

Interview with Jim Sedell, Tuesday, February 14, 1998, 9:00 a.m., Dr. Sedell's office, U.S. Forest Service, Forestry Sciences Laboratory, Corvallis. Interviewed by Max Geier. Transcribed by Keesje Hoekstra

Born and raised in Oregon, Jim Sedell completed a BA in philosophy at Willamette University and a PhD in stream ecology at University of Pittsburg before returning to Oregon to take a post-doc position at OSU and the Andrews Forest in 1972. He quickly exercised his energetic leadership style in the Stream Team community across the OSU campus and the International Biological Program stream research. He led the Andrews Forest-based Oregon site in the highly influential River Continuum Project before moving to Weyerhaeuser Co. in the late 1970s. He was an early leader of aquatic ecosystem research at Mount St. Helens after the 1980 eruption and returned to a stream ecologist position at the US Forest Service Pacific Northwest Research Station in Corvallis around that time. A broad, synthetic thinker, Sedell advanced research on roles of big wood in rivers and other connections among ecological, forest, and watershed processes. He moved to Forest Service research administration position in the Washington Office and Directorship of the Pacific Southwest Research Station, before retiring and working for the National Fish and Wildlife Foundation. Even after leaving the region, he continued to support Andrews Forest programs.

Max Geier: To start the interview, what I've been trying to do with people is get a little bit of background on their origins and how they came to be working out here in the first place. My understanding is you started out here as a post-doc out of Pittsburgh, right? In about 1971?

Jim Sedell: Right, I came here the fall of 1971.

Geier: Same time as Stan Gregory. Maybe you could just talk about your personal origins. What attracted you out in the Northwest here originally? You went to Willamette University.

Sedell: Right. I was a philosophy major in political science and was going to go into a law career. About my junior year, I had a friend and we'd go out on the beach, and he'd name all the algae and all these animals, and I thought, "Boy, this beats a law library!" So, I kept the majors, but I started taking more science courses. I wrote to a whole bunch of graduate schools saying, "Hey, I'm a liberal arts student. I've taken this much biology and statistics, would you even consider my application?" Because I didn't want to pay the 20 dollar application fee or the 35 dollar application fee, if they weren't even going to take me seriously. The good ones didn't write back, the first few said, "We've got more applicants than we need, so don't bother." Then, I had a choice between Nebraska, UBC, and Pittsburgh. I'd never been east, was born and raised in Oregon. So, going to the East was a big deal, and they [U of Pittsburgh] gave me money to go year-round. I didn't know whether I wanted plants or animals in the Department of Biology.

That's when I ran into Ken Cummins, and Cummins was into streams. I liked his energy, and liked what he was doing and his enthusiasm. That kind of hooked me. I'd always been into fishing, wandering around lakes and streams in Central Oregon, and the high lakes. But he really focused me. Then he left and went to Michigan State, and I'd already taken all my prelims. I thought, "Why move to another school and go through all that crap again?" So, I stayed there, and we stayed in close contact. I used his laboratory. I'd drive from Pittsburgh to Kalamazoo to work on research in his lab. He was off my committee, and I got Dan Nelson from Oak Ridge because I was doing these isotope experiments and they had equipment there, and plus he was good at it, so I got Dan Nelson on my committee.

He was instrumental in letting Jim Hall know that I was available. I originally thought I was going to be working on aquatic insects with Norm Anderson and maybe a little bit of fish work with Jim Hall. Before I came out here, I did a quick tour and went and saw Stuart Fisher. And I thought, "Well, if this is a watershed and a forest project, let's go see what Stuart Fisher is doing." And that turned into one of the more productive arrangements, although his group and ours have not published a whole lot together, we've each published a bunch of things. But a whole bunch of students had cycled through in the early to mid-70's, including Nancy Grimm, who now is his wife and colleague at Arizona State. So, that got me thinking something bigger than just bugs and fish. The good thing about coming out here, as I tell Norm and Jim, was soon as I got here, they both left on sabbatical, and when they came back, they were working for me. Because I didn't see a future at all doing classical insect and fish work, I thought, "My salary is on the line. I'm going to align with Dick Waring and Jerry Franklin. There's more of a future there and more of a chance for learning new things there than carrying on in insects and fish." When I got here, they assigned Jack Donaldson, who was in the Department of Fisheries and Wildlife and who was Stan Gregory's major professor, to watch over me, because they needed a regular faculty member to do that. Jack is a wonderful friend. And I looked at Stan Gregory and I said, "I don't need this stuff." Because what Jack and Stan were having Stan do for a master's thesis I thought just didn't fit at all. So, I spent the first six months trying to get rid of Gregory, and it was the best thing that ever happened to me that I didn't succeed. (Laughter)

Geier: You didn't see what he was working on as fitting in?

Sedell: Oh, it wasn't going to be relevant. It was a little ahead of it's time in some ways, when I look back at it now, but it didn't fit right then. But, we were even good friends. It's been just a wonderful arrangement. Then I brought in another person to help me get litter traps and everything else up, Gary Brown, who was a colleague of mine at the University of Pittsburgh. That's what got the litter traps and the nylon netting up there to do the carbon and nitrogen budgets on Watershed 10, which I think is what helped put us into the ecosystem game. That's the year that Franco Harris had the "immaculate reception" game. [Penn State football player playing for Pittsburgh Steelers who caught "miracle" touchdown pass in NFL playoff game.]

Geier: Oh, yeah. (Laughter)

Sedell: Against Oakland (Laughter). So, I don't know when that was. I think it was '73.

Geier: Can we use copies of this?

Sedell: Oh yeah, right. I brought these in because these are notebooks and sketches of where we put leaf packs? I was going to give those to the archives. Here's some of these that just kind of got ripped away. Here is where we put it and this was in 1972 when we started there.

Geier: Fred has asked me to start putting some of this material together into an archive collection, so to speak.

Sedell: Here were the original dimensions of the Watershed 10 weir.

Geier: Oh, yeah.

Sedell: What it cost, an original budget number. Probably Dick Waring's, either that or Jerry Franklin's, that he didn't know about.

Geier: Was that in '72 also then?

Sedell: Yeah.

Geier: Well, you talked about your intellectual mentors, maybe you could talk about your first impressions of the H.J. Andrews, in I think it was, '71, you said?

Sedell: Yeah, fall of '71. Well, I went up and looked at this place. Like I said, most of the action was fairly traditional. I mean, we were sampling fish. What I kept looking for, what the storyline was. What was going to be our theme? And the theme wasn't really poppin' out at me. So, we created a theme and a core, and that was around material and nutrient cycling. Then we also started looking at decomposition processes in leaf packs. That was a carryover of being influenced by Ken Cummins. At the time the big Kahuna here was Charles Warren, who'd done an awful lot of energy budgets. He was the intellectual driver of a major group here at Oregon State working in streams, happening in the '60's and very early '70's. I came from a different tradition, and we started to get these people together. It became apparent for one reason or the other, that we would be cordial to one another, but it wasn't going to be an intellectual fit, because we were off and on to another tangent. So, we got up there and looked at this thing, and we very quickly decided we weren't going to do a whole lot on big streams, because there's just too much water coming off in the winter time, that we would focus on smaller streams, and keep our community work and some decomposition work. That was in '71-'72.

After that, we got into '75, and started to get into the River Continuum project, and started branching to larger streams and different kinds of questions. The first one was essentially reworking the original proposal into a coherent storyline that nested within a forest biome setting. The [conceptual] setting was really okay; what's the influence of forests on these

streams? Nutrient-wise, and leaf litter-wise, what kind of processing goes on? Then it became a better match and we had a storyline that we could sell, and then it was a matter of shoring up different parts of it. And in '73, Frank Triska came out here. That was a huge addition to us. He'd worked with fungi and decomposition in streams. He took over a lot of the nitrogen and the decomposition work. When Norm and Jim came back, they were a bit surprised about the change, but the amazing thing about the stream group, was they all just stayed together. I mean, people went out of their way, Stan and I went out of the way to keep Jim and Norm and Jack Lyford engaged and involved in productive work, and they had very good students that did parts of things on the Andrews. Even though it wasn't the big driving part, it was a crucial part for the intellectual robustness. Jim Hall would drag this way and he'd pull back, he was more conservative than us raucous post-docs. He was a superb mentor and editor, and that helped us.

I can't say enough about the role Jerry Franklin and Dick Waring played. They had their own interesting, strange dance, between one another. And it worked, because as long as they didn't take one another seriously, they could play good cop, bad cop; detail, big picture, onward, and it worked well. Well, they found early that we would deliver products, and that we were interested in making the whole thing go. We had a corporate responsibility as well as just a discipline and a professional interest in there. And maybe you could say that was because our jobs depended on it, but I don't think that's the way most people thought at that time. Most people were still thinking of their individual part in it. This cadre of post-docs started to think much more corporately for the good of the whole, and then would move out of their comfort zone in research and try some different things and try some interactive things. There was a lot of good encouragement from Dick Waring and from Jerry Franklin.

Geier: Was this your first experience with the IBP when you started working with this group?

Sedell: Knew nothing about it. Other than I knew that the energetics work of IBP was done in the 60's, which was a lot more zoologically-focused where they looked at energetics of lake organisms and stream organisms. There was a huge push on energy flow in the '60's, as the Europeans did. And then the movement really came over to North America, and that's when people got serious on the big [ecosystem] front here.

Geier: So, people you'd worked with at Pittsburgh hadn't really been involved.

Sedell: Oh no. Pittsburgh had good people, but they weren't into the mainstream of starting with ecosystem work. We'd read papers and all in classes, but no one was doing any of that at University of Pittsburgh. We just basically learned here as we went. I think that our mentors would be something like Mann in England, who went to Dalhousie in Canada, did a lot with energetics in streams research. Then our other mentors were at Hubbard Brook. Gene Likens and Stuart Fisher [Hubbard Brook-based] really got into streams, in Likens' laboratory, and then Judy Meyer followed with work at Georgia later.

Geier: As you think about your perceptions of the work that was being done at the Andrews at the time you started working there, did it fit with any other previous experience with experimental forestry? Things like that that you'd seen anywhere?

Sedell: No. When I got here, it was big confusion. There was a major tussle between the University of Washington and Oregon State. I wasn't privy to all of the insides of that, but I did know that we had to consolidate a team. If we were going to survive in the long term, virtually everyone that worked on the project had better be helping one another out, and had better be focused on something that we could all do together, or we didn't stand a chance. The place was too isolated, and the problems too big. That was one of the pushes with Stan Gregory. It wasn't a personal problem, it's just that Jack Donaldson wanted him to do this [salmon] carcass study, and I said, "This is just bogus." So, Stan did it, and then carried on with the Andrews. Stan is really one of the great people that made the Stream Team what it was. In retrospect, everything was looking our way when I was successful in getting him away from that in the first six months.

But, then we started to get together as a group, organized who's doing what, starting joint experiments in which people had different roles they could play, so that everyone knew where the core was and how they fit in on that. We met regularly, and it was just a good bunch of people that wanted to make it work. And we all had our different personalities and styles, but the common theme was to try to make it work. Once we got our collective thoughts together on streams, then when we go back to Waring and Franklin and tell them we were the best thing they got, because we were organized. Then we basically jockeyed for funds from there. At the time, the aquatics [IBP research program] was split between University of Washington and Oregon State. We had streams, they had lakes, and they also had a stream program. We were really shocked and disappointed when they hired the aquatic coordinator Bob Wissmar to operate out of the University of Washington. I felt that job should be mine. I was real hurt. Then Bob came down to find out what we were doing and how we were doing it. We very quickly established a rapport, and talked about how aquatics was treated as a fringe in the IBP, it was an interesting fringe, but a fringe. Our point together was that we were going to be marginalized in this thing if we didn't stay fully engaged, and that meant we had to get real coordinated together. The aquatics program had to stand or fall as a collective, not as a struggle between Oregon State streams and University of Washington lakes, and Wissmar saw that also. What we did was cut the stream stuff out of the University of Washington, and leave that down here, and they focused on Findley Lake, and then we would look after one another. We'd go to review meetings, and there'd be all this squabbling between different prima donnas at the University of Washington, and then there'd be kind of little struggles between Oregon State and University of Washington. And the solid [aquatics] group, whether it was at Washington or at Oregon State, hung together. So, if we were out of the room, the lake people from University of Washington would look after us, along with Dick and Jerry. The aquatics got real coordinated and was very interactive from the start. That was real important, and that's really what kept us in the game in many ways. We didn't get very parochial, and we didn't enter into the big struggles between the two institutions.

Geier: You're talking about looking after your interests, was there a struggle for budgets?

Sedell: Yeah, the budget was always a big critical item. Everyone could put 5-6 times what they were going to get, down, and probably do a good job of spending that money on their own research. And just the realities of big grants, you had a feeling you had to work within that. So then, it was a struggle to show you could come through with results, you could interact and draw more people in, and you weren't just on the fringes of the core program. And we showed that.

Geier: Had you been involved in that kind of fundraising effort before?

Sedell: Never, never.

Geier: Learning as you go, huh?

Sedell: Right. My motto was whatever it took. (Laughter)

Geier: Maybe you could talk a little bit about the facilities or resource needs at the time. First of all, what there was and then, what kinds of things you had to go get?

Sedell: Oh, there was nothing. Norm Anderson called it a hut or lab out at Oak Creek, and that was where Gary Brown did his work. We would buy used parachutes, and I'd get dimensions of lumber and how much lumber was needed. We had to jury-rig virtually everything we did. For early accommodations, we got Jack Donaldson's little camper, and we put that up at Mack Creek. First of all, we put it at Watershed 10, because we were collecting the leaves doing storm-watch [stream sampling during runoff events]. Then we would set up a rotation, and everyone had a weekend that they had to go up into the snow in the woods to tend these nets, so they didn't clog and we'd lose data. But, it was very primitive. I was there when Al Levno and Art McKee and Dick Fredriksen made the settling basins in Watershed 10. That was just with a chainsaw, and we put a bunch of gunk in the bottom to try to seal it, but it was not "The Ritz" that we worked with. We did a lot with very little, and I think every research group that was trying to play with ecosystems did that. The only big ticket item that I saw, and I don't know whether IBP paid for the whole thing or not, was Leo Fritchen's weighing lysimeter that you can have a whole tree in. You can measure what was happening in this whole tree, in terms of water balance and nutrient balances. That was the only big ticket item that we saw, and the rest of it was scrapin' and trying to put together a chem lab, trying to put together weather stations, gage stations, and upgrade those. It was always on a shoestring to keep it going. I left before they really got the great accommodations up at Mack Creek with the little cabin up there, and in the '80's [early 1990s] when they got the nice new buildings in the Andrews complex there now. It's really a lovely facility. But back then, we had a trailer out at Rainbow, and maybe two trailers out there [HJA]. One of them was really falling apart, the other one was pretty comfortable. Everyone was expected to get in there and share. Dennis Harr, when he was up looking at hydrology in Watershed 10, would bring his 12-string guitar and practice that in '72 and '73. So, there was a lot of informal sharing. It was a smaller group

than it is now. It's much more wide-flung than it was back then. It was cozy. You'd smell a lot of wet socks (Chuckle).

Geier: At that time, how would you characterize the ratio of staff to post-docs or others?

Sedell: There were really only two-post docs, myself and Frank Triska. It was later in the game, probably '75 or so that we started to get technicians like Barbara Buckley and Lynn Roberts. In the early days, it was run by graduate students; Ed Grafius and Dick Aho on the fish, and Stan Gregory. But Stan Gregory very quickly operated a much higher level. He was never in my opinion an ordinary (chuckle) graduate student. Aside from being funny and mouthy, he had a real sense of corporate responsibility, and I mean that in the sense that he was interested in his team and the dynamics. And I think what the Stream Team here, in fact, what the Andrews is known for nation and international-wide, is real openness, a give-and-take. We didn't hold back on what we were doing and we'd enter into the intellectual dialogue. People would take away ideas and people felt open to express ideas. People like Stuart Fisher and Jack Webster came here and were very pleased with the openness, even when they didn't agree with it. There were these tremendous intellectual discussions and disagreements, everyone drank beer and figured out how we could continue to collaborate. Stan was a huge part of that, so while he was a graduate student, he functioned a much larger level, I thought.

Geier: You said you hadn't really heard of this place before you started working here?

Sedell: No one, other than Charlie Warren's group with Pete Duteroff, and the group out at Berry Creek and Oak Creek. Now, they've done a lot of ecosystem research, and that's what I thought we were going to get into. Well, they weren't involved in it, and then, it became obvious that they were going on one intellectual bent and style, and we were locked into a very different one. And while we were cordial, we needed to go our separate ways and we did, and then established a whole different bent.

Geier: From what you're saying, it sounds that by the time you left to go to Weyerhaeuser, there was a reputation that had built up?

Sedell: Oh yeah, we had an excellent reputation. People would come in from elsewhere when we got the continuum grant. That really put the stream group, other/rather than Charles Warren's group, back onto a map that there was serious stream research going here. And Ken Cummins came on sabbatical, but through the continuum work, we got [Bob] Naiman, Chuck Hawkins, Dale McCullough, and Karen Luchessa. That just added to the robustness, and it was the first major project that deviated from our central core, and yet was germane to expanding the size of streams that we did. But we were still known in that time as not only interacting with the Coniferous Forest Biome, but could start in major new directions, and we did. And the Stream Team grew from a room no bigger than probably 10 x 12, a little office room where five or six of us would gather, in '71, 2, and 3, to a Monday morning meeting that attracted 15 different departments from five or six colleges, and 50 or 60 people, just to hear graduate students grope around and talk about what they were planning to do. That was pretty novel.

We didn't want formal talks, but wanted people to talk about what they thought, and take risks and stimulate the base. That wasn't an extremely successful way to go about doing research and keeping in touch every week, but it really defined what we were about, which was a much more inclusive and interdisciplinary approach that this university had seen, in such a big way.

Geier: So, it started out as just an informal gathering of people and individuals?

Sedell: Yeah, it was just a necessity to figure out how we were, on a shoestring, going to do ecosystem work with part-time people and graduate students, who had their own work to do, as well as help us. That's when we established that everyone had to take part in the core projects. So, graduate students, whether they liked it or not, had to go up there and tend the nets. And the full professors had to go up and tend the nets. So, everyone got involved in it, and that was just part of the spirit, and it continues today. We still interact with Jim and Norm, and I think that's one of the real marks. There hasn't been a person that worked with us or came through here in a major way, that we still wouldn't or still don't collaborate with. And I think that's a testimony to the spirit and the openness of the place.

Geier: You mentioned several times the evolution towards the River Continuum idea. Maybe you could elaborate on that a bit, the origins and the concepts, and how all that got put together.

Sedell: Well, that got started, really in '72, the winter, actually spring of '72, February of '72. It was right after I got here. I said, "Well, we need to bring in some of the big stream people, the stream groups." Robin Vannote's group had filled out the academy in Stroud [Water Research Center in southeastern Pennsylvania], and it was just really starting to crank and make waves. I'd stopped to see him before I came back here to Corvallis. Charlie Warren had ideas of what was going on here at Oregon State, so I asked Professor Warren if he would be willing to put on a workshop, and we would bring people like Ken Mann who'd done all this energetics in England and who's now at Dalhousie. We'd also bring in Ken Mann, Ken Cummins, Wayne Minshall, Robin Vannote and their graduate students. So, they all arrived here in '72. Charles Warren laid out what he thought his ideas of the world were. It took for two-and-a-half days, and it was a courageous thing for Charles to do, just to unload all this in front of all these critics and spectators. What evolved out of that was we were not going to follow the path that Charles took. What we were going to do was work much more with nutrient material balances. What it did do was establish the fact that we were serious, and we started this dialogue back in '72 with Minshall and Cummins and Vannote. We would gather together at ecological society meetings, and pretty soon followed it up with a small grant in 1975 that put ideas together. We gathered at Hickory Corners there at Michigan State's field station, at Cold Lake, and agreed that out of this would come a paper, and that we would write a big proposal. So, we wrote a proposal, and that got funded. Part of the reason it got funded was that Jerry Franklin was the program officer at National Science Foundation. It was a time when there was a huge struggle between people who had done population and small community ecology, and the ecosystem scientists who came up through IBP. The population and community ecologists didn't think it was good science and it was taking out of a common pie, so there's less money available for

them. So, there was just this classic beating up on us. Well, the classic ecologists always rated ecosystem proposals low in part, because they were big, messy proposals.

Jerry took a look at ours, which got an interesting mix of marks, and he said, “We’ve got to take a risk with some new ideas.” Because that was what Jerry felt that his legacy was sizing up to be. And he’d sized up the group and figured that they would come through. So, we got that and he was very instrumental, along with the other brass at National Science Foundation, of getting us started. And once it got started, it ran on its own really well. That started in ’72, and was because we kept in touch and started to talk about the lack of a defining concept for streams, and whether we had anything to offer. Robin then served up his River Continuum idea and we knew we had a winner. Then it was a matter of structuring how we were going to test or describe this concept empirically, at four different sites and in a common place, and it just took off from there. But it was because we were a viable site that were a part of that. And then, we were one of the major energy and intellectual drivers to keep it going, and to get the proposal in the first place.

Geier: So, part of what made it successful was the established climate of interdisciplinary exchange that you were already instituting?

Sedell: Very definitely. We got good encouragement from Dick Waring and Jerry. They knew the biome, the IBP, was winding down, and they wanted to keep a core here. The River Continuum provided enough of a core that would lead us in some new directions, and still feed off what we’d already done. That was the whole point; define a core, and then, grants you get are just spokes off that core. Occasionally, you get a flyer that leads you into another direction, but for the most part, we tried to build on what we’d already done. That way, we had a robust program. At the same time, we tried to take our overhead in the early days, and put it either into new equipment or new pilot studies, like when some of the riparian vegetation was funded out of our overhead, to Alsie Campbell, and that started a whole set of riparian work and papers. Likewise, the wood work got started on a flyer and developed its own grant and proposals.

Geier: So, in this kind of interim period, from the slowing down of the IBP to the startup of the EER, was that in any way beneficial to the Continuum [River] project?

Sedell: Right. There was another step in succession, intake succession, with a new decomposition grant, that Chuck Hawkins, Stan, and Frank Triska were involved in, that really bridged into the EER. Again, the players had all interacted in various things. Like Chuck Hawkins went on to get a Ph.D. with Norm Anderson, but he was very much thinking and working in the whole group in the mid to late ’70’s. Then, he post-doc’ed here a while, as did Cliff Dahm, and suddenly we were getting oceanographers starting to look at dissolved organic carbon in small woodland streams as well as the Columbia Basin. So, people started to take these little streams which formed the core of our information, and then looked all the way to the Columbia River on carbon flow and carbon standing crops. So, we thought, again, that the attitude of allowing and encouraging other scientists to come in was good. Cliff Dahm gave us

an entrée into oceanography. When Mt. St. Helens blew in 1980, I came back here, and many of the same players that worked in the Andrews, were able to join with players that hadn't worked in the Andrews, like John Barros and Marv Lilley, who were working at deep-sea hydrothermal vents, and get them involved in terrestrial issues. It's a tribute to the kinds of post-docs they got. When Ken Cummins got here, some colleagues of his, Amy and Milt Ward came out, and they interacted with Nick Aumen, who was getting his Ph. D. in microbiology at OSU. So, there's just a whole cycle of people that played and worked well together.

Geier: When you think back to 1971 when you first arrived here, did the place strike you at that time as being uniquely interdisciplinary?

Sedell: Not at all. I was drawn initially because of Charles Warren and Jim Hall and Norm Anderson. I knew less about Jim Hall other than he was a friend of Ken Cummins, than I knew about Norm Anderson's work. And I was into caddis flies and Norm was into caddis flies, so I thought, "Ah, great match." And then, Charles Warren was doing all this, at the time, real cutting-edge work, stemming from a water pollution grant. I thought, "Well, this is the right place to go for streams." And I got here and the pull actually was towards forestry, and that's when I just made the choice that I was better off working with Franklin and Waring. They were people that gave you a lot of room to maneuver, and you had to come through, but I could deal with that. You could rant and rave and have intense dialogues with them, and get along just great. They were very encouraging, and they would find extra dollars to help us out. We didn't go to the well that often, but when we did, we got help. That certainly put us over the hump. They were the ones that helped us get a technician for six months to get us started on weirs and litter traps and things like that at Watershed 10, or we'd never have gotten off the board.

Geier: What you had going was a relatively small group at that time. It was not a campus-wide phenomenon was it?

Sedell: No, I didn't think so at all. It grew as success and the intellectual excitement grew. More people started to show up at meetings. It was a decision that Waring and Franklin had made, saying, "Hey, for our money, we're getting more out of a cadre of post-docs than we are out of full professors." Both Dick and Jerry really pounded hard at the University of Washington. And then, Dick Waring really went after the old guard in forestry here at Oregon State. He said, "This just isn't a pork barrel thing, we've got to come through. You can't be isolated intellectually. Use the money to fund students." The post-docs we did have here were actually delivering for them. We were always encouraged to show up at those groups. Bill Denison, when I first came here, had a whole bunch of wonderful and strange people that were working on lichens and mushrooms, and he and George Carroll had a good thing going. I got invited to their brown bag discussions, and they would bring people in and talk about neurological networks, highway networks, tree branch networks, and stream networks. We'd get there, eat our food, and get to know one another. I never did anything with that group other than I still respect them and still interact with them, but they had these informal get-togethers once a month. From playing ping-pong to just brown bags and taking any wild-hair idea and tossing it around, it was really exciting. Bill Denison had wonderful ideas. I had done a

little of that at the University of Pittsburgh in graduate seminars, where electrical engineers and neurologists got together with some of the stream types to talk about networks and patterns. But this was the first time where you knew what you came up with, you could put down on paper and get funded, to check out or actually do something with what you had and get a paper on it.

Geier: So, it was an added component here, and it was funded.

Sedell: Oh yeah, it was just a lot of good intellectual excitement. Once we resolved our issues with the University of Washington, and mostly with my hurt feelings, then the University of Washington and Oregon State aquatics group knew that we were going to stay together. We still do interact with those people a quarter century later. We didn't have to worry about institutional issues. We worried about working with Jerry and Dick, and we kept in touch with Jeff Ritchie, Bob Wissmar and those people. We just didn't worry about the inter-institutional haggling. We could do our job and were encouraged to go do it. And it paid off. I mean, in the end, certainly the aquatics group was one of the stronger groups, as a collective, in the biome.

Geier: There was apparently less of a division between the two universities in the aquatic program than the terrestrial?

Sedell: Not at all. The post-docs purged the major professors out of fisheries at the University of Washington, and that was high risk for those individuals like Wissmar and Ritchie. But, it was a necessary evolution if you were going to take a scarce resource and make it go further. All those people have gone on to nice careers and jobs, and made major contributions in limnology.

Geier: When you say they purged the major professors out, did they take control of the funding?

Sedell: That's right. And again, people like Waring, and people like Gessel and Dale Cole, allowed that to happen, but they would not have succeeded if they hadn't done some major surgery on some of the old guard. The fisheries research institute cut 'em off from the funds, because their work was peripheral and they weren't intellectually engaged.

Geier: I gather you hadn't worked with the Forest Service before you came here and started working with this group. I thought maybe you could talk a little bit about your interactions with the district staff.

Sedell: We didn't interact a whole lot with them. Jerry Franklin and Dick Waring did most of the interactions, although we would inform them when we were going up. But it was a pretty conservative bunch of rangers [Blue River R.D.]. The wonderful relationship they've got with the supervisor, and particularly with Lynn Burditt, the ranger, that they now consider them all corporate partners. In the old days, we could be considered a bunch of "weirdo beardos" out there that were irreverent, sometimes a bit rude, and focused on themselves. They didn't see

what they would gain from it. In fact, they looked at us as more an impediment and something they had to put up with while they were running the forest. We didn't think too much about it. I know towards the middle of the '70's, we had the infamous Lightfoot [timber] sale, which us stream types called the "heavy-foot" sale, because it allowed salvage of wood out of streams, and we were into large wood in streams and doing mapping. They came along with a salvage operation and some road maintenance, and there was a trade-off made. We did not think that was in the best interests of the experiment station or of our stream work in particular. But it happened, and that was just a part of the trade-off that had to go on, or people perceived that had to go on at that time. But, for the most part, it's been a steady evolution of more interactive people.

When Steve Eubanks came, he said, "Okay, you guys say this, this, and this. Well, here's what it looks like to me." We might have tossed some idea out a bit flippantly or it was poorly thought out. He took it and ran it back at us. That was the first time we got a ranger that not only took the science seriously, encouraged it, but kept us honest with it. That was healthy for both sides, and that really continued with Lynn Burditt. The last ten to twelve years have been highly interactive. I've been more on the periphery of the Andrews group these last ten or twelve years. I look at the contrasts, and see where these people have gone in the Forest Service. They're still pretty conservative in terms of getting on with integrating the best of science and trying to take a situation like an experimental forest and say, "How can we make this work for us?" I think that was the attitude of Steve Eubanks and Lynn Burditt during all this transition from the old way of doing forestry [HJA set up to study harvesting methods and impacts on water, soils, erosion, etc.] to a more ecosystem-based approach to try to manage forests for biodiversity, clean water, as well as for trees. I think the group was lucky to get people like Eubanks and Burditt. These are the leaders making things happen within the legal constraints and are driving change with the times, in my opinion.

Geier: So, it's a matter of replacement of personnel, not conversion of people who are there?

Sedell: No, no. I don't know whether Jim Caswell [Blue River RD Ranger before Eubanks] really thought he learned anything from the experiences, other than we were a pain to him a good share of the time. Those old rangers would be worth interviewing.

Geier: I talked to Mike Kerrick, who was supervisor of the Willamette.

Sedell: Well, Kerrick is one of the new guard. He was there when Eubanks was there, but there was Jim Caswell, but the guy before him was.....it'll come to me in a minute, was more into the timber stuff. We had some run-ins with him, and he was always someone who was ready to throw us out, yet they always allowed us to filter samples there in the basement [Blue River Ranger Station]. That's where we did some of our original water sample processing and titration. And I think having Art McKee and Ross Mersereau up there really helped us out. I mean there's no question we wouldn't have survived if Art McKee hadn't been up there to smooth out the rough edges, much less take care of the niceties.

Geier: Of living there close to the people and getting along?

Sedell: You couldn't say enough about the need for that.

Geier: While you were working closely with the group there in the '70's, what was the degree of your involvement with the local community? Did you go down there to stay overnight?

Sedell: We would spend the night there, and it would either be in tents or these trailers. And we would drink beer in the local tavern at that time. It was a split-personality community; hardcore logging with the Forest Service community. We were just moving out of the '60's, and so you had a lot of the flower children and the drug trade was focused in Blue River. And I remember in the mid '70's going into the Blue River Tavern for a beer, and we took over one of these foosball machines. We'd been up there working hard and a couple of us thought we were really pretty good. We were having a good time, and pretty soon a couple of people came up and put a quarter on the game and it was their turn. Mike Murphy and I were playing, and they started to ask us questions, and we got the first two or three points. Then, when they figured out that we were just a bunch of scientists and not narcs, we didn't see the ball again. They just blew us off the table and none of us had a shot at either pool (chuckle) or foosball with these people. Then we'd go in the bathrooms and there were deals being made.

There was a crowd that checked out the scientists pretty close. Occasionally we gave high school talks, and I remember giving a couple of those at McKenzie High School, the McKenzie Eagles. There was a high school right out of Blue River. So, we didn't do a whole bunch with them. We did more with the Eugene crowd, with the natural history museum and University of Oregon, and naturalist groups, Audubon groups out of Eugene, than we did with the real locals. Other than the fact we hired locals, when we had to do bricks and stuff, and that was always interesting. I'm sorry Stan isn't here to help jog my memory because we've had some weird ones, some of them that were just really spaced out, and a couple of them that were really hardcore workers. I mean, they gave you an honest day's work for your pay, but the spaced-ones, they hardly knew, seemed to ask where they were. I think, at that time, there were some thefts of rope and some things like that that we'd left on the Andrews, so we started to gate different roads.

Geier: You started a road closure system?

Sedell: Right. I think that came on in the late '70's, because at that time Bill Denison and George Carroll, had spun-off and gotten their canopy work going. They'd rig up a tree [for climbing], and they'd lose several hundred dollars worth of rope a time or two. And they said, "Wait a second, we can't keep having this go on."

Geier: When you were working up there, was there some sort of a formal check-in system to let people know where you were working?

Sedell: No. In those early '70's, Art McKee would know when we were coming up there. And sometimes we would need help from the Forest Service for some of our people, and he would try to arrange that. We would tell him where we were gonna be. We rarely went out as just a single person. It was always usually two or more people out there.

Geier: Was that for safety reasons?

Sedell: Yeah, pretty much. Those times were really amazing. I was talking with Stan just before he interviewed with you. We did some real firsts in the Department of Fish and Wildlife [OSU]. We were the first to get women on our field crew. At the time, we had to talk to the wives of people there to ask if it was okay. We were told to do this, and we thought, why should we do that? I mean people are out there working and people are responsible for their behavior. So, in a sense, we did a bunch of gender integration. And then, we hired a woman who was had polio, and could not move very well. And we figured a way to use her in the field too. We would go out and take her up into the woods, and she would be the recorder. We would go bring the bricks with these leaf packs on them, or sticks that had been marked to Barbara Buckley, and she would record them and get them in bags, and generally take care of them. Even though she wasn't very mobile, she was an essential part of getting the samples collected. When we got back to the lab, she did a lot of the grinding and getting things ready for the chem lab, and looking after our people and keeping track of the different projects. She was kind of our first lead technician.

Geier: You started to make a move towards hiring more technicians and fewer graduate students.

Sedell: Yeah, that was probably, that was the mid '70's. We were getting to the place where we needed full-time help and just couldn't rely on graduate student help. They were doing their own things, and it was important to the group, but we needed a core of full-time workers.

Geier: Did that involve a different source of funding or anything?

Sedell: No, we basically got increases. Like we always told Jerry we were the best thing he had going. He and Dick Waring were always grateful, and they gave us the boost. Now they might have a little different version of it, if they were here, but they took care of us pretty well. And they could see that, if we were going to come through, we needed the full-time help.

Geier: Yes, Jim Hall kept raising that in the meeting the other day. They just looked upon each other as the best thing they had going.

Sedell: (chuckle) Yeah.

Geier: What was your personal transition from there?

End of Side A, Tape 1 (of 1)

Beginning of Side B, Tape 1 (of 1)

Sedell: Back in '77, being a long-term post-doc wasn't going to be a viable future. I had a growing family when we were being paid ten or eleven thousand a year, and we were all on soft money. If we didn't get our own money, then our salary didn't get paid. At that time, we'd built up a number of grad students, a number of technicians, and a couple of us post-docs, and I was in charge of getting money for them all. Well, that was getting old, and there was no guarantee the university was going to back us up. So, I had a colleague, Pete Bisson, at Weyerhaeuser say, "Hey, we've got a research manager job open at Weyerhaeuser, would you apply?" I did, and interviewed with them, and they offered me three times the salary I was getting at Oregon State, so I took that. And at the time, there were some difficulties with my marriage, and we decided we would just leave Corvallis. When I got to Weyerhaeuser, I stayed with the group two ways. Dick Waring was extremely helpful, and he said, "Now, really Jim, you've got three years at Weyerhaeuser before you cease to be a viable research scientist." He said, "Think hard about what's going to happen in those three years." And Jerry also had the same advice. But Jerry, in his wonderful style, would always have these "pulse" studies in which he gathered a large group together for a week or two of field work in a beautiful, wild place. We had a big Weyerhaeuser group that went up on a pulse study to the Hoh River [west side of the Olympic Peninsula] where we looked around for a couple weeks on the Hoh River.

And that led to a number of research projects that got funded, and a number of theses and papers. At the same time, Dick Waring and Jerry helped get me on National Science Foundation panels for reviewing grant proposals. Well, that kept me into the mainstream of science. I mean they'd recommend me to Tom Callahan and to John Brooks at the National Science Foundation, and I reviewed proposals for National Science Foundation from '78 to '80. That really kept me in the game. It became apparent after a couple years, that Weyerhaeuser was not my cup of tea, so I talked to the [PNW] station director. At the time it was Bob Tarrant, and I asked him if he was going to replace a scientist who had gone back up to Alaska, Bill Meehan. He said, "Are you interested?" I said, "Well, I'm always interested." He said, "Well, send me a CV." And he just ordered Bill Meehan to write a position around my CV. I don't think Bill Meehan was too thrilled with it. Then, between Jerry and Tarrant, we negotiated a start-time and a salary. But Meehan didn't tell me when the position was open, I just had a premonition I ought to call him and ask him what's happening to this position. I had five days to go before the whole thing closed and I didn't even know it. So, I got my application in, I got the job, and I came here.

Two things happened. One, it became obvious that the future in Forest Service research was with anadromous fish, and we didn't have those at the Andrews. It also became obvious to me that my power-base, intellectually and friendship-wise, was with the Andrews group. And about that time, Mount St. Helens blew. So, I was back ridin' in there with my old buddies. Stan was over his head with Andrews' work, so he got involved with some of the St. Helens work, but really Fred Swanson, Jerry Franklin and I, got full-bore into St. Helens work, and I think still, the chance of a lifetime. To take some of what we'd learned on the Andrews, and think, "Well, here's the most colossal event we've seen in our careers. What's the recovery,

what are some of the processes we'd look for?" Then I was able to draw on Cliff Dahm, bring the oceanographers in, Milt and Amy Ward, Gary Lamberti was post-doc-ing with Stan at the time, and Al Steinman. A whole bunch of the Andrews group also did work up on the mountain. So that kept that base going, but by-and-large, I've stuck more onto the anadromous fish side of things, and Stan and crew have continued the core of the Andrews. Although I haven't been directly involved in much of their discussions, I'm a huge supporter. Through a position here, I've funded studies up there through Stan and Fred Swanson. At the time, Fred Swanson had a different assistant station director than I did, so we would go for year-end money for joint projects on the Andrews. The collaboration and respect has always been there, it's just that I wasn't as actively involved in it as I used to be. I follow it closely, I keep up on it through Stan and Fred Swanson, but really the salmon issues drew me away from the Andrews. Other than when St. Helens blew, there was a period of time through 1987 where that same crowd in the Andrews was working real intensely on recovery of Mount St. Helens, too.

Geier: So, if I'm following you correctly, Fred Swanson is the one that kind of pulled you back into that?

Sedell: Yeah, well, Fred and Jerry both. Jerry's been one of the finest mentors I've ever had, and is a close friend today. Fred Swanson was at the University of Oregon, I think in '74 or '75, and we would run into each other, and I had him over to dinner and to talk to our stream ecology class. I think our friendship, as well as the excitement going on the Andrews, was pivotal in him coming here to OSU on a post-doc, and basically getting on with the Forest Service. Those are ties that aren't going to go away. I feel like all of this group of people looks after one another, whether still working with them or not. I think that's a sense of research community that doesn't go away; in my case, it's real important to me. People like Art McKee, Stan, Fred Swanson, Jerry, always invited me and consider me part of the team, so I've never felt shunned. I felt embarrassed I wasn't able to do more work with them, but I've felt like the future of our Forest Service research here was going to be with the anadromous fish. With the exception of using that tremendous intellectual capacity when the mountain blew. I think the response that the ecological community had, and particularly we on the water, was just amazing. Bringing in oceanographers to look at chemotrophs and all those weird forms that were there the first couple of years, compared to what they were normally seeing in their streams here.

Geier: So, the way you characterize it is that sort of energy after the explosion of Mount St. Helens was a derivative of this pre-existing connection you had?

Sedell: Yeah, you had a cadre of people. They were busy people already trying to come through with this, but when that thing blew, we were able to entice them and encourage them, and that encouragement came from Fred Swanson and Jerry Franklin. And like I say, while Stan was not able to get into it big-time, but we were able to get a lot of his post-docs involved in it, like Cliff Dahm and Milt and Amy Ward.

Geier: I wonder if you could talk a little bit about how your understanding of stream biology evolved from the time you started working at the Andrews through to the present, and particularly, about the intellectual models that you followed or tried to follow?

Sedell: I did my early work with trophic feeding behavior and did mostly descriptive work on two species, then did some experiments on what they ate, and so that flow always attracted me. I'm an empirical descriptive ecologist. I'm not a good experimentalist. Dick Waring's a good experimentalist. I'm not. So, you'd try to gather people around that were more experimental than I was. What it really boiled down to was, it became real important that we solidify the contribution that forests made to streams. Everyone waved an arm at it, but nobody, other than Stuart Fisher in Likens' group, had really formalized the connection. I think Ken Mann in England, did on the River Thames. But there was no data on energetics and material flows and balances in streams or the contributions of forests. The decision to go in that direction was a good one, because that panned out, and we could show clearly the domination of the forests on smaller streams, and then the emergence, trophically, of different contributions of algae and moss on larger rivers. That had been observed, the Europeans had seen that a lot, but they hadn't really formalized it in an ecosystem context. I think that's what we did. It wasn't that we were breaking total new ground. I think that we were riding the shoulders of Stuart Fisher and Gene Likens.

I think the "river continuum" came off of that. We really checked how robust the interaction and the connectedness of the forest was, and that stimulated a whole new line of thinking in stream ecology. We had a defining concept for ecologists and ecosystem types to play with. And they did that. Another thing that stemmed off this was, when everyone was into leaf breakdown and leaf packs, they were using leaves as a great energetic driver, which they are in small streams. Everyone tried to test the continuum ideas we put together, which were really based a lot on context from geomorphology, that were poorly-tested and just based on notions of minimizing variance and maximizing efficiency, and more on the thermodynamics of streams. Well, that never really got tested. What really got tested was the contribution of forests to small streams, and whether there was a succession as you went to larger streams from leaf-dominated organisms and processing to more algal-dominated large rivers with high light availability. That concept has been amazingly successful. Not only as a straw-person, as a straw argument, for people to bang away at. It's got modified and reworked. When you go to the classroom and back, as long as water is going to run downhill, you're going to come back to some sort of continuum and succession or graduation of different energy-processing. That was real exciting and stimulating.

The other part was the logs. We used to not think about logs in streams as anything until we did organic matter and energy budgets, and found out all this wood was the total dominant. While the energetic driver was these leaves, they were a minor-league part of the organic story. So, even a very small fraction of processing of wood was a huge contribution to an energy budget on a stream. That got us looking into structural roles of wood, which is still going on today. Large wood in streams turned out to be important, instead of something that we cursed when we tripped over it as we went to gather leaf packs or do our sampling. Now, we looked at

it as something really unique and worthy of study in itself, and that included connections with the forest. We were told by many of the mainstream scientists, “Oh, it’s just an artifact of you being in the West that you’ve got wood in streams.” We might have the only big wood left in the streams, but it was no artifact. That sent me back to the archives, and I started to look at streams all over North America, via the Army Corps of Engineers snag records, how they pulled wood out of streams. I found the story of wood-dominated small streams was the same in West Virginia and Appalachia as it was in Minnesota and Michigan, as much as it was in the Gulf Coast of Mississippi or in St. Johns River in Florida. Anywhere I looked, even the Wabash River of Indiana, if there was a forest, it influenced the stream, not only in the leaf litter and the kind of insect community, but the wood. We usually took the big wood out because it took out bridges or plugged culverts.

The work here at the Andrews basically got people worldwide starting to look at large wood in streams. Not only as an essential part of maintaining habitat structure and biodiversity options, but as that intimate link between forest and stream. This allowed for the slowing downstream transport of leaf litter as it was trapped behind these debris jams, so it could persist long enough to get processed and decomposed. You then work through the total stream community all the way up to the fish. I think all those things interacted and worked together. Then we got into Fred Swanson’s thing. After you get a picture of a static budget, you find a major component like logs, you also find a switch in energy resources, and you get to a larger stream. Then you start to look and see what all these landslides are doing. What are all these different geomorphic processes? I think that’s where Fred really started to kick in big-time, was in the mid ‘80’s and the ‘90’s, in that suddenly this dynamic landscape that we had in the West, was important not only forests, but also worked within the river network. It was a disturbance mechanism, but an essential one for providing long-term fish habitat, and setting up situations which allowed more wood to come in than just what was delivered from the adjacent stand. The ideas just kept evolving off of what was a simple budget exercise, drawing up an accounting of carbon or nitrogen. That led to a whole series of questions about the dynamics of wood, not only it’s role, but what is the role of landslides or wind or fire in getting wood into streams. Those ideas found their way into the way we manage streams to maintain process and structure in forests worldwide. So, we had an intellectual component, but we also had a message and a story to give back to the managers, which is being used today.

Geier: I want to get back to that in a second. I wanted to ask clarification on one point. You mentioned that someone had raised the point about the debris of wood in the streams as an artifact of being of the West. Who raised that, or what was the context?

Sedell: At the time, the people back East felt we’d see wood here, but we probably wouldn’t see it anywhere else. I don’t know whether they thought that it was all gone because of humans. I remember Gene Likens and some of the East coast crowd, would say, “We’ve got wood, but it’s more leg size or arm size than really big wood.” They had never considered the trunks and the branches as a big part of their systems. Well, Likens immediately saw that it couldn’t be missed in his forest, either. He and Bob Bilby did a lot of very good work with wood. But, we were told that it was just a western stream phenomena. But our own work here

and then the archival work we did, showed very clearly that worldwide wood was a major component of streams and rivers.

For the Mississippi River, Mark Twain wrote books about it, and about the snags. They made double-hulled boats in the 1830's to the '60's, so if the first one got punctured, the boat didn't sink, because it had a second hull. That was a concern because you had huge amounts of wood in big rivers. The Mississippi is our biggest river, and yet, between St. Louis and New Orleans, in a 50-60 year period the Corps pulled out a million snags. Well, those aren't just sitting there waiting to bag a boat. You had all sorts of biological organisms working off of those wood surfaces, or the fish using that habitat or the structures functioning as a sorting mechanism for fine organic material. A number of people then, folks like Art Binky and Bruce Wallace in Georgia and then again Jack Webster and some of the others at Coweeta and Virginia Tech, started to pick up and work on this idea too. But, the Europeans never believed it at all. You could go back into their archives and find out that as long as rivers were highways, which they were, they had to be clean. Theirs were just cleaned in the 14th century to get masts for war machines in the form of naval boats and naval ships down the rivers. So, when you went back there, they wouldn't see much wood, but where they left a forest, there you would still see it.

Geier: I'm still puzzled about the comment, "Artifact of the West."

Sedell: Well, it was because no one else had these great big pristine forests left. They've all been fragmented, and particularly in the East coast, they've gone through three or four rotations or converted to farms from forests. So, they just thought that this was something that would just be germane to the forests of the Northwest, the big old-growth forests here.

Geier: The image I had was they had this vision of really sloppy logging practices in the West.

Sedell: They did, and there was no question that people were outraged when they saw pictures of the West, showing all the slash in streams. In fact, we got into the big wood story primarily off of that. There was a study that Hank Froehlich was doing with a Swiss student. And the issue was, we're being asked to clean up these streams, because of all the logging slash. What if we felled trees away from streams and then we did a "before-and-after" look at the amount of wood in the stream? I saw that and I thought, "Bingo! They're looking at it to cut costs and to up the amount of fiber delivered to the mill." They figured if they felled uphill, they wouldn't shatter the tree, and they'd get more valuable logs out of the deal. They were looking at the economics of it. I was looking at it from an ecological perspective, so, and I said, "Well, these people know how to measure this stuff, let's just use their sampling techniques and measure wood, because we're interested in grams and kilograms of carbon for organic matter budgeting." Well then, that's been brushed away, and Fred Swanson came in and said, "Hey, these are big structural components also." We were more looking at them as tons of carbon and a way to measure them, and it was really the impetus that Hank Froehlich and a couple of his students that really got us into measuring wood systematically and in a big way. And then considering its role in the ecosystem, in terms of storage of organics or habitat or surfaces for other kinds of organisms.

Geier: Hank Froehlich, you said?

Sedell: Hank Froehlich, yeah. He was never part of the Andrews group. We'd always argue about logging and sediment and things like that. But I would give him a lot of credit for us getting into the big wood game.

Geier: I wanted to ask you if you could give me kind of a long-term perspective from your situation of the changes in structure and focus in areas of the Andrews group and its leadership, since 1971 when you first got here.

Sedell: Before then, there was no question up until probably close to 1980, it was a two-person show, Jerry and Dick Waring. They both did very different kinds of things, but they functioned very well as a team. And then Dick had his own interests, and Jerry did too, but Jerry's been the spiritual father keeping the Andrews going, and that's his thing. He really passed that torch onto Fred Swanson, as nobody wanted to pick it up after Jerry, just because, well, Jerry is hard to replace. But, Fred stepped up, and Fred's passion is keeping the Andrews going. What I see now is that we have got to take some of our mid-career scientists, many of them on soft money, and find institutional homes for them. Either at Oregon State or within PNW, or together, to pass the torch. Fred's burning out, just like Jerry did, and you need someone else with a little fire in the belly and commitment. It's a labor of love to keep that going because, the headaches of facilities, the headaches of personnel; they take a toll. And always coming up with a new intellectual idea. I think that the succession's been pretty good. Fred's done a superb job, but Jerry hasn't gone away, and when they need to pull in Jerry, they do, and Jerry has stayed involved in the LTER network. He's looked after the group from a different vantage point, and he's not uninterested, he's just not intimately engaged in the day-to-day or in getting the new grant prepared and things like that. But he's committed to the place, both intellectually and that it's just a passion. It's a huge legacy. I think his research natural areas work and the work he did at the Andrews, that'll be the long-lasting legacy, in my opinion, of Jerry Franklin. And the work he did with describing forests, and what his students have done; it's been superb.

And Fred Swanson will do the same. He's kept the group together, and there is a cadre that wants to stay together that just does not want to leave it. That group now is a position where they're going to have to make a transition. Fred is tired, he needs a break, and some new people have to step up. I think the institutions of Oregon State [University] and the Pacific Northwest Research Station, if in fact we want this to go forward, have got an obligation to get those people on a harder-money base, so that they can devote themselves full-time, not to securing their own funding, but they can devote their time to facilitating the group and taking care of those details that Fred and Jerry did all these years. So, it's changed. It's much more diverse group than it was. It was a little family back in the '70's, looked over by the teutonic "captain midnight" Dick Waring, and this effervescent arm-waver and synthesizer in Jerry Franklin. I mean, Jerry just totally amazed me the first few years that he could sit down like you and I are doing now, and say, "Well, tell me what you've learned. What to do you think. What's

your story?" You'd reel off a storyline, he'd take some notes, and then he'd write it. He was a very good writer and he knew how to get right to the "splash." So, he was very good at marketing the Andrews as well as keeping and attracting people, and Dick, who may not be as good a marketer, but had major contributions in certain areas. I think the Andrews has got to keep up that marketing, if it's going to survive in the long term. I think the mid-career people are ready to give it a go, and I think the institutions now have to step up and help it out.

Geier: The way you characterize this is that this is a group that attracts people, and once they leave they don't ever really leave.

Sedell: I don't think so. If you got to a meeting of the North American Benthological Society or ESA [Ecological Society of America], we'd talk about old times as if it never quit. Many of us still interact professionally together on grants or papers, or in my position now, I'll contract with people like Bob Wissmar, Stan Gregory or Chuck Hawkins at Utah State. They've gone their different ways, and yet you come back, and the feelings are all very, very positive. I just could not imagine a better launch to my career than to have come here, and I didn't know it at the time. To have people like Jerry and Dick look after me when I when I left Oregon State and went to Weyerhaeuser, and to look after me when I came back! I mean you can't ask for more than that. They just did that, and that was because there was a sense of commitment to one another.

Geier: Several people have used this term, the Georgia Mafia, to describe the way that people go to Georgia [University of Georgia], and then go from there to get leadership positions. Do you see things like that happening with this group?

Sedell: Yeah, I think that the group has put out a lot of people. But this here is the place that people send people to learn how to do interdisciplinary work together. Because, while the Georgia Mafia is very, very good, I think that the Andrews group has done an incredible job also. If people in stream research want to see how to interact, they came here. Because people were open, there was a feeling of excitement and a lot of things happening. I'm not putting Georgia down at all, they do put out good leaders, and I think this place has put out, I think people have through here have also been highly successful, wherever they've gone. Some are now leaders in international limnological circles. Like Amy Ward, Ken Cummins still feels a connectedness to the Andrews and Oregon State. And Charles Hawkins at Utah State. All of them have carved out very good careers. Also Cliff Dahm at University of New Mexico.

Geier: So the problem isn't that leaders aren't available here, it's that the funding is lacking?

Sedell: Well, I think each primary institution here has to have someone that says, "Hey, there's a commitment to keep this thing going." And right now, in the early stages that was shared, and then it got more shared as the post-docs got empowered so the workload gets divvied up. Jerry could focus on what he did best, and Dick could too, and we all fed into that. Jerry had the primary responsibility in the original LTERs, and then passed that on to Fred. Well, it's more than a one person show now, it's too big. You've got to have insiders work to move the program forward. And they can still attract the Georgia Mafia, the Hubbard Brooks, and other

places. That's what the LTER system is doing. It's a lot of collaboration between the different groups in the LTER network. But that's going to take more than just one person. And I think people like Mark Harmon or Barbara [Bond] Yoder are the type of people that Oregon State and Pacific Northwest Research Station ought to make offers to as full faculty members, and they assume the leadership into the next decade. Will Fred Swanson go away? Not on your life, and Jerry Franklin too. And so, they can still rely on those two, but I think it's time for the institutions to step up. The institution says, "This has been good to us," both, for the Forest Service and for Oregon State, and I think they're going to have to step up, and put some money into that.

Geier: What is your perception as a scientist of who your primary audience is, and how that relates to the kind of science that you do?

Sedell: The public. I've been criticized for that. I'm a marketer. I like to take the ideas that we've come up with, put them in real simple form, and give them with a punch line. And so that if it's the governor, if it's the Garden Club, or the Rotary Club or the Kiwanis Club, or some member of the Ecological Society, they understand what we're doing. In a sense, that takes the mystery out of it, and shows that, in fact, we are relevant. We're just not bunch of people that go off and talk about chaos theory to our colleagues. I'm not particularly good about chaos theory, even though my office is chaos (chuckle). So, I look more at translation and seeing how do we make the work relevant. How do we distill it down to the good ideas? I learned that from Jerry Franklin. Jerry is very good about distilling a story down to an essence, or getting some defining statement or experiment that has some splash that you can rally around. I think that has served the Andrews well and it serves science well because it has allowed more people access to say, "This place is relevant to us." Then I look at translating a lot of the work, particularly what the Andrews has done, to managers. How get do we get that thinking into how to manage forests? A good share of my work the last ten, fifteen years, has been, in fact, adding to some new knowledge, but as much translating to managers and saying, "This is real, you've got to manage to maintain this or restore it or preserve it." So, that's where I come from.

Geier: Who are some of the managers you've worked with most closely with the last few years?

Sedell: I've been really fortunate. It started out working with district rangers of national forests. But, in the last five or six years it's been the chief of the Forest Service, director of BLM, regional foresters, state directors, governors.

Geier: So, national level?

Sedell: It's at the national level. Stan Gregory is on sabbatical now working on wood in streams. And working with Europeans who are trying to get a six or seven country proposal, on the role of wood in woodland streams in Europe. They're saying, "Here's all this North American stuff, does it really work here? Given our culture and what we've got left?" And, there's no doubt in my mind it's going to work. Change some of those Napoleonic laws that require all streams be

cleaned of wood. I feel like this place – the Andrews – has given me a platform to speak to scientists at the highest levels of NSF and in science forums. And it's also given me a forum to speak very authoritatively in the huge debates over how we're going to manage natural resources.

Geier: I think, you'll relate to this theme I would like you to address. I've asked this question of most people I've interviewed, to get a sense of how this concept has changed over time. Maybe you could tell me how you would, at this point in your career, differentiate the relative priorities between applied research and basic science, and how would you characterize the distinction?

Sedell: Well, I think you need to mix them. The thing about the Andrews was, you had some people doing some pretty esoteric work, and they were all part of the group. And you had people that weren't particularly inclined to talk to managers, and now I've found in the last several years, they're out there talking to managers. These distinctions are really breaking down. I think, particularly in ecosystem science and forests and streams, that, as soon as we learn some new things about the ecosystem basic-wise, they're almost always immediately relevant on the applied side. And then, it's a matter of well how far can we extend this? How far can we go? And in some cases, it seems so intuitively obvious that we have to go forward and overplay the hand, and the science catches up, we're shoring it up. In some places, the management is so hungry for any information, that our information is on a time trajectory and they want to take this slice and they try to modify strains or forests, and it's just the wrong thing to do, because we didn't get the whole picture. I really think that the basic vs. applied distinctions are blurring. And there's no question that technologically, we're entering into much more sophisticated math and modeling and remote sensing. Also chemical analysis, and that's telling a story we didn't know before, but that's playing right into the management and the robustness of the story. I don't see a whole lot of difference. I see that Forest Service research is moving more towards applied, and that might be just part of the times, given this Congress and fact we're in a major transition in the way we're managing forests. With incomplete knowledge, people want what we do know translated. Then there'll be a period where we've run out, the Schlitz is gone, we're gonna have to develop some new beer. And I see that the pendulum will swing back to the fundamental side.

Geier: For now it sounds like [end of question covered over by Sedell's response]

Sedell: I think the relevancy to the policy issues is really going to keep funding going. But the National Science Foundation, you can see their new programs emphasizing the sociological impacts of forests and some of the economics. They're trying to get more integrative of the social sciences within the basic sciences. And I say that this place will go the same way. Now you say, "Well, is this basic research or applied research?" I think it's a lot of basic, we don't have the tools and in many ways the concepts, to integrate social, economic and ecosystem processes. It might be that it's always going to be apples and oranges, but everyone's trying to see how they can integrate it better than we've done. And those are going to end up immediately as applied tools through policy makers to weigh risks and opportunities. That's

going to take some basic research in some fields that haven't traditionally been in ecosystem science.

Geier: So, there's relevance to policy issues in funding lines, but it also raises new questions.

Sedell: Oh yeah. I think there's a push-pull, and in many ways in this current "basic vs. applied" debate, is like the old debates of the early '70's we used to have with Dick and Jerry. Jerry's a natural historian and descriptive ecologist. I'm the same way. Well, the experimentalists, if you weren't describing what was going on and giving them an idea of what the world was, very often, the experiments would be a lot less meaningful. So, once you give them the hypothesis or description, they say, "If that's so, then we ought to be able to find this, this and this." They set up the experiment to falsify or see where they lead. I think you need 'em, just like theoretical physics needs people to spend their time blasting gold or platinum with high energy particles to see whether you can get a statistical pattern that fits. So, you need the descriptive parts, you need the theory parts, and you need the experimental parts. I think that at different times, the science community has tried to emphasize one or look down on the other, but in the long run, it's a mix. I see the "applied vs. basic" as the same mix. Right now, I'm more interested in saying, "What do you have that I can apply?" At least, that's where I'm at in my career. But, we fund a lot of basic research.

Geier: So, what you're saying is that dichotomy has always really been there in this group?

Sedell: I think so.

Geier: And it's always been an element of this group. Maybe you could give me your encapsulation of how you would characterize the relative success of the Andrews group to market what it is that they do as a group over time?

Sedell: I think within the science community, they've done a superb job, and within the Forest Service the last ten years, they've established themselves as a relevant player to the Willamette National Forest within the region. I think what we've learned collectively on the Andrews, has been the foundation for some of the huge changes in forest management that have gone on in the West and are rolling East. There's no question that on the collective understanding of streams and forests and geomorphology and wood, they've had a major influence. And the owl, because Eric Forsman originally did his work there in the Andrews, has been a cornerstone for allowing major policy shifts. And that came out of fundamental science, it didn't come out of industry-financed science. In many ways, a lot of what the industry was funding was, "How can we up production, cut costs, or what's the minimum we need, as opposed to say how does the system work, and are we doing the things that keep the system working that way." It's kind of like agriculture with the chemicals. A lot of research in agriculture was around chemicals and what are minimums, what are maximums, and not what it's going to take to keep soil fertile.

That foundation, when people were tired of the clear-cuts and we were running out of the old trees, then became the cornerstone. We could say, "Hey, this is what people think about how

an old growth forest works.” And that all stemmed out of a bunch of us sitting around in 1979 or ‘80, or was it ‘77-’78, at Wind River Experimental Forest, and Jerry Franklin and I were there with about six or eight of us authors that put together this book on the characteristics and function of old-growth forests. That was just synthesizing bits and pieces that we’d all done, to put it into a coherent story. That was the first look at those forests that way [*Ecological Characteristics of Old-growth Douglas-fir Forests*, Franklin, et. al., 1981] I think that little government technical report had more to do with focusing on old-growth forests than anything. Subsequently, they’ve sent more books out, but that was the pivotal book right around 1980, and that all came out of basic research. Then people started to ask, “How do we manage to maintain any of these processes or some of these features, these structural features,” and it certainly influenced the way we managed streams.

Geier: Those were scientists who wrote that synthesis, is that right?

Sedell: Right, and just to summarize what was known about old-growth forests and streams. Jerry put it together in his own wonderful style, and it was highly readable. Managers can pick that up and understand the implications of it.

Geier: I gather from what you’re saying that the Andrews has been critical to this shift in thinking about ecosystem management.

Sedell: No question.

Geier: But, is it recognized outside of the scientific community as being central in that shift?

Sedell: Well, I think the managers here know it is when they stop to think about it. A lot of things in the policy arena are moving fast. And when you start to look at documents like FEMAT [Forest Ecosystem Management Assessment Team] report, which was the foundation of the Northwest Forest Plan, the fundamental citations are out of the Andrews. That’s not saying other people haven’t done things, they have, and that’s fed into it, too. But in terms of a collective, these people have been instrumental. I mean, Jerry Franklin, Stan Gregory, Gordon Grant, Fred Swanson, Tom Spies, Dave Perry, have all been major players in the policy debates.

Geier: I think I’ll let you go with that.

End of Tape 1 (of 1)