of a weapon causing very considerable amounts

of damage at great distances. This underwater explosion was one way of testing this out. In order to get some idea of the magnitude and the phenomenology, Mike O'Brien, who was then Dean of the College of Engineering at Berkeley, organized a series of tests, test explosions, which they photographed with high-speed photography. Afterwards, we looked back on those tests, and we saw something which really occurred as a theme. That was something called a base surge. Did you ever hear of a base surge?

RC: No.

RR: Base surge is the most frightening thing I ever saw in my life, the first time you see it. It was a...what looked like a wall of solid water, several hundred feet high, moving very rapidly out from the point of the explosion. But ~~after~~ after it got about half a mile out, it stopped ~~and~~ and it lifted from the surface, and then there was a huge rain. It was essentially a very dense cloud. It was a density current. What happened was that the underwater explosion shot a column of water and air, a huge pillar, about half a mile across, up into the air. Then this thing settled down again and was, essentially, a mist or a cloud. But it was heavier than air because there was so much water in it ~~so~~ so it came down like this and then spread out like this. Then, as enough water dropped out of the thing, it was hot. Therefore, the air was able to lift ~~it~~ above the surface. So the base surge, after about something like 30 seconds, just lifted off the water, turned out not to be a wave at all. There were waves, in addition, and we were able to measure the waves. The waves were like ten feet high, ~~or~~ 15 feet high. This thing ^g was, to begin with, a couple of hundred feet high--just looked terrible--and moved very, very rapidly. It was what we call a density current coming out like that. This was a very interesting phenomenon which we ^{*} never would have really been able to understand or even to record if it hadn't been for the enormous amount of work ^{that} of John Isaacs and other guys

did in the setting up ^{of} these photographic towers and the photographic systems.

P A year later I organized something called the Resurvey of Bikini ~~(T)~~ (this was after I was in ONR.) The resurvey went back there with a much smaller task force to see what had happened, what was it like a year later. We had a group of biologists and radiochemists there. That was one of the hardest jobs I ever did in my life, ~~was~~ organizing that, the scientific part of that resurvey, because nobody wanted to be involved. But there were lots of radiochemists, and I managed to get Charlie Coriello, who later became professor of chemistry at MIT. I somehow got...I guess I knew him before the war; he was at UCLA. He'd been involved with the radiochemistry business all during the war, and he managed to recruit a group of good radiochemists to go out there and work on this problem. If it hadn't been for him, I would have ^{been} sunk. I used to stay up till 2:00 or 3:00 in the morning, calling people all over the country, trying to get them to go out on this resurvey of Bikini. This was in 1947. It worked out very well. The most important outcome, however, scientifically, was the drilling that we did on Bikini Atoll. We got an oilwell drilling company contracted to drill a hole in the reef because the results we'd gotten the previous year from our seismic work indicated that the limestone was several thousand feet thick, or the reef was several thousand feet thick, and that, maybe at the depth of a mile or more, you finally came to basalt. This was, at that time, one of the great unsolved questions: what was the origin of coral reefs? Darwin had had the idea long ago, 150 years ago, that coral reefs were sunken volcanoes and that, as the volcano sank, the coral reef built up. First you had a volcano here with a fringing reef around it. And then, as the volcano sank, the reef moved out and became a barrier reef. And, as the volcano sank still more, you had nothing except the reef and the lagoon in the middle. There were other people who said this was just nonsense, that the reefs were formed during the Pleistocene. And so it ought to be a very important thing to do,

to find out just what they were like. And it turned out that at least Bikini, and later ~~Enewetok~~^{Ni} and other ones that have been studied since, were all flat-top sea mounts, where the mountain was planed off at sea level-- there was a pile of volcanic ash and lava cut off by waves at sea level. And the coral grew around the edges of these things, and then they sank. If they sank slowly enough, the coral could keep up, and you eventually ended up with an atoll. The lagoon in an atoll, however, is a Pleistocene phenomenon. When the sea level was about 200 feet lower than it is now, ~~there~~^S these things were all planed off. And so the islands around the edges of the atoll-- I mean the atoll itself, ~~the~~^A islands around the lagoon are all not more than a million years old, formed on a platform planed off during the Pleistocene, which is the bottom of the lagoon, the deepest part of the lagoon. But underneath that is a bonepile ~~6,000~~^{4,000 to 6,000} feet thick, or 4,000 to 6,000 feet thick, of the remains of coral and coralline algae which grew on a submerged volcanic platform. Now, you find lots of things called gios, ~~of~~^f flat-top sea mounts, which sank too fast and which have a little bit of coral on them, but not very much. And they're covered mostly with manganese, crusts of manganese, with occasional shallow-water fossils--enough so that you can tell their age and that they were in shallow water when they were planed off. And that was one of the discoveries we made during our mid Pacific expedition: that these sea mounts, far from being a billion or two billion years old, were about a hundred million years old, of Cretaceous age. And we found that by finding fossils that were shallow-water fossils, ~~now~~^{at} depths of 6,000 feet were Cretaceous fossils. But the first part of this study was the seismic work during "Operation Crossroads."

RC: But in the follow-up?

RR: And then the follow-up in the Bikini resurvey.

RC: Should we have another Bikini test, by the way, now that the bombs are more

powerful?

RR: Well, we have had a whole series of the hydrogen bomb tests on Bikini and on Enewetok. They tested all the big thermonuclear weapons there. You're thinking about a test against ships?

RC: Yes.

RR: Well, that's very hard to do. We did have a test of an underwater atomic weapon down here off Mexico. We lowered the weapon to about 3,000 feet and exploded it. But, as far as I know, nobody's ever exploded a thermonuclear device underwater. I may be wrong about that. In any case, they're so big that, if you explode one in a lagoon like the Bikini Lagoon, you'd simply blow the water out of the lagoon.

RC: I only have one other question about this part in your career I want to ask you. Why not a career in the Navy? Did you ever consider a career in the Navy?

RR: Yes, I ^{did} but after World War II, not before World War II.

RC: No, I meant in the period between '45 and '47, really.

RR: I really wanted to go back to Scripps. The only reason for considering a career in the Navy was if they didn't really want me back at Scripps very much. And a lot of guys didn't. This is a fairly complex story. Carl Eckhart and Harold Sverdrup were just bound and determined that I should be Sverdrup's successor as ^dDirector of the Scripps Institution. There were several other people on the faculty that didn't think this was a good idea at all, including ^{ci}Francis Shepherd and Carl Hubbs of Fisheries. They had different reasons. My analysis would be that Hubbs thought we ought to have a biologist as director, not a military man, not a Naval man, of all things not a Naval man! Shepherd's opposition was basically because we hadn't gotten along very well when he had this project, DSA project, out there. He's really not very bright. If Lyman Spitzer has an IQ 40 points higher than mine, my IQ is considerably higher than Francis Shepherd's. Shepherd's got other qualities, including indefatigable curiosity and enormous energy;

but he's not awfully smart. And that's kind of hard--to work with a much younger man who's a lot smarter than you are, if you don't have a generous spirit. So, in any case, they were opposed to my being director. Finally, a compromise was hit on, and that was that Carl Eckhart should become director. So, he was director for two years, '48-'50.

RC: '48 to '50, okay, that makes sense.

RR: And he asked me to come back as his associate director, which I did, and I was promoted to the professorship at the same time. So I'm the only guy who ever went from being an instructor to being a professor without having the intermediate steps. I was instructor when I went into the Navy and came back as a professor, that is, associate director.

RC: My impression is ^{that} you were carried on the faculty listing in Scripps Institution the whole time that you were in the Navy.

RR: That's right, sure. They may even have promoted me in absentia; I'm not quite sure about that. After two years, Carl resigned as director, and I was appointed acting director. I was acting director for the year 1950. This opposition to my being director still existed, still persisted. But I had some powerful allies. ^{XM} Many of them were not here at Scripps, including Datlef Bronk, (?) who was at that time president of Johns Hopkins University and president of the National Academy of Sciences and President of A.A.A.S. Office; and another was a man named Bert Walford in the Bureau of Commercial Fisheries. Bert suggested that we have a conference on the future of the Scripps Institution, that we bring to this conference people from all over the world. And this was sometime in the spring, I think, of 1950. I'm pretty sure that's when it was. Although I'm not quite sure, I'm pretty sure it was the spring of 1950. Maybe it was in the fall. The reason I think it was in the spring...it was before the loyalty oath controversy erupted. And ~~Sprawl~~ (?) was, of course, very much in favor of my being director, Bob

Sprawl
John

Spraw (John)

~~Spraw~~, president of the university, ~~XA~~ and that's why he was willing to sponsor the conference. He did, and Bronk came out, too, and Columbus Islin from Woods Hole and a lot of guys who were good friends of mine. And we spent three or four days talking about the future of oceanography and the future of the Scripps Institution. It was an inspiring affair in many ways. Later, at the end of it, we dedicated the Thomas Waylan Vaughn Aquarium Museum. Bronk made the dedication speech, and I was supposed to play a record that T.W. Vaughn had made. He was too old and too ill to come out himself, but he made a recording. (I'm not sure exactly whether it was a tape recording or a phonograph recording or disc recording.) I forgot to play it, the damn thing! But, anyhow, other than that, ~~Spraw~~ ^{Spraw (John)} was in good form, and Bronk was in good form, ~~XA~~ and it was quite clear after that that I was going to be the next director. ~~P~~ But then all hell broke loose because the ~~P~~ Regents--those were the days of McCarthy--passed a resolution that all faculty members had to sign a loyalty oath saying that they were not a communist. And a lot of people at Berkeley and UCLA didn't want to sign that oath; not that they were communists, but they thought that this was an infringement of academic freedom. Maybe even some of them were communist, for all I know, but I don't think many of them were. The worse thing about it was that the ~~P~~ Regents said that if you don't sign the oath, you would get fired, you would get discharged. And that violated the principle of academic tenure, which I think is an absolutely fundamental principle in a university. So, none of our guys out here signed it, more or less, under my leadership. We were brought into the academic senate councils of UCLA and Berkeley; and a lot of my colleagues, like Mikes Bryan in the field of engineering, were on the same side. He didn't quite understand why; but, if I was on it, he was going to be on it, too. David Saks (?) was a very powerful, not powerful but very dedicated, man who was fighting the ~~P~~ Regents, too. He 's the new

president of the University of California. He was the young assistant professor in physics then. And we managed to get old Hollie Mad Smith on our side. He lived here in La Jolla--Holland M. Smith, the great Marine Corps General, a wonderful man. He was not a West Point or Annapolis man or anything; he was a lawyer. He got a law degree and then went into the Marines and became a great general. But he still remembered his law~~land~~ and, as a lawyer, he decided this was ridiculous, that this was really an infringement of academic freedom and violation of tenure. So, he became an ardent supporter of our side and went to see Governor Warren. I remember very well a couple of these little old ladies in tennis shoes who infest La Jolla came up to him and said, "General, why won't those professors sign those simple oaths saying they're not communists?" and the General turned to them in his sternest voice and then he said, "Mad'm, if somebody asked you to sign a paper saying you were not a whore, what would you do?" And that was the last time he ever spoke to them. I was one of the witnesses before the alumni committee. It was quite clear I was not a communist, having been in the Navy for seven years and never really taking any part in any political activity. So, I was a good witness, and John B. McLaughlin and Bek^o, Steve Beko, (?) head of the Beko Corporation, were members of this alumni committee, and various other very decent citizens and great men in California. And they proposed what was called the "Alumni Compromise," which was that there would be a committee appointed by the academic senate to investigate all those people who had not signed the oath~~s~~ and that committee would recommend^d what should be done in each individual case--like nothing, for example. And this was considered by the R^egents at a meeting in Davis. Governor Warren was there presiding. The Governor of California is ex-officio to the president of the Board of Regents, not the chairman, but the president. And when he is there, he presides. The R^egents passed this by a pretty large majority, this "Alumni Compromise."

I never will forget. I was sitting in the audience, of course, and I never will forget that occasion because Mario Geonini, son of A.P. Geonini, was then... Mario ^{who} was then head of the Bank of America, the biggest bank in the country. ^{he} said, "The flags will fly in the Kremlin tonight. I hereby resign from this Board of Regents, and I'm going to devote the rest of my life to organizing vigilantes to fight communism." He said this in exactly those words. And this just gives you some idea of the spirit of that awful time.

RC: Well, you know, in Texas we had to sign it by law. It was passed by state law: any state employee had to sign a loyalty oath, because I can remember...

RR: Well, if you have to do it by law, you have to do it. The ^Regents did not have that legal authority. We said, at least, we would obey the law; we wouldn't sign something that was just imposed by the ^Regents. And Governor Warren said, "Mario, you don't want to do that. The democratic thing for you to do is to stay here and try to convince us that we're wrong. Don't resign from the Board; stay on the Board and try to persuade us to change our minds." Well, nevertheless, Geonini did resign ^X and I took an oath that I would vote for Governor Warren from then on, no matter what he did.

Unfortunately, he never ran for anything again. Later he was appointed Chief Justice of the Supreme Court. That was a couple of years later; this was in 1950. ^R And the ^N Russ Raitt and I and, I guess, not many other members of the faculty went out on our mid Pacific expedition in the summer ~~of~~ 1950. That was a two ship expedition. That was the first great exploring expedition of the Scripps Institution ^y in 1950. One of the things we discovered was the mid Pacific mountains, where the mountain ranges run west of Hawaii. Another was we measured the heat flow coming through the sea floor and found ~~that~~ it was about the same as ^{he} ~~teh~~ continental heat flow. Russ Raitt made a lot of seismic measurements which showed that the sediments were only about

a hundred meters thick. This was the beginning, really, of the great series of discoveries that have ended up in plate-tectonics. These sea mounts 6,000 feet below the surface, flat-top and _____, all bent at the surface, are very young, not old. There was no place in the ocean that very old. It was thought that they were perhaps two billion years old because they were so deep. There were very thin sediments which tied in exactly with the idea of the sea floor being very young. The heat flow, which couldn't be explained in any other way except by convection ^{is} in the mantle ~~and~~ and then later, of course, ~~later~~ expeditions were conducted in the trenches and the mid-ocean ridges, and particularly the magnetic stripping, which is another story. We started with the ~~MidPac~~ ^{MILPAC} expedition in 1950. We organized more and more exploring expeditions over larger and larger areas of the Pacific. These were led by a series of inspired expedition leaders. I led the Mid Pacific expedition and what we called the Capricorn Expedition; but most of the expeditions were led by Warren Wooster and by Phil Menard and by Bob Fisher. Those are the three that occur to me offhand as the people who really were very good at this. ^P And we gradually built up our fleet of ships, and we had more and more kinds of expeditions. For example, we had one in the South China Sea that lasted for nearly three years--South China Sea and the Gulf of Thailand ^A which we called the Naugah Expedition. The leader of that started out to be a Dane named Anton Bruun; but he got sick, dissentary, and it was taken over by the captain, Jim Vaughn, also an inspired leader. It takes a very special kind of guy to lead an expedition. Maurice Ewing was very good at it. ~~X~~ Different people do it in different ways. But the essential thing is to make everybody feel that he's getting his ~~S~~ share of the pie, that all the people ~~on~~ [/] aboard are accomplishing, doing something that advances their own research interests, and that you have adequate equipment, adequate crewing, and competent personnel, complicated planning and logistics, as well

as operational business. Another man very good at this was Johnny ~~Canades~~ ^{Knauss} (?). In fact, we sort of specialized in producing oceanographic administrators at Scripps. Most of the oceanographic institutions in this country are led by Scripps graduates, or partially led. For example, Art Maxwell at Woods Hole, Johnny ~~Canades~~ ^{Knauss} at Rhode Island, Warren Wooster who just retired from the Rosentiel Laboratory at the University of Miami, Dick Fleming in Seattle, Dale Leifer ^P at Texas A&M. Anyhow, good training for being director of an institution is to lead some expeditions, and I was pretty good at that. I was never much of an administrator ashore, but I was a good administrator at sea. ^P Anyhow, about 1955, I never went to sea anymore. One of the reasons for this was that I decided that we were not treating our graduate students very well. We were, I felt, ~~two~~ two things, and I always have felt one of them. And that is that research and teaching are inseparable, that you just can't do a good job of teaching without research and that you can't do a good job of research without teaching. And if you look at laboratories that are divorced from universities, they're never very good for more than a generation. They don't reproduce themselves. You need to have young people ~~and~~ and you need to have the enthusiasm, the energy, the iconoclasm, and the confidence that young people, young scientists, have. And so, it was quite essential, from my point of view, that we should have good graduate students here at Scripps. But guys like Art Maxwell and Johnny ~~Canades~~ ^{Knauss} and some of the others had a hard time with their doctoral exam. I thought the reason for it was... a lot of them did, not just those two, but those were two outstanding people who had trouble with their doctor's exam. I thought that the reason for it was that they were divorced from their basic sciences. We were 120 miles away from UCLA. It was a killing trip to get up there. The people who came ~~down~~ here intended to stay here and not to take any more courses in physics or chemistry or biology or mathematics or engineering, which are basic to

oceanographic research, or to oceanographic work. Oceanography is not really a science. ~~XO~~ Oceanography is the application of science to an object of study, namely, the ocean ~~and~~ and what lives in the ocean, what's beneath the ocean ~~and~~ and what's above the ocean-- the whole range of phenomena of the watery part of our planet. Therefore, what you need is good physicists, good chemists, and good biologists, who, at the same time, are fascinated by this complex set of phenomena that occur in the ocean. And I think you need people who are... you need at least some of these people to be trained in oceanographic institutions as graduate students. The reason I do is that going to sea is more than just an art--doing science at sea. There's an artistic component to it, but it also takes a certain kind of scientific imagination and scientific planning. And, even more important, every part of the ocean is related to every other part of the ocean. You can't do both ocean and marine physics very well without also knowing something about marine chemistry and marine biology. The story I told you about the snapping shrimp and the deep scattering layer illustrates that very well indeed. You need to have respect for and understanding of pure physical oceanography: what the biologists know and what they're interested in and vice versa. That means that you really need some people to be trained in an oceanographic institution, who have this breadth of view and special kind of experience. On the other hand, they also need to know as much basic science as possible. So it's a tough business to do both those things. And it's essentially impossible when the nearest basic university is 120 miles away. Woods Hole has discovered the same thing. So, I thought of a way to solve this problem, which was to establish down here a graduate school of science and engineering, which I thought of as like Cal Tech. If you look at Cal Tech, it's got mostly graduate students, only got 500 undergraduates and about 1,500 graduate students. The only difference would be this would be a state-supported institution

rather than a privately supported institution. But, in those days, that difference didn't look like it was very much of a difference because everybody was riding high. Prosperity was not only around the corner, it was here with a bang. And the income of the University of California was increasing by leaps and bounds, even though everybody always poormouthed it. And I got a good deal of support from the Chamber of Commerce and Convair and the other industrial people in San Diego. And I brought Rawson Bennett out here, the man I told you about before, who, by this time, was Chief of Naval Research, head of the Office of Naval Research. He made a speech. He had been here before; he was well known in San Diego because his last job before becoming Chief of the Bureau of Naval Research was commander of a naval electronics laboratory, Director of the Naval Electronics Laboratory, which was the old Navy Radio and Sound Laboratory. That's a big job. He was well known. He came out and gave a speech to the local citizens, including the city manager and the mayor and the big wigs of San Diego, and said that the Navy was all for this and would support it vigorously and would support research there. He also said it was a damn good thing for San Diego, which he knew well. It was a very powerful and very effective speech and the people of San Diego wanted this anyhow, so this was just an added bonus. But the main thing that Rawson did was to overcome the opposition of other campuses of the university, particularly UCLA, who were always very jealous of us because we never did much teaching and we got lots of money from the government and we had lots of members of the National Academy and things like that. Not many by that time, but still several, at that time, more than they had at UCLA. The worst blow of all to the UCLA people was when we persuaded Harold Uri (?) to come out here. I'd gotten to know Harold pretty well in Washington in connection with my interest in geochemistry. I went to see him and persuaded him to come to Scripps Institution, having in mind, however, that we would have a school of science and engineering.

About the same time, Jim Arnold came here; Ham Craig, and Hans Suse (?),
~~who were~~ --particularly Hans Suse ^{who} was a great scientist, recognized as one, ---
I guess ~~they~~ ^{Jim} came first, and then Harold. Anyhow, these guys at UCLA
took a very dim view of our getting Uri and insisted that Scrowly (?) not
be a professor at Scripps ~~but~~ a Professor at Large in the university ~~so~~ that
they could claim him, too. Particularly a man named Louis Slickter (?),
who was ^D director of the Institute of Geophysics, felt this way. He said if
I hadn't talked Harold into coming here, he would have come to UCLA. He
might or might not have. But, anyhow, there was a series of committees from
the other campuses that took a very dim view of this school of science and
engineering. The engineers said, "We can do the whole job with a university
extention." One of the reasons for having it was to upgrade the quality of
the engineers and scientists that were working for Rohr (?) and Solar (?)
and Convair and the other high technology industries here. The School of
Engineering at UCLA had a program down here which was designed to do this.
And they said, "Why can't we just expand that?" But Glen Seaborg came down
on one of his committees, and Earnest Lawrence was on that committee, too.
They were from Berkeley, which was far enough away so we weren't a threat.
They always like big ideas, so they enthusiastically backed it. And the
^Pregents voted to establish it. This was about 1956 or '57; I'm confused a
little about the actual detailed sequence of events. The fact was that the
School of Science and Engineering never got off the ground, even though the
city had voted us this 50 acres of land. And the reason was that the
^Pregents got a bigger idea. There was a State Office of Education report,
based upon demographic projections of the future of California, that said
that we were going to have 50 million people here by the year 2000. Those
were the days when the birth rate was right in the middle of the baby boom;
the birth rate was still going up, and people were flocking into California

because of the rise of high technology industry here. All the projections showed the population of California just going up and up, as I say, 50 million people by the end of this century. It was recommended all three segments of public higher education in California should be expanded. Those were the junior colleges which were locally paid for and locally managed by cities, out of city taxes, like public schools but with a contribution from the state; state colleges which offered a full four-year college education to a rather large percentage of high school graduates; and the university which offered not only undergraduate but also graduate teaching and research and had a very restrictive admissions policy, only the top one-eighth of the high school graduates. This admissions policy was formalized by the Coordinating Board of Higher Education in California, composed of four segments--three public segments plus the private college: that the junior colleges would admit every high school graduate, state colleges would admit the top 35 per cent, and the University of California the top twelve and one-half percent, as freshmen. The university would also admit graduates of the junior colleges who were qualified. And they decided, on that basis, that they needed three new campuses with 27,500 students each. And they would also expand Santa Barbara and Riverside and Davis, but they would have these three big, new campuses. And one of these was going to be in northern California and two were in southern California. It seemed logical that one of them would be in San Diego County, one new one in the Los Angeles area (plus UCLA), one new one in northern California. And so they sort of put the School of Science and Engineering on the back burner. They decided to go ahead with a search for sites for these three new campuses. The architecture firm of Perrin and Lupmann got the job of making a survey of where the new campus in San Diego County should be. I thought right from the very beginning it should be as close as possible to Scripps Institution of Oceanography. The alternative would have been to build us a school of science and engineering here, which

they'd agreed to and which we had the land for.

RC: So, as far as you were concerned, then, the campus ought to be located as close to Scripps as possible?

RR: That's right. The alternative, as I say, was to put the School of Science and Engineering here on the 50 acres of land the city had given ^S up and put the university somewhere else, the big new university campus. I had several reasons for thinking this. One was ^{at} that I thought it would be good for the Scripps Institution. Another was that it was a beautiful site, by far the most handsome site in the whole area. And the third was that the existence of the Scripps Institution would give credibility to the whole enterprise. This is a great place, a worldwide known place. It would be easy to recruit faculty members. The great antagonist to this position was one of the ^Regents, a man named Ed Pauley. He just really didn't want to have a campus at San Diego at all. He never actually said that because other ^Regents were pretty enthusiastic about it. From his political point of view, it would have been counter-productive to mount a frontal attack on the new campus. But, he proposed that it be in ^{Bella (John)} Dowbol Park, ~~X~~ Dowbol Park is the crown jewel of San Diego, sort of like Golden Gate Park in San Francisco. The people of San Diego wouldn't give it up for anything, let alone for a new university which they don't understand too well anyway. So, that was just obviously a ploy on his part. His second ploy was...after we'd more or less agreed-- it was not agreed, but I was enthusiastic about it and quite a few other people were--to have it there where it is now ~~X~~. The second ploy was that the airplane noise level would be too high because of Marimar Naval Air Station. My assistant director was a man named Charles Wheelock, Charles de Larma Wheelock, a retired admiral of the Bureau of Ships, a naval instructor, kind of a funny guy to have as associate director of an oceanographic institution because he just got seasick looking at the ocean. That's ^Why he was a naval architect and not a line officer. But he was an absolutely wonderful

man, modest, decent, honest, persuasive, bright, a good guy~~X~~ and I was just awfully lucky to con the Navy into letting me have him, letting him retire early to come out here to Scripps. I'd worked on this for years, getting him, and he was able to talk the people at Marimar into changing their flight pattern. Instead of flying directly over Scripps, as they had been on their takeoff, they turned and made a sharp right turn and flew out over Tory Pines Park, Tory Pines State Canyon, between Tory Pines and Del Mar--which is pretty hard on General Dynamics Laboratories, General Electronics Laboratories, because it flew right over them. But, we then ~~got~~ measured lots of noise levels all over the whole campus, and we showed that they are not really very bad. And we looked up other campuses that were under a flight pattern, like University of Minnesota and University of Arizona and Riverside campus of the University of California. They all seemed to be getting along okay. But Pauley is a very serious fighter when he gets involved with something, and he took the Regents out to his estate on Cocoanut Island in the middle of _____ Bay, in Awahoo (?). About two miles from his house there, his estate, is a marine air station. And he got the commandant of that station to fly a flight of marine fighter planes right over his house at about 200 feet and turn on their afterburners just as they got over his house. This scared the Regents to death, as you can imagine--like an explosion; it's awful. Parrera was a very good guy, Bill Parrera. Charles Lupman was a former president of Lever Brothers--essentially in soap suds. He also had a degree in architecture, I guess, but he was basically a salesman. By this time, they had split up, after this search had been going on for years for a new campus. And Lupmann was being a very political type and, knowing that Pauley was the most powerful Regent, found all sorts of reasons why we shouldn't have the campus here. He said it might cost \$20 million more for noise protection. Unfortunately, he was also the architect for the Scripps

Memorial Hospital, which had a site about two miles closer to Marimar than we were and much closer to the flight pattern than we were. He wrote a letter to the administrator of the hospital saying that airplane noise would be no problem at this new site for the hospital. One of the advantages of living in a place for a long time is you get to know a lot of people pretty well. The administrator gave me a copy of this letter--wasn't confidential--so I gave it to Clark Currey. We had another meeting at Davis; ^{Kenn (ph)} This was about 1960 or '59, I guess. Lupmann got up and told us about all this terrible noise problem, and Clark said, "Well, very interesting, but I have a letter here you wrote to the administrator of the Scripps Hospital that there wouldn't be any noise problem." The upshot was that the Regents voted ~~there~~ there were two upshots. One was that the Regents voted 21-1 to locate the site in La Jolla; ⁺ The other upshot was that I never became ^C Chancellor. It was a pyrrhic victory, as far as I was concerned. I was the Dean of the School of Science and Engineering, this paper organization; I was chief campus officer; I was responsible for making the academic plans for the university and recruiting people for it. We had several ideas. The Regents put all sorts of conditions on having the site here: one of the conditions was they should get 1,200 acres of land, free and clear, from somebody; Another condition was that they ^{re} should be a university town, a university community, built up and planned by the city in cooperation with the Regents, with the office of architects, and engineers of the University. I don't remember what other conditions there were, but those were the principle ones ^u university plans, plan for the Regents, and the land free and clear. We had a campaign, a political campaign, in San Diego, in which the City of San Diego voted to give us something like 800 acres of land, Pueblo land, the same kind of land I told you about before ^u right next to the 180 acres that the Scripps Institution owned. I've forgotten the exact number; maybe it was only 60 acres, quite a

large chunk of land. Then we started to work on the Marine Corps. Charles Wheelock, of course, contributed yeoman service there. The Marine Corps really had to move because they were such a nuisance--there was a Marine Corps rifle range called Camp Matthews. Eventually, the Navy agreed to turn over Camp Matthews to the university. We got 1,200 acres of land, free and clear. The city cooperated handsomely in the planning of the whole region. They got something called University City, the university town planned just east of campus. The second idea was that, besides this first idea that it ought to be here, it ought to be primarily a research university--not necessarily, primarily--with a strong emphasis on research and graduate instruction; and that, therefore, it had to have outstanding research people; and that the way we would do this would be to build up one department at a time to critical mass. We would not start out with an undergraduate faculty covering the waterfront. We would have just one department, if necessary, to begin with, and another one and another one, each one with enough first rate researchers in it to have a good doctoral program and a good chance of getting support for research from the federal government. They would support each other in ideas and in inspiration. That intellectual exchange is quite essential to this experience. Clark Kerr backed that wholeheartedly, so we did just that. I got three people who were brilliant academic recruiters and organizers: Jim Arnold in chemistry, Keith Bruckner in physics, and David Bonner in biology. They were all thrilled with the idea of building a new university and with the idea that we'd do it this way and not simply start from the bottom and build up. Start from the top and build down--build the roof first, as we used to call it. And they all had a whole list of first-rate people that they knew, many of whom were young, not very well known, we thought would be good to have here, and they all went to town and persuaded them. I, of course, did a great deal of this, in the sense that everybody that would come here...we had kind of a routine. We would

talk about our ideas of the new university, and we'd draw diagrams, and we'd talk about the idea. I'll tell you about it in a minute. I'd take people out and show them where the campus was going to be. There was a pile out there left over from the days when it was an Army camp. It was a pillar that supported a roof, and the thing was still standing there. There was a fallen chimney right next to it. I'd get people to stand on top of this fallen chimney ~~X~~ you could see for about two miles in every direction. It was quite a sight. Then we'd always have a party for them with the most interesting people we could find at these parties. Everybody thought this was a dull place, and it was a dull place, but there were some interesting people here. A lot of people got excited about it. And, of course, the more we got, the more we got. It snowballed. One of our prime recruiting grounds was the University of Chicago. The reason for that was that Bob Hutchins, God rest him, had let the neighborhood deteriorate. It had become the center of a black ghetto. These blacks were all pretty rough, very rough. It was dangerous for a white man or a white woman to go out on the streets, even in the daytime, let alone at night. That's one of the principal reasons why Harold Uri came here. And then we got Joel Mayer and his wife, Maria Mayer, who ~~later~~, after she came here, won a Nobel Prize from the University of Chicago, ^{and} ~~and~~ several other people. ~~X~~ Bonner himself came from Yale, and Bruckner from the University of Pennsylvania. Bruckner emphasized mainly solid-state physics. He got half a dozen top people in solid-state physics. That was essentially a new field in those days. Harry Mateus and Harry Sewell and Walter Cohen were three of them, ^a And he got Norman Cole in theoretical physics. He himself is a theoretical nuclear physicist. Most of these people were between 30 and 40 years old. Many of them worked for the Bell labs--that was another prime recruiting ground.

I had a principle: We will not pay anybody a higher salary than the salary

he was getting where he was on the theory that, if you could buy people, somebody else could buy them away from you. That's a high-sounding theory alright. The fact is, the people who came here from the Bell lab just about doubled their salary because they were able to consult here in the summertime. Working for the Bell lab they got \$23,000 a year, which was a pretty good salary in those days. That's all they got. They couldn't consult at all. Here, if we offered them \$23,000, they could earn another \$15,000 or \$20,000 in the summertime. Keith never told me this, but I eventually caught on. Keith was always in favor of paying people as much as you could possibly pay them. This was his principle. It wasn't a question of stealing people or buying them; it was a question that they deserved a high income. They were good physicists, and this is what good physicists should have, a high income. We got some very good people in the earth sciences, very good people in biology, very good people in both chemistry and physics. Proof of the pudding is that, now, 15 years later, something like 45 members of the National Academy of Sciences are on this campus. Only two or three universities in the country have a higher number. Harvard does, and the University of California at Berkeley does, and that's about it. Oh, Rockefeller University. Ed Bronk and I used to compare notes. He was doing what I really would have liked to do, but couldn't do--building up, primarily, a small graduate school. They've got Nobel Prize winners running out of their ears at Rockefeller University. Every other guy you see is a Nobel Prize winner, and they're all members of the National Academy of Sciences. Anyhow, we did pretty well under the circumstances. And the most important idea, without really realizing it, was to find a first-rate librarian. I was pushed hard on that by the people who came from regular universities rather than places like Scripps Institution. They thought a library was very important; and it is very important, terribly important. We had a man from Kansas, a librarian from

the University of Kansas named Mel Foyd, who's done an impossible job of building up a pretty good library here in ~~the~~ 15 years with very inadequate resources. We've got well over a million volumes, about a million and a half. Let's see, it's divided into five different libraries--the central library, the science and engineering library, the Scripps Institution library, the undergraduate library, and the medical-biomedical. Each of these is a model of organization and management and service, plus having the books and the journals. The way I started on this was, when it was planned to have these three new campuses, he volunteered to collect an undergraduate library for all three of them in duplicate. And since he was on the ground and the other two campuses hadn't gotten started yet, we were started because we had Scripps. He did just that. He got 75,000 volumes in triplicate. He gave one to Santa Cruz, one to Irvine, and we have one here. The ^Regents were so impressed with this that for a few years they gave him almost anything he wanted, or the university administration did.

[←] The second chancellor here was a man named John Galbred^{raith}~~red(?)~~. He was an historian and very much aware of the necessity of a research library for the humanists, more so for the humanists than for natural scientists or social scientists. So, he pushed the library very hard. Clark Kerr was his long-time friend and ^{he} got Clark quite sympathetic, too. Unfortunately, Clark got fired. Clark used to say, "I entered my job and ^e left it the same way-- fired with enthusiasm." ^P The idea we used to attract people, and it was a very good idea...we really had three ideas: one of them was we would divide the place up into colleges, not Oxford's type, small colleges, primarily undergraduate teaching institutions, essentially little universities side by side, universities of about 2,000 students; and we planned to have twelve of them--2,000-2,500 students, one-third graduates and two-thirds undergraduates. We could mix it, I thought. The reason for this was that each

college could be semi-autonomous to provide a liberal education in all branches of human thought. And, at the same time, the faculty would be sane enough so they could think and act and not only react, which is the way it was at Berkeley because the faculty had gotten that big. Berkeley has ~~always~~ ^{traditionally} been run by the faculty, but they don't really run it ^{be} cause they never get any ideas put across. It is impossible to experiment with a faculty of 3,000 people, or something not that big, about 2,000. My idea was to have maybe 150-200 people as the faculty of each of these little universities. They would be able to think up their own curriculum and their own requirements and their own programs, and they would be different. You were sure they were run by the right kind of people you get. The first college I thought of as essentially a place to produce graduate students-- in other words, a scholarly place, not just in science, but in humanities, social sciences, as well as in natural science. I thought of the second one as perhaps a place to produce artists and people related to arts, like architects, journalists, writers. The third college would be, perhaps, a graduate school, the school of business administration; the fourth maybe a school of law--undergraduates pushed in that direction towards administration or government. And you could have different sets of departments in each one. Each set of departments would, however, cover the waterfronts of human knowledge. In one, say, you'd have additional biology. This would be in the one that emphasized art because the great motivation for a traditional biologist is beauty, the aesthetics, of organisms. In our first college, our graduate school college, we'd have high-powered, experimental biology. It's a little different when we have ecology, applied biology, leaning towards the government and management. You'd have different kinds of earth sciences, different kinds of physics: all the space physics in one, theoretical physics in another, hydrodynamics in a third, and so on. Well, this didn't work out,

but it sounded good. All professors traditionally deplore departments. They're enthusiastic about multi-disciplinary and inter-disciplinary studies on paper. In fact, they don't give a damn about multi-disciplinary or inter-disciplinary studies; and they love departments, as you know.

RC: Right.

RR: But that's how it was. They thought it would be nice to try it the other way. The second idea was that we should emphasize creativity in the arts rather than criticism. In other words, we'd have musicians, painters, actors, playwrights, stage designers, sculptors, composers, and performing musicians, not people who wrote about what other people did in the arts. In other words, humanities would be our complement of the sciences in the sense that they would be creative rather than critical. One of the reasons for that was that we didn't have much of a library; we couldn't be very critical. The third idea was that we'd mix up the graduates and the undergraduates and they would all...the undergraduates would get into contact with research just as soon as they possibly could because I said and Seymore said and many of us agreed that the object of the college of education is to learn how to learn, not to learn something but to learn how to learn, because times are changing so fast that nothing you learn makes much sense ten years later. So, therefore, we would try to get the undergraduates involved with research and we wanted to get the graduates involved with the breadth of learning, to get them in contact with the humanities and the social sciences, if you're in the natural sciences--not necessarily in courses, but in seminars and informal activities. So we wanted each college to have a mix of graduates and undergraduates. Well, I guess none of these ideas have really worked out exactly. The colleges, as it turned out, turned out to be places primarily different in their undergraduate curriculum. They do have some differences in the undergraduate curriculum, and the students think it's great to belong to a college, as far as I can make out and what the faculty members tell me.

But, as far as the graduates mixing with the undergraduates, that just doesn't happen very much. The departments are university-wide departments. They don't belong to the college.[†] They belong to the university. They serve all the colleges,^{and} they have a life of their own. It is true we have a lot of creative art, not necessarily a good thing, because the creative artists are all wild artists, if you can imagine. The art today amounts to what most people don't understand. The one idea that did work out pretty well, very well, in fact, was the idea of building from the top down and building department by department. And we have a series of very good departments, not necessarily very broad departments. For example, our economists are all econometricians. God knows ^{he} what ^{of} econometricians do in the modern world, but that's what our guys do anyhow. And our psychologists are all experimental psychologists. Nevertheless, they're good. And we have a group of young, enthusiastic political scientists. We have a good group of anthropologists. I think we've failed in linguistics, more or less. I'm not sure we're very good in comparative literature. But, we're very good in basic sciences, pretty good in mathematics, I guess fair in philosophy--I'm not sure about that. Well, I guess the only thing, the thing which really is a sort of permanent relic or permanent residue of these great ideas is the university physical structure--the dormitories and the academic buildings for each college are right together. And we have four of them now: Revelle College; Kerr College, what's called Third College; and what's called Fourth College.

RC: So, in each case, then, the students live in, or right by, the buildings themselves?

RR: Right. And they have mostly the informal student life in their colleges; not always, of course, but the cafeteria, the lounges, the ice cream parlors, the book stores, the sundry stores. One thing that hasn't worked out is the ratio of undergraduates to graduates. We have mostly undergraduates and only

about 1,500 graduates. That's including the medical school and Scripps Institution. ~~And~~ I don't know quite why that is. Certainly, one powerful reason is the lack of academic jobs. Another is that they just don't have money to support them. The tradition is that you support all of your graduate students. They don't have enough teaching assistantships or enough research assistantships to support many more graduate students. But maybe there's something more fundamental. I won't even back this. I've been away 12 years and only been back a few months. I'm a slow learner, and I don't really understand what's happening. The other thing which, I guess, bothers me is that the collegia^{te} spirit which is so characteristic of Harvard--the tradition is that all faculty members help each other out--doesn't exist here. Faculty members do help each other out in the sense that, if I go on a trip, somebody will take my class, sort of like a mother cow takes care of other cow's calves as long as she doesn't have to nurse it. But the intellectual interchange between different parts of the university, ~~it~~ I don't think exists very much. It's more like Berkeley than it is like Harvard. I don't know why that is. There ^{are} some faculty members who are enthusiastic about increasing and improving intellectual and social interchange. These are dedicated, idealistic-type guys, ^a And they include the Chancellor. The Chancellor himself...he's a pretty remote figure. I think if I were chancellor, I'd spend at least 50 per cent of my time wandering around the university talking to people, as I used to do when I was Director of Scripps, which I learned from Columbus Islin, who preceded me as Director of Woods Hole by about ten years. I more or less patterned myself after him in terms of how I behaved with members of the staff, always trying to find out what people were doing, why they were doing it, and being more interested in what they were doing than anybody else. When I was talking to them, they were the chief guy in the world. This is very important, the morale.

RC: Do you think Scripps has grown too large?

RR: I don't know. What do you think?

RC: I don't know. I just wondered. I wonder if there's an optimum place where an institution or a university becomes so large it's difficult to have an interchange of ideas.

RR: Well, I ^{ll} tell you one thing. This is discouraging to me. I was reading about it in the minutes of the Scripps staff meeting. When I was a graduate student, it was ^{or} traditional that we had a staff lunch once a week, at which somebody would talk about his research. Sverdrup kept that up, and I kept it up. I always went myself and sort of made it quite clear that it was what everybody was supposed to do. This custom has gone into abeyance the last four or five years. They tried to ^vrevive it this year as a result of a questionnaire they sent around to the staff. About 100 people responded saying they wanted to have this continued, but only 10-50 of them ever showed up. So, they've abandoned it again, which suggests to me that the place is too large.

RC: Why did you stop teaching full time?

RR: I'm 67 years old. I am teaching full time. But I'm teaching in two places. What do you mean "stop teaching full time"?

RC: Well, I all of a sudden see you drifting more and more to administration. It may be an erroneous picture.

RR: You mean long ago?

RC: Long ago while you're at Scripps. I see you all of a sudden director at two institutions almost simultaneously--Scripps and the University of California at La Jolla. And I wondered if that didn't eliminate you teaching. What was your reaction to that, personally, when that occurred?

RR: Well, I didn't mind it. I was creating something. I'm a good guy at beginning things, not necessarily a good guy at managing them after they're begun.

What I was doing was transforming the Scripps Institution from a small, locally-oriented laboratory to a world-exploring, multi-disciplinary activity. You know, we ended up with 12 ships the last year I was director.

RC: Would you estimate that to be your major accomplishment while director at Scripps, changing Scripps from a localized institution to a ^{may I call it} a "multiversity"? That's a wrong choice of words.

RR: I wouldn't use that word at all. It was a...it became a world institution concerned with the whole ocean, not just with the waters off southern California. If I had an epitaph as Director of Scripps, I would say, "He sent Scripps to sea." I did the same thing at Scripps that--I hope you won't think this is an egocentric comparison, but it illustrates what I'm talking about--~~I did the same thing at Scripps that~~ Prince Henry the Navigator did for Portugal. He made it a world country instead of a little tip on the Iberian Peninsula.

RC: I might add that you chose the same words, too, "age of exploration," when you described what you thought the activities ought to be in the periods of the seventeenth century vis-a-vis the 1950's. Was ^{at} the deliberate?

RR: How's that? I'm...

RC: Well, I'm saying that you and Prince Henry chose the same words. He said "age of exploration" when he talks about his seventeenth century ~~and~~ and you say "age of exploration," in effect, when you talk about your century.

RR: Are you talking about Prince Henry the Navigator?

RC: No. I'm talking about when you referred back to the time that began with Prince Henry and to the time it closes, on down through the seventeenth century.

RR: Oh, I see.

RC: You speak of that in terms of age of exploration.

RR: Right, right.

RC: And I'm saying that, in the decades of the '50's and the early '60's, you

choose to use the same words. Was that deliberate?

RR: That's right. And if you ask a fellow like Walter Munk what kind of a guy I am, he'll say I'm essentially an adventurer, much like these guys that sailed for Prince Henry.

RC: He also calls you a naturalist-- at least that's in the things I read-- which, I assume, is the same sort of thing in this sense, a sort of common interest about the world.

RR: Well, what he means by that is I was interested in and effective at describing things, not very good at experimentation or analysis. I didn't have the mathematical background or the experimental skill. But, when it comes to knowing something about a lot of different things and being able to put it together and asking piercing or penetrating questions, that's something I-- that's always been my specialty--nag. And I helped him a lot, ~~by~~, for example, by asking questions about why is he doing what he's doing, [?] what does it mean?

RC: Do you have any opinion on the relatively large crop of graduate students who are now appearing from the oceanographic institutions in the country? Do you think there are too many coming out, too few? Is this what you visioned for Scripps?

RR: Well, I guess I'm worried about what happened to the astronomers. You know, there are ten astronomers for every job in astronomy. What are all these guys going to do? I don't know.

RC: I don't either. Sometime I'll tell you what historians do. Is a specialized school, such as Texas A&M University, a good way to produce society's professionals, [?] or should the school be more generalized, ~~in~~ in your concept of what you visualized it to be?

RR: Well, I think that, as I said, the basic purpose of a university education should be to learn how to learn, and that's not as simple as it sounds. It means, in the first place, you need to learn the language. I don't mean

French and German; I mean the language of the subject. You need to believe that you can find out something that hasn't been found out before. And you need to think it's fun to find out something that hasn't been found out yet. None of those things say much about breadth. In principle, you could do all three of those things and study nothing but physics. But that's only part of it. Another aspect of learning how to learn is to be interested in things. The way you get interested is to know something about them. At least, that's the way I get interested. And, therefore, I think people need to know something about a lot of different things, enough so that they're interested-- not necessarily enough so they're experts, but enough so that they think it would be nice to know more.

RC: I'd like for the next series of questions to concern theories of international cooperation. You've done much in terms of the spirit of international cooperation. Would you comment, please, on some of these and what you feel about them? What about NORPAC? How did it develop? Your attitude towards it? Do you think international cooperation works?

RR: Well, NORPAC was an idea of Joe Reed, who was a descriptive physical oceanographer at Scripps. His idea was that we ought to get a kind of a photograph of the North Pacific Ocean, the water masses of the North Pacific Ocean, during one season, one year. In order to do that, we had to get a lot of ships out simultaneously. And so, we went to Japan--I went to Japan. I think somebody from the Office of Naval Research went with me. I'm not quite sure about that; I don't quite remember. I worked with the head of the Hydrographic Office in Japan, a nice little old man whose name slips my mind for the moment, and also with the Fisheries people there. And they arranged to have several of their ships take part in it. And the Canadian oceanographic group at Unalaska under Taugie took part, also, and the Bureau of Commercial Fisheries' ships in Hawaii. We were able to produce a kind of

atlas of the North Pacific for that one time. That's something that meteorologists do all the time. They have synoptic weather maps. But it's very difficult in oceanography because it's so much more difficult to make the observations. You can make a map of ocean surface temperature, more or less, at least along the routes that merchant ships travel; but there are many areas of the ocean where there aren't any merchant ships. Of course, they never stop. They never take observations below the surface. I guess this was the first international operation that we were engaged in here at Scripps, although there'd been quite a bit of the same kind of thing in the Atlantic, sponsored by the International Council for the Exploration of the Sea. And the man I told you about yesterday, Buren Helend-Hansen, had been involved with several of these multi-ship expeditions. Then, I guess, the biggest cooperative venture of this kind was the International Indian Ocean Expedition, which was a project first proposed and detailed by the Scientific Committee on Oceanic Research, which was appointed by the International Council of Scientific Unions in 1957, I think. It was called SCOR and still exists. I think the idea came first from Columbus Islin. We had a meeting at Woods Hole, an organizing meeting for SCOR, and I was the president of it. I was the first president of SCOR and presided at this organizing meeting. And I picked the members of SCOR and decided that Columbus would represent the International Geographical Union, and George Deacon, the International Union of Geodesy and Geophysics, and so forth. And the executive board of ICSU appointed these people as representing the different unions. I guess the unions had to be consulted, too. There was a man named Bernike from Germany who was the civilian head of the German Hydrographic Office--it also had a German name. There were people from several European countries. Of course, eventually we had representatives from Australia. The man from Australia, George Humphries, a man from Japan...it was really an international

committee, but representing, not countries, but unions. There was a Russian member, Vladimir Corig, who was their great exploring-type physical oceanographer. I think Ztankievitch was a member. It's easy to find out who the members were; I'm not sure I can give you anywhere near a complete list of the first group. Anyhow, Columbus proposed, at this meeting in Woods Hole, one large area which needed a lot more field work was the Indian Ocean. It was the unknown ocean then, in 1957, in a way that the Atlantic and the Pacific were not. And we all took this up with very great enthusiasm. We appointed a coordinator who was not an oceanographer at all, but a social scientist, who had worked for the Navy for several years as a project officer for the experimental testing unit of the Navy. There's a fleet task force that tested equipment. He was one of their project officers. His name was Bob Snyder. He travelled around the world, drumming up enthusiasm for this big operation that we proposed in the Indian Ocean and getting ships, ship plans, and altogether ended up with about 20 ships going to the Indian Ocean and doing different things-- everybody telling everybody else what they were doing and fitting together where they could. We never had the kind of thing we had in NORPAC, where everybody was out there simultaneously so we could get a picture of the physical oceanography. Rather it was a series of studies of particular phenomena. For example, Johnny ^{Knauer} ~~Canades~~ went out with his ship from Rhode Island and got a couple of our ships involved, too, two of our ships, I guess, to study whether there was an equatorial undercurrent like the Cromwell Current in the Pacific, in the Indian Ocean. The interesting thing about the circulation of the Indian Ocean is that it reverses itself every year. Part of the time the currents go west, and part of the time the currents go east, because of the monsoons, the wind driven current. And, under those circumstances, Johnny's question was whether you would get an equatorial undercurrent. Warren Wooster and Henry Stammel and the English

group concentrated on the Somali current, which was a very fast, deep current on the northeast side of Africa. It's like the Gulf Stream except even stronger and bigger, because it's in low latitudes. The Coriolis force is quite weak. Bob Fisher and Bruce Hazen and quite a few other people concentrated on the bottom, on the geology and geophysics, particularly on the bottom topography and bottom sediments and geophysical measurements of the thickness of sediments and the depths of the mole hole and things like that. We end up ten years later, or five, I guess...early '60's was the most intense concentration of ships there. Australians were involved, and Russians were very involved. We gradually built up quite a bit of cooperation with the Russians, particularly with the geophysicists, a man named Udintsev. We ended up at the end of about five or six years with two results: one, that we knew an awfully lot more about the Indian Ocean than we did before, and the other is that there were a lot of oceanographers interested in it. They've been going there ever since.

RC: Is the approach to oceanography quite similar in all these countries?

I know this is a difficult question to answer, but if you, for example, had to compare Australian, Russian, and American oceanographers, do you use about the same techniques, about the same tools, ask about the same sorts of questions?

RR: Well, the Russians and the Americans do, ⁿ not the Australians. The Russians and the three major oceanographic institutions in this country--Woods Hole, Lamont, and Scripps--are big, multidisciplinary, multipurpose, multiobjective, heavily financed institutions. They send their ships out on these complex expeditions where they try to do all these things at once. That's also the technique the Russians use, and they're concerned basically with the world ocean, not so much with any particular part of it. On the other hand, the Australians have much less effort going into oceanography, much less emphasis

on the continuing operation of big ships and are more concerned with their part of the ocean, the waters around Australia [^] very much more provincial. The British and the Germans and the Japanese have operated much like we have, but they've had a special interest in some parts of the ocean. For example, the British have always had a great interest in the Antarctic and the Japanese in areas where they catch fish or where they hope they might catch fish. The Germans in the, in two areas--fishing North Sea fisheries and the tropical Atlantic. These are scientific interests. ^P The French have... you can't characterize the French very well because there ^{are} several groups in France, one of them lead by Jacques Cousteau. You can't say that any particular kind of oceanography is very typical in France, because they have different groups that have different ways of doing things and different objectives. One group is led by Jacques Cousteau. He's primarily an engineer rather than a scientist. He doesn't claim to be a scientist. He's interested in using underwater photography and underwater vehicles and seeing what you can do with them--developing the capabilities of the devices and developing new devices, new kinds of underwater gear, new kinds of seeing underwater. And, of course, they take beautiful photographs, and they use these underwater vehicles in very many interesting ways. The bathyscaphe, for example...it's a thing that they use, a submarine, or this diving turtle, that they use in more shallow water. Then there's another group that works for the Organization de Reserche Scientifique et Technique autre Mer _____.

They're interested in either the present French overseas possessions or former French overseas possessions, like Madagascar, present French overseas possessions like New Caledonia and Tahiti. One of my good friends is a man named Henri Roche who works for _____ in New Caledonia. He has a ship which does physical and chemical and biological oceanography in the southern tropical Pacific, around the basin, with headquarters at New

Caledonia. Another one, who was also at Scripps, Michael Engo, was stationed for many years at Madagascar, and he was concerned primarily with fisheries. They do just standard oceanography, particularly biological oceanography, but confining themselves to mostly the waters within 1,000 miles of the place where they're stationed. Then there's a third group in the University of Paris and in other universities in France who do either theoretical or quasi-theoretical oceanography. Some of them are very good. Particularly, they've been concerned with hydrodynamics, or the motions of the ocean, and with visible light, the propagation of light in the ocean. They do a lot of more or less classical marine biology in the Mediterranean and the Bay of Biscayne and places like that. There are at least three or four different kinds of oceanography done in France. In general, it hasn't been supported very well, and they haven't done very much to bring new people into it. It's a very small group in France. That's in general true. In fact, oceanography, as a small science, is generally true of most countries. The only countries where it's a big science, in the sense that a lot of money is spent, are the United States and the Soviet Union and, to a lesser extent, the United Kingdom and Japan. Everywhere else they try to do things, more or less, on the cheap. And, of course, most of the less developed countries have very limited facilities and very limited personnel, and the result is that it's hard to do much cooperative oceanography with the less developed countries.

RC: Now, you proposed, one time, that a sort of international foundation for supporting science should be established to encourage science in underdeveloped countries. Whatever happened to that idea?

RR: Well, it's going; it exists. That was not to do with oceanography, supporting individual sciences.

RC: Right.

RR: The headquarters are up near Stockholm. It's called the International

Foundation for Science. They're spending about \$250,000 a year in supporting young scientists in developing countries on practical...research that looks like it has, it would, produce ~~practical~~ practical benefits.

RC: This was proposed in Venice, in 1965, if I remember correctly. When was the foundation organized?

RR: Well, it had its first general assembly last fall. But it's been going now for several years. It's building up the last three or four years under the leadership of the Swedish Academy of Engineering and participation by the American Academy of Arts and Sciences and the National Academy of Sciences. However, the Americans have never put any money into it except a little bit from the Rockefeller Foundation. The money for supporting it, at the present time, comes from the Swedish government, the Canadian government, and Japan and the Netherlands and Belgium, and a little bit from other Scandinavian countries and Germany. One of the things that I've got to do some time, if I possibly can, is to try to find ways of getting American support for it, too. They're supporting ~~of~~ now ~~22~~ about 50 young scientists in developing countries, working on all sorts of various interesting projects that have two characteristics: they're good science, and they also have practical objectives. For example, one of the specialties in Scandinavia is the study of microrisa. Microrisa are fungi that live on the roots of pine trees. And, in fact, pine trees can't live without these symbiotic fungi that absorb nutrients from the soil and transmit them to the roots. It's a rather recent discovery that these things exist; the recent discovery is that they're very widespread. They've been known to exist for 75 years, I guess. Apparently, all tropical plants require these microrisa, not just the pine trees, ~~and~~ ^A and they act as a transmission belt for nutrients so that the nutrients are recycled in the biosphere and don't go through the soil. This is a Scandinavian specialty, as you might imagine. They have lots of pine trees.

They have found young scientists in Malaysia and in Nigeria and in South America and other parts of Africa and other parts of Asia. Each of them is working on microrisa--the physiology, the role that they play in the symbiotic relationship with the tree, and so forth. This has several good things about it. One of them is that these young people have formed a network of their own--they're communicating with each other from one less developed country to another. They didn't even know the others existed before. The other thing is that the Scandinavian scientists monitor this and go out and see them every now and then and help out with ideas and with literature and with equipment and with encouragement, giving them the feeling that they're doing something worth doing, that they're not living in complete isolation. I think it's working out wonderfully well. The only problem is, it's on far too small a scale, but that's a far cry from oceanography. But, in terms of oceanography, we have a very serious problem in the developing countries. Those of us who were involved with forming the Intergovernmental Oceanographic Commission are partly responsible for this, although I'm not sure that I know what else we could have done. The Intergovernmental Oceanographic Commission was formed under the sponsorship of UNESCO about 1960, starting in 1959. Most of the preliminary work was done by a small committee that I was chairman of, a small international committee.

RC: That's the International Advisory Committee on Marine Sciences?

RR: Oh, ^{no,} that was earlier.

RC: That was '57 to '63, okay~~X~~

RR: I think, ^a Actually, the International Advisory Committee on Marine Sciences was much older than that, about 1951 or '52. That was the UNESCO advisory committee. No, this was a special group of experts called together by UNESCO in, I think, 1959. ¹⁾ John Lyman was one of them, and Vladimir Court was another from Russia. I was the American. We made a preliminary plan for the IOC.

Now, the essence of our plan was that any country could join it who wanted to do cooperative research at sea. Well, that was a very narrow definition, unfortunately, because ~~it~~ really said that those countries that could afford to do oceanographic research at sea could join it. And that limited it basically to...it didn't really limit it. It de facto is a club of rich countries: Soviet Union, Germany, France, U.S.^K, Japan, United States, Canada. Italy and Spain were members; the Scandinavian countries were all members, including Finland. They're not rich in the sense that they have big populations with big incomes or big gross national products, but they have a little fat on their bones. They have high per capita income, and they're able to devote considerable effort to science, whereas poor countries like most of the African countries, most of the South American countries, most of the Asian countries, are in such desperate shape just keeping their people alive that they can't really...that oceanography is a luxury for them. Yet they feel that somehow as much of the ocean as possible should belong to them.

^PThis has come out very clearly in these Law of the Sea Conferences. I don't think it's unfair to say that a great many of these less developed countries would just as soon not see oceanography done off their coasts, because they feel that ~~somehow~~, this gives the rich countries an advantage in exploiting what they call "their resources." Since the Law of the Sea Conference is run by politicians and diplomats, with very little concept of what science is all about, ~~the~~ oceanographic science is, I'm afraid, taking a bad fall in that Law of the Sea negotiations. The problem is basically this: the less developed countries, the group of 77, and some developed countries like Canada and Australia want what we oceanographers have called the consent regime, which means that the coastal state must give its consent for any oceanographic research done within its economic zone, however the economic zone may be defined. A convenient definition is 200 miles beyond the

territorial sea, which itself would extend 12 miles from shore. About 40 per cent of the ocean will be in somebody's economic zone. Most of the interesting phenomena in the ocean occur within this 200 mile limit--most of the geology, most of the geophysics, most of the currents, most of the organisms, most of the resources, the water resources. So, this consent regime, which looks innocent enough, will, in fact, maybe destroy oceanography because of the way it's been done in the past. We've had experience with it in the application of the Continental Shelf Convention, an international convention established in 1958 and now come into force, which provides that the coastal state must give its consent for research on the seabed and the subsoil of the continental shelf off its coast, as far out as its continental shelf extends. You know, you write a letter to some of these countries and they don't even reply. And, if they do reply, they not uncommonly deny consent. So, the oceanographers have proposed what we call a regime of rights and obligations. This has slowly evolved over the past 10, 12 years. The right is that anybody can do oceanographic research anywhere outside the territorial sea, provided he lives up to the following obligations: 1) prompt notification of the coastal state that you want to do the research there, 2) coastal states will have the right of full participation in the research at sea (in other words, send one or more people on board the ship. They will have complete access to everything on the ship. One of the reasons for this is to avoid another Phoenix incident, in other words, so as to insure that these ships are not used for espionage), 3) the right of the coastal state and obligation on the part of the researching state is to share all data and samples, give copies of all the data to the coastal state and parts of all the samples, 4) prompt publication in the open scientific literature of the results, 5) the duty on the part of the researching state to help the coastal state in interpreting the results (The coastal states say, "What good are all these data to us? We don't have the scientists to understand what they mean."

and that's a legitimate complaint), 6) and finally--we have not been very enthusiastic about this, but we've agreed, I think--that the coastal state also has the right to object to the research. And if it does object and if it has valid grounds for objection, the research will not be done. The decision as to whether the grounds for objection are valid would be a subject of arbitration, perhaps by a group of experts appointed by UNESCO, by the IOC (Intergovernmental Oceanographic Commission), or in some other way. They've got to be negotiated. And it seems to us oceanographers that this set of rights and obligations protects all of the interests of the coastal states. ^P There have been various alterations of this proposal. The most plausible, and yet the most difficult to use operationally, would be that research related to the resources would be subject to consent of the coastal state; research related to fundamental science would be done on a freer basis. The trouble is, how do you define research related to resources, or how do you define fundamental research? Oceanographers have tried to define it by saying that it shall be published in the open scientific literature; it will not be proprietary in any way. Many of the coastal states in the less developed countries don't want that. They want to keep information private about the waters off their coast. They object, among other reasons, because...well, they have all sorts of objections ~~X~~ ^B but the reason which would have seemed logical to some of our people a few years ago is that, if you find out about waters within 200 miles of the coastline, you get useful military information: how a submarine can hide or how effectively a sonar gear will operate, things like that. I guess that it's not unlikely that this consent regime will be the one that's approved by the Law of the Sea Conference. If so, I think it would be better not to have any treaty at all because this will...well, as far as the ocean research is concerned, it will be a very serious setback to it. ^P One of the things that's been put

in lately, in what's called a single negotiating text, is that the coastal states shall have the right to disallow, prohibit, or prevent publication of the results. Well, you just can't get scientists to work unless they can publish the results of what they do. So, all in all, it will be very damaging. And what it'll do is, it'll tend to turn oceanography towards local problems and towards experimental and theoretical science, rather than descriptive science or international ocean science--international in the sense that the whole ocean is studied. We'd have done better if we'd paid more attention to the less developed countries in the early days of the IOC. The IOC has continually given sort of lip service to helping the less developed countries, but nobody's ever known quite what to do. It's hard to help them in ocean science because it's way down their list of priorities. And, as I say, most of the politicians in these countries feel that science is a rich man's weapon or a rich man's tool, therefore something that doesn't serve their interest.

RC: Have you noticed much conflict between the developed countries in terms of defense industry and oceanographic science?

RR: What do you mean by that?

RC: Well, do you think that, possibly, the concepts of defense and protection interfere with international cooperation, let's say, between the Soviet Union, ourselves, Australia, Japan, for example?

RR: Well, it certainly interferes in some aspects of international cooperation with the Soviets. The Soviets have always taken a dim view of anybody getting too close to their shores. And, for example, in the Arctic, our view is to pretty much stay out of the Soviet sector of the Arctic; and they pretty much stay out of ours, though nowhere near as much as we stay out of theirs. I don't think that, in general, it's a very serious matter for most of the rich countries. It's a serious matter with the poor countries--for

example, with all the South American countries and with India, very serious with India. You asked me earlier on what was the necessity or virtue or benefits of international cooperation. The benefits come mostly in studying the large scale oceanographic phenomena like the Kuroshio, or the Gulf Stream, or the Somali Current, or the equatorial current systems, or, in general, problems that require the operation of several ships at the same time and in a coordinated way. For example, some of the work that's being done under the Global Atmospheric Research Program--the GATE Experiment in the Atlantic--the work that's being done on a network of buoys fairly close together, it's a couple of ^{hundred} 100 miles pouring into an area of a few hundred miles on the side, which has a special name (I can't quite remember the name right now). But work like that, where oceanographers of several countries want to work together, to study a phenomenon that they can't really study with the resources available to any one institution, certainly, it's a good reason for international cooperation. It doesn't have to be worldwide cooperation; it could be cooperation bilaterally between the United States and the United Kingdom, for example. I think one of the most successful examples of international cooperation is the deep-sea drilling program which is now actually being supported financially by six countries--the Soviet Union, the United Kingdom, Germany (I guess it's five countries), ~~Soviet Union~~ United States, and Japan. And the Soviets are putting a million dollars a year into it. So are the English. This is probably the most productive work in geophysics ever done. It's just fantastically productive of new knowledge and new results. ~~It's~~ As you probably know, it has an elaborate advisory structure under someone called JOIDES, the Joint Ocean's Institution Deep-Sea Explorations, Exploratory Studies, or something like that. (I've forgotten what DES stands for.) But Woods Hole, Lamont, Scripps, University of Washington, Texas A&M, Miami, I think Rhode Island, and all these foreign countries are

in it. They planned these things cooperatively and, I think, with a great deal of success. And scientists from all these different countries take part in the expeditions and each leg of the program. There's a different chief scientist from a different institution, maybe French or German or British, Japanese or American or Russian. Then the participants are from a variety of countries. This program is managed by the Scripps Institution, and we have a special department at Scripps that runs it, this deep-sea drilling program. And it's a very expensive operation. This is clearly a case where international cooperation is highly desirable in a funny sort of way--that is, there's one facility, the "Glomar-Challenger," the only ship of its kind in the world, and it's very expensive to operate. And yet, to conserve the interest of scientists from many different countries--just exactly the opposite of the other justification I gave a few minutes ago, where you need facilities from many different countries to study one phenomenon--here you have the possibility of studying many phenomenon, which scientists from many different countries are interested in. But, no one country, except the United States, feels that it can afford to operate the equipment singlehandedly, and we welcome very much the financial participation or the scientific participation of people from other countries. In both the justifications I have given, the cooperation is basically in terms of facilities, facilities belonging to different countries, or facilities belonging to one country but used by many different countries. The third reason for international cooperation is to share results and ideas. This is accomplished by various kinds of scientific unions or other organizations for that purpose. In the case of oceanography, as we said yesterday, oceanography is not really a science; it's a lot of different sciences doing research on an object of study, namely, the ocean, but mainly that part of the earth that is covered in seawater--that is, the air and the solid earth and the water itself and the critters that live in the water. So, there is no international union, a

disciplinary union, that deals specifically with ^{the} ocean. Lots of them--the physicist, the chemist, the biologist, the geologist, the geodesist, and geophysicist--they're all interested in ocean science. So, what you need is an organization of sorts that is orthogonal to the disciplinary unions, an object of study union. That's furnished by the Scientific Committee on Oceanic Research, SCOR, which consists of representatives of the different unions. In each union, there are one or more associations particularly concerned with the ocean--~~not~~ in every union, but in three or four of them. For example, in the International Union of Geodesy and Geophysics, there's the Association of Physical Sciences of the Ocean; in the Union of Biological Sciences, there's the Association for Marine Biology; in the Union of Geological Sciences, there's a section or a commission on submarine geology; and so forth. All these associations come together under the leadership of SCOR. They have a joint oceanographic congress, or International Oceanographic Congress, every three or four years. The next one will be at Edinburgh this fall. There was one in Tokyo two or three years ago and, before that, one in Moscow. The first one was the one that you referred to, which was held at the United Nations headquarters in New York, called the First International Oceanographic Congress. That was the one that was masterminded by Mary Sears. I was the president of it. This was a great event; I thought it was a tremendous occasion ~~X~~ and ~~they~~ ^{IT} they're all very good occasions, these oceanographic congresses. They're the only way in which you can bring together people from all the various sciences that are connected with the ocean that are interested in the ocean, to talk to each other about the ocean and not about their science, not about their particular discipline of science. This also leads to another kind of cooperation, which is scientists working in each other's laboratories or on each other's ships. For example, on our mid Pacific expedition, Henri Rochi, my friend who's now in New Caledonia,

took part in that, and Yudensev, the Soviet geophysicist I've been telling you about, and several of his associates have been out on our ships. We've been out on the Russian ships. There is often an exchange in the laboratory, too, when working out the results. These are very valuable learning experiences on both sides; they're mutually beneficial. I think that...well, I would say several things about international cooperation. One is that it's darn difficult. Therefore, you don't want to do it unless you have to. It's more difficult on a worldwide basis than it is on a regional basis. Cooperation between two countries is much easier than cooperation between three, and the difficulties go up exponentially as the number of countries increases, and the effectiveness goes down. ~~The~~ The sunk (?) is, you have to have many countries. ^RIn my experience in oceanography, international cooperation never works unless the Americans take the lead. There are several reasons for this. One reason is that we have so many scientists. We have about one-third of all of the scientists in all the world. The more important reason is that we're used to cooperation; we're kind of a League of Nations. ~~X~~

The United States all by itself is kind of a League of Nations in the sense that it's so big, and we have so many institutions that have to learn how to cooperate with each other. And we have a tradition of what Detopia (?) called voluntary associations. Remember Detopia said that whenever two or more Americans have an objective, something that they want to accomplish, they form a voluntary association for every conceivable purpose; whereas in France, you find the government taking the lead to push different interests of different objectives; and in England, you'd find some aristocratic leader pushing an objective. In the United States you have voluntary association. These voluntary associations are ways in which we Americans learn how to get things done together. So we have a great advantage in international ^SScientific cooperation because we've had a lot of experience thinking of ways to

cooperate. In any case, I think the record would show that it works best when the Americans are very much involved. Now, one of the aspects of being very much involved is that we don't ever get out in front. You almost never find an American as a president of an association, an international cooperative enterprise, but you very often find him the secretary. This is a part of the technique of doing the job. For example, the first secretary of the UNESCO Office of Oceanography was an American named Warren Wooster, who at that time was at Scripps and later became the director of the Rosentiel Marine Laboratories at the University of Miami and has now just gone to the University of Washington. He's probably the most talented scientific bureaucrat I've ever seen in my life. Remember I mentioned him yesterday and the great expedition he did. He's just very good at administering big scientific enterprises. Then, later, he was secretary and president of SCOR, and that's what really made SCOR work, his very great effectiveness in running it. Another aspect of scientific cooperation is that it just doesn't work if you let politics get involved with it, and this is what, unfortunately, happened to the IOC, the Intergovernmental Oceanographic Commission. It has really been taken over by the diplomats. Its effectiveness is very, very low now. For the first ten years, the delegates were scientists, and the scientists had no trouble agreeing. But once you get the politicians into the act, everything slows down to a grinding halt. I wrote a paper, or I gave a paper, about a year ago, which I call "Ten Commandments for International Cooperation," and what I'm telling you now is some of these commandments. There are several others, but they're all sort of second to the ones I just stated, like, for example, you really want to prepare for it and no meeting really works unless you have it in the right place with the right people, a carefully prepared agenda, and careful attention to everything from housing to food to entertainment to lack of distraction.

For example, we had a very successful meeting organized by Warren Wooster on the ⁵island of Ponza, ^{off}the Italian coast, and the reason it was successful was: (a) it was a beautiful place, (b) they couldn't do anything else but talk oceanography for a week because there was nothing else there.

Well, those are all sorts of experienced details, details learned by experience. So, I'm proud of several things here, but basically I'm proud of the fact that many American oceanographers now are enthusiastic about international scientific cooperation, whereas, to begin with, that was not the case.

RC: Now, what issues did you advise the Secretary of Interior on from '61 to '63?

RR: What issues?

RC: Yes. Why would you be attached to the Secretary of Interior's Office in the sense of a special advisor?

RR: Well, that's a good question, but the answer is not very simple. I was a good friend of Jerry Wiesler, who was President Kennedy's science advisor.

We'd gotten to know each other through the Pugwash and also on various other scientific meetings. So, when Kennedy came into office and Jerry became his special assistant for science and technology, he felt that every department should have what amounted to an assistant Secretary of Science and Technology.

There was one already in the Navy, Jim Wakelin, one of these guys on....

By the way, the name of that Coordinator for Research and Development was Fuhrer. He had been on Admiral Fuhrer's staff and later became assistant Secretary of the Navy for Science and Technology. Jerry was able to get the assistant secretaryship established in the Department of Commerce. Herb Hollowmen took that job. And in the Army and in the Air Force and in some other departments, it was impossible to establish another assistant secretary, but they instead put in this Office of Science Advisor. That was true of HEW, true of Interior, and true of State, the State Department; and, I guess, it was also more or less true of Agriculture. Science

Advisor was a political appointment, but it didn't have to be confirmed by the Senate. The Secretary himself could make it. The title is Special Assistant to the Secretary. Well, Stewart Udall didn't really want a science advisor very much; he didn't know quite what you'd do with one. But he didn't object. He thought it might be a good idea. Well, the Department of the Interior is a collection of baronies, or dukedoms, the heads of which are... all of them are more powerful, more or less, than the Secretary, including the Geological Survey, the Bureau of Reclamation, the Bureau of Land Management. At the same time, the Bureau of Commercial Fisheries, which supports fisheries and wildlife; the National Park Service; The Bureau of Mines; and various offices, particularly the old-line agencies, like the Geological Survey and the Bureau of Reclamation, had developed constituencies in Congress and in the states. So they had enormous continuing strength. I remember very well Floyd Dominey, the Commissioner of Reclamation, a slippery character and really quite arrogantly powerful for a very good reason: he had his congressman eating out of his hand. The under secretary was a man named... when I was there, I guess, it was Carr-(I'm thinking of the one right after him, Carr), Carter. That's it. He was an idealistic politician who took a really dim view of the wheeling and dealing of the Bureau of Reclamation, political plums, and political punishment that they dealt out to congressmen depending on whether they supported them or not. They severely criticized Dominey one time. Dominey said, "If you don't like what I'm doing, why don't you fire me?" Well, the upshot was that a week later Carter was out of a job as under secretary. Dominey was just too powerful. So there was really very little to do, for a science advisor to do, in the Office of the Secretary of Interior. I did some things. I had a very good assistant named Howard Ekelsy. One of the things we did was to get a National Academy study of the National Park Service, which resulted in the firing of the director,

which was a major coup in Stewart's cap, or a feather in his cap, just for this reason I've been outlining--that these guys all had their constituencies, including Conrad Worth, the ^director of the National Park Services. It was clear from a National Academy study that the Park Service really was not doing a very good job. We had another study of solar energy; again I got a National Academy committee to look into the possibilities for solar energy. Hubert Humphrey was pushing very hard for us to establish an Office of Solar Energy, but the trouble was he backed away from getting any congressional authorization or appropriation for it. He wanted us to do it on our own. We had a study made, which spelled out more or less the possibilities for research and development of solar energy. At that time, they were rather dim, and the reason was the price of oil and coal was so much lower than it is now. Now the prospects of solar energy are very much better, but they weren't very good when oil was \$3 a barrel. They don't look very good now, but they look an awful lot better. The other option, which is nuclear energy, worries a lot of people. Another thing I did was to make a study of, or I was in charge of, the study of possibilities of combining nuclear electric power generation with saltwater conversion, in other words, using the low temperature steam for ^vaporat^{ion} seawater and high temperature steam for making electric power. I was a member of the Federal Council of Science and Technology and chairman of their Committee on Water Resources Research. And we actually managed to get an Office of Water Resources Research established in the Department of the Interior, which in turn supports institutions for water resources research in every state in the country, partly on a formula basis, and partly on a ~~grant~~ research-grant basis. You have one in Texas; every state has one. This was a very difficult thing to do because half a dozen departments were interested in water, and every department fights like a wounded tiger for its cognizance and its perogatives. And the

Agriculture Department and the Commerce Department and the Department of Defense--I guess that was all--none of them thought it was a good idea to give this job to Interior. They would rather not have it done at all than have Interior do it. But, it looked like it might have interfered with their prerogative. Fortunately, Orville Freeman and Stewart Udall were good friends. So the Agriculture Department finally agreed to it, and the law was passed. That was, perhaps, the most important long range thing I did for the Department of the Interior. I tried to do something about the Geological Survey, particularly getting them to support more research in colleges and universities, following along the lines of ONR and NSF, but never had any success. They, in general, took a very dim view of the Secretary's office, let alone the science advisor. It was a guy named Tom Knowlin who ran the Survey. Geological Survey people always appear at a government meeting and sign themselves not Department of Interior, but U.S. Geological Survey. They regard themselves as kind of a separate branch of the government. And that's true, in general, of these old-line agencies.

The principle job I did while I was in that position...this was nothing to do with Interior of the United States at all. It was a problem of waterlogging and salinity in the irrigated areas of West Pakistan. On the partition of India--of the two countries--most of the well-irrigated area went to Pakistan. This was in the Punjab, the western part of the Punjab, the part that went to Pakistan, and in the Scin, a province of Pakistan just south of Punjab. For a hundred years, the British had been developing vast irrigation systems in this area, using the waters from the five tributaries of the Punjab: the Jhelum, the Chenab, the Rhabi, the Sutlej, and the Bahas. The word Punjab means five rivers--"Punjab," the same word as "Pentagon" or "Pentapalon." These are all the tributaries of the Indus River which is the great river that rides the Himalayas and flows west, then into the Arabian Sea.

Geologically, it's the whole Indoganjety Plain. The plain from the Indus all the way to the Bay of Bengal is one geological phenomenon formed by the crushing of the Indian subcontinent as it moved north cross the Indian Ocean, came up against the continent of Asia, pushed up the Himalayas, and pushed down this huge trough. And during a good part of the last few million years, the waters of most of that plain flowed westward into the Arabian Sea. Now most of the water flows east into the Bay of Bengal--that's the Ganges River--but about a third of the water flows southwest into the Arabian Sea in the Indus River and its tributaries. British had built what are called barages across the tributaries of the Indus and across the Indus itself. The biggest barage in the world, the biggest single irrigation system in the world, is the great Sucker Barage, which diverts the Indus River into four or five different huge canals in the Scin. There were lots of other barages. And connected to each barage were huge irrigation canals which carried water over what we call doabs, that is, the areas between two of the tributaries. For example, the Upper Barry Doab Canal came off the Rhabi River and irrigated the area between the Rhabi and the Sutlej. There was Upper Chenab Canal that did the same thing--came off the Chenab and irrigated what is called Rechlum Doab between the Chenag and the Rechlum area and so forth-- all together about 25 million acres of irrigated land. That's more than the total irrigated land in the United States. It's all one big system. And after partition, the Indians felt very much ^gaprieved because the waters of these tributaries arrived in India, in Kashmir. And one of the substantive issues in the dispute about Kashmir was the river waters. Finally, in about 1960, the dispute was settled by something called the Indus Waters Treaty which provided that the waters of the three eastern tributaries--the Bahas, the Sutlej, and the Rhabi--would go to India, and the waters of the three western rivers--the Indus, the Jhelum, and the Chenab--would go to Pakistan. This averted a war between the two countries, and the price for averting that

was an enormous change in the irrigation so that the water from the western rivers could be diverted to irrigate the part of Pakistan which had previously been irrigated from the Thabi and the Sutlej. And this consisted of the building of great link-canals between the rivers, and siphons, and dams. Two big dams were contemplated: the Mungadahm Dam on the Jhelum and the Torbela Dam on the Indus. These are just about the largest dams in the world, although they don't hold very much water. (They hold about ten million acre feet of water, at most, whereas the Gleny Canyon Dam holds about 100 million acres of water on the Colorado River.) As part of this project, it was felt that something also had to be done to provide drainage for the irrigated system because something was happening, which always happens in an irrigation system without drainage, and that is that salt was accumulating in the soil and the water table had risen to the point where the low lying areas were what are called waterlogged--that is, water is actually standing in the surface. By far, the most serious of these two things was the accumulation of salt. The way we got involved with it was... you may remember that in 1961, President Kennedy decreed that we would not provide any more arms to Pakistan. President Ihue came over here and made an impassionate speech to Congress about this, and he was quite popular in Congress. He was a very impressive man; he was a big man; he was a petan. He looked like an English general, talks like an English general, and behaved like an English general) but a very intelligent general, not a stupid one by any means--very appealing guy. Kennedy felt that he had to do something for Pakistan, and it had previously been worked out by Wiesner and by Abdul Salon, who is the science advisor to Ihue, (he's a Pakistani physicist, professor at the University of London) that Ihue would suggest that we try to work on this problem of waterlogging and salinity. It was a very distressing and well-publicized problem in Pakistan. Kennedy said that's just the kind of problem my science

advisor can solve." Jerry knew nothing whatever about waterlogging and salinity, so he called on me to get involved with it. I didn't know anything about waterlogging and salinity either, but I was an oceanographer, and I was supposed to know something about salt, saltwater. I organized a task force, a panel, which was called the White House Interior Panel on Waterlogging and Salinity in West Pakistan. I didn't know quite who to get on the panel, but I got a lot of help from a lot of people, including a group of guys at Harvard. Harvey Brooks, at that time, was Wiesner's deputy and vice chairman of the PSAC (the President's Science Advisory Committee). And he and I were old friends. And he said that there was a group at Harvard who had been working on problems of water resources development, particularly an economist named Bob Dorphman and an engineer named Carol Thomas and their students. We put them on the panel. One of the other panel members was Ron Echols, who was at that time President of the Richfield Oil Company, an old friend of mine. Another one was Wadley, who was senior agronomist for USDA. Another was a man from the AID who was a specialist of administration; his name was Blanford, John Blanford. And we had the head of the salinity laboratory at Riverside, Charlie Bower, and one of his good men. We got a bunch of idea men, including a man named Leonard Cass, who was an entrepreneur and inventor in Cambridge; John Isaacs, who was in idea man here at Scripps. And this group went out to Pakistan and looked at the Punjab and at the Scen, made a trip across country, driving across country. I never will forget the day the Charlie Bower looked at a corn plant, looked at the leaves of a corn plant, and said, "The problem with this corn is not that it's got too much salt in the soil, but there isn't any nitrogen in the soil. This plant is lacking in chlorophyll because it has insufficient nitrogen." That was the payoff to the whole problem. Waterlogging and salinity were in a sense a non-problem; the real problem was poor agricultural practice.

The irrigation system had been started in the middle of the 19th century and long before irrigation or agriculture or high yielding agriculture was well understood. They were spreading water too thin on too much land. The plants were starved with water, as well as being starved with nutrients. The reason that they had done that was they were trying to develop subsistence agriculture, and subsistence agriculture depends on every farmer having as much land as he can cultivate. So, they spread out over a lot of land and didn't give enough water to any of them. But, at the same time, the canals leaked, and the whole canal system leaked, and about 40 per cent of the water went down into the underground. So, this represented an enormous additional water supply which could be pumped out and used for irrigation. And it was by far the best way to store the water; storing it underground does not evaporate it very much. It evaporated some when you got the water table close to the surface, and that evaporation was what caused the salt to accumulate at the surface. And the salt problem...it looked bad. The English, in their systematic way, had established villages in all these doabs on a two mile grid--a village every two miles square, a village every two miles. There were large areas where these villages had disappeared, or half disappeared, where the ground had become covered with salt and the farmers had to abandon agriculture. The Pakistanis thought they were losing about 50 thousand acres a year to salt accumulation, and maybe they were. The technical solution was to drill what are called tube wells, which are big wells ~~and~~^{and} powered by machinery, by motors--either electric motors or diesel engines-- to pump out the water, and thereby lower the water table, and then use the water that you pump out for irrigation--use enough water so some of it ran back down into the ground. And in the process of doing that you would wash the salt out of the soil back into the underground. This is not a permanent solution, but it is a solution that will do very well for 20 or 30 years. On a permanent basis, you have to build horizontal drains,

that is, drains that will actually carry as much salt out of the region as is coming into the regions from the rivers. That's not very much, though. The rivers are remarkably fresh, but they do have about 200 parts per million of salt--that's quite a bit of salt year after year. And you have just got to get rid of that salt by carrying it away, even to the Arabian Sea or to the Rogerstan Desert. That's not been done yet, but this vertical drainage that we recommended was well known. If you do it in the way that we recommended, you can actually lower the water table, eliminate waterlogging, wash the salt down, and reclaim the land. But more importantly you've got a lot more water--about 35 percent or 40 per cent more water. This was on a very big scale. The total water supply, including the underground water, is about 100 million acre feet. That's enough to irrigate 25 million acres to a depth of four feet, if it all gets on the irrigated field and doesn't either evaporate or sink into the ground. If it sinks into the ground, you can reclaim it--pump it up and use it--and that's where this 100 million acre feet figure comes from. The problem was that this vertical drainage had been tried on too small a scale. They had put in a few wells and water, underground water, would flow in from the side as fast as you pumped it out, with the result that they were not able to lower the water table. We did a lot of...Harold Thomas and also a guy named Herb Scobiskey of the Geological Survey made models of the underground flow, both digital computer and analogue models. We showed that you had to put wells in an area as big as somewhere between 100 thousand and a million acres or it wouldn't work. You had to drain a large area so you could pump it out faster than the water would flow in from the side) and that depends on the ratio between area and perimeter. You see, if you build a well field in an area of one square mile, you've got four miles of perimeter in one mile of area. In that case, the water flows in from the side as fast as you pump it out. Let's say you have, instead

of one square mile, you have 1600 square miles. Then you have an area of 40 miles on the side, or 160 miles perimeter for 1600 square miles of area, in contrast to the other extreme where I said there were ~~3~~⁴ miles of perimeter for one square mile of area. If you use a scale like that, then there's no problem about pumping it out. And that's what we were able to show with our models. ^{JP} But far more important than that was that this was really not a problem of waterlogging and salinity, but a problem of improving agricultural technology, modernizing the agriculture. This meant using fertilizer; it meant using high yielding varieties, which were just appearing in Mexico at that time; it meant using pesticides; it meant giving instructions and extension services to the farmers, making it worthwhile for the farmers to increase their yields, by stabilizing prices and things like that. We said, and this is the most important thing we said, was that this was one of the great natural resources of the earth. And it could be a garden, could be like the Imperial Valley, but instead of 500 thousand acres, like the Imperial Valley, like 25 million acres--50 times the size of the Imperial Valley. This was a sort of a revolutionary idea to the bureaucrats who were running Pakistan. One of my best friends became a...a man named Bulan Ishak Khan, who was chairman of the Water and Power Development Authority of West Pakistan. He was a member of the elite Civil Service of Pakistan, the CSP, a remarkably able man. He had about as high an IQ as I've ever seen on anybody. He could handle about a dozen of us. We were on one side of the table; he was on the other all by himself. It was no problem. He was so darn popular and so knowledgeable and so intelligent, able to grasp new ideas. What we proposed was--and this was a mistake, but it was a mistake that everybody had made we went along with--that the government should do this job in million acre tracts. That means a tract 1600 miles in area. There would be 25 of these tracts, and they should go one after another in succession, on

the theory that that was about all we could do to really introduce modernized agriculture. Maybe they could do one a year, something like that, on a 25 year program. The only shock objective to this, and rightly so, ^{was} on the basis that politically this was impossible: you had to give some benefit to everybody. The problem was really solved, in spite of both of us, by the farmers of Pakistan who, when they found out about these tube wells, drilled them all by themselves; and they drilled something like 70,000 tube wells in three years, while the government was drilling about 2,000. What none of us had realized was that these farmers were very responsive to change, if they could really see the benefits of the change. I have a saying which I often use that there's no change agent quite as effective as an Indian or Pakistani farmer in hot pursuit of a ruby. It was just fantastic what these people did. Within a few years, agricultural production in Pakistan was increasing by about 6 per cent a year, about twice as fast as the population. The new seeds came in and the fertilizer came in and they developed the water. All of this was really quite a spectacular change. I was back in Pakistan last summer, and agriculture is stagnant in Pakistan again, you know; it's not growing. One of the things I'm very puzzled about is why this is so. Pakistanis have their old complaint that we need more water and we need more drainage. I don't really think that's the problem. I think the problem is the economic and social structure of the country which essentially puts agriculture at the lowest possible priority. The second problem is that most of the land that could now be increasing its production is in quite small farms, farms of about five acres or less. If you look at the economics of it, small farmers like that don't really have much incentive to increase their production. They just can't make enough profit. We need to find ways in which to develop a farming system for small farmers. This is a social, economic, political, technical complex of

of problems which is not well understood by anybody. That's the next big step that needs to be undertaken. I'm going to try to go back to Pakistan this summer and look into this much more thoroughly than I have been able to up to now. At least, up until about 1968 or '69, agriculture looked like a great success story in Pakistan. It was being modernized and the report that we wrote, we called it "Land and Water Development of the Indus Plain," became a kind of a bible called the Revelle Report. It was superseded by a very much more elaborate World Bank study which was published about 1969 or '70, but which essentially came to exactly the same conclusion that we did, in terms of the possibilities for development, but laying much more stress, as they should, on the farmers themselves--helping the farmers help themselves. ^P Of course, in the process of writing this report--we didn't get it done until 1963, long after I had left the Interior and gone back to the University of California--I got to be very good friends with people at Harvard, particularly Dorfman Thomas and Bob Berden and one or two other people there. They'd been planning to organize a center for population study at Harvard, and Harold Thomas suggested my name for the directorship of it. I was really quite unhappy out here. I had two jobs here, and this was impossible. I was Director of Scripps Institution and also University Dean of Research at the same time. University Dean of Research turned out to be a non-job; nobody, but nobody, went to the University Dean of Research. Professors just don't like to have anybody to tell them about what they should do in ^{the} research business. At the same time, I was really neglecting the Scripps Institution. Herb York had resigned as chancellor, and it turned out that they didn't want me to be chancellor. So, I decided I'd go back to Harvard and become head of the Center for Population Studies, which I started actually in 1964, which I just this past summer retired from, ^b Basically because of my interest in resources ~~X~~ the Center had always

contemplated more on matching resources to population rather than thinking about how you reduce population. In other words, we've thought about that, too, a lot; but we've thought in terms of economic and social development rather than in terms of family planning programs. This was ~~a~~ not really a way to win friends and influence people among the population established. They were all hooked on family planning. ^u And, moreover, they're not really much interested in development. But the kind of people who support ^u population center are people like Cornelius Scaphe May of the Melon family of Pittsburg, who basically believes there are too many niggers in the world and is much more concerned about damage to the environment than she is about people. And ^r these are fanatics on the right wing of the population, you know, that say the world is going to hell because of overpopulation. It isn't going to hell because of overpopulation. It may be going to hell, but for other reasons. Our feeling was, and is, that rapid population growth, which is the real problem--more than overpopulation--is as much a symptom of poverty as it is a cause of poverty and that what really needs to be done is to do something about poverty. So, one of our big projects at the Center is a Ford Foundation-supported project for systematic analysis and ^o modeling of resources development in the Indian subcontinent, particularly agricultural water and land development. And we'd been working particularly on the Ganges Plain the last few years. Before that we worked on Bangladesh. We're going to expand it to include Swelanka and Nepal and Pakistan. That's under the direction of one of Harold Thomas' students who's now become a professor named Peter Rogers. He was one of the young men, ~~that worked on~~ ~~our~~, at that time graduate student#, who worked on our Indus Basin Program with Harold. I'm going back this fall primarily to work with Peter ^{*} with this work foundation project. What it consists of is that we bring Indian engineers and economists and government officials to Cambridge in

this period of six months to a year and have them do research on problems of agricultural land and water development.

RC: So you're going back to Harvard, then, to continue this work in the fall?

RR: Right, then come back here again in the spring. After your 66th birthday, you have to retire from administrative jobs at Harvard. You can continue to teach on a halftime basis for four years, till you're 70; I'm 67 now. So, I'm teaching there in the fall semester and here at UCSD in the spring.

RC: When you come back to UCSD, what sort of courses do you offer?

RR: Well, I might.... My new professorship is Professor of Science and Public Policy, which is a kind of a professorship that old profesors ~~don't~~ have, ^{it doesn't} much ^{have} science in it. I taught an undergraduate course in the spring called Technology and the Poor Countries, ^{which} I talked about the kind of stuff we've been talking about. And at Harvard I teach an undergraduate course called Human Populations and Natural Resources. Then I have a group of graduate students who are getting their doctorate degrees, and I take part in three or four other courses in population, courses that are given under the auspices of the Center for Population Studies. But the only course I'm solely responsible for is this big undergraduate course.

RC: I now have a series of questions and comments I'd like for you to make ^{up} on various things that you've done, or things you've said, in your career. First, the Capricorn Expedition. Was the major purpose of the Capricorn Expedition to measure heat loss?

RR: Heat flow, you mean?

RC: Heat flow, rather.

RR: No. Like most of these big expeditions, it had several purposes, but the principal purpose was the geology and geophysics of the deep-sea floor of the Pacific. And we studied that, basically, in four or five different ways. One was the Russ Raitt's main concern. ^T That was the seismic refraction studies which basically gave the depth of the sediments, the depth of what's called

the second layer, and the depth to the Moho (Mohorovitch Discontinuity), and some idea about the sound velocity, or a good idea about the sound velocity of these different layers. The second was the deep-sea sounding, and Bill Menard and Bob Fisher were in charge of that part of the operation. We were particularly interested here in the Tonga Trench, which is one of the great oceanic deeps of the world--maybe even the deepest--although there are four deeps that are just about the same depth: the Mindanao Trench, the Mariana Trench, the Tonga Trench, the Kuril Trench. We wanted to find out what its shape was, what it actually looked like, because Bob Fisher particularly had developed a hypothesis that new trenches would be V-shaped in cross section, have practically no sediments in them, whereas old trenches would have a flat bottom; it would be U-shaped (not necessarily old trenches, but trenches that had very considerable sediment source nearby, would have a flat bottom where the sediments would accumulate in). And, of course, it turned out the Tonga Trench was V-shaped, flat V. The other part of the job of studying the trench was, what were the sediments like and what sediments there were practically all volcanic [?] ~~there~~? And this was, as you might expect, because only the very youngest sediments were still there; the others were being carried down into the crust of the earth. One of the interesting discoveries was the great ~~seamount~~ on the side of the trench, the volcano that'd been carried down into the trench. It was already several hundred meters deep. One of the other interesting things we did was to look at a place called Alexis Bank, which is north of the Fiji islands between the Fiji islands and the Mariana islands, or the Marshall islands, which was a sunken atoll, an atoll that never recovered after the Pleistocene. The top of it is about 100, 200 feet below the surface, and we had divers ^{scuba divers} ~~scuba divers~~ that went down and looked at this. This was just a typical atoll, but one which was never able to build up as fast as the water rose. And the result

was that it was now a bottom atoll. So, another thing that we were interested in was the topography of what was called--what is now called--the Easter~~ly~~ Island Rise, the mid-ocean ridge in the South Pacific which contains all the way into the Gulf of California and California itself. It's really a part of the Easter~~ly~~ Island Rise, separated by what we now call the Pacific Plate~~th~~ from the North American Plate. Another thing was, of course, the measurement of the heat flow. We'd already measured that on the MIDPAC Expedition, the area between the Marshall islands and the Hawaiian islands. We got a lot more measurements on the Capricorn than on other.... Ronald Mason was on board, and his interest was in making magnetic surveys, essentially~~ly~~ towing the magnetometer and then making the magnetic surveys around some of the South Pacific atolls. We were also very much interested in the sediments, and we took ~~a~~ a lot of long cores. And we had a new tool, a new toy, if you will. We had a huge winch that was capable of getting to the bottom of the Tonga Trench. This winch didn't work very well, and it was like a fishing reel--it'd overrun, it'd get out of control, and then the wire would get snarled, badly snarled. This happened to us before we went to Fiji, and we spent quite a bit of time in the Fiji's getting the wire spliced and the winch refitted. It happened again to us in the Tonga Trench, and we had about 30,000 feet of cable over the side. This was a tapered cable, small at the outer end and big at the inner end. Otherwise, the cable wouldn't hold its own weight at those great depths. This was one of the worst days I ever spent in my life. The wire got badly kinked, and so what we had to do was to cut it and remove the kinked portion and then splice it together again. And we had about--I don't know^{U)}-- six or seven tons of weight hanging over the ship and the A-frame on the stern of the ship, which was on the Horizon. The Horizon was the one that had the big winch--

not the Horizon, I'm sorry, the Spencer Baird. This was a two-ship expedition, the Horizon and the Spencer Baird, and it took 24 hours to do this. All the time you were afraid that something was going to ship. But, it didn't, and they put wire clamps on the outer end of the cable and had enough room back of this clamp so that you could splice it, so that you could make a long splice--a very painful, difficult process with a three-quarter inch cable. Anyhow, we did it and brought it out. And we used the winch after that, but quite successfully. That was the last time that the God damn thing fouled up. That was an awful business. We gradually learned in the oceanographic business to handle heavier and heavier weights, more and more massive equipment. And that, of course, the ultimate result of this, is this huge Glomar Challenger, which stores 100 times the height from the bottom. So the sediments, the heat flow, the bottom topography, the magnetic measurements.... Looking at shallow water features like the _____ bank, which is a volcano that gets cleaned off in a few years every time it erupts, and it creates a pile of volcanic debris several 100 feet high--it's an island--and then gets washed away by the waves. And in between eruptions, it's about 50 feet deep, or at least 25 feet deep from the surface. I remember we were very touchy about going out and approaching the _____ islands. We were afraid we might drown. And I remember Art Maxwell coming up to me and saying, "We're aground!" And what had happened was, we'd backed up and we were catching the bubbles from our own weight. But, we weren't aground, thank God! We did a lot of work in the islands, like in the Fiji islands--bottom topography and shallow water. We started out from Bikini, where both ships had been involved in one of the big hydrogen-bomb tests, and then went from there to the Fiji islands, from the Fiji's to the Tonga islands, from the Tongas to Tahiti, and from Tahiti to the Teramoty, from the Tiramotus to the Maranesas, and then

home across the eastern North Pacific. Altogether, it lasted about four months. I know we spent Christmas in the Tonga islands. And we'd have many subsidiary projects. For example, one of the most interesting things about high islands in the Pacific, like some of the Tonga islands, is that there's a notch cut just in the tidal zone: these are limestone islands, and you get an erosion of the limestone in the intertidal zone. The profile of the island would be a cliff like this, cut back like this, and then a reef out here. What in the world cases that nobody really knows what causes it because the ocean is supersaturated, the surface ocean water is supersaturated with calcium carbonate. But apparently the intertidal zone, you have sufficient biological activity, so that the ph is lower part of the time by organic production of CO_2 and organic production of organic acids, which allows the calcium carbonate to dissolve. Walter Munk was on the expedition and Bill Menard, Bill Ridel, Bob Fisher, Russ Raitt, Helen Raitt were on it. (She joined us in Tonga and went the rest of the way with it.) Gustaf Arthenius... I think Andre Croce was onboard, too. I'm sure he was. We had most of the geologists and the geophysicists of Scripps Institution on this expedition. And it was.... We didn't really learn as much from that expedition as we did from MIDPAC, which was the first one of our expeditions; but it was there that Russ Raitt definitely showed that the sediments were very thin, and our heat flow measurements showed that the heat flow was very high. But, we learned a lot about the trench and that was perhaps the most important thing we did. Then there was the fisheries work on the trenches off the American coast--Acapulco Trench, the Peru-Chile Trench--and we learned a good deal about the South Pacific deep-sea sediments. And gradually, over the years, we had a series of expeditions like this. The MIDPAC and the Capricorn were the only two that I lead, but we had a lot of others lead by these other people I was describing yesterday. I think we contributed very fundamentally

to the ideas of seafloor spreading and plate tectonics. I guess the most significant thing that we did, as it turned out, was the work that Ronald Mason did right off the California coast. Using this towed magnetometer that Victor Roche had invented during World War II, he wanted to make a magnetic survey. The Coastal Geodetic Survey was at that time making a topographic survey of the seafloor, up to 200 to 300 miles off the coast of California, and Ron thought it would be a good idea to make a magnetic survey at the same time as this topographic survey. And I thought it would, too, although I didn't quite know why. But it was a new geophysical tool and was bound to show something. What we found when we worked up the data was that the remnant magnetism had a very ~~a~~ remarkable pattern; it had a pattern of long stripes of high and low magnetic field, magnetic ridges that would be a 100 miles long or so, 150 miles long, and when separated by a magnetic valley, if you will, rigid valley structures in the field, a series of these things. And these were cut by what Bill Menard called fracture zones. And they didn't continue across the fracture zones. Later, Victor Bockne and Arthur Roth continued this work, and they^y showed that, in fact, the whole pattern of ridges and troughs were displaced on the two sides of a fracture zone by distances of up to several ~~100~~^{hundred} kilometers, which is obviously a lot. That was really the real confirmation of the ideas of movement of plates of the seafloor. It was later that people in England got the idea that these magnetic stripes were alternate dikes of volcanic rock coming up along the mid-ocean ridges and moving across the ocean floor to the edges of the plates. That's the notion of seafloor spreading which is one of the basic concepts of plate tectonics. The ocean floor was continually being renewed along the mid-ocean ridge and continually moving towards the edges of the ocean basin. ¹¹² Bob Fisher and I wrote a paper which we called "The Trenches of the Pacific," about 1954, I guess, in which we'd analyzed the remarkable features

of the trenches. And it seemed quite clear to us that there must be downward movement under the trenches. And that's what we said, in fact, and the reason was that they had no sediment in them; they had negative gravity anomalies; they had very low heat flow. And this meant that you had a force acting downward to pull the rock down, to cool it off, to pull the heat down, whereas in the mid-ocean ridge there was an upward moving arm of a convection cell believed to flow so that you had a high heat flow over the ridges. And that was what Maxwell had found on a later expedition to the eastern South Pacific. Over the Easter Island Rise there was high heat flow intermediate between the ridge and the trench and very low heat flow over trench or near the trench. Then we wrote a paper--Maxwell, Borg, and I--on the measurements of heat flow, in which we said that we couldn't explain in any other way but by huge convection cells in the mantle rising along the ridges and sinking along the trenches, and that the rock was being carried along the seafloor from the ridge to the trench. But we didn't have the wit to make the connection between this process and this magnetic striping, which, in fact, occurs all over the ocean, all over the deep sea, which is the clearest evidence there is of that movement of the bottom of the ocean floor away from the ridges and toward the trenches, or toward the continental boundary. This, of course, also explains why there's so little sediment. Previously, it had been thought that there were thousands of feet of the deep-sea sediment which had been accumulating since the beginning of geologic time. It turned out there were only about 100 meters of sediment and mud in much of the seafloor, and nobody could say that this had been accumulating since the beginning of geologic time. There just wasn't anywhere near enough of it; something must of been getting our sediments. What was getting them was that they were moving away into the trenches and being carried down into the interior of the earth. And nobody's ever found anything on the ocean floor older than

about mid Cretaceous, about 150 million years old. On our MIDPAC ~~E~~ expedition, we found on the tops of these deep 6,000 feet sea-mounts, shallow water fossils, which were about 100 million years old, late Cretaceous. That was another indication that the ocean floor was very young. All these phenomena had occurred in just the last, really, few minutes of geologic time. I thought for awhile that maybe the explanation for the deep-sea mounts was that the volume of the ocean had increased a lot in the last 100 million years. But I think Bill Menard gave the true explanation, and that was that the sea-mounts formed on the ridges--the volcano^s on the ridge~~s~~--and then moved down the slope from the ridge to the deep-sea. And they just gradually got deeper and deeper as they went further from the ridge.

RC: When you say "increase in the volume of the ocean," is that when you suggested that the Pacific seabed dropped drastically a 100 million years ago--sometimes within the last 100 million years?

RR: I don't think that. That's probably not the correct explanation and certainly not the necessary explanation. I think it's more likely that what happened is the volcano simply moved into deeper water. Their tops were at the surface, cleaned off at the surface, and moved into water that was about 6,000 feet deeper. And their tops were, therefore, now 6,000 feet deep. But they're planed off, retaining the shallow water character, and the atolls were formed the same way, the only difference being that the atolls moved slowly enough so that the coral reef animals could keep up with the sinking, whereas these deep-sea mounts, these geos, apparently moved too fast before the corals could get started or could really maintain themselves. We had a remarkable collection of people who were working on these different aspects of the geology and the geophysics: Russ Raitt and George Shor, Art Maxwell and Vic Von Hertson, Art Roth, Ronald Mason, Victor Bockne, Gustof Arrhenius, Ed Goldberg, Henri Roche, and later Anne Craig and Hans Sus. That's why I say

Hamm (ph)

it was one of the great ages of exploration, because this was done. We really found that the ocean floor is quite different in many ways than the continents are, very fundamental ways different than the continents, which is quite obvious now that we have this idea of seafloor spreading. And on the seafloor, you have half a dozen features that are unique. One of them is the sea-mounts and the atolls, and another is the trenches, another is the mid-ocean ridges. So another is what are called abyssal plains, absolutely flat plains that extend hundreds of miles. These were first discovered by Maurice Ewing in the Atlantic. They exist also in the Pacific. These are due to huge masses of sediment being carried down by density currents from the continental slope and spreading in something like a base surge at Bikini over very wide areas and then settling down. Still another, which is Bill Menard's major discovery, was the fracture zones, which we now know cut across the mid-ocean ridges. The mid-ocean ridges are displaced about every 100 or 200 miles by a fracture zone which was explained by Hugo Wilson. These are transcurrent faults. The problem is, what happens at the end of these things. At the end of them, you have to have sinking or rising of material so they extend all the way to the place where the sinking is occurring--the trenches, in other words. There are a whole lot of them here in the Pacific. Then they were later discovered in the Atlantic, too. The difference in who made the discoveries was basically what ocean you were working on. In the Atlantic, the mid-ocean ridge was a much more clear-cut feature and had this crack down the middle of it, a whole series of cracks in places where the lava was rising. In the Pacific, the Easter Island Rise was the same kind of a feature; but, a much gentler feature, it didn't exhibit these mid-ridge cracks. On the other hand, the fracture zones were much more clearly present than seen in the Pacific, and the trenches are almost, but not quite, entirely a Pacific Ocean phenomenon. These were the basic building blocks of the theory of seafloor spreading

and the theory of plate tectonics. I think the credit for actual putting things together, the synthesis, belongs to a group of people, none of whom are at Scripps: Bob Reeds, who was here as a graduate getting his Ph.D. but later working for NOAA; Harry Hess at Princeton; a couple of guys from Cambridge, England; Matthews, Drummond Matthews; and I can't think of the others. I don't know the number of them, but Matthews was one of them. Neither Ewing, who did a tremendous amount of actual data gathering, nor we really had the imagination to put it together as the seafloor spreaders and plate tectonics people did. But, about...I guess I would say a large part of the essential observations, the measurements, were made here. Ewing also contributed basically two things: one was his hypothesis that the mid-ocean ridge was a continuous phenomenon throughout all the oceans and his idea that there was a crack down the middle of it which was being filled with volcanic material; the other major observation made was the abyssal plain.

RC: No, ^w you once suggested, too, or at least I read and assumed that you'd suggested, that the oceans are the ultimate reciprocal of the waste of land.

RR: They're a big hole in the ground, obviously.

RC: Okay. Does that also apply to radioactive wastes? Twice you've mentioned that in the course of the interview yesterday.

RR: Did I?

RC: Yes. Twice you were explaining that on bombs and wastes, as they were, are being involved in the coming off the atoll...

RR: That was part of our.... What we were interested in there was not disposal of those things, but what happened to them.

RC: And then, ⁿ yesterday, also you mentioned the fact that nuclear energy worries now a lot of people. Now, I assume by that, too, you were speaking of disposal of wastes of nuclear energy.

RR: Well, I'm speaking of several things there. One of them would be the danger

of accidents--nuclear reactions. Another is the disposal of the wastes, which is sometimes called the "Faustian Bargain" because the wastes are not really being prepared for a long, long time and maybe they'll outlast our civilization. So you're putting an awful burden on future generations. The third is what's called the safeguard problem, the problem of plutonium which is made in reactors and can be very easily used to make ^{an} atomic weapon. The one part of that that you're asking me now is, is it possible to dispose of radioactive wastes in the ocean? Walter Munk is investigating a study of this, and he thinks it is possible, essentially by burying them in bottom sediments in such a way that the heat produced by the radioactivity can get out, but the radioactivity can't get out. Whether that's possible or not, I don't know. That's always been a problem--these darn things-- that the fission products are highly radioactive, and the absorption of the radioactive radiation produces heat, and it produces a lot of it. You've ~~to~~ ^{got} to find a way to get rid of that heat and retain the radioactive material, keep ^{ing} the radioactivity away from hurting anybody or anything. Those are sort of incompatible objectives. You have to keep the radioactivity enclosed in something that will absorb all the radioactivity, like a big lead box. That big lead box is also a very poor conductor of heat, so the thing tends to get awfully hot. But, it may very well be that, in the long run, we'll come to the ocean bottom sediments in an undisturbed area as a means of disposing radioactive wastes as an alternative to the present favorite of the atomic energy people, which is old salt mines. Of course, the other alternative is just not to have too many nuclear reactants. That, I must say, I'm in favor of, not having the darn thing, but I now feel that solar energy is a much more rational alternative as an ultimate source of energy. But, ^{one} of the great resources of the ocean not generally appreciated is the fact that it is a great hole in the ground, a way to get rid of a lot of

garbage that human beings produce. And much of the garbage that you can dump in the ocean is actually beneficial. For example, if you dump nutrients like phosphate and nitrate into the ocean, that may actually encourage the growth of organisms that we like to eat like fish and marine invertebrates. It's not at all a one-way proposition; these guys say you can't pollute the ocean at all are, I think, just really not thinking very clearly. The ocean's always been polluted by human activities; and, if we do it right, we're not going to hurt it. We may help it. On the other hand, there's some things that we may be putting in that are harmful--some trace materials, some poisonous chemicals. For awhile, it was thought that DDT was one of these things that ~~it~~ might accumulate at the surfact^e of the ocean because they're soluble in oil, and that that would interfere with the photosynthesis of planktons and thereby may be doing something to the fish production. It seems that DDT doesn't accumulate ~~here~~^{near} the surface very much; it's not clear what does happen to it. But among the things like that that might be harmful to phytoplankton, or the plankton organisms, and to the fish, certainly in shallow waters and in semi-enclosed waters, you can do all sorts of harmful things--like the Minomoto disease in Japan, which was too much methylmercury in shallow waters that got into the fish and people and had very serious complications. And you also have problems with sewage sludge which settles to the bottom and seems to create an environment which very few creatures can live in and change the ecology from the various kinds of wide variety of organisms to a small number of rather disgusting organisms like worms.

RC: Do you think there ought to be some sort of a beginning on ways to control the waste-dumping in the ocean? Would you designate that a nation^sl problem? Should it have national priorities?

RR: Well, we are doing it too much, I think~~X~~ I think it's always a good idea to know what you're doing; it's a good idea to look at these outfalls, these

waste outfalls, to see what happens to the material, whether it's beneficial or harmful. But I'm not at all in agreement with Cousteau who says we're killing the ocean. I don't really think we are killing it. You ought to talk to John Isaacs about that.

RC: ^W Nor I have another quote I'd like for you to comment on. You suggested one time, in some things that you had written, that scientists should be involved in politics. Do you still believe that scientists should be more or less active politically?

RR: Well, when I said involved with politics, I don't think I meant that they should be politicians.

RC: Oh, no, I assumed you meant to influence, as a matter of fact, political decisions.

RR: I think that politicians and scientists have got to learn how to work together. I wrote my presidential address at AAAS. It was called "The Politician and the Scientist." Did you see that?

RC: Yes.

RR: Including nothing I can say that I didn't say there, I meant every word of that, ^{XA} and I basically mean that our world is changing so rapidly because of changing technology and increasing knowledge, its very complex effects, in terms of population growth, resource depletion, changes of ways of life, and the social changes that result from those changes in the ways of life, like urbanization. Politicians need to have as good an understanding as possible, as they can, of what their options are and what the probabilities are. The difficulty here is that politicians and scientists are different kinds of people, and they're quite different in many important respects. But the fundamental difference is that the politician always has to act; he has* to make a decision regardless of whether it's based on knowledge or just based on an intuition, or hunch, or a gamble. He's got to do something--not

doing anything is really the same as doing something, in many cases. The result of this necessity to act and the lack of sufficient information to act on is that politicians always make short-range decisions, and they try to leave the future options open as much as they can--the only thing they can do. That's reinforcement of the democratic society by the fact that the poor bastards have to run for election every two years so that some people say that infinity to a politician is the election after the next one. But it's true of all politicians, even if they're dictators or have some kind of a legitimacy not based on election, just because of the nature of the problems that they face. Whereas the scientist never wants to act, he always wants to find out more before acting, and he always feels that he doesn't know enough to be able to make a positive statement. Well, the way to get around this, I think, is that politicians and scientists both have to learn to think more in statistical terms, in terms of probabilities and ranges of probabilities. Take a simple case: what's the population of the United States going to be in the year 2000? Well, the probability is, let's say, 50% that it'll be 230 million people plus or minus 5 million; the probability is maybe 20% that it will be somewhere between 240 and 220 million people; the probability is maybe 10% that it will be somewhere in between 245 and 210 million people, and so forth. And one has to prepare for this range of probabilities as best one can, not saying that we're going to plan for 230 million people plus or minus 0, but that is the most likely thing to do. So we put most of our effort into preparing for that, less effort (but some effort) in preparing for, more or less in accordance with our range of probabilities. And demographers have to learn to make their forecasts, or their projections, in terms of probabilities; but meteorologists have already learned this. The meteorologists say that the chances are 20% that it's going to rain tomorrow, or they're 80%. Well, they never say 100%

chance it's going to ^{rain}tomorrow, or 0% chance ~~is~~ that it's going to rain tomorrow^w. But everybody concerned about the weather learns to act on these probabilities, and you try to take ~~max~~insurance out against the unlikely event occurring--some kind of insurance, either monetary insurance, alternate plans, or something. Going on a picnic, you usually have to have an alternate plan because you're dealing with probabilities, unless you're Jak Birknis, who's dead now, unfortunately. || But the other thing that politicians and scientists need to learn to work together is to understand how these two different groups of people think. Politicians like to use an adversary procedure because they're mostly lawyers; scientists ~~take~~ a very dim view of adversary procedures. They much prefer committee discussion in which they pound out a kind of a consensus or compromising position. But you don't simply present the arguments of both sides and leave it to the politician to decide between these two sides. And both modes of operation have their place and their value, but they're different methods. Albert Wienberg has talked about this a good deal in what he calls "scientific problems and trans-scientific problems." Trans-science problems involved values that are not scientific: what you like or what you dislike, what you want or what you don't want, what you think is moral and what you think is immoral; ^oNone of these are scientific questions. You can't really argue about a scientific question; you can argue about values. And the reason you can argue about values is that values are always in conflict, and you therefore have to analyze, or to try to give relative weights to, different values. You do this by arguing. Science really is not an arguable subject. It's a subject which you either know or you don't know. Most of the time you don't know, but you do know something. The whole business of technology assessment is a good ~~ex~~ample of the area in which scientists and technologists and politicians need to interact. Another of them deals with policy per science. The politicians

need to be convinced that science operates in a somewhat different way than they're used to. You can't go out and say, "I'm going to make a discovery; I'm going to discover how to cure cancer." and maybe the best way to cure cancer is not to work on cancer at all but to work on the basic biology of metabolism and cell reproduction. And this is hard for the politician to get through his head. He always is liable to say, "What have you done for me lately?" Well, you can't say what you've done for him lately. You've changed the world over a course of a hundred years, but you don't know quite what is going to happen next. This is the essence of scientific approach, very unsatisfactory to the politician, and vice versa. And these are all barriers and difficulties, constraints, in the way of scientists and politicians working together. It doesn't mean there's any less need for them to work together. I'm just repeating what I said in the presidential address.

RC: But you still essentially agree with that, don't you? You've not changed your mind at all? ~~Okay.~~

RR: It was only a couple of years ago.

RC: People's minds change quickly, believe me. In 1958, you said that oceanography was far behind land sciences. Would you still agree with that? Do you think in the last 18 years that that has changed?

RR: Well, Athelstan Spilhaus used to put it pretty well. He said, "We're beginning to know more about the moon's backside than we are about the ocean's bottom." I don't know. The ocean's ^avery much more difficult place to find out about than the land is. On the other hand, I guess, if I ask myself about the problems of land science and ocean science that are more or less related to each other, namely the ecological problems, and problems of climate and weather, the problems of geological processes on land and in the sea, geological and geophysical problems, and consider the relative importance of the two... For example, food production is almost entirely a land science,

only to a minor extent an ocean science. I guess with my broader perspective about agriculture and about food production and about the environment [^] which is, again, for most people a land environment [^] I would say that on the problems that are important to people, practical problems that affect people's lives in fundamental ways, the land sciences are pretty backward, too. We have no idea how to predict climate; we can't predict weather more than about four or five days in advance; we don't really understand how eco-systems work on land any more than we do in the ocean; we don't know what we're doing to the environment on the land in many fundamental and important ways. So, I guess it's comparatively odious in that case. In both areas our ignorance is too large, too great.

RC: Well, the next two questions I have ~~consider~~ consider both of those areas I have listed on my questions to ask you. Both hazard, as a matter of fact, a scientific guess. What do you suppose the development of the oceans in the immediate future--let's say, define the immediate future in the next 50 years--will be? Will we concentrate more on mineral resources, food resources, or will we continue in this area of exploration of the oceans?

RR: I don't think that we will continue with the explorations for a variety of reasons. One reason is the Law of the Sea Conference is going to make it difficult. The other reason is that we've done most of the things, not everything, but most of the things we can with the tools we've had and we understand what we've had. What we need now is to get more understanding of ocean processes. For example, what is the size of the circulation cells in the ocean? It looks as if they're relatively small compared to the, let's say, the Hadley cells in the atmosphere. You can deal with most motions of the ocean on a scale of a couple of ^{hundred} ~~100~~ miles. These big eddies in the Gulfstream Kuroshio are not the size of a planetary eddy in the atmosphere. They have a different scale, and we don't really understand that scale very well.

We need to be able, ~~to do~~ as Walter Munk is thing^King of doing, to make integrated measurements, measurements which tell you ^What the average conditions are for over, let's say, a 50-mile square, 50 miles on the side. One way to do that is to use acoustics, underwater acoustics, statistics of underwater acoustics. Always in the past we've made stop measurements in the ocean, either vertical series or a series of horizontal points--~~we~~ usually a lot of vertical series. But the distances between those vertical series are so large and the intervals in time so great, that the results are very unsatisfactory. We need to be able to measure the state of the ocean all over, or over a large ^S piece of it, within a very short time. You ~~can't~~ do this kind by this standard oceanographic technique of measuring in ^Ibig oceanographic stations, nor can you do it from the satellite. The satellite can't see below the surface but for a few meters. We may be able to do it with a great many buoys, but that's certainly a hard way to do it. Possibly the best way to do it would be with integrating measurements: you have two buoys which measure the sound velocity and the sound scattering and the sound absorption and the refraction ^S of rays on a continuing basis, which give you an average condition on a line between the two buoys in the area between the two buoys, or between two ships--something like that. In other words, we need to be able to get more meaningful measurements with fewer observing points. And until we are able to do that, until we are able to develop a new set of tools, new set of instruments for exploration, I think we might very well be coming to running out our string on exploration with the present tools. ^P And, of course, the third reason is that the people are much more concerned now about the resources of the ocean than they used to be. It looks like we're coming close to the limit of how many fish we can catch. We need to understand the ecology of the fishes, and I mean the ecology, not the life history or the migrations of single species, but how fishes

live in their environments with other organisms much better than we do. We need to understand the location and the means of recovery, the potential reservoirs of oil in the near-shore zone, within 200 miles of shore. We probably need to know more about the distribution and content of the manganese nodules in the deep sea. We need to know more about coastal engineering and about the problems of ports, harbors, and navigation, as ocean traffic builds up. Overseas trade is now at a level that nobody could even imagine 20 years ago. And the big bulk carriers represent various kinds of hazards; they're so hard to maneuver. They need, perhaps, a new system for handling those things--perhaps something like we handle airplanes: rigorously control the position at all times. I would guess that--I'm not necessarily saying I'm in favor of this, but I would think it's likely that--ocean engineering and applied ocean science are going to be more prominent and fundamental scientific research less prominent over the next 50 years. I say this myself with some unhappiness because I think it's an awful lot of fun to find out things and not to use what you find out. One of the things that we may be doing in the future is building large floating installations in the open oceans--maybe cities in the sea, maybe nuclear reactors in the sea (that seems very likely), maybe big pleasure domes offshore. This may be particularly true in some crowded parts of the world, like Java, for example. Maybe you'll just have to extend the land area of Java by putting a lot of it on the oceans. This could even happen with some of the other megalopolises that are near the sea coast, like the Boston or the Portsmouth to Richmond Megalopolis off the east coast. I think it's much more likely to happen off Java than it is off the east coast because our population looks like it's stabilizing. But there may be other reasons for putting installations offshore. That's a host of engineering problems involved with doing all of that. Guys like Bill Nierenberg are interested in getting energy out of the oceans; for example, possibly

taking advantage of the temperature reading between the surface of the deep water, maybe even getting wave energy which is a form of wind energy, but more concentrated than wind energy. You can concentrate the energy from a large into a small area because of the fact that waves lose very little energy as they propagate across the ocean. So that may be a useful thing to look at. Maybe you can use marine organisms like kelp and other sea weeds as a source of energy, turning it into alcohol or methane or something like that.

HP At the present time, the ocean is primarily a source of food protein--in terms of food, I'm not talking about total resources of the ocean; in terms both food for human beings, fish that people like to eat and invertebrates that people like to eat and food for animals, fish meal, to feed chickens and the livestock--and the total protein supply we get this way, in terms of human food, is of the order of 12 million tons a year, 12 to 15 million tons a year of proteing. For the world as a whole, in the protein requirement, the ocean provides about 17% of the source of the world's protein, human protein. It's actually less than the world's protein consumption, but the Americans, Europeans, and Japanese eat a lot more protein. But this is what people require. Well, it's not really a major factor, human protein supply, and it's probably true that we can't get more than a 100 million tons of fish out of the ocean with our present fishing techniques. We've exploited all our resources intelligently and very effectively. Moreover, this would be quite an expensive way to get protein. Deep-sea fishing is an energy-intensive business--it's a lot of energy. So, I think that, in general, the notion that the ocean is a great big reservoir of protein, with present fishing techniques, is just not so (or great reservoir of food supply). Of course, the total requirement right now is something like a billion 500 million tons of food mostly for calories. But, by the development of ocean farming, we might be able to increase this production of things

that people like and need, maybe several fold over. And the interesting thing about ocean farming is that you do it in a different way than you do it on land. The best example is the mussel farming that the Spaniards do. They get a 100 tons to the acre of mussels and maybe 20 tons to the acre of protein, which is just fantastically high. The reason that they're able to do this is that they put their mussels on ropes, or ~~po~~lings, but mostly ropes, I think, in an area where the current constantly sweeps by them so that the ocean is fertilizing itself there. You're taking advantage of the organic production on a large part of the ocean, sweeping by these mussels, and the mussels all they do is just filter it out of the water as it goes by. That way you've got these high yields. And that's the way to do marine farming--not to take a piece of ocean and fertilize it (you might as well do ordinary agriculture for that) but to take advantage of the motions of the ocean, to make the ocean fertilize itself, and to bring nutrients to a small area from which you then get a large crop. And I think that the energy requirements here are rather low. The energy requirements are primarily catching and handling, not so much energy for fertilizer or energy for plowing or energy for pumping water. The ocean will do all that for you. You have to catch the mussels; you have to hang them there; you have to pull them ~~off~~ up and take them out of their shells and do something with them. I would think particularly mariculture of organisms like mussels, oyster, clams.... The mussels, which are filter-feeders...they're just essentially little plankton nets to convert the.... Plankton is something that we like to eat, seafood. Plankton is impossible to eat for a variety of reasons. One thing, it has a lot of silicone, and I wouldn't be surprised if we learn how to do this on a large scale. We could double the protein production of the ocean, which would be a significant step in the right direction. Still, it wouldn't probably be on the 20% or 25% of the total human world protein

population, protein requirements. I don't see how the world population can possibly be stabilized with 10 or 12 million people. Well, so much for mariculture.

RC: I had another question I was going to ask you before we got on the mariculture. It had to do with modifying the climate on the earth. You had mentioned twice ecology, and do you think we can safely experiment, as a matter of fact, with modifying the climate on the earth?

RR: Well, we're doing it. In fact, I'm almost certain we're modifying the climate by adding carbon dioxide to the atmosphere. And, we're adding a lot. We're adding now about a half of one per cent of every year. We're adding a half of one percent of carbon dioxide to the atmosphere, or pretty close to that. So, this is something that we don't want to do; we're just doing it by burning lots of fuels. And the real question is, what's going to happen? It's a great geophysical experiment in one way; it's also maybe a great concern of human beings. Now the ocean enters into this in a profound way. The carbon dioxide production is about one per cent of the atmospheric carbon dioxide every year, going up to three or four per cent maybe in a 100 years, if we keep on burning coal. But, about half of this, at the present time, goes into the ocean, we think. An unknown portion goes into the biosphere, maybe not any goes into the biosphere, but maybe a significant amount (the land biosphere, I mean). The reason that we're able to absorb half of it in the ocean is that the ocean has something called a buffer mechanism of seawater; that is, there are carbon ions in the ocean which have two negative charges that can be turned into bicarbonate if you add CO_2 ; bicarbonate has only one negative charge. As long as the carbonate ion is present, the ocean can absorb CO_2 by transforming the carbon into bicarbonate. However, it looks as if, as time goes on, we're going to use up the carbonate ion in the ocean, and, therefore, the ocean won't be able to absorb very much

of CO₂. Most of it will stay in the atmosphere^X so that will make the situation worse than it is now, ^f For[→] if we do this, we're going to heat up the surface waters of the ocean because the atmosphere will get warmer^X and they won't be able to hold so much carbonic acid gas so more carbon dioxide will get into the air from the ocean, because the ocean will be losing it. Moreover, as the atmosphere heats up and as the ocean surface layer heats up, there will be more evaporation and more cloud formations. This may be a counterveiling effect. No one really knows what the effect will be, whether it will increase the albedo of the earth as a reflectivity--we get less incoming radiation--or whether it will, in fact, increase the temperature ~~in order to get~~ because clouds radiate infrared at a lower temperature than the ground surface does. And maybe that will raise the temperature in order to get enough radiation to make a heat balance. It's quite uncertain what's going to happen. All the climatic models are fatally weak because they're unable to model the ocean, and they're unable to modify the cloud formations of the ocean. We have to learn how to do this, and this will be, I think, a combination of observation and theory. It can't be done by either one alone. You observe cloud formation over the ocean, you observe the albedo of the clouds and their infrared radiation, and see what happens under the different variations we now get, and from this, develop a better theory of cloud formation and a better theory of the interaction between the sea and the air. One thing that's characteristic of the ocean is that it carries about half of the heat from low latitudes to high altitudes; the other half is carried in the air. Nobody knows whether it's 40% or 50% or 60%. That makes a lot of difference in models, in atmospheric models. Present climatological models assume one of two things about the ocean: either that the ocean is a swamp from which water can evaporate but no heat can be absorbed; it's such a thin layer of water; or it has an infinite heat capacity--neither of which is true. The

truth is somewhere in between. And the models just can't take account of that fact. I don't think you're going to be able to get very much further unless you get a lot more studies of the interaction between the sea and the air. Rather than talk about climate modification, I'd like to talk about climate and what's going to happen to it. And the only thing that we can say with any certainty now about climate is that the climate fluctuates--sometimes it's one way and sometimes it's another. And we know that it always has fluctuated. The longer the period of observation, the greater the fluctuation. That's characteristic of what are called stochastic processes. And it's very likely that the climate's going to get worse. It's no more likely that the climate will get better, but it's equally likely it'll get worse. It's going to vary somehow, and we have to be prudent and to act as if it were going to get worse, even though it may not. That means we have to do a lot of things about agriculture. But in order to know what to do about agriculture, we have to know what's going to happen. If climate does get worse, what does that mean? Does it mean that the force latitude shifts north or south, will we get more or less precipitation, does it mean that the pattern of precipitation changes, does it mean that the temperature patterns change, and, if so, in what way are they going to change? Climatology, at the present time, is a very primitive science, and half of climatology is oceanography. So the Global Atmospheric Research Program, which involves oceanography as well as meteorology, is just a very important thing to be doing, from a purely practical point of view, let alone from a theoretical and scientifically interesting point of view. But, you know, the notion that you dam up the Bering Sea, or run the Amur river into the Arctic, or something like that...these are all trivial problems compared to the real problem of what's going to happen to the climate while we're not doing a God damn thing except putting CO₂ into it on a very large scale. Do we have to abandon

carbon dioxide in production? Do we have to abandon the use of fossil fuels because we're going to change the climate so drastically by continuing to use them, or can we continue to use them and prepare for what changes will occur?

RC: That leads directly, in effect, to another question I have. The one thing that strikes me as consistent in your career, if I may use that word, or consistently appear in your career, is the scientist as public man. Do you have any particular philosophical views about what the scientist's responsibility is to the public?

RR: Well, in the first place, you have the same responsibility as everybody else-- to be a good citizen. But you also have special responsibilities because he either knows some thing that other people don't know, or because he has ways of finding out about things that other people don't know about. I think that the scientist has maybe two responsibilities: one is to be entertainer, to be like other artists, to give people pleasure and happiness, and this is particularly obvious with astronomy. People really are fascinated by astronomy, and it has some very deep meaning to people--hard to describe but you can see it when you look at an audience talked to be an astronomer or by somebody who talks about astronomy. They often have tears in their eyes; they are often overwhelmingly emotionally involved with it, and that's the entertainment aspect of science ~~vs~~ seafloor spreadings, plate tectonics, whether there is life on Mars, what's the history of the earth, what's the history of living creatures, what's the diversity of living creatures, all their peculiar and funny ways, interesting ways, what's beneath that intangible curtain that we see out there in front of us--these are all the kinds of things that Courseau does, and many people read. These are very important aspects of a scientist's responsibility to the public. After all, the public's paying for what he does. He ought to get something out of it. The other

thing, of course, is that the scientist has the responsibility of laying the basis for a humane technology, technology which serves the interest of people and doesn't disserve the interests of people. The technologists themselves have a major responsibility here, but most modern technology is based on science, based on knowledge of nature and knowledge of ourselves, the knowledge of other living creatures, biological knowledge, scientific knowledge of living things and of the inanimate world. And so, I think one of the responsibilities of the scientist is to try to consider, think, and talk about--put it in that order--what are the likely consequences of increases in scientific knowledge; and to do that in such a way that you don't scare people, but you give them a chance to make intelligent choices and intelligent decisions. You can't make the decisions for them, but you can give them the knowledge by which they can make better decisions. And then the final thing is to do good science. That's a responsibility, too, because science is something that people can do. Finding out about the world and ~~about the world~~ and about ourselves is an important human activity, something that makes people human, gives them a higher humanity, a more intense experience. Most scientists, of course, feel this very strongly, and I think they're right. But one of the wonderful things about human beings is that they can understand nature, up to a point at least. And we haven't reached that limit yet. We might just move ahead as fast as we can and find out as much as we can. Maybe we will find, eventually, ^{that} one of two things will happen: that science becomes so complicated that it loses its unity and its integrity, and therefore people won't be interested in it. I mean, it's quite possible that we've developed a civilization that couldn't care less about knowing about things. Alternatively, the possibility is that we'll come up against ~~barriers~~ barriers, limitations, of our own minds and our own ability to understand. That isn't in sight yet, but it may be within the next 100 years or so.

I guess that you can think of science sort of like you think about population-- it can't go on forever. The rate at which we're learning things is accelerating. That leads to a discontinuity of some type. You know, an exponential curve is fundamentally an unstable curve that leads to a discontinuity of some kind. It can't go on exponentially. In other words, maybe we'll go like this and level off like that; then maybe we'll go like this and go like that. One of those two things might happen.

RC: Now, I have one final question. In your long and distinguished career, what do you consider your most important accomplishment?

RR: Gosh, I think that's an impossible question to answer. You'll have to ask me that 50 years after I'm dead. I guess the thing that I feel proudest of-- I'm not saying it's the most important, but one that I feel is a high point of my life--was starting the University of California at San Diego. You know, that may not turn out very well after all. Something else might happen more important. But, so far, I think it's turned out pretty well, and that was a great experience, an experience that very few people ever have. It couldn't have happened at any other time in history just like it happened: technology and science were riding high, there was plenty of money, people thought that there were going to be a lot of people in California--everything fitted together. We were just awfully lucky. But, because we were lucky, we had to do something with it that was, in some sense, unique.