

Small Watersheds Group Interview: George Lienkaemper, Fred Swanson, Don Henshaw, Ted Dyrness, Gordon Grant, Al Levno, and Ross Mersereau; 8:30 a.m. October 16, 1997, Peavy Hall, Rm. 222, Oregon State University, by Max Geier.

Watershed research has been a staple of Andrews Forest science since the earliest days of the experimental forest. This includes the experimental watershed studies of streamflow, sediment yield, vegetation, and biogeochemistry, and also observations of stream-channel cross sections, landslide movement, and other topics related to water quantity and quality. Participants in this group interview represent a wide variety of disciplines and roles over much of this history: Forest Service soil scientist Dyrness, Forest Service hydrologist Grant, Forest Service information manager Henshaw, Forest Service field crew managers and members Levno, Lienkaemper, and Mersereau, and Forest Service geologist Swanson. The program involves a great deal of field work to keep instruments running and harvest the data, followed by office work to manage and analyze data, and then to publish the findings.

Max Geier: If everybody's signed-off on this sheet here, let's get started. Just for purposes of the transcriptionist trying to keep things straight, it would be helpful if we'd go around the table and have everybody here introduce themselves, and as you're introducing yourself, if you could just identify the time frame of your first involvement at the Andrews with watersheds research, which is the focus of this group, and your position at that time. Then, give a quick thumbnail sketch of how your position may have changed since that time, and what your current research interest is. I'm going to start here with George, and then go that way around the table [clockwise].

George Lienkaemper: I'm George Lienkaemper. I first started working at the Andrews in 1975 as a student. I was doing some cataloguing of large, woody debris for Fred [Swanson], Jim Sedell and Bob [Beschta]. Spent a couple summers doing that, and then in 1978, I was hired by the PNW Station to work with the upper watershed research group here. I worked for Al Levno for a dozen years or so, with Ross [Mersereau] and Al, and the other changing characters that came through our field crew. In the late '80s, I began doing more work with computers, and eventually moved into a role of overseeing a geographic information system. The data covers the Andrews and other areas in the Northwest. Primarily, my role has been working as an analyst for GIS, the last 7 or 8 years.

Max Geier: Okay, Fred?

Fred Swanson: I'm Fred Swanson, and I first heard about the small watersheds in 1968, when I was doing a geology project in the Coyote Creek area [South Umpqua EF], where there are also experimental watersheds. I began working in the Andrews in '72, and I was involved in mapping geology of the whole Andrews Forest, and that led to mapping landslides of various types, which included the small watersheds. Then, about 1975, I was involved in sediment budget studies associated with Watershed 10, although Fred [Glenn] and Mike [McCorison] contracted for the

International Biological Program [IBP] studies were the primary people responsible for field measurements in Watersheds 9 and 10. And then, I've continued to be interested since.

Max Geier: Okay, Don?

Don Henshaw: I was hired the same day that George [Lienkaemper] was. (Laughter)

Al Levno: We had to get some new blood! (Laughter)

Don Henshaw: I got a nice, nice corner office, and I thought, well I'll pretty much be locked in there for about five years or so, until all the old legacy data was computerized. That was actually my guess about how long it would take, and I was totally wrong. (Laughter)

Al Levno: How many years have you been working again?

Don Henshaw: I think where my job has changed is that I was originally trying to salvage a lot, not salvage, but change from paper copies and just print-out copies, to archived computer files. And where that job has really changed in scope, where originally, it was mostly Forest Service data only, and it's really much broader now. When the IBP and LTER programs got started and Fred [Swanson] took over, there were both groups and a lot of integration of the LTER and the OSU and the Forest Service, and it really broadened the scope of my job tremendously, because suddenly, we were taking on everything from the Andrews. Where before, we were really just looking at the Forest Service side, because the watershed stuff has always been like a primary emphasis, the emphasis is really a lot broader now.

Ted Dyrness: I'm Ted Dyrness. I first saw one of the gauging stations in 1955 on a field trip to the Andrews, and at that time, Roy Silen was doing his [tree] regeneration work. Then I really got involved, starting in the summer of '62, when I designed a study to follow succession after logging and slash burning on Watersheds 1 and 3. Subsequent to that, I undertook this fairly intensive soil survey on the three small watersheds. What we were concerned with at that time, were in trying to understand treatment effects, mostly involving logging and slash burning. And we felt we really didn't know what was going on much above the gauging station, so we just wanted to get as much information as we could in order to help to understand the results of the treatments. So, I think I'm going to pause. I went to Alaska in '74, and got back in '90, and over that period, I was just amazed how much was going on at the Andrews that wasn't going on when I left. (Laughs).

Gordon Grant: I'm Gordon Grant, and I first learned of the Andrews Forest from Fred [Swanson] when I took a forest geomorphology class from him at the university [UO-Eugene] in 1973. I heard about the small watersheds and the whole Andrews program, and I thought it was pretty boring. I was interested in big rivers at the time, and studying little creeks seemed pretty inconsequential to me. I got reconnected to the program in '82, when I came back to Oregon, partly due to some discussions with Fred and Jim Sedell, that got me out here to do dissertation research, and I changed my opinions about the small watersheds program somewhat. (Laughter).

They can still be pretty boring (laughter), but I've been really impressed at how the cycles of interest, of waxing and morphing interest in the small watersheds, has occupied, 20 or 30 years of professional life in the career. And I've been impressed by the perseverance of people who obviously don't think these things are "boring," in keeping the place going, and allowing those cycles of waxing and morphing to occur. I was hired full time back in '85, just as Dick Fredrickson's job [watersheds] was ending. [Fredriksen retired due to Alzheimer's diagnosis.]

Al Levno: I'm Al Levno, and let's see, I first became involved with the Andrews in, I think it was, 1963? And I took a cut in pay. I was getting \$100 a week at [Reed and Cross] Nursery [Eugene], and I came to work for the Forest Service for less than that! (Laughter) And I came to work with Dick Frederickson and Jack Rothacher and Ted [Dyrness], the watershed staff at that time, to help with the succession plots, and finding the plots after logging treatments and burning. I then became a watershed technician at that time, and I kind of stayed with that whole program for my career, which is, 30+ years? I'm still pretty much involved with the watersheds. Maybe different people are doing a lot of the work, but I'm carefully peering over their shoulders to make sure it's being done the same as it has been (laughing) for 35 years.

Max Geier: Ross?

Ross Mersereau: Well, my name is Ross Mersereau, and the first I heard of it was when I was trying to get a master's degree in fisheries, and I talked to Jay Gashwiler, who had a study going on the Andrews, and he said, "I need a technician down there." I hadn't been getting any good vibes about whether I was going to be getting any good jobs out of fisheries, so I applied to Dick Fredriksen, and he hired me. In the fall of '66, I moved down to the Andrews as the resident technician, and go-fer (laughter) for everything down there. Al [Levno] started me out the first week, to learn all the procedures and things I was going to have to learn to do, and in fact, I think he must have spent, what, two-thirds of the days in the next year, down there, (laughter) pushing me around. But along with that, and trying to keep the district and the station in such a state that they didn't squabble over everything. (Laughter) Sometimes it wasn't easy.

Max Geier: Just to get started, we might all talk about impressions of how we might characterize priorities of watershed research at the Andrews, at the time of your personal involvement there. We don't have to do this in chronological order, but if you could think a little bit about what your leading priorities for watershed research were at the time you first became involved, what your perception was at that time about what was being done at the Andrews, versus what you had hoped to see being done, in terms of watershed research in general. Who wants to get started? Al?

Al Levno: When I came, the big priority in watershed research was just analyzing the streamflow records. At that time, it was being done by hand on a Monroe calculator. A line was drawn to get the number of hours, then it was converted to the number of seconds for an average streamflow, and that was multiplied times the [some factor]. Then they put that down in a table, added it all up, and that's how they determined the streamflow for the year. That was pretty much the job for Dick and Fred, and I don't think Ted had gotten involved in that, but they spent most of their

winters calculating streamflow from the A-35 charts. And when I got there, they were just beginning to convert to machines, and we had to do what we called “Form-8’s.” And these Form 8’s had the stages at different points along the hydrograph, straight-line points, and these Form-8’s were sent to Portland, where punch-tape people were available to take the data from that [computer] card. Then it was run through the big computer, and this was all done at Portland.

Ted Dyrness: And the computer was at Bonneville?

Al Levno: Bonneville Power. That’s right. When it came back, it was some big mess! (Laughter) We’d have to go back through everything, straighten it all out! So, that was one of the big changes that was happening. The other thing happening on the watershed front was Dick Fredriksen, who was interested in soil nutrients and nutrient studies, and he began shortly after I got there, to look at nutrient loss from logged watersheds. And he set up that 4-year study on watersheds 9 and 10, and one of the watersheds that was outside the Andrews. He was going to do a quick-and-dirty Ph.D. thesis, and then, that would be no more. So, that got started very shortly after I got started.

Ted Dyrness: What year was that? I was trying to remember the year he started that.

Al Levno: I think that was in ’67 or so.

Ted Dyrness: That’s when 9 and 10 were selected?

Al Levno: Well, maybe it was later. He came to Corvallis and started working on his program, and then started looking for small watersheds after that.

Ted Dyrness: Do you remember when Gene Likens came? To my recollection, one of the motivating factors behind Dick getting interested in the chemical composition of stream water, was that Gene Likens came out from Hubbard Brook. I remember taking him around out at the Andrews, and he was saying what they were doing, and they were concentrating on nitrates and so on, and the water, and I think that had something to do with Dick becoming interested, and saying, “Hey! We can do the same thing here. We should be doing it. We’ve not done it on Watersheds 1 and 3, in the calibration stage, that’s why we need a couple more small basins, and so we can get it undisturbed, and then use the paired-watershed approach.” Because, remember, they were first called “chem-sheds,” weren’t they? That’s right. Does anyone call them that now?

Fred Swanson: No, but you can still see the term in some old documents, and I think that’s one of the important aspects of this. We may not have time today to develop it, but what was the science and management policy context of this work. And on the science front, Hubbard Brook was really getting out there, and then, Likens and Bormann, were making a big splash with articles in *Science* and other places about this approach, and about some management impacts. In the meanwhile, there was a lot of controversy about clear-cutting. I don’t know if that was the time of the Monongahela case, but also, there was this guy, Bob Curry, who was in the region, a

fellow down in California, who was very vocal and gave congressional testimony, and raised issues around the mining of the nutrient capital of watersheds through logging operations. So, there was part of the policy/science context, ranging from the basic end of Likens and Bormann, although they got into policy, all the way over to the hard-policy and management, mismanagement issues.

Max Geier: Would this be the late '60s, early '70s you're talking about?

Fred Swanson: Yeah.

Al Levno: I think one of the things that probably occurred before the watershed-nutrient thing heated up, was the '64 flood happened, and in '64 that started us on the landslide inventory.

Ted Dyrness: I had been assigned to soils, doing research, and before that flood, I was thinking in terms of more rainfall-caused surface erosion, and after that flood, there were so many mass soil movements, and a lot of over-steepened drainages, I said, "This is the important form of erosion here." It's interesting to look back and see how one event can just change your ideas completely.

Fred Swanson: I was wondering, Ted, about your interaction with Dick Fredriksen on the slide business? Because Dick began to document some of the slides in the small watershed before '64, and then, in the '64 event. You then proceeded to take the full Andrews Forest-level look at the landslides of the '64-'65 flood.

Ted Dyrness: Yeah.

Fred Swanson: Did his [Dick Fredriksen] work on the small watersheds help to get you going on the larger area?

Ted Dyrness: I really can't remember in detail what the genesis of that was.

Al Levno: Well, there was the slide in the watersheds in 1959, and Dick wrote that first paper on the mud and snow slides? And after that, boy, he was a real believer! (Laughter)

Fred Swanson: Yeah. They were in the *Research Notes* [PNW Publication], so he got the message, before '64, and '64 helped bring it home. (Laughter)

Al Levno: In '64, Dick was ready for them, yeah. (Laughter)

Gordon Grant: I'm basing this on something you said Al, but is it fair to say that the early focus in terms of the time-scale interest, was more the annual working of numbers characteristic of an annual production, annual streamflow, annual sediment, and annual nutrients? Is that true? Was that was the major focus? Because I'm wondering if the '64 event may have also changed the focus and expanded the awareness. Because a lot of things happened, being aware of that.

Al Levno: Yeah, and the other thing that was happening in watershed management, when they were trying to make water by logging, taking off the trees, and having more water available for city, municipal uses.

George Lienkaemper: Especially during the dry season. (Laughter)

Ted Dyrness: We always looked at that as important also. Not just the annual streamflow. [more expansive comments obscured by voices expressing agreement].

Gordon Grant: But not the event. I mean, it didn't seem as though it was so focused on looking at individual events. Look at Rothacher's early papers, and it seems like it's [unintelligible].

Ted Dyrness: Well, I think that's fair to say, and then, of course Ross was sent out before every storm. He said, "We've got to get our sample during the storm, you know! We can't go on a weekly basis and get grab samples and so on. We have to be out." Ross would hear rainfall on the roof, and they had to go get it, you know. (Laughter)

Gordon Grant: Yeah. In that sense, you were very-focused. I mean, you were very-focused.

George Lienkaemper: For years, whenever it rained, I listened for the phone to ring! It's going to ring soon, and it's going to be AI! (Laughter)

Fred Swanson: It was reporting, and maybe this didn't really emerge until the mid '70s, but reporting concentrations of nitrates and suspended sediment at the instantaneous, grab-sample scale. That was in the context of water quality issues, and the Clean Water Act. I forget when that was passed [1972], it must have been the early '70s. Yeah, '72. So, I can see it going from the annual scale, water-yield issues, dominating, then, storm events. I remember Dick Fredriksen plotting hysteresis-loop form of sediment agitations (laughter), and then reporting of instantaneous concentrations related to water quality, and in the national context, water quality getting big because the Cuyahoga River caught fire. Back east, there were some really nasty things in the water, and the best we could come up with was a little bit of nitrate, and [sedimentation]. It is interesting how these things loop around, like the initial attention to Watershed 3, with the slide in '59. Then Watershed 3 boomed, in terms of land-sliding, more so than probably any other place in the Andrews, in '64, '65. And then, the same thing happened in '96. (Laughs)

Ted Dyrness: Yeah.

Fred Swanson: So it's a bit of an "n"-member example which we happen to have stumbled on, (laughter) and we have to be careful to put it in that context.

Gordon Grant: One question I've wondered about is, well, there are two parts to this. One is, to what extent were other places like Hubbard Brook, other experimental forests, used as models for what happened at the Andrews? In other words, you mentioned Gene Likens coming out on

this trip, and you said, well, maybe we'll do nutrients, and I was thinking if that were done today, if we wanted to start the Andrews today, we'd send somebody on a grand tour, we'd hit every other site, we'd take lots of notes, and we'd pull people in, and was that kind of cross-talk happening? Or did this just kind of evolve sort of independently, but, we all read the same papers?

Ted Dyrness: No, my impression was that the watershed hydrology group, nationwide, was fairly tight-knit. You know, we knew what was going on, through personal contacts and professional meetings and so on, and I remember really thinking about how the Hubbard Brook area was very much different than the Andrews in terms of soil conditions and so on. And you've got to remember that what they did was not just logging, and then, allowing natural revegetation. They had at least one watershed where they kept the vegetation down with herbicides. And that's where they showed the vast amounts of nitrates in the water, to dangerous levels, and people outside of that said, "Well, we can't clear-cut log," and so on. But no, we knew.

Al Levno: Yeah, I think so. I think there were bi-weekly or semi-annual reports that were circulated.

Ted Dyrness: And of course, we were all using that basic paired-watershed approach, and using the same kind of velocity of watershed research.

Fred Swanson: I think you can see it in the literature of symposia on watershed themes. My impression is they might have been more common back then, but clearly there are some that show that people were getting together and talking about these things. One aspect of your comment, Gordon, has to do with, what were sources of ideas, and my take would be the hydrology work itself had very deep roots at Coweeta, which had a bunch of stuff going on long before Likens and Bormann came on the scene, and then, they made the break, conceptual breakthrough, of seeing these as sort of systems, where you could do nutrient-analysis. And then they ran with it in spades, and it took them to the highest levels of science recognition. For just the U.S., they had national academy of science membership that other sites didn't go so far, in terms of getting scientific recognition for that. On the other hand, I look at Ted's landslide inventory, published in '67?

Ted Dyrness: Uh-huh. (Laughing) Took a long while to get it published!

Fred Swanson: Yeah, but still, you look at George Ice's 1985 catalogue of landslide inventories, and there were over 50 in the region, and there've been quite a few in the 12 years since then. I still look at yours as the landmark in how to do it, and most of the subsequent ones have not met those standards, particularly with respect to looking at rates of sliding in relation to the acres available, slope aspect, or slope steepness class, or one thing or another. And you see a lot of garbled results, because they didn't meet your standards. But anyway, that would be a case where something came out of here and had an impact elsewhere.

Al Levno: There were a lot of people in watersheds before 1960. I think there were small watersheds all over the place. And we had hydrologists at, the [PNW] station had a hydrologist in Roseburg? It was Eugene [Herring?] at the time. And there was a hydrologist in Portland?

Ted Dyrness: Yeah. Ned Benton.

Al Levno: He was at the Wenatchee watershed.

Ted Dyrness: And he was at the Portland watershed at Bull Run. Does anybody know how the 3 small watersheds [at H.J. Andrews] were selected? Who did it? Do you know?

Gordon Grant: No, I don't. And I realize, no one sitting around this table dates back to the initial selection. The guy, I picture three guys on horses. (Laughter). Well, we'll put it here, you know. (Laughter) Do you know any stories, Fred?

Fred Swanson: Art [McKee] may know some stories.

Ted Dyrness: Yeah, it's too bad he's not here.

George Lienkaemper: Let's see, was Silen the one that was? Seems like Roy was the one.

Fred Swanson: Yeah, now that makes a lot of sense, because his tenure ran to '54 or so, and didn't they go on-line in about '55, in terms of gauging? So, he must have been – [involved]

Al Levno: Before that. They went on-line in '52.

Fred Swanson: '53.

George Lienkaemper: October '52.

Ted Dyrness: Is that right? They're older than I am?

Fred Swanson: Roy would, I bet Roy would have some insight.

Ted Dyrness: But we've got to have somebody, because at that time, the [PNW] station didn't have anybody in hydrology. And the one, I had heard that they got somebody from Coweeta, to come out and look at the recommendable locations, and give his blessing, and then, he was the one that designed the flume, which we're still using.

George Lienkaemper: You know, we've got a bunch of those old blueprints for those flumes. I wonder if there's any information on that.

Ross Mersereau: Not only that, but there's the stuff in the files about the flumes.

Ted Dyrness: I may be wrong, but Marvin [Hoover?] was at Coweeta about that time, and he was the one that, you know, this was probably handed down to me from Jack [Rothacher], and I don't know if I'm remembering right. We really need to get that information.

Al Levno: That was a stroke of genius, to put the flumes in there.

Ted Dyrness: Yeah. Yeah.

Al Levno: You know, weirs were used almost every place else. They'd be getting so plugged up by now! (Laughter)

Ted Dyrness: Yeah, and we still use the same flumes! That's amazing! This guy really did a good job!

Fred Swanson: Well, we should put in the book a photo sequence that shows the success of "Operation Burial," and "Re-excavation" (laughter), the history of this flume. Another thing that comes to mind from the discussion, both within the region, but also maybe nationally, the history of turning on and turning off experimental watersheds. If we plot a time-line and where the different watershed sets are located, when they came on and when they were turned off, it would probably be a wave of initiation, and then a wave of stopping, and there aren't any that I can think of going on-line in the U.S. in the last 20 years. Yet now, we have a whole bunch of new kinds of questions, new ways of looking at the system, such as sediment-budget thinking, or new silvicultural questions, like thinning and stuff like that. The initial treatments were focused on, for the most part, first entry into natural stands, and now, we get a lot of people calling us up and asking questions about the thinning in plantations, and we're not really following up on that. In this area, and also in other areas, like the veg-plots, there's a neat display of that Steve Acker put together, on the establishment of vegetation plots. There were ways of establishment for different objectives, and now, some of those plots are being managed in our group for a set of common ecological objectives, although they didn't initiate as objectives for different cohorts.

Max Geier: That was a question I wanted to address in this group, ways in which the initial selection criteria for locating the watershed studies have influenced the kind of research that can be done, or that should be done, what kinds of data sets have emerged that have imposed constraints on what can be done and how, or opened up opportunities?

Al Levno: I'm not quite sure I understand you?

Max Geier: Well, I guess what I'm saying is, if you look back on it, is the way the watersheds are set up now, the optimal way for doing it on the Andrews? For current research needs?

George Lienkaemper: It seems to me that the stuff that goes on in an experimental watershed is sort of limited by succession, whether it's the vegetation, or the unraveling of the landscape. For example, there's a lot of things you can't do in Watershed 3 because it's fallen apart a couple of times that you might be able to do if it hadn't. But things and events happen if you don't control,

maybe the way that vegetation has come back in Watershed 1, or hasn't, which is a real controlling factor of what can be done there. I think that it's very hard to have broad studies that can sort of be re-established in particular areas, just because of the way, you don't have much control over the way things are going to succeed in due time.

Gordon Grant: To throw it open, I'm not sure that answers your question, Max. One of the things that really impresses me, from having looked at some aspects of these studies, is just the importance of contingency, in defining what goes on. I mean, here are 3 guys on horseback, and they say, "Well, these 3 watersheds look about right." (Laughter) I'm not going to let go of that. These 3 watersheds look, you know, they're all facing the same way, all the same kinds of trees, well, alright. They're about the same size. You know, we'll cut.

George Lienkaemper: Easiest ones to get to.

Gordon Grant: Easiest to get to, and there are a whole bunch of good reasons for that. What could be simpler? Well, this is our control, we'll cut this one, this way, cut this one, that way, and let 'er rip! And then you go back, 10, 20, 30 years afterwards, try to figure out what the experiment tells you, and it turns out that one of these places, there're all these trees, it turns out to have a lot more slumping ground than that one over there. And this one over here, well, it's got a lot of rock in it, and the channel kind of behaves a little differently, because of the rock. And then, this one had to be cut before the big flood, and this one cut after or during the big flood. That sends the place on two very different trajectories. And suddenly, these historical and natural accidents and circumstances turn out to dominate the results of the experiment, and become exceedingly difficult to come back to. If we had the people who started this experiment in the room, and they asked us, "We started this experiment, maybe 20 years ago, what'd you guys find out? What'd you find out about clear-cut logging?" (Laughter) And we'd go, "Weeeelllllll."

Ted Dyrness: That all depends.

Gordon Grant: That all depends! (Laughter) Let me tell you a story! (Laughter) You know. And that's what these things become. They become stories.

Ted Dyrness: Almost. Almost, because they are now case histories.

Gordon Grant: Whereas, I think when you set them up, or when they were set up, I'd be interested to know what the mind-set was, but my sense is that they were viewed as "real experimental units" in the sense of "science." If we were to do this experiment over and over and over again, using this experimental unit, we would get about the same result. And I think what we've learned is that we would not get the same results. That they are case studies, and they will only be, and can only be used that way.

Fred Swanson: To elaborate on that a little bit, you asked the question, Max, in terms of initial selection. My reaction is, selection of location and selection of treatments, maybe. Given the contingencies that Gordon mentioned, initial selection doesn't mean you can't anticipate what

the consequences are going to be, because there's going to be an evolution of at least 3 sorts, developments that involve a lot of contingencies. One is the watershed itself, including the vegetation, the different climatic events, the geomorphic responses, etc. So the watershed itself is changing, and there are a lot of contingencies. If a big flood, or a heavy wet snow occurs in year 1, or 10, or 30, you're going to have very different consequences. The second source of contingencies that are going to be very difficult to anticipate, and I think impossible, are the concepts. What are the key science issues and concepts? What are the key social issues? Which may emerge in other parts of the country or the world; biodiversity, or water quality, or stuff like that.

Then, they have a play in our scene. And they can be the science concepts, just the way we're viewing them, and there's the watersheds themselves, like 1, 2, and 3, but then, now we're interested in landscapes, so having the Hi-15 [watershed] up there, and 9 and 10 down lower, is part of our ability to address landscapes. Although they weren't selected, they were "selected" somewhat to do that, but not completely. Then, the third area is the people-involvement evolution. So these watersheds have had an interesting history. They had a group sort of start them, and then it seems like Dick Fredrickson was running them fairly heavily as a personal enterprise for a while. I mean, as a science leader, then IBP happened, and there was some overlap there, although the IBP emphasis in the '70s was on 9 and 10, and of course there was guys that who were more, 1, 2, 3, Hi-15, and Coyote and the Fox Creek watersheds.

Al Levno: Then there was the period of dismantling, I think when Dennis Harr was here. It was like Gordon said, "these are just case histories, they're not worth keeping, they're expensive, and we ought to dismantle them, and shut down watersheds 6, 7, and 8, and Coyote Creek.

George Lienkaemper: Well, we still collected the streamflow. We terminated the chemistry data.

Al Levno: Yeah, but the streamflow was also supposed to go down, but it didn't.

George Lienkaemper: How did that happen?

Fred Swanson: That gets back to the people business, and the early reports, it seemed like, with any responsible party, potentials were created and eliminated. Take a look at Likens and Bormann, making hay off theirs, their efforts with experimental watersheds, and our case was much more limited. On the other hand, by maintaining the long-term effort in sort of a corporate way, it provides more opportunity, so now, when Gordon [Grant], and Julia [Jones], and Reed and Beverley, and a bunch of other people come in the picture, to make hay.

George Lienkaemper: One thing they did on 1, 2, and 3, is that they did start them in '53, and they didn't cut them until '62. To wait 10 years, little did they know that 40 years later, after the cutting, we'd still be measuring them. We had this incredible amount of post-treatment data, and very little pre-treatment, but at least on 1, 2, and 3, I thought they showed a lot of foresight in waiting that long. If you look at some of the stream chemistry work that Fredriksen did, he'll have like 2, on some of those, on Fox Creek and Coyote Creek, maybe a couple, 3 years at the

most, of pre-treatment chemistry data, and then you've got, 0 or 20, or more years, of post-treatment. I think overall, they did let those, they set them up, and they did let them go for a while. I think that was pretty good. It's hard in these days, when you need to produce something, to just set something in motion and then wait.

Ted Dyrness: Of course, during that period, they did have the road treatment in 3, so they wanted sufficient time to start studying the effects.

George Lienkaemper: But even that wasn't until '59.

Al Levno: It was after 5 or 6 years to start collecting.

George Lienkaemper: Yeah, and then watersheds 9 and 10 had to have, maybe, 4 years?

Al Levno: They had quite a few.

George Lienkaemper: They had quite a few pre-treatment years, I guess, at 10 [Watershed].

Fred Swanson: Well, for 7 [watershed], anyway. Ten [watershed] was extremely limited in number of years, and some of the years were compromised, because people were walking around in the creek to study bugs, one thing or the other, and I didn't believe things were coordinated.

George Lienkaemper: The other thing that I noticed when I came in was, there had been, I came in '78, and computers, mainframe computers were only in use up until that time. There were no PCs around. There was hardly any mini-computers. I mean, the whole notion of a computer was a mainframe. The fact that, I guess it was in 1975 before I started, Dennis Harr and some guy who had been doing data analysis with him, made a run up to Portland, and salvaged hundreds of boxes of computer cards, and they loaded them in the back of the truck (laughs), and they drove them back to Corvallis. Then, later in '79 I went up and cleaned out the rest of their boxes, because we had been having all the analysis done in Portland, and they used the Bonneville Power computer, and there's a woman up there, Sue Skinner, who wrote all these programs and did all this. Those days, for me to be able to come in and at least have all that data on computer cards, the fact that we have all this data put together, and it wasn't that huge of an effort to reconstruct, all that information, it was a big effort, but it was possible. That also showed me that there was a lot of attention paid to these studies. You look at Bonanza Creek [Alaska], and they've been spending the last 2 or 3 years going back and trying to restore and recover those early records. If we were faced with having to recreate all that from scratch, and we have recreated a lot, it would have been impossible. So, the fact that we have all of those records back to 1952, is impressive.

Al Levno: I think Logan Norris deserves a big pat on the back for getting that going. He had a lot of foresight. He came on when pesticide money, there was a lot of it, and he skimmed off a lot of pesticide money for the watershed program to keep it going. He also was instrumental in giving

permission to hire Don [Henshaw], because laughing we needed somebody to help put it [data] in shape. (Laughing) It was in bad shape!

Fred Swanson: What wasn't an obvious thing, is that as you began the discussion, Al, and you said, "Well, the scientists and technical staff would go back to the office in the winter and just process the data." That made me think about how staffing has changed, and how assignments have changed, and so, we've staffed up to do that, and we're doing it with much higher capability, in both personnel and support technology. Meanwhile, scientists go off and are directing other people working on this stuff, student projects and stuff like that, to make more of the analysis, and also spending a lot of time out there on the road on the management and the policy interface stuff. So, I think where Gordon and I, and watershed analysis, Northwest Forest Plan, GAO trips, and all this kind of stuff, is really at the policy-management interface realm, which is going on at a huge rate compared to what it used to.

Gordon Grant: Yes. I was thinking about exactly the same thing. I mean, there's something very homey about (laughing) this kind of thing.

Al Levno: Sitting around the fire and laughing.

Gordon Grant: Sitting around the fire, and getting out your A-35 flipchart. (Laughter) But it's an interesting point, because it's more than just the image of the old-timers doing it the hard way and doing it right. But, it also meant there was an investment in the data collection enterprise. That one person was likely to experience the continuity from the collection to the analysis to the interpretation to the write-up. That was a job one person did, and they had it as one kind of, that was their mission. That was their job. That's what they did. What we've done, we've really fragmented. Al and Ross go out and collect the data, and then there's the handoff to Don, and Don processes the data, and then the handoff leads you to a student, who works the data up, and then the handoff is to somebody, who in between meetings gets to go, "Oh! That's very interesting data you've got here. I don't know what the hell it means!" (Laughter) But, the thing is that the nature of the job seems like it has changed, sufficiently that no one person has the continuity. And I've looked at Don [Henshaw], probably, as kind of the lynch-pin in the operation. I mean, Al [Levno] and Don. I mean, you guys really know, is it good data, or is it bad data. You know, do we trust it, or do we not? Why, how does it fit? Is it big numbers or little numbers? And that almost, the very nature of that lack of continuity, I think, changes the nature of the questions you start asking.

Ted Dyrness: But in the old days, you say the same person collected the data, and that's somewhat of an anomaly, but I can remember, time-after-time, when we got together and our project leader was meeting with Jack [Rothacher], and he said, "We've got drawers full of charts that we haven't looked at! What are we going to do? Should we keep collecting the information?" It was the source of vast frustration for Jack, and for the rest of us. Do you remember that? He would say that all the time.

Don Henshaw: You look at the old charts, and you look at the ones from like '56, 7, 8, 9, and you can just see, they've been pored over. They have red (laughing) and blue lines, pencil marks and lines, (laughter) and every part of the chart is, like, annotated. And then, as it goes on, there's less and less and less of that, until now, these charts come in, and they go to Tina [Umberger], the student helper, or whoever, and they handle it. Nobody else looks at them before that. Whereas, in the early days, they were. They were pored over by the science people, and you can see that on the charts, less and less is written on them, less and less over time, although now we have computer techniques, to check the record. I mean, they're still being scrutinized heavily, but it's just being done in a different way.

George Lienkaemper: And it's transitional, because when Ross and I were collecting data, we spent a lot of time with the charts before they got to Don.

Ted Dyrness: That's right.

George Lienkaemper: That was part of our winter work, going over the charts, and correcting for differences in the hook gauges. That was part of the job. And it's much more automated now.

Ross Mersereau: Starting with a new water meter, we were trying to get the old charts ready to send to you.

Don Henshaw: So, it's the transition of technology that's the issue, and I can see that as a research scientist, you might thing back fondly to a time when you could have your hand on the data, as much, and spend less time in the car. (Laughter)

Max Geier: Well, there are some interesting points raised here. Compare this to Gordon's comments about the paired watersheds, and how that evolved in different ways. You can see, also, how the computer situation could influence, some of you were suggesting, the kind of questions that would be asked. If we can talk a little bit about that, the ways in which the storage of records, or record-keeping and record-manipulation, has changed over time, and how has that influenced the kind of science that's been done on the watersheds?

George Lienkaemper: Well, I can just touch on one area, which is just working with GIS data sets. We can answer and address questions we couldn't even think about 10 years ago, just the fact of the computing power over a large amount of collected data. Stuff that we would have contemplated doing, hiring somebody to write a program, or a series of programs that, when you think of landscape-level data, for instance, we just never would have never tackled it. It would have been too expensive, people might have dreamt of the questions, but they wouldn't have hardly talked about them at that level. So, so the technology itself has allowed us to sort of expand our view of the landscape in which we operate. Now that's one, the mega-scale, where would have never have asked the question in the first place, because we just couldn't deal with it.

Don Henshaw: Well the other point, early on, a different one; there was only some pre-treatment records, and then many years of post-treatment. Now there was a long time, when I

came in, when they were just collecting the data. I don't think there was any real active, new questions being asked at that point. They had written all the papers that had to do with the change from the treatment, and they were just kind of in a steady state of just monitoring the streamflow. Then along came Gordon and Julia, who probably weren't aware of a lot of the early constraints that all of these early guys knew (laughter), and they're asking very significant and broad and tough questions to have that data answer, and also because there's a much longer record there. I think that just the fact that the record has survived so long, has enabled us to come in and re-look at and ask new questions. Gordon could probably describe this better. But it does seem like you were not privy to all the constraints, or the problems that we had early on, in setting them up, where I think we learned by a lot of research and studying the data, we learned a lot of these things. But it is in a long-term study, where you're changing the hands of the people, and then the questions of new people coming in, different questions, and I think, it's unique.

Gordon Grant: Well, it's an interesting point. I don't know if I've really thought about it, but I do recall, when we got into looking at the streamflow, the questions, what my motivations were. Here's this issue that we've been talking about now, locally, we've been talking about it for 40 or 50 or 60 years, but you can go back and ask to see if people have been talking about, what is the effect of cutting trees on floods, or streamflow references? And I remember thinking, where can we get the data to answer the question? It was one of the luxuries of not being completely plugged in to the watershed business. I was sort of casting about for data, and there was USGS data, and then, oh yeah! This is, hey! We've been doing this 40, 50 years on our end! That's the logical place, but a different scale, different size. Then immediately you could do different things, if you did it in small scale, so the little guys might give you a clearer idea of what the big guys could do. For me, it was a combination of being sufficiently invested in the scheme that I knew what was going on here, that I knew there was data to be had, but not being so invested in that so watersheds 1, 2, and 3 were the only places to look. I could imagine, earlier on, it was the way you have described it, that the early folks for whom maintaining the watershed record and maintaining the continuity of that record, was their *life*, was their *career*. But that, in a sense, defined the question.

So getting back to your question, I think the fact that we can archive data, and we can retrieve data, and we can manipulate data easily, means the data becomes in a sense, something that is somewhat abstracted from the place in a way you really couldn't do before. In a way, you could think of it as, just give me the data. Just give me some data. Any data will do. I think the interesting thing is, I think now we are training students with that mindset. So that the old idea, thinking that they could go out and *generate* data, is potentially something that could be unattractive? That may be too, the dedication to keep it up year after year after year, to work with, you know, little recompense, and a lot of frustration, where it's, because you can't keep up with it. But you're, you know, this whole thing is just asking new questions of old data.

Max Geier: In some ways, regarding your earlier point about how the science and technology becoming more alienated from the data collection process, what you're suggesting is there may be some advantages to that in terms of the kinds of questions that you could ask?

Gordon Grant: Yeah, I think it cuts both ways. But I think that with the proliferation of data, and the fact that we now have things like the internet and web, where somebody who's never even set foot on the Andrews, and he maybe doesn't even the history, they can go out and retrieve the data- sets and manipulate them and work with them, without the constraints of knowing what's of value. That's right, so I think, this is a significant evolution, or de-evolution. (Laughs).

Fred Swanson: Well, that's a very big issue we were addressing some yesterday, and in a general data management, data access arena, it just reflects how science, in general, is changing. I think, from having maybe more emphasis on individual investigators and small labs doing their own thing, with a series of short-term studies, shifting from that to more, larger enterprises, interdisciplinary, long-term, and the LTER is a model for that. The Forest Service activities, of which this is a significant component, is another part of that, and it's interesting we have only one, well, we don't have any academics here, but I'd hoped we were going to have. You know, I was hoping we'd have a bunch of grad students here, because there's an interesting aspect. We have to help them learn how to operate in this brave new world, and there's the data management part, and there's taking from it and capitalizing on it, but also giving back to it. And there's the field part of getting enough field exposure to know what to be queasy about and what not. And so, that's a part of our training regimen, and I feel like we haven't really brought ourselves up to date on why this this kind of enterprise operates as it does.

End of Side A, Tape 1 (of 2)

Beginning of Side B, Tape 1 (of 2)

Fred Swanson: What it does do, is to allow doing things, and asking questions that we couldn't ask before. If you had to start from scratch, there's no way you could get around to asking, because you would be retired and dead, if we had some of these longer-term questions. Both for questions that require long records, and interdisciplinary work, you've got to do business this way.

Gordon Grant: Yeah. I think back on your comment about how contingency and natural, the social sphere, the people sphere, all interacted. I think that's a really important way of thinking about it, but you look at the social sphere, and the kinds of questions that are being raised, I mean, in many respects, the issues that were maybe in the back or front of the minds of folks that set up this whole watershed program; what's the effect of cutting trees on streamflow, sediment, nutrients, and so forth. Well, at that time, I would imagine there were not very many people who held those questions, who could articulate those questions. You had to be kind of a geek to even think that way. Now, that stuff is out there in the common realm, so when we have a flood, or even when we don't have a flood, those are the issues people are peppering the agencies with, and those are the issues that are deciding policy initiatives, and laws, and legal challenges. So, the nature of the questions, and the intensity of the questioning process, has really burgeoned as well. And so, that too, is forcing us into the arena where we have to be using long-time, series data, and we have to be using big spatial data, and that will, I think, inevitably,

create some conflicts and differences between the people who get trained to think and do in that arena.

Then I think about you guys, and my early images of a field crew, and I don't want to be painting false romanticism on this, but laughter there was nothing terribly romantic about living in a stinky trailer with wet clothes for three months of every year, and as I recall, the cooking was pretty lousy (Laughter). But, I do remember that, and looking at that enterprise as a student, and being really impressed with, well this is "where the science meets the river" I mean, this is really the boundary where everything we do or say and try to manipulate, this is where it gets done. It gets done by some folks putting on raincoats and sloshing around in the creek all day. And I sense among my students, not a reluctance, but it doesn't come easily, or naturally to do just do that, in the way that it might have at one time. We don't have, there's no real apprenticeship program. They don't see "me" doing that. They're trying to be a scientist, and their model of what a scientist is somebody that sits and plays with a computer and goes to meetings. That's what science is. That isn't someone who gets their feet wet and mucks around in the creek. And so the question of how you, and I do think the Andrews, probably more than most places, offers opportunities. I mean, we do have a place where people can go and live for the summer as a student. We have a field crew that has been very open and accessible, you know, to taking people in under their wing and making, that kind of learning opportunity happen. But I think we're the exception, and I don't think that, as Fred puts it, I don't think that we do all that well.

Al Levno: I want to add my bit to this whole thing. It seems like when we started, the idea that, we're talking about new ideas and new thinking, and I was thinking, "Wow," and scratching my head, "I don't see them!" (Laughs). My whole job, my whole career, for 35 years, and the first few years, I had Jack [Rothacher] and Ted [Dyrness] around. Then I think how Ross [Mersereau] would fit into this, who has been learning how to do that, learning what's important with watersheds, learning how to keep the records the same. And through all those years, we very, very carefully analyzed each new technique that came along, asking, "Is it going to give us the same numbers we had before?" If it doesn't, we'd throw 'em out. And now they're saying, "This crazy old fart wants to keep those A-35s on there still, and they're 1950 instruments."

Ted Dyrness: A good example of that is what we went through with measuring soil moisture in soil moisture studies.

Al Levno: Yeah.

Ted Dyrness: You go in through fiberglass blocks, and they got fouled up. We had good plots and went to neutron scattering, and tried to keep up with technology, and like it or not, it hits you, it didn't work as advertised. Dick was so ground down by just calibrating the neutron-scattering device that we couldn't meet our measurement schedule, and it gobbled up huge amounts of time.

Al Levno: Yeah, the same thing happened after the '64 flood. We got new money for the

“newest on the block” type of automatic instruments, or digital punch-tape recorders. Man, we got hundreds of those things. Put ‘em out in the field, then we looked at ‘em and said, “Wow, we can’t read the hydrograph with these things.” (Laughter). Threw ‘em all out. Had to do it all over again. We had a couple years of data that we lost or were incomplete.

George Lienkaemper: I’d like to follow up on something that Gordon said. It does not come naturally to do the kind of work that we ended up doing. This is the man who makes it be natural to you after a year or two of insisting that it’s done properly. And remove the sloppiness. It doesn’t, it’s not a natural thing that you just sort of walk out there and, say, it feels good to be out here in the rain doing this. No, it’s not, you know.

Fred Swanson: It’s boot camp.

George Lienkaemper: That’s right. (Laughter)

Ross Mersereau: One of the things, when you were talking about having everything on the computer and bringing it up, if you can you go on the computer anywhere and get data from anywhere, and be sure that it’s data that’s true enough so that you can use it with the idea that what you’re doing is still true? Whereas, if you know something about how the data was collected, and the procedures that you had to go through in order to keep it as straight and narrow as you could, then you’d have a little better idea of what you’re doing.

Don Henshaw: Yeah, I would like to see that. Basically, data has a shelf-life. If you collect information, from the moment you collect information, it tends to begin to degrade. Because as soon as the summer is by, and the field people that collected that information are off, with other jobs or gone, then questions come up, and you slowly begin to lose little pieces of information. Where we’ve really been lucky is to have people like you guys still around; Al, Ross, and George. Especially Al, though. Like when we go back and revisit this information, and Gordon can read, publish on this information, and then when the reviewers come up with questions, or whatever, we can turn to Al and actually fill in what the meta-data is. This description that you were talking about; describing what was done, describing how and why the rating curves, for instance, were built. Things like that. By having Gordon come back and ask these questions and write a paper, and get reviewers comments, has really forced us to turn to Al, who’s been the continuity that has really enabled us to go back and piece together those answers to all this stuff. In a sense, what we’re doing is reinforcing, and basically stabilizing the degradation of the streamflow data. And so, this effort, just by you coming in and looking at it again has really helped, and now we’re set. Now we know we raised up some of the metadata and bolstered up some of the background information, so that we know that this will always have some usefulness into the future, where many data sets, if you don’t have the continuity, the people leave, and you don’t spend the time to document information, that information is going to be absolutely worthless. I think that’s what Ross is saying. How do you know, by going to a data-set, whether it is worthless? If you don’t know some of that background information, that data may not be worth anything to you.

Gordon Grant: That’s right on, but even the sensibility that says you have to ask the question about whether the data, I mean, that something that’s so deeply engrained in you, and you

taught George, and you've got that sense, and a lot of what you think is, "How good is this, and do we trust it? And is it good enough for us to depend on? I think that unless we've gone through that experience of collecting, so you have a deep personal experience of how there are good numbers and bad numbers, you don't necessarily come to a data set with that in mind.

Ross Mersereau: Yeah, that's right.

Gordon Grant: You come to it with, "Can I answer the question that I want to answer?" And that's why the measurement, you know, the measurements or the resolution is okay, but whether it's good data or bad data doesn't even enter people's minds.

Don Henshaw: I think, as soon as you see it, like on a printed page, or a nice computer printout, it's that "This is official." This is it doesn't matter, it could be totally bogus, but you have no idea.

Al Levno: The exterior may have been blue and yellow and green, but -- (laughter)

Ted Dyrness: What you were saying about the interest of the general public, is really true. When we first started treatments of the three small watersheds, the public wasn't really interested. It was just that we were answering questions before they were asked, in a very real sense. I can't believe the sea-change that has occurred since then. Now, we've got all these new laws, etc., people are just up in arms, and watching your every move. That's different from the way it used to be.

Fred Swanson: At the Ph.D. level, people and other very savvy people, savvy to the technical aspects, but are really working the political front to that, are full-time employed with passion to raise these issues, so (laughing) we've gone from disinterest to professional-interest instigators.

Gordon Grant: Yeah. Did you ever think how much of your time was spent showing other people around, or talking with other people about what you were doing in the field? Did you do a lot of field trips back then? Early on?

Al Levno: I don't remember anything. (Laughter)

Ross Mersereau: Every summer during that IBP period, I took classes from Lane Community College and Central Oregon Community College.

Ted Dyrness: I think that was a point of a lot of change. I think during IBP, we started including a lot of academics in our work, and they had their own work, the lichen people, etc. And that was the time of change. But before that, I can't remember many field trips. You know, we had a few, but it was a red-letter day when a reporter would show up. (Laughter) We'd quake in our boots, and we were sure that we were going to be misquoted and so on, and we certainly didn't want that.

Gordon Grant: That hasn't changed! (Laughter)

Ted Dyrness: We certainly didn't go out of our way to encourage that kind of thing. Maybe that's partly it, but you know, we were, and I keep saying it, but it's true: we were more or less ignored by the district people, too. They weren't much interested in what we were doing. "Oh, this crazy guy," they'd say. "They're ivory tower and posey-pickers," and "let 'em do their own thing, as long as they don't get in our way." And so.

George Lienkaemper: And there was plenty of board feet out there. (Laughter)

Ted Dyrness: Yeah. Yet, they were coming to Jack and wanting to get some of their allowable cut on the Andrews. Jack would come back from these annual, semi-annual planning meetings, all wrung out, because he was trying to preserve the Andrews for just experimental purposes.

Fred Swanson: Huh. I remember one gathering in the basement of Blue River Ranger Station, and we were going out with the Congressman from Lane County. Now what was his name?

Ted Dyrness: Dombeck, or Porter?

Fred Swanson: No. Weaver. Jim Weaver.

Ross Mersereau: Oh, yeah. I remember him.

Fred Swanson: Jerry got us down there and he said, "Okay, what's your silver bullet? I don't want any rambles." (Laughter) He said to have our silver bullet, just a one-sentence statement of what we'd found that was important. (Laughter).

Max Geier: If I can recap, or revisit some themes we've been talking about. Fred had a pretty good framework here, these three branches of concern, and I wanted to suggest from what you've been saying here, there's a fourth one here, and that's the technological shift, in addition to people, that contingencies to natural and human environments, there's a technological aspect to what goes on out there, and possibly a fifth one here, which would be conceptual science. Maybe we could take it back to the paired-watersheds concept again. Are there ways in which the experience for new technology becoming available, have affected the science concepts, conceptual constructs? Which way does that arrow go? Or does it go both ways?

Fred Swanson: I think so. One thing that comes to mind is the role of modeling. But our work involves long-term field study with short-term field studies that fill information gaps. We have modeling to try to portray system functions in more general ways, and then, modelling may be one way to identify the kind of information gaps we pursue in short-term studies, then, conceptually, the nature of our questions change. For example, now we're interested not in the initial treatment effects of whole watersheds, but the effects of different rates and patterns of vegetation recovery, and the role of hardwoods versus needle-leaved trees in regulating water output. And the role of nitrogen-fixing species, such as alder along streams or *Ceanothus* in the uplands affecting the nitrogen economy, and whether that appears in the outlet of the watershed

at all, or not. And also, the watersheds were set up initially, it seemed to me, and I can be corrected, sort of in a black-box fashion of inputs and outputs for water, and then nutrients.

Then in IBP with the work of hydrology by Dick Waring and Jim Rogers and Dennis Harr, and probably others, who got into the internal workings from a water point-of-view, in modeling and studies, and then from a nutrient point-of-view. There were some studies, for example, on nitrogen pools and fluxes, through the soil profile from upper to middle to lower slope positions, and there's a bunch of stream work, going from the seeps, where the water first enters the stream, and then down the stream. From the erosion point-of-view; that was always treated as sort of what perked out the bottom, so I got interested in sediment budgets and routing and the movement, of soil and sediment within watersheds. I felt like in the '70s with the funding through IBP, that provided the opportunity to really get into the inner workings, and so it had modeling aspects, but it also had sort of budgeting and cycling and routing aspects to the thing. That would be an example of an advance that really occurred across a number of complimentary, solo works.

Ted Dyrness: So what you're saying is that the "black box" approach was really broken down as of the IBP era, and that funding allowed us to get more creative.

Fred Swanson: Yeah.

Ted Dyrness: I can remember, before IBP, when it was just a skeleton crew out there. We used to sit around, maybe for lunch, and say, "We don't really know what the soil hydrology is like here, and we do have hydrographs, so how do we explain the rapid response to rainfall? Are we getting surface runoff?" All these kind of questions, and we don't have the people to work on that. That's when we started to get people like Dennis Harr involved, and you're right, it was a black box. That's why I felt that, you know, I should do something (laughs) on the watersheds, up above the gauging station that nobody else was doing, because we needed to know.

Fred Swanson: Which led to - ?

Ted Dyrness: Huh?

Fred Swanson: Which led to? The veg-plot?

Al Levno: Yeah, the veg plot, soil movement, soil mapping, all kinds of things.

Ted Dyrness: Which then was of more importance than just for the watershed studies. That led to our emphasis on secondary succession on the Andrews. Yeah.

Gordon Grant: But back to Max's question. I would argue little of that conceptual development was driven by technology, *per se*. I think that it had more to do with nature, and maybe scientific minds, abhorring a vacuum. You know, at some point the black box of the system, it wasn't just the watersheds, it was the whole concept, the whole ecosystem, the forest ecosystem was being

addressed, as being more than just a biological desert, I guess, is the terminology that would apply. There was a canopy, there was a root structure, there were the mosses, there were many dynamics occurring. So, that was the development occurring in the science sense, as the sophistication of thinking about ecosystems improved, but I see that happening somewhat independently, or at least, not driven by technology. There are real technological advances, that when I think about them, there's just the proliferation of computers. I think that's unbelievable, what's happened in my career-span. Because that leaves us more room to develop, studies that could only be done by a lot of people, for one study. Now you could do lots of studies. So some of the proliferation of studies is being driven by that. That's probably, in order of magnitude, more important than anything else.

Then the details and other things that have gone on, including the development of GIS, the ability to look at the coincidence of data; you could do that before, but never really in very quantitative fashion. GIS is changing the nature of the questions we're asking and can ask, and particularly, the combination of GIS plus remote sensing. Because the ability to capture the information, then display and manipulate it, is really something quite different. Another technological advance is the ability to more easily record information. The fact that when Beverley [Wemple] wants to study how roots affect [soils and water], technologically, she can deploy to a dozen sumps, where she can measure streamflow, accurately compile it, display it in digital fashion almost instantaneously, that lends itself to that sort of short-term, site-level investigation Fred was referring to. We can literally have a "SWAT team" approach: "Got a question? We got an answer." That kind of a turn-around time that you don't have to wait 15, 20 years or more, anymore. That does change some of the questions that you can ask. You know, what else is there?

Al Levno: Laboratory techniques. Instrumentation for measuring, analysis, laboratory analysis, which was basic 15 years ago, but now (laughs) it's all replaced by computers. That [technological changes] did play a big part in the program, our methods and abilities.

Don Henshaw: Other people you see out there now, with the LTER, there's all this inter-site work possible, but people that don't even collect their own data, somebody who doesn't even collect their own data, can now go out on the internet and can pull in the Andrews, pull in several different stream-flows from several different sites, and actually do a comparative study. Of course, many of these things are sanctioned by the sites [LTER], but I think, having the internet available, creates another approach and makes it possible to do these cross-site comparisons. We have never gotten to that level before, as we were so locked into what we were doing, and just trying to produce at our own sites. There is that to think about also.

Fred Swanson: Right. And NSF really got internet moved up to the public, and pressed LTER early to develop the capability to do inter-site work through networking, through electronic connectivity, and then, active networking. For example, Mark Harmon said that he would not have tried to do LIDEX—The Litter Decomposition Experiment that involves 28 sites—if he hadn't had internet. Still, it was a pain in the butt to muster, constructive response from all the sites participating. But, if he had to rely on phone and paper mail, it just would have been impossible.

Max Geier: I have a question here. With IBP and LTER as turning points, is the key there, to those interdisciplinary, broad-question kind of opportunities, monetary or conceptual? In other words, were there people already working for the Andrews that had ideas of things they wanted to do, but couldn't because of money constraints? Or was the availability of money, as Fred was pointing out, pressure from NSF, bringing in new people, and new disciplines from outside, broadening the range of things being done on the Andrews?

Al Levno: Yeah, new people.

Ted Dyrness: We needed new people. I think that we were pretty well-occupied, stretched pretty thin. But we needed people with new expertise and interests that we didn't have in our little group. With 3 or 4 people, you can't have expertise in everything, and we recognized that, certainly.

Al Levno: But at the same time, those people, Dick [Fredriksen] and Jack [Rothacher] and you, were instrumental in hiring a lot of the original people for IBP that could help answer those questions. They did have a big hand in selecting the people, the newcomers.

Fred Swanson: Right. If you go back to that exercise I suggested earlier, and look regionally and especially nationally, or across the West, then plot through time, the start-up and shut-down of experimental watershed studies, I would bet those that had limited lifetimes had limited questions, and were pretty much hunkered down on the hydrology questions. These are real important, but there was a period, and we have an interesting paper by a tremendous thought-leader in this area, Luna Leopold, when important people in hydrology were challenging the value of watershed studies. They cost a lot, and they aren't giving us good answers, but where we're asking a whole broad suite of questions, then the science payoff per units of dollar investment looks a lot higher. And so, I think concept development and breadth of effort, helped to bring more light into small watershed studies, and then keep them going in the places where they have happened to have kept going. So, you can do a little bit of a demographic and mortality study, and get some clues on that.

Gordon Grant: If one were to do that, I think it would be interesting to plot the waxing and waning of key policy issues on the same time-scale. Because, it's interesting to hear discussions about the 1950s, when the interest was in creating water for municipal watersheds. That's 1950, and then in 1970, you've got the Clean Water Act, you've got sediment, and now in the 1990s, we've got sediment, we've still got water quality being applied to the forests that are suddenly being recognized as the source of all this nice clean water for this burgeoning population in the valley, and all these chip manufacturers that are coming out here. Maybe there's about a 20-year cycle on that one, where this thing sort of emerges, and each time it comes up, it's a little bit different than it was before. But each time it comes up, we look around for data to answer the questions. And where do we look? We go back to the small watersheds, because it's the only place plus the long-term streamflow records kept by the [USGS], which are not, for a variety of reasons, are not all that good, for answering some of these questions.

George Lienkaemper: A question just came to mind, I don't mean to go too far afield. Why was the Andrews chosen as an IBP site? Who promoted that?

Al Levno: Jerry did.

Fred Swanson: Yeah, Jerry Franklin.

Ted Dyrness: I remember about the IBP. He [Franklin] said he read about it in *Science* or somewhere, that this was a big, new science initiative based on systems ecology. He said, "If we want our program [HJA] to continue, we've got to get aboard on this. As far as I remember it, he waged kind of a one-man, what do you call it, campaign? I remember a meeting, here on campus, where he invited people from all different disciplines to explore the possibility of us going forward with a proposal for the Andrews to be "the" intensive study site for the Coniferous Forest Biome category in the IBP. At the same time, there was considerable interest on the part of the University of Washington to pre-empt the leadership of the Coniferous Forest Biome of IBP, and there was a certain interplay between the two sites. It was kind of acrimonious at times, but then, we got people together. It was Stan Gessel up there [UW], and Jerry Franklin and several others down here [OSU/USFS]. Early on, I got over to it and was active, in kind of a leadership role, and we decided that there would be two sites that would be the intensive study sites: Cedar River [UW] and the H.J. Andrews [OSU]. And then, what was the name of that lake?

Fred Swanson: Findlay Lake.

Ted Dyrness: Findlay Lake was really a bone of contention, because they wanted to add Findlay Lake as another site. And I remember Jerry was dead-set against it, as he thought we don't have enough [resources] to do Findlay Lake. We're stretching resources too thin to do that.

George Lienkaemper: The whole thing was like a hinge [paradigm shift], as we went from the limited resources of PNW researchers, to opening possibilities for different kinds of research.

Ted Dyrness: I think that was a turning point that you can't underestimate the importance of.

George Lienkaemper: Not technological, in that sense.

Fred Swanson: And it was social, and there were a couple aspects that. I think it was Jerry who said that the NSF had said, "Either Seattle and Corvallis get it together, or there will be no Coniferous Forest Biome." [Decide on a site (s), work together, or lose funding support]

Ted Dyrness: That's right.

Fred Swanson: So that forced this duality. Then, you can look at the way things developed in each area, and one has a legacy. Ours is built around the Andrews, small watersheds are an important part of it, but there's a lot more to it. So it's built around place and it's built around a

collection of people, and an important legacy is the collection of post-docs and technical staff here, which was not developed through IBP in Seattle. Also, I think the small watersheds are a better meeting-ground for interdisciplinary work. It's mind-boggling now to look at Watershed 10 and think that there we were all working together. We had different colored flagging so we wouldn't cross on each other (laughter) and now, you know, we're working at the landscape, the regional level. And they were hunkered on a lake, and hunkered on stands and glacial outwash, where there wasn't an aquatic component, so both of those ecosystem media for science ended up being pretty limited. But I think mostly, it was the way the sociology of things was run.

Gordon Grant: Let me follow up on that though, because I think I missed that whole chapter. But one of the things I could imagine, that really motivated a turning point, was the fact that you really had academic science in the game for the first time. Is that a fair statement? That was the major incursion of university [academia], and the strong university presence [OSU], at the Andrews.

Ted Dyrness: Definitely. Up to then, we had graduate students coming and going, but my, my.

Gordon Grant: One hypothesis, which would be interesting to look at the record, the turning off and on of these small watersheds, how many of the places had university involvement, and at what time? Because it's my impression that what the addition of the university did, was that it added a science profile that I don't think the agency scientists alone had. Regardless of how good the quality of the science was, I don't think the agencies, the government research agencies, were looked to as the real repository and leaders. That wasn't where the leading edge of science was to be found. And so, one hypothesis would be that as the universities entered the fray, and for those sites where the university did enter the fray, they brought with them credibility for science.

George Lienkaemper: Academic respectability.

Gordon Grant: Academic respectability, to the watershed issues that up until then had primarily been viewed as being, you know, having strong management implications, but --

George Lienkaemper: -- But not pure science.

Gordon Grant: But not pure science. Going back to Likens' and Bormann's writing in putting these forest issues out there, and having them blessed as major science issues in their own right, that having the university and IBP program, which was built around that same concept, I imagine that was a major intellectual turning point, because that's really what gave the site staying power. I throw that out there, but it would be useful and interesting to look at the record of sites and see how many of them got it. The ones that we point to: Coweeta, Hubbard Brook, some, more than others, you know, have that that sort of [scientific pedigree and credibility].

Ted Dyrness: And that was traumatic at first, because as Jerry said, "We've gotta get up to speed on this systems ecology." We were ecologists, but we were [in subspecialized fields of ecology],

not into systems ecology at the systems level. Fortunately, when this IBP thing broke and our attention was focused on it, at the same time, there was a multi-week short course given at the University of Wisconsin campus on systems ecology. Orie Loucks was there, and several other well-known, early pioneers were instructing this short course, and so, both Jerry and I, and maybe several others, went back there to this short course to see what is systems ecology all about. That fit in well with plans to develop a program here. But as I look back at it, the merging of Forest Service researchers and academics, sure it had its issues. Sometimes personalities didn't get along very well, but when we got it up and running, we did have committees, including Forest Service people, those from the University of Washington, and so on, and we met on a regular basis. We were always driving back-and-forth to Seattle at that time, and we were working pretty good. After all, we were interested in the same research questions, but we did have to get this jealousy between academia and Forest Service Research sorted out. A lot of the credit, and I say, most of the credit, has to go to Jerry, to insist that we gotta get on board. You know, or our program's dead [HJA]. We needed that.

Don Henshaw: And later, Fred [Swanson] after that [bridging cultural gaps], because after I came on, there was still a real separateness between Forest Service and LTER. We were talking to each other a lot, but there was still a lot of things happening independently. I think Fred really helped bridge the gap by not looking at boundaries between the institutions, kind of seeing those things as seamless. It made my job simpler and more effective, but also helped me look at this as one big picture, and not worry that if I'm Forest Service, I'm only looking at this piece of the same puzzle.

Fred Swanson: Well, there's a bit of a continuing struggle there. I think by just going forward and saying, we've got a bigger job to do, has been really important. But, it's interesting to look across sites and see the differences. Because, like you guys on the data management front, you're right in there, and that is potentially a huge hurdle, because institutional cultures are so different. The mindset of security the Forest Service brings to information management, because of the National Forest System's dominance of that, versus openness on the academic front, exemplified by that internet, that kind of openness, and yet the closeness in terms of a sense of personal property. Meanwhile, we're operating in the NSF context that everything has to be public, and we're guinea pigs for trying to do that, which has some really interesting implications. If you look nationally, and you look internationally, it raises really interesting issues, and it is partly this Forest Service vs. university stuff. But nationally, some of the other major sites doing small watershed work, they still have their Forest Service and their university data management systems, separate.

Ted Dyrness: Like Coweeta.

Fred Swanson: Yeah.

Don Henshaw: And Coweeta's been really hurt by that in some ways.

Fred Swanson: Yeah. Yeah.

Don Henshaw: By not getting those, by not getting past that.

Fred Swanson: Right, and then you look at other countries. Like last week, I was in Japan, and a grad student asked a question; “Why can’t we do LTER in Japan?” (Laughing) You know, it really all comes down to this cultural stuff, between labs, even within the same institution.

Ted Dyrness: Just a rivalry that would not operate?

Fred Swanson: Yeah, just very little.

George Lienkaemper: The whole idea of interdisciplinary is sort of, different work.

Fred Swanson: Yeah.

Gordon Grant: Or interconnected and just the idea that you do better. I mean, my image of Japan is you go to one agency, or one department, and they have a room full of flumes. And then you go down the room, and there’s another room full of flumes. That’s this department, and that’s that department, and nobody’s using anything. (Laughter) It’s astoundingly expensive and inefficient, and a lot of energy is consumed in maintaining those firewalls, and I think what I mean, there are problems and hurdles and things, but I think the benefits just so far outweigh the costs in that way, and yet it is a rare phenomenon. I think that we are probably much more the exception than the rule, in terms of the permeability of that membrane between the university and the [agency].

Ted Dyrness: I was just thinking, it goes back to the foresight of the Forest Service Research people that said, “We’re going to put our research labs on university campuses.” [Several voices of agreement at once]

Fred Swanson: Right now, as I heard this only yesterday, that it’s a Department of Agriculture issue about what’s permissible, or ethical involvement of Forest Service scientists in academic affairs, and where I come back to on that, and which I think is the model that Jerry gave us, is that we as Forest Service scientists must be full participants of the scientific community. That means, to be able to compete for competitive grants, we need to publish in the best places, we need to be full participants in scientific societies, and all that. And Jerry, in one way, he didn’t give much priority attention to university, this is university and this is Forest Service. He said, “We gotta get out there and (laughs) understand this old growth, save the world, and we’ve got these bigger issues to deal with. We’ve gotta be full participants.” He always was that way, and he went to NSF to learn the inner workings, so he could add more hay when he came back.

Ted Dyrness: He was always so strong that we should have these courtesy appointments and faculty appointments.

Fred Swanson: Yeah.

Ted Dyrness: You know, nobody put much weight in that way back when. You just went out and did your own thing, and you just liked the fact that you were both doing it on a university campus. But it's important, it works both ways, and so today, you go in the lab and you really don't know who works for who. And that, to my way of thinking, is ideal.

Max Geier: Well, a couple of interesting points here I wanted to get to. Ted suggested in his earlier comments that he and Jerry went out looking for academic input into this model of what was being done at the Andrews and with Forest Service Research, and I'm curious about the direction (cough), excuse me, the direction of ideas and concepts into the Forest Service and the Andrews, from academia. The question in my mind is: Does the Andrews drive science, or does science drive the Andrews? I know it's a mix of those, but maybe you could talk about that, the beginning of IBP to the present, and what are the more significant academic shifts that have been brought into the Andrews group? In other words, what is the focus of academic research as it relates to watershed research, and how is that integrated into what happens on the Andrews?

Fred Swanson: I would say a major one, and I wouldn't just call it academic, but it's got an interesting link with the Forest Service; that is, the landscape perspective, and landscape ecology. That has roots 25+ years deep in Europe, of one ilk, and Richard Forman of Harvard, who went to graduate school in the Seine, helped introduce it to the U.S. 15 years or so ago. Meanwhile, Forest Service issues; fire, watersheds, wildlife, have strong landscape aspects. And I feel like landscape ecology gave somewhat of a new conceptual framework, or better labeling for pulling together a lot of experience in somewhat disparate fields of work that Forest Service Research and land managers were already very involved in. So, I see small watersheds as we went from pretty much black boxes, thinking about their inner workings, but they were still small. And then, over the last 15 years or so, we've gone to bigger watersheds and bigger landscape units, and integrated our thinking much more richly about how and what forest dynamics do to succession, and variations with geographic and environmental location and disturbance, and then, how biodiversity and hydrology and carbon stores and flows and fluxes varies in larger spatial scales due to land use and natural processes. That's a dominant question within LTER, but the flow over into management and policy is huge. A lot of that conceptual framework appears in different policy things, including the Northwest Forest Plan, and certainly, watershed analysis, which is an important part of the Northwest Forest Plan, and is a medium for carrying some of that landscape thinking to the land manager. So, the stuff sort of came out of academic circles, merged with Forest Service experience, moved on into policy, and came ripping back out to management.

Gordon Grant: I would agree with all that. I think another thrust, though, is what I'm aware of, and I don't know how prevalent it is in ecology, although it probably should work there, too. One thing that comes out of the academic environment is there is an orientation to, geomorphology, towards process-based studies, which relates to this business of moving from the black-box description to something more detailed, where you're actually looking at the workings of the system in a way you could eventually, maybe hope to model it in some fashion. I see that as a major injection that is coming from the academic sphere. The histories of a lot of these disciplines are coming out of a kind of applied physics, at least in my area, the application of physics to river systems and watershed processes that go through the system in the same way.

And I think that it's interesting to see the way science is conducted within the National Forest system, because they employ people who have job descriptions that describe themselves as scientists, in other words, probably not research scientists. A lot of that work is either descriptive, or black-boxy, or kind of ad-hoc. It's not very connected, although there are people trying to make it more connected than it already is. And my impression is that had we had not had that university and academic injection, Forest Service research could have developed along those same lines. I have a feeling about that. Do you agree with that?

Ted Dyrness: Yeah, I think a lot of that. I'm glad you mentioned modeling, because I think the modeling came out of the more academic side of things. The models haven't panned out that well, as far as having a grand, systems model. But what it has done, and I'm always impressed with this, is showing us the gaps in knowledge. Thus, we've gotten more sophisticated climatic stations, much more, than we would have had otherwise. That's all, and I keep thinking about that utility model to show where our gaps in knowledge are.

George Lienkaemper: There's a real interesting interplay that seems to go on, and I think of it in terms of the long-term record, that an agency, such as the PNW is managing to maintain, through whatever means. It's important. Yet, from the academic side, that sort of investment, I believe, until the LTER, was not going to be made, and so the process-study, or the focus on process, is something that can be captured in the time-frame of a graduate student's tenure. Or the interplay between those two things, really made for the strength of watching it going from the, I don't know if you'd call it simplistic, but the black box approach to lots of "busy-ness" up in the watershed, looking at processes, and taking all of that together into some sort of synthesis.

Gordon Grant: And landscape view.

George Lienkaemper: And landscape view.

Gordon Grant: It lets you do the landscape view, because you have both. You need both to expand to that.

George Lienkaemper: To get back to Max's question, it sort of gets kicked back-and-forth, the underpinning long-term frame, and the value of that information has sort of resurfaced. And maybe it's the policy issues from the non-academic side that's driving some things right now, but through the middle of all this, it's been the injection of process-level work that's kind of kicked the science this way and that, so it's sort of an interplay, it seems to me, over the last 40 years.

Fred Swanson: I have two other points. One is the things we are able to do at the Andrews, even just in the domain of the small watershed, which requires larger collections of people than would be possible to bring from the Forest Service alone, the way we're structured, broken into teams or projects and all that, and we've operated under several organizational structures at PNW. Also, there are aspects, some of which were intimidated by Bob Tarrant at Carpenter Mountain Lookout, on hiring procedures and the like; hiring procedures and the importance of bringing in young people with different skills than the Forest Service had, which tends to keep people a long time. The age-class distribution for the Forest Service people, looks one way, and that of the

university participants, is probably bi-modal with a few senior people, and a lot of really young people.

Ted Dyrness: Yeah.

Fred Swanson: But to get back to the question, we talked a little bit about the landscape view as sort of coming significantly from the academic scene, a process view. Another one that seems to be emerging, you might say it's already emerged; would be the conceptual focus on things like chaos and indeterminacy and non-equilibrium systems. And IBP, I remember being told that part of the framework was, we had all the numbers we needed, and we just needed to plug them into these models. (Laughter) We'd have to spend a lot of time out in the field, and be there with Dick Waring (laughter), and so, the modeling aspect at that point, and budgeting, seemed to take a sort of equilibrium or deterministic world-view. Then the contingency thing that Gordon mentioned at present, the stuff around chaos theory and attractors, and that systems tend to go in certain directions, but there's certainly a lot of little jigs and jogs along the way. That is being discussed a lot in academic circles: complexity, understanding and managing flux and all that. Also, that it's going to have a big impact in management, because we've had these really tight and fast rules. Now, even as we switch from dispersed-patch clearcutting or broadcast burning to something else, we were exploring use of disturbance regimes and designing future management plans. Now they're coming out pretty deterministic-looking, and yet the system and information we have on the history of the system behind that, indicates a hell of a lot of sloppiness.

So, we're trying to turn our look at landscape dynamics around, saying, "How does microclimate topography and so forth, and veg-type, influence disturbance patterns?" It's really a mess. So let's turn it around and ask, "How did the ecosystem capitalize on that messiness?" There was this sustaining of a lot of properties of old-growth in our Cascade landscapes, just because fire was so sloppy in severity, in frequency, in spatial and geographical pattern, that there is sort of an hypothesis we're working on, there was sort of an attractor there. The system tended to have a bunch of old-growth attributes with scattered, big, old, live trees and a lot of woody debris, and practically all the time, there was that sloppiness. So, at any rate, that's sort of a manifestation that has policy-management implications, and there was general stuff, just a lot of interest in very esoteric circles in science, in chaos and complexity, things like that.

Max Geier: What's the level of interest in management for those kinds of questions?

Fred Swanson: Well, that one is still pretty remote, at this point. Although I think --

Gordon Grant: -- they don't know they're interested yet. (Laughter)

Fred Swanson: They've got a lot at stake, because that's where they'll get flexed, but they've gotta restructure their sociology so they can deal with flex, because they're used to having 14 feet of Forest Service manuals on the wall, telling them everything, you know, how to go to the bathroom. (Laughter) So, we've got some cultural shifts in order to deal with that. But I think on

the process front, and on the landscape front, we've made a lot of headway. And it was sort of a natural. Whereas this other one will be sort of socially unnatural.

Al Levno: I wanted to say, that's been the PNW role, and maybe the Forest Service role, to provide the long-term continuity for most of the work up there.

Fred Swanson: I agree. We were in discussion with some of the academics about the small watershed stuff, and all they're doing is being critical, and we said, "Well you guys are, you know, doing the Alsea watershed and -0

End of Side B, Tape 1 (of 2)

Beginning of Side A of Tape 2 (of 2)

Fred Swanson: I agree. And that brought it home when the other parties weren't keeping up the standards. But it didn't slow them down from using our data. (Laughter)

Gordon Grant: Following up on that, when you came out with the paper on using our data, the reaction I would get all the way around, was people would be kind of critical of it, but they wanted to reproduce it, replicate it. But they wanted to replicate it with our data. It was not a replication as in biochemistry, when you report on an experiment and somebody tries to re-do it. It was like, "Please give me your test tubes, so I can do your experiment in my laboratory!" (Laughter) That's really a key point, and there is no other repository. And you can turn that around into, "What's the responsibility of an organization that has that?" That's a question I keep coming up with. What should we be doing, because we're the only ones on the block with this stuff? It's like that discussion we had the other day, with Don and Al talking about what to do with the small watershed programs into the future. Do we have some money to upgrade them? How do we think about the next generation? And we're having to make decisions in the blind, because we don't know what the issues are going to be, and we don't know what the social setting is going to be, and we don't know who's going to come up and use this data.

But we have this responsibility, and here's the municipal water quality issue, which is going to be big, because it just isn't going to go away. And I look around say, "Who's got the sediment data?" Well, no one does. Alright, we do. We're the only ones on the block. So we have to step forward, much as you guys did, asking the questions before people got around to asking them. We have to be forward-thinking enough to be able to articulate those questions, to get them on the ground, to invest in them, including cobbling together the money to do it, which is danged hard when people haven't figured out they want to ask as the question [research]. You then have to sell it, and it's a gamble. But I think that's really important, because we're the only ones who will do this. I don't see anybody else out there. I don't think the survey [USGS] is, I don't think any of the others are. EPA doesn't do it, the Forest Service, by virtue of having this long-tenure land ownership and land management-stewardship charge, is gonna be the only ones to do it. And so it falls to us.

Fred Swanson: I was reflecting on this earlier and then this brings it out, to do it, and we don't market our team and our budget based on small watersheds. This has been a small watershed pep rally here [laughter]. But we don't market it that way, and if our marketing changes through time, and then we just sort of keep it going. I don't feel like we've been challenged by the leadership to cut it off. We've just had to deal with those issues internally. At least that's the way we've approached it. I'd be curious to know if Hubbard Brook and Coweeta market more heavily, or just, the way they present themselves, or what their problem analysis looks like. I suspect it would be a much more dominant part of their marketing representation of themselves. Because Coweeta is a hydro-, the hydrologic laboratory, and the whole landscape, physically and conceptually, seems structured around the small watershed. So I picture them as sort of being way over toward that end, whereas we, I don't even remember if we mentioned small watersheds in the one page we have to describe ourselves in the program, what used to be a research work description.

Gordon Grant: But it's interesting, because we don't, maybe because we've never really have, but we've viewed the small watersheds as taking care of business, more than anything that we're really out waving the flag over.

Fred Swanson: Yeah.

Gordon Grant: We've probably missed opportunities, too. I was just thinking yesterday, as I was on the phone two hours with USFS and BLM around a little brush fire they're fighting down on the Umpqua, and about timber sales, yadda, yadda, yadda. It's all coming down to peak-flow. The issue is peak-flow problems. We have Coyote Creek and discussions about turning on Coyote Creek [Monitoring program suspended for years]. You want "real" information about peak flow responses in the Umpqua watershed? We can give that to you. If I had a marketing sensibility, I'd say, by God, have I got a deal for you! (Laughter) For \$25,000 a year, you can have all the information you want about the Umpqua's peak flow! Somebody, maybe a Jerry [Franklin] would have been better at that, pricking up his ears at that last point. But these issues gets back to the responsibility thing. We all look to that, not just because we have good science, but also because we have raw data that lets us study something relevant, as these brush fires emerge and subside, and we might do a better job of advertising this enormous infrastructure, which is all hidden. I don't think people really understand, and probably less and less, because of the easy availability of the data, just how much effort it takes to keep this thing going. If we turned the key on a bunch of new watersheds, the whole thing, which is huge, multi-site, and comes to a couple of million dollars of infrastructure, rotting in the woods, for example.

Max Geier: Well, we've been here for quite a while, and I know some of you have other time commitments. Maybe we could end with a discussion, going around here, what we did at the beginning, of what you consider to be the most important research issues and questions that your group is facing now, and what some of the problems or constraints might be, as it relates to the history of where the group has come from? Do you want to tackle that?

George Lienkaemper: Well, my focus now is dealing with the spatial data. The big issue and challenge for me, is documenting the spatial data sets as thoroughly as possible. That may be a theme that you hear more than once, but certainly the biggest challenge. We have these great data sets, and we just need to make sure they're appropriately documented so, in fact, people can say, "These are good data, because we've got the documentation."

Fred Swanson: Right. I feel like we're at a really amazing place in the history of the small watershed studies with respect to the amount of work that is going on in them. There's Beverley's [Wemple] road work, there's a lot of the veg work, and I've got to go to an 11:00 meeting, and there's a bunch of that interfacing with the veg people. And we need the vegetation distributed spatially in the small watersheds for the much more refined hydrologic analyses that are underway. So, like we talked about going from the small watersheds as black boxes to looking at their inner workings in sort of a budgetary kind of way, back in the '70s, I feel like we're making a whole quantum step, or two or three right now, on that for hydrology, somewhat in nutrients, and maybe in some other realms. There's Steve Wondzell's hyporheic work, which is adding a new dimension to the small watershed studies, and the fact that Watershed 3 got reamed out, which was commented on early in the interview, was an incredible experiment. It was so thorough in hitting the alder in there, just remarkably thorough, and so that has a nitrogen and a water story to it.

So anyway, I'm just really impressed by the diversity of work going on inside those watersheds, and that also links with new remote sensing work, which will, by trying to detect cover, veg cover and leaf area index, can link to biomass, nutrient capital, and carbon studies, as well as with the water studies. So, I feel like there's a level of research attention that's much more intensive and diverse than it's ever been before. And there are improvements of databases and data analysis from long-term data sets, including the spatial aspect. I'd like to have a whole series of layers and conditions mapped through time, to help us interpret the progress of the watershed. All this is happening in the 5-8 year period, probably. Then I think, we're going to have another set of questions. Are we going to go in and do some thinning, and do some more treatments, or go manipulate the roads? And see if we can measure hydrologic effects? The young stands were getting pretty crowded, but then there was a bunch of snow down, and there may be new questions around that. And then, there's the whole flood aspect of the story. So, I'm wondering if we're going to have an episode, maybe a decade of intensive looks at the small watersheds. And then, we're going to possibly, drop back from it significantly. I don't know. And then, do we make sure we've maintained our corporate intention to keep the baseline stuff going? And how do we do that under a new administration. Al had his "graduate boot camp" for a long time, and so, maybe if we can (laughing) keep it going. So anyway, now we're starting to look ahead and be concerned about that next phase, although we're really busy in the current one.

Don Henshaw: Well, I'm an information manager, so I don't have quite the science approach, but I do look at information management as a science. I think my participation with the LTER and going to these national meetings, has really helped promote the fact that, this is a science. We are developing ways to promote inter-site science through the internet, promote development of meta-data structures, or documentation structures, and improve data access among sites, and

also within the sites. One of the things we're trying to do here, with the watershed study, is to be able to take George's spatial layers and maps and be able to link them with other plot data sets, link them to their publications. Also link them to a set of key words, to a set of locations, to people, to maybe remote-sensing layers, so we have all these different pieces that we'd like to have a database scheme, but to accommodate it so that you could come in through the web, and pick a location on a map, for instance, and be able to see the studies that took place on that watershed, and who the people were that were involved, or come in with a person and see what they did, or come in with a key word and be able to see all these associated pieces of information. So, for me, that's a huge challenge. That's going to be the next 5, 10 years, as I see it, but that's really what we're trying to do, and to develop the mechanisms to do that. And to have an information system to be able to accommodate those types of questions. Make that data available, on your fingertips.

Fred Swanson: I have to go to this meeting, and I'm real interested in what these guys say, so I'd like to be able to read it in the transcript. So I was wondering if you'd noted our names around the table, so that at least, for the beginning and these ending comments, that you'll make sure that we, know you.

Max Geier: Sure.

Don Henshaw: That was Don. (Laughter)

Ted Dyrness: That's why we went and introduced ourselves, because the transcriber recognizes our voice, and every time anybody says something, it will be attributed to the correct person. I just am struck by the tremendous legacy we have in the small watersheds. Yeah, it's a credit to the guys like Al, Ross, and George, that through the lean years, that they kept at it, as far as collecting the data, and made sure it was high quality and so on. But on the other hand, it's rather like having the tiger by the tail. We do have now, a tremendous responsibility for continuity, to continue. It's the same way, in talking to Steve Acker on his permanent plots, that it's a responsibility to go out there and collect new data, in his case, on mortality and growth rate on trees, and so on. But it's a cumulative thing. That as long as you do that, it's worth more, but it's a heavier responsibility to continue it. And you're continuing it, knowing not what questions will be asked in the future. You know, when we started, we didn't dream the questions we'd be asking right now. And now, we can't dream the questions that the people will be asking in the future. I know I'm sounding like a pep rally for the small watersheds, but in a way, we need to look at the small watersheds as just another set of permanent plots that we are responsible for, and it's incumbent on us to be a faithful steward. And that, in the future, all these measurements will continue, and the implementation will be serviced. It's a tremendous responsibility. And it's getting heavier, because there's more and more of what we are a steward for coming generations. Being fit, you know. (Laughter)

Gordon Grant: I could be frank and say, "I agree with that." But I'm not gonna do that. (Laughter) I've never done that. I too, am struck by the fact we're sitting in this room today, talking about this, which owes itself to a couple guys who chose the place, and then, a lot of

people who kept it going. And I think the challenges for the future are going to be how to look at that list of drivers, and the contingencies embedded in all those drivers—the social, the people, the science, the technological, and the full suite of that changing landscape, the natural system. The challenge for us right now, is how do we pass on the legacy we've been handed? I think that's incumbent upon us as the beneficiaries of that legacy, to then pass it on. And that's really going to be the challenge. Because I think the nature of the environment has shifted, so that what Ted says is, "We gotta keep the place running." Well that's, you know, fundamental. That's a given. But it's even more than that. And the question is, how much can we anticipate the questions that are going to be useful down the road? And how do we set ourselves up now, so that when those questions get asked, we already have 10 or 15 years of data?

That way, the enterprise really pays for itself. And at that moment, the whole enterprise pays for itself. How do we deal with the people issue? I'm looking at the age distribution of people sitting in this room. We're all a bunch of white guys with gray hair, you know? (Laughter) Where is the next generation? Where is the next legacy of field people, science people, and data information people going to come from? Are we looking for that? Are we training those people? Are we training them the way that they need to be trained? Fred talked a lot about the science issues, and how these are likely to evolve. I see other places, because I think a very important point which has already been brought up, which is this inter-site business, but if we already have people, like David Post here, who's looking at our watershed data in the context of a set of other watersheds from around the country, and I spent the time in Italy and saw some of their experimental watersheds, and these experimental watersheds are international phenomena. Nobody has ever tried to capitalize on that full set. Nobody's even put a data set together of where these places are. One of the things I see happening in the future is that we're going to be increasingly involved in the larger-scale, multi-site, international-in-scope enterprise. What role do we have, and how do we best play that role? How do we also play the role of helping other sites come on-line? I mean, there are developing countries that are now looking to us.

Don Henshaw: Right.

Gordon Grant: For expertise, "where should we put our watersheds? What should we be studying with them?" These are the challenges I think we're faced with. I think these are the real issues, and our success or failure, the success or failure of this generation, my generation, is gonna be whether we did a good job in handing-off around that set of other issues.

Al Levno: Well, I think I'm more simplistic. As I'm looking toward my gray years and ready to retire, I'm just concerned that we have the foundation down and we can continue all the things we've been doing into the future. I keep wondering what's fallen through the cracks, and whether we can continue. It can be a small thing like, the snow-cats [transports for winter weather]. Are we looking out for ourselves? And that's always been a pet project for me. Making sure that we have snow-cats. For years we haven't had a snow-cat, and we have had challenging absolute travel conditions. I guess I'm hopeful that enough of a foundation has been laid that would allow us to connect the baseline data that we need, that we would continue to do that.

Ross Mersereau: I keep thinking, it's been 9 years since I retired, and things have really changed.

Ted Dyrness: In just 9 years?

Ross Mersereau: In just the 9 years. There are things that have been built, are being built, on the Andrews that I never would have even considered when I first started, and I think of all the things that the Andrews has in the small watershed area, that stands out to me, is the type of people who have been doing the work, and why I believe the data that comes from that is as good as it can be from anywhere. And even when you think of the fact we were using those old A-35s, because although you have these fancy machines around, they still don't give you data that you can trust like you do the A-35s. I think that's part of it there. If we could, if you could go out and find a machine, and reach down in your pack, shove another one in its place and keep on going with it, you might be able to do something like that, but when you go up and you look at an A-35 and it's down, you know you can take it apart and put it together and get it to work. And it keeps right on a-going. And that's the big thing. You don't lose any data.

Ted Dyrness: Yeah, yeah.

Ros Mersereau: So what I keep hoping is the people that are following us are being real mindful of how they're doing the work.

Ted Dyrness: Yeah, that's right. If that stuff is lost, everything is lost.

Al Levno: I think the people who have been associated with the Andrews have been special, hard-working people who are willing to, to put their job ahead of everything else.

Ross Mersereau: Oh, yeah.

Ted Dyrness: Go out at 2 o'clock in the morning and put the stuff out.

Ross Mersereau: And doing like Roy did, going around.

Ted Dyrness: On the roads, yeah, and just walking.

Ross Mersereau: Yeah. That's why I don't go for your horsey thing. When they went out there and searched for those watersheds, it was bi-pedal! (Laughter)

Gordon Grant: I like your point.

Max Geier: I've got a lot more questions, but we really don't have time for that, so I appreciate your time here, and thanks for coming. And if you have anything you want to add about this, or what we've talked about here, feel free to contact me directly, via e-mail or drop a note in my box.

Ted Dyrness: You haven't talked to Julia [Jones] at all?

Max Geier: Not yet. I'll be talking to her soon.

Ted Dyrness: Yeah, I think, especially in view of the fact that she's not here, that she needs to be contacted alone or something.

Max Geier: Yeah, I was planning on doing that whether she was here or.

Ted Dyrness: That's too bad, because she really expressed enthusiasm for this.

Gordon Grant: I also wonder about talking with the next generation, people like Beverly and Dave Post and Sherri Johnson, people who are using the small watersheds.

George Lienkaemper: Reed Perkins.

Gordon Grant: Reed Perkins. Right. And it might be useful to get a group of them together, because I think this setting works really well for bouncing off people.

Ted Dyrness: That's why Fred had this dream, and I mentioned in my memo, remember, that the grad students would come in later, and I'm just sitting here thinking, and when we're discussing, my grad students would really eat this up. To have this kind of a conversation, you know, it would give them a historical perspective. I hope they would eat it up. I don't know. (Laughter)

Al Levno: I was wondering about continuing this, maybe, you know the hydrology meeting ["Stream Team"], the weekly meeting, we could get together and discuss just one small watershed meeting, and then 1, 2, and 3, and go through it like that?

Ted Dyrness: Sure, they had that hydro thing going weekly, didn't they?

Gordon Grant: We haven't done much of that this year, but we initialized it.

George Lienkaemper: You mean, just talk about what the history has been and where it's heading?

Ted Dyrness: Yeah, and sort of cross-cutting. That point is well taken. We need to encourage the younger generation. Where are they?

George Lienkaemper: They need to hear about the good old days. (Laughter)

Al Levno: Yeah, that's quite a thing. To look at those pictures. (recording ends)

End of Side A, Tape 2 (of 2)

End of Interview