

Interview with Art McKee, by Max Geier, September 12, 1996, Corvallis, Oregon.

A native of New England where he did undergraduate schooling, Art McKee pursued graduate study at University of Georgia before heading west to OSU and the Andrews in the early 1970s. He expected to continue his PhD program, but found a career as Forest Director guiding development of the headquarters facilities, other infrastructure on the forest, and various elements of the research program, especially riparian systems. At critical times he teamed up with Jerry Franklin to secure NSF funding for facilities development. Broadly eclectic in his ecological knowledge, he had important roles in framing LTER proposals and other big-picture endeavors.

Art McKee: There were things I might have worded differently that I felt was probably not worth bringing up because of the sort of political license you ought to be able to enjoy. Things like that. I don't see anything that I felt any heartburn about. Let's put it that way. I might have used a different term, but I had no strong basis for a word that you and Fred [Swanson] had used or said, that was inappropriate. Just one I wouldn't have chosen, things like that. [Art and Max were discussing a draft outline for Max's research/book project that became *Necessary Work*, a history of the Andrews Forest.]

Max Geier: Okay. You know, if anything occurs to you want to change, this would be a good time for me to get some feedback, especially on the chapter organization.

McKee: Why don't I give you just a couple of comments on that, because I thought my memory last weekend [was imperfect], and this is unfair to you actually, and I should remember it [the outline] that long, but I have difficulty with some of the details.

Geier: I've got a copy of it in here somewhere, if it would help. This is it. It's stuck together.

McKee: Nothing major, it would be in --

Geier: Now, what I was trying to do is layer some history together instead of trying to deal with an entire period in one --

McKee: Right.

Geier: -- chapter.

McKee: Let's say, the policy concerns --

Geier: Uh-huh.

McKee: -- Really didn't emerge until, maybe the mid-eighties. There was an awful lot of research going on in the seventies, '70 to '80, that put us in a sometimes awkward position of being the only people around having the information for whatever environmental concerns that emerged in the mid-to-late '80s. Did not quite know whether that was what you were after here.

Geier: One thing I was really getting into with the data, especially, was the beginning of these kinds of environmental concerns.

McKee: Concerns?

Geier: Public concerns about the sort of pressures that come on the federal agency, and how that translates in questions that were posed.

McKee: Jerry Franklin might tell you that the kinds of research we were doing during the Coniferous Forest Biome [International Biological Program] years in the early '70s,

Geier: Okay.

McKee: This was in response to the wave of environmentalism that was sweeping the country, the first Earth Day and that sort of thing. I came out here from graduate school into what I thought was kind of a Neanderthal environmental ethic. That they weren't sensitive to that, except they were, they were sympathetic, but not sensitive? They were listening to it and it resonated, but they weren't actively engaged. And their research was being conducted pretty much from the idea that we need to know how the system works in order to wisely manage it. That's a perfectly legitimate thing to come from, but my perception was, it wasn't reacting to environmental things. Jerry occasionally seems to take credit for having done a lot of research in old growth. Well, that's about being well positioned when the old-growth issue came out. To me, that was just circumstantial. We went out to one research site to conduct research, focused on that one research site, which happened to have an old-growth stand. The reason we were there wasn't because it was an old-growth site, but because the previous scientist, Dick Fredriksen, had invested a lot of energy and time in describing these two watersheds which were being set up for manipulation [Watersheds 9 and 10 at H.J. Andrews], so we could do energy flow and nutrient cycling on those watersheds. The stage was set and he had chosen them because of their ease of access, not because of what kind of vegetation they have. A lot of the work we did at the Andrews Forest did in fact study the processes in old-growth forests. That was part of it. It wasn't all the planning that was done. I did so much in old growth, because so much of the Andrews was old growth. The Andrews was our study site.

On a proportional basis, you look at those budgets in the early '70's, and you say, "Okay, this amount of money was going for research in old-growth patches. And this amount of money for research in plantations or young stands." It wouldn't surprise me a bit if

you saw, well, I know you'd see a disproportionately large amount in the young stands. And it may not be the majority of it, but only 1/3 of the Andrews is young stands, maybe 45 or 50 percent of the budget was going to research in young stands. And yet we're colored a group that studied old growth because we did study old growth. It was there. It was part of it. And nobody else was bothering to, in forestry circles, because this was the stuff that was to be cut, converted to thrifty young stands. I'm not going to make a big issue of it, because we did end up in the forefront of a lot of the old-growth issues by virtue of having done what we did. But it wasn't of any clairvoyance that this was going to be much of an issue. I really get heartburn when I hear that suggested. I really do.

Geier: I liked your point about access for those study sites. When you say access, what do you mean?

McKee: You can get to it in the wintertime.

Geier: Okay. Was that because of the road system?

McKee: If they were next to a road structure. Next to existing roads.

Geier: Yeah?

McKee: And they weren't even on the Andrews. Dick Fredriksen picked these because the lowest elevation sites were Watersheds 9 and Watersheds 10.

Geier: Uh-huh.

McKee: And they're right along this main curve there or here [pointing to a map]. So, they were just short. This one's right on the main road. This one's just off it 100 yards. And that's why they were picked. You could get to them. Nothing is plowed in the wintertime, and this usually had logging traffic and so it was pounded flat or kept down.

Geier: Yeah.

McKee: And we went past Watersheds 1, 2, and 3, in the Andrews, as you had to go way the hell up into here, or here [pointing to a map], before you could get to an undisturbed watershed. The creation of the road system and logging units have been put in without prolonged consideration for future unit watershed studies. You can take just about any one of these units, like this one right here, flops-over a ridge, this one, no this isn't a watershed. This one flops-over a ridge instead of being within the boundaries of our watershed. Every time they put in a unit, it seems like they picked a place...here's one that flops-over a ridge, this one, no this one doesn't, this one does. In part, it was because the kind of logging system that they used. You wanted a ridge on which to plant the landing system. They went on either side of it because it was more efficient. In the process, they bugged all the bases up to it. Other than the ones that

had already been established. Then these roads were established. And there's some opportunities up in here still. In the late '70s, when we went through the exercise of assessing how many undisturbed watersheds that are second-order or larger left on the Andrews Forest, we came up with four, out of roughly forty.

Geier: Hmm. 10 percent.

McKee: Yeah, 10 percent.

Geier: Sites that Dick Fredriksen chose, established there near the road, would be on National Forest land, and so their survival would depend a great deal on land managers?

McKee: Special use permit. And that was no problem. We've rarely, rarely run up against hassles going off the forest. In fact, I'm carrying for Stan Gregory's group, a special use permit in my name for a site that they may never go back to, but we did a bunch of work on in the early '80s for about three years. This particular clear-cut got a special-use permit to do some work, and I just feel they might want to go back in once the canopy in there closes. So, I end up keeping that one active under my own name. But, it's really easy. The Willamette's been extremely cooperative in contrast to some of the conditions that exist in other places I've seen around the nation, where the research community and management community are really at odds with each other. Usually, they treat each other with benign neglect.

Geier: It's not something I want to get into here, but let me back up here a little bit and get started. I want to talk a little bit about when you started to work at the Andrews in '70, '71. Is that right?

McKee: Yeah.

Geier: You mentioned earlier that you had the intent of getting two years of Northwest experience, and then going back to New England. Would you set the framework a little bit? What was your academic and personal background priority at the time you made that decision to come out here?

McKee: Prior to coming out here, I went to the University of Vermont and the University of Maine. Worked for two years at Brookhaven National Laboratory as a research associate there. I went there to work on a nutrient-cycling study, and when I got there, their pesticide study person had taken a job teaching high school. I ended up working on pesticide accumulation in food chains. Quite different from my plant physiology and nutrient-cycling background that I had been trained in, but it was fun. I had a great time.

Geier: Yeah.

McKee: Among other things, it's so hard today to get a permit to collect wild animals. Even to collect, let's say, a half a dozen herring gulls right now, you might run a year-long gauntlet to get permission.

Geier: Hmm.

McKee: I was given carte blanche to collect anything on the east coast. I had a permit from the Boston office of the Fish and Wildlife Service [U.S.] that authorized me to collect fish and game in abundance of numbers appropriate for purposes of a study on accumulation of pesticides.

Geier: Hmm.

McKee: It was crazy in some respects, so we capitalized on natural mortality. We were collecting all kinds of stuff.

Geier: Who were you working with back there?

McKee: A fellow named George Woodwell. He was a famous ecologist who's done a lot of different things. At any rate, I left there and went to graduate school at the University of Georgia. When I left the University of Georgia, I was kind of shopping around in shotgun fashion for anything that was available. I was in a pretty tight financial bind, and was pretty eager to get the first thing that was going to provide me a paycheck. I had this offer in Boise, Idaho. The guy kept telling me he'd hire me, hire me, hire me, every time I'd talk to him, but the paperwork had been lost one place or another. Then I got the job offer here, and it sounded really good. They basically said they had this set of tasks they'd like you to do, and asked, what are your interests? And what would you like to do in addition? We could always fit these areas of interest, and so, come on up.

Geier: Who was the person that was doing the hiring then?

McKee: Jerry Franklin and Dick Warring were the two in-charge at that time. They were the two local co-investigators. The original grant was to the University of Washington and Oregon State University. A fellow named Stan Gessel was the principal investigator at the University of Washington, along with someone else's name I can't remember. Dick Waring and Jerry Franklin were the co-investigators here at this campus. I don't know the exact history, but I had sent out letters to at least three of the people here on this campus, saying I admire your research, here are copies of my work in certain areas, and that I am now interested in seeking employment. Do you know of any niches? I'd really appreciate hearing from you. It was a different hiring world that what's out there today, that's for sure. There's not a snowball's chance in hell of that happening now. Basically, they made up their mind to hire me before Dick Fredriksen happened to be on

campus for other purposes. They told him to look me up, and see whether or not this was somebody that looked as good in reality as he did on paper.

Geier: That was what you were talking about earlier?

McKee: So, he staggers up to me in the bar and introduces himself. It was very funny.

Geier: Who had you been working with in Georgia?

McKee: My major professor was a fellow named Philip Johnson, who I had worked with in the Arctic a year earlier. He was a plant physiologist, and they called him “Mr. Philip Scientist.”

Geier: You worked with him in the Arctic?

McKee: I worked with him in the Arctic. He and I were very, very good friends. And he was a wonderful mentor and a good teacher, one of those people who just virtually turns every situation into a learning experience for people around him. And doing it without being a horse’s ass about it. Fun to be with. High-energy guy. Among the members of the committee are two people in particular had national reputations – Karl Malk and Dick Wiegert, who has worked in thermal ecosystems and done a lot of population and nutrient cycling monitoring. And a fellow named Frank Golley, who was one of the few early people to look at energetics in terrestrial ecosystems. Another member of the committee was a fellow named Karl Malk, who was very, very creative in many ways, probably the best scientist of the four. He had a happy addiction to golf (laughter), as he’d really rather be out on the links than sitting down writing. I got a lot out of those four people in particular. Eugene Odum was there. He was interesting to be with once in a while. There were people like Dak Crossley, Lynn Swain, and others who had national reputations who would go outside their interests; frankly, good thinkers, clear thinkers.

Geier: Was that what attracted you to Georgia?

McKee: Actually, it’s quirky, and kind of embarrassing. I was accepted at three institutions: University of Wisconsin, to work with Jim Boyle, a soil scientist now in the faculty at the [OSU] College of Forestry. He was department head of Forest Resources for many years. I liked the idea a lot of working with Jim on forest soils. At the time, he was working on the idea of plants being able to create special microhabitats around their roots, in which a lot of other things happen. That was interesting stuff. And that one had a big, fat, juicy fellowship. I had a good National Science Foundation Fellowship anyway, but that one was much better. But, my wife at the time, didn’t want to live in a cold climate. So, we thought that maybe Wisconsin was a great opportunity, but it wasn’t something she was excited about. My other opportunity was at Yale. I had been accepted with another soil scientist by the name of Grant Boyd. He was a brilliant

scholar and a wonderful person. But my sister lived nearby. She and my folks were having problems at the time, and it was getting ugly enough that I felt that if I was in that proximity, I was going to get sucked into it. I just didn't want any complications. Keep it simple.

Geier: Hmm.

McKee: It was a strange one, because I wrote what I thought were very matter-of-fact letters for both Wisconsin and Yale, saying that I'd chosen to go to Georgia. Jim Boyle was very diplomatic. He called and said, "Well, you got to think about your future, your diploma. Georgia didn't have as much marketability as a diploma from Wisconsin." Boyle was also a student of this Grant Boyd, who said, "If I were you, I'd go to Yale. That's where I'd go. Because there's no place with a better, more marketable degree." He said, "It's your decision. I'm not trying to pressure you. Just wanted to give you some fatherly advice." I appreciated that. I thought about and agonized over this. Francois Mergen, the Dean of the College [Forestry] at Yale at the time, and man, he got on the phone, and was vitriolic. Oh shit. Didn't I know what the fuck of a big mistake I was making? He had this heavy French accent and wasn't exactly saying it that way, but that was the essence of it. (Laughter) He was going to give me 24 hours to reconsider, and he wasn't giving that to many people, but was going to make an exception in my case, because I was George Woodwell's research associate, and George was on their faculty, so, he was going to cut me a break. But don't fuck up again! (Laughter) So, I went and sat down with George, told him the situation. He said, "You should go to Yale. But, I can understand why you don't want to." And they were right, of course. They were absolutely right. (Laughter) But, I think that probably, for my own mental health, I did the right thing. You've probably heard a similar kind of thing, how grad school decisions have been made, and you get knocked all around from point A to point B.

Geier: You don't have enough time for mine. I went to eight different schools. (Laughter)

McKee: Working at Brookhaven with George had given me the opportunity to spend a lot of time at Yale, using the library resources there. I had realized that was a wonderful intellectual climate it really was. And it wasn't a case where I didn't know what that environment was. I certainly did, so I knew what I was walking away from.

Geier: It's kind of interesting that Wisconsin, when you're from Vermont, and the winters in Wisconsin can't be any worse than they are in Vermont.

McKee: I don't know, it was a quirky thing. We didn't gel. We were married for about 10 years, and I think probably both of us knew by the end of the second year this was a big mistake. But we tried to make it work.

Geier: Hmmm.

McKee: Wisconsin at that time had the Wisconsin Alumni Research Fund. They still do. It doesn't make as much money as it did then. Moneys for that came from the rat poison WARF room, which is the acronym for Wisconsin Alumni Research Fund. Scientists there produced rat poison, and all the moneys from the sale of that, well, not all, went to supply dollars to graduate students. I mean, they were high rollers. That was a fat one.

Geier: So, your background in pesticide studies would be attractive, I suppose, there?

McKee: Oh, I think that they were – (laughing). It was awarded on a competitive basis to promising grad students, regardless of discipline in the department.

Geier: Yeah.

McKee: I attribute a lot of my success in that competition to Jim Boyle. I was really fortunate to have worked with and be associated with good people, with lots of admirable qualities and considerable integrity.

Geier: Did you have long-term career goals at that point? Where you wanting to be in like 10 years?

McKee: Yep! Sure did. They're funny when I look back at them today. I wanted to be, if not an Ivy League school tenure-track position, or at least a school where I could move into a tenure-track position. Somewhere close to the family, where my roots were.

Geier: So, you were looking pretty strongly along academic lines then?

McKee: Yes. I felt that that's where I wanted to be.

Geier: Hmm.

McKee: And the atmosphere there at Brookhaven was definitely one of fostering that. I felt real comfortable there.

Geier: Hmm.

McKee: They had a good library. It was a place to really be in an ivory tower, an intellectually-committed star, if you wanted it to be. It was a great environment for that.

Geier: Any tradition of that in your family?

McKee: Not really. My interest in forestry issues was because my mother's side of the family had nearly lost a fortune in lumber, that ancestry. My dad's side of the family was largely dairy farmers and cabinet-makers, and craftsmen. And, good dairy farmers. But there was in both sides of the family, a deep respect for scholarship and education. My grandmother McKee, who probably never even looked at anything resembling a high school, was an avid reader who would just explore at home, so that was her inquiry. On the other side of my family there were several people who worked in research libraries, and so, there was that spin on it too. Second cousin of mine.

Geier: So, your plans got a little bit sidetracked when you came out?

McKee: Came out here and discovered all this public land. I had been involved in group research a little bit at Brookhaven because George Ruhl collaborated with a lot of people, and was involved in projects. Many projects that had 2 and 3 collaborators, you know, 2 or 3 different people, and he had his hands active in a lot of different projects, and as people worked with him. I similarly engaged in a variety of things. My last year there, I was looking at pesticides, studying succession in old fields, and looking at nutrient-cycling in environmentally-stressed situations. Pesticides, radiation stress, thermal stress, possibly. That was typical of the way George worked. So, I got through grad school and landed here. Here's this multidisciplinary program, and a couple of dozen different people. It was like being a kid in a candy store. There were all kinds of neat things going on. And the people here, oh, there were moments when things didn't work together. But basically, people worked together extremely well. So, when I travelled around and tried to find other places that are like this, in pieces of geography where I thought I might like living, Colorado Rockies, New England, I couldn't find it.

Geier: Hmm.

McKee: So, it's an unusual environment in the way people took time to listen to their colleagues in other disciplines, and seemed to respect their boundaries, but not be afraid to offer suggestions in their area, too. It was just a very healthy intellectual environment. A real good one. I liked it. And I liked the Northwest. I mean I liked the Willamette Valley winters, but the rest of it, I didn't get.

Geier: What was your first impression when you came here? What about the place struck you?

McKee: Ah, well -

Geier: The Andrews in particular, but the region in general.

McKee: I drove the southern route, and there was heavy snow from central Nevada all the way north, and I didn't see a tree very clearly until the crest of the Willamette Pass, and for a balance of halfway up the pass down to Oakridge, I was driving through blinding snow. This was 10-miles-an-hour kind of driving. When the snow stopped, I

looked up at these old-growth Douglas-fir. That corridor used to be lined with beautiful Douglas-fir. I just pulled over and I went [gasping], “Do you believe this?” I had sons at the time that were like 7 and 4, no, they were 6 and 3, and they were asleep. I woke them up, saying, “Come on out,” I was so jazzed by these huge trees. And they were all laden with snow. It was just absolutely gorgeous. I said, “We’re going to be around things like this. This is great.” That was my first impression. My other travels through the Northwest had been touch-and-go; Seattle airport on my way to Alaska. And the Alaska forests are majestic and beautiful, and that wilderness is incomparable.

Geier: Hmm.

McKee: But this was a wonderful, phew, revelation! It’s like being blindfolded, and then suddenly having to take the blindfold off. Then, an hour later, or more, a little more than that, driving into Blue River, which was where I was supposed to live when I first came out here. I had been given directions, but because of the snow, I was running about 8 hours late. We got there at about 11 o’clock [pm] instead of the middle of the afternoon. I called, and the guy said, “Instead of going to the office to meet, why don’t you just come by my house.” He gave me directions to his house. But I drove past the office, and here’s this little shit-hole of an office that was the old ranger station at the time, and I went, “Uhhhh. (Laughing) Whoa, this is really minimal. This is really, really minimal.” Let’s see, we’ve got a program with roughly two dozen people, I had been given a copy of the proposal in the mail. I knew the scope of the program. I just thought, no way it could operate out of what was there. And so, I was thinking mobile home, these modular homes, but try to plan a research facility in that 50-minute drive from the office to where this guy lived. The first thing I said when I got out was, “Well, I drove past the offices. Not much, is it?” He said, “No...no...” With the scope of the program what it is, we’ve got to start thinking about how were going to accommodate these people. That was a big program, big ideas, and zero facilities to work out of. That was in the first hour of my being in the McKenzie Valley.

Geier: What were your responsibilities at that point?

McKee: I was hired as a scientist in the field to try and coordinate the research activity.

Geier: Hmmm.

McKee: But also do a lot of hands-on science. Most properly it would be described as sort of a “super-technician” position, where people already had these ideas, and I was to organize myself and other technicians to collect the numbers. As of that point, I had no intellectual investment in the program. Because of the variety of tasks, and initially, the relatively menial nature of many of those tasks, it would be glossing it to call it a post-doctoral position. But this wonderful group, within a month, a day-and-a-half or two days a week, were basically mine, to work on my own and on what I wanted to work on.

Geier: Hmm.

McKee: Very quickly it became more professional, but initially, it was organizing myself and other technicians to collect a lot of field data.

Geier: So, facilities were an immediate concern then?

McKee: Oh, yeah. I knew we weren't going to be able to mount the kind of program they were looking for without initial facilities. So, we bought one trailer one year, one trailer the next, put up a warehouse to house things at the site, and got a couple of camper trailers on extended rental for people to work out of in the field. Bought a shit-load of camping gear so that people could work out of tents. Designated a campground there on the forest. Pissed everybody off with that.

Geier: Just because they wanted to camp other places?

McKee: No, the Forest Service woke up one day and suddenly there was this *de facto* camp ground on the experimental forest.

Geier: Oh.

McKee: So, we had port-a-potties brought in. We'd nailed a couple of planks between some wind-throw trees and put up a toilet seat, and people sat on that and crapped into the big hole into the blow-down between the trees. It was very informal. (Laughter) People loved the openness, actually, and most of them loved it a lot. Had some very creative kitchens, two in particular that capitalized on these wind-throws. The root-wad of the wind-throws made shelves for things to be stacked on.

Geier: Okay.

McKee: And there were tarps spread out. It was primitive. There was a lot of logistical stuff early on.

Geier: What kinds of projects were you involved in? You were doing some research yourself?

McKee: The one I focused on was mapping permanent vegetation plots, so we could track changes in vegetation through time. There were successional studies of vegetation with these, but the generic aspect of plots had several different aspects to them. Looking at the timing of different kinds of biological events in different species of plants, timing of leaf-out, timing of flowering, timing of fruit sap and fruit dispersal, and leaf fall, under a general topic called phenology. P-h-e-n-o-l-o-g-y. Then, I was involved in the nutrient cycling aspects on these watersheds, and moisture-use and some moisture draw-down patterns. There were timing and transpiration from these forests

to measure. Then wherever else needed help. I mean, that was the other “duties-as-assigned kind-of-category.” One week, I might be doing some surveying, as they discovered I had a background in land-surveying, so I found myself heading a surveying crew.

Geier: Hmm.

McKee: Umm.

Geier: You mentioned that Ted Dyrness and Jerry Franklin had brought you along and brought you up to speed when you got there?

McKee: Ted, Jerry, and I, early on, went out for a walk in the forest. Actually, we were picking a site to put in some plots. We had two or three very fine days this one particular week, early in my tenure out here. Those two seemed to have a very positive, very, very healthy professional relationship, at the time. Jerry was a plant ecologist, and also a good soil scientist. Ted was a soil scientist, and also a good plant ecologist. They came at it from slightly different disciplines, if you will, from two different directions. Both were extremely well-read, solid, and Jerry was quick to speculate, while Ted is much more cautious. You can imagine how that kind of difference in personality might play out in the field with a third-party present that’s willing to kind of “goose” one or the other, to get that interaction going, interaction running. So, it was a lot of fun to say, “Jerry thinks this is the case,” and, “Ted, what do you think?” Ted would be so reluctant to speculate. He’d say, “We don’t have enough information to say, but ...,” and he’d be forced into it. Jerry would react to Ted’s conservatism. It was a lot of fun.

But they also had worked out here a lot, and it was a crash-course in northwestern forest ecosystems. It was great. And it ended up with Jerry getting us royally-stuck in a mud hole on a road way up in the woods. We had to walk out, with Ted pissing and moaning about the fact that Jerry got us stuck. (Laughter) Let’s see, it was a fun day. We got a ride quickly, and got down to get a truck that could pull us out. There was almost something symbolic about the fact Jerry was extending himself so much, trying to get to a place where I had been told that morning that the road wasn’t open because of snow, just another 1/2 mile beyond where Jerry got stuck. We’d been told that we should be wary of this one spot where the hillside had slid off and had this huge puddle. Jerry just wanted to test it anyway. (Laughter) Man, we were in so deep. Couldn’t open the doors on one side, we were in so deep. It was a good time. (Laughter) They were good people to work with and for. One of the wonderful things about Jerry’s personality is that he makes everybody feel welcome and important. I mean, everybody’s making a significant contribution. Jerry’s a wonderful cheerleader, you know, “It’s good to hear that.”

Geier: You were familiar with his work before that, but had you met him before?

McKee: I was very impressed by this thing we talked about. I had seen an early draft of this book that came out in paperback in 1970 [*Natural Vegetation of Oregon and Washington*, Franklin & Dyrness]. This was revised a couple of years later. I see that my earlier version is gone. There is not another place in the country that has anything like this, and these guys bootlegged this, did it, despite their boss. Ted mentioned he had supervisors that were supportive of that, if it didn't suck up too much time. I was familiar with Ted's work more than Jerry's. Jerry had not, at that point, published much. Jerry's work was pretty "fluffy" and unfocused, and Ted, in contrast, had done really significant soils and vegetation work that was very state-of-the-art quality. Really topnotch.

Geier: Hmm.

McKee: Ted was one of the people I had written to, Jerry too. I was aware of him too.

Geier: Uh-huh.

McKee: Because of that particular work. So yeah, I was impressed by their work.

Geier: What was it that put yourself back in that time frame, as of 1973? What was your perception of what an experimental forest should be for, or should be doing, at that time?

McKee: I don't think I can do that very honestly. It would be really-colored. When I came out here, I had no intention of being here 25 years.

Geier: Yeah.

McKee: I viewed, and still do view, the whole landscape as a place to be researched. If that turns out to be any place, the landscape is bound to be researched. That's where conditions are best met. But I also was very appreciative of the fact that it was important to have sites specifically designated for research. I experienced the Arctic, and it taught me that if you didn't have some places that were really designated for research, they would get trashed, and years of valuable research goes down the tube, and is lost. So, designated sights are important.

Geier: Where were you working out of?

McKee: Near Barrow.

Geier: Okay.

McKee: South of Barrow on the North Slope. This was all pretty influenced by the pipeline. You're looking at how plant communities were distributed along the

environmental gradient found up in that part of the world, where, over hundreds of square miles, you might have four feet of relief.

Geier: Uh-huh.

McKee: Real flat.

Geier: I'm a little fuzzy on the dates here. But, was that about the time that Project Chariot was under way up there?

McKee: This was post-Chariot. [Project proposed by U.S. Atomic Energy Commission to explode nuclear weapons to create deep harbor on Alaska's north shore/Arctic Ocean]

Geier: Okay.

McKee: This was '65, '66, and Project Chariot was from '59 to '62, something like that.

Geier: Somewhere in there. Okay.

McKee: I used to have a Project Chariot compendium here, but it must be behind you somewhere, a big black book. We interacted with some of the Project Chariot people who were still out there on post-Chariot stuff, and flew down to where Project Plowshare [U.S. government program using nuclear weapons for construction purposes – 27 locales] was supposed to occur, and saw that part of the world. It was great. Oh, god, in my sort of perfect world, I would be spending summers up there, and winters in the northern Rockies, but at any rate, (laughter), that's beautiful country. Wild-ass country. Damn.

Geier: You're probably a little bit fuzzy on what you were thinking about in terms of experimental forests in that period?

McKee: Well, I didn't view it as something I was going to try and build a career on, by any means. Nor did I view that as part of an assembly of lands designated for research purposes.

Geier: Uh-huh.

McKee: These "Research Natural Areas" are important. Experimental forests are important. Experimental ranges and national laboratories like Oak Ridge, that's another government lab, those are important. National parks. All of them I view as important as research sites. Each has infrastructure, and there ought to be some kind of flexible vision as to what these things are for, but they're all important. If you hunker down too much in those, to the exclusion of going outside of those pieces of real estate, you have no idea how representative any of it is. So, one of the things that fries my ass, that we

get criticized about at times, is how provincial we are, those of us that work at the Andrews. You take any one of the people that work out there, they also work in the Coast Range, way up in the hills, Olympic Peninsula, eastern Oregon, northern California, Peru, Chile, China; they aren't provincial people. That's an inappropriate focus of criticism, I think.

Geier: Ted was telling me yesterday he never considered himself to be really scientist material, and he kind of put Jerry in the same category. What I think he meant by that, the kind of standard stereotypical view of the scientist, who is detailed-oriented and things like that. I don't know if you have a reaction to that.

McKee: Neither one of them are detail freaks. That's why they are such good scientists. They're synthesizers. They're able to see patterns where others get lost in the detail.

Geier: Huh?

McKee: I guess that'd be my first reaction.

Geier: My understanding of your role, at the time you came there was to coordinate the administration of the details, to get those ideas and get the right people in there.

McKee: Right. Try to find technicians that matched. Somebody was talking today about hiring, and I was reflecting back on it. I was frequently asked to find technicians for various people for projects, as sort of a clearinghouse for summer hires. I believed that. I felt it was more appropriate for me to know what scientists were doing, to get a better idea as to what they really wanted in terms of skills. But I did it only a few times where it was in the black. I needed to know more. It's kind of a crap-shoot when someone is hired, anyway. To invest the time to interview them to be sure.

Geier: You came in about the time the IBP [International Biological Program] was taking off. Right?

McKee: I was the first hire in IBP.

Geier: Oh, you were the first one.

McKee: Yeah.

Geier: Then, there was a major infusion on from that point.

McKee: I didn't hold the purse strings, per se, but it was clear to me that I should feel free to make suggestions as to how moneys were invested. I didn't have budgets under my control, but I certainly was able to direct funds in a lot of different ways.

Geier: Uh-huh.

McKee: I hate snowmobiles, and I said we needed one to get around out here. And we had snowmobiles. Snowmobiles aren't my favorite. They didn't prove to be adequate, but, if I had to bite the bullet with snowmobiles, then, I'd do it. But they just couldn't hack it, to get us through this winter stuff [heavy, wet snow]. Get a snow-cat up here [better suited for conditions and purposes], but there wasn't money.

Geier: Uh-huh.

McKee: To say these moneys will go for that Snow-cat, those were Jerry's responsibilities, plainly. But, I felt like I had a lot of input into that.

Geier: With the IBP starting up, there's a whole new set of responsibilities for the scientists on the forest out there. Did you get a sense that there was any kind of coordinated strategy to deal with those by the time you arrived?

McKee: Yes. There were a couple of different ways. The proposal presented a structure where people work on different aspects geared towards a general understanding of how the forest ticks. This was an old way of thinking about what a general model ought to do. To a large extent, the plan was to coordinate research activities to provide information to build a huge forest ecology model. A fellow named Scott Overton was the general modeler on that early project. Scott was too theoretically-oriented for Jerry's taste, and Jerry wanted some quick victories in terms of publications that gave information on particular aspects of the system, sort of like "bing, bing, bing." And Scott's approach was slower, more broad-based, broad-footed. This is my personal interpretation.

Geier: Huh-huh.

McKee: It didn't mesh well with Jerry's need to get stuff out quickly.

Geier: Hmm.

McKee: And so, was a general systems-approach fragmented into many small approaches, and people like Phil Sollins and Mary Strand were brought on to model different populations of insects or nutrient flows in the watershed. Things that were trackable, doable, and you could get a publication out of.

Geier: Uh-huh.

McKee: So, Scott went his separate way. He produced a body of modeling theory, which dealt with hierarchical structures of models, and hierarchical modeling. It was a full 15 years ahead of its time.

Geier: Hmm.

McKee: It was the basis for a lot of high-profile, hierarchical-modeling work that came out in the mid-eighties, a full 15 years ahead of others, and based on Scott knowing concepts and how to structure these things.

Geier: That's -- ?

McKee: I hope the tape's not running. (Laughing) We're revisiting that kind of structure now, 20 to 25 years after Scott came up with it, and finding it conceptually very useful.

Geier: Hmm.

McKee: But, the animosity between Scott and Jerry left a cloud. We can't call him on the activities. So, I don't feel free to call these modeling activities by the names Scott used. I have to talk about them in terms of hierarchical models, and not use terms that Scott used.

Geier: Hmm.

McKee: So, there's still some weird, strange bad blood, there.

Geier: You developed those theories after he left the group, or is this a product he was working on?

McKee: They were in development. He talked about them. One of the first seminars I sat in on when I got here, he was presenting how a general systems model could be constructed where you compartmentalize the processes and have them operating at different time frames, and providing input to the model at a higher level.

Geier: Hmm.

McKee: And then, that higher level model might interrogate another model, ask it what its condition was, or what the rate of something was. These things [component sub-models] could be nested in a way that would allow them to talk among themselves, ask questions, ask what are the rates or states within that particular sub-compartment, and what we were getting out of it.

Geier: What kinds of computer systems were available to do that?

McKee: Those were old, clunky things. You know, there's more power in this right here [tapping a PC] than in the mainframe that was on campus.

Geier: Were you using the OSU campus computer?

McKee: Yeah, we were using punch cards, and it was a nightmarish thing to run that thing. Yeah, it was a nightmarish thing to run that.

Geier: When you have those kinds of administrative issues, you need to hire someone to fill a particular slot or a niche, or replace somebody who leaves for some reason, how was that decision managed? Is there a group?

McKee: There is a group, yeah. The spirit of the program has been one of consensus since I arrived. Now sometimes it was faulty in that Jerry would cleverly give people the feeling that we were coming to a consensus, and then we would go with a decision that he had already made. And sometimes it was transparent. But there were times when Jerry would say, "Well, you gotta get on their case for doing this while I'm gone. Go this way." Sometimes a majority would be ignored. And that caused some heartburn and there were several people who left the group due to that over the years. People like Mike Newton, Scott Overton, Denny Lavender. But they were not people who have shown any ability to work in group research, other than with one other person, since then anyway. They tended to be lone rangers, more typical of academia, and the reward structure still tends to reward that type of behavior more than group research. It's still hard to get recognition for participation in group research. We are on a campus which has given us relative freedom. There are some departments that don't seem to avoid doing that.

Geier: That's probably something worth looking into. You were saying earlier your long-term goal was an academic position at some point. You became involved here at OSU after '71, but what kinds of involvement did you have at the university in the early '70s, if anything?

McKee: All employment initially, in the money department (tape ends).

End of Side A, Tape 1 (of 1)

Begin Side B, Tape 1 (of 1)

McKee: Then, the position got me involved with the College of Forestry [OSU]. Those were term appointments, and when those ended, I got another series of term appointments working for federal agencies, or filling in for people on sabbatical here on campus. That went on for about three years, I think. Jerry had gone off to the National Science Foundation as a program officer there. He returned, I talked with him a few times about my desires to stay in the Northwest. I was going through a divorce, wanted to stay near my children, and he had an idea of creating a thing like we have today. He had seen some opportunities at the National Science Foundation he felt we should capitalize on, but he was going to need some help to do that. Would I want to help him with the proposal? I said sure, and basically wrote a proposal to create the job I have.

Geier: Okay.

McKee: That was to establish the Andrews as a National Field Research Facility, alias “Experimental Ecological Reserve.” [EER] It was clear in Jerry’s mind the stage was set for something to happen in the near future at NSF, to create what eventually became the Long-Term Ecological Research (LTER) program.

Geier: Hmm.

McKee: So, he and I worked very closely in that initial proposal, to structure both the Andrews research program and structure the administration of that research program that would be both an example for NSF and the program approach as to how things might evolve. We would be well positioned to be extremely competitive. Word was that the National Science Foundation wanted this [process] to start sometime in 1976.

Geier: Hmm.

McKee: Sometime in the next 3, 5, 6 years, they wanted to establish a program that would allow ecosystem studies to be conducted over long periods of time. Our group here was well positioned to offer advice on that. Many of us participated in a series of planning workshops. This eventually evolved into the LTER program. Here’s a folder which was part of the first conference [McKee shows Geier literature].

Geier: Hmm.

McKee: Here’s another copy of it.

Geier: Do you have a spare copy of this that I can borrow for a while?

McKee: Yeah, you sure can. Why don’t you take the one that’s not falling apart? And you might be interested in this as well. There’s a series of four, actually, I think.

Geier: Hmm.

McKee: This one’s got a map in case the other does not. [Flipping through files.] Maybe there are five. I forget how many I went to.

Geier: The first one’s in ‘77.

McKee: There’s a ‘77, ‘78, ‘79, over a three-year period. This one’s in ‘82. This is LTER-dated, but these were all precursors to the request for proposals for Long-Term Ecological Research program.

Geier: Hmm.

McKee: I don't know if they have rosters in all of these or not.

Geier: Hmm.

McKee: They'd be at the back I think. Check off people from our group who participated. Well, Paul Risser, who's now our president [OSU], was there.

Geier: Huh.

McKee: Could've sworn I was there on this one, too, because I was staying with George. Where was the second one? Well, I wasn't at the second one. I guess it was the first one.

Geier: Hmm.

McKee: You get down to some nitty-gritty in these subsequent workshops.

Geier: George Woodwell was somebody you'd worked with before?

McKee: Yeah, George was there. I guess I went to three of the four of them. I was thinking I was at every one of them.

Geier: You mentioned there was a good climate at OSU for cooperative work. Is this something you had been aware of before you came here, or was that kind of news to you?

McKee: Well, Jerry Franklin's most productive work had been built on a collaborative effort. There were always one or two other participants. When I was at Georgia, my major professor was involved with the IBP program on the east coast, for the Eastern Deciduous Forest Biome. I was peripheral to that, I wasn't part of that mainstream. I saw what was going on there, and the group there worked well together, but nothing like the group worked when I got here.

Geier: Hmm.

McKee: A much more coherent team than what had been back east. The model being used there was collaborative research involved in getting people together who would determine ahead of time where their boundaries were, what their responsibilities were, they went their separate ways, then reported back once a year with data or a product.

Geier: Uh-huh.

McKee: Whereas here, they were having monthly meetings, and there was a lot of "shucking and jiving." I didn't know anything about the aquatic research program when

I arrived, and when I read the proposal, I found myself, along with many others outside of the aquatic research program, volunteering to help people in that, during storms that first winter. Because they just didn't have the manpower to do it otherwise.

Geier: Hmm.

McKee: There was that kind of "one hand washing the other" community that I just hadn't experienced. To me, it was healthy, professional, and it was great to be a part of a community like this.

Geier: Uh-huh.

McKee: The other neat spin was, in addition to the sense of community and sense of wanting to do good science, that it was relevant to natural resource management. At least part of my background had been related to forest management. My family had a lumber business and lots of land holdings. I like that practical aspect. A lot of others do too. I think because of people like Ted and Jerry, they want field research to be relevant--

Geier: Uh-huh.

McKee: -- to natural resource managers. From the very outset, there was a sense of community. We'd give periodic updates, and invite people to seminars. And it wasn't an active attempt to be made leader, but to make them fully-fledged members of the community. The door was open, invitations were extended, and then people were also prone to say, well, this is an interesting piece of information. Wouldn't it be neat to do this with it? Or, wouldn't you like try to apply it in this manner? Or, you realize what this means, don't you? If we keep this up, then such-and-such is going to start unraveling out there. That's quite different than the attitude I'd been exposed to by some of the people I had listened to while working at Hubbard Brook Experimental Forest. My exposure with scientists up to that point-in-time was either, they're totally nuts-and-bolts oriented, like how many trees do you leave per acre to get a production of a certain amount? All empirically-derived, not based on any knowledge of how the system works, but let's just test these different levels, and leave them and see what happens. Or it was, well, I'm not interested in telling people what I've learned, or, esoteric publications that I expect to promote me up the [science] career ladder.

Geier: Hmm.

McKee: I interviewed for a job in the early in the '80s at the University of Colorado, in the Department of Environmental Population and Evolutionary Biology. I actually offended the department head by saying one of the things I enjoyed about my work where I was now, was that much of it was directly-applied. And how many of our graduate students here would like that. He was very quick to point out that was all well

and good, but at his institution the students worked on questions and not on how it applied. We're not here to worry about how it affects forest management.

Geier: You said that there were open monthly meetings beginning when you first got here. Were those open meetings for everybody?

McKee: They were open to the community of people working at the Andrews Forest, those to be working on the Coniferous Forest Biome project, and their graduate students, post-docs, and others who want to come and sit in and listen to what's going on.

Geier: Okay.

McKee: The way those used to be structured was, we would begin a seminar with a 45-minute presentation, and we'd talk about research. Then we start talking about that particular research they'd presented, and then, we'd start talking about the overall program. That would last for a brief period of time. There were almost always inadequate amounts of time to do all the different things that needed to be done. So, subcommittees would be formed to address it later in the day.

Geier: Hmm.

McKee: I was living on the river [upper McKenzie River valley], and I'd come in somewhere around 9:00 [a.m.] to whenever, maybe it was at noon-time. I spent all afternoon here, and it was evening before I got home. I think I started out a noon-time and then it was a long day, and then it became an all-day affair. Long days sometimes.

Geier: I'm curious about the connection between this pattern of open meetings, people committing themselves, their time, to meetings like that, and this other point you made about the relative unconcern about what action that was not necessarily producing a publication, not leading to tenure. You don't seem to worry about that too much.

McKee: A lot of the people who were principal at that point-in-history, were people who were already tenured. There weren't many junior faculty members. Some of those who were junior faculty members were in fact risking their careers by investing so much time on some of these meetings.

Geier: Uh-huh.

McKee: At other institutions they would definitely be risking their careers. But here, it wasn't so risky.

Geier: Can you think of any examples of people like that?

McKee: Well, Dick Waring would be an example of that.

Geier: Okay.

McKee: Dick did not have tenure at the beginning. He became tenured two or three years after I was here. He was in the department that was then called Forest Management. He agonized over whether he ought to stay with it. And he chose to, and he got tenure. Let's see, Don Zobel was in botany. He was heavily involved, and he struggled. Didn't get tenure the first time he was up for it. He had to wait a year.

Geier: Okay.

McKee: It was clear the reason he had to wait was because of his involvement with the group. He had multiple-authored papers, and not a large number of those. The department was unappreciative of the amount of effort necessary to produce some of those pieces of research. There are people who worked with us for a while, as junior faculty or post-docs, and then moved on, not especially liking the environment here.

Geier: Hmm.

McKee: And, feeling that group research was, it may be fun, but a little stifling in terms of career options.

Geier: Okay.

McKee: Frank Triska, in the aquatic program, would be an example of that. He went to the U.S. Geological Survey in San Francisco, where he thrived.

Geier: It sounds like the people who stuck around, with some exceptions, were mostly already-tenured faculty members?

McKee: They had a safety net there. They could take the time to participate in the amount of work necessary to make a group effort fly, and not be penalized.

Geier: Is there a connection there also, in terms of people who had a contingent of better students that could do some of the work?

McKee: No. Not necessarily. Because there were faculty who had lots of graduate students, and they chose to not participate.

Geier: Hmm.

McKee: The decision was made early on that we would not use money to bring on graduate students. We'd rather hire post-docs to do tasks, and summer technicians. I

was an example, along with Jim Sedell, Fred Swanson, and Kermit Cromack. I'm forgetting people now. It was the prerogative with some of those people to do specific pieces of research, working with summer hires, and there were not many graduate students. There were a few, but considering the relative bulk of the program, there were not many graduate students. Other sites used a lot of graduate students.

Geier: It sounds like a conscious decision to go to post-docs instead of graduate students?

McKee: Yes, it was.

Geier: What was the rationale for that, the reasoning?

McKee: I think it was the product focus. They weren't sure if the graduate students would produce.

Geier: They wanted, basically, "proven stars." Is that what you're saying?

McKee: The university [OSU] did not at that point have a lot of experience with National Science Foundation awards. The people that were involved, they were all pretty new to the process. They were new to that kind of funding. They were worried that they would not be able to stay competitive in that arena. I think, at least the things I picked up over the years, these were the reasons why.

Geier: Okay.

McKee: They wanted to make sure that they did a really fine job on research at the outset. Then they could spread their wings.

Geier: So, was it a university-level decision then?

McKee: No. This was the people that turned in the proposal.

Geier: Okay.

McKee: It was not a university decision by any means. The university was actually very upset that they did not use a large number of graduate students.

Geier: Okay.

McKee: And at that time, we also were pretty flush with money. We could bring on graduate students with other funding, state money. The [OSU] Forest Research Lab ran a current budget that used to allow for many graduate students. Now it's zero.

Geier: Hmm.

McKee: Sometimes I wonder why I stay, because, if you look back over the history of state support, for forest research, it's pretty abysmal.

Geier: Uh-huh.

McKee: And the university system state-wide is getting worse [funding-wise].

Geier: So, you were getting, essentially, funding through the research laboratory at the state level then, to do work on the Andrews?

McKee: Many of the graduate students working up there were hired with state money.

Geier: Hmm.

McKee: Most of the [OSU] College of Forestry faculty who were participating were getting paid out of state money. They weren't getting as much of their time paid for by NSF as they would actually have expected. Some were getting dollars from their involvement with NSF, and there were some state subsidies.

Geier: Is this before the formal memorandum of understanding? [First MOU between OSU and the US Forest Service regarding the HJA-EF, was signed in 1961.]

McKee: That was in 1970.

Geier: I was with Al Levno the other day talking about the changing of the guard in terms of people working on the Andrews from when IBP started in 1970. Al pointed out that a younger contingent came out in the early '70s, and he saw that as qualitatively different in terms of how it related to the Blue River District or the community of Blue River. It's an older group, and then, these younger scientists up there. Does that sound right?

McKee: There was a very small number of scientists prior to the beginning of this bigger program. Most of them had lived for a short time on the district compound, socialized with those people, were better known by the district people, and then this program comes here. We had to have a facility. There's no place to put them on the compound. So, we had a thing called the "Rainbow Ranch Adult Mobile Park." That got a lot of sparks going in the community. (Laughter) The people that lived out of that particular facility rarely interacted in the district movement. University people didn't have a history or tradition of interacting with district people, so that's right. It was a big change.

Geier: Preceding about 1970, when people are living closely together with the district, Ted and Al said that there was a period of benign neglect. There was not much attention being to what they were doing up there by the district, and in some cases overt hostility.

Then you start camping up on the site at the Andrews. Isn't that about the time when the closer connection begins to develop?

McKee: Yeah, it did. But not so much at the day-to-day operational level, as it was at the higher organizational levels.

Geier: Hmm.

McKee: I suspect the forest supervisor, in 1970 it was Zane Gray Smith, could not have given me three names of scientists up there. Five years later, the forest supervisor was Mike Kerrick, and I bet he could give you a dozen names.

Geier: Hmm.

McKee: Probably more. And he was more aware of what was going on than, damn, I forget who was in there until then, but somebody was for a short time. The connections were being made at a different level.

Geier: Okay.

McKee: And the information was flowing at different levels, the levels that are probably more productive to getting changes made faster. If you're socializing with people marking trees, laying out pre-commercial sales, and you tell them about your new research, they groove on it, but then they have no way of putting into practice, other than bringing it up at meetings, etc. If you persuade a forest supervisor he needs to do this flood research, then it'll get done. I'm not trying to be too cynical about this, but it's a more effective level of tech transfer. You have to work at a higher level of the national forest system, and that's when we began to try to improve communications with that group. On a personal level, the kind of thing that Al was talking about are things we don't handle.

Geier: Uh-huh.

McKee: He's still working on that. Many people in a district, have converted to a bunch of new faces, and many of the new people have no whiff of what we're about up there. They feel we're a bunch of arrogant twits. Well, "Whoa! Where did that come from?" We say things to try and get them to see us as real people.

Geier: That's kind of a turning point you are at here. That's kind of similar to what happened in 1970 when you put up that new camp up, and suddenly there's a new presence. Now there's this permanent facility up there.

McKee: Permanent facility. And there's such a high turnover in the agency, that we're losing contact. Saturday morning there were some Forest Service people who I

happened to cross paths with up on Andrews as I was headed out, and I was commenting that I had interacted with this guy for about 15 years. He was with another fellow who was pretty new, and he was giving him this line, and, basically, he wasn't buying it. It was, "Well, that tree would have to be taken out in a salvage sale." I was filtering it based upon previous experience with this guy, and I was kidding him about all this bullshit, and we could joke about it. We negotiated a few things there by the tailgate of his pickup truck. I had seen the announcement that he was a couple years away from retiring, and I said, "Geez, Bill, what am I going to do?" How am I going to be able to negotiate, without you to answer questions from young people coming in? They're so constrained by what they perceive as proper protocol, that the kind of interaction that Bill and I were doing, they don't have any more. It would take a whole lot more effort to accomplish the goals. Less logging activity on the [Willamette] forest, less opportunities for day-to-day interaction between the people doing research and the people who are working for the national forest system. There is this whole list of responsibilities that people feel they have to observe. We'd jolly well better adjust, and, if we don't, they turn their nose up at us.

Geier: Hmm.

McKee: At any rate, people have been there a long time, and that kind of sociology, the social dynamics doesn't apply.

Geier: This struck me that the social contacts that were going on earlier, the people that they were talking with had much responsibility for what was done on that part of the [Willamette] forest. They were the ones implementing the policy, but the responsibility for that policy was at a higher level.

McKee: Yeah.

Geier: You described how, through first the IBP and then later the LTER, the Andrews Forest became sort of a prototype for the LTER. I was wondering if you could talk a little bit more on how it came to that status.

McKee: It was the proposal I mentioned earlier that we wrote in '76, Jerry and I, in anticipation of something like an LTER program coming down the pike. We wanted to show it could be done and offer an example structure for it. One of the reasons we were so heavily involved in this series of meetings, which you've got in your left hand [meeting reports], was because of that proposal and because it was successful, and because we had begun to do LTER-like things with the funding that came. I have a couple of examples. We described both a local management committee as being a part of our organizational structure, and we wanted a national review committee, which would initially meet every year, then it went to every other year. We offered this as a model to make sure research that was being done was addressing national as well as

local needs or interests. The program, as it was developing, would pertain to national needs, too.

Geier: Was it national in focus or programmatically?

McKee: One is national-focused, one is programmatic. When the request for proposals went out for LTER sites, they were basically called upon to organize themselves along those lines, to have a local research management committee and to form a national advisory committee.

Geier: How about the evolution of the roles of the Andrews. How did your responsibilities begin to change from 1970 to about 1980?

McKee: Well, when I left the program, I was in essentially a post-doc at that point of program in '74. When I started again in '77, which was as the Director of the Andrews Forest, I had greater responsibilities to develop the infrastructure and to promote a program than what I'd had when I left. When I left, it was a set of research objectives that I was responsible for and coordinating help among all the different projects, then making sure that people didn't step on each other's toes or try to use the same piece of real-estate. I still do that, but there was a greater leadership role. Also, I had not been writing proposals to pay money prior to that. It's been a big chunk of my work to kind of focus operations. I had written proposals, but it wasn't so much of at the broad, programmatic level, as for a specific set of projects.

Geier: You moved into that role because that's about the time Franklin leaves. Isn't it?

McKee: Well, in '77, it was just that I was ready to do it. Dick Waring was back in Hawaii, Jerry was taking the lead. There was a need. There were plans to double our program, so we just needed somebody that could take the lead in getting dollars to do this. Jerry and some of the others were the best I had ever seen at coming up with the money, and we complemented each other. But, there was that change in responsibilities.

Geier: What are the strong points that you complemented each other with?

McKee: I think it would be better to write your proposal before you present arguments as to the whys about the things than Jerry does.

Geier: Hmm.

McKee: Better at rationalizing and creating an argument for facilities and equipment than Jerry. Jerry's won't invest the time necessary. It's very hard to get bricks and mortar money.

Geier: Uh-huh.

McKee: So you have to persuade the reviewer that that's really something that NSF ought to be spending money on.

Geier: Okay.

McKee: I've done okay in that arena. Jerry tends to do a good job at showing the elements of research to application, of conventional resource management. That's probably his strongest skill level. I think both of us are about equally balanced with Fred and Mark Harmon and others, in pursuing why this this particular piece of research is exciting and so germane.

Geier: Uh-huh.

McKee: Why it's good science, we're all pretty good at that part. You need those different skills in preparing a proposal. There are different programs you compete for money and support, so you have to use different kinds of skills.

Geier: What were your priorities on that forest up until the early 80s, in that period of the early LTER work?

McKee: Priorities, I don't think they've changed much there. Priorities have always been to invest the money into research from your facility, and to worry about research first, facilities and infrastructure, second. Structure the research program in a way that doesn't produce a lot of overlap, but produces a lot of work. We have three groups that are working on the small mammals, two groups doing aquatic research. We have one group that does aquatic research, and they do it very well. And we have one group that works with spotted owls, and population dynamics, their prey species, and they do it very well, and one group that does physiology.

Geier: What's your impression of things that tend to eat up money the fastest out there?

McKee: Well, facilities would be the biggest thing.

Geier: Sure.

McKee: I could give a fresh figure, our current budget on salaries. That's what would be the biggest, and then travel, instruments, sample preparation, sample analysis costs, 30 percent maybe. The rest for science.

Geier: Hmm.

McKee: You have to be very sensitive to that, because you can get burned very quickly.

Geier: One of the things that we were dealing with was the Mount St. Helens explosion, and some of the frenzy of activities that accompanied that. What kind of an impact did that have on the group at the Andrews from your perspective?

McKee: I don't think it had much of an effect in terms of production that was coming off the Andrews. It had a tremendous effect in broadening our perspective as to importance of certain aspects of the system in terms of certain biological legacies. The volcano really rubbed our nose in the importance of the plants and animals that would be carried through a disturbance in re-establishing the ecosystem and recovery processes and rates.

Geier: Hmm.

McKee: On that piece of real-estate. And that was the single biggest lesson for that.

Geier: Uh-huh.

McKee: Many of us did not invest more than, say one month a year there. Two weeks in the field and two weeks writing the stuff up. So, it wasn't a huge drain at that time. But it was a very interesting intellectual period. A lot of opportunities for what and how the different species behave under severe disturbance conditions. Aquatic species, terrestrial species, and those things were very interesting. Really, really very important.

Geier: The amount of resources that the group had to expend on it, though, was relatively small, it sounds like, compared to the benefits?

McKee: There was some bootlegging we had to do from our other research. We didn't have a lot of trouble initially getting funding to work up there. But by '83, '84, bingo, the dollars vanished.

Geier: Okay.

McKee: Then it became really hard, almost impossible, to get research dollars. It was like, we had a great idea for a research project, but if you mentioned it was at Mount St Helens, forget it.

Geier: Hmm.

McKee: That was all ancient history. That was all past. That was boring.

Geier: Just too much over-exposure or something?

McKee: No, I mean it was it was one ten-thousandth of one percent of real-estate of the United States. Why the hell do you want to go study at someplace that's been hammered by a single blast?

Geier: Yeah.

McKee: That was the reaction. And it was very expensive to work up there. So, the money vanished. Couldn't sustain it. But it's a wonderful natural laboratory, and every high school in the region offered to spend time over there. Every college was out there.

Geier: But initially there was a lot of funding?

McKee: Initially, funding was no problem whatsoever. Helicopters are very expensive. They are four thousand, back then they were \$4000 a day, to rent a helicopter. I thought it was \$20,000 we had to pay for a helicopter, at \$400 dollars an hour.

Geier: Uh-huh.

McKee: Shit, we had unlimited use of helicopters for a while. For the two weeks we'd be working up there.

Geier: Hmm.

McKee: We could take joy rides if we wanted to. Most of the time, we wanted to be down on the ground doing things rather than flying around. We'd take flights and look around the crater and crank the motor high so we could take a look at stuff on the wall, and watch how rocks would crash down in the crater. That was a real gas to look at, but it was disappointing as hell, when it, it evaporated overnight [funding].

Geier: Is that common in funding cycles?

McKee: In something as unusual as that. Yeah, I think is very common.

Geier: Okay.

McKee: When Hurricane Hugo really clobbered the South Carolina coast in '89, I think, money was available to follow that natural disaster.

Geier: Yeah.

McKee: Then the site that was following that natural disaster turned in its proposal in '92. The proposal got hammered. Got clobbered. They could have capitalized on a natural disturbance. The NSF panel was badly administered. They just got hammered.

Geier: The Andrews kind of stands out for doing this long-term research. Certain opportunistic events like that can lend themselves to acquiring funding. If that's a reoccurring problem, are their problems with essentially people getting bored, because everybody's done that already?

McKee: For mega-disasters, yeah. But you can almost always come up with a good reason for why you need to continue doing measurements on certain kinds of things. Changes in plant community populations or changes in stream water chemistry, precipitation acidity. In the recent past it's not been too difficult to argue the value of those continuing data and samples. It's hard to sell disaster sites. The Exxon Valdez oil disaster is a perfect example.

Geier: Yeah.

McKee: They wouldn't be doing what they're doing up there now if it wasn't for penalty money from the Exxon Valdez. If they had to rely on federal dollars to launch the recovery of that system up there, kiss it off. Attention span is pretty short for those big disasters. We have an easier time, I admit. Things that are germane over larger pieces of real estate, they have a hard time with that.

Geier: Something that's interesting about the Mount St. Helens disaster was it, Fred was talking about how it renewed their contacts. That scientists with the Andrews went out and got involved with other groups of scientists at that site. And I'm curious about how that feedback would work in the other direction in terms of attracting different kinds of scientists, perhaps to the Andrews, or a different focus?

Geier: So much spun out of that particular case, but I can think of some other instances where through LTER things have happened, like the hurricane that clobbered Puerto Rico. Fred was on their advisory committee at the time, and we've had some people come here to work since then. We've had people come up here to spend time with us to learn how we handle studying the floods.

End of Interview