

Interview with the H.J. Andrews International Biological Program Group, 10 a.m., February 10, 1998 at the Siuslaw National Forest Headquarters; Interviewer – Max G. Geier; Participants: Jerry Franklin, Dick Waring, Jim Hall, Martha Brookes, Bill Denison, Fred Swanson, Ted Dyrness, Al Levno, Don Henshaw, Art McKee, and Max Geier.

The International Biological Program (IBP) was the major research program at Andrews Forest during the 1970s. IBP funding from the National Science Foundation and many associated grants made it possible for a large team of academic and Forest Service researchers to study a wide range of ecosystem topics – vegetation, fauna, soils, stream ecology, geomorphology, microclimate, landscape history, and more. Participants in this group interview represent a wide variety of disciplines and roles: writer-editor Brookes, OSU ecology professors Denison and Waring, stream ecologist Hall, Forest Service soil scientist Dyrness, U of Washington forest ecology professor Franklin, Forest Service information manager Henshaw, Forest Service field crew manager Levno, Forest Service geologist Swanson. This was a critical period of development of whole ecosystem concepts, especially of old-growth forest and stream systems.

Max Geier: I want to start out with a brief context statement about where this meeting comes from and where it stands in relation to the study we're working on. We're doing series of small group discussions that center on the primary themes of the history project as last designed, two summers ago. The first one, which some of you were at, focused on conceptualizing the landscape of the Andrews Experimental Forest, and we went to the H.J. Andrews and Carpenter Mountain Lookout. We talked about the ways in which the Andrews and its landscape were perceived through the generations of scientific research and land management that occurred at that site. Then, several different long-term research groups took a longitudinal view on particular strains of research at the Andrews. The first was the watershed group interview, and the second one was the riparian and stream group interview. This one is little different than those previous interviews in that it's more of a window-of-opportunity focus. The IBP group aims at a period with a relatively clear beginning and end, as opposed to, for example, the watershed studies group. There's a spurt of energy that takes place at the Andrews with the IBP group and program. Kind of redundant, in and around the Andrews and the Andrews group, but it's a turning point for the forest [HJA], and the science community that builds around that forest.

With that said, I'd like to start out with each of you introducing yourselves. Identify the origins of your involvement with the IBP, and then, the way in which that intersected with your work at the Andrews, because I'm not sure about that. Some of you may have been involved with the IBP before you got involved at the Andrews. What were your activities and responsibilities at that time, and what are your current activities as you see them relating to your experience in the IBP? In other words, how did that affect your subsequent career? We'll start off here with Dick.

Dick Waring: I got involved in the program [IBP] in '69. There was a meeting called at the University of Washington, and all the people from around the Northwest that were interested in participating in this program, went to that meeting. They had a work plan already drawn up, and we decided maybe, we should have more of Oregon in it.

Jerry Franklin: Understatement! (Laughter from group).

Waring: I think we actually had an election, and it turns out I got elected to represent the Oregon contingent, partly because I already knew some of the faculty; Dale Cole and Dick Walker, at the University of Washington. On the way home, I think we probably decided there were only two places to work in Oregon, either McDonald Forest or the Andrews. We decided that because it had [experimental] watersheds and additional resources coming in, that it would be much better to do it on the Andrews, so that was the end of the discussion about it being anywhere else; a simple decision. There was a problem on the Andrews; we didn't easily see how we could link the streams to a lake or reservoirs as part of the IBP lake program. I was really interested in IBP, because the work I'd done in the Siskiyou had reached an end-point, where in order to put together the measured physiological gradients, we had to go to modeling. So, I was very interested in all this talk and discussion about system modeling. If I'd known then what I'd know now. (Chuckle) I was really anxious to see how those pieces fit together, and we had workshops about how to do it, particularly at Colorado. George Van Dyne called these workshops, and so we had a lot of them. But that's a detail. When Jerry and I actually got started writing a grant, I think we must have written a component here, and then merged with them up at Seattle.

Jerry Franklin: That's a nice word – merge. (Laughter)

Waring: We used to "merge" with them on the Oregon-Washington border near the airport. [Portland International] I guess we figured it was "neutral" territory. That's another story. To conclude, and I will skip a whole bunch of years in there, the opportunity the IBP gave me as a scientist was an opportunity to see that departments don't work. I mean, they work in what departments are set up to do, but they don't work well integrating across fields. There were two reasons for that. The main reason was the people within those departments got tenure and advancement by staying within a fairly well-defined field. Until they became full professors, they weren't many of them free to take a deep breath and cross fields. When they tried to cross fields with their graduate students, the graduate students had the same constraints they originally had as young professors; the work has to be within your field. So, about halfway through, two or three years, it couldn't have been any longer than that, we began to add a whole bunch of post-docs. These people came in from other fields, including some of Dale Cole's students, and that began to be the bridge. The best product that would come out of this [IBP] is not the models, and boy, was that an understatement! (Group laughter) So, I rest my case.

Franklin: Well, I'm Jerry Franklin, and I was one of the people along with Dick, who was in on the infamous Pack Forest [UW experimental forest west of Mt. Rainier] meeting in which we

decided there was either going to be an Oregon component or there wasn't going to be a coniferous biome. And it almost came down to that, as a matter-of-fact -- (Group laughter.)

Waring: -- a couple of times, especially when you were at NSF.

Franklin: That was actually before I was at NSF.

Waring: Yeah I know, but.....

Franklin: My initial involvement came out of a concern that Ted [Dyrness] and I had to build a program at the Andrews. We really had to begin building a critical mass, and this seemed like a really outstanding opportunity and a good fit between the history of what had gone on, especially with small watersheds, and the opportunity to begin to bring a new set of people in. At the time, it was kind of exciting. We'd been back to Colorado State and were proselytized by George Van Dyne and the grassland biome people, and we'd beat up on the deciduous forest biome people. But the truth is, I didn't know what the hell they meant by structure and function of ecosystems. I didn't know squat about ecosystem science, but it sounded neat. In any case, it was a chance to begin getting an expansion of the program going at the Andrews. And it certainly, as far as I can see, completely changed the history of the Andrews. It was truly the predecessor of all that went afterwards, because everything that came afterwards was clearly derivative of what happened in IBP. It changed my life completely, because I went in a totally different direction in my own science after that. And it led directly into our old-growth characterization and our involvement with policy analysis. So, that was how I got into it, which, as far as I could see, had pretty profound consequences for the property [Andrews Forest], the program, and me personally. Are those the topics you wanted covered?

Geier: Yeah, that's good.

Fred Swanson: I'm Fred Swanson, and I was one of the post-docs that came in the early '70's. I arrived in '72, and completed my Ph.D. at University of Oregon. I was given the opportunity to edge my way in. I started in geology, doing geologic mapping of the Andrews, and progressively got more involved, and didn't actually move from Eugene to Corvallis until '75. From my point-of-view, the IBP was great because there were enough resources that somebody who was peripheral in a disciplinary sense, and maybe in other senses, was given an opportunity to work their way in. So, I worked my way in through an integrative topic of geomorphic processes and ecology, including woody debris in streams, gradually getting more relevant to the ecological work that was taking place. That link came through working with Ted [Dyrness] on landslides, and Jim Sedell on the link with streams. It took me off to Utah to talk about erosion modeling, trying to fit into the systems modeling context that was part of IBP. IBP was a really neat opportunity for me personally, but also programmatically, because it allowed the Andrews to do some things that Dick alluded to; that departments were doing departmental type things. I think IBP gave the Andrews unity, which is the theme of the book Max is writing, and the opportunity for a great deal of self-expression across institutions and disciplines, for people that got involved. The managers got involved in subsequent decades, IBP really kicked that off,

and I think that's been a real interesting hallmark of the program in the decades since IBP. You guys, Dick and Jerry, you've been great role models for doing that; the sort of "Just do it!" approach to science, and communicating with managers and policy makers, which bore lots of fruit after the '70's.

Jim Hall: I'm Jim Hall. I was also involved in the meeting at Pack Forest in 1969.

Jerry Franklin and Dick Waring (talking together): I think it was '68 that we wrote the first proposal, because the first money came in '69. [Queries on IBP/application process, started in 1967-68; approved/funded/implemented, 1969-70.]

Hall: I got involved because I was coordinator for the Alsea Watershed Study [started in 1959, Coast Range], it was winding down, and the IBP looked like quite an opportunity for watershed studies. Norm Anderson and I led the initial aquatic components. Jack Lyford joined to help with modeling. One thing I remember about the Pack Forest meeting was Jerry championing old growth at a time when no one else was, well, Dick too, but particularly, Jerry. At a time when the U of W folks said, "Hell, it's all in second growth." [Reference to forest at their study site in Washington, seemed to cement the Oregon commitment to the Andrews.]

Waring: Or it soon will be. Right?

Hall: I had the privilege, I guess, of following Jerry, I think it was Jerry, and then Dick, as site director [HJA], for a brief time.

Waring: On sabbatical.

Hall: As soon as I got through with that, I went on sabbatical, but before, we did something that made the most difference in the project, as far as aquatics saw it. We formed a pretty good model, for not only the Andrews, but elsewhere, and what most of you have been involved with in one way or another [integrated, collaborative, long-term monitoring and research]. I think that has really paid off, for both science and as a model of personal cooperation. That's what I remember about the IBP program, for making it all possible.

Waring: I have to comment a bit on how wonderfully challenging it was maintaining the stream team within this university. First, we moved all the money through Jack Lyford, because there was no overhead taken out by his department. Then, we channeled it back inside the campus. And then, when Jim Sedell did his work during when you were on sabbatical and came back, we promised him a raise. Then, the acting department head, didn't want to give it to him, at the last minute. So, I went across the street to the [OSU] Assistant Dean of Agriculture, who was a neighbor, and did it anyway. (Laughter from group) I really thought that was a pretty clever way to do it, but it took eight years before things calmed down. Anyway, Jim got his raise. I guess Bill also had a challenge, in that with his department head, he didn't realize it was a debt, and if it was, he wasn't going to cover it. Gee, it's fun working with all that stuff. Maybe I'll get a full professorship from here. It was well worth it, I must say, but it was rough for a while,

before opportunities came along. Most of the problems were administrative; they weren't among the faculty, and they weren't among the other things. Jerry had his problems with his project leader. I think the thing he couldn't figure out, was that it was kind of a shell game. We'd have money under one shell, and move it before people could figure out how to steal it. (Group laughter)

Unidentified Voice: How to share it.

Hall: I learned a lot. I've always been pretty adaptive at this, but it was really a great experience.

Franklin: Nothing has changed. (Group laughter)

Hall: I rest my case.

Franklin: Oh, it has changed, in terms of the bureaucratic stuff.

Hall: Oh yeah. You got a lot closer in a hurry.

Franklin: That's plugged up all the rat holes to accomplish something.

Unidentified Voice: And it's hard to hide.

Hall: You can't deny that you didn't do this one time ten years ago.

Franklin: There's one other thing that I ought to mention; the "great compromise." That is, the aquatic people didn't want to do anything in streams that didn't have fish. You guys wanted fish, and we insisted you work in Watershed 10, and there were no fish in Watershed 10 [HJA EF]. (Group laughter)

Franklin: This is virtual reality stuff before it's time; fake fish.

Hall: It was alright when Sedell came along, because he was a microbe man anyway, and it worked well, because we agreed that we'd have a site at Mack Creek with fish, and do Watershed 10. One site with fish, and one without.

Unidentified Voice: We all had to give a little bit.

Franklin: One thing that does need to be recorded, since Jim [Sedell] isn't here to say it, "We're the best thing you got going. Streams are the best thing you got going, Jerry. So, you gotta give us all of the budget from here on out." [Paraphrasing what Sedell might have said]

Waring: I remember, we had these meetings where we'd try judge cases on merit. Jim would come up and give these talks, and somebody was really upset because I took money out of

Forest Science [OSU-COF Dept.] and used it for the stream team. So, he was unhappy. But well-spent.

Geier: Jim, did you want to add anything about implications of the IBP for your subsequent career?

Hall: Actually, as soon as Sedell was on the ground, I sort of faded out of the picture, not totally, but remained mainly as a supervisor of the graduate students. With Sedell, all you had to do was stay out of his way.

Geier: Martha?

Martha Brookes: I'm Martha Brookes, and I'm here somewhat under false pretenses, because I have never actually worked at the Andrews. But I have worked on the work of the Andrews. I was an editor for the [OSU] College of Forestry starting in 1965, and there were some Andrews' things that came out. Then the IBP came, and IBP was a real spark. You could see a difference in the sort of intensity that you didn't see normally, as there was a high degree of excitement about IBP. The editing room is not usually the most exciting place to be. One thing that was a big breakthrough to me, was when IBP came, that is when the IBP office on the second floor of the Forest Research Lab was occupied by a bunch of scientists, and Mary Anne Strand.

Geier: Mary Anne Strand.

Brookes: Who was a graduate student in botany, she was also there. It was amazing. There was somebody to have coffee with who wasn't an editor.

Unidentified Voice: Or male.

Brookes: Then, for a long time after, the College of Forestry [OSU] was not integrated. A senior male used to have a Christmas party, and he would send notices to all the men, telling them they had to give money to buy the "girls" presents. The women all got letters that said to bring cookies for the men. The women were furious about it for years. I kept saying, "No one's ever been fired for not going to the Christmas party. Just don't go." Then Joan, because she was a scientist, got the letter about sending money. Also, because she was a woman, she got the letter about making cookies. That was the end of that. IBP did a lot of good things. (Group laughter)

Franklin: Did any of you have College of Forestry undergraduates who were working off their field apprenticeships? Because there was a questionnaire that came out of the dean's office that asked about their behavior. I got one of those for Marie Root, who was one of my recruits that year. This was meant to be easy on the employer, because it had the characteristics down one side, and then grades along this way, but they had word versions for them. There tended to be things like, "just one of the boys."

Brookes: Oh, really?

Franklin: And I had a lot of problems sending that back. (Group laughter) There were no girls in school at that time; not allowed.

Brookes: There were precious few undergraduates.

Hall: I remember that evaluation, Jerry.

Unknown Voice: The forms?

Franklin: Not the form, but the history. Because I failed my summers then.

Waring: He wasn't one of the boys. (Group laughter) I rest my case.

Brookes: One of the things that's always been part of my agenda, which is really not in the job description of editor, is that being an editor forces me to read across all the disciplines, and I have training as an ecologist a hundred years ago, when it was a lot simpler. I would read a paper and say, "This person needs to talk to that person because this economist is building houses in the year 2025, that this ecologist says aren't going to be there." I had very low success in getting them to talk to each other. I think that's what we've been talking about; the disciplinary cave everybody is in. I started to see interdisciplinary work in the papers I got to edit from the Andrews. It's why I've been an Andrews research supporter. One of the things they used to do to patronize us with "the girls" kind of thing, is they would take us on a trip someplace. Everybody got to bring a sack lunch and go see something. I remember my first trip. I can't tell you what year, because I'm very bad with years. I can't even tell you what years my children were born. I finally got to go see the Andrews, and I could understand the love of this place.

Franklin: I think you started that Dick. I think you're the one that started that.

Waring: Well, I like an editor's company a lot better than most other people. They just help me improve rather than make me work. (Group laughter)

Franklin: The notion of regularly taking staff down so they could experience that. I think you started that.

Brookes: It was a wonderful idea. I would also say that one of my contributions is the subsequent history of the Andrews, because I talked Dick Waring out of using the word "preservation."

Waring: I only used it once, and I misspelled it. It wasn't too hard.

Geier: Art?

Art McKee: I'm Art McKee, and I arrived at the Andrews and IBP program in March of '71. A few years prior to that, I'd been at a laboratory associated with a public works department, and then, graduate school in Georgia. I had perceptions of how the IBP was supposed to be structured and function, based on experiences with the Eastern Deciduous Biome (at Hubbard Brook and Coweeta). I got here to find a different structure than expected. Here, there was a lot freer communication among the different components. As a student, I was delighted to see it was easy to offer suggestions and hang around certain people. There was an openness here you didn't get there [Georgia], a really good climate for people at entry level, like Fred, Jim Sedell, and myself. The IBP years, as Jerry mentioned earlier, set the stage for how programs at the Andrews evolved into LTER and drew in other sources of funding. These were very definitely exciting years, and it helped to crystallize in a lot of people, the value of group research. I was especially impressed early on with the way people interacted. I really want to say one more time, is that it was such a dramatic contrast to the other two programs, where people very carefully defined their roles. The chance to move far outside of that was, one way or another, difficult – a very different kind of atmosphere. It's been important for me in my career, setting the stage for the proposal that Jerry and I wrote in '76 that established the Andrews as a National Field Research facility, and sort of kicked off the LTER project. The request for proposals for the LTER program contained a lot of elements that were in our facility. So, it's been fun, and a good experience.

Hall: Well, we can't let this go by without acknowledging Arthur's literary contributions.

McKee: Yeah, we can. (Group laughter)

Hall: I can't begin to even describe it.

Franklin: I'm trying to remember the bed.

Hall: The Procrustean bed, yes.

Brookes: It's not printable. (Group laughter)

Waring: It was printable. He just chose his words poorly.

Brookes: Oh, I see. I have the feeling that administrative anger is really a danger.

McKee: Well, it's not that I couldn't remember them, not that I couldn't repeat them. Some of them are best not spread around.

Brookes: Yes, that's my thought.

Franklin: Yeah, I think so.

Geier: Could you give me copies for a big chapter heading? (Group laughter)

Waring: You should write the “Andrews hymn book”.

Unidentified Voice: An anthology at least.

Franklin: “Ode to -” (Group laughter)

Waring: No, Jerry.....(Laughter) Who could that be?

Swanson: Speaking to your vantage point of comparative analysis, I was wondering what you thought about the hypothesis, that the Andrews scene or the Corvallis scene in general, benefited by having the Seattle scene as a point-of-reference. Because Dick and Jerry are so different in the way they do science and the way they think about things, I wonder if they hadn’t had that common enemy there could have been more internal friction. I think the differences between you guys were really good for the group, just to have two different paths for science displayed so thoroughly. If there hadn’t been this external “boogie man” to react to, I can see where it could have been a creative force of tension over a long period of time.

Waring: Jerry was never any fun because he would just cry. (Laughter) Dale Cole never cried, so he was a lot more fun.

Hall: That does bring to mind some interactions that were scalding.

Waring: Someone said I often made the right decision, but I shouldn’t smile when I put the knife in.

Franklin: There were a lot of encounters, there’s no question about that. What it finally came down to was that NSF sent a team out here to close down the coniferous biome. Chuck Cooper was the head of that team.

Unidentified Voice: When was that?

Franklin: That was probably ‘70, it might have been ‘71. But it was at the time when we were either going to get three-year funding, or not. We’d done a one-year funding package before that, they came out here, and that was their intent. Basically, Dale [Cole] and Dick and I, got together and made our peace, and agreed that even if we weren’t going to work together, we weren’t going to overtly work against each other, and we were able to persuade NSF to go ahead. But I gotta tell you, it’s not fun being the rubber between Dick and Jerry. (Group laughter) Because I do cry. (Group laughter)

Waring: Yeah, but they think you’re kidding!

Franklin: I wasn’t.

Swanson: Two things with some of this tension. One has to do with both jobs. There was the board about to close down IBP and there was the board about to close down the Andrews Experimental Forest. I was interested in the conversations you mentioned, where all that could have gone on at Pack Forest, which would have sent things, including the whole region, on a different track. So, there's some things around that. But some of these things we portray as conflict, were actually tensions that had positive side-effects.

Waring: You remember that we had Chuck Grier here, one of the former students of Dale Cole. And we had Ken Reed, Dick Holbo, and one other person. Also, Phil Sollins started here [OSU], and went there [UW], and came back.

Franklin: Another bullet, if you want to think of it that way, was the decision Dick made, and it was really you who made the decision, to not pursue major work with graduate students. You took a great deal of crap for that, but if we had continued to go that way, the outcome would have been very different, at least mediocre, and perhaps that would have ended the whole thing.

Waring: The decision involved going for a post-docs more, and phasing out graduate students. We didn't cut graduate students off over night, but we just couldn't see the integration of employees, and we couldn't see the graduate students being able to travel to these other meetings during the academic year. We really needed soft money, so I think the post-docs, most or many of them, ended up also with masters students, like Bob Fogel. And was that a strong contingent!

Franklin: Yeah, and the post-docs became leaders in second- and third-generation programs, and the contrast there is with the University of Washington, where there's no legacy of IBP.

Hall: Well, they're sort of off in different areas now, but there's no real focus in the same way that the Andrews is.

Waring: No.

Swanson: And the interdisciplinary aspect took the group approach out, so I think that's really important in the intergenerational tensions of managing for certain unexpected advances. Another interesting thing was all the "big Kahuna" terrestrial types duking it out. In contrast, Jim Sedell was such a networker. He was working really intensively with [Bob] Wissmar, although they were working in very different systems. But they were developing good working relationships that then flourished at Mount St. Helens [After May 1980 eruption].

Waring: I used to go out to dinner with Wissmar and his group when I went to Seattle because they were a lot more fun to talk to than anybody else up there.

Hall: I think from the beginning the aquatic system group was more congenial. Perhaps because we weren't really directly competing, we had quite a different fix on it. But, I had been

a faculty member briefly at UW, so I knew some of those folks. We got along pretty well, but I think it had a lot to do with politics. There definitely wasn't the tension on the aquatic side that there was on the terrestrial side, at least that we could see.

Franklin: Well of all the required programming, and it was the most integrated.

Hall: We had a comparison study in 1978.

Swanson: Yeah, so.

Franklin: Isn't this where I say to Cole, "Hey, we're the best thing you got going?"

Waring: If you did, it wouldn't have mattered. There were some neat things that came out this program. It was just hard to get the data together so that modelers up there could model. So, one of the sensitive issues was that Phil [Sollins], and I can't remember the other guy?

Unidentified Voice: Gordy.

Waring: Gordy Swartzman. They started modeling the Andrews when they were stationed in Seattle. I think they moved Phil up there when our computer system here was going to get a revamping. So, we said, "If you're going to charge us \$25,000 a year, it's a lot of money, we'll just move the post-docs up there." The next year, we said, "Well, that was a bad idea."

Swanson: Nobody ever believed you'd do these things.

Waring: I mean this was, you won't take that money and put it into stream people. You won't, and then you'd do it and then they'd worry even more about you and the IBP program.

Franklin: One of the strengths of the Andrews was that it was not institutionalized. The institution could not control it, which meant that they weren't going to help you a lot either way, but they couldn't prevent you from doing it.

Waring: We worked towards benign neglect (group laughter).

McKee: I think it's time to mention the conversation between Carl Stoltenberg and I on the evening he would be retiring. I had called him up, and he told me how pleased he was the Andrews was doing so well. I said, "Geez, Carl I don't remember you providing a whole lot of support out there," and he said, "Well, you guys have so much energy and so much ambition that there's no way the College of Forestry could ever match that." So, I said to myself, this group is gonna do it on their own. (Group laughter) I said, "Is that the Boy-named-Sue approach to funding research?" and he said, "What"? I said, "You know, the Johnny Cash song." (Laughter) Everything kind of disintegrated after that.

Waring: One critical thing was when NSF came out to do a review, and it was required there be some university commitment for this to be carried on. It was the end of the IBP, I think.

Franklin: It was the end of the IBP.

Waring: I think it was just a transition. John Byrne, the Dean of Research [OSU] at that time, said, "Well, yes, the university will do that." Of course, that meant that the College of Forestry would pick up our salaries.

McKee: Yeah, my wife told that story.

Franklin: I don't know how we did that.

McKee: John Byrne was instrumental. Carl was less helpful.

Franklin: Carl [Stoltenberg], at least at one point, also indicated his disapproval of the whole notion, that a property as valuable as the Andrews, would be basically controlled [not logged] and run by a bunch of scientists. He saw it as a significant waste of resources, no question about it. There wasn't anything you could do about it, but it was very clear, once he made it clear to me, saying, "It's a pretty bad system where a bunch of people like you are able to basically sit on ten, fifteen thousand acres of old growth. This is ridiculous. And you smile too much."

McKee: They also said that any program longer than two or three years is a waste.

Hall: Yeah, that was a common attitude.

McKee: He would strongly disapprove of the concepts of long-term or ecosystem science, saying, "This is just like all those other watershed studies where they throw money down a rat hole, and get nothing out of it." So, benign neglect is a good thing.

Franklin: I discovered what happens when you institutionalize something like the Olympic Natural Resources Center. [In Olympic National Forest, Washington]

Geier: Let's move along here.

Don Henshaw: This is Don Henshaw. I came out to Oregon in 1974 to go to school, and I was in the statistics department. I mentioned I was interested in doing something related to ecology, so they immediately sent me down to Scott Overton, and he became my major professor. I ended up working part-time for Forest Service Research. I was a liaison between Scott and Boyd Wickman, Dick Mason and other Forest Service people working on tussock moth population models. I think Scott was also working at the time on hydrology models with Dennis Harr, which kind of related to the IBP program at the same time. I had heard about some of

these various projects going on, and meanwhile, I was working a temporary job at the Forest Service.

Then in '78, I think it was Elly Holcombe, who worked in the IBP stream chemistry lab, mentioned to me they were looking for someone to do data work. For some reason, she liked me and said that I should talk to Al Levno. She wanted me to work there because she was unhappy with some of the people she had to work with then. I'm not sure who she meant, but Dick Fredriksen kind of unnerved her, made her skin crawl, when he walked into their lab [Dick was likely in early stages of Alzheimer's]. They'd had that job on the books for a while, and hadn't been able to fill it. I walked up to Al and he said, they're collecting lots and lots of data and need someone to help straighten out the situation. It wasn't really until after the IBP program was largely concluded, but in July of 1978 I was hired by the Forest Service to work in the watershed group. Logan Norris was project leader at the time, and Fred and the IBP people were hanging around. One of the first things Al told me is that I was going to be doing streamflow data, and I was going to be dealing with stream chemistry data. I was sent to immediately to two people.

The first one was Al Brown, who was doing a lot of the data management and data control for the IBP. I believe he'd been involved for quite some time. And JoAnne Kristaponis, who was doing management of the laboratory data. What I discovered from Al Brown, is that during the IBP period he had had definite contact with folks around the country doing data management within the IBP. They had really formed the early framework the LTER ended up building on for taking data sets and documenting the variables collected [metadata], writing data abstracts and creating a whole set of forms to document information I'm not sure had ever been used before. I don't think the notion of documenting this stuff so that it could exist for the long-term had ever really been fully considered. I learned quite a bit from Al, took a lot of the forms he had been using to document information, and started to document all the rest of the Andrews data-sets. He had already actually worked with the streamflow data. I think Scott was still active with that group and Dennis Muscato, I believe, was active. And this would be before the LTER.

Waring: They had no idea because this was a big change.

Henshaw: When Susan [Stafford] came in and Scott [Overton] was not involved anymore, they brought in Rod Slagle, Greg Koerper and Paul Alaback. I think those folks repackaged a lot of what IBP had done, and presented it as the Forest Science Databank, and as a whole new effort. I think they did get some credit, but I think there should be a lot of credit given to the IBP effort in terms of starting that documentation of information. I really think there is a great advantage from the work done in that period. There were a lot of data sets collected, and then, with the documentation, you could actually go back, still find the information and do something with it. A lot of them [data-sets] would be worth bringing forward I think.

Waring: How about card readers? (Group laughter)

Henshaw: For instance, vegetation data that's listed on 50 or 60 thousand data cards. Charlie Halpern ended up going down to the vault and pulling them out, with the help of Paul Alaback, and I think I was involved somewhat early on. It was surprising that they were able to document and preserve the data from a hands-on era that was before computers like we have now. It's really impressive what they were able to do. I really learned a lot from the IBP efforts, especially from Al Brown and JoAnne Kristaponis. They also had done some things with the stream chemistry data I was able to capitalize on and make use of, and that we've really taken into the LTER program, and data sets which still are strong here, because of the early days of the IBP in terms of data and information management.

Franklin: The Forest Service gets some credit, too, for the tradition of documentation and keeping good records. It doesn't do that now, but in the days when Ted [Dyrness] and I came into the Forest Service, we were on the tail end of that tradition. There weren't computerized records in those days, but they kept very high quality paper records. And they had establishment reports for studies, so they had good documentation of what went on. There wasn't a tradition at all in academia to do that kind of thing.

Henshaw: I would say that's true, too. The Forest Service collected data sets like streamflow and stream chemistry, and they were fairly well-documented. Those were the ones that I ended up working on primarily.

Franklin: Now that's all forgotten in the Forest Service research organization today.

Henshaw: Is that right?

Franklin: Yeah. The tradition that came out of the '20's and '30's and '40's. But we learned with that system.

Dyrness: When I started working for the Forest Service, the documentation went so far as to have a daily diary that you filled out. [They also used field notebooks when in the field.]

Franklin: I sort of forgot about that.


Levno: You know, what you did, hour-by-hour at the lake.

Waring: Just like the surveying records that you had to have an area crossed through, and you initialed it. I only worked for the Forest Service one year, but I still have a Forest Service key, however. (Group laughter)

Swanson: I did want to reinforce the point that Don made about the importance of the IBP period for getting data management tremendously ramped up, and that some echoes of that effort continue today with web pages and the internet. We are seeing publicly, through information management, in a way that's just way beyond anything we would have imagined in those days, but the baseline was set, and that's tended to bring scientists closer together with

information managers. We have to work together on things, and that had been a struggle for a long time, because our paths tended to separate. The information managers would be sort of technology- oriented, and the scientists would be science question-oriented, but now, a number of factors have pulled us back together in some very close ways. There are interesting legacies today. I also feel like NSF knew information management was one of the things they tried to manipulate to help LTER or institutions like ours, the Andrews, be a prototype for ways of doing science in other settings in the U.S. and internationally. Information management is an important part of that. NSF really tweaked us or manipulated us or pushed us on the information management front. Within IBP, some of that behavior really got going because there were projects that were running for longer than a single grant period. They wanted us to do good data management. Weren't they telling us, "You have to have at least 15% of your budgeted efforts in data management?" Weren't we getting pushes like that in IBP, or does that only emerge in LTER?

Franklin: No, we were getting pushes, though I don't know if that's where the 15% thing started. They didn't know what they were pushing for.

Waring: They were all pushing for modeling, too. Well, one part about this, is that originally this was mentioned. They had the modeling community, which was very small, said, "We did the data, and this is the form we use." The big problem was getting to apply it in any kind of research, first getting it, and secondly, getting it documented. And most of the scientists would not have worked with data that way. They would have done simple correlations or regressions, this kind of thing. These people were trying to get used to very, very complicated models, much more complicated than we use today. Until network computer systems came in, which was long after IBP, scientists really didn't have easy access back into data-bases, just the modelers did, and even they had problems with it. We wouldn't always document the units or certain things. So, there's a lot of problems, that I think today, both for the workstations and the network, have made many scientists more open to saying, "To do this paper, I need these data sets." The policy was often; no data in, you don't get any data out unless you put the key, that you and other people need, to get back in. And so suddenly, it was easier to do, and I think there's been a big  change.

Swanson: The motivations for good data management have changed through time. It was the modelers early, and now it's more in terms of long-term record keeping that can be used to ask questions of the data sets that we might have imagined early on.

Waring: Yeah, and still modeling. To go back 20 years you need the met station data, you need the stream flow data, and depending on your case, you might need to know changes in lichen populations, next to roads, and the 30-year old data to compare with today. I mean you can't --

Swanson: And met station data.

Franklin: And met station data, too. So, all the climate change stuff is pushed towards ways to best correlate with radiation data, where we took that data in some places. We had radiation monitored for a couple years.

Henshaw: I think they developed systems for retrieving information off mainframe tapes that came in, maybe in the early '80's. Even so, I think you're probably right. The only ones doing that were modelers in the late '70's and early '80's, and they knew how to retrieve data.

Waring: It was not easy.

Henshaw: Al's [Brown] and the other efforts that were going on back then, did leave a nice legacy of lots of paper forms. He documented everything on paper in those days as well as having computer files of the data and documentation.

Levno: And [Brown] annotated the hydrograph notes.

Henshaw: Whenever he sat down with somebody bringing in a data set, he would immediately assign a study code and they developed this theme of categorizing the data and assigning codes, and write down what he knew. He started, literally, tens, maybe hundreds of folders of studies that still form a lot of the old historical files in our data bank. We've taken many of those and they have been automated. Now we're in our current data-base. Those old forms have solid database structures, and we use them to do automated quality control checking and many things that I don't think Al had visualized way back then in how we might use that information.

Al Levno: I'm talking along the same lines as Don did. Well, I should go back and say I would get started with a project and -- [missing words, see next graph on new tape for context]

End of Side B, Tape 1 (of 2)

Begin Side A, Tape 2 (of 2)

Levno: [Tape begins mid-sentence]-- or it was '65? I had no education, really, and was just interested in learning about the watersheds and procedures used in the program. So, beginning in '65 until things started happening in IBP, it was just me and a Dodge pickup running around the Andrews. (Group laughter) It was a great life, and then, all of the sudden all these other people started showing up, kids were all over, and, my gosh, you almost had to take your swimsuit off in order to swim. Boy, what an invasion. So my job, even through all this constant interplay and change, was to maintain some kind of decency within the program and with my projects, watersheds and stream sampling, and also try to adjust to the IBP program. I knew that Dick Fredriksen was working on a Ph.D., using Watersheds 9 and 10 in his project. That was supposed to be a two-year project to compare burned versus unburned [watersheds] data, and then we were to dismantle the Watershed 9 and 10 gaging stations. So, we just built those out of cardboard and plywood shells, and with a very small instrumentation

package in the watershed. Then IBP came along, and all of the sudden Watershed 10 turned into a major, major project.

Franklin: “The million dollar watershed.”

Al Levno: But he was delayed five, six, seven, years, until we got all the data that we needed. We had to go back in and rebuild the gauging stations so that they would hold up under this unexpected impact. All kinds of data and new techniques came along. It’s been my job to be the quality control engineer and stay the course and know how everybody was exploring new fields. I was trying to keep things together and keep data useable for long-term records. In the end, I think I survived the effort and appreciate the results that IBP and LTER have brought to the Andrews. We branched out, kept a hold on our work, and participated through the management changes. My perspective is a little different, but I think it is important that the program evolve, and we need to have the feeling that there is a ladder on which to build, from the data collected in the watersheds, such as climate information. That’s an important part of this whole story.

Geier: So, one of the long-term legacies you’re talking about is just the long-term presence of people there?

Swanson: That’s been extremely valuable. For example, graduate students are coming to do projects related to experimental watersheds, and Al and others have been around to supervise them. All in all, it’s been very important for the students, and there’s a legacy of data and a legacy of people with a great deal of experience. This is an interesting thing made evident in a recent inter-site hydrology workshop, was that some of the benefits of corporatizing activities, like experimental watersheds, there’s a downside, but there’s also some important upsides. At some of the other sites unlike the Andrews, their scientists say the experimental watershed work is their own domain and they hold the data tight. It’s not available for much more analysis, and they just hold off on other things. Data access is more limited than in the case where somebody really took responsibility for quality data, like Al and his crew, kept the project going, but weren’t territorial about the uses of the data. So, now we’re having a tremendous period of use of the long-term data. Quite a few masters and Ph.D. projects, and senior scientists, are doing neat work. That’s been an interesting development that’s played out over a couple decades.

Hall: Yeah, I can make a note of one of my fisheries doctoral students who went back and used the stream data. That’s not something that you expect to happen.

Henshaw: There’ve been countless times too, when we’ve been revisiting these old data sets and we’ve said, “Let’s ask Al and find out what he remembers!” (Laughter) More times than not, Al has been able to provide some insight into many of these older studies. I think the long-term people, and the legacy of what they remember is critical in terms of developing these historical descriptions of the older data-sets.

Swanson: The teamwork of the field people and the data people has been real important. The importance of that came home to me recently when some people were criticizing the Jones and Grant paper about land-use effects on streamflow revealed in long-term records, and the analysis of them. Those people have had responsibility for the Alsea watersheds that Jim [Hall] was instrumental in getting going back in the '50's, but record-keeping and sharing faded out.

Hall: I think it started in '54.

Swanson: Those people were some of the complainants. So we said, "Okay. Why don't you use the Alsea data?" And we think it's appropriate to ask questions of them. The records aren't of good enough quality, the data aren't available, beaver dams screw things up, just one thing or another. Neither the field part or the data part, has been tended to in a way that made them useful. It's reinforced in my mind the value of good field work and good data management.

Geier: Let's move on to Bill.

Bill Denison: Well, these guys dragged me into this sometime in the late '60's or early '70's. What they laid on my plate was responsibility for the decomposers, and I'm familiar with fungi. But I thought, there's a lot of decomposers out there other than the fungi. Once you start thinking about bacteria in an ecological setting, then you're beginning to have to look at the nitrogen cycle. So I went down and wandered around down there, and couldn't find any of the traditional nitrogen fixers in the closed stands of old growth. I knew from literature that people had found fixation taking place in lichens. And at the same time, the acetylene reduction technique had just come on the market as a simple way of asking the question in the real world, "Are you or are you not fixing nitrogen?" So, we tried that on ground samples and got positive results. The issue then was, is there enough of that stuff up there to be of any real interest to the system as a whole. At that point I had two young women undergraduates both funded by the National Science Foundation Undergraduate Research Participation Program, in its terminal year, who came to me and said, "We don't care what we do, but we really want to do it outdoors."

So, I talked to a number of people about, "What do you when you want to work in a canopy?" Basically, they said, "We don't. But, given what you want to do, we'll work with you and find a stand comparable to what's in the Andrews that's already dedicated to be cut, find you a good faller, go in and drop a tree, and find out what results." We did that, and what resulted was an incomprehensible mess when that stuff falls down on the forest floor. Back to square one.

In fact, they spent some time crawling up and down the trunk. The following Tuesday, I think it was, Diane Tracy came into my office, sort of hemmed and hawed for a while, and finally, she said, "Well, I've done a lot of technical rock work and I think I could climb one of those trees." So, the following Saturday, the four of us went down and she proved that in fact, she could. Of course, I was then confronted with talking about administrative problems and this "problem" of going to the Forest Service and asking for permission to have two young women who didn't look as if they were out of high school, climb around a canopy of trees. (Laughter)

Waring: Before OSHA. [Occupational Safety and Health Association]

Dennison: No way. What I did on a couple of occasions, was to bring the two of them to IBP meetings, introduced them as my assistants for the following summer, and endured the “knowing looks” over their heads, then had Diane Tracy explain how she had done it, and took that approach over the summer. There were a number of people that wandered over, knew there were two young women climbing trees, and tried it, more or less. But as much as this was deliberate exploitation on my part, such is the nature of the male ego that nobody has since, or during that time, ever suggested this was too dangerous. Actually, our safety record was and is excellent.

Denison: Martha, I have a question for you. The first publication that came out, which was in fact, a Forest Service publication?

Franklin: Pepper Dempster [for publication just referenced].

Denison: Was Pepper responsible for that? At any rate, when I was redrafting the final draft of the paper that described the technology, I realized the only two people who had really done it were those two young women. So, I changed all the pronouns to female pronouns, and the Forest Service published it in 1972. I have had three women come to me and say something to the effect, if not for that or the *Scientific American* paper, I would never have thought of doing something like that myself. I brought this [list of people involved with tree-climbing effort] with me, because the record doesn’t exist anywhere else, but I have the names and where they are now. They were mostly undergraduates, one graduate student, and my son, who was a high school student. Here are the eleven people actually doing the tree climbing in the early days, and where they are currently. I have one other anecdote. About five or six years ago, a local sixth-grade teacher asked if he could bring his class to talk to me about what botanists do for a living. I brought in some climbing gear and talked about various things I’d done at one time or another. After having demonstrated the hardware and ropes, I said, “Probably this is so dangerous, that it’s only for rough, tough, young men.” I then displayed a photograph, and asked the kids, “Do you recognize this woman?” (Laughs) It was Kathy. The kids looked at it and said, “Is that Jackie’s mom?” Jackie was a kid in sixth grade. She found her home, moved there, and there she was in the top of these trees. [photo of her climbing a tree in Barro Colorado Island] (Laughter)

I’m going on four years of retirement, but I have a graduate student working with me on the two principle nitrogen-fixing lichens. One other consequence of the kinds of lack of departmental support that’s been talked about here, has had an impact on my career in two ways. One was in 1983 when I went out and organized my own company. I must tell you, being the owner and CEO of a corporation that does primarily research and development, is a lot more fun than having to obtain funds up through the ranks. If we’re going to send somebody to China for example, the whole group sits down and we vote on it, I get out the corporate checkbook and write the appropriate check. Because that’s a for-profit, it has some disadvantages. I also have been involved with the organization of one business which is now in

an embarrassing situation, since it's an educational corporation with a considerable cash surplus in the bank that has accumulated through two different groups, which you might all appreciate in one way or another. We take only ten percent to manage a grant, and if somebody is doing field work that does not require any other institutional support, it's a whole lot cheaper to run it through us than any other institution. If you have an elderly or not-so-elderly Forest Service employee, who carries in his head a great deal of valuable information, and he is retired for essentially fiscal reasons, he cannot contract back to the Forest Service. Then we can hire him.

Brookes: Can you do that for me?

Denison: Absolutely! (Group laughter) It hasn't happened, actually. But absolutely. There have been a number of other consequences. There is now something called the International Canopy Network, a major international organization, which came directly out of this thing we started. There's also a below-ground one, which also came along.

Waring: Those were two places it was difficult to get to for completely different reasons.

Geier: Okay.

Ted Dyrness: Well, I'm Ted Dyrness. I was one of the very few people that worked on the Andrews almost exclusively, before IBP. And I well remember how we sat around often at lunch breaks or in the evening, and bemoaned the fact that we were so short of people. It was just Al, Dick, myself, and Jerry. And Jerry had other things to do.

Franklin: I was a part-timer.

Dyrness: Yeah, he'd come around about two or three weeks in the summer. And of course, Jack Rothacher. We were always saying, "If we had the manpower or the money, think what we could do in terms of additional research." We know nothing about soil hydrology, for example. We'd have these discussions, and then, one day Jerry came to the lab and said, "Have you heard about this new International Biological Program that's going to be funded by the National Science Foundation? We have to get on board on that. It's directed towards ecosystem analysis." Systems ecology was a term that was thrown about; ecosystem structure and function. Well, we decided that we didn't know much about those fields, but that we needed to get on board.

Franklin: You shouldn't let a little lack of knowledge deter you from something.

Dyrness: Yeah, right! Do you remember that meeting we got together with faculty at OSU, to talk them into being interested in this program? Do you remember that meeting?

Waring: I do.

Franklin: Yeah, yeah.

Dyrness: Also, at the same time that summer, there was a short course at the University of Wisconsin-Madison, put on by the Forest Service, in systems ecology. Jerry came to me and said, "We gotta go to that short course, find out what this field is all about." So, I went, and maybe one or two others went. I forget who went, but Jerry "conveniently" had a conflict the first week. It was a two-week short course.

Franklin: Don Minore went.

Dyrness: Oh. Don might have?

Franklin: Yeah.

Dyrness: Joan Hett was an instructor, and Orie Loucks. Yeah, all these pioneers in systems ecology. I remember Joan gave a demonstration on computer modeling or something. It was a "gee-whiz" thing. But the trouble with the short course was that it was all lecture. We didn't have any kind of discussion. That's the first time I ever saw Bob Romancier. He was in the Washington office with the Forest Service at that time, and we accused him of being a Washington office spy at the short course. Jerry showed up the second week, and I'll never forget, he says, "How do you guys put up with this for so long?" Because it was kind of deadly. It was the verge of summer and you sit there. (Laughter) That was quite an attempt to get us up to speed. And as far as the contact with the other people, it was a good deal. We were just starting off from ground zero as far as all the emphasis on ecosystem, multi-disciplinary research. Jerry said, "We've gotta get involved." That carried on to discussions with the University of Washington, who would head up the Western Coniferous Biome. I remember well when NSF said, "You guys gotta get together. There's only gonna be one western coniferous biome. There's not gonna be two programs." That led to the combination between the two universities. I remember, once we got it set up, we had these committees. I was involved in the biology and chemical cycling committee, with Dale Cole on it. We had a participation and a logistics nightmare, because we needed to meet at periodic intervals, so we'd meet sometimes in Portland. I remember meetings at a motel near the airport.

Franklin: Weyerhaeuser's lab in Centralia.

Dyrness: From that standpoint it was good, because we got acquainted with our colleagues at the University of Washington. But I left in '74, and one of the hardest parts of leaving in '74, was that by then, the IBP was up and running. We'd set up all kinds of reference stands. We had people taking pre-dawn moisture stress measurements. We had to install thermographs for soil temperatures, etc. One of the hardest parts of leaving to go to Alaska, was that I was leaving this program which was very exciting. In almost a bewildering kind of way, the explosion of personnel on the

Andrews was astronomical, and we had trouble even keeping track of who was doing what and why. I remember that was the time Jerry was back at NSF. I called Jerry, and said, "They've offered me a post in Fairbanks." I thought Jerry would say, "Well, we need you at Corvallis, and don't take it." I remember Jerry encouraged me to take it, which he denies sometimes now, but.....

Waring: Well, we had too many people on the Andrews. Don't pray for something. It might happen. (Group laughter)

Dyrness: What you said as I remember, "Sometimes with these kind of radical changes you have to sometimes kind of pick yourself by your nape of your neck and go for it." You ask how this influenced my subsequent career. That was a time that Jerry was also trying to get new proposals for ecosystem research, and I joined a colleague, Keith VanCleve, at the University of Alaska, to do the same. The first thing we did was to work on a proposal for a mini-IBP program in the taiga ecosystem, which I think was a good outcome. I really never regretted it, despite the fact that it was kind of hard, because I was involved thoroughly in the IBP program on the Andrews. When I got to Alaska it was tough, as Keith had been involved a little bit.

Franklin: In the biome.

Dyrness: In the biome, but he felt he was just getting tossed bones.

Franklin: Small as we could make them.

Dyrness: Right. (Group laughter)

Dyrness: I must admit that it was Jerry's cooperation and urging that got us through the proposal stage, and got a mini-IBP program funded in Alaska. Of course, that's grown into a LTER program in interior Alaska [Bonanza Creek].

Franklin: Well, I came up with Earl Stone the year after you moved up there. I think I may have even said something to you about that; "You know, if you go up there, we'll come up and see if we can help you out."

Dyrness: Yeah, yeah.

Franklin: Earl and I went up there and got looking. Earl was very helpful to you guys.

Dyrness: Oh, yeah. We found out there was great support on the campus of University of Alaska [Fairbanks] for this kind of a program, and we had studies of nitrogen fixation and with processes you were talking about. The difference between University of Alaska and here, which I never got over marveling at, was that up there, the troops were so thin that you only had one person in each field [discipline/project], whereas down here, you could have several in a field. Up there, everybody was pretty essential.

Geier: I wonder if we could talk for a little while about the operating assumptions driving research in the IBP program, about the time it got started.

Waring: All the workshops I was involved in had box-and-arrow diagrams. They all had structure, and the arrows were supposed to represent function or transfer, so if you didn't measure litter-fall, you couldn't get the transfer of the organic matter or nutrients for the forest floor. If you didn't have some kind of attempt to follow decomposition, you couldn't get the stuff mineralized in the leaf. If you didn't have water coursing through the system going to the stream, you couldn't explain how it ever ran out the bottom and what would be in it. So, you had these box-and-arrow diagrams, which were not original with our biome. They had been published in books before. The whole emphasis of the IBP was to take "undisturbed systems," and document, not manipulate them. It was almost manipulation-free, other than Watershed 10, which actually got cut. To look at the steady-state conditions and transfer rates, and then describe those in mathematical terms, if not explain them. You had primary production, you had decomposition, and you had the hydrologic models imposed on everybody across the United States working on forests. They had basically the same diagrams, except they were more detailed, sometimes.

Franklin: You're right. The only thing that we really knew to do at that point was to make budgets. (Group laughter). So, we're going to do a carbon budget, we're gonna do a water budget, we're gonna do a nutrient budget, as well as we can. That was the level of sophistication. There were a lot of things defined that had to be studied. You hired people, and funded people to fill in those boxes or those arrows. So there was a certain element of opportunism as you sorted out who was going to deliver and who wasn't going to deliver. It was very clear there was significant mortality of participants

Hall: You guys didn't have to man the net on Watershed 10 in the middle of the freezing night when the storm was all over you. (Laughter)

Waring: We modelled it.

Franklin: I guess to a certain extent, you make a good point though, Jim, because –

Hall: -- We produced.

Franklin: The stream team delivered.

Hall: Yep.

Franklin: And there was another element to it, too. It wasn't just a matter of parameterizing the boxes and arrows. There was a lot of flex in that and how well you were going to estimate this and that variable. But there was also the aspect that you're

just somebody who's producing a product, writing it up and getting it out. So, there was an element of opportunism as to who the real producers were, not just in terms of numbers, but also some indication there was going to be output as far as publications. Again, it became pretty clear that there are some people that just weren't going to deliver.

Denison: Well, it seems as though there was a kind of educational process going on, in terms of taking people who had been trained as scientists to think in terms of precision at the third and fourth decimal place. Or, at the subspecies level and getting them to look at some questions that hadn't really been looked at before. Some people were willing to do that, and some people weren't willing to do that.

Franklin: Right, right.

Denison: I think before IBP, essentially, no mycologist would have thought it a serious question if somebody had said, "How many kilometers of mycelium are in so many cubic feet of soil?" It wouldn't be a serious question and there would be no way of approaching it. It was a really important mind shift and education that went on amongst those of us that got involved, either peripherally or otherwise. For me, that shift in emphasis was one of the most important concepts, saying maybe I don't need to know things in terms of milligrams per cubic centimeters. Maybe I need to know in terms of five-yard dump truck loads per hectare. Just asking that question, or simply saying "Well, it's going to be less than something, and it's not going to be that astronomical figure, it's got to be more than some value. What can we assume or estimate realistically?" That kind of thing.

Hall: I think that one of the things that IBP really focused on was the connection between land and water. There had been some work on it, but it was a major, major emphasis of IBP, and that certainly changed the way a lot of people thought about things. I mean, we were terrestrial, and we were aquatic when we started, but -

Waring: Then the logs fell in. Yeah. (Group laughter)

Dyrness: That's true.

Hall: I think that was a major contribution.

Denison: All these insects came up out of the water and our spiders ate 'em.

Waring: Food chains, was one of the more difficult things that I don't think we did well. We started to fund it. Ron Nussbaum did the survey on that, and it was really difficult. I went to a British Ecological Society meeting, celebrating I think their 75th anniversary, and they came up with difficult things that said things like, after 75 years they still

hadn't figured out how to really generalize about food chains. Partly because the links change, and it was not a simple system.

Swanson: A key point Bill made is there were things that emerged within the constructs we were encouraged to use for IBP, where we had to start thinking about the system we were working with, in different ways. Like from a geomorphology point-of-view, starting to think about sediment budgets and routing, not a traditional way to do things. Also, it seemed that some of our major discoveries were interface issues like woody debris, where people worried about live [biota] and hadn't seen the dead or forest-stream interactions, and it puts old growth in a dimension where both sides of frameworks we were given.

Hall: That reminds me that one of the, probably the biggest management implications of the IBP from an aquatic standpoint, takes place in the woody debris. We were paying people to take those beaver dams down and take out the wood.

Franklin: Right.

Hall: Of course, now, I have to remind you that there was a hell of a lot of wood that you guys put in the streams. We didn't know how to log in the early days.

Franklin: I didn't do anything. (Group laughter)

Hall: The whole concept of the role of large woody debris was quite a revolution.

Franklin: I still remember a trip, it had to be in the '70's, when we were taking Glenn Jorgensen around the Andrews. We were telling him about woody debris, and he told us, "We're just going to Congress for a big line item, a big budget item, to do debris clean-up, so, for God's sake don't mess up the story." I remember, he was pretty upset.

Unidentified Voice: Because one big thing about the concept is that it [woody debris] wasn't waste.

Swanson: There was a big "logging debris in streams" conference organized by George Brown in 1975, with an encore in 1977. Well, of course, Jim [Sedell] was there, and interacted with Hank Froehlich.

Waring: Is that the word? To interact? Well.

Swanson: It was one of those great examples where forestry interests didn't want to have the loggers running these big towers and crews of six or eight people there to log high-value old growth, just pulling wood out of streams. We wanted to demonstrate the role of natural wood in streams, so then Jim picked that up and ran with it, ecologically, all the way to the point of saying, "Streams need merchantable timber!" (Laughter)

McKee: Don't forget Jim's "three c's" to explain the richness of Pacific Northwest rivers: Cedar, sediment, and salmonids. [Group laughter - joke about Jim's spelling.]

Dyrness: I think another theme of IBP that we got into before is really characterizing the different forest associations in terms of environment. We had categorized them, but with IBP, we had the wherewithal, the manpower, and money for instruments, to really get out and look at the environmental differences between various forest communities. That's why we set up all these reference stands. Do you agree?

Franklin: You bet.

Denison: I have a historical/geography story. The two young women that climbed [trees] in 1970, would go down for three or four days at a time and they'd sleep in a station wagon on the little spur road off the main road. Then, the next summer, I had four climbers working. At that point, the biome administration decided we would not have anybody spend the night in the Andrews. That changed a little bit.

Levno: I thought you took the tents up?

Denison: No, there was going to be nobody sleeping in the Andrews, and a trailer was set up in the trailer park across from the Blue River Ranger Station for people who were going to need to be there. My crew consisted of two young men, my high school student son, and Fred Rhoades, both of whom had hair down to here, and looked like the hippies they were, and two young women. One of whom was Jane Thomas, and one of whom was Diane Tracy, and they were clearly cohabitating in this trailer that was supposedly to be occupied by Forest Service families. After it became obvious this was going on, and mind you this was 1971, the word trickled down to me that maybe it would be all right if my crew spent the night on the Andrews. So, for the next, I think six years, what has now become for some reason that I don't understand, called "Gypsy Camp," was in fact, the encampment for tree climbers. It was only after they abandoned it that nobody was there to defend the huge-growth tree in the middle of it. [Tree was removed by Blue Ranger District because it was considered a "hazard tree."]

McKee: That came down on a Saturday morning Bill. It was cut after the top --

Denison: -- The top came out of it? Ah, okay.

McKee: The top 50 feet flew across Lookout Creek.

Denison: Okay.

McKee: I had the misfortune of being there, and watched it come right at me

Denison: Is that right?

McKee: Yeah.

Franklin: There were a lot of interesting dynamics in those days. I remember after I came back from NSF in '75 when Bob Burns was the ranger. [Blue River R.D.]

Waring: I remember Bob.

Franklin: Burns believed you ran a ranger district like a military post.

Waring: So basically, he was the Gestapo. (Group laughter)

Franklin: Yeah, and he was an old friend too, an old acquaintance, and he was really upset. He was upset about the fact that a lot of these university people were sleeping during the day. (Group laughter)

Waring: They worked out there all night, you know.

Franklin: And he was concerned that there was all this skinny dippin' going on in Lookout Creek. (Group laughter)

Waring: Yeah, that's true.

McKee: Absolutely.

Franklin: And then, there was the time that --

Waring: -- Phil Sollins?

Franklin: That Phil Sollins was leaning --

Waring: Yeah.

Franklin: -- Against his vehicle, and he [Bob Burns] came out and it looked like it was going to be a fist fight between him and Phil Sollins. And you know Phil was out there with his typical, you know, arrogant sort of --

Waring: Well, I think he was drinking a beer. While leaning against the -

Franklin: Yes he was, that's right. (Group laughter)

Waring: Against the ranger's car in federal territory.

Franklin: On a federal reserve.

Waring: Other than that, I couldn't see anything wrong. (Group laughter)

Levno: I think there were some gestures (Group laughter). I also remember Bob Burns strongly suggesting to Jerry that he go up and shave one morning.

Dyrness: What?

Franklin: No kidding? I don't remember that.

Levno: He did it! He got up and shaved! (Laughter)

Waring: Jesus! He'd given up on the rest of the group!

Franklin: When he left, I recall we had a campaign that stated, "When you recruit a new ranger for this district, recruit somebody sympathetic to research!" And they did.

Waring: They did.

Dyrness: That's when Steve [Eubanks] came?

Franklin: No, there was Jim [Caswell] first. Jim was pretty positive, but he wasn't as pro-research as Ranger Steve [Eubanks].

Dyrness: What year did Steve come?

Dick Waring: Oh.

Levno: He was much later.

Waring: During the LTER.

Swanson: He was here in '89, I think.

Franklin: Not '89, much earlier than that.

Waring: He was here in '83, not 89.

Franklin: That sounds right.

Waring: Does any of this help?

Geier: Well, actually, one of the questions I was going to ask was about the impact of IBP on management-science interactions?

Levno: Got there in a hurry, didn't we?

Geier: From what you're saying, it sounds like near the end of IBP, there was an obvious need for adjustment there. Is that accurate?

Waring: You mean when we closed up the Andrews?

Franklin: Yeah, there was there was a lot of friction between the National Forest [Willamette] and the Andrews, most of I'd say, all the history of IBP.

Waring: Oh yeah, yeah.

Denison: I think it was a grad student that suggested that to Forest Service employees that they re-calibrate their truck speedometers.

McKee: That was you. You wanted to convert it to the "stoned furlough fortnight."
(Group laughter)

Waring: In order to match their attitude?

Franklin: Yeah. There's a whole social dynamic associated with this. There was the normal friction between a can-do management organization that likes to have things buttoned down pretty tight, follow the rules, and this laid back bunch of scientists. Bad enough when they're your own people, but when they're a bunch of university people, then it really gets bad. But in a broader context, it was also the time when science was beginning to emerge as something "dangerous." There was this context in which the normal friction was just sort of accentuated, because these scientists were beginning to find out things that were real problems. And the clearcutting controversy had emerged.

Waring: Culverts, roads.

Franklin: And these --

Waring: -- Spotted owls?

Franklin: These scientists were appearing in forests.

Waring: No, the spotted owl wasn't there.

Denison: A ways off yet.

Waring: We didn't have any spotted owls. Well, we denied it. (Laughter) At least for a while, there weren't any owls in the forest. There was no wisdom in residence, right?

Franklin: But that wasn't unique to the Andrews. There was that kind of friction, extreme friction, at the Wind River Experimental Forest. Wherever you had researchers in direct regular contact, and with joint responsibilities, there was always, almost always, friction. And that was true of all the experimental forests.

Swanson: That's an interesting point because Virginia Andrews Burns, H.J. Andrews' daughter, sent a box of interesting papers about so thick [showing width with hands], which includes speeches and other presentations that H.J. had given. And then a series of letters including a handwritten note from William O. Douglas --

Franklin: No kidding, really! Wow.

Swanson: -- On the occasion of his death. I should have brought it over. It seemed to me, H.J. [Andrews – the man] was operating at a time when the Forest Service was switching from conservation to commodity production. The man was pivotal at that point. He was interacting with industry a great deal, sort of a gearing-up process. In Max's early chapter, which is built around Roy Silen's work, and what Roy was doing [developing forestry system for "Timber Era"], we just looked at documents of the early days, like the establishment documents for the Andrews, then the Blue River Experimental Forest. It's really interesting. The first piece of road was going in with support from the Federal Housing Administration, because it was to go in and get wood to build houses. It's like the Andrews Forest came on board the second half-century of the Forest Service, to do this job and H.J. [Andrews-the-man] was helping engineer the development of "Timber Era" forestry. Roy was part of that, too. The science assignment, which we wouldn't call much of that science today, was really to contribute to this development. The Andrews seemed like it was working towards those objectives for a couple decades. The IBP era was a real switcheroo for the experimental forest, which set the stage for science for the Andrews, the scientists and their manager colleagues, to participate in the major transition that's been occurring this decade. That's sort of a hypothesis in terms of what the history has been, how the Andrews program people, including H.J.A. himself, figured in this history. I think I'm resonating with Jerry's comment about the role of the '70s in the way the property was addressed, the way the program changed to set the stage for big change.

Waring: There's one other thing we sort of make fun of about the models that were initially developed because they had, you know, with 400 separate values, and you couldn't measure them except once in your life, and only in one place.

Franklin: They produced six thousand pound cows and -- (Group laughter).

Waring: Right. Consider the "spherical cow," a postulate from some -- (Group laughter). Yeah, it simplifies the model. One thing that came out near the end and was published

in '82, but obviously was written back in there, was a cross-comparison of hydrological models for Coweeta, and, I forget the place in Arizona --

Franklin: -- Beaver Creek. [In Verde River watershed south of Camp Verde, Arizona.]

Waring: Beaver Creek and the Andrews. Those models routed water and they predicted streamflow within the accuracy of measurement on the wettest and driest years across a whole range of vegetation types. And those kinds of models were the beginning of a very dangerous thought, the thought being that if you develop general models, and test them widely across the country, you can make predictions and understand what's going on. And you don't have to do continuous small studies everywhere. You have to do them broadly and you have to do them well. From those kinds of modeling successes, because those were fairly decent successes backed up with all kinds of other measurements, the climate change implications came in. First, the hydrology, then, the primary production, and then, concerns about nutrient cycling and whether that'll feedback. But without some success, and almost all initially was in hydrology, because it was the easiest. It was the cleanest part of physics. A big discovery we made was that big trees operate differently than small trees. As soon as we plugged in the stuff for small trees, and used up all the water in four days, so then we had to go back. That really made people think differently. Just because you're working here, you begin to extrapolate through these models and test them elsewhere. That's a very, very powerful approach. I think it certainly came from the IBP. I'm not going to say that the IBP models were by themselves stand-alone wonders. I'm thinking, for example, of sediment transport, these kinds of things, came into play.

Swanson: In thinking about Jerry's comment of scientists being "scary," I didn't have much of a sense of how that might have operated in the '70s, but I do in the '90s. It makes me think that our relationship with our manager colleagues is probably really important. That Lynn Burditt can stand up there with us, and she's a pretty traditional person in a lot of respects, and she can be friends with us and work with us, and we can come up with stuff that make a lot of problems for her and her kind, I think that's super important. I hadn't thought about it this way. It just seemed to me that our long-term collegial working relationships with our manager partners have been really important, and to model that kind of behavior, despite all the problems we've created for them. That may go a long way, because we could easily be in very severe conflict.

Dyrness: Oh, yes.

Swanson: And we've picked it up in some other settings. I think we're doing pretty well because we've got collegial working relations with a bunch of real good folks.

Dyrness: Yeah. I just couldn't believe it when I came back in '90, and attended my first LTER meetings, that Lynn Burditt was there. It would never have occurred earlier. I think before IBP, what occurred was benign neglect. They would just ignore us. We did

our own things, and they did their things, and never the twain should meet. I remember one time, the district ranger stopped by a plot site. That was the first time that happened. "What are you guys doing here?" I'll never forget it, because it was so unusual. With the advent of IBP, there was actually animosity. The number of people, and they were doing strange things, going off in the middle of the night and....it was just weird. It developed into animosity. That's why it's amazing with Steve Eubanks adopting, adapting some suggestions and cooperating; a real sea change. I think a lot of people did the right thing to get together and treat 'em as colleagues. It's amazing. It's just been a big change.

McKee: Part of that process started with the first proposal establishing the Andrews as a National Field Research Facility, in which, Jerry and I suggested we have two advisory committees, the local advisory committee and the national advisory committee. The local advisory committee was going to have people on it from Willamette National Forest, the district ranger, and somebody from the forest supervisor's office [Will. NF]. The intent was to get them involved. A lot of what had been going on before spun out of ignorance and the creation of stereotypes. That was a major force in turning that around. We persuaded Mike Kerrick to make liaisons with research a component of the district ranger's job description, just on the transition between Burns and Caswell. We went back to him, saying, "It was great, you were doing a good job with Caswell, but he's moving on. We would really like to have somebody who's enthusiastic, if that's at all possible." Steve [Eubanks] was ready to go. He made a lateral transfer, not a promotional transfer, to take the position, because he wanted it. He wanted to interact with the research community. For better or worse, the reasons he wanted to get involved was that Chris Maser had brainwashed him about what a great group there was down at the Andrews.

Franklin: Well, Chris was still a functional member, though somewhat marginal, but he was a functional member of the group still at that point-in-time.

McKee: Yeah.

Waring: He was still working for BLM at that time?

Franklin: No, he wasn't. Well, yes, he was, too.

McKee: '83 would have been the last year.

Waring: He went down to Arizona at some time, for a couple of years.

Franklin: Did he?

McKee: Yeah.

Waring: Yeah, he quit, and he was into consulting and --

McKee: -- He worked for EPA in Las Vegas.

Franklin: Yeah, he went to Las Vegas, and that's where he met his current wife.

End of Side B, Tape 1 [of 2]
Beginning of Side A, Tape 2 [2 of 2]

Interview with the H.J. Andrews International Biological Program Group, Pt. 2,
10 am, February 10, 1998, Siuslaw National Forest Headquarters; same group.

Swanson: -- [picks up mid-sentence] I read some documents concerning the Andrews' history, early parts of what Max has been coming up with and producing from his studies. So, I thought it'd also be good for us to do some "futuring" this year, to capitalize on our historical perspectives. I was wondering if we could spend an hour to talk some about that, about how we might do it. Then we might exercise our brains a little bit and do some of this. One thing intriguing to me, is the folks here who have 20 to 30 plus years and a sense of the past. And I ask, is that at all relevant to thinking about the future?

Franklin: Nah. (Group chuckles)

Swanson: If we want to think about where we're gonna be in ten years, or where we would like to be in ten years or twenty years, I'd like to balance that a little bit by thinking where we were ten or twenty years ago. What might have been predictable, and what might have not? Is that an okay agenda for people? To go for about an hour with more IBP stuff, and then, about an hour of futuring and then call it closed. Is that okay?

So, we'll see how it goes. We may be sort of eviscerating before three.

Geier: You guys covered a lot of questions I had prepared in this ad hoc discussion that was going on. It's been, from my perspective, really productive so far. I did want to focus on a little more on things Art was alluding to, that is the transition from IBP to what came after. My understanding of the IBP is that people became part of it, and worked through the IBP with the knowledge this was finite in term, there was an endpoint, although it was sometimes vague where that endpoint was. I was thinking of a two-part, or two-point question. One, how did that foreknowledge the short-term, finite time-frame, influence research goals and priorities. Or did it? The second question; as that endpoint is reached, how do you make decisions about how to downsize or scale back? What is saved and what's thrown out? Maybe take the first part of that first, the impact of working under those constraints, where you can bring in a lot of people and transform the place and the community, knowing that it's not going to be a permanent transformation?

Denison: People were reluctantly transformed. Once they were transformed, the resources were not available to follow up on it.

Geier: Was there pre-planning for that? How do you cope with that is what I'm asking? As a scientist or as just a member of the community trying to design research projects, do you bring on graduate students knowing that funds may run out before they finish their work? How do you deal with that?

Denison: Well, my situation was maybe unique, so I'm not sure that's germane. Simply because of my departmental situation, my recollection is that there were a number of my colleagues who had gotten fired up and were excited about it. Partly about the fact that there was money to be had there, but also, for me at least, and I think for others, there was the lure of interaction across departmental and disciplinary lines. That possibility proved to be very naïve, in both short-term model development and more or less instant sharing of data as an early selling point, if I recall. To have that evaporate was a blow.

Waring: I think we were in a fortunate era in the sense that almost all graduate students and post-docs in that system, had ample opportunity to leave at any time. Some left early, like Chuck Grier, right in the middle of when we needed him, and took jobs. It wasn't a problem that you had to hold them, because there was no place else to go. Another thing that came up was ecosystem analysis began to have its own panel at NSF. People, even before LTER, had an opportunity to go in and focus on a smaller part of what they'd been associated with, but still often in a modeling framework, and at least, have a justification of why that was important. We saw a lot of grants go in, and people had learned to write grant proposals of some merit. And there were people who were used to reviewing them.

Denison: We don't really know when or where --

Waring: No.

Denison: -- Where you draw the line, the end of the IBP era, or end of the post-IBP era.

Waring: Right.

Denison: There was probably a three-year transitional period in there somewhere. It's that period that I recall with some degree of sadness.

Franklin: Well, I saw it from the national level because I was program officer [at NSF] in that transition. There were some pretty scary times. If you remember the year before I went in [to NSF], Tom Callahan and the program officer were running around and telling everybody, "The money's gonna end." [for IBP]

Waring: Yeah.

Franklin: “So you better start telling all your post-docs and everyone to find other jobs.” I was running around behind them saying, “No, it’s not gonna end!” (Group laughter) What happened, was that IBP line items rolled over and became ecosystems studies [EER, or Experimental Ecological Reserve, name used between IBP & LTER, 1977-79].

Waring: So, a lot of money was still in the pool.

Franklin: Right, that really made the whole thing manageable at the national level. I saw the transition that occurred in the Andrews, and maybe again, I’ve lost a lot of it in time, but it was an amazingly easy transition. What happened was, in IBP, we developed a whole lot of good ideas, then people were able to develop more traditional kinds of proposals, that is, hypothesis-based proposals as opposed to budget-based proposals.

Waring: Three or four people it was not. We had a hundred people at one time on the payroll. Okay? One hundred people.

Geier: Is that right?

Waring: Yeah, that’s right.

Franklin: Then basically, those ideas became the sons and daughters of IBP, the next generation. Some of them were successful, some of them weren’t so successful. Individually, I was particularly concerned about the canopy stuff – and that I wasn’t nearly as successful as a program officer to keep that up, but overall, it was amazing the volume of successful proposals that the Andrews-based group was able to develop. Then, the only problem was, how are we going to keep the infrastructure going? And this was at the time that we put together the first facility proposal. We sort of took advantage of NSF’s ignorance, and captured a lot of money that they wouldn’t give to people today.

McKee: I remember going to a meeting in 1980, a meeting of directors of biological field stations. We were asked to present funding sources for different field stations, and I had a one-page handout that contained financial support for a national research facility, a couple hundred thousand a year, whatever it was. Man! That was like tossing shit into the fan. It got people really pissed, because they didn’t know where the hell we’d gotten the money for that. There was no program for that. Where did that come from? Jerry knew the window was there to move on, so we sent it back to NSF and we did it.

Waring: But then, there were 18 original LTER sites.

Franklin: No, it started with eight.

Waring: That's right.

Swanson: I thought it was six or seven, then it went down to --

Waring: You have a better memory than we do!

Dyrness: All we worried about was that the Andrews was in there. There were no other biases. (Group laughter)

Franklin: Anyway, it didn't seem that way at the time. It cost me a lot of sleep and a lot of worry, and I'm sure for other people, too. But basically, we went from IBP to an intermediate period where we generated a whole bunch of small group grants, the first facilities grant, and then into LTER.

Waring: Right.

Franklin: So, the thing was able to maintain its momentum and its continuity.

Dyrness: What year did IBP finish? What was its last year?

Franklin: 1975. Fiscal year '75.

Denison: Was it really? Did it run so long as that?

Waring: Oh, yeah.

Franklin: I went to NSF in '73, and we would be planning the transition then for the next two years. Basically, the transition had been made by the time I left in '75.

Waring: Yeah.

Denison: So, somewhere in '74

Dyrness: Yeah.

Swanson: Another thing I picture you two guys working together on, was to get most of that post-doc cadre into some more permanent positions.

Waring: Yep. That was part of our responsibilities.

Swanson: That's sad. (Group laughter)

Waring: How else are you going to corrupt the system?

Franklin: We were lucky, but we had a good track record. We had a good group of people, so we were able to maintain continuity. The fact of the matter is the program grew in terms of the total dollars that were coming in to the Andrews. When IBP ended, it didn't drop, it just sort of continued upward in terms of the total budget of research directed to the Andrews.

McKee: We had one phenomenal streak in '76 to '81 or so, when I don't think a single proposal was declined for funding from the group we had.

Franklin: That is amazing!

McKee: It was just --

Dyrness: Not a single one?

McKee: Not a single one of the ones that were going in.

Dyrness: Quite a record!

Swanson: There was also the Forest Service funding that was sort of chunking along and I don't know if it changed, but probably was staying pretty steady.

Franklin: It got pretty corrupted about that time, because when I came back from NSF, I was made project leader. Bob Ruth did a retirement, so I then had all the project resources that I could utilize. (Group laughter)

Waring: And Bob Tarrant was the station director [PNW], also.

Franklin: That's right, it's about that time.

Swanson: While Tarrant was director, I think he gave the okay for hiring a few people through a hiring freeze.

Waring: While he was shedding entomologists, right and left.

Franklin: That's right.

Waring: That's a sensitive issue, or it was. For moving them to, Wenatchee, or -- ?

Several Voices at Once: -- La Grande. [in NE Oregon]

Waring: La Grande, right.

Geier: I gather there was kind of a dual sense here. On the one hand, there's an impression that this phase of research was ending and that some people at least are going to be ushered out. But from a budgetary standpoint, what you're saying is that the money was still there, but it was going to be different people in some cases?

Franklin: Yeah, and it was coming in different kinds of packages. It wasn't coming through a central program, but rather you had a half a dozen significant research proposals, rather than one grand, big one.

Waring: That's right.

Denison: I think Dick's point was, at the height of the early IBP there were a hundred people in the program. The people that I'm thinking of are the ones that didn't make it through this transition, and probably in most cases, that was either wise from the Andrews' perspective, wise from their perspective, or both.

Waring: It was still hard. I didn't mean to gloss over that. I think it is instructive that, at least for a decade after the IBP, almost none of the other forestry colleges in the country were able to get NSF grants, sometimes even when they had a graduate from our system, like Bob Boloet, who went to Michigan. They just didn't have, what do you call it, the --

Dyrness: -- The critical mass.

Waring: The critical mass to do integrated science, even though they were ready and anxious to do it. Henry Gholz, of Florida, it took him until Bob Tesky, and much, much later, like ten years later, before they finally got a big grant to do it down there, but otherwise, there wasn't any. Berkeley [Univ. of Cal.] was really a nice case, because they all fought one another, and they never did get a program going. You know what the resources are at Berkeley. So, you just look and say, "Well, how could this have happened to Oregon? How could Oregon, before they had molecular biology, get these kinds of grants? How could the biology be so strong here?" And, it wasn't just the original faculty. Because a lot of people like Bill Nagel, left, and we lost a lot of faculty.

Franklin: Oh, yeah.

Waring: But it was this experience. I think of being able to write grants, do it together, and do it on deadlines. The deadline was that we were going to cut Watershed 10. Okay?

Dyrness: Speaking about depression! (Laughter)

Waring: Yeah. And partly, not just to check the predictions. Jesus, like more water's gonna run off, and, surprise, surprise! But it was that we wanted to end the data gathering, and that did end it. I mean, on the IBP budget [continued on USFS budget].

Franklin: We also needed to cover all the impacts we'd had on that watershed, and the only way to obscure all of our sins was to cut the damn thing.

Denison: I still remember when I was called up. I can't remember now, whether it was somebody from Forest Service or whether it was the timber company, and that was

when was I going to remove the bolts from the eleven trees that we had climbed, and I asked what the pay rate was for us to go down and pull them out. (Group laughter)

Franklin: Ohhhh. (Laughter)

Denison: They're still standing.

Waring: The trees are still standing? No. Not unless you're on Watershed 10.

Henshaw: No, Watershed 10. Actually, you had trees.

Denison: One of our trees is still standing, one sugar pine we did, which is still there.

Waring: Okay.

Denison: Way up at the top of the edge of the watershed. The rest of them all went down.

Waring: Okay. I stand corrected.

Swanson: One thing I was wondering about is the transition out of IBP, and the way science leadership within the group transitioned. I don't have a good sense of it. I think it was substantially dispersed during IBP, although money went through one funnel for the most part. Then, with this move towards lots of different grants, then those teams became somewhat more autonomous, didn't they? How do you see that?

Waring: I'm sure that happened by the time the LTER started, because if it was before then, they had to compete independently. I think our big concern was how to keep the data bank together. I mean, we had lots of other concerns, but we'd be in big trouble if the data bank was destroyed in this interim period, and we weren't going to have this legacy for everybody to build on. That was about the time Susan Stafford came on. I think that was '78 or something like that. That was pretty important.

Henshaw: A little after that. Well, she might have come in then, but I don't think it really changed over until she got involved with the FSDB [Forest Science Data Bank].

Waring: I think you're right. She was hired as a faculty member in '78, but she didn't take this other responsibility until a little later. She was teaching.

Henshaw: I think it was after LTER was on board, after 1980, was when it switched.

Waring: It was a problem for how to get money out of these separate grants. How are you going to support this infrastructure when each grant needs support and has a little bit of money? That's when the college had to start talking about databanks and this kind of stuff. At least they talked for about a year. And Al Brown really wasn't cut out

for that; those kinds of interactions with all the administrators. That's where Susan helped us, because she was able to do that.

Henshaw: Al Brown, I don't know what happened. He left, I think in about June of '80?, or was it '79? One of those years.

Waring: I think it was '79.

Henshaw: He worked for a long time in Portland.

Waring: And we had Dennis Muscato after Al Brown, I think.

Denison: Al Brown left a different legacy here I'll bet none of you are aware of. After the episode where the Memorial Union refused to give the Black Student Union access to the Memorial Union, Al came to me and said, "We need to do something about this." So, I became faculty advisor to the Association for Conscientious Thoughtful Students, which developed a structure that allowed for use of facilities usually available to student groups. And the first organization to take advantage of it is presently called the First Alternative.

Waring: Oh, the co-op.

Denison: So, that's where the co-op came from. Now, this was a very laid-back group. Any person who wanted to become a member could go over to the student center and simply sign up. But there was a sunset provision, so you had to renew every year. Any three members of the organization could establish a troika, and act on behalf of the group as a whole, and that was how the co-op came into being. The second and only other activity was the sponsorship of a foreign movie festival on this campus, and then it quietly went out of business. But it didn't --

Waring: But it did need a troika. (Laughter) It did need a troika.

Denison: Yes, it did need a troika.

Waring: Al couldn't have done it alone.

Denison: I don't think Al had anything to do with that. I think somebody else found out about the existence of the organization and figured this was the vehicle. I wasn't consulted, that's part of the reason why it went out of business.

Geier: I'm a little bit curious about the transition from IBP funding, this funnel that kind of focuses upward, as Fred was saying, to multiple grants. What are the implications of that for ideas like long-term research? I don't get the impression the study elements of IBP were focused on long-term research, but more on data-gathering and modeling.

Franklin: Well, there were a number of things that were working. First of all, most of the people were working on ideas that were second or third generation ideas at IBP. There'd been quite bit of integration that just continued, but it continued on a voluntary basis. Second, a lot of it was focused on the Andrews, so you still had a geographic focus. So you had history, you had geography, and then you had the fact that most of the people were interested in continuing to work with each other. They didn't care whether or not there was a fiscal relationship. The fact was, it was fun, you know, to go out and splash around in the creek with Fred and Jim and Jerry, just swap ideas, and "create fairy tales," as Sedell [Jim] would say. We really enjoyed each other and were stimulated by each other. I look back on my career, and almost anything worthwhile that I've ever been associated with, was a consequence of interactions. It was an opportunity to put things together. One of the things that I really do well is to put things together, but I have to be exposed to ideas and data to do that. And so, history, geography, and just a desire to continue to be associated with one another, and the notion that sooner or later you probably were going to be collaborating with somebody on a research project. That just provided a natural transition, a bridge to LTER, where once again we could have a central event that would provide a lot of the continuity and structure.

Waring: There are instances, also, of very large databases like our tree database, for example, which we knew we had not begun to mine the information out of it.

Franklin: Yeah, sure.

Waring: So, preservation of that, and it eventually that becomes historical, any of those becomes historical data-banks, as well. So, I think that may have been looked at as an asset, and that came into the picture.

Denison: I would say there are two things that are a legacy, not so much of the detail work. One was having to meet deadlines where it didn't go on like Forest Service projects, for 20-30 years. It didn't go on like a university project where a graduate student graduated, and then maybe another one would pick it up or maybe they wouldn't. It was something more focused than either one of those extremes. To do that you had to have colleagues willing to come to the table with something to make a commitment, because integration only happens if everybody plays their part. If only one person plays their part, the grant didn't get renewed. I think in the IBP, people learned that. That's true, because they could see the rewards for doing it and the penalties for not. The program wasn't always fair in how it ended, but it did end.

The other thing, I see a bunch of other agencies, or maybe at least a couple other agencies, that picked up on this. The oceanographers started this. You have a ship that goes to sea, and you have to be on it and ready to make the most of it. We never had that in the terrestrial kind of people, because we could always go there any time and

wouldn't have to be transported in a group. Well, now we're talking about simultaneous measurements of some of these things. Handing the branches down that make these measurements after you've been up there collecting them. It's not a random kind of thing, and in order to be successful, you have to coordinate. I think that kind of coordination and what comes out of it, was an experience that not all of us had had before we came into the IBP. It sure changed the way that research was done, and I think, still is being done.

Swanson: Yeah, there was a neat feedback back there, that we had a competitive advantage by working together. We were getting feedback and success. It would be useful to be using this gathering to try to carry the discussion all the way to the instigation of LTER, which gets us into the beginning of the 80's. Lots of things were happening. There were planning workshops, for example. I expect you guys were probably involved, and Jane Lubchenko and maybe Risser [Paul], at the national level, that helped carry things from the IBP era to the LTER era. Would you comment on that, please?

Franklin: That transition actually began about 1973, and it began with a concern at NSF that originally had to do with field stations. Some of us then transmuted that to a concern for how we were going to keep the centers of excellence developed under IBP going, and what kind of structure would allow us to provide long-term support for these. That's where the LTER grew out of, with its roots back in the early 70's. At least in some minds at NSF, we were thinking, "We're getting some real centers here. When IBP ends, how are we going to assure with our current funding structure that we're going to be able to maintain these centers of excellence?" That was how LTER fundamentally came about.

Waring: It wasn't so much for the field stations, because we'd seen field stations come and go.

Franklin: No, the field stations --

Waring: That wasn't it. It was the critical mass of people and the kind of research that we were building on, and how to see that through. That was the real concern. There were lots of great places you could have done research, but they weren't potential LTER sites. And not all the potential LTER sites still had a critical mass of people that were willing to build on what they previously had.

Geier: How much correlation is there between IBP sites and current LTER sites?

Franklin: Well, there weren't that many IBP sites, but 50% of the IBP sites are represented today in LTER, maybe. What do we got?

Waring: We've got Wisconsin.

Franklin: We've got Coweeta, we've got Wisconsin [Northern Temperate Lakes].

Waring: We've got Short Grass Steppe [Kansas].

Franklin: And we've got Hubbard Brook [New Hampshire].

Waring: Hubbard Brook, that wasn't, or maybe it was?

Franklin: That wasn't an IBP site?

Waring: No.

McKee: It was a satellite site along the lines of the Jornada [New Mexico] satellite site, and the grassland biome, and --

Franklin: -- the Andrews, Central Plains, the Pawnee, Jornada.

Waring: Was there a tundra?

Franklin: Where was the tundra?

Dyrness: Yeah, there was a tundra, wasn't there? Jerry Brown [PI]. That was it.

Waring: That was Jerry Brown.

Dyrness: Jerry Brown's outfit was out of Eagle Research at Barrow [Alaska].

McKee: Yeah. Some at Barrow and other places within the Tundra Biome.

Franklin: Well, I think what you do see in the LTER sites is a surge of excellence, some of which had been IBP sites, and some of which developed in a different venue like Hubbard Brook and Luquillo. They are well represented in the LTER sites. So, in that sense, the vision that some of us at NSF had in 1973 has sort of come to pass.

Denison: In connection with LTER, which I've had no connection with, have people gone and taken a look at Rothamsted? [United Kingdom]

Franklin: Yeah. In fact, the Cedar Creek guy, David Tilman, took his sabbatical at Rothamsted.

Denison: I had an indirect family connection within the IBP itself, in that my sister was involved in developing the first translation of that. So, I came to it with some experience of what modelers could and couldn't do, which was part of why I was interested. My oldest son was at Davis [Univ. of Cal. at Davis], working with profs and students on modeling projects. My impression from Rothamsted was, that despite the

physiological research there, the most valuable information that's come out of it has been the archival tradition, that changes taking place in systems has greatly reinforced the importance of keeping good records. An interesting perspective, which I don't think has been resolved, is that before starting out on hydrological and traditional soil studies, getting documentation for the initial conditions, was relatively easy. But the issue of documenting what was there in terms of invertebrates and microbes, there's no real precedent for, and I don't there's a good one yet. What, if anything, is being done as far as the Andrews is concerned, to document changes in the really small critters? Some of these reference stands have got macro-fungi, and we've got information for some of the small stuff out of the canopies, but there's an awful lot of other stuff in the soil.

Franklin: Well, we've never done any kind of archival job on the Andrews. That's a challenge we've never picked up. [Program records were curated, 2013-2019]

Waring: We started with litter collections to archive some of those. I don't want to say that we did it right, but on some of the reference stands, there were litter-fall collected, and some of that was archived.

McKee: We're still maintaining the archives.

Waring: Reference Stand 2 was one.

McKee: We have half-a-dozen sites with collections of one kind or another.

Waring: Right. Our herbarium archives have some of the stuff, but that was only with lichens or mushrooms or some of that sort of stuff.

Franklin: But, fundamentally, we really haven't done archiving. And if you want to look at an organization that's done archiving, you'd look at Hubbard Brook, because that's something that Gene Likens was really committed to. There have also been soil cores done from a cross-section of the Sierras that are archived at Berkeley. They've become very valuable to people who've been looking at the turnover rates of bomb carbon, making those comparisons, because without that, they didn't have initial data.

Levno: I think one thing else that's carrying on from IBP into the LTERs, has been [U.S.] Forest Service contributions. There were periods when things look pretty tough for them and the experimental forest idea. Logan Norris was instrumental in diverting a bunch of pesticide money when he was project leader, for watershed programs, to keep them going. Fred has done the same thing, diverted money when it's not popular to do so.

Swanson: There's been a lot of resources now, like with Don [Henshaw] and Hazel [Hammond], as Forest Service employees, who are the ones who are really doing the bulk of the heavy work on the webpage and the databank, which have an LTER labels on

them. So, this is an interesting blend of the continuity and some inflexibility maybe on the Forest Service front, because it's the academic front that has more flexibility.

Geier: I was wondering in relation to the earlier discussion about operating paradigms, and also, IBP modeling, filling in the boxes. How had that paradigm of research changed by the time of the transition between the end of IBP and the beginning of LTER? What were the operating paradigms which structured the kinds of research that would be pursued at the Andrews? [Going forward into LTER era]

Franklin: Well, it switched. Basically, the IBP was structured around doing budgets, doing fundamental descriptions. By the next generation, they were almost all hypothesis-based proposals of some kind, or question-based proposals, having to do with pursuing some interesting facet that emerged from the budget work.

Waring: And there was a fair amount of experimentation coming in to complement the observations. You saw Phil Sollins' people moving litter around in an old-growth forest, and then monitoring leaf shade coming out under different things, but nothing else changed. Other experimentation included putting logs into the stream or forest, putting different kinds of sub-strata in the streams, and watching how rapidly they decompose, with or without insects first working on them, and the microbes first providing substrate, and that kind of thing. Maybe some of that came in the IBP. I don't want to rule that out.

Swanson: I get the impression that the long-term aspects snuck up on us a little bit, so in the second half of the 70's, which is sort of the post-IBP era, we made the switch from the budgets, filling in the boxes in the budgets, to more hypothesis-driven research. It wasn't yet long-term, but it was at the individual NSF grant-scale of time dimension.

Waring: Three-year's worth?

Swanson: Yeah, but then, LTER came on, and that was.....we were writing a proposal on that, in '79? Early '79?

McKee: The RFP [request for proposal] came out in '79, and the proposals were going to be submitted by March 1 of 1980 [For what became "LTER I"].

Swanson: I remember being in the FSL [U.S. Forest Service Forest Sciences Lab] large conference room, and I think Dick was there. He said, "Okay, the way this is going to work, you need to design long-term experiments." That was the key.

Waring: So, we had a 200-year old log study. [Mark Harmon's log decomposition work]

Franklin: Yeah, right! (Group laughter)

Swanson: There were several of these experiments that were developed. And it's interesting, you look across LTER today, and some of the sites are firmly rooted long-term in field experiments, others less so. The infrastructure for long-term research has really been a growing part of LTER, it seems to me. So, it seems like the operating premises were switching [between IBP and LTER], and yet we had some threads that weave all the way through them. Some of them were maintained by the Forest Service like the small watersheds or the vegetation plots, and there may be some other things like those logs in streams beginning in '75, which were more of an OSU responsibility.

Waring: Well, the landslides.

Swanson: Yeah, landslides.

Waring: I mean, doing a whole survey after a flood, and then waiting and waiting and waiting for a major storm to finally happen. Or having to dig wheelbarrow loads of sediment out of sediment basins in our watersheds.

Swanson: Yeah. Ted set the stage for that.

Waring: The good news is we had a storm, the bad news is that basin was filled with silt.

Swanson: Is that a fair characterization of the research premises?

Waring: I think we had some other monitoring, too. Maybe we were forced to do it, but we were supposed to be looking at wildlife, and about that time we had, we had the owl [northern spotted owl] listed in 1974 -- [Endangered Species Act]

Swanson: Oh, neat!

Waring: [continuing] -- in these things [indicating IBP "gray literature" publication], so the spotted owl is listed by Nussbaum's article. It's not that it wasn't recognized before.

Henshaw: That's been a great resource in going back and trying to document old data sets; that whole series of reports created in the IBP, that doesn't exist in LTER at all.

Dyrness: What's the title of that?

Swanson: The "Annual Report." [IBP publications, series by subject/site/biome.]

Henshaw: Was it annual reports, or --?

Waring: It was number five, it says here.

Swanson: It was "Integrated Research in the Coniferous Forest."

Dyrness: Oh, okay.

Henshaw: That's one thing that was not ever carried through in LTER. That's a really valuable collection of information, contained in those internal reports.

Waring: That was great literature we'd get out in a hurry in lieu of referenced journal articles.

Henshaw: Right, and I'll assume that you may not have gotten the credit.

Waring: Hey, we got funded. Did a magnificent job of getting us funded. (Laughter)

Dyrness: You get things out faster that way, you know?

Henshaw: Yeah, but a lot of stuff that got printed, that's normally like documentation of things that you would never see in a referenced article. There was a lot of documentation that was actually important.

Dyrness: There's limitations on length and all that.

McKee: Colorado's Niwot Ridge site had a series of this kind of grey literature report. I don't think they did them very long, but for the first several years of LTER, they'd give one to each of the other sites.

Franklin: This has probably been replaced now by on-line data sets.

Henshaw: Yes, I think so.

Franklin: And we document our data-sets with metadata.

Dyrness: On the web site.

Franklin: You know better than I that you're gonna see a lot of that.

Henshaw: Now there is a proliferation of the grey literature again. But there was a long period when there really wasn't much.

Franklin: No.

Henshaw: Those study plans we talked about? Like how the Forest Service always had done that, and those didn't exist. And other types of reports that we have.

Franklin: Establishment reports. [U.S. Forest Service manila-color repts., 1920s-80s.]

Henshaw: Establishment reports, and things like that. I think there's a period in there where that wasn't happening, I'd say, throughout the '80's.

Franklin: I'd just kind of like to underline again what Al said about the continuity provided by Forest Service, because I think that was very important. We tended to focus on the NSF grants. And it's the base that's been there. The willingness of people like Fred and I to use that base to keep the whole program going, that's really made a difference. The majority of the LTER sites are collaborations of academic with agency scientists. So, it's not a pattern unique to the Andrews. The other thing I just wanted to mention is to go back to the issue, "What's kept us together?" I think another thing has been our shared commitment, belief in the value of our science to policy, and to consistently focus on getting this stuff into practice. I think it's made a difference.

Geier: You're talking about links with management in that area?

Franklin: Yeah, in point-of-fact, we really believed the science we're generating is important, and it really ought to be incorporated into EISs and policies.

Waring: President Clinton's Plan.

Franklin: President Clinton's. In the late '70's and early '80's, we were very frustrated because we weren't being involved, and a lot of it [science] was not being incorporated.

Pause: [Jim Hall leaves, Ted Dyrness thanks him for coming.]

Franklin: And of course, what happened in the late '80's and '90's, was that we did get incorporated along with a lot of this stuff. The commitment to application has been something that's kept the group together, whether it's us here or Jim Sedell or Ken Cummins or Stan Gregory. I mean, we all shared that.

Swanson: Yeah, I think that's true. And I think having these very positive interactions with land managers really rings true.

Waring: The only way to test these things is to have them actually applied in science, and to see what happens. Make these predictions and you think this is the way the system works. So, we really needed those people, as much as we hope they eventually needed us.

McKee: But at any rate, we sit around the table and nod our heads in agreement, there's unanimity on this issue, and yet at other sites, not so. I can remember in the early '80's talking to graduate students at the University of Colorado, and many of the graduate students really felt good about the possibility that their work would be incorporated in management. And there was this awkward silence.

Waring: Many?

McKee: Like, “Then you must be doing applied research.” I had a conversation in the early ‘80’s with Tilman, who’s now getting a lot of press about his research seeing the importance of biological diversity and stability with climatic shifts, talking with him about how much of our work is spinning off into management. And the sneers, you know, it was like that in the ‘80s. Other sites don’t sense this value and this group [HJA] always has. It really has. There’s a big difference between this group and any other site. It’s funny how often we’ll get requests to provide information with examples to NSF.

Waring: Application.

McKee: Applications of the basic research. How is this germane or relevant to the nation’s interests? Boy, there was a time when if a proposal even hinted that it was headed in that direction, it was the kiss of death. (Laughter)

Waring: There’s an interesting outgrowth of that. One of the people we had working as a teenager, and also as a master’s student graduate is Steve Running, during IBP. After he got his Ph.D. at Colorado and went to Montana, he eventually became the most richly- rewarded faculty member in the state, doing a one million dollar a year NASA grant for ten years. And NASA, in order to support those kinds of [satellite] systems that had to go up and gather these data, more and more relied on Steve to explain the applications. It wasn’t just that there were hydrologic models and they predicted photosynthesis, but he could show for the entire state of Montana, that he’d been able to predict the forest capabilities in all landscapes. It turned out it was a grant from the Montana Tax Bureau. So, people were able to assess land from these maps. If you were or were not producing, they could tell. Steve was basically once or twice a month going before Congress. The history of where he got those ideas of modeling stem from here, because we were modeling here. The application part – the scaling up applications – came from his experience here. He had a botany degree originally, and then he came in with a masters and Ph.D. in forestry, but I don’t think he ever took forestry courses.

Geier: How much of the unique stuff from this group here originated with the management of the Andrews, or was it by the Forest Service with Oregon State University? Was that unique in terms of other IBP sites, or LTER sites?

McKee: You mean the interest of having interaction with management? I don’t think it was solely with respect to management of the Andrews. It’s always been important, because, at least my perception, was that there were people within the agencies who had these interests. I would include, in my experience, the EPA, in those days, and AES, the Agriculture Experiment Station-USDA. There was a fairly high degree of academic-management cooperation.

Waring: Yeah, Integrated Pest Management was on this campus. Entomologists.

Dyrness: That was the early, early stuff, but there was, by the time I arrived, which was in the '60s, I thought there was a fairly-high degree of cooperation between applied people and scientists, having experienced something quite different at other institutions

Denison: We had a very strong chapter of the Ecological Society of America here, and it had two key things that it was involved in during the '70's. One was evaluating the quality of the air before the pulp mills went in down at Harrisburg, right? Or, Halsey?

Swanson: Halsey.

Waring: The other one was to take on those dams built by the Army Corps of Engineers, and illustrate where new ones potentially were going to go in at Cascadia and elsewhere, and basically, build a cost-benefit ratio that looked ridiculous. Okay? We had meetings and meetings on that. Along with the population bomb, this kind of stuff. Monthly, if not more frequently. I remember this as really bringing people together across campus.

Franklin: That was way beyond the boundaries of IBP.

Denison: That's right, but -

Waring: But IBP, I think may have been more essential to it.

Denison: That's what I'm asking you.

Waring: I also remember when that was getting organized, somebody asking about local chapters.

Dyrness: Right.

Denison: And we were told there would be no local chapters, only regional chapters.

Waring: That's right.

Denison: How many people do you need to have?

Waring: We had a hundred.

Denison: We had something like four times as many as you needed for a regional chapter, just in Corvallis. And it did draw from the agencies.

Waring: A lot of the membership on those committees were also on the IBP. We had a lot of people who came because we already knew each other. It was pretty much a lot

of ferment. I mean, a lot of activity at this time. So, application or concerns about how science could look at these bigger pictures that were already being discussed.

Denison: Looking back at it, I think people were not looking at it as, "Well, if I take on this applied thing, is it gonna allow me to do what I want to do and get paid for it?" I really think it was the other way around.

Waring: Yeah, in that case nobody got any money, if we just didn't want any dams on Cascadia, or we wanted at least everybody to know about it.

Denison: Well, they cleaned it up quite bit compared to what they were gonna do. And, you had Hewlett Packard.

Waring: Yeah. There's always a lot of concern in the community.

Geier: So, what you're saying is that with this applied science tradition at OSU, (laughter) you're --

Denison: -- Too conspicuous.

Waring: OSU and the Forest Service and the EPA. They were all here in Corvallis. They were all neighbors.

Geier: So the general climate in Corvallis was somewhat more unique?

Waring: I think there was good science and lots of applications happening in the same campus. I'm trying to think of Cornell and Berkeley, as land-grant colleges, but I don't think the same thing goes on at either.

Denison: I don't think so either.

Waring: I think application.

Denison: I don't know Berkeley that well, but I know Cornell pretty well.

Waring: Well, I know Berkeley pretty well.

Swanson: The convergence was one of having this decade of the '70's, when a lot of the effort went into just understanding how the system worked.

Franklin: Right.

Swanson: Good science, credible science was funded by --

Waring: -- NSF.

Swanson: -- through NSF. And then, the issues --

Waring: -- Came. ["Forest Wars"- 1980s/1990s; northern spotted owl, old-growth, etc.]

Swanson: Started heating up. So, the scientists were trained, they were used to working together, they were used to understanding the system, and the issues bubbled up. Then there was this progressive schooling of ourselves about participation in the process [management and policy considerations], and that you could make a difference. We got some lessons about how to perform and maybe how not to perform, from watching the herbicide battles with Logan's [Norris] and Mike Newton's different performance styles. Then, bingo, when the shit really hit the fan, it took quite a bit of skill and credibility. It's really quite remarkable. I was at the Forestry for the 21st Century Conference, talking to some Australians, and they were just marveling at what was going on. There were more people in the Northwest, or even just in Corvallis, senior scientists, who could stand up and talk science and talk management-policy implications with passion and ability, than in all of Australia, they felt.

Waring: Well, they can only talk out of one side of their mouth, because the flies keep gettin' in the other side. (Group laughter) We learn to talk out of both sides. Australia's tops on environmental issues, really. I think the other thing that most of the other groups didn't have is individual trees worth five thousand dollars.

Swanson: A lot at stake.

Waring: A lot at stake, a lot at stake. They're smaller trees now, and they're still worth five thousand dollars. That kind of resource, and if you look at it across the United States, you see there really aren't any other forest regions quite like this in terms of the capacity, and we documented that in the IBP times, maybe at the very end. I know we had one article that came out in '79 in *Science*.

Geier: So, you're saying that means the management part of it is more important?

Waring: It made it more important. It didn't mean that deserts weren't being beat down and lots of problems were there that people were concerned about. This was such a valuable resource per acre, and had such a potential for good and bad, that people were very concerned about it for a whole variety of reasons. But it was extremely valuable.

Denison: But it also was of regional interest, these sorts of issues, perhaps more so than some other regions. This was that era in an environmentally-sensitive region; 1973 was the year for passage of Oregon Senate Bill 100 that mandated across the state, land-use planning, essentially. [Consistent across the state within certain guidelines]

Waring: Yeah, in Oregon.

Denison: The consequences of this, for individual landowners, for farmers, for industrial forest lands, and for counties mandated to implement, were right there in the newspapers.

Waring: That was followed up with the Oregon Forest Practices Act [passed in 1971], and that has been continually modified, and was statewide. Eventually, both Washington and California enacted some kind of forest practice act, too. So, they don't want to ignore that at one time Oregon provided 25% of the timber for the entire U.S., and it was during this time that the IBP was developing. The market was there and they were really "gettin' out the cut," so it was a tremendously valuable resource. The lake states couldn't do that, the Northeast couldn't do it, even the southeast, like in Coweeta, where they picked it, they couldn't have the big cuts. And then you had the deserts and the shortgrass prairies.

End of Tape 2 (2 of 2), Side A
Beginning of Tape 2 (2 of 2), Side B

Swanson: What we want to be doing here is some "futuring." It sounded like it was somewhat infrastructure-oriented, rather than thematically-oriented. I was wondering about getting people's ideas about what might be fun and useful to do. I don't see us spending much time at it. We've been investing a lot of energy in 50th [anniversary] activities, and using them to get us updating a whole bunch of our general infrastructure, such as the brochures, and working with Max and another writer, on two books, and a publication list update, and a whole series of things like that. The festivities of August 21st, we'll be advertising for them, and the webpage presence around the 50th. We've generated a lot of action. So, it'll be good to think about the future and not just wallow in the past. I'd like to have some of that direction invade our August 21st celebration. We'll have that, but we'd also like to have some ideas to tuck in our mental pockets for use in various occasions when some of us talk with the media or others, about the Andrews program in the context of the 50th and looking forward, or in other contexts. Also, we are at a pretty important juncture it seems, and I think we may be facing a number of effects. Our facility development has been really dramatic, so do we need to change how we view changes in our approach to facility development? Our science needs to evolve quite a bit. I'm not sure if there're any step functions we undertake. I'll just toss that out. I'll be interested in hearing your thoughts on a process and on futuring.

Dyrness: Did I understand we have an "endowment"?

Swanson: Endowment, yes.

Denison: That's a great way to get yourself into conflict with your academic institutions. If you're interested in long-term, and you've got funding which has potential for fluctuating with political fortunes, one of the things that would be handy would be to have a couple million-dollar endowment set up for steady income. Seriously, I suspect that there are people and foundations who might look at something like that.

Swanson: That's an interesting idea. We've gone as far as the initiative of Jack Lattin to start an Andrews Forest Fund. It's not advertised, but it has accumulated about \$8,000 already, without going out and looking for a penny. But that could be a new initiative.

Denison: And, you've got a facility to maintain.

Swanson: Yeah.

Denison: Just an endowment fund. Having that for an emergency.

Waring: Well, a maintenance fee, because almost all these grants will build. I know there's this brand new center that was put in when the senator retired, well, now they have to put in \$10-15 million worth. And that's not coming from the federal government. So, I think there's another potential, and I had to think about this when I wrote the book with Steve [Running], which is coming out the end of this month, but there's two things that've come out of the Andrews that you may not fully appreciate. If you focus on the Andrews, you don't quite see it. One is when Henry Gholz was a masters' student working here on the Andrews, that we subsidized establishment of what is called the Oregon Transect [OTTER – NASA funding], where we had Cascade Head and McDonald Forest, and all the way over east, including the Metolius [Research Natural Area], and all of that. We had the Andrews as one of the original parts of it, but it was not on the same road as the Santiam Pass, so it sort of got left aside. Well, there have probably been something like 70, 80 articles published on that transect. What the Andrews group has also done, now you have to realize what you've done, is you've taken the satellite imagery, not just over the Andrews (HJA has in great detail), but you've also done it for the whole Northwest over the last 20 years with a bunch of Andrews-related people, including Warren Cohen. So, that's now a mosaic that's changing.

And in a way, we monitor things on the Andrews that change, the things that turned up. This is monitoring the entire state, at least the west side. Every year, or twice a year, you fill in the blanks, and this is an opportunity for researchers that follow those changes, to explain the changes. When you don't understand them, I don't want to call it a philosophy, as I don't like that word, but an ecological philosophy, and you say, "Why is this thing changing when we don't have an explanation for it?" It's not climate change, it's not management change; this forest is dying. Is this a pathogen that's been introduced? Then you send a team out where the symptoms are not easily explained. You can help the state evaluate changes in the capacity of the landscape, changes in the

actual production of the landscape, changes in urbanization or flooding. Or these things, could be perceived as principles, including landslides, that occasionally occur on the Andrews. In order to test your models, you need this huge landscape, to say, "Aha, I told you so, and look what we learned here that we predicted it would." I think there's no competition, almost everybody else has the images and no understanding of the details, historically. You have some of these things locked up in these wooden data banks, the isotope signals where you pick out over the years. When you had a major drought across Oregon, we looked in the isotope signatures. But, you also had these twenty years of satellite data, and you also had people beginning to model with that. I think it's a tremendous opportunity to test the reliability of the principles, and like I say, you know the accuracy, the principles that have developed from the cadre of scientists working close to, along with or across the Atlantic. I think, there's no competition.

Swanson: Yeah, exactly. I think we're moving beautifully along that path and Warren has been really an outstanding leader in moving that along.

(Group interview continued, but was not transcribed from this point on in the tape)

End of Transcription