

Regional Oral History Office
The Bancroft Library

University of California
Berkeley, California

UC'S SAGEHEN CREEK FIELD STATION AT FIFTY

Interviews Conducted by
Sally Smith Hughes
in 2004

Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of northern California, the West, and the nation. Oral history is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

All uses of this manuscript are covered by legal agreements between The Regents of the University of California and Glenn Flittner, Albert Jones, Richard Gard, Robert Behnke, David Taylor, and Don Erman, dated October 1, 2005, September 29, 2005, October 6, 2005, November 10, 2005, and September 29, 2004. The manuscript is thereby made available for research purposes. All literary rights in the manuscript, including the right to publish, are reserved to The Bancroft Library of the University of California, Berkeley. No part of the manuscript may be quoted for publication without the written permission of the Director of The Bancroft Library of the University of California, Berkeley.

Requests for permission to quote for publication should be addressed to the Regional Oral History Office, The Bancroft Library, Mail Code 6000, University of California, Berkeley 94720-6000, and should include identification of the specific passages to be quoted, anticipated use of the passages, and identification of the user.

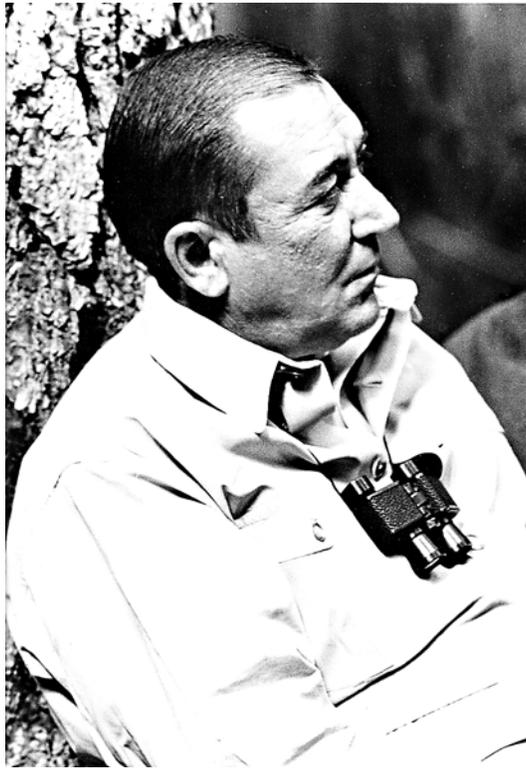
It is recommended that this oral history be cited as follows:
Glenn Flittner, Albert Jones, Richard Gard, Robert Behnke, David Taylor, and Don Erman, "UC's Sagehen Creek Field Station at Fifty," an oral history conducted in 2004 by Sally Smith Hughes, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 2006.

Copy no. _____



The Fifteen-Square-Mile Sagehen Basin in Winter

Photo by Jerry Booth



Starker Leopold
Photo by Norden H. Cheatham



P. R. Needham and Glenn Flittner, pumping and draining Photo courtesy Reg Barret

TABLE OF CONTENTS—Sagehen Creek

INTERVIEW HISTORY	vii
GLENN FLITTNER	1
ALBERT JONES	27
RICHARD GARD	47
ROBERT BEHNKE	65
DAVID AND JUNE TAYLOR	83
DON ERMAN	103

INTERVIEW HISTORY—UC's Sagehen Creek Field Station at Fifty

These interviews reflect the research and experiences of six wildlife scientists who worked at the Sagehen Creek Field Station between the 1950s and the present. Described by the *San Francisco Chronicle* as “one of the most venerable of the University of California’s 35 research sites for the natural sciences,” Sagehen is located on 452 acres just east of the Sierra Nevada crest, about twenty miles north of Lake Tahoe.¹ Professor Alexander Glazer, director of the University of California Natural Reserve System, together with his associate Jerry Booth, pursued the idea of interviewing some of the first researchers at Sagehen, who in August 2004 were to convene at the field station to celebrate its fiftieth anniversary.

With funding from the Natural Reserve System and the office of UC Berkeley Vice Chancellor for Research Beth Burnside, targeted interviews were conducted at the field station at the time of the celebration. Every researcher interviewed told revealing anecdotes about two pioneering scientists: Starker Leopold, initiator of the station’s wildlife program (and son of the eminent naturalist Aldo Leopold), and Paul Robert “PR”Needham, who launched its fisheries program.

Sagehen’s history began in 1949, when Leopold convinced the California State Legislature to include a line item in the university budget for a Wildlife and Fisheries Program. The histories recorded here begin a few years later, when the first students arrived to work under Leopold and PR. The reader will learn of long-term studies, cumulative databases on weather patterns, wild trout, and beaver, and the compilation of biological inventories of vertebrates and invertebrates. The narrators discuss sometimes troubled relations with the University, Sagehen’s decline in the 1990s, and its revival and inclusion in the next decade in the Natural Reserve System. They also comment on changes in the station’s guiding purpose. In the beginning, under Leopold and PR, it was predominantly fish and game preservation for the benefit of anglers and hunters. More recently, an ecological perspective has come to dominate, reflecting a wider trend in wilderness and wildlife management.

The interviews, restricted by limited funding in research preparation, length, and number, do not constitute a full history of Sagehen. However, used with documents in the Bancroft Library—especially the field logs dating from the 1950s—and the Natural Reserve System’s newsletter *Transect*, they provide a useful historical resource, replete with highly personal accounts and word portraits of the colorful personalities associated with this field station over its half-century-plus of existence.

The interviewer wishes to thank Professor Glazer and Mr. Booth for their guidance, their collegiality before and during the three pleasant days she spent at Sagehen, and their patience while this project very slowly wound to completion.

1. Glen Martin, “UC Outpost is a Natural Survivor,” *San Francisco Chronicle*, 08/25/2004, www.sfgate.com.

The Regional Oral History Office was established in 1954 to augment through tape-recorded memoirs the Library's materials on the history of California and the West. Copies of all interviews are available for research use in The Bancroft Library and in the UCLA Department of Special Collections. The office is under the direction of Richard Cándida Smith, and the administrative direction of Charles B. Faulhaber, The James D. Hart Director of The Bancroft Library, University of California, Berkeley.

Sally Smith Hughes, Ph.D.
Historian of Science
Regional Oral History Office
The Bancroft Library
University of California, Berkeley
February 2006



Glenn Flittner at Sagehen Creek, August 2004

GLENN A. FLITTNER, PH.D.

[Interview #1, 8-20-04]

[Begin Tape 1A] ##¹

Hughes: Would you tell me about your family and your education?

Flittner: I'll be happy to. I'm a native Californian. I was born in Los Angeles, in what is now the Watts area, and my father, Arthur Edward Flittner, was a farm boy from Indiana, who came out as a youngster in his late teens. He had an eighth-grade education. He became a part of the community that built southern California. He was in the construction trades. He was a floor layer, carpenter, soft tile, floors, linoleum. My mother, Helen Florence Marsh, has a very interesting history. She was born in 1906, in Ardmore, Oklahoma Indian Territory, as the daughter of a vaudeville team. My grandfather and grandmother Marsh were vaudeville persons who finally ended up in the southern California metropolitan area, namely Hollywood, at a time when the movie industry was rapidly growing, and the vaudevillian of yesteryear was fading from the scene. And of course my grandfather and grandmother did not make the movies, and so they became shop managers and ran a store for pets, and pet supplies, and they bred Scottish Terriers dogs. My mother grew up on the road; she had no opportunity to even get an elementary school education certificate. She always valued education, and I think she was a primary driving force. So when I expressed interest in going on from high school to the university, she was very very pleased that I was going to do it, but my father was a wage earner and he could not in any way afford to finance it.

I was the oldest boy of three, three hungry strapping boys. It was all he could do to keep the table set for us. So, one point that I would like to emphasize is that the University of California in its day, and still is, one of the most powerful instruments of social progress, as well as science and technology, that there is. And I feel that I've been the beneficiary of the gift of the public of California, especially at the time when I came north to California in 1949, and the incidental fee for in-state students I think was thirty-five or fifty dollars per semester. Tuition was nominal. I myself had to pay my way through school, so I worked at any opportunity, after classes, on holidays and weekends during spring break, Christmas break. I actually got started working in southern California in the forties, for a nurseryman, so I became conversant with landscaping. I lived in Berkeley in a rooming house on Dana Street, two doors down from Termite Terrace. I remember the name of the building to this day [laughter], and I think that was part of the area that was razed for the People's Park.

Hughes: Was that Dana pretty close to the campus?

1.## This symbol indicates that a tape segment has begun or ended.

Flittner: Dana and Durant.

Hughes: Yes, that's where People's Park is.

Flittner: The Gaisford House was a three-story frame building that was built in the 1880s; had gas pipes in the ceiling still, and when Ted Gregg walked up the stairs to the third story, the whole house shook. He was one of the students at the time. I obviously had to struggle to meet tuition and rent payments and the like, so I put in, worked—some of the students called me a grind, because I was so highly motivated and determined. But the Korean War came along; I had just avoided being drafted into World War II. The Congress ended the draft in July of 1946, for World War II, and I became eighteen in September. Had the draft been continued, I would have been a post-World-War-II draftee at the time. So I was grateful for that, and as a consequence I studied hard as a JC (junior college) transfer. Leading up to high school graduation my grades weren't very good, simply because we were all draft bait. All the males were going off to the wars; my cousin came back after three months overseas, wounded at the Battle of the Bulge and so scholarship was not on the top of our list. So I spent the first two years in the junior college system in Pasadena, which was a public funded system, and was one of the best of its time.

Hughes: And were you getting interested in science?

Flittner: Yes, I was. I spent a year after high school working for the Pasadena City Schools as a groundskeeper, and I saw that that was a dead end very quickly, so I went back to night school at Pasadena Junior College. I would like it as a matter of record that the English composition instructor took me aside one night and said, "Glenn, you're really highly motivated in the field of wildlife and environmental sciences. I can tell by your writings that you've got capability, and I think you should be aware of the fact that the University of California is opening up a new program in wildlife conservation, under Starker Leopold and Paul R. Needham, and it's in the curriculum catalog for—I think 1948 was the time—and you should set your sights on it."

Hughes: You were taking a related course so he knew you were strong—

Flittner: It was one of the core undergraduate courses for advancement into the university system. Pasadena Junior College actually gave courses that had equivalents in terms of transferability. So I started working on the Cal curriculum at junior college level, and so when I transferred I had German, I had botany, I had basic zoology, I had all of the core courses for JC transfer. So I came to the University of California in September of '49, as a JC transfer. So I was well on my way to the baccalaureate degree in June of 1950, when the Korean War broke, and I was drafted—screened for draft—and was declared 1-A in September, on my birthday, in 1950. And I was paying my way through school, and I was determined I was not going to have that cut short. So I had to compete and be in the upper 25 percent of the class to maintain my deferment. And at that time my two younger brothers were threatened with draft also. They thought they would avoid it by doing National Guard, and the 40th Division of the California National Guard was federalized, and thrown into the Korean War within six months. So my two younger brothers were overseas, in the battle zone, at a time when I was enjoying life as a student on a deferment at Berkeley.

Hughes: How did you feel about that?

Flittner: Well, I was bothered. But at the same time, my brothers told me, "Go for it." And my mother, she stood by me.

Hughes: Good for her. Had your courses in junior college prepared you pretty well—

Flittner: They were all transferred, and I had full credit for them. I remember a couple of the instructors, one of them with whom I still correspond, believe it or not, he is still alive. His name is Robert Ball; he now lives in San Luis Obispo; he's well into his nineties and he can no longer write; his wife sends the note to me at Christmastime. And the instructor who pointed me in the direction I pursued was Rodney Ellsworth. So when I came to Berkeley I had ticked off all the basic requirements, required in the catalog curriculum for freshmen and sophomore years. So I transferred straight in, and of course I had corrected my grade deficiencies with purpose. [laughter]

Hughes: Did you have any memorable courses as an undergraduate?

Flittner: As an undergraduate, the most motivating, stimulating course was Starker Leopold's basic course in wildlife management. And the second most interesting course to me was fishery biology, which was Paul Needham's first course. He came in 1948 or 1949, I don't recall, and we'll have to dig that up in the archives. But his first lecture, I believe, as I recall, was in the spring of 1950.

Hughes: First ever?

Flittner: First ever. And he told the class, "You'll have to bear with me, I'm putting the syllabus together, and I'm going to cover a series of topics," and that is the way he went at it. Of course, I was extremely interested in the fisheries, in particular—

Hughes: Why?

Flittner: Because I had grown up, as a child, exploring the mountains in back of Pasadena, and at that time there was a trout population in the Eaton Canyon, which John Muir had named the "Yosemite of the Sierra Madre" range because of its cliffs and gorges. And a teenage person and I used to climb into that canyon and fish for the trout that were in those plunge pools. Way back in the front range of the Sierra Madres.

Hughes: Where nobody else much went.

Flittner: Where nobody else entered because of the dangerous cliffs and crumbly rock. And I remember my dad said, "There are no trout in that part of the mountains." I said, "I've seen them, Pop, I've seen them." And so my friend and I set out one May the first to prove to my dad that there were trout in there [laughter] at a time when the trout limit was twenty-five. [laughter] And when I came home after that little backpack trip, I won him over. But I also, at that time, became very very interested in fly-fishing, in particular trout fishing, which I pursue to this very day. This is my release, relaxation. I've always enjoyed the outdoors, the environment, the plants and animals.

I aced the botany course at John Muir Junior College, which was the other part of the Pasadena Junior College system. At a time when one other student and I were competing, her name was Connie, Constance Dimmick. She got her PhD in botany at the University of California in 1952 or '53. She was my competitor. I beat her in the final examination in that course when we were at the junior college. So, the wildlife conservation course was really made for me. It had an admixture of forestry, range management, fisheries, wildlife management policy, philosophy. Starker Leopold was getting involved in the business of dealing with the hunting practices at that time that fostered the preservation of does in the Clear Lake area, and the Jawbone Ridge deer herds were grazing and starving in the wintertime. He was recommending that we harvest both males and females so that we could balance the population in balance with its environment. Paul Needham was interested in preserving wild stocks of trout. In his later years he pursued species, some species variations. He was the principal stimulator of Bob Behnke's career, and Dick Gard in particular, and Bob Behnke accompanied Paul on some of his expeditions into Mexico, searching for native trout and describing them for the first time, for the record.

Hughes: Was the preservation of native stocks in opposition to people wishing to promote hatcheries and artificial stocking?

Flittner: At the time, the widespread practice-- Well, the practice began in the 1880s. We had just opened up this country.

[End Tape 1A] ##

[Begin Tape 1B]

As early as 1880 striped bass were brought from the East Coast to the West Coast, and in the 1880s the rainbow trout strain out of the McCloud River of the Sacramento system was transferred to the eastern seaboard. Fishery policy and practices in those days was to experiment, bringing new exotics into environments without any regard for the impact on the native stocks. It was widespread experimentation at a time when we had no information.

Hughes: The prime motive was to please the fishermen?

Flittner: The policy, the philosophy, was to improve upon Mother Nature--the Judeo-Christian approach to things. We're able to improve upon Mother Nature.

Hughes: I see. Because man's at the top of it all.

Flittner: Man's at the top of the pyramid, so everything is subservient to mankind. Until we pollute the environment enough that we suddenly become subservient to the environment in which we live. [laughter] We're not there yet.

So I truly enjoyed my undergraduate years, and when Dr. Needham asked me to help him start the facility here, I took it.

Hughes: Tell me how that opportunity even came up.

Flittner: Because I was dependent on my own self-derived income, I had up to the time of graduation, each summer I had worked for the US Forest Service in the mountains of California in the blister rust control program and, when necessary, fire suppression. I lived in those labor camps which were left over from the CCC [Civilian Conservation Corps] days, and of course we had housing and food for I think two dollars and a quarter a day, if I remember correctly. It was a bargain, and I was able to work in the woods for three solid summer months, and save my blue IBM cards, which were the checks in those days, and then I would come back out of the woods and rent the room at the Gaisford House for the winter. That was my stipend. In '51, when I graduated with my baccalaureate degree, Needham asked me if I would work for him as a part-time laboratory assistant. I needed the pay, and I think somewhere in here¹ I have recorded the initial pay that he offered me was \$138 a month, in 1951. At that time we became aware of the fact that PR had already made an arrangement with the US Forest Service for access to his property up here. I think he scouted it with Warren Freihofer in 1950. Warren Freihofer was teaching assistant at the time [he later became Curator of Fishes, California Academy of Sciences]. Warren was a World War II veteran of the merchant marine, and he had a very interesting history. He survived several ship sinkings. He was one of the most kind, and cool, and collected and calm gentlemen I have ever known. He was the rock behind Dr. Needham. He was a TA in the department at that time. So I, with Warren and Gunnar Soder. You might want to stop the tape here while I—

[tape interruption]

Flittner: I'm reading from a letter that I wrote to my mother, who was my constant source of encouragement during all of these travails. On May 6, '51, "Paul Needham asked me into his office last Friday morning and asked me whether I was interested in joining up with him on a new experimental project going up on Sagehen Creek, north of Tahoe and Truckee, to be initiated this summer. He wanted me to work with him up there. The pay was \$250 per month for the summer season, room was free, but we had to pay for board." A deal just like the old blister rust control program. "This will give me a chance to get in on some new research and perhaps learn something at the same time. Also Dr. Leopold is going to operate a joint project with Needham, so I would be very close to the two of them and gain from both sides. The University is financing the whole project." At that time, Dr. Needham had a sponsor on the Board of Regents, and I don't recall who the gentleman was. But the initial money for the wildlife conservation curriculum came as a board action, if I recall correctly.

[tape interruption]

Flittner: That's the origin, at least the initial phase of the establishment of the project. As I recall, Dr. Needham got a \$35,000 grant, if I remember correctly, from the Max C. Fleischmann Foundation, and that was the money with which he had to work to start building the facility here.

Hughes: Do you know anything about that foundation?

1. Flittner refers to a document he wrote: Glenn A. Flittner, Ph.D., "Summary of Experience," June 20, 2004.

Flittner: No, I don't.

Hughes: But it does mean that the University wasn't putting any money into this.

Flittner: No. They put the money into the curriculum, but not the facility. That was a constant cause for conflict later on. I became acutely aware of it. I'm a person who picks up on things of that sort, and so I saw that Dr. Needham was having to fight an uphill battle in a department that was very fixated on its old practices. It was patterned along the old Grinnell-Storer lines of taxonomy and descriptive biology, for biology's sake, but not any practical applications.

Hughes: I see. And you didn't go out in the field, necessarily, except to classify—

Flittner: Classify and describe and put it down in writing and bring back specimens for the Museum of Vertebrate Zoology, and there they lay in state forever.

Hughes: [laughs]

Flittner: We were doing what Paul Needham himself called rock biology.

Hughes: What do you mean by that?

Flittner: Well, we had to roll rocks to discover—

Hughes: He wasn't interested in the rocks per se.

Flittner: That's right, that's right. And so I found myself, on the 26th of June, "Only a day or two ago we finally got a tent cabin, and an outhouse, and some tables banged together. Before that there was nothing at all here but the meadow, the creek, and a lot of pine trees." This is reading from my letter to my mother on June 26th.

Hughes: 1951.

Flittner: 1951. "The country here is big, wide, and beautiful. We're situated at about 6,600 feet in country similar to that above Crowley Lake towards Mammoth Lakes. Nights are cold, days warm; the work is not too hard. I think that I'm beginning to gain weight again." I was a skinny guy at that time. "Mail gets here very slowly; there's no air mail service except that to Reno, so I'm sort of behind on everyone's mail. We're not too far away from civilization at that, fifteen miles from Truckee, thirty-two to Reno." But there was no telephone at that time, no electrical service. We had Coleman lanterns and a two-burner Coleman cook stove.

Hughes: And was it really just you and Needham?

Flittner: No, as it turned out, when Dr. Needham came up here, Freihofer and I and a contractor by the name of Gunnar Soder. Dr. Needham made the first foray, if you will, into the place in late May. Needham wanted to show me where he wanted to start the facility. But it wasn't until sometime in July—I have it recorded here someplace. When we got

here there was a giant truckload, ten tons worth, of construction material dropped on the vacant site, and that was our start.

Hughes: Which Needham had ordered—

Flittner: —ordered up. Well, I think Gunnar Soder had told him what to order. Dr. Needham was a big-picture person. He would draw a little sketch and outline things that he wanted, and he'd trust somebody else to come up with the details. That later turned out to be a point of conflict.

[reading letter dated July 7, 1951] “The project is coming along very slowly; we still don't have a shower set up; our stove is in the tent. We eat outside, and all the dust and crud, under a tarp slung between two trees. This sort of setup is all right for a week or two, but I'm already beginning to get fed up with it. Can't keep anything clean, including myself, and freeze while eating breakfast in the morning. Believe it or not, we have had frost every morning without exception since the 18th of June. And the way it feels now, we're going to have the same in the morning for quite a while.” So that was an interesting aspect. At that time, Dr. Needham hired two young undergraduate students, Tom Haratani, an American-born Japanese who had an interesting personal history; he spent some time as a youth in one of the detention camps during World War II. His father was, if I remember correctly, a Presbyterian minister in Alameda, in the Bay Area, but they were incarcerated for a while. But Tom did not show any outward attitudinal dimensions of this experience there; he was a very warm person, had a great sense of humor. He appeared on the scene sometime in the first part of July, and he said “I'm being hired as a camp cook.” I said, “Tom, you got any experience”? He said, “No, but I'll learn.”

Hughes: [laughs]

Flittner: Tom was the camp cook. What he did was all the procurement of the food, and made the trips to Truckee to get the supplies, and then we divided up the cost amongst us. He was an inveterate gambler, and on Sunday the kitchen was closed. So he would go down on Saturday evening after the work closed, and he'd spend all night gambling at the blackjack table, and come back on Monday morning bleary-eyed.

Hughes: [laughs] With any money in his pocket?

Flittner: A couple of times he came back with nearly a thousand dollars, and other times he came back with nothing.

Hughes: But nobody else was accompanying him.

Flittner: Nobody else was accompanying him, no. I'm pawing through my letters, here.

Hughes: Now, would somebody like him have participated in the research aspect, as well?

Flittner: No. He was hired simply to maintain the camp for us. But he was a student in the program. He was in one of PR's classes, and that's how he came to—

Did he go on? Hughes:

Flittner: No. He was interested in trout fishing, and fly-fishing in particular, and he used to go down after work, especially down to the riffle where the North Fork of the Truckee comes in, at Boca, and he would come back with some very very beautiful brown trout.

Hughes: Was trout quite a feature of your menus?

Flittner: It was often a feature of the menu. Yes, as a matter of practicality. He was very skillful. He later got into the sporting goods business and sold fly rods, among other things. So that was his connection.

Hughes: I imagine that just getting the camp up and running was quite a large part of the early days, but when and on what did you begin your research?

Flittner: Well, Dr. Needham clearly wanted to start a pumping and draining program. And in 1951, we simply spent all of the summer getting the facility put into place.

Hughes: Why did he want to do that?

Flittner: Well, he had intended to make this a year-round facility, and he wanted to know what happened to the--

[End Tape 1B] ##

[Begin Tape 2A]

Flittner: --activities of the fish under the snow cover in the winter, because nobody really knew how they survived. And so one of the things he intended to do was to put in an underwater observation tank, and get people down in there writing down what they observed.

Hughes: Was that a new idea in fisheries biology?

Flittner: Yes, it was. Other than what people observed in hatcheries, we knew very little about the behavior of fish in the wild, particularly under the ice. At that time we thought they were adverse conditions. As it turns out, I think this program here proved that the fish remained active all winter long, but at a reduced level because of the water temperature, but when they needed to feed they fed. And of course when springtime came they responded very quickly to the rising temperature, and those who were spring spawners, spawned; those who were fall spawners spawned with the falling temperature regime. So we worked all through September putting together the facility.

In that period of time we also went down to Pyramid Lake on one weekend. We set some gill nets in the deep water near Anaho Island, and we caught only warm-water species, plus an exotic from the Sacramento River, the Sacramento River perch. Paul Needham was interested in Lahontan cutthroat at that time, because the irrigation management practices in the Fallon area had literally exterminated the brood stock coming up out of Pyramid Lake--we thought. Because some years there was absolutely no water going into Pyramid Lake, so there was no area for the fish to reproduce. Now,

the Bureau of Fisheries, in 1916, if I recall correctly, has a picture of the record-sized Lahontan cutthroat trout that was taken by the Paiute Indian fishery at that time. About 1890 or 1900, they started fishing. I think it is in the forty or fifty pound range. A huge fish. So the Lahontan cutthroat was an object of his interest. One of the other things that we did was explore the basin. We found ourselves on Independence Lake at the time the cutthroats were spawning in the inlet to the lake. I have a couple of snapshots which I'll dig out here today of the stock. Those were nice fish; they were adult fish in the 18-inch range. At that time, Warren Freihofer and Starker had told Dr. Needham, "Look, they may be part of the relict stock of the Lahontan cutthroat." But Cal Fish and Game people had already put eastern brook trout, and brown trout, and rainbow trout into the lake, and so they were not informed as to whether or not the stock had started hybridizing. I think subsequently we found that the Independence Lake stock had some different genes, but I think Behnke was probably the one who made progress on that. So anyway, we knew that we were witnessing the demise of the endemic Lahontan cutthroat. So one of the things we were looking for in the sampling in that first year, in 1952, was, were there any Lahontan cutthroats left in this stream here, because this was their original habitat.

Hughes: You were interested in all this because these are the original fish of this area?

Flittner: Yes, the original fish.

Hughes: Were you upset, at some level, because the lake was being stocked and consequently your studies were being disturbed?

Flittner: Needham had a phrase that he used frequently; he didn't like the fact that the state was putting the "lily-livered Hot Creek hatchery trout" in every stream in the state. And he actually protested the practices of California Fish and Game in '51 and '52 and '53 to Alex Calhoun, who was the director of Inland Fisheries, who was enamored with the fact that Seth Gordon had come out from Pennsylvania to show the state how to improve their hatchery operation, which is what the sportsmen and the public wanted. Needham was way ahead of his time, in the whole concept of preserving endemic stocks. He was farsighted, very farsighted.

Hughes: Why do you think he had that idea? At a time when other people didn't?

Flittner: It's a carry-forward from the early era days of descriptive biology when you described new forms in new areas, and he was aware of the trout stocks in the western United States, were very very plastic in the sense that they evolved in their natural environments in different ways, and sub-speciation was fairly widespread. And so one of his missions in life was to try to describe the last of the sub-species. Bob Behnke actually finished the work.

Hughes: When you say plastic, meaning—

Flittner: Resilient.

Hughes: --more so than other species, for example? Other species of fish, even?

Flittner: Not necessarily other species of fish, because we didn't know that much about the fact that caught us unprepared; there were sculpin in the creek, which were so prevalent, we had no idea, until we pumped and drained. We found that the total biomass in some of the areas was mostly species other than trout.

Hughes: And you were pretty sure that what you were looking at here was all the native stock.

Flittner: Well, we were looking for remnants.

Hughes: But Sagehen Creek had introduced species?

Flittner: Yes. Sagehen Creek had brook trout, and rainbow trout were introduced, and brown trout, by the state. Dr. Needham asked the state to stop stocking brook trout. They didn't stop until around '53, '54. We'll have to check with Dick Gard. The brown trout stocking program had ended earlier, because we very quickly learned that they became predatory and dominated the native stocks wherever they were planted in California. Most of the brown trout were put in this watershed probably in the late '30s. Rainbow trout were routinely stocked from the hatcheries much later. We did learn at Independence Lake that they continued to take spawn from the brood stock of cutthroats in Independence Lake. But they took the fertilized eggs down to the Alpine Hatchery in Markleeville, where the water temperatures were in the forties. The fish didn't grow properly; they didn't do well, so the Independence Lake stock—brood stock—were scattered pretty widely, but I don't think they took anywhere. And this is why the Pyramid Lake stock, which is in the stream now, is so valuable. And Tom Trelease of Nevada Fish and Game of that time had identified a group of Lahontan cutthroats that had been planted on a small lake on an Indian reservation in northern Nevada and Independence Lake. Those three places were focal points of whatever was left of the residual native stock.

So we were interested in it right away. And one of the things that came out of my sampling in 1952 was the cutthroats were gone, and they were so totally hybridized with the rainbows, we couldn't find any cutthroat characteristics in any other fishery sample.

Hughes: At that time, you couldn't have been doing genetic studies.

Flittner: No.

Hughes: How were you—

Flittner: This was all morphometrics

Hughes: I see. And that was—

Flittner: Scale counts, gill counts, and body coloration, and the typical taxonomic techniques. So, that was one of the things that we—

Hughes: Now, this pumping and draining, that's a new concept to me, the neophyte. Is that literally, you get rid of the water, and you see what was in the pond or stream?

- Flittner: Yes.
- Hughes: Now, was that established technique?
- Flittner: No, Needham felt that the only way you were going to find what was actually there was to dry the stream up and count what was—pick them up.
- Hughes: People would frown on that today, wouldn't they?
- Flittner: Yes. Well, we learned, and I think Dr. Dick Gard learned, after repeated visitations to the same sampling sites, that the population actually changed because of the way we altered the environment. Because the aquatic infauna, the invertebrates, didn't survive the periodic—even though it was less than eight or nine hours, dessication of the area changed the aquatic infauna. We put the fish back in, but if there were any eggs in the substrate, they didn't survive the dessication.
- Hughes: Oh, you weren't killing the fish, then?
- Flittner: No, we put them right back. We were inventorying and then returning them to the stream, we thought. We found out later it was cumulatively destructive.
- Hughes: I'm trying to get at the mind-set of the Fish and Game people. Was their reason for existence to help fishers and hunters? Or was it because there was a lack of appreciation at that period for endemic original species?
- Flittner: I think the orientation of the Fish and Game organizations, nationwide, in those days, was still exploitative. And wherever possible we "improved on nature," quote unquote, by introducing exotics, and thereby wreaking havoc in many cases on the existing ecosystems. So that was one of the things that Paul Needham was interested in. He wanted the brook trout stocking stopped; he wanted the rainbow trout stocking stopped, and of course the brown trout stocking had already been terminated in this watershed. He had been hoping that we might find some relict Lahontan cutthroats but that was not discovered. So he was ahead of his time in that sense, intuitively, four decades.
- Hughes: Had he gotten to what I understand is the contemporary idea that you not only appreciate the original species, but you also try to improve their habitat so that it maybe favors them over the introduced species?
- Flittner: Well, his experience on the Convict Creek experiment station, which later became part of the Fish and Wildlife Service operation, was that habitat destruction was widespread by grazing practices in particular in the high mountains of California. The principal results of the Convict Creek experiment was that when you fenced off the range from the cattle, the ecosystem responded very quickly, and of course habitat capacity for the wild fishes improved, increased. He wanted to start here and try to limit the exploitation. So he wanted to set up a reserve, which it now is, and it's closed to all fishing in this immediate area, and we want to observe the changes with time. And I have to tell you, after fifty years the changes are dramatic. Because in the early days the sheep were run through here going up to the high basin and back down a season, and so they kept everything literally mowed and grazed down, and of course cattle were grazed in the lower portion of Sagehen Basin. Cattle grazing was overgrazing. We had too

many animal units per acre. P. R. knew that, and he felt that to get a picture on what the capacity of the environment really told us was being impaired by animal grazing practices. He felt that we had to restrict that before we knew what the environment could hold.

Hughes: And had he done that by the time he got the money and this place?

Flittner: No, I think most of the information came after his time. But he was ahead of his time in that respect.

Hughes: So in those early days, you were still having the shepherders and the cattlemen and all that to contend with?

Flittner: The first pumping and draining experiment was to describe what was there. Which I did.

Hughes: Was that your thesis?

Flittner: Yes. And because I was 1-A, susceptible to the draft, I had to do something that was going to be wrapped up in short order. So the pumping and draining took place in 1952, and I was writing it up frantically in the spring of 1953. The draft board told me that as soon as the thesis was filed with the librarian, they would start the machinery going for my being inducted.

[End Tape 2B] ##

[Begin Tape 3A]

Hughes: I have a few more questions about Sagehen proper.

Flittner: Okay.

Hughes: One of them is simply about the fish observatory, which you mentioned, as being something quite new in fisheries. I wondered what there was to say about how it helped your studies, and even what constructing it entailed.

Flittner: I had a stormy working relationship with P. R., as we called him, on that fish observatory, because he attempted to do it on the cheap. When we got the tank here, it was so poorly constructed that the window frames in which he was going to clamp the glass were so badly out of true that if you were to cinch them down tight you would break the glass. I told him, I said, "You're in too big a hurry; I don't think you should put them in; it should be re-fabricated." But he was driven by the fact that he had to show some results, and so I said, "Well, okay, but whatever we do is going to be temporary; it's not going to last any length of time, and it's going to be inappropriate for long-term work."

Hughes: Who was he feeling he had to show the results to?

Flittner: I think that person on the Board of Regents, for one, and the Department of Zoology was still looking down their noses at P. R.'s work. I was very upset by that, but they

were imperious. And some of the most imperious ones were the junior ones on the faculty, Daniel Mazia and Frank Pitelka.

Hughes: Who went on to quite some careers.

Flittner: Quite good careers, yes. I was frustrated by that; I saw the internal machinations in the department. I did not like what I saw, but that's faculty politics, as I see it now. After I went to Washington, one of the world's worst places for politics, I saw it repeated many times over in every agency of government.

Hughes: Was it solely a philosophical difference?

Flittner: Yes, it was.

Hughes: So you weren't taking money out of their pockets by having this field station supported?

Flittner: No, but we were competing for the attention of the student population, in particular. I remember I was a little upset at the attitude of a couple of the grad students who came, reflected the museum of zoology approach, and I won't name names because I think they reflected their sponsors.

Hughes: They were here to collect specimens and—

Flittner: What Needham was doing was applied research.

Hughes: But why were they here in the first place, then?

Flittner: Well, they were doing basic research. Classic Grinnel-Storer—

Hughes: Was there formal coursework begun by then? Did you have groups coming up to Sagehen?

Flittner: No. Things were still so iffy. The Max C. Fleischmann money was starting to run out, and P. R., as we called him affectionately, was driven. He had to fight hard for dollars, and I think his faculty sponsor may have gone off the Board of Regents by that time. So he was all alone. His relationship with Starker Leopold was, I would call it, arm's length, because Starker came out of the Museum of Vertebrate Zoology, and he had to adhere to his mentor's philosophy and policy. And when P. R. later was in difficulty, Starker came around substantially, and Dick Gard I think can attest to that.

Hughes: You mean, giving support to him.

Flittner: And especially Sagehen. Because it was competing for dollars from the department level. The time that they spent, faculty time, up here, came out of the department budget.

Hughes: Does that mean that Starker was sticking his neck out?

Flittner: Oh yeah. He did.

Hughes: And yet, from the little I know about his background, you would think that Sagehen and what it represented would be dear to his heart.

Flittner: Well, I think he came late to discovering that and I believe that Dick Gard can relate to that turnaround, because Dick Gard was here at the time that Starker came up and set up his little cabin up the road from the main camp. It was his own sort of a replica of his father's little farm in Wisconsin. This was where Starker did his contemplative moments. This was while I was in the US Navy.

Hughes: I see. That's interesting, isn't it? That it wasn't an immediate love affair.

Flittner: No, it was viewed strictly as a competitor for budget and the administration's attention.

Hughes: But good research was already coming out of it. How could the museum people avoid acknowledging that?

Flittner: My master's thesis was in their view, applied research.

Hughes: I see. So applied research was immediately out of their equation.

Flittner: I felt this schism very early in my career at Berkeley.

Hughes: Do you think that may have made a difference in your career direction?

Flittner: On reflection, no.

Hughes: You never considered an academic career?

Flittner: Yes, I did, but the military intervened, and years went by, and at the time when I was ready to come back into the environment, I was married. It was all I could do to complete the PhD work, which I hadn't originally set out to do. Dr. Needham had persuaded me that I was worth it, I had the stuff, I could do it. I had to discover, my post-graduate worth, piece-by-piece. I had to repair a bad high school record, become eligible for transfer to the University of California. I set myself short-term goals, one at a time. I didn't have a long-term goal, because my father had an eighth-grade education, my mother was the daughter of a vaudeville team, and never even had elementary school education; she was self-educated. She ended up being the Executive Secretary to the Superintendent of the Board of Education in Pasadena, California.

Hughes: Remarkable woman. I can see where you got some of your motivation. [laughter]

Flittner: Yes. So I set short-term goals that I could achieve. My first goal was to get a baccalaureate degree, because I was the first Flittner who had ever gone that far in school. And then the master's degree was a short-term goal because conscription was going to get me.

[Note added in editing: I was indirectly referring to the conscription practices of some, but not all draft boards, of exempting married students from the draft. That bothered me greatly – the college-age blue-collar offspring were more vulnerable to conscription. This issue continued until the great social upheaval during the Vietnam War when the

country decided to turn to an all-volunteer military force after it ended. I am still upset at the iniquity of the system.]

In reply to: was single, because I worked my way through the university; I didn't find myself into the love life that would have changed things. I postponed it all. And fortunately, I married Evelyn Kathryn Rhodes with whom I've been in love for forty-seven years.

Hughes: Yes. That's quite nice.

Flittner: When I came back from the Navy, of course, the PhD was a given, in that I had been effectively persuaded by Starker Leopold and P. R. I had had a stormy relationship with P. R., because he was headstrong, and I was headstrong, and I felt he should be doing things a little differently. Starker said, "You really need to go to another institution, because you'll get a broader perspective of things, you'll get a broader exposure to the curriculum all things, and in the long term it's best for you." Out of that advice, I actually applied to the University of Michigan, by mail, from the Phillipines.

Hughes: Now, what did you find at the University of Michigan in terms of what the museum people at Cal would have called "applied research"? Was Michigan more open?

Flittner: Hughes: Michigan was more open to it. Karl F. Lagler fishery biology text at that time was the first of its kind, and we used it in Paul Needham's fishery biology course. And of course, the taxonomic work of the day was all being done at the University of Michigan, and Reeve Bailey and Robert Rush Miller were there. They were students of Carl L. Hubbs [the modern-day leading ichthyologist]. In fact, Robert Rush Miller married the daughter of Carl L. Hubbs. They were descriptive biologists of the first order, and they were great people, and I'm grateful for the exposure to the best people. When I came back, I had to have employment of some kind, so I negotiated with the Great Lakes fishery investigation, in Ann Arbor, via remote mail from the Philippines, because I had gone to a couple of American Fisheries Society meetings while I was in the Navy. I just took leave and traveled to AFS meetings, and I met James W. Mofett and Fenton ("Fent") Carbine [leaders of the Great Lakes Fishery Investigation] in Toronto at an American Fisheries Society meeting. Moffit looked at my credentials, and he said, "Well, we'll find something for you when you get out of the navy." So that was four years later.

Then my notes say that in 1974--that's skipping ahead, of course--that you and Dick Gard coauthored a report, would you call it, on Needham's fisheries study.

Flittner: I have it here. Dick did the bulk of the work; I have to give credit to him, because he was here. He wrestled with all the shortcomings of the data, but I've got the reprint here. That's the culmination of ten years of pumping and draining sampling.

Hughes: Can I have this?

Flittner: Yes, you can have that. Dick and I, while I was draft bait, did this one; you can have that one too, and that's about one of the nature reserves in the California system.

Hughes: Isn't that interesting?

Flittner: Well, we were Leopold's students, too, and Leopold knew that I was under the draft gun, and he, bless his soul, was looking out for his students as best he could, and he said, "I can't help you much more than thirty days." [Note added in editing: At that time, 1A males couldn't obtain permanent work in the public agencies because of the uncertainties associated with local Draft Boards meeting their manpower quotas assigned by General Hershey's Selective Service. It was very much like a fishing situation—one never knew when the would be hooked.]

So Dick and I went down and we helped Fred L. Jones do the survey of the bighorn sheep population on Game Refuge 4-C.

Hughes: Well, I heard you talking last night about how you went from waterhole to waterhole, so to speak. Can you summarize what you found in that study?

Flittner: Well, I think we wanted to enumerate the population, as best we could. Which consisted of us sitting near a waterhole with field glasses, discreetly, at strategic points, and monitoring the comings and goings of the bighorn sheep population to the waterholes. And I think we counted some 140-odd animals. [scans paper] Yes, here we are. And we evaluated the sex distribution and the number of lambs. We only counted ten lambs. We counted fifty-five ewes, and we evaluated the rams by the amount of curl on their horns. A full curl meant an adult ram, and we counted eight of those. We counted thirty-two with three-quarters of a curl. The youngsters, we counted seven of them. We counted 141 total observed. We estimated from that that there were probably three hundred and fifty in the population. And these were the places where we went. We actually backpacked into that country in July, because that was the time when the rams and the ewes stay close to the waterholes. Fred had overflowed the area and had put an "x" on the map where he saw green. We assumed water was there. In some places there was not water. We had a couple of tense days when we ran out of water. Fred did get some pictures. Stark, stark Sonoran desert. And Dick and I really enjoyed that.

Hughes: Were there greater numbers than you had anticipated? What prompted all this?

Flittner: We didn't even know. The state had no idea what was there.

Hughes: Had this started from the state being puzzled about what it had?

Flittner: Yes.

Hughes: And did the state pay for the study?

Flittner: The state paid for the study, and Starker Leopold got a contract, and he doled it out to Dick and I. And of course at that time poaching was a constant drain on the population. The other was, the environment was being altered by the wild domesticated animal, the burros, and they were destroying the water holes and polluting them with parasites.

Hughes: Did the state take any action after your report?

Flittner: Like all these things, it was a function of available state budgets. The range of these species has been constricted further by permanent development on the west side of the Coachella Valley, and so I think the censuses are now much smaller than they were at

that time. I really enjoyed this trip. It was a personal challenge, physically. We were in some temperatures near the 120 mark. We consumed six quarts of water a day.

- Hughes: You told us last night about having to dig down by a cottonwood to get water.
- Flittner: We found a nose hole where the animals knew the water was, and we spent all evening straining the water so we had our six quarts each for the next day. That was a tense time, because we had a long march out to the nearest road.
- Hughes: Tell me your impressions of Needham as a personality, and then maybe we could juxtapose what your impressions of Starker are.
- Flittner: Well, P. R., as we called him, was impulsive, emotional. When he found something new, he just trembled with excitement. He was the old style discoverer. He was temperamental, and he wanted it done his way, or not at all. I must admit I've been strong-willed—
- Hughes: [laughs] It probably took that to exist with him.
- Flittner: He and I collided on how do you do things, so I told him he had to have certain things, and I collided with him on that tank.
- Hughes: You mentioned at the outset his big-picture philosophy and how that led to problems.
- Flittner: He wanted to describe what went on in that stream when it was buried under deep snow cover. In the winter of 1952, he wanted to get a start on that. The two of us, John Sabath, I don't know where John was picked up, but he was hired to help me. I told Needham I was not going to do it solo. We were too remote.
- Hughes: Was he a student?
- Flittner: No. I don't know where P. R. picked him up. Anyway, he came up, and the two of us continued ongoing monitoring operations until the snow was about ten feet deep on the ground. The Forest Service had lent us a bulldozer to improve the site, but also had lent us a two-way radio, and when we were snowed in completely we communicated with the Forest Service District ranger's office, in Truckee, by radio. And in December we had to throw in the towel. We couldn't stick it out. The snow burden had gotten so heavy on the roof of the cabin--the west end of that long building—we couldn't open the door. We had to go in and out of a window. And that's when I radioed, "We've got to wrap it up."
- Hughes: What about food? You couldn't get out to get food.
- Flittner: No. We were existing on provisions. We ran out of fresh provisions right away. But canned goods can carry you a long way, and we didn't have to worry about preserving meat. At that time, I did inherit a German short-haired pointer. A disappointed hunter came through in the fall of the deer season in 1952, and he left the animal with me. His name was Bruce. The dog was gun-shy. It literally came apart when a rifle went off. Well, I thought I'd cure him; that was my chance. No. He was hopeless. As a hunting dog, he was useless. Anyway, Bruce and I and John—gosh, what was his last name?

Hughes: It'll come to you.

Flittner: When we left, we mushed out onto the middle of the meadow and sawed off a dead lodgepole pine to mark the snow line. It was ten feet off the ground when we came in next spring. And we snowshoed out, because the Caterpillar tractor that the Forest Service had loaned us simply couldn't walk through the snow any longer. The Caterpillar treads would run, but it was high-centered on ice and snow that was packed under the tractor. So we snowshoed out, in December of '52. I remember that vividly. The other thing the Forest Service did was to lend us a two-way radio. We had a generator here, of course, a little Homelite generator, and so each day we checked in with the Forest Service so they knew we were okay. I told them to get P. R. on the radio and tell him that he is going to have to fold, because we just can't hold out any longer. It was just too difficult, and the two of us weren't sufficiently equipped. And so P. R. was angry about that, because he wanted to start observations that first winter. Well, Al Jones followed him the next year, and he folded sometime in December or January, for the same reasons.

I remember vividly, it was deathly still here. Oh, it was beautiful. Beautiful, beautiful. Clear mornings, temperature down or below zero; in the daytime it'd be up to the high 30s. We could hear the snowplow; the rotary snowplow had to run up to Hobart Mills. They didn't snowplow that road. So they were driving through a ten-foot-high drift along that road, so we could hear them that morning, when we had decided we were going to come out. We said, well, we'd snowshoe out. We snowshoed and dragged a toboggan with our belongings out to the end of the road, and we could hear the rotary snowplow grinding, grinding, grinding all morning as we were mushing out there. Finally, when we got out there—it took us a while to close up the camp, drain the toilets, put anti-freeze in each of the toilet bowls, and things of that sort--it was about noon or one o'clock. The rotary snowplow had plowed up to the side road to Sagehen and stopped. And we came out on our snowshoes. And I remember that vividly because, P. R. had one fault—the way he handled and dealt with pressure was to drink.

Hughes: And he got terribly drunk?

Flittner: Dick Gard had more experience with P.R.'s alcoholism than I did. Well, when we came out, there was a fifth of Jim Beam on the top fender of the snowplow, and he says "Here Glen-sy-Wensy"—he called me Glen-sy-Wensy, which I *hated*—

Hughes: [laughs]

Flittner: But that was his sign of affection. "Come on, Glen-sy-Wensy, have a drink. Have a Sagehen cocktail." And he plunged a glass, a tumbler, into the snow and then he poured bourbon on it.

Hughes: [laughs] And you drank?

Flittner: And I drank. Well, I have enjoyed alcohol to some extent. P. R. had a sportswriter, which I think you'll find in the record, who came up and did an article on *Doc's Gold Fish Bowl*.

Hughes: I haven't seen it, but I'm sure it's around.

- Flittner Gosh, his name escapes me now—my memory is starting to go. They were great friends. Joe Mears! Sportswriter for the *Pasadena Star News*, if I remember correctly; he published in that paper, among others. He came up in the fall of '53—'52 first, then '53—and did a write-up on the underwater tank. He called it *Doc's Gold Fish Bowl* and described the early work that was being done at the time. They were great friends. And when the two of them got inebriated together, they were so funny. I remember one occasion when I was the designated driver, so I resisted doing as much as they did, and took them down to the Boca riffle—that's Spanish for mouth—the mouth of the North Fork of the Truckee. There was a riffle in there that was loaded with fish. And there were some beautiful evening mayfly hatches that brought the big rainbow and brown trout rising to the surface. And Joe and P. R. were starting on Sagehen cocktails before we left here, which was a glass plunged into the snow, packed down and then bourbon poured on it. Anyway, we went down to the Truckee River, and P. R., as we affectionately called him, got so excited! He really enjoyed fly-fishing for trout. He got so excited that, as he got out of the car, he was all trembly and fitting his rod together, and he said, "Okay, come on, Joe, let's go!" And he slammed the door—on the tip of Joe's fly rod.
- Hughes: Oh, shoot.
- Flittner: It was a very, very expensive fly rod. Joe, with tears in his eyes, said, "Paul, you sure know how to test a friendship." [laughs] Fortunately I had a fly rod also, so I gave it to Joe and said, "Here, you guys go ahead." I was the guy that extracted them from the Boca riffle and brought them back. They were thoroughly soused when they got back. That was P.R.'s way of relieving the pressure.
- Hughes: Were there other times when his—Needham, that is—overlooking details made a difference to you, or to what was happening at Sagehen?
- Flittner Well, I knew he was under terrible pressure. He was under terrible pressure to produce results. He was just driven. The Board of Regents man had to fight the Board of Regents for the money that he had gotten. He was a driven man, and the department was looking down their nose at him: a descriptive biologist belongs over in Davis.
- Hughes: So his career at Berkeley may have been threatened.
- Flittner: Even though the Board of Regents was influential in bringing him here.
- Hughes: I think we should now end with your views of how the philosophical approach to natural resources and land management and that whole issue of how we look at the natural world may have changed in the course of your career.
- Flittner: Well, it's evolved significantly. P. R. was ahead of his time; he wanted to find a place that could be left alone, and the contract he negotiated with the Forest Service in the beginning was to allow him to exclude grazing, and to even exclude fishing, because he wanted to see what the environment could sustain in its own right. And also, he wanted to chronicle the recovery of this basin, because it was wasted in the days of the building of the railroad. Hobart Mills existed to produce lumber for all the trestles and railroad ties for the transcontinental railroad.

Hughes: Was it clear-cutting?

Flittner: It was all clear-cut. So we're looking at an environment that's already been modified and it's now recovering. And it's going to take two centuries to recover. In due course it will recover. The one thing that has been changed is, forest management policies have prevented fire from moving through here, and so the composition of the forest has changed, too.

Hughes: How do you feel about that?

Flittner: Well, having worked for the Forest Service for four summer seasons, to sustain my college days as an undergraduate, forest management practices have had to change. And John Zivnuska of the Department of Forestry on the campus was forewarning that fire was a management tool that Mother Nature had applied, and sooner or later we were going to have to resort to it.

Hughes: How early was he manifesting that?

Flittner 1950.

Hughes: Was he the first, maybe? Or close to it?

Flittner: I don't know whether he was the first; I was not a forestry major. Two of his students who lived in the same room in houses where I did were forestry majors. But Zivnuska was one of the people I remember. One of the faculty members that I really enjoyed was Arthur W. Sampson, who gave a course in Range Management in the Forestry Department. He wrote a McGraw Hill

[End Tape 3A] ##

[BeginTape 3B]

—textbook on range management. When he gave the final examination, he'd come round and put his hand on your shoulder, and, "How are you doing, son?"

Hughes: Was it his personality that was so attractive, or—

Flittner: [poor recording] Dr. Sampson would look over your shoulder, look concerned, and then walk away, saying nothing. That was a clue to go back and check your answers once more. Starker Leopold had quite a bit to do with the Fish and Game Commission and game management policy in the state. He became very influential in changing the public's attitude toward deer harvesting, and ultimately changing the deer hunting regulations. He was well aware of the fact that they were cutting off all the virgin old-growth timber in the state, and everything was going to be in secondary stages of succession, fostering even greater increases in the deer population.

Hughes: How far did the state go in preserving certain areas?

Flittner: I don't think the state really got smart until halfway through the last fifty years, with the leadership of the university. I don't know who turned the corner on that, maybe faculty

philosophy. But the early faculty used Sagehen as a place to do some of their research. But *used* it; they didn't indicate their interest in supporting it. They invited in range management people, invited in entomology--Robert L. Usinger who was a wonderful addition to the community up here in aquatic entomology. He came up with some general research.

Hughes: He was at Berkeley?

Flittner: He was in the department of entomology. These were natural history people, and did some good work.

Hughes: When did the faculty really begin to use this place as a teaching tool, bring their classes up—

Flittner: It was after my time. I think Dick Gard can help you there.

Hughes: I didn't follow up with your profile of Starker.

Flittner: Starker Leopold? A very interesting person, very careful. Very warm person, when you got to know him. But in the beginning, he was sort of arm's-length. You had to prove yourself. Once you won his confidence, he was very close and warm. I think in part he was the way he was because he was the new kid on the block in the department at that time, in the Museum of Vertebrate Zoology, and he had to prove himself. Paul, P. R., was contentious. Some of the old guard really treated him at arm's length, and P. R. was frustrated about that. I don't think Starker came around until the second or third year of the project. And Dick Gard I think is one that can help you on how Starker became more and more involved with the project, and probably was instrumental in saving it. P. R. had a coronary.

Hughes: Yes, that's right. And then Starker himself died precipitously as well.

Flittner: Did he have a coronary too?

Hughes: I think so, but I'm not sure about that.

Flittner: He was a young man.

Hughes: Yes! I couldn't tell you exactly, but somewhere in his sixties, and I only know that because I read his oral history, and a second interview was scheduled, and he died before it could be done. Do you suppose that because of the family he came from, obviously his father, that helped him a little in terms of entry into the department?

Flittner: I'm sure that helped quite a bit.

Hughes: And of course, his interests were different than Needham's. Was it somehow more acceptable to be interested in wildlife than to be interested in fish?

Flittner: I don't think there was a great deal of distinction applied one to the other. I do know that the California Department of Fish and Game looked down upon "*Doc's Gold Fish Bowl*" up here, which is the way—gosh, that writer

- Hughes: Mears?
- Flittner: Joe Mears! Joe Mears wrote up *Doc's Gold Fish Bowl*. [Note added in editing: My memory lapse at that moment prevented me from noting that the DFG leaders were all Stanford graduates from the Jordan and Evermann mold. They were contemptuous of Needham, and P. R. fought them at every opportunity. P. R.'s students couldn't help but note the conflict.]
- Hughes: I can see how that appellation may not have helped.
- Flittner: That didn't help, to the imperious MVZ people. But Leopold came to discover, because of his students he sent up here, and he changed. He realized the true value of the place. And at that time I think he was pretty well implanted within the Museum of Vertebrate Zoology, so he could start exercising his own power.
- Hughes: Well, I think for various reasons we have to stop. But is there anything that you want to say in conclusion, in overview of your life's experience?
- Flittner: In hindsight, if I had to do it all over again, I'd probably do it the same way.
- Hughes: That's good. That's good.
- Flittner: I have absolutely no regrets, and I'm grateful over the long term for the opportunity that the university offered to me.
- Hughes: That's very nice. If I asked you what you consider your greatest contribution, how would you answer?
- Flittner: Well, my work relatively speaking was insignificant.
- Hughes: Are you sure?
- Flittner: Well, I think I did start something useful. [The pumping and draining research went on for ten years, and Dick Gard's summary was one of the few comprehensive long-term studies ever done on a small stream.] I did not enter the hard science field; I found myself in management and administration. Frankly, I probably have more capabilities and qualifications there. I lack the patience that it requires to do seminal work, to do research for research's sake.
- Hughes: Well then, let me re-orient my question to administration and management. Then, what would be the highlight?
- Flittner: Well, I have helped build a part of the system now contained within NOAA, National Oceanic and Atmospheric Administration, which has been collecting a plethora of environmental information, which now is accessible by the science community here. [The NOAA global environmental information system didn't exist when Gard and Flittner started their study of Sagehen Creek.] And Jeff is just getting plugged in to it. It's going to broaden the horizons of this community significantly.
- Hughes: And others as well, I imagine.

Flittner: I can at least attest to that kind of interaction going on.

Hughes: That's quite an accomplishment.

Flittner: And I have no regrets for doing what I did. My contribution is there, small as it is, and it's part of the record.

Hughes: We'll let other people judge the use of your adjective. Anyway, I thank you; it was a pleasure.

Flittner: It's been a pleasure. Sometimes one's fulfillment requires five decades.

[laughter]

[End Tape 3B] ##

[End of interview]



Albert Jones at Sagehen, August 2004

ALBERT JONES, PH.D.

[Interview # 1: 8-20-04]

[Begin Tape 1A]##

Hughes: Would you tell me a little bit about your family background and your education?

Jones: Yes, I'll be glad to, Sally. My name is Albert Jones; I was born in the San Joaquin Valley, in Coalinga, which is on the western side of Fresno County, and this was on August 18, 1929. Coalinga was a small oil town at that time, about 6,000 people. I think it's now, today, about 6,000 people, so the population hasn't changed much in that number of years. I grew up in the school system in Coalinga, rather an interesting school system, because it was an oil town. It had revenue from the oil companies; the oil companies paid school taxes on every other section of land, so there was really a well-financed school district. They had a junior college, which was one of the first ones in the San Joaquin Valley, and I remember that we had a swimming pool in the school system. Interestingly enough, the school had a summer camp over on the Pacific Coast, at the little town of Cambria Pines, so they would take the school children over there for a two-week summer camp, by age group. Very early then, I went over as a camper, and then later on when I got in junior high school and senior high school I went over on the staff and stayed during the entire summer working. And that was my introduction to aquatic biology and marine biology. This sparked my interest and led to my eventual career in fishery science.

[tape skip]

Hughes: Please describe what your parents did.

Jones: My father, Albert Cleveland Jones, Sr., had a dry cleaning establishment in Coalinga. He had come to Coalinga in 1908 from Pennsylvania. He was born in a little town, Midway, which was close to Pittsburgh. When he was about seventeen or eighteen years old, he left Midway and, with a friend, he rode the railroad, on the freight cars, from Pennsylvania to California. He had a half-brother who was living in Bakersfield. So the two of them, he and his friend, gradually made their way from Pennsylvania—they spent the winter in Cheyenne, Wyoming—a little bit too cold to travel on the train—but they got to Bakersfield. He asked his half-brother what he thought he should do, and where he should go. Well, his half-brother recommended that he get on the train and ride to the end of the line, which was Coalinga. Coalinga was literally at the end of the line, didn't even have drinking water. Even when I was growing up, they hauled their drinking water forty miles on the railroad. It was delivered in tank cars and piped through the community, because the

local water was too hard to drink. We had the distinction, in Coalinga, of each house's kitchen sink having three water faucets: one for hot water, one for cold water, and one for drinking water.

Hughes: Not to this day, though, I imagine.

Jones: Not to this day, no.

Hughes: And your mother?

Jones: My mother, Carolyn Goodrich Jones, was an art teacher. She had come to Coalinga after having taught in several towns in California and also in Jerome, Arizona. She was born in Ukiah, in Mendocino County, and she and an older sister had gone to the California College of Arts and Crafts in Oakland and graduated from there. She met my father in Coalinga. His first wife had died in the flu epidemic of 1918-1919. They were married in 1926, and I was born in 1929.

Hughes: Do you have brothers and sisters?

Jones: No, I'm an only child.

Hughes: So then what happened after early schooling.

Jones: My parents always wanted to leave Coalinga, especially my mother. She had relatives and family in northern California. So my father sold the dry cleaning business when I was a senior in high school, and they moved to Petaluma, where my aunt lived, and they lived there the rest of their retirement years. Then I went to Petaluma High School for my senior year, graduated from Petaluma High School.

Hughes: So they retired rather young, did they not? Or were they older?

Jones: They were somewhat older, yes.

Hughes: So it was the right age for them to retire.

Jones: Yes. My father was born in 1888; my mother was born in 1892. So they were fifty-eight and fifty-five years old.

Hughes: And how was high school in Petaluma? Did that encourage your interest in the outdoors and science?

Jones: Not particularly, I guess, it was just kind of an ordinary year. My aunt was an art teacher in Petaluma High School; I knew a lot of the teachers. Being there only one year, I didn't develop a great number of friends in high school. I had a friend who's living in Truckee now, Claus Shelling. He had been born in Germany, and his parents had come to the United States when Claus was just a youngster, I guess about 1938-39, something like then, and his father had been in the feed business in Germany. He established a feed business in Petaluma.

Hughes: Makes sense that he would end up there, doesn't it?

Jones: There were a lot of European-descended chicken farmers and dairy farmers in Petaluma, so he developed a business catering to them. Claus was one of my high school friends. Another friend was Willard Gibson. Willard and I graduated together, and Willard wanted to be a pharmacist. He wanted to go to Berkeley, so we both entered UC as freshmen in September 1947, and joined about 21,000 students at Berkeley, a lot of them returning GI veterans.

Hughes: That was a big influx.

Jones: Yes, that was a big influx. I thought that was quite a large number of students. I was in Dr. Joel Hildebrand's Chemistry 1A class, which had about 700 students in the lecture hall, which was quite an experience.

Hughes: Had you always—well, from a reasonable age had you always intended to go to college? Was that a given?

Jones: I think probably so. That was kind of a planned-out family thing.

Hughes: The family expected you to—

Jones: I did pretty well scholastically in school; Coalinga was a small school, and there were two or three of us who were at the top of the class all the way through our school years.

Hughes: By the time you got to Berkeley had you decided what area you were going to specialize in?

Jones: I was interested in outdoors work of some kind, forestry, fisheries, or wildlife.

Hughes: Had that come from your camping experience?

Jones: Yes, I think it had come mainly from my experiences at summer camp, Cambria Pines. The interesting thing about that, they took us out to the tidepools and let us collect in the tidepools, and when we were on the junior staff we would go out at low tides in the early morning and get abalone, and bring the abalone back for the camp. That was when there were lots of abalone. We could feed the whole camp abalone dinners. We would go fishing on the pier at San Simeon.

[tape interruption]

William Randolph Hearst had built this pier to unload his building materials for the Hearst Castle, and we kids would go out on the pier and fish for live-bearing surf perch. Those things kind of sparked my interest in biology and marine biology. That started me on a career interested in fish.

Hughes: Were there some turning points in your undergraduate career that steered you more towards—

Jones: What I did, my freshman year at Berkeley was chemistry, physics, and English. I was a little overwhelmed by the size of the classes there. See the squirrel on the tree out there?

Hughes: Yeah!

Jones: I was interested in fisheries and Berkeley did not have a fisheries program at that time. I was a little overwhelmed at the 21,000 students there, so I transferred to the University of Washington in Seattle, where they had a School of Fisheries, so I got my Bachelor of Science degree in fisheries from the University of Washington [1951].

Hughes: Had you chosen Washington because it had a strong—

Jones: Because it had a fisheries school, yes, and Berkeley didn't at that time. Dr. Needham had not come to Berkeley in 1947 when I was a freshman. They had a good Museum of Vertebrate Zoology, but I was more interested in fish than in wildlife.

Hughes: Why fish?

Jones: I don't know. Just because of my interest in marine biology, and I'd been at the seashore. At the University of Washington I took mostly fisheries courses and not very many zoology courses, so after graduating I stayed around Washington for a little over a year, but felt the need to get back into zoology with some more basic courses. I left Washington not really knowing what I was doing or where I was going. By that time I had heard that Dr. Needham was at Berkeley, and there was a fish program there, so I called him up and said, "Could I come down and talk to you?" He said, "Sure." It was about March of 1953; I walked into his office, and we had a talk, and he asked me what I was interested in, and told me about Sagehen and the program there, and asked me if I would like to come up there and work for the summer, if I'd like to select a topic for a thesis. He suggested working on the biology of the sculpins *Cottus beldingi*, and it sounded good to me, so I said yes. I arrived up here in either late April or early May, somewhere around then, and stayed during the summer, and actually I was in charge of the station during the summer.

Hughes: Did Needham go back home to the Bay Area?

Jones: He was back and forth.

Hughes: You were there permanently.

Jones: Yes. We had a crew of about eight people at times; it fluctuated. We also had the construction people up here during the summer.

Hughes: Tell me what was here in 1953, when you arrived.

Jones: There was the original main cabin and that was what was here at the beginning, plus this little pumphouse that we're looking out the window at right now, and several tent cabins. And that was really the extent of the building. There was a lot of building that went on during the summer, because during that summer the laboratory building was built. Actually there was the main cabin, and an extension, an open-air dining room next to that. And during the summer the laboratory building was added on to the end of that. The garage was constructed and a water line was laid from the spring up the road down to the camp, so there'd be a supply of running water during the winter.

Hughes: And was the Swede, Gunnar—I can't remember his last name--

Jones: Gunnar Soder was a builder who was doing a lot of residential construction in Lafayette and Walnut Creek. Dr. Needham knew him, and Gunnar came up several times during the summer and worked on the building that was going on, the laboratory building that summer and also the garage. He'd come up here with his crew, as I recall three or four people, maybe four or five people, and they'd work hard during the week on construction. Then at least some of them would go to Reno for the weekend and spend down there to get a little bit of R&R. Some of us young scientists on the crew would sometimes ride down with him in the back of his pickup truck, and we'd find something to do in Reno while—Gunnar was a great Keno player; he liked to play Keno. He did very well at it financially, monetarily.

Hughes: You weren't interested in the gambling part of it?

Jones: We didn't have the resources so we just pretty much looked. We were spectators.

Hughes: Where was the money coming from?

Jones: The Max C. Fleischmann Foundation had given the University a \$30,000 grant, and that supplied the money for the construction that was going on.

Hughes: And Dr. Needham had secured that?

Jones: Yes. That was Dr. Needham's doing; he secured those grant funds. That really kind of got the station started.

Hughes: Is that an environmental foundation?

Jones: I'm not sure, Sally; I don't know much about the foundation itself. We had a couple of interesting visitors during the summer. One visitor, Buck Wheeler, was somehow or other associated with the Fleischmann Foundation. He was up here, and I have a picture of him in my photograph collection. I remember seeing him with—Dr. Needham was escorting him around the creek, and so forth. We also had another distinguished visitor that summer, and that was Gerard Piel. Gerard Piel was the editor of *Scientific American*. He got *Scientific American* magazine started. Dr. Needham was writing an article for *Scientific American*, and Gerard Piel was interested in trout fishing. He came up and visited the station. I'm sure he wanted to see what was going on, if he was going to publish an article about Sagehen in the magazine, too. So those were two distinguished visitors we had, during the summer.

The major part of the work crew, the eight people, did our diversion and pumping operation from about the twelfth of August until the fourth of September. I guess that's when we had the most number of people. These were mainly high-school kids that were friends of Dr. Needham's son William. Those kids came up and helped with the diversion and pumping operation, which took a lot of labor. And actually they may have been here earlier in the summer and helped with some of the construction, I'm not sure.

Hughes: And Needham's son was one of the ones who came up?

- Jones: Yes, he was one of the ones. Let me just go over some of the names of people who were up here. Elbert Brock was up here working on his master's degree. He was working on the alga *Nostoc* and the chironomid midge larva that was associated with that. So he was here all during the summer. Dick Gard was up here off and on during the summer, especially when we were doing the pumping and draining. I call it pumping and draining; they call it diversion and pumping. I've slipped back into that. Glenn Flittner was finishing up his master's degree work down at Berkeley, but Glenn came up to help with the pumping and draining work, because Glenn had done it the previous two summers, and he knew where the sections were, and he knew the procedure for laying out the flash boards and diverting the water from the main channel.
- Hughes: What was the purpose of the sections?
- Jones: The purpose of draining the sections was to get an estimate of the fish population that was present in the section.
- Hughes: So it was kind of an arbitrary dividing up of the waterway?
- Jones: Yes, this had been started in 1951, when they had selected four sections and sampled these. And then in 1952 they increased the number to ten sections. And those I think were kept standard after that. In 1953 we repeated all ten sections. We started with section number one, up at Kiln Meadow, toward the head of Sagehen Creek, and then worked down to section ten, which was below Highway 89.
- Hughes: Tell me exactly how that works. I mean, how do you drain and pump?
- Jones: We selected a section of stream that could be diverted over to an alternate channel, and what we did was to set flash boards across the upper end of the section to divert the flow into that auxiliary channel, and then we put a stop net down at the lower end of the section. So between the flash boards and the stop net was our sample section. We would collect the fish from the riffles. Any water that was in the pools, we would pump out of the pool and collect the fish out of the pool. So we got all the fish from the section that we could; hopefully we got a hundred percent. We did get a hundred percent of the trout; we probably didn't get a hundred percent of the sculpins, especially the young of the year. But anyway, we'd count those, measure those, I'd preserve samples of sculpins from all the ten sections.
- Hughes: And you were keeping them alive?
- Jones: Yes, we kept them alive. After we got the scientific data collected, we removed the flash boards to return the water flow to the section and returned the live fish to the section..
- Hughes: What came out of all this? What did you find?
- Jones: Well, we got an estimate of the numbers of fish, the species of fish, how the species composition varied from the headwaters of the stream down to the lower sections. There were three species of trout, one species of sculpin, two species of suckers, and two species of minnows in Sagehen Creek. The species composition in each section of course varied, so we got that information. We also got information on the size of the fish, the age composition of the fish, and an index on the numbers. Especially going

from year to year, it was interesting on the changes in numbers. The information I got on the sculpins: there was one-year class of sculpins that was just wiped out by apparently the heavy spring floods in the spring of 1952, because that whole-year class was missing through the next two or three years.

Hughes: What happened to them, do you think?

Jones: I think they were probably just washed away. It was either that or it was cold weather of the winter of 1952, because that was an extremely cold winter. That was when the snow was over the top of the cabin roof, and there were no temperature readings taken at the station here because nobody stayed over that winter. But at Boca, which was down toward Prosser Creek, they recorded a minus forty-one degrees Fahrenheit. The winter that Elbert Brock and I stayed here, we recorded temperatures as low as minus eight, which is more like an average winter for this area.

Hughes: Were you still in tents, or had the cabins been built?

Jones: No, we were in the cabin. During the winter, after the summer crew left, Elbert Brock, I, and Ray Allen, who was the caretaker, stayed on during the winter.

Hughes: You had wood-burning stoves?

Jones: No, we had an oil-burning stove. Wood-burning stoves are great, really, but we burned heating oil. We had a propane tank up on the hill. Propane people came out from Truckee in the early fall and filled it up, so we had a large tank of propane which would last through the winter for cooking and for running the electric generator.

Let me just mention some of the names of the high-school kids who were up here. I've mentioned the university people, but William Needham was Dr. Needham's son, and then basically the others were his friends, Donald Wideline, Tom Kerns, Larry Helm, and William Bedard. There was a total of about eight people on the staff. Those five, and then Elbert Brock, and Ray Allen, and myself. Dick Gard, Glenn Flittner and Dr. Needham were in and out during the summer.

Hughes: Do you know if any of those young people went on to a science major?

Jones: I don't know. It would be interesting to track them down.

I should mention too that Dr. Leopold, Starker Leopold, had a couple of graduate students up here that summer, Bob Hoffman was up here working on blue grouse and meadow mice, and Joe Hall was here working on beaver. They were in and out, I remember Bob Hoffman was in and out quite a lot during the summer with his trapping for meadow mice. He'd dissect them in the laboratory, and collect reproductive data on them. And Joe Hall was out looking for beaver, setting beaver traps down at the beaver dam, trying to observe them.

Hughes: Was there a lot of cross-talk over meals or whatever?

Jones: It was a very small close group, and we developed great friendships. After I went back to Berkeley, Bob Hoffman, Joe Hall and I carpooled together. We lived in the married

student housing in Albany, and we carpooled back and forth to the campus. We developed that association here at Sagehen. Bob and Joe were both in the Museum of Vertebrate Zoology, and I was in fisheries.

Hughes: Did the atmosphere change when Needham arrived?

Jones: No, I don't think so, no.

Hughes: Tell me about him as a personality.

Jones: Dr. Needham was a very visionary person. He had worked for the US Fish and Wildlife Service, and was stationed at Stanford University, and was very much interested in trout, had done research on the eastern side of the Sierra Nevadas, in Convict Creek. So he had a passion for wild trout and fishing. After he left the Fish and Wildlife Service, he was head of the Oregon Fish and Game Commission in the state of Oregon for I don't know how long, a few years. That was just before he came to Berkeley as a professor. He came with the idea that he wanted to study what happened to trout in high mountain environments. And to enhance fishing. He didn't particularly like the way fishery management was going at the time, with hatchery introductions and so forth. He didn't think that was really self-sustaining. I think time has proven how visionary his insight was on that.

Meaning that he was interested in preserving the original species?Hughes:

Jones: Just natural systems—the original species composition, yes, not mixing things up.

Hughes: Did his work help to establish that principle, that yes, the original species could be self-sustaining if they weren't interfered with?

Jones: Yes, I think that's really an important contribution that Sagehen has made, that you can have a stream like this which is sustainable and productive.

Hughes: Was he thinking further than the benefit to fishermen? Did he have a wider perspective, that there was something valuable about preserving original species?

Jones: I think he must have, but I'm not sure it was apparent to me. He was interested in fishing, but not obsessively so. He seemed to be more interested in the fish themselves. He wanted to find out what the fish were doing, and it was a real challenge in this sort of environment to find out what they were doing during harsh weather conditions. He was especially interested in things like anchor ice, and related mortality factors.

Hughes: We haven't talked specifically about the fish observatory. I have read that one of the things that came out of that was not only more information about what fish actually do, particularly in the winter, but about ice formation. Please talk about those things.

Jones: 1953 was probably the first year we were trying to get into that, and I don't know that we made a lot of progress, partly because we had limited observation capability. We had the observation tank. It was not situated in such a way that the fish really liked to get up close to it. The water was shallow adjacent to the observation windows, and we did not see a great amount of fish activity there. But we went dutifully down into the

tank quite often, very early in the morning and in the evening hours, and took our sleeping bags and our heavy jackets, and laid there and took notes as to what we could see, and so forth. We took a lot of weather observations that winter, too, and got solar radiation measurements, and water and air temperature measurements. Those are laid out in that paper in *Ecology* that I did with Dr. Needham.

Hughes: Were you seeing correlations between fish activity and the solar conditions, or any of the external environment?

Jones: What the annual cycle of water temperature showed was that there was kind of a long period of spring acclimation when the water warmed up, and a long period of acclimation in the fall, when the water cooled down, which we hypothesized kind of prepared the fish for the harsh winter conditions, when they just didn't grow and tried to survive over the winter, and then the summer conditions, in which they got all their growth, and an opportunity to feed, and so forth. Sculpins were interesting; they had a very limited spawning season. All the spawning took place in June in about two weeks.

[End Tape 1A] ##

[Begin Tape 1B]

Hughes: What governed when that spawning period occurred?

Jones: I think it was probably water temperature. We collected gravid females full of eggs at one time, and then about ten days later you couldn't find any females that had any eggs; they had made their nests under the rocks, laid their eggs, and the spawning season was basically over

Hughes: What could you see happening in those acclimatization periods? Both the one that prepared them for the winter and then working up to the spring? Was it just a decrease in activity and then an increase in activity or was it more than that?

Jones: We didn't associate that much with what was going on with the fish, because we didn't have that much opportunity to observe them. We didn't measure activity of the fish; we just had the physical measurements, and we hypothesized what must have been going on. And we related this to things like hatchery operations. Sometimes hatchery fish are raised in one temperature, released in waters that have quite a marked different temperature, and you get a high mortality because of the shock of the temperature change.

Hughes: Do you think your work had an impact on hatchery protocol?

Jones: I think it's had a great impact over the years. I think people have come to the realization that you just don't mix things up in unnatural ways. If you do, there are consequences which you don't intend. It's kind of like the introduction of exotics; you introduce problems that you don't expect. Even with the same species in habitats that they're not native to, there are micro-differences in their genetics. They've adapted over the generations to specific conditions, and if you put them in different conditions, you're introducing a "non-native" population.

- Hughes: Were you yourself interested in the sculpin behavior per se, or were you thinking more broadly than that, that it was how the sculpins interacted with their environment. I got from interviewing Glenn Flittner that this latter way of studying nature was not what had been done previously, at least at Berkeley. You classified, and you brought specimens back for the museum, and it was more looking at the organism per se, but not at the organism's behavior and relationship to other factors and how the organism existed in ecology.
- Jones: I think I probably concentrated pretty much on doing my master's thesis just on the age, growth, and reproduction of the sculpin. I think in the background there's a real history in the Museum of Vertebrate Zoology, and also with Dr. Needham's ideas that were closely associated with looking at how these animals behave and interact in their environment. All the people I can think of from MVZ, the way they approached this, was very much environmentally related. They did an awful lot of fieldwork. It wasn't bringing specimens back to the museum so much per se, although they did a lot of that, and they had a wonderful collection, but there was also a lot of field work that was going on in understanding how these animals were living.
- Hughes: I had the feeling from talking with Glenn that there was some tension between certain people in the museum and the department in general about the way studies were being done here as opposed to how the people on the faculty might have conducted their studies. Were you aware of any tensions in that area?
- Jones: When Dr. Needham came to Berkeley, Dr. Harold Kirby was the chairman of the zoology department. I know Dr. Needham felt that he had full support from Dr. Kirby. After Dr. Kirby died, and with a new chairman of the zoology department, I don't recall exactly who it was, things were a little different. The department was more supportive of work in the emerging fields of molecular and cell biology. The tension was more a result of this difference in emphasis. MVZ probably experienced the same tension, but it was less obvious because of the larger, older, and longer-standing composition of MVZ compared to the newer, smaller fisheries group.
- Hughes: So it wasn't aimed just at Sagehen.
- Jones: No. Partly I think it was also the success that Dr. Needham had in getting outside financial support. At that time, I think most of the professors were relying pretty much on their own resources, the University's resources. For somebody to come in and suddenly come up with what seemed like fairly large amounts of money to come out and build a station out in the field, I mean, "What are you going to do with it? Is this a motel?" I think they lacked the understanding. I don't think that understanding can develop in just a short period of time. I think only maybe now, the University is beginning to fully understand the value of having a place like this, and having a long history of research that you can have only by having a facility like this.
- Hughes: Well, talk a little bit about the long-termness of the studies that have been here, and how they may differ from the sort of work that was done before, which I gather was: you did your research, you wrote it up, and then maybe you moved on. But there wasn't this long period of data collection in the way there has been apparently at Sagehen.

- Jones: Yes. MVZ people were doing a lot of this kind of research, but they were doing it in different places. There was a lot of understanding developed about how different species of animals lived, in different places, and so forth, and that didn't necessarily come together in an ecosystem sense the way that if you had entomologists, and mammalogists, and ornithologists, and geologists, and hydrologists, and meteorologists all working in the same area. Then you can begin to see how all these things interact together. But if all these people were doing their studies in separate places, it doesn't quite all come together in an ecosystem sense. I think the value of Sagehen, or any reserve area like this, is that you can have all these things going on in the same place. And you begin to get a fuller understanding of the interactions between them.
- Hughes: Is that the main selling point to the University? That the concept of the-multidisciplinary-approach-in-one-place has real value?
- Jones: I think that has value; I'm not sure it's the only value. In thinking about Sagehen and what it has contributed, I think first of all the research that's gone on here, and then, secondly the education that it's provided in training of researchers, and then, thirdly, the resources it's provided to the community in terms of education and ideas of conservation and so forth.
- Hughes: You mean the wider community?
- Jones: Wider community, yes, partly the local community, but the wider community too, just the lay people, to spin off what's come from that, and the conservation ethic that you can introduce into the public from having something like this. And you have something to say about it. It's like what Jerry Booth has written in the material for this meeting. There's a story to tell.
- Hughes: When did classes begin to come up here on a relatively routine basis? Was that after your time?
- Jones: That was after my time. There were short field trips up here in 1954. I remember coming back for a short field trip up here with either aquatic entomology or ichthyology, I'm not sure exactly which one. But in 1967, I believe there was a summer course up here that John Hopkirk was the teaching assistant for. John is actually up here this time.
- Hughes: Yes, I know he is.
- Jones: He is certainly someone who can contribute to your oral history, and I'm sure he'd love to talk to you.
- Hughes: Did your thesis pass with flying colors?
- Jones: I guess it did; it passed. It got accepted, and I wrote it up later for publication in *California Fish and Game*. So it's there in two places for people to look at. I think it added something about the biology of sculpins.
- Hughes: Do you have more in your notes that we should discuss?

- Jones: Probably so; I could say a little bit about the general maintenance of Sagehen. We talked a little bit about Gunnar Soder and his work crew that was up here during the summer, building. Another major project that summer was laying the water line from the spring up the road. I guess it's about a half a mile up the road. That water line was laid down so we had a continuous supply of water. What we would do, we'd just let it run all winter so it didn't freeze up. But that was a major project. One of the local people came out with a trencher and they dug a deep trench down the middle of the road and laid the water line. We had an engineer—again, this was one of Dr. Needham's friends, and he came up, was the supervisor from an engineering standpoint for laying the water line. I do not remember his name. Some of the other people may remember. I remember he was a man in his seventies or something like that, and he was so energetic and so enthusiastic, he was actually trotting up and down this half mile of road about six times a day, watching the installation, making sure it was done to specification.
- Hughes: My heavens. Was there an assumption that if you came up to Sagehen it wasn't just to do your own research, but you were going to be helping to build, literally to build this institution? I mean, to pitch in and help with things like laying water pipe, et cetera?
- Jones: I don't think anybody ever really thought about that. The MVZ students didn't do that, as I recall; Bob Hoffman and Joe Hall, were off doing their research, but I think those of us in fisheries, and this may have been mostly the high school crew, we did whatever there was to do. We may have helped Gunnar Soder and his crew with the building. We probably didn't do much with the water line, but I remember we installed the Kohler generator and rigged it up so it would run on propane instead of gasoline.
- Hughes: And you did all that willingly; you weren't saying, "You're taking away my research time"?
- Jones: No, I doubt it really ever crossed our minds, at least it didn't cross my mind to do that. When you ask me the question I guess, thinking about it, I was employed and paid during the summer, and I was also working on my thesis on my own time. But the two kind of got mixed up together. So, it was a job up here. So we did whatever there was that needed to be done.
- Hughes: And that's what Needham expected of all his students? There was a sort of blur between research and labor?
- Jones: I'm not sure. Elbert Brock and I were going to stay on during the winter, so we obviously had an interest in getting things shipshape, getting the supplies in, and so forth, and getting our skis ready so we could get out when we could no longer drive out. It was just all part of the experience. I never really thought about it as this and that; it was all part of being here.
- Hughes: It could have been otherwise. I mean, it could have been, you were a research person, and that's what you were here to do, and other people would have to—
- Jones: I think if you look at the history of the station managers up here, most of them had a research project that they worked on. Vernon Hawthorne worked on coyotes, and so forth. I think they all had their own research interests.

Hughes: Is that also true of Jeff Brown?

Jones: I'm not sure about Jeff. Jeff has made the point several times that he's not a scientist, so he may be delving more into just the station management. And when you stop to think about it, that may be a plus, because when Sagehen has gotten to the point where it has as much use as it has now, it may take a full-time station manager. Jeff, I think, has some unique characteristics that make him ideal for this; even though he's not a scientist, he's a very sharp, inquisitive person, and I think that he functions well in his job in this situation.

Hughes: Well, certainly the physical layout speaks to his good administration, doesn't it? The improvements in physical conditions.

Jones: Yes. Sagehen was getting a bit run down. I have visited three or four times during the years, and this is certainly the best condition that it's been in that I've seen.

Hughes: Was that deterioration largely due to the thinning stream of University financing?

Jones: I think that contributed to it, yes. And I think also after Dr. Needham's death, there wasn't anybody really to take hold—

Hughes: Not even Starker?

Jones: I can't really speak to that, because I don't know. I was gone at that time, so I don't know.

Hughes: Needham died in 1962—

Jones: 1964.

Hughes: '64. Okay. And you were long gone by then.

Jones: I left in 1959.

Hughes: Did you ever cross paths with Starker?

Jones: Oh, sure

Hughes: In the department.

Jones: Yes, I was a teaching assistant for his wildlife course.

Hughes: Well, tell me about him as a personality. What are your impressions?

Jones: Hard to describe. I don't know really what to say. I think some of his students could probably, and Dick Gard could give you a better description. Obviously, I was terribly impressed with all the people in the Museum of Vertebrate Zoology. I thought they were all just brilliant. And they were brilliant in their different ways. They were all characters. All extremely professional-type people. You can tell stories about each one of them. You can tell a story about Alden Miller. It is said that he would do his research

on birds, keep meticulous journal notes and records, and when it came time to write up a publication, he would sit down, write it once; that would be it. He never went back and had to edit anything; it was a perfect manuscript when he finished.

Hughes: Most of us can't do that.

Jones: Few of us can do that. Starker Leopold was I think a great combination of scientist and woodsman. He was a kind of outdoors person, and he must have picked this up from his father. He couldn't be more comfortable with a group of hunters or with a group of scientists or with a group of conservationists. He fit in with any of those groups and was dedicated and sincere in talking to any of those interest groups. I liked him very much. I liked Dr. Robert C. Stebbins, the herpetologist in MVZ. He did a lot of good systematics work in herpetology. Dr. Seth Benson was the mammalogist, another interesting person and good scientist. And Dr. Robert Usinger from the Entomology Department took an active interest in Sagehen.

Hughes: Dr. Frank Pitelka? Was he in the department?

Jones: Yes, as an ecologist, right. I didn't know him as well, but he was a little more reserved, but a nice person, always smiling.

Hughes: Did you feel close to Leopold? Was he the sort of person that you could develop a real friendship with, or was it always kind of the professor and the student?

Jones: I think he treated his students pretty much as equals. Again, Dick Gard, Bob Hoffman, Joe Hall and all the other people would be better sources, because I was not in the Museum of Vertebrate Zoology—I wasn't in that core group. Fish didn't get included in MVZ.

Hughes: Why was that?

Jones: I don't know why it was set up that way.

Hughes: Somebody [Bob Behnke] at lunch was saying—now who was it?

Jones: Stebbins?

Hughes: No, Starr Jordan.

Jones: David Starr Jordan?

Hughes: David Starr Jordan, who as you know was an ichthyologist, had laid down the law, and I can't tell you when, or why, that Stanford was going to focus on fish.

Jones: That's right. There was an original agreement between the two universities that Stanford was to do fish, yes, and Berkeley was to do the higher vertebrates. That was the reason why Dr. Needham, when he was hired, didn't get put in MVZ.

Hughes: So that pronouncement by Jordan only applied to the Museum?

- Jones: Dr. Leopold was interested in getting a fisheries program started at Berkeley and was influential in getting money to support a professorship to do that. So it was probably easier to slip it into the zoology department than to stick it in MVZ, which would have made it maybe a thorn in Stanford's side. Dr. Needham was hired at Berkeley as a full professor because of his background and reputation in fisheries. The budget for the new fisheries program also included a position for secretary. Ms. Pauline Shorb, a petite, grey-haired lady, filled this position. Pauline handled the administrative duties at Berkeley and for Sagehen. She also acted as "mother-hen" to the revolving group of Needham's graduate students.
- Hughes: And as far as you know, having fisheries in the department did not provoke Stanford?
- Jones: I don't know whether it did or not. It might have, but I think at that point, Cal was probably willing to thumb their nose at Stanford—
- Hughes: Yes, exactly. We've got the money, boys.
- Jones: We don't care.
- Hughes: [laughs] On a more general note, do you have any observations on the philosophy and general outlook towards the management of natural resources and the study of wildlife? This world that we're talking about has changed over your career. How are things perhaps different, or are they different from when you started out?
- Jones: I got my degree in 1959, and the 1960s was called the decade of oceanography, so there was the expectation that oceanography was going to just suddenly blossom, like NASA blossomed with the space program. It happened with NASA; it didn't happen with oceanography.
- Do you have any thoughts about why that was?Hughes:
- Jones: I think politically it seemed like a better sell to put a program in space, and a man in space, than to put a program in the ocean and people in the ocean.
- Hughes: It was more glamorous.
- Jones: It just seemed like it was more glamorous. I'm not sure that the payoff would have been that much different, whichever way it had gone. I was talking to Alex Glazer about that earlier today, and he was pointing out some conversations that he had had with some people, and in a sense they were saying that, "Look, all this spinoff from space research has really promoted environmental science. We're able to get spatial sensing and so forth." And that's true: without NASA money we wouldn't have done that. But I think that if NASA money, had been applied to ocean studies, it might have given very similar spin-off results. Let's see, what's your question now, Sally?
- Hughes: Well, I'm just trying to get at possible changes in outlook on nature, over your career. Needham's interest was in part as a fisherman. I'm speculating that today it's more the idea of preserving species and our natural environment, and it's not so much how things relate to man. Am I right in that?

Jones: I'm not sure, because I think there's still a very conscious attempt to try to manage resources for the benefit of people. I think in history we have taken some very short-sighted, incorrect ways of doing this. The fish hatchery program, for example. Salmon management, and so forth. So I think we're beginning to have some understanding that this isn't going to be an easy thing to do—you can't have it all immediately. It's going to have to be sort of a give and take process, and as we have more and more people, the problems increase in complexity. I think that there's just not as much to go around as there used to be. That's kind of what we're seeing in natural resources, that not only are we having to limit the use of resources, we're having in some cases to say, "Hey, we can't even use them at all, because the use of them is destroying them."

Hughes: But isn't that new?

Jones: It's a concept that has developed over a period of time, I think.

Hughes: The early nineteenth-century view was that nature would always provide, that there were resources that were endless, virtually speaking. Do you think that change in thinking had occurred, or was beginning to occur, before you stepped into the field?

Jones: I think so; I can see that in fisheries. Early on, people were saying that, "Hey, the oceans are not inexhaustible." They weren't acting that way, but there were visionaries out there who were saying this, even back in the 1930s and before. For example, with the Pacific halibut management, W. F. Thompson was pointing out that "You're overfishing stocks of fish," and put in programs to limit effort and bring back the halibut resource.

Hughes: How do you feel about current policy?

Jones: Well, there's so much current policy it's hard to say. I would say in terms of fishery management, as a whole, from my perspective, and looking at the U.S. Fishery Conservation and Management Act, I think that we have done a lot to bring about responsible management and allow a more rational use of fishery resources. Now that's mainly in the marine field, which is my particular interest.

[End Tape 1B] ##

[Begin Tape 2A]

Many depleted fishery resources are now recovered or are beginning to recover. I'm seeing strict Federal requirements for doing this, and I'm seeing the people who were involved with the resources—conservationists and fishermen—having to work together to decide how to do this, but having to do this within rather prescribed procedures. In other words, you've got to produce, you've got to get results, you can't just say, "Hey, we'll try to do better." We're actually measuring progress, and I'm hopeful, in the marine field, that we're stemming the tide and changing direction. In terms of overall human population growth, I think that we're facing some really difficult problems, because the more people you have, and the more you promote economic growth, the more you're going to consume. The more you consume, the more natural resources you use. The more natural resources you use, the more pollution you produce. It's a cycle,

and I'm not sure what the answer to this is. With marine fisheries, however, I am encouraged.

Hughes: Well, at least that's one area. Do you think you've left any holes in the story?

Jones: I think we've pretty well covered the points I can think of here. I can't think of anything else, really.

Hughes: If I asked you what you consider at this moment to be your greatest scientific contribution, what would you say?

Jones: I don't think I have made any great scientific contribution. I talk to people who I think have made great scientific contributions; I think I've done some interesting research, and made some modest contributions to knowledge, and I've been, I guess, what you'd call a middle manager in fisheries. I've done some very interesting things from my point of view, published maybe sixty papers or something like that. You can look on my curriculum vitae and see what's there and see what you think of it, but I wouldn't claim any great fame.

Hughes: [laughs] Well, people might disagree with you.

Jones: I don't think so. It's been fun. I have enjoyed my career in fisheries immensely. I think I got a good start at Berkeley, a good foundation in basic zoology, and I think the more time goes on the more I appreciated the people and the faculty I knew there. They were all extremely hard-working dedicated people. Starker Leopold, Paul Needham, Robert Usinger, everybody in zoology, everybody in MVZ. People in the entomology department, I just can't think of people working more diligently and conscientiously than those people.

Hughes: It was a remarkable group of people, wasn't it?

Jones: Really, really so. I think the graduate students then were remarkable people, too. I'm not sure that the students today have that same attitude, and I say that not so much from my observation, but just talking to other people who have more direct contact with graduate students. There seems to be a different attitude and different philosophy.

Hughes: Which is less dedicated?

Jones: I think so.

Hughes: Well, Berkeley students, and maybe students all over, are labeled, rightly or wrongly, as being more career oriented.

Jones: And I don't know really whether that's true or not. I think it's probably true for some people, but not true for all people. I've talked to some of the graduate students here, and they seem to be very dedicated. I think part of it is the insecurity of the world that we live in. When I got out of school, that's it, it was the beginning of the decade of oceanography. Nobody really thought about *if* you were going to get a job, it was just *where* you were going to get a job. And now, it's different; it's more a question of *if* you

are going to get a job. You may go through several postdoctoral positions before you get a permanent job these days.

Hughes: Well, thank you for your time. It certainly has been a pleasure.

Jones: And I've enjoyed it. I've enjoyed being at Sagehen. It has been a real great experience to come back each time that I have.

Hughes: I imagine you've seen some real changes this time, have you not?

Jones: This is quite different. This is the best. When we sit in a place like this, look out this window and see the squirrels running up and down the lodgepole pines.

Hughes: It's a special place. [laughter]

[End Tape 2A] ##

[End of Interview]



Richard Gard, approximately 1972, the last summer he spent at Sagehen

RICHARD GARD, PH.D.

[Interview # 1: 8-20-04]

[Begin Tape 1A] ##

Hughes: Please tell me about your family background and your education

Gard: Well, I was brought up in southern California, in a little town called Altadena, just toward the mountains from Pasadena. Later my folks moved a short distance away to La Canada, Flintridge area, and I went through the school system in Altadena and Pasadena, and went to Pasadena Junior College, now City College, and went through four years of that. So I spent the two first years of college there at the junior college in Pasadena. Then I went to Berkeley, as a junior, and at that time I was a pre-med major, and spent two years—that was in 1948--there, and I got a degree in zoology then in 1950.

Hughes: Still thinking you were going into medicine?

Gard: Well, I had applied and had not gotten in, and so I was looking at other options, and the one that interested me the most was wildlife management, and fisheries. It was a toss-up as to which one I wanted to follow through in. I didn't give up entirely the idea of medicine. But after I graduated from Berkeley I wanted to go on then into some graduate work there, and they said to me, "Why don't you go somewhere else for a year or so and show us what you can do." Normally they would take any of their graduates in, but they were starting to get a little more critical, and so I said, "Well, okay, that's fine, where do you suggest I go?" They said, "Oh, most anyplace." "Well, I can live at home and go to USC for a year." They said, "Fine." So I went down there and I went in to see Dr. Leopold then, and said "These are the things that I might take down there. What do you think are the important things?" He emphasized botany. I had never had any botany.

Hughes: He wasn't particularly interested in botany himself, was he?

Gard: Oh yes, he was. In the meeting you might have heard today that people that somebody was saying, even though you say you're a fisheries biologist you find out—it was John Hopkirk who said that—you find out you end up studying insects, you end up studying plants or something, that you never thought you were going to study. You had to know these things, because they were so interrelated with what you thought you were really interested in. So I went down there, and spent a year there, and I did very well. After the Zoology admissions officer at Berkeley reviewed my transcript, he said,

“Well, you did fine down there, come on back, we’ll take you in our graduate program. So I went back to Berkeley in the fall of ’51.

Just to follow up on this medicine thing--this isn’t really very important now, but it was then—a very strange thing happened. I’d gotten well into the graduate work at Berkeley, and had a master’s degree pretty well along, and was going to finish soon, and I happened to be down at USC. While I was a student there I became interested in a girl who was a teaching assistant in the USC Zoology Department. If I had a chance, of course, I’d go back down there. So I was down there for a long weekend, and I was studying in the Hancock Library while she was taking her doctoral exams. Of all things, I look up and here she comes walking in the door, and there’s some guy with her, and she pointed over to where I was sitting, and then she walked on over and said, “This is Dr. So-and-So from the USC Medical School; he wanted to see you. He came over to the zoology department and I was there, and I said I’d show him where you were.” I said “What’s going on here?” He stopped to talk to me there. He said, “I hear you’re interested in medical school.” I said, “Well, I was once.” “And you applied to us?” I said, “Yes, it couple of years ago.” He said, “Do you still want to go?” So this was a decision I had to make.

Hughes: Right on the spot!

Gard: I thought, the way it was presented, I had to make it right on the spot. So here I have this offer to go to USC Medical School, which I thought—well, it wasn’t a firm {offer}, but it was very suspiciously like a firm offer; otherwise, why would he come over and search me out? I’m sure that he had gotten word through the zoology department there that I was worth looking at. I said, “Gee, I’m well into a graduate program at Berkeley; I’m back there; I’m about to get a master’s degree—I don’t think I want to change now.”

Hughes: So you had the last word?

Gard: So, he left. [laughs] So I did have a chance to go back, but I must have been twenty-four...put four years on top of that, and at least a year interning, that’s five. I ended up spending more time than that as a graduate student, anyway, so it wouldn’t have really made any difference time-wise. So I elected not to do that.

Hughes: Have you ever had any regrets?

Gard: No, I don’t think so. Sometimes I do. I don’t know if I would have had a very good bedside manner, but I think I could have been very interested in medical research. Some of the things you read about today are just fascinating, what they’re doing. The way even something as cut and dried, pun intended, as surgery, isn’t that way anymore. There’s all these new things like laparoscopic surgery and so forth. I think that research in those things would have been fascinating. At any rate, I decided not to do that, and I decided to do what has always been my avocation, which is hunting and fishing, and anything related to the two.

Hughes: Had you grown up with a lot of exposure to the outdoors? Is that where it started?

Gard: Yes, I'm sure it was. My dad, Brant Gard, was an avid fisherman and quite an avid hunter, more a fisherman than he was a hunter.

Hughes: What did he do?

Gard: He ran a gas station. I worked in the gas station. He owned one, and he also had other business properties. He had a drive-in which contained several units that he rented out. There was a grocery store, a meat department, an entirely different place, and a fruits and vegetables department. There were those three things. Those old drive-ins, started in the twenties and became very popular when people started getting cars and they could actually drive in and park, and not park on the street. There was also a little restaurant there, and a radio shop, and mechanic's shop.

Hughes: He introduced you to the outdoors.

Gard: Yes. The war had started, and everybody who worked for him had to go to war. He was all alone there, and the war was going, so even though I was thirteen I had to start pumping gas, and moving cars, and doing things at a very young age. Kind of home mechanic things that have been of interest to me ever since then. But getting back to your question, yes, he was the one who, I'm sure, directed me toward fishing and hunting both. It's amazing how in the later years, he also loved coming up here [to Sagehen]. Oh, he loved it here. We'd go into the back country and go deer hunting, back over Carpenter Ridge and on back. He loved fishing; he'd fish all the time here. You could fish anywhere in Sagehen then, and you could keep them, and you didn't need barbless hooks. So, yes, we had a great time together.

Oh, what I was going to say is, I think a lot of parent-child relationships do this. The people who were your mentors, your teachers, your idols when you were a little kid growing up, when they got older, and you came up through life a ways, sometimes you got more proficient at some of the things they were teaching you than they were. And it ends up going the other way, like I became a much better hunter than my father was. I was telling him how to do it, in his last years that he could hunt, whereas in the first years he was telling me how to do it. I've heard a number of people say that's what's happened in the course of their lives.

Hughes: And he didn't have any resentment that you were the dominant one?

Gard: No, he was very easy-going that way, very easy to get along with. So we had wonderful times together. He actually did quite a bit of work on the station when he was up here. During the day when I was busy, he'd be painting the community room, or something or other. He knew Doc Needham very well. The three of us went hunting together at times, and so that was fun.

Hughes: What happened when you decided that medical school was not going to be what you were going to do with your career? You went back to Berkeley—

Gard: Yes, I went back. Did you interview Glenn Flittner?

Hughes: Yes, I did.

Gard: Well, he and I were at the same stage then. He was writing up his Sagehen stuff in the summer of '53, and I was writing up my trout taxonomy thing. We both turned in our theses at the same time, and both under Doc, and so we—what was the question?

Hughes: I was just asking what happened at Berkeley.

Gard: Well, we both lived in southern California, not too far from each other, our parents did. So when we got our theses turned in, we had a couple of hunting and fishing trips there, and then he went in the navy, and I went back up to Berkeley. Then it was what to do for a doctoral project. After working here with Al Jones later in '53, in August, I was talking to Doc about—"Those beaver dams down there, it's amazing what's in them, especially brown trout. I know there have been a couple of studies done on beaver-trout relations, and I think one ought to be done here at Sagehen." He said, "Yeah!" He was always very effervescent, very expressive when he got into a discussion about anything. He'd pound the table—very expressive man. He thought it was a great idea, and I thought it was a great idea to do that up here, and I talked to Starker about it, and he thought it was a great idea. So in the summer of '54 it got going in a big way. But I believe I did some work up here in the winter of '53-'54, later in the spring, but mainly it started in the summer. We had these sections, ten sections here, that are what used to be called pump and drain, but I prefer to call it diversion and drain. But Doc always said pumping and draining, which seemed to me was redundant, but we sampled the fish populations. That was the main study, and it was going to go on for ten years, and it did. We had these ten stream sections, but we didn't have any beaver ponds. I said, "Doc, we've got to add a beaver pond to this and find one where we can divert the water out of it, so we can drain it out and get the total fish population in there. He says, "Yeah, Dick, yeah, we've got to do that. We'll add one. You pick the best one down there, and I'll let you have the crew there, and you can add that to the pumping and draining this summer." So we did pump out a beaver pond down here not very far, just up from the rock slide area, and that first year, '54, we got all these great big brown trout out of there. Rosalie [a huge stuffed fish], who's on the wall, was one of them.

Hughes: Yes, I see Rosalie.

Gard: That's the first time we ever had Rosalie in our hands. Rosalie was bigger then than she is now; she's shrunk. She was twenty-four inches long and weighed seven pounds, and we took her every year up to '58, at which time she was shorter than twenty-four inches, maybe half an inch or something, and her weight had gone down from seven pounds to—I'd have to look up in the record book--but maybe five pounds.

Hughes: Quite a weight loss.

Gard: Yeah, there was a weight loss. That I think is quite well documented with fishes, but to have them lose size as well—I don't know why it should surprise us; I'm shorter than I was when I was a kid by about an inch, and a lot of old people get skinny. Not everybody is obese in this world. My dad was skinny when he was old, and I don't weigh as much as I used to weigh. But that's where Rosalie came into the thing.

Hughes: Do you think she was unusual? Living as long as she lived?

Gard: Oh, I think so. We got scales of course from her, and we knew how old she was, and I believe, if I remember right, she was eight years. It'll be in the record somewhere. She was pretty old. But that is unusual. Brown trout may live to be older than some of the others, but the brook trout don't live very long, and rainbow are somewhat in between brook trout and brown trout, the three trout we have here.

Hughes: In thinking about the beaver study, did you have an hypothesis of what the relationship might be? Were you pretty sure that there was going to be some kind of a relationship?

Gard: Oh well, sure there would be a relationship. I wasn't sure what it was going to be.

Hughes: More than just that the beavers would dam up water and there would be a pond there.

Gard: Well, that in itself is one of the main reasons that there is a difference. There were studies done in New England, in New York state in particular, on this topic, and the conclusions to those studies generally were that damming of streams by beaver result in decreased trout populations. The reason given is that damming the ponds there permitted the ponds to get warm, and they got too warm for optimal growth of trout, and so they became a detriment. The trout streams in New England are not as cold as our trout streams here in the Sierras. There hadn't been much of anything done in the Sierras, but there was some stuff done in the Rockies, in Wyoming. A study there on beaver-trout relations indicated that the beaver were actually beneficial to, in that case, the cut-throat trout there in the Rockies, the stated reason being that the streams were probably too cold for optimal trout growth. So if there was a little bit of warming, that was a benefit to the growth of the trout there.

Our conditions here are much like they are in the Rockies, and so the hypothesis, if I were to make one, would be that beaver are of benefit to trout in Sagehen Creek. So, let's either prove or disprove that. And that's what I set out to do and tried to do it objectively, even though I thought that might be the way it would come out. My conclusion was that beaver here have definitely been a boon to the trout fishing in Sagehen Creek, the reasons being, one, just the physical damming of the stream gives you much more area of water. When you dam it you get more depth of water, so the volume of water is greater to begin with, and a lot of the trout foods come from the bottom; they come from insects that live in the bottom. So if you have more bottom, which you do in a beaver pond, versus what that section of stream was like when it was a stream, a typical stream, then you have more trout food there, and presumably if you have more trout food, you'd get more pounds of trout out of it.

Hughes: And you did?

Gard: And we did. We got not only more pounds, but we got more in numbers. There is a difference in the species that you find; you don't find rainbow quite so much in the deeper ponds. They're in the more typical stream sections, since they seem to do better there. But brown trout just love them, and brook trout do very well in deeper ponds, so brook trout and brown trout populations just went way up. How do we know they were way up? Well, first we didn't really know that was so. But when we started pumping that one beaver pond out, you could compare the pounds and numbers of trout in there per unit of area to a similar stretch of stream that wasn't ponded that we were also

sampling; we had ten other sections in there, and the beaver pond was just far and away above the poundage per unit area that we'd find in stream sections.

Then one other event, which really clinched it, was that we had a great flood on December 23, 1955, and most of the beaver ponds that I was studying washed out, and the stream went back, much of it, to a typical stream section. So here was a marvelous before-and-after thing to let them stabilize for one year. And then we went in there, and we didn't do the diversion and draining of sections that used to be beaver pond, but we took areas there, and then we did electric shocking of the fish in there to get the numbers and weight of the trout in there. The numbers had just [dropped] way down below what we got in the same patch of stream when it was a beaver pond.

Hughes: Electrical shocking just stuns the fish?

Gard: Yes.

Hughes: And then they're fine after it?

Gard: Usually. There's some mortality, but if it's done with direct current, which is what we've always used here, trout mortality is minimal. We haven't used alternating current because it is more harmful to trout than direct current and it is potentially more harmful to the person doing the stocking. If you fell into the stream and you were using alternating current, you might be electrocuted if there were many electrolytes in the stream. But with direct current it's not quite so dangerous, but still it's dangerous, so you of course have various safeguards. There's somebody by the generator who could push the stop switch, and also you have a wand that you move around under stream banks and around rocks, and all you'd have to do is stick that wand straight up in the air, and then there'd be no current flowing. You can't shock fish unless you have lots of ions in the water, and sometimes you think you're going to go in and shock a place out and you can't do it, and then if you test the water you find out there's nothing much in it in the way of ions. Also cold water transmits electricity better than warm water, so if you're going to shock, shock in the morning and not in the late afternoon when the stream warms up a little bit. But at any rate, that's how we did that.

Also, along with fish populations, there were stomach analyses, extensive analysis of that and the bottom fauna, the organisms that live in the bottom. We took many, many dozens of samples of those, and there are much higher weights and numbers of the larvae of insects, in a beaver pond bottom than exist in any stream section--here in Sagehen.

Hughes: But not necessarily elsewhere?

Gard: Maybe not elsewhere.

Hughes: Was that a somewhat controversial finding, to find that beavers were actually good for trout populations?

Gard: It wasn't completely unusual, because as I said, in Wyoming there had been a study done which showed that they were of benefit. But anybody from the East who knew

about the studies back there, they would be surprised. I would say in general that there was some surprise that it was as clear-cut as it was.

Hughes: Were you surprised?

Gard: I thought it was going to come out that way, but I didn't think it was going to be as decisive as it was. Of course, I was happy that it was, because so often in biological studies, you can't reach a very definite conclusion. In such studies your conclusion is qualified, but I didn't have to qualify that except to say, "In Sagehen Creek, beaver are of benefit to trout." And let it go at that. And probably it would be so in any other cold stream in the Sierras.

Hughes: I heard today that the beaver originally were introduced.

Gard: Yes. They were discussing the possibility of that not being noticed, but as far as we know they were introduced.

[End Tape 1A] ##

[Begin Tape 1B]

Gard: I suppose people who were trapping beavers for the pelts would have been in favor of introducing beavers. Another reason that they might have wanted to do that is to impound water. It's a very cheap way to hold water back. At no expense you can accomplish what you can accomplish with a man-made dam, by letting the beavers do the work. It holds water so that the water table would rise in the meadows, say, surrounding the creek and that of course is beneficial if you have grazing, which we had. Thank God we don't have it now. Those sheep were very destructive, especially the upper watershed here. We're going to look at some of the upper areas tomorrow.

Hughes: When did the sheep stop coming in?

Gard: I don't know exactly when.

Hughes: After your time?

Gard: Yeah, I think it was after I left [1962]. We kept trying to get them out, but they weren't out. I'm trying to think—I know, in our upper creek studies, the sheep were still in there. It had to be after 1961, some time after that. It wasn't all bad, because there was a very nice Basque family that ran the sheep up here. Every Fourth of July they'd butcher a sheep and bring it down and we'd have a big barbecue down here. We'd barbecue this lamb on the bedsprings right out there. Salvador Urutia was his name, and he and his Basque shepherders, they could hardly speak English. Salvador could speak English, but the people who worked for him were brought right from the Pyrenees, and they couldn't speak very much.

Hughes: And were they out for the whole summer?

Gard: Yes, they had a camp in Kiln Meadow. They'd bake their bread in that kiln; that's what Kiln Meadow is named after. They had a stone kiln in the middle of the meadow, and

they had a camp right where you cross over, I guess the bridge is still there, over at Kiln Meadow, and they had their camp there in the summer.

Hughes: Did they hunt and fish?

Gard: I don't think they ever hunted. I'm not even sure if they ever fished.

Hughes: What were they eating, then?

Gard: I guess they were eating sheep. [laughter]

Hughes: That seems sort of self-defeating.

Gard: Well, you're right; there'd be fewer sheep to sell. I know they were eating sheep down here on the Fourth of July. They'd bring their *la botas*, you know what *la bota* is?

Hughes: Yes.

Gard: They'd bring those down, and fill them with red wine, and then we'd all try them out, and they were very adept at that; they wouldn't miss a drop. There was a wonderful picture of Doc doing that. He's trying to squirt that thing, and all he did was put a bunch of red wine right down the front—

Hughes: —of his shirt.

Gard: Yes, I think Joe Hall took that picture. I can see the picture right in my mind's eye. That was funny. It's too bad you aren't able to interview Joe Hall, because he's a marvelous person, and he worked here on beaver also. There were three of us, Leopold's students, who worked on beaver. Dave Taylor, Joe Hall, and myself. Have you met Dave?

Hughes: Yes, and I'll be talking to Dave tomorrow.

Gard: Dave worked on beaver populations and migrations. I'm sure the longest study ever done—he'll talk about this—on beaver populations. I meant to ask him—I haven't gotten around to it—whether he's still doing that. He came up every year on his own, and did that; he was always doing it on his own, because he never had any employment here. He was an airline pilot; he's retired now. I think he did take leave of absence—you'll have to ask him this—for either one or two semesters. He was taking classes on the Berkeley campus there. I was teaching the fisheries class in 1958. I had just gotten my doctorate and [it was] the first teaching assignment I had, and Dave happened to be in my class. Strange to have him in my class. We worked together up here, but that's what happened. And Joe Hall was doing the beaver food supply; his doctoral thesis was "Willow and Aspen in the Ecology of Beaver," something like that. Aspen would be their prime food, and then willow would be a close second. So that's what he did. But Joe had polio as a child and got over it and has been able to walk fairly normally for decades. He ran around here; he was always a little bit frail, never had an ounce of fat on him or anything, but he could carry a pack around, and run around on top of beaver dams—you had to. He and I worked together a lot, because we were working on the same dams. I'd help him, and then he'd help me, and so we had a lot to do. But his polio has come back on him, and he can barely walk now, and that's why he's not here [at the

fiftieth anniversary of Sagehen]. I talked to him about a week ago. He lives in Grand Junction. I'm sure he'd be here if he could. He said he could barely walk across the bedroom now. So his last years are sort of a repeat of what many of his younger years were, fighting the same disease with a lot of the same symptoms.

Hughes: Doesn't seem fair, does it?

Gard: It doesn't. Well, Joe would be a marvelous person to [talk with] not only because he was around here even longer than I; he didn't actually work for a living here, as I did. Taylor didn't work for a living here either, he took a class up here, and then did his beaver work, and he and his family were up here camping, down there at Taylor Meadow, we called it, just down from the camp a little ways there. Joe had the best wit of anybody of that era. He always had something very clever and poignant to say. He was a great good friend, and I'm so sorry that he's having these troubles now in later years. He's a bit older than any of the rest of us here; I think he's four years older than I am. He must be eighty-ish.

Hughes: He had no connection with the University?

Gard: Oh no, he was a graduate student. He got a doctorate and then he taught at San Francisco State. That's where he went after he graduated.

Hughes: Well, tell me, because you were the one, I understand, who was largely responsible for writing up Needham's studies. There's a story connected with that, I understand.

Gard: Probably many.

Hughes: The one I'm thinking of is when you were sailing around the world or some such.

Gard: How much detail do you want? Shall I just start off on the sailboat, or shall I even go back before then?

Hughes: Start off on the sailboat, because we're running a little short of time.

Gard: Okay. My wife, Sylvia Gard, and I—I was working in Alaska--and we'd actually got married up there. It actually started when we were down here. She was a student here while I was working here, and she took botany here one summer, 1960, and I was running the field end of the program in those years. Any rate, she came to Alaska, and we got married, and one of the questions I had for her before asking her was, "What would you think of buying a sailboat after a few years, and sailing around the world?" She said "Yes, yes, yes! That's what I want to do!"

Hughes: No hesitation.

Gard: She didn't hesitate for a microsecond. So we did just that. I worked for the Fish and Wildlife [Service] in Juneau, Alaska at that time, worked for a year, and she taught in the high school there. Then we bought this boat, in southern California, actually, and took it back up there for the last year, and then we lived on the boat in Juneau during the winter of 1965-66, one of the coldest winters on record—

Hughes: Wow!

Gard: Then, the next August we sailed away from there, we both quit our jobs and sailed away and came down, spent some time in the Bay Area, where she's from. I've spent so many years at Berkeley I almost feel like I'm from here, too. We started—the idea was to go around the world. We did not go around the world, but we did have some wonderful trips. First, we went to Hawaii, and then the Marquesas, Tuamotus, and Societies, where Tahiti is, and on through the Cook Islands and so forth, and Tonga, and then down to New Zealand. We spent the hurricane season in New Zealand. We were going to take off from there and go across the Indian Ocean and go through the Suez Canal. Well, while we were in New Zealand, they closed the Suez Canal because the 1967 Arab-Israeli war had started. Since the Suez Canal was closed, so we couldn't go through there, and the alternative was to go around South Africa. We thought, "Well, that's going to make it a lot longer," and then we had some medical problems. My father was sick, and I was going to have to make a trip back—I don't know, just the combination of having our trip extended, without being able to go through the Suez Canal, and sickness at home, we decided that we just weren't going to do what we set out to do.

Hughes: Had you done any sailing up until this point?

Gard: A little bit, but not extensively. My brother had a twenty-foot sailboat. I had sailed it from LA Harbor there, San Pedro, to Catalina Island. My family had a house at Avalon, on Catalina Island, so I spent summers there when I was a kid, and I loved it there. At any rate, my brother had this little boat; I used to sail it when we were there, but that didn't involve any navigation.

Hughes: So how did you learn how to navigate?

Gard: Just by doing it, I guess. He had been in the navy, and he knew something about boats—

Hughes: I meant you.

Gard: I learned from him. (he's a lot older than I), by reading, and just doing it.

Hughes: And it was enough? You didn't get into any trouble?

Gard: With my big boat? Oh yes, we got into trouble. Quite a bit of it. It's quite a jump from a twenty-foot boat to a forty-two foot boat; it's more than a quantum of two times; it's quite a bit more than that, 'cause you're dealing with much larger sails, and weight, and everything that goes along with it, so there was an awful lot to learn. We brought the boat up to Alaska, and learned a lot about it on that trip, though a good share of that was motoring, not sailing. You don't sail to Alaska up the coast in summer, because the winds are going against you all the time. The way you have to sail from southern California to Alaska is, you go about halfway to Hawaii and then turn and go north, and then catch the westerlies and come in. You can't sail straight up the coast. Although the square-riggers did it, but then they went way out to sea, on long tacks, and it took them forever to get up the California coast. It's raging along there; you think California, everything's quiet. It isn't quiet.

Hughes: It's not pacific.

Gard: You get hammered at Point Conception, Point Sur, Point Reyes, or up in Oregon what everyone calls Cape Blanco, but it should be [pronounced] Blahn-ko. I don't know why we don't pronounce things the way the Spaniards do, but we don't; we don't say "Sahn Frahn-seesko," do we?

Hughes: No.

Gard: Anyway, on that trip up I learned a lot. I taught myself how to navigate. Tried to take a class in it, and the instructor got drunk and never came back to the class about the time we were getting right into it, so I didn't learn a thing from him. So that didn't help. I just read about it, and while we were getting the boat ready to leave from San Pedro, no, we were down at Newport Beach then, I'd go down to the beach there at noon and take sights of the sun standing at the beach, and see if I could tell where I was on the shore from that. Of course, I knew exactly where I was, so when I ran the sights, I knew if I was wrong or not. I found that I could do that. The next thing is, maybe you can do it standing on dry land, but when you're out at sea with everything rocking around and you start taking sights with your sextant, how in the world do you know when to take it, because you're getting all sorts of different measurements. That's one of the hardest parts, celestial navigation by the only method we had then, to use a sextant. But getting to your question—this is too much buildup isn't it?

Hughes: Yes.

Gard: We had got to New Zealand; we were coming back, and we got into Hilo, Hawaii, and there was a letter waiting for me from Starker. He said, "I want you to come back here and write up Needham's work and get it published. I've got nine months of salary for you at associate research zoologist level." How could I turn that down? Here I can go back to Berkeley and work with my idol, who's the man that I most respect of anybody on this campus, and write up things that I did the major part of collecting. I snapped that right up, because I didn't know what I was going to do when I got back. Don Seegrist had used that material for a doctorate, but he wasn't getting it published, I guess, and Starker didn't think that he was ever going to do it, so he literally gave me the material to work up. We actually divided it into two parts. One was the effect of floods on trout, and the other was the Sagehen trout fishery, natural trout populations and how a trout fishery went during these ten years. Seegrist and I both have our names on both papers. "The Effect of Floods on Trout" has his first, Seegrist and Gard. The big one, though, has my name first and Seegrist second, though he did some of the work as his doctoral work. Of course, Needham originally planned to write it up himself, but he didn't live long enough to do it.

Hughes: And you were working from his raw data?

Gard: Yes, and from some of the things that Don Seegrist had worked up.

Hughes: How long did that take you?

Gard: I worked on it for a year. I couldn't work on it all the time. I think I started January 1, 1969, and then Starker said, no! "I'd like you to come up to Sagehen and teach the fisheries part of the wildlife and fisheries course next semester." Every other year a wildlife and fisheries class was given here, a field class in wildlife and fisheries field

methods. He said, "I want you to do that. I'm going to teach the wildlife part of it, but you come up and do the other." I said "Well, sure, I'd love to do that. It's something I can handle beautifully, because that's all the kind of stuff I've been doing the last fifteen years, the things that you want me to teach." So I taught that class in '69, along with him. Then Starker ran out of money and couldn't keep me on, but I kept working on the manuscripts anyway.

I got interested in gray whales, and interested in Mexico. While we were talking about that I said, "Starker, what do you think about sailing down to Scammon's Lagoon and do something on censusing gray whales?" This caught his interest, so he was influential in getting me a series of grants to go down there. Very modest support, but enough to keep us alive. It wasn't large amounts of money, like maybe \$3,000 or something, which kept us going there for a year on our sailboat, so we went down and did that. All the time we were doing that I was working on the Sagehen stuff, and then came back and worked on it here at times, but often without pay or anything, just did it because I wanted to get it out. It finally came out, and Starker was as much help as he could be. He was a very busy man. He had more things going than anybody has any time to really do. I wonder how in the world he ever did it. The only answer is, he's Mister Efficiency, or was; he was so efficient in the way he budgeted his time and that he could do all of the myriad things—had people all over the country pulling him in this direction and that direction, to come to this meeting, or champion this cause, or whatever. He was president of the Wildlife Society, and just all kinds of things going on all the time.

Hughes: Did he usually say yes to those invitations, or was he discriminating.

Gard: Well, I think he said yes to too many of them. Life/Time got him to do one of those Life/Time series on the desert, so he wrote that for them one summer, for pay. But he was involved in other things. He helped me; Ken Bechtel of the Bechtel engineers, Ken wasn't an engineer, but he was the one who did the insurance of the Bechtel stuff. Starker knew him, and he liked Baja California, so this worked in beautifully. Starker helped me get a proposal in, and Bechtel funded that a couple of times.

Hughes: Was Starker available to you when you needed him?

Gard: Yes.

Hughes: So you didn't have any trouble with access?

Gard: No, he was always available if I had anything on my mind. So was Doc Needham available. They were two unique professors, now that I compare them to others

[End Tape 1B] ##

[Begin Tape 2A]

Gard: They didn't seem aloof; they didn't try to dodge their students or anything. Some of the professors at Berkeley are hard to approach. They get so engrossed in their own little—well, not necessarily little--in their own interests. Then of course, they deal with their own graduate students, but if it's anybody even a little bit off to the side they don't want to spend their time with them. Both Doc and Starker were very generous with their

students, or with anybody who might walk in and want their help or their advice or whatever.

Hughes: We could probably say a lot more, but I think we'd probably better wind up.

Gard: What time is it? Oh, it's 9:20 [p.m.].

Hughes: What are your thoughts on any changes in the attitude towards natural resources and wildlife over the course of your career? Have you noticed changes in how people regard them?

Gard: I certainly have, and that came out today. Did you sit and listen to some of the [discussion this afternoon]?

Hughes: Yes, I did.

Gard: I think it's absolutely true. When I started at Berkeley [1948] there were quite a few of the old-time naturalists there, people who did a lot of field work. You could just name them off. Not only was there Leopold and Needham, but there was Bob Usinger, the aquatic entomologist, who seemed to be on everybody's committee in fisheries, including both of my committees. Great field person. Herbert Mason, in botany, who worked on the bog here, which now is called the Mason Bog by many of us, maybe all of us; it's right over there. There was Seth Benson, who was the mammalogist; I'm sure he's retired; I don't even know if he's alive; do you know Seth Benson?

Hughes: I don't, no.

Gard: He was a field person, and there were some other people around like Ward Russell, who was the preparator for the museum there. He was always out in the field doing things with Starker. I have to mention Alden Miller, who was head of the MVZ [Museum of Vertebrate Zoology] for, I think, all of the years that I was a graduate student there. He was definitely field oriented and the old naturalist type. Like Joseph Grinnell, who was Starker's major professor at Berkeley, or Tracy Storer at Davis. It was changing, and it changed gradually, of course, through time. It went from the old-time naturalist type field people to laboratory people, and it went from field work to laboratory work, and there was a big change in the mathematical methods that were used, statistical methods. These old-time naturalists, they didn't know very much of anything about statistics, and now that's become very important. Of course, computers were nonexistent, practically. There was one at Berkeley, but there were only six of them in the whole United States in the 1950's.

Hughes: My heavens!

Gard: In the early fifties, six computers! Ancient things, that would fill whole rooms full of vacuum tubes. Now, you can do more on a laptop than they could do in these rooms full of vacuum tubes on the Berkeley campus. The methods of analysis changed so much, so much of the field work wasn't done in the field; it was done in the laboratory. The behaviorists worked in the laboratory now on different things; they didn't go out and study things in the wild very much. The whole complexion of things changed. Some people, maybe their fields didn't change so much--people like Curt Stern, the geneticist.

I suppose the statistical methods changed, but I don't know that the current person teaching genetics—well, there would be changes, even in that too, sure, ever since the discovery of the double helix and—

Hughes: All that molecular biology.

Gard: That's it! That's been changed, and there's so much now on electrophoretic work, DNA, and so forth. Yes, that's changed. The behaviorists I think have tended to move into the labs more than they used to. They used to go out in the field and watch the behavior of things in the field. I think it's changed.

Hughes: Do you think of yourself as a field biologist?

Gard: Yes.

Hughes: What do you think about these changes?

Gard: I think that they think that field biologists, the old naturalists, are an anachronism and are not very important anymore, that what they're doing is the more important thing. I'm not sure that the feeling is so strong the other way around. I think maybe we recognize the things that they're doing are important, but those of us from the old school also try to point out that it would be too bad if field biology ceased to exist. There are a lot of things that you cannot take into the laboratory and study very well.

Hughes: Like a beaver dam.

Gard: You can't study the beaver dams in the laboratory. They both have their strong points, but since I belong to the old school I guess I favor that. It's amazing some of the discoveries made by the old-time naturalists that were made with almost no equipment or methods or anything but their notebooks and their methods of observation. I think that one of the most outstanding examples of that, and there are many, but one was by a fellow named [Karl] von Frisch, a German who studied bees. He wrote a book called *The Language of the Bees* [*The Dance Language and Orientation of Bees*. Harvard University Press, 1967]. His equipment was a little pen, a little brush, and some white paint. He'd put little daubs of paint on these bees, and then he'd follow them around, and follow where they'd go, and the way they transmitted language from one bee to the next. It's just unbelievable. All of this was done with the crudest of methods. He discovered amazing things. A bee would go out and find honey, and then come back and do a dance around the top of the beehive. They do it in an ellipse, and the orientation of this ellipse pointed toward where the honey was out there, and the frequency of the dance, of the steps that they did around there, indicated to the other bees how far it was. And then they've done work since then on how do the bees navigate out there, and they've used plain polarized light. The basis of all this was done by a guy with nothing but a paintbrush and a very keen mind and good observation.

Hughes: And patience.

Gard: And patience; that's right. That would never have been discovered inside a laboratory. I've picked one of the best [examples], but there are plenty of others you could say, just like your beaver dams that you pointed out. Luna Leopold [Starker's brother] did both;

he did field work—he wrote a book about the meanders of streams, and how the meanders form. And all of it's quantified. He did set up artificial streams in the laboratories and then ran streams down different substrates at different slants to these things. Very pioneering work, but part of his work was field work.

Another guy on the Berkeley faculty who was very human was, I've mentioned him, Dr. Benson, the mammalogist. He was a very unique person. He came up here a lot, that's why I bring him into it. He brought his mammalogy class up here, and I would go out with them, because I'm interested in mammalogy. He had time for the graduate students. They didn't have to be his. I thought that was amazing. You could go in to see him at anytime. He'd talk to you about your research project, help you out with it. I remember I went in with a study I did on insulation of a Sierra stream by snow cover, which was done right on Sagehen here up above. You get snow over a stream; how does that protect the stream underneath the snow cover in winter from the cold temperatures? And what relations does this have to the trout that are in the stream? Is it helpful to them, and so forth? I went in with this manuscript on that. That isn't anything that he ever studied, but he was a very smart man and very accommodating. I asked him if he'd review my paper, and he said, "Sure." He reviewed it, and had some very insightful things to say about it. He was a field biologist, of course—almost had to be. Avid hunter, he and I went hunting up here. He'd come up; he'd stay an extra day or two, and we'd go back up the mountain together. Strong hiker, he was a boxer; he was a Stanford graduate [laughs].

Hughes: I think we probably better stop. Do you have anything you want to say as a wrap-up comment?

Gard: Well, yes. I think that the people who started this place, who were primarily Dr. Needham, Dr. Leopold, and, tertiarily, Bob Usinger, deserve a lot of credit for putting this together, Needham most. I really can't thank him, and them, enough for doing this, because it's made a wonderful facility available for graduate students, and also a place where faculty can come and do their research, like Dr. Mason, the botanist, who worked up here. I say, "Good on all of them," and I thank them so much for what they did for all of us who were graduate students then, and for what we gained here.

I guess my other comment would be, "Don't sell the field biologists short," even in this age of computers and DNA work, and all of the fancy gadgetry. A lot can be done without that, although some of the equipment has definitely been a boon, like these pH meters where you can just stick something in and turn a dial and get the pH instantly, or these things that can tell you what the alkalinity of the water is, or the amount of oxygen in the water. We'd bring these bottles back to the lab and have to do titrations and all this chemistry and everything to get a little answer. Now—bing!—you get the answer. So there are advantages that the field biologists of course grab onto too. They take these machines out in the field, and it means with these aids that they can do a lot more than they could do before.

Hughes: Well, I thank you.

Gard: Oh, I thank you for the opportunity. I'm afraid I just rambled too much.

Hughes: No, there's a lot of good information here.

Gard: It's fun for me to reminisce and think about these things, because Sagehen was a very important and interesting part of my past. When I started working here I thought, "This is it; I'm going to spend the rest of my life right here."

Hughes: But your life went on to other exciting things.

Gard: One thinks that, but then there are times for changes; I think that one probably should make a change from time to time. Not too regularly, or you're always just learning; you're not producing. There's a certain period it takes you to learn about what you're doing, and then produce, and after a while maybe you do get kind of jaded, or set in your ways. Maybe somebody new should come and take on while you go out looking for new challenges. But first I thought, "This is where I want to stay and work, in this glorious place and get my gold watch right at Sagehen."

Hughes: Well, you did ten years.

Gard: I did quite a few.

[End of Tape 2A.]

[End of Interview] ##



Robert Behnke, 2001

Photo: Clarke Photo, Fort Collins, Colorado

ROBERT BEHNKE, PH.D.

[Interview #1: 08-21-04]

[Begin Tape 1A]##

Hughes: Let's start back with your family background and your education. Would you tell me?

Behnke: I was born and raised in Stamford, Connecticut. Both my father and mother were second or third generation Americans, but none of their families had ever had any higher education. Most had finished high school, but then the typical route to live a respectable middle-class life would have been to learn a trade. So my father was a draftsman, model-maker for the Yale & Towne Hardware factory in Stamford. So when I graduated from high school I went to work for Yale & Towne. My brother, like my father, was much more mechanically oriented, I would say, could do things with his hands, and I couldn't. I had no interest in it. I loved the outdoors; there wasn't much of the great outdoors in Stamford at that time--twenty acres of open space in back of the house. There was an old mill pond, so I got obsessed with fish at a very early age, life under water. I had no concept of pursuing that as a career. That opportunity didn't come 'til I was drafted into the army in 1952.

When I came out of the army in 1954, I was going to go back to work for Yale & Towne, but they were moving down South, and I had no job, so I went to the Veterans Service. I got counseling from the Veterans, what do I do with my life now? I have no job. So they gave me all kinds of tests, and I talked to a counselor, how I just loved fish. I just studied, read everything I could about fish. He said, "Well, you can go to college and you can study fish." I said, "Oh, I didn't know that." So I had the GI Bill coming to me. I said, "Well, I'll do that." At that time you got a hundred and ten dollars a month for thirty-six months; that was supposed to be enough for a four-year college degree. But you couldn't afford much tuition, so I went to the state college, the University of Connecticut at Storrs. And actually I got a pretty good education, and probably because most of the faculty there were very sympathetic and encouraging for me, even though there was no great expertise about fish there. So that caused me to learn more on my own, which was very good. What I did was, I would take twenty, twenty-one credits a semester, so I could graduate in three years. I still had twelve months of GI Bill coming to me, I could apply to graduate school. I was just so enthused over college; I had a distinction degree and Phi Beta Kappa. My credentials were very good, so I started to apply for graduate school, to study fish. I had lots of response, people trying to recruit me. Paul Needham called me from the University of California, and he said that he just has this grant, and he's going to take off in June to make a round trip from California to Alaska and back. He'll be collecting cutthroat trout and rainbow trout that were going to

extend his study that he did with Dick Gard on Mexican trout, just finishing up, and were going to extend that to Alaska and all over the western United States. That call, boy, that sold me, so I was on my way out. I remember I didn't even wait for graduation. I had an old Willys car and I drove it out [laughs]

Hughes: And you drove all the way across the continent?

Behnke: Yeah, made it out. Had a few flat tires and so forth. I got here and almost right away we took off. We spent the first day or so at Sagehen here, that was my introduction to what was going on here.

Hughes: This was where you came first?

Behnke: Yes. From here we went up to Idaho, Montana, British Columbia. That was my first introduction to wild trout. You see, coming from the East and Connecticut, I always thought all trout came out of hatcheries, and I wouldn't make any distinction, a trout was a trout was a trout. What kind didn't matter. But I got so enthused over wild trout. That was the whole thing that Sagehen was set up to demonstrate, that wild trout with good habitat could maintain a very good fishery, quite a satisfactory fishery, without stocking hatchery fish. That was the premise of Sagehen, that was why it was built.

Hughes: And that came from Needham.

Behnke: Yes, that was Needham, because he had some experience there. He had worked with the Fish and Wildlife Service back in the 1930s, and early '40s. They had a Convict Creek Research Station where they had an experimental stream down there, so he kind of transplanted that idea up here. Anyway, the idea of catching wild trout was something. I actually spent the whole summer going all the way up to Alaska, and we got flown around, and I'd go back in the fall.

Hughes: And you were looking for wild trout on that trip?

Behnke: He and I were going to co-author a book on the finds. We brought back thousands of specimens. This was before the era of modern genetics. I just would interpret their evolutionary history from their morphologies and look at some geographic, geological background, supplemental information, putting together the story of the evolution of the western trout, the rainbow and cutthroat.

Hughes: And you could really do that with the education you'd gotten in Connecticut? How did you know enough?

Behnke: As I said, most of it really depended on—the University of California library. I went to the Bancroft Library. The library, that was very essential. But essentially you become self-taught, because you're not going to learn this stuff in classes very much.

Hughes: Did Needham pass on quite a bit of information to you as well?

Behnke: They were just getting ready to publish what's called the Needham and Gard University of California publication on the rainbow trout in California and Mexico. We were going to take off from there and go to the rest of North America, which included enormously

more material. After '57, every summer, '58, '59, '60, I'd be up at Sagehen but we used this as a base for field trips, collecting in Nevada and the Great Basin. The next logical step was okay, wild trout: Sagehen Creek was demonstrating wild trout, how you can give them good habitat. You've got lots of fish, and anglers are happy with them. But I recognized that hey, the brook trout, the brown trout, and the rainbow trout in Sagehen are not native; they're not native fish. The Lahontan cutthroat was the native trout here. And at the time, in the literature, it said they were extinct. They didn't exist anymore in the pure form. They'd been replaced by non-natives. The Lahontan Basin is very big, however.

Hughes: Replaced or interbred?

Behnke: They're replaced by brook trout and brown trout, and they interbred with the rainbow trout. There's no doubt they were very rare, but I couldn't believe they were completely extinct, so I would run down leads. I think it was 1959, I was up at Sagehen at the end of the season here. I was going to take a week and go backpacking into the headwaters of the East Carson River to run down some leads I had on perhaps cutthroat trout still existing in a couple of little creeks up there above waterfalls. That was my first real backpack trip. I remember putting the backpack on—I did things like stick a six-pack of beer in there—crazy--

Hughes: [laughs]

Behnke: —because I had to go over two thousand feet the first pass I went over. Boy, you could imagine I learned my lesson about packing light. But I got there and sure enough, I found two tiny streams that had the native Lahontan cutthroat. I brought the specimens out, and verified that they were the pure native Lahontan cutthroat.

Hughes: How were you catching them?

Behnke: Oh, I took a fly rod.

Hughes: Oh, so you were just reeling them in?

Behnke: Yep. Almost all the specimens I got in my career were caught by fly fishing.

Hughes: But if you didn't catch one, you couldn't conclude that there weren't Lahontan—

Behnke: At these places that I get these fish from, they're remote areas; there's no fishing going on. The fish are very abundant; a fly hits the water, wham! if they're there. So there's no trouble; if you can find them you can catch them.

Hughes: I see.

Behnke: So there was not too much trouble. I would have to bring formalin along, and then I would catch them, and inject formalin into the body cavity and wrap them in cheesecloth and soak it in preservative and plastic bags, and pack them out. So that's how most of my collections over the years were done. Paul Needham wanted to publish this work we'd been doing, so in 1964, the year he died, I got the original manuscript; we wrote it up, and I was not happy with it. I said, "Jesus, there's so much we don't

know yet.” So, he died that summer, and we were going to publish with the University of California—it was out for review--and I withdrew it. I said, “Give me some more time to work on it.” It finally came out as a comprehensive publication in 1992, as American Fisheries Society – monograph six—

Hughes: Almost thirty years—

Behnke: —called the *Native Trout of Western North America*. In there you’ll see how the genesis of this work started with my 1957 field trip with Paul Needham.

Hughes: Isn’t that something.

Behnke: I wrote another book, *Trout and Salmon of North America*, in 2002, aimed more for a nonprofessional audience. In that book I acknowledged the Sagehen experience that got me started, first on wild trout and then native trout. I became obsessed with trying to find and preserve native trout. Especially native cutthroat trout all over the West, because several subspecies were believed to be extinct at the time. Two are extinct but I did discover three others that were still in existence.

Hughes: Did you find them right around Sagehen?

Behnke: It took a little more study and later verified with genetics that Independence Lake here has a pure native population of Lahontan cutthroat. That wasn’t believed to be true, because they knew Independence Lake had been stocked with non-native cutthroat and rainbows over the years. There are also brown and brook trout, so it was assumed that Independence Lake couldn’t have a pure stock. When I looked at them, I found some unique characters. I said, “There’s no way you could explain these characters from any kind of hybridization. This is a pure native population.” This later proved to be true. So we had a pure native population in Independence Lake, next door to Sagehen.

Hughes: Why didn’t they interbreed?

Behnke: There’s a few examples of some of the lake populations that were just very specifically adapted to highly specialized conditions. When the non-native trout were put in, they just didn’t survive. The Humboldt River in the Nevada drainage is another such example. The Humboldt cutthroat represents a different sub-species of Lahontan cutthroat, or is classified with Lahontan cut, but it’s more fluvial adapted; it’s adapted to live in streams that are highly fluctuating. Very unstable climate: great floods in the spring, droughts. In a drought year those streambeds are dry; little intermittent spring seeps here and there keep the population going. And several populations of native cutthroat, are still going where all non-native trout couldn’t survive. It’s an amazing fish.

Hughes: That’s amazing. Had you found that before Needham had died? Because he must have been—

Behnke: Most of my revelations don’t come like a “Eureka”! you know, it takes years to put stuff kind of a little bit together, and then it dawns on you, we have a unique adaptation here. I wanted to get more and more to write really a more comprehensive definitive type

statement on the evolution and ecology of the native trout. That didn't come around until 1992 [laughter] [when] the American Fisheries Society published my *Native Trout of Western North America*. You'll see in there I mention how it all got started in 1957 with Paul Needham at Sagehen Creek.

Hughes: Were you publishing papers in between?

Behnke: Oh yes, I kept up a regular publishing, notes on various things. As new things came to light I would publish stories, articles on them, and finally I got to pull it all together and come out with a comprehensive review. I sent my resume up here; you'll see quite a long list at publications.

Hughes: It should be right here.

Behnke: Twenty years ago, I started writing more for the popular literature, so I could better promote conservation issues.

Hughes: Let's see here. [shuffles papers]

Behnke: That academic curriculum—you can see the list of publications. Actually I started publishing in 1956; I spent that summer down in— The University of Maryland has a Chesapeake Bay research station. They had a little newsletter, the Chesapeake Science Newsletter. We'd go all around the bay collecting fishes. It was fascinating. In brackish water, you got a mixture of fresh water and marine species. I published in 1956 a note on that. As an undergraduate at the University of Connecticut I published a paper on the freshwater fishes of Connecticut for a "distinction degree" in zoology; a good addition for my resume when applying for grad school.

I used to use the Stanford University collections. Oh, that was another thing besides the Berkeley library. The collections from the nineteenth century, where a lot of these supposedly extinct trout, they were in the Stanford collection, which is now at the California Academy of Sciences. So I right away went down to Stanford to arrange to use their trout collection. When Stanford University started, David Starr Jordan was made the first president. David Starr Jordan was an illustrious figure who was also America's leading ichthyologist. He collected a lot of trout, starting in the 1870s. He also brought his associate from Indiana University, Barton Warren Evermann, out to become the chief of the California Academy of Sciences. So between the two collections, all these great collections taken during the early days in the West came out here. George Myers was the ichthyologist at Stanford at the time, and there was a big rivalry between Berkeley and Stanford. George didn't like the idea of Berkeley people doing any ichthyology work. He said that was supposed to be done at Stanford. But anyway, I got to know George and he said, "Well, you know, I wouldn't let any of my students touch those trout with a ten-foot pole. Nobody's ever going to be able to make any sense out of them; they're so mixed up, transplanted, and hybridized." He said he'd never let a Stanford student touch them for thesis work. But he said, "You're from Berkeley." [laughter] He said, "Yeah, sure, come on down." Every week I used to take a pickup truck, and I'd bring the specimens back to Berkeley, work on them, and then bring them back and take another batch back. But he said he was very happy to have somebody work on Jordan's old collection, but he'd never let a Stanford student do it.

Hughes: And could you make head or tails of it?

Behnke: Yes! I got quite a bit out of it. My interpretation clarified many of the questions that had confused Jordan one hundred years ago. In the Columbia River basin there's an area where identification of trout as rainbow or cutthroat was long confused. Using enough of those specimens, especially when extending out into southern Oregon regions, and down into the Pit and the McCloud River in California, I realized there was a separate group of primitive rainbow trout that was long thought to be cutthroat, and they were really rainbows. Again, this has been verified with genetic work. By having such a large comprehensive series of data, I was able to put all this together many years ago. But I wasn't quite ready in 1964; that's why I said I wasn't ready to publish on that at that time. But Needham wanted to go ahead and get that manuscript written up.

Hughes: Is that the one you pulled?

Behnke: I pulled it, withdrew it. You'll see in the 1992 monograph I mentioned, I wrote it up in 1964, but it was never published. The '92 monograph was really you might say the manifestation, the culmination, of that manuscript of 1964 and the work I started in '57.

Hughes: That's quite a record. Well, tell me your impressions of Needham himself.

Behnke: Paul and I were very—I think he almost looked at me like a son, because his own son Buddy was not a very—he married well and everything, but he was a fraternity boy. But Buddy was not a good person, you might say. He didn't have any respect for his father. So I would go over like on Sunday and cut the lawn. Buddy would never do anything like that.

Hughes: You were both living in Berkeley?

Behnke: Yeah. We met at the house--before he got married--but he wouldn't do anything. We [Paul and I] got to be quite close friends, and he looked on me more like his own son. And he had a daughter who was going to Cornell as a freshman. She ran off and got married to this fellow. I know that Needham was very unhappy when it happened, but when they started giving him grandchildren... Sometimes the little grandchildren would be up here. You know, he doted on those grandchildren. Two grandchildren in southern California called me on the phone a couple of times. They were trying to put together all the information they could on Paul Needham, all his publications and everything. I was trying to help them out. But they mentioned that Uncle Buddy wouldn't give them anything; Uncle Buddy wouldn't say a word. Then Uncle Buddy died. They didn't know why Uncle Buddy would never help out with anything to do with his father. They were quite interested in—they were two grandchildren; there were four of them altogether, I remember. I guess the Needhams forgave the daughter, because they just loved those little grandkids.

Hughes: Needham was upset because his daughter dropped out of school?

Behnke: Yeah. And her husband, I don't know if he graduated. He became a drug salesman. He wasn't a great success in life, but they had a happy family. The main thing for the Needhams was that they had these four lovely grandchildren.

Hughes: Well, that's interesting, because I guess it was Dave Taylor who was telling me that the Leopolds used to come up here in the summer with their families. So it was a family place. Did you come up with your wife and—

Behnke: I didn't get married until 1963. I went to Colorado in '66, and then in '64-'65, we spent that sabbatical year with the American Academy of Sciences. I went to work on the Russian fish collection over in St. Petersburg. I expanded my PhD thesis on the trout of the world. Paul Needham and I went to Europe in 1960. We went around collecting trout and visiting museums and everything. I went to Russia the first time then, and then I went back; I got a post-doctoral fellowship to spend ten months there in '64-'65.

Hughes: From Berkeley?

Behnke: I was still with the University of California. I was finishing my PhD. I got this exchange program that my wife came with me from the fall of '64 to the fall of '65 before we came home. She was up here at Sagehen with me. In fact, today when we drove up to look at the head of the ridge looking over Independence Lake. In '64, that summer we went up and camped out at the head of Independence Lake because we had a graduate student doing a study on the cutthroat trout of the lake. He had a tent set up. So we were up there; we looked down to see where we used to camp out.

Hughes: Tell me what it was like to do research with Needham?

Behnke: Most of my work was pretty independent.

Hughes: So you didn't spend too much time on field trips together?

Behnke: Well, yes, but I said that I would do all the examinations of the specimens, write up the conclusions and discuss my work with him. We did co-author some papers. When I first found out about this unusual type of red-band rainbow trout based on Jordan's Stanford collection--I said, "You know, the first rainbow trout they used in fish culture was actually mixed with steelhead. But there's a peculiar trout that lived in the tributaries up there that's really different." So we wrote a paper on that, about the origin of the original McCloud River rainbow trout. It wasn't, you might say, a single origin; it was a mixture of different stock. We went to Alaska in 1957. Doc picked up some rainbow trout in a place called Teabay Lake. He said, "Here's a funny trout; look." You cut him open and his whole viscera were encased in, like a giant spider web or something. I preserved specimens, and we did dissections and found out it was a response to a roundworm parasite, *philonema*, that would infect the trout and it would crawl around in there. The tissue would respond to it by putting a big sheath of protective tissue around the viscera.

Hughes: How amazing.

Behnke: It was very interesting. So we co-authored on things like that.

Hughes: Were the fish perfectly healthy once they had that protection?

Behnke: Yes, I mentioned that; I couldn't see how the eggs could ever get out of the ovaries, because it was all encased. Obviously they were spawning well enough, because there

were a lot of trout in that lake; they certainly weren't going extinct. So it might have taken several years before it got to a point where they no longer could spawn. So every time we found some kind of oddity like that we would write a paper on it.

Hughes: I see. Well, say something about what Sagehen itself was like. What actually was here when you first started coming?

Behnke: The research study was designed to go for ten years; ten stations on the creek were sampled every year. We'd pump all the water out, divert the flow, and then pick every fish out, weigh and measure them, record the species, and see any population changes—Dr. Needham had started something like that when he worked for the Fish and Wildlife Service down at Convict Creek Research. So that was the first thing we did here.

Hughes: Was that a tried and true method in fisheries research?

Behnke: You wouldn't now, because it's very labor intensive.

Hughes: I can imagine.

Behnke: Very costly. So we had a lot of undergraduate students we'd employ in the summer up here. It was "rock" biology; because there's a lot of manual labor involved. It's much more precise data than you would get from netting or electro-fishing. Especially the little minnows, and suckers, you'd never get all of them. So we did have a nice data set over the years, and all these different stations. And it would be quite accurate, because we'd get every fish out of our sampling stations. It was very labor intensive, so I don't think anything like that's being done today.

Hughes: Now is that what Dick Gard wrote up?

Behnke: Dick came up here—he did his PhD dissertation on trout and beaver interactions. How many other theses? Glenn Flittner was probably the first. A lot of papers came out. Sagehen also had wildlife research. Leopold would bring wildlife students up and do research. And then I guess to help pay the way here, they started teaching summer courses—botany was popular. Herbert Mason, famous botanist, would come up and have summer courses.

Hughes: Were these courses all for graduate students?

Behnke: Well, they might have had a graduate number, but seniors could take them. Bill Nisbet, who was one of the early students up here, wasn't a great academic scholar, but did good work. So both Needham and Leopold wanted to get Bill a Fish and Game job. But when it came time his senior year, his grade point average wasn't quite what he had to have to graduate. So Bill took the summer course with Needham and Leopold, and of course he got an A on it. That allowed him to graduate, and then he took a job with Nevada Fish and Game. Sagehen was popular for those reasons too, but students also liked to come up and have a summer experience in the wilderness.

Hughes: Did you teach any of those courses?

Behnke: I wasn't a teaching assistant at Sagehen, I was Needham's research assistant. I did help run the field trips. It was both fish and wildlife students, and some of the field trips would be combined, say you'd go to Pyramid Lake one day, look at the fisheries, then go maybe to a wildlife refuge the next, and see what wildlife work was going on, have lectures on deer management.

[End Tape 1A] ##

[Begin Tape 1B]

Hughes: Was the multidisciplinary approach that happened at Sagehen and I guess still happens, one of the big pluses?

Behnke: If you're going to justify your existence, the more disciplines that are interested in working here, say, botany, the better. Herbert Mason wrote a big book on the flora of the marsh. He was famous for his research on wetland vegetation, and he did a lot of his work up here. Some of his plots I think are still around. And one thing would lead to another. We used to also have meetings up here, we used to have a group called the Systematic Society, with both botanists and zoologists from Berkeley, Stanford, and Davis. We'd get together up here for a conference. I was just a student, so most of my involvement was either helping to prepare the meals or washing the dishes.

Hughes: [laughs] You weren't allowed to have the big discussions?

Behnke: I'd be peripheral to any discussion, but my primary duties was as a hired hand, you might say.

Hughes: Yes, but I bet your ears were wagging.

Behnke: Oh, I met a lot of people and talked to them.

Hughes: Did you know Leopold well?

Behnke: Yeah. I just gave it Peter Moyle a copy of my column, or actually a letter to the *Fly Fishing Museum Bulletin*, that had a story I called the Leopold legacy or the Leopold tradition in fly fishing. Also the American Fisheries Society this week is having an annual meeting in Madison, Wisconsin, where Aldo Leopold taught at the university, and the theme of the meeting is the Leopold legacy in fishing. What I pointed out is that the Leopold legacy for fisheries really came through Starker, because Starker was an avid hunter like his father; his father was also a fisherman, but Starker was really an avid fly fisherman. So Starker's influence on students, his love of nature and conservation, came from his father through Starker to people like me. After Paul Needham died, Starker was my major adviser; he took over the wildlife/fishery sub-major there. So I was in almost daily contact with him. What I was pointing out in my note is how the Leopold tradition and influence in fisheries were from his father through Starker, through Berkeley, to students like me. Also I mentioned that Phil Pister was an undergraduate student at that time and took Needham and Leopold's course on fish and wildlife. Starker showed Phil the *Sand County Almanac*, and he was overwhelmed. It really changed his life. He became an internationally known conservationist. Phil

worked for California Fish and Game. This is unusual because this was in the “hook and bullet” era. The State Fish and Game agency, they didn’t discriminate native or non-native species. During the hook-and-bullet period there were good animals, which you hunted, or you could eat, and you had bad, the vermin, the predators; that were to be eradicated. And everything in between was considered worthless.

Hughes: So very human-oriented—

Behnke: Very anthropocentric. That was the way Fish and Game agencies operated, federal and state agencies. Of course, that led to the virtual demise of native cutthroat trout because the policy was to replace them with these non-native trout. They were more popular for fishermen. So Phil became obsessed with carrying out the Leopold tradition. Being a regional biologist for California Fish and Game was difficult. They said, “This guy’s a dickey-bird watcher.” But Phil, to his credit, became quite famous because he was the one who saved a species of desert pupfish from extinction. One of the marshes had gone dry; they also had stocked large-mouth bass in there. There was only one little pool left with pupfish. The water had dried up, and Phil went down with a bucket and saved the whole species. Because the Fish and Game department said, “What are you doing, wasting your time out there with the dickey-bird fish”? In my book I point this out, how the Leopold legacy through Starker to people like Phil Pister and myself was promulgated as this ethic that there’s no such thing as worthless species; everything belongs if it’s native and wild.

Hughes: How long did it take, and when did that idea catch on, or did it?

Behnke: There was a gradual transition. For example, with the native Lahontan cutthroat at Independence Lake, tried to convince the California Fish and Game department to cease stocking nonnative trout. Then in 1977 I found that rainbow trout were again stocked in Independence Lake. The Sierra Pacific Power Company owned the area. Their caretaker would get maybe a dollar fee from campers who fished the lake. If there were more fish in the lake he’d get more money. So he would complain; he said he wanted some fish to get more people up there so he could make a few extra dollars. So the California Fish and Game sent a truckload of hatchery rainbows up and dumped them in—ah! This was 1977—you’d never do that today. But at the time, this was how people who grew up in that era, the hook and bullet era, had no sensitivity whatsoever to preserve a rare and native fish. So the desert pupfish were considered worthless. They were treated as food for the largemouth bass. That would be the logic at the time.

Hughes: But the stocking of lakes continues today.

Behnke: Oh, yes. I just had an article come out; I do these conservational columns for *Trout* magazine. I reach a much broader audience than I do with the scientific fish literature, and I can be more free in what I say. I pointed out that in 1952 the Sagehen project began that showed the values of wild trout and how there was no need to stock. The main emphasis should be on maintaining the habitat and the trout would take care of themselves. In the case of California Fish and Game, they did stop stocking Sagehen. But in near-by Prosser Creek they would still dump trout every summer down off the bridge. How much influence do we have on them? Well, I’ve written papers over the years, and I pointed out, using California Fish and Game’s own data, that between thirty-five, forty percent of their fishery budget was going in to support fish culture, and

ninety-seven percent of that was producing catchable, quote, “put-and-take” trout. In turn, they took up say at least one third of the budget; they produce no more than seven percent of all the angler day recreation days in this state, two million out of thirty million days. And also, those days have low economic value, because people are willing to pay much more for wild trout fishing than catchable trout. Then I documented that there was no correlation between license sales and stocking, that is, you could find no correlation between the number of catchable trout stocked and how many licenses were sold. Because most of the people who fish in California don’t fish for trout anyway. In fact, there was negative correlation in the 1980s. The more trout stocked, the fewer licenses were being sold.

So last year in the state legislature a bill was introduced to earmark forty-five percent of all fishing license money for raising trout in hatcheries. I saw that—my god! As I said, Aldo Leopold mentioned that a conservationist lives a life of wounds; you’re always frustrated and upset, so you’ve got to persevere, take time. But here in California where all the wild trout research began at Sagehen Creek, after all these years, a legislator proposes to increase spending on hatcheries. A reality is that most of your political power is in southern California, and southern California stocks a lot of catchable trout in their reservoirs, and they feed striped bass and largemouth bass with them. It costs a hundred and fifty dollars to produce a ten-pound bass by this method that all the anglers around the state have to pay for this waste. But to them that doesn’t matter; they just wanted to make sure that they got their share of trout. So here’s this bill in the legislature after all this work we’ve done over these years to try to emphasize wild trout management. In fact, California spends one percent, at most two percent, on their native trout and wild trout. So, fifty years later, you could get very frustrated. Most biologists are favorable, but some outdoor writers, particularly the local barroom/barbershop hook and bullet crowd, will get somebody in the legislature to introduce a bill. Science has no meaning there. They can just tell the state fish and game department what to do.

Hughes: Well, that’s unfortunate.

Behnke: So anyway, that can be very discouraging. I had a conservation biology course, and I would tell the students if they were kind of naive and enthusiastic, “If you really want to stay at this for a career, you’re going to have to develop a thick hide; you’re going to have to persevere. Aldo Leopold pointed this out back in the 1930s: “You’re going to live a life of wounds.” And you see, it hasn’t changed that much, but the perception has. For example, to restore native Lahontan cutthroat trout you’ve got to get rid of all the non-native trout. You have to put chemicals in the water. Poison them out. There’s no other feasible way.

Hughes: Is that so?

Behnke: So now you have among some hard-core environmentalists, they have an ideology on the purity of nature. They fight tooth and claw about trying to put any kind of toxic chemicals no matter what to restore the last most rarest trout in the world—nope! They have a chemophobia. They have this naive very bizarre view of nature’s purity. So those are the ones now hindering our restoration program for native trout.

Hughes: Have you gone head-to-head with those people?

- Behnke: Not really. California's one of the worst places. Don Erman's wife is one. [laughs]
[Note in editing: In July 2005, I testified at the California Water Board hearing on use of rotenone (a piscicide) to restore native trout.]
- Hughes: Is she?
- Behnke: Yes. You might kill a bug. [laughs]
- Hughes: But you'd actually kill a lot of things, wouldn't you?
- Behnke: Yeah, but it's very temporary because the invertebrates, you don't get many, and they bounce back in a big hurry. In fact, often in a month or two there are more than there were before because the fish are eliminated, so there's nobody eating them. There's no danger of any permanent damage. In California, court injunctions against chemical treatment have blocked native trout restoration for the past several years. When I went to Colorado, the green-backed cutthroat was thought to be extinct, and I went out and I found a population. So we started restoring them, and I used Rocky Mountain National Park. It was very easy in those days to bring some student volunteers, maybe a couple of dollars worth of chemicals, a bag of the native trout you want to restore, put the chemicals in, get rid of the non-native trout—very simple operation. You can't do that now. You have to write an environmental impact statement. Then you're going to have these environmental groups go to court and get an injunction. It's interesting that the strong opposition, is not coming from the old hook-and-bullet crowd, but from hard-core environmental purists with an ideology that's unreal.
- Hughes: Do you find that you're in opposition a lot of times to the Sierra Club and its philosophy?
- Behnke: The Sierra Club is not against hunting, or fishing, but now you're getting more and more of these environmental and animal rights groups that are against any kind of sport hunting or sport fishing. Any use of animals for human enjoyment is considered evil. They're a minority, but by getting on boards of directors, or volunteering for these environmental organizations, they wield undue influence. These are the ones who go to court to block our trout restorations.
- Hughes: That must be very frustrating. Do you want to say something in a general vein about what you observed happening in your field over the years?
- Behnke: As I said, in 1957, Paul Needham and I came back from Alaska. I met him down in Las Vegas; it was the meeting of the American Fisheries Society. I was so enthused about native trout and wild trout. I would say that the attitude of the people there, the agency biologists, at the time; nobody really told me to bug off; they were sympathetic, but essentially they would say, "It's too late. Unfortunate, but there's not much you can do about it now." And all their policies on stocking depended on stocking non-native trout. If we were going to hold a meeting at the 1957 American Fisheries Society on restoration or protection of native trout, you probably could have held it in a phone booth. There wasn't that much interest. Over the last ten years or so, I don't know how many American Fisheries Society-sponsored symposiums I've been to on native trout. Very popular topic. Preservation, conservation, restoration, sometimes just for one sub-species. This enthusiasm has evolved over the years.

Hughes: How do you explain it?

Behnke: Well, it's the whole big change in how we look at the environment, from good, bad, and worthless species to more like integrated ecosystems and functions, and all this—even though most people don't really have an in-depth understanding they accept it as "good". To most anglers, a trout is a trout, what do they care what kind—hatchery, wild, or native. Changes are occurring, however. In Idaho, they now have both the state fish and game agency, and sportsmen's clubs trying to preserve the native cutthroat trout in the Snake River by putting weirs across the spawning tributaries and taking out rainbows and hybrids. You've got a big volunteer effort of people going out and contributing to save the native cutthroat. Thirty or forty years, this wouldn't have happened.

Hughes: It wouldn't go.

Behnke: They'd have said "What do you want to do that for? The rainbow trout fight harder; the hybrids are better." That was official policy: get those rainbows to replace native cutthroat and we'll have better fishing.

Hughes: Do you think your work has had an impact on this?

Behnke: Well, I like to think so. I certainly have been writing about these topics for a long time, and going around giving talks at symposia and publishing in proceedings. I would mention Phil Pister again as a very good example of changing attitudes. He was considered such a bizarre person, a dicky-bird tree-hugger, saving desert pupfish when he's supposed to be out there stocking trout for the Los Angeles crowd coming to his region. His office was out of Bishop. The director of California Fish and Game was Ray Arnett, who also became director of the National Rifle Association. I asked Phil, "How did you ever get along with Ray? I would think he would have wanted to kick you out." He said, "Actually, he was one of the better directors." Arnett's position was: "Ah, let him alone; let him do his thing; he's a good man." Although Arnett was one of the typical old-time directors that you wouldn't expect to get any sympathy for saving native fish, or rare fish, or anything like that.

Hughes: Can you summarize what you think the value of Sagehen is?

Behnke: Well, it's kind of selfish, but I don't think I would ever have done what I did or be where I am without my early influence of Sagehen. Sometimes I'd be along Sagehen Creek all by myself, and I'd sit on the bank, and I'd conceptualize things like production/biomass ratios. And conceive models in my head, and ask the question, "Could we catch more pounds of trout out of this stretch of water than actually lives here?" If we measured it, we'd say, "There's twenty pounds of trout per section." And I said, "Fishermen could come in and catch twenty-five pounds out, and we'd still have twenty pounds next year." Things like that. Well, yes, actually we were doing very close to that here, but how do you do it? You've got to sit and think. So, Sagehen gave me that opportunity to do some deep thinking about trout biology, life history, even things on evolution. I made a lot of my inferences sitting out on the banks of Sagehen Creek just thinking.

Hughes: In a way that you couldn't or wouldn't do in the university?

- Behnke: I certainly did a lot of that using the literature down in Berkeley, but when I'd come up here it just seemed like I could think clearer.
- Hughes: Well, you probably could.
- Behnke: A change of scenery.
- Hughes: Right. And the lack of interruptions.
- Behnke: Yeah, and this wonderful setting keeps your enthusiasm going. The trout, look at where they live; look at the watershed. You never could do that sitting in an office.
- Hughes: So it's the place itself that has significance, as well.
- Behnke: Oh, yeah. It made a big impression on me. At first just the idea of *wild* trout. What are the factors that make them successful? How do you get so much reproduction when you need different types of habitat, different parts of their life history. The next thing was *native* trout. Lahontan cutthroat was the trout that lived in this creek. And then I got obsessed with finding Lahontan cutthroats.
- Hughes: I hear people here also talking a lot about the continuity of research. The fact that research here has been going on in the same place for many years, and there's a whole long life of data available. How do you value that?
- Behnke: There are very few places, very few research stations that have such a long history of continuity, especially in the fish data. So that certainly has value. You could say, well, we could do a computer model about this, but there's no way to do something like that and have actual facts and figures in the data.
- Hughes: You've spoken several times of the genetics coming in. Did you yourself eventually use a genetics approach?
- Behnke: I myself didn't become a geneticist but I did have students and associates who were geneticists, so the classification that's used in my new book is an eclectic mix of evidence. Genetics allowed me to better assess degrees of relationships. Evolution can occur quite rapidly, especially in life history, and filling different niches--a few thousand years. So some of the trout that are really quite distinctive really aren't that ancient; they just happened to come under very strong natural selection and evolve real specialties, like the Lahontan cutthroat trout. It, as the top predator, continued to co-evolve with the original fish fauna and attain the greatest size of any cutthroat trout in Pyramid Lake. It was believed to be extinct by 1938, and a population was discovered on the Utah border. In fact, that's the one that's right up in the creek now. Peter Moyle got some from the hatchery; he's got them in the creek. So that's an example, that the Pyramid Lake fish would reach a size at least twice that of any other Lahontan cutthroat. And it was only because they had evolved, and continued to evolve; they were a top-level predator and had all the other fish species to prey on. Pyramid was the only place that occurred.
- Hughes: Do you have anything more to say about Sagehen itself?

Behnke: I really can't think of anything, unless someone else has brought something up I could add to. I think tomorrow Jeff is going to have people give presentations and slide shows; I'm sure when I watch that some things are going to pop up that you might be interested in.

Hughes: Well, when you get the transcripts, you can add. I'm just flipping through my notes to see if there's anything. I think we've covered it pretty well.

Behnke: Oh, I'm sure we have.

Hughes: Well, I thank you.

[End Tape 1B.]

[End of Interview]##



David and June Taylor, 2005

JUNE AND DAVID TAYLOR, PH.D.

[Interview # 1: 08-21-04]

[Begin Tape 1A]##

- Hughes: Would you tell me a little bit about your family background and your education?
- D. Taylor: Golly, well, we've had a lot of interesting people in the family. My grandfather on my mother's side [Walter Porter White] was a PhD in physics. He worked for the Carnegie Institution in Washington. My father [Henry Gordon Taylor] was all the way to candidacy at the University of Wisconsin in geology, but that was in the early '20s. He had figured out how to use seismographs to find oil fields, which was the big thing down on the gulf coast of Texas at that time. So he left academia and started his own oil exploration company, in fact found an oil field. That's a quick sketch. I didn't invent the desire for education all by myself.
- Hughes: No, I can see it came from quite a background.
- D. Taylor: So anyway, when I graduated from high school I was thinking of going to a local college, but I'd been accepted at Stanford, and Pop said, "Well, you know, Stanford's really the best school out here. Why don't you go there"? I said, "Well, okay." I'd never been there, so I went out—
- Hughes: You had been brought up in Texas?
- D. Taylor: No, this is down in southern California. We were brought up in Washington, DC during the war. Wellesley, Massachusetts, and then Washington DC. So anyway, I went off to Stanford, and spent actually a total of six years; I took a couple of quarters off to go skiing. I really majored in skiing. [laughter].
- Hughes: I imagine that came in handy later on.
- D. Taylor: Oh, yeah. We skied in the intercollegiates. (Later the navy sent me to the Olympic Tryouts.) Anyway, I eventually got a degree in biology, and then I took another year in psychology. I was mostly interested in animal behavior, at that time.
- Hughes: Why did that attract you?
- D. Taylor: I was always very interested in wildlife, and in natural things, and animal behavior I found very interesting. I did a little psychology, but that gets a little dull, and animal behavior was a new field at that time. They were just inventing terms like ethology and doing things like the experiments of how

bees find their honey sources, and how birds navigate, and that sort of thing. Fancy stuff. That was during the Korean War. The navy ROTC had put me through Stanford, so I got a commission and went off to the navy, and started out initially on a destroyer. But then they sent me to flight school, so I became a pilot. By then it was '57, and I went off with some buddies who were going with the airlines, so I thought, that sounds like fun, so I joined Trans World Airlines. But a year later, a couple of things happened. I met June, and I got furloughed, so we decided that we'd like to take off and go travel to Europe. Of course, to do that you had to get married in those days, so I had to marry her. [laughter]

J. Taylor: You already mentioned you did, of course.

D. Taylor: Besides that, she said she made me marry her, because she says I broke her leg.

Hughes: Oh? What's the story there?

J. Taylor: He took me skiing. I'd never been skiing. And he worked with me one morning on the slopes, how to ski, and then I let him go off by himself to do some skiing. I was practicing going down through the runs, but this was back East where there are a lot of trees on either side. I was in control, doing very fine, but a young man who was out of control came out of another path and hit me from behind, and I broke my leg.

Hughes: The first day on skis.

J. Taylor: First day on skis.

D. Taylor: So anyway, she says she made me marry her—

J. Taylor: I was married with my leg in a cast. So then we went to Europe.

D. Taylor: We went to Europe. Prior to going over I realized that gee, I have to do something, so I thought gee, I've got the GI Bill, and I can be in the Navy Reserve and get a few bucks there. So I came up to Berkeley and went into the Life Sciences Building—I actually talked to P. R. Needham first—

Hughes: Just by chance, or had you—

D. Taylor: Well, I was interested in wildlife fisheries. It just happened that I went up to his office and knocked on the door and started talking to him.

Hughes: Had you connected his name with wildlife fisheries?

D. Taylor: No, I didn't really know. He said, "I think what you really should do is go down and talk to Starker Leopold. He's closer to what you really—

Hughes: Because you were saying wildlife, rather than fisheries.

D. Taylor: Well, I was also interested in fisheries. But anyway, I ended up talking with Starker, and he said, "We're going to have a class in wildlife fisheries. Needham and I are going to do it up at Sagehen Creek." I didn't know where Sagehen was, but it sounded like fun to

me, so I said, "Great! I'll be there." Then we went off to Europe, and came back, and we bought a tent and some sleeping bags, and arrived at Sagehen--I guess it was probably late June of 1958--and set up a camp down there in the meadow, about a quarter of a mile below camp there, along the stream.

Hughes: You didn't have children yet?

J. Taylor: I was pregnant.

D. Taylor: Well, yes, we did. [laughter] The first was on the way. So we took the course, and I got started. Joe Hall had begun a study on the beavers in 1952. In 1952 to 1955 he had done some very fine work on this beaver colony, which actually started with an introduction in 1945. Joe was able to actually kind of interpolate backwards to the original introduction of four animals in '45.

Hughes: They were introduced?

D. Taylor: Yes, they were introduced.

Hughes: By whom?

D. Taylor: By Cal Fish and Game, or the Forest Service, I'm not sure exactly.

Hughes: Do you know what the rationale was?

D. Taylor: Yes. Beaver had pretty much been extirpated from most of the lower forty-eight states, and they were to a very low level in much of the West.

Hughes: Because of hunting?

D. Taylor: Because of trapping. They're quite easily trapped out. When they're heavily trapped, the population is greatly depressed or even extirpated. Then, every farm kid had a couple of traps nailed to the barn, and if beaver showed up he'd go out and catch them and make a few bucks for the pelts. So they were very very low. But then with the Depression in the '30s and the public works, we had a lot of talented idealistic people looking for work, and one of the things they did were all kinds of conservation projects. There was all this folklore from the old naturalist days about what wonderful things beaver colonies did for the environment, specifically a stream system. All these stories about, "Well the west fork used to flow, but now it's dry; nothing's there but sagebrush. But you know, the beavers got back in there and now it flows all year round." Stories like that. And so, in the '30s there were these people looking for things to do, and one of the things they did was to reintroduce beaver into much of their former range. There were no beaver on the east side of the Sierra at that time. There were still beaver in the Sacramento/San Joaquin system, but not on the east side. That's an oversimplification; there are a few. There had been attempts at beaver farming, but essentially there were none. But they were introduced in the '30s, and then World War II came along, and nobody hunted or trapped or fished much; they were all busy. [Beaver] were actually introduced in 1945. By about 1950, there were beaver everywhere, and they were damming up irrigation ditches and water supplies, cutting down orchards [laughter]; so they got lots of attention. They were very controversial. In fact, our beavers here at

Sagehen were perennially damming up the water supply for Hobart Mills. So they were always trying to trap them out, and we were watching them like a hawk, and trying to devise little things to outwit the beavers and keep them from damming up their water supply [laughter].

But anyway, to get back to the story, we arrived in 1958 and I took the course and got started. Joe had worked from '52 to '55. Dick Gard had worked from '53 to '57. Joe had worked more on the beavers themselves and had started trapping and marking. So we had a genealogy of known animals. We know the parents and their offspring, and their grandchildren and their great-grandchildren, probably to about the twelfth generation by now.

Hughes: What's the average lifespan of a beaver?

D. Taylor: Well, the oldest one that we had, that we actually trapped as a kit, we know his entire life was about fifteen years. A lot of them don't live that long; all kinds of things can happen.

Hughes: Do they have predators in this area, other than man?

D. Taylor: Historically, of course, their predators were very important. For example, here there would have been wolves, mountain lions, wolverines, bears probably. Today I don't think there really are very effective predators. Mountain lions do occasionally, but they're really specialized on deer, and there are plenty of deer, so mountain lions don't have much impact. There are probably a lot of things that take baby beavers, take them when they're little kits, but a full-grown beaver is a very strong animal. They're quite passive, good-natured, but they have those chisels that are pretty—I mean, they can take your finger off, just chunk! Just like that! I don't think a coyote could handle a full-grown beaver. I think if a coyote attempted to attack a beaver, the beaver would just go into the pond and under the water, and he'd let go. Bears might, but there again-- They might even tear apart a lodge, but if they did the beavers have underwater escape routes, so if the bear started tearing apart their lodge the beavers would just go out underneath. So, the answer is, their main predator is man. Certainly that's what caused their extinction. Of course, much of the North and the West were colonized by beaver trappers, as you know. They were the first ones out. In the case of the West, for example, these were big parties. The American Fur Company, people like Jedediah Smith and James Ohio Patty, they came out with small armies of fifty men, and they just really trapped hard.

Hughes: And of course, with no idea of conservation in that age, was there?

D. Taylor: No, no.

Hughes: There was a feeling of an endless supply?

D. Taylor: Well, they knew it wasn't, because behind them they left waste. They'd already extirpated or drawn the population way down all the way to the Rockies in the 1830s, 1840s, just prior to the Gold Rush. From about 1810 to 1840 was the period of the mountain men moving into the Rockies. They came up the Missouri and you've heard a lot of those stories--things like *Men to Match My Mountains*.

- Hughes: And they were right through this Tahoe area as well?
- D. Taylor: Well, there were big parties in the Sacramento system, and they trapped the Sacramento system. They would send scouts up the west side rivers. But as they got up the rivers, there weren't enough beaver to make it worth their while, so they didn't pursue that. And people like Frémont and Jedediah Smith and so on, who crossed over from the east, came up over the Sierras, didn't report any beaver. So we assume that there were not beaver in the Tahoe area. At least, in any significant number.
- Hughes: I'm sorry, I'm diverting you. But it's interesting.
- D. Taylor: It's fascinating stuff. So anyway, Starker got me started. I trapped all that summer, and marked, and we'd sit in the trees. June went too. We had these little observation stands. It turns out if you build a little platform up in a tree, the beaver are down here and they're kind of working around at ground level, so they don't pay much attention to you. You don't really have to be concealed that much if you're up above them. So we did that all summer, and we caught pretty much all the beaver on the stream. We had various schemes for marking them. We'd paint their tails, [which] was one of the first things we did. One of the first ones we caught, we brought the class up to show them what we'd done. Starker was there, and June was there, and we painted its tail yellow. It turned out that that was a beaver that we caught again frequently, and saw for the next seven years, the color. We called him Old Yeller. We have a wonderful picture down in our study of Old Yeller, naturally in the water.
- Hughes: What were you trying to figure out?
- D. Taylor: Well, one of the things that was most interesting to me—a lot of the birds, especially the big long-lived birds, are monogamous. They mate for life. Things like geese, and swans, and some of the big predators. Many of the big long-lived birds. Birds, once you lay the eggs, why, you're equal. But that's not the case with mammals. The female has special duties when it comes to bearing and nurturing. So it turns out that monogamy is pretty rare in mammals. And at that time, if you started researching the question of mating for life, monogamy, there just weren't any good examples. The ones that you'd usually come across were wolves and beavers. Well, it turns out that wolves aren't really monogamous. In a pack, the dominant male and the dominant female are the only ones to breed, and they will continue to breed as long as each are number one. But if either one of them loses his or her status, then they're replaced. So I wouldn't call that—
- J. Taylor: That's not monogamous.
- D. Taylor: No, I wouldn't call it that. Beaver had been said to be, but it was controversial, quite controversial. So that was something that we thought well, gee, if we really have a family history of marked animals, which is a long-term one, which is unusual, and this kind of longevity, we keep coming back to the idea of long-term longevity--that's what Sagehen's all about. We were very interested in that, and I think the data we have here was one of the first that could really clearly demonstrate that yes, they do.
- J. Taylor: They are monogamous.
- D. Taylor: They are.

Hughes: What would be the advantage to the animal to be monogamous?

D. Taylor: Well, beavers of course put a lot of work into their territory or their range. “Range” technically for ecologists is well, this is where the animal is; “territory” implies that you defend it. I’m sure you know this. Beaver do in fact—this is again something we had to demonstrate, because a lot of people didn’t believe that; there were reports otherwise. But in our case, why, a family of beavers pretty well does mark and defend its territory. They have set boundaries, and then they invest a lot in it. The young take at least two years to mature. They put up a food store. They build ponds, and they build lodges and burrows, and they store food in the pond. There’s a lot of work to be done, and they want to reap the benefits themselves. And also, it’s interesting that in a lot of mammals, there’s a lot of sexual dimorphism. Usually you’ll find that it’s because they do different jobs. But in the case of the beaver, why, they share jobs pretty equally. Of course, the original bearing and nursing is by the females. But once the kits are out there swimming around, you don’t notice a big difference in behavior between the sexes. They’re both working on the dams, they’re both working on the lodge, they’re both working on the food store. They’re doing much the same things, which is an advantage, a big advantage. In the case of the large longer-lived birds, where it’s easier for them to have this family form, why, you see it often. That tells you something. So yeah, I think there’s a real advantage.

Hughes: Was that study your dissertation?

D. Taylor: Yes.

Hughes: And did it make a wave? The fact that you had shown that beaver are monogamous?

D. Taylor: Not particularly [laughter].

So anyway, to get back to what Sagehen was all about, what I was saying yesterday when I had my little turn to say my piece, the feel of the place when we got here was that, “This is a field station; these are wonderful people.” Paul Needham, and Herbert Baker, the botanist, and Starker, they were really interested in being here in a wild setting, and living it. We’d get together around the fire and play the guitar and sing songs at night. Starker was camping out in a tent frame, cooking on a wood fire with his family, Betty and his—

J. Taylor: They were literally my adopted parents.

Hughes: Really?

J. Taylor: Oh, yes. I had just left the East Coast, never been to California, never been married before, never been to wilderness type of living, and I was a little homesick for my family, and they became my family. They were mom and dad all my western living life. They really meant a lot to me. But they were kind of like that to not just me—there weren’t that many girls—even the guys, they just took everyone under their wing. They were always there for you. They weren’t just your professor; they were dear friends.

Hughes: It’s an astounding family, isn’t it?

D. Taylor: Oh, absolutely.

Hughes: They could have been above all that.

J. Taylor: It wasn't that way.

Hughes: You have had probably one of the longest relationships with Starker, haven't you?

D. Taylor: Yes.

J. Taylor: Yes, we have.

D. Taylor: I went down to Mexico with him to the Sierra del Nido. There was a population of grizzly bears down there that Starker was very very interested in. In fact, he almost talked me into taking them to go down and study [laughter]. I'm glad we didn't, but that's another story. But one of the things that struck me last night when we were sitting around—I think you were there quite a while, and then you sort of drifted off—

Hughes: Yes.

D. Taylor: Glenn Flittner was going on and on about how sophisticated the science was, and all this data collection, and putting it all into computers, and saying the usual things about need to make it continue for posterity, et cetera.. But the overwhelming feel for us when we arrived was, this had its roots in natural history. This went back not to computer experts, but to people like the classical naturalists, like Aldo Leopold and Joseph Grinnell and Ernest Thompson Seton. When we started, Starker was in the Museum of Vertebrate Zoology, in the Department of Zoology. At that time, in the class we trapped and collected and sat around preparing museum specimens. You stuff them. Have you ever stuffed a hummingbird? Or a shrew? [laughter]

Hughes: Nope. [laughter]

D. Taylor: These are real hands-on, hip-boots, binoculars, field notebook kind of natural history. Not automatic data collecting and putting it all in databases and so on. It was something that came much later. When it did come, it changed the nature of Sagehen greatly. These are also the crisis years of Sagehen, because the cost of all this escalated out of sight. It didn't cost anything for us; we paid our own way. From the time we started 'til I turned in the thesis. I was employed elsewhere. I worked for eleven years on these beaver. Well, twelve I guess, actually. The point is that people were doing it because they really were all wrapped up in it, and the roots were in coming out and sitting on a log and looking around, rather than all this automatic data collection. And when this did happen, why, the cost escalated tremendously. The nature of Sagehen changed. Starker, for example, moved from the Department of Zoology to the School of Forestry with his wildlife fisheries unit, as did Needham—well, Needham actually died about that time. Wildlife Fisheries went to the School of Forestry at that time. The School of Forestry had their own field station already. So not only was there greatly increased cost, but there also was a competing station already, and decreasing budgets. So the result of this crunch was that the guys who followed Starker—Reg Barrett and Dale McCullough were the senior professors there in Wildlife Fisheries at that time, and Don Erman. They were kind of unhappy because they wanted—[UC] Davis was giving a class here, and

they wanted Davis to share the cost, and Davis didn't want to pick up the tab. So there was a little fight over who's going to pay the bill. They had classes and research at the other field stations. After Starker died, and the next twenty years, Sagehen kind of withered, all the way to the point where in the middle '90s it almost died. And that's what's so wonderful about what's happened recently. But in talking about where we came from, and where we want to go, as we did yesterday, why—there was a lot of talk about modern science and automatic data collecting, and putting it in forms that can be used around the world today and into the future, which of course obviously we want to do. But there's also just the roots in natural history, and just coming out here and sitting on a log and looking around you.

Do you think that way of doing science will survive with all the pressure from new technology and the genetic molecular approach, and all of that which seems to be attracting so much of the funding these days? Hughes:

D. Taylor: That's right, and it has in fact become déclassé. It's hard for the old-fashioned guy to compete, both for prestige and for money. You don't need a lot of money. But that's where the real insights come; that's where the wisdom comes. The rest of this is vitally important for documenting and analyzing and explaining and so on, once you see where it all fits together and what the needs are. We don't really have any good way of collecting data and computerizing values. What I think this is really all about here is values. And those values include baking bread in a dutch oven at the campfire—

[End Tape 1A]##

[Begin Tape 1B]

D. Taylor: For a graduate student to be able to sit around a fire with Starker and Luna Leopold. As a matter of fact, the time I'm thinking of in particular, Luna had just come from a directors' meeting of the Sierra Club over at the Clair Tappaan Lodge. And he came over here and we sat around the fire debating. At that time the big thing was the Diablo Canyon issue. Prior to that time the Sierra Club had been a small, relatively esoteric group, and basically we could do anything we wanted without worrying about the consequences. I mean, there was this great rhinoceros out there, and we had this little BB gun, and we were trying to get its attention and get it to shift its course slightly. We didn't worry about solutions; we just knew that what was happening was wrong, and we tried to make as much noise and as much trouble as we could.

Well, in this atomic power thing, they started up at Bodega. They were well on their way to building an atomic power plant up there, on top of an earthquake fault. But anyway, a lot of people and PG&E had put millions of bucks into this at the point where the protest finally got up a head of steam. And to everyone's surprise, one day they showed up and knocked on the door and said, "Hey! What do you think we ought to do"? Guys like Dave Brower said, "Hold up! What do you mean"? [laughter] They said, "Well, you don't want us to build it here. Where do you us to build it"? The alternatives to nuclear were things like coal and so on; they weren't all that great either. Again, there was a lot of difference of opinion about whether we should have talked at all. But anyway, one group said, "Well, don't build it here. Why don't you build it down at Diablo Canyon?" PG&E said "Oh, okay." So they went down there and started

spending millions of bucks at Diablo Canyon, and it took a long time for people down there to realize that they didn't want it either. What I'm trying to say is that this was an era when the environmental movement in general, and the Sierra Club in particular, sort of came of age and was asked to find solutions rather than just be against things. And that's what they'd been talking about at the Tappaan Lodge when Luna, a director, came over here and we sat around the fire. And Starker was a former director. We talked about this issue: "Can we just be against things, or do we have to compromise, and find solutions that can be worked"?

Hughes: Did you come to a conclusion about that?

D. Taylor: No. [laughter] No, but here's little Sagehen way off in the Sierras, and we're talking about the future of atomic power plants.

Hughes: Yeah. So, it seems to me what you're saying is, this place is not only a source of research information, and especially research over the years, but it's drawn people together to have dialogue, to talk and discuss the major issues in ecology.

D. Taylor: Oh, sure, exactly. People of different disciplines, and different backgrounds, and different ages.

Hughes: In a way, would you say, that a university can't?

D. Taylor: This sort of thing, I think is always serendipity; it happens, and who knows when and where and why it happens. It happens in little groups here and there. But this is one place where it does happen, and did happen.

Hughes: And the multi-disciplinary approach, which maybe isn't as readily available in the university, where the departmental lines are formally drawn--

D. Taylor: Exactly.

Hughes: —to keep people separated sometimes.

D. Taylor: Oh, yeah. It was about this time, Starker attempted to get together a seminar. It was to be composed of a graduate student and a faculty member, from all of the various— At that time, there were colleges, and schools, and departments of—I think there were at least half a dozen ecology courses. Zoology had one; the school of forestry had one; the school of ag economics had one; the bug people had one, and these were all ecology courses. And none of them talked to each other. Let alone, for example, here, the big starting research program for Sagehen was the ten-year fishery study. But the minute they started doing that they got to be connected to everything else. They started studying the beavers; they wanted to study the hydrology of the stream, and then they wanted to study all the ecosystems that flourish in the beaver ponds, in the riparian zone. You get people in different disciplines, and once there's a database there, and a pool of information, then they can plug into that and go on. And that's where the multidisciplinary and long-term dimensions....

Hughes: You've talked about Starker, but say something about him as a personality.

- D. Taylor: Oh, he's a wonderful guy. [Addressing June] Tell us about Starker.
- J. Taylor: Of course, when you speak about Starker you're also talking about Betty and his family. He could really play the guitar very well, and his daughter did, and they'd sit around the fire at night. He and Sally would also get us going on the guitars, and singing. Betty was very very social; she was even more outgoing than Starker. But Starker in his very humble—he had a gentle voice, and when you talked to him he'd look directly at you, and he would draw you out, and anybody—David who was getting his degree. When he'd talk with me, he'd be interested in what I would be doing, and it had nothing to do with research, but I felt that I could really talk to him.
- Hughes: And he paid attention.
- J. Taylor: And he paid attention, and he was gentle and kind—helpful! Always so helpful.
- Hughes: He was older, so his children were considerably older than yours?
- J. Taylor: Actually, Sally was nine, and I was twenty-three, and Fritz was sixteen. And like I said, I was very homesick for my brothers, who were ten years younger than me. They were my playmates, Sally and Fritz. The three of us would go down the creek on rubber rafts, little tubes, and we would play, the three of us. I adored them. After we got back to Berkeley we would baby-sit Sally sometimes. She'd come stay with us for a week if they had to go someplace. And then Sally ended up babysitting my children.
- Hughes: Well, tell me about Taylor Meadow and the summers there.
- J. Taylor: Like we just said, that was our first married home, and it was a little tent right down by the water, down in that lovely meadow. The other students that were up here, at night we'd often want to get together and sing and have a party. It was such a lovely place to have a little circle and build a little fire that we'd often say, "Oh, well, come on down to the tent and we'll have a little get-together, sing-along, or party, whatever you want to call it." "Sure, we'll see you down there," and then the word would spread: "Oh yeah, we're going to all get together down where the Taylors are camped in the meadow." And that's the way it kind of went part of the summer: "Down in the meadow where the Taylors are camped." After a while, they said, "Oh yeah, we're having a party, you know, down at Taylor Meadow." And that's how it came about.
- Hughes: Everybody was to a certain extent a separate unit. The Leopolds would have been cooking for themselves, and you would be cooking for yourselves, but then there would be these occasions that brought you together.
- J. Taylor: Right. And the Leopolds were way up at the first wonderful place, furthest away at that time from main camp, not that it's real far away, but it was the furthest cabin away from this main area. And since we were a married couple, and I think we were the only married couple the first year—
- D. Taylor: Jim was.
- J. Taylor: But they weren't researching, they were the cook and the maintenance man.

- D. Taylor: Carol was the cook, he actually had done a Master's, I think
- Hughes: Jim who?
- J. Taylor: I forget their last name. But she was hired on that summer as the cook. Like David said, Jim was doing research. So they were married.
- Hughes: So you did eat together.
- D. Taylor: Originally the class, and then also the crew that did the pumping and draining fish census. There was a cook and meals scheduled up here.
- J. Taylor: We could have eaten there if we'd wanted to, but we chose not to. Occasionally maybe, if it rained for three or four days in a row. David and I would cook out down there, but the rest of the guys were all bachelors. [Vierst?] and second year John Hopkins, and Bill Nesbit, and Jerry—What was Jerry's last name?
- D. Taylor: Jerry Plutner
- J. Taylor: Plutner, and Steve Pennoyer; they were all bachelors. Their A-frames were right here, right by the main building, and of course Carol was a good cook, so they all ate there.
- Hughes: How many children did you eventually bring up here?
- J. Taylor: Well, I was pregnant with our first child, who was a girl, and her name is Kit.
- Hughes: [laughter] Imagine!
- J. Taylor: She was named [laughter] Sagehen beaver kit. Because in those days we didn't know if we were going to have a boy or a girl, didn't matter, it would have been called Kit. Two years later we had our son, and so his name had to go along with nature, so, Cliff. Two years later we had another, and his name is Tarn, which is a small glacial lake. We'd come up here in the summertime, and I had my three babies, and we lived by the water. I bet they loved it.
- Hughes: Do they talk about it?
- J. Taylor: Absolutely. It's their second home, and the values that they learned living here were unique.
- Hughes: Respect for nature.
- J. Taylor: Yes, very much so. And it gave them a lot of resources in learning things to be kind of brave and confident and adventuresome, and they adapted. But they also learned about the simple life.
- Hughes: All good things.
- J. Taylor: All good things. And they loved it; it was also fun; it was a wonderful way to learn a lot of the true values. Wonderful experience.

- Hughes: I guess you haven't said that you were a TWA pilot, but that's eventually what you became, right?
- D. Taylor: Yeah.
- Hughes: Say something about how that interacted with your interests here?
- D. Taylor: The first year, before I came here, I got laid off, and we got married. Then we were laid off for two full school years, so that fall, I took classes at Berkeley.
- Hughes: You were laid off—they had too many pilots?
- D. Taylor: They did that a lot in those days. It was kind of up and down. They called it furloughing; they'd just say, "Goodbye. We'll call you when we need you." [laughter]
- Hughes: That could be kind of difficult for people, couldn't it?
- D. Taylor: Well, yeah.
- Hughes: It was good for you, because then you could pick up on this other interest.
- D. Taylor: Yeah, some people regarded it as—
- Hughes: I don't imagine that there were too many TWA pilots who were studying beaver.
- D. Taylor: No, there were not [laughter]
- J. Taylor: It didn't cost a lot to live here.
- D. Taylor: The first year, that summer we did fieldwork, and then in the school year I took full-time classes. Then that next summer we went down to Mexico with Starker, on a field trip to the Sierra del Nido. And worked up here the other half of the summer, doing research. And then that next year I worked as a research assistant at the Museum of Vertebrate Zoology, working on the collections.
- Hughes: Was that commonplace for Starker's students to work in the museum?
- D. Taylor: Starker had a way—he looked out for his students; he would find a job for you as a TA or a research assistant or something. Yes, it was; he did do that; he took a lot of responsibility on his side to try to— And then I got recalled, and then we went back to Boston, and then Chicago, and then I got furloughed again and came out, and that year I was a TA. So I spent another year in Berkeley as a TA. And of course all of this time we were continuing the work up here.
- Hughes: And was that always a pleasure? Because it seems like two very different lives.
- D. Taylor: Well, kids, you're used to burning the candle at both ends. Our kids all do. I mean I can't believe the work load that my kids have. I was doing two jobs, but lots of people do that. And this was an awful lot of fun. For example, trapping was an ongoing part of—we tried to catch the animals with the mark, so we knew who was here and when

they moved, where they had come from, and their family connections. When I was here for an extended period of time, that was fine; we could set the traps up, and go out—you had to go out at night and check—I checked traps at midnight because we needed to. Which was fine, but when I was down at school taking classes, being a TA and et cetera, et cetera, why, it was just weekends that I could come up. When I did that, it was hard to get more than one trap night in. For example, I'd come up Saturday morning and set traps out, and I'd check them Saturday night, but then I'd leave the traps in Sunday night until midnight. About two-thirds of the catches would come in the first half of the night. So then at midnight I'd go out and pull the traps out of the crick and stow them away, and then I'd get in the car and drive down to Berkeley.

Hughes: Were you in on these weekends?

J. Taylor: No, because by then we had children in school.

D. Taylor: The road wasn't what it is now.

J. Taylor: It took longer.

Hughes: One motivation would have been getting the degree. But then you continued after that. Did you ever think, why am I doing all this? [laughter]

D. Taylor: I never doubted that I wanted to do this. A pilot does have time to do other things.

Hughes: Is there any other study that comes close to yours in the duration of observation?

D. Taylor: I completed my thesis in 1970. At that point, with mine and Joe's and Dick Gard's, we were able to pretty much go backward to 1945. So there we had like twenty-four years of more or less continuous history of this group of colonies. Then, just a couple of years later, a guy named Pete Busher came and started working with them. He trapped, and marked, and he got a thesis. He actually did a lot of his work over in Nevada. He worked here, and at Cal, and I think he actually got his degree from the University of Nevada. So that continued the continuation, and then after him a girl named Ellen Woodward started in the middle '80s. By this time they were using telemetry; they were taping little transmitters to the beavers. So really what we have is a pretty good continuity from 1945 through about—I think Ellen finished about '90. Forty-five years. There aren't many cases where we have that kind of long-term record.

Hughes: Also, I know from talking with Dick Gard yesterday, you got slightly different takes on what these beaver colonies were doing. He was interested in their interaction with the trout. So it's not a story of just looking at this colony from one perspective; it's many different angles over this period of time, which I think would give it even more value.

D. Taylor: Oh, yes, definitely. But still there's the common core of marking and recording and getting the ebb and flow of the population. One of the things about beaver on a small stream like this is the fact that there isn't a food base for a sustained yield of a high population; the population kind of ebbs and flows.

Hughes: And that's what you've seen?

D. Taylor: Yes.

Hughes: What about classes up here? You first came in 1958?

D. Taylor: Yes.

Hughes: Were classes at that point regularly coming up here? When did they begin?

D. Taylor: Well, early on, there would be a wildlife class and a botany class and an entomology class at least every other year; they were here. But there were classes going on pretty much every year. That sort of tapered off. Well, it has kept on to this day; I think there's still an entomology class every other year, and I think Davis gave its botany courses periodically. Davis actually has done more of this. They haven't had a wildlife class for quite some time.

Hughes: Davis hasn't?

D. Taylor: Well, Berkeley put on that original wildlife class, and several after that. A lot of the classes like mammalogy and ornithology, they'd come up for field trips, or used to. Our daughter Kit took the botany class when she was at Davis, so that was eighteen or nineteen years later. So the classes are a continuing tradition, and that's one of the things that Jeff would like very much to really get going strongly again.

Hughes: The '90s was a bad period, wasn't it?

D. Taylor: Yes, it was.

Hughes: What kind of impressions do you have of how the department at Berkeley looks at Sagehen, both in the past and now? Does it value what Sagehen represents?

D. Taylor: Well, I can tell you quite a bit about that, if you want.

Hughes: I want to hear it. [laughter]

D. Taylor: Sagehen was originated by a combination of wildlife and fisheries. Starker Leopold and Paul Needham were the two—and also the entomologist named Usinger. Herbert Baker was the botanist, and they were all very interested, very enthusiastic. I guess all of them were in the zoology department at Berkeley. I don't know about Usinger. Seven or eight years later, there were people, for example, at the molecular biology end who were kind of pooh-poohing the old natural history approach. At that point Paul Needham died, for one thing, and Starker moved Wildlife Fisheries over to the school of forestry, which is still at the school of forestry.

Hughes: Why did he do that?

D. Taylor: Well, he felt that he fit better over there. Wildlife management is really land management. You don't manage animals much. [laughter]

J. Taylor: Just try.

D. Taylor: Also, forestry was trying to reinvent itself. It had already foreseen the need to reinvent itself. They were very interested in having a wildlife section. The combination of push and pull was that he moved from the Museum of Vertebrate Zoology to the school of forestry. I guess Don Erman was hired at that point as the fisheries guy to replace Needham. So there was a wildlife fisheries unit then in the school of forestry. That was a going concern, and Starker and Don were enthusiastic about it, and enthusiastic about Sagehen. But the school of forestry had their own camp, over here on the west side, so that was where the forestry students did their field trips. There was a great deal of redundancy between Sagehen and there. The tendency was for the school of forestry to continue using their own camp, rather than Sagehen, and at the same time, there was somewhat of a downgrading of the old rubber-boots and field-glasses approach as opposed to the various sophisticated technical stuff in the zoology department. And there was a money crunch. So the combination of these things kind of started the downward slide of Sagehen. When Starker died, he was replaced by Reg Barrett, and later Dale McCullough, and Marshall White was in there too, during that period. Marshall was interested in Sagehen, but he was the junior man. Reg was up here a little bit. He and I went out and helped Ellen Woodward get started on her beaver study here. But he was very busy elsewhere. Dale did his work on the tule elk and deer, and he never really was that interested in Sagehen. And then along that time, why, there was a continuing money crunch, and there was actually some turf fights and even bad feeling about who's going to pick up the bill, between Berkeley and Davis and others. This kind of led to this decline through the '80s and early '90s. I just talked to Reg Barrett and Dale McCullough, down there in the school of—what is it now? The school of environmental or natural resources, and boy, they are just singing the blues. Their budget has been cut way way back, and they really can't afford Sagehen, and neither of them wants any part of it.

[End Tape 1B] ##

[Begin Tape 2A]

D. Taylor: — [loss?] of Sagehen. [Entered at edit: What I am saying is, neither Reg nor Dale have the time or resources to fight for Sagehen.]

Hughes: Mainly because of the money drain?

D. Taylor: Mostly the money.

Hughes: And it's low priority, probably.

D. Taylor: Yes. Dale is now retiring this month, and Reg has about five years to go, and he's up to here in other things. Now, they just hired a new guy, and I don't know a thing about him, or what his interests will be, or how he would feel about getting involved with Sagehen.

Hughes: What significance is there that Sagehen is now officially in the NRS, the Natural Reserve System, and [Vice Chancellor for Research] Beth Burnside's office has some kind of association.

D. Taylor: I'm not the person to evaluate that, but I'm very excited about it; I think it's wonderful.

- Hughes: At least it means that the University is paying some attention. And one hopes that the money comes as well.
- D. Taylor: Oh, I hope so. And also, if they can share the load, and get other schools, like Davis for example, who will want to come up here and give classes, or send their students, to share some of the cost. That's what Dale and Reg keep talking about.
- Hughes: Anything more that either of you have to say about Sagehen?
- D. Taylor: In terms of my perspective with the beaver, I'd love to know more about how beavers affect hydrology and stream flow and the riparian environment. It'd be natural to continue with that. Jim Kirchner is interested in that, and he has a student that's interested. And also, for the really long term, the geomorphology of a valley that's been formed. Ernest Thompson Seton said, "Oh yeah, these are beaver meadows; they've been formed by series of depositions of silt behind beaver dams since the Pleistocene." That's what we see, the shape of these lovely mountain valleys that we see around the Rockies. I don't think anybody has done any really good work on it. If we can get some geologists up here—of course there is a lot of interest of hydrology in general, and the flow of the Truckee system in particular. In addition to that, there's the endangered species thing, because the Lahontan cut-throat trout was a staple of the Paiutes. They came up out of Pyramid Lake like salmon; they were big, big fish, and they were a major food source. And there are treaty rights to that, and I'm sure you're aware that treaty rights are heavy stuff when it comes to priorities and allocating funds, and things like that.
- Hughes: It seems as though Sagehen should be able to tap into some of that state money.
- D. Taylor: Yes, yes.
- Hughes: June, do you have anything more to say?
- D. Taylor: No, I think we've covered it.
- Hughes: David, do you want to say something on a broader level, how interests may have shifted in the course of your career?
- D. Taylor: We have a ranch up north, and we have friends who, it happens, belong to a field station up on the Malheur Lake. I think a field station truly can be a formative thing in a person's life. The need is still there, there are still the jobs that require somebody to go into a place, and identify the plants, and find the endangered species, and say what needs to be done to preserve them. These people are getting harder and harder to find. Certainly this whole era of collecting data, much of it automated, and putting it in the computer so it's available, that's the present and the way of the future. But I don't think we'll ever get away from the need to have people that are sensitive to the environment, just be here on the ground and—
- Hughes: Look.
- D. Taylor: --look. I think this involves traditions and ways of looking at things, philosophies, and this is something that for many people, a field station has been very important.

- Hughes: Yeah, and it seems to me that in a field like this, a mentorship would take a really significant role. Because a lot of what you seem to be talking about is values; it's not things that you get from the textbook.
- D. Taylor: Yeah, I think that mentorship is a key word. It's more likely to enrich and grow and become richer in this sort of environment.
- J. Taylor: When you get away from those kind of student-teacher relationships, it's easier to get to know each other as friends in a place like this, where everything is casual, and informal, and hopefully welcoming. So there seems to be many dimensions in which a place like this contributes.
- D. Taylor: Oh, definitely. Well, boy, I'm just so happy to see what's going on.
- Hughes: What did you think when you walked in? How long had it been since you'd been up here?
- D. Taylor: Well, I was up here last summer.
- J. Taylor: The longest period that I went with not being here is just this past ten years. I'm working now and of course I right away saw a difference. I realize the hard work that Jeff and his wife have been doing, and other people. But just the whole idea that Sagehen's going to stay and be preserved and taken *care* of. It's wonderful.
- Hughes: Well, I certainly thank you both.
- J. Taylor: All right.

[End Tape 2A] ##
[End of Interview]



Don Erman, August 1989

DON C. ERMAN, PH.D.

[Interview # 1: 9-29-2004]

[Begin Tape 1A]##

Hughes: Let's start back with your family of origin, with who your parents were, and your education.

Erman: Like the names of my parents?

Hughes: Yes, and what they did, and where you lived.

Erman: Father's name was Melvin Francis Erman, and mother's name was Verma Erman, both mid-westerners, born just at the turn of the twentieth century, 1908-11, somewhere in there, neither of whom finished high school. My father was born in Chicago, worked for Wrigley's [chewing gum], married my mother, who was a Wrigley girl (an entertainer). He served in the Second World War, and when he came out, somehow he decided that he wanted to open a toy store. He and some friends from the service opened a toy store in Indianapolis, which was my home. I was born in Indiana, but not in Indianapolis. They started this small store, and after I think six months or a year, the two fellows who were partners decided they didn't want to be in the toy business, and so my father and mother bought them out, and my father and mother then ran the toy store in Indianapolis until it was condemned by the state so they could build a parking lot. Downtown Indianapolis. So that was in about 1960, around in there, just after I finished high school. I graduated high school in '58.

Hughes: I imagine as a young person you were very popular, with parents who had a toy store.

Erman: They were very, very reasonable on not just opening up the candy store. We never got everything we asked for; we weren't overwhelmed with toys. It was very modest. It was a very modest living as well.

Hughes: You have brothers and sisters?

Erman: Yes, older brother, four years older, whose name is Melvin Francis Erman, Junior, and a sister who's a year younger than I am, Carol.

Hughes: Well, tell me what happened after high school. Were you getting interested in science at that point?

Erman: Well, I was always interested in the out-of-doors kind of thing, and I was a fishing nut. Both Mother and Father liked to fish, so it was quite common,

after work or on weekends, we would pack up and go out to a river or a lake or someplace in Indianapolis and go fishing. So that was my introduction to that. But I just liked everything that I'd seen, everything that was out there--birds, and turtles, and frogs, and whatever it was. I was interested in those things, and it didn't seem like that was something you could actually do. So probably like most I thought, well I'll be a doctor, and that was what my folks thought, "Oh, well, you can be a doctor." So I liked to memorize parts and pieces of things, and I took a lot of zoology and botany when I was in high school, and enjoyed that very much. I wasn't very hot on the physical sciences; that was always difficult.

I went to a school in Indianapolis that's called Arsenal Technical High School. I don't know if you've ever heard about it—very unusual school. It was in an old civil war arsenal, and the school at one time had as many as eight or nine or ten thousand students, and it had a whole campus, so it was very large; big enough that a freshman needed a map. When I was there it was like five thousand students. "Technical" because it had everything from high school college prep sorts of things, but included this huge technical component, so you could major in auto shop, for example. Oakland Tech would be comparable. It had this very diverse, very wide student body as well. But being large, it had a very good curriculum also. Ph.D.'s were teaching biology and English.

Hughes: Were you on the non-technical side?

Erman: I never took any of the technical side of the things. If you majored in the other things there wasn't much room to pick up a shop class of one sort or another, or radio repair or whatever it was. So I didn't, but a lot of my friends did. And I thought it was kind of neat that there was all this stuff there.

Hughes: And were you always headed for college?

Erman: I think so. I liked studying, and I liked learning, and it wasn't that hard for me, so I got lots of As, and I thought well, I should go to college. Particularly if I wanted to be a doctor, since I liked biology.

Hughes: Yes, that is necessary. And then what were the choices when you came to college age?

Erman: Places that wouldn't cost any money, basically, so it was either going to be IU, Indiana University— I didn't think much about Purdue, because my brother, four years older, had gone to IU, so I was already tuned in to that. Or else another school that I could get a scholarship from. Friends and counselors said, "Well, you probably can get a scholarship. You've done things." I was in athletics, plus a reasonably good student, so it seemed I could probably—that's actually what happened. I also applied to a small liberal arts school that a lot of friends from high school had gone to called DePauw University. That's Dan Quayle's old school. You know former vice-president Dan Quayle, but you might not know DePauw. It's a small liberal arts college, just forty miles from Indianapolis.

Hughes: And that's where you went?

Erman: And that's where I went .

Hughes: Now, why there rather than Indiana?

Erman: There were several friends that I knew from high school who had gone to DePauw, and they were just extremely enthusiastic about it, and said, "Oh, you really have to come here; you would really love it; it's really a good school." And weekends, to go visit and stay with them in fraternities, and see things, and it was nice, and I liked it, and I did get a scholarship there, so it seemed reasonable.

Hughes: Did you consider what the science curriculum was?

Erman: I knew very little about it. Again, what few high school teachers I talked to said that DePauw was a very good school for pre-meds. That's about all I knew, and I figured, well, they must have science there. I assumed that IU, a big school, would probably have everything, but I didn't look into it beyond that. I didn't know what they had. As it turned out, knowing now what I know, their biology and so forth was really focused on pre-meds.

Hughes: At DePauw.

Erman: Yes. It was a very limited curriculum. It was just basic anatomy, physiology, embryology—

Hughes: Very human-oriented.

Erman: Oh, yeah. They had a nursing program and a pre-med program.

Hughes: You were pre-med?

Erman: And I was a pre-med. So that was fine. My interest didn't really change; I was always interested in zoology and things like that. I didn't do too well in college. I had a hard time at first, particularly in physical sciences. I was just barely hanging on, and I thought, why am I doing this, because I don't particularly care whether I'm a doctor. So I finally in sophomore year, junior, somehow I changed and said, "I'll just be a zoology major."

Hughes: And then were there courses in zoology that appealed to you?

Erman: They all sort of appealed, because I still liked learning the parts and the pieces and so forth, but not very much that was relevant to the natural history side of things. There was one course in the botany department; it was Indiana local flora that I took when I was a senior, and I was just delighted by that. My senior year, they also hired a new zoology professor who was from Wisconsin, and interested in fish, and taught ecology. I took the ecology course, and I took a special seminar with him, and we did a field trip the senior year with some students to the Appalachians and collected fish, and I went out with him on weekends, and we did collecting in local streams, because he wanted to know what was there. That really—

Hughes: That did it.

Erman: —capped it, yeah. I said, "This is what I really want to do."

Hughes: And who was he?

Erman: A man named Dr. Jim Gammon. He had just finished his Ph.D., and it was on fish, at the University of Wisconsin, which was a big school for fish biology and the study of fish and so forth. It was probably a premier place at that time in the U.S., a very long history of faculty, the first faculty in the area, lots of ecological things came out of Wisconsin. Aldo Leopold was from Wisconsin, and it was sort of *the* place. I thought about graduate school at the time that I finished, too, and Wisconsin was one of the places I was thinking of. Michigan was the other big aquatic place that I was thinking of.

Hughes: And was Gammon encouraging you?

Erman: Yes, he did; he encouraged me to go on. But a strange thing happened in my senior year [laughs] that changed things. I did a lot of odd jobs, because the scholarship didn't pay everything, so I did lots of odd jobs, and I had managed to save a little bit of money. I was always saving, because I figured I was going to have to put my way through. De Pauw had an exchange program with schools in Europe, and England was one of the places, and also with Germany, and there was a faculty member who organized that. And what happened was, an English student was coming over for that year, but somehow had fallen through the cracks in getting everything arranged. So suddenly this guy arrived, and none of the arrangements had been made about where he was going to stay. So the faculty member came to our fraternity, and said, "Well, this guy is here. If you have any room, can you work him in? Can you do that?" And the fraternity said, "Yeah, sure, one more guy, it's not going to make any difference." He said, "We'll also have to figure out as we go along, there are some other payments that have to be made, but we'll worry about that later."

So as it got to the end of the year, and the fellow was in the fraternity. I got to know him a little bit. The faculty member came back to the fraternity and said, "Well, since you really footed the bill for him, you paid his room and board, more or less, why don't you pick who will be the student that would be the exchange student to go back to England the next year," because that's how it worked. And the way it also worked was, the student that would be in the U.S., the American student, would pay his fees, and when you were at his university, he would pay your fees, such as they were. So it came time toward the end of the spring semester, and the fraternity held some kind of a "who wants to go, and why do you think you should?" There were only a couple of people who put their names in, and I put my name in. I thought, "Well, that would be great." Ordinarily it's a junior year abroad. I was a senior, but again, the faculty sponsor said, "That's fine. It's up to you; we'll work it out." So I got it; they picked me over somebody else, and then I had to pay the fee that was due for this guy. It just so happened I had managed to save up just enough money, because I thought I was going to need that for graduate school. \$250; it was all I had. So I said, "Well, okay, sure."

Hughes: Opportunity.

Erman: The opportunity, yeah, I thought, "That's fairly cheap; it won't cost me anything." I hadn't thought about how do you get there and get back. So I had a year in England [1962-1963].

Hughes: Where was it?

Erman: Durham University.

Hughes: And doing what?

Erman: Wide open, since I was a strange kettle of fish. I wasn't a junior year abroad; I'd already had a degree by that time, and all I said--I corresponded a few times--was I was interested in zoology; I was a zoology major, so whatever I could take. So when I got there, I just went over to the sciences colleges and met a few faculty, and they sort of tried to find out what I was in, and they said, "From our judgment, a Yank who's spent four years is about equivalent to our two-year students, so you'll probably fit in all right for our third-year honors student. I thought, well, I'm not so sure, but that's fine. It was a good year.

Hughes: So you took courses.

Erman: I took courses, right.

Hughes: Were they related to what you eventually hoped to do?

Erman: Only one. It turned out that there was a new faculty member there who taught a course that was experimental for that year called limnology, the study of lakes. I took that class, of course, and the other courses were fairly a repetition of what I'd had. They were again anatomy courses—you heard them again with a different slant on it. I'd already had some parts and pieces of it. But an awful lot of the English curriculum isn't based on going to classes. In fact, no class had more than one lecture a week. In your third year, unfortunately, it's even more so, because the students are preparing themselves to pass comprehensive exams. They're expected to make up their three years of knowledge by studying on their own and preparing for these comprehensive written finals. Since I wasn't going to be part of that, it didn't really apply to me. So the only other thing that happened, a lot of independent studies and seminars, and I'd work on writing some paper. It was educational in other ways. Because I could see that their emphasis was also on what you learned and how you learned had to be totally self-driven.

Hughes: That's very English, isn't it? The English educational system.

Erman: It's more like our graduate school. The emphasis is not on going to classes and having it dumped on you and poured in, "This is what you need to know," but rather, "This is a field of study. You should go and find out what you need to know. Read whatever books, and here are references." But you didn't have a text and things of that sort. So it was very different from what I'd had at DePauw. Even in the labs, it was really astonishing to me, because I'd had a lot of anatomy courses and dissections and so forth, and here was another one with a slightly different slant on it. So we came into the lab one day, and the person who was the lab assistant started handing out—I can't even remember what it was, whether it was a lizard, or a snake, or something, and I saw this thing, and [he] said, "Do a general dissection." What? There's no lab manual; there's no "Start here; do that." It was just: "Here's the specimen, there's your scalpel; do a

general dissection.” I was thinking, “Well, that’s an entirely different way of doing it.” I had never, ever had someone say to me, “Here it is; do it.”

Subsequent to that, I found in historical reading of Louis Agassiz, about the way he got into biology was, he was studying at wherever it was, Harvard or someplace like that, and went in to see the professor who said, “What is it you want to do?” Agassiz said, “Well, I want to study birds,” and he said, “Okay, go into the lab there and I’ll come in in a minute.” He came in and he dropped a fish in front of him. Agassiz said, “What do you want me to do with that?” He said, “I want you to find out everything you can with that, and I’ll ask you some questions in a week.” And Agassiz just started doing that for a week, and that’s all the professor told him, was just, “I’ll ask you some things about what you learned.” So he said he started with the outside and worked in.

Hughes: Not a bad system.

Erman: [laughs] No, but a very different way of doing it.

Hughes: You had been in the Midwest, and now you were in Britain where the whole ecology is different. Did that enter in, or were you at such a general level that you could have been anywhere?

Erman: I almost could have been anywhere. I did have an ecology course over there that was talking about populations and a lot of the same principles, and very often the instructor would ask a question. I would have, from the ecology class at DePauw, a nicely memorized definition, and I often provided them. He said, “That is such a distinctive thing about the Yanks”—they had this way of always saying “Yanks”—that you have this simple definition of a very complicated thing. This was a stunning put-down. I thought, “Oh, okay, no more definitions, [laughs] no more definitions.” The English have a way of just really putting you down.

Hughes: Yes, putting you in your place.

Erman: But it wasn’t really so locally based. But I did take some field trips with people that were out to natural areas, and looking at those, and it was places that were so far away from anything I’d ever seen--moorlands in Scotland, and fens, and things of that sort.

Hughes: Did both these concrete observations and also this philosophy of learning that you were picking up carry through in your life?

Erman: I think the philosophy did, the way of approaching knowledge, both institutionally and also in the way the other students took it, and how they assumed they should learn, and their approach to it.

Hughes: Also, when you began to teach, did it affect the way you taught?

Erman: Yes, those experiences did, as did my wife’s. Nancy was in the same area, and had taught school, and was of the educational background that students learn by doing rather than being told, “This is what you do in order to get the right answers.” So, these were among all the experiences that affected me.

- Hughes: I would think that would be a very good approach to zoological observation. Not starting with any real premises, but just, what am I seeing?, and delving deeper.
- Erman: Right. I could talk quite a lot about how I taught my class here. In fact, it was written up one time, so actually there's a little paper somewhere that the campus did on using research in teaching. But the approach I had—I'd spent like a couple of days with the class talking about the philosophy of how to learn, and how I was teaching, and what I wanted them to do and how they were to approach it. We took field trips where I talked about the same thing. The basics of it were that you learn because you ask yourself a question and then you go out to find the answer. You don't learn by sitting there and just waiting for things to be packed into your head. You can't do it that way.
- Hughes: So you have a premise that you're trying to prove or disprove?
- Erman: Or just an observation. You wonder about something. It doesn't have to be a hard hypothesis. That works sometimes in certain kinds of fields, or if you've got one, that's fine, but very often you just ask yourself a question, like, "I wonder if, I wonder what...?" That's enough. That's really the basis of how science goes.
- Hughes: Was that new? That approach?
- Erman: It seemed to be for the students in the class. Even here (at Berkeley), and I was surprised by that. Many students told me afterwards that they had never had science courses that were approached that way. That it was always, "Here's a set of principles and facts and ideas that you have to learn, because this is what the field is, and this is what science is." Very little of it that came from inside them and which they said, "Here's something that we ought to know, and how do I go about learning about it; what do I do." So the laboratory portion of it, if you could call it that, was a major part of class, rather than me telling them, "These are the facts you have to learn; pack it in." I more or less gave them interesting stories, things to get them excited.
- Hughes: And then it was up to them to explore. Interesting. Okay, so you came back from Britain. What happened next?
- Erman: I worked that summer for the Indiana Department of Fish and Game, which was my first real contact with actual work in the field, and began applying to graduate schools. I had applied to graduate schools before I went to Britain and had been admitted to the University of Michigan, and then I said, "Well, I've got this thing for a year," thinking I'd just delay it for a year. They said, "No, you can't—apply again next year." I had done that again, started writing letters of application, and I didn't get admitted the second time, after the year in England. At the other place, I can't even remember too many details about that. So I was sitting there at the end of that job with the Indiana Fish and Game, which was just a summer job, with really no prospects. This was in '63, and that was at the time when things were beginning to heat up in Vietnam. I knew that probably I would either be drafted or I should join the National Guard or something like that. I actually had gone, the Labor Day weekend, the Friday before that, I'd gone over to Fort Benjamin Harrison in Indianapolis, picked up my papers to join the National Guard, and the guy said, "Fill it out and bring it in on Monday." When I came back that day, my father was home from something. He said, "By the way, one of your professors called, and wants you to call him right away, no matter what time, or the weekend or

anything, but call him right away; he says he has something very important for you.” It was the zoology professor at DePauw. I called him up, and he said, “A friend of mine at Purdue, in wildlife, needs a graduate student and has a project, and I thought about you. Do you have anything? Can you get up there?” Purdue had already started classes; it had started three or four days earlier. I said, “It sounds terrific; even though I’m interested in fish, I’m interested in wildlife too.” So he gave me the guy’s name, and told me to call him and go on up. I went up there and saw him on Monday, right after the Labor Day weekend. He [Dr. Charles Kirkpatrick (“Kirk”)] talked to me and said, “Get your stuff in, and it’s good enough if you’ve got a recommendation from this professor, and how are your grades?” I said, “Well, B-minus or so.” He said, “We require a B average flat minimum to get into Purdue, but we can take you for a semester on probation, and if you do okay then we’ll go on.”

Hughes: Lucky break.

Erman: That was a lucky break.. I was probably an hour away from joining the Army. I was at Purdue there for two years, did a master’s degree in wildlife management. That was the first time I began taking coursework that was really down the alley of things I was interested in.

Hughes: Fisheries?

Erman: But not fisheries. No, but they had natural history of the vertebrates, and one semester of that dealt with fish and reptiles, so I did a lot of collecting, and learned a lot through that of sort of the natural history of fish, and—

Hughes: Had you—were you heading in that direction already?

Erman: Right.

Hughes: You were thinking fisheries is what you wanted?

Erman: Yes, and I’d applied to graduate schools at Michigan and other places in their fisheries programs. So I picked up whatever I could, took other ecology courses in plant ecology and animal ecology, insect ecology, and as many different things as I could. I had a pretty broad background, and I did independent stuff on fish, but meanwhile continued to work. As it turned out, the project I worked on that he asked me there for fell through because of a problem with Fish and Game doing something to the plots. I was studying quail use of wildlife cover crops called *lespedeza*, or something like that. So I had to come up with some other project. He said, “Well, the funds are here, so it’s whatever you want to do.”

Hughes: Oh, wow!

Erman: As long as it’s somehow connected to wildlife. It turned out that Fish and Game also had a restored marsh north of Lafayette, and they wanted someone to study that, because they thought that waterfowl used these restored marshes differently depending on features that were in different marsh ponds. So I did my master’s basically on comparing a couple of restored marshes in an area just sixty miles from Purdue.

- Hughes: So you were getting closer to your interests all the time.
- Erman: Right. Again, that study was basically all aquatic. Even though the intention of it was to reveal information about how the waterfowl might have nested or used it. It really was just a straight aquatic ecological study.
- Hughes: Did you have mentors there that made a difference?
- Erman: Yes and no. In the technical sense of the field, no, because there were no real aquatic biologists at Purdue. They had no fisheries people, because at that time Purdue said, "We handle the wildlife; Indiana University handles the fisheries. And never the twain shall meet." Just after I left they added some aquatic people. But, I just picked it up through other areas. It was again, I thought, well, I'll learn the fisheries and aquatic stuff on my own, and I'll learn the wildlife through these other things. My major professor, "Kirk", was very good on helping me try to write, compose, understand, ask questions, do science. He knew very little about what I was actually working on.
- Hughes: He could give you the framework.
- Erman: Right. I had other advisors, one in entomology, Dr. Leland Chandler, and one in plant ecology, Dr. Alton A. Lindsey, who were just good ecologists.
- Hughes: Did this broader background prove useful later on?
- Erman: Well, I think so. I think that's missing more and more. The disciplines always collapse in, narrowing things.
- Hughes: Yes, they do.
- Erman: And the whole idea of the way I was learning ecology was, it was something that was going out the other way. It was to get out of the discipline of zoology, or botany, or entomology, or whatever it was, and you're dealing with the interactions at a system level. I just believed that. That makes perfect sense. If you're going to understand a system, it isn't just animals, it isn't just plants, it's also the physical environment, so it's all there together.
- Hughes: But that way of looking at nature wasn't imprinted on the educational system, from what you're saying. That was still all rigid disciplines.
- Erman: Fairly compartmentalized, yes.
- Hughes: But that's not true nowadays, is it?
- Erman: I think it goes back and forth. Depending on where you are, it can either be subdivided into the small slices, but some programs are talking now in terms of systems. So they're trying to meld it back together.
- Hughes: Multi-disciplinary approach, right?

Erman: I still observe, less so since I'm not so much in contact with students, how often, if they are in a biologically oriented program in even something like ecology, that their understanding of the physical environment that the organisms are living in is pretty thin. So I found in my classes when I would spend a lot of time talking about the oxygen concentration or the flow of the water, and the movement, and the interaction, and the physical ways, again, the students often said that they'd never heard that. They'd never thought that had much effect, and these were students that often came from MVZ or the Integrative Biology, where they knew tremendous depth and detail on the evolutionary biology and the biology of the organisms, fish or whatever. They had never seen that organism in a physical environment. It was frustrating to me sometimes, and I hope this doesn't offend some of my colleagues, that there would be Ph.D. students, who would be studying fish, and that wouldn't know how to measure oxygen. God, they live in an environment where that's critical! How could you ever have gone through a doctoral program involving an aquatic organism and not even know, when you talk about oxygen concentrations and saturations, that this is essential material?

Hughes: Did that lead later in your career to some clashes?

Erman: No, no, probably not. Probably there were some students that avoided me on exams. But I never killed people because they just didn't know some of those things. Usually it was in their own area that they stumbled in, rather than something I would have asked them.

Hughes: All right. So then what is the next step?

Erman: Finishing up Purdue, thinking about, okay, I want to continue to go on, get a Ph.D., and I want to actually do something in fisheries, and applied again to different places, and got a research assistantship to Utah State University. Again, it was a person who I was in contact with who was also a University of Wisconsin graduate who had known the ecology professor that I had at DePauw, Jim Gammon, and it turned out that the wildlife professor, Charles Kirkpatrick, that I had at Purdue was also a Wisconsin Ph.D.

[End Tape 1A] ##

[Begin Tape 1B]

Hughes: Was your wife also in the same field?

Erman: She was in terms of the general area, although she thought early on that probably she was primarily to be a teacher, which is what she did for a time. She was a senior when I was a first-year graduate student at Purdue. So we met that year, and then when she graduated, she took a job teaching in Long Island, general biology and science, sixth, seventh, eighth, and ninth grades. We got married after her year, which was the end of my MS. When we went to Utah State her intention was that she would probably also continue teaching in that general area. But we had the same interests; we met in plant ecology classes, and we had some of the same classes, so we were studying the same things. She'd been a naturalist for a year at a park in Indiana, and knew more about plants and trees than I ever did. So we really hit it off in terms of that. She liked water, and she liked insects, and she'd done all kinds of insect collecting, and so on. When we

got to Utah State I had a grant I worked under that covered my educational costs, and she looked around and found a job, got a job as an assistant in a soil physics lab. Did that for a while and then finally a professor who was in the same department I was in said he needed another student, and would Nancy want to work in that area, because he'd met her, and knew about her. So she also started in the same department, working on something quite similar to what I was working on, for her master's. Because she figured she'd need an MS anyway, for teaching. When she finished that, then our last year at Utah State, she taught in Logan High School, again, general biology, general science and so forth. So, I spent four years at Utah State [1966-1969].

Hughes: What was the grant? What were you doing there?

Erman: Again, it started out as one thing and ended up something totally different.

Hughes: [laughs] This seems to be a pattern.

Erman: Yes, it does. My major professor, Bill Helm, had a grant from the Atomic Energy Commission (AEC) to study the effects of x-rays on fish behavior.

Hughes: My heavens.

Erman: He didn't know a thing about x-rays, or how they would affect fish, but there was a lot of money from the Atomic Energy Commission at that time.

Hughes: You mean natural x-rays, or induced?

Erman: Induced. Because he made up some idea that x-rays, or radiation, let's say, radiation, might affect behavior; even if you didn't see injury reflected in outright death, it might change behavior and therefore you would have communities that would be affected. The Atomic Energy Commission, you know, anything that seemed reasonable. So we got an x-ray machine and we were down there in a basement, and we were also studying a very small fish that lived only in one lake in North America, that is this lake up on the border of Idaho and Utah. It was called Bear Lake, and it was the Bear Lake sculpin. It was a strange little fish, and it was hard to keep in captivity. After a couple of years of trying to know exactly how much radiation you were hitting on the medulla oblongata, the base of the stem of the brain, with just a targeted amount of x-ray, well... I finally wrote up something and said, "I don't think this is really a profitable venture." The AEC was just delighted that somebody said they didn't seem to find anything, or [laughs] it was not a good idea to keep pursuing. Nobody ever does this.

Hughes: You were looking for gross changes?

Erman: Well, we were trying to study the behavioral changes after you gave them a certain amount of x-ray. But giving them the dose and keeping them alive was just a major problem, because you're dealing with totally wild fish. So you have to think, how do you keep them alive, how do you feed them, how do you provide all that? So there was all of this working up before you could ever get to the problem.

Hughes: I see. And of course the AEC had probably skipped over that part of it.

- Erman: Well, no, they thought, you said you could do it, so do it. So after two years of doing that, finally that ended. Then I had to come up with another project.
- Hughes: But at least you'd landed in fish!
- Erman: Yes, yes, yes, and I was still taking lots of interesting courses and going out in the field and learning more about everything that lived in water. I eventually did something different.
- Hughes: And then the University of California, Berkeley [1969-1998] is the next step?
- Erman: That's right.
- Hughes: How did that appointment [Assistant Professor, School of Forestry and Conservation, 1969-1975] come about?
- Erman: I began applying for positions my last year at Utah State, when I was writing up my thesis, in '68-'69. I had applied to the University of Wisconsin. They had a position open, and then I'd heard sort of belatedly that Berkeley had an open position. At first Berkeley thought it was going to be a position that would just be a research position at Sagehen, and then it changed. That was a little bit of the history of things, and Marshall White came out of an academic position and took the research associate position and freed up an FTE in an academic position.
- Hughes: So it made it even more appealing, presumably.
- Erman: Yes. And then they were advertising it as a regular tenure-track faculty position, half teaching, half research in the UC Agriculture Experiment Station.
- Hughes: Had you, prior to that, decided that an academic career was what you were after?
- Erman: Not totally. I wasn't quite sure where I was going to go from there, and I didn't know how difficult it would be to get faculty positions, but I pretty much had thought, I like research, and I wanted to do that, and I didn't know how much research I'd be able to do in any kind of an agency-related position. But I just really didn't know how many positions-- The word was that academic positions in fisheries, aquatic ecology, were few and far between, and it certainly seemed like that. So I applied to a number of different places. Bemidji State, University of Wisconsin, University of Maine, Berkeley. I got an interview at Wisconsin. It was for a limnology position, which was the study of lakes, and my thesis actually turned out to be that. Then the position at Berkeley, and I applied for that, although when I was first told about it by a friend of mine, a faculty member at Utah State, I said, "Berkeley would be the last place on earth I would want to go." He said, "Why that." I said, "Well, it's just crazy." All the riots were occurring then. And it just seemed like it was a place that was chaotic. I knew its reputation as an institution, but it seemed like it was this crazy place where stuff was going on; there were already all these gassings going on on campus. Well, I didn't get invited at Wisconsin, and I hadn't heard back from Berkeley, so some friends of mine plus Nancy, my wife, said, "Why don't you just tell them that you're going to be out there, and ask if you can drop in"? So I did; I wrote to the School of Forestry and said, "I'm going to be in Berkeley on some other business; can I drop by and maybe get to

stand around and say hello?" They said, "Oh, well that works great; come on, yeah." So I did, and they said they were going to invite me anyway, so they paid for the travel.

Hughes: That was nice. Did you meet anybody there of significance on that first visit?

Erman: I met everybody in the School of Forestry and Conservation, and I immediately liked everybody I met. My visit became a normal interview visit. It was so interesting, and the thing that was also interesting and I thought exciting, was that it was so broad. It dealt from the economic policy side all the way through to the plant ecology, plus they had wildlife, and range. It just seemed like here was a school that really did embrace this extremely wide area of natural resources. My impression—impressions aren't always reality--but my impression, also having Leopold there, there was this strong sort of conservation element. It was part of the school title, and a lot of the people I spoke to talked about conservation issues. They liked to talk about that. It was not so discipline oriented, because anybody you talked to was in another discipline, if you will. Somebody'd be in range, somebody'd be in this, but they all talked about things that were, broadly speaking, issues of broad conservation. And I found that really exciting.

The day I interviewed, by the way, I must tell you. The day before, there had been a major National Guard gassing. So when I came across for the interview on that Monday morning, you could still smell CS gas lingering [laughs] as I came across Sproul Plaza.

Hughes: Did that give you pause?

Erman: No. The other thing that excited me, I stayed in the Durant Hotel, so I walked cater-corner to Mulford Hall all across campus early in the morning, and again, it was almost mythical, it was kind of like, "This is what a college campus is supposed to look like." It was beautiful, and the stream running through it, and there were all these birds hopping around that were wild birds, brown towhees; it was really like walking through a park. That was impressive, too.

Hughes: So you came.

Erman: I came. They wanted me to come that summer, and I had already, because I didn't know if I was going to have a position, I'd worked out a temporary summer job working in Yellowstone National Park, counting fish from fishermen, how many fish they'd caught. That was going to go from when I got out of school, graduation, through probably September; I think I could have worked through October, and that was as long as they could keep me as a temporary. So I thought, well, that's a job; I can continue to look for other jobs while we're up there. So I thought since I'd committed to that, even though they wanted me to report and teach a course at Sagehen that first summer—

Hughes: Did they?

Erman: Yeah, because alternate years they taught this wildlife-fisheries course, and '69 was the odd year it was to be taught. They didn't have anybody; they wanted me to come, and I said, "Well, I've already said I'd do this job, and I really feel if I said I would, I should." So—

Hughes: You did.

- Erman: I did. Worked for the U.S. Fish and Wildlife Service in Yellowstone National Park interviewing fishermen.
- Hughes: So that meant you came in the Fall of '69?
- Erman: September first.
- Hughes: And what did you teach?
- Erman: The first year I developed a seminar, 198. The Dean, John Zivnuska, said, "Take the fall quarter and travel around." Gave me some extra money and said, "Go find out who your colleagues are, since we don't have anybody here in fisheries. Go around." I got some help, and I went to Humboldt State, and I went to Oregon State, and the University of Washington, and stopped off at British Columbia. I just did the tour. It was fantastic.
- Hughes: And made connections.
- Erman: Made connections, yeah, met people that I stayed in contact with through a whole career.
- Hughes: When do you first go to Sagehen?
- Erman: I went up there that fall, probably. Nancy and I did the tour of the Pacific Coast like the 15th of October to the 15th of November, something like that, and I think I went up to Sagehen with Marshall White on the first of October, end of September.
- Hughes: And what was your first impression?
- Erman: It was really neat, just all the things that I saw. I told this story to Jeff Brown, and I think I told it also to Jerry Booth, that something happened on that very first trip that ended up directing a lot of our research. Marshall was taking me out to this real interesting place that was at Sagehen, which was this area of wetland that they called Mason's Bog. I don't know if you went out to it, but it's this unique feature that's right across the creek from the main station. Several investigators, professors from UC, had studied it and so forth. And he said, "You're an aquatic ecologist; how did something like this form?" And my training and all the classical literature in the field was that bogs are the endpoint of succession from lakes. And this is the Wisconsin idea, the original textbook from Wisconsin limnologists. They saw lakes gradually filling in until they became marshes, and eventually it became a bog. And that was it. So I said, "Well, that's how these things form." Looking out here, this thing is on a slope like this, you see, and I said, "But I don't see how this could have formed as a lake." [laughter] I was having a little trouble with that. And he said, "Well, what lives in this thing?" And I said, "That I can be sure of, because bogs don't have much oxygen, and they're acidic. And that means for absolute certain that if you look in this stuff it'll be full of a little invertebrate that has a lot of hemoglobin, and it's called a midge of a certain kind. I could go at length about all this, because I've read about this."
- Marshall said, "Well, are they there?" I said, "Well, let's look." So I reached down and I grabbed a big handful of this moss and peat and pulled it out and cracked it open, and

instead of seeing what I thought I would see, I saw pure white, clear midges. Now, the only reason this is significant is that I also knew that those forms were the indicator of the exact opposite conditions of a bog, that those forms lived in very, very, very nutrient-poor lakes like Lake Tahoe, or Lake Superior, and places that had lots of oxygen. And so again, I was just totally flummoxed. I said, “Well, that doesn’t fit. I don’t know what’s going on here!” It was like, I wondered why, what was going on with this place and these animals?

Hughes: That was intriguing.

Erman: Yeah, right. So, one of the first things I was asked to do in the School was to write up what’s called an experiment station project; to get funding through the half of your Agricultural Experiment Station (AES) appointment you have to have an approved AES project. So I wrote up this exploration of these peat lands, and wrote up a good thing. At first the administration of the experiment station looked at federal funding, and it went back to Washington for review, and it kept coming back: “This doesn’t have anything to do with either forestry or agriculture; there’s no reason why we should fund this.” Finally the dean of the school just said, “Well, we’ll just make it a state project.” I have lived long enough to see all that come back to bite the agencies; they should have been paying attention to the things like peat lands in forested environments, which are now recognized as very important features for management.

Hughes: That was indicative of the knowledge of the times. And so that is what you did? That was your first Sagehen research project?

Erman: That’s right. And then the second thing that happened was, I think in fall ’70 or ’71, probably, I started my own class, and I did a weekend field trip where we came up to Sagehen and did some electro-fishing and looked around. Just before that Marsh all White had come up fishing in the spring of 1970 with Starker Leopold, and they’d gone fishing on a little tiny tributary that comes down into Sagehen, because Sagehen [Creek] in the spring was running too high to catch fish. They went into this small tributary and started catching fish [snaps fingers, pop! pop! pop! pop!] like that, and they didn’t catch any in the mainstream. They also caught all male trout, all rainbows. Marshall came back and said, “Fish guy, can you tell me what’s going on?” I said, “All males?” It sounds like they would be spawning, but I don’t know why it would be all males.” He said, “They were all about the same size.” So anyway, I got intrigued by this. When I finally got back up there that summer and looked at that stream, it was dry. Again, I thought, what’s going on? That was the other line of research that I got interested in and began studying the fish that migrated up, and when, and why, and so forth.

Hughes: Intermittent streams, I gather, had been pretty much written off in terms of any kind of land management or ecology.

Erman: Absolutely.

Hughes: Was your work a way of calling attention to the importance of these intermittent streams?

Erman: It was, although that wasn't really the driving force; the driving force was, why are they using this stream? And why are they going there? And do they use it? And how many? And how often? And so forth. It was sort of the phenomenon. But because I was in a forestry program, and they had just finished at that time— '69 was the year of the passage of the Z'berg-Nedjedly Act, which was the forestry act for California. And there was an awful lot of argument about the protection of streams. That was fundamentally why forestry became regulated in California; it was because of the impact of logging on water. They had argued to the point where the industry and professionals would accept that certainly the big rivers and the fish—that maybe we needed to take a look at that and protect it. But once you left that connection, it didn't matter, they thought, and there was no way you could convince anybody that a stream that didn't flow all the time was important to anything. That background was there, and I knew about it.

Hughes: Even when you could show that this was a spawning area? How could they dismiss that?

Erman: It took a long time. And I think for a long time it was considered to be an exception, something unusual. You'd think that by now we've crossed that boundary, but it's just moved it a little farther. They will accept that there are such things as intermittent streams which still are important—this is in the timber industry. They don't see that the idea applies to the next level of intermittency, which they call "ephemeral." That means that streams flow only during the spring runoff period. That's something that Nancy has worked on. Her work clearly demonstrates also that life goes on wherever it can, and if there's water around, there are forms that are adapted to be able to [snaps fingers twice] get in and get out, and use and complete a life cycle in whatever stretch of time there is. It's just a fact that most people don't look at those kinds of habitats. Since there wasn't much known about them, it was assumed that they were unimportant. But as soon as you begin to look at them, you find it is important. And in those years when it does flow, it's extremely important, because the life cycles may have to be in abeyance until that combination of events when conditions are ripe that it can flow long enough.

Hughes: What kind of impact did findings like this have, and did it take a while for them to have an impact on larger issues, such as land management and water conservation and all of that?

Erman: It has an impact, and things change for all those reasons. You talk to students, and students take positions; they've heard about the research, and they've been there, and so slowly it creeps into the management agencies. But you never ever are told, "It's because of what you did that we've changed our minds." That never happens. A similar area that I did research--it was prompted by interactions with faculty in the school-- was that they did need to leave buffer near streams. That whether or not you could log down to the stream as long as you didn't drop stuff into the creek and were careful. The timber industry felt you didn't need to leave any vegetation, or if you did, we didn't know how much or whether it really made any difference. One of the first major other research projects I did, other than the work on Sagehen, was to study how much buffer buffers around streams where they've logged. We worked on that for many years. Again, we demonstrated the value of buffers, and it's certainly established in the literature, but I don't think anybody takes it back all the way and says, "Here are the people that actually broke that story."

Hughes: Is that upsetting to you?

Erman: No. It's more interesting to see that policy and understanding changes—

Hughes: Yes, right.

Erman: —and you see it become accepted, and people talk about it, and you think to yourself, well, but you don't know why; you don't know how the connections go.

Hughes: But I know. [laughter]That's interesting. But let's go back to Sagehen proper. When you first arrived, what was there?

Erman: Well, I haven't been up since the latest things. What was there?—you mean in terms of facilities?

Hughes: Yes, and also programs and research.

Erman: The research had sort of gone through a fallow period when we arrived.

Hughes: Now, this is about 1970?

Erman: Yes, it was down to—pretty slim.

Hughes: Why was that, now?

Erman: Well, [sighs] when Needham was there, as the first person, his focus of research was just full of questions of what he wanted to—he had a very clear idea, full of questions. His students, and the students he had, and other students that would be interested all converged there and did a lot of basic work. He had specific ideas of what he wanted to find, and then other students did other things. Once you sort of create a center of energy it brings in other people, so it brought in others, and they began learning things.

After he died—and it brought in wildlife people through Leopold's influence, and Starker spent time up there--that same center of energy just disappeared. And if you don't have that, it just doesn't attract people. The students began drifting away, and those who had done work there went on to other things, and there wasn't any replacement for Needham. Although Leopold went there and sort of had his summer camp there for a couple of weeks each summer, and maybe would teach some in this alternate class, he himself didn't have any students doing projects there. It was more like a retreat. Marshall White was interested, but Marshall had a hard time organizing things and getting going. It was a flaw of his. He really is a scholar, digs into things, but he didn't have that same sort of [ability of] organizing a research project and getting it going. He had some good students, but not very many, and it didn't attract very many people, because he also wasn't that kind of an individual.

So when I arrived there, there just wasn't much going on. What mainly went on was, every other year there would be classes taught in wildlife-fisheries. One year it would be the fisheries-wildlife course and entomology, and in the alternate year it would be a botany class. That's really what went on in the summer, it was teaching those classes and few graduate students. The first year I taught that class with Marshall White was in

'71, and we had six students?, five students? I think one, maybe two were from Berkeley. The next time I taught it was in '73, and we again had five or six students, and I don't think any of them were from Berkeley. I was thinking, if this is a research station, and everything about the station from A to Z is paid for out of a research fund—the whole operation from it was originally for research. But they got these classes going, and it seemed like when I was there, the only thing that was happening was that there were these classes, and they were taking all the efforts of the station manager, the summer helper, here were two faculty from Berkeley teaching every other year, on top of whatever we were doing. When the entomology class, which was always extremely popular, and it was from students in entomology from Davis that were there, it sort of took over everything, and similarly the botany class. But the botany class was starting to decline in interest, and it was headed the same way. When they'd offer the course, it was students from somewhere else; it wasn't even from botany majors at Berkeley. It just seemed to me that Sagehen was losing its reserach focus, sort of.

Hughes: It's such an interactive thing, it's probably hard to separate the two; but you've got the field station and then you've got the academic unit. I realize that the field station is part of the academic unit. Needham was gone, and Starker was getting older, right? Was some of the reason that Sagehen was fading is there weren't advocates right here on the Berkeley campus for the sort of research that Sagehen was best at doing?

Erman: Yes, I think that's part of it. One of the roles I had, and Nancy as well, is we had research projects there. We ourselves had a bunch of things we wanted to get done, and they were research projects. I got a student that started his master's degree up there. When you're there, and you're active, and you talk to people, and you've got interests, it just builds. If you don't have that, it's hard to stimulate it.

Hughes: Yes, I can see that.

Erman: If you have a few people who are there, and they're actively working, it forms the nucleus that becomes a magnet to bring in others, and it grows on itself.

Hughes: In 1978, you become director of Sagehen [1978-1984]. What was behind that?

Erman: Starker retired, and the School of Forestry and Conservation had become a department, and I was the vice-chairman of the department and had just come back from sabbatical. The chairman, Dennis Teegarden, said, "Well, would you serve as the director of Sagehen?" I said, "Yes, I would; there are a lot of things I'd like to see different. I'd like to get some things done." He said, "Great"!

Hughes: What were those things?

Erman: I wanted to improve the facilities; I wanted to get more of a focus that the reason the station was there was for research, and that it should not be a slave to the teaching program. The teaching program was important, it was useful, but Sagehen shouldn't be there just to serve that. The main function for the station, and its value, was as a research facility. It didn't make sense to me that graduate students who might want to do work there couldn't even stay there because all of the living accommodations were taken by summer students. And that everything had to revolve around what the classes

needed for their activities. So, I began slowly changing things, maybe somewhat clumsily, but—

Hughes: Shifting the orientation.

Erman: Yes. The other thing I found was, budgets were never adequate, and when I began examining, what is it that these classes, and the departments that are sponsoring, what do they contribute to Sagehen? It turned out, nothing. Nothing! They didn't pay a dime.

Hughes: You're kidding.

Erman: No! The only thing that the students would pay for was their meals, because somebody had to actually buy the food. That was it. Sagehen, and basically whoever was operating it, had to pick up all of the maintenance that you think about, all of the time of a station manager and his assistant, the utilities, hiring a cook, buying the food. The more I looked into it the more I became so appalled that there was this huge, huge cost that was basically being footed by this very meager research budget, and there was nothing being contributed by classes. They would do things once in a while, like the botany program, they were always fond of pointing out, bought the herbaria cases for Sagehen. Well, for your plants! Really good, you know! A few of those little sorts of things, but there was nothing that really helped defray anything close to their costs. So I instituted a charge. I said, "You've got to pay something." A couple of hundred bucks, at least, to help defray the propane that you're using in the showers.

Hughes: Was that a hard battle?

Erman: Yes.

Hughes: Who was resisting?

Erman: The faculty members of the classes were resisting, and then the chairs of the departments were resisting, and everybody resisted. Just the way they said, "That's what these field stations are for!" I said, "Well, I agree, but we don't have the money to pay for it."

Hughes: This is happening, what, in the late 1970s into the eighties? What's happening to the discipline as a whole? I'm thinking specifically of having new technologies, and particularly the new genetic technologies. Is field biology getting downgraded? Is that some of the problem?

Erman: Not at that time; in fact, it was just the opposite, because that period was a resurgence of natural history/ecology. It was probably the biggest period, both on campus and in interest in field work—

Hughes: Why?

Erman: That was the environmental movement. There's that time lag. The environmental movement of the late sixties, '69, with the passage of the acts [National Environmental Policies Act, the Wilderness Act], and *Silent Spring*, and suddenly kids--

[End Tape 1B]##

[Begin Tape 2A]

Erman: --developed a huge interest in natural history kinds of work and protecting the environment. Certainly some of the growth in that period, both in graduate students and certainly undergraduates was that tremendous interest in learning and studying and trying to do something that would be relevant to protecting the earth and the environment. And a lot of the students that I came in contact with at that time were certainly of that orientation—with all of the problems attendant to it too, the radical spirits, and not wanting to abide by any regulations or rules. It was a tough time, both on campus and off, to try and deal with students.

Hughes: I can imagine.

Erman: And their sort of libertine life styles as well; it was really interesting.

Hughes: On the other hand, the fact that there was demand, that people wanted this kind of education, must have helped your cause.

Erman: Yes, it did. I had also instituted certain accountability of what was actually going on. The field station had just been sitting there; no one looked at it; it just sort of went on, so there wasn't any concept of how many students and researchers it was handling. If you can count the number of students, how many hours, or something, and how many graduate students, what was published, there was not even, if you can imagine, a complete written down list of publications.

Hughes: You're kidding.

Erman: No! There was a library, and there were all the theses, and there were reprints scattered, but you could not say what has Sagehen produced. You couldn't easily find that out.

Hughes: Did you change that?

Erman: Sure. Put together the first list of publications—dealing directly with research at Sagehen—organized that. In fact Nancy then continued that even into the nineties. Made sure that things got added in all the time. There was no accounting, ever, of how many people actually came through the station. To visit, to stay, to work, to collaborate—scientific colleagues came from all over the country. Sagehen, by that time, actually had a fairly widespread reputation in certain research areas, so collectors from all over the world would come through Sagehen to look for something, because they knew, because of earlier publications and other work, that you could find particular species there. There was no accounting for that use, so I began a program, saying to the station manager “at the end of the year, I want you to have kept a log,” which was a tradition, like a journal. So we had it all there, but it just never got an organized written report. So I said, “Compile it. I want to know how many visitor days, if you can call it that, or research days, people from abroad, people from visiting universities, I want that all catalogued.” So we started that, and just over that period that I was director visitor days either doubled or tripled. It was obvious that we could demonstrate both increasing use and a wide range of uses by students and scholars.

The other thing was the number of publications: from the period of time I took over to the period I ended, it tripled. Because again, we started creating some energy; we were doing work there; I was there a lot of the time, spent an awful lot of the summer there; we started holding seminars while people were there—they'd never done that! They never even got together among the researchers, after hours, on weekends, to talk about what they were doing. People if they lived in the same tent cabin got to talk, or if they interacted at dinner, but there was no cross-connection. So no matter what people were working on, there were other people who might not know anything else, so it was completely scattered.

We started, partly out of defense, also having more formal meetings with the Forest Service, because that also is and will always continue to be an issue. They just look at you as leaseholders, and very little obligation. We started, actually with Starker, we said, "We've got to meet with them, and you've got to do it," because he had the clout and stature that he could call that meeting. So we started meeting with the local district ranger. Then when I became director, I got the department chairman to bump this up at the supervisor's level, so it was the whole Tahoe Forest, because the ranger who handles just one region--it's the Truckee Ranger District that we're in--he's pretty autonomous, but we can't really affect his agenda. But if the supervisor of the forest says, "You will do this," he'll do it. So we knew we had to get it bumped up to a higher level, and it would take the chairman at the level of the supervisor of the forest to force that, and they'd both be there.

We started holding those meetings, and we began also getting them to recognize we were more than just a university place sitting on a hundred acres. The reason for that was, they wanted to continue to manage the basin without regard for anything that was going on in the field station, no matter what it was. Whether it was a timber sale, or a fuel wood-cutting program, or recreation for off-road vehicles, anything, they just did it, and we found out about it after it happened. Some of the worst examples were where we'd have students that would have plots out in the forest, and then the next day they'd go out there and their plot would be cut down, or sprayed with an herbicide.

Sometimes these would even be projects that would be totally on an applied problem that would be relevant to the Forest Service and how to manage, and their first reaction would be, "Well, you guys got your lease area, we don't know what you're doing out here, how are we supposed to know"? We said, "Why, if we're just ten feet away, and you're going to do something, why don't you let us know, and then we'd tell you"? So we raised it to the level of having these annual meetings where we'd try to do that. We also expanded the area that was considered to be our "lease area," doubled the size of that, or tripled it, from a hundred acres to three hundred, because it didn't even include the fen.

Hughes: Was that a struggle to get it extended?

Erman: Well, the Forest Service thought, "If we let them expand to that, we're not going to harvest any trees in the fen, and a little bit of this and that, it doesn't make any difference. So keep them happy, they've got their three hundred acres, and they'll not be so disturbed." They agreed to that, but that didn't really solve the conflicts over their basic management, and our—

- Hughes: You essentially were just a thorn in their side, as far as they were concerned.
- Erman: Right, right.
- Hughes: Presumably, at these meetings, one of your points would have been to show the relevance of what you were doing?
- Erman: That's right.
- Hughes: Could they hear that?
- Erman: They seemed to. They were always fascinated when they started hearing about all the things that we were doing, and the things that actually connected to what it was they were supposed to be doing. We often emphasized the fact that even projects that may have started out as seeming irrelevant to them—studying dickey-birds, like they used to say; if it wasn't a game bird, it was a dickey-bird, or something like that; it didn't count—those were things that were going to be important for them in management later. I can't tell you about how many things were done at Sagehen that turned out to be prophetic. Because people were studying things—small birds that fed on insects that were associated with habitats have turned out to become things now the Forest Service has to spend a major amount of their time making sure they don't bother. Some of the early work, in fact, the best of the earliest work on the relationship between snags in the forest was done at Sagehen, by one of Marshall White's students, Marty Rhaphael, who measured snag densities, because the Forest Service had the policy of routinely getting rid of snags, because they were considered a fire hazard and a source of insect infestation. So they were eliminating the habitat for one of the major components for the species of birds that became later the problems that they were having trouble managing. And they never connected the habitat with the organisms.
- Hughes: At these meetings, did you slowly begin to make inroads?
- Erman: We did. And unfortunately, those meetings, after I left, just sort of dwindled and disappeared.
- Hughes: And that's the status now?
- Erman: As far as I know. They may be with the district ranger, I don't know, but they're not at the forest supervisor level.
- Hughes: That seems a mistake.
- Erman: It is. It is.
- Hughes: I remember other people saying that there was also a struggle to keep the cows--or the sheep?--out of the field station?
- Erman: Sheep. That's right.
- Hughes: What was the status there when you became director?

Erman: We learned about the sheep when they came through the station. That was it. And then we'd go in and say, "They're coming through the station," and they'd say, "Oh, yeah, they're not supposed to go through the station; we tell them not to." It was just this casual attitude. So we had to sit down with them, and say, "Okay. When are the sheep coming through, and at the minimum they need to go this way, and around it. Now, will you talk with the herder, so that he knows it, not just the guy who's the owner of the band, but the herder. He's the one that runs the sheep through." We eventually got them also to realize, "Now, the way you're managing this doesn't make any sense ecologically, because you're taking them through all these sensitive areas." And we started talking about the fen, the stream, all these meadow areas, and they tried to slowly get the herder and the leaseholder to try and move their livestock in a different way. Their story was always, "This guy's got such a tough time; it's so hard to make a living, and really you can't do much.." "Well, all we're talking about is the herder who's with them moving over a hundred feet to take the sheep a little different path! But if you didn't do it every year—the next year, if you didn't have that meeting, go over specifically [snaps fingers] next year, the problem was there. There was absolutely no carryover.

Hughes: Was that because people came and left, or was this kind of a convenient forgetting?

Erman: Probably a bit of both. We didn't know how often the herder was the same. They were still using the Basque herders, who spoke very little English. Sometimes we'd see the same guy, who'd be there two or three years in a row; other times it'd be whoever they could get.

Hughes: He probably never got the word, or didn't understand if he did.

Erman: And didn't know whether it was important or not.

Hughes: Exactly. When if ever does the feeling in the department here change about the importance of Sagehen to the academic mission?

Erman: I think it became important in the seventies, even before I became the director, because the School of Forestry always had a high regard for field stations, because they already had, under their umbrella, quite a few. The summer field camp at Meadow Valley, the Blodgett Experimental Forest, they had Whitaker's Forest down in southern California, they had one out in Lafayette. Maybe still do. All the faculty, I think, recognized that field stations were fundamental to this kind of study. But saying it and then actually having people working there is a different thing.

Hughes: Or providing adequate funding, right? Wasn't that a problem as well?

Erman: Sure. Sagehen, it wasn't so bad when I was there; I mean, the budget didn't change much, but at least there was a budget. When I began to start also getting funds by other means—charging users, it was obvious to me. I got grants, and if a student had to go someplace, the grant had to pay for the expenses. If they stayed in a motel, it paid their motel; if they camped out and had to pay overnight [fees]—I mean, even the Forest Service charged five dollars to camp—but we didn't charge anything for the people who stayed at Sagehen. So I said, "We've got to institute something that covers a little portion of what [the cost] is." I don't want to take it to the point where it's so expensive

that it discourages anybody, and if somebody was really in that spot then they should come to me and we'll figure out a way of saying, "Here's a grant." But there had never been instituted any system of charges for anybody using the facilities. It was just open. I had mentioned the summer classes, but what else happened is that in the spring and the fall, classes from Berkeley and Davis would come up with their thirty, forty students, and use the station from Friday night to Sunday afternoon. Eating, using the facilities, taking showers, running the lights, all the whole operation with the station manager's time and energy. They paid nothing.

Hughes: Amazing.

Erman: It's amazing. If you have a box in front where you drop in a dollar in a free museum, you do get a lot of money. Saying, "Well, you have to pay a little bit to use the facilities here," they did a lot of grumbling, but it—

Hughes: Helped.

Erman: It helps. Actually, after a couple of years of doing this and getting it established, we were able to bring the budget up so we had maybe \$5,000 or something like that to be able to do things that we hadn't been able to do before. Buy a new something-or-other, replace this, and do that.

Hughes: You stepped down in 1984.

Erman: Yes.

Hughes: Why was that?

Erman: Well, two reasons. [laughs] One— [pause]

Hughes: I notice a long pause.

Erman: Well, diplomatically, I had become too successful, I think, for some, and that creates jealousies. When that happens, their reaction is to try to limit and interfere and to criticize and do other things rather than just saying, "Hey, it's successful; it works for all of us." So they immediately wanted to—there was a small advisory committee. They wanted to have control over the funding and the budgeting and the managing. I said, "Look, this is a small operation. We're talking about a few thousand bucks. If I'm going to have to deal with a committee to decide on what it is the manager and the director decide should be done, it's really not going to be working." Well, they wanted to argue about everything, whether it was spent well, spent wisely—and I said, "Is there something wrong? Do you think the station is not being managed well? But you're not; you just want to have control." People began to argue--I would hear this second-hand--that we were being given too much, because Nancy and I were working up there all the time, and we would stay up there. We were giving too much favoritism, and this was helping us to the detriment of somebody else. I said, "Anybody else that wants to come up here can stay here and do whatever they want. We're not getting paid; we have no money, this is all on our own support. What's the limitation to your being able to do what you want to do?" There was just the nastiest kind of academic stuff. I had already

become associate dean at the same time, and then this is going on, and—aah! I just said, “Well, it’s time for a change. Seven years, go ahead.” So I said, “You run it; I’m out.”

Hughes: And is that when things really went bad?

Erman: To me they did, because immediately it became run-- The way Starker had run it was, people down at Berkeley who had absolutely nothing to do with what went on there, they’d maybe drive up for a day or a weekend once in a blue moon, had no investment in what went on, had no knowledge of what went on, and just basically became absentee landlords. It doesn’t run that way.

Hughes: Yes, you’ve made that clear, that there has to be a pull, a people pull.

Erman: There’s an intellectual investment as well as an energy investment. When that’s there, things happen. You can affect it in all kinds of ways that are hard to translate. You can create funding; you can create opportunities. Everybody always says, “The department didn’t give me the money.” If that were true, nothing would ever happen on this campus. No department gives somebody money. You cadge it in some way, you figure out, you leverage things because you got something from here, and something from there, and you work a deal, and you get it done. Up until the second or third year that I was the station director, the station had lived with two problems that were terrible for a field station. The first was that we were polluting the stream, because the whole sewage system was inadequate. It had been hand-dug by students, with redwood boxes as the septic-tank holders too close to the stream, and every time more than five people were there, it overflowed and went into the creek..

Hughes: Boy!

Erman: We got away with this. You’d walk by certain paths, and it just—whoa! It would be bubbling to the surface. So periodically a couple of students would be reassigned the job of going to dig out the septic system, and it would take care of it for another season.

The other thing was that since the mid-fifties, there had been no phone at the field station. The first phone was, actually-- They had strung wire across the brush all the way and actually had a phone line in there for about five years, which Needham said was essential for people living in a remote area; you’ve got to have a phone. Well, they had to go out every year and connect up the wires; they’d be broken by snow and everything else. Finally it was just rolled up and taken down, for whatever reason, so there’d been twenty years, probably, no phone. And we had all kinds of things happen, where people would be in near-emergency situations, and they’d have to be loaded up and driven to Truckee. So I talked to Provost George Maslach. I wrote him a letter and said, “We’ve got to have this. It’s terrible. It’s an emergency; we’re liable.” I didn’t ask my chair; I didn’t ask the dean; I just wrote to the provost. I said, “You’ve got campus money for deferred maintenance and things; you’ve got to help us.” He wrote back this letter to the dean, it might have been Bill Waters at the time, “I’ve received this plaintive letter from Dr. Erman; seems like you really need to do something.” Gave us the money [snaps fingers] like that, boom!

Hughes: Wow.

- Erman: I made a squawk that in fact, the University was going to be fined by the water quality boards, because we were polluting a domestic water source. We couldn't continue to do that. They got the money out of the deferred maintenance; we put in a whole new proper septic system. The fish tank diversions were falling apart; the concrete was rotten. I got an angling club, gave them a weekend up there, gave them seminars and field trips. We talked about fish. They got to stay, and hear, and look at the fish tank. They raised the money to fix the fish tank and the diversion structure. The department didn't give us any money. They didn't have any. So it's just doing that.
- Hughes: Yes, but it takes somebody dedicated to do that. You can't do that sitting in Berkeley. After a career of doing research, this is a terrible question, but could you summarize the main lines of your research? I have a sheet here that speaks of three areas—
- Erman: At Sagehen, or everywhere?
- Hughes: Well, the first one is definitely Sagehen, isn't it?
- Erman: There were two areas—
- Hughes: All right, well, start there. We don't have time to go into proper detail, but that's probably in your publications anyway. But just to give people an idea of what you were interested in.
- Erman: One was the study of the full ecology of the fen systems there in Sagehen, and it's known for that. It was even considered as a possibility of a special federal designation at one time, because it was so rich. The Park Service knew about it. The fens are unique; there's a high concentration of them, and I think our work, mine and Nancy's, was thorough enough and complete enough they could see its importance, its value. How they functioned, why they were different—they weren't even bogs, or filled-in lakes.
- Hughes: Has that made a difference in the conservation aspect?
- Erman: Yes. In fact, now if you look at the standard documents that are produced in the Sierra by the national forests, when they talk about management, in their forest plan revisions and so forth, they specifically mention fens, springs, seeps, and other special aquatic habitats.
- Hughes: Do they? Good.
- Erman: It's never been recognized, but it is mostly because of the work of Nancy and myself. Entirely.
- Hughes: So that's a big achievement right there.
- Erman: It's mentioned always, now, and it's throughout—they don't even question that these are important unique features that were never even on a map in a forest planning exercise.
- Hughes: That must make you feel good.

Erman: Yes! It does. We still don't think they do too well at protecting them, but at least they say they must consider them in planning. The other area was on intermittent streams, and the use by fish and other organisms in the intermittent streams. Those two things, the fens and the intermittent streams, also then sort of connected with Nancy's spring work, because she worked on the aquatic insects, particularly the caddisflies in the springs. The springs are often the sources for the fens themselves. Some of the fen springs are streams themselves, and are intermittent and temporary types of habitats. While I was doing one aspect on the fens, she started looking at these others, and they sort of connected, so there are the physical dimensions of it, the biological, ecological, and the broader picture came out.

The third area then, which was mostly hers, was the description and ecological characterization of the spring habitats of Sagehen and also some other connected areas, to show that actually what we saw there was unique, in the sense that springs and related habitats had these certain kinds of forms, and what they lived in and what they required for life. That's branched out now into some other work that involves some researchers from Santa Barbara on the use of water aging, how to date water. We got Professor Jordan Clark and his students involved because he had the technology and the expertise, so now we can actually say, of those springs at Sagehen, how long it takes before that water emerges. It fit in to a whole theory, a hypothesis that Nancy developed, that the richness of insect life in those springs was related to not only the physical environment and the features of the habitat, but also how persistent and stable the springs were--stable both in terms of how they fluctuated in volume, but also whether they persisted as a flowing system through a long period of time. It turned out there was a very close correlation to how long the water was accumulated to give it, of course, a greater persistence and stability through time. We developed the idea through a whole different set of properties, biological and some physio-chemical characteristics, and in her work on the species of caddis flies, and in the next study we tested the hypothesis with actually aging water of springs. It was very nice; just the way it's supposed to work—the more persistent and stable springs had more species and these had the oldest water.

My other major area of research was on the effects of forestry activities on streams, first with buffers, but then on other sorts of things related to that. That's sort of been ongoing. Part of the fisheries stuff at Sagehen was also just—I picked up on the long-term study of the fish populations, which was ten years before I got there, and I would come back and resample, and I have done that also; the last time was in '98.

Hughes: Comment, please, on the virtue of having a location where long-term research has been going on.

Erman: As you probably know, I was also the science team leader for the Sierra Nevada Ecosystem Project (SNEP), and the first thing we were asked by Congress to do was to not do new studies, but to assess the status of the Sierra. When you start saying "assess," what you immediately start saying is, "What have things been like in the past; what are they like now, and where do they tend to be going"? To do that you have to have somewhere where you can get those pictures of what conditions have been like, what the interactions have been, and where do we have sources of information about how things function and relate in the Sierra. You're hard-pressed to put your finger on places where you have that kind of assembly of information for any length of time.

Sagehen turns out to be the longest-running place where you have continuous observation and research of anything in the Sierra. Even though you have much longer histories for established reserves and parks and so forth, very often those aren't examined; they're just set aside. So even in places like Yosemite, there isn't as much known about what's going on there as the last fifty years at Sagehen.

Hughes: Is this widely recognized in the field? So that data that's been collected over all these years at Sagehen is widely used by people who maybe never have seen the place?

Erman: I think it is by scholars. Like in any field, the scholars know about these sorts of things. It tends not to have, perhaps, a wider area of knowledge. That's a problem with not tooting your own horn, so people don't know about it in a broader environment. But it is known for its long history of research, and it is known, and that's why I mentioned that people from all over the country and the world come by Sagehen for certain kinds of things, because they know of previous work, and there's this history, and they can find things there. So it is known to that extent.

The other important thing, though, that I think about any field station, particularly one that is always going, is that—I think there is an unfortunate trend in any kind of research now, but more so I think it shows up in field work, where, because we can get back and forth a little easier, people tend to define a project, define what they need to get in the way of their collections and their samples, and then they go up there, get them—so that you can do an awful lot of research where you lay out your plots, and you go up there and take your measurements—and you're gone. That takes away, to me, one of the most important things in any observational field: the chance observation of something happening that you didn't go out to observe, but that did happen, that changes your mind about something. The advantage of a field station is that you can, at least the opportunity is there, you can stay there for a period of time and see things, rather than just run out to a forest and you're staying at a motel in town, and you've got to grab your samples and then get home. The field station provides you a live-in, work-in environment. You're there.

[EndTape 2 Side 1] ##

[Begin Tape 2 Side 2]

Nancy, again, I can use her examples, is one of the best. She made major discoveries of things that were under everybody's nose and had never been observed, just because they hadn't thought to look at that time or place. She was working on the fens and the springs and so forth, and caddis flies, and for some reason wondered what it would be like to go out there and look at night. We'd done night work and winter work, but she hadn't gone out there to look specifically at her area. When she walked out to the stream, she found these insects that she knew were living in the stream but they were crawling out of the water up on the stems of the plants. They were becoming terrestrial, in other words, breathing and living out of the water. That terrestrial part was sort of known, but the extent of it, and the depth of what was going on, had never been observed. It was a totally new observation. And it was done because of curiosity, but also she was there. It was the time in their life cycle when she walked out she could see it. If it had just been go out and get your samples, well, it would have been the eight-to-five, you go out there

and you take your stuff, you put it in your bottle, you go home. You'd never have made that observation. Well, think of how many years, how many entomology classes, how many faculty, have gone out there, walked through the same area that she did—it was right there; it was in the field station itself—had never ever seen that. That's what serendipity is. And you can't underestimate or undervalue that. You could say, Marshall and Starker's occasion, they'd gone up there and stayed to go fishing, and they chanced to see those fish using that little intermittent stream when it was flowing in the spring. That stream wasn't even on the map the Forest Service used for managing the basin.

Hughes: I would like more explanation of the material that Jerry Booth gave me. One of them was that apparently you found and presumably still find a problem with the old pump and drain system.

Erman: That system was developed by Needham, and he transferred it from his work where he was a biologist with a federal agency at Convict Creek.. He'd used that there, but he also had learned there, and I don't know why he continued to persist with it, that when you diverted a section of stream, and dried up one section, it was pretty hard on everything that was living in—

Hughes: [laughs] Yes, I would think so.

Erman: —the section. In fact, there's even a publication of his, and I always forget about it, that he was observing, when they would do bottom samples of stream insects, in succeeding years in a section that had been drained, the populations in the pumped sections were going down in abundance. They were starting to lose some aquatic life in that section. So he knew it was pretty hard on things. But when he went to Sagehen that's—

Hughes: Still what he did.

Erman: Yeah, because he thought at that time, that was the best way to get a count. It was the closest thing you could get to a total census. I don't think it's necessary to do that. In fact, we even argue about it in counting people in the United States, that it would be actually much better if we did a very thorough statistically stratified sample to get the population than it would to actually try and count everybody. It's an awful lot of work for what you get back. The same is true of sampling most populations. You don't need a census. Then there's the other side; it's pretty rough on the environment when you just dry up a section of stream.

The second thing is whether or not you should always go to the same section to sample fish. You can argue that pro and con. If you always go back to the same section you have repeatability through time, but the habitat doesn't remain the same. While you're saying, "I'm in the same section," floods, droughts, trees falling, the section is changing all the time. What you have to hope is that you've got enough of these places stretched through the place that it's still nevertheless representative of the overall pattern. One of the biggest things I began to observe was the downstream sections, below Highway 89, down now near the reservoir, one of the sections is under the lake, and one of them is sometimes under the lake, section nine, and eight is always above the lake. But then, those two sections down there I've documented through sampling that it's had huge, huge changes in the fish populations since the reservoir built in 1969. A student, Lynn

Decker, said, “I think I’d like to find out what the populations are like not just when you go down there in the traditional time, which is late August or early September, but what it’s like all season.” What she found, by using skin-diving or snorkeling underwater, was that a lot of the fish were there in mid-July, and that by the time the August-September period came along, a lot of the fish had moved out into the reservoir. So what we were finding in this very absent population was now maybe not so much a change in the fish not existing, but that they changed their life cycles. Because now there’s a lake-stream interaction that wasn’t there in the past few thousand years.

Hughes: And that hadn’t been appreciated before.

Erman: No, because there wasn’t a lake there, so we didn’t think about it. But if you think about the species of native fish, that’s not at all unusual, and I won’t go into the reasons why that’s so.

Hughes: You spoke earlier about the old disconnect between the physical and the biological conditions. Do you think at least partially through your work that people look in a more unified fashion?

Erman: I’d say yes, at least to one extent. I don’t know about the educational process, but I think from the research side I think yes. There is another series of studies we did, when a colleague of mine, Dr. Edmund (Ned) Andrews, who was a Berkeley grad here. He’s with the US Geological Survey now, and he studies sediment movement. We worked together at Sagehen, because we just talked about—he was Luna Leopold’s student, who was in geology. He talked about the physical flow parts of the stream; I talked about the ecological, and we had lots of good discussions. I was interested in this whole idea of the frequency of movement of rocks, as he was, on the bottom, under what flow conditions, and how that would affect the stream life. Because it seemed to me if there was some frequency of an event that this ought to play back into the whole ecology.

I still have questions I never got around to answering, but the one thing we did do, was we got the station manager, when we weren’t physically there, to—we built a bridge across the stream, and he went out over that bridge, and when the water began to rise in the winter or the spring from the snowmelt, he collected samples across this in a sort of basket, and the proper way to get the bed movement. I sampled the fish population the year after that, and it turned out it was a big El Niño year, so we had a big flood in winter and spring and a lot of rock movement. What I documented in the fish sampling was another event that caused a tremendous reduction in certain species of fish. In particular, one of the species of fish hadn’t been appreciated that it was affected was the sculpin that lives on the bottom. Of course, it’s going to get tumbled when all the rocks get tumbled, and its populations were decimated that next year. (We knew what they were like before, because Needham’s pump and drain sections sampled them, too.) And it turned out even when the station manager was doing the rock sampling, with this basket, he caught a few sculpins being carried by the rocks in the stream into the net.

So that observation, plus the fish sampling, plus we knew that the rocks moved, and then we realized that the events that set that up are not the normal spring runoff events, but it’s in fact the winter flood events that occur when there’s snow on the ground and then there’s rain. The snow along the stream actually acts as a wall, so that the water level instead of spilling over the bank rises up, and it’s like the weight of the water that

really is the energy. Ordinarily Sagehen Creek would get there at flood stage and then flow out over the banks in the spring, but in the winter flow just keeps going higher and then you have this tremendous erosive power. When we went back and reconstructed the data, all of a sudden the whole pattern—because we had, that’s the other thing, information on the flow of Sagehen and the weather of Sagehen and the snowfall at Sagehen back to 1954. So we could reconstruct it, and it fit perfectly. That was another thing we did, through my contact with Ned and the fact that we had so much biology going on. Sagehen is a benchmark station for the US Geological Survey that Ned and I arranged, which means that USGS will cut other places they don’t have money for to collect water data, but not Sagehen.

Hughes: Good.

Erman: Vital importance. Anybody who is looking at what’s going on in a stream, you’ve got to know what the flow conditions are. It’s amazing how many places, and how many studies, are still done—they don’t know what the discharge is, or what it was last week or last summer, last winter.

Hughes: You’ve been in this field for a long time. What kind of changes have you seen? Not only in the philosophical, intellectual, conceptual, but I’m also thinking, there have been some technological revolutions.

Erman: These are the kinds of questions I used to love to ask at PhD oral exams. Ask somebody to think outside of just what they’d collected, like, “What’s been the big change in your field, and what’s happened”? Now I have trouble answering the same questions.

Hughes: Now you have to do it. [laughs]

Erman: Well, the DNA, genetic side of it has certainly been one, because it answers questions that you wondered about and had to infer from less precise means.

Hughes: Like evolutionary questions.

Erman: Evolutionary connections, rates of evolution, whether things are really as distinctive as they seem, and how distinctive and what they’re connected to. That has certainly changed the field, and an awful lot of work in both fisheries and other fields look at that. What it points out, of course, is that you have sort of these built in feedbacks for things to develop, characteristics for where they live, even though it might be the same species. It’s a song that field biologists have sung for a long time, which is, just because this organism lives someplace else, you have to remember that it’s attuned to what’s here now at this place, so when you eliminate it here, or you change things here, you’re having a bigger impact than just saying, “Well, the species is still extant.” So there’s that connection there. I think that’s helped bring that home to others who would have said, as long as a representative of the thing is somewhere, that’s enough.

We go back and forth on whether we understand ecosystems, and whether we appreciate those. I think it’s still difficult to deal with as a concept, because there’s no way of easily saying how the system itself is doing. So while we go through some periods where there seems to be a burst of energy and enthusiasm and sort of this recognition, it quickly falls back to, I think, an easier mode, which is to study things in

isolation. That will probably always bedevil the field, so to speak. It's easier not to think of something connected to a bigger picture.

Hughes: Well, it's easier to study in isolation, isn't it?

Erman: From my experience in working on the Sierra Nevada Ecosystem Project, it was one of the most disappointing aspects of that, where here's this relatively large team of scientists, and how little they could, or would, communicate with one another. They were extremely deep in the area where they had expertise, but if it wandered beyond that, they either did not want to ask a question, to appear perhaps ignorant, or couldn't. And I felt at the end that it was probably more a reflection of the academic mindset, which is that you are the big cheese, and you never can show weakness or ignorance of something, so you just don't venture out to where you could be exposed as not being the all-knowing. That's a rather cynical view, but I really didn't know why else some people—there was no threat; I mean, there were no class grades or anything like that, and often good questions count more than a good answer. And yet, there were only a few people who, in that large group, would constantly ask, "Well, this is a dumb question, but explain this to me."

Hughes: That's one of the real drawbacks of academia, isn't it?

Erman: It is. And I do think it is a problem with what we see as our models, and probably the institutions--the bigger they are, the more that tends to be—the god who knows it all.

Hughes: Do you think because you did see things in a holistic and interactive way, that you were set apart from the mainstream of the school?

Erman: I don't know. Set apart in the way my colleagues treated me?

Hughes: There's that Erman out there; we don't quite know what he's doing, and in essence we don't care?

Erman: I don't think I was ever that far out. But you don't hear about things like that. In fact, since I've retired, things finally get back to you, and some of the things that I've heard have been so hurtful—people that I had no idea had that attitude. They come back through secondary sources and so forth, and you think, why did they have that opinion? I never knew it. I mean, I never sensed it while we were talking. Even though we might have disagreed on things, I thought, well, that's what you do: you talk about things, you disagree, you agree, but you try and hammer it out. But it went deeper than that. In fact, I found out subsequently, particularly after you leave a place, maybe that some of that was there, and I don't know if that was my style or my substance. Probably both.

Hughes: What have we missed?

Erman: [laughs] I don't know about the future for Sagehen. It seems like putting it inside the [natural] reserve system is probably its best security. That was fought all the time when the reserve system first came up. The department, and the college, and people who were where I was, deathly wanted to avoid that.

Hughes: Why?

Erman: They thought it would take away authority, which it would have, to a certain extent, and they wanted to have that control. But I never understood why they particularly wanted the control, because they really didn't do anything with it—sort of used it as a hunting and fishing camp. There was a lot of that--old guys coming back and going fishing, staying at the camp because it was a neat place. What were they doing there? Well, friends.

Hughes: What tipped it over, so that it did become part of the reserve system?

Erman: The biggest thing was that in the early nineties, economic crunch hit, and departments and colleges said, "You've got to come up with a 15 percent budget cut," or whatever it was, suddenly found they didn't have much to cut. I think the College of Natural Resources in particular had used some money to buffer some problems and had accumulated some large debt, so it might have been larger there. And the experiment station in particular was hit harder, because when the governor says he has to cut state-funded research, what he means by that is almost exclusively the Agricultural Experiment Station. The rest of the cut was state-funded research. That's only the experiment station, almost. So, you start talking about a 5 percent cut in the state-funded research, you have to realize that's focused down to a much smaller piece of the campus, so it was huge, huge crunches. Well, Sagehen, I believe, at that time, had become this sort of absentee thing up there. Nobody was involved; it was just drifting along; people in that department, I understand, just looked and said, "Hey, look! There's a position, there's a station manager, there's operational funds, thirty, forty thousand, fifty thousand a year, or whatever their budget was." I think when I left it was like \$77,000 in hard cash that we were able to use, and more that we were raising. I think they just looked at that and said, "Let's liquidate. Use some of that to pay off the bills, and we gather in an FTE." You always want an FTE. "We can add a new faculty member, or an RA [research assistant] for us, or whatever." So they decided that there wasn't anybody interested, it belonged to us; we could do with it what we want to, and let's do that. A few of us said, "No, you won't."

Hughes: [laughs]

Erman: "No, you won't. It's bigger than that." It wasn't set up by the department; it was set up out of the legislature, as part of the wildlife-fisheries program, and the budget for that was appropriated by the legislature, so it wasn't up to a few faculty members in the department just to say, "We'll take that and use it for our own purposes."

Hughes: Did you have to do more than point that out?

Erman: Yeah. I had to work from every angle, politically and otherwise. I'm sure that engendered a lot of ill-feeling on one side. But one of the things that they, without saying names, but people decided that the station was going to be taken and given back to the Forest Service and done away with, and so they went up there and just raided it. I don't know if you heard that part of the story.

Hughes: No, I didn't.

Erman: A couple of faculty members in the Department of Forestry who were in wildlife just went up there and took everything that wasn't nailed down. Said it belonged to the

department and wildlife/fisheries, and it wasn't property that belonged to Sagehen. They took beds; they took every microscope that was up there, had been up there since it was part of the department of zoology. They cleaned out everything that they could get their hands on, and just took it out.

Hughes: And put it in the department?

Erman: Oh, it just went into storage. It wasn't a question of whether or not they needed it or wanted it; they didn't even know it was there, it was just—

Hughes: So it was vindictive.

Erman: It was just lifting your leg, you know, and saying, "It's ours." Part of the thing that also brought reality back was, Sagehen has a lease by the Forest Service, and the Forest Service says, "Fine, you want to end your lease, great! Revert the site to its natural state!" So, \$500,000 later to take down those buildings and undo the whole thing, I think they thought maybe that wasn't going to be a cost saving. But that wasn't something the department was going to worry about, or those individuals. "Well, let the chancellor worry about it." But it did marshal some support for it also [from] the community.

Which is another thing that I started. Sagehen was kind of isolated, and yet it wasn't, and so here we are at Truckee; people in Truckee don't even know what goes on. There were all these rumors, and anytime you get kids running around in university cars, then you get rumors about, you know, what's going on; they're wasting our money. So I thought, let's have a field day for the citizens. One weekend of the summer, we'll have open house for Truckee. We advertised, and we told people, and anybody that wanted to come out; we had a small charge which we used to buy food and put on a big lunch and a barbecue, and took them out and just treated them like students. We went in the stream, we collected, we sampled, they went on bird hikes, we did everything, down in the fish tank, and it was just great. Did those at least the last two years, three years I was there, and then I think that's another thing that quickly went away.

Hughes: But it must have really fostered community support.

Erman: It did! And people remembered that, and people would say, "Man, that's a really neat spot; they're doing all these neat things, and this stuff goes on, and they've learned so much," and they become boosters.

Hughes: Well, let's hope that one effect of doing this oral history will be to put some of these ideas back in people's minds, and maybe they'll be re-instituted. That seems like such a relatively easy thing to do one weekend a year.

Erman: Sure, sure. There's a tremendous hunger in the population anyway for those kinds of activities. I was thinking long ahead that probably this thing would snowball, and we would have actually to think about how we could regulate it, and whether we could do it all in one weekend. Because when it was just volunteer graduate students and faculty, a few of us would take everybody out, and when it was a couple of dozen, it was easy enough to do. But I was thinking, they're going to tell their friends, and then their families are going to come up here, and this is going to be kind of like an event. I

thought, that'd be kind of nice, but we ought to think about how we want to do that. It turned out also, when the Sagehen breakup was contemplated, I was having some political, off-the-table discussions with some people, because I was on the California Biodiversity Council for the University, which was, I don't know if you know about it, or—

Hughes: I saw it on your CV.

Erman: You talk to people, so you make connections. It turned out that one of the people who was a representative from the local counties happened to be on the board of supervisors for Nevada County. He, Chauncey Poston, was very interested in Truckee and had heard about Sagehen, so when this happened, I called him. I said, "We're going to need some support. We're going to need support from the community that says that this is not just up to the University, a department, to eliminate. It's bigger than that." He said, "Well, it is." So he got on the phone; he called people in Truckee and Berkeley; next thing you know there are letters that are landing in the chancellor's office and in the president's office, "What the hell are you doing"? Then they said, "Who's responsible"? and quickly send that to a dean, who sends it to a chair, who says, "Oh, my god"! It was set up under Governor Wilson, and it was his resources secretary, Doug Wheeler, that said, "We ought to confer among all of ourselves, state, federal, and otherwise, about managing things," and in fact they signed a document, which was a memorandum of understanding for the necessity to preserve biodiversity in California. The University of California is a signatory. The Dean of the College of Natural Resources signed it for the vice president. So we are a signatory to this MOU [memorandum of understanding], but then they sort of [laughs] "We don't want anything to do with that." So I said, "I'll be glad to go." I was director of the wildlands center [UC Wildlands Resources Center], and director of the [UC] Centers for Water and Wildland Resources, and I said, "This is something important, god, the University has to be involved!" Why would you want all of the management agencies and everybody else talking about the preservation of biodiversity and not have the University involved? Not just a member and signatory, but *involved*. Talking about that for the state. They said, "Well, if you want to do it, go do it." So I did.

Hughes: [laughs] Snowballed.

Erman: That's right. I don't know if you want to put that in there in detail.

Hughes: No, I think it's good to have. Let's stop with one last question, unless there's something that you want to say.

Erman: No, I've talked enough.

Hughes: If you could give advice about the future direction of Sagehen to an anonymous future director, what points would you make?

Erman: I think there's really only one, and that is that a place like Sagehen, from the research perspective, lives and dies on whether or not there's some vitality in who's doing work up there, and it can't have vitality in absentia. It can never operate with a station manager who tries to stimulate but who's not a researcher; he doesn't have the students. No matter how powerful and energetic and good at organizing things, that won't work

in the final analysis. It's got to be a faculty member who has interest in that place and what goes on there. The direction it takes, and so forth. I could never predict what direction that might be. Take it in whatever direction you can, in wherever your interest goes, but there's some energy, and interest, and studying things that are coming out of the station, in that place. It draws in all the other stuff. You can't control it; you hear about it and there's connection, and suddenly there it is. There aren't the same problems attendant to managing the educational component of classes, field trips, schools visiting, because we're used to that. It's the park, it's this place and that place, and we're used to that kind of a setup; it's drop in, drop out; it's nothing really you have to think about. But I don't think that adds to what a field station and research can add. It never can. They're not there for that purpose. Finding the balance is going to be tricky, because until you get faculty here—one or two, and it doesn't take very many, who have that kind of interest and energy--it's going to fall by default to saying, well, then it's classes and drop-ins. So I hope some people can capture some enthusiasm there, and then stay there for periods of time, bring students, and it grows from there. That's my only advice.

Hughes: Well said. And I thank you.

[End Tape 2A] ##

[End of Interview]

SALLY SMITH HUGHES

Sally Hughes is an historian of science whose research interest is the history of recent biological science and its commercialization. She began work at the Bancroft Library in 1978 and has conducted over 150 in-depth oral histories, with subjects ranging from the AIDS epidemic to medical physics to virology. Her main focus for over a decade has been on the biotechnology industry in California and its interrelationships with basic science at Stanford, the University of California, San Francisco, and the University of California, Berkeley. She is the author of *The Virus: A History of the Concept* and an article in *Isis*, "Making Dollars Out of DNA: The First Major Patent in Biotechnology and the Commercialization of Molecular Biology, 1974-1980."

