

**Interview with Richard (Dick) Waring, by Max Geier, 2 p.m., Friday, September 26, 1997, in Dr. Waring's office at Oregon State University; Transcribed by Elizabeth Foster.**

*As a young forest ecology professor in the College of Forestry ca. 1970, Dick Waring teamed up with Jerry Franklin to lead the Andrews Forest component of the Conifer Forest Biome project within the International Biological Program. He was a critical leader of the program during the 1970s and into the very early 1980s as LTER began. His forte was ecosystem science and hypothesis testing; a constructive, though sometimes conflicted, counterpoint to Jerry Franklin's more descriptive approach to science. He helped recruit and mentor the cadre of post-docs who were central to IBP ecosystem science and the LTER program that followed, and he supervised some key junior scientists, like Bill Emmingham. In the early 1980s he peeled off to undertake a more tightly focused research agenda with major funding from NASA, but continued to advise Andrews scientists.*

**Geier:** How much do you know of what I'm working on here?

**Waring:** I don't remember exactly, but I read what you were trying to do for the history project, to find out why people came in, and various philosophies of the Andrews and why they got out.

**Geier:** Good. Yeah.

**Waring:** I guess that's not too far from the main theme.

**Geier:** Yeah, that's the main thrust. What we're trying to do is a history of the science and community out there. I want to look at not just the history of the Andrews Forest, but also the group of scientists that were involved.

**Waring:** Sure.

**Geier:** To get started, I understand that you joined the OSU faculty about 1963 out of UC (University of California)-Berkeley, is that right?

**Waring:** Uh-huh.

**Geier:** Maybe you could discuss the origins of your decision to come to OSU at that point and your academic career at that time.

**Waring:** I had a background in forestry at University of Minnesota, both a B.S. and master's degree. And got accepted to go to either the University of Michigan with Stephen Spurr or to go to Davis (University of California-Davis). Spurr became president of the University of Texas, so I went to Berkeley rather than Davis because I could actually teach in the forestry school, and get a Ph.D. in botany and physiology with a minor in soils. When I graduated, there were a number of jobs, and this was the one that was initially full-time research, but with a potential to fuse the Forest Research Lab with the College of Forestry, at that time the School of Forestry [UC-Berkeley], so that you could have some teaching, at least, at the graduate level. There weren't any jobs compared to today, so this was one of the better jobs to start out at.

**Geier:** Were you from Minnesota?

**Waring:** No, I was born in Chicago. But my wife is from Minnesota. And there were no forestry schools at that time other than Minnesota, in the Midwest. There wasn't any in Illinois. I don't think there was any in Wisconsin. So, I guess I could have gone to Michigan. We used to vacation and go for canoe trips in Wisconsin and up in Minnesota. I had contacts through the scouts. And through my father's business contacts, I knew a guy named Hubachek, who had purchased land as a Chicago lawyer and then dedicated it to research. So I started out as a young lad right out of high school, met all these scientists and started doing science. Then when I started forestry school that fall, I already knew lots of Latin names of plants. So, I had already started into research. I came out West in '55, and worked in Idaho for the [U.S.] Forest Service.

**Geier:** What did you do in that?

**Waring:** I was on a study with Don Leephart and our job was to look at pine roots and see historically whether trees that were on droughty sites had more damage that might account for their dying back. We created artificial drought by building big platforms and trying to shed all the rain and snow off of these platforms onto the ditches. So, it was my first experimental manipulation, almost my last. (Chuckles)

**Geier:** Why was that?

**Waring:** Hard work! Digging in rock, putting up platforms.

**Geier:** Right.

**Waring:** I started out in the Siskiyou in southern Oregon, probably the most complex forest system in terms of vegetation and soils, certainly in the West and possibly in the U.S. At that time, we had about seven years down there to develop techniques that gave us a measure of what we thought was the effective environment for the plants. Rather describing what slopes they're on and what elevation and what kind of soils and that, we started to use physiological measures of drought, and physiological measures of availability of nutrients. That was a new framework for forestry research, and was the basis that we thought might be able to move from different areas to make comparisons and to model not only distribution of plants, but also the growth of plants, at that time in forestry, that was the key thing. So, I started to do a little bit of work on the Andrews on the east side [of Willamette Valley] as I was moving out of the Siskiyou, and I was also beginning to be very interested in the idea of modeling.

About that time, just about when I'm getting tenure, at seven years, the International Biological Program [IBP] had started. We had a conference at the University of Washington-Seattle. Jerry Franklin was there and others, and we had to debate if we were going to be a part of this, and where were we going to do the research. Two big choices were either McDonald Forest or the Andrews. The Andrews was the only place then that had a few wildlife studies, particularly pens where they were keeping out birds and rodents and were looking at the seed mortality, and they also had gauged watersheds. And you had some control over where and when it was logged. Whereas at McDonald Forest, it was more market-driven when there was an opportunity to log. So, it was much more difficult to control an experiment on McDonald

Forest than the Andrews, even though the Andrews was much further away and on federal land.

In 1969 we wrote the first sub-proposal to go into this large IBP project with the University of Washington and other universities. We had the University of Washington lead it, and we were the next principle, I guess you would say. Over the next five years, we developed different philosophies than the University of Washington. They kept the motif of professors and graduate students, and never really developed, except in oceanography and stream work, a philosophy that brought new people in that could be post-docs that could cross fields. We actually reduced our commitments to full professors and graduate students, actually took them out of the program. Not maybe the wisest thing to do as a young associate professor, but we did it. We brought in people that had a background from other fields. This includes Fred Swanson, Kermit Cromack, eventually Jim Sedell, and at the very tail end, also Stan Gregory, in terms of graduate students that came after the IBP and got an appointment as an assistant professor in fish and wildlife.

**Geier:** Now, what the rationale for that?

**Waring:** The rationale is that we needed the full-time commitment of people that already had a degree because we were doing field projects and lots of workshops. And we couldn't do it with graduate students, and the professors weren't able to make this leap of commitment to learn these new fields and to integrate them. So we assigned that to the post-docs, and usually that was a person who had a master's degree or was getting a master's degree. There's quite a bit of resources with the responsibility. It wasn't just, "You have to do this." It was like, "You have to do this, but here are the resources to do it." Phil Sollins also was one of these. At that time we could see jobs on the horizon either at the Forest Service or at the university [OSU], so we didn't feel guilty about exploiting the talents of people from other fields when you could see that there was an opportunity to move them into classically forestry enterprises where you had to have a degree in forestry, otherwise you wouldn't be accepted. We broke that convention at that time, big time. We did it! And it made a large difference because we began to look at decomposition as a process, independent growth, and how the whole system was put together. There were certain difficulties. One was developing things before the models were in place. And the modelers were trying to get data from graduate students and professors who weren't ready to give it up, either because they weren't sure it was okay, or they weren't sure they should release it to these people and not know what they were going to do with it. So, these very elaborate models turned out to be basically usable only on a few sites where you had all the data to drive it, and then basically described what we measured. They weren't dynamic in the sense of what happens when the climate changes or what happens when we cut the forest or something like that.

About five years through that, I became very interested, at least in looking ahead in two ways. One is how do you extend these models so they could be used to study clearcutting of Watershed 10, which we were going to do, and make predictions on hydrology. The easiest thing we could do is to see what kind of process models we had in hydrology, which is the most physical-based and least biological-based of all the processes that go on in watersheds and in ecosystems. We linked up with people in Coweeta [USFS experimental forest and IBP/LTER

site] and Wayne Swank, who also got his Ph. D. from University of Washington, so we knew that. And we developed a general model that we could test in Arizona, Oregon, and North Carolina [where Coweeta is located; near Georgia border]. That was published actually as the major synthesis out of this book here. Along with it, the University of Washington had a series of people, Dale Cole in particular, that put together the nutrient-cycling balances of not just Oregon and Washington. This is the premier first attempt to actually show generalities among biomes and to put them into quantitative models that were generally based on processes rather than statistical correlations. Until then, almost all the watershed models were tuned to how much water flowed out and how much came into paired watersheds. You couldn't extrapolate them. That was quite a refreshing insight that perhaps this was an opportunity to go further in this direction.

**Geier:** So this was in 1981?

**Waring:** Yeah. So, I personally began to look for more ways to do experiments to test hypotheses that would allow us to have fundamental models. At that time it meant taking another sabbatical to Europe and working with Paul Jarvis, who we'd brought over also into Seattle at the tail end of the IBP projects. We started using radioisotopes, not the stabilized stuff, in order to quantify the estimates of fluxes through trees, soils and the system. That worked out, although we started on the Andrews and a little bit up at the Washington site. We really wanted to test these broadly. The disadvantage of the Andrews is that it's basically a spot. If you were to generalize anything from there about drought, you hardly have drought. If you were to generalize about heavy winter snow-packs where you have permafrost, there's no permafrost. So, it isn't inadequate for many things, but it's perfectly inadequate for reaching generalized models. It's a great spot, but it has limits. In 1982, actually earlier than that, it was in the '70s, one of my graduate students, Henry Gholz, had gotten funding to work on the Andrews, but his Ph. D. studied a gradient that goes from the Oregon Coast to the juniper woodlands [Central Oregon], that's called the Oregon Transect. That had all the variation in production that exists in the forests in North America, in just 250 kilometers. Since then, there's been 50-60 publications, including major projects that NASA has sponsored. And to run those projects, you have to have somebody fully dedicated, just like you did for the IBP, for something like a five-year period.

I guess my abilities run toward projects that have a beginning and an end. I'll do anything for five years involving up to a hundred people. But, I won't do it for life because I can't see how to finish these things up, do the synthesis, and then go on my learning curve about how to scale or do things further. So, every five years or so, I'm about ready to take a break. It was about ten years, actually, on the IBP, but then it transferred into the LTER. We wrote the cover grants, Jerry Franklin, and I think, Fred Swanson, may have been on the first one, to make sure there was a continuity. And we knew that things were going to change, the perspectives and the focus and the applications. But we thought it was in good hands. And I think it was.

**Geier:** Your primary position in the group started somewhere in the mid-sixties with the IBP, and then extended through the late seventies?

**Waring:** That's right. It went, certainly through the late seventies. This book came out in '81, so that's still involved in '80. I wrote the first rough draft before I went on sabbatical in '69, so that's about ten years.

**Geier:** What was your perception at that time of the site that is the Andrews? Do you recall first impressions of the people working there at that time?

**Waring:** I thought it had a lot of potential because it had a road system and it had control. The McDonald Forest had a road system, but not the control. It had the gauged watershed, so you already knew what the runoff was, but what you didn't know was how to predict it. So that's an opportunity. I thought most of the people that started out were highly descriptive. I started from that same vein in soils and botany, but as I moved around at three different regions, Minnesota, California and Oregon, I realized that you had to generalize and you had to scale differently. So, I began to look for these common properties that work everywhere. And anything we did on the Andrews we hoped would fit in everywhere else, but it was just a point.

**Geier:** At that point, the Andrews was basically one site of many you were involved with.

**Waring:** Right. I couldn't do that because I was involved 80 hours a week holding up everybody and trying to keep the money flowing. I was the flight director. We'd cut up the budget each year with the University of Washington. It was a tremendous amount of administration.

**Geier:** I was going to ask you, were you involved in 1968 with a meeting at the University of Washington with you and Jerry Franklin and, who was it, Dale Cole?

**Waring:** Dale Cole and Stan Gessel.

**Geier:** That was concerning the establishment of the project, right? Maybe you could talk a little bit about your reflection on that meeting and what your concerns were.

**Waring:** I think concerns that people who attended it had, outside the University of Washington, is that it looked like everything was going to start at the University of Washington. Maybe, maybe not, but there wouldn't be much off-site. We thought there were some good people at the University of Washington. I had close friends in physiology and they had really excellent work going on. But, we didn't think it was wide enough to really develop these models. So, there was certainly a concern about organizing very quickly to have more balance. And I don't think there was a tremendous uproar about not allowing that to happen. I mean it was difficult, because we had all kinds of university representatives there. So, we couldn't have ten sites fully supported; we might have one-and-a-half, and we were the half. There were sites also in Alaska, individual little stations where people worked on making a few measurements and trying to coordinate. But we didn't have the resources to do everything at the same time, without quite knowing how models were going to work and before we knew what measurements should be made.

**Geier:** So then the concern was one of institutional involvement or was it more a concern of where the program would go if you weren't involved in it?

**Waring:** I think we wanted to be involved in it because we were interested in modeling ecosystems. And to be fully involved in it, at that time, we thought we had to have a site nearby. You'll find now the world has changed, that you can have these large projects and you

can pull people together for three years. You can put them anywhere in the world; Bora Bora, Amazon basin. That wasn't an opportunity during those days. It wasn't just because we didn't have, we did have airplanes, but we didn't have the electronic communications and network setup, where other sites, not the LTER kind of network, but other more permanent sites, were available for us to move in. Other than the Forest Service research stations, most of those just had a station, and a building, and we visited a lot of those and used them, but they didn't have the cadre of people around to really do the research. The great advantage of the IBP, was we did reach out and with people and with other biomes, particularly the tundra and eastern deciduous biome people working in decomposition and water and carbon-cycling. We would link up with those because we needed each other to see the methods, and that really brought us together across the country. But more at meetings, rather than actually going to each other's site and gathering data.

**Geier:** You became aware of what other people were doing but you weren't directly involved in the curriculum?

**Waring:** Well, except in writing up the synthesis.

**Geier:** My understanding is there were some concerns around 1962-63, somewhere in there, about the possible closure of the Andrews by the Forest Service. Do you recall anything along those lines and how people responded to that?

**Waring:** Well, I think that was because they thought they had done all the research they needed once they demonstrated that you could do cable-logging.

**Geier:** Uh-huh.

**Waring:** Because that was the big thing, to get away from clearcutting with roads everywhere. They demonstrated that, and also, I think they had success partly from the Forest Research Lab work, and partly from regeneration specialists with the Forest Service in getting seedlings to grow. Before, they were dumping seed on the ground and rodents were eating it. So, they said, "We solved the problems of getting regeneration back, and of getting roads that don't immediately erode when we take the timber off." I think was part of it, and it is expensive research, the monitoring of water quality and water flow. It got a lot more expensive (Chuckle).

**Geier:** Was there a shift in focus from what was expected?

**Waring:** Well, I don't think the Forest Service had fully decided to close this experimental forest [Andrews]. Then we came in. I think they were concerned about the investment and return, now that some products would come off of it. We actually took advantage of that by saying, "Well, what you're doing is fine. We can build on top of that and if you keep it open, everyone will benefit." Ever since, that's been the relationship between the U.S. Forest Service here and the university [Oregon State], and I think most people would say that everyone benefits.

**Geier:** At that point, you took the risk before the IBP was established, so that the university was just concerned with it as a possible research site?

**Waring:** That's right. Just as a research site. And I knew Ted Dyrness and Jerry Franklin, back when I came.

**Geier:** They told me they were trying to recruit people down there.

**Waring:** Yeah. It's been a long relationship. I was working up in Cougar Creek, which is one of the drainages that goes into the McKenzie, and making measurements of climate and tree growth in clear-cuts and non-clear-cuts. So, it was not too far away from the Andrews already.

**Geier:** Cougar Creek is up the McKenzie a little ways?

**Waring:** It's just a drainage near the Cougar Reservoir before Cougar Reservoir existed.

**Geier:** Okay. You all were interested in the general area.

**Waring:** Sure. Sure. Because I wanted to extend the principles and the ideas that we started in the Siskiyou, to see whether we could generalize them.

**Geier:** At the time just prior to you being established at Andrews, what was the degree of interaction between your OSU colleagues and the Forest Service scientists?

**Waring:** Oh, one-on-one. I had a fishing buddy over there. We were really good friends, we reviewed each other's manuscripts, that kind of thing. But there wasn't any formal arrangement for seminars and there wasn't any formal attempt for project leaders to involve the University, or vice versa.

**Geier:** Who was your contact?

**Waring:** Ken Krueger was my friend. He died very young. And then Jerry Franklin. But Ken Krueger was a physiologist, and so that was my closest link. I knew Eileen Boyce. I knew a lot of people over there, but I was interested in research. We were full-time research. We didn't want to completely duplicate what they did on the same place so we would communicate. But, I don't remember graduate students being trained. There may have been a few; mostly technicians at that time in both our shops. I had one graduate student, maybe two or three, in those first seven years. One was Bill Emmingham. Another one was Brian Cleary.

**Geier:** Who would have been a founder of the core HJA projects? Jack Rothacher?

**Waring:** Yeah, he was there. And Richard Fredriksen was their hydrologist, so I obviously knew him.

**Geier:** I was also curious, at that time, did that strike you as unusual degree or a different type of interaction than other places you had been?

**Waring:** At [University of] Minnesota they had an experiment station upstairs in the same building as the forestry building. So, I had worked for those people in the summer, so no, it didn't strike me as unusual. I was used to that kind of relationship. I worked for the Forest Service at least two summers, so I highly approved of it.

**Geier:** So, it was something you'd kind of come to expect by the time you got here?

**Waring:** Right. Also, in my Ph. D., I worked with three other professors and three other graduate students on a joint project in the redwood region. So, I already knew, both the tremendous benefits and also some of the problems that arise in joint research, where everybody else, even though you help them, data doesn't come when you need it. That kind of thing. Usually three-quarters of it works out, and three-quarters is a pretty good investment.

Usually one quarter does not. And that shouldn't surprise you. I mean, once you recognize those averages as what you should expect under the best of conditions, then you can live with them. Or not live with them.

**Geier:** The biggest hurdles were coordination of the work, not necessarily the institutional differences?

**Waring:** Yes, and people. I mean, people are the biggest opportunity, and the biggest problem. Because, if you can't get the personalities to match, you can't very well send them out and expect them to get much work done. We'd have people saying, "We collected it, or we're still collecting it." And the other people would say, "Well, they haven't given it to us yet." Then, pretty soon you have to figure out a way to replace both those people.

**Geier:** Any examples of that?

**Waring:** We had some tremendous people climbing trees, studying lichens and growth and all that. Then we had people down on the ground waiting for branches to come down and to do the elementary analyses, and they weren't coming down. Actually, the people doing the tremendous work on lichens, overran their budget by \$10,000 a year. Then the department head wouldn't back them up. So, you're looking at how to solve the problem, not how to create more of it. You could find a graduate student and try to save that person and get the work done, and put the post-docs to do the other things, and it all works out. But, you just can't have two professors blaming one another and having their students caught in-between. And of course, the product isn't coming out that you need for the models. Somebody's gotta move, and I never had any problems moving. I tend to have very short meetings with a very short agenda. You come, you know what you're doing, then that is great. Then you can have a long meeting. I can't keep my attention on all the possible things that come up in a long meeting, so I don't have long meetings. Lots of other people like long meetings, and do very well at them. But I don't go to them, because I can't focus on all of it. I can't integrate in my mind where to move next, what consensus we have, if it's basically a long, wide-open discussion. I like wide-open discussions, but I don't call meetings for those. I'll have a workshop in the evenings, something like that. I have a different philosophy that I've lived with. It works for me great. It sure gets a lot done in a short time, and most of the people I work with are highly productive. You can sometimes sense that this is really hard to do with a huge group. You need to break them into groups of five and ten people, and have them reach a consensus, come with their reports, and have a decision. Because you're shifting money from one budget to another that time, or from a joint venture, or from a small pool of money, into that. We made some really hard decisions in switching away from forestry kind of things over into the stream group, because we thought they had more exciting things to do with the money, and we shifted it.

**Geier:** When you say we, was there a --

**Waring:** -- Jerry Franklin and I.

**Geier:** Oh. Okay. Sat down together --

**Waring:** -- With a huge crowd of twenty people in the room, it's not --



**Geier:** Okay.

**Waring:** -- but they would come in, present their reasons why they needed this. We would look for alternatives. We would have the meeting, hear the alternatives, make the decisions, and commit the money. I suppose it still happens today on the small projects of three to five people. But once you get up to a hundred people or something like that, it's very, very hard to do that.

**Geier:** There were a lot of people there.

**Waring:** Yeah, but we still reached these kinds of decisions.

**Geier:** Of course, there was an end in sight.

**Waring:** There was an end in sight, right. Also, there was a sort of crisis, if we don't do this then, we can't go to the national meeting, we can't renew the grant, etc.

**Geier:** Right.

**Waring:** I mean, we had to show what we were doing. They came out and reviewed our work because these were big grants, a million dollars a year, back in the sixties. A year? (Chuckles)

**Geier:** Yeah.

**Waring:** When people were being paid \$12,000 a year full-time as an assistant professor

**Geier:** Yeah, right.

**Waring:** So, you got a lot and had almost no overhead. I mean, a very small overhead. So it was the kind of thing, they'd come out here, and they talked, in fact, we wanted them to talk to the graduate students and talk to the post-docs to see how each one fit into the larger project. That's what we'd been working on, how they fit into the larger project, so they knew why this was important, before something that might be on their thesis came up as important. So they could explain that to the NSF reviewers. That wasn't true at the University of Washington.

**Geier:** Is that right?

**Waring:** Yeah, because they didn't have the structure to really encourage that to happen. It took a big, big plan. I said in this one little paper published, an internal thing in the Coniferous Forest Biome pubs, about integration. "The most valuable product of the International Biological Program in the United States will not be the systems models that will aid making decisions concerning land and water management, but training people that will bridge the communication gap between disciplines and institutions." It's not exactly profound, and there wasn't necessarily a consensus with the University of Washington. I think he was a post-doc at University of Washington, and I was an associate professor here. We got it in writing, looking at the organization of the biome as a system, the actual organization of it. Whether or not it's an honest interpretation, history will judge that. That's what we put out at that time so people could see how the thing was organized. Obviously, there's some political sensitivity in trying to not create big waves, because we were different. They had post-docs at the University of Washington, but they didn't assign them the same responsibility we did. Or where they did, they couldn't seem to get the data when they needed it. Sometimes, it's people again.

Sometimes it's the post-docs that were involved, although not anything is wrong with the professors.

**Geier:** So, in general terms, your perception is that the University of Washington program was a little bit less efficient, at least, in reaching these goals that IBP had laid out?

**Waring:** If they did reach it, they reached it in a different way. A lot of things came after the IBP. Because there were still data-sets, and sometimes some really sad things would happen. A very young and good graduate student would die. The thesis would barely be in the form that you could extract anything from it. You can't blame the University of Washington when a graduate student dies, one that has a big responsibility for the program. It wasn't a post-doc. That's one of the reasons why we brought Paul Jarvis over as a professor to see if he could help us pull these pieces together. And he did. So, as long as you had alternatives and you could make the decision in a hurry, then you could usually solve the problem. But you had to have money and you had to be able to make the decisions. But if you didn't have money and you couldn't make decisions, well, then, basically, you couldn't succeed.

**Geier:** So the difference would be that you might have a post-doc who might die, but if you could replace him right away.

**Waring:** Right. (Chuckles)

**Geier:** I see. Okay.

**Waring:** Or they'd have more than one person that knew how that stuff fit in. We failed, too. We tried and tried with some of the wildlife people to get it together, and we didn't have a good enough feeling of how the role of wildlife in the forest really fit in, because we were just studying undisturbed forests. Most of the role of wildlife is in what happens after the disturbance, in terms of how quickly these animals and insects speed up recolonization of the system once it's been really disturbed. You look at the disturbed conditions, they're hardly eating anything. I mean they're keeping alive, the system is supporting them. But, what are they doing? Until we started looking at disturbance, and insect outbreaks and disease outbreaks, as mechanisms to reset the system, help cycle nutrients more efficiently, and bring the thing back up to a standard that had been sinking into a very-stressed state, we couldn't really see the role. And that's not the wildlife people's fault. They were trapping animals, looking where they lived, and finding out the gradients and densities of animals. That was great stuff. But we couldn't really quantify, until much, much later, some of the roles that they played.

**Geier:** I'm sure all the wildlife studies were one of the reasons the Andrews was selected.

**Waring:** Right, but those studies were almost done. They still had the fences up and you still could see they were working, but they were already being published. They had the actual experiments on the ground so you could say there was a history of experimentation, weirs and sites where people had manipulated the access to seeds by birds and mammals.

**Geier:** Both of them noted the lack of wildlife studies since the IBP days.

**Waring:** Well, maybe since the IBP days, yeah.

**Geier:** There was an NSF inspection that took place in 1971, I understand, where they came out and evaluated the program. Can you recall those inspection tours? Apparently, that took place every year, right? If I'm not mistaken.

**Waring:** It was certainly every other year. I'm not sure it was every year. When was it? '71?

**Geier:** What I've got here says 1971. I'm just curious how the group responded to that, if there was a group response to how those sessions were conducted, and what the product of them was.

**Waring:** We had a series of them. The two most important, I can't remember the exact years, would be where the NSF people would be asking, as they walk through the woods, people next to them, how the whole thing fit in terms of the arrangements and how things were working. One of the things that we did to improve our chances of actually having an integrated program, is we would go out ahead of time with a group and we would first listen to their stories, and then, we practiced it. And to make more fun out of it, we would tell each other's stories. So, I might tell the stream story, and Jim Sedell would talk about how water moves up trees, or something like this. But we knew enough of the story to be dangerous. Well, when Jerry and I are writing the final reports each year, this experience is invaluable, because this allows us to write in the same style with the same major "hits." We'd look for important "hits," meaning linkages between programs and ideas, and on the field trips we'd actually have, "Oh, let's do it this way. Let's put this chemical in and turn this thing green, and make these rocks look like chlorophyll. Let's do it, and now let's go from this series to here." We actually figure out the most reasonable thing to do. We would start at the bottom of a watershed and walk the people up, when we should have taken some of the older people to the top and walked them down, because they were very, very tired and not too happy (chuckles) in cases where we walked them up and around, which is a 45- minute trail, minimum. It's a long, 25-acre watershed, and it's steep. [Watershed 10 - HJA EF]

**Geier:** The people on that committee would be senior scientists from back East?

**Waring:** Well, they would be from all over, including other biomes.

**Geier:** Okay.

**Waring:** But, we had already been reviewing some and we had workshops. But, there would also be some senior administrators from NSF. This is a huge amount of money, line itemed by Congress to NSF for these things, and most people didn't think it was going to be a good investment. At the end of it, I think most of the reviews would say it wasn't a good investment.

**Geier:** Is that right?

**Waring:** Yeah. Very, very few positive things about it, measuring everything, rushing back and forth, and the models are too complex. I mean, all of that's true, okay? It's only when you look at the legacy of the next generation, and how they were able to better integrate and see across the systems and begin to see applications where other people didn't, then you could really evaluate it. It's sort of like bringing up kids. I mean, you're not sure when they're in high school whether you want them to grow up. (Chuckles) But you better wait a while, because you have a big investment. Another decade later, then you begin to make your judgments.

**Geier:** The criticism came from one NSF director or from --?

**Waring:** No. They had independent reviewers doing these things. They just couldn't see for the money spent that we had advanced our understandings. The models were oversold. The computer systems weren't able to handle all this. The stuff was not well-documented, I mean, in huge data banks. Think about all these people trying to get this stuff in different formats. And our big concern was to get just enough so we could see how the pieces fit together and get something out. We even started with these gray literature things. Lots and lots of gray literature. Well, that doesn't count in the scientific establishment. Particularly, it doesn't count when the precursors to the IBP in the United States were the watersheds at Hubbard Brook. Everything that was published in science was very, very carefully done by a group of very fine scientists – Bormann and Likens and some very good graduate students, but not too many, and over quite a long time. That's the standard and here, all these people are practically trying to do it. You're not going to do as well with that kind of thing. On the other hand, the training, the other perspectives and cross-fertilization of ideas, made a difference in our lives, because we were able to travel to Hubbard Brook, travel to Coweeta, and see these things, and meet young people who were coming up the career ladder. So, you have these more collegial relationships than you would normally have in those days. Good and bad. (Chuckle)

**Geier:** In about 1970 or '71, you were involved in a decision to hire Art McKee, I understand. Maybe you could talk a little bit about your expectations of Art's role at the Andrews?

**Waring:** Originally, he was hired to be the local site coordinator, or director. I guess I was supposed to be the site director, so he must have had a different name.

**Geier:** What was your title officially?

**Waring:** Site Director.

**Geier:** Site Director.

**Waring:** Of the Oregon site.

**Geier:** Oh, Okay.

**Waring:** And I was stationed in Corvallis. Art initially was stationed in the Andrews, so he must have been the site coordinator.

**Geier:** I think that's right.

**Waring:** He must have had a different title. I liked the idea he'd come from another part of the country, so that gave him a different perspective. Art's also a good writer, and I always appreciated that because you can see opportunities to have ideas put together and shared at that time, as we still wrote memos and letters. Even later, Art had a potential to be a Ph. D. candidate, and we knew that. He had the brain power and the writing abilities. Yet, at that time, he thought it would be fine to be on the Andrews for a while. We worked very, very closely with him, in all the coordination. You can imagine, this is basically a phone call every night for five years or so. That was pretty intense. He stayed up there, must have been a decade. He knows. Then he had kids and wanted to get into a different school system, and for other reasons, he wanted to move here [Corvallis]. Then, I hired another site coordinator who

struggled very, very hard to do everything, and who didn't have the same innate abilities. Things wouldn't get done quite right, no matter how hard he worked. It was not the same as what Art could do.

**Geier:** Who was this?

**Waring:** Who was that? (Chuckle) I'd have to look up the name.

**Geier:** He wasn't there very long then?

**Waring:** He was there a couple years, but the met [meteorological] stations became uncalibrated. You run into these people two times in your life. They're really working hard and you're trying to help them, and they're working right at their capacity, or maybe just slightly above it. When something happens where you have to do more than that, that's really unfair. You push them above that. You can get people that can work at about three-quarter capacity, they're the same for crises and they're motivated, so you're usually home free. It takes a long time to recognize them. There's a whole bunch of them I've found that really want to do the work. They want to be a musician. I wanted to be an athlete. And the attitude is just so good. They'll come to work for nothing the first month, that kind of attitude. I used to be that way, too, when I was starting out. But they don't know what their real capacity is. And then, there's a later stage where you should be on guard, and that's where they're good. They're in their forties and they still haven't had a job for more than one year at any place. Then, all you have to do is check, because there's something else wrong here. If it isn't this trying to do more than what your ability is, there's something even worse involved. Those are the two things that catch. So, you begin to look for talent, and then, you burnish that talent. It's much easier with your own graduate students than it is with other people, but you still find if you talk to people and ask them about ideas and that, that at least when you get into these kind of programs, you can sort out who might be a good site manager from those who might really struggle. We never found anyone that didn't want to do the job and didn't try hard. We never got anybody who was lazy at that job. You've got people to work with, you've got reports to write, you have instruments to calibrate, and it's a tough job.

**Geier:** I'm curious here about the rationale behind why a site coordinator was deemed necessary in '71, because there hadn't been one up until that point. Is that right?

**Waring:** Well, we had so many people coming that didn't know where to go and where the sites were. We had phones and we had to put locks on those phones so people didn't call everywhere. We initially set up a whole bunch of tents, and then, we had trailers off-site. Eventually, we brought in used trailers and set them up where the present stations are. At the very end of it, we had to redesign plumbing systems with the state and federal government together. That obviously became more than Dick [self-reference] was able to handle for research interests, because you're now building bricks and mortar. That's a different kind of person than a scientist who's interested in the beginning and end of a project. It's sort of like a boat builder for oceanography. You know, somebody will have to build the boat and equip it, and then the oceanographers get on to do the cruise to gather the data to publish together. Well, I'm great at integrating the oceanographers to get on and publish it, but I don't do plumbing.

**Geier:** Yeah, I was gonna ask that.

**Waring:** It was hours and hours of these things, and things would break. I think there was two years, that if I hadn't hired a masters-level person, I wouldn't have had any publications out. And with that masters' person, we got nine out. I'm re-writing a textbook with him right now.

**Geier:** Are you co-authors with him in other words?

**Waring:** Yeah. But, I couldn't do it alone because the logistical problems that have to be part of your daily concern when you're running a large program and people need these facilities. You're gonna have little things like the dogs are in their things and people are using the phone, I mean, just little things that eat up forty hours a week.

**Geier:** You were administrator for a while, and you were also a university professor.

**Waring:** Right.

**Geier:** And writing publications. Sounds like you were having some difficulty reconciling those.

**Waring:** Well, for a year. I don't take forever to reach a decision (chuckles) when it's not working. When you're working eighty hours a week and you're still not getting any publications out, then you say, "Well, obviously I can't work more than eighty hours a week. I've got a young family and this kind of thing, so what are you going to do?" Well, you're going to figure out a way so that somebody else has to resolve it for you.

**Geier:** Okay.

**Waring:** Or you're going to hire more people, and we had the resources to hire more people. We had the need. Paul Jarvis documented the need. Jerry Franklin was in Washington, D.C. at that time, and we got a supplement. The supplement supported this master's student so that I could take a deep breath and still think research and do the rest. Pretty cheap investment for nine publications in two years.

**Geier:** Who's the student?

**Waring:** Steve Running

**Geier:** Okay. I've heard his name.

**Waring:** Steve Running, yeah.

**Geier:** Gabe Tucker was talking about him.

**Waring:** University of Montana. We're just finishing the second edition of this textbook. He brings all the expertise in remote-sensing and large-scale modeling to the book.

**Geier:** He went on to get a Ph.D. later?

**Waring:** Yeah, at the University of Colorado.

**Geier:** Any reason why he didn't do that here?

**Waring:** A couple reasons. My general philosophy for students, remembering that I went to University of Minnesota and [Cal] Berkeley, was you don't get your upper degrees at the same institution.

**Geier:** Okay.

**Waring:** Because one, you learn how to see different perspectives. And two, you have a whole set of new people you know. So, unless you've got a terribly constraining family relationship so you can't move, something like that, it's really desirable to go somewhere else. Now, that was also before there were post-docs. Now, I'm a little more tolerant because most people have to have at least one post-doc, if not two post-docs. Now, if they stay at the same place as they got their undergraduate degrees, their Ph.D. and post-doc, then I have all kinds of problems with that for their careers and a whole bunch of other things. Because, it's too incestuous a relationship.

**Geier:** That's what you advised people?

**Waring:** Oh, yeah. You bet.

**Geier:** One other question I had about Art was, how'd you go about selecting a site manager, and what kind of connections were expected?

**Waring:** Well, he came recommended from people in the Northeast. We knew from other people in the system that this guy had qualities we wanted, so he wasn't a complete unknown.

**Geier:** You mean in the IBP system?

**Waring:** In the IBP system, or, I don't think it was Hubbard Brook. I think he came from Maine.

**Geier:** I think he had been at Hubbard Brook.

**Waring:** I think so. He had a linkage. They said, we had a person who's coming west, or who'd like to come west, and is interested, so are you? I said, well, yeah. Actually, he had two chances to get a Ph. D., one with me and one with a professor in botany. In both cases, he didn't finish.

**Geier:** Is there a reason?

**Waring:** I think in one case there was family. And I'm not exactly sure what, divorce or things like that were going on at that time. But I think he had probably waited quite a long time to get a Ph. D. in those days, and it was going to take a lot of effort, and maybe not the same pay and responsibilities. So the question was, "Do I really need this to do the kinds of things I like to do?" In retrospect, he would have had different options had he had a Ph. D. at that time. Today, I'm not sure a Ph. D. does you any good at all. But, at that time, it would have given him different options. But, he still would have been writing grants, or teaching five courses a year. Those are the options. You don't get this thing, like somebody's going to give you a 12-month salary and research money. They might give you a 9-month salary, but they're not going to give you any research support, and you still have to do all the other things. I think maybe by putting him on the Andrews, put him in a little bit more isolated situation than was good for that final decision. But, he made the decision. He actually was on campus, and I felt sad about it because he had the capability. Okay? We're not arguing that. To do it, you have to jump some hoops. The longer you wait to jump the hoops, the harder it is. But it wasn't that we didn't want to see this person have the opportunity to get a Ph.D. and would force him to stay at the Andrews forever and not achieve that goal. That's not one of the outcomes. He had two chances.

**Geier:** And he doesn't sound too worried about it currently.

**Waring:** Oh, no. I don't think he is.

**Geier:** During the period of IBP, how would you characterize your priorities as an Oregon State University professor involved in research at the Andrews?

**Waring:** Well, I put it as the highest priority. I always have one high priority and just a couple modest ones beyond that. Because I can only have one major priority in science. I can't do three things at once, never have, never will. I can schedule things, so I have different months, different promises, I made. But I've got enough of a Midwestern background that my promises are sort of my word. So, if I tell you it's going to happen on that day, it's going to happen. Occasionally, when people told me something was going to happen and it didn't happen, we'd have some "public hangings," because I just wasn't used to that. From the Midwest, you promise this and we're planning to put this together, and I'm having to assemble this, and you haven't done it. And you didn't tell me ahead of time that you were having problems so I could help you. Well, that's something you don't want to do to too many Midwesterners. Because they're used to blizzards and expecting to have the chains on and water in the radiator. You don't do these things and you told them you would. This is not conducive, we had quite a big mix of people, and probably a third of them fell into that latter category. Just by chance, it's not that I wanted to do it, I think. They just had other things come up. This is when you're working eighty hours a week and you're meeting your deadlines and they're telling you, well.....(Chuckle)

**Geier:** How did those kinds of issues get resolved?

**Waring:** Well, in most cases, if the private and public hearings didn't get their attention, they weren't on the payroll the next year.

**Geier:** Oh, I see. Any examples of that?

**Waring:** Somebody pointed out to me once, that each person is supposed to bring a paragraph so we could help assemble these ideas and I could get to writing these integrated reports for the year. We were going to have them presented at these meetings so everyone else could hear what this person would say. I remember Phil Sollins was one of those persons, and he didn't do it. So he got publicly taken apart. It's like, you promised this. We got other people in line. Here you are today and you haven't done it.

**Geier:** Yeah.

**Waring:** You know, there isn't any excuse. Well, there was an excuse, but it wasn't something I bought. Then everyone else is terrorized, which is one thing that my professor at Berkeley taught me. You don't have to shoot the whole group. Just shoot one, for God's sake. There'd be other cases, where, mostly graduate students were trying, but they couldn't quite share their data yet. Then you wonder, how can we get the data? What more money do you need, or how can we do this? And then their professor would say, "Well, he's not ready to do that until the thesis is completely done." Well, that wasn't the deal. The deal was that as part of an IBP, when we're doing these workshops and things, we need to know what you have now. We allow you to correct them later, but we have to do these things, so we usually supported the



graduate student for one more year, and then terminate. We did that with three different professors in forest science, and I was an associate professor.

**Geier:** Is that right? Did that create tension in the department?

**Waring:** Sure.

**Geier:** Who was department chair then?

**Waring:** We had Dale Bever and Richard Dillworth. They were caught in-between.

**Geier:** And of course, money.

**Waring:** Well, I was looking for a way to do it.

**End of Side A, Tape 1 (of 1)**

**(Start of Side B, Tape 1 (of 1))**

**Waring:** We never quit speaking to the people. I'd still go to soccer games, and we'd still do it. At least I thought it had to be done, so it was up to me to go figure out how to do it. We'd hire post-docs, and we had some wonderful post-docs, too, that have gone on to be department chairs and other things.

**Geier:** What was the typical path how you identified post-docs to bring in? How did that occur?

**Waring:** They usually had been somehow attached as a graduate student to one of the other IBP groups. So, they already had been involved learning the ropes in a special field. Chuck Grier came down as one of Dale Cole's students, and he was great. He knew the soils and nutrient cycling, and he worked with one of my graduate students, Bob Logan, and they did some beautiful pieces of work together. That could be one example. Bill Emmingham, who was a post-doc here, no, he was still getting his Ph.D., and I think he also had about one year as a post-doc. It was much harder on the Ph.D.s, because when you try to give input --

**Geier:** That time, what would you guarantee up front?

**Waring:** Yeah, sort of. We could see that, and then at the end, either they'd find opportunities or we'd find opportunities within the university system. The system was much wider than just Oregon State, with all the contacts that we had.

**Geier:** I was curious, also, what person was assigned to do that?

**Waring:** We would have workshops where we'd try to formulate the major structure of the model and what variables we measure at what time. We didn't really have the models running before we started gathering data. There was one time we knew we had to, for example, upgrade the meteorological station. The basic models ran on more than minimum-maximum temperature and daily precipitation. Preferably, I'd like to have hourly precip so you could know whether it was snowing at night or raining during the day. Otherwise, your model can't predict the difference, as they needed to have radiation and humidity. So, even though the watershed stuff was there with the met station, it wasn't an adequate met station, and you

couldn't drive any models - photosynthesis, hydrology or even nutrient-cycling - without either estimating that or measuring it. So you would say, we better measure it. Actually, I'll bet it took us another decade after I quit before we convinced the other IBP sites, or LTER sites, to have a standardized set of quality met-data to drive these models. They didn't have them, or some did, yet didn't address that [standards]. But at the end of IBP when we were moving towards LTER, that was one of the things I thought was so obvious we wouldn't have to talk about it. Well, we talked about it for a year and it didn't happen. It was not ready yet. We have to get these models out, people have to use them, because what was good enough for the Forest Service twenty years ago on precipitation and temperature, wasn't good enough if you were going to generalize these models. Later on, we found out ways to correlate minimum-maximum temperature with radiation. But, at that time we had to have the good data out there to make a comparison. We didn't have to do it forever, but you had to do it for a year. That's just one example.

**Geier:** What turned the corner on convincing other sites to do it?

**Waring:** Probably that, and the reviews of their proposals. Those of us that were successful and publishing, ended up on the review board. It doesn't happen instantly, but if you keep using the wrong equations and these kinds of things keep happening, eventually somebody will say, "This isn't perfect, but at least you've got to have..." Also, NASA had done collaboration with LTERs for measuring and estimating incoming radiation, and they're estimating variables from satellites to compare predicted versus observed. There are additional reasons to put both stations out there. All things in time. Some things that we thought were important turned out not to be.

**Geier:** Any examples of things you thought were important that weren't?

**Waring:** I think it was important to know methods that did not work, and one was root production. We spent time in this country, and the world, hours and hours digging up and coring for roots, trying to separate them out and get production values by looking at cores every month or so, to estimate the change in weight of fine roots in soils extracted every month. Sometimes that gives you a pretty decent value, and sometimes it doesn't. No matter how hard you work, you get big errors. So, we've had to wait until periscopes came, where you could actually put these tubes into the soil and put a video camera down there to photograph the changes from white roots to brown roots to dead roots. Also, monitor the system, and then have independent estimates of how much carbon is coming in, how much is used above ground, how much is allowed to come below ground, and what fraction of that is likely going to the roots. So, you begin to get closure on your models by knowing more and having methods that work. And we had a lot of people making a tremendous effort trying to get fine root production and turnover. And some of those estimates are great. Some are one-third to more carbon than you possibly have available left over after you've accounted for above ground production. These were sort of originally going to be our standards. Well, when your standards are not reliable then, and --

**Geier:** Yeah.

**Waring:** -- you're not quite sure you shouldn't have done it [continuation of previous graph]. You're very unsure that's the real standard. That would be one example. Leaking watersheds were the original concern. You have these watersheds where you think you've measured everything that comes in and everything that goes out, but there's a fracture [in the weir or in bedrock] and a lot of water doesn't go out over the weir. That doesn't mean you quit measuring the watersheds, but it means that you really try a number of different ways to see whether or not you can get closure on those water balances. One way was to put isotopes and heat pulses into trees to keep track of the daily and seasonal transpiration. So, you made sure that was right. Then, changes in the soil water volumes. And if that was right, then, what's left over? At a certain rate, water should go into the stream. So, to check all your assumptions through the model, not just what the model said. That would be an example. I guess the latest check on a lot of our models is to estimate the total amount of carbon, for example, that was needed to grow a forest. We did that at that time by adding up the change in biomass. Here's a tree and it's growing this much, and here's the roots and they're growing this much, and then, it costs this much to maintain these leaves, and it costs this much to build this foliage, and so forth. Then add it up and say that's got to be how much carbon you have. When we did that and published on it from the Andrews, in one case, I think, we overestimated by two-fold. In print.

**Geier:** (Chuckle)

**Waring:** Other people overestimated by two-fold also. They made assumptions that turned out to be wrong in the models. The way we got closure was with techniques worked out by a micro-meteorologist and some people working with the biome, Leo Fritchen up at the University of Washington, who worked and worked and worked on techniques that required very high-speed computers and very accurate instruments. It's called eddy-flux measurements of carbon dioxide and water vapor, continuously above a forest, or above grass or any vegetation. When you do that continuously, you get the total amount of water vapor actually moving by the top, you get the carbon net flux in and out and at night and fall respiration. And you can get closure on these models. And, oh my God! It's off by two! This kind of thing. You get into the processes of the photosynthesis, stomatal control, and water uptake. Once you understand those, you have these ultimate constraints that I suppose in retrospect, you should have known anyway. Okay? But, you were adding it up this way and everybody else was doing it this way, and you were putting these figures in the models.

**Geier:** What, just reducing the errors of estimations?

**Waring:** Yes, and going back to first principles. We had principles about what the theoretical conversion of light into photosynthesis is. We had surpassed that. We had other cases where we would sample diameters of trees and correlate with the amount of leaves the trees had. When the trees got bigger and bigger, we would only fell trees that would not hit other trees. Think about it. Those turned out to have a high amount of leaves on them because they had more growing space than trees shaded by close neighbors. Then you take these diameter correlations, that are algorithmic, and you'd go out and measure diameters of trees and calculate the leaf area. What you'd end up with is values twice the amount than all the light that had already been absorbed.

**Geier:** Sure.

**Waring:** Well, we could have seen this in 20 minutes, and known it was wrong. We eventually corrected the literature. But a lot of stuff in the data-bank, and a lot of the stuff that has been published, it still has the equations that are off by two. These were really hard methods. We were felling eighty-, ninety-meter tall trees, and the Japanese were helping us collecting all the biomass and showing us how they do these things. It really wasn't any lack of effort or actually structured analyses that caused the sample to be biased.

**Geier:** What do you do once you find that problem?

**Waring:** What we did is we had three or four independent methods, and we got a publication out where a graduate student went to one of the stands that had already been estimated with this other equation, measured the amount of light coming through the canopy using the Lambert Law, and found out that with much light, the leaf area can't be above eight. The original method says 16 layers of leaves. Okay? We have litter fall estimates because we'd collected litter fall every month for two years. We knew that about 25% to a third of the leaf litter, turns over each year. Multiplied by four, that came out to eight. And then, we had a correlation with water relations with the part of the tree that conducts water, the sapwood. We had a linear correlation between the cross-sectional area of that sapwood and the amount of leaves on that tree. That also came out to eight. So, we had three methods that said eight, and we had one that said sixteen. This is how you trick things up you that need better estimates for. It hurt because our original standard was wrong. It's the same with the fine roots. What looked like the easiest thing to measure was the above-ground stuff. We felt we'll be able to do this and the Japanese were doing it. Turned out that we measured it wrong. And it was just because, if we felled the other trees that were representative, we would have taken down a lot of the neighboring trees and not been able to separate out which branch belonged to which tree.

**Geier:** Then, what it leads to is additional studies and more publications.

**Waring:** Right. I mean, you always publish and correct your mistakes. The principle I got out of it is, don't measure and make your estimates just by modeling and one method. Try to have two or three independent methods that estimate these on the same sites, even if you have to bring in NASA aircrafts to do one, and use two different sensors to do the others. That's what we did on the same days on the same publications. You can tell whether you are right or wrong. It gives you a lot more confidence. We still have problems, but that would be one example.

**Geier:** When did NASA start cooperating on these things at the Andrews?

**Waring:** Oh, I would say the big thing came when I came back from NASA headquarters. No, before I went to NASA headquarters.

**Geier:** You were there? I didn't know that.

**Waring:** Year-and-a-half. I'd been on committees with NASA, probably starting in the early eighties, because I was trying to learn about what these things could do to drive models from space. There was a lot of hype in engineering that these things do everything. But of course,

the engineers don't know what they really measure. They're correlated with things. So, I was called in as a physiologist to help with pigments and physiology. We would really start checking these things by writing joint grants with some of the people I worked on committees with, and Steve Running and some of his students. We would do it across this Oregon transect that Henry Gholz had already established, so we knew something about the history of it, the climates, and so forth. They would fly over these sites on the same day with eight different planes, anything they had, plus our own lightweight aircraft. So, we started our own "air force" here. We had three aircraft and all the remote sensing gear. We would make those comparisons and try to scale it up. We're still doing this now with people from other countries. This came out in '94, but most of the fieldwork in Oregon was done in '89 and '90. *Remote Sensing Environment* [The journal] has special issues, and this was from a special issue. This is one of our planes (showing Geier image in magazine). The link was that we could come down on the ground and make measurements, or in the laboratory check these principles that they thought the remote sensing instruments were measuring. Other groups of biologists and physiologists were involved, too. Once we had a bit more confidence, and once the remote sensing group had more confidence, then you could see these ideas being scaled, going regional, going to the earth level. We still had a lot of unknowns, but by having all these aircraft and ground measurements across a wide area of vegetation that gave them confidence that, "There's something here. There's the same instruments, the same principles, and it's global." Now you can see why a scientist like me was interested.

**Geier:** Sure.

**Waring:** You're always looking for ways to scale. This is one tool. The model is another tool. Stable isotopes are a third tool. For stable isotopes, I took a year's sabbatical at Woods Hole to learn about them.

**Geier:** At where?

**Waring:** Woods Hole Biological Lab in Massachusetts, for stable isotopes. I also worked some time that year with a group in New York. For me to learn these techniques and get these ideas, I had to leave Oregon State, or mount a new project where these people come in. I may leave for a year just to mount another five-year project for going to coordinate the NASA "air force."

**Geier:** When did you go to Woods Hole?

**Waring:** Uh...

**Geier:** In the mid-eighties?

**Waring:** Partly on a NASA grant to see whether these ideas would come together, partly on a stipend that they paid.

**Geier:** It's my understanding this is taking place mainly after you've kind of backed away from the Andrews group.

**Waring:** Oh, yeah. I still occasionally go up to the Andrews, usually to help somebody out like Kate White or Phil Sollins. But, if we're going to do the work that we're now funded to do for NASA, we need fairly uniform topography for these eddy flux things and for planes to fly.

**Geier:** Uh-huh.

**Waring:** Extremely dangerous for light aircraft to fly in the Andrews. We flew it once and just about killed the pilot because we hit a downdraft. We got bigger planes and better planes, but still, it's a very dangerous area. It's got these tremendous downdrafts.

**Geier:** Did he actually crash?

**Waring:** No. No. But he barely pulled out, because he didn't have much power left to pull out. He needed a bigger plane. So, I wouldn't have anything more to do with any flights over the Andrews in any plane that I was responsible for. With the eddy-flux stuff, we need about a mile of fairly uniform topography where you can fly safely over it. We work with NOAA, which now has our light aircraft and also some bigger ones of their own. And they actually monitor the CO<sub>2</sub> and water vapor flux from each area they're above. That's really difficult to do in the Andrews. You can't have big slopes like this and that. Also, we were interested in the big differences. We were interested in ponderosa pine forest and juniper forest, and the spruce forest at Cascade Head. The Andrews is a spot. A great spot. But, it's only a spot.

**Geier:** Sure. You need a bigger scale.

**Waring:** I need a bigger scale, right.

**Geier:** I wanted to track down a little bit more clearly your connection with NASA. What were you doing in those committees where you --

**Waring:** -- They called up and asked if I would be willing to advise them on the potential design of a new satellite that would have very, very fine resolution for spectrometry and be able to do pigment analysis from space. I said, "I don't know anything about satellites, but everybody has a textbook on pigments. So, yeah, I can come. I'll learn something." I'd already been reading about NASA, otherwise they wouldn't have known my phone number. And I'd been to workshops Dan Botkin had held where they were looking at ideas about what's the minimum amount of life and structure you would have to have to have a self-functioning ecosystem in space. Since all ecosystems are open -- they're not closed systems -- I don't know what the answer is. But, maybe you could recycle the water and everything and use the sun's energy still to drive it from space. That might still do it. We'd had lots of contacts from these workshops that NASA would invite us to be part of. That got me maybe deeper than I wanted to get. I could see the way for my graduate students to write grants to get funding to test the underlying assumptions for these models. Barbara Yoder [later Bond] was one of my first students to really test this. At the same time we had this big NASA project over Oregon. I think I had three of my graduate students involved in that -- two Ph.Ds. and one masters. All these other people were coming in and they had a full-time pilot, but they only had two years. We would allow you to come in, but we weren't responsible for funding, so it was opening a wider door.

**Geier:** Nice.

**Waring:** Thousands and thousands of images and data and met station data.

**Geier:** This is all going back to that first transect? The Oregon Transect?

**Waring:** Yeah, the Oregon Transect. It isn't that I've abandoned Oregon. I like beginnings and ends of projects, and I like to see them scale. I like to have the underlying things checked out. The Andrews is more of an ongoing kind of thing. It's not a beginning or an end. At least, we hope not. Most of the people now involved are fully involved doing what, what they already planned to do. You just overload the system if you actually went to the Andrews every time. Got a hundred graduate students up there now or something like that. And now I'm going to bring in the NASA aircraft. It wasn't even considered. I didn't even think about going to the Andrews.

**Geier:** It's interesting, you've been talking quite a bit about how you had a fair number of graduate students over the years.

**Waring:** I've had 21 in 33 years, I know where they all are and we still publish, or they publish with one another. We're very close. It's family. That's where I do my closest learning, and I'm very much interested in what they're doing, and I'm very interested in their success, their careers, philosophies of science, and networking. That's where I spend most of my time.

**Geier:** You've mentioned other professors and other scientists, but your collaborative work tends to be often with graduate students, it sounds like.

**Waring:** Well, originally it was. But now I'm using post-docs, because there're no jobs for graduate students. I've collaborated with other fine scientists and I've also published fairly recently with probably 3/4 of my former Ph.D. students, which is sort of like coming around again. And I've always had foreign students very much in mind. We've sponsored visiting scientists, three from Costa Rica, and one from Austria, and then, 22 years later, his son came back and lived in our house as an undergraduate student [OSU]. I sponsored one and an offspring, I guess. We've had visiting scientists from Sweden, from Norway, from Germany, from Scotland, from New Zealand, too. And I work with these people. We publish with these people when we go there. So, my world is very close as far as people are concerned.

**Geier:** Far flung.

**Waring:** They're far flung, right. And the graduate students help, because there's one in Quebec and one in Florida and one in Idaho. Barbara Yoder and Mike Ryan, they're working in Panama. It doesn't matter where you are anymore. The big change in science is if you can write the grant together and cleanly get it, you can go anywhere in the world for two or three years and get the work done. Even when you were starting that wasn't true. But it's true now.

**Geier:** You mentioned the recent shift to post-docs and the lack of jobs. What you mean by that? You don't see job prospects for graduate students so you don't take them on as often or what?

**Waring:** I'm not taking any right now, but I'm getting up in years too. I've got three of my last graduates that have been working on half-million to million-dollar grants that they're PI or co-PI on. They're still post-docs. Well, they're more than post-docs, because in some cases, they're the PI on the grant. But they're still in an unstable position. Until they get into a more stable position, like a five-year appointment, something like that, I shouldn't be training more graduate students. I'm flooding the market with good people, and these are good people.

These people can write, they can think, and work with other people, and they've had to take physical chemistry and have to speak a foreign language. These are not normal requirements, and they have to work with me for three or four years (Chuck). That's quite a cross to bear for both of us, if they're not successful. Nobody's requiring me not to take graduate students, but that's my rationale for it. Also, I could have retired some years ago. I'm really looking for talent anywhere I can find it. I'll see if I can help them. That's very rewarding, and I'm at a position where I can do that.

**Geier:** What you're saying is that teaching has been a pretty high priority for you all along?

**Waring:** Oh yeah. Very much so. Not that I'm a great teacher, okay? But one-on-one, I really enjoy, and small graduate classes. Undergraduates, they just don't know the background well enough and I'm always trying to push them, and some of the class can be pushed, but the rest aren't ready to be pushed. Unless I'm fully in charge of that and I can work with them, one-on-one through email, I can't really solve the problems they're facing. You're a teacher too; this is really hard, it's really frustrating.

**Geier:** Yeah. And the pressure is always for larger classes, and less time one-on-one.

**Waring:** Right.

**Geier:** There was an effort to formalize an agreement between OSU and the Forest Service concerning managing the Andrews in '76, I think it started, and continued through '78. What were the concerns and reasons for trying to update the agreement at that point?

**Waring:** I think the big concern was how much investment NSF at that time was going to continue to make in the Andrews and the university [OSU]. At that time the vice president of research, who I think was John Byrne, who would become president, made a commitment following an NSF review to pick up all of Art McKee's salary. Now this may have come back to the College of Forestry, but he made that formal agreement when this review panel was here from the NSF. If we continue to support this from the Andrews, what's the university going to do? Okay? And then in turn, and what's the Forest Service going to do? Now we're talking about building with federal money permanent buildings on the Andrews and plumbing them. What kind of commitment do we have that there's going to be control on the kind of the facilities and operations. We've got to have an agreement here. We can't go into building buildings and expecting great things and then neither the university nor the Forest Service show us that they have a long-term commitment too. So that was the driving thing. It was pretty obvious.

**Geier:** That was about the time that the Long-Term Ecological Research grants were beginning?

**Waring:** Yeah, you'd have to check with Art on that. You can see that the only reason I remember dates is because I write them down. I'm sorry. Dates are important, but I might get them out of sequence. But that was really important and that was the first time Art had full support independent of the NSF grants.

**Geier:** Oh, is that right?



**Waring:** It came, I think, from that university agreement with NSF, that at the same time or shortly thereafter, pushed the Forest Service to have to sign on the line that we would have control that would take the forest out of the allowable cut.

**Geier:** Oh.

**Waring:** Otherwise, the allowable cuts they are always driving, you have to take something out. Well, let's put the pressure off and physically take them out of the allowable cuts.

**Geier:** Did you manage to get that through?

**Waring:** Yeah.

**Geier:** And was Bob Tarrant a signer on that?

**Waring:** Yeah, Bob Tarrant. (Chuckle)

**Geier:** That was just before he retired, wasn't it?

**Waring:** Oh yeah, all these people were doing great things for us just before they retired. Bob Tarrant was the mentor for Jerry Franklin and me. I knew people in the Forest Service, and he and Jim Trappe were the best writers and best thinkers of broad ideas. Jerry Franklin and I were young scientists at that time and we were looking at these two guys as really being exemplary, in different ways. But exemplary types of people that, maybe they weren't our direct mentors. I had other mentors, but they really were important. They're both good writers and both of them have edited, particularly Tarrant, some of my stuff when I was trying to reach a more general audience and not get too political. He would help me on that. So, he was really important.

**Geier:** That was while he was director? [PNW Station]

**Waring:** No, that was before he was director.

**Geier:** I wouldn't think he'd have time.

**Waring:** Oh no. We'd still talk, but he had all kinds of problems in trying to move people off the payroll and all kinds of really difficult things.

**Geier:** There were a lot of changes here at that time, '78 or somewhere in there, '78 to '80, as you moved into the FSL. [Forest Sciences Laboratory/USFS-Corvallis]

**Waring:** Well, sometimes I had split things where I would do one out of one building and have an office here and there. But most people moved. I had an office over there, too. As I've moved each time, the office has gotten smaller. At one time, I had a huge lab with all my reprints and everything around it, and now I'm down to the smallest office I think I've ever had.

**Geier:** Was there a reason why you moved over here instead of staying in the FSL?

**Waring:** There's a lab at the end of the hall, and another office. Those were the only ones available to put physiological equipment and not have direct access to it on weekends and evenings when the Forest Service is shut down and the power is off. The Forest Service had a lot of constraints. They used to turn the power off and the Xerox machine when you're working on grants on the weekends. This will bring you to your knees right away. It's their policy, but

we were supposed to be in these joint buildings. So we had choices; maybe moving, maybe not moving. But there was an office, this place where the graduate students worked away, there was room to put the equipment to be shared, and I could have somebody in there that I knew would take care of the instruments. So, rather than having them trashed or lost, it was an advantage here. I teach with Kermit Cromack, he's over there. Probably most of my associates now are in Atmospheric Science and Soils across campus anyway, and Kate Lajtha, she's in Botany. I've always been campus-wide. This department has actually been taken out of Forest Management and made into Forest Science. In the last ten years or so it's gotten more into management and co-ops and things that are not science. They're not advancing the field of science, they're applying the things from science. So, it's sort of a hybrid, anyway. And I could work either place, but my loyalty is to the campus.

**Geier:** Do you have a pretty strong connection to the geosciences?

**Waring:** Oh, yeah, oceanographers I know. We've had joint seminars with them with NASA money. We've written grants to bring satellite downlinks here with Atmospheric Science. It's a whole university, and with Forest Science, particularly the part that has to go correctly into extension and co-ops, that, almost by definition, is not advancing the field of science or scaling. It's trying to meet the client's needs, the local part of Oregon, almost one-on-one. Nothing wrong with that. It's not good, necessary for either one to do it that way. Once you do it that way, you've pretty well made a split. And you and I don't think it's an ideal split, at all. But when somebody makes that thing, the system says, well, you're co-op full time, and you're extension full time, and maybe you're going to do research and have graduate students. That's not the same kind of research to find how to get this species of this seedling or genus type to survive in this soil and under this management scheme, than it is to say what principles can we go around the world on? It's the difference between researching the best buy on something, or the best option you have for an investment, or the best refrigerator. What principle could we find to cool food that didn't require putting it in a box? It's a completely different kind of question. The tools you use, the analytic techniques you use, the audience you use, and where you get your money from, they don't overlap, initially. Later, if they work, everything comes back together.

**Geier:** You moved over here then early in the early '90s, is that right?

**Waring:** Yeah, I must have just come back from a sabbatical at NASA headquarters and they had all my stuff out in the hall, with about half of it not going to fit. And somebody from the Forest Service had moved into my other office, and I didn't quite swear never to go back to the other building again, but it wasn't the greatest thing.

**Geier:** Yeah, I'm starting with only three months here since I got on this grant.

**Waring:** '92, '93 so it must have been, must have been '92 or something, yeah. Pretty recent.

**Geier:** Yeah, they changed the locks and the name on my door already. (Chuckles)

**Waring:** Ooooh? That'll tell you something.

**Geier:** I was interested to hear a phrase that Jerry Franklin used when I was talking to him. He said that you opted out of the Andrews in 1978-79. Is that a fair characterization of your history?

**Waring:** Yeah, I would say, but there's some reasons for the opting out. One is we thought we'd picked good people to carry on. Two, my dad said once you delegate, don't go back and try to tell them how to do work the way you would do it. I would have one-hour meetings, for example, with an agenda for one-hour meetings. That doesn't mean you're wrong, that just means that's better than the way I would do it, and for lots of reasons I wouldn't do it any other way. Well, partly, I've got other things to do.

**Geier:** Uh-huh. Delegating.

**Waring:** Oh yeah, we had people we thought were ready for it, and if we didn't give it to them then, you know, like the "King of England." You wait too long and then they're not ready, and then they're "yes" people. They were ready to do it.

**Geier:** Who're you talking about?

**Waring:** Fred. Yeah, Fred [Swanson] was the major one. We had hope for some other ones too, but Fred's taken the major responsibility. Susan Stafford has taken quite a bit, and certainly made a success. Without her there wouldn't be a quantitative science center. Okay?

**Geier:** Yeah, right.

**Waring:** She's a great teacher, she's given her life to service there, and hopefully, she is recognized now for her outstanding service. She was also working for a vice president at one time, and she also was in Washington, D.C. So she got some recognition, but service jobs are sort of like, everybody wants more.

**Geier:** That's through NSF.

**Waring:** Yeah, right.

**Geier:** How would you characterize your involvement with the Andrews group since you have distanced yourself from it?

**Waring:** I think wherever I can really help them, we've come back together. And I think the biggest example, and Fred wrote me a note thanking me so I remembered what this was. When I was at NASA headquarters, I helped to arrange a workshop where NASA and LTER people got together and began to share ideas of collaboration and money shifted mostly from NASA, into LTER. I've gone on annual field trips occasionally to see what they're doing. But it seems like a lot of it is very long range, waiting for the storm years out so that we finally got to test the ideas. My research career is built on three years of measurements and two years of analysis, and I've run out of time. I've gotten an awful lot done this way, and felt really good about the LTER going on, but it would be different if I were in it, because it would be much more functional models and much more experimentation. But the Andrews is a very hard place to do the experimental models. Some of them, I mean, you can change some things, but you can't change all of them. And finally, because it's very, very undulating land where you can't have lots of replications, and [there's a challenge with] the permit system. Because it is so

sacred, the land now, what to do with it? It'll take you two years to get all the agreements and all the permits. Well, we can go off onto other Forest Service land. We can get agreements in six months or something and we're thinking of doing that in some of the other areas. It's just sometimes easier to go ahead and do the experiments off the Andrews, and not get in the way of other people. Especially, if they have to be big experiments, if you have to dump fertilizer from the air. You want to make sure you dump it into something uniform, not into the stream, and so forth.

**Geier:** So you're --

**Waring:** No animosities at all. I see a lot of the people at the national meetings, the ESA meetings. But, I don't go to most of the seminars.

**Geier:** You're involved then with the decisions about how to manage the forest?

**Waring:** Oh, none. No, when I left (chuckle) the site director position, I knew I was not going to enter into any more decisions on how it's managed and the making of the decisions. That's a different position. My father warned me about those kind of situations -- don't go back and help make the decisions. You were making the decisions, don't step down and then...

**Geier:** Oh, if you were going to look at the scope of your first involvement there to the present, what would you characterize as being the period of closest integration?

**Waring:** When I was site director.

**Geier:** That's you, but I mean --

**Waring:** Oh, closest integration?

**Geier:** Forest Science people and the management out there when they were working?

**Waring:** Oh, I think they've still done some very close coordination. Mark Harmon and Phil Sollins and Kermit Cromack, these are people I know intimately. They're all Forest Science [Department, OSU], and yet they're working with people like Tom Spies on the modeling. That's not all on the Andrews. Dave Perry had a project up there. He's retired now, he was up there. I don't know how many graduate students. And they take my ecosystem course. I think we have a hundred graduate students now, so almost by definition, if a third of those work on the Andrews, then this is our highest integration. But on the day-to-day meetings when we were actually moving ideas and people around, we were doing an awful lot of that with the post-docs back in the IBP, then also, shortly after the LTER started. Then, we still had full-time post-docs. They weren't off teaching and doing other things. Post-docs do an awful lot of integration. I think if you've got sixty hours a week out of them on one project; that's a lot. The graduate students work with them, and hopefully, that's still happening. I know some professors have 10 or so graduate students. I've had 21 in 33 years, so you can imagine if 4 or 5 of those can't do the same kind of integration, when you as the professor have 10 graduate students or 50 people in the classes, four classes, as you can when you have no more than 5 graduate students at one time, usually only three, and you're working with three post-docs. You can't compare those things.

**Geier:** Tell me something about post-doc workers who might be more interested in talking.

**Waring:** Because we hired them with another background than what we're actually asking them to do. And we're asking them to do integration, to cross fields. We would do that on the grounds that there is opportunity in cross-field work there wouldn't be in their own field. Because we're asking them to make a career adjustment, not a shift, an adjustment. You don't do that lightly. My students now are going more with atmospheric science, remote sensing, and stable isotopes people. Because there's tremendous opportunities in those fields compared to classical forest ecology, classical tree physiology, which are basically very, very limited.

**Geier:** You're talking about employment opportunities now?

**Waring:** Employment opportunities, and also intellectual opportunities. Because this integration is still yet to be done. And most of the things that biologists do in science, with a few lovely exceptions, they do through integration. We don't do the best physics, we don't do the best math, we don't do the best computer programming. Now what we do, is we see opportunities to use those tools in a theoretical framework that maybe other people have talked about, but not actually pulled together. And partly because of the original background as naturalists, our heritage, that caused us to write in prose that people might still feel akin to. If we take that prose and we begin to explain how those isotopes move around and use some analogy, like remote sensing eyes in the sky, and what they can see through and this kind of thing. Well then the people that are interested in this and its importance, they may understand it much more from that perspective than if an engineer wrote or a detailed super competent scientist in that kind of field. Ecologists, in particular, have this challenge to integrate. But to do it, they have to have enough math, physics, chemistry and English, or you fill in the foreign language to do it. That's the training that we try to give at least some of our students because that's where the opportunity is. And then you don't have to say, well, it's stable isotopes and it's remote sensing, it's in the integration and not being afraid to talk to the people that have this fundamental background in physics, math, chemistry. A lot of ecologists don't have that background. In fact, I'm afraid that most ecologists don't have physical chemistry. And it puts them at a tremendous disadvantage.

**Geier:** Difficulty communicating with these specialists.

**Waring:** Yeah, and to even work with the specialists because they're having to dumb down the principal ideas and the original references to talk to you as a colleague. Well, we all expect some of that, I mean, thank God. But we at least should say, oh, we need to be able to communicate a little bit other than just superficially. That takes a lot of learning, continuous learning. If we can train for that, then we have a really good generation of scientists. I think my students could all do this much better than I can, but I know the reason why it's important.

**Geier:** Why do you think the students, your students, are better at this?

**Waring:** Well, I've had students that had lots and lots of forestry background, and they come here with statistics and correlation matrixes and these things. After I sit down and ask them, "What am I doing wrong? Because you're talking a completely different kind of science than I've ever been trained in the forestry school." They might say, "Oh, you're right." So we'll sit down, and for three months, we'll do philosophy of science. I'm not a philosopher, but I have

some of the books. I know the standards of it, and we can read some of these things and see how people from different religions work in different fields at different levels, and how ideas come together when they're right, but not before. You can see what's an important idea and one that's testable now. And does it make any difference? It makes a difference if the international audience at this time thinks it's different. Does it make any differences to people in forestry in Oregon? Maybe, but probably not. The big problems in insect outbreaks and disease outbreaks are that we don't know enough before they happen to solve them. So, you pour all kinds of money on them. All right then, you don't have a serious test. You're sort of discovering who killed what, but you don't have a fundamental theory. Do the theory first, try to find these general principles, and then see whether or not they work everywhere. You actually give them examples, and in your research you try to do this to your limited ability. Well, they're starting out fresh, coming in often without a forestry background. This makes them scientists, from the start. And I never had that advantage. But I wouldn't give up my forestry background, because I see a lot of the opportunities for application. I had a very broad training at Minnesota in hydrology, wildlife, meteorology and all these things. I never thought I'd use any of it. Turned out I used all of it.

**Geier:** So, when you're looking for a graduate student - you're not looking for them now, but when you were - you were looking outside of forestry?

**Waring:** Definitely, almost entirely, and almost all outside of Oregon.

**Geier:** Outside Oregon, the region, the Northwest region?

**Waring:** I'd take them from the University of Washington or from California. It wasn't anything malicious. I have had graduate students from Oregon. The qualities that seem to do best are the intellectual rigor of the Northeast, the ethics and the working habits of the Midwest, and sort of the laid-back kind of a thing like it's not a crisis, we'll get it done, of the Far West. If you drop any one of those out, you know, it's not fun to work with them. Does that make sense to you?

**Geier:** Three different institutions probably come here, it sounds like.

**Waring:** I've had people sent to me that have worked at different institutions. Most of the time once you get up in your career, the graduate students that are applying for your Ph. D., they know your literature, they know your reputation and other people. What you're trying to find out is are they thinking already? Are they open to thought, or are they technically an expert? Because there are great technical experts around. I can't help them much, make them a better programmer, make them a better remote sensor. All I can is say, this too may be important here. What I want to test is, what do you think is going to happen and why? What's the basis for that? What do you think we measure? What will we find? You may be wrong, but I want to see that kind of thought process. And how do we do it? Where do we do it? Those are details.

**Geier:** How would you characterize the work that's been done and being done at the Andrews by the Andrews group now?

**Waring:** I think it falls within a philosophical framework of science with the ideas of trying to scale it, and make it applicable elsewhere. But I would think an awful lot of it is still empirical.

And lots and lots of measurements, and lots and lots of comparative measurements, comparative physiology isn't bad either, and comparative genetics, and comparative that. But, we get into trouble in real important fields like biodiversity. We can describe it, we can show where it's rich and then we can speculate on why it is. But we need testable hypotheses that can predict before we make the measurements what the diversity's going to be and where it's going to change. And then we'll begin to feel a little more solid about using the GIS maps to decide where we're going to put a preserve that may or may not change with the climate. That doesn't mean that biodiversity isn't important. Good grief, it's one of the keynote things in the ecological society. But it's one of those things that's not easy to approach in clean scientific ways yet. I'd like to see more theory and less empirical measurements, or the theory coming before so rather than say, let's say variants which you're familiar with, we're going to lay out a treatment, or we're going to look at things and we're going to see whether there are differences among treatments, or you fill in the blanks. Believe me, if we go far enough, I'll guarantee they'll be differences among treatments. This is not advancing science. Science asks, why will there be differences and how will the response surface change over time, and over space and environment? Now we're seeing what would happen over any environment, over any time, over any space. That's a different perspective. It's a different way of approaching science.

Initially, forestry had to do the empirical work; they haven't had a choice. The trees are growing, the trees are dying, they're trying to understand why. Without understanding photosynthesis and growth, other than the names, they had to go do it. I think we're reaching the stage in many of these fields where the theory and the methods, if we worked on it, would come to the fore. And I can't think of a better group that hasn't had all this empirical experience to try to step back and begin to do that. But, you're committed every hour of the day every week to do and perform empirical measurements, especially since you have storms now, because now you expect that things are going to change. We're going to quantify how those things change. But, do we have a theory of where we should have expected the change, and how much and what rate they will come back? Well, to some degree we do. We have succession theory and we have disturbance theory. But, those work at very broad levels. They tell you alder will come in along the stream, but they don't tell you how many alder. When we make the measurements really detailed, we actually get backed up on how many alders there are. Where the question is sometimes best tested looking back and saying, "Well, it's hardwoods that grow fast versus slowly developing evergreens that grow slowly, and this is the reason why." It's a different philosophy that still requires measurements, and I'm not against measurements. I certainly don't see that, even in the present hydrology program, which I've sort of put my nose in because students have been taking my class. Lots of good statistical correlations which locally should show something, and maybe regionally. But, I don't know what the reasons are for those statistical correlations.

**Geier:** The initiative that was applied.

**Waring:** Well, that's fine. I think the strength of it is right around the Andrews, and that should work. I'd always like to have more second-growth and more "herbicide" areas and things that are not being done on the Andrews. There is no "herbicide" on federal lands. You're trying to advise what will happen without having that in your context, so you need to broaden it to

contain the full range of options that are now being used, whether or not you agree with them. Okay? I mean, that'd be one thing. The other thing is, if you said that you needed huge amounts of woody debris, based on the Andrews, and you went up to Alaska and found huge amounts of woody debris actually lead to degradation of the site, and you read in California, it leads to fire hazards. Well, if you only had the Andrews to make this judgment on, you'd include one thing and you would exclude the other two options, which means let's get rid of the debris up here and let's let the fires burn this up here, because otherwise we got a fire hazard from hell.

**End of Side B, Tape 1 (of 2)**

**Start of Side A, Tape 2 (of 2)**

**Waring:** If we can't do that, it's not one of the options. The only other option is to base it on scientific principles, that you predict where you are going to find the owl, where you are going to predict where you are going to find these things, and what will happen if you change that. You have to change your philosophy from first principles to a modeling philosophy, and account for large changes in it before you go there and after you change things. That's better than observing and then telling, simply reporting, what you observed. I liked the observing, but you need the theory at the front end, and that takes a lot a work to develop a theory. You have to read very widely and very conceptually, and you have to think and you have to figure out where to try it, and sometimes you can't do it. Sometimes you're a decade away, just because the message like that can't be heard yet. Sometimes, you're not the person to do the theory. In physics, they don't ask the people that do the measurements of neutrons and these things, to theorize-- they just have to take measurements. In biology, we are supposed to be theorists, measuring people, and administrators. And guess what sometimes suffers? Usually the theory.

**Geier:** You say the LTER network is addressing some of those concerns?

**Waring:** Not by themselves, because it will take the right people. We knew this when we had the IBP, that we had to look to the theories method often outside our immediate field, or with people who that had lots of experience and who are no longer overly involved in measurements. It takes time to think. When you see the Andrews people writing text books, then, they have time to think. But, it takes six-and-a-half years of your life to write a text book, on sabbatical, 1100 references, you know each one. Well, it's great! It's the greatest learning experience you can do. But, it's something you can't do if overly involved with a huge number of graduate students and huge numbers of administrative needs and huge numbers of measurements. Because, how are you going to do it? You are already working 60 hours a week on this text book.

**Geier:** What you are saying it's a case of grant writing, the collecting of data, or teaching?

**Waring:** Or teaching, yeah.

**Geier:** What professor?

**Waring:** Trying to take breaks to think and revitalize yourself, and pause and see what you have after five years; that is integrative. Remember that is the only thing we are ever going to do that makes a difference. It's not going to be our new methods or, what's the German quote for



science's advance by new methods, new ideas and funerals? Okay, funerals help a lot, but you could go back a hundred years. What I am telling you is nothing more than scientific philosophy sort of updated with really successful integration. What makes it different? Good ideas that are testable. Do they have to be right? No, they have to be interesting and important. Lots and lots of problems in the world are not yet clearly testable. Doesn't mean that they are not real problems. Doesn't mean we shouldn't be spending some money on it, but, if you're asking me, is that a really good investment for our best scientific talent in universities on problems that we are not quite sure what the theoretical basis is? And I'm saying, some is there, but we need the theory. And we need general models, and we need testable models in places where we haven't made the measurements yet, so we're not calibrating the models. We are testing our understanding of these things. Further away the better.

**Geier:** What you are saying, is that you haven't seen much of that taking place at the Andrews or following the Andrews group?

**Waring:** No, I wouldn't expect they could. Because of the terrible commitment in time and effort just to keep the Andrews moving. Now, a lot of them are interbedded with it so they can't really break away and get independent funding. You have to write a clean hypothesis, and you have to show the new methods that can be developed from that. Some of them can do comparison at other sites. But that compares morphology. There's nothing wrong with that, but it may not break the theory streak.

**Geier:** Can you give an example of people doing this kind of research you're talking about?

**Waring:** The guy that just got the award from the University of Minnesota. Little bitty seals, giraffes, and fertilizing, and doing those things. Doesn't mean he is right, but he's got some ideas and he's trying to test these things, and look at how many species you need before you are redundant. And what do a large number of species do in terms of when you have a drought. It's sort of a test. And, not everything has to be experimental. The other thing you begin to predict, let's say you go to tropical forest and it is a very infertile tropic forest with just nitrogen fixers and very low growth, and in those cases you say I would only expect very intelligent primates to be present. Now why would you say that? Because theory says that intelligent primates are the only ones to find fruits which is all that is edible and then call their buddies to find out, "Hey, the fruits over here." If it is a very fertile place, then you have lots of leaves on a half dozen trees that are edible. You don't need a big brain for that. Therefore, your primates in most cases and maybe more primates than in a richer place, those shouldn't have to be as bright. It doesn't have to be right, okay? It comes from the literature, but what an example.

It is examples like those that you give to your students with their own problem, they have a problem and you make them think, or you help them think. Well, give me a theory, what do you think is the key variable, what, were, how does it change? How would you test it? Let's go to the literature. That takes time. It takes a minimum of six months for a graduate student, usually a full year in this office, and talking and learning the field before you both feel comfortable that we can really test this. Sometimes, you have already done the work and it's a master's student. You have a journeyman opportunity; you are going to use these tools, this is going to come out of it, it will be interesting, you will have mastered these tools. That is a

master's degree. That's the certification that you can do this, write it up, meet the deadline and we will give reasonable thanks. It is different than a Ph.D. Or, of course, that is different than my idea of a Ph.D. And I don't want you to run blind for five years like the British system does and say, well, if you do well - great, if not, tough. (Chuckle) Because we will lose some people from the Midwest that are still struggling with what is science. They are working hard and they are dedicated, but we forgot to introduce them to science. I do have a philosophy anyway, it is not a random drift is it.

**Geier:** Where were you from in Minnesota, by the way?

**Waring:** Ah, I worked out of Ely, and my wife is from Crookston.

**Geier:** I was born in Crookston.

**Waring:** So, you understand my Midwest. (Laughter)

**Geier:** Can you think of an institution that might embody some of the ideals that you have been talking about here?

**Waring:** Hubbard Brook, and The Ecosystem Center at Woods Hole [MA], particularly when I was there in '80, whenever it was. They had Jim Rollo and John Hobbie, so they had an oceanographer and a terrestrial ecologist that are administrators, trying to worry about money and people and contacts with Washington to make sure the opportunities are greased with good grants. They're also private institutions. And in this little bitty building, they will have a bunch of people that are all in soft money and they are all writing grants, and the secretaries, they're not called secretaries, but the administrative assistants are all involved. When a grant gets funded, everybody celebrates. Okay, they have post-docs in from universities, and they have graduate students working, and when they are writing stuff the senior scientists are helping the young scientists learn how to phrase it and write these grants. They focus on the hypothesis, which is a biological approach. Other people promise to your product. You know how to put the references in, and then just cheer when they get it funded. Well, they are all dependent on this soft money and they are all dependent on the next generation. And people peeling out of there have all worked in these collaborative relationships with other universities and other students, so they have a big network. They are doing teaching, and they have seminars every Friday, and they sometimes have the janitor come, because he found it very interesting. He would come and they would play music. It was sort of a weird group in an old wooden building. The library was 100 feet away, open 24 hours a day, every day of the year, including Christmas, and books, journals were all lined as any biologist could figure it out, alphabetically, from the bottom floor to the top.

**Geier:** Hmm.

**Waring:** In summer, the whole thing fills up with all the scientists from the Ivy Leagues, wanting to come there and establish themselves in the first place. That is when all of these other people go to do their field work. So, they have it the nine months during the quiet times, so they can do lab work. That's my example right now. And they have a nice LTER thing at Harvard Forest that they are participating in. We work there, too. It's this little building in this unit of people, and watching them grow and spin off and go elsewhere and do well, and keep

the contacts and bring in the new people; it is really exciting to see. I think a good scientific philosophy is there, good methods are there, and the *esprit-de-corps* or whatever you want to call it, is still there because it isn't too big and too loose. You see this really, really fine thing. If you get a chance, you should go sometime.

**Geier:** Yeah, if I do this much longer, I probably will. I was going to ask for you to characterize that system more as a mentoring process or more of a cooperative process?

**Waring:** It's mentoring. I had mentors. You can't be a mentor unless somebody has done it to you. How powerful it is early in your career, and how much difference it makes in how you think and who you work with, and you have all kinds of friends. The people you actually sign on to do this thing jointly with, you have to trust them. You don't have to take them home for dinner, but you have to trust them. They are going to keep their word and this is going to be fruitful, and preferably really exciting. And when that happens, you will never forget the interrelationship and neither will they. That doesn't always happen. Sometimes you have to work with that, but if you have a choice, that is what you work for.

**Geier:** You mentioned at one point, your mentor at Berkeley, but I don't think I got his name.

**Waring:** It's Ed Stone

**Geier:** Ed Stone

**Waring:** Yes Berkeley. And, Egolfs Macusis

**Geier:** How do you spell it?

**Waring:** E-g-o-l-f-s, Egolfs, Egolfs.

**Geier:** I want to make sure I have it spelled right.

**Waring:** He is Latvian. Pretty sure that is right. He was a scholar who spoke three languages. I had another guy, Jack Major, who I had originally came to work with me at Davis [UC]. He was another scholar that spoke three languages. So, I always felt like I was sort of an absolutely inadequate scholar and absolutely inadequate at languages, but I could see the value of both. I had a chance to go overseas, and I actually did speak and teach in German once. I love the library. I find things in the library that I can integrate and think about in a way that I can't just by traveling. I like to travel too. Stone ran a very large integrative project with his graduate students where they had all kinds of controlled environment chambers and all kinds of equipment, and I worked with those graduate students. I helped them and they helped me. He supported me because I was his TA in ecology. I was not getting my Ph.D. from him, he was just on my committee. But, he still supported me. He's 80ish.

**Geier:** Still alive, huh?

**Waring:** Yes, he is still alive, well, both of them are alive [Stone and Major], and I dedicated my part of the second book to them. There's other people, too. University of Washington, Richard Walker; he is a mentor. I helped develop with Brian Clearly, a model of a pressure chamber that came out to be very useful for taking the pulse of a tree and find out how much water stress it is under. Well, he asked us to build one and then he introduced me to a whole bunch of his German colleagues, because he really does speak German. And it ended up that I worked

with two of them on my first sabbatical. So, that introduced me into 14 countries in Europe, and German things and the only article I ever published in German. So, there is Richard Walker and he is very active in the IBP. He was one of the big professors up there that I worked with.

**Geier:** At UW?

**Waring:** At UW, yeah. It really makes a difference. Us Midwest boys wouldn't do well if we hadn't run into these mentors.

**Geier:** I was just curious, the difference between this university [OSU] and University of Washington. Something about teaching, graduate degrees? Do you have any regrets that you came here?

**Waring:** My regrets are more that we have lost some key faculty and we never replaced them until very recently. One was in atmospheric science, we used to have a long time ago some very strong atmospheric science people right in the department here. One got a job and the other guy didn't get tenure, and for about 8 years there was nobody here to work with. Those at the University of Washington were harder to work with, but they had two of them. So, I was thinking at one time about a position up there in botany, because they had the talent and lots of money. What they didn't have there, and Jerry Franklin discovered right way, is a lot of tolerance for people that have completely different viewpoints, which we have here. Jerry and I used to argue a lot about his theories and my theories. But, there is a certain tolerance where my students aren't caught up in having to make a decision on whether to take that class or not take that class. They have always had a problem at University of Washington, particularly the College of Forestry, with in-fighting. I think it was the original dean that I knew. He encouraged this. And it was like, God, you should never encourage the faculty to in-fight. I mean, they are predisposed to do that for space and for students and for philosophy. Oh, you want to encourage it (chuckle) for money. Then Dale Cole became an associate dean, and Dale has a lots of problems in dealing with people. It just perpetuated until he retired this year. I mean, they still have a different dean, and that. I go up there and I love to talk to (Tom) Hinkley and Franklin and (Chad) Oliver, but it isn't equal.

We have our problems, too, particularly my philosophy that I would like to have more of a science department and move the management and extension over to forest resources. We originally split the departments in order to allow that to happen, but that is history and not many people want to be quoted either. (Chuckle) I just worry about the next generation of scientists in the department, because I can't worry about the next generation of extension agents. Bill Emmingham, I'm sure, is worried about it. I worry about seeing whether we can get these proven post-docs that are now around the country, including some here, into the department. This would include Mark Harmon, Barbara Yoder, I mean, they have over-proven themselves. It gets to be mental stress where you can't continue any longer unless you get up to a certain level of doing all these things without having nine months or five months of salary. They are proven. It is not a case of I wonder if they can work with our people? Gee, I wonder if they are good at writing grants? I wonder if they will graduate? They have already done it. It is what you get tenure for. And, they have already done it in spades with a couple, three million dollars, in grants.

So, I'm concerned how to get that those kinds of people into the department. Not necessarily in abundance, but we have had three or four retirements, Perry, Herman, Lavender, who've not been replaced. These are all full-time scientists. These weren't extension people. They were the guts, at least, the core group of the older generation of international scientists and travelers. There is no question that in their time they were scientists that made the Forest Science Department recognized. Then there is my generation, which includes Dave Perry, who I think I did mention. I am near retirement and [Mike] Newton's near retirement. All of this is good, if you believe in funerals. (Chuckle) But, for the next generation, there is going to be an underpinning of science in the general relation to NASA and NSF and global kinds of things. You've got to have those kind of people not advertised as an assistant professor position, when there is nobody left on the staff that does that. Phil Sollins is still here, we're down to the last one. The rest are soft money.

**Geier:** They keep on retiring and not replacing them.

**Waring:** So, you may have the same problem. They must have somebody teach four or five courses.

**Geier:** Well, we have been doing fairly well. Although at one time, we were nationally ranked in geography. One of the top ten. That was twenty years ago. And since then, it got down to three faculty and they almost didn't replace the last one.

**Waring:** It is interesting the normal life span of leadership, actual leadership, is about ten years, and of apparent leadership ideas, about 20 years.

**Geier:** I like that term of "apparent leadership."

**Waring:** Well if you ask all the deans in the country to rank these universities, they rank them from a ten-year heritage. They don't rank them for today. If you ask the graduate students that are recently out and the post-docs, they will rank them for today.

**Geier:** A lot of truth in that.

#### **End of Tape and Interview**