ORAL HISTORY DEPARTMENT
TEXAS A&M UNIVERSITY

INTERVIEWEE: John Isaacs

INTERVIEWER: Robert A. Calvert

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RC: Now, first of all, I should like to know what interested you in oceanography.

JI: Specifically, you mean?

RC: Specifically, yes.

JI: Well, my first real involvement in oceanography was during World War II with surf and beaches, working out of the University of California on landing craft, performance of waves, depths. That was my first actual scientific involvement--1945, I guess, '44 or thereabouts. More generally, I'd been interested in oceanography, or in the oceans, quite a long time before that. I was a commercial fisherman for three years off the Oregon coast and always, since childhood, was interested in the oceans and got a great thrill out of fishing in the surfs or off the boat or out of a skiff.

RC: When were you a commercial fisherman?

JI: About 1936 to '39.

RC: I have you at Oregon State University from '30 to '41.

JI: That's about right. It took me about that long to get through.

RC: But I don't have you receiving a degree.

JI: I didn't. I got a degree at Berkeley.

RC: That's right. What did you do at Oregon State University?

JI: Oh, I took things as long as I could stand it and then did something else.

I went into the forestry service, worked in the three seas with the ______________ administration, worked as a logger, went fishing, and then go back to school. Fortunately, I sort of kept up my academic record to a rather reasonably respectible level. It was a good experience because it allowed me to take all kinds of different subjects which I always felt I'd profitted from--engineering, science, literature... much better, I think, in my particular case than going straight through school in some sort of rigorous curriculum.

RC: Was it economic pressure, the Depression, that sent you to these various jobs, or was it just wanderlust?

JI: It was mainly.... Well, it was all three plus my poor handling of my finances at various times, yes--such finances that were possible in those days.

RC: Did you primarily concentrate on science while you were at Oregon State or just....

JI: Yes, science, math...I took quite a bit of literature...history, paleontology, engineering, chemistry, chemical engineering.

RC: Do you think this sort of broad education is necessary for scientists?

JI: I think this kind of broad education is an essential if we're going to solve our problems. We have to have both penetrating minds and broadly comprehensive ones--people who have broad interests. I will take exception to your point on education because I think a person who is broadly interested will educate themselves into these broader areas. But the whole idea that science is going to make its breakthroughs in understanding of the universe and man's position in it from a narrow approach, of course, is balderdash; it's got to have both narrowly penetrating approaches and very broadly comprehensive ones. Science, of course, has capitalized for the last few centuries on taking things apart, and now it's clear the problem is putting things together. And that's going to take people who have looked at all the parts spread around the floor, you see. We are much in the position of the twelve-year-old taking the TV apart on the kitchen floor, you know, without seeing quite how it was together in the first place...in our science.

RC: I notice when you go to Berkeley you major in engineering. That's what your degree is, a B.S. in engineering.

JI: That's right. The fastest degree I could get.

RC: Oh, okay. I started to say, was there such a thing as marine engineering then?

JI: No. Well, there was marine engineering, but that's boat building type of thing--the old fashion marine engineering, yes. No, I took civil.

RC: And expediency steered you in this direction?

JI: No, the reason I had engineering is earlier, just before the war, I ~~had~~ took a job as surveyor on a big naval base and rather rapidly became the chief engineer without any engineering training. And this interested me quite a bit. Several factors.... First, engineering was interesting--which is only applied physics really--and secondly, the fact that I could do that well without any formal engineering training seemed to me that indicates that might be my forte. And so, I thought I could get an engineering degree from the best engineering school. So, I did so.

RC: What were your career goals when you received an engineering degree?

JI: My career goals were to do .... You mean, when I did that?

RC: Yes.

JI: I thought I was going to be an engineer, I truly did. My grandfather had been an engineer in charge of the Harriman Lines, chief engineer for the Harriman Lines; my father had been chief engineer for the Canadian Pacific; and I'd always been told I was going to be an engineer.

I'd always resented it and refused to consider it, and ~~that~~ then, at this time, why, I decided, yes, that's what I'm going to be. Then when I went out looking for a job, they all sounded like being the sectional engineer durge mirror or something like that. I went back and asked Dean O'Brian if I couldn't do some research in the engineering department, and that's where I got started on the waves, beaches, and surfs.

RC: Oh, okay.

JI: That's where I first met Revelle, too, incidentally.

RC: Where, here?

JI: No, on the waves, beaches, and surfs.

RC: Oh, okay, that's what he said. I was shocked all of a sudden when you introduced something that didn't work out. Do you ever regret not taking a higher degree?

JI: Do I ever regret it? No, I don't think so. I'm often told to make sure I never get one because it's a phenomenon to have somebody to not have one, you see.

RC: Do you think the failure to not earn one has limited your career, if I may use that word?

JI: I don't know. I haven't really thought about it a hell of a lot. It's never worried me, anyway.

RC: Let me try it a different way. Does it seem to you as if the movement towards professional degrees since World War II, when, in effect, graduate degrees have become essential to the various academic professions, do you think that's potentially a mistake?

JI: Well, I think I would have progressed farther and accomplished less, or I could have progressed farther by accomplishing less, let me say, if I had had a higher degree there. That a good statement?

RC: Yes. What strikes me is that there are all kinds of historians and scientists who come out of WWII with either no degrees or B.A. degrees, who accomplish a great deal, that end someplace in the '50's as society becomes more technical; in a way, it doesn't seem to be as creative now as it once was. Would you agree with that?

JI: Well, I don't know. The pros and cons about the higher degrees.... There tends to be a document that is an entree, of course. I one time had a student who was, I thought, quite dull; and I tried to persuade him to forget planning to get a higher degree. He said, "You mean all this five years of work is wasted?" and I said, "You mean you didn't learn anything? What were you doing? It can't be wasted." He stopped for a minute and then said the most intelligent thing he has ever said. He says, "You know, Dr. Isaacs, (which was an interesting statement in itself) I'm not just smart enough to get anywhere without a Ph.D." And he wasn't, that's for sure. So, you see, the Ph.D., it seems to me, and the higher degrees have their pros and cons associated with them.

RC: Now, I want to break away from chronology for a second, and I want you to describe and give the significance of these things I've listed.

JI: Good heavens!

RC: Okay. Now, first of all, before I begin, as an engineer, were you asked to solve specialized problems? Is this what accounts for this list of inventions, or, as a matter of fact, did these come to you?

JI: You're telling me now that you're going to give me a list of inventions, I figure.

RC: That's right.

JI: No, I always selected my own problems, as far as I know.

RC: Okay. And people didn't come to you with research problems and say, "How would you, as an engineer, solve this?"

JI: Sometimes.

RC: Sometimes, alright. Well, if that happened....

JI: __________came to me and asked me about the mid-water trawl, how you'd make a mid-water trawl frame.

RC: Well, if that occurred, in the process of mentioning these, would you explain who came to you and what were the circumstances, too, involved?

JI: Sure. Good heavens, that sounds like an endless list.

RC: No, I only listed 12 or 13--not all of them, as a matter of fact. 1945--a device for traversing surf zones and beaches.

JI: I was working then on amphibious craft in several aspects--how in the devil did you get a line out through these tremendous surfs in northern California and Oregon and Washington. Well, you could shoot one out. And I would watch, during the war, boats come in, land on the beach, get broadside swamped and somebody trying to get a line through to pull them off or to help them one way or another; and they get boats that swamp and so forth. So, wouldn't it be nice if you could just send a line out? Wouldn't it be nice if you could put something in the surfs that would take an instrument out there so you could measure the waves or put a rod on it and measure the surfs? So, it's obvious that waves have plenty of power to do this kind of thing. So you make a flap, and you rectify the wave so that it forces it on the out. And you put it together. Nobody came to me on that one, of course.

RC: Does your work with this lead you later on to working with wave power?

JI: Wave power, wave propulsion? Sure, years later. Also, in understanding something about undertow or worrying about sand motion or quite a few things.

RC: Alright. And this began, in effect, from a military project?

JI: Yes, a military project of understanding amphibious craft.

RC: Okay. Would you judge the military, if I may say that, to play a sizeable role in the development and advancement of oceanography in this period?

JI: Of course. ONR, which was Revelle's baby, was the first powerful influence and a very, very valuable one, one of the best sponsors of research that ever existed.

RC: I was also thinking, possibly, about experience gained in things in anti-submarine warfare, for example, and anti-mine activities.

JI: Sure. And a lot of those things have worked out. You really have to look at the environment of the submarine. This requires considerable thought, and just exactly what is the meaning of placing this object in the ocean and its influence on it. Some of that, of course, is classified material, but the mine itself.... The object on the bottom, the manner in which scour takes place, the release of particles, the release of bubbles, the influence of the collection of fish around it, as part of the biological response to this object, all these things are part of beginning to build up a model of how the ocean puts together, even though they're directed towards the specific military need, I think. If you look at them not as a highly specified task, but rather from a broader standpoint of: here's an object within the natural environment, how does it make its presence known, and what are its influences, then, of course, it has not just the military limitation. ONR allowed you to do these sorts of things. ONR, I think, was one of the freest and most valuable types of sponsorship that oceanography has ever had.

RC: Do you think ONR is more mission-oriented now than it was then?

JI: Sure, I think so. I hardly know what it's like; I have very little contact with it. I think it's gotten very mission-oriented.

RC: Is there any particular explanation for that?

JI: Oh, I think the whole United States has never kind of quite recovered from the "more bangs for a buck," or "how many" philosophy--that is, fixed objectives towards what you progress. That never allows for the unexpected to emerge, does it? The unexpected is almost always more valuable than the expected because the expected is something you obviously know can exist, and the unexpected can be unlimited in its importance.

RC: What about the magnesium rod release device in 1947?

JI: Oh, heavens! Well, I was trying to make a pressure gauge for measuring waves in deep water and did so, but I needed to load it and cock it after it got to depths. I needed to open up a reservoir so it flooded with gas, and I needed something that was simple that would release it. And Dean O'Brian said to me, as a matter of fact (Dean O'Brian,the head of the Engineering School of Berkeley), "Why don't you look into magnesium? It makes a good little battery with steel or brass." So I put it together, and it worked fine--just held the valve open on the magnesium wire. And, after an easily calcuable time, it released. And that all led into looking at magnesium batteries and magnesium as a power source.

RC: That's what I wanted to know. Okay, what about the torpedo net in 1947?

JI: Yes, yes, that was, more or less, a specific project that the...and that was one of the few projects I ever worked with the...what was then the Bureau of Yards and Docks, I think, in which they had a great deal of trouble with torpedo nets tearing apart. Well, a torpedo net is a very strange object in that the manner in which stress is distributed through it is complicated. But more particularly, that was the first hint I had of what's more recently developed into several things called, for instance, the break-water, in that these big net floats behave very curiously in the sea. They behave very curiously because they were non-Archimedian bodies, to use the word improperly; that is, they were bodies on the sea surface that had much more displacement than they had mass so they were forced to move in an exaggerated fashion. And this is one of the reasons that the torpedo nets were torn apart, because the floats were driven by something other than the orbital velocities of the waves. And this put them out of phase with the net that they were supporting. This is the first hint I got of.... As you begin to get larger and larger things in the ocean, accelerated forces play the more dominant role than surface forces.

RC: Does this end up later on, then, also involved in the energy project?

JI: Oh, sure.

RC: Does it also end up....

JI: But the principal thing, you see, is something that is a concept which other people have probably understood much better than I have. But, as you go to these larger and larger objects in a varying pressure field, like a wave, the smaller objects are driven by ordinary drag forces--hydrodynamic drag. And then, even though you keep everything symmetrical and you merely scale it up into larger and larger dimensions, there is one aspect, and that's the accelerated forces, that scales on a one-to-one basis; that is, the forces are always equal to the volume--in other words, the dimension to the third power--whereas surface forces go like the dimension squared. And thus, when you begin to get into very large objects, but not too large objects, in the wave field, such as the big legs of the Texas Towers Offshore Drilling Platform, it's clear that they're driven far more by the accelerated forces which act on the total volume of them than by the surface forces. Then you might as well forget the surface forces. And these are aspects that are still forgotten in, for instance, these big boat tech programs where you're starting to put very large, slow moving things in the ocean for ocean thermal energy conversion. But the people, very erudite people at APL and Lockheed, have totally forgotten the fact that they're mainly dealing with accelerations, inertial forces, rather than the ordinary conventional, frictional, hydrodynamic drag. And so they've made some mistakes. See, one thing scales like the square of the dimension, and the other scales like the cube of the dimension. And even though it's a trivial effect in very small dimensions, it becomes the dominant one in very large dimensions since it's going up like the cube. After you've been out in the desert and you've seen these great boulders as big as a room that are way out in the desert somewhere, how did they get there? They were obviously carried there by water. Well, at the moment that a flood wave hits a boulder like that, the crest of the flood wave hits the boulder, the boulder is as easily moved as a sand grain because, you see, it's now, at that moment, subject to volume forces that scale just exactly like its weight. The frictional forces...it can care less. Once the flood flow is established, the boulder sits there like a lead toad, of course. But at that instance when the crest comes, it's as easily moved as a sand grain. So, for an instant, it's moved. Then in succession.... You know how it is out in the desert miles away; you can't conceive of how it got there. This is all a part of the same pheno-nenology, and I think it's an interesting one. And I believe I've capi-talized on it in a couple of cases. That came from the submarine net problem, yes.

RC: Well, what about the thermopile wavemeter?

JI: Well, that's.... These are all very old things. That was a special problem, I think. I wanted to see if we couldn't establish a very simple wave manner measuring device that could be put out to sea simply by a landing craft, dropped by a landing craft, so a beach master, trying to bring landing craft in through the surf, could observe the oncoming waves before they became breakers on a meter, of some sort, on the beach and guide the landing craft into the lower breakers to come to the surf zone. Now, it didn't need to be exact; all it needed to be was relatively correct. He would see big waves coming in, and he'd see the breakers. He'd know they couldn't survive that, and then he'd see a series of lower waves coming in, and he could beckon them in at that point or rector them in over the radio. So, this was the simplest device I could think of--a bag of gas that's adiabati-cally heated and cooled by the wave pressure in there and a simple little thermopile inside, up against conductive seawater on the other side, to make the cold junction, or the reference junction. And it just responded to the waves. He could see the high ones, and he could see the low ones; that's all he had to see. He didn't have to know how high they were. He knew that big ones were producing wave breakers that he didn't want his craft to get in. And he could tell them to retract or to danter at the optimum times; that was the purpose of that whole thing.

RC: Alright. What about the Isaacs-Kidd mid-Water Trawl?

JI: There's a case where Dr. Hubbs came to me. He wanted a net to really rake the ....run deep, fast, and take the bathypelagic fish. Lou Kid and I worked on that. And, I must say, I think we lucked out on that one because it's always been a bit strange to me that we got that net home. We got that diving vane made; we got the bridles cut; we took it out to sea; and it immediately filled beautifully, dove beautifully, went down deep. And we could tow it at three, four thousand meters at reasonable speeds. And even today, if those bridles are cut a few inches different, the damn thing doesn't work. I never understood. You know, we could have been so discouraged we'd have never followed it through. And I see that thing all over the world, you know, and that bridle is right to the same inch, you know. That was pretty much of a thrill, really, in those days. I remember those days quite well. In our first trips out, we would make hauls that would increase the number of specimens of some rare fish in the museums of the world by ~~fifty~~ 50--you know, one haul! When there had only been ten in all the museums in the world, we get ~~five hundred~~ 500 of the things. Then, of course, specimen after specimen was totally unidentified. That was quite a thrill. We made a 20-foot one, one time, and rammed it into the bottom through an accident, more or less...took one of the greatest mid-bottom trawls that anybody ever took--rare brachiopods and glass sponges.

RC: That's probably considered, in terms of oceanography, your most important and famous invention. Would you agree with that?

JI: I wouldn't try to judge it. It certainly has been very broadly spread around the world. And I've kind of suffered from it to some extent, because I go to some outlandish place, and that's what I'm know for, you see--the Isaac-Kidd mid-Water Trawl.

RC: Well, I'm really trying to edge a question into you unfairly, so let me ask you directly. That is, if you should like to name what you consider your major accomplishment, what would you name?

JI: You mean something specific?

RC: Something specific, yes. If you just wanted.... You know, it was always said John Adams wanted such and such carved upon his tombstone. If you just had to be remembered for one thing, what would you like to be remembered for?

JI: Oh, good heavens, I'd hate to be remembered for one thing. Well, I really do think this whole approach of this dynamic breakwater is going to be a considerable breakthrough. I think it's going to revolutionize the whole method of defending ourselves against the destructive influences of waves--beach erosion, harbors. I think it has the potential of allowing the defense of our eroding coastlines, of protecting structures offshore, of making harbors of refuge in the open sea, of protecting small beaches of fishing villages in remote places, in a way that can be afforded so that people can pull their primitive boats out over the beach and get themselves some fish and make swimming beaches in places where surf now makes it essentially impossible, and on and on. It's quite a different approach to harbor protection, for example, than these damn rubber mound breakwaters. And, of course, it's totally the opposite direction. It's interesting to conceive that no matter how long you develop the rubber mound breakwater, you'd never come up with the idea of putting the lightest possible thing in the ocean.

It takes a 180-degree shift to do that, and it works on a different principle. I think maybe that's it. I expect to see the iceberg really solve some problems. I think the Israelis would do it now. They don't really have the money, but they would do it, except the minimum iceberg they could bring is too big. It covers the whole damn state with three feet of water. The Arabs are going to do it, or so have announced; and it certainly is a natural one for them. The U.S. is too fouled up, I think, to do anything quite so dramatic.

RC: First of all, now, about breakwater. What do you mean, it's an altogether different principle, or principle conceived of differently, than it had been in the past?

JI: Well, in the past, the whole matter of defending, let's say, a coast or harbor against waves has been to pile in a lot of rock. This does some absorption and some reflection; it interferes with sand flow; it can be disastrous; it's reversible;there's no way to really tear it down once it's built. We see these kind of disasters all around the world where it's set up waves of beach erosion. This is a static approach of sitting up there obstructing the waves by putting in massive chunks of rock, sometimes wired together, cemented together. This is a much more subtle way of destroying waves, which is to set something that causes the waves to destroy themselves. It's driven by the pressure field and acts against the orbits of the wave. And what you want is the least dense material rather than the most dense material. You want it to have the greatest freedom of motion rather than the least freedom of motion. And it truly degrades the waves into eddies rather than reflecting it to a great extent, you see. This is why the approaches to some coasts that the breakwaters are rather dangerous, because you have these highly reflected waves. So, it's a totally different principle. But I don't really limit it to that. The whole business of intervening into the phase of the wave rather than the orbits of the wave is quite a different approach. You have linked yourself into something that is, in principle, much more energetic. That isn't quite the right way to say it, but rather that the velocities involved are much higher than they are. The rate at which the wave form is going across the ocean is much higher than the rate at which the water is moving. Even Leonardo da Vinci understood that quite well. What I mean... Leonardo da Vinci understood that one quite well. But, you see, if you can link things into this rapid motion of the velocity of the wave form across the ocean so there's a magnitude greater than the actual water motion....Most things, like a rubber mound breakwater, intervene with the orbital motion, the smaller motion. This intervenes into the wave form itself. Now, if you can link that in so that you get energy out also, then you've done a much more effective job. You ordinarily only see this kind of linkage in the case of surf boarding, for example. And then it's much degraded because it's been much slowed down. But the velocities involved, you can already see, are very high. But if you can do this in the open sea, you see, you're talking about velocities of 40 or 50 miles an hour, if you've linked to the pressure gradient.

RC: I know one can't say when an idea first came to him along this way. I realize that, but, roughly, when did you begin to work on the problem in terms of....

JI: Which one?

RC: The problem of...

JI: The breakwater?

RC: The breakwater problem.

JI: Well, I had always had this in mind, more or less, in a kind of a vague, unformed, and diaphanous way, since I'd worked on the torpedo net problem.

Every time I saw buoys that were partly hauled down, I was always impressed by their tremendously erratic motion. And then somehow I was talking about it in class, and I said, "Why don't we just really see.... Why doesn't someone take the project of really seeing how one of these spheres behaves and how much energy from the wave it really does dissipate?" And one of the kids in the class took it up and looked at it and decided, yes, it was out of phase and did dissipate 30 or 40 times the energy that it would if it were held rigidly. And then he dropped it, and another kid took it up and really designed it through. And he is now...has his Ph.D. and is working for the state, building these things for the state of California, investigating the full potential. So, how do you exactly say when such a thing comes up? You see, I began to understand some years ago when we were working with deep-sea moorings where we made some deep-sea moorings that survived out in the Pacific three or four years in more than 3,000 fathoms of water. One of the reasons these survived was that they did behave something in this fashion, and this allowed them in a storm to dodge the high waves--they moved around the high waves rather than over them. See, in a storm, the sea is kind of a stochastic topography of the rapidly moving crests, of short crests. And if you now put a buoy out there that is hauled down strongly and doesn't weigh much, it threads its way between these crests in a complicated pattern. So it never gets hit by the high waves. It dodges them automatically. And it's clear this energy is coming from the waves and that it's considerably more than just if the buoy were floating there and the waves were dragging against it. So all of this sort of fits together, and you finally decide, well, maybe this is the real way to go.

RC: So, in other words, a whole combination of ideas, you would argue in terms of science, experimenting with both small things and large things, drifts you into handling big problems. And that's poorly put. Let my try it this way:

Is it that you begin dealing with specifics and then, all of a sudden, I look up and you're....

JI: And you observe something that's irrelevant, perhaps, to the specifics you're looking at. And you start that-a-way. And it's an experience.

RC: Now, like the iceberg, for example.

JI: The other day, for instance, down on the beaches in southern California, in Baja California, there are loads of sand dollars. And the sand is eroding by wind there on the beach, in the dry beach; and the sand is being driven very rapidly. And these sand dollars, all of them, are slowly tilted up on little pinnacles of sand and take an attitude very much like the living organism does in the ocean. And now that's strange. You look at 100 to 200 of these things all standing up at the same angle with a little pinnacle of sand behind them, holding them in that position. And this is much the position, without the pinnacle of sand, that you see them in the actual ocean. And you begin to wonder how much of this is mediated through just the hydrodynamic shape of the sand dollar, you see--all facing into the wind, all with their top sides towards the winds, all at about almost 60 degrees inclination, all of them sitting there--an array of these. And sooner or later, I get a chance to maybe do some experiments on various sorts of creatures that live in a high, strong wave current regime, like sand dollars and clams and what not. Is it their shape, a particular hydrodynamic shape, that gives it desirable properties, allows it to bury itself or something, or is this organically mediated through musculature? Interesting, isn't it? So I think all kinds of little observations fit together sometimes. And this is a case of the breakwater point where loads of small observations lead you in some direction or another.

RC: I was also thinking about the concept of bringing down icebergs to furnish freshwater.

JI: How'd that occur to me?

RC: Well, I want to know both how it occurred to you, and then I want your reactions.

JI: I remember how it occurred to me exactly. There was quite a bit of interest many years ago--and it keeps coming back periodically--of an underwater pipeline for southern California to get its water from, God knows, Oregon, Washington, British Columbia, Alaska, whatnot--stupid idea in my way of thinking. I mean, you don't know what's going on down there well enough. There are landslides, there are instabilities, there are all kinds of things--ships dropping their anchors and on and on. But, nevertheless, you can make the following calculation: let's say, California needs five million acre feet of water a year. Okay, you can start to optimize this; you can make a pipeline. Now the power to drive that water down that pipeline doesn't come free. Even though you build a dam up there somewhere, you could have made electricity out of it, you see, rather than driving water. So you think, well, I will optimize it. You very graphically realize that presently optimization just from the standpoint of loss, of frictional loss, comes to bigger and bigger pipelines. And presently you have a pipeline that has all the five million acre feet in it. Well, now you might as well shirk it up more and just roll it into the ocean and tow it down. Now you begin to optimize that. It's a different concept. Well, I was preparing very early in my days in Scripps, was preparing a talk about the water problem, and one of the things I wanted to look at was the pipeline. And when I optimized this thing, I came out with something that was half a mile in diameter and five miles long, or some such a thing, and it was optimum towing size. And I thought, "Jesus, such things exist. They're secondary tabular icebergs of the South Atlantic. Let's see how these go. What would be the melting rate? What would be the thing?" And I made some calculations. And, of course, you come up with this interesting case that to tow them north from Anarctica, you really tow them to the east like mad because of their rotation and their Coriolis Force, if you wish. And so these more recent studies by the Rann Corporation and so on--rather thorough studies--go into this, go into the Coriolis Force effect in a rather erudite fashion. But, of course, the simplest concept in the world gives you exactly the same answer. It just says you've got to get that iceberg going 1,000 miles an hour to get it (not in respect to the water, but in respect to a fixed reference point) over the equator, and you've got to tow it to the east to do this. You've got to get it going a 1,000 miles an hour to the east--to the west, sorry--you've got to get it going a 1,000 miles an hour to the west to get it over the equator. And you've got to tow, essentially, westwardly. That's correct, isn't it? Good God, let's see. I always have to go through this thing. You tow it to the east, good heavens, yes. I was rotated there for a second. You have to tow it to the east. You have to get it going a 1,000 miles an hour to bring it essentially to the velocity of the earth.

RC: When I first remember reading about concepts of pulling icebergs to southern California, now, I remember scientists making fun, in fact, of sort of "jiving" this concept, if I may use that word. How do you feel as if, when you introduced these ideas that long ago, that seems to be reaffirmed here in the present, in terms of water shortages and ecological clashes?

JI; Well, I don't think they'll ever do it in California.

RC: You don't think they'll ever do it.

JI: No, some other country will do it. It's too outlandish. The United States has lost its sense of adventure. It will examine that and examine until hell froze over. Worry about fogs and worry about freshwater influences and worry about this and that; never try it.

RC: What do you attribute the loss of adventure to?

JI: Oh, I guess sort of a degree of senescence, I would guess. I don't know. You could erect a thousand theories on this, I think. We started out with a great deal of hybrid vigor in the United States. We had people of all sorts of social proclivities mixing together. Then, to some degree, they don't mix anymore, you see. And I believe in heterosis; that is, the strength of hybridization. And it only goes a generation or two, don't forget, you know. Well, that may be it, or it may be we're in the same position of the person on Easy Street--keep your finger on your number, as it were; don't take any chances. There are the real concerns of the environmentalists, of course. You have to look at these things, and they're real. You can't kill off things in vast numbers. But we've gone overboard on that. It's a pendulum swinging problem. It may swing back again, but I think that we can't do this sort of thing anymore in the United States. We can't. Such approach takes the agreement of a lot of people that you do need the water, you're going to do it; you're going to have to take some environmental losses. You try and minimize those. And it's too controversial. The congressman has to take the path of least irritation rather than the path of rationality.

RC: As long as I have you here on the environment, I had some questions to ask you later. May I ask them now? Nuclear power.

JI: Carry on.

RC: You've just been through a referendum here in California. Do you thing referendums on nuclear power are essentially good things?

JI: Referendums in which way? You mean, for or against?

RC: Against or for. Do you think the public ought to make these kinds of decisions concerning the environment? That's what I'm trying to find out.

JI: Well, of course, in the United States we've fallen into the advocative position, and our decisions are made by a series of advocates. This is kind of a long story. I don't want to get into it too deeply, but there's obviously a disparity between the scientific approach and the approach of advocacy. The lawyer picks only that evidence that supports his position. That's the way the advocate does. The scientist is forced into this system, then, to advise. And the advocative system says you're either with us or against us and no intermediate way. The scientist is unable to express his reservations; he's got to be gung ho. He's got to be in that direction or against that direction, one or the other--no if's, and's, or but's. And this is essentially an anathema to a scientist who really wants to evaluate things. He can't be all for or all against. This is why the budding scientist has such a hell of a problem with true-false questions. There aren't any true-false questions that are answered true and false. There are always reservations one way or another. He either balances the reservations, in which he probably fails, or he knows what the hell stupid answer the instructor really thinks is correct. And that's the guy that makes it. But, you see, once he gets out into the real world and is asked for advice, he really can't do that and preserve this scientific equilibrium. And so I think we get into this position of the scientist being essentially in a hopeless posture as far as advising the public. The public can't see the uncertainties; and if he expresses any uncertainties, then he's against it or whatever. Well, in the case of nuclear power, it's a long argument. There are risks to everything, obviously. I was out in the Pacific and saw the explosion of the great H-bombs, many of them--28 nuclear shots I've seen. I've seen a vast amount of radioactivity thrown into the Coral atolls. The effect of it has been almost unobservable. The ocean has an immense resiliency and capacity. Exactly what a major spill would be from a nuclear power plant, or a failure of containment of fuel... I really haven't looked into it. Clearly you'd have some areas that would be off limits. But it can't be a disaster, you see; it can't be a vast disaster. But, of course, you see.... Then again we're up against other problems of the utilities always planned for growth of existing areas. So we have this self-fulfilling forecast in which you want the power plant to supply power for the city of Los Angeles because the city of Los Angeles is going to grow. And then that's the only place that it can grow, because that's the only place there's power, and so on--you know, the idiotic situation we're in. I would like to see power sources put in places where there ~~aren't~~ isn't anybody. Then they might live there instead of.... Well, if they all want to live in Los Angeles, that's fine with me. It leaves other places a little freer. But the number of forces that tend to concentrate us into these areas are insensate forces. There are many of them. One is preparation for utilities; that's one. The other is, of course, lowering an insurance policy and things of this nature. You can't go out in the back country and build a house; you can't get a loan on it; and you can't...you understand.

RC: Yes.

JI: So, we're forced then to put these nuclear power plants--if we're going to put them--very close to highly utilized coastlines and intervening into the system that's already, perhaps, dense use. Under those circumstances, that may be undesirable. If this was a free world, really planning the accommodation of man and this planet together, why, one would put one down in Baja California somewhere.

RC: I both admire that and resent that. I don't want any power plants in Baja California or any place in the wilderness, I don't think. But I know that makes sense ecologically, what has to be done. As long as I have you on resilience of ocean, I read in Newsweek magazine last night--this is like the fourth or fifth time I read that--where Jacques Cousteau, a naturalist, says that in 30 years, the ocean will be dead. Do you resent that as a scientist?

JI: Let's keep that off the record. I don't want to be sued till I get my defense formed together.

RC: Alright. I'll rephrase the question another way then without involving any names. I assume you believe the ocean is amazingly resilent in terms of....

JI: Resilient.

RC: Resilient, I'm sorry. Resilient in terms of recovery. Do you think the ocean should be exploited more, move into things like mariculture....?

JI: Well, let's stop at the first statement. Exploitation perhaps has the wrong connotation. It certainly should be utilized more and with good sense, rationality, and dignity, you know. I think it can be a great deal more. We know a great deal more about it than we did 30 years ago. I've published rather extensively on exactly this same business including Cousteau, if you wish, and it's right here. You probably have that. In my answer about Cousteau, you can read that into the record right there.

RC: It's on page ten.

JI: Middle paragraph.

RC: Yes, right. As a matter of fact, I had looked at that, and that was one of the things that interested me last night, vis-a-vis this. Do you think.... Well, I assume you believe this sort of adverse publicity towards utilization of the ocean affects wrongly the process of oceanographers?

JI: Affects what?

RC: That it has a negative reaction upon oceanographers, that this kind of public opinion that these sorts of things stir up affects your work.

JI: Well, sure, of course. I don't really know quite how to answer that question, but let me say that I myself and a great number of other oceanographers have devoted a great deal of their lives trying to solve problems that society has, in some way or another, annunciated; that it's said that they were important problems. In several of these cases, we've solved these problems and we've solved them exquisitely--I mean, much better than one would have expected they could have been solved. The understanding of the pelagic fishery resources of the California Current, for instance, I think, is understood much better than anybody 20 years ago would have expected was ever obtainable. We understand, really, something about the total nature of the population: how it fluctuates, what the interrelationships are, and what it's been like for 2,000 years. But now, I think, we really understand that. And yet public opinion is set up as a number of thoughtless people who keep saying, "Don't let the anchovy go as the sardine did by overfishing," on and on, "and not allow the proper utilization of this environment." Now, what you tend to leave behind you here is the debris of dedicated scientists who are trying to serve society. There is not much to ask of society to be able to serve to the best of your intelligence. Society should be set up in such a way that you can. I think the desalination problem, the fisheries problems, have left behind them in their wake a great deal of disillusioned people who have tried to serve society, advise it, and then find, not that society doesn't act the way they thought it should act, but that it doesn't even consider the findings in the decision, doesn't even consider them. That's the French d'_______, isn't it? That's the ultimate coup d'etat. And so now you find them doing their own thing. Why in the hell should they try and serve society? I mean, science is interesting. It's a career. Why not just do it? Do the things you want to do. Don't try to serve it. It doesn't pay any attention to you. You see, it's not a question that you say, well, there are 600,000 tons of anchovies that you ought to be able to take out here--maybe a million tons--and enrich California, and, to a considerable extent, learn a great deal more about the utilization of this part of the food web and so forth, that you should do this. The fact that society doesn't do that and has archaic legislation that doesn't allow it to do it and an engraved public opinion that will now allow it to do it.... That 's not the question that they didn't do it, but they don't even consider the finding; that the people who act on these are acting only on the basis of the advocates who argue, the public advocates that argue, on one side and don't even consider the evidence. So you find yourself in the position of having devoted quite a bit of your life to really looking into, attempting to advise, society in these practical utilizations of the ocean and then finding not that your findings are not acted on, but rather that your findings are not even considered in the decisions. It's quite a disappointment.

RC: In 1952, I have listed here a ball-breaker signalling device.

JI:Well, that was kind of fun. And I must say, as usual these things have a little more implication than they sound like. That was Art Maxwell and me, I think.

RC: Right.

JI: At any rate, at that time there was a lot of importance of measuring, taking cores on the ocean bottom, and taking temperature gradients in the ocean bottom, which required very precise lowering of devices to great depths in the ocean--temperature probes. And it was important to know when you were on bottom. And these were depths of 4,000 fathoms or so, you see--3,000 fathoms. Well, you've got a lot of cable out; there's a fairly light instrument on the bottom of it; there's really no way to tell when you're on the bottom of it. And so, you needed something that told you when you hit bottom. Well, there was quite a lot of possibilities of how you did this, and there have certainly been much more sophisticated devices since. But this.... We rigged up one real fast. It was a glass ball with a hammer and a trip; and when it hit bottom, the hammer came down and presumably smashed the glass ball. And the ocean, finding itself unsupported since there's now a hole in the middle of it at two or three thousand fathoms, rushes in; and the noise it makes when it meets itself coming from all sides is considerable. There's a lot of power in there. And you hear this bang on the surface so you know you hit bottom. Well, it turns out to be much more difficult to break these damn glass balls--I suspected it--than.... You have a hammer that will just smash into pieces on the surface. But under this pressure, it didn't do anything; it just bounced. You make a point on the hammer--big hammer dropping--and the hammer would sometimes go into the ball and seal. Other times it would go in and knock a shell off and the ball would be intact when you came up. Sometimes the ball would crack all to pieces but not break. It would all be held together by the pressure. Well, we findly made the proper kind of point that it always broke the ball. But this, of course, had led.... That paper, incidentally, and our remarks about the difficulty breaking the ball have led to the utilization of glass capsules in underwater vehicles and some spherical capsules and underwater instruments of all sorts because glass, obviously, acquires quite different properties under these very high pressures.

RC: I have high-speed plankton sampler in 1958.

JI: Yes, that was a kind of fun device. When we were first starting out, 1958... we designed that much earlier than that. That was when the paper came out, I guess. It came out years later. That was back in 1951 or so, or '53 maybe. It was a torpedo-shaped plankton sampler that could be towed at high speed at considerable depths. And it had a neat little recorder in it that told you how deep it was and how much water was passing through it at each depth. It worked very well. It was used for quite a few years.

RC: I have in 1966 an Isaacs-Brown opening-closing trawl.

JI: That's just a modification of the mid-water trawl that allows you to open and close it at some depth.

RC: Now, the last two things that I would like some elaboration on: 1968, a deep-sea observational camera system.

JI: Well, over a series of years, we developed these cameras. They were free-fall cameras that you could chuck over the ship's side. And then they went to increasing sophistication; we designed them in several different forms. They went to the bottom. They usually had a bait in the foreground somewhere, a mass of fish or something. And they sat down there taking pictures every five minutes or ten minutes--sometimes still pictures, sometimes movies, motion pictures, sometimes stereo motion pictures. We have some interesting stereo motion pictures. And we discovered a great deal about the deep benthic fauna and how it behaved and what distribution of the creatures were and how they probably searched for their food, and a whole different food web of very active deep creatures. I think I'll try and revise that program because it's kind of a natural for cooperation with small coastal countries, because the cameras can be operated from unspecialized vehicles. They can be dropped over the side and sections taken down across the shelf and find out what really lives in these deeps. We find, for instance, along this coast that some of the commercial fish of British Columbia and southern Alaska and Washington, probably their main population, is down here off Baja California at great depths. They're creatures of the cold, dark water; and they merely outcrop in the arctic regions or near arctic regions. And that's where the principal commercial fishery is based. But the population itself extends to increasing depths down farther and farther, closer to the equator. We find a big population of great arctic sharks off the Baja California coast, for example, the shark very much like the Icelandic or Greenland shark, huge one. Lots of shrimp, we find deep lobsters; we find all sort of things.

RC: When did you first become interested in photography?

JI: What do you mean? In underwater photography?

RC: Underwater photography and above water photography.

JI: Well, I became much impressed by the competency of first-rate cameras to record things at Bikini, I think. Well, before that, I was in charge of the photography of the two A-bomb shots in 1946. And the fact that these big aerial cameras would allow you to place in space and position ships and explosive phenomenon and so forth when they were originally based on land, was a very powerful technique. I'd used these kinds of cameras in determining the motion of an amphibious craft in the surf zone in that early work at Berkeley, you see, by mounting cameras onshore and aiming them--aerial cameras, big mapping aerial cameras onshore--at amphibious craft coming through the surf. You then could position the craft exactly in space continuously, see just what its motions were. On a big craft, you could even measure the bending of the hull. So I was much impressed by the competency of first-class mapping cameras used in those kinds of modes. And, of course, I always wanted to see...what did the ocean do with a dead whale on its bottom? What in the devil does it do with it? It increases the food supply in square miles for years, a dead whale. It must be adapted to utilize it. And so, it turns out it is.

RC: How were you tapped for the Bikini operation?

JI: Revelle tapped me on that.

RC: Okay, now....

JI: Because I'd been doing this radio-controlled photography. We not only had two aerial cameras shooting at amphibious craft in the surf, but a radio-controlled camera and a plane or a blimp up above photographing simultaneously by radio control; and you triggered the shots--the automatic system.

RC: And then Revelle asked you to go to Bikini and mount the cameras?

JI: Be in charge of the photography.

RC: Be in charge of the photography, right.

JI: Well, the land-based photography and the, well, the radio controlled photography.

RC: Did you use the same techniques there that you had used in....

JI: Essentially.

RC: Did you make any substantial changes?

JI: Oh, well, of course, it had to have lots more backups and things because there was no going back and repairing the cameras. So we built the towers and we designed the backups, and the backups were.... Of course, it was uncertain at that time what would happen to radio signals at the time of an atomic explosion. So we had not only a central radio triggering synchronizers for the total camers--airborn and land--but we had backup individual timers on each camera. So if it failed to get its pulses, it asked for another one; if it got another one and didn't respond, then it took its own pictures anyway. And, as a matter of fact, that saved our neck just at the instant of the explosion.

RC: That's what Revelle said. Exactly what happened?

JI: He did?

RC: Yes.

JI: When was that?

RC: When I interviewed him. He suggested that the camera work here was not only splendid, but it involved innovation which had not been done before in camera work.

JI: Yes, that's right.

RC: So exactly what happened? Why did the cameras fail the first time?

JI: Well, just because of the tremendous electrostatic charge that is built up at that instance, you see--vast amount of static just at the instance of shock. So we got pictures. When the bomb had gone off underwater, the bottom of the fleet was lit up and the shock wave had not yet arrived at the surface, in 30 fathoms of water. The bottom of the fleet is lit, and there's not a pimple on the surface yet. That's pretty remarkable photography.

RC: Now, was it contact here on Bikini atoll? Is this the contact that brings you to Scripps Institution, this contact with Revelle here?

JI: No, I don't know. The first time I ever remember Revelle was an interesting case, as a matter of fact. I'd built the first of these sea sleds. We were at Mara Bay, California, and I'd built it. And then I was out on the beach, and we were going to test it for the first time. We had a DUKW, and we had a big spool of rope, just rope, manila rope. And I had attached this--I had two helpers, two DUKW drivers--I had attached this rope, and I had done a standard. And I said, well.... We had just started in the surf, the first time we ever started a sea sled. And the commandant of the little naval base there came down and said, "There's an important young lieutenant at the airport--no, at the train in San Luis Abispo--you're supposed to go pick up." I'd never heard of Revelle before. So I got in the car and went down and picked up this young, slender, tall lieutenant and brought him back. And we came down on the beach, and here these two guys.... I was telling him about this sea sled taking line through the surf. Here these two guys.... 6,000 feet of line extending out in the ocean, the cable spool is pulled off the standards, they've got an oar shoved through the cable spool, and it's plowing the sand down towards the beach, you see. So obviously they're being pulled into the surf, both of them. I grabbed the DUKW then and ran a wire out from the winch at the stern of it and got hold of it. They hadn't had time to do this to the things, was obvious. So it was an interesting first introduction to Revelle and our work there. He was immensely impressed by the damn sea sled, I must say. You know, it's kind of mysterious, your line going out into the ocean and these two guys; and, of course, I'm obviously shocked and excited. So that was my first introduction to him. We then took him out in the surf in the DUKW. He'd never been out in a DUKW, a wheeled vehicle--marvelous vehicles, just marvelous. We never really managed to sink one in any reasonable sort of surf. I have a picture of one where there is a 28-foot breaker coming up on. I was in.... I survived it, obviously. Well, anyway...so that was my first introduction to Revelle--maybe 1945, I don't know when it was. He was just organizing ONR--a marvelous man.

RC: Well, who specifically taps you to do the photography at Bikini? He does, doesn't he?

JI: Revelle, yes.

RC: Alright. And then after the photography there....

JI: I got interested in radioactivity and waves. In fact, that is one of the reasons we did the photography. It turned out it really was necessary to place all the ships in exact positions. But the, of course, the main purpose was the wave theory: what kind of explosive waves were produced by these sorts of things. And that's what we followed then on the bigger shots later, the waves.

RC: What sort of wave did you expect?

JI: From that?

RC: Yes.

JI: Oh, just about what came, what were produced, yes.

RC: Some people argue that the wave would be, you know, untold feet high and would....

JI: Well, that was.... Those were interesting days when we first shot off the big ones out there because it wasn't a bit clear that the atoll would necessarily not collapse. And we had some of the best, world famous...

Kitschkiakovski, the famous soil mechanician. You see, the coral atoll was really kind of a barrel full of mud--kind of thin shell, a brittle shell--and it's clear the coral atolls only exist where there are no earthquakes. So, there was a real question: was the thing stable or not? The bomb itself probably couldn't make a highly destructive wave over the atoll. It might in critical depth in deep water; but if part of the atoll collapsed, then the amount of energy they got linked into a wave is gigantic, you see. And we surely didn't know whether that would take place or not. And there lies the tale where we almost evacuated the Pacific basin by accident. Someday, we may go into that one.

RC: Was that the cloud mist that appeared?

JI: Oh, no. We.... I don't know if this tale has ever quite been told. This was at "Ivy Mike," the first big H-bomb. And it was shot off on an island northwest of Bikini--totally obliterated the island. And, at that time,we didn't know whether it would produce a huge wave or not. On a seamount about 700 fathoms deep, we mounted two floating wave gauges. And we had the Horizon , one of our ships, and Walter Munk was there, and Bill Baskin was there, and a fellow by the name of Commander Munk Hendricks. And we had these two, and we were going to stay with them ~~twelve~~ 12 minutes after the bomb went off and see if there was going to be a great change in water level. We had a set of signals, and everything was set up to start the evacuation of Pacific coast cities if we had a big one. Well, the bomb went off. We stayed there the 12 minutes. The cloud was overhead at this time. We'd practiced getting these skiffs back aboard--not our moorings, just the skiffs we ran. So we watched that first H-bomb from skiffs 26 miles away out in the open water. The Horizon was well away. Well, we had practiced getting those skiffs back aboard and us aboard, and the earliest we could do it was something like 15 minutes. But with that cloud overhead, we got it done in something like five, you know, and every hand was there pulling. Well, we then got in the fallout. We took more fallout than any fixed point on the earth's surface has ever gotten because we were conned into following a path that eventually turned out we were following the point of maximum fallout for something like 17 hours. Anyway, days later we got back to the skiffs and picked them up. And one minute after we left that skiff, one of these moorings showed a record of a 50-foot wave. Something had gone wrong with it. What had happened was the pressure gauge had not locked into its position and had suddenly let loose and slid down the cable 50 feet. But, of course, we wouldn't have known that. It went out 50 feet and stayed there. But if we'd been there and seen the 50-foot wave, we would ~~have~~ certainly have called for the evacuation of the North Pacific,cities of the North Pacific. And the fiasco would have been absolutely gigantic; Scripps' name would have been totally mud.

RC: When did you come to Scripps?

JI: Oh, I oscillated between Scripps and Berkeley for a year or more in about 1948 or thereabouts. I first applied to Scripps in 1936 for a job. Sverdrup was here. I had been commercial fishing. I had gotten a lot of questions in my mind about the oceans. I asked a very puzzling thing--still puzzles me. I had written a letter to Scripps asking why these things were taking place. I got an answer which really didn't address the questions I'd asked. Just like today, you know, why, you get all kinds of questions from fishermen and one thing and another. So I thought, "Boy, those guys need some help down there! It seems to me as if they can't answer these kinds of questions." So I applied for a job. Took me 10 years to get here. Sverdrup was director then, just freshly director, in 1936.

RC: Did it seem to you as if Scripps Institution functioned better when it was smaller?

JI: Well, it depends on what you mean by "better". It was a hell of a lot more fun, I guess, in many ways. There was more rapport, and there was more of a spirit of adventure, I think.

RC: I was thinking about a change of ideas. I'm shocked at, you know...a guy comes to you and says, "How about a trawl?" You're involved with Munk, and.... Did you seem to have more exchange with your colleagues? Was it departmentalized then; is it departmentalized now?

JI: Well, I think in any oceanographic institution, this is fundamentally... has to be interdisciplinary. I think it perpetually tends to crystallize into special forms of each of the disciplines again, you see. So, there's a perpetual necessity to be breaking up. There's a polymerization that goes on, a crystallization, that has to be continuously combatted. People like to talk to people who are in their same field. There's no question about it. Yet really the penetration and understanding of the universe in general and the oceans in particular and the planet in particular certainly require a communication of all sorts. And yet I think the ideal has to be an accumulation around temporary nuclei that then break up and form new associations. And that's a hard thing to form.

RC: Follow up on this. Now, when Revelle goes to Pakistan, don't you go with him?

JI: No, I have been with him.

RC: You have been with him, I understand, Well, that's what I meant. On his first trip to Pakistan to study the soils, weren't you part of that?

JI: Sure.

RC: Okay. How would you be involved in that? What was your connection with the desalinization process?

JI: Well, obviously the salt entry had its origin in the oceans. And then as

Revelle says, "If you can understand a planet with oceans, you also have to understand a planet without them." That's why you should Mars. Control-- one planet with, one planet without. From that viewpoint, it's not much of a stretch to say you ought to also look at the doabs of west Pakistan, where the problem is salt. And I had worked some on the salt of the Colorado River and various other things. I was interested in halophytic plants. I think they're really a vast potential, halophytic plants for this world. The ones that are really capable, the higher plants that are perfectly capable, of living in seawater...totally unexplored, almost totally unexplored. We have no idea what the total genetics of all the halophytic plants of this world are. Every time we even look at it, we find new ones. Halophytic avocado, for instance, fortunellas, legumes, you see, our crop plants, the crop plants on which man depends for his entire existence, strangely were all developed by prehistoric man. We have never developed a crop plant of any consequence in historic times. We've made improvements, or sort of improvements, but all the principal, tremendous selection of polyploidy and everthing were made by primitive man. Only thing we've developed in historic time is the grapefruit and brussel sprouts. And, you know, nobody is going to feed the human race on those. So the whole curious matter of the increasing salination around the world, of the increase need for utilization of seawater for agriculture, of aquaculture and so forth, and this vast array of plants, higher plants, know perfectly well how to desalinate seawater all by themselves using some light at high-level energy rather than the degraded form we can use it in. This has really received no attention-- something Texas A&M ought to get into.

RC: Was this what originally attracted you to your work on the Colorado River, or was it vice versa?

JI: Well, I got interested in the Colorado River for a very curious reason. I was plotting the total dissolved solids of the Colorado River one time and several other rivers, including the Indus, as a function of stream flow, annual stream flow. And it turns out that instead of that line of plot of total dissolved solids per year and total water flow per year extrapolating through the zero-zero axis--which is to say, zero dissolved solids transported at zero flow--it goes through the dissolved solid axis at rather high level. It's an incline line which says, in the case of the Colorado, that at zero flow it's transferring three million six hundred thousand tons of salt. Well, the only possible explanation for ~~thisxisxtha~~ such.... A decent river like the Mississippi goes through the zero-zero axis, but both the Indus and the Colorado do not. The only possible explanation for this is that there are brine springs feeding into the river--not salt springs, but concentrated brines which are continuously flowing so that as the river goes down, a year of low flow, the salinity is higher. Well, we start hunting for those springs, which we assume must exist, and we found none of them in the hydrological literature at all; we found them in the anthropological literature. The Indians knew about these brine springs flowing into the Colorado, and the first one we looked for was a great series of volcano-like structures of pure salt with pure saturated brine flowing down into the Colorado--600,000 tons a year. And I thought, you know, the hydrological mythology is that the river supplies you with water that God has determined the quality; that is now your problem. Nobody ever inquired how it got that quality; that it now your problem. Nobody ever inquires how it got that quality. So now here just this one spring, 600,000 tons is enough to put 15,000 acres out of existence every year by salination. But it's a small flow. Why not build a little brick retainer and pump the damn stuff back up on the mesa or inject it into the ground and keep it out of the Colorado. Can't get anybody interested in that. The Reclamation Department doesn't do that sort of thing. The Agriculture Department could care less; that's not part of their problem. And so it still flows into the Colorado, as far as I know. And there are undoubtedly, six more like that.

RC: And, out of curiosity, you think anthropological sites can locate these?

JI: That's how we located them.

RC: That's marvelous!

JI: Because the Indians had a real mythology about the trek, the salt trek, which was kind of a young manhood trek to go down in this great canyon and climb down these cliffs and gather the salt and bring it back. And that's all recorded in 1896. That's where we found it. We even found the old ladder that the first man that went down there early in this century had built to get down on the cliffs, all full of salt drip and crystals of salt.

RC: What made you decide to check anthropological literature?

JI: Well, it's obvious that if you're going to find out what the Indian names of these damn streams are, they'd surely know whether they had salt in them or not. And, of course, the Indus does the same thing. I mean, it's easy. There's a place called Salt Mountain or Salt Hill. It's by Sargoda and it flows into the Indus, saltwater. That's only part of it, but it's interesting, it seems to me. And it's an interesting commentary on our bureaucracy. We are now looking into the salinity power, a thing that is very little recognized. For instance, when that brine flows into the Colorado River and it's diluted, the free energy is as though that brine had fallen down a 7,000-foot dam, 7,000-foot dam! Now that's high energy density. If you're going to let it get into the Colorado anyway, why, you might as well get the power out of it. Now that's kind of a foolish thing because if you're going to dam it up, you might as well shoot it in the gulf and get the power out of it. But this is something we are now working on, which is salinity power. It's a totally invisible sort of power. But every little agricultural drainage ditch that's going into the Great Salt Lake, for instance, has a mega-watt per-cubic-foot per-second as the free energy of that going into the Great Salt Lake. It's a high.... Of all the natural forms of energy, it's certainly the most concentrated--the natural forms of energy of that sort of level, not coal or oil or lightning, of course. And so we're not going to overlook it. And we're trying to see.... It's a strange sort of energy because of the curiosities of saltwater. But that's a whole other subject. I don't know whether you want to go into it or not.

RC: Now I want to know one thing. Why is saltwater....

JI: Only one thing?

RC: Well, no, I want to know a lot, but I want to know one thing immediately. Why is saltwater energy differently construed than freshwater energy?

JI: Oh, well, no, you've got me wrong.

RC: Okay.

JI: If you take, say, brine and mix it with saltwater or with freshwater--but you could mix it with seawater because it's so much more concentrated--if you separated them by a semipermeable membrane, osmotic membrane, and allowed this to go to equilibrium, the freshwater would go into the brine. And, if you allowed this to take place without much dilution, it would raise the brine 7,000 feet high. You could now pour it off up there and run a ____________ with it or something that would generate energy. Well, this is a real expression of the free energy. The peculiarity about the thing is that, if you release that much energy in the reaction, like putting brine with freshwater ~~xxxxxxxx~~ together, you would expect it to heat up. If you release that much energy, the energy's got to go somewhere. You'd expect it to heat up, but it doesn't. The temperature change is essentially nothing, and that's because of the peculiarities of the substance sodium chloride. Sodium chloride has a strange property--almost nothing else has it--and that is that it produces disorder in the water at just about the rate it releases energy, so it all goes into what's called entropy. Now, if the Great Salt Lake had been anything other than sodium chloride--let's say it had been magnesium sulfate--people on their inner-tubes floating down the little streams in the Great Salt Lake would have burned their rears off. Or, if it had been something else--ammonium nitrate, God forbid, or ammonium sulfate, yeah, ammonium sulfate--it would have frozen their rears. There'd been big temperature changes when that freshwater flows in. Nobody would have ever doubted that there was plenty of energy. But, because of sodium chloride as the dominant ion or as the dominant molecule in there--electrolyte in there--what happens is that the energy is soaked up into increasing heat capacity at just the rate it's made, and so it doesn't appear as temperature. It's there perfectly well in a different form. All we have to do is learn how to utilize it. It's a tremendous loss. You see, all the Midwest is underlain by brines, as you may know; and there's also.... Texas has, God knows, your oil wells. When do they quit drilling? When they hit brine. And that all is a power source--every bit of it--and by no means an inconsiderable one.

RC: Did you first become interested in the issue of food and food expansion through the ocean when you were in Pakistan?

JI: No, I have always been more or less. I've been interested in it for a long time. That was '62, I think it was.

RC: Right.

JI: Now when did you interview Revelle?

RC: Oh, over the July 4th weekend.

JI: You did. He's an interesting...isn't he a great guy? At his home?

RC: Yes. What I want to know is, as a man involved in oceans looking and has been looking a long time at things like food production, mariculture, and so forth in the ocean, what was the impact upon you psychologically of an under-developed country for the first time, like Pakistan?

JI: Well, that wasn't exactly the first time I'd been in that country. Well, your reaction to a country like that, of course, is very complicated; and one can spend hours talking about that one subject. A little peculiar, isn't it? There is so much misery; there is so much poverty; and people accept it so well. The most frightening thing probably about India and Pakistan is how fast you can get used to it. I mean, you shouldn't be able to get used to it. You should not be able to get used to it. Yet, you know, it's not.... These people are not without their total disadvantages--I mean without all advantages in some way. You look at all the crippled people in that country and the defective people, and you see them on the streets and your horrified by it. What do we do with our defective people in the United States? We hide them. They have a hell of a poor time, don't they? They're old people, the old cripple people, the insane people. They're all hidden somewhere. I mean, even people just with bad facial scars--they're hidden. Those people, at any rate, have a better time, don't they? They're with society. And those are kind of interesting points, it seems to me. The United States has such a culture of perfection, you see. You really can't even be bald, much less have your nose missing or a disfiguration, crippled arm or something. In that country, those people live a much freer life. So, as I say, what would our society look like if we really left all of our people on the streets and accepted them? Quite different. And the old people, the old, crippled people that would be on the streets in India and Pakistan, are hidden away in our country and living miserable lives. As a matter of fact, I was talking to a Texan on an airplane just about that the other day. "You sure got a point there, " he said, "I'd never thought of."

RC: Does this turn your work into a ....

JI: I'd been to India quite a bit because my sister lived there for 20 years,or

18 years, in India and Pakistan. Well, her husband is an architect and she's an artist, and they built buildings and designed buildings and built schools. They built the University of Karachi and the Institute of the District Studies at, oh heavens, in Burma, and quite a number of structures throughout India. And I used to visit her occasionally, or visit them. So I was not totally unacquainted with the area.

RC: Now, in 1968, I have boot-strap corer.

JI: Oh, yes. That was kind of fun. Is that a statement of fact or are you asking me a question?

RC: Well, I'm asking you how it came about and....

JI: Well, that was an idea. And I'm not so sure how well it's ever going to be utilized, but it's an idea of a corer that pulled itself down without these tremendous weights that you use for corer barrels. And it took a long corer, a very light corer; I think it was the only corer that ever took a.... Corer that weighed much more than the device itself. It's just an interesting configuration of a corer, it seems to me, that it's not without merit for taking very deep, very long cores of the ocean bottom with simple equipment so that you don't have to have these immense winches...very large cores.

RC: It appears that in recent years you've published more on marine biology problems.

JI: Yes.

RC: Okay, is there any...such as fisheries and productivity. That was the reason for my question about Pakistan. Why turn in this direction?

JI: Well, I don't think it's a turn exactly; it's a combination of some ideas that I've been working on for quite a few years. There are different theories, alternate theories, of food webs--how they're put together. The marine biologist suffers from, I think, the delusion that he can see everything that goes forth, that goes on in the ocean. And given enough plankton nets, enough ships, enough graduate students, and one thing and another, he can trace what happens to the food material in the ocean. I think that's totally a delusion. He can't do it. The food web is far too complicated. Just take the curious case of reproductive products. Every adult creature in the ocean is throwing out eggs and sperm which account for maybe 50% of the potential growth of that population. And it's all invisibly flowing down into the food web. You see, we are so misled by our terrestrial experience. We think that the immediate effect of reproduction is to add to the adult stocks by direct transfer of biomass--that is, the cow has the calf; the calf is a large proportion of an adult; it feeds on paternal sources; it immediately then, after it is weaned, feeds on the same source that the adults feed on; and it adds to the adult biomass. In the ocean, there is a totally different situation, except for the marine mammals and some sharks. This isn't the way it is at all. The reproductive products are subtracted from the adult biomass; the eggs are pulled out of the adult biomass; the amount of survival of that material that comes back to the adult is absolutely zero. The egg that survives down here goes through the larval form, juvenile form, and finally becomes an adult. The survival of that egg is zero. So, we forget that the ocean is, in many respects, opposed, opposite, from the land. And reproductive products, which are a major tax on all creatures, is not added to the adult biomass of the ocean but is subtracted from it. And I think we need these kinds of totally different viewpoints. And we need to formalize them in some way. That's what I've been doing the last few years. You see, we are much misled by the fact that we are terrestrial creatures. The ocean is, to a great extent, a foreign and dimly understood system, and the life in it is differently adapted. We forget those differences; we tend to treat it like it were a land ecology.

RC: Is it fair to say that you're more optimistic about food production from the oceans than....

JI: Than what?

RC: Than your colleagues.

JI: I wouldn't speak for my colleagues, but I'm quite optimistic in food production in the ocean, yes. I think we've got to understand that we have to act rationally. I don't think we can act this sort of a way of.... For instance, passing a law that we must protect the porpoises. We have to first understand what's the relationship between porpoises and tuna. It may be in the 30 or 40 years of intensive tuna fishery that we've greatly stimulated the porpoise. It may be they're all out of proportion. It's a marvel to me that tuna can survive in competition with the porpoises, this profligate, warm-blooded animal inhabiting the ocean. If you've ever seen a school of porpoises sweep through a gulf, it's incredible. Frothing water--they clean everything out as they go. Now, isn't it funny that we should, because of soft hearts or something, now have passed a law that says, "Ah, we must do this thing," which must be the worst possible tactic--to pull the tuna out without hurting the porpoises, which are in direct competition with one another, or do we know whether they're in direct competition? Do we know that 40 years of tuna fishery has stimulated the porpoise or not? Do we know that they're in abnormal abundance or normal abundance? We don't know; we just pass a law that says you've got to protect the porpoise. That may really extinguish the tuna fishery. You see, all our marine fisheries are conducted in this way, that stimulate the unwanted species. We have no use for the porpoise. Even the Japanese can hardly use them. And, by law now, we make a technique that reduces the wanted species and increases the unwanted species. I went up to British Columbia not long ago, and here they have radio advisories to the salmon fishery about where you can get fish without getting your net full of dogfish. Now, this is the same technique as the boy that learns how to catch the trout out of the pond without catching a sucker, isn't it? He increases the suckers and decreases the trout. Now, here's a whole organization that decreases the salmon and increases the dogfish, you see. You've got to take the weeds! The fisheries in the world are conducted like a blind man who can't see the weeds in his garden and is always pulling up the corn instead of the weeds and then wondering why he's got less and less corn each year.

RC: So, are you suggesting,then,that more study ought to be done?

JI: No. I'm just trying to point out these viewpoints, which are different from the conventional, the traditional, way in which people have gone to look at these things, yes.

RC: Many people say that fishing is already an optimum, that the ocean cannot produce many more fisheries, that fishing is a very inefficient way.

JI: Well, it depends on what level you do this, of course, at what level you conduct your fisheries. If you were to look at it in terrestrial terms, we're conducting our fisheries now as though we were trying to feed the human race on tigers. It just takes too goddamn many cows to feed the tigers, doesn't it? Actually, we are probably even higher than that. Whatever eats tigers...I've forgotten if there is anything? We're conducting our fisheries at that level. No tribe in its right mind ever tried to feed itself on tigers--took too many deer or buffalo or whatever it was. So the closer we get down to things like the anchovetta fishery in Peru and so forth, the larger and larger...and the more we go towards the juveniles ___________, the mortality of these things in the ocean is so great that probably we are justified in taking juveniles. But the thing we do have to do is see what the effects of our fishery are on our competitors. We're always stimulating the competitors of what we want to catch. And those kind of things have to be pointed out. Yet the law is so indurate that there's really now way to have any flexibility, you see. There is no way to set up a commercial fishery that really trims and controls the fishery in mode of species, modes that it should. There is no theory to handle it. I tried to develop a theory, and it looks like it might make some sense.

RC: What was Joint Task Force Number One to whom you submitted numerous secret reports?

JI: That was the joint task force at Bikini, our first test, Abel Baker Test.

RC: Now, much of the material you published from '45 to '47 from Berkeley was related to the physical surveillances of beaches along the Pacific.

JI: Yes.

RC: Was this defense information for amphibious landings primarily?

JI: Well, it was more general than that. It was trying to understand the nature of beaches and amphibious landings, wherever it was, not defending the Pacific coast against amphibious landings, no, but rather guarding our own amphibious landings.

RC: Could you describe any work that you did on sea mines that's no longer classified?

JI: Let's see, can I come up with that? Well, as I said very early, there's quite a bit I can't--that's certainly still classified, quite different ideas about sea mines. One thing I can say is I took almost a year off back in 1952 or so, '53 maybe, to study land mines, under the theory that a land mine is essentially the same kind of a weapon as a sea mine, except it has the ability to evolve much more rapidly. Every Texas farmer that gets into the Army thinks he can go out and play around with some primer cord and explosives and develop a new land mine, and nobody in their right mind fiddles around with a sea mine. There you're talking about.... You're on a ship. You can't possibly screw around being innovative with 300 pounds of TNT. But everybody feels they know something about land mines, and they can make innovations, and there are some tremendous innovations in land mines. In fact, they damn near beat us in Korea and in Vietnam, also. The innovations were tremendous. What I was trying to see was, does this evolution of land mines have a forecasting ability on the evolution of sea mines? Are some of these technologies that were developed and which were so extremely difficult to combat--that were developed with land mines by a small scale innovation--transferable to sea mines? I thought that was a reasonably good contribution that I made on that subject. I mean, here you have an evolutionary sequence that you can begin to understand and what the steps have been between measures and countermeasures and counter-countermeasures and so forth. You see if those were transferable into the ocean, and indeed they are.

RC: Is nuclear blasting to form harbors and channels still a feasible idea?

JI: Oh, sure. The Russians have been doing it, of course. It's not a feasible idea for the United States to do. I think the French might do it to make harbors. Those great harbors of Bikini...I rode around in one of them two weeks after blast, you know--beautiful, vast blue harbors that make super tankers a reality, bring in a 200-foot draft ship. It can be made like that. But the whole Plowshare Program was conducted with such insensitivity, I think, that it does not become a possibility for the United States to proceed in any way. Yes, they were going to blast a channel through the Sierra Nevadas to let the smog out of Los Angeles or start out with blasting a new sea level canal through Panama--things that scared people to death.

RC: WEre you involved, by the way, with that plan to open up a new Panama Canal?

JI: To some extent, yes.

RC: Did you think it was a feasible idea?

JI: From an engineering standpoint?

RC: Yes, from an engineering standpoint.

JI: Of course, from an engineering standpoint; but from a social engineering standpoint, it was idiocy. You've got to start out small on something like that. You know what I wanted to do? I wanted to take my family and go to Bikini and live around the edge of the crater out there and take some National Geographic-type articles, living on the cliff of a nuclear crater, this great mile and a half harbor that's 300 feet deep with new fish coming in, new corals, new development, and one thing and another. That was kind of the way that one wanted to feed this into the social system. A National Geographic harbor of a family of scientists went up and lived on the lips of the Bikini crater. That would have made sense to me.

RC: To me, too. What happened to that idea?

JI: I never could get anywhere with it. In fact, some of my best writing is undoubtedly in proposals that never got funded. My most persuasive, my most thoughtful, my most penetrating writing is lost in proposals.

RC: What is the future of deep-sea ports?

JI: Open seaports?

RC: Open seaports.

JI: Oh, well, or course, as far as petroleum and liquid fuels and things like that, it's gigantic. That's where it's all going to be conducted. Ships are going to be built at sea, are being built at sea--assembled at sea, at any rate--and they're never going to touch shore.

RC: Do you consider this inevitable?

JI: It's not only inevitable. It's here.

RC: What about the idea of the proposed Sea Dock in the gulf?

JI: I don't know anything about the Sea Dock in the gulf.

RC: Well, the concept of....

JI: Is that a physician? A hospital?

RC: So, in other words, is it fair to say that you see these docks being built for the transport of oil in places like the gulf here and the gulf....

JI: I don't know if they're going to be docks. They're going to be mount-a-buoys of some sort. Why should they be docks? But I see no reason.... Well, one thing I really don't understand--you can perhaps illucidate me--is, what are the tremendous virtues of a free port? I mean, look at Hong Kong; and, of course, now it's a special case because it's an outlet of goods from mainland China. But free ports around the world prosper tremendously. What happens if you have a free port right in the middle of the ocean somewhere? All the ships have to come in and trade goods. You can have a nuclear power plant there to run it. It's a big enclosed thing. You can have some fisheries. All kinds of things you can do that would not be accepted in coastal waters, like the release of fisheries' wastes and so on, the nuisance level of nuclear waste. And you can have a free port. Is there virtue in having just free port out in the middle of the ocean somewhere where people can trade without tax? What is it?

RC: I'm not sure countries can move any longer to free trade.

JI: They what?

RC: I'm not sure countries can move any longer to free trade.

JI: They can't.

RC: I'm not sure that's economically feasible for countries.

JI: Well, very few of them have.... Well, export limits are much fewer than import limits, so in principle you can have some sort of free trade in the middle of the sea. Is this an idiotic idea?

RC: No.

JI: You don't think so.

RC: No. What about Spilhaus's cities on the sea?

JI: Well, I think that's all fun; and John Craven, too, of course, is working on that. That's all great fun. I don't know why people won't live there.

I can count to a billion places where you can set down Los Angeles and never find it again; but most of the world is uninhabited, as you well know. It's only the coastal strips that are covered over by this encrustacean of human beings. I can think of some reasons to live under the ocean. I think it's a great place for nightclubs, for instance. I can think of great nightclubs. There's a beautiful one in the Gulf of Elat that the Israelis have. And you sit down there underneath looking up through glass windows at everything going on in that beautiful branch of the Red Sea. I can think of condominiums for people with emphysema; living under one atmosphere of extra pressure, two atmospheres of pressure, they could live perfectly normal lives, couldn't they? That's all they need. Why should people go around gasping for breath in the last ten years of their life when they could live under one extra atmosphere and breath normally? And this might be a reasonable sort of a location for a condominium, under a couple of atmospheres of pressure down some beautiful parts of the Florida Straits. And it makes it much easier to build and less dangerous if it's underwater, where the pressure is equalized on the inside and outside. But as far as people just inhabiting the bottom of the ocean for reasons of space, I'd rather consider taking Jupiter apart and making some more planets. I think that's more feasible. That's a big engineering job. You could make dozens and dozens of planets.

RC: I'd like to be around for that one, by the way.

JI: Well, maybe you don't want to do that; maybe what you want to do is just add a little bit to Mars from Jupiter, you see, and make it more habitable--a real chemical engineering job, too, changing that ammonia into something else, getting water and so on, getting a decent atmosphere.

RC: Were you surprised, by the way, with reports coming back from Mars in terms of the satellite pictures or the landing pictures we're receiving now?

JI: I've been in Baja California. I've been looking at Martian landscape for quite a while down there, but what are these like? They're boulder-strewn, aren't they?

RC: They are boulder-strewn and the possibility that many of them are cut by water.

JI: Oh, yes, sure. Well, obviously Mars has its ice caps of some sort. It has water or some condensible fluid that's uncondensible gas. And it certainly had probably more in the past. No, I don't think I was particularly surprised. Not that I was believing the canals of Mars, but I think its.... What I was really surprised at was the surface of Venus. I mean, it really didn't occur to me, or, I guess, anybody else, that with such a dense atmosphere as Venus, and even with such tremendously high wind velocity, that the boundary layer should be so thick that there was no wind at the surface, you see. Essentially, wind was at the surface. I think that was strange.

RC: I wanted to ask you two or three questions about aquaculture. Do you see the future in aquaculture in such things as planned oyster beds, or do you see it in terms of mining what we've always classified before as seaweeds, or both?

JI: Well, you know, we start out with thinking we've just thought of aquaculture; and, of course, every place in the world other than the United States has conducted it for a long time. The Japanese have tremendous industries all through Indonesia; almost everybody in the world knows more about aquaculture than we do. We start out as now we're going to solve all these problems. I was looking at East Pakistan, and East Pakistan produces in its ponds--now, this is freshwater, ponds and ________--about as much... that little country produces about as much fish as a third of the U.S. catch, just from its ponds and __________. It's essentially cultured. Well, hell, everybody knows more about this than we do. And so I'm not quite sure what you mean when you ask me about the future of it. If you're asking me if the United States is going to learn from anybody, I'm not so sure.

They're going to go through all the difficulties of trying to solve the problems all by themselves. They're going to make it so expensive that the only thing that can possibly be used is high luxury food that has nothing to do about the impact of protein food on the world supply, as far as I can see. Now, it does seem to me that we're trying to jump.... If you look at...you're an agricultural historian and I may be quite wrong about this history, but it seems to me like there's a sequential series from the hunting economy to the gathering economy to what I would call the ranch, where you use preferred herbivores to crop the land with their own energy to the farm in which you start to raise the supplies for your domestic animals to the battery culture in which you bring food into the immobilized or essentially immobilized creatures from whatever worldwide sources are necessary. Now, we are immediately leaping to that last step, the battery culture. But the ocean is a poor environment. It's a thin broth. It has various.... The food is highly disbursed in a dense medium. It's a million times more dilute than, let's say, grass field, field of grass--million times more dilute. Traditionally and even today in modern society, where you have a poor environment, you use ranging techniques. You send your preferred creature out on to this. You don't use diesel oil. You do a little bit of diesel oil to cultivate or gather food and bring it into them. You let them burn some part of the crop that's sparsely distributed out here, and then you have the round-up. You bring them back, in some way, and you have made some net gain. Well, one interesting thing you have in the ocean is this number of species that will round themselves up, all the anadromous fishes, the ones that come in to spawn. And some of these are primitive feeders, some of the shads, the "hilsa"; the Indian shad is a particularly flying food fish. So, it seems to me like a sensible step would really be ranging; that is, you culture a population that you now turn loose in this thin environment to graze it and then to return to you, you see. And you can do some predator-control and maybe some seeding of sorts, essentially the way you do range land management, essentially the same sort of level. But our effort here is to overleap all of these steps that we've taken 3,000 years to go through on land and go right to battery culture. Well, I don't know. The problems obviously of animal husbandry, once you get to these more concentrated levels, have totally been disease; and we've spent a tremendous lot of time--there is disease of any concentration of organisms--solving this, in the case of our preferred species on land. Each one of these organisms of the ocean is going to have a series of disasters if we try battery culture. Exactly the same nature, I think: one new disease after another, and all immunization and all research that has to go behind it. But the United States, I don't think, will progress beyond luxury food.

RC: Because it's not necessary for us to increase the protein supply that much?

JI: Well, I think the increase of both protein supply around the world--and Revelle may have talked to you about this--is something of a delusion anyway; but actually the world is equally short on fats and oils, probably a more limiting nutrient. But, no, because it will make things too expensive. Every time we build a lobster industry, we'll have to build a sewage disposal plant just to get rid of the seawater that went through that lobster culture and so forth. There's the manpower involved and things are going to make it impossible to culture anything except luxury species. I think we will find out some interesting things. But the ranging and using some sort of primitive feeder like shad to go forth into the ocean and concentrate this thin soup of the ocean and bring it back to us, now that seems to me to be a much more viable approach.

RC: But one you don't think we'll choose?

JI: I don't think we'll choose it, no.

RC: I want you to describe how fish population histories can be inferred from fish scales found in sediments. I wasn't sure I understood that. And what ecological and evolutionary implications does this technique have?

JI: Well, it is somewhat unlimited, I think, eventually; but let me describe the technique to start with. In a few places...the ocean bottom is continuously building up, as you know, and continuously moving and doing all these things that we've begun to understand from seafloor spreading and continental drift. But in a few places around the world, the bottom waters usually in near-shore basins are anaerobic or anoxic--they have no oxygen in them and nothing lives there. Even though over most of the ocean the sediments are raining down every year, there are things living there that mix them up, and so the time resolution that you can get is of the order of 10,000 years at best. The sediments are mixed continuously. But in these regions, they're laid down in layers that you can identify year by year. You can put your finger on it and say, within a few years, that year is the year 0, right there; that's when the sediments were laid down, in the year 0. As you go back in time, it becomes somewhat less certain, but you can just count back and you can check your count with radioactive dating. Well, such sediments exist on land, on the whole Monterey formation, and you can split it open and you can see it peppered with fish scales and other kinds of debris, diatoms, foraminifera. So you know something in that year of what the Pacific was doing, or what the ocean was doing, and something about what fish were present, you see, because you can identify these scales. You can date these scales; you can tell how old they were and how fast they were growing; you know what species it was; you know how old the fish was and how fast it had grown. So, you know a great deal for that year, in fact. Well, the kind of Rosetta Stone in this whole business is, of course, if you try this, how well does it agree to the known history. And when we went through that exercise, we found that it agreed very well with the known history. Where we really know what the populations of pelagic fish are out here, our results from looking at these sediments and analyzing the number of scales per year coming down totally independently, gives us the same curve of the variation between the number of Lake, the number of anchovies, the number of sauries, and the number of sardines. This gives us confidence we can make these same kind of counts back into the past, as far as we want, and see what has been the relationship between these fish. And what we find is something very surprising: we find that the sardine, which was the dominant pelagic fish here when man first started his fisheries, has never been here for long periods of time, but only for short bursts; maybe eight times in the last 1600 years has it been here in quantity. The anchovy, however, has been here continuously over the whole time, you see. So, it's really quite an interesting picture. And in my earlier parts of my conversation when I said societies seem to be unable to act on information, this is very powerful information. You would really never expect it to have such a real insight into the way these species interacted in the past, and what their natural abundance was and how it fluctuated. And now to conceive that a fishery on the anchovy, a species that has maintained a sizeable population here for at least the last 1600 years, would be the same as a fishery on the sardine, a species that was never able to maintain a sizeable population here for more than 40 or 50 or 80 years at a time, and then only in a few periods, it's asinine. I mean, you couldn't expect it to have the same result--fishery on a transient species and fishery on a permanent species. So, in my earlier remarks, I said I thought we had gotten exquisite answers on some of these problems that society had annunciated as a worthwhile problem to work on. And yet, the disappointment is, of course, not that they don't act on this, but they don't consider it even in their actions, you see, to reiterate.

RC: What about in terms of evolutionary implications? Do you foresee being able to foresee the evolution in general of fishes and fish species from this kind of....

JI: Well, of course, that's a traditional way of going because there 10,000-year resolution is of no particular consequence but to see the interrelationships between the microorganisms, for instance. So we know how the Pacific was behaving when the sardine was here, now, and how it behaved when they changed from one species to another. We will eventually know what environmental conditions mediated these shifts between species because we can see how much mid-Pacific influence there was, how strong the California Current was, how much sub-tropical water was flowing up, how strong was the counter-current, and do this on a year-by-year basis. That's a huge job. You have to identify all the diatoms and foraminifera, you see, in these little thin layers--seven tenths of a millimeter thick. But, you see, if we now go to a place like the Bay of Bengal and the Indians are going to start a fishery, say, on their __________, wouldn't it be a wonderful thing to be able to tell them is that a permanent species here, is it occasionally replaced by others, is it just a transient species, how steady is the population, how steady has it been for 2,000 years. These are big answers for people who want to guide their fisheries in some rational way.

RC: Would you judge us to be proceeding cautiously enough or not cautiously enough in developing the sea?

JI: What do you mean by "us"?

RC: "Us" as Americans. Would you encourage us to exercise more caution or less caution?

JI: I think "caution" is the wrong word there. I would advise us to use more "rationality." "Caution" is really totally the wrong word. For example, the whole matter of domestic waste disposal in the ocean...we want to call caution there. Caution is putting out a billion dollars in Los Angeles to build tertiary treatment plants, is it? Five hundred dollars per capita, when we have so many social needs? Is that caution? No, all the evidence says the ocean is perfectly competent to absorb these kinds of waste. That's its business, absorbing organic material. So I'm not quite sure what caution.... "Rational" is the word. Yes, we need a great deal more, a great deal more. We need people other than Cousteau advising our government on how to do it. He has reasons to scare people to death.

RC: Which are?

JI: Funds for his institution. It's becoming extremely costly to society as a whole. It doesn't take a balanced viewpoint. It has pictures of us climbing to the highest mountain tops and clutching our throats because we're dying of anoxia because we've killed all the phtoplankton. No evidence, no evidence.

RC: What do you see your role in the future of oceanography? How do you perceive yourself in the coming future?

JI: Well, I'd like to know. I have no idea. I've always been enthusiastic and melioristic, I guess is the word--somewhere between optimistic and pessimistic. I think melioristic is optimism; only you have to do something about it, rather than just have it happen. I've, to some real degree, right at the moment, lost confidence that defense can be influenced very promptly, that our political system is such that it really is not able to respond to the evidence. It's acclaimation...government by acclaimation of some sort. A politician moves so that he causes the least stir in some way; he gets the fewest people mad at him. And I really think maybe we just can't act this way. Now maybe we can afford not to act this way. We can afford to put an extra million dollars, billion dollars, in sewer plants in every major city along the coast for no purpose. And maybe we need to. Maybe it's a WPA of sorts. Maybe it's the science that we've worked on that's a WPA of sorts at a higher level. Maybe the WPA concept is evolved so that I'm nothing but a WPA worker making bridges in some unknown creek in some unknown mountain that will never be seen again. I dislike thinking this, but I'm beginning to wonder if the WPA concept hasn't spread quite a bit. It worries me to see this wake of scientists who have tried to serve this country, unable to serve it. The great number of people that worked on desalination just announced today that something you knew 20 years ago when desalination program, when the OSW, was established, that it was impossible to do this for agricultural water. And you can make that calculation in an instant. At that time, you knew that, if the ocean were freshwater—that is, it were given to you at sea level—it would have very little effect on our utilization of water in these United States. It's too big a continent. I'm not a bit sure. I came to the conclusion that Israel was full of smart people. They have reason to be conservative of their resources. They're as screwed up as we are. They've brought our culture there. They are a piece of the United States more or less. They have the same screw ups that we have. So I'm not quite sure. Mexico, Mexico looks pretty good. I think they...you know, they don't necessarily follow the tradition. They can have their ________ instead of stringing copper wires around forever because the Anaconda insists on it or whatever. They can have their communication system by some totally modern approach, rather than by the conventional ones. Can they conduct a fishery that way? Yes. These are the countries that are going to maybe profit from this research. They're starting in anchovy fishery. And they are going to start it on the basis of the data we got to try to help the people of California. Survival is of the fittest. I think this same old law still acts.

RC: Are you still intimately involved, by the way, with Israel?

JI: I don't know what you mean intimately. We're putting a breakwater over there.

RC: Yes. I mean, is that process still going on?

JI: Oh, sure.

RC: Okay.

JI: Sure. They're bright people, but they're screwed up, also. They have their own political system that's copied much after ours. You know, they also can't do the rational thing.

RC: What role do you see for Scripps Institution?

JI: In what?

RC: In the future of oceanography.

JI: Oh, well, I think Scripps will be a leader for a long time. It will be a leader for the simple reason that there are a lot of bright people here, for the foreseeable future. I think that it has to avoid getting into huge projects which totally dominate it in any way, say, for instance, the Seri (?) project. It certainly may have a role in that, but I think it's unhealthy for very large projects to dominate an institution like this, unless they're very highly generalized ones. I think it should have a better role somehow in advising the government. I've been looking at a series of reports lately that...in which we really act.... I collect together various groups of scientists as rather fixed panels to review various research projects various proposals, and really criticize them; and I think that's having a salutary influence. I think that's working rather well, but it's rather small-scaled. I think maybe that's the way one might act: really get confident people together so, when Lockheed or API or somebody like that has a big project, there is a competent review by a competent scientist undertaking to really see that it makes sense so that you don't get into this funny situation of their still dealing with small-scale hydrodynamics when it's really very large-scale hydrodynamics and things of this sort.

RC: Do you think that the role of oceanography or research in oceanography, if I may use that word, or developemnt of the oceans....

JI: It's explosive. Sure, I think that it still suffers, like most research in the United States, from the final and enveloping shock waves of Alamagordo, which is if you throw enough money at it you can solve it. That was foreseeable, wasn't it? You put a billion dollars in and you release this vast amount of energy. But somehow the important thing is.... I remember the days when I was peripherally associated with these great men who were responsible for nuclear energy: Caronk, Gamma, Fermi, all these people. And I remember the excitement there was at that time. Here was really of the new world, Columbus entering the new world, many ideas. Nobody had the idea it was going to be just nuclear reactors of the conventional type todya. There were all kinds of good ideas of how to use this nuclear energy, including "Plowshare" just as an explosive. We've kind of lost that sense of adventure. Everybody's treading too lightly. Somebody else is going to have to do it. The French, possibly. I don't know. They seem more adventurous right now. They have a different kind of a government, too. Forty people can argue about something rather than just...forty sides rather than twenty on two sides. All kinds of viewpoints can be raised in a Napoleonic society that can't be raised in an English common law society just because of the influence, I think, of our judiciary system.

RC: The influence of our judiciary system?

JI: Of our judiciary system, our advocative system.

RC: Oh, okay. Then you conceive....

JI: It's a curious output, as I understand it. And you probably know much more about this, and your wife probably knows more than you do about it. But it seems to me that a result of the Napoleonic system is that you try to get at the truth, not just...the grey area is considered--pros and con. It's a court of equity rather than a court of law, if I used these terms correctly. In the United States all these things are decided as though they were court of law. They're either right or wrong, one or the other. Science isn't that way. I would like to see somebody, an anthropologist, really look at this influence of how our legal system, our judiciary system, our legislative system, which is totally related to the nature of the laws. Government is made of laws, not men, I take it; but it nevertheless influences men in their way of thinking. And there's a tremendously and fundamental inconsistency between our system of advocacy and science, and the manner in which advice, guidance, can be exercised, whereas other political systems--I'm just guessing it's a Napoleanic one; it's of that nature--may not be quite so constrained. All sides and all doubts and all questions and all levels of certainty can be discussed more freely in a system of that nature perhaps.

RC: So you see science as sort of an explosive institution where everybody puts in sorts of ideas and argues; and eventually you would, in effect, ~~would you~~ say, calculate risk. Like you say, the risk of a nuclear plant is 0.2 % in terms of doing damage, so you ought to go ahead then and do what's necessary to develop one. Is that fair to say? That sort of open debate?

JI: I'm not a bit sure how the nuclear power plant business has gone really. I think what has happened is that many companies developed nuclear power plants, and they were really looking for patentable aspects of them so that they could have control of them. I know General Atomics has some very excellent part of their development. Somehow all of this should be put together and the best parts of all of these put together, you see; but that hasn't happened, I believe. So each company has its weaknesses and its strengths. What we need is the most certain sort of a nuclear power plant.

But, if you want to talk about certainty and risk, I truly believe that, in the brain of the archangel that runs this universe, there is some sort of a curve which says what are the risks of the human race. And the risks of the human race are not probably the ones we recognize, just exactly like the risks in war are very often not the ones we recognize. And that if one really knew what this curve looked like and could make the produce of risks and stakes or, on the positive side, risks or possibilities and advantages, it's the rare ones that are going to count, the very rare ones. The fact that we can foresee you might have a nuclear accident is probably a trivial risk anyway. If you multiply the stakes times the risks, it's a very low number. On the other hand, there are a great number of other things that are probably not so, natural events. And I truly think it's science's role to explore these more unlikely possibilities. Let's look at the positive side, for an instance. There is no science that rules out the existence of a particle, which is a normal particle that has an opposite charge of gravity. It's a negative gravity particle. There's no law that rules such a particle out. There is a rule that says that, if nothing rules it out, it probably exists. Well, what are the possibilities of such a particle existing and that we could make them? Billion to one? Maybe so. But what are the advantages if we could? Christ, an anti-gravity particle? We'd be in real business, if it were normal matter,not anti-matter. We'd really be in business, wouldn't we? The boom to the world would be absolutely incredible. The product of possibility and value is an immense one, even though it's only a billion to one or whatever it is. The product is huge. We must explore both on the positive side and on the negative side, these much more outlandish possibilities, it seems to me. And we've got to think, and the anthropologist also and the historians should really think about what are the risks and advantages that are critical to the survival of this species, which, I take it, is one of our principal concerns. I don't think we're progressing very well now on that subject.