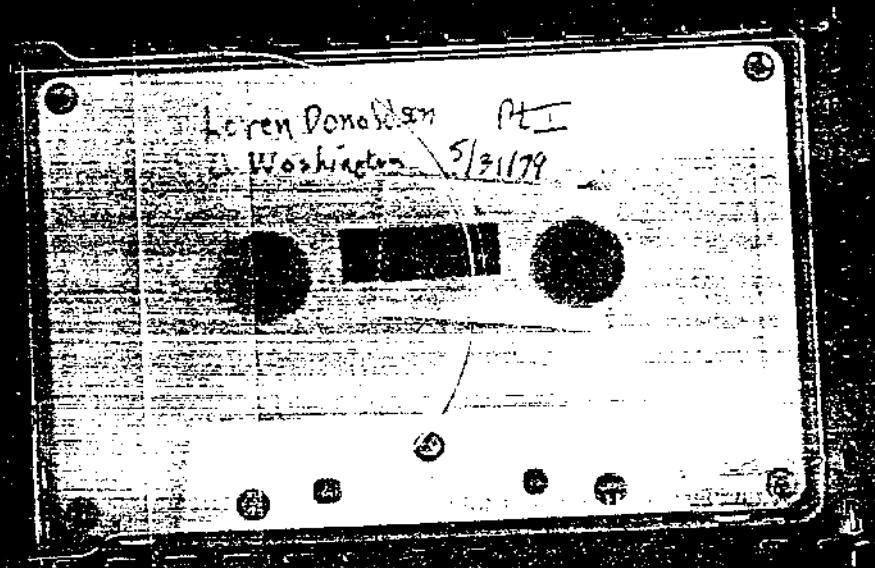
DONALDSON, Lauren Dr.

University of Washington

05/31/79



INTERVIEW WITH LOREN DONALDSON - SYNOPSIS
5-31-79

- Start up
- Last paper - just recently - many thousands of fish later
- Convincing others of need for long term studies
- Evolution of concepts of special strains, etc

  Applied genetics
- Lab experiment vs total ecosystem - thinking of Columbia River as a system was new
- His book on fish culture
- Continuity of his group - since 1943!
- Neal Hines' second book - this on Donaldson's life
- Rat population on Engebi Island
- Resistance of Chinook population to radiation - "equivalent of 7200 reactors"
- Sensitivity of ecosystems
- Buffering capacity of the sea
- A reservoir of iodine
- Waterfowl work on Columbia
- Ducks on ponds that were contaminated
- Stafford Warren - support from him on ecological work
- Ecosystem vs laboratory research
- His role in Japan - gathering tuna, etc, after Bravo shot
- Mercury story - sea water contamination

  Orange peelings - chelation

  Also vitamin C
- His experience at NTS - driving a truck!

- Expectations from NTS work
- More on experiences there - Kermit Larson
- Hanford - role of Parker and Kornberg
- Frank Lowman and Puerto Rico
- More Pacific - Johnson Island, Bikini natives
- Fern Lake work
- Neoplasms in plants - no relation to radiation
- Re/Behavioral studies in animals

  The "homing" of salmon and trout
- See Bill Shell for NRDL reports

INTERVIEW WITH DR. LORAN DONALDSON
University of Washington
College of Fisheries

May 31, 1979
Seattle, Washington

STANNARD: We just started to talk a bit about the criteria for deciding on when a radiobiological effect had or hadn't happened.

DONALDSON: I don't know how you wish this conversation to go but since we just came from the pond where they were working. Of course we had to develop some facility like that. We had to develop stocks of salmon that we could have under our control. Once we got the facility, got the stock, developed them, then we were ready to do complete life cycle for several generations. Radiation exposure, release, send them to sea, and then recapture them back. From that initial plan (actually it was an extremely egotistical statement to call it a plan) in 1943, we labored away until the terminal paper was published this last year. That involves thousands, hundreds of thousands of fish.

STANNARD: Do you have a copy of it or a reference?

DONALDSON: Well, I have copies of the references. I'll give you the references if you wish. [Some discussion of plans for book omitted, N.S.]

STANNARD: I had a series of specific questions that have come along...

DONALDSON: I have those right here.

STANNARD: Let's reminisce just a little bit first. My impression from reading Proving Ground and from many of the things you've said is that you had constantly to convince people both in the university and in the Manhattan District, then later on in AEC, of the need to

do long-term studies. They always wanted to sign off and say, "I think we know enough now. We'd better quit." Is that right?

DONALDSON: Well, I think we came along in a difficult period of time, really ahead of the environmental era when people began to look at the whole ecosystem as a unit, if you wish, or look at whole life cycles. You remember the state of the art in research was do a single experiment and then publish a paper on it. Run quick and find out what the LD-50's were, do a quick nutrition study, some of them take six weeks or six months, and you know all there was to know about the subject! The actual development of strains of fish took years. Applied genetics was really just in its infancy about then. We didn't have the broad-breasted turkey, and we didn't have Donaldson trout and salmon and what not. Those concepts were just evolving. That is, the manipulation of a population. We didn't have people who were thinking outside of laboratories. They weren't thinking in terms of total systems. When we came along and began to talk about the Columbia River as a unit and note impact on the whole river. That was revolutionary. Of course, when the testing program started in the Pacific, the concept was to go out, shoot a big firecracker, run back home and it was all over with. We were essentially alone in insisting that, No! - What happens this year, next year, ten years from now? We really could never get support for that. I guess the principle blame has to go directly back to AEC or Department of Defense and the relationship with AEC. I think it was just that timing was bad. I don't think people purposely were bypassing important things. Oh, there were a few that were.

STANNARD: Ahead of time by about 25 years.

DONALDSON: Yes. But in retrospect, people now are coming along and doing environmental impact statements on everything from here to there and back again in a ridiculous way because I think much of that information is now available. It wasn't available 35 years ago. But too many people now don't read. They've forgotten how to read.

STANNARD: Well, that's the reason for my taking on this writing project in part, not that it's going to make anybody read. Sometimes I think I'm crazy to take this on.
Did you find the outline and the question?

DONALDSON: Yes, they're right here.

STANNARD: I might ask a very general question first. From what you've seen of the outline, do you think it's possible for one man with a little help, perhaps a lot of help from many people like you, but with a little help from instrumentation people writing chapters, to do it. What I wrote was an outline of the book I'd like to read. I'm not sure it's the book I'm going to be able to write, especially with an invalid wife and having to pay nursing care anytime I'm off anywhere doing these things.

DONALDSON: I guess I'm not the one to answer that. I've not written a book. I've written parts of books. At the present time I'm much involved in writing a book on fish culture. It will be published by Academic Press as soon as I get some pieces in.

STANNARD: On what?

DONALDSON: Fish culture.

STANNARD: I see.

DONALDSON: But it's to cover problems around the world with particular emphasis on the cell limits

STANNARD: Well, one of the reasons I think that DOE and Battelle are willing to support somebody to do this is that they're finding just the thing that you mentioned, that the current generations coming in have no idea of what's back there in the literature, what is the tradition and body of knowledge that they're building on, reinventing the wheel.

DONALDSON: I just popped part of the things from this laboratory alone into boxes. That's just a part of it. Here's another part down here in these boxes. And that's just one small segment of it.

STANNARD: I'm sure.

DONALDSON: I guess the only claim to fame is we've been at it longer than others. There's been a continuity both in people and objectivity that's quite unique in scientific research. All of the original people that started with me in 1943 are still all actively engaged.

STANNARD: That's astounding! That's great.

DONALDSON: Isn't it? They're active like I, most of them are retired but they're still plugging along. They show up. Dick Foster is still at Hanford and working away. Kelly Bonham is here. He comes early in the morning for some reason or other. Gets here about 5 o'clock.

Art Welander is here. And of course Al Seymour. Frank Lowman and on and on. But these key people have been plugging along for these many years. So there's a human resource here that is unusual. With the other laboratories the direction has changed a dozen times. They have changed like with the weather.

STANNARD: Well, they seem to have also changed their research interests, too. They have gone away from what was really pertinent at the time and gone into molecular biology or biophysics. I have as much interest in basic mechanisms as anyone else, but there are all kinds of laboratories in the world that can do that. I don't see why these specific laboratories should diverge so much. Rochester is a good example, I think, of a place that's been gradually going away from what they were really good at and into something that's very interesting, but they're not any better at it than a hundred other laboratories in the world.

DONALDSON: There's surely been a shift in all. Maybe it's more fun to jump around, but you have to stay pretty much in the general area, a field you can really be comfortable with. The people here, why, we seem to be comfortable with the kind of problem that does bring many different disciplines together with a closely knit group. We have to have some people who are, of course, specialists in different pieces; but, in general, they're working in this same overall environmental ecological area.

STANNARD: I was delighted to find in the back of Proving Ground a list of the laboratory reports. Before I leave, I'd like to bring it

up to date, so I'd have almost a complete list. Maybe Allyn could help me get that together. One of the things I, of course, wanted to be sure we discussed was your perspective. This book stopped in '64, what in your view has happened since then?

DONALDSON: Neal Hines has written another book you probably haven't seen.

STANNARD: Oh, I didn't realize that.

DONALDSON: It's a completely different slant.

STANNARD: This is great. This is a copy I may...? [Showed me Hines' other book: "Fish of Rare Breeding", N.S.]

DONALDSON: Yes. Neal could come by, and he'll autograph it for you.

STANNARD: Oh! Does that pick up the Pacific story, or is it more on the species that you developed?

DONALDSON: Actually it's a story of my life.

STANNARD: I see.

DONALDSON: Well, people shouldn't hold others up. Wait till they're dead, then write up their biography rather than... That's why I kind of pulled it out back there.

STANNARD: This book is very well written.

DONALDSON: Neal is a master.

STANNARD: If I can write half as well, I'll be very pleased.

DONALDSON: He wrote this one really at the request of Smithsonian. He did some things for them. They had Proving Ground and another thing that he wrote. But people there said that he only did half the job. What about the other half. So he worked away at this for several years. Finally Smithsonian published it.

STANNARD: I'm going to have to be very careful to limit my self to internal emitters (radioactive materials) and not get off into external irradiation except as it contributes directly. As I recall, you first started in the external radiation field.

DONALDSON: Total effects in almost all instances involved both the internal and external [sources.] Of course, it's one thing to do a sweep across an area and measure what the levels are; it's quite another to put that in combination with both the ingested, absorbed context sort of thing. It's hard to conceive of a rat, for example, on Engebi Island in terms of any one isolated incident. They're all involved. A direct study out there is one I think is tremendously important. It gets practically no attention because it wasn't done in the laboratory; it was done in the field. Well, they say you couldn't measure everything. I know, but that's the real world where you have these mixed responses. From the fact that we never killed a rat on Engebi Island, never exterminated the population is in reality a very important thing, and it should be cranked high in our discussion. The fact that it took the equivalent of 7,200 reactors like the ones at Hanford to make a measurable change in the genetic pattern of Chinook Salmon is important. But people say, "Oh well, it could never happen. Of course it could never happen." But when you have that much radiation about both internal and external, why it gives some very comforting feelings to the world we live in. I get so uptight about these people who start screaming and hollering micro-micro-micro or picocurie is going to cause genetic damage. I think they just neglect the evidence.

STANNARD: What do you think of the EPA's present trends and attitudes?

DONALDSON: I think they're insane.

STANNARD: I think I do, too. Somehow we've got to laugh. I'm just not going to be able to avoid a certain amount; well, I go up to 1975 in my chapter. EPA was just coming on the scene then, so I don't have to say much about them. But I don't think I can steel myself not to say something to the extent that these years and years and millions of dollars of work are directly pertinent to the very current questions that are not being suitably utilized.

DONALDSON: I guess if we had the wildest extrapolations, those of us who've been involved for 35, 40 years had all long been dead. We all would have two-headed monster children or some ridiculous thing if we believed the rabble rousers of the moment, the maybe-maybe people I call them, maybe-maybe, if-if.

STANNARD: I've made this statement, in a book that we got out from one of the Rochester toxicity conferences which I guess is my swan song at Rochester, "Radioisotopes in the Aquatic Environment" that if there were going to be any unpleasant surprises for us, it probably would be in the marine ecology field. My reason for that statement was that the marine system is so much vaster than most terrestrial systems that the chances of something important occurring that was not found are greater. I'm not so sure that that's correct anymore.

DONALDSON: I think we could really discuss that from several different

angles. My personal feeling is that when you talk marine ecology and particularly mineral uptake, I stop at uptake, you have to realize that there's a tremendous buffering capacity in the sea that you don't have on terrestrial areas. This tremendous buffering capacity, for example strontium, well, look at the gillions of atoms of calcium there are that can dilute down the strontium uptake. So it's spread out while environments like this terrestrial environment here you practically have zero calcium. Our water has a part per million. The water you have back at Rochester isn't too much, but the water I grew up with at central Minnesota had 60, 70 parts per million. So you have these variations. But then in the sea you have tremendous buffering capacity. It's hard to conceive. Surely there were tremendous amounts of say iodine uptake in algae. But who knows what iodine does in the way of damaging genetic components in algae. On the other hand, if one considers the iodine uptake in human thyroids, that's something different again because you don't have that great backlog of iodine that you do in the sea.

STANNARD: Well that's an important point. It mentions in here some work of Coryell on fission products, but I couldn't find much about it. In fact, I'm having a very hard time finding the old records. Every library I've visited so far, "Oh! We...!"

DONALDSON: "We threw that away." Yes.

STANNARD: I thought it would be a lot easier than it's turning out to be. Even in Washington, even in Technical Information Division

at Oak Ridge. A lot of the reports that I thought would be very useful aren't available. Do you remember this Coryell report?

DONALDSON: No. I don't remember that reference.

STANNARD: Another question I had was in the early work. Am I correct in stating that probably the first real ecological study, radioactive or not, was what you started up on the Columbia River during and before the opening of the piles?

DONALDSON: Yes.

STANNARD: This would be true for lead or zinc or mercury or anything?

DONALDSON: Yes. A complete ecosystem study.

STANNARD: Yes. As I recall there was some work done with water fowl, too.

DONALDSON: Oh yes. A good deal. But that came a bit later. That was triggered by our direct observations there once they were in operation. I remember the original plans had cooling tanks. That became a tremendous aquatic garden. The algal growth in there was beautiful and lush, and it was nice and warm. In the winter time particularly, here came all the ducks and sat on these swimming tanks or along the river. There was a beautiful growth of algae. The ducks ate the algal growth, and of course they picked up concentrations of isotopes, or selectively we took some of the isotopes out. So we were concerned. Their droppings were looked at and some of them were slaughtered and looked at. We were so concerned that we thought we better put a skimmer in here some way and take this accumulation of algal growth out and bury it so the ducks couldn't get at it. You have to realize

that everything was going "quick-like." That is, how big a tank did you need? Well, we did some measurement from that stone to that sagebrush. It was that big. And they'd start digging. "Well, why didn't you set up a study?" Well, who knew what...? But as soon as we knew what I'm trying to get around to, as soon as we knew there was something then it was possible at that time to get action, particularly at Hanford. The people there were extremely helpful, Herb Parker and going back Col. Methias, the original. He was one of the few people who could grasp these things and move. Now there were other people, well, it didn't make any difference. "We're fighting a war. We haven't got time to monkey around with this monkey business."

STANNARD: I've been surprised at how many times Staff Warren's name appeared in here and how much interest he took and how far-sighted he was.

DONALDSON: Really, he was the guardian of our flanks on several sides. His experience with radiation and his position within the government at that time. But most important was his personal interest, something you referred to. When we had questions why it was just a case of a phone call. At no time did he ever give us the run around, I'm busy, or this sort of thing which so frequently was the case. I think it was Staff's assistance that Bikini studies were included at that level. Others were just going to do some pig studies or a few white rats. What happened to the white rats? They sat them in the sun and they died of sunstroke. Staff kept saying, "We have to look at the things that are there." Well, of course, we were

always pushing from behind. What about the native things? Why can't we use them? Well, they aren't good laboratory. Well, but they're the laboratory we're working in, was our insistance all the time. Then he would carry the message into the command division.

STANNARD: That brings me back to a problem, question that many of the pure laboratory scientists keep throwing up, even back when I took the job as test director for one of the Plumbbob shots. All you know is what happened in that particular environment under those particular circumstances. If you do it in the laboratory you can control many sets of circumstances, and isn't it much better to imitate the environment under controlled laboratory conditions rather than just get what you have here? Because you got a single shot and then you've got to get it under other circumstances. And what applies to Nevada desert doesn't apply in the Pacific. You've really got just as much work, if not more to do, to study the natural ecosystem. What's the rebuttal to that?

DONALDSON: My reaction is that if you're studying living things, living systems, the place to do that is where the living things are in place. You learn just a fraction of a fraction of the total impact when you go to a laboratory. That's a nice comfortable place, you can walk in the morning and be greeted by a secretary and a white lab coat will be handed you and this sort of thing. And it's kind of dirty and grimey out there often, particularly in the Pacific studies. Some of the ships we were working on out there were awful!

STANNARD: They looked it in the pictures.

DONALDSON: Well, they were awful! That is, put 12 men in a hold of a ship that was about half as big as this and keep them there for three months. It takes a pretty dedicated operation. And the isolation, and the... But the point is that provided our whole concept to the ecological system.

STANNARD: And without it?

DONALDSON: Without it, you have unrelated pieces. I don't see any way you could fit these little pieces together into a meaningful pattern.

STANNARD: Well, you've seen the outline of the book. As you can see, I've got a pretty wide subject to cover. I'm not an ecologist as you know, but I'm very intrigued by the subject. My section three is built around what I'm calling "New Dimension No. 3" brought on by the fallout problems in general. It seemed to me that any summary of the radioisotope work done during those years has to spend a reasonable amount of time on it. But in your perspective, are there certain areas which you think are worth special emphasis. What would you like to read in a general summary?

DONALDSON: Well, unfortunately, the Rongelap experience is an extremely valuable laboratory, field laboratory, involving all aspects of living things. I think to ignore that or even to play down that, as bitter as that experience was, is to miss the point of what the subject's all about. We hate to drag those people back in and point to their [mistakes], but it's [the residual activity] there! And the

greatest mistake that the State Department, the Department of Defense, and the AEC made is when they tried to play down that Bikini March 1, 1954 incident. I know because I lived through that very brutally in Japan. It wasn't until we began to level directly with the people there that we had any semblance of squaring that away. By levelling I mean just, "This is what happened, and here are our numbers." I don't know if you're aware of how direct that became because we had to go outside the regular government procedure. Our procedure was to get a fund of money set up to me, and I wrote a personal contract with the some people in the Ministry of Health in Japan (Dr. Garbata?) that they would collect the samples, particularly of tuna fish, for example. They would collect the samples. They would keep half of them and send half of them to us. Then we would get together at the end of our measurements. They'd come here or we'd go there. And we would have all of our measurements, and then we would jointly put out stuff. Here you see there was no question about the veracity. At the first conference, Dr. Seymour and Dr. Plumba, who was here at that time, and I went to Japan; and with Japanese counterparts, we got together. We had all our data, and we put it together. We were in such close agreement; it was beyond belief. The important thing was there were two units working on a common subject of tremendous public interest. Well, at the end of the conference we decided we'd have a press conference. The TV's, the newspapers, and feature writers and whatnot all came, all set up. They were going to really blow this one up big. They got about that much play because on about page ten, a little notice that

there'd been a joint meeting of scientists from the United States and Japan discussing problems of radiation in the Pacific, and they have agreed for the most part in their measurements. That's all it said.

STANNARD: It wasn't news as long as you agreed.

DONALDSON: It wasn't noticed as long as we levelled with them. Now if we'd gone off and hid our numbers under the table, there'd been a tremendous furor. They could do the same sort of thing. They could blow it up to any proportion. Oh, well, the usual statement, large amounts, horrendous amounts of radiation. Well, that's become if you can detect it. I sometimes think they're measuring K-40 and thinking there's horrendous amounts of radiation because there goes a beep once in a while on a counter.

STANNARD: Now is this after the furor had calmed down?

DONALDSON: Yes. Subsequently.

STANNARD: How long after it was it, about?

DONALDSON: Well, we got underway I guess, the event was March 1, during the summer, and then the following year we began to really get our data. It took a lot of doing on my part personally. Lots of trips to Washington to get this agreement through because they just... you know, the bureaucracy just couldn't see how we could have a personal relationship here that would be meaningful.

STANNARD: Is it all right for me to mention this general arrangement publicly? It's well-known?

DONALDSON: Sure. No, it isn't well-known. It isn't well-known

at all.

STANNARD: I see. Is it the sort of thing that would be best left unsaid?

DONALDSON: Well, I don't know the tone you're going to write. If I were writing a book, I think I would surely emphasize some of the public relations problems....

STANNARD: I do intend to.

DONALDSON: That brought us into our present situation where I think hysteria rules rather than reason.

STANNARD: In many respects it's hard for anyone who's just been going along teaching students and doing research to understand what's gotten into people, what makes them so irrational on whole subjects.

DONALDSON: I think there are areas that pinpoint this. Going back to our Japanese experience. Dr. Boss and I went to Japan in the spring of 1954, and I found a whole nation that was hysterical. The whole nation was absolutely frightened beyond all reason. That has gradually filtered out into other areas now. Again, I think it's because there's been many things blown up disproportionately either purposely or by accident, so they have developed a feeling of the field of radiation that everything about it is bad. It's beyond belief. The only thing I can compare it with, more or less directly, is another thing that we've had to live with. There was a problem associated with the ocean for years about the mercury level. We set that at a half a part per million. Well, anyone who's been working in the sea, and

anything about it, knows that normal levels of mercury exceed half a part per million. Has for the last billion years. There are parts of the sea where it's even higher than other parts because of volcanic activity and the contribution that's made from the earth. The Japanese have these very well pinpointed. They know about it. Then we say half a part per million. Well, that simply means that all the older fishes exceed that half part per million level as they concentrate along the food chain. One has to go to great big pains then to say well, "Nothing is going to happen then." That's a natural phenomenon. Mercury has always been part of the environment. I can even make a better case for chlorine in our drinking water supply.

STANNARD: Yes.

DONALDSON: Somebody ought to really be excited. The mercury situation got so bad here that we had to do something about it before we could get on with developing our fish farm.

I begin to digress for just a moment. I think it's worth telling this story because if we were feeding fish, that were going to market, scrap from the sea that had some mercury and it concentrated down to 20%, say, then the mercury then might be transferred to the fish you're feeding, and you'd have more than a half a part per million. If that should get out then, of course, you're going to go to jail if you sold it or be fined beyond belief. That's been going on also since the creation of the oceans. But now suddenly it becomes a legal entity that it can't go on anymore. So I searched the literature and lots of contacts with my associates in animal husbandry field. They found that say the milk you get

in San Diego from that water shed comes from alfafa that's grown in Arizona. There may be a high copper level or other mineral levels. Well, the regulations would make it so that you couldn't market that milk. So what do they do? Well, they feed the cattle orange peelings. And it chelates mercury.
Well, that was good enough for the cows, so maybe it will work with fish. So I worked with the Sunkist people, and they had a good deal of input into it. But we set up a program where we'd feed ground up orange peelings to our fish and it worked beautifully. It tied up the mercury, so it passed through the fish.
The methyl mercury was eliminated, chelated and passed out. Okay. Then we found that in orange peeling there was a high level of vitamin C, usable vitamin C, and the fish have to have that or they become arthritic. If a fish becomes arthritic, it chelates the cartilage in the gill, and they choke to death, or actually I've seen their back broken from vitamin C deficiency. So we accomplished a good breakthrough there. The vitamin C was available, and it prevented this arthritic condition. The old "apple-a-day" as a youngster you see that prevents arthritis when you're old. Of course, you can't document that with people, but we can do it very nicely with fish, that is, with the cartilage in the gill and where people get arthritis in other places. Then we found that the carotenes, those oils that are in the orange peeling are essential, too, and they help fat metabolism. Some are deposited in, and they give a nice color to the flesh of the commercially raised salmon. That ups your price. More important, these orange peelings impart a very delicate flavor. Well, the very delicate flavor you see, people eat these farm-raised fish that we're raising in our farm

down in Oregon. "Gee, that's a delightful fish! I've never tasted such nice tasting fish!" But we must not say at any time that we fed them orange peeling because that is an item unnatural to the diet of fish, and EPA would throw us in jail for doing that under the Pure Food and Drug Act. So you use subterfuge to do things that are natural because of the ridiculous regulations we have. It's a natural food, a natural food source, but it's not natural to a fish to eat, so we mustn't do it.

STANNARD: Who says that?

DONALDSON: Pure Food and Drug

STANNARD: Food and Drug.

DONALDSON: Well, these are the things that you don't learn unless you think in total concept of the environment you're working on.

STANNARD: One of the things I was interested in when I saw Staff Warren was the fact that in toto...

DONALDSON: Did you see Staff recently?

STANNARD: Yes, just a couple months ago. He seems fine. Coming in and still doing laboratory work. He's gone off completely from his old field. He is doing arthritis research in chickens. One of the things I asked him was, "Suppose in retrospect for the Manhattan District you had to choose what were your worst problems. Were they chemical hazards or radiation?" He answered, "Oh, chemical, by all means! We had phosgene. We had fluorine, this, and that. The chemical hazard. The radiation hazard was under good control. In terms of the jobs we had, the chemical toxicology was a far worse problem." And somehow I

don't think that's gotten really properly emphasized except for those that were around. Chemical hazards are every day common things.

DONALDSON: Amen for Staff. I wish he'd get up on the stump and proclaim it loud and strong.

STANNARD: I wish he would. I'm inclined to say something about it. I won't quote it without his permission.

DONALDSON: See all this roundabout talk I've been making the last few minutes about mercury and its maybe-maybe-maybe effects. A few years ago everybody was screaming mercury. Now they've sort of forgotten about it. They've got a new something to kick around since we've had the Three Mile accident. That provided the maybe people with a wonderful vehicle to make noise.

STANNARD: Well, to come back to the section three in the book, I've been a consultant to the Nevada Applied Ecology group for some little time. How much liaison was there between the laboratory here and the Nevada group? I know there was some.

DONALDSON: Yes. We acted as, I guess, "informed chore boys" down there. I went down on two occasions and Kermit assigned

DONALDSON: That's unfortuate because there should be many, many useful things there.

STANNARD: It's not easy to come by. The work I was concerned with was plutonium. I'm not expecting much in the way of plutonium effects there. But of course there were effects from the external radiation from the shots. Van Romney tells me, which I didn't know, that they have a fixed source over, I guess, to the west of Mercury where they're doing studies somewhat similar to what Brookhaven was doing with its fixed source. They bring it up, and they're studying desert organisms from the standpoint of effects simply to try to get a handle on relative radiosensitivity, etc.

DONALDSON: Is that comparable to what they had in Puerto Rico in the rain forest there and the Brookhaven, that environment?

STANNARD: Yes.

DONALDSON: I didn't know about the desert one. That must be fairly recent.

STANNARD: For one thing, as you know, UCLA has a lot to do with it. A man named Kermit Larson tromped around out there and gathered specimens of soil.

DONALDSON: Kermit's here in town.

STANNARD: Is he here?

DONALDSON: Yes.

STANNARD: I see. Well, you probably know a lot more about what the status is. But the information always leaves me with the feeling that they found out a lot about where isotopes went and what not, but they just never got much on effects as such at low levels; that the

samples are not sufficient. Quite unlike the aquatic studies that the Applied Fisheries Laboratory had to do with. Sure, they had some things they can say; but on the environmental effects side, they're really very weak. It's hard to document anything really having happened. But it's hard to say nothing did happen.

DONALDSON: Well, that's a tragedy because that's equally important.

STANNARD: I think it is. What worries me so, Loran, is that I may be missing important things. I've been speaking of my problem in getting to the real literature. There may be things that are well known that I'm just not finding. And yet having been a consultant to them I've read their stuff pretty thoroughly in the past. And it looks as though Kermit is the only one that had much interest, he and Staff Warren.

DONALDSON: That is Kermit's personality. That is, he wanted to be General Larson. He didn't take advantage of the opportunities there really because he kept everything completely under his own domain. That is, the two sessions that I went through down there, there was never such a thing as a staff meeting. You were simply assigned. "This is your assignment. You go do it." Like a military command. When you were through, why, you went home. There was no follow-up. "Well, what happened?" "Well, we chased a cloud over here. We set up a camp in this place." And that was the end of it, as far as we were involved. And so the people that were here in the laboratory got kind of wary of doing any more.

STANNARD: I got the impression that the Oak Ridge work under

Stan Auerbach has given much more definitive information than the Nevada test site work in terms of biology and ecology in general.

DONALDSON: Well, he had his pond, and he had his animal groups.

STANNARD: I may be doing somebody a great injustice, and I don't want to do that. Would you agree to that statement from your background?

DONALDSON: I don't think I have enough direct contact to comment on that. The Hanford people, are you going to see Leo Bustad?

STANNARD: Yes. Oh, I think that the Hanford story, that's superb.

DONALDSON: That's well in hand?

STANNARD: That's well in hand. That was the pioneer number one in terms of starting the whole thing. But I think of that as part of the Applied Fisheries Laboratory, although it wasn't.

DONALDSON: They don't think of it that way. Well, at first it was.

STANNARD: Yes, I know.

DONALDSON: To start with. Then they sort of grew, grew, grew and we were just small, the aquatic branch. They had some fine people there and did a lot of very useful things. Again I think this is because of Herb Parker and Harry Kornberg and some of those people who came along and were genuinely interested in doing this sort of thing. I mentioned Leo. He's the one I see most frequently, Leo Bustad. He's at Washington State now as the Dean of Veterinary Medicine over there. Leo always was gung ho and measuring directly with animals, his pigs, and his other animals. He's a very thorough worker.

STANNARD: It's too bad he's so busy being Dean.

DONALDSON: Yes, but we need people like Leo in education. He's

stimulating.

STNNARD: I'm sure he's still being effective. Knowing Leo, I don't think he could ever really leave it.

DONALDSON: He'll stick his head around the corner here. He'd say, "Did you get over your operation?" He still adds that personal interest in people.

Go and talk to him, too. See what his present views are.

STANNARD: Yes. I associate Leo more with the direct experimental work than field ecology studies. Am I correct?

DONALDSON: Yes. Yes. He's a very knowledgeable person.

STANNARD: The other group you've already mentioned a little bit was the Puerto Rico rain forest study. I was going to say very little about that because I thought it had practically nothing to do with internal emitters. I have to stop somewhere. But now Lowman was involved with that, was he?

DONALDSON: No. Lowman, Frank was at the other end of the island.

STANNARD: Oh, he was over at...

DONALDSON: Myaguez.

STANNARD: Myaguez, yes.

DONALDSON: Frank was much involved with internal radiation. Frank is such a versatile and freewheeling sort of person that it's hard to pinpoint him into an area. Absolutely brilliant.

STANNARD: One thing that is quite apparent in the book here

is that of course you do primarily beta-gamma measurements. There isn't much said about alpha.

DONALDSON: Well, there weren't any facilities for measuring them then. Now, there's a tremendous backlog. But at that time, we were just beginning to get some equipment.

STANNARD: Well, the big fuss now is around alpha, as I understand, plutonium particularly.

DONALDSON: Yes.

STANNARD: To a lesser extent, cesium, which is not an alpha.

DONALDSON: When you're talking to Al, you'll have to get involved in Johnson Island and plutonium. You know about that accident out there?

STANNARD: There's a thread through the book that the natives at any moment were going to go back to Bikini. But it never seemed to be quite right to get them back. Did they ever get back to stay?

DONALDSON: to Bikini?

STANNARD: to Bikini.

DONALDSON: They were back this last year; then they hauled them off again.

STANNARD: I mean until this last incident, in that whole period during '47 or 48.

DONALDSON: Well, they moved them out in '46.

STANNARD: What was the raison d'etre? It seems as though the levels were down, and still they never could quite get to the point where the United States government would say, "Okay, go back

home."

DONALDSON: Because there was always the threat of a suit. That's my interpretation. There was always the threat that the attorneys would come back and sue the government if this happened or that happened.

STANNARD: I see.

DONALDSON: Well, in 1946, January or so, January or February, I've forgotten which month, when they moved them over to Rongerik, they were a pretty sad group. We were over there that '46, and we looked at them. We went back in '47, they were in awful shape.

...try to get...never had that, oh, Shields Warren. But Shields is all so gol'darn busy.

STANNARD: Yes.

DONALDSON: If he had little time to, but I always felt his interest is very good. But he was so busy sleeping on the train going up to give a lecture at Boston, coming back the next day. I don't know how he stood it all the years.

STANNARD: I don't know how either. I'd like to ask you on the tape if I may, Loren, "Were the objectives of the biomedical program for the thermonuclear tests any different than the fission tests at Bikini? It was just a larger scale?"

DONALDSON: That's right.

STANNARD: The one thing I remember when Pearson was, during that short period as head of the DVM.

DONALDSON: Paul Pearson?

STANNARD: Paul Pearson. He asked, "Why did you want to use mixed fission products instead of known isotopes?" I didn't find the answer. Do you remember? This was a laboratory experiment.

DONALDSON: I don't remember the context. My guess now would be that again we were thinking in terms of field experience, and we have them overlap. That probably was in context with some of the things we were going to do at Fern Lake where we did some field experiments that I guess now we could all go to jail for because we contaminated a whole watershed to follow the movement of radioisotopes.

STANNARD: Where's Fern Lake?

DONALDSON: It's 50 miles from here, southwest of here. Beautiful little spot. We had a whole watershed there that we could do these sorts of things in. That should have been expanded out, but we lost funding completely for it. The present group at the AEC are't interested in field studies of that kind. But there we could take these things and put them in to see how much went up the tree, how much has come back down the trunk. Here's the study of how they separated out in the mud in the bottom of the lake.

STANNARD: I see here you have a collection of theses (Ph.D. and M.S. theses). I had a similar collection before I left Rochester. I had to hand them over to the library.

DONALDSON: But these are mine.

STANNARD: Well, these were mine, too.

DONALDSON: Your copies?

STANNARD: Yes. I just didn't feel I had a place to put them. We moved to a condominium. I'm not so sure that we did the right thing

except for getting Grace out of the Rochester winters. For me it's been a very hard transition.

DONALDSON: Yes, when you lose your library, you're really handicapped.

STANNARD: Yes. I feel that more than anything.

DONALDSON: I do. Without my library I...

STANNARD: This is your bit of immortality in a way.

DONALDSON: I wish I had it all. That's about 20% of it.

STANNARD: I'm sure there's more. Another question I had was: there's a mention occasionally of neoplastic growths occurring in plants. I wondered if that ever was studied in post-Proving Ground.

DONALDSON: Yes, it was studied, but it has no relationship to anything due to radiation. You get exactly the same thing happening here in similar plants right here on campus, right here in our yard.

STANNARD: I see. So it's some other cause?

DONALDSON: Yes. There was a good deal of comment about the misshapen coconut fronds. We followed that out to other atolls far away with no fallout, and the same sort of thing happened.

STANNARD: I see. Well, it's nice to know that everything isn't due to radiation.

DONALDSON: I think we were grasping at straws lots of times in the early days. We always do. Well, there must be something! I remember when we first started our first experiments; we couldn't believe anything was happening until we killed something. But once we

killed something, then we knew something was happening.

STANNARD: Well, that was the attitude of the day.

DONALDSON: Right. Then we could work back down and feel a little bit more comfortable looking for more subtle things.

STANNARD: Well, what do you feel as an environmentalist and ecologist of this general view that if you arrange standards to protect man, you probably have protected the environment?

DONALDSON: If we just had some guidelines on man why it would be helpful, but we've got to go back to warm-blooded vertebrates. And I think if you protect them, why, everything is in good shape.

STANNARD: There's no indication that there are highly sensitive steps in the eco-chain in the invertebrates or in the coldblooded vertebrates even?

DONALDSON: Well, there are during, no, no, not in the systems. There are stages in development, as you're well aware, certain stages of cleavage where cells are more sensitive.
That's why our "product radiation studies" is so important. Whether it's external or internal, those are important because they measure the total response. I don't know any studies, and this isn't just egotism, where the total response is involved like that because these animals have to go out into the sea, and they must compete in a vicious competitive natural environment. But importantly, all systems have to go, and they have to have memorized their way back. They have to have that mental response to redo that chain back up here. If there's any impairment of any system, it won't

work. You may not know what system it is; but if you didn't impair something, it's tremendously comforting.

STANNARD: That's very interesting. It's almost a behavioral study, isn't it?

DONALDSON: Yes, but in the most vicious competitive environment we can think of where they have to leave this little pond, go out in the sea, be gone for 2, 3, 4 years, and then come back and repeat that. Come up through the locks and come into that little pond and back.

STANNARD: I hadn't thought of it in that light. Rochester, as you know, has a behavioral toxicology program. They fuss around a lot about pigeons pecking, monkeys doing things and trying to find changes due to low levels of many toxic agents including radiation. They've found things with mercury. They haven't found anything with radiation yet, as far as I know. But it is a natural behavioral pattern.

DONALDSON: A tremendously complex one.

STANNARD: Yes.

DONALDSON: You know the migratory pattern of birds. Well, that's exciting, the homing of the bee. But he left and came back again within a short time. But here, it's gone a long time and made a tour of maybe 10,000 miles, two trips around the gulf of Alaska, and then back. Part of them go north ; part of them go south. I'm just working on a paper now on distribution patterns for the fish because now we have tag returns for recoveries in the sea. And they're

down as far as northern California and as far up as almost the Aleutian Islands to the north. And then they home back.

STANNARD: Nature's got a lot of secrets, still.

DONALDSON: You bet it has! DNA is the most marvelous thing that was ever put together.

STANNARD: There was a note that the omnivores came up with the highest concentrations of radioisotopes rather than the carnivores. I'm not sure I understand why.

DONALDSON: Well, the loss of successive steps, if you go back to the omnivores eating algae, for example, that have the first absorption and that transfers then. And then the carnivore comes along and he loses some and step by step. It's pretty well documented.

STANNARD: But the omnivores had much more than the carnivores?

DONALDSON: Well, the carnivores you see are eating the omnivores and moving up the chain one more notch; instead of concentrating, there's a biological loss.

STANNARD: There isn't much said about the pyramiding effect. Did that not happen?

DONALDSON: Yes, it happened, but I guess it wasn't emphasized.

STANNARD: Wasn't emphasized?

DONALDSON: Let's see. Probably Art can pinpoint that. We'll bring it up when you talk to Art. That was his area.

STANNARD: The NRDL has become kind of a question mark, a difficult area. I thought I could just go in and pull out all the NRDL reports and know what they did, what they found out. Instead,

to my utter astonishment, Ed Alpen tells me that when they decommissioned that laboratory in San Francisco they had some crusty admiral that insisted that decommissioning meant just that. The records went out along with everything else. Ed was so furious about the whole thing; he didn't try to save anything himself. And I'm finding getting NRDL reports and getting even a clear picture of what NRDL's mission was and how they fulfilled it very difficult. Can you from memory in your contact with NRDL people help me a little bit?

DONALDSON: Bill Shell probably can do better.

STANNARD: Who?

DONALDSON: Dr. Shell, Bill Shell.

STANNARD: Oh, right.

DONALDSON: You'll be seeing him. He's here. He worked at NRDL. He used to get teed off with them, so he came over and worked with us.

STANNARD: I see.

DONALDSON: And we had some instruments they didn't have. Our relation to them were always just they were there and we were here. But there was no really great interchange.

5/31/79 (49)

Norm Donaldson - U Wash College of Fisheries.

Tape 90 - Never believe total stay in rad effects
1943 until get through entire cycle.

Oct 46 Terrible tape full this last year
Hines - New book -
Cancers may be internal + external
radiation.

90B - ——— No measurements

57A - Security re/Pu was
classified as they saw it
but couldn't talk about it
because of classification -
Bugher was their main staffer
DBM Director -
BEC took & briefly made the rounds -

Steps at 12 Bill Stelle re/NRDL ← Director of Fisheries
NIRS-RSP-98 nat. - Saw him on Friday P.M.

Radioactivity Survey Data in Japan
Number 48

March 1979

NIRS, Chile

Interviews at Tacoma
Univ. Wash. Fisheries 1-6
May 31 June 1 1979

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re/ Pictures

(41)

NEAL O. HINES
8141 NE POINT NO POINT ROAD
HANSVILLE WA. 98340

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ote: 5/31/79

Of the UWFL refers to see: -

UWFL-91 Gamma Ray rates of Rongelap Atoll
1954-63 EE Held

Radionuclides in Foods of the Central Pacific, 1962
Palumbo, Seymour, Welander
Nature 204 # 5029. pp 1176-2
March 19, 1966

Uptake and Translocation of Radionuclides in Plants
Palumbo:
Am. Biology Teacher 27, 471, 1965

UWFL-92 Atoll Soil Types in relation to Retention of Fallout Radionuclides
EE Held, S.P. Cassel & R.B. Walker

Comparative radionuclides in Marine Life samples off the mouth of the Columbia River
UCRL_____ - Col number

Radiological Survey of Marine Organisms
"Evaluation of Life Thereon Region. UCRL-50 [illegible]
Vol. [illegible]

(42)

Accumulation of Zinc 65 by Oysters in a Natural Environment

From Disposal of Radioactive Wastes into Seas, Oceans and Surface Waters

DASEA - Vienna 1966 SM/72/38

p605

Seymour

Investigations Chemical Drift of Salmon Eggs and Alevins

David Bingham and Ronaldson

(Presented by Seymour

p869 SM-72/59

Homing Instinct in Salmon

Ronaldson - UCWF

Paper presented in Tokyo - 10 Sept 1966

Soos Creek Hatchery

Further interview of Lauren Donaldson and Allyn Seymour

10/7/83

See Seymour folder

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0026953

(1) 10/7/83 U. Wash Interview @ Lauren Donaldson

Lauren Donaldson

Real effective iron Cement Fuel
Matthias - Coal Mgr. of
Comm. of Calif. Engineers
for Hanford -
- hired to effectivate -
His assistant -
Must set up bioassay -

Donaldson - To Edmond Tolo as a
Metal biologist
Donaldson & Jaynes -
Scientific American

Built a salmon run in middle of
city
Simply coal to heat tubes
600 X10^6
Ch. Cabot fuel - Oregon Wash
Chinook runs fly to tide
U.W. campus -
July 1983 -

at Allyn
Segmy Picture

(2) 10/7/83

Rosenau Elvo
Sherwood Fawcett
Kirby Smith

Chapter II - @ Allyn Segments of

re/ Internal emitter committee activities -
From Little -
Early - little on internal dose
became external monitoring -

Rogelap situation could be cited
as evidence of concern for
internal emitters -
Program in Pacific in a not to
influence basis
Lauren echelon - to much help -

John Wolfe influence
(Cabot Chemist series)
changed emphasis as before and
after emphasis of environment
Cape Thompson volume - LLL
Alaska - Plowshare -
This was first "environmental impact
statement" type of document

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0026954

(53) 10/7/83

Cofa Thompson " 4/2 chapters

Comm of th Cake Thurston Physics
Alkire
Naval Weldmonoly - Ed.
Jack N. Wolfe

U.S AEC - Civ Tech Info -

Arch Neil Foster - if this was
first E.I.S in essence -

Chap II - Preface to 6 Fam Chapters
NHS #655 - Radioactive Waste
→ Disposal in Gulf + Coastal
Waters -
NHS #658 - Comm of Disposal of Radioactive
Wastes from Ships & Marine
Corr -
NHS #955 - 1962 - Radioactive Strontium
Rainout into Pacific
There were MPC's established for ocean - Later (CR(NE) reduced (1971)
Intended - Sporadic activity allowed - Process

Some of the factors mentioned in
Comm NHS-NEC apply
(Effective Atomic Radiation ------)

0026955

10/7/83 (54)

are frequently neglected

#551

Jr Club 10 09

Factors listed in Chap 7
are the ones -

Indiv Related to numerous factors related in
VM -

Bikinian people brought back as a
work force not as a stepulation

Cinematok people -

Lomen -
Should add Col Mathias to
staff Warren
Parker always there -

Explosion from start or
cold fish - Stalin wash up 30's
X-ray } - one eyeth on a dose
effects -
X-ray - We slander did not do
Phd work on equilibrium
Thesis was classified -

16/7/83 (5)

Change =

Lorenz — Numbers & animals 100:000

Problems was bigger than inside the plants -

While concept of things go for multiplant.

2 eg - 2 dog comes US multiplex

P — Mechanical etc problems much upsetting and problems -

No connection between goldfish effluent metals & water -

~~Manufactured Korea~~

Old X-ray machine 1943 still operated until early 1980s

Chief fish Kelly Barton x

16/7/83 (6)

No precedents for this amount of warm water -

Aquatic, cancer etc and Pacific Coasts started alone

Number of specimens near Hanford + 10 X. Brood reactor - Priest Rapids had changed water flows, etc.

cc/HRDL - Evan C Evans, myself, reporter Honolulu -

Lorenz and Bob Rose visit to Japan - May 1954

Japanese did not want to see physicists & engineers

Was a nightmare!

Quiet use fisheries

Japanese mental hygiene not facing up to it right away -

"Celebrate Japanese ecology again"

Because people know disturbances are early -

10/7/83 (9) (7)

Anecdotes
Heavenly = Resident Hawk
Foster & Egan Fish collect bird
1/2 LD while fish from
Columbia River
Did whole body counts

11-2 - Change of Boundary

11-3 Hanford
Mean Foster-Welander

11-3 how number data for
Libetan reptation - Henry
institute would be counting
radiation effects
re/ Inclusion and groups alone
and groups retained on
somewhat larger -
Quantitative significance
[Deck boron]
Return

Send hard - LST -
Battelle Center - PO Box 5395
Seattle 98165

10/7/83 (10) (6)

11-5
11-6 Trey & Jack Italian reference

11-16 Table of 500 mG

11-19
Fallon pap "Able - Baker"
deliveries - Constants
mound stem loaded at
natural population & finish
flies -
No's in mom and pop Bikini
after Able - Baker -

11-20 Phil Academy of Scien
Ruth Patrick -

Cadmium copies via Jeremy -
11-22 Date & Formulation of
Institute of Radioecology at
Univ of Georgia
Obtain from Mercer Award for the
work described on p 11-22

Table 11.2 - Hamilton, etc.

11/7 (8) (11) (9)

11.2-8 – How much LaBlanc knowledge & committed aspects?

11.29 Reinforcement – multiple Odens & Oak Ridge Earthquake.

11.30 RHS Personnel meeting –

11-35 Lab knowledge is a paroge.

11-36 More ? on Table 3 –
Windmills w/ 2000 c/ms.
Hanford w/ 1000 c/day at Penn

re/Fuller: Chp 10 of USRIME report.

11-37 – Annuity? Fire P.

11-42 – Influence of John Wolfe –

11-43 – Theories of the bubble geysers

11-45 – re/Alex Fuller
Not much Hanford work
* U Wash Rolla Seemingly

Table 11-4 Guideline only