

TEXAS A&M UNIVERSITY
ORAL HISTORY REPORT

INTERVIEWEE: Joel W. Hedgpeth

INTERVIEWER: Robert A. Calvert

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TIME: 2:00 p.m.

RC: Professor Hedgpeth, I was interested in your discussion of your earlier education. You speak of the influence of your grandfather's library. Would you elaborate some upon that?

JH: Well, my grandfather was an attorney, who liked to go around book stores; and he always gave my aunts and my mother second-hand books for Christmas, among other things. At any rate, there were a lot of books in his library. He crossed the plains--well, he crossed the isthmus in 1859, and then later he went back in the year of Federman's Massacre, whenever that was. I think that was before Custer. So he developed a great interest in Indians and Western history, and he had a lot of books like that. But the main books that I could reach in the library were very general things. One of them was a big old fat book called Sea and Land, by a man named W. J. Buel, and it was one of those things they sold by house-to-house only; and it had this long--half of it was on the sea and the other half was on the land, and the middle was "The Ancient Mariner." Very badly printed, but anyway I read a lot of that. Because the sea was first, I guess I read most of that and never got very far in the land section.

RC: Then you always intended to work in and around marine biology or marine sciences?

JH: I'm not sure I really intended that. Another thing was, of course, next door there used to live a very famous shell collector, Henry Hemphill, who was one of the great collectors of his day. He came from San Diego, and he died the year I was born, so I don't remember him, naturally, or maybe two or three years later, anyway. But the family lived there for years, and they had his shell

collection there. We used to go over there, we--myself and my cousins-- and look at all those sea shells in the cabinets. It was sort of--as I look back on it now, I suppose it was sort of like the cabin in Captain Nemo's "Nautilus", all those shells and sea things hanging around. So that helped, too.

RC: That was where your interest in the sea, you think, possibly came from?

JH: Well, of course, this was all in Oakland, California. It wasn't very far from the sea. My aunt, or several of my aunts used to take us over to the ocean beach by the cliff house, and we'd walk along the shore. Then, after that, I moved inland, and it was sometime before I really got back to the sea

RC: Well, in '26 to '29, when you were involved in San Mateo Junior College, did you specialize there in biology, or the sciences?

JH: Well, wait a minute, now...'29 was when I graduated from high school.

RC: I'm sorry, '29 to '31.

JH: Yes. Well, I went there with the idea of studying journalism. I'd been associate editor of the high school paper, so I was going to be a writer, I guess; but very soon I found it very easy to get good grades in biology, and they made me the lab assistant for a while--go out and collect things for the classes, like starfish, and so on--so I guess I got started in biology that way. Also, I was influenced by the teacher there,...

RC: ...who was...

JH: ...a man named Cliver. He had just gotten a degree from Stanford. He'd gotten a teaching position there at the San Mateo Junior College. He stayed with it for his entire career.

RC: Is he the one who suggested that you go to Berkeley in '31?

JH: No, not really. Berkeley was just the place you went to in those days, once you got eligible to go. Maybe it was partly because I'd helped--well, participated--in certain unscheduled chemical experiments which nearly blew up the high school that I wasn't recommended; so I had to go to a junior college before going to Berkeley, anyhow. But that was always supposed to be the ultimate destination.

RC: From the very beginning at Berkeley, were you guided toward marine...

JH: Well, I was majoring in zoology, and most of the zoology I took happened to be marine; though for a while I was thinking seriously of entomology.

RC: But your master's thesis topic, though, was...

JH: Well, that's--fresh-water copepods are little fellows that live in temporary ponds. There used to be a lot of them around the bay area, which are no longer there because the subdivisions and freeways have obliterated a lot of the low places. There use to be quite an interesting variety of these accessible in the spring, so I studied those for a master's degree.

RC: Well, then, I lose track of you for four years during the Depression. Seems to be odd jobs here and there that you take.

JH: That's right; all kinds of odd jobs.

RC: Is that because there were no jobs available in this period in your specialty, or were you...

JH: No, there were no jobs, or very few, and so we did whatever we could. I even worked in a print shop for a while.

RC: Did you continue to read in this period?

JH: Oh yes.

RC: What I'm interested in is what explains your attraction toward literature.

JH: Heavens...well, I don't know. When I was writing for the high school paper, I got a letter from one of my aunts, who was living in Oregon; and she said, "I hear you are writing for the paper. Whatever you do, don't be another Ambrose Bierce." I'd never heard of Ambrose Bierce at that time, and my reaction to such advice was to immediately find out, so I went down to the library and read Ambrose Bierce from cover to cover. It was probably the worse thing she could have done for me, I suppose, develop a certain satiric or sarcastic propensity, since I became quite attached to Mr. Bierce's writing.

RC: There seems to be this drawing back in the first period, back toward literature; and yet there seems to be no attempt to pick up literature as a major. Did you ever consider writing, for example?

JH: Well, I entered college as a journalism major, but quickly found I wasn't learning very much doing that, except helping the kids write the school paper. And so I got directed into biology by Mr. Cliver--one of these happenstance things.

RC: All right, when did you become interested in Thoreau?

JH: My father used to say I ought to read Thoreau, but I don't think he ever read Thoreau; he had Emerson. My father had a fifth-grade education, and he never read too thoroughly. But somewhere along the line, he had heard about these people. Of course, all of his uncles are Methodist ministers, and I expect he'd either been read to or heard from some of these things, though his main book was the Bible.

RC: What, then, do you think directs you toward your work in naturalism?

JH: Well, it was very soon after I was born that I was taken out into the country. Since my father was a blacksmith, and that's the beginning of the time when blacksmith's were beginning to fade out. There were very few blacksmiths employed in cities when I was a little boy; that'd be in 1919 or so. Let's see, I was born in 1911. So we were always out in small communities, next to nature. I would go out and look at the insects and things, especially ants. I liked to watch ant hills.

RC: In 1948 and '49 you moved to the University of Texas, which is, again, a sizable gap I have in your life. During the war years, exactly what did you do?

JH: Well, let's see if we can unscramble this. I--well, the Depression wasn't getting any better there for a long while. In fact, the Depression really didn't break until World War II, as you may realize. I went after various jobs. I worked for some of the big projects, like Shasta Dam and Corps of Engineers, as a biologist. They had started this idea of damming a lot of places in California; and then, the sports fishermen, especially, began to raise the question, including the Isaac Walton League, about the danger of these things to migrating salmon. So after they'd already determined what to do--they were going to build the dam anyway, and all of that--then they hired crews of biologists

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and I had passed a Civil Service examination--of course, I took a lot of them--so in '38, or thereabouts, I began to work as a biologist. Before that, I'd had odd jobs and things; and I'd also had a year in Washington. Having also inadvertently taken a general Civil Service examination, I was, for a year, in Washington as a Civil Service clerk for the Treasury, and we processed the bonus bonds. Since I was swing shift, I would spend the mornings down in the National Museum, doing odd chores, and so forth, with Waldo Schmitt, who is still living, incidentally. That's a gentleman you really ought to work over before he goes. He must be 85, now. He's rather deaf. He's a whole history of the Smithsonian, the natural-history museum part of it.

RC: Now he's still connected with the Smithsonian, is he not?

JH: He's retired, but he has an office there; and I suppose the Smithsonian may have looked into him and recorded his reminissances. I hope so. He's practically a walking Archives of the place. Through his life, now, he's known almost everybody. He started out in 1914 in San Francisco Bay, though he's a native of Washington, D.C. That's S-c-h-m-i-t-t. So, now where do we go?

RC: Well, you're still in Washington, and you're still working bonus bonds.

JH: Yes, well, I took that for a year, and then I went back to California and took another stop-gap job; and then this business of the salmon survey came along, the stream survey, and the rivers. So that's when I began working as a biologist.

RC: Now was it salmon that first interested you in ecology?

JH: I don't know. "Ecology" is one of those nice, lovely, vague words.

RC: That part in composition to damming and streams, in this case, I'm thinking of.

JH: Well, no, that, interestingly enough, goes back a lot further than that, actually. It goes back to a time when I was nine years old, or was it eight? My father had a job up in the Sierras, in a Hetch-Hetchy project. The city of San Francisco dammed a mountain valley for its water supply, and the valley was very much like Yosemite. I remember seeing that while they were building the dam. They had to

cut all the trees down. They were blasting the keyways in this solid granite; and maybe that's when I started worrying about the environment. But anyway, I certainly didn't like what I saw there. Well, I suppose it was all the noise and racket, but whatever it was, it didn't set too well with me; I got suspicious, I guess.

RC: Well, I just happen to have a quote here; I'd like to see if this places it in historic prospective. "When the first of these dams was being built at Bullard's Bar, on the north fork of the Yuba River, during the years '21 to '24, so many salmon died below the rising dam, they had to be carted away and disposed of."

JWH: Oh, well, that's another matter. I was never at Bullard's Bar until after-- This is a paper on salmon, the effects of dams on salmon, which I wrote; and that's a historical statement about what happened when the first debris dam was built. See, the Hetch-Hetchy dam was begun way back there before World War I, well, essentially before we got involved in World War I, and it wasn't finished til '23. For instance, that was what killed John Muir. In 1914 the decision was made to sacrifice Hetch-Hetchy, an Indian name for the valley. And the dam, I don't think, was finished until 1923. I was up there about 1919 and '20. My father had a job there. So it was during the building of that dam, I never knew anything about Bullard's Bar until I got on this salmon job in the 1940's.

RC: What do you mean, "that's what killed John Muir"?

JWH: Well, this is, I think, a fairly well-known story. He fought bitterly against appropriation of Hetch-Hetchy, because it was a beautiful valley. The developer said, "Well, it's going to look just like Yosemite; we've got one Yosemite, we don't need two". Of course, history has proved them wrong because of the tremendous increase in visitors and use of that part of the world. But John Muir, I guess, was pretty old and ill; and when the word came that that fight had been lost, well that was about it. He died a few days later, as I recall, in the hospital. That's not quite fair to say that, but it wasn't a very pleasant thing for him to think about at

that stage in life.

RC: So your interest, then, in terms of conservation, you'd say, precedes your work for the Corps of Engineers?

JWH: I would think so; of course, I wasn't consciously this way.

RC: In '48 and '49, you seemed to go to Graduate School at the University of Texas at Austin.

JWH: Yes.

RC: What brought you there?

JWH: That was an interlude. I was actually brought to Texas to work for the Game, Fish and Oyster Commission, at the insistence of Gordon Gunter. He'd been out on this salmon survey a couple of years before, or actually several years before. The crew was assembled by Civil Service exams from all over the country, and Gunter was one of them. So when this job was open in Texas, in '45, he asked me to come down and take it, though I didn't know anything about oysters. Of course, I'd had a fair amount of invertebrate zoology. There was no such thing as marine biology in those days. People didn't think in terms like that.

RC: So you go to Texas, then, in '45, to work with the Fish and Game Commission there?

JWH: It used to be called the Game, Fish, and Oyster Commission. What's it called now? Games and Parks, or some crazy thing? That was a political education.

RC: Did you work with Gunter specifically during then?

JWH: Yes. I was associated with Gunter a couple of years, and then he pulled out and went to the University of Miami; and the people at Austin brought in another gentleman who was not trained academically as a biologist and had some rather odd ideas. We finally came to a parting of the ways.

RC: You're talking, now, about '48-9, . . .

JWH: Well, I'm up to '47.

RC: '47, O.K.

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at Port Aransas--that is, they were trying to get it going. They'd gotten the property from the Coast Guard--maybe it was the Corps of Engineers. That was an old Corps of Engineers building, the original building there, that had been put up, I think. Well, the original building was a house on top of a foundation made for a big water tank. And there was another old building that was a house for workers on the jetties to live in. I think that's gone now. In fact, both those buildings must be gone now. And so that was the nucleus for the Institute of Marine Science. Part of it, though, was building up funds for the famous project nine.

RC: . . .which is what?

JWH: Well, that was one of the first big environmental consulting deals, actually. We didn't think of it as that. Some people, oyster growers in Louisiana, were suing the petroleum companies for ruining--for leaking oil from their drill rigs. These were in the bayous and the bays then, not off shore. Turns out, of course, that human taste buds are so sensitive, they can taste oil in much lower concentrations than you can really measure definitely. So it does impair the flavor. And so the oil companies were being sued for a million, so in defense they funded this massive investigation to find out just how much--what--oil did to oysters and all of this sort of thing. And it was, in part--I forget how it began, though; you may find out from others--but it was primarily directed by A. A. Jakkula, this smart Finnish boy from the Engineering Department at A&M. It became quite a thing. I think there were hundreds of people employed, and they brought in various foreigners to visit the place. They saw all the great oyster people in the world, sooner or later, come in to talk to them and us, and so forth. They made quite a cosmopolitan affair. Well, anyway, Dr. Lund had a good part of this contract, enough to build a building there--bootleg a little building, and so on--and get quite a start. And so we started that laboratory. They needed somebody right off to protect the insurance, or in fact, I think even, just to validate the insurance. So in, I guess it was about '48 or

late '47, I was asked to go over there and be a resident biologist. So about the same time I signed up for a Ph.D. at Texas; it wasn't intended to be an easy deal, of course, but at any rate, that was part of the fringe benefits. As it turned out, there was a certain amount of faculty problems, or administrative problems, with Dr. Lund, which made life a little difficult for students working under him, through no fault of their own.

RC: Lund, you mean, was in conflict with the rest of the members of the department?

JWH: He was in conflict with some of them. As I heard--I didn't see all of this, because I was down at Port Aransas; I didn't often get up to Austin. But I saw some of it that went on, and I heard about other things. People came to meetings complaining about things with little 3 x 5 cards to report such things as 15 years ago when they went off on a holiday, they came back to find the budget committee had met and cut off two of their teaching assistants, and stuff like this. People had been building up for this for years. I honestly don't know, now, what it was all about. ~~But~~ I had a suspicion it went way back before the University of Texas; certain personalities. That's someone else's story. Unfortunately, Dr. Lund is no longer with us. I don't know who else of that group is.

RC: But the way it affected you, then, was making. . .

JWH: Well, it made it pretty plain that, if I wanted to finish my Ph.D., I'd better find someplace else. Fortunately, Dr. Lund had stated his case in about, a very thick mimeographed document which he sent to the entire mailing list of the American Society of Zoologists. So when I wrote a note back to the Department of Zoology at Berkeley that maybe I ought to try to come back and finish off there, the chairman says, "I've just received Dr. Lund's broad side and quite understand. Welcome back."

RC: I noticed though that your Ph.D. dissertation still involved work along the Texas and Louisiana coast.

JWH: Well, that's right. Well, I worked for, the thing I did there, since so little was known about the funnel life of the Gulf Coast, especially the invertebrates--Dr. Gunter worked on the fishes--that I started this survey

and so they suggested I finish it up for my thesis rather than start something new all over again at Berkeley.

RC: Well, when do you meet Ed Ricketts?

JWH: That's another story. That was in '36 or '38. My mother had a very dear friend down at Pacific Grove, which is part of Monterey, well, it's next to Monterey, and I'd been corresponding with Ed because he was getting this book out on sea life, it became known as Between Pacific Tides, and a whole lot of us were introduced to Ed via letters from Waldo Schmitt. He wanted to know how to be helped with certain animals, and I was a specialist since college days in a little group I'd done for my term paper. The name of the sea spiders are pycnogonids, which are fairly common in some places. So they were corresponding on which species to put in the book, and identifying material for them and that sort of thing. So one of those days when I drove my mother down there to visit her old friend, I went around to see Ed. So that was how that began.

RC: How were you chosen, then, to write the introduction to, and revise later on that work?

JWH: It's a long story. Well, not too long. I met him a number of times. He started to work on a new edition, and so I began to help him out. He asked for various things; and also, I suggested a couple of things like this, some of which I drew for him, and so it went on that way. But before we got that edition really done, he got into this auto accident and was killed. And then when the Stanford Press wanted to finish another edition; of course when they found all this material in his files, they were aware of me, so they asked me to take it over.

RC: Well, had you done any free lance writing before Stanford contacted you?

JWH: Oh yes, I was writing things for Nature Magazine and other such things. In fact, I was doing that in the late '30's and early '40's. I wasn't making very much at it; I didn't really make much money at anything in

those days.

RC: But did you chose free lance writing as a way to acquire a living or was it because you had a problem moving from, let's say, Rockford, Texas, into a profession?

JWH: Oh, no, no. I was doing that before. I didn't do much of it afterwards.

RC: Well, I have a major monograph listed here in 1941.

JWH: Oh, yes. Well, another thing you do when you don't have much work to do. I was asked by Dr. Schmitt, who had all these collections, and I was the only person who was working in this little group of animals, the pycnogonids, so he asked me if I would tackle this. And in sort of carelessly, euphoric moment, I said "Yes", and the result was I got what seemed to be half a freight carload of specimens, collected since the 1870's when the United States first began deep sea dredging. It took me months to grind this out, and I proceeded to write a monograph. In fact, I wrote two of them. The second one was on Japanese specimens.

RC: But you completed this really before you were turned back to Berkeley, because you already have a major monograph before you enter graduate school?

JWH: Right. And some people thought that ought to be the thesis, but the department had certain qualms about accepting something already done, and since I was doing this other thing, they said do that. You see, there were certain dispensations because of this Lund-debacle, which became, for awhile, a rather well-known case in university circles.

RC: Alright. And so you know Ed Ricketts then, you meet Ed Ricketts before you write the monograph?

JWH: Right.

RC: And then you contact with him through letters, and then contact from Stanford University Press to write the introduction?

JWH: Well, that was a long time later. That was after I returned to California, as a matter of fact, it was about the same time.

RC: Who collaborated with you on the introduction?

JWH: Well, the first--see, actually the bulk part of the work was bringing the text up to date, changing some of the things in the back. So we wrote an introduction with Jack Calvin, whom I'd never met at that time. He was the original collaborator with Ricketts; Calvin was a professional writer, he didn't know much zoology, but since he knew Ed better than I did, and had part in the first edition, why, I asked him to do this. He wrote the draft, and I changed it a little bit, so on. But that's not a very extensive piece of writing, really. I've done some longer things about Ricketts since in this volume published by Oregon State University on Steinbeck: The Man and His Work.

RC: Well, what I'm interested in, there's a line in your writings that talks about the demand for independence, or at least I inferred it from your writing, maybe you didn't mean to imply it. And I'm interested somehow where in this period that you end up sacrificing independence, that you leave free lance writing and this sort of work, you move into the Texas game and fish work, and then in '47 you move to teaching.

JWH: I didn't move to teaching really. Except for summer teaching courses, I wasn't involved with academic teaching for a long while. Seeing as I was at Scripps for six years and never taught a course, I was hired there, particularly to get out this Treatise on Marine Ecology. So you can say I was a professional writer in that sense. Not many people are employed full time by scholarly institutions to write a research monograph. So, of course, it was a non-profit monograph and paid no royalties, but then it paid five or six years salary. It came out alright.

RC: With whom were you associated with at Scripps? This is '51 to '57, right?

JWH: Right. Well, oddly enough, I was associated primarily with myself. I was working actually through the National Research Council and the U.S. Geological Survey, which had more or less inherited this idea of writing in Treatise on Marine Ecology from T. Wayland Vaughan. Vaughan, you know. . .

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RC: Now did your interest in the history of marine biology come from this period?

JWH: I already had that because I published '45 or so. Some of the research papers you see in that list are actually things that I wrote that couldn't be sold and just send them to scholarly journals. I hadn't developed the more mercenary habit of never writing anything you don't sell.

RC: You arrived then at Scripps primarily a writer and not a researcher. Is that fair to say? Would you disagree with that?

JWH: Yes, that was the understanding in which I went down there was to see this treaties through, which I did.

RC: And then you go from Scripps to the University of the Pacific?

JWH: Right. Then I became the faculty type around the laboratory there for several years.

RC: Okay. Were you primarily a teacher there or were you once more back to research?

JWH: No, well, I was an administrator.

RC: You were an administrator. What ideas did you bring from Scripps with you to the University of the Pacific?

JWH: Well, of course, having edited the Treatise on Marine Ecology and ransacked the brains of a whole lot of people, two continents at least, and so forth, I guess I brought all those ideas with me, as far as course content and the like.

RC: Well, you were there in the University of Pacific's boon period of growth. Did you feel any stress, let's say, intellectually?

JWH: No, I was 100 miles west of the campus, the campus was in Stockton and the laboratory's on the seashore, west of Petaluma, and it's out on the coast, of course. I was left pretty much to my own devices. In fact, maybe some of them stood a little in awe of me. I don't know why. So they always met my budget request, which always amazed me. I think there were two or three people on the Board of Trustees, it was a Board of Regents

after it became a university. They were not interested so much in me but as in seeing a marine laboratory as a going concern. So the budget was always underwritten. I understood there was some intervention of this kind.

The president then, Dr. Burns, didn't have too much understanding of science, but he tried. On the whole, I think he did very well.

RC: Now don't you set up a lab, too, when you go to Oregon State?

JWH: Yeah, I left Pacific to go up to Oregon State. I'd been at Pacific about seven years, and things were beginning to slow down, and I didn't think they were going to get anywhere. And I was afraid I believed in certain more optimistic versions of what was going to happen at Oregon State than actually happened.

RC: Now let's work with those for awhile, if you don't mind. What did you have for an end goal for the lab at the University of Pacific?

JWH: Well, simply to make it a good, general teaching lab with modest research program. We did have, our main concern was to study this little bay there, Tomalas Bay, which is about 20 miles, 15 miles long, a mile wide in spots, through a long term period of time to get some idea of the range of natural changes and of course, you didn't listen to the talks in the program today, the shelf eco-system and so forth, but this came up again and again, this problem of management conservation. We can't really study the impact of man on an environment until we know the writings of natural impacts. And this is what we were setting out to do. And then we had with us a man from the University of Chicago, who had been looking for some place like this and he found Dome Beach, and so we had that interchange of faculty members and students there for some years. We did fairly well at this. In fact, one of the things we did that was interesting, we got an NSF program for high school teachers and we brought two of these teachers in every summer for several years; in fact, about ten years, I think, it went on after I left with Dr. Johnson of the University of Chicago

running it, training them in the methods of research, what actually you should do. It all grew out of how to manage field trips to the seashore, to make them a little bit more than just picnics, which so often happens in high schools and grade schools. This program turned out fairly well, but then the NSF finally decided not to fund it anymore. The reasons, in fact, Dr. Pequegnat had something to do with this program for awhile. He was the program director, I think. I'm not sure exactly this specific program, but he knows quite a bit about it if you want to ask him sometime.

RC: Now that's then the research participation for high school and junior college teachers from the National Science Foundation?

JWH: Right. Yes.

RC: That would not also involve the study of curriculum to see how the curriculum of junior colleges and highschools could be upgraded? That's a different program.

JWH: That's a different program. No, it didn't have anything to do with that. It's harder to change a curriculum than it is to move a cemetery. You know that. It's an old dean's saying, I believe.

RC: Alright. So you were there then to set up this lab at the University of Pacific, and you stayed with it several years. You say, it seems as if the end result disappointed you now. Why?

JWH: Well, it's not that. It was just things weren't happening, and the deans told me they weren't going to have any more money, and so forth. And of course it didn't lead to the doctoral level of students. So they came along from Oregon, and they had some ideas which sounded pretty good, so I went up there--not the wisest thing I should have done, but anyhow, I was up there for nearly ten years.

RC: What sorts of ideas did they entice you with from Oregon?

JWH: Students, facilities, money, and everything, you know, none of which, turned out, they had.

RC: I have in my notes here that I should like for you to give me a brief history of the establishing of that lab at Oregon State.

JWH: Well, I was on a couple of visiting committees for some years before, and what happened was that there was originally a laboratory started by Oregon State at Charleston, Oregon, many, many years ago. After World War II, that property apparently, somehow, wound up in the hands of the University of Oregon, not Oregon State. Now those two institutions have yet to speak to each other politely. It started in 1927, when the state legislature ordered all science to move to Corvallis, 45 miles from Eugene, and all humanities to stay at Eugene and move from Corvallis to Eugene. Of course, the faculties of both places refused to budge. Lifelong fights were begun, which have never been resolved to this day in many of the departments. It was a very strange affair. Well anyway, when the University of Oregon wound up with Charleston, that was--I don't know--I think one of the problems with the people, with Oregon State, is that there were too many administrators who were born in Corvallis and went through all the grades and high school in Corvallis, and then became deans--a pretty tight little circle. But anyway, the fact remains is that when an Oceanography Department was put in at Corvallis, primarily through the efforts of Wayne Burt, and through the support of the National Science Foundation, and the Office of Naval Research, because nothing was being done at Seattle, now this is the part where some people are going to blow their stacks, but this is the decision to fund Oregon State was because of the inaction at Seattle. Now there's real trouble, because on the national basis, there's not enough money to keep all these places running. And I don't know what's going to happen up there. But that's another and more complicated story. Well, to get back to the laboratory, Oregon State was running an Oceanography Department and a vessel, which was docked at Newport--that's 100 miles north of Charleston.

So originally, they wanted some kind of a service building there, which they expanded a laboratory proposal and asked funding for, and didn't get it, because this long-standing problem between the two universities was not resolved to the satisfaction of the granting committees. Well, then, the laboratory got built because there was a program for public works projects for depressed areas, and I forget what department of the government that came out of, now. So the whole laboratory was built out of these funds, because it included a public aquarium and an auditorium, and so forth. The thing grew on the drawing board without much real planning.

RC: Do you suppose that was WPA funds?

JWH: No, that was long after WPA. This was Housing, Development, and Urban Something, whatever; I forget the exact title. But then, you see, this was '63, '64, or '62, I suppose, the funding happened. The main thing was, it was primarily Dr. Burt and the citizens of Newport maneuvering this; and suddenly some of these other departments woke up and found this thing was a reality. And they had the feeling that anybody who was brought in to work at that place was being brought in as a Trojan horse to further certain schemes in Corvallis. That's what made it very difficult to get anywhere. All that's ancient history now, but that was the kind of situation, which I didn't understand, until I got up there and started working with it. Ofcourse, now, all is forgiven. They cry deeply because I'm not with them any more and all of that sort of thing. It was virtually impossible to do anything.

RC: Were you limited for funds always while you were there?

JWH: Well, I think one of the things that isn't understood about a lot of the big oceanography departments in the universities, I expect this is true at A&M, I don't know to what extent it is, that the funding is not state money, it is federal money to an extremely high degree. Whatever else is done, say

that the overhead is manicured or laundered, or whatever they do with it, and then they call it state money and they pay other people. So these things are houses of cards, even more so in Oregon than anyplace else, because it's the only one there; and people don't understand that the state is not supporting this stuff.

RC: Which just happens to lead me to several more questions I had down on that. What do you think about the role of the ONR in funding Oceanography programs?

JWH: Well, the ONR more or less started oceanography on a big basis. As a matter of fact, my work at Scripps was funded out of an ONR grant, through Dr. Revelle. There was a fairly--in the palmier days of ONR, there was a fairly large amount of discretion there with money. At that time, of course, there was only about two places being funded by ONR, Scripps and Woods Hole, and some individuals here and there, to any massive extent. So ONR essentially made Oceanography; ONR, incidentally, started the National Science Foundation, which I think you must know.

RC: Right, but I have some comments I want you to make on that in a second, but I want to stay with ONR right at first. It seems from my reading that ONR is becoming more, let's say, mission oriented, rather than pure science oriented. Would you agree with that?

JWH: Yes. I had a grant with them for many years. They were supporting this long-term study in Tomolas Bay, and I tried to take part of this to Newport with me. Some of us split off; some of us still held Tomolas Bay. But then Senator Mansfield, I think it was, jumped down on us and said that Navy agencies should only do things appropriate to the mission of the Navy. The result is they cut all of this stuff out and we were given about a year's notice. But in Oregon, there was no way to pick it up. There was no salary support from the state or anything. In fact, I was supposed to

be making my own salary out of grants which was something that wasn't made too plain to me at the beginning. If I'd known exactly how they planned to fund it, I wouldn't have touched it.

RC: In this battle of pure vs. applied scientists, and military roles, I've received counter opinions. In your opinion, is the ONR moving too much towards mission orientation?

JWH: Well, I don't know though about it now. I used to be a member of the ONR's Biology Committee. I haven't heard much about, shall we say, academic, pure, or non-applied science, whatever you want to call it, except that they have a sentimental penchant for funding studies of sharks and things; as if they expected sailors to be exposed to sharks, like in the days of wooden vessels. Now, when you're in an atomic submarine, if the thing goes "plup", the last thing you're going to worry about is a shark. But nevertheless, a lot of that is still being funded. The sorts of things ONR used to support are just no longer done; and unfortunately, the great funding for Sea Grant, a lot of other things are not getting funded, because the Sea Grant will say "look at the National Science Foundation should support this. It's too academic for us." Then you go to the National Science Foundation and they say "well, look at this university-- your university's got all this money for Sea Grant. Why don't you get some of that? Don't bother us." This has been one of the say things; this, especially, hits studies in the sea more so, of course, than in the land, or--well we don't have something called terrestrial biology, do we. Maybe we should.

RC: Do you think it's fairly common to bring in academicians, particularly in the sciences, with the assumption that they'll fund their own salary through grants.

JWH: It's being done in many state and more and more private institutions.

Some institutions have been that way all the time. Now, another institution, of course, that had to do this is Wood's Hole, which has no endowment and no main academic affiliation. Woods Hole was a creature of the National Academy of Sciences. I think the philosophy which recommended that the Atlantic side of the continent have a strong oceanographic program carried with it the implicit obligation that the federal government was going to fund it. Well, they have done for many years, and now they're balking. One of the problems is the kinds of people who are serving as program directors in, say, the National Science Foundation. There's now a person who doesn't understand most of what is going on in Oceanography, who is trained, apparently, to study salt marshes. So he'll fund salt marshes, and he won't fund large-scale studies at sea.

RC: Is this Richard Vetter?

JWH: No. Vetter's not in a funding position; he's . . . No, this is a program director in NSF. Vetter is, of course, I guess, a member of one of the committees but these are committees that are--try--to just determine policy, recommend things. I think he's fairly civilized.

RC: Now how does the NSF come into being? How do you remember the story?

JWH: Well, it started out as a shootoff of the Office of Naval Research, which felt that it ought to fund some general things. This is partly through Sid Galler--I suppose you've got that name somewhere--and a man named Frank Cambell--I don't know whether he's still alive or not. He would be living around the Cosmos Club in Washington, and then used to edit the old Scientific Monthly, which has since been absorbed into Science. I don't know all the ins and outs, but then when the National Science Foundation was established, these people from ONR were taken in as the prototypes or leading spirits to get at least part of that program going. This is my recollection of it; I may stand correction for certain inaccuracies, but I

think the transition was through these people, though Sid Galler stayed with ONR for a long time.

RC: Do you think it was mistake to take the NSF out of ONR?

JWH: Well, no, it wasn't really taken out of it. What I mean to say is that the traditional kinds of funding and approach that Galler had started for Marine Biology became part of the NSF approach to things. It owed a great deal, I think, to his influence. But ONR Marine Biology stayed for the most part, where it was; and a lot of the things that they were funding before-- well, I wouldn't know if this is specifically the case, but an example would be that if ONR had funded studies in honey bees because of their ability to detect polarized light and go back to the hive, or whatever they do, well, that obviously was a tenuous relation to the Navy. That kind of thing was taken up by NSF along with a great deal more systematics; and then of course the whole thing is broadened out. One of the peculiar things about NSF is that each time they instituted a particular program, they more or less started the whole parade of university departments. For instance, they developed the program of molecular biology because they didn't know what to call these various fringy things in that field. So now we have Departments of Molecular Biology. There are several examples of that kind of thing.

RC: Like oceanography. Do you think there's such a thing as oceanography?

JWH: Not as--it's a synthesis of everything; so's Marine Biology, actually. It's very difficult to define "Oceanography" or "Marine Biology". It's what you do in the sea, I suppose, for the most part; though there are certain things, of course, that nothing else but oceanography is, like studies of currents and water movements and all of that thing. The cost of oceanography is increasing so much that the University of California Library, for example, has notified Scripps that they have ultimately planned to fund only books in oceanography and marine biology for the Scripps Library. This could close the place down. We thought of one way of

replying to this would be to list the contributions in the annual series that wouldn't qualify in either field and send them. And I went through the Treatise on Marine Ecology; and through one chapter, alone, I found that 43 or 45 of the 93 references were either in journals or had titles that fall outside these admittedly vague and comprehensive definitions. For a long time, there was some move, especially among AIBS, to get somebody to write a definition of marine biology, and I refused to do this. I said that when you do that, it becomes a formal program, and the NSF, anything that doesn't quite fit what you define is going to be in trouble. I didn't want to have any part of that.

RC: If I understand NOAA correctly, NOAA was created to take up some of the slack in the seeming loss in research funds in oceanography as ONR and NSF funds diminished.

JWH: That's so.

RC: You have no comment to make on that?

JWH: Well, NOAA has with it the Bureau of Commercial Fisheries, it used to be called and that institution has had a very long and unhappy history of arbitrary diversion and changing of direction of funding, quite often in midstream. And they're still at it. In fact, they cut people off back to the bone, transfer them around, and so forth. And quite often, what they've had, if they fund anybody outside--they don't do very much of this, I think, even now--if they need the money inside, they'll just cut your grant off and say "Sorry, we're out of funds" to keep something in their own program going. In other words, it isn't a very good policy of funding and continuity of research in that organization and never has been, apparently, not since--I'm not sure just when it began, but the tradition for a long time, in the Commissioner of Fisheries, was that he was a political hack. I remember once seeing a warrant-of-appointment to one gentleman as United States Commissioner of Fisheries, signed by William McKinley. Right now I forget the name of that person, but he was a lieutenant in the Navy. He was considered the worst appointment ever made to that position. I always regretted I didn't buy it: it was being sold in a

bookstore for \$7.50 as a McKinley autograph. At that time, I told that bookseller that it had a great more historical value as one of the worst federal appointments of record. Well that, of course, was long since. Now everybody's got to be Civil Service. But NOAA has not, really, done very much for anybody outside NOAA, or at least the old fisheries boys.

RC: Are there any particular people in Congress in general, or in the Executive Branch in particular, whom you would cite as being particularly friendly toward research and development in oceanography?

JWH: At the moment, I honestly don't know. I always thought that Cranston, in California, was reasonably intelligent, and of course, both Jackson and Mansfield in the state of Washington are eager to see oceanography move, primarily, to Washington; and, of course, to do that, they're willing to see it funded elsewhere as well, but less magnificently, naturally. But I honestly don't know. I've been out of the committee business for some time; at one time I was on both ONR and NSF pannels.

RC: What is the California Water Plan?

JWH: "Oh God, Our Help in Ages Past"! This is where I came in, I guess. The first professional job I had was studying the salmon in relation to Shasta Dam; and that was part of the Central Valley Plan, which has now become the California Water Plan. You ought to understand something about the geography of California before we get too deep in this. The central valley is one large valley with the northern end of the Sacramento to the San Joaquin. Essentially a large basin, with a break in the west side, which is the entrance in the San Francisco Bay. It rains up in the northern end, and it doesn't rain in the southern end. So the idea has been to build this great system of aqueducts, dams, and diversions to push water down the lower end of the San Joaquin Valley to irrigate it; and now, of course, all the way across the Tehachapi into Los Angeles. The water is actually moving down there now. The idea was to continue the growth of Los Angeles; and of course there's a Colorado River Association, which is absolutely rabid. Once they get all the water in Califor-

nia allocated, they hope to have the right to allocate the water in the Columbia, because the water rights will accrue to the people with the most political power, the most votes. This is the story of the Owens Valley. But in doing this, the water would bypass San Francisco Bay; the original plan was not to allow very much to get into the Bay, because it was felt by some of these people the Bay didn't amount to anything and that, after all, it got along without fresh water, didn't it--just a failure to understand the system. So we got embroiled in these hearings over whether or not one should allow some water to flow naturally into San Francisco Bay, rather than to be pumped all the way down to Los Angeles or the lower valleys. The first round, we won our point; and the decision was that the flow into San Francisco Bay would have to be increased to at least twice what the water plan wanted to do.

RC: What year is this?

JWH: Well, let's see, was it '65, '66, along in there; I forget the exact year now. I think the decision 1397 was that--'67 or '68--I don't have these dates with me. That was the preliminary decision, which is being fought bitterly in the courts. The water plan is not simple, because there are two things involved. One is the State of California; the other is the United States Bureau of Reclamation, which has told the State of California that it does not have to obey any of these rulings about water flow or anything; that it has the right of pre-emption, and it can simply tell the state to go to hell. Well, this is intolerable, because if this happens, they'll have lots of problems. On top of this Los Angeles isn't growing as fast as they anticipated; and if the seismologists are right, about six or eight months from now, it's going to not be worth being around there. If that Palmdale Bulge really pops, giving us a force-eight earthquake, I think every freeway in LA will be flat. San Fernando Valley was shake up enough. So then, there's a lot of people to move out of there. Then Mr. Peel says "that's good; we've got to have all these people; we can take

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care of them all. Technology will solve all our problems."

RC: Well now, the issue of California water rights is still in the courts, is it not?

JWH: Well, yes. The State of California is appealing to the Supreme Court or what-not, to settle the argument between the Federal Government and the state as to who has the right to move the internal state water around. Colorado River is an interstate compact, and that is sort of another matter.

RC: Have you actively been involved in this fight now, for how long?

JWH: Well, when I first started studying the salmon, I got involved in it by writing some things about it back in 1945 or so, '44; so I've been involved in it for 30 or more years. Maybe I was involved in it even before then, because our family was--had as one of the family friends one of the governors of the state, George C. Pardee; he was chairman of the first California Water Commission, which is the grandparent of all this problem. Old Governor Pardee once said to me, I guess I was in high school, that he liked the cut of my jib and don't change it. That was bad advice, like telling me not to read Ambrose Bierce, I guess. He would be a little bemused right now to find I'm one of the principal opponents of some of the things he stood for.

RC: I have a list of conferences here which I want you to describe your role in. Not very many, actually--First International Congress of Oceanography of the UN, in 1959.

JWH: Yes. Well, I was convener, and I organized one of the sessions of that, mostly on shallow-water animals and so forth.

RC: It has been suggested that out of this First International Congress of Oceanography in '59, more cooperation and exchange comes between the Soviet Union and the United States. Would you agree with that?

JWH: Well, we met a lot of the people there, and we became quite friendly with them. Some of them still exchange papers on that basis; some of us are still friends. I think the funniest thing about that Congress, it doesn't have anything to do with your question, is the great cocktail party held in the American Museum.

They had two or three enormous square stands in that great big lobby of all kinds of booze, sort of like British military squares it was all centered; and what I saw was the group of Russians reading, or one of them was reading to his colleagues those rather pompous statements of Theodore Roosevelt that are engraved in great columns: "I say, boys, the bull, the strenuous life, is the best life." The rest of the Russians were obviously convinced that their colleague's English had fallen apart, that he couldn't be translating what he was reading. You could see them looking, shaking their heads at the whole affair; well, the kind of thing I suppose I expected to find in the halls of Moscow. Then, of course, the Russians held one in Leningrad. I never got to that one. I didn't have the funding at the time. But since then, and quite so, they didn't show up in any extent in Tokyo, and that was not our fault, that was the Japanese, I gather. A whole boatload of them came in in a Russian research vessel and demanded to attend the Congress compliments of the Japanese government; and the Japanese says, "So sorry, everybody pays." I don't know, it was ten dollars admission, or something. So the Russians sulked in their ship through the conference.

RC: Who contacted you about the International Conference, or Congress, of Oceanography?

JWH: Mary Sears wrote and asked me if I'd help. I think that's how it began. She simply wrote and asked me if I'd help organize one of the sessions, so I did.

RC: What about the Ecological Aspects of International Development, in Virginia, in 1968?

JWH: Oh well, that was the Conservation Foundation and Barry Commoner's outfit. That was why the thing wasn't published for nearly five years. Aside from the changing of editors of the book about three times, and each editor had another idea of how to rearrange things. It was a mess. Well, that was done at the instigation of Ray Dasmann. Practically everything that was in there

was terrestrial, or a good deal of it concerned Africa, Southeast Asia, and all this sort of thing. So Ray thought somebody ought to speak for the ocean, so that was my part.

RC: What was the thrust of that conference?

JWH: Well, the book that was published out of it was called The Careless Technology, and the thrust, I guess you might say, was this feeling of those reasonably informed people who were doing a lot of things in our effort to carry our idea of industrial development to underprivileged countries, who weren't necessarily doing the things we thought they were going to do and, indeed, might be doing some harm. Classic things that now have been demonstrated "in spades," and that was building the Aswan Dam. It was predicted what that would do, and it is exactly what has happened; namely, cutting the flow of the Nile has increased the erosion of the Delta itself, so the sand is winding up in front of Israel--extending that country maybe a little--and in controlling the flow of the Nile, the annual washout, or what you might say, the rejuvenation and silting of the soil no longer takes place. And then when you get the regular irrigation pattern in semi-arid or arid countries, why you immediately start bringing the salt up and then you ruin the soil. So, after 8,000 years, Egypt may not have much future, thanks to the Aswan Dam. The other aspect is, of course, in the Mohammedan country, it is impossible to get rid of schistosomiasis. You have to work the fields standing in the water; and boots, if you have a pinhole, the larva of the flux will get in through the pinholes in the boots. Of course, in Mohammedan countries, if you go to the Mosque, you'll know what I mean--anybody can tell you that. I suppose you know what they do when they purify themselves. They have the Holy Water, and they put their hand in it, and they rub their ass with it first; and then they wash it again, and then they put it in their mouth. Well, that is one way to keep parasites happy. So, well, there are a lot of examples of that kind of thing. For example, of

taking milk to Southeast Asia, feeding them powdered milk. Southeast Asian children are weaned very young; adults don't have milk, or eat cheese. They lose the ability to digest milk; it's bad for them when they're adults; and you'd think that some people would know this before some of these massive . . . well, this is what that meeting was all about, things like that.

RC: Who is Frank Campbell?

JWH: Well, Frank Campbell was the editor of the old Scientific Monthly, which was finally abandoned in favor of The Sciences, which is the only thing Triple A publishes now as a regular journal; they used to publish two. Well, The Scientific American was doing it in, and the other thing is they decided to have longer papers in science and would occasionally take up this. And then the other thing is the American Scientist, the monthly of the Sigma Psi Society, also does about the same thing the old Scientific Monthly used to do.

RC: Who would be primary organizers of this conference in Virginia?

JWH: Oh, John Milton, who's working with the Conservation Foundation, and a guy named Tigee Vavor, who was working with Commoner, were the people who did most of the work. I suppose that Barry Commoner and Russ Strain had a great deal to do with it. He was, at that time, the secretary or director of the Conservation Foundation.

RC: When did you become interested in the Conservation Foundation?

JWH: Well, I didn't really know much about it. I've known Ray Dasmann for years.

RC: Spell his name, please?

JWH: D-a-s-m-a-n-n, Raymond F., I guess. He's an old collage schoolmate sort of person. He is in charge of, has something to do with, the International Union of the Conservation of Nature in Morges, Switzerland.

RC: And when the contact was made, they simply said that this was the sort of problem we're going to investigate; or, in other words, was the conference preplanned this way or did these ideas just evolve from the planning of . . . ?

JWH: Oh no, they preplanned; they had all these various topics to discuss. What I

discussed primarily was the effect, the possible effects, of atomic waste disposal in the seas. This was one of the immediate technological problems we are faced with in the ocean. What effect, if any, it's going to have, we still don't honestly know very much about it.

RC: A conference called "No Deposit, No Return," UNESCO, San Francisco, 1969?

JWH: How did I get mixed up with that?! Dasmann was there again, but I don't know; now I can't remember who invited me to that. I had an awful frosty reception from Dave Brower, that's neither here nor there. There were two or three things that happened right along there. There was another one in Chicago. I think this primary Conservation Foundation sort of kept on reacting for awhile.

RC: Then was "No Deposit, No Return" involved . . .

JWH: That was a two-day meeting, and mostly of younger, environmental-activist types at the time; then a lot of old, mossy heads, like Arthur Godfrey, were there and the local California leaders. _____ and Irlick were there fighting for the headlines, each trying to say something more astounding than the other.

RC: Primarily, then, you were involved in the idea of population control, population zero, there?

JWH: Well, that was one of the main themes of the thing, but I don't recall what I did, if anything. I think I was just invited to attend, to represent--I was in Oregon at the time; I guess I was just representing Oregon or something. I don't remember just what--it didn't amount to much, and there wasn't any published proceedings of consequence out of it, except for a little paperback.

RC: Alright, now, what I was interested in, really, was the Society for the Prevention of Progress. When did you become involved in this, or, in effect, organize this; what was "progress"; did these previous conferences help extend this idea?

JWH: The Society for the Prevention of Progress was established about 1941, long before any of these conferences and things, simply pointing out that the days of man were numbered at the present rate of exploitation.

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I suppose--whether this had any influence on Correlli, apparently it had some on Garrett Hardin, that is, some of the other things I wrote at the time. That was when I wrote the thing called Progress, the Flower of the Poppy. I don't know whether you've got that.

RC: Yes, I have.

JWH: Well, you see now, all these things added to these other--so I guess I got invited to these things as a result of these various environmental polemics through the years.

RC: Who else was involved in the Society for the Prevention of Progress?

JWH: Well, it was simply a letterhead I cooked up an old Frocteur type to provoke people, and so forth. For a while, it was considered very unseemly to say things like that, but now even the Sierra Club seems to act like a chapter of the Society for the Prevention of Progress. At that time, they didn't want to be identified with that sort of thing. Times change, I guess.

RC: Before we leave this idea, I want you to say just a few words about atomic wastes. Did your comments on atomic wastes and your opposition to atomic wastes being placed in the ocean, did these things follow from, for example, your early work in conservation; did one seem, logically, to follow another?

JWH: Well, I got mixed up in the atomic waste problem in a very different way, really. It was a Dillon Beach, there, six miles north is Pedega Lead, and the outfit that calls itself the world's largest power company--or public utility, pardon me--announced that they were going to build an atomic power plant on Pedega Head. What they announced was that they were going to build a power plant. They hadn't decided whether it was going to be atomic power or not. First thing everybody discovered was there was no way anybody could stop this, according to California law, because public utility has power condemnation--they can go right in and say they want the land and buy it, even if people don't want it there. But, of course, they had worked for a couple of years

before with the local politicians, getting them all stirred up about all the taxes they'd collect out of this thing. Well, basically, I didn't like the idea of the way they were proceeding, so I started to needle them. My first needle was, "Well, have you thought anything about the San Andreas Fault?". Well, it turns out they hadn't. They dashed out and hired a geologist, and they kept playing Pat and Mike this way, and I kept fiddling with them. Well, some other people got annoyed at the whole thing, and some of them started raising the specters of atomic power. I didn't know enough about it to say anything about that, and I've stayed away from saying overtly some of these mean things about how dangerous they are, because I don't honestly know. I do think that they're so damned complicated, it's going to be almost impossible to avoid some kinds of incidents. Most of these may not be associated with nuclear power, but with the operation of the whole plant, which means that power is unreliable. That's neither here nor there. The thing is that this went through condemnation and got fought tooth and nail. The other was that the University of California wanted to build its laboratory there, and they were the first to tell me about it, before the power company made it public; and so it was partly for them that I didn't like the whole damned idea. So we fought this, mainly, as far as I was concerned, on the grounds that it was not the wisest and best use for that particular piece of real estate. The original idea and the original plan was that it was going to be a park. It's a very spectacular piece of scenery. So it's now a park. Well, in the course of that, of course, I got identified with power. Dasmonn, I guess, was the fellow who asked me to write that thing for the careless technology, and I said, "I don't know that much about things, but I can always work up the piece." So I did. I have not done any original work or anything in atomic energy. Just a bystander.

RC: Did your conservation work help to shape your ideas on the history of marine biology?

JWH: Not too much. That's another thing entirely, except that I was interested in

the changes in the seashore, naturally. But at that time, no. Some of the other people, Mary Austin, for example--Ever read the Land of Little Rain?

RC: Only after you cited it. What made your eras since the "Challenger" expedition; would you cite in marine biology?

JWH: Well, of course, the "Challenger" set in the age of world-wide exploration. Mostly to find out what was there. And then the thing that happened next, I think, was mostly fisheries investigations, the International Commission of Exploration of the Sea. A lot of this was oceanography in a very refined sense. Some of the critical work in oceanography was done in Scandinavia, partly in response to this kind of need, I think. That is the Diekness, and Eckmann, Nunson--Hetterhenson, and so forth. Mostly Scandinavians who apparently went on long Arctic cruises and sat around hot stoves to think in their ships. Well, that's another story. Then there was a break. It wasn't until the 1930's, really, that people began to be concerned about oceanography again. The National Science Committee--and this was due in large part to the influence of T. Waylen Vaughan, who felt we didn't know enough about the oceans, and was a member of the National Academy and director of Scripps; he was the person who changed Scripps from Marine Biology Institute to Scripps Institution of Oceanography. That was what--1924 or so? And so he started some committees. The result of this was, then, the big National Academy Committee, which recommended more funding of Scripps, of Seattle, and the establishment of Woods Hole. I think the date of that was 1930. So then Woods Hole was built, and the "Atlantis" was purchased, and they started poking around in the Atlantic. A good deal of the work was still shore related. Some of it consisted of physiology and marine ecology sorts of things. I suppose the next really big impetus came in World War II, when we discovered how little we knew of certain kinds of things, like the way the waves roll up over coral, and stuff. That cost a lot of lives, of course. One of my cousins was killed in Tarawa. So, then we had the postwar epic, in which we went to the idea of relatively

small ships doing separate missions, rather than very large ships of multi-mission, like the Russians do. And that flourished up into the '60's, and now it's tapering off, in spite of these occasional big splurges like IDOE and this thing out here in Africa and the Atlantic--what the heck was the name of that thing--Equalarms? Northpack, and so forth. There have been some attempts at international cooperation with the ships of several nations. I think this "Law of the Sea" fiasco is crimping all that for the time being.

RC: By "Law of the Sea", do you mean claiming miles?

JWH: Yes, well, all these meetings claiming the milage had already--down in the Falkland Islands, they had a little trouble, the Argentines wanted the closing off to any research. Everybody's out looking for everybody else's oil, now, or something bad.

RC: What about your interest in the Arctic?

JWH: The Arctic! I don't have much interest in the Arctic. It's the Antarctic, I guess, I got interested in. Well, of course, a long time ago, since my little sea spiders are most abundant down there, I've been interested in where they live, and so forth. And one thing led to another, and I got a grant to study the things down there.

RC: And how long were you there?

JWH: Well, I was down there for short trips. I had a student down there for 15 months, in Palmer; so I was down there, what, about three weeks one year, a month in 1970, and then I was sent down there a couple of years ago to do some work on an impact study, which I don't know what they're going to do with now. I sent them my part of it; I gather they don't like it too well; so I went to the bank, smiling, and will let them figure out what they want to do with it.

RC: Do you do many impact studies?

JWH: Not the whole thing, no. I've been helping out with a couple of bits for

friends of mine who are in this business, and mainly criticizing them, and that sort of thing.

RC: Given your interest, it seems you would do counter-impact studies...

JWH: Yes, you're right.

RC: Most impact studies I've seen, they already know what they're going to do and all they want are impact studies to reinforce it.

JWH: I know; and they're full of garbage. In terms of a list of species--in fact, a number of us have thought maybe of people who were involved in handbooks, and manuells, and things of that type. We ought to charge these people royalties for copying whole pages of the appendix of these handbooks of species lists and things. That's kind of cheating, if you ask me. Sometimes they don't even tell you where they got them. They keep everything in the exact order, and sometimes the same typographical errors, so that gives it away.

RC: Now, in addition to your work in science, and in ecology, also other writings I'm interested in--Poems in Contempt of Progress.

JWH: Well, that started mainly as a lot of polemics about this Padega thing, various things. Malvina Reynolds was going to sing some of them on the radio at Berkeley, along with one or two she had written; and the power company threatened to sue the radio station for libel, though she offered them equal time for the Ready Killowatt song. So that's how that began, actually; but I've always written doggeral, which is mostly what this is, of course, I used one of my old family names to gloss it over.

RC: Well, I'm interested in your attraction for the arts. Is it good to combine the arts and the sciences?

JWH: Well, I don't see why not; hardly what I do is what you'd call art.

RC: Should scientists write with more of a flair? Let me try it another way . . .

JWH: I don't know about that; they ought to write decent English.

RC: Is your attraction for what seems to be a combination of the arts and sciences--does that come, perhaps, from your contact with Ed Ricketts?

JWH: Not particularly. We were already that way. We were very much alike in that sort of thing. I guess neither of us influenced the other. There wasn't any need. His main influence was on Steinbeck.

RC: Which was . . .

JWH: Well, Steinbeck didn't know anybody like Ed, who had a general fund of culture and interest in all kinds of things. He sopped it all up. Some ways, Steinbeck was like a country boy, absorbing everything that came down the road. Those early stories, for example, he picked up around Monterey and Pacific Grove. There were some people there who were telling these all the time; and he used them in Tortilla Flat, Pastures of Heaven, and so forth. I think he told them he was going to do that. Some people resented it, and some people didn't. Anyhow, that's another story.

RC: How much contact did you have with Steinbeck?

JWH: Well, I met him--I don't know--three or four times, I guess. Last time I saw him was after Ed's death, and he was in Pacific Grove. The family owned the house there. In fact, one of his sisters lives in it now. They've always had that house. Steinbeck used to go to Pacific Grove and met Ed there. It was a very interesting thing; about half the people involved in that group, except Steinbeck, I don't know, were so-called "termites." Ed wasn't, because he came from Chicago. Termites were the bright children selected by the Stanford psychologist, Turnin, for studies of their development. He gave a series of tests to them in California schools. They picked out these kids who--I don't know if it was IQ or what he used; there were several tests, I think; he checked all the schools. But anyway, out of a group of ten or a dozen, they were always running around together, I think seven or eight of them were termites. A high percentage. Ed's friend, Tony, was; and, well anyway, in the Depression days, when Steinbeck started to learn to write, there was a group there working for WPA, writing a guide, and all that sort of thing, living on wine and spaghetti, and so on.

RC: Were you a termite?

JWH: Nope.

RC: Then you were drawn into this sort of coterie of his by these letters.

JWH: That in part, but I take my mother down there to call on her family friends. Her friends would go on talking all night, practically. I'd go down to Ed's to talk around and find out what he was up to and drink wine along with everybody else. That was long before Cannery Row, of course. In fact, we always worried about Cannery Row, whether Ed would try to live up to it.

RC: Did he?

JWH: Not really.

RC: I see. My impression is, from the introduction to the book, that you seem to think that Steinbeck caught only one-half of Ed Ricketts, that the other half was just Steinbeck's imagination.

JWH: Well, I don't know if it was imagination; a lot of people knew them longer and better than I did. They all agree that Steinbeck really didn't understand what Ed was all about. That includes Joseph Campbell, who used to live there. Joe ought to do the "Private Mythology of Joseph Campbell" for his next book. I told him so, but I don't know if--he's kept a kind of a journal, I think. I don't know how many years, writing a few pages every day and putting it in a filing cabinet.

RC: I have a series of questions I'd like to ask you to begin commenting on. Nature and freedom vs. materialism--the basic thrust of some of the things I read is that materialism would have to be sacrificed for the purpose of nature, which seems to equal freedom.

JWH: Good heavens! It's kind of late in the afternoon to get involved in such a topic as that! I suppose that's the general idea. Well, the more you're a slave to material things, the more you're going to be a slave to all the rest that goes with it, like earning money, and grubbing around . . .

RC: In this context--I want you to think about this--should there be less science?

JWH: I don't know; it's hard to--what do you mean by "less science"? When you look

back in the 18th century, a lot of the country gentlemen were doing very interesting things, and they were as competent as a lot of university professors, at least for their time and place, and what they were doing and thinking.

RC: Well, my second question then: Should science concentrate on natural history again, which is what you're implying?

JWH: Well, we've certainly neglected a lot of it, and now we find ourselves in the middle of trying to decide what's going to happen to our environment with all the things we're doing for material gain. Certainly we have to find out more about the natural history. We've tended to neglect it, that is, natural history in the sense of "natural philosophy" in the broad sense.

RC: Would you suggest that the way such things as the disciplines of oceanography have been funded naturally leads to science not being science but being technology?

JWH: I'm not sure about that, especially in oceanography. There's not--a good deal of it hasn't resulted in any particular technology, except designing instruments and that kind of stuff, to go out and find more data. That is in the sense that chemical research has produced all the stuff we live in--and out, and with.

RC: I think what I'm asking is--in the hard sciences, or applied sciences, do they seem to be funded with other things, less funded than they had been 15 years in the past?

JWH: I think there's a little less funding, but I don't know whether the proportion is too much changed. The hard sciences have been favored, because I guess they've put up the better case, at least to the administrators and fund grantors.

RC: Do you feel as if your career has been furthered by the funding of hard sciences?

JWH: I haven't really noticed.

RC: Well, as I close this off, what I'm really aiming at--let me be fair to you. This idea of nature and freedom in terms of how it's helped to develop your life, is it possible for a scientist to enjoy this kind of freedom if he, indeed, accepts funds or works for universities?

JWH: In one way yes, but in another way no, because, if you're going to get yourself

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tied into being supported for all these things, then you've got this treadmill of having to write more and more proposals and spend more and more time, one way or the other, trying to keep the money continuing to flow in.

RC: I've charted what seems to me like three or four patterns in your life, significant to appear; and possibly you'd like to correct me on these or comment on these, as we close off this first interview. First of all, it seems like you sort of drift--"drift" is a poor choice of words--you guide yourself in academic research and writing; then you withdraw and you freelance for a while. Is this an attempt on your part, personally, to attain this freedom that you seem to . . .

JWH: No, I had the most perfect freedom; I didn't have a job.

RC: Okay, so rather than being deliberate, this is a way of employment.

JWH: Right.

RC: Secondly, there seems to be a heavy overtone throughout all your writings of a search for literary excellence. Did you find your work in the scientific field hampered you, so that you move back in this direction, away from working in the--

JWH: No, but there isn't too much that you can do just if you're writing technical monographs on strange little animals, hardly a vehicle for writing anything about ideas or anything.

RC: Third, you seem to work more in marine laboratories than at sea. Was this deliberate, or was this just the nature of your research?

JWH: Just the nature of the research, I think. Also, if you want to work at sea, you've really got to justify your proposal to pay for the ship. That's something I've never bothered with.

RC: And your work in laboratories has kept you, in effect, on the Pacific coast. Is this where your kinds of creatures are, or is this your environment that you prefer?

JWH: Well, the environment's fine, but you get into this problem that there are different kinds of animals on different coasts. For example, Howie Saunders, who works in Woods Hole, turned down several offers to come to the West Coast because then he'd have to learn a whole new formula; he wouldn't feel at home for several years

in it. And a naturalist does get somewhat associated with this sort of thing unless you're working with certain special groups on a world-wide basis.

RC: In summing up what you would consider your accomplishments, are there any particular accomplishments which give you greater scientific satisfaction than others?

JWH: Well, I suppose editing the Treatise was one thing. I found a great source of satisfaction producing a nice fat monograph, even if the reading is limited. However, we'll have a world conference with our friends in London in October, on the pycnogonids, believe it or not.

RC: So you think this sort of scientific work is what you feel you derive the most satisfaction from in terms of . . . Alright, what about your other work? Have you found that more satisfying, let's say, or equally as satisfying, as your work in the "sciences"?

JWH: Oh yes, I think so. I just haven't finished it yet. I've still got some things to do that I haven't done to date before I get put in a box and shoved off.

RC: Would you care to list the academicians or friends who've had the most influence on your work and on yourself, people whom you perhaps have not mentioned on the way through?

JWH: Well, let's see. I used to have this handy; I don't know why I can't think of it right off the moment. I suppose that Dr. Lite was one of them. One of the people I suppose most influenced me was George Stuart, who had the only really good writing course I had in college.

RC: He taught?

JWH: Non-fiction writing. Actually, he taught a course in biography, the vehicle for teaching non-fiction. He restricted it to a few students every other year whenever he gave it. And well, Carl Sour, I would say, was pretty much an influence on me, especially later college years.

RC: What did he do?

JWH: Well, he's deceased now. He was supposed to be one of the great American geographers. Carl Ortwen Sour. He wrote a very nice--well he didn't write; it was a collection of his essays put together called Land and Life, University of Cali-

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fornia Press.

RC: What direction do you intend for your career to go in the future?

JWH: Well, I hope to finish up a couple of these things I'm doing, I should say.

This historical treatment of marine biology, for example. And I'm working on-- finishing up--the book about Ricketts and Steinbeck, or actually about Ricketts' unpublished papers. We have pretty good evidence of how much influence Ed had over John Steinbeck in this material alone. For instance, the parts of Sea of Cortez that everybody thinks most Steinbecky is really Ricketts, the non-theological thinking parts, that sort of stuff.

RC: Did Ricketts read Steinbeck's work and make comments on it in draft form?

JWH: I think so, yes. As far as Cannery Row goes, I think it was simply presented to him as a fait accompli. I think that it was a published book that he saw first. He knew Steinbeck was doing something. No, I know he read a lot of Steinbeck's stuff now and then. I had a great argument about The Grapes of Wrath and about that last scene. Ed was one of the people that persuaded Steinbeck to keep it. The publisher questioned it, whether or not he ought to get rid of it. That's the scene where this starving man suckles this woman whose child has died, and all this sort of thing. It's a bit mawkish when you describe it that way, isn't it?

RC: It doesn't read quite that way, though.

JWH: Yeah, I know, that is true. Anyway, . . .

RC: Looking back on past accomplishments then, do you see your future, perhaps, in more literary trends?

JWH: Probably.

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TEXAS A&M UNIVERSITY
ORAL HISTORY REPORT
INTERVIEW #2

INTERVIEWEE: Joel W. Hedgpeth

INTERVIEWER: Robert A. Calvert

PLACE: New Orleans, Louisiana

DATE: June 1, 1976

TIME: 9:50 a.m.

RC: Professor Hedgpeth, you mentioned your father. I have a note saying your mother taught Indians in California. Is that correct?

JWH: Yes. It was a Presbyterian teaching missionary, called the home field in those days. She went to northern California in about 1900, Hopi were the Indians.

RC: What Indians now?

JWH: Hopi.

RC: Hopi. Okay.

JWH: Where the Trinity River meets the Clamouth.

RC: Well, how did your father meet your mother?

JWH: Well, she was then, her second assignment was down in Medara County, a place called Northfork. That's up in the hills back of Fresnoe. He was living there.

RC: Who is Henry Hemphill?

JWH: Oh, Henry Hemphill, he was an old, I think he was originally a Mason, but he's a brickmason, a brick layer. And he got interested in collecting shells, he had a collection of them. He became very profient and collected for a lot of the big museums and things like that. And the bulk of his collections forms the nucleus of the collection at Stanford University.

RC: What about Clibert?

JWH: Well, no, that's Klyver. K-l-y-v-e-r, I believe it is a Danish name. He was my freshman, sophomore teacher in biology in college. Fred J., I think was the rest of his name.

RC: You say "unscheduled chemical experiments." What in the world were unscheduled chemical experiments?

JWH: Oh, I was mixing up various things which, of course, would explode. We were making torpedos to put on the streetcar tracks. You're not supposed to do that in a school chemistry lab!

RC: I take it they took umbrage at it.

JWH: Yes, they gave me various kinds of lectures and so forth.

RC: What exactly is the role of naturalism, or naturalist, excuse me, the role of the naturalist in oceanography? For example, Jacques Cousteau.

JWH: Well, I don't think one should call him a naturalist. He was an engineer and gunnery officer. He invented the critical little valve along with a man named Ganya that made the free diving system possible, and they still prefer to call it the Cousteau-Ganya apparatus; and we call it scuba now, which is an inelligent, accurate M2 scuba. The nearest thing I could find to any word or any language which meant that after asking the people, apparently it's a Norwegian word which means to jostle around or something, but that's neither here nor there. No, Cousteau, of course, became interested in the exploring shallow seas, since he knew how to use this apparatus, having invented it naturally. And it was quite a breakthrough, of course. The primary purpose, I suspect, originally in doing this at all was military, for salvage and other under-water operations. The French, you should realize, have always been leaders in diving. I think the original hard diving suit was a French invention, essentially. And so Cousteau wrote a book about this in collaboration with someone else, mostly observations of what you see there. He also made a great sensation in Archeology by finding various sunken vessels in the Mediterranean from the Classical era. The work had a great deal of public appeal, so his books sold very widely, and that was how-- I really don't know too much about Cousteau's actual history; but now, of course, he has this ship, the "Calypso", which I believe he owns, so he has to keep that going, mostly by sale of books and the television pictures, and so forth.

RC: I was wondering if you, as a trained scientist, objected to what seems to be the

impact of naturalists on public opinion.

JWH: Well, I think what annoys most of the oceanographers is that Cousteau's very careless with some of his facts. He went around predicting that the oceans will die in 30 years, which cannot be substantiated. And so there's been a certain, well, possibly resentment, certainly irritation on some of the--on account of some of these things. On the other hand, he is marshalling considerable public opinion on behalf of conservation and caution with what we're doing to the world, so he's a mixed blessing.

RC: When you worked in the Civil Service in Washington, were there other clerks working around you at that time who also had scientific training?

JWH: Not in that particular unit. I don't know about some of the others. Oh yeah, there was one fellow there who was a zoologist from Mississippi; he went on and got a job, finally, teaching at George Washington U. And unless he's retired now, he's probably still there. I used to see him once in a while. But most of the rest of the people were just ordinary AB's and mostly--oh anything, sociology, accountants, and so on.

RC: So your relationship with the Smithsonian leading to your publication of the monograph was reasonably unique then?

JWH: I would say so. Yeah, I was the only one I know of to do anything like that. Of course, this grew out of the fact that I had already been corresponding with Dr. Schmitt, and so on.

RC: You'd corresponded with Dr. Schmitt before you arrived in Washington?

JWH: Yes. He was the curator of invertebrates, a matter of identifying the specimens, and so forth.

RC: Did you have a comment that you make about developers, and is it Hetch-Hetchy?

JWH: Yes.

RC: Was this a housing development?

JWH: Heavens no! Hetch-Hetchy is a dam; that is, Hetch-Hetchy is a valley. It's north of Yosemite Valley, and it's very similar. It's also a U-shaped, glacially-formed

valley. It's on the Tuolumne River. That's something you'd better spell. Maybe easier if we write that out. Hetch-Hetchy is H-e-t-c-h-H-e-t-c-h-y. It's supposed to be some sort of Indian name. And Tuolumne is T-u-o-l-u-m-n-e River. And the dam was to provide a water supply for the city of San Francisco. But of course, it was right at the end of the glaciated region, and so it was very high up. So naturally it produced some electric power, too. The water has to fall, so they ran it through tunnels and generated a lot of power. And this is still--Congress passed an act requiring that this power be sold to the public, and that it be handled in a public way, but they have leased it to the Pacific Gas and Electric Company. I think this is still considered a technical violation of the law which was the Raker Act (R-a-k-e-r).

RC: Now in your interview concerning oysters and how you arrived at the University of Texas, is this the same project whereby oil companies financed people to discover if oil were killing the oyster shelves and oyster beds?

JWH: No. I got there--I went to the Game Commission; I was there before that. And I was never part of Project 9 that I knew of. Of course, at that time, I didn't ask where the money came from at the University of Texas. But Dr. Lundt--I think it was a regularly designated position.

RC: What is Dr. Lundt's first name?

JWH: Well, he always went by E.J. His name was Elmer Julius, quite unlovingly known to some of his students as "Elmer Jesus."

RC: You say that Dr. Lundt sent you down there to protect and validate the insurance. Does that mean to make sure the building was occupied?

JWH: Right. Well I was already there. I was at Rockport, about 15 miles across the bay, so I really didn't have to move very far, because Port Aransas is out on Mustang Island.

RC: Now you say that you drew maps and diagrams, and that sort of thing for Rickett's book. Do you have that kind of training?

JWH: Well no. I was just pretty handy with pen and ink. You copy things; I'm not an original map maker.

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RC: Then these were copies and not original cartography.

JWH: Yes. Well, of course, the things we were doing was putting on the isotherms and that sort of thing, showing where the temperature conditions and that sort of thing in the ocean, near shore.

RC: Did you write your monograph on your Japanese before you went to Berkeley, too?

JWH: No, in between. I went to Berkeley as an upper division student, graduated in '33.

RC: Right. And then, when you come back to Berkeley, you already have a monograph?

JWH: No. Then I went off on this salmon job. I came back and did a master's degree on some copepods and special-order tongs. And after that, in '45, I went to Texas. I had written the Gladding monograph and finished the Japanese one down in Texas, while working on the oysters and so forth. Then, after I got through the University of Texas and decided to leave there--let them stew in their own juice--why then I went back to Berkeley as a Doctoral candidate.

RC: Then when you returned to Berkeley, you had both a monograph based on your American research and a monograph based on your Japanese research.

JWH: Yeah, well actually it was all American specimens. They were taken by the research vessel, "Albatross," which in about 1903-06 did a lot of dredging around northern Japan and the Philippines, primarily the Philippines.

RC: Exactly what do you mean when you say "bring a text up to date"?

JWH: In this text is a handbook--well, it's between Pacific tides--bringing the text, not the book. It's not really a text, it has been used that way; it's a guide to the seashore animals and commentary on their habits, and new things turn up. Things have to be changed, and all this sort of editorial refurbishing, and so forth.

RC: You say when you go to University of the Pacific that you revise the course content and the like. Does that mean that you established a new course curriculum at the University of the Pacific Lab?

JWH: Well, yes, I guess we did, at that. We established a number of new courses. Mostly, we invited people to teach these courses during the summer term, the way a lot of marine stations have worked up until fairly recently. Quite often inland in-

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stitutions require their students to study zoology, botany, biology, generally, spend some time in the field station--they came from Kansas, or someplace like this--preferably on the seashore, so they'd have some first-hand experience of this kind of thing. So we had to allow quite a clientele. Unfortunately, that requirement is pretty much faded away in most institutions. Biology has become so diffuse, I guess that's the reason.

RC: Why in the world would regents want a marine lab? It sounds very unregentlike to me.

JWH: The regents don't want--I don't know what the regents want. They're an amorphous body. No, what happened was that one of the members of the faculty there wanted a marine lab. That was Dr. Alden Noble, and he had a wealthy friend who gave the initial impetus and provided the funds to build it and the materials. His business was making pencil stock, a very special kind of lumber business, and cedar, of course. You may not know it, but most of the European fancy drawing pencils we get are shipped in as lead; and then the wood is put around them in this country, and they're stamped, and so forth. He had, I think, a special monopoly on the cedar pencil stock in the United States. He used the incense cedar in California. So he had these three-inch planks--that's three inches through--of cedar. There's a dry rot that gets into cedar, so there's big spaces and holes, as I remember, all the way through this plank this thick. It's called Pecky Cedar; it's now considered very artistic to build with it. The rot stops when the tree is cut. Quite often you take--of course, a pencil is only eight inches long, or so--and they cut little blocks, and then they cut that into thin slats. Quite a specialized drilling operation; so I call him The Pencil Baron; but anyhow, he donated this lumber. It's still there, still very sound.

RC: Who was the man from the University of Chicago who cooperated with you?

JWH: Ralph Johnson, Ralph G. Johnson. He's now Chairman of the Department of Geophysics, at the University of Chicago. I wonder whether they call it Geophysical Sciences? He's actually a paleontologist.

RC: You say that it's going to take a national-basis to keep all these labs running in the Pacific Northwest. You mean national funding to keep all the labs in the Pacific Northwest operating?

JWH: Well, yes. Stanford University has a SAE established fund for operating their station, which is the oldest on the Pacific coast. It was started in 1892; it was in Pacific Grove, back near Cannery Row. But I think I, mainly, was talking about the oceanographic laboratories and ships, rather than the small marine labs. Some of these run pretty well on tuition, like the one at Charleston, University of Oregon, for example. There are a number of small, independent ones; the Adventists have their own laboratory on Puget Sound. I don't know just where it is. The University of Washington has a marine laboratory, Friday Harbor; it's on San Juan Island. That is run primarily by the University, though it gets research funding from federal sources.

RC: Well, have we always been more active on the Pacific coast, in terms of work in oceanography, than the Atlantic coast?

JWH: No. Some of the earliest oceanographic work started on the Atlantic coast. Louis Agassiz and Count Fotalis were using research vessel, "Blake," and Alexander Agassiz and his son used "Blake;" the other one was some other government ship. So the first explorations were in the Atlantic. Then, about 1883, the emphasis shifted slightly to the Pacific, when they started surveying cable routes across the Pacific. "Tuskarora" did that. What I think would be oceanography really began to grow in the 1930's. They had two centers of interest, or, at least, promise, on the Pacific coast, Seattle and La Jolla; and the other one was put in at Woods Hole, because there are many more states in the--most of the universities and so on on the eastern seaboard are not right on the shore. The big ones--well, Yale may come nearest to it, being there at the edge of Long Island Sound. So the emphasis did shift to the Pacific for a while. It's a much larger ocean.

RC: Is this the reason why the National Academy of Sciences wanted to establish Woods Hole?

JWH: It was part of the reason, to have a two-ocean effort, yes. They felt, of course, that actually there's a lot of things we need to know about the American side of the Atlantic Ocean. They still do, of course. Of course, as you realize, I suppose, for the first oceanographic observations of any consequence about the Atlantic Ocean were made by Benjamin Franklin, who took bucket temperatures while going to England and back and proposed the first tentative map of the Gulf Stream.

RC: What'd you do in Tokyo? What takes you to Tokyo? You mentioned the fact that you were in Tokyo . . .

JWH: Oh yes. Well, there was an international oceanographic meeting, and I was asked to chair a session there.

RC: Is this because of that earlier monograph you wrote?

JWH: No, not entirely. I guess it grew, in part, out of the Treatise on Marine Ecology, of course; but they had to find somebody to represent the biology. The other gentleman was a paleontologist. He was from Germany.

RC: You said you received a "frosty reception from David Brower." Is that b-a-u-e-r?

JWH: Brower. Oh, that was just. . .Brower, he's the arch-Druid of the Sierra Club, now the Friends of the Earth. No, what happened was I wrote a somewhat critical review of a book about the Galapagos published by the Sierra Club. He didn't take kindly to a couple of my comments. But I did feel that in preparing that book they missed a good opportunity to put in some necessary things about the Galapagos. The idea was to make it look completely primitive, and so forth. They could have brought in some other things, and we did have a bit of purple prose in it; but anyway, that was--the review never hurt the sale of the book. It never does, in those coffee-table books.

RC: That's true. Your work in the Antarctic, some comments on it. Exactly what were you doing in the Antarctic?

JWH: Well, these lovely little animals, the sea spiders, the pycnogonids, are very abundant in the Antarctic, and the very largest kind live there. I was interested

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in observing them in life, and possibly finding out something about their chromosomes, and the various phases of mioses, and so forth, made up a fish, a white fish. They came out very clearly. But there's so many chromosomes in that thing, in spite of the fact that it's been used as a classroom display for years, the total number was never accurately counted; so when you have an awful lot of them, you can't really make them out. At any rate, we were interested in observing these things in life. One fellow wanted to find out what kind of blood pigment they had, and so on. So I went down there...The first was more or less an exploratory trip, to see what was there. At that time, they were trying to encourage a lot of people to get into Antarctic work, rather generous about shipping people down there to look it over, and so on. And out of this I developed a grant and sent a young man from England there for two or three months the following year.

RC: From whom did you receive your grant?

JWH: The National Science Foundation.

RC: Why do you say Ed Rickett did not understand Steinbeck?

JWH: No, I said the other way around. Steinbeck didn't understand Rickett. He-- well, Steinbeck was a very interesting person in some ways. He wasn't much of a reader, and all the critics say his books don't quite come off, or something. Though, whether this is entirely true . . . But in discussing philosophy, the kind of philosophy that Ed was trying to propound, I don't think he quite knew what was going on. It's a long complicated story.

RC: Is the Joseph Campbell you speak of the man who does the work in Mask of Primitive Ecology, and so forth?

JWH: Right.

RC: You say you have mixed emotions about atomic uses. Do you mean mixed emotions about the establishing of nuclear energy plants?

JWH: Yes. Of course, the problem is the waste disposal problem, mainly. What on earth are we going to do with all that stuff? At the rate we're going, it

doesn't look like we're going to have any sort of perpetual motion, alias fusion-power for quite a few years, by which time we're going to have an awful stock pile of stuff around.

RC: Well, in the present controversy in California, over referendums on nuclear plants or nonreferendum on nuclear plants, do you stand in with the referendum forces of the nonreferendum forces?

JWH: It's not a referendum, it's an initiative. You know the difference?

RC: Yes.

JWH: Because the legislature right now is being spurred by the possibility of this thing passing. I think they just passed up to the State Senate, now, three safety bills trying to put some more control on these things. Of course, the federal government says they'll pre-empt some of this; I don't know. But, there's several aspects here. One of them is the possibility of leakage if things go bad. I don't think they'll ever blow up, I'm not a physicist or anything, but I've been assured they don't; but they will fizzle. Another thing is that it's a very large complicated piece of machinery, and they use an awful lot of water; sea water, and its substance should never be put into a plumbing system anyway. Anybody at the marine laboratory will tell you the biggest problem in the marine lab is handling sea water with pumps and pipes, especially if you're going to keep anything out of the sea water. Just the one brass valve in the system may kill off your embryos, and little animals. So I've taken a position primarily, but they still don't seem to be able to keep these things going, at peak long enough to be reliable sources of power. You know, we don't really notice when we have an outage out of ordinary in the present system of water and coal plants because the grid automatically compensates. And if one power plant shuts down, within a few seconds the power is redistributed all over North America. When somebody dropped a paper clip or something in the machinery back East, that was that darn brown out. So then when the nuclear power plant has to go off the line,

you usually don't hear about that since we don't hear about the standard plants. This is taken up automatically; you suddenly find one of these things has been out of order for days or weeks, usually some physical defect or something has to be reconstructed or changed. So I originally got opposed to a particular power plant, more because of the land-use aspect and the behavior of the power company, than anything else. In fact, they got pretty nervous about that. They were afraid that they were going to be considered ogres like the Southern Pacific Railroad. You know, there's an old long California history about the big corporation and how it ran the state of California, the Southern Pacific; the result of its manuevers was the establishment of the California Utility Commissions. And so, there was some concern about all this publicity that was building up. I think the power company backed off and parked because of the bad publicity they were getting.

JRC: The next series of questions I have are taken from some of the things, some of the sources I've read that you've written. One of the ideas that I drew at least from your work was that you seemed to feel as if that the cities would destroy the environment and this, in effect, have a negative use. Now, for the series of questions about urban areas. First of all, can there be intellectual attainment without cities, that is, traditionally and historically, man has argued that a city is both the place to develop freedom and to exchange ideas. Would you agree or disagree with that?

JWH: Well, I think that is half amiss. I'm really not much of the city type. I like to live some distance out, eat my cake and habit, I guess. I don't know what the critical mass should be. How large should the city be? A lot of intellectual development and so forth has happened in relatively small communities in terms of what we have now. Now we've got cities of millions, and they're simply too big. I think that's the real problem there. It's not the city in itself, but it's the growing sizes of them.

RC: You're not optimistic, then, about the idea of planning urban growth?

JWH: Well, we've had some rather unfortunate examples. San Francisco had a plan that redesigned itself after the earthquake and fire, usually called the fire of San Francisco, where earthquake isn't mentioned. But of course, prevailing property rights and so forth, controlled it, so they rebuilt the same old street pattern again, and so on.

RC: Is your objection toward these urban areas the draw that they have on energy sources?

JWH: Well, they certainly draw a lot of unnecessary energy. Unfortunately, San Francisco's been allowed to grow and look like Chicago now. We're all waiting to see what's going to happen with the next earthquake, because all these tall buildings with powerful elevators--those elevators have to be almost perfectly formed to operate well. It may not take very much on that filled land to put the elevators out of order. I don't think anybody'd like to go up and down 30 flights of floors in lieu of an earthquake. But it is an extravagant way to use power. Of course, also, the second time the City of San Francisco's been doing an interesting thing in disbursing certain types of activities outside the city. That is, it has an inventory tax. The result of this is that most of the publishers' warehouses have been moved out of the cities; and, in some cases, their staffs--that may be for a few-people office, or if they are a San Francisco firm like Freeman--I think the, of course, the inventory's kept somewhere else in the middle of the continent or something. Books are especially susceptible; it's small parts and things like that, to an inventory tax. They get charged out of proportion to the space they use. So they're just splitting the cities. So at the same time they're building all these office buildings, certain types of people are building centers scattered around; and also, insurance companies have a whole lot of people and a lot of calculating machinery, you know.

RC: In terms of energy sources, you've spoken rather critically, for example, the mining industry--"but it seemed as if, probably, that coal mining will be the energy source of the immediate future for us."

JWH: With mining industry . . . that's the coal mining business. Yes. Well, that's going to be a bad business, I think, unless it gets some control on it. The considerable areas that they have laid out to be stripped off in the mid-west and mountain-west areas is pretty frightening. The thing there that I wonder how they're going to solve that is that this also takes a lot of water. There may not be enough water to handle all this.

RC: Well, do you see, as a scientist, do you see any alternate source of energy?

JWH: I'm not sure at this time. What we have to do, I think, at least at the present state, is certainly reduce our use of energy. We use much more than we have to use; we're terribly extravagant with it--50 per cent or more we don't need, or that we can get along without, I think. I think the main thing that bothers me is that there--that we're rapidly over populating the earth. That, I think, is the real problem. How we're going to solve that I don't know yet. Steve Peel is a blithering optimist. He said we could feed everybody, and we're going to have power to burn, for all foreseeable time. I don't know what kind of earth will be left for the ants to live in after we get through with it.

RC: Well, do you see the ocean as a possible alternate source of food supply?

JWH: Oh, we talk a great deal, at least some people do, about oyster culture and various kinds of things. To do that is going to require a better care of our estuaries. A lot of these things are going to develop--shrimp and fish culture things. We'll have to clean up the estuaries and shallow waters, because this can be done best in controlled areas. Farming out in the open sea, I think, is kind of a pipe dream.

RC: Well, should the sea be exploited for man on land?

JWH: That's what we're doing.

RC: Is the sea just, in itself, an extension of the land islands?

JWH: Well, not really. I don't want to get into this business of the law of the sea, because it's something; I'm not a lawyer, and I don't know enough about it, really to be quoted as any kind of expert on it. I think that's where you were leading when you said "the sea is an extension of the land." That's something for the lawyers and the treaties, Most laws are tribal rituals. Matter of fact, it's estab-

lished by argument, not by observation, contrary to the scientific approach, of course. So basically, I just feel that we really don't need so many people in the world. To change the world will probably change us. I don't know whether we're going to turn out any of those horrible monsters in science fiction or not. I doubt that, anyhow.

RC: Well, what is man's rightful share, then, of the effect of nature?

JWH: Interesting question. I would say his share is judged by the effect. If he has a profound devastating effect on the natural systems, why then, I think he's exceeded it.

RC: You think he's exceeded it now?

JWH: In certain areas, yes.

RC: I take it from reading that you think that the United States is a materialistically oriented culture. Does this mean he has exceeded his rightful share? Do you see any hope for reversing that?

JWH: Well, I'm afraid I'll have to be a little pessimistic. I don't see much hope and we keep on trying. You know, like Voltaire; we've got a garden to cultivate, and so we keep discussing these problems in those terms. If we continue at the rate we're going, we'll probably wind up on the fossil beds before our time, whenever that may have been. I don't think, in terms of the long cycle of life on this planet, we're immortal. We will probably have our day, eventually.

RC: In this assumption of nature and freedom that seems to run through most of the material that I've read about you, the basic thrust is that materialism should be played down in favor of nature and freedom. Would you agree with that?

JWH: Yes.

RC: Alright then, should there be less science?

JWH: Well, no. But I don't know--you have to distinguish between science and technology. Mr. Peel says the third time you perform an experiment it's technology; I'm not sure that's quite right, but anyway . . . Science is simply a way of approaching nature, verifiable observations and hypotheses.

RC: What is Peel's first name?

JWH: Gerard, I believe.

RC: How can a large population of people, which is what else you're interested in, be handled efficiently without scientific development? For example: medicine and food technology? Or should we, as a matter of fact, revert to a smaller-systems level, allow civilization to be weeded out by natural selection?

JWH: Well, it may take the latter. One of the things that's becoming apparent to some people, I think this includes Bill Williams, is that we don't seem to be able to handle successfully very large groups of people--countries and societies. Things tend to break down and get too complicated, and so maybe, eventually, there will be smaller countries or political entities. We'll have somewhat more coherence.

RC: In the creation of a new community, then, of smaller units, you would say, rather than national communities.

JWH: Quite possibly. That is, with more environmental harmony. I don't know if you understood, we're considering this in terms of hundreds or thousands of years, not something that has to be done tomorrow. This is the way mankind may eventually manage himself.

RC: Well, is a back-to-nature movement practical today?

JWH: Well no, not in the sense of the--individuals can do this, of course, but you can't return the whole society back this way. At least not rapidly.

RC: Is it more practical in some countries than in others? I notice, for example, your criticism of our slapdash, haphazard way to aid underdeveloped countries.

JWH: Well, let's see. I haven't made too much criticism of that. What I did was discuss the things that were said in The Careless Technology, a lot of people's criticism. There, I think, what was happening was that we had gone into these countries without, apparently, trying to study adequately the natural conditions of these countries, which may be different. One of the examples that was discussed in The Careless Technology, rather typical of the way we do things, I'm afraid, the chemical industry sold fertilizer and insecticide and pesticides and all this

to certain South Americans and countries, and they developed mono-cultural systems. In fact, it got to where they couldn't afford it very well, so some other people came down there a few years ago, down there and pointed out that it would return, at least partway, to the original mixed-culture farming in some of these areas--very similar to the North American Indian-type agriculture, incidentally--squash and beans and everything all mixed up together. You still get good crops and you wouldn't have to spend so much money on fertilizers and pesticides. But when you have a mono-culture, why then all the bugs that like that crop move in on it and thrive, so you've got to kill the bugs. But if you've got things mixed up, why then the things that eat the bugs are also there; the only thing wrong is it doesn't yield as much food. But they--I think it's part of the Point IV Program I forget which country was involved in this--they got them to doing this, cutting down on the fertilizer, and the younger generation of farmers trained to do this. This was not a full approach, but it was a part-way recognition of what you could do within the environment. Well, then, of course, the chemical industry sent their salesmen down again, and they corrupted the whole thing. They're now back to mono-culture and buying stuff from chemical companies, which, incidentally, have the least conscience of any organizations. They certainly oppose all kinds of attempts to control them or make them certify or examine what the heck they're using.

RC: Is it possible that we could apply new concepts of conservation to the oceans, or is it already too late? You speak of the estuaries.

JWH: Well, we're trying to do this. It may not be realized, but, as Dr. McCue informed us about this the other day--that's Laurie McCue, of Stony Brook. The recent 200-mile limit law has some very interesting provisions in it. In order to do anything like this at all, make other people, countries recognize it, we have to set up certain things also, in order to be entitled to fish in other people's 200-mile limit; one of these is, if we're not using the fishery stock, a foreign fishing nation may use it, because they have to apply to us. Now this means we've got to know much more about that stock and be able to justify that either we are using

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it or not using it, or it can be fished. And some interesting provisions are in that law which may not generally be realized. It's going to cost a great deal of money for investigation, but that they do tacitly assume an attempt to conserve the resources. Of course, we've had very unfortunate experiences with whales, and much more fortunate experience of halibut in the North Pacific, where incidently, these principles of abstention and non-use and so forth have been long established.

We've also had pretty good luck with the Pribilof seals. Of course, that's a rather special case. It's only a few small islands where the seals to come to shore to have their young and so on. And the surplus males can be harvested that way. Now we're talking about the piracy of the Crill in the Antarctic Oceans, the stuff the whales fed on; you can't get rid of ocean whales.

RC: Well, there's other things, too, that I want to ask you to comment on in terms of the ocean. What about mining manganese modules? Do you think that's practical and should it be controlled?

JWH: Well, I suspect ultimately it will be practical. We've found that most anything's practical if the cost justifies getting it. Right now, I don't think--it obviously isn't, or they'd be dragging up manganese modules by the bushel. Now I don't see why we shouldn't try to get them out of the bottom of the sea, I suppose. Incidentally, those things, while not exactly renewable, are fairly recent formations. I suppose that so-called--I forget what the process is called--chemistry, but they sort of scavenge and build up other chemicals of the sea. So they're not pure manganese. But anyhow, the control gets to international law when it's in deep waters--whose right is it, how official these things are. The mining industry wants guarantees from the federal government, even if they are in the middle of nowhere. Their rights will be protected from a foreign country. Sounds like they're afraid of piracy.

RC: What about offshore oil?

JWH: Well, there's apparently a lot of it in various places, including the Antarctic, evidently. There's a couple of suspicious--suspicious drilling sites already

suggest there's probably a fair amount of it there. Well, we insist on burning up the oil. I guess we'll get it wherever we can.

RC: What about "culturing organisms" in the ocean?

JWH: Well, they're not the open ocean, but I don't think that's very practical. There again, the problem of the commons, and how you're going to control what you invest in the culture. It's suggested, for example, that one way to get rid of hot water in the power plants would be to run it clear out into the ocean and form a sort of an upflowing system, where the water would come up; you put a few nutrients in there or use a sewer line and get the sewage out there with it, too. Well, you'd have a buildup of plankton, and a consequent buildup of fishes, and so forth, around this up-pull. Well, if it's way out where you can't see it or something, well, somebody else is going to come along and catch the fish away. And how we can fence a piece of the open ocean is a good guess. And then, of course, a lot of the fish will probably just go move from one place to the other. These kinds of things make it seem pretty impractical to talk of culturing in the high seas.

RC: How about harnessing tidal power?

JWH: It's a subject I know nothing about. I know there are some experimental installations that do this, one in France and one up here in Nova Scotia, or Maine, or somewhere. This works in places where you have narrow estuaries and considerable buildup of tidal differences. Of course, in Alaska you have this, but in Alaska they don't have to worry, they have all these big rivers.

RC: The point of this is that if you mine the ocean, if you control, as a matter of fact, our mariculture, if you harness tidal power, if you drill the offshore oil, will you have done exactly the same thing to the ocean you've done to the land?

JWH: Just about. But I don't think you can run mariculture and tidal lands at the same time very easily. You have to segregate the water.

RC: Well, do you suppose, then, that the ultimate fate of the ocean will be to go the way that the original land went?

JWH: I hope not. It's much larger. Actually, most of the kinds of culture of marine

organisms tried out in estuaries and things have done no great damage to the estuaries. In fact, they add some interest to it. It's about the oldest kind of shell fish culture known. The Romans practiced it.

RC: Is there such a thing as a pristine environment, in terms of land or the ocean?

JWH: I fear not. The ocean now is littered all over with little plastic balls, ground-up bits of styrofoam, and some of this or that. There's some places where this is not quite so. It's almost impossible to go anywhere without finding trace of man. Of course, you're there yourself, though. You mustn't forget that either.

RC: Well, you've stressed, for example, the importance of personal freedom. How would you apply this importance of personal freedom to the sea, for example: fisheries, mining rights?

JWH: Well, anybody's personally free to go pick up what he wants, and then when you start to make a business out of it, that changes the impact and the ground rules. Then I think it has to be controlled so it's not excessive.

RC: If your basic pessimism about the outcome of man, that appears in your writing, is representative of your present intellectual place, let's say, what are other ways to have intended progress, or a better life style, in this country? Is that possible at all?

JWH: Well, I would hope it would be, in the long run. There's probably going to have to be some readjustments. Personally, I can't see living in a large, sprawling city or some parts of the downtown; nobody lives in those, really. Smaller, more widely separated communities, I think, would be a much better way.

RC: Is there a way to arrive at a positive cost-benefit analysis to determine--to control--progress?

JWH: I don't really believe in the cost-benefit analysis; I think it's the arithmetic of apples and oranges, bananas and things. And you're trying to, when you start this cost-benefit business, you're trying to put very subjective things like aesthetics, a sense of well-being, or whatever, against other things in dollars and cents or material substances. I don't really think you can do it that way. It tends to develop a very routine approach to things, also materialistic. But to

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avoid--for example, if you get into court testimony and the like--putting dollars and cents value on certain types of things, especially recreation and resources. Or, for that matter, try to put a dollar and cents value on a particular species. One thing that's disturbed me is the idea that when we have an endangered species, perhaps it ought to be put to a public vote whether it should be allowed to be exterminated by whatever's going to continue to endanger it, or whether the people should vote to preserve that species. That is a --well, we're deciding to be God there, I guess. But the thing is that most people probably wouldn't give a damn anyhow.

RC: If a philosophical point of view--scientists have become particularly under attack for identifying progress and advancement in terms of technological advancement. As a scientist, have you been uneasy about this attack from . . .

JWH: Well, since I fancy that I don't use technology as you have said there, I haven't been concerned about it. I think some people are a little confused about what science is as opposed to technology, and so on. Science is more or less a state of mind and an approach to nature, I think, rather than what you're doing with material things.