

**Interview with Mark Harmon by Max Geier on 1 October 1997, 2:00 p.m.,
in Dr. Harmon's office, Oregon State University / Side A, Tape 1 (of 1)**

After growing up in Massachusetts, attending Amherst College, and doing some work in the Great Smoky Mountain National Park, it was a big step for Mark Harmon to travel west in 1980 to OSU to undertake a PhD with Jerry Franklin and working on wood decomposition. While still a PhD student in 1985 he installed the monumental 200-year log decomposition experiment and led a dozen more senior researchers on publication of a 170-page, now much-cited synthesis of knowledge of dead wood in temperate forest and stream ecosystem. In the context of global change, he subsequently expanded the scope of research, writing, and teaching activities to carbon dynamics and sequestration in forest and human-use systems.

Max Geier: I understand you joined the faculty here and the group [Andrews EF] in 1985. Maybe you could talk a little about your personal background up until that point and how you came to be here.

Mark Harmon: How I came to be here. Well, I was thinking about that the other day, and the first time I actually met people who were from the Andrews, I remember meeting Jim Sedell and Kermit Cromack, I'm not sure who else, but it was at the ESA meeting in Athens, Georgia. That was back in 1976, or maybe '77. They were really hot on coarse woody debris and streams, and working in Mack Creek. Now that I've been to all these places, I can sort of reconstruct what they must have told me. (Chuckles)

Geier: Yeah.

Harmon: But, that was my first inkling of it. I think the first time I met Jerry Franklin was down in Florida. I went down to Florida for a MAB [UNESCO Man and the Biosphere Program] conference with the Soviets. At that time I was just down there to get exposed to it, and see Florida. So, I met Jerry and Art McKee, not really in a formal way or in an interview or anything, but just getting to know these guys. I remember in another conference seeing Art and a whole bunch of fellas, probably Jim Sedell, and Jerry, who talked about the Hoh "pulse" [Hoh River, Olympic National Park], which was done just before. I was starting to think about going to OSU, so, I kind of had an awareness of the group. Even then, they were known for real integrated studies. We would hear crazy things. This was when I was in Tennessee at the University of Tennessee-Knoxville. We would hear things about people climbing trees with ropes, and all kinds of strange things going on out in the Northwest. So, it sounded like a really interesting place. Then I was a guide for Jerry during the fall. I was working in the Great Smokies [National Park]. So I guided him; he came right at the height of flower season, a perfect time for Jerry. We talked about graduate school, and that was the first time I really got the idea that there might be some opportunity to come up. The whole idea of integrated work and also working in a team appealed to me.

Geier: Uh-huh.

Harmon: I read a lot of IBP reports and that was emphasized, but as I was working, it was very hard to find. You know, a lot of people talked it, but they didn't walk it, sort of stuff, and this looked like a place where they really did that, so this was very exciting. At that point, Jerry came up with a project which had spun out of this Hoh "pulse," what they did up on the Olympics -- [Pulse – A gathering of science people in the field for a week-plus of intensive field work and bonding experiences.]

Geier: Uh-huh.

Harmon: -- dealing with nurse logs. At that point, I decided that that would be fun, and he [Jerry Franklin] had funding for me to attend graduate school and get a Ph.D.

Geier: Okay.

Harmon: So, I came out, and that was in December of 1980. I would say over about a three-year period I learned about them, and then sort of suddenly within the year, like '79-'80, I really got a good idea of the project that Jerry had been working on.

Geier: I'd like to go back a little bit when you did undergraduate work at Amherst.

Harmon: Amherst College. I was a biology major. They actually had a natural history major at that time that they were trying to get rid of.

Geier: Okay.

Harmon: I was always interested in ecology, natural history, that kind of thing. They phased it out, so, for a while there I didn't have a major. They actually stuck me in chemistry.

Geier: Oh.

Harmon: I had to struggle to get into biology. And they had some very good people there at the time, but ecology, there was this debate in the faculty about if ecology was a science. If you weren't pre-med, were you really going to be up to snuff? There were I think out of thirty students, there were three biology majors who were not pre-med.

Geier: Uh-Huh.

Harmon: The rest were all going to med school. So, it was a great education, but they were not really into ecology, and pretty much turned out pre-med students.

Geier: What attracted you to go to Amherst? Were you from that area?

Harmon: I'm from Massachusetts originally, about 20 miles west of Boston. I got actual opportunities to go to lots of different places. I was a football player and a pretty good student, so lots of colleges were interested in me. Sort of that or Harvard was the choice, and Harvard was just too big, too remote, even though it was near the city that I lived near. But, it just seemed too big and too remote for the professors. Amherst was small and rural, and you didn't have graduate students teaching, you had professors teaching.

Geier: Hmm.

Harmon: So that was real appealing. And plus, it had a great reputation.

Geier: What attracted you from Amherst then to Tennessee?

Harmon: Well, I worked in Glacier National Park for a fella named Steve Casell. He was an Amherst graduate, working at Cornell. And Steve had this project on vegetation and fires and management of national parks, and I was really interested in that. I was thinking, originally, I would go into that sort of more management aspect.

Geier: In an internship?

Harmon: Well sort of. It was actually a volunteer program.

Geier: Okay.

Harmon: Forty bucks a week, and they supplied the food. So, it was okay. We camped out, so there weren't the expenses. I worked on this crew there during the summer in the west part of Glacier, and then stayed on until, let's see, I think December, doing other things. That was great in terms of staying there, but it wasn't as exciting as the field work. From that I went home and couldn't really find any jobs that I wanted. I had a sense I wanted to work outside, work in forestry, or something like management. Not real forestry in the sense of cutting down trees, but management of a forest area, like a national park. So, I ended up working at a gas station. I ended up working at a paper factory, and then someone called me. Actually, it was a guy I'd met at Glacier, a fellow I worked with on my crew. And he said, "Hey, you know, there's a job down here in the Great Smokies." His name was Mark Huff, by the way. He works in the PNW Station now. He called me up and said, "I'm doing a project myself here in the Smokies, and there's this guy who's doing this fish project, and I know you like to fish and all that." And he said, "Well, it's only like forty bucks a week." So, I thought about it for about two seconds and said, "Fine, I'm getting on a bus and going."

Geier: (Chuckle).

Harmon: I had an alternative to be moved up to inspecting tea bag labels for \$250 a week. I thought I'll just do tromping around in streams for forty. I'm going down to tromp in streams. So, I got on a bus and just rode down there. Endless ride to Knoxville, Tennessee. When I got to the park there, I did that project for the summer and I kind of linked projects. I worked on a wild boar project for a while. I was just sort of figuring, what did I really want to do? What did I want to study outside? It turned out it was plants and ecosystem-related stuff. What I'd worked on at in the Smokies and Glacier dealt with fire and fuels. I realized that there was a whole, this area of ecosystem analysis where you could actually do more than just measure what's there, but kind of predict what's there and why it's there, and it wasn't being applied. I thought, that would be fun to do. That's what kind of got me into ecosystems. So, I had a project in the Smokies. Actually, there were some wildfires and I went out there and looked at them and decided I'll do my masters on that. I completed that work in about three years, I guess.

Geier: Were there any particular faculty members or other people that you worked with at Amherst and Tennessee, who kind of steered you along the way?

Harmon: Yeah, actually it was kind of funny, because I got probably better guidance on that from the people in the Park Service.

Geier: Oh yeah?

Harmon: At Amherst, the two ecologists that were a lot of help were Lincoln Brauer, who I think ended up at Florida, eventually. The other was Stuart Fisher, who'd come from Hubbard Brook. He was an aquatic biologist, a more ecosystem type, and he had a big influence on the way I thought. He moved on, I think, to Arizona. I know he was in the Southwest. He had a lot of really good students come out. So anyways, that was fun. Then, at Tennessee I actually came in with a whole designed project I funded myself. I had my own project and worked that out with Susan Brattan, who was the leader of a thing called the Uplands Field Laboratory, which was in the Great Smokies. This was sort of during a revival of Park Service financing. They were great. They set up a laboratory at Sequoia [NP], and in Florida at Everglades [NP], and Great Smokies [NP], and maybe some other places. But it was sort of a building of their science program.

Geier: Susan was at Tennessee, or with the Park Service?

Harmon: She was at the Park Service. She came out of Cornell and worked on the wild boar and its effect on plants, particularly understory plants, wild flowers and smokies, which is a big attraction of that park. The boars were just decimating them, so she got into this area of research on the boar, and their effects. Plus, she just started a whole expansion of studies on the vegetation, fire, and aquatic studies; all kinds of things started to happen about then. Another big influence was Peter White, who was a Park Service employee and he became the director of the North Carolina Botanical Gardens.

Geier: He was at Tennessee when you were there?

Harmon: Well, he was actually Park Service. What I did was I went in and found someone who was generally interested in the subject. That was Ed Klauch, who has since retired. He was in botany there. Ed was very interested and supportive, and he spent a lot of his time talking to me because every week I'd go in and pester him about some question, not necessarily on my studies, just about ecology.

Geier: Uh-huh.

Harmon: What did he think about this or that, or how could someone think this? They must be idiots. You know, typical, arrogant, young person talk. But, I learned a lot by that. Another big influence was Jerry Olsen, who was at the Oakridge National Laboratory. I read a lot of Jerry's stuff on carbon cycling and decomposition. I went out and talked with him a couple times about how you make a litter bag, which seems kind of silly now, but I talked with him a lot about that. I had courses with him, special courses I set up, reading-and-discussion. I absorbed a lot from Jerry. He's a very

interesting guy. He's sort of other-worldly. I mean, he's the epitome of the absent-minded professor.

Geier: Hmm.

Harmon: I remember once, I hope this isn't too much of a digression, but just to show you; he (Olsen) had a regular class, and we had our reading-and-conference before that. The class started, say, at one o'clock. We go to a restaurant to eat lunch, it's five minutes before one, and he says to the waitress, "we're in a hurry, can you hurry?" So, half past one, after we've stuffed everything down and eaten way too fast, he appears for his class half-hour late, totally unfazed, and just starts talking to his students. He was amazing that way, but he has lots of good insights into ecosystems, carbon-cycling, that stuff.

Geier: You kept contact with him over the years?

Harmon: Not too much, but in terms of an influence, he's always been an influence on me. How to think about a problem; I think that was the key. He gave me a real insight on how to think about the kinds of problems that I'm dealing with now. And he actually set me on the track 20 years ago to do the kind of research I'm doing now, got me interested in the whole idea.

Geier: Your research priorities while you were at Tennessee, what kind of things you were focusing on? I assume you were going to conferences to present your work.

Harmon: Some. We had at that time, still do, I think, a conference for Smokies research in the spring. When I went to Athens, Georgia, I presented some of my work there. Gave a poster session, not a single person showed up. It was really embarrassing. Next to me was Jerry Malaloff, who is a very famous person in decomposition kind of work. People were streaming by, and they were mostly saying hello to Jerry. It was pretty humbling. But that's where I met Kermit Cromack, Jim Sedell, and that group. So I did that. They also had Park Service conferences back then. They had two of them, I think. They had one in New Orleans, and I presented at that, and there was one in San Francisco. They were a couple, three years apart. Those were real good experiences.

Geier: Had you been out to the Northwest before you ran into Jerry at the conference?

Harmon: Yeah, actually I had. When I was in undergraduate school, my brother lived in Nevada and we hitchhiked out with my girlfriend and to help him build his house. So, we went through the Northwest, across Washington, and down through Seattle. We didn't really enjoy Oregon that much because it was so hard hitchhiking through it.

Geier: (Chuckle)

Harmon: So, I hadn't had this great opinion of the state.

Geier: You mean no one would pick you up?

Harmon: Yeah, it was hard to get rides. Actually, we went down 101, and we didn't have time to get out and see anything, we just saw 101. Lincoln City, for example was just like, "Oh my God, what are we doing here?" It's still like that today, but I kinda can

see the other stuff. But at that time, it was all Lincoln City, which seemed endless. Like it took about ten rides to get through Lincoln City. But I spent time in Nevada, and seeing the West. I'd also spent some time in seeing Montana. I went to a geology summer camp in Montana, which was a big influence. I found out I was not going to be a geologist because I just absolutely sucked as a geologist. But, I found myself always looking at the trees. Trying to figure out what a Douglas-fir looked like, which I didn't have a clue, but I knew that somewhere out West there were these Douglas-fir trees.

Geier: Uh-huh.

Harmon: So that was kind of a shaping event in that I realized geology wasn't my forte, but maybe understanding vegetation would be because I was very interested in it.

Geier: So then, you made the decision to come out here, when I guess Jerry offered a graduate assistantship to you? Was that it?

Harmon: Yeah, it was through the Park Service. Let's see. What was his work for? Well, those were back in the days when Jerry could go to agencies and say, "I would like to have this kind of money, and I'll do this project." And they would say, "That sounds good, we'll fund it." Particularly in the Park Service, which didn't have a real rigorous review system of proposals. He had a student, Miles Hemstrom, who was working on a vegetation model. And out of that project he had enough money to bootleg some studies of the nurse log phenomena, which they really were impressed with up at the Hoh rainforest [Olympic National Park]. In fact, they were puzzled the whole time. Why were all the seedlings on logs? You could go out in these big open areas, and there just wouldn't be seedlings on the forest floor or on the ground. So, it became a real puzzle, and so that was what the project was about.

Geier: You mentioned you had some inkling of what was going on at the Andrews before Jerry made the offer. How would you characterize your understanding of the mission of the research group or the Andrews itself, at that point?

Harmon: Well, I knew they had an important role in the Coniferous Forest Biome. I knew they were studying old-growth forests, and finding out things about them and taking some real fresh perspectives, like, if you want to find out what's going on in a tree, why don't you go climb it and see what's going on up in a tree. And the stuff on woody detritus was all very new. Also, they were doing some of these very classic studies at the time; well, they turned out to be classic studies, on nutrient-cycling with Phil Sollins. Phil had been at University of Tennessee, so I had read his thesis on carbon-cycling. Chuck Grier was talking about production ecology and biomass of forests, so I was kind of aware of that. I'd read a lot at Oak Ridge which had the Eastern Deciduous Biome [IBP site in Eastern Deciduous Biome category]. I think it was one of the places. I'd just read tons of stuff from Oak Ridge about what they were doing. And part of that was because of Jerry Olsen, but just because I knew there were some really good people there, so I wanted to find out what they were doing.

Geier: Hmm.

Harmon: So, I had a good sense they were doing very solid ecosystem work in the old-growth forests. The connection to management didn't strike me at the time. Some other things I just didn't know. Not at all. The basic history before that, oblivious.

Geier: You came in when the LTER was in a transition period?

Harmon: Yeah.

Geier: How well were you aware of the development of that program at that point?

Harmon: Well, I knew Jerry was involved, and one of the things I remember asking him was that maybe he and Dick Waring should run a course on how you run this thing. And Jerry kinda looked at me like (chuckles), "Well, I don't know how I run it either so I can't tell anyone." But, I was impressed as a student that anyone could manage something so ambitious. Jerry took us up there on some tours at the Andrews and that were really fun. I met Art McKee. I know I'd seen him before at these meetings, but I met him on one of the tours in the winter, and he was the first person to really welcome my wife and I to Oregon. It was a very strange scene when we first arrived. Because mostly, I interacted with Jerry's students, and they were, I couldn't put my finger on it. At Tennessee we had a very small group, but friendly. We kind of were all outsiders in town. Here, it seemed people were more like us, but they weren't as friendly. They probably had other interests and stuff. So, I'll never forget that Art was so supportive and interested in us being here. We were kind of, after that field trip, like, gee, we're amazed. Other than that, I really wasn't too involved in the LTER. There were these pulses that Jerry had. There was one in Sequoia, and that was part of the LTER, but I gather there were some disagreements about that. So, we went on that, but it was sort of a part-LTER thing. And it was sort of extending the findings to other regions I guess was the idea. Then there was the work at Mount St. Helens. So, I guess what I'm saying is, I interacted with LTER scientists quite a bit, but not really in an LTER sense.

Geier: Yeah, okay.

Harmon: I mean, I knew they were there. I knew who they were. Sometimes we'd go on field trips together, but generally I was not part of that program.

Geier: The people you worked most closely at that time, were primarily Jerry and other graduate students?

Harmon: Jerry, other graduate students, and the Park Service, because I was working up at the Hoh, and I worked a lot with people, mainly arranging logistics.

Geier: Uh-huh.

Harmon: One job I took on, for example, was measuring the elk exclosures they put up on the south fork of the Hoh. One thing they were impressed with in the pulse, was the lack of vegetation on the forest floor. Elk were known to eat it, deer too. They put up a big exclosure, and I ended up with the data. It's sitting in here, unpublished. I've taken that on since 1980. I worked at Cascade Head as well, which had a similar situation to the Hoh, vegetation-wise. So, I worked a lot with Jack Booth. He showed me where things were, helped me make gizmos, and set up experiments, that kind of stuff.

Geier: You already mentioned how the social environment was when you came to OSU. What were your perceptions at that time or how could you characterize OSU in relation to other universities you'd been at up until then, in terms of faculty interaction among the different disciplines and such?

Harmon: It's kind of hard to say. I feel that my situation was a bit unusual. For example, I was in Botany and Plant Pathology, and that was my major, but my office was over here [OSU College of Forestry] because I worked with Jerry. Jerry was an adjunct professor there, but, actually, Jerry worked a lot more with the forest science people. So, in a very odd way, the Forest Science Department kind of adopted me. I mean, I would go over to botany for official stuff, and I had a mailbox over there. I took courses in the department, but this was really kind of my home. If I had to order something, I wouldn't go over to botany, I'd go to John Gordon, the department head of Forest Science. When I gave my thesis presentation defense, Logan Norris showed up [Logan was Head of Forest Science and a supporter of the Andrews Forest]. But my department chair over in botany probably wasn't even aware I was defending, so I had a very weird relationship. I tied in more closely with the Forest Science faculty than the botany faculty.

Geier: Okay.

Harmon: I had Dick Waring on my committee, and he was in Forest Science, and Kermit Cromack. Jerry was in Botany, but actually, he was mostly working with Forest Science. The only people in botany I had were Mark Wilson and Everett Hansen. They were real helpful, but somehow, I was a forest science student trapped in the Botany Department. It's hard for me to comment on that, because it was an odd situation right from the get-go.

Geier: It sounds like what you're saying is that there wasn't a lot of connection that you felt with the botany program, other graduate students in that program.

Harmon: No, not really. What was happening then, was I probably should have enrolled in Forest Science. But the dean and the department had very rigorous demands on what you knew in forestry as background. Basically, you had to have an undergraduate degree in forest management. It was a matter of did I want to take a year's worth of undergraduate courses, or just enroll in botany and get on with things. So, that's what I did. Until recently, our department had those restrictions, and we got rid of them about three years ago and it's really helped. We now take really strong environmental science background students of any type, and they don't have to be forest managers to get in, which is the way it used to be.

Geier: About the time you came here, I think it was when the Forest Science faculty moved into this building. Were you able to?

Harmon: Yeah. I was here, and they were down at the FRL [Forest Research Lab-OSU], so they were split at that time, if I remember right. Some were here, a lot were down at the FRL. In the early '80s, thanks to Forest Service downsizing, a lot of office space opened up, so, a lot of people came up here. That helped tremendously, but again, the

people I saw a lot, were forest science professors. They were doing the kind of work I was interested in, like Dick Waring's work and Phil Sollins' work. That was what I was interested in, more than straight community-analysis. That's what botany was good at.

Geier: You say that the move over here helped tremendously?

Harmon: When the Forest Science faculty came up here it was much better. Then there was more interaction with the faculty.

Geier: Oh, okay. You mean between graduate students?

Harmon: Yeah, that was great, and also better integration with the Forest Service.

Geier: Okay.

Harmon: I mean, physically, the way they did it. We didn't get floors. We got offices.

Geier: Mixed in?

Harmon: Always mixed in.

Geier: I was going to ask. You worked a lot with the National Park Service, and it sounds like the faculty in Tennessee [University] were pretty close with the Park Service. I was wondering if you could characterize the interaction among Forest Service and OSU scientists in comparison with that [interactions] in Tennessee with the Park Service.

Harmon: I think it's much, much tighter between the Forest Service and the academic scientists, than the Park Service and academic scientists. That eventually convinced me not to work for the Park Service, because that was my goal. There was a real good idea going at the Smokies at the time. Let's have our management based on science and not just guess things, which had been going on. Or ignore things, there aren't problems, which they were doing. So, it became very apparent through the years, the Park Service was actually more applied than the Forest Service. The Forest Service research program was big enough that there were applied people, but there was basic research going on which eventually had applications. But they were not obsessed with applications, and the Park Service tended to be very oriented towards putting out brush fires.

I'll give you an example, and I hope this isn't too unfair. I'll use this example from the Smokies. The wild boar were a major problem, but they spent more time studying about the wild boar, which they could almost do nothing about, than studying the vegetation they were damaging. In other words, they weren't documenting what the vegetation was like at all. In some cases, it was lost through the boar. So, they just couldn't see the big picture. We could never, for example, sell them on the idea that they should have a map of the vegetation. For the Smokies, that's important because it's one of the most important parks in terms of vegetation in the U.S. It's known for its vegetation. But we had to kind of bootleg every vegetation study we did. It was really something. Whereas, that kind of thing was going on regularly in the National Forest Service, where if you need to know the vegetation types to monitor or manage it, let's get a map of the vegetation. It was too much of a reach for the Park Service. I would say the connection of the Forest Service to the academics was much, much stronger than the Park Service.

Geier: Had you had much interaction with Forest Service before?

Harmon: No. It was very ironic, as I would have envisioned that the Forest Service would be all into just application. How do we cut trees better and plant them better? But there were other things, too, and it was very impressive.

Geier: Maybe we could shift the focus a little bit here. At the time you started working with Jerry, I'm assuming your work was focused, some of it at the Andrews, in that period. Is that right?

Harmon: For my Ph.D. work?

Geier: No?

Harmon: No, actually it wasn't. It was focused at Cascade Head and the Olympic National Park.

Geier: Yeah, you mentioned that. I thought you might have been down there. When was the first time you started working at the Andrews?

Harmon: It was about '84.

Geier: Okay.

Harmon: I'd taken trips, of course, and been around there. But it was about '84 or maybe midway through '83. But really starting in '84.

Geier: Maybe you could think back and give me your impressions of that experimental forest compared with other places you've been, like Cascade Head or some of the others?

Harmon: Well, it was not as pristine as, say, Olympic National Park. But there was lots of old growth. It was a very interesting place. The facilities were pretty primitive at the time, but they were acceptable. They had trailers, they weren't as bad as what some of the earlier people put up with. So, the way it sort of happened, was I finished my field work for my Ph.D., and an opportunity came to take a post-doc, basically, on wood decomposition. I was interested in that because of the fire/fuel issue in Glacier. And I did some of that work in Smokies. I'd been out with population work on logs with seedlings of trees, and I thought, I really want to go into ecosystem work, so this is a perfect entree. Most of my early time there was spent driving around looking for study sites, trying to get a sense of where things were. So, I remember being impressed with the forests themselves. They were definitely drier than Cascade Head or the Olympics. So, they weren't quite as green; they seemed kind of brown to me. I remember being impressed with some of the areas that had been cut; how much had been cut, particularly on the sort of north-facing part of the Andrews. It was quite an adventure. I'd just get in the car and go around to all these roads and these unlocked gates, and hope I didn't drive down a landslide chute or something, and just look around. I covered most of the Andrews on foot during that period.

Geier: Was there any kind of a check-in system or anything? Someone who kept track of where you were going and what you were doing?

Harmon: Not really. We really didn't have something like that.

Geier: Where did you stay at the time?

Harmon: They had trailers there. Some of them are still there. The big multi-storied, two-storied quads, those sites had trailers. We'd stay at those. You'd check in, let people know which trailer you were in.

Geier: Where you went in the Andrews, you were pretty much on your own? No one went with you?

Harmon: Sometime I went with Art, because Art knew a lot of things and he was very interested in the project. Actually, we did spend a lot of time when I was looking for places to put logs, because this was really to set up this long-term log decay study. I kind of did that on my own. But, when I was looking for trees to use in the study, Art went out with me because he just knew where certain things were. If I needed cedars, he knew where to go for cedars. Once he identified it, I'd do more work on my own. But, it wasn't that far off the road, and relatively safe. A different era than today.

Geier: Who offered you the full scholarship?

Harmon: Jerry did. One thing they sold in the LTER was the idea of doing a long-term decay study on logs. One of Jerry's approaches, and it's been effective, is to find sort of "gee-whiz" science projects. And this was a definite, "Oh my God!" You know, "They must be crazy, but maybe not," kind of project. So, in the LTER proposals, they said, "If you want a long-term project, we've got one for you." We'll just put out logs for a couple hundred years. And that's about as far as it went. Of course, they made good arguments about why it was important to do that, but the thinking behind it was not well-developed.

Geier: Uh-huh.

Harmon: They kind of knew what species they wanted, but that was about it. I was hired to take that on, develop the study, install it, and write a review, or help write a review, let's put it that way, of the functions of coarse wood in forest ecosystems. So, a lot of my time in '83-'84 and about half of '85, was writing that review and getting it together.

Geier: You were still finishing your Ph.D. at that time?

Harmon: I was still finishing my Ph.D. Actually, I finished my Ph.D. after I finished that review. That added a year-and-a-half to my work. I had first drafts of all my chapters ready, and it was a matter of getting back to them and correcting them, and getting them in shape for the defense. It [dissertation] had all been written in '83 or so.

Geier: So, you didn't see this as too much disruption?

Harmon: Not really, but there were times when I wondered when on earth I was going to get back to it and really finish it up. I was very busy, and learning an awful lot about the subject at that time.

Geier: While you were doing work on the log study, what was your level of interaction with other staff or other personnel at the Andrews besides Art, in off-hours, or whenever?

Harmon: At the Andrews site?

Geier: At the site.

Harmon: Well, I got to meet John Moreau and work with him. He was real helpful, and was the main person. I worked with John Moreau [HJA field technician for decades] mostly. There weren't many people there; mainly Fred Bierlmaier and John Moreau. Once things got hopping, I worked with Don Nielly who built a lot of gizmos and a drying oven, and things like that. He was the main person at that point.

Geier: Did you have any contact with people with the district [Blue River] down there?

Harmon: Actually, we did. I should have mentioned that. Once we convinced them that the project should be installed, we did a number of things, and they were very helpful on that. I'm trying to remember the different people. First of all, we discussed an overall strategy, because this was literally a crazy idea. To take sound trees, cut them down, drag them out into the woods and stick them out to rot, was, you know, "Oh my God." So, we decided on a number of things. We contacted people in the forest industry, and we had them go out and look at the sites, and talk with them about the feasibility. Could they do certain things? Would someone bid on a project like this? That kind of thing. Also, just to educate them in a way that this project would be coming along, and this would be an opportunity to get some old growth. We were going to basically have a swap of timber for work. So, we strategized on that.

Geier: What kind of reaction did they have?

Harmon: We all kept our humor about it. The nice thing was that this was bizarre enough, that you could just crack jokes constantly about it, and kinda keep it light. I think they realized this wasn't a frivolous request. Actually, that was going to be a concern. See, the other thing was, at that time they were doing all this piling up of unmerchantable material and yarding unmerchantable material. And that really cost them a lot of money. So, underneath it all, they were thinking, maybe this research will help us stop doing that. Because they were losing money on that big time. Of all the many reasons that had been developed for that, the rationales were not true, and also, they weren't able to do anything with that material. It wasn't being used, it was just in the way. And then they had to burn it. It just cost them tons of money to move it out and burn it. They'd rather leave it there, frankly. So, they could see that connection. A person I worked with a lot there was Vince Puleo. He was the silviculturist at the time. Vince was very important in helping me decide on the sites to take trees from, the feasibility of getting the trees out. He spent a lot of time, once I picked the sites, working on how we'd actually get access to it, if we could use old roads, or new roads, where they'd go, that kind of thing. At that point, they [Forest Service] were very, very helpful.

I'm trying to remember who the ranger was at the time. It strikes me it was Jim Caswell. I think that's who it was. I'll have to check on that. He was supportive, though, "I still think you guys are nuts." He was followed by Steve Eubanks. And Steve was such a ball of energy and was totally into the concept of managing dead wood, and its value. He saw this as an important experiment and he really cranked things up. That's when the Forest Service was very, very involved, setting up the actual sale, and also, strategizing just how this thing would work. I remember he developed a critical path diagram of this project. And I think the fear was that it was just complicated and complex. We just had to make sure that this wasn't an embarrassment and a boondoggle. So, we spent a lot of time asking questions, like, "What if it snows in September?" Or, "What if something breaks down?" And, "How late can we go," or "How will we address this problem?" So, as we approached, sort of the D-day, D-day for "duck day," they really were highly involved in the whole operation. We went from planning the basic experiment to how we would design it, and the Forest Service was not too involved in that, to how were we going to deal with people who may react poorly to this whole idea, to where we actually put it on the ground. As that progressed, the Forest Service got more and more involved.

Geier: I was going to ask you about the people who came in from the forest industries. Do you have any examples of people that were brought in?

Harmon: Well, I can't recall names, it was a long time ago and I'm not real good with names, anyways. Art would probably remember the names. He's great with names. But, there was a fellow who was a representative of, I think it was Oregon woodworkers, or something like that, or maybe it was timber workers. Some group, it was an organization that represented and lobbied for them. So, we got someone involved from that organization. And we also had some people who actually did timber harvesting, asking them how tight a situation could they work in.

Geier: Did you go more for local people, or did you go for people outside the local area?

Harmon: No, it was mostly local. I'm trying to remember. I think we had someone in the McKenzie Valley come up and give us some advice. And I know Art asked a lot of questions in terms of feasibility. At one point, we weren't sure how we would get these logs into the forests, so we thought of cable systems into intact forests. But eventually we got convinced that if we put a small load in, they could use regular logging machinery, loaders and stuff, and they could put the stuff almost anywhere. And they were right, actually. Out of five hundred logs, we had to work on the place it went, and cut down a small tree, on just one bedding site. They got them almost everywhere you could imagine. Very impressive.

Geier: How long did that take?

Harmon: It took a month, from the first day, from the first tree being felled, to the last log being placed. We really had that thing scoped out.

Geier: Any of the nightmare scenarios work out?

Harmon: No. Nothing really happened. There were a couple days of confusion. They had a really terrible yarder for the silver fir. They got some cable thing that was just awful and always breaking down. But most of it went like clockwork. The thing that was interesting, not only that it went so smoothly, but I'll never forget the first day when I went up to check on the work, and the guy who was in the loader; I'm trying to remember his name. Well, I forget. But he got out of there and just cursed a blue streak. He thought that this was just the stupidest, damn idea he ever heard of. "What idiot came up with this?" He went on and on. By the end of the month, they were quite proud of the work they did, and they understood why they did it. So, we spent a lot of time telling them, not overreacting, but just, "Oh, all right, all right." And explain, well, "Think about it this way." And they would get convinced.

Geier: Did you have any contact from the local community around there, any feedback from them on this project?

Harmon: No. We didn't have that at that time. Not from the local community. Not like today. You couldn't do this today. You'd get fried.

Geier: When you were up at the Andrews, you didn't spend much time going down to Blue River? [Town]

Harmon: Oh, we did, but it was eating, shopping, things like that, but not to convince the community or community leaders about this project. That wasn't a problem. Actually, they weren't that concerned.

Geier: Yeah. Not much interest.

Harmon: Not much interest at that time.

Geier: Could you just talk a little bit about your work? I'm assuming at this point you finished placing the logs, and you moved up to finish your other obligations.

Harmon: Well, we finished up, although it took a long time because we did an awful of work to describe what they looked like at first. We finished in October, and then we had sections cut off all these logs at both ends. We mapped them and we built some insect exposures. It wasn't until the end of March that I got through with all the work. We took those cross-sections to a cooler down at Blue River [Ranger Station] they used for tree seedlings. It was about 16 feet deep and 10 feet wide and 15 feet high, or something like that. We had that thing full of wood "cookies." Full. I mean, you could barely get the door shut. So, you had to do all this laboratory work after that, and that took forever.

Geier: I was going to ask about that because it's kind of a critical point in your career; this long-term study. I assume you were looking for outlets and ways to explain this to the scientific community.

Harmon: Well, mostly, it was to take them out there and show them what we did.

Geier: Is that right?

Harmon: That was the most convincing thing. And we would say, yeah, that's the main thing. It's sorta like, "My god, what have they done?" And it was pretty hard to convince people early on. Although for the more basic scientists it was enough of a "gee whiz" thing that they didn't think about it practically. They just thought, "Wow, what a set up." You can really understand what's going on here, and really follow what's going on. But, there were lots of skeptics at that time. So, it was important to convince people about how it would actually play out, and that took a lot of imagination. It didn't bother me because I'd worked with permanent plots for forest growth, and some of them we'd been working on had been tagged and measured for 50, 60, 70 years, and I realized that unless those people didn't put those tags out originally, we wouldn't be doing what we were doing now. We wouldn't have all that information. To me it wasn't that different.

The only difference was, instead of the trees just growing on their own, we actually had to put these things out. We couldn't just go with what was there, and not have a good experiment. So that just struck me as a no-brainer. People make fun of it, but as it becomes older, it's going to become an important resource. If they can't see it now; that's their problem, it's not my problem. Then the other thing I realized, was that it wasn't enough to put out a bunch of logs and say, "gee whiz." You had to have some pretty tight hypotheses. So actually, before I ever went down to the Andrews, I spent a lot of time just thinking what those would be, and how they would sort of lead to a model of decay. I remember some people wondering when I'd get down to the Andrews finally, because I spent a lot of time laying on my couch in Portland, staring at the ceiling thinking about things. Coming up with and then actually doing some small little models on my calculator, and graphically coming up with some ideas I thought would be very strong and stand the tests of time. Be very basic with strong hypotheses.

Geier: Uh-huh.

Harmon: So, I had done that, and I felt secure about that. We weren't just saying, well, they decay or they don't decay. We were saying, why are these species different. We think they're different because of the following reasons. We think size is different because of these reasons. We had some real solid hypotheses there. Some of the comments were hard to take, but that didn't bother me. There was this sort of background. I was convinced that it was an important thing to do and we were doing the right thing. So, I could always fall back on that.

Geier: So, the concept of the long-term study was something that was already familiar enough academically for you and for other people.

Harmon: To me, although I must say, at that time, the value of long-term studies in ecological work was still up in the air. In the last five years, there's just been no question about it. There's been enough results from enough fields that people realize that a lot of short-term results are off, and just misleading. That you have to actually look at it in a longer framework or you don't know what the results mean. But at that time it wasn't necessarily clear. We got lots of comments like, "Two-hundred year study, that's a long time to wait for results" and, "You'll be dead." Nice things like that.

Other things like, “Well, the world’s going to blow up anyways,” which is ridiculous. I’ll tell you, one of the hardest criticisms we got was from our dean of the [OSU] College of Forestry, who I sat across from at the dinner after I’d shown the site to people. And he just basically said, “This is the most stupid f-ing thing I’d ever heard of in my life.” And then I had to finish my meal, sitting across from this guy. It was like so unsupportive, it was incredible.

Geier: That was Stoltenberg? [Carl Stoltenberg]

Harmon: Stoltenberg. A real.....supporter of ecological work, I must say.

Geier: Did he say anything after that, or was that the end of it?

Harmon: You know, it was a little hard to speak. Fortunately, there were other people around, so they talked, but I was just kind of speechless. I was still a graduate student.

Geier: Sure.

Harmon: And hardly secure at that, getting that kind of abuse. But afterwards people said, “Well, what do you expect getting from Carl?” So, I came up with a series of thoughts. Okay, are we going to wait two hundred years for results? I realized that really what this study does is provide a temporal backbone, in a way, to get an analogy. At times you’ll learn about the early phases, and you’ll do all kinds of experiments and observations. And you’ll do the long-term measurements later. What we’ll do is provide a linkage in time of all these studies. So, it’s a way to link short-term studies. In fact, it’s just a series of linked short-term studies. You’d have to be an idiot to wait two hundred years. And we were not idiots. We were gonna keep working on this.

Geier: Hmm.

Harmon: To flash forward twelve years, we had people from an LTER meeting come out this spring. These people had come out ten years before when the logs looked fresh, and they were curious about the study. When they came back out, I was really amazed how interested in it they were. “How do you do these kinds of long-term studies?” and “What have you learned?” They were not just interested in the results, but how it started, “How did that work?” They thought it was a good example of planning ahead. It was only 5% through its lifespan, and already, people had turned around to see what it was doing.

Geier: I gather it wasn’t hard to get people to come out there and look at this. There was a lot of interest in this.

Harmon: Well, there was that, plus we had it integrated into a general tour. This wasn’t the only study going on at the Andrews, so we had tours on many different things, and this was probably one of the highlights, I guess. At least, in a “bizarro” sense. You could say you went and you saw where the “insane” people were.

Geier: As you were designing this study, were there any models of previous long-term studies that you may have looked at that you considered?

Harmon: Mostly it was analogies to growth and yield plots that we had worked on. Because at that time, even litter decay, like leaves and small things, a long study was a year. People hadn't even acknowledged that even that took decades. So, there weren't many decomposition experiments that went beyond a year or two. In fact, there was one published that was five years long, or maybe it was just two years long, and that was outstanding, fantastic. Seen as a real long-term study. Kind of a joke now. It was good work, but I mean, two years is a joke in terms of that process.

Geier: Were there any particular growth and yield studies that worked out?

End of Side A, Tape 1 (of 1)

Begin Side B, Tape 1 (of 1)

Harmon: No, that it was just with Jerry, you see, the great thing about analogous things, how those studies influenced my thinking on it in the long-term. One of them was Jerry's involvement with students, and my involvement in making measurements in forest plots. With Jerry, you don't just do your thesis work. He'll drag you into working on the measurement or establishment of a plot or something else in the forest. I could see the value of a long-term view of succession. We actually had data on how things change. You weren't making things up, you were actually measuring it. It was very interesting. So, I just thought of it as kind of the inverse process, and I'll study how things disappear.

Geier: While you were getting into the study, you were finishing up your dissertation, probably thinking ahead to what you were going to do next. Was there something you were doing away from the Andrews that may have had some bearing on your decision?

Harmon: No, not really. I realized that, if I was doing this – the logs – at this point, it was a real opportunity for me, because first of all, originally Jerry was going to be the senior author of the review paper. But, he realized I'd done so much writing on it, and had done a more thorough job than I had originally planned, that he basically thought I should be the lead author. I think that was at Colorado State, or somewhere, he basically came up and said, "Well, you should really be the author on this." Because, I'd presented a general review of what we'd been finding at that time to the ESA [Annual meeting of the Ecological Society of America]. I realized at that time that hardly anyone was doing work in this field outside this region, and people who had been doing the work, Jerry, Kermit Cromack, and Phil Sollins, were eventually going to pass on the mantle. They were viewing that review paper as kind of the end of what they'd found, and I was viewing it as the beginning. So, it was a real good way to sort of pass the baton. I interacted with a lot of people who did the work on that, the basic work on that, and many of them, no most, are authors on that paper. So, I realized it was just a total opening, plus I realized it was such a basic question. You could develop all kinds of things. Also, in terms of things to study, it's like a mini-ecosystem. Lots of times we have a hard time measuring what's going on in a big ecosystem. But, this was like a little, mini-ecosystem. You could do experiments on it. You could measure all the things you could measure on an ecosystem, only easier. You could explore all the ideas about

ecosystems in this little system. To me, that just seemed very appealing. I thought, "Great, let's do it."

Geier: Would it be accurate to say at that point your research began to focus more narrowly on the Andrews?

Harmon: Yes.

Geier: And you have a long-term investment.

Harmon: Yes. After having set it up and all that, and done the review, I'd sort of bought into all that before I really got going. Once I'd decided I was going on it, and I could see in the review, all these areas that were open for research that just had not been done, that had not really been thought about.

Geier: Now, this is also about the time that Jerry leaves, isn't it?

Harmon: Yes, he left for sabbatical, a Bullard fellowship at Harvard Forest about the Spring of '85, something like that. We had pretty much gotten all the planning done, and he left. He, ironically, wasn't there to see the thing set up. I kind of regret that because he really pushed it and had the idea originally, and was so supportive in many aspects of getting the study going. So, that was actually all set up by the time he got back. And at that time he then started negotiating about his future position. There were a lot of people that wanted Jerry at that time. The University of Washington was a very strong contender. So, I think by '86 or '87, he'd left.

Geier: Yeah, it was '86.

Harmon: Yeah. Well, he actually spent a lot of time here in the first year or two. You know, in transition. It was '86 when we had a renewal and I think that's when Fred took over, about then. So, there was a little overlap between Jerry and Fred.

Geier: So at that point, sounds like you were essentially in the lead on this project.

Harmon: Yeah. Actually, it started real early. I was brought in as this post-doc, as a pre-post-doc, let's say, to the LTER, and at that point, they just put it into my hands. You design the experiment. I got very involved with the PIs at that point. They were very good, I mean, there was none of this, you're a junior peon. They were like, okay, tell us what you think should happen. They put a lot of responsibility in my hands. And they also allowed me to explain this to not only them, but we had an advisory board, we still do, but they were very active at that time. So, in at least two or three of these meetings, I explained at least the thinking behind this experiment and how it's progressing. At that time I really get exposed to the overall LTER project, because I'd sit in on all the meetings as well as presenting my own stuff. And they were very open. It was very nice.

Geier: So, you were pretty well into the administrative decisions, for example, hiring staff or getting contracts?

Harmon: Well, some. I had a pretty small budget and we did a lot with volunteers. And the work for setting it up was done through this contract with the Forest Service. Even

at that point, I wasn't that involved in the budgets or anything. It was a pretty small part of LTER at that point.

Geier: At what level were you involved with identifying the personnel for the project?

Harmon: There weren't any!

Geier: There weren't any?

Harmon: Yes. I was everything. I mean, any head technicians, like Jack Booth, for example, and John Moreau. When we needed them, we would have them available and work on it. I recruited my wife to help around here. I had a technician from some Park Service-related stuff; I dragooned her into it. I had a lot of volunteers. The Stream Team, for example, when we needed bunches of people in the field, they would just show up that day with ten people and say, "Tell us what to do." So, a lot of it got done, not by hiring a big staff, but actually just coordinating what we already had.

Geier: I see. You mentioned your wife. Was she working in this field also?

Harmon: No. We met in Tennessee and got married just before we came out. And she got a job with the Army Corp of Engineers, and she lived in Portland. So, I lived in Portland and commuted, and sometimes, I had a place down here. That was just getting to be undoable, and we wanted to have a child, and got tired of not living together. So, we got a place in Corvallis, she quit her job and then, she spent about six, seven months helping me get this study set up, working for me.

Geier: You mentioned her name?

Harmon: Jan, Jan Harmon. Eventually, I had to pay her back for that effort on her own project. She had a project that in '87 involved riparian vegetation seed banks, and she had our son just as these seeds were sprouting. So, I spent several months watering her seedlings for her and recording data for her, to pay her back.

Geier: Was it an LTER?

Harmon: No, actually it was done through the riparian grant on which Stan Gregory, Art McKee and Jerry Franklin, and I think, Fred was on that, too. Yeah, Fred was on that. Gordon Grant also. A whole bunch of people on that. So, she did a seed bank study on that. I paid her back a couple months later, about five.

Geier: In terms of the people who worked directly on that study with you, though, it was mostly people who were already connected with the group, and they just kind of reallocated the resources already existing?

Harmon: Yes. I guess there was one part that wasn't that way. During June of '85, we set up a stream, sort of a mini-study, in which we took logs that were only about two meters long and a small diameter, so people could pick them up. And we put them in a stream, and we put them in an upland area. That was kind of a small-scale version of this thing. We built our own logging systems, and it was funny. We had a skyline system made out of cable and it ran off of an electric winch down this frame. We had a yarding system that was run off of a pickup that ran up and down the road. Jack Booth

developed all that and that was kinda fun. In that case, we actually had ten or so volunteers. At that time, the Andrews had a summer volunteer program to do vegetation measurements and other things. Now we use summer technicians. But then, we were using these volunteers. So, they spent their first month putting in this study. They were recruited by Art McKee. He had them available for all kinds of projects, and I took advantage of them.

Geier: This is volunteers again?

Harmon: Yeah. Student volunteers. A lot of them were students, people in-between school, wanted more experience in ecology kind of work, and we'd give it to them. We gave them food, plus a stipend, mostly.

Geier: Were those mostly local students?

Harmon: No. From all over.

Geier: Really?

Harmon: All over. I've seen some of them since. Someone from New York, for example. Some were local, and some were from all over. They recruited nationally.

Geier: Art had?

Harmon: Yeah, I think he did.

Geier: I'm curious about the networks that students get.

Harmon: At that point, that's what we were doing. We were going with these volunteers. I was taking a resource of people that was there, and organizing them to do the job. But I wasn't literally recruiting at that time.

Geier: That sounds like one of the few times that you actually brought people in physically for the job.

Harmon: Yeah.

Geier: How you would characterize the changes in the structure, focus, and priorities of the group since you first became involved?

Harmon: The LTER group?

Geier: Yeah, right.

Harmon: The first period was focused on two things; setting up some observation systems. This is in streams as well as in the uplands. You know, they put in a whole series of blocks, in the Hagan Block [west of HJA EF] watershed, too. It was consolidating the measurement system. I think Stan was doing stuff in Mack Creek and several other places. But the other thing was long-term experiments. That was the focus. The sort of ultimate, long-term experiment was the log one. But there was one on snowbrush [*Ceanothus velutinus*] that Phil Sollins was in charge of. There was one Dave Perry had on stand-thinning, but it was really young-stand development. At that time Stan was setting up for a stream manipulation experiment, and I think it was in the Hagan Block, in which they would actually change the amount of woody

debris in the stream and the nature of the forest cover, and set that up. Those were the ones I recall. There may have been others, but those seemed like the main ones. So, it was very experimental in nature, long-term experiments. Several of them involved woody debris. Obviously, the log decay on the riparian manipulation involved it as well. And nitrogen fixation, that was the other thing that was key. A lot of those got set up, like the snow brush experiment. Dave Perry got his young stands thinned, manipulated and fertilized. One of them, the stream manipulation, never got going because of environmental concerns. It just never became viable. So, it just basically turned into a long-term monitoring project of the stream, because they were getting background, or pre-treatment data.

Geier: When you say environmental concerns, what do you mean by that?

Harmon: At that time, it was in a road-less area review process [RARE 2, which limited logging as areas were studied for possible exclusion from further roading]. So, there became a concern about developing it. Should it really be developed? The answer basically came down, "No." That's probably not worth sacrificing this area for this experiment. Also, the public was becoming more and more concerned about this issue. So the whole national forest plan, the five-year plan that took ten years to develop, or whatever it was, was starting to really heat up at that point. That got abandoned. Of those four [research and monitoring projects], the ones that are really active now are Dave's young stand one and the log study. The snowbrush study, although it started off quite well, the plants we were interested in studying, really didn't show up. We don't know why. I think Phil will really talk to you about that. But it just didn't materialize the way we thought so, most of our treatments weren't treatments we expected to study.

Geier: Are you speculating it was the elk?

Harmon: I'm not sure we know, but I think that's as good of an idea there is right now. It sure developed differently than an adjacent stand that's not very far from there, that's completely different. Then it was experiments, big experiments, long-term experiments. We struggled with the idea of adding more in LTER 2, so at that point, I actually contributed a proposal. I wrote the stuff on logs. I was still a graduate student at that point. But I kind of knew that inside and out, and we developed an idea that maybe we should look at several things. We should look at long-term site productivity. I think that was viewed as something that would come on board late in LTER 2, if I remember right. And Dave Perry, well actually, it wasn't clear who was running that project at that time. It's always been a bit of a problem with that project, probably because it involves so many disciplines, but it led to some problems. We had this endless debate about adding in a thing called the "Phoenix Project," which was another one of Jerry's "let's do something big and bold and exciting," in which we were going to torch off a big area of the Andrews. I don't know how big, but a clear patch of a catastrophic fire, so we could actually look at catastrophic fire effects. That was actually a neat idea. But, in the meantime, mother- nature has actually burned up plenty for us. But, it was kind of, "Let's do some pretty wild work," but it never crystallized. It just never crystallized. And I think, that was maybe a bit of a glimpse of what was going to happen on long-term site productivity, because it was a bit more complex than anybody could quite take on. But the Phoenix one really sizzled, because it

was very clear we could never get consensus on a central idea or theme or anything.

Geier: Sounds kind of simple on the face of it.

Harmon: Yeah. They both sound simple, but that one was very risky in terms of the resource. Would we burn up the Andrews? Plus, it just was not coalescing. Too many cooks were in that soup than it took to do it.

Geier: Right.

Harmon: It ended up tasting like nothing. Gray paste. Lots of good ideas on their own, but it just was never adding up to anything. By the time LTER 2 got over, it was really clear, based on the budgets and things that we were not going to be able to keep adding these mega-projects. We were having real problems getting the ones we had, tracking, and like I said, two of them sort of dropped out. Not necessarily for any fault of attention, it just didn't work out. So, we were aware that they were risky. Site productivity was not really working the way, I don't think, anyone had envisioned. It had eaten up a huge amount of resources and we had nothing to show. Very little to show for it, relative to the money that we had put into it. In contrast, it took about \$30,000 a year to set up, and a year-and-a-half, to set up the log experiment, which was very tight and narrowly focused. But, long-term site productivity, if I remember right, chewed up \$70,000 for five years, it still hadn't been installed, and it wasn't clear what they were doing. It was a real contrast and I think an eye-opener to people that, gee, they're not only risky, but they're expensive and we can't afford it. Then, another thing was coming along. We had a lot of synthesis in IBP. Experiments came out of LTER like the log experiment and the snowbrush experiment, etc. We were going back into a period of synthesis. And that really came up strong in the LTER 3 proposal. We were going to start doing more modeling. Our group hadn't done modeling since IBP. Sort of synthetic modeling. We were also realizing that a lot of these problems involved a landscape analysis. They couldn't be done as a little, teeny process anymore, or even an ecosystem in the sense of a stand and its nutrient cycles. We had done that in IBP and it was clear we had kinda gone as far as that could go, conceptually.

Geier: Uh-huh.

Harmon: We needed to push it to a landscape level. So, those were the challenges really laid out in LTER 3.

Geier: Was there a shift when all these decisions were made?

Harmon: No. I think we'd always done it by consensus. The group would buy into a general issue. It wasn't always total agreement, but these decisions by-and-large have been made by the group. It's been a blessing, and it's been a curse. If you had a benevolent dictator, maybe things would be more efficient. But, it also brings a breadth to any decision. So many aspects are considered, and that's been helpful. Even budget decisions are made largely by the group. This was a time, entering LTER 3, when budget restrictions really were starting to show up. Even though we were getting annual increases, there was inflation in running projects. The log study started for \$30,000, but it was starting to creep up. You couldn't do anything for \$30,000 a year.

Geier: How does the budget develop over the years?

Harmon: Well, it started at maybe \$20,000 or \$30,000 a year, and it stayed that way for a long time, right through LTER 2. The money had to come from NSF grants. There was no money. I remember being so frustrated by NSF, because they kept saying this was an LTER project, and I literally had to write in the proposal there was only \$20,000 a year. That couldn't pay for even one PI to supervise us, there was no nutrient analysis being done, nothing was happening, if they didn't put this money in. And they complained that I complained, but they gave us the money. They thought it was outrageous, but it had gotten to that point. There was a conflict in NSF and with the reviewers. Some people thought LTER is getting too much money. Now, as a new program with new money, they wanted that money. So, whenever you came in to compete in another ecosystem program, they would just dismiss your proposal out-of-hand. You've already got too much money. It's been a long-term battle at NSF, and with the reviewers in particular. They don't understand how the money gets divided up. They look at it at half a million dollars a year, or whatever it is now, 560 [thousand dollars], a lot of money per year. But it's divided among all these projects, so nobody gets anything. We struggled a lot to get that funding. The last couple years, as I've expanded out into different areas, like the carbon sequestration. I've gotten more money, but the log project still only gets 20-30,000 [dollars] a year. Quarter-time technician. That's all that's on LTER. Everything else has to come from somewhere else.

Geier: In the case of that study and others you've been involved in, was there ever a concern that you'd identified or could recognize from NSF reviewers about the fact these are group decisions?

Harmon: No. I think you can make the point, at least in site reviews. That doesn't really show up so much in NSF reviews. They're mostly concerned with the science and feasibility of it. I think it mystified people, but they're very supportive of it. I mean, they're always mystified by the command structure. "Who's in charge? How do decisions get made? How do the people work here? Draw us a flow diagram." We never could, because it just worked. Stan Gregory always used to say, "We don't want to know how it works, because we'll break it. We're just gonna do it. We know it works. Why screw around with it?" It really had to do with people. People trusting each other and respecting each other. And being able to disagree and yell at each other, and still be able to get in a meeting the next time and talk rationally to each other.

Geier: Did you ever encounter people who didn't fit into that?

Harmon: Oh, yeah. That happened. I think several people tried to get into the group and never were able to do it. Should I name names?

Geier: Yeah, if you can.

Harmon: Mark Wilson was an example. He got money for quite a few years. But he'd only show up when the money was handed out, he never came to any meetings, and he never involved himself in the group. We decided, as a group, that this was not a mini-grants program. You had to be involved. Mark could make a contribution, but he just wanted the money. He

didn't want any strings attached to it, and that didn't work out. People did come in and ask, "Why aren't you supporting me? I'm on soft money. I need money. Give me money." And that never went over big either. There's always been a feeling that you needed to make an investment in the group. The group will try and look after you as best it can, and it's been harder, as the budget's been fixed. Kathy Whitlock would be an example recently, where we can't afford to pay her salary, but we can do things, like get her to important meetings, especially LTER meetings, so that she can contribute to the community. That's kind of what we're restricted to doing now. But, there's always been that, "You buy into the system, you invest in it, you tell us about your work, make it part of our thinking, and you're part of the group." That's the only real admission price.

Geier: You're talking about going to meetings, site visits, group tours, things like that?

Harmon: Yeah. Things like that. Some people are put off by that. They don't want to make that investment, they can't make that investment, or they're not sure it's going to pay off.

Geier: Can you think of any reasons or hurdles that might discourage people from doing that?

Harmon: Well, first of all, the funding's gotten so tight recently, that you really have to go after a sure thing. There are not many things you can go after that are high risk anymore. Even at NSF, the success rate is 20% on average, and some programs, it's 10%. You just cannot afford that time when you're writing maybe 10 proposals a year. Because you also have to publish. So, the soft money people just can't afford to do that anymore. You can no longer be on only one proposal. The amounts have gone down and the success rate has gone down. So, you have to be working on four or five [grants] at once for funding.

Geier: But, that's grant writing, you mean?

Harmon: Yeah, grant writing. You just have to go with things you're sure of. Even if you're success rate is 50%, which is really good, five times the average. Say, at that rate, you gotta work on two or three solid proposals a year. That's a lot, if you have to do all these other things. So, I think that's a real restriction now. There are several people I'd like to involve, and I think I can involve them. Or the group would involve them. It's actually been a group discussion, particularly in the physiology of plants. We haven't had anyone doing that for 20 years at the Andrews. Not really, since Dick Waring dropped out, and he's not really a physiologist. It's really been the IBP days since physiology's been done. So, getting someone involved would cost \$20-30,000 a year, for a student to do the work. We don't have that kind of money anymore

Geier: Does a connection with the LTER group here tend to increase the chances of getting funding, do you think?

Harmon: I don't know. I think on some fronts, yes. I think it doesn't hurt you anymore. I think it hurt for a while, now I think it doesn't really hurt so much.

Geier: I was just asking because you mentioned the case of that person coming in looking for money, but not being real involved with the group.

Harmon: Well, no. I think some people didn't understand what the program was about. They thought it was sort of easy, that NSF gave the LTER grants, then LTERs gave out grants, too. It was never supposed to operate that way. To complete a thought, because we got a little bit sidetracked, for LTER 4, you were talking about progression, and on LTER 3, we struggled with synthesis, endlessly. We knew the direction we wanted to go. We weren't quite sure how to do it; there was a lot of internal strife about how to do it. We were pursuing "holy grails," like a grand synthesis. Maybe there was one, and there might be, but it is kind of a "holy grail" pursuit. While we weren't very productive in synthesis per se, I think we learned a lot more about what we needed to do and how to approach it. By LTER 4, which is also geared towards synthesis, it's much more focused and has particular areas that are going to be explored. They're very specific and they have specific hypotheses, so the direction is there. There are the encompassing questions. It allows these things to fit underneath it. It's not like deep-think, or whatever it is, like in *Hitchhiker's Guide [to the Galaxy]*, that cranks out 49 or something after five years. We don't think there's some ultimate answer there. We've sort of given up on that for a while.

Geier: Is there a turning point? Or are there different personalities involved now, or is it simply in the process of that?

Harmon: No, there are different personalities involved. Phil Sollins tried very hard to lead the synthesis effort. I think he'd admit he wasn't able to do it. Part of it was, I think, this baggage of the "holy grail." Part of it came to personality. I think Phil loved working with big groups, but he's hard to work with in big groups. He has a very strong personality, and he enjoys the fight. He enjoys the process. Let's put it that way. And it doesn't bother him to revisit questions a lot. A lot of us, however, don't relish the process of meetings. I mean, we want the meetings to actually lead somewhere, not to more debate. That was a big conflict. And I think, he basically, on his own decision, decided to drop his participation and to back off from that synthesis area. So now, instead of a grand synthesis, we have different people leading the different areas. There are four. Thinking about it, Phil was trying to lead those four, and probably another four. I don't think anyone could really achieve it. Also, there were some personality problems. I think, it needed somebody who would push it in a direction with a sense that you were getting something done, and that was just not happening with Phil.

Geier: A question occurred to me while you were going through your progression from 2 to 3. You were talking about the Phoenix Project, things that got started and didn't quite gel somehow. I was curious about the decision-making process in that. Was there kind of a decision point?

Harmon: Well, I think in the Phoenix Project, it was very clear to everybody, we were not making progress. It kind of faded away. We did have some decisions, nice discussions, but we're going nowhere on this. This does not happen on long-term site productivity, and this is probably going to hurt some people's feelings, but I think it was very poorly-managed. It was poorly-managed by the group, because when progress wasn't made, we were not hard enough as a group to cut out funding. We would always give it another chance, which just meant that the same thing happened. And that was a very, very painful experience for the group, because

they didn't want to be hard-asses, but they probably needed to be hard-asses. Originally, they wanted me to take over that project. But I'd just got done with the log project and I just didn't want to be the super-technician. I really wanted to stay within that and develop it science-wise.

So, they had to find someone to do it, and at one point, it was run by Phil Sollins, Joe Means in the Forest Service, and Dave Perry. They were hoping Joe would take it on, but Joe's personality just wasn't right for that. He's not a great organizer, and it was a great opportunity for him, but it just didn't happen. So, he kind of dropped out of it. But it was kind of a slow death. A lot of this thing in site productivity was like a real slow death. Nothing seemed to be chopped off. So, he just sort of faded away, and then Phil and Dave were running it. They had some interesting ideas. Phil got the snowbrush study organized quite well. He had shown some organizational skills, but he left for Yale University at this time. So, his on-site presence was not very high. He was like non-existent for about two years. I'll say this for Phil, he always came back and kept us informed, and he kept involved. But this needed day-to-day supervision. So, Dave was left doing it, and he's not an organization man. He's a big thinker and all that, but he's not strong in organizing experiments, and he isn't an organized person. Well, they hired a post-doc. Because everybody admitted this, we know what we're all like. And people owned up to it, and we were gonna hire a post-doc, and we got a real cracker-jack guy, Steve Hart, who is now, an associate professor at Northern Arizona University. Quite successful. He arrived somewhere around '87, maybe '88. He arrived there and he never had a meeting that I heard about with Dave, for a year. So, the guy spent a whole year wondering what on earth he was supposed to be doing here. He went to meetings, but evidently, he and Dave just never got it together. So, the project just sort of started off like that. And then Steve decided, frankly, he was just going to do his own thing, to finish out his post-doc, so it wouldn't be a total failure. He basically set up a bunch of studies and he's published on those. But in terms of setting up the project, nothing happened.

Geier: Those studies that he did, were they integrated into the group?

Harmon: No, but they were related to the topic of long-term study productivity. But it wasn't the way I approached the log thing. "Let's get the hypothesis, what's the study design, let's get on with it here." It just never crystallized at all. Then Dave was having second thoughts about cutting an old-growth stand just to put in a young stand on the Andrews. And a whole bunch of things started to crop up. There were owl sites nearby. Given the fact that there was never any momentum, it was impossible to get any. It was like we were just stuck in this goo. And Dave wanted more and more money, and there was less and less to give out. And the productivity was not excellent, I mean, it was pretty poor, actually. And they weren't going outside at that point. In the log study, the strategy was get the basic thing set up, and get out and get money from NSF to do the rest. For site productivity, it was all done internally in LTER, so instead of getting into writing proposals to get the basic thing funded, it was always, go to LTER to get more. Well, the problem was that we were not very good with accountability as a group. We were actually awful, and we're all to blame there. For something to have happened, Fred

would have to have stepped forward, been the real “bad guy” and just said, “No more money. Period.” He didn’t want to do that. He’s an optimist and felt it would turn around eventually. But it never really did. So then, Dave, I’ll never forget, one day declared that the project was over, after maybe about \$350,000 had been spent on it. He hadn’t gotten one paper out on it. There was just a little bit of field work, and everyone was dumbfounded. It looked totally dead. Eventually, the Forest Service picked it up and moved it to the Isolation Block [Willamette NF land south of Vida, Oregon]. They, frankly, did a much better job with it than we did. But it was a shame because when it started, it was a hot topic and you could have gotten money from the National Forest Service or the National Forest or the National Science Foundation, to do it. But it all just withered away. I would count it as the biggest disappointment in the whole LTER existence.

Geier: The Forest Service picked it up? Who?

Harmon: Bernard Bormann eventually came in and started to work with Carol and Dave. And then eventually when Dave decided to just drop the whole thing. He wasn’t gonna fight to have it done on the Andrews. Then it actually became a Forest Service project, and they found a place to do it off the Andrews.

Geier: Where was that?

Harmon: The Isolation Block.

Geier: Where exactly is.....

Harmon: Well, let’s see. I guess it’s like at Leaburg. [West of Vida in McKenzie River Valley]

Geier: Okay.

Harmon: It’s a separated part of the Willamette National Forest, surrounded by private land. It has mature timber. Actually, at that point, Jim Boyle, who’s over in forest resources [OSU], took it on. He was the LSU connection, and Bernard was the Forest Service connection of scientists working on it. They finally they got the thing started to be installed, at least. But, probably about a decade behind the LTER plan. Kind of a shame.

Geier: That’s a good case study though.

Harmon: Yeah, there’s lots for everybody to feel bad about in that one.

Geier: You raised some interesting points here about your concern about the about possibly moving away from the log study into the long-term productivity study, and you were concerned about becoming a super-technician.

Harmon: Yeah, because I knew I could do the logistics stuff. I thought I’d done that, and I didn’t want to keep doing it.

Geier: I thought maybe you could talk a little bit about your concept here of the science of the log study, versus the technical aspects of it.

Harmon: Well, there’s some involvement of science in setting up the study, because I believe

you have to have some pretty broad hypotheses that aren't trivial. I remember someone said, "This process is the sum of the interactions between biotic and a-biotic factors." And I thought, what the hell has been left out here? This is like, everything is important to everything. So, it's really quite challenging to come up with something that isn't trite, but may stand the test of time, and is flexible enough to do a lot of things in. It's really challenging. That's where the science of that goes on. The technician part comes with those kinds of hypotheses and the mechanisms to test them. And, plan how you put this latest experiment out and get it out. To do it right is a really time-consuming process. For the long-term site productivity study, they never did that. I can remember Art saying that they when went out to look at the site with the Forest Service, they were thinking about how they would do the cutting and asked, "Well, what are the treatments?" Dave had no treatments. He said, "Well, wait a minute." And he sketched out these treatments on his car hood. That's all he put into the thinking of the treatments. It was incredible.

Whereas in the log study, we need these species and we need this many logs, and we're going to put this bunch in tents [insect exclosures], and this bunch here. We could tell them exactly. To do that right, it sounds trivial to the scientist, but it's really difficult. It's not a job you can half do. It's like half-baking a cake. It doesn't really work. If you're gonna bake the cake, bake the cake. Don't half bake it and put frosting on it. A crummy analogy, but it's like that. And it takes an incredible amount of energy to do it seriously. That's not where I wanted to go. I knew I could do that, and I've done it since. There was this LIDET experiment, this long-term, inter-site decomposition experiment. [Long-Term Intersite Decomposition Experiment] I was chosen, I'm not sure why, but someone called me up on behalf of the network and said, "Well, you're on this thing. We know you can do it because you have these skills." But, that was a few years after the log thing happened. Plus, I know a lot about the science and what needs to be done, and studied on these logs. I want some time to actually do that, pursue it. Reap the rewards of all that effort.

Geier: This series of short studies that you talked about, you had a vision?

Harmon: Yes, the various ones we should do. Some of them didn't work out, but a lot did.

Geier: Maybe you could talk a little bit about your priorities in terms of where you saw that long-term study going when you laid it out and for these short-term ones?

Harmon: Well, some of the things were very simple, like, questions about the water balance. There are so many areas in coarse wood where people didn't really look at the system, and they just sort of seat-of-their-pants said something, which became "the fact." And as we sent the proposal in, we are often reminded of these facts. We wanted to do a study that would actually show what was going on. For example, in the water balance study we did a very simple study where we watched the water that came through a canopy and rolled off the tops of these logs and came out the bottom. How much was in the log? People had said, prior to that, that the logs had no influence on the water balance. Their idea was that the logs are always wet. Therefore, they neither absorb water, nor release water. They're just inert. When actually, they absorb water and they release water. That is why they're not static. The system is

dynamic, not static. But everybody thought, "It lasts forever, it is static. It's just like a rock with bark on it." That seemed like a big priority. We found it influences the water budget of an old-growth forest significantly. One way we think about that is, there is the interception, how much water gets taken out by the canopy or the litter layer before it enters the soil. They had that term at zero for loss. We found out it was 2-6 percent. Now, that's a small part of a hundred, but you have to realize the canopy takes out 10-15 percent. So, 6 percent is a lot to miss. It was like that on many things. Every time we looked at nutrients, the logs, we thought, acted like big sponges. But we found out, actually, they were releasing nutrients every year. They weren't absorbing nutrients like everyone said. They were releasing them. That leads to the questions about the mechanisms. How does that work? Then there were basic things like food-webs. Who breaks down the wood? Which insects are involved? Which fungi? The insect part kind of worked out, but the fungal part was too difficult. We couldn't do it then. Maybe we could do it now.

Geier: Was there a knowledge base?

Harmon: Well, we didn't have good isolation methods, and we didn't have good identification methods. All we ended up with was, "There's a pile of brown fuzz, and white fuzz, and darker brown fuzz, and black fuzz." Out of 7,000 isolates, they got a thousand down to a genus. And a lot of those are penicillim, everyone knows what penicillin looks like. I could ID that. It's just green; green-blue. Over half of those were penicillium. We basically didn't know what on earth those things were. So, we started looking at it a different way and got some good results. The trouble is, there's a lot of small fungi that don't have big mushrooms. We still don't know what they are. But now there are some methodologies and techniques that will help you do that. Looking for genetic tags and stuff, for example. People are just starting to do that.

Geier: When you got into these various studies, like water balance and nutrient studies, how do you characterize the significance of the interactions you had with people at the Andrews' site or here at OSU in developing those fronts?

Harmon: Well, it was a rocky road at first. I was one of the first PIs to kind of take over when this process came in. I was still a graduate student. Tim Schowalter was chosen to do all this process as a Ph.D. scientist. In one of the proposals, my name wasn't even on it, and I was really bummed. I had just spent a year-and-a-half putting this study together, and it looked like I was not going to be involved. That was kind of a bummer. Then I got involved, and then, I got very frustrated with Tim's leadership because he didn't really understand the system. He's got great insight into insects and canopy work, but the log work I don't think he really has that, and I wasn't alone. A lot of people didn't think he had the insight. He actually came up with this biotic/abiotic hypothesis, which left a lot of us wondering, "Oh my God. This is a problem." Because of that, NSF will not fund a study like this. And then he had a whole two-page list of hypotheses, which were like, "Temperature does not have an effect on decay. Water does not." It was a whole bunch of null hypotheses which weren't well-thought out, and I wanted to have them very well-integrated and very-focused. So, he and I started butting heads immediately. And we just did not get along. Eventually, the group that was involved, most of them were not LTER scientists by the way, but most of them were scientists, just lost

confidence in him and wouldn't follow his leadership. They just didn't want to be on a proposal with him.

In the meantime, they learned more about what I was thinking. They didn't agree with everything I'd said, but they had a lot more faith that I could kind of go through all this and put it together as they intended. So, that's how it evolved, that I would take over all that process stuff. By then, I had my Ph.D. and had gotten the degree. I was trying to be a team player, but Tim was just not prioritizing things. He was taking money out of the project and spending it on other places that were not paying off. It was a pretty tense time for him and me. We now talk to each other, but I think there's some reservations still there, on a personal level. We tried to get a big group. We got an entomologist, Jack Lattin, involved, Andy Moldenke, and Tim. Then we had people working on fungi, and that was initially, Jim Trappe and a fella named Steve Carpenter. Jim was not that interested in it. He kind of dropped out. I think he was mostly helping Steve get established, so, Steve eventually took over. Who else? Myself, and that was about it. I think others came on board later. Actually, we had Rick Kelsey. He was hired on as a post-doc, and he's at the Forest Service now. Rick was working on attractants for the insects. Steve Carpenter was working on identifying the fungi carried by the insects, and also, that got into the logs. So, it was kind of interesting to see if they were different. I was doing most of the ecosystem type measurements; decay-rates, nutrient-changes, water-balance, leeching, respiration. I was doing all that stuff. I was actually doing about 80 percent of the work and not getting paid very much. Eventually, it involved Elaine Ingham, who's in botany [OSU] and she came with a background in non-arthropod invertebrates, things like nematodes and bacteria and protozoa.

From that point it was very population and biodiversity-driven. I think those were really good ideas. We were kind of covering all our bases, but the NSF reviews coming back were not very good. First of all, in ecosystems, there wasn't a lot of support for knowing what the organism was. I think it's wrong, but there just wasn't. So, they weren't going to pay that money. We were not that focused. We were trying to do it all. Actually, we had one proposal that was like \$4 million for five years. Or maybe it was two; I guess it was 2 1/2. But at that time, that was astronomical. That was an LTER study. We didn't have the common sense to realize what we were asking for. (Chuckle) It's funny in retrospect, but we were just all learning, and I was learning in particular, what we needed to do to get it funded. So, it was turning out well. Tim ran it for the first couple of years, and then, we put in our proposal and it didn't get funded, and he was going to give up. I called Jerry Melillo at NSF and said, at least give us the money to shut this thing down. Don't just turn us off. We got all these fuzzy things we want to save.

End of Side B, Tape 1 (of 2)

Begin Side A, Tape 2 (of 2)

Harmon: I guess I convinced him. Tim was a little bit upset because I kind of went behind his back. I told him I was going to do it. Actually, I told him I thought he should do it, but he just wasn't. He was pretty dispirited at that point. He put a lot of work into it. I just don't think it was going much where he wanted to push it. So, we got money to shut it down and brought up

another year. At that point people said, “You’re the PI on this project. We’re not going to follow Tim anymore.” Actually, what happened for that money was, I wrote the proposal and Tim put his name on it first. People got Tim’s draft, they just laughed and said, “We’re not going to send this in, Tim.” Tim went and revised it, but people came to me and said, “You write the proposal. You seem to know what’s going on. These are the ideas, we’ll work with you.” And then we gave it to Tim as he was the PI. At that point, it was pretty clear people were not going to follow Tim any longer. They just were not. Eventually, we kind of worked it down to a nice hypothesis that was doable, which involved what happened to the carbon. We broke it down and in what we call a brown rot system or a white rot system. Just to explain that a little bit, a white rot degrades about everything, and a brown rot doesn’t degrade lignin. If you have a brown rot system, it accumulates a lot of this brown mush on the ground, and in a white rot, it doesn’t. It had a lot of impacts on nutrient cycling and carbon stores. We sold it on that basis. At that point, most everybody had dropped out. They were discouraged, maybe because it was clear NSF was not going to support a biodiversity emphasis. They’re not gonna do it. Since then, we went in for a renewal, and NSF was very reluctant to fund it. I must say, I’ve been very disappointed in their level of support. Sometimes it got bad reviews, but even when it got good reviews, the program director never put it in a priority slot. I’ve temporarily given up on them as a source of funding for us. They don’t seem to think it’s worthy. I’ve presented lots of talks on it for NSF and LTER. They’re always well-received, but it never translates into funding. It’s a mystery to me.

Geier: Sounds like you had a connection with the program director?

Harmon: Well, I knew who he was, what he did. Normally, I never call the program director. I’m a person who will develop my ideas independent of the program director. I’d realized in the log research, I had to stick with real sound, basic science and not go for fads. I could not keep up with fads, and I couldn’t apply them to the log research. If I was gonna do that, I had to convince people there was a lot to be learned, and it was good science. I found that I never got anything out of the program directors. It was impossible. My habit has always been to kind of not do that much, and this was a rare exception. I just felt desperate that we needed to not throw things out. Now, this happened to George Carroll on his tree [canopy] research. He had an NSF project going, they were doing some real exciting work, and then they had, I guess, a so-so proposal that could have been improved. But NSF basically said, “Don’t send this in again. We’re just cutting you off.” And a lot of that data has never been examined to this day. They just turned it right off. I kind of reminded them of that disaster, because they were finding out lots of neat things in the canopy. Which is starting to be learned over again at Wind River [Experimental Forest in southern Washington, where they had a canopy crane for years at that point].

Geier: There was a recognition that NSF said that there had been a mistake?

Harmon: They had made a mistake in doing that. But, they threw their investment away is what they did. So, I made that argument, plus pleaded. I think I was pretty convincing. I was really concerned, I think that’s how that came across. They found the money to just shut it down.

Geier: Maybe you could talk a little bit about your perception of the advantages and disadvantages of doing long-term research with grant-supported programs?

Harmon: It's not necessarily a study, but the advantage of a site where you can keep making these investments, it really does start to add up. And it may not be that synergistic in the sense that you get a lot more out of it than the parts. At least all the parts are there to build from, and assumptions about things can be much stronger. So, there's that. But also, I like to think of the synthesis before, after the fact. In other words, usually we take a whole bunch of facts and information, and we know we get a convincing story out of it, but a lot of times it's very qualitative. Whereas, when you have a synthesis before, you actually think of what the connections should be and go out and investigate them, you often can get more than a qualitative analysis. You can make a real quantitative analysis, and sometimes that can tell you things.

Here's an example: invertebrates are important in the decomposition of litter. There's no doubt about that. But, if you look at sites in the whole LTER network, you'll find there are some sites, like in the Arctic, they don't make a difference. They're not that big a deal. In the desert where there's termites, they're a big deal. They really control how litter disappears. So, in one place, they're eating a little bit, and in another place, they're eating most of it. You don't see that kind of stuff, a lot of times in after-the-fact synthesis. You don't see that kind of stuff. Now, that example is what we did in LIDET, which is more a spatial dimension than a temporal dimension. They're somewhat related. I think, the thing of being able over time to make these investments in a system, we'll set this up and we'll observe it, and periodically make experiments, you learn a lot more. And it just holds together as a story, without any artifice there. That's the story, versus making a likely story that sounds good, consistent with the facts, but God knows if it works.

I'll give you an example of that. A classic way to get at longtime dimensions is to substitute space for time. In other words, you go out and find old fields that are different ages, or forests that came from them, or in this case, find logs that are different ages in the same site, same time. They did that work on the Andrews, and they found decay rates. The big story was that Douglas-fir decays at half the rate of hemlock. People took that data and said, "Douglas-fir decays very slowly compared to Coweeta because it's really cold and wet out there. Coweeta is warm and wet." It's really temperature then, isn't it? Yeah, it's just colder at Andrews. The problem with that, and it sounded logical, but when we actually started putting it into an experiment, and we'd gotten the results, we found a given size hemlock decays at about the same rate as Douglas-fir, at least the first decade or so. Same rate. Why did they get that early result? Because they were measuring Doug-fir this big, like a meter across, and hemlocks, 30 centimeters across. What we found was, there's a tremendous effect of size. And Doug-fir does decay slightly slower than hemlock, don't get me wrong. But the major effect they were recording, was one of size. And that was the classic result, still out in the literature there. When I first came on board, that was one of the classic stories. Wow, these species are really different. So that was just an example of where this chronosequence approach can be

misleading. When you put together what sounds like a good story, but then when you test it with an experiment over time, it isn't necessarily true.

Geier: Part of the study you were working on, to put together a long-term study like that, sounds like what you're saying is you need to have a very clear idea up front, so you have good testing points. This is what we've done so far, and this is what we're looking for next.

Harmon: Yes. That's right.

Geier: So that grant structure kind of encourages that kind of thought process.

Harmon: Yeah, I think so. It isn't a fishing expedition. And that's part of the problem we had early on, we were on a fishing expedition. We covered things from the nutrient-cycling, to the insects, to the plants that grow on the logs, to all these things. There hadn't been anything done, so we were going to do it all. We were going to be the wood decay center of the world. Which we kind of are now, de facto. But nobody else is interested in creating such a center. And we had to kind of learn that fact slowly. We were pretty stubborn.

Geier: I was going to say, flexibility sounds like an asset, and what you're saying is the reverse?

Harmon: Well, no, there is some flexibility. But I think that you need that to keep a really solid thing to build on. It can't be, as Fred Cox thought, the inverse of Sir Hillary's comment, "Why did you climb it? Because it's there." Scientists use the inverse, "Why did you study it? Because nothing's there." And that didn't sell real well. It's just not competitive. Plus, it's sloppy thinking. If you think hard enough about it, you'll come up with some things that are pretty profound. For example, that basic model we had of why size and species were different that I came up with back in '83, is now the basis of a simulation model that we're building that's gonna be applied all over the world, because, basically, we found the underlying controls. Now, the rates of those things are all different everywhere. But, it applies. It's a very general kind of model we had and were investigating. Now we know that it basically works, we can run with it.

Geier: What about some disadvantages, some problems, with that structure? Do you see any?

Harmon: Well, you're talking about inflexibility, but I think right now, I have probably a lot of long-term studies. Maybe more than average, if I listed them: there's the log study, we have a branch study, we have a long-term root decay study that I have with a graduate student that's a decade-long study. I have another, sort of a global-level branch decay study and log study, that's like a ten-year duration. I followed these elk exclosures for 15 years. And I've worked with Steve Acker and Sarah Greene and Art McKee on long-term vegetation measurements. All these long-term studies, I love them. But it's to the point where I can't do any short-term studies anymore. It's hard for me to do. I don't have the time. The commitment is overwhelming. Now I admit, I've gone overboard. And what I'm trying to do as I move through is try and turn those things over to graduate students or other people, so I can get some flexibility.

Geier: What are your priorities for teaching or communicating the results of your work? You talk quite a bit about graduate students, and my understanding was that for quite a while here

you were on a courtesy appointment here at OSU, but you integrate a lot of the work you've done with the graduate program.

Harmon: Yeah, several things went on. For years, I had money allocated for graduate students, and the budgets would be cut by about that amount. I don't know if it was deliberate, but it was impossible for me to get graduate students. So, the first priority actually was to get technician support. Jay Sexton was a technician who's worked on the log study for probably to seven to eight years, something like that. So, my priority was to get funding for him. Then, with that being secured, I had some success, and then, I started looking into students. The first student I had was Chen Hua, who's from China, and I got involved with him on an exchange, an LTER-related exchange, sponsored by the Man and Biosphere program, but on wood decay. He was doing a project like that, I helped advise him, then he came to the US and got some training, he went back to China, and came back. He was the first one. I knew Chen real well, and we were very interested in doing some kind of project. So, we got into this root decay thing. Then, I got funding for another student on a grant. I was amazed. Finally, I had funding for two students. Then I started to realize that for me to advance, I need to have students and involve students more with the research I'm doing. I'm just doing too many things. There was a lot of stuff I was missing by just having technicians. The education element wasn't really there that much.

One of the problems I had early on when I was promoted to assistant professor, was they failed to tell me at OSU I had to apply to be on the graduate faculty. These are six-year academic appointments, so I had a three-year, mid-term review, and they asked me why I didn't have any students. I didn't know, because I was amazed no students had shown up. Well, I wasn't on their list, so no one knew I was available. Right after that, I started to get a lot of students, and I'd be on committees as grad rep or substituting for people who had dropped off. I got involved in a whole bunch of committees and started realizing I needed to get more involved with the students. That they were bringing in a dimension that technicians didn't bring.

Geier: How would you characterize that?

Harmon: The technicians are professionals, and they're paid to know the answers. I think up the absurd things we have to do, and sometimes we think them up together. But they really translate them into doable projects. I help them. I've always believed I should be out there helping, especially on big projects, in setting them up. So a lot of times I'll work with them as an extra hand. Nonetheless, they sort of bring that professional dimension and experience, as they usually have a lot of experience. The graduate students don't bring much experience, but they bring a lot of energy, and they bring a focus to specific questions that neither I or the technicians do. We're stretched thin maintaining things. But, we're maintaining lots of things. And the students can bring a focus that neither I nor the technicians can bring. I try to integrate, and I do integrate the technicians in with the students. They're introduced. The technicians provide guidance, and sometimes manpower, and extra hands, but they're not a substitute for the graduate students. I mean, the graduate students have to take command of their project. So initially, with Chen Hua, we probably did a little bit more than we should have. When we saw him floundering, we just stepped in and did things. But we're kind of working

out a system now, where we can let students twist in the wind just long enough to learn something. If it's really going south, we'll come in and save the day. But we try and let them go as far as they can.

Geier: The shift to using graduate students sounds like it's fairly recent?

Harmon: It is. I wasn't a player at all in the beginning and I became pretty key in setting up an experiment. Then I sort of played a key advisor role, and it increased through time. At the end of LTER 3, Fred was relying on me a lot for advice, sort of as a sounding board. There were other people helping too, though. I'm not the only one. I was becoming more important in that way. Actually, as we wrote LTER 4, I played a major role in taking lessons of LTER 3 and applying them to that proposal and how it was structured. Fred had been at the helm for a long time. So, the plan would be eventually, that I would be the LTER main PI, conditioned on several things. But I realized that I really need more technician support and a good set of students to carry out my work, because I would soon not be able to do it. It's like I don't know how I'm gonna do, but I do know this. It's like the distance between setting up a study, the technical part, and the science part. If you're gonna do the technical set-up, you have to do 100 percent, or 99 percent, something like that. A large proportion. It's not a trivial exercise. If you're gonna be the scientist, you've gotta do that. And if you're going to be trying to integrate these people, you can't be trying to do your science. You can do some of it, but really, it's a full-time job. You can't kind of half do that job. If you're gonna do it, you have to do the job.

Geier: Do you see yourself now moving more towards -- ? [LTER context/HJA-PI work]

Harmon: Well, I'm hoping to be sort of a cheerleader and a supporter, like Fred has been. You know, Fred really did get a lot of money in LTER during his tenure to do his stuff on disturbance. He got a lot of that through the Forest Service. But he put an incredible amount into guiding research and giving direction and focus, and getting the group to try and come into consensus. That takes a lot of work. Both figuring out what the consensus might be, so he could lead, but also, pushing the group when they need to be pushed. The main hang-up now is I'm still all soft money, and I can't do that job in this position. I have to have support, because I'll have to drop the thing that keeps me going on soft money. I can't be a mediocre scientist and a stellar leader. The science funding will dry up. That won't work. So more and more, I've realized that this transition is coming. Also, as I got promoted from assistant to associate professor, I realized that the things I need to do are different. The expectations are different. It's not just enough to be a slam bang scientist doing lots of research. You're expected to do more, provide more leadership.

Geier: And you've become more student-focused, too?

Harmon: More student-focused, the time for having an actual course. I taught small courses and honors courses for students, but a real course. A real curriculum, a contribution, but can't do it on soft money. So, there's lots of things I cannot do. My peers expect me to be able to go on review proposals. I've done some of that. But I can't afford six weeks to do that.

Geier: No. Your list is long-term projects. It's pretty mind-boggling to think of the time

commitment involved there.

Harmon: There's a lot. In fact, I've paid a price for that. I've had problems mentally trying to hang on, and find a different way to do it. I've sorta come from the mold of you just push harder, you just keep pushing harder. Well, I found out you can only get so far, and then you need to think about another way. It doesn't serve you forever as a way of doing it.

Geier: So, your concept is to mentor people like Chen Hua along to pick up some of this science work to give time for more?

Harmon: Yeah. And I had one post-doc, too. I have a whole bunch of studies going in Russia, but that's not all my work. I'm doing other projects too.

Geier: Yeah. Sure.

Harmon: The idea is to find people to pick up the work at the post-doc level, eventually. So, maybe Chen Hua or someone else will do that for the decay studies. I've already got someone working on stuff we do in Russia, who kind is in charge of those now and he runs those studies.

Geier: You were talking earlier about applied research and basic plans for your work. Could you talk just a little bit about who you consider your most important audience, or audiences, and how important it is for scientists to convey knowledge?

Harmon: Well, there's a whole bunch there. I think our primary audience is scientists. We should be preparing for peer-reviewed, very-high quality science that will have an impact on how we think about the world, basically is the message there. I think, ultimately, that does trickle out in how the educated public is concerned about and thinks about the world. You can look at the issues that are brought out now about preservation of old growth, and really a lot of those issues had their origins in the IBP days. Discoveries about what's going on an actual old-growth forest. That's kind of a slow process, but it's not the only kind of communication that's important with the peers. I'm a very strong believer in what we call gray literature. There are some things that we owe in terms of documenting our science and methodologies and databases that we have to do. It's not just getting the cream of the cream, but we've gotta have some milk in that pitcher, too. I think the role in terms of the public is important, although I haven't spent a lot of time on that. We mostly do it through news contacts, so they can help us translate. It's sort of depending on when issues are hot or not. Right now, there's a lot of stuff on carbon sequestration and greenhouse gases for the upcoming summit. I'm getting called a lot about that recently.

Geier: Do you have journalists who kind of keep in regular contact with you?

Harmon: No. It's been a different crop each time. On that front, glad to provide information. We've had a couple press releases. Frankly, we've not had good luck through OSU getting our message out. Hasn't been good luck. We've done better on our own with reporters. Fred has good contacts and there are some people, but largely, we have to touch bases every time on issues we cycled, in the five years or whatever it takes to recycle. I've done things with school

kids, I don't know if I have this year. I did a thing called the Magic Schoolbus. I didn't do it, but I was a consultant on it. Basically, it tells kids about decay, which is funny. It actually tells some things about wildlife management, wood, and ecosystems.

Geier: That was incorporated into the program?

Harmon: They knew about me, asked if I'd help, and I said yes. So there's a series called the Magic Schoolbus on PBS. And then, there's one on the Rod Squad. That's the one I helped on. And they had a book from that too. Actually, that was developed from the video series. I wasn't directly involved in the book, but a lot of ideas and guidance were there in the book nonetheless.

Geier: Who were they?

Harmon: Scholastic puts it out.

Geier: Okay.

Harmon: I'll bring one in for you to see. It's pretty funny. There have been other cases that haven't worked. There was this thing called Microcosmos or something, which got in touch with me but never really went anywhere. Some of that's for specific kids. And *National Geographic*, we'll be asked to provide photos, or mostly captions to photos, actually they get their own photos, better than we can take. I've done that for the canopy crane and there was a description of old-growth forests in the Northwest, and a drawing of a log and I had to check out whether they had it right. Well, for example, in the Magic Schoolbus, they had mosses growing inside a log with no light. I had to point out that these were photosynthetic plants so they needed some photons. They said, "Oh, we thought it'd look nice with moss artistically, but....." So, I think it's real important, but I had to balance it with my needs of survival in the soft money arena. So, I can't do all of that. But, when it's not gonna be a huge involvement, sure I get involved.

Geier: Do you think the Andrews group is typical or atypical in doing those kinds of things?

Harmon: Well, I don't know. I have a sense we're atypical, but I could be wrong. Let's say I haven't done a lot of surveying on my own. I think the LTERs are pretty involved in that kind of thing in general. I know the Palmer Station had an interactive web-page, live feed from Antarctic, where you could ask Francis questions. I think we might do some of that in the future, things that don't cost us a lot in time, but would get us interaction with the public more, also in terms of policy sometimes. I just wrote a letter to a Congressional committee on resources about a proposed thing, on carbon stores and advising them, in fact, there were some real problems with what they had there. They had to really think about what was going on here. I've also done it by publishing articles. The best story I'd give you is the Forest Service is supposed to clear up old growth to store more carbon. The idea being, to take up carbon from the atmosphere that the old forests weren't growing and the new forests were. So logically, get rid of those non-growing forests and get growing forests. But, we knew from our research that there were some problems with that perspective. In fact, major problems, as replacing old forest with young forest was going to release carbon into the atmosphere. So,

Jerry Franklin and I and Bill Ferrell wrote a little article to *Science*. We had a press release come out with it. Within days two things happened: the Forest Service disavowed any knowledge of this proposed program, and they were searching for ways to get me fired. But I didn't work for them. And senators, I think Barbara Mikulski [cited in footnotes in the publication, they were Senators R.H. Murkowski and T. Stevens, R-AK], was one of them who had made lots of statements about this, suddenly said, "Oh, I never said that. No, no, no." So that really short-circuited that whole program, just by publishing the paper. We did advise them earlier and that was a mistake, and they were not listening. So, by putting it out in *Science* and having all these papers cover, it got covered in Arabia, and everywhere. People were sending me clippings from all over the world.

Geier: Do you have a copy of that?

Harmon: 1990. Yeah, I can give you a copy of the paper.

Geier: Yeah, I'd like to get a copy of that.

Harmon: It's interesting. I think it got published in Japan. I think the people who proposed this were really embarrassed that they hadn't listened.

Geier: Huh.

Harmon: Unfortunately, with the high leadership in the Forest Service, that has been true a lot. Maybe until recently.

Geier: That reminds me a lot of the reaction to a lot of the work.

Harmon: He probably told people who were managing that this is a concern, and they just lit up. That's what we got.

Geier: Do your kids ever go up to your site?

Harmon: I have one child, and yeah, he went up. He knows all about it. He's always giving me grief, "Oh Dad, you're just some deadwood nut." We joke about it. He's helped me set up research. When we first started, we were surveying these logs. He's getting a little better, but we were surveying where the logs were and were putting these posts in. He'd go around as a toddler, he'd yank the posts out and he'd throw them. They were brightly colored. You'd have someone tapping at your leg and he'd have the post that you'd just spent an hour surveying in.

Geier: (Chuckle).

Harmon: But now, he helps out. We went on a trip to do a study in Wyoming and he helped me with that. The other day we were doing studies on termites, because they release methane. So, this tree was getting eaten by termites, so let's see what they do. How much do they "fart" out in methane? That interested him. He chopped it up and brought them into the lab and measured it.

Geier: How old is he again?

Harmon: He's ten.

Geier: Okay.

Harmon: So, I'll probably be getting more involved in the school because he's getting old enough that the kids' science actually means something to him, and he's interested by it.

Geier: That's Russell?

Harmon: Yeah, Russell. So, especially if I wasn't going back to this hunting for salary all the time, I'd probably do more of that, actually. Maybe we'd set some things up on the web for students. That'd be kind of fun I think. Interesting fun facts about forests.

Geier: You'd need to get funding from somewhere besides NSF to do that?

Harmon: Maybe, maybe not. Or, we'd just do it. The funny thing about the web is, in relative terms, it's pretty cheap. You don't have to print anything, and not only that, but in terms of reviewing and stuff. It's not quite as rigorous. There's not the time commitment. Doesn't have to be as polished. In fact, you know, kind of outlining stuff is easier to access than sheets of print. So, there's something that you can do that isn't that expensive. I don't know. We'll have to think about it. We wouldn't turn down money. But, I wouldn't want that hanging us up, because I figure that's a pretty important connection.

Geier: I am curious if Russell had an impact on your decision to do the Magic Schoolbus?

Harmon: Oh sure. I knew that would be a gas for him. I probably would have done it anyway because it was pretty fun. But, actually what happened was, I read those kinds of books to him, the originals that were excellent. There was the trip to the water works, and to the center of the earth, and to the ocean, just excellent. I think this series, the new one is not quite as good. But I knew how good they were and how much they influenced his thinking about science. It wasn't so much Russell, per se, but he exposed me to that aspect. And I kind of could see how he reacted. So, I knew it was a good investment.

Geier: Well, I think we'll call it quits.

End of Interview.