Interview with Gordon Grant by Max Geier on 6 and 10 October 97 at the Corvallis FSL as transcribed by Elizabeth Foster.

Gordon Grant traded in a career as a western river guide for fluvial geomorphology researcher. He completed his PhD at Johns Hopkins University in Baltimore, but did his dissertation work in the Andrews and French Pete Creek under the advisorship of Hopkins professor and fluvial geomorphology guru "Reds" Wolman. He made his way into a Forest Service research position and studied topics such as channel form at multiple scales, sediment transport, cumulative watershed effects of forest practices, and geologic effects on streamflow.

Side A, Tape 1 (of 3)

Max Geier: How much do you know about the project so far?

Gordon Grant: I don't where things stand. I knew that you had been contracted to do it [history and oral histories], that you had been talking around, and then I saw some things recently about trying to convene some small group to come up with some thematic dialogue. Which I think is a really good idea. That will be quite interesting, and I'd enjoy participating in that.

Geier: Yeah, Tim's trying to schedule that for me.

Grant: I think that is a great idea. I think stuff will come in the round robin that you might not otherwise extract. I read the early prospectus for it, and so I have some sense of some of the cross-cutting themes being developed, in chronological fashion. It looks really interesting and like you are really trying to set the thing up in an intellectually challenging and sophisticated way that is faithful to the history. You know, I was impressed by the structure, but that is about it. I haven't talked to anyone who's been interviewed so I don't know how you --

Geier: What I am really trying to get is a good, clear understanding of your background and the people who seemed to work together at the Andrews. By background, I mean, your personal experience and training, your personal origins, and what attracted you to the Northwest.

Grant: Well, I was born in New York to a New York family, a strongly east coast family. My exposure to Oregon really began at the age of 13, when my father took a position at University of Oregon's Biology Department. At that time, I remember thinking that Oregon was even more exotic than California, as far as I could tell. I didn't know anything about it, came out here, in '66, and it took me about three years here before I began to realize that I was falling in love with it. Up until that time, I was just amazed at the difference between Oregon and elsewhere, and just what people did. I remember the first year in Oregon, we went to see the McKenzie River White Water Parade, an event held in the McKenzie the weekend before fishing season opened. And I remember sitting on the shore at Martin rapids, watching these drunks go through the rapids drunk, and the sheriff going up-and-down there fishing people out of the drink. I was just amazed events like this happened, and I remember thinking how much I wanted to be on the river. It was so different from my background, which had a lot of water risks, because I grew up, spent my summers, in Cape Cod. I had a very ocean-oriented life as a boy, but hadn't really been in a place in which there was so much water, rivers as well as ocean. So my background really was that I was born in the east, moved to the west at the end of my "Wonder Bread" years, but was very much of an east coast family. It's interesting that all my three other brothers and sisters and parents are all back on the east coast. I am the only one that bonded with the landscape. But that really didn't happen for me until I went off to college, in '69.

Geier: U of O?

Grant: Actually, I went to Reed [College, Portland, Ore.], for at least a couple of years. I fell in with a motley crew of adventurous souls who were interested in exploring, in all senses of the word, what there was to find in the local geography. And so, for me, the real connection came because my roommate, who was from Tennessee, brought a boat that he had built for navigating Tennessee waterways back out with him to Reed. It was one of these flat-bottomed, things that you go down to black water swamp in Tennessee with -- the cottonmouths hanging from the trees and the alligators -- and he was prepared to try it on the whitewater rivers of Oregon. Which, as it turns out, was a major mistake. But I was all too eager to join him in this folly. So, I went out and bought myself a raft at a time when, this is about 1970-71, people didn't do it a lot. And I fell in love with, I mean, we tried desperately to kill ourselves in several different ways. Stories that I won't tell, but I fell in love with the rivers in particular and the landscape in general, and so that really became the formative, I mean in terms, of giving me a focal point, a geographical focal point. I realized the Northwest was where I wanted to be. And that persisted, for me through years of, well, dropping out school, working on a fishing boat, and planting trees and landscaping. Doing stuff like that, and eventually going back and finishing a degree at the University of Oregon. That is where I got my undergraduate [degree]; the University of Oregon.

Geier: So, you were doing landscaping work in between there?

Grant: Yeah, landscaping, and in general, you know, getting my hands dirty and pretty wet.

Geier: What was your major at Reed?

Grant: At Reed it was biochemistry, and I was all set to be a biochemist just like my father. But somewhere along the line, I didn't like living around the smell of benzene all the time, was what it really came out to. It was discovering all of this wonderfulness in the great outdoors, and the contrast between that and working in the laboratory. Even that stuff is pretty interesting to me, intellectually, the aesthetics of it just didn't fit with what I felt myself moving towards. So I dropped out of that, but retained a pretty strong interest in science and scientific pursuits. But then I took this long, end run, of really getting into rivers in a very big way. Became a white-water rafting guide and worked for 12-plus years as a professional.

Geier: Where was that?

Grant: Mostly in Oregon, but also in California and Idaho. For a while, I worked for other companies, and then, I actually owned a river company in Eugene for about five years. And led trips to haul them in. That really was my identity. I saw myself as a river guide first, and a banjo player, and somewhere along the line, a student. When I was an undergraduate at the U of O, I was in a program, in fact the only way I could talk myself into going back to college, was a special program in the Honors College, that essentially, let you do whatever you wanted to do, and call it a college degree, as long as you could get three faculty members to go along with it. Not too different from graduate school in that sense. I was fortunate to be in a program where people thought my idea of going out and studying the Willamette River was good, so basically, I used the Willamette River as the focal point of my undergraduate education. Then, I took lots of things around it on the geology, the history, the ecology, and political science of the river, and I really didn't know what I was most interested in. I mean, I was interested in the river and it seemed a pretty interesting entity to try to build on from the sciences and political sciences, hang everything together around a common theme with understanding the history of the river and the way the river was used in the past and how that translates to the future.

Geier: Who were some of the people you were working with on that?

Grant: The guy who was the most influential faculty member, really guided me through most of that, was Stan Cook, who was in the Biology Department, but he was also an ecologist and very committed to getting an environmental studies program started. This was when environmental studies were just beginning to be talked about as a kind of integrated whole. We tried to get our environmental studies program going at U of O and get a grant to underwrite it, and that is actually where I met Fred Swanson. Fred was just finishing up his Ph.D., and I think Stan was a member of his committee, or at least had been an associate. And so, I remember sitting in Stan Cook's office, just having some random discussion about the state of the universe and things like environmentalists. And Fred Swanson poked his head in the door, and Stan introduced us. And that turned out to be an important meeting, although at the time it didn't register.

Geier: Register?

Grant: Didn't entirely register. Well Stan said, "You guys ought to, you know, get together at some point." And I did end up taking Fred's class which he was teaching at the University of Oregon, a class in forest geomorphology. Which in a class, I think Fred was the first person I heard use the word geomorphology in a sentence, far as I can tell. And that was my first time of hearing about the H.J. Andrews, because Fred at that point was beginning to do work up at the Andrews, and was using examples from the Andrews to flesh out a class, and to study the effect of human management on forests, natural processes with in forests, stream channels, and so forth. At that point, I was completely focused on rivers, not this business about talking about dirt and organic matter, or trees. Let's cut to the chase and talk about rivers. So, I enjoyed the class and I enjoyed Fred. Fred at the time, struck me as very interesting and a really committed guy to the subject matter, committed to educating people on the subject matter. But, it wasn't really what I was doing, so it was kind of a footnote at that time to my other classes.

Geier: Did you take trips up to the Andrews then?

Grant: I don't recall. I don't think so, as I think there was a class field trip to the Andrews I missed for some reason, is what I remember. So, no bells went off there and I didn't actually see the place. But I can remember my first trip up there, and that was, if I can jump ahead in the history. I graduated from U of O in '77. Again, with a very sort of eclectic degree. What happened, was I got a grant from the Oregon Committee for the Humanities to fund a traveling "dog-and-pony" show. We called it the Willamette River Chautauqua. It was a slide show, one of these six projector slide shows together with oral histories, followed by a discussion about the role of the river in the lives of the people sitting in the room.

Geier: You say we. Who was involved with that?

Grant: There was another guy I worked with who was a photographer. He took the pictures, and I did the taped interviews, and it was really fun. Again, I hadn't finished my degree, and this was work towards a thesis I was pulling together as a senior. I traveled up and down the Willamette River, interviewing old-timers, people who had been steamboat pilots or had run log drives in the river, or Indians who had fished at the falls. This was my real excursion to history at that point. I really enjoyed and had a good time with it. Finding these people and doing exactly what you're doing; sitting with a tape recorder and asking them questions about the river. I wrote it up as a thesis, then we put it together as a slide show, and took it up and down. Put a show on in Corvallis, Independence, Portland, and Eugene, and we would usually attract people to come and talk about the river in some form or another. We got Barry Lopez to come and read Indian stories about the river, had some guys from the political science department talk about it, and a guy from landscape architecture. We had a lot of fun with it, but it was interesting for me because it gave me a chance to really try on the humanities in a sense. I really enjoyed it, but I realized it was not going to be my life's work. I mean, there were other things pulling on me.

A lot of that really came down to time spent, as part of my career at U of O, I found myself teaching classes about rivers. It was an experimental college kind of thing, and I did a one semester thing on rivers, culminating in a raft trip down the Rogue River. And it was clear that "the river" was going to be my focal point, and at some point I began to think about, well, maybe there is a science of rivers. I need to know what to call it. I had heard about geomorphology, but I thought that was associated with forests because that is the way we had studied and talked about it. But I began to read about it and realized that there were people who made careers studying rivers, and that was really the motivation for me to go on to graduate school, which I did, in '79. And I decided at that point that I wanted to go somewhere other than the Northwest for graduate school. I loved the Northwest and I expected to come back to it, and was running rivers full-time in the summertime, so it was always going to pull me back. But I really wanted to go the East coast. I think I wanted to test that, see what my affinities for that as well were.

Geier: Had your family gone back there?

Grant: They were still in Eugene. Maybe there may have been something there, who knows? But I was interested in trying on a different way of looking at it, and the concept of going east to get educated, appealed to me. I ended up going back in '79; to Johns Hopkins in Baltimore, which scared me no end. The idea of trading in the foot-loose and fancy-free, or academic-free lifestyle, for what I took to be a fairly-focused educational environment at Hopkins.

Geier: Why Hopkins?

Grant: That's interesting. I had heard through the grapevine, because most of my decisions at that time were being made in a somewhat muddled way, as they are now. But, I had heard through the grapevine that people who were doing rivers, a lot of people who were doing rivers for the U.S. Geological Survey, had been trained at John Hopkins. That's all I knew. I literally went to the college library, and I got some catalogues out. I was thumbing around and my search image was something that looked like, well, environmental studies with a river focus. And I wasn't sure whether it was going to be rivers, or ecology, or rivers, or politics of rivers. It was all kind of muddled together. So, the places I had looked at, were places like the Yale School of Forestry that looked like you could sort of go there and do anything you wanted. Cornell, because it had a department of natural resources, which sounded like rivers, although it turned out to be mostly lakes. Hopkins was kind of a dark horse, because I had heard about it through the grapevine. Well, I got there, and again in this muddled way that I did things at the time, I said, "Well, I am going to go look and see what these places look like." So, in the dead of winter, I went back there, and without telling anybody I was coming, I just sort of showed up.

I walked into the basement of Johns Hopkins, the department office, and was looking at the blackboard all academic buildings have, which have everybody's name and door on it, and I saw, M. Gordon Wolman, Chair. Where have I seen that name before, M. Gordon? Then, I realized years prior, I had been sitting in the main office of the river company I had been working for, and picked a book out of the shelf. It was called, "Fluvial Processes and Geomorphology," a book about rivers, the science of rivers, and I realized this was the guy, that the second author was M. Gordon Wolman. It all sort of came to me. I said, "Oh, I bet he knows something about rivers!" (Chuckle). Turns out to be one of the great gurus of the field. But I had, it was pure serendipity. It was utterly unplanned on my part. I just discovered all of this, while I am staring at this blackboard. On top of that, he wasn't even there. So, I didn't even get a chance to visit with him while I was there. But everyone I talked to, I said "Here I am, and I am Gordon Grant, and am interested in rivers." Everyone said "Well, you've got to talk to Reds [Wolman]."

So, that is how I ended up at Hopkins. I actually met Reds on the phone when he called to say, "Well, we will let you in but we don't know what you are going to do. Seems like you are clearly interested in rivers and have done a lot." But we "Just want to let you know that when you come out of here, we expect you to know something about something." And so, at the time, I wasn't quite sure what that "something" was, but I knew it had to do with rivers and this guy seemed, well, I really enjoyed talking with the guy on the phone. He is just very interesting, and this charming and insightful guy, and you could just tell this from 3000 miles away.

Geier: Had you read his books before that?

Grant: Well, I had read the first chapter. And the book, it turns out to be one the key books; it's like a bible. It turns out this is literally the classic in the field, and still is. It is a classic, not only because in one book it sort of encoded everything that people knew up to that time, and had learned and studied about the science of rivers. But it is a marvelously lucidly-written book. You read this thing, and it is the model of how a textbook ought to be written. And I remember actually sitting and reading it as a textbook. You know, it read well, and it is a very rare kind of thing. So, it's pure happenstance the way things worked out, but at the time I didn't know any of this stuff. Even today, when you're entering a new field in fluvial geomorphology, it is not uncommon for people to go here just to find out what people, what these guys, the three authors on the book; Luna Leopold, Reds Wolman, and John Miller, are doing. All three of them have just been very, very significant people in the field.

Geier: Your work, though, was on rivers of western Oregon, wasn't it?

Grant: Well what happened was, and this gets me back again, if I am going on too long, just let me know. What you're probably asking, how I got back here, and that too has bit of a story to it.

Geier: Or if they had any problem with you doing the research that far out.

Grant: Absolutely not. I mean Hopkins was completely delighted, as far as I could tell. You sort of swam in, swam around the place, or floundered as the case may be, until you figured out something you wanted to do. Reds would sort of help you a bit, but he kind of left it up to you to figure out what it was you were really interested in doing. For me, it's what worked, what got me back to Oregon, which I always kind of thought I was going to do, but hadn't really figured out how or why. I was running the McKenzie River with a commercial outfit, and I ran into a group of people that I knew at the take out who invited me to a pot luck. At the pot luck I happened to meet another friend of theirs, Gordie Reeves. And as things go at potlucks, we got to talking and I asked, "Well, what do you do?" "I study fish." "'Oh, that is interesting, where do you do that?" "Well, I am at Oregon State." "That is interesting.' We start talking about our various interests and he said, "I'll send you some papers." Well, I would like to find out what people in Oregon are doing about rivers and fish and all this stuff. He sent me a bunch of papers.

And I noticed when I looked at the papers, that at least two-thirds of them were written by Fred Swanson; Swanson and Lienkaemper, Lienkaemper and Swanson. These were all early papers of describing the forest environment out there. I was fascinated. "Gee, I remember this guy, he taught me," and this was now seven years later. That would have just been it except for the fact that Gordie Reeves wrote me a note, somewhat angry, saying, "Could you please send me my papers back. At the time, he is a struggling graduate student, I am a struggling graduate student, and the Xerox machines were sold a dime a copy, and none of us had a dime. So, the idea was, I will send you the papers, you make your copies, and send it back. Well, I had obviously not fulfilled my end of the bargain. So, not having a dime, I sent Gordie his papers back, but I said, maybe if I write this guy Swanson, he will send me a free copy of this thing. So, I wrote Fred a note and said, "Look, you probably don't remember me. I was the guy back in Oregon, took geomorphology from you seven years ago and asked far too many questions, and could you please send me the papers?' He wrote me back a very nice note, "Oh yeah, I remember you, sort of a trouble-maker in the third row. Here are your papers, and by the way, what's the Wolman group doing?" Fred, being in this field, knew very well this guy I was working with. Well, "Wolman group" was kind of a joke, because, there was no group, I mean, we were all kind of studying with this guy. This was a marvelous guy, but, it was not like you had an audience with the pope. You would go in there and have your twenty, twenty-minute audience, and he would impart wisdom and intellectual nourishment, and you'd leave. That was the Wolman group.

Geier: Did he have a lot of students?

Grant: He had a lot, a surprising number of students, and there was a certain self-selection process, because you had to be able to handle being with a guy, who you would see only on these occasional forays, and his one class, which was wonderful, basically this book, but it was really a lifetime experience if you want to call it. He had about six or seven students at the time. He has had an amazing history of turning lots of people out, all with sort of the same laissez-faire style. A footnote to this thing was something Fred said, "By the way, if you are ever in Oregon, stop by and say hello." I said, "If I get to Corvallis, I will do that." But the following summer I went out to Oregon and happened to be at the University of Oregon's campus, when a symposium on Mount St. Helens, which had blown up the year before, was going to be held. So, this must have been like '81. I was told about it, and walked into a darkened room. I couldn't see who was giving the talk. But it was a pretty interesting talk about the ecological and geomorphic responses of the mountain and mountain side vegetation to eruption. Lights come on, and it's, Fred Swanson, and I thought "I know that guy!" So we, after the talk, we got to talking. And he said, "Well, I was interested in knowing what you're doing, and why don't you come out to the field with us. We are going up to look at this other river next week." So I tagged along, and by the end of that day, we had sort of reached some kind of gentlemen's agreement that said, if I wanted to do something out in Oregon, I could do it and he would see to it that somehow, I wouldn't starve while I was doing it.

That's really how I got back out here, I mean, that was really the connection that launched me into the Andrews program. Because I remember that spring when I came back out, when I finished my course work at Hopkins, I came back out to start working on a dissertation, sort of a vague notion that I was going to study something about the effects of timber harvest on streams. But I didn't really know what exactly that was going to be. Early on, Fred said, "Why don't you come up, we are going to have a group of students up at the Andrews." Kind of like HJA Days, an early version of that, and so this is, this is now '80-, it must have been '82, spring of '82. So this is Jerry [Franklin], Dick Waring, and Phil Sollins, and I don't remember Ted [Dyrness] being a part of it, but he might have been, Dick Fredriksen and Dennis Harr. It was a whole group of folks, and I remember standing in a circle in a parking lot in Blue River, going around the circle and doing the name thing, and being really impressed by the diversity of people and different disciplines represented in the circle of students and faculty. And it seemed like a big, friendly scene, pretty open in the sense that I could just waltz in as sort of a walk-on from the east coast, and find myself there. The main thing I found myself reflecting on was, how do all these people manage to work in one place without getting in each other's way? It seemed like there was one woman who was studying mammals, small mammals and riparian zones. Then there was someone else digging pits in riparian zones. (Chuckle) It seemed like there were going to be all kinds of problems, just getting them, keeping the small mammals out of the pits.

I remember driving around up in the Andrews that day, and that slightly nauseous state one gets in sitting in the back of a crowded van on logging roads, you know, lots of un-washed bodies, and I remember again having that feeling I had in Fred's class, which was, "Well, I always do rivers and it doesn't seem like anyone else is doing rivers." Then, I started running into guys like Stan Gregory and Jim Sedell. I think Jim, he had been on that first field trip I had taken, and he was doing fish things, but clearly was invested some of the uses of habitat and what was defined as "habitat" by people, that became the word I used to describe how what I did fit into what everyone else did. Because I was really focused on the physical structure and dynamics of mountain streams, what I was beginning to get quite interested in. That fit with my river guide background, and I'd spent a lot of time looking at rapids, and thinking about how they formed and why they formed, but the linkage for me seemed to be to talk about habitat because most of the people in this group were not geomorphologists but were ecologists. So there was a certain kind of learning process that goes on in terms of learning how to couch something you are interested in a way that is accessible and interesting to a larger group of people. I think that is an important theme, I mean, I feel that to this day. The constant challenge, particularly coming at it from the physical science end, where it is not strongly represented in the Andrews group. Fred, and Ted [Dyrness], I mean, there is a long history, in fact, as soon as I got into learning some of the real history of the Andrews program, I realized a lot of it was built around understanding basic physical processes of watersheds, small watershed studies. How this water sediment would move through these channel systems. But it was not immediately obvious when we first started talking and traveling around and watching Dick Waring shooting twigs out of a tree with a shotgun, which I was very impressed by. Even now, some of the woody debris stuff struck me as more ecology than physics.

And so, for me, it was really a learning process to start letting this broader conception of an ecosystem intrude upon what has been, what has been at least in a sense, very strongly of physics, you know, physics of rock and water and sediment. And the geomorphic training is heavily orientated towards that, because that tends to be those processes that are well represented by engineering type equations and things. So, a lot of the physics of rivers forms the bases for geomorphic education, and ecology is always recognized as an influence. But it is never really explored in that sense, at least it hadn't been at that time, with a couple notable exceptions. There was a paper back in 1960 by Hack and Goodlett, contemporaries of the guys that I had to report on. Again, another serendipitous type thing. Turns out to have been in that one paper which was about forestry geomorphology in the Appalachians, they foreshadowed, I think, 50 years of geomorphic and ecological work, much of which is now represented at the Andrews. I remember being very intrigued by the paper, because it seemed to be a very integrated view of how the natural world worked. I mean, you weren't limited to just talking about when does one rock turn over another. You are really trying to see how the vegetation interacts at the level of the land form with processes operating within this watershed. And they really did a very nice job of that, and so I'm hearing echoes of their work in this larger group discussion, and in my own attempts to try and merge my own discipline-thinking with this large group.

Geier: Up until that point, you hadn't really heard of much about the group besides Fred [Swanson]'s work, and, sounds like maybe Jim Sedell.

Grant: Yeah, mostly Fred, Jim, and this guy named George, and I couldn't pronounce his last name, Lienkaemper. At the time, I was very much on the lookout for papers and people who were looking at questions about the effects of human timber harvest activities on rivers and watersheds. I was aware that there wasn't a lot or work, for something I had presumed was a very well-documented area, I mean this was from my river guide days. And one thing I didn't emphasize, was that when I was at the University of Oregon, I was very, very strongly involved with the outdoor community, which was really the environmental community at the time. People who were very interested in preserving the wilderness, I had a very strong feeling about wanting to protect and preserve wilderness, and as far as I could tell, the timber companies were raping and pillaging the land. Everyone knew that that had to effect everything else, right? Of course, it affected the fish, and the creeks were filling up with sediment because this was happening.
I figured it was so much a part of the common parlance around these issues, that as soon as one got into the literature, one would find a basis for all these opinions. Well, it turns out that I got into it and discovered that, no, there wasn't that basis at all, and in fact, very few people had actually looked at it in any systematic kind of way. What I became aware of, was that there was a distinction between the people who were looking at it at a very small scale, and I put the Andrews work into that category. That is, people who are interested in this little tiny creek. You know, you log it, and see what happens. That seemed to be something that was happening at the Andrews; you use small watersheds for letting you look at the movement of sediments, nutrients, or just water. I became aware of the wealth of work that had already occurred along these lines. But I was more interested in these larger channels, at least I thought I was. As it turns out, my dissertation work was kind of betwixt and between the larger channels that I had floated five years, and these little tiny things. So, it's kind of in the meeting ground, sort of an intermediate scale, between the two [small and large].

I really became aware of the Andrews work as being sort of micro-scale stuff. But it was interesting, because as I talked with Jim [Sedell] and Fred [Swanson] and Stan [Gregory] and others, I became aware that they too, were interested in this larger scale, they just hadn't done anything with it yet. The work at that time, probably the work in Redwood Creek in northern California which Fred seemed to be involved in, at least in a consultative nature, was the largest enterprise that was looking at some of the big channel issues and what became known as the issue of cumulative effects. That's really the question of how does the landscape level series of what Reds Wolman referred to as insults, translate into something that changes the channel. That began to emerge as a key policy issue as well as a science issue, and the Clean Water Act requires that you dealt with that, as did NEPA, the National Environmental Protection Act. And the federal agencies were just beginning to grapple with how do you deal with cumulative effects, you know, something which by its nature not the effect of one thing, but the whole. My work was taking one aspect of that cumulative effect question, looking if there is a kind of fingerprint or legacy manifestation of watershed impact in the channels, inset within that larger role. The Andrews work, thought more of direct impacts as opposed to cumulative impacts, which began to be featured, was featured prominently as a kind of thought experiment. You know, people would say, "Well, let's take a watershed, let's say you took Watershed 1 and you did it all over, then what?" They say, it became a way of arguing from the smaller scale to the larger scale. And that's really where I began to encounter the Andrews work for the first time.

Geier: You said earlier that you at least had an understanding with Fred, that there would be some funding from the Forest Service. Is that right?

Grant: A pittance (Chuckle). As it turns out, he managed to work some deal, putting something together with what I was getting from Hopkins, basically, cobbled-together. Mainly, it was having an office, a place to sit down; room 363 [USFS Forest Sciences Lab in Corvallis], on a bench. You know, just have a place to land, in which I could say I was doing the science of rivers in Oregon. That was really valuable, and beyond that, I began to realize was that it gave me an entree into this whole world of other people who were doing this kind of work.

Geier: Had you had any exposure up to that point of other people working for the PNW Station or Forest Service outside the Andrews?

Grant: No, my first exposure was coming here to this lab and sitting by the hallway and getting introduced to guys like Dennis Harr and Logan Norris, and it was the first time I had even been in a federal laboratory of any sort. And I had all these misconceptions, some of them turned out not to be misconceptions, about what federal laboratories are like. But it was interesting for me to be in a laboratory, where I had been in a biochemistry laboratory before, and there was still that kind of work going on. There was a water quality, a water chemistry, and a large pesticide effort at that time, because of the interest in dioxins, that Logan was leading, the effects of forest spraying on dioxins. But I didn't really know much about what went on in the lab. I knew the people who were working on things; "He's a chemist and is working with spraying and the issues around that," but I didn't really get a conception of the breadth of the work. It seemed too much of a headache to figure it out. I mean, I was always impressed with guys like Fred [Swanson] or Jerry Franklin who seemed to have in their minds, and Logan too. Logan was project leader with the Forest Service. And at that time, Jerry and the terrestrials were down on the second floor under Jerry [Franklin], and the water and stream physical types, plus the chemists, were all marching under Logan. It was sort of the never-the-twain-shall-meet except when we would go up to the Andrews and you would hear about these guys talking about this stuff.

The other thing that happened I remember, was that I went on one of Jerry Franklin's "pulses" to the Sierras. That really was my first really extended entree into the group. That was three days in a van getting down to the Sierras and camping out, it rained as I recall, and a lot of time spent around the fire. Art McKee, I remember being very impressed with his ability to spin a yarn around a campfire. And I was impressed by the overall friendliness of the group. One of the issues with any kind of large group that has an identity and a focus, is the question, how does it admit newcomers and outsiders, people who had not been with the group up to that point. I found it to be a very permeable membrane, in large part because there was an established conduit for graduates. There was a graduate student experience that seemed to lend itself well to bringing new people in. But I wonder if we are still doing that as well as we did back then. I remember at the time, just feeling that there was a very effortless sort of acceleration into this group thing. But the other thing, I've never been much of a groupie kind of guy, and so, if you'd asked me what I was doing and what group I was attached with, I was probably more inclined to say Hopkins at that time, than I would have said the Andrews program. I didn't put that "hat" on for a long time, and a lot of it, I think there may have been some "disciplinism" attached to that. I didn't feel a natural affinity because of the number of other people doing physical processes on rivers in my graduate school experience. I mean, everybody was focused around doing rivers, or something like that. Here, I was alone in my area with the exception of Fred, and Fred's focus was more ecological at that point. He was clearly very interested in what I was doing, but he was doing a lot of things on landslides. No one else was doing physical processes of rivers. And it is hard when you are a graduate student or even coming out of that role where you sort of say, this is my turf, and I am going to make something happen here and I was sort of doing that, but not with any real conscious intent to do so. I mean, I was just trying to finish my dissertation.

Geier: Were there other graduate students there that you were working with at all, or it sounds more like of a scientist that you were?

Grant: No, it was a mix. I think Fred was definitely my major point-of-contact. But I shared an office with Margaret [McHugh], another one of Fred's students, who was working on landslides, and I would meet other students who would sort of fade in and out. Fred had a number of students who were going to finish "any day now" for several years (chuckle) as I recall, and they would sort of appear and then disappear again, go back into the woodwork. But then, I began to establish some friendships with some of the people in the ecology realm, and Gordie had been sort of the guy to get me here, and he and I were friends. But he was clearly into the fisheries end of things. So, my memory of my dissertation work was that I was going outside this group, when I needed field assistance. I would run an ad in the paper and try to get people to agree to spend a couple days with me in mapping a creek, in exchange for some, you know, macaroni and cheese and chanterelles, with maybe a bottle of wine to wash it down with. That was that. But I don't recall having strong contacts, friendships, or relationships with other students at that time.

Geier: In terms of recruiting assistance, sounds like at that time you were operating pretty low budget for support in your research?

Grant: I was sort of at a low budget level, and I just sort of pictured myself as having eked out some little niche somewhere where I had enough to keep me alive, trying to stay focused on finishing my degree. Same time I had a long-distance relationship with Barbara, my current, (chuckle) wife. Part of what was happening as well, was that I would go back to visit her, and some of those visits were extended visits of several months, where I would also go back to Hopkins. But it's funny because there too, the community was changing, as most of the people in my generation were out doing things like what I was, and there was a new crop of graduate students coming up, so I became friends with them. But that was a limited kind of exposure.

Geier: What facilities or resource needs did you have when you started to work, in terms of things you actually needed to get the work done? You mentioned the bench to sit down here.

Grant: Well, I'll tell you. See that? Barbara gave that to me when I got my degree. It is a little bit presumptuous with the degree thing, but, those tools you see, are marble. That is a silver compass, and that is a stadia rod, and that is about it. That is what I used. I was doing mostly mapping and some air photo work. And it was really a very low-budget production. The other thing that I got, the one facility that I loved, was a word processor. This was right when computers were beginning to come on line. We didn't have computers around here for the first couple of years. I mean there were computers, but you needed a card reader, and a computer was something somebody else used. But the idea of having a word processor, access to a word processor, that was marvelous. And then, shortly after that, the early PC's and IBM's began to make themselves felt. Then, when I discovered Lotus 1-2-3, hey, I was off. So, that that was the other facility that really made things better. And the other thing was a resource, although it was not a physical resource, but the field crew. I was impressed by Al [Levno], and Craig [Creel] [field technicians]. George Lienkaemper was working for them, Ross Mersereau, and I am forgetting names, but there were other people there. Al and Ross and George; I was very drawn to that group. Because these were guys who really enjoyed getting wet and going out and doing things, and were very interested in streams and the whole work of keeping the gauges going, the weather station going; really the core of their stuff. And while I don't recall using them directly, as they weren't so much a part of my research, I would stay in their cabin at night when I came back to the Andrews. So, there was a kind of osmosis that happened there, and I began to realize these were guys that could solve any problem. If I needed to figure out how to paint numbers on rocks so I could measure them moving down stream and the numbers would stay, these were guys that could figure it out. Why don't you get a blow torch and dry them out, and then paint it. You know, and they would come up with solutions like that for what ails you.

Geier: Maybe you could talk a little bit about your perceptions at that time of how the Andrews compared with other experimental forests. Had you any previous experience with them?
Grant: That is an interesting question, but I had had no real exposure to other experimental forests. The one thing close that I had had a lot of exposure to was Woods Hole, Massachusetts. And Woods Hole is a science town. You have to remember that my father was a biologist, I grew up in the world of biologists, so to suddenly be in this world of ecologists, there was a certain resonance there. I recognized the type. But Woods Hole, I don't know if you have ever been there, it's sort of a scientific mecca. It is what happens if you were to take a New England fishing village and marry it to three major scientific enterprises, then add a tourist ferry to take tourists to the island, all of this in a town smaller than Philomath, a little tiny dot on the map.

Geier: Hole in the woods, right?

Grant: Well, it's on the ocean. I mean it's a peninsula, it juts out, it is in the "armpit" of Cape Cod. But a marvelous place, particularly if you're a boy. The ocean is there, it is a warm ocean, you can swim in it, you can boat in it. I mean, I got this major dose of boat [referencing love of water craft]. But the reason it's there, is it's got these major scientific laboratories there, the Marine Biological lab, and the Woods Hole Oceanographic Lab, and Fisheries Service. The town lives and breathes science. I mean, you go to the beach and it's just littered, you know, there will be three Nobel Prize winners sitting in their baggy shorts eating peanut butter and jelly sandwiches, it's that deeply steeped in science enterprise. Students come in every summer, and so that's the model I had. That's what I was comparing the Andrew's to, was that, what really was fully-configured, big buildings and all the high-tech sophistication of modern biology.

All the collecting boats I used to work on as a kid, and so there would be field crews, sort of field technical people helping. And so the Andrews was like a little tiny, tiny, tiny version of this, stuck up in the trees. That is what I thought of it, and the contrast was kind of funny because Woods Hole, you have a sense of this kind of lofty scientific enterprise. The buildings are made out of brick, you walk in and there is a hallway that has sort of been padded on by all the greats, in the field, you know, the people you read about in the 1920's and '30's, these guys have all done work in Woods Hole. And so, you contrast that and the smell of dead squid and formaldehyde, and ethanol in the halls to this funky trailer stuck out in the woods that had been skunked the summer before and hasn't quite lost its aroma. It was a marvelous, but at the same time a somewhat off-putting contrast.

End of Side A, Tape 1 (of 3)

Begin Side B, Tape 1 (of 3)

Grant: It slowly began to dawn on me as I hung out at the place, that this was a microcosm and maybe even the way the MBL [Marine Biological Lab] started and how it got going. But it was a slow realization that I was in on something which was already clearly a going concern, you know, I had no role in getting it going, but it still felt young and fresh.

Geier: Feeling like you contributed to the evolution of the place?

Grant: That is right.

Geier: You had done some river guide work up there on the McKenzie River. Had you been at all aware of the forest at that time? I also imagine you went through Blue River?

Grant: Only in the sense of having a feeling for the forest. I mean, it was just a feeling, sort of an aesthetic sense of smell and texture and rain and mist. But, I had been by the Andrews turn off road many, many times, but I don't recall ever having gone up there, and I didn't really have a sense of it, that there was science going on in this river valley as well as everything else. So for me, it has been an interesting experience. I think about this sometimes because, the McKenzie, when I was a river guide, was like my bread and butter. The bread and butter river and so we would do, I don't know, 50, 75 day trips, a summer. Driving that road was like, back of my hand. I really knew that road. And it was interesting to start driving that same road but taking the turnoff and going off way over there. I was aware that my life had taken a different direction than that. But other than that, I hadn't really thought much about what was going on.

Geier: Can you recall the communities along that road. You know like Blue River itself, or McKenzie Bridge or --?

Grant: I remember Vi's Pies. There was a good pie shop, Doris' Cafe, which burned down. I remember, this goes back to my early year in Oregon, when I came in '66. Now I realize that was two years after the 1964 flood. I remember some very early images of the McKenzie River from that time because my family, they were all east coasters but my parents to their credit, really wanted to give us some exposure to this new home. We would take these Sunday drives, usually my father, for some reason or other, would pick a reservoir we would go to visit. We would go up and see Hills Creek reservoir. We would go see Cougar Reservoir. I mean, they were lakes, big, big lakes. For all I know, we may have actually driven up to Blue River Reservoir on one these long drives. Which again, I remember mostly for fighting with my brothers and sisters in the back seat. But I remember driving along the river, and it was kind of wide and open. I am not talking about communities now, but Blue River actually, what I was thinking about was that there was some stuff happening on the road around Blue River, that I think was part of what forced them to put the highway outside town, because the old road used to go through Blue River, and then they moved it. I remember that when you drove up, when you drove the McKenzie, that you actually drove through this town called Blue River, and at some point, there was a lot of road construction, and then you didn't drive through the town anymore.

I do remember the river was wide, it seems like there was a lot of gravel in it. It was easy to look out of your car window and see the river. Now, it's got vegetation around it and it is hard to see the river, except in a couple places. So, I realize now that what I was seeing was the aftermath of the flood in ‘64. But at the time it just seemed, that was just the river.

Geier: You mentioned that you were involved in some groups while you were at U of O who were kind of interested in the natural world. Had those groups been active in the valley at all?

Grant: Very interesting question. The issue, the focal point, for those outdoor groups, was exclusively wilderness. Anything that wasn’t wilderness was sort of passed over, stuff that you passed over to get to the wilderness, from what I remember. And there was, it was never really talked about, but the wilderness was considered to be so sacred, everything else was profane. See, you didn’t spend much time in the profane. I mean, the profane was not that interesting. What I wanted when I thought of rivers, was wilderness rivers, wilderness areas. I wanted to protect wild and scenic rivers. That was the motivation, everything was entirely focused on that.

That is an interesting point, because I remember for me to start working in the Andrews in a place that had been logged and roaded and had lots of people and history, I had to kind of swallow something to do that. Because my instincts were all pushing me, in fact my dissertation work, it is interesting because I wanted to study the effect of human beings on rivers. I did most of my work in a wilderness area, because I needed a way of comparing streams in wilderness versus streams that had been cut over, and there was no good description of what a mountain stream looked like, whether in a wilderness area or in some place else. So what I thought I needed to do was to go to a wilderness area and establish a reference point, and then go somewhere else. Well, I ended up spending most of my time in the wilderness area, but I distinctly remember the feeling for me, it was almost kind of a come down. I had to come down from the mountain, the lofty mountain peaks and the wilderness, to think about working in the Andrews. I remember that even with Fred, and in Fred’s class, wanting to talk about wilderness rivers and wilderness processes, and he wanted to talk about logging, and I didn’t want to talk about logging. That seemed liked more of what was screwing the world, and I don’t want to talk about that. So it’s an interesting kind of thing, and I wonder if that’s still the case for people, and I think one of the major changes in the way we think about environmental issues is that we are more willing to view the human issues as being geographically integrated in a place.

Geier: I was just curious, as an undergraduate at U of O, if you might ever noticed the difference in culture, going up the valley from Eugene to Blue River, or did people there notice you?

Grant: They didn’t notice me that I know. What I noticed, what I was most aware of going up on the McKenzie, well, was the fishing. That is what I thought of when I drove up here, was a lot of people fishing. I didn’t really think about logging. I thought about fishing and I thought about, I knew, some people who had a summer getaway home up there. That is how I viewed these little dots of cabins going up the road. These are getaway homes. I sort of view the McKenzie, and certainly my experience reflects that, was that it was sort of a recreational place.

You know, people went fishing there or you went on the reservoir to go fishing. So I don't recall any feelings other than that about the environment.

Geier: When working on the dissertation, eventually you went back to John Hopkins to finish?

Grant: Actually, I didn't. What happened, was about 1985 or so, '84, '85, Dick Fredriksen, who was a hydrologist along with Dennis Harr. There were a number of people doing rivers. Dick was one of them. He was really the man behind the small watershed studies, and he retired. And that opened up his position, By this time Logan [Norris] had left to go to the university, Dennis Harr was the project leader, I was still not finished with my Ph.D., and they were looking to find somebody who could come in behind, to sort of take Dick's job. I remember Dennis saying to me at the time, "Would this be something that you would be interested in?" I said, "Of course, yes." He said, "Well, we don't know what we are going to do, and there was some talk about having a broader search." But it's interesting because the stories about people sort of getting their jobs because they were in the right place at the right time, and the "good ol' boy system" sure worked. That sort of happened for me, I was studying something considered to be relevant, I was in place, I was a known quantity, and although I was a known, I was known as a graduate student. What I now realize is that people sort of had to take a chance. Was I going to pan out or what? It was very unlike today, a very easy segue into a job. There was a call for it, I applied for it, and I wrote the job description. Dennis said, "What do you think this person ought to do?" And I said, "Something about mountain rivers." (Chuckle). It is interesting how many people do get their jobs that way, at least in the past. I think it has broadened, and the nature of the job seemed to change, so it is much harder to do that now. Basically, it was, sort of show up, hang up, like Woody Allen says, "you show up." That is kind of what happened for me.

Geier: From that point on, from '85 on, who would you identify as being your closest collaborator, people that you worked with mostly on a day to day basis?

Grant: Fred [Swanson] remains that to me, and in some senses he has always been my closest collaborator in this environment because of his disciplinary focus, and our long history now, which is going on 25 years. That has really expanded the last 10 years to include a much wider group. But starting in '85, it was Fred and George [Lienkaemper], who at this point was still in the field, but I think beginning to look other places to be a professional. Also, Jim Sedell somewhat. Through Jim, I worked on one project in parallel with my dissertation up on the Breitenbush River, and Jim was heavily involved in that. Then Dennis, who was never really a close colleague, he was more of a presence. Dennis was a singular guy, kind of a very spirited guy who did very good hydrology. But he enjoyed to joke as much as any man I ever met. He once wrote an entire scientific paper as a joke, I mean, with fake references, a major piece of work to craft this thing with this level of detail and precision. I forget what it was about. So, Dennis and I would have great conversations, but I wasn't that close to him, really, in a collegial way. In many respects, I wish that I had picked his brain a little more than I did at the time. He was more of a hydrologist. I mean, he was doing water. Part of it was my own tendency to put people in boxes. Well there's Fred, who was doing landslides, Dennis was doing water, and I was doing channels, so it's kind of interesting, because in a group scene like this.

I'm just globalizing a bit, but in a group scene, there really are two countervailing instincts that are asked of participants. You have to be able to speak with authority on at least one area that is of general interest. In Wolman's words, you have to know something about something. And so, there is a kind of, I want to call it turf, but I don't think people defend it, although some of that goes on too. It's a domain, a plan of action, a portfolio, if you will, of issues and tools and techniques and studies you have already done, or studies you are doing now that defines your piece of the puzzle. When you're early on in a group, a lot of your struggles, and I suspect this may be more true for men than for women, but I suspect this is true for anyone trying to break into a group to define a turf or define a domain, and it's particularly difficult if there is somebody already in that domain, as there is a lot of niche selection going on. It's an ecosystem, you come into that like a new organism trying to figure out where it can go. You find those places where you can feed and do your thing, while everyone else does their thing in ways that don't conflict and, and extract too much energy. So, there is that going on and at the same time, you're asked to, to sort of submerge your own instincts, which might be to say, "Well, stream channels are the most important thing in the entire universe, and everybody should pay attention." You have to sort of submerge that and say, "Wait a minute, there are other people doing other things that are also interesting and relevant and important. I have to listen and pay attention to them as well as ask them to pay attention to me." That is something that comes with time, and it comes with you as you grow confidence in your area, your own field, and your own abilities, and then it becomes easier in some cases, to hear other people. You don't have always be out to prove yourself anymore, and in order for the collective scene to work well, you have to have a critical mass of people who are doing both; both supporting and sort of developing their own professional "tap root," but at the same time letting their branches intertwine, if you don't mind the metaphor.

Geier: You mentioned earlier that the pulse to the Sierras was kind of a point where you kind of began to be part of that group, and so that '85 you already felt pretty well intersected with them?

Grant: Yes and no. I mean, it was interesting because of how things go, how to feel a part of a group. You get invited to things, or you show up even if you're not invited, and they take you in. It's that kind of thing. I never really lived for a summer at the Andrews the way some people did, or lived for a year at the Andrews the way other people did. It was more of a general agreement that if there was going to be an evening party, or if there was going to be a discussion, or a symposium about something, that one way you feel a part of it, is if your piece of the action gets recognized. You get on the agenda, you get face time. At the Andrews, one thing I learned early on is that the field trip, which was largely a process of sitting, driving around nauseous in a van, getting out and having somebody stand in front of something and talk at you. The Andrews field trips were of that sort. That was a major mode of communication, so participation in those was what I used as a metric of involvement or engagement. Much more so than going to meetings. I didn't start going to the LTER meetings on any kind of regular way, and I still do it irregularly, as I can take some of that in measured doses.

The other thing that happened at the time, that was really the significant melding event for me, was based on work that Fred was doing and I was doing, including stuff spinning out of my Ph.D. research. We got together with Stan Gregory and his troops, and put together a second-generation riparian grant. When I got out to the Andrews, the riparian one was a major focus, and there was a lot of interest in this sort of "river continuum" idea. Much of that work escaped me. That was going on around me, but I never really understood it, to be honest, I mean, I never fully got the picture of what people were trying to do. I would hear bits and pieces of it. There was a lot of interest in organic matter cycling, and I remember going out with Fred and Steve Cline. They were interested in sticky wickets and how sticks sort of get smashed together to form a debris jam, and they had some experiments going, and people were dumping gingko leaves into streams to see how far they went. Some of this seemed to me to be pretty ecological. I mean, again I was really focused on the physical stuff. I didn't quite get it all, the picture of what all this different work was headed towards. But then, I think it was '85, or so, maybe '86, somewhere in there, that grant ended. There were some discussions we had been having up until, that started happening as that grant was ending. They were both acquainting people with what had been discovered, but also sort of preparing for the next go-around, where the idea of using the physical structure of the stream as an organizing principle, began to emerge. Basically, the "Riparian One" in a nut shell, was, "You tell me what trees are next to the stream and I will tell you what's in the stream." "Riparian Two" changed that, saying, "You tell me about the geomorphic setting the stream is flowing through, or creating the stream, and I will tell you what is in the stream." That was really the place. So that was really for me, the coming together of the physical and the ecological in a grant, in a joint enterprise.
Geier: This was one that you helped write with Stan Gregory and --

Grant: -- And Fred [Swanson]. And there were a lot of discussions that led up to that. It was my first real exposure to grant-writing, and you have to understand that when you're a graduate student, you don't know nothing, you don't ever get to see how people work. How do you write a paper, how do you put a grant proposal together, how do you do the basic stuff of science? You learn that by osmosis, by hanging out with people, and so, for me, what I got here, and it was probably the most important thing I got here, was I got to watch how people did that, up front and personal. I got to see it. I got to see people having the discussions that led up to a grant proposal, and as I recall I wrote some pieces of it. My memory is that Stan [Gregory] really, really pushed it hard, and Fred did as well.

Geier: If the LTER meetings weren't that important, and in the sense that the field trips were more, people talking about their work, where did you get this exposure to this process that went into the groundwork?

Grant: Those were the meetings that I went to. At the meetings, people would come and talk about here's what we did in Riparian One, what are we going to do in Riparian Two. And then, next meeting, come and put a little synopsis together of what you got. You know, write, then I would see someone take those synopses and pull them together into something, and write some text to go around it. A lot of what I was doing, was watching how people wrote. And one of the things that I really got, Fred [Swanson] particularly, Fred was a great editor. He would take my long-winded, rambling, unfocused prose and do something with it, turn it into something that actually said what I wanted it to say in about half the words. He hasn't been able to help verbal expression very much, but that's where I began to learn how, how you think science and how you really do it in a day-to-day kind of way.

Geier: When you started out in the position that Dick Fredriksen left, you were coming into the Forest Service at that point. You got an appointment at OSU, a courtesy appointment, about the same time didn't you, about '85?

Grant: Another thing that was happening then is the first course that I took from Fred. He was still teaching it over in the geoscience department, and early on in my tenure here he asked, do you want to help me with the course? So, I find myself suddenly helping to teach. And it worked out pretty well because his course, he had done mostly landslides, but some hill slope processes and some streams, and I came in with a very strong stream orientation, but not knowing a whole lot about hill slopes. So we began to sort of divide it. He would take the hill slope and I would take the stream, and as a product of that I began to learn how to teach, which meant I had to learn it all over again. And so I had been an instructor, I was a courtesy instructor in geosciences, even before I got my degree. And then that's sort of translated through the years. But, yeah, that was another thing that was happening.

Geier: I was curious, all the interaction you talked about this learning process in grant writing. I was wondering how much interaction with colleagues in programs in geology or other programs, did you have on campus here?

Grant: Stan was my major point of contact at that time as I recall. Bob Beschta as well.

Geier: Bob?

Grant: Beschta. In the Forest Engineering department [OSU]. That was in part because in '85 or '86, we got the idea of putting together an international symposium on erosion and sedimentation in the Pacific Rim, in Corvallis. Fred [Swanson], Bob Beschta, George Ice, and I, worked on that together. That was really a one-and-a-half to two-year effort. That was another sort of a learning crucible for me; how do you put a meeting together, how do you think about a meeting. So, I had some interactions with Bob around that, and scattered interactions with lots of people throughout my tenure here. I just don't remember any really close, on-going, involvement with anyone other than Fred.

One of the things clear to me early on was that the distinction between the Forest Service and the university, was a very subtle one. Basically, I saw Fred and people like Logan [Norris], treating their jobs with the Forest Service the way I would imagine anyone at the university treats their job. That was modeled for me as the way one conduct's one's self. That felt very comfortable for me. I was quite used to the academic lifestyle, and I felt, given my background, working for Forest Service Research was never exactly what I had in mind. I always pictured going out and getting an academic job somewhere, and continued to think I should apply for jobs in addition to that after I got my Ph.D., but began to realize that it was not the best path forward. I could do as much research as I wanted to, or as little, (chuckle), but did as much as I wanted. But I could also have a university presence, and I could teach. And so, all of that contributed to my sense of what it meant to be a scientist, and I didn't define that by university or non-university.

I think the theme for me was one of slowly pushing my head above the murk of being a graduate student and thinking like a graduate student. When you think like a graduate student, you think about your world and your universe. I was aware there were lots of people thinking broader than that, thinking about a group. I began to think a little bit like that, but mostly I still thought about myself as a graduate student. And it's been a slow process, which is on-going, to think more broadly and globally about the group as an entity to which one has personal responsibility. That's emerged only with time. It isn't something you just do, and I can see institutional forces that could make that either easy or hard. In this setting, my experience with it was really easy. The environment we lived in was so loose and flexible, that it was entirely possible to be working for the Forest Service, to have close connections with the university, and to be doing things which might have looked to the outside world as if they were coming from the university, even though they were coming through Forest Service Research. And nobody cared.

Now, that has changed. That is still one of the most significant changes in my professional career, those distinctions have begun to play more of a role, and there are pluses and minuses to both ways. At the time though, I was just really impressed by the fluidity of the work environment. Largely that was because of guys like Fred [Swanson] and Jerry [Franklin]. Jerry was really central, as was Logan [Norris]. I think Logan was really a key guy in terms of helping people adopt a "just do it" mentality. I mean, just do it, don't break any laws, don't get people pissed at you, but just do it. Jerry certainly operated out of that philosophy, Fred and Dennis too. Dennis Harr is another example of that loose leadership style. I think that is very important. I think that probably is one of the major ingredients that made the Andrews work early on.

Geier: Was Logan Norris department chair when you got here?

Grant: No, he was project leader. He did what Fred's job is now. Then, there was Dennis [Harr], and then he went over to university. But the thing that always impressed me about Logan was the very thoughtful and intelligent take he gave on things, and with good instincts. I mean, good feeling for where "the line" was. That's one of the things you had to be aware of, was you just couldn't do anything you wanted, but you had to pay attention, for example, there are issues out there in the world, and you can pretend you're not interested in them, but then, they come in and get you. It's a much better philosophy to be attentive to what is going on. You think, “What if this thing sort of burbles up into a major kind of thing? What can I be doing now that is going to put me in the right place if that happens?” They are gambles, and you have to trust your instincts and what your intuition tells you is going to be something important versus something that is not. But, that seemed to be working back then. There were various issues that were burbling along.

Someone I haven’t talked about but was another undercurrent, to try and paint at this time, is the policy. I presume that this is something relevant to this discussion, but the policy context for all of this work was another piece of the learning curve for me, personally, and in retrospect, is what I now recognize as one of the most salient aspects of the whole environment around here. Early on, I came into the game thinking, well, I am going to drive a stake through the heart of the timber companies, proving that they’re raping and pillaging the land. Now, I had some notion what this was, the sort of issues motivating this work. Then, as I watched Logan deal with the spraying [herbicides] issues, I became aware, no pun intended, how volatile the environment around forest issues was or could be. In general, the arena I was in seemed to be fairly sedate by comparison. Old growth, or forest protection issues, were really key, but for the most part these were framed very broadly in terms of protecting old trees and old growth and old forests. And as the ‘80’s went on, the birds were attached to that, and then, fish and other things as well. And there was always a line in these things about how cutting trees causes sedimentation which fills up the gravel for the fish. Again, as if this is known to everybody everywhere. Incidentally, I should mention that one of the products of my Ph.D. research, is that I had a devil of a time finding streams that were radically different as a result of timber harvest. This was one of the big come-uppances for me, is that I expected to find that cutting trees turns everything blue. (Chuckles) I couldn’t find blue streams, you know, metaphorically-speaking.

The policy issues out there emerged for me mostly around this issue of cumulative effects, that the agencies of National Forest systems were beginning to try to work out. I found that my colleagues and relationships developed with people, often with other researchers, sort of in my position but at another laboratory. Like Redwood Sciences Lab, in [Arcata] California, is another lab where there is a much higher concentration of people actually doing river-watershed type-of-processes, and here also with people who were actually on national forests themselves, working to solve these problems, or people who were regional hydrologists. So a lot of my professional work and linkages, and the transfer of the science, was directed towards that, as opposed, for example, towards students or the university, for my career. That just turns out to have foreshadowed what really began to coalesce in the late ‘80’s as critical policy issues formed around, certainly fish, and the relationship between physical processes and fish habitat, became more and more of a concern, and people began to discover more and more about linkages between forests and logs, and logs and streams, and logs and fish.

And this again is what started in many cases, or at least was really given a boost, by the early Andrews’ reports. So people coming back ten years later, and really emphasizing these issues. There was a big meeting in Seattle, I remember, in ‘87, in which a lot of this forest-fish interaction was really being highlighted. And it was the same meeting, you know, they had the same meeting in '92, and we were commenting at how some people may have not even taken their slides out of their trays from '87, there was a lot of the same stuff, so this was becoming much more current. But it began to be manifest a number of different ways, as the old-growth issues really got booming, and guys like Jerry Franklin began talking about "new forestry," and asking, what does this mean? I saw some real implications, some of the effects on hydrology, on sediment and wood and so forth. Some of my work was orientated towards trying to bring some of these issues out, but it's really been the '90's when these watershed things have just gone, "ka-boom," and the complete merging of the fish issues into the forest protection issues. The recognition of the fish problems has been regional in scope, sort of mega-regional in scope. And then the major science assessment effort and strong role in the fish issue there, that really brought this policy, set it with these policy issues, out and into the open.

Geier: In terms of these issues [hydrology and forestry], were there people at the region or district that you have worked with in terms of those issues?

Grant: Yes, guys that were early regional hydrologists. These were people I would need to interact with at meetings, like Jerry [Swank], Dallas Hughes, and Jerry [Christner], early on at the Willamette [National Forest], a major entrée for me. Jerry Christner was really a key guy, in fact, during my dissertation, he played a very important role, helping me develop it. There was a whole bunch of guys from California, John Rictor, the Redwood Science people, Dick [Janda], Harvey Kelsey, other geomorphologists, but Dick Janda was a major influence for me.

Geier: Where was he?

Grant: Originally, he was with the Redwood National Park, and then he moved to the U.S. Geological Survey, and eventually to Vancouver, Washington. He was a guy who was very focused in his professional world on the relationship between understanding and using basic geological principles, then translating them into ways that could be interpreted and understood by policy makers and the public-at-large. He did this in Redwood [National] Park, when he argued for the expansion of the park to include trees outside of the place that was being eroded, as a watershed protection measure. He was very involved in the [Mount] St. Helens issue, and was a close colleague of Fred Swanson as well. I should add a side note. St. Helens was a place for me, that although aware a lot of stuff was happening, because it came on line in '80, '81, while I was still back at Hopkins, I only got in on kind of the tail end of that. So, I missed a lot of the bonding that happened around the St. Helens experience. I was very aware, that for guys like Stan [Gregory], Fred, George [Lienkaemper], Jerry Franklin, and others, that this was really a major thing. Interactions between Jerry, Fred, and Stan; a lot of development of ideas happened in that crucible. I kind of came in at the tail end of that. I did a little bit of work up there, but nothing that amounted to much. Dick [Janda] had been very much a piece of that, and I think the policy issues around the park, he continued with that. So he was kind of a role model for me, for how you start thinking about these issues, and how you think about the policy implications at the same time that you are doing the science.

Let me just add something. The work I did with Jim Sedell on the Breitenbush River became involved in a lawsuit again over this cumulative-effects issue. So, I had my one and only experience being dragged into court as a reluctant, what do they call it, hostile witness, and which was actually quite interesting, because I was in the position of having information which I thought was potentially useful to the plaintiffs in this case who were suing the government over logging in the Breitenbush River.

Geier: Who were the plaintiffs?

Grant: There was the Breitenbush community, and then the Sierra Club might have been involved, and there were a couple others, but they sued to stop logging, sort of around this cumulative-effects issue. That was for me, a real singular event of being sort of pulled into policy issues, having them overtake a place where I had just been doing work on when the logs move in streams. I hadn't been studying this cumulative effect issue at all, in fact, in this place, but I have been overtaken by circumstance. So that taught me, A) That that can actually happen, B) When that happens, you sort of lose control of it, and C) It's a pretty squirrelly process. Hard to ride, hard to know what is going on, happens quick, lots of surprises, people don't always act rationally, or diplomatically. There was a steep learning curve about some of the follow-up on policy, and what happens when you get too close to that business. That was in the legal arena, but then that also showed up, for example, in FEMAT and other instances, I saw a lot of the same behaviors down the road.

Geier: How would you describe the situation getting out of your control in that lawsuit?

Grant: Just that I remember, for example, we had been asked to have phone conversations with people who were interested in pursuing this suit from the plaintiff's side. And I would send them papers, four or five, done. I always ran to Fred, and asked, "Fred can I do this?" He said, "Yeah, it's published, it's public, you can send it." And I didn't really have a huge agenda there, I was just interested to see what was going to happen. But I also felt that they could reasonably pursue a case, that there was some evidence that suggested logging had had some effect here, unlike some other places. I began to be aware that these guys didn't seem to know a whole a lot about what they were talking about, I mean, they were just sort of using these papers as clubs, as if, literally, you rolled them up and hit people with them. Without really understanding the details, and maybe you didn't need to, but it seemed to me that there was more of a use if you actually understood and read them, than if you just used them as a club. Then there were other issues. For example, I got subpoenaed to give oral testimony, and just being in that room with people trying to extract information from me with a lawyer who was trying to keep me from giving information, for the government. Then my own instincts, which were just focused on trying to figure out what it was I thought about the questions as they came at me, and then sort of going one direction, and having the guy sort of switch the questioning, go from some having to give some very general statements about how rivers work, to being asked, "And all of this would be true about the Breitenbush River, would it not?" I said, "Wait a minute, I have been talking very generally here, I would have to talk --" The lawyer said, "Well what wouldn't be true?" "We have just talked for two hours?" and "We will read it back to you." Suddenly, you realize you've sort of been led down the primrose path, and I was naïve in the way I was dealing with it. Just being aware that once that happens, the rules of science and scientific rational discourse, looking at and weighing the evidence, doesn't apply enough in a court setting very much. It probably does, maybe at the very end, but everything leading up to it is a battle and a fight.

A lot of policy issues have this sort of military model in them with battle metaphors attached. And it took me awhile, and I am still learning, how do you talk from the point-of-view of a scientist in the context of where your work is embedded in this highly-charged, polarized environment, where your words can easily be taken out of context. Where some part of you might even *want* your words taken out of context. I mean, how do you handle yourself in that setting? How do you not overstep your bounds? I think that I have become very, very sensitive to that issue, and I think it's really important not to overstep one's bounds, and it is so easy to do, and scientists are given many unbridled or unchecked opportunities to express themselves in some cases. There is still some lingering respect for the profession, but it is easily eroded and I think if people will overstep it, the complete loss of respect for the scientific enterprise vastly outweighs whatever small gains one can make towards what you think on any particular issue. I think it is really critical for people to take it serious. But we are getting afield here.

Geier: Actually --

Grant: Maybe we ought to take a raincheck here, because --

Geier: Do you have a meeting coming up?

Grant: Not in the next while, I don't know how long you want. I've been wandering, I feel like I have been wandering all over the map here.

Geier: You covered a lot of the turf I wanted to cover, but in a different order than I planned, so it's good.

Grant: Means you have to go back though and try and resume it.

Geier: Not a major problem, but I probably could use another hour, but we don't have to do it today if you would rather. However, I did want to follow up here on one thing here.

Grant: My suggestion would be, if you didn't mind, because there are a couple fires, imminent fires that are brewing in policy lines that I sort of have to deal with, and I'm delighted to talk with you. You ask good questions, and mainly, you put up with my rambles.

Geier: There is one thing that I want to get before I forget.

Grant: And present the question, and then we will check the calendar.

Geier: I was just thinking, if your kind of experience you had over the Breitenbush, affected the way in which you think of yourself as a scientist and who your audiences are? Who do you write for when you write an article? Do you think that changed at all since that experience, or maybe you should, in general terms, describe what your audience is when you write as a scientist?

Grant: That's a very good question, Max. Let me reflect on that. I'll tell you my instinctual reaction, and as I am talking about it, let me edit some of my words. My model for scientific writing has always been books like this, from Reds Wolman. I was told by my advisors, in a sense I had two advisors, Wolman and I had Swanson. Reds [Wolman] said to me, "You know, you write every paper as if it's going to be a U.S. Geological Survey professional paper." Which is sort of a cream of the crop, or "You write every paper that is going to be a journal article." I haven't always followed that advice, but the model he gave was that you want everything you do, every talk you give, every paper you do, to be oriented towards the best in the field, and the audience you're writing to as a scientist, is mostly an audience of your peers, of other scientists, of other people who are interested about the same things you are. I have always tried to hold to that because I think that that's ultimately the court of last resort in what we do.

That it is much more important in the long run to be writing for the scientific community, not because science is some kind of god, or I don't have a shrine in my house, and I don't worship at the altar of science. It's a process, and it's an imperfect process, but it is one of the better processes human beings have come up with for figuring out how the world works, how the universe is put together. And there are certain rules about how the process works, and rules. There's got to be repeatability and verifiability, doing things rigorously, and thinking things through. I mean, these are sort of the basic rules, and I think those are principles that I have always tried, but not always been successfully, to cleave to in my writing and in my public speaking. The Breitenbush didn't really change that, but it made me increasingly aware, and I've grown, of the curious environment in which a number of us here do science, compared to, say, the way my father does science. I mean he's doing developmental neuro-biology, and he's working on some model of how a system works. He thinks it's a good model, but maybe it won't pan out, and he'll do some experiments and figure it out, and write a paper one way or the other, and no one really cares, except other people in the field. That is about who he's writing to.

But, it's never going to show up in the newspaper. That's different here. That's one of the most distinctive differences. We're much more in the public eye because of the nature of the issues we work on and because of the level of interest the public has of those issues. And so, when I write a paper, I now have to remember that it's read by lots of people, including lots of people without any training from the field I am in. And I have not fully, to use a word out of the sixties, "grokked," and what that means, but I have come to respect it, and I have come to rue it. I co-authored a paper with Julia Jones, a year-and-a-half ago, and it came out. And you refer to it euphemistically, as the paper from hell. I think it's a good paper, but I don't think that if had a real appreciation for who was going to read it, how they were going to read, how it was going to be used, and how it was going to be misused, there is a lot more that I would have said. Fortunately, I have another shot at it. I have other papers that I can write and other things on that issue. But that paper, probably more than any other, has convinced me of this business of who do you speak to, who do you write to, and the much broader range of people you have to keep in mind when you're writing. That's a lesson I think others have learned at other points in their career, and for me, it's been a growing awareness, rather than any one thing, although this paper on issues surrounding the recent floods just catapulted the work we do into the spotlight. At least for, you know, its 15 minutes of fame.

Geier: Who did you write the paper for?

Grant: The paper was published in *Water Resources Research,* the main hydrology and water journal. I mean, you would be amazed how many people have gotten a copy of that paper. I don't know how many have read it. But a lot of people have read it, and a lot of people don't understand what's in it, even people who should understand what's in it, don't understand. I don't fully understand it, I mean it's a complex paper. But it's brought the lesson home to me, that in this field, at this time, you cannot pretend that you're just writing for you scientific peers, because you're not. And again, that is something that no one teaches you in graduate school. It's only through experience, and only through certain types of experiences, that you, you get that feeling. And I don't know how it's going to play out. One of things I try to do and I've put a lot of effort into it the last couple of years, is now I deliberately write for non-scientific audiences. I write for the newspaper, I will take it upon myself or working with Fred or others to put together an opinion piece so that I have an outlet, and so that I can clearly try to articulate, what I hope are ways of thinking about the papers when they come out. That's part of what I am trying to do is help people understand it so that when you see a paper about this, here's how you might think about it rather than just sort of using it or misusing it one way or the other.

Geier: Seems like a preface to your actual scientific work; what you are going to publish, not what you have published.

Grant: Well, it's both. It's trying to bring a broader audience along with what I view as really an on-going improvisation around multiple scientific scenarios. That's how I see my work. I think of it in, coming from a musical perspective, having music in my past, I think of it in many respects as a theme, a major, there's clearly some major themes, but there is a lot of variation and a lot of improvisation. A lot of it is seeing things that could be done, or you know, have some ways of pushing this a little bit further, getting something useful out of it, and then trying to bring people along both into the importance of the theme itself, the themes themselves, but then also trying to focus and help people understand how these different variations kind of play off.

Geier: Well maybe we should call a halt here, it sounds like we've got to reschedule here anyway. Um, do you have a time that would work for you?

Grant: Sure.

End of Side B, Tape 1 (of 3)

Interview with Gordon Grant by Max Geier, begun on October 6, 1997, continuing October 10, 1997 in Dr. Grant's Office, Forestry Sciences Lab, Corvallis, Oregon; Transcribed by Elizabeth Foster and Keesja Hoechstra

Side A, Tape 2 (of 3)

Geier: Some of the things you were talking about last time were really good, and I wanted to pick up where we left off. You were talking about what you considered to be your audience, the types of research and writing, and if I recall it right, you were talking about your current efforts to deliberately target outlooks in popular media, kind of warn general publics about the research that is coming out before it actually hits the scientific press, as you called it. Maybe you could talk about what you consider to be the most critical ideas and issues for people to consider about the kind of work you're doing at the Andrews and elsewhere.

Grant: I just want to clarify one thing that I was reflecting on, a lot of what we were talking about last time was me. And I was I was taking what you were asking as questions involving my individual perspective on the thing. I didn't have a corporate hat on and tried and articulate the Andrews program or anything like that. I assumed that's by design on your part as well, that your job is synthesizing. I have absorbed enough of the ethos around here that I can't clearly distinguish what's my perspective from a broader group perspective, an interesting thing about what happens in this kind of setting. One begins to take on a kind of group, a group-think, it becomes a part of one's own thinking. But I was reflecting upon our conversation before, realizing that a lot of it was, I'm talking about my own background, my own sort of way about going at it, so I'm sort of leaving it to you in that case to tease these things apart.

Geier: Yeah that is exactly what I am looking for. I would be frankly, kind of worried, if someone stood up and said, "This is what the group thinks."

Grant: Yeah, right. So the question you posed, what are the critical issues that people, that I think people ought to be looking at and thinking about with respect to my work and perhaps the broader Andrews work. Is that right?

Geier: Right, and if you think about how your work has evolved from maybe 1985 to the present, what you have learned in that period, and what are the critical issues or content that people would need to understand about the current work is and what the concerns are.

Grant: Good question, let me think about it a minute. When you ask someone in science a question, you typically get an answer that can be more confusing than the original question. And so, one of the things I think we do, I know I do that personally, in respect to a lot of the forest issues, when people ask that if I was given a chance to stand up in front of a million people, sort of, what would you say. To say, "Well, stop, people. Here's what I think you should think about." I think the basic theme that comes through and that I try to articulate, is that issues are a little more complicated than you may perhaps have been lead to believe, and that doesn't mean that it's unapproachably complicated. Or what I try to communicate is, and what I would like people to understand is, if you will give me a little bit of your time, I may be able to help you think this thing through. But at least give you some perspectives that you might find useful in your own thinking and musing about it. That is a very broad statement, but it applies almost across the board to any specific issues I can think of that are focal points in the Andrews. I think a lot of what we are trying to communicate is a little flavor of the complexity of natural systems, and that flavor is an antidote to simple, syllogistic and polarized thinking, which has been one of the dominant characteristics of the debate about the use of forests, and the whole issue of how are we going to manage our forests into the future So what I feel myself doing is reacting in a sense to the framework which a lot of our work is conducted within this larger, polarized debate. Are we going to cut the trees or are we going to leave them? Are we going to have jobs or owls? Are we going to have fish or economic development? That's the kind a very simple, polarized view, and if you shine the light on anybody in the debate, they'll go, "Oh, we recognize it's more complicated than that." But at the point you're talking to large audiences, almost inevitably, what you are speaking against is that backdrop or context of this polarized thinking.

So I view my role as a scientist is to bring some of the flavor of complexity and uncertainty, including uncertainty around those things for which a lot of people have already made up their minds. This reflects back to some things that we were talking about in the last session. When I got started in my work, I knew that we could go up there and find all kinds of evidence of stuff that had happened because we had logged the hell out of these landscapes. I just knew it. And the more I looked into it, the less convinced I was that this was going to be a simple process of showing egregious environmental damage. There was egregious visual damage, I mean, I didn't like the look of these things. But if you're asking how impacted systems actually respond, I found myself more muddled than I expected to be. So, part of my goal in talking about these larger issues, and I am going to get to what the issues are, I haven't forgotten what your question is; I am sort of creating a context or setting for this.

My answer is to try and communicate the flavor of something, and that we don't know these things we think we know as well as one might think. But at the same time, we do know things, and so, it's a fine line to communicate both that, hey, there's uncertainty there, there's more complexity there than you might have previously thought, but we still know something. And the first part turns out to be, I think, easier to do than the second part. The second part, which is to say what it is that we really know, what is it that you can really bank on and can count on, that those turn out to be in some ways more difficult, because in science we're trained to view, to sort of live with uncertainty and teach how we move forward in the face of uncertainty, and to avoid the definitive and the absolute. A lot of what we do is very relative, so it becomes difficult to simultaneously communicate both of those things. It is more uncertain than you may have thought, but we really do know some things.

This translates into the arena in which I actually work; how mountain forested watersheds work. Which is not what most people view as a burning issue in and of itself, as I can't imagine many people in the public outside this field waking up and going, "God, I wonder how and why mountain streams, look the way they do with all these steps and pools and things?" That's a question you only come to after you've sort of dabbled in this stuff for a while. On the other hand, I think the questions many people do wonder about is, what are the influences and effects of human activity on watersheds in various ecosystems? The effects and issues, the things people pay attention to, have changed dramatically over the tenure of my career. When I first started to work on these issues, there had been a history of questions like the following: What happens if you cut this piece of hill slope? What does it do to the stream? Does it change the amount of water in the stream? Does it change the amount of sediment in the stream? Does it change the temperature of the stream? Does it change the nutrient composition of the stream?

The early Andrews program, the watershed program, was set up to answer specifically those questions, in as rigorous of a way as it was possible to do. And the reasons why those were viewed as important questions, ranged from issues around water quality. People wanted to know if we cut our forests, are rivers going to be perpetually muddy? If you go back into the original
enabling legislation for the Forest Service, the issue of human activity and relationships between trees and water, is fundamental to the organic act. The idea that the national forests were established to, "Maintain favorable conditions of flow," which congress left undefined, it gives equal weight to providing a sustained-yield [logging]. And one could view the history of forestry in this country as being one in which we largely have had a notion that forests were there, literally for conservation purposes.

So, the issue of forests and water, the history of forests in this country, has been one of Gifford Pinchot's rational forestry with a strong conservation flavor. The forests were initially set aside to keep them out of the hands of the timber factories. But then over the latter part of the century [20th], we have gotten very heavily into one of the purposes, which was a sustainable production of wood. One could view more recent history as being in part, sort of the pendulum swinging to the other side, as we begin to look at the other purposes for which forests are maintained. I think the public in general recognizes this, and certainly over my career, issues other than timber have grown larger and larger until now, they have a really loud voice. The issues of the day are really the warp and woof and tension between these different uses. Congress, with its enabling legislation [U.S. Forest Service Organic Act], may not have initially seen those two uses as incompatible, but because of the nature of population and just time, they've become viewed as increasingly incompatible, although there are ways of reconciling at least some of that. My impression is that the issues, the issues over the course of my career, the issues of what effect does cutting trees have, not just on clean water, but the supply of water in general? If you cut the trees, do you increase the propensity of rivers to flood? Do you increase their propensity to run dry in the summertime? These are the issues that have been, they have really been with us forever, but they were certainly the focal point of the early watershed program.

Then with time as the issues grew, we began to grapple with not just the effects of this timber sale on this hillside on this creek, but the fact that the landscape was now having to respond to multiple sales and roads and other activities distributed across a broad landscape, and that the scale of the human modification of the landscape was growing, and that we anticipated, we expected, or we conjectured, that there may be a commensurably larger scale of the impacts as well. So that is really where my career began, and my dissertation research was specifically designed to address the cumulative effects issue. Which is a very ill-defined issue, when you think about it, and it's a particularly hard one for science to get its hands around, because it's not just the effect of A on B. It's the effects of lots of A's on A, B, C, D and E, you know. And when those effects are manifest over different time and space scales, how do we put that all together? That's really what I got interested in early on. The Andrews program was set up to deal, at least initially, with this smaller-scale question. And it's only with time that we've been able to sort of expand the scope. But at the same time, the issues that motivated that expansion, have kind of gone along. It's sometimes hard to tell which is the cart and which is the dog, that is, the early interest in the expansion of studies, from the effects of cutting on timber sales or regeneration. Sort of a pre-level or plot-level, scaled inquiry, to those broader ecosystems, watershed or landscape scales, what we speak of as the landscape scale, now the regional scale.

There has been this steady progression and expansion of scales and as we've done this, and of course, the issues, have similarly changed. We're now talking about trying to manage and think about our effects on the fish. Fish may be the biggest challenge of all, because it started with the owls. The owls could, in a sense, could be viewed more as a plot, or, as a natural extension of the plot- or patch-scale phenomenon. The idea around the spotted owl was you needed big patches, big areas [habitat]. But it was still that same notion of a range of territory that an organism needs, and we can just extend it to fit our management schemes and recognize there are organisms out there that if we keep nibbling away at the place, they're going to be squished out of their territory-habitat range. Fish pose a completely different challenge, because fish use the whole place. They use the ocean, they use the estuaries, they use the big rivers, they use the small rivers. So suddenly, the scale at which you have to start thinking and doing science, is much larger. But at the same time, the sciences have been evolving, we now have satellites that give us pictures of regions where we can actually interpret the environmental changes over a region in the same way, which is somewhat commensurate in scale to the way certain organisms, we believe, are using the landscape. So, there is a kind of parallelism and it's also, the issues feed the science, and the science feeds the issues.

But getting way back to your first question, the really critical issues are around this question of how we interpret the effect of human activities, over the range of scales at which those effects or impacts are manifest. A lot of what we are trying to do, and a lot of the critical issue in the communication with the public, this is coming full circle, is getting people to think about the right scale, getting ourselves to think about the right scale, and then trying to communicate what the right scale is. Is it, for example, the right scale, if we are concerned about water quality? One of the big issues is municipal water, the quality of water being supplied to municipalities. In the forests, some huge proportion, 70, 80, and in some cases, 90 percent, of the total production of water that's delivered to a city like Eugene or Corvallis or Salem, comes from the forests. One of the outputs of a forest is water. And as cities grow and they push up against the forests, they're creating increased demand, and at the same time, the level of impacts on the landscape are increasing. What we want to know is, "What is the relationship there?" How do we think about our own development and our own use of forests for other purposes, as it effects this one very vital function of forests? We are inevitably faced with the scale question, which is, if we are faced with a creek that's too muddy to drink, is it because we have one bad actor up there, one bad timber sale with a slide that came down hit a creek and muddied the place, because one slide can, can turn up a lot of mud, or is it the pervasive, region-wide set of impacts? Are all of these sort of small, bleeding sores, collectively and cumulatively, creating an effect? Because the strategy you employ to deal with that problem is very different. So, this scale issue, being able to communicate this scale issue, and being able to figure out the scale issue, is a critical piece of the undertaking now, and I see the Andrews work as being one of the places, one of the intellectual playgrounds, if you will, or proving grounds, where some of these issues get tried.

Geier: A couple of questions or comments to follow up on that. One, if you could think about how and why your perception of the appropriate scale at some point in your research shifted, and secondly, and this is probably related, do you have any sense of how or whether the expectations of the public, or by the public, of scientists and what science can do, has that changed much in the last ten years, in your experience?

Grant: You're asking some real zingers. Let me take the first one. When I got into it, I thought I was biting off a big piece by jumping up to this cumulative-effects question. This was really an extension for me, and really, for the science. Because again, the science, were all the little plot or small watershed scales, and there were only a few examples in Redwood Creek in northern California, a couple larger experimental basins where people were trying to ask about the effect of cumulative impacts. I felt I was moving up at that time, and for me personally, it's been a struggle and constant challenge, to figure out how to do science at these broader scales. Because as soon as you get away from the plot, the thing you go out and actually get your hands around and measure, and feel and walk and observe, suddenly you're in a whole other ball game. It's easy to feel outside one's comfort zone on this, and I have felt outside my comfort zone a lot, particularly as more and more questions began to be raised about cumulative effects, and at this larger landscape scale. Then you take it all the way up to the regional scale, and my god, how do you do science at that scale?

So, the problem for me has always been reconciling where these questions are being asked, and where you've got to be able to say something with the tools of science and the standard precepts you get taught in school, the idea of being able to reproduce an experiment, that where the modification itself is kind of a grand experiment happening at the regional scale, with no replication? I mean, it's fundamentally alien to the way you think about science. It so totally challenges your presumption of how to do it. And I personally, have always felt myself to be a bit conservative with respect to how eagerly we take on these larger scale issues. On one hand, I recognize that the scientific community in general, and I would add, the Andrews community in particular, has a lot to say about these larger scale, regional-scale issues. But what we have to say is very often informed speculation, rather than something for which we have hard data. It's a projection, an estimation based on what we think, or what we feel confident saying. Does that get at your first question, which was, which was how; remind me again what that was?

Geier: I was wondering if there were ways in which your understanding of the appropriateness of scale, shifted over time, and I was also thinking about things like major flood events.

Grant: Well, I worked on that from the get-go, so I've always appreciated at some level, that larger scale. I remember watching the flood of '96, and I had a couple of reactions to it. It was a fantastic event for me, and I know there was human misery and suffering attached to this thing, but at the time it was happening, I realized I was watching something I might never see again in my entire life, and how fortunate I was to have witnessed this thing. Here was this little creek that I had waded in and barely got my knees wet. Now this thundering, broad-shouldered beast, was taking logs and rolling boulders with impunity. I was watching this and was utterly fascinated. I couldn't see enough of it. Then, I remember thinking, what I am watching at right now is happening up and down the spine of this entire region, and I was trying to get a feel for that, and I couldn't quite get it. It was really not until we were driving from one place to another in the forest, as we just happened to turn on the radio, at about three or four o'clock on the day, when, really, all hell broke loose. We began to hear, on the radio station in Eugene, and it was clear they had turned all their programming over to dealing with the flood. "We have got so-and-so with a horse stuck out in a middle of a field that needs help, can someone please bring a trailer." You know, "We have people needing assistance over here."

It was clear this thing was playing out at this grand scale, so here was this thing that we here were up there, really seeing it, touching it, observing it, and trying to take movies of it. Suddenly, you are forced to think how this is playing out at this much broader scale. But in terms of the intellectual and scientific basis for that, my training really helped me think. I think I've always recognized things like floods or fires, I mean, you're trained as a geologist and you think about how mountains and plates move and long-time scales. That's fundamental to the training, so it's not that my thinking about the importance of scales that has evolved. I think what I have more of is an appreciation that one can actually do something that looks like science, even if it's not perfectly the scientific method, at these broader scales. That's what's really changed. I have more confidence than I did before that we can say something meaningful. Again, I don't think it's the same thing that happens in a biochemistry lab, where you have an experiment, that if you do this same experiment the same way, you are going to get the same results. It's not like that, but you can apply scientific understanding and test ideas at the landscape level.

The second part of your question is, "How's the public perception changed?" I think there's always been latent reactions in the debate about how forests ought to be used, the scale issue. The issue of wilderness, for example, one I kind of cut my teeth on, was fundamentally a scale issue. Which is, if you got all this area that is cut over here, and here is the pie of natural and public lands, and a huge chunk of it is in this category, and very little of it is in this category. Well, maybe we should be paying attention to that total bounds. That's in a sense what the wilderness issue was about. People were motivated to say, "We ought to be making this slice of the pie bigger, because once it gets into this other slice of the pie, it's, it's incontrovertible. I mean, you can't go back the other way." So, the scale issue has always been there, but I think there has been an increasing sophistication of how we articulate it, and now, with the focus on organisms, like fish, has expanded so that now we have a regional, a widely-accepted, regional problem: the salmon. Now, what I don't think has happened yet, is that I don't think this idea of landscape complexity and dynamics has really introduced itself into the public debate. This is another addition to your question, "What are the critical issues, or what is it that the Andrews group in particular has a role or has played a role in the past in helping to advance?" I think one of the things that we've certainly played with among ourselves, and I think perhaps have tried to communicate with varying degrees of success to the public, is this idea of landscapes as dynamic entities. Reflecting back to the flood, you may have seen this. Have you seen the flood video?

Geier: No, actually.

Grant: You ought to. I'll give you a copy. If I don't do it now, I won't remember.

Geier: Mark was telling me about it. I haven't seen it.

Grant: Have a copy. It's interesting, and there are some interesting issues that surface in that.
Geier: These are from the Andrews mostly?

Grant: The Andrews and Siuslaw [National Forest].

Geier: Okay.

Grant: And it [*Landscapes of Change* -- probable title is *Torrents of Change*] includes some of the footage we took during the flood of '96, but on the tape, I was asked, "How do I think about the landscape?" And this just popped into my mind when the guy asked me the question. This wasn't a preprogrammed answer, although it now has a certain program quality, because I've said it more than once. But it relates to this issue of dynamic landscapes. I was asked about the way I think about the landscape, one which goes through decades of really not much happening. Decades of boredom, and then something like a flood or fire hits, and you've got hours of chaos. And this concept, it's funny too, because when I was thinking of decades of boredom, I was also thinking about all the hours I've spent up in the Andrews, sort of rattling around in the back of a van, or listening to somebody talk about the forests, and feeling like I really wanted to go to sleep. (Laughter) You know, it's palpable sometimes, the place, but then all of a sudden, boom, you get a flood, or, we haven't had a fire on the Andrews, but I can imagine. If we got a big fire there, it would just blow us into a whole other orbit. So, our own science, could be characterized as decades of boredom punctuated by hours of chaos.

But the concept that is the hardest for ourselves to grasp and communicate, is complexity, even though we live it and deal with it, then communicate that to a wide audience in the public. This is a really difficult challenge because people want to see things in simplistic ways. It's either natural or it's unnatural, it's scared or it's profane; it's pristine or it's manipulated. It's that kind of thing. Well, there are lots of times, probably when this landscape was not an aesthetically-pleasing place, and yet it changed into something else. The nature of that change, the processes that drive the change, and the consequences of that change are things that I don't think we have really grappled with and understood. If I can take a quick aside, you know, it's like going to Europe. You go to a place like Switzerland, and Switzerland is gorgeous. It's a mountain landscape, and there's cute little chalets, you hear the "sound of music" [reference to movie] from every hill, and it is a gorgeous place. So aesthetically, it resonates with our human instincts, yet it's absolutely "unnatural." I mean, it's been walked on since the bronze age. The forests have all been cut, were cut 2000 years ago. It's all been converted to another kind of existence, and yet it still retains some basic qualities and quantities of what it had before.

And so, here in this country, we have not yet converted, we still have remnant pieces, in some cases some pretty good chunks of remnant pieces of what the place looked like before human beings really changed it. And we have a very strong affinity and a feeling that, maybe we ought to retain some of this. And yet what we're faced with is the fact that that in the broad scheme of things, all of these landscapes changes and human beings are just another agent of landscape change. And I don't think we have fully internalized that lesson, it's almost like going from Newtonian physics to Einsteinian, relativistic physics. We still want to see things in the sort of simple-cause-and-effect model. And yet what we are learning about landscapes, is there is no absolute state that any of these places have to be in. They are constantly evolving and changing and turning into other things, and these things have consequences. I mean, there are times in the historical record when salmon completely disappeared from the Columbia River system. But then they came back. They disappeared for like hundreds, if not thousands of years, due to big landslides or something else. So these big disturbances happen, and they change the nature of the real estate, they change who's on the real estate. And that's, I think, one of the issues that we deal with and we are learning to deal with, at the scale of the Andrews or a small forest, but I don't think that concept has really entered the public debate yet.

Geier: I was thinking, one example of your effort to communicate was a good column you had in the *Oregonian* shortly after the floods and the debate over the landslides. Maybe you can talk about your concerns at that point in writing that article, and what you specifically had in mind.

Grant: What motivated that was coming in, having this long history of discussion about the effects of landslides, and not just landslides, but all effects of forest management on processes like floods. There'd been considerable debate about how human activities exacerbate floods? If so, how much? And for what processes? And there was for me, and I think for Fred [Swanson] as well, a sense there was a lot of common wisdom that was galvanized by the flood itself. That is, people immediately went to what they thought they knew. Which was, thinking this thing's got to be, you know, the Forest Service's flood. We all know that roads and clear cuts and so forth cause landslides, and there were some early reports out where people had gotten up early on in an airplane and flew around, thinking, "Oh yeah, there are a lot of landslides and a lot of them started at roads and clear cuts." That just seemed to me, immediately, the spin-meisters on that side were, you know, talking. Then, on the other hand, there were people saying, "This is just such a huge event, this doesn't have anything to do with human beings; "It's God's flood." So again, we were getting into the simple, syllogistic debate, "Is it God's flood, or is it man's flood?" What motivated us to put that piece together, and I think we were only marginally successful in retrospect, I think we didn't do a good job and think we just further muddied the water by introducing this idea again of complexity, and trying to clarify what that means.

If you read that opinion piece, what we are really trying to say is, "First, we don't know for sure. It's going to take some careful looking to figure out who's flood this is. Second, we do know some things, and the things we know are based on a lot of history and a lot of study by some pretty reputable people, and here's what we think some of those things are pointing to. Third thing, before you start using this flood as an example of something to motivate the current debate about what policy we ought to take, keep in mind that the policy has shifted, but that this has not yet appeared on the landscape yet." There are lag times inherent in this, and a momentum or inertia, landscape inertia, that by the time we decide to do something, the system is already moving in a another direction, and that is like turning an ocean liner around; you got to go a long way before you can go in the other direction. And there have been some significant changes that have not really appeared on the landscape yet, so be careful about how you interpret the results and effects of this flood in light of these new practices. Those were the motivating ideas behind that piece, so you can see where and what it goes back to and what I started with, which is trying to add a little flavor, saying, "It's not as simple as you might want to believe out there." But then, it was interesting because it was then played off against another piece in the same page, which was much more of a here's what we know piece.

Geier: Was there much response to that?

Grant: Not huge, but we got a couple of letters from people. People were actually quite interested in the contrast between those two pieces, and what I discovered in lots of these things, is that these "public" opinions, and I don't know exactly what "the public" is, people hold their opinions in such a way that even when new information pops up, that new information is typically screened and filtered in such a way that those pieces of new information that extract one's deeply held views. It's true in science, too. You read a paper and you narrow in on that piece in that, that supports your biases. It's rare that you get somebody to react by saying: "You know, I used to think of it this way, but because of what you said, I now think of it that way." So, most of the reaction we got was sort of like a "thank you" for writing and confirming me in my bias. (Chuckle)

Geier: That's what I would have expected, actually. I'm curious. You talked quite a bit here about scale and the way in which that tends to create more problems and complexity for doing science. I am curious about two things: One, how much of your work you do off the Andrews, given that, as you were talking, where the scale of the Andrews is a limiting factor? Secondly, at what level does that work on and off the Andrews, satisfy your concern about scientific rigor? In other words, how do you balance the concern about scale and fuzziness of science versus, as you pointed out, your conservative tendency towards scientific rigor? How do you feel about that?

Grant: Boy, I would love to get a list of these questions, because a lot of them are the sort to meditate over for some time.

Geier: This one sort of occurred to me as you were talking.

Grant: No, you're right on and are picking up on some very interesting, and for me, unexplored areas. That's what I am intrigued by, but it is a very useful to think about, first, the facts. The facts are, the Andrews work probably represents, maybe 25 to 35 percent, of my total effort. But that changes with time and projects. After the flood, for example, a lot of what I was doing and thinking was Andrews-based, because here we had this rich store of previous data, we had had the big event, it fell right in my domain, and I was realizing this is something we can really mine. Then, as we got into it, we realized, the Andrews, even for this flood, is only one piece, and there are other places we should go to try and put the Andrews in context. So, we went and took on places formally outside the Andrews, but which displayed phenomenon we were interested in, like other streams that either did or did not experience large flood change. And then, I have been recently doing a lot of work in the Coast Range. Most of it started in trying to understand and develop ways of thinking about the Coast Range in its "regionalness," rather than going out and studying one creek or one bunch of creeks. Then, I've also got a lot of my work, while some pieces were done either at the Andrews or in its environs, it tries to get at things that are not linked to any one place. Ultimately, we would like work at the Andrews to be something that says something about fundamental processes, rather than processes at the Andrews. So, there's a warp and a woof as far as how engaged and involved I am. It's also hard to say, to really add it all up, because a lot of my students, or people I advise, are doing work at the Andrews, which I kind of consider in this fuzzy umbrella of issues on which I have some interest in and activity. The second part of your question was, what's, why don't you repeat it back to me, as it had to do with the how I reconcile the larger scale.

Geier: Yeah, you're concerned with the problem of doing larger scale research and the tendency for that to become more difficult to replicate, and your traditional standard in scientific research. I was wondering what strategies you have evolved to try and deal with that?

Grant: Well, it's very much a work in progress. One idea and one thought, is that I've kind of backed into uses of models and modeling, taking advantage of some of the technology, which has really just been amazing. I'm sure you've been getting this from other people, too. The proliferation of technology, in many ways, science is led by technological advances, just like everyone else. The fact that we can do remote sensing, the fact that we can do GIS analysis, has kind of an imperative attached to it: You can, therefore, you must. I resisted some of that, I mean, I know how to make a computer work, but I've drawn the line at learning GIS. Not because I can't do it, but because I don't want that, I don't want to get good at it. Because then I suspect everything, every question, I have would start looking at a question I could only answer with the GIS system. I don't know if that is true or not, it may just be laziness, but I've certainly found myself thinking along those lines, and this modeling is along these same lines. That is, the models, for example, tell me the climate, how much it rains and where, and give me a landscape, and I'll tell you how much water is in the creek over, not just how much, but the time-trend of water in the creek. That's a hydrologic model and I've played some with those. These are things that you really do over big landscapes. You know, you can predict what the flow in a Lookout Creek or McKenzie River is going to be. In the work in the Coast Range, we've begun trying to link these hydrologic models with landslide and debris-flow models, ultimately linking them to channel models. So, you can sort of build these games where you take models in their own right, and then link them together to create larger models or models of models.

All this is very compelling and fascinating, in part, because what it does, is it gives you numbers to play with. You create something and then you run it, and it gives you what looks like data coming out of it. It looks like but it's not, it's kind of a fun house, where what you're getting reflected back at you is, in many respects, what you put into it. That's what I've learned, what I've been trying to learn, as to how to deal with these larger issues, which is that the models give you is a way of generating hypotheses at this larger scale. They give you a basis for saying, "If what I think is true as encoded in this model, the model becomes the encoding of what I think is true, then the system ought to look like this." That then provides something you can go out and look for. And suddenly, I've become more comfortable with using that as the basis for operating at these grander scales. The problem has always been you can't really generate, it's very hard to generate hypotheses, in these large scales. What you can do and what you end up doing, is describing here is what the pattern looks like.

There is a lot of work right now in pattern description which is absolutely vital to science. You've got to start with the taxonomy before you start talking about evolution. You have to know why a bug is different from a lion. But for myself, I find the intellectual challenge to be bound up in saying, "Why does the pattern look the way it does, and how would I know why the pattern looks the way it does? How can I test that?' I mean, that sort of instinct of wanting, and I think this is true for all people who are trying to describe pattern. They are doing it for a reason; it's not just for pattern description. But I really want to jump ahead to that point, you know, I want to find some patterns that have already been described, and then understand why, why are they the way they are. And so that's been the way I've tried to reconcile this larger set of scale issues which is, can we use another way to frame the question, such as, "What are those ways of describing patterns that are really useful in terms of testing models of how a pattern got there?" There's an infinite number of descriptions of pattern. We could run a sensor over the Coast Range and ask, "What is the distribution of gases coming off this landscape, or what is the distribution of water vapor coming off this landscape? Now, that may be very important to try and understand the way the land interacts with the climate or something like that. But for other things, it may not be that important. It may just be a description, and description employs within it that you have an idea about how you are going to use it. And so, that's part of what I've been focusing on in my own work; "What are the really useful descriptions of patterns out there, and what are the scales at which those, that we make those pattern descriptions, and then, how do we use that to actually say something about how it got that way?"

Geier: Good entry into a question I was going to ask later, but this might be a good place to focus. Talking about the use of ideas and descriptives in relation to this hypothesis testing, maybe you could talk a little bit about your understanding of the sort of differences and concerns between what people might call applied research and basic science? What is your take on that?

Grant: Let me jump ahead beyond what I would predict to be the most common, I mean there are easy answers to that; "Well, it's a continuum, and obviously one informs the other." Let's take that as a given, and go to the next step and ask, "What really are the differences?" I think there really are differences, and if you had asked me that question five years ago, I would have probably stayed with the simple answer. I feel that I've been fortunate to have stumbled upon something which is a basic scientific discovery. Basic. I've only done this once in my career. It has such a novel feel to it that I realized it's a gift from the gods, and that if you only do it once in your career, you're very lucky. It feels really different than everything else I've done. It was an insight into what I think is a fundamental property of nature. I don't want this to sound bigger than it is, but it was for me personally, a very big experience. And it's funny too, because it's an experience, that despite the fact I work in a scientific enterprise, is one that I have a very difficult time sharing with my colleagues, because it feels so different than most of what I do, and most of what everyone does. And I constantly struggle here to try and separate my own ego from this. I feel it's a gift. I don't feel it's something, I feel that I was fortunate because of my own experience to be in a place where I could see something. But that's what it was, I mean I just saw it, something that nobody has seen before, or at least has not seen in quite that way.

When I graduated and got my Ph.D., my father gave me a little thing here, by the guy who discovered Vitamin C, Albert St. Georgie, who researches to see what everyone else has not seen, and to think what no one else has thought. I got this from my father, and "Thanks dad," that's really a nice thing and thank you for sharing that, and I put it up on my wall. But I didn't really understand it until I had this experience. I did not realize that there really are times when you can think about things in ways that everyone else has thought about them, and suddenly you see something no one else has seen. So, that is what I consider basic research. I mean, that is the fundamental research experience. And to have that insight, and then try to go and show it, to test it against some real data. That feels different at a fundamental level than most of what I do, which I think of it as much more applied. I want to describe the channel in a way that I can distinguish between logged and unlogged systems. I want to build a model that allows me to test whether the changing pattern of vegetation has had an influence on the character of debris flows and consequences for stream channels. I want to examine the effects of this dam on sediment transport and channel morphology of the lower river. All those questions, they are wonderful questions, and intellectually engaging questions. They have many ramifications, you can extract very useful insights into how nature works from any one of those things, but it feels different.

That's the distinction in my mind. I don't know if that comes through or not, but there's something about the process of looking, of trying, and you can't even do it by trying, that's the thing, as I can't make myself have an insight. You can think about something, you can sort of prepare yourself for it, you can constantly be challenging your own interpretations and experiences, and there are sort of things you can do to create a seed bed, but you can't make it happen. I don't expect it will ever happen again. I mean I really don't. If it does, it's a miracle, because it, the feeling, the other thing about it, is the insight, which I'm happy to explain to you, feels to me to be deeply embedded in what it means to do science. Unlike anything else. What I mean by that, is you can prove it right or wrong. And I also know that if I'm dead wrong on this thing, that is, if I completely am off on the wrong course --

End of Side A, Tape 2 (of 3)

Start of Side B, Tape 2 (of 3),

Grant: -- it doesn't matter, and that too, is a marvelous experience. What it means, I don't really care. If I am asked, I say I don't really care if I am right or I'm wrong, because either way, it makes a contribution. Now, the thing fascinating about this, is that I think this is the greatest thing since sliced-bread, and I've sort of thrown this out there, published on it, and have been met with deafening silence. So, the road, if you look at history of science, it is full of crack pots, and some people think the moon is made of Swiss cheese. But the worst part, is thinking, what I really want is for someone to take this thing on, and prove it up or down. The worst thing is silence, then it's like, excuse me! But, that's part of the game, too. Part of the game is either that this thing is so trivial, which I don't think it is, so trivial that everyone knows it, and alright, what's the big deal about it, or else it's out there on a limb where the stuff in-between hasn't quite developed to the point. Or, it's just dead wrong of what everyone thinks of it, maybe it's not so trivial, but maybe so off-base, no one wants to spend time on it. I haven't quite sorted that out yet. What I really think is that I would love to do is spend some time on this idea, like a year on the reality, the linkage, while there is linkage there between the specific problems of how up the river and so forth, the linkage is miles away. You have to develop this so that it's a tool, or so that it solves somebody else's problem in a very direct way. It's going to take some integration and it's going to take more than I can do. It's a bigger job.

Geier: So part of your problem is resources and funding?

Grant: And just recognizing that this might be important. You see, in the context in which I work, it's not very important. That's the irony of it, is that in the management-driven world, and getting back to your question, what does applied research mean? Well, applied is you got some basic, you got some maybe very basic, but you got some specific problem in mind that you really want to have solved. For example, what is the effect of A on B, even if they're very broadly characterized? What is the effect of cutting trees on streamflow and water quality? That's a very big topic, a career's worth of work right there, but that's where I draw that distinction, because it may not be appropriate to call having a scientific insight as being the only thing that constitutes basic research. But, it's the one thing in my professional experience that has such a different feel to it that I want to call it something else. And I think the applied universe in which I spend most of my time, 90 percent of it, I'd say, is one in which the nature of the work is much more. In some respects, it's basic in that we are trying to understand, how do mountain streams respond to floods? That's a basic kind of question, but it's not very far to jump to ask, "What effect does human activities have on it, and how might we predict the effects in such a way to mitigate consequences of floods on whatever? And we, all of us, use that very short linkage there, even if our focal point is just really to understand floods. We're very quick to grab the human dimension of the problem, the specific, here's what we are going to do, we're going to solve this problem for you. And the environment we live in, we're very eager to go that way, mostly. I mean, part of what I love about my job is I get to do things which I think are relevant to a larger set of societal issues. But, it's been an interesting experience to try and reconcile this thing that feels like it has very deeply, a kind of "science spirituality" quality, and you feel like, "Wow I get it, I really see it, I get what St. Georgie is talking about!" And I don't want to call it more mundane, because the quality of the problem requires just as much intellectual effort, just as much blood sweat and tears to pull off, but there is something different about them.

Geier: It's probably a gross oversimplification of what you're saying, but just to get it straight, because I want to talk about first what the insight was, and then, what the publication was, it sounds like a good example, but my oversimplification is that you just said that basic science is something that you don't really care if it matters to anyone else or not, and applied research is something where one of your concerns is that other people are going to care about this.

Grant: Well there's something, there's some truth to that. It has to do with the level of work and where one cares, the locus of caring. Do I care whether people read about the basic thing? As I was saying, the silence is the worst part of it. I do care about that. Not to the point that I am going to make a big deal of it. For me, it was absolutely essential that I publish this. I mean, it was like a gnawing feeling in my gut until I got this thing out. I had to get it out. And it was an interesting process there, too, because I shot it at nature and science, and said this is tough and almost got into it, you know, very hard to break into that kind of thing. And particularly with an idea that is not really well-developed, not fully-developed, which it wasn't. So, it's not just, in one case I care and in the other case I don't. It's more that I think it has more to do with that I think that true basic science, to be put out there in a way, that whether it's right or whether it's wrong, it represents a contribution to two things: One, is it's put out there in a way that can be shown to be right or wrong. Most of what we do cannot be shown to be right or wrong. I mean you can't, most people won't replicate it, and even if they do, you can't. And most of tests of it are not clear cut. It's interesting, because at about the same time I put this paper out, I put another paper out co-authored with Julia Jones, which had to do with the effects of clear-cutting and road construction on peak flows. This hit the street two months after the flood in February '96. And the reaction to that paper has been 180 degrees. There has been storm and fury around that paper unlike anything I have ever experienced. So, this discussion about what's the difference, is really, specifically looking at the effects of human activities on a natural system using very long-term data and some sophisticated statistical techniques, and we came up with things that could be viewed, in many cases, as either, A, a dramatic new insight, or B, corroboration of something everyone already knew anyways, or C, either absolutely wrong or really sloppy science. The reactions to that have been all over the board, especially in the newspaper, which promoted two different groups to come back and write papers in their own right, specifically directed at this one. The timber industry hired a statistician for six months to completely reproduce our analysis. So, there's reproducibility in the sense of they literally took our data and redid it using their own approaches and techniques.

Geier: What was the journal that came out with it?

Grant: Both of them came out in the same journal, *Water Resources Research*. I'm happy to provide you with them.

Geier: Yeah, if you have reprints that'd be great.

Grant: So that for me, contrasts how these two things had played out, and now one of the distinctions is truly, that is the difference between these, well, it is the difference. People care about this and they don't care about that, when you come right down to it. That's what it is. And so, maybe the difference has to do, just not so much with whether you care, but whether the work is done in a way that someone else cares. I've never put it quite that way but it's an interesting distinction, it's a potentially important distinction.

Geier: It's an interesting one, because it's a different way of addressing it than most people I've asked that question of have come up with. Maybe you could take this example and talk about it, because I think you're on to something here. First of all, where were you when the insight struck, was this on the Andrews or --?

Grant: Sitting on the beach.

Geier: On the beach?

Grant: On the beach

Geier: Hmm.

Grant: It shows you how odd things happen. The insight hit me on the beach. I was on the beach with my family, and I had always been fascinated by these little channels that flow over the beach. These little tiny channels your kids love and throw sticks in them, and so here's a picture of one. I've always loved them, because they've always been the kind of river you could, get your hands and your feet around, you could walk in them and watch them, and the insight was a recognition that every one of these channels that I had ever encountered in Oregon, which has some specific properties of the nature of the beach, was doing something, they were all doing it. And the fact that these channels that were flowing over the beach, were kind of ultimate channels, in a certain way. By ultimate channels, I mean is they were channels that could do anything that they wanted to do. They are high energy streams because the coastline in Oregon is a high-energy coast. The slope of the beach actually comes from the wave, the ocean wave. And they, because they are high energy waves, they impose a fair gradient on the beach front. These old streams come down off the headlands and they hit this beach front, and they have a high energy with the slope which is kind of imposed upon them by the wave, it's a given, in a sense. At the same time, they have, what they are falling over is sand, and at the slope that the streams flow over the beach, they can move all the sand they want. In other words, they're at that kind of capacity. They are not limited, as most streams are, by the amount of energy they have relative to the amount of the stuff they are flowing through. If we go out to that stream at that flow, you can see the steps. Nothing is moving, the flow and the bed of the channel are not in any kind of equilibrium, or that equilibrium point is way out there. It is only during floods that that thing picks up and moves around. But here is a stream that is totally in a dynamic kind of setting, because it has all the energy it needs, the sand is perfect, because it is completely homogenous. It is nothing else but sand, and then I realized that the fact that all these streams were doing the same thing was telling us something.

But the insight went beyond that, because what I realized was that the process by which these streams were changing, and they were. If you watch these streams, what you see is these little waves building and then breaking, and then, the stream goes back to being kind of flat. Have you seen this? You can if you watch these things. I realized that where I had seen essentially that same process when I had been working in Japan, on why these boulder bed streams form these kinds of steps. And that the concepts of mechanisms of step-pool nature that I had developed in the mountain streams was actually identical to the mechanism that these sand-beds create. So suddenly, I realized I had an idea that spanned, everything from a sand-bed stream to a boulder-bed stream, and everything in-between. So, what happened for me was a combination of being fascinated by these little dinky streams, experience working in these big boulder-bed streams, and then just having this idea because I was interested in how these things form, and then seeing that same mechanism happening in the sand bed. Realizing these were ultimate streams, they could do anything they wanted, realizing that this is a fundamental tendency of all streams that are steep enough and energetic enough to display it; that's what this paper is about.

Geier: In a sense, your scale thing, where you took things off this locale, where you apply it to, almost the global scale?

Grant: It turns out it might even work on Mars. Seriously, I've been in communication with the guy who's the head of the Martian Pathfinder enterprise. It turns out it may even work there.

Geier: They are finding these same channels?

Grant: They are finding sub-channels, and if you start doing flow reconstruction; we are going to be talking in December about that. I was glued to my television set when this stuff came out. But you see how different that is with anything that has to do with forestry, a whole other scale of enterprise, because it represents something that isn't proved, if it's a stream, whether forested, mountain, sand bed, or whatever. It is cross-cutting, so in a nutshell, that's what I came up with.

Geier: Getting back to what you were mentioning earlier about group things. This would be an example where a group thing becomes part of an individual insight. I mean, you were basically involved in this effort to expand the concept of scale in your individual work, and with the LTER. You go on this individual trip to Japan, come back to the Oregon beach, and here's an individual insight that comes out of that.

Grant: That's right. That's exactly right. And something you just said made me think of something that I hadn't really thought of before, which is that it is the experience of thinking across scales, and thinking about how does what I'm seeing here, how might that apply to the larger scale. On one level, that's something intrinsic, as I said, to the geologic or geomorphic training. You know, you get taught that. On the other hand, the experience of doing that is something that I think I got from the Andrews group, and I think I have never properly credited that, and so I appreciate what you just said. I think that's true.

Geier: Maybe you could talk a little bit about who from OSU and the Andrews, would you identify as your closest colleagues or associates, over the last, say five years?

Grant: Probably two. Can I give you two? Dave Montgomery.

Geier: Where is he?

Grant: He is at the University of Washington, and Tom Lisle.

Geier: Tom Lisle?

Grant: Lisle. L-I-S-L-E. At the Arcata [California] Forest Service lab.

Geier: Are they hydrologists?

Grant: Geomorphologists. And a third would be a friend from graduate school who has really been my closest friend/colleague, although we haven't ever written a paper together. And that would be Rob Jacobson with the U.S. Geological Survey.

Geier: Also a geomorphologist?

Grant: Yeah.

Geier: Did you want to add more?

Grant: No. That's it.

Geier: One of the reasons I'm asking this is to try and get a concept of how the group was interconnected with other programs. When you think about your experiences, graduate school, particular universities, John Hopkins, University of Oregon, and Oregon State University, how would you characterize opportunities for interdisciplinary exchange and cooperation here as compared with elsewhere? It's interesting that your closest colleagues away from here are all geomorphologists.

Grant: Well, Hopkins was distinguished by its interdisciplinary program. In one department you have a guy who was on Reagan's council of economic advisors, sharing a hallway with a guy who developed Marxist geography as a discipline. They hated each other, but shared the same hallway. It was a department in which you had people who were studying geomorphology as I was, who were doing sanitary engineering, who were doing systems analysis. It was a department of geography and environmental engineering. So, if you can imagine "sludge" and people who were studying plants and where do you put your pipeline to Alaska, and why are the inner-city folks of Baltimore being displaced by urban renewal, all under one roof, you have a concept of what the place was like. The theme that most commonly brought people together was water. So, there was a kind of organization that was thematic, but the real theme, what really brought people together, was the fact that we had a chairman, Wolman, who was eclectic in his thinking and interests, and who was trans-disciplinary in his outlook. He embodied thinking about everything about everything. Suddenly, everything had value. I got that in my graduate education. I actually got my undergraduate degree through doing an interdisciplinary thing about rivers. When I came here, it wasn't that different from my own experience, so I felt very comfortable. What I felt was comfortable, for it was in fact the lack of a strong complement of people who did what I did. I felt that more, and I've continued to in some ways feel that more than I have felt the interdisciplinary business. And it may be my own failings in that I have not abandoned myself to the true interdisciplinary exercise.

I believe there are people in our group [HJA], Fred obviously jumps to mind, who have themselves sort of abandoned the notion that they represent a discipline. They are really forging their own set of links and their own boundaries, sort of emerging boundaries themselves. I don't think that is what I am doing. I think I am more again, conservative, in a sense that I feel that there's value in having a strong disciplinary base to operate from, and to bring that into a group setting. And so it's true, when I look around, when I need nourishment and sustenance, what I do is interact with colleagues in my discipline, because I really don't have many around here to interact with. In doing so, I really come back in touch with many things that I have a harder time articulating. This business is foreign, I mean these are people for the most part, I work with every day of my life. But, while I've talked about it, the implications and the ability to sort of critically examine it and interact with it, with me over it, and call me wrong, is not inherent in what we do. It's not part of this place. So, I have a different take on this interdisciplinary thing, which is, I clearly have chosen to work within it. I get great value from it in the sense that I feel it has really pushed me out of what would otherwise be a very narrow or much more narrow, disciplinary tendency. I feel that it has really enlarged my world, and that now I can think about implications and consequences in a broader realm than I used to. On the other hand, I also see it as a challenge to sort of be faithful to one's roots.

It's particularly critical, when you think about training students. Because, I now see students being trained as if interdisciplinary-ism was the answer, or something. I think it's an open-ended question as to how you train students to operate in that environment. Do you do it by submerging them in it and teaching them about it? There are some people who I think believe it. I'm not sure I do. I have found too much of my professional consequence comes from feeling like I really bring something to the table that others don't have. It's not "turfism," but it's a sense that there are a set of tools here, or a set of perspectives, that are very useful to this broader enterprise, and I think what happens. I am getting on a soap box here, but my concern in terms of training, is that unless you train people to have that kind of base, a confidence that they can return to draw nourishment from kind of a tap root or spring, you're always in sort of a fuzziness that becomes much more a standard practice. I happen to believe that these disciplines, particularly the ones that have been around for a while, there is a certain set of ideas, there's an identity, an intellectual identity that one can have, that one can bring, and that is in and of itself, and valuable. So, I am afraid that I got afield.

Geier: That's good. You're raising some good points. I want to ask you about the issue of recruiting assistants, because you were talking about graduate students and the concern for instilling in them some base for knowledge. Maybe you could talk about your philosophy or strategy of identifying and recruiting qualified assistants, what kind of graduate students –

Grant: Do I look for?

Geier: Yeah. What kind of qualities do you look for?

Grant: Boy, that is a good question and it's one that's very much a work in progress. I don't feel I've gotten that one down. My history has been that, five, six, seven years ago, I decided that I wanted to have graduate students, that I wanted to make room in my life to be training students, in that kind of more intimate way than one does in a classroom. And initially, I was kind of an opportunist. People would come over and knock on my door, and say, "Well, I'm over here in the geography department, but I'm not really getting what I need or want, and I'm interested in what you're doing. Can I work with you?" And I would say, "Sure, here's something you might work on or have you considered." That really became more formalized after a while, in the sense that I was now getting applications of students and I would start sort of looking at them more critically. But what I was looking for, I didn't have any clear picture of what I was looking for, other than the usual, "I want somebody who can do everything and, you know, comes in with a stellar record and you hit the ground running." I've come to admit that I am lousy at predicting who's going to be good and who's not. And at the same time my search image has changed, based in part on my experience, which has been almost 95 percent good.

But, part of what I've learned, is that having students and training them is a big commitment and investment of my time, and my intellectual capital, and [chuckle] both of which are in short supply. The other thing I've learned is personality counts, and I'm telling you most of my experiences have been great, good to great, so I don't have this long history of either myself getting burned out on the students or them getting burned out on me, in any particular way. But, I know some configurations work better than others. There are some people, for example, the way I train students is the way that I myself was trained, which was a very strongly laissez-faire attitude. Students, the way it worked with Wolman was, you took a number, you got 20 minutes with "the pope," he described, he basically just threw out a bunch of stuff at you, and left you to your own devices to figure out what was important and what wasn't. And it was all important, and you had to figure it out, and I don't pretend that I am anywhere near as insightful, on target as he was. I know I'm not, but I still do the same thing, which is students come in, I say, sit right down, ask what they are doing, and I go off in a zillion different directions, and at the end of their 30 minutes, I try to give them a little more time than he gave me, but basically the same idea, and they go off. And I'm sure about half of them leave here more confused than when they came in. There are some students who take that confusion as a challenge, then work on it and do something with it, and there are other students who just go, "I'm confused and I'm pissed, because this guy is supposed to help me and he is confusing me." So, part of what I've learned to do is to try and select students who fit my style. So for me, the ideal student is someone who is stellar with a proven track record, comes in, instant rapport, requires nothing more than a sentence or two, and is launched. I mean, that's the ideal. It hasn't happened yet, but my concept is really that. Beyond that, I think that I look for students who are able, who are self-starters, who are able to navigate what really are uncharted waters in terms of extracting an education from a place as diverse as OSU, where there isn't at least in my area, a well-trod path.

More students have trod it, but there are some real potholes in that path, and there's not a strong institutional commitment to geomorphology as evidenced by faculty, full-time faculty. I've essentially, Fred [Swanson] and I, sort of, and Julia [Jones], have been filling the holes in what is otherwise not supported strongly by any one department here. I'm not a university professor, I don't whistle, I don't dance to, to university tunes. I'm probably lousy at helping a student navigate the real requirements for serving the university. I know that, and most of the problems I've encountered with students have been that I've had them go off, I see them as doing a research program where it takes a couple years to get your head into the idea of doing research. By that time, university says we want you out of here. So, there's those kind of issues, they're institutional problems that make it hard to function, in the past, even the recent past, I've sort of blithely sort of said, well I'm a Forest Service employee, but I really act like a university professor. Well, I don't really. I have to be careful about that and I've been little bit too glib.

Geier: As a Forest Service employee, you have the option of dealing only with Forest Service technicians and post-docs for filling positions, but you've made this decision to go towards grad students. Maybe you could talk a little bit about the relative merits and problems you've encountered with that very option here.

Grant: It's really a choice. I mean, the choice of going with students, is motivated by several factors. First, I enjoy the interaction with students directly. Doesn't mean that I don't enjoy the interaction with Forest Service technicians also, but I enjoy having these bright minds, and it helps me because they keep me on my toes, and the nature of the discussion is always very, very stimulating. But the other thing is that the way the economics work, there is some equation of time and money. And basically, if you go the Forest Service route, at least this is the way I've thought about it, it costs more money, but takes less time. To go the student route, it costs less money but takes more time. That's an over simplification, but in some respects it's true. A lot of what I do, a lot of my research, is funded in a very cobbled-together kind of way. Not the model that I get this big chunk of money, and then I get to sort of rationally decide how I invested it. And there's a little bit over here and a little bit over there, and then this thing comes up and I got to go. How am I going to spend ten thousand dollars? Well, I'll get a student, you know. It's that kind of stuff, so, some it is just driven by that, the nature of that sort of institutional setting. The other thing is that I find in many respects, the university is easier to deal with, institutionally and bureaucratically, easier for me to stick money in a co-op agreement on the university side, and then have students work on projects out of that, than it is to try and push through a position with the Forest Service. Now, I have to be honest that I haven't really tried. Other people have been successful at it, so I suspect that it is doable too, but in large measure, I find that for many of the things I need, I find the university much more responsive and able to accommodate the somewhat chaotic nature of the work scene more than the Forest Service. So, I tend to use it more, I think, for that reason.

Geier: Some good issues came up here, leading us towards a conclusion here.

Grant: Lead on.

Geier: One of the concerns that I have been trying to grapple with here, is what kinds of issues people find important to bring before LTER meetings, and how that relates to leadership decisions and the decision-making process of the group. These kinds of things; getting funding to hire assistants, what kind of programs get priority funding and to which area of this cobble stone structure of the LTER group. Maybe you could talk a little bit about your perception of how the decision-making process evolved over the last ten years or so.

Grant: The way I think about it is that, the different scales at which decisions get made, there is the scale of allocation of resources in which they are allocated. And that tends to be a process that is driven strongly by the grant writing and funding process. That is when we, the science enterprise, LTER, in my view, gets its clearest focus around writing grants to justify projects and obtain funding. I suspect this is not uncommon. But from a science enterprise standpoint, I think never are we more together or apart, because it has both elements of it, than when we have to sit down and justify our existence to each other. As a part of that, the decisions, the large-scale decisions, about how resources, and particularly new resources, get allocated, tends to be made at that time. As we put budgets together, we define alliances, we define who is doing what, and what their responsibilities are. In the past, there has not been much revisiting of those decisions. So, those decisions, in my view, typically get made at about 5-year intervals, and then you get launched on a trajectory that involves multiple years. And I don't think we have been very good, maybe I shouldn't put it quite that way, but I think it's true, doing the business of seeing how we are doing. Here's what we said we were going to do. Here's who we said was going to do it. Here's how far we expected to be at this point. Where are we? And then making course corrections accordingly. That can be viewed as a kind of rational model of how one ought to proceed. I don't think it works that way around here.

My view is that we tend to make these decisions by establishing what the order of feeding is going to be, the sequence of tasks. Generally, never is there a lot of discretionary money floating around in this outfit, so basically what we do with the grants is we establish who will be in the long-term, who will we support, because there are people whose entire existence is bound up in that. So the way the budgeting works, is we take care of those people first, the people we've decided we want to keep around. And then, we have a little bit left over to play with, and the playing gets doled out amongst a lot of players so that no one person ends up with more than, very much of anything, if anything. I mean, there are people who play without any research funding. Most of my work with LTER has not been funded directly by the LTER grant. Now, there's another piece of that, which is that the LTER funds a whole bunch of ancillary activities. For example, the field crew that goes out and collects all this data, their salaries aren't covered for the most part by LTER, but covered by the Forest Service. But that whole enterprise feeds data directly into the enterprise I then make use of. So literally, there is a whole data, QSG shop and all the computer systems, and there is an LTER contribution there, so we are just talking about resources there. That's one way to look at the way the power and decisions get made. It's basically ad-hoc, with the exception that there are these clear funding priorities that we all agree on, typically without much discussion, and that tends to be arrived at, more or less by consensus, and then, there's a sort of pot-splitting-up that is built around specific tasks and rough lists, and the rough scaling of how much things are going to cost, and what we expect to get done by it. There is some follow through on that, but typically, not a whole lot.

So that is one enterprise, the other thing is the budgetary loop, the resource allocations and so forth, and the set of issues that just keep coming up. Some of these come out of the discussions we have, the science-based discussions, some of them come because of external circumstance, like we have a flood, and then things fall apart during the flood and we got to put them back together again, how do we put them back together? You got money, more money than we need to just put it back the way it was, so how do we change it to make something better. What are some long-range goals for long range measurement? Facilities like the climate station. We expanded the climate station over the last five years. How was that decision made? Well, a climate group met and put forward proposals and put a budget together, and managed get external support. And we are kind of told to do that by the group as a whole, and there was also a blast at the end. So, again operating sort of on a consensus level, there was generally enough credibility with people, and members of the group held in high esteem stand up and says we really need to do this thing, there often is not a lot of debate. It tends to be more, well so and so thinks we ought to do, I guess we ought to do it. There is a lot of that that

I think goes on, to the Andrews' credit, and, basically, it's a group that functions well at that level.

I think there's some places were the group doesn't function as well, specifically because of that. We don't deal with conflict very well, and don't have a lot of experience dealing with conflict well. We tend to suppress it, and so some of that even comes out as suppressing debate, in other words, we have a tradition of not having contentious forums for debating these things. It's much more that people of different domains, identified and accepted domains of activity, in which they make decisions that the group as a whole operates to kind of bless decisions that often get made at a finer scale. And there isn't a lot of real challenging that goes on, except every so often something happens, and then, you really are faced with the fact that, not only might you not agree about everything, but you really don't have a lot of history in disagreement, so it has a different feel. It's like two people who never fight, suddenly having a fight. Suddenly, whoa, what's going on here? As opposed to two people who're always sort of having discourse. You know, then the weighting looks different. I'm getting a feel here, I think it has to do with not so much with how the decisions get made, but with the process of the group is for dealing with it.

Geier: How has the group dealt with that kind of conflict then? Lacking that experience and background, what's the outcome when that kind of thing does occur?

Grant: There is a lot of one-on-one talking that goes on. The group as a whole tends not to deal with it. It tends to fall to a few people in leadership positions to reconcile and work it out, often in ways that are not clear, not transparent to the group. But that's where the grapevine and the other webs and back-channel ways that people communicate among themselves kicks in.

Geier: So, that's where people like Art and Fred would make the decisions?

Grant: That would be a perfect example. Then, I don't know how much you know, but this is an issue that has gone on a long time. You are talking about Art and Fred in leadership roles?

Geier: Yeah, that's right.

Grant: But there are other issues. I mean, it's a bunch of people, and people have issues and problems with each other, and different styles of doing things. So that would work in different situations, but typically, issues tend to be isolated, and what happens, my impression, is people who don't feel that those issues were fairly dealt with in the resolution of those issues, they typically just disappear from the group. They dropout. Now again, I don't have another model for this, there are lots of other models, but I'm not arguing that there should be another model. I'm just looking at what happens, and again, this is my perception of it. But it is an interesting dynamic, because I think it's a little bit like the dynamic that goes on, if I may globalize a bit, it's like the way in a pluralistic society, there is a real premium on getting along. And we're in a pluralistic, an interdisciplinary, pluralistic society. There's a real premium on getting along.

In large measure, because in most cases, there is only one fish biologist or one geomorphologist. From a purely professional level, I don't understand what a fish biologist does. I don't know what is important. I count on him to tell me what is important. So it's interesting, and again, I'm maybe getting a little far field, but I think that the way our [Andrews group] ecosystem works is that there are niches that people fill, and it's probably very difficult to be another person, like another geomorphologist wanting to break in to this group where there is already somebody occupying that niche. So, in that sense, I haven't gone all the way with this. One of the problems, I think with training, is that we want really good people to come in, but you have to open, you have to make room for them in some way, and that room has to include intellectual room, has to include disciplinary room, has to include social room.

I think the way we have tended to operate is that we have a limited number of people, and they each have a portfolio, and then we count on everybody to represent that portfolio. Typically, there is a certain amount of, we don't talk about how do we make sure what's happening within that portfolio is really first-rate stuff? We leave it to the people within to do that themselves. How do we make sure that there is the next generation that's emerging within each of those portfolios, assuming we want the same group, the same set of issues, to be represented? I think, instead, we accept that we are going to evolve in some other direction, people will come in, and in many cases they come in, identifying and then occupying uninhabited niches, using an ecosystem model. I think that, I haven't put it all together like that, but I think that's true. Like we got into remote sensing, because suddenly there wasn't anybody doing that, and somebody shows up and says, "What a coincidence, you know, I can do this." Hey, great.

Geier: So, if any sort of a peer evaluation goes on or takes place in the group, it sounds like with your geomorphology work, how people outside the group view it, is kind of your gauge of excellence?

Grant: Right. Exactly. Entirely. Entirely.
Geier: Some people have talked before about how, say someone comes in and isn't meeting somehow the standards of the group, the common way of dealing with that, is that person simply doesn't get funded. The way you were just describing this, if I was following you right, is that those funding decisions are group efforts, in the grant-writing process and decisions of where is this money going to be allocated. So, from what you've been just saying here, it's sounds to me, and correct me if I'm wrong, it sounds to me like there's this political component too, that gets to be some kind of group acceptance to maintain that level of funding, and that it could be, as likely to be a community-based decision as it would be, say science-based or peer-evaluation based?

Grant: Well that's an interesting question. I don't know if we have ever been really challenged on that, because what the challenge would be is if we had someone who was doing first-rate science and didn't get along. I don't think that has ever happened. I think, and my suspicion, would be based on the way the group tends to function, is that if there is somebody doing first-rate science that nobody could get along with, they would not automatically be pushed out of the group. In fact, quite the opposite. It would be more likely that you would, the way the group is more inclined to work, is somebody not doing first-rate science, who everyone likes, would be continued ad infinitum. I have nobody in mind when I'm saying it, but that would be more in keeping with the tendency of this group, because of the common culture.

Geier: Several other people have made the same observations.

Grant: Yeah, that you would, because there is such a strong imperative for getting along. Because there is no one else who speaks your language, or you could convince yourself that you don't really speak somebody else's language. That nobody is really held accountable, I mean the accountability is really the big issue, a big issue. And I look at my own actions. I'm not sure I've been accountable for things I've gotten from the group. And there have been, but we have no mechanism really for even holding people accountable, and I don't really think anyone really wants a mechanism like that, to be honest.

Geier: Keep the informal structure, is what they're saying?

Grant: Yeah, it's all informal. Now as I say, most of the time that works very well. And I think it's the success of the group that we've been able to survive without a lot of formal structure around it. It has worked remarkably well for having been so informal, and at the same time, allowing different leaders to emerge, who are real leaders and really do have those abilities. I think this system would break down if there wasn't at least one person who could play a leadership role. If that happened, the structure could easily fall apart, because we don't have a strong set of internal mechanisms and processes for resolving conflict. It's mostly been done, because we identify somebody will be a leader, and they will resolve it if it shows up. That's what I think happens. If we lose a leader, it just puts us in certain kind of vulnerability because the ability to resolve and endure is not necessarily the way we would go in a group. The turning point would be if you have an identified leader who does it, but people don't think of them as a leader. If somebody were to advance into that role, and no one could agree that they were in fact the leader, I think, that it is probably the key vulnerability of our group.

Geier: It's an interesting point, because as you were talking about that, it dawned on me that I've never asked this question of anybody, and that is really simply, how leaders are selected? I know that with Jerry [Franklin], he just kind of evolved into the group, and then, I know that there was a transition from Jerry to Fred [Swanson]. Jerry essentially identified Fred as the likely successor. But I was wondering if there was any process of ratification by the group of that decision, or how did it go?

Grant: There may have been, but I was not part of it. I want to make sure you understand who you're talking to here, in that I don't consider myself to be strongly embedded in the power structure. I consider myself a player, but I've not been in the room when a lot of decisions got made, either by personal choice or schedule. Most of it's been by personal choice, and I always have the sense that if I wanted to play more of a role there, I could, because my program is in research. But I go off in a lot of different directions, not all of which are the Andrews, and I feel myself intensively mortgaged for time, and recognize some of the limitations. I'm just telling you that so that you understand that that you're getting a limited view of the way things work. But my impression is that it's done on a very ad-hoc basis, and that Fred just, "naturally stepped forward." For Fred, one of the things that made that easy was that he had a longstanding tradition with the group, has a personality style in which he doesn't create a lot of enemies, so there wasn't an invested constituency, an anti-Fred constituency, at any point. He has been very, very effective at that. And the third thing was, he had a different enough style from Jerry, that the people who were sort of burned out, there were some ways that Jerry managed things that Fred did differently, so that in a sense made it easy for everybody to coalesce around his leadership.

In my view, he has been very effective in that role, and finally, he's one of the few people I know who can actually hold in his mind what everyone is doing. He probably is the only guy in the whole operation who really understands, at least to a first approximation, what everyone is up to. The organization needs at least one of those. That requires someone truly interdisciplinary in their own thinking, because you have to be able to not go into a discussion with somebody thinking, my discipline is the most important and everything else is sort of a support. Fred generally doesn't do that. If he's talking to someone who's doing plant ecology, or GIS, microbial maps, or something like that, he can understand and think about it at the level at which they're thinking about it, rather than his own view. So, in this constellation of a group, that's a very, very important glue, because that way, you have at least one person who sort of does what Wolman did in the department in which I was training. Probably what I would have to say, the most important ingredient for a successful LTER operation is to have least one of those.

Geier: I've talked with Fred about this, and Art [McKee] also, but it's useful to get the insight from someone who's, as you say, not at the heart of the whole issue. I was curious if you had any perceptions of how the mechanics of the transfer of leadership roles worked. Does it involve something as simple as taking leadership in the LTER discussions or-.

Grant: Oath of office. (Chuckle) I think, it grew out of, um you know those times.

<u>**End of Side B, Tape 2 (of 3)**</u>

Begin Side A, Tape 3 (of 3)

Grant: The leadership transition occurred when Jerry would be off somewhere, and then Fred would take over. Or the group would have to choose who to send to a coordinating committee meeting, and there would be a short list of people who everyone wanted to make sure attended. In this line of work there are people who want very much to be involved when there are a lot of people who are interested in the process and interested in the outcome, but are not themselves willing to advance themselves forward into leadership roles, except where certain things come up. So I think the transfer was, really very smooth, because the group had experience with, people like Fred operating in that role prior, and the transition stage with Jerry was not an abrupt one. It was a gradual one. So, the gradual nature of the transition was what allowed it to happen without any kind of formal thing, and at some point, I think someone said, "All right, I'll be PI," and I think Fred was ready to serve in that role. Fred has a very nice way of phrasing those things in ways that make it easy for you, to go along with, "Unless there are strong objections to that, I'll just continue in that role." They just nod their heads. That's kind of how it happened, and again, it fit within the general strength of the group of not bringing these things into a sort of contentious place and finding the course of least resistance and following that.

Geier: As you say, if that's essential for success in an LTER program, is there any conscious effort on the part of the group now to identify people that are likely possible candidates for?

Grant: I think probably more so than I am fully aware of. The thing is that everybody, we're all grown-ups now, and we all look around the room, to see who is playing a role in which they're likely, in which that role is likely. And again, people who step forward and say, "I am sufficiently attached to this enterprise and I'm going to make it a main piece of my focal point." There is a very short list of people who do that. So yes, those people are identified and are identifying themselves, and for the most part I think the group accepts that. To not accept that would be, either means that you yourself would have to step forward, or else you would have to challenge somebody else, neither one of which are most people's inclination, particularly "if it ain't broke." I suspect that Fred thinks about that, and I'm sure it weighs on him, that "If I get hit by a truck, what's going to happen?" And it's like, if you have a feeling for the enterprise as being greater than any one person or the sum of its parts, you have to look to how we do that. I don't know, but that's where I think things stand now. I don't think there is a clear, designated, heir apparent. I think there are people who are operating in a way that would let them step into that role if they needed to. I think the group, I think any transition, in part because the nature of the enterprise has become more diffused over a wider group of people, wider set of disciplines, less geographical identity to the Andrews itself, or participants because they want to participate, that the next transition might be a little more bumpy. Even with somebody sort of working in the same way as the past, just because there might be some discussion about how we want to do it. Do we want to do it the same way we've done it? I could maybe project that a little bit.

Geier: As you suggested, it's not been a contentious process, and as you noted, if there is a conflict, the group doesn't have a lot of experience dealing with that. I guess Jerry Franklin and Dick Waring would be one example of how a conflict was resolved without disturbing the structure.

Grant: Right. That predated me, and to this day I don't fully understand that, and part of my personal thing is, my wife is a social worker. She lives her entire day in process and is entirely focused on it, and she has experiences in which all people talk about is the process by which they interact. We're not likely to do any of that, or if we do, it's only in a very artificial and almost, ludicrous fashion. We're not, we ain't good at it, and we've never been good at. And we don't have people who are naturally, are leaders in that way, although there could be. I suspect there are people who actually could play more of a role in that than they've been allowed to, in part because we don't acknowledge that something is an important ingredient. Where was I going with that? I think, well I was talking about Jerry, and so it's like a conflict that happened in the past, and we don't really look to that part of the history. It's like, that was then and this is now. It's a little bit like a family that wants to just forget about feuds, or divorces, or what have you.

Geier: Sounds like some discussions do take place take place between individuals.

Grant: That's exactly right.

Geier: Well I've taken up two hours of your time.

<u>End of Side A, Tape 3 (of 3)</u>

<u>End of Interview</u>