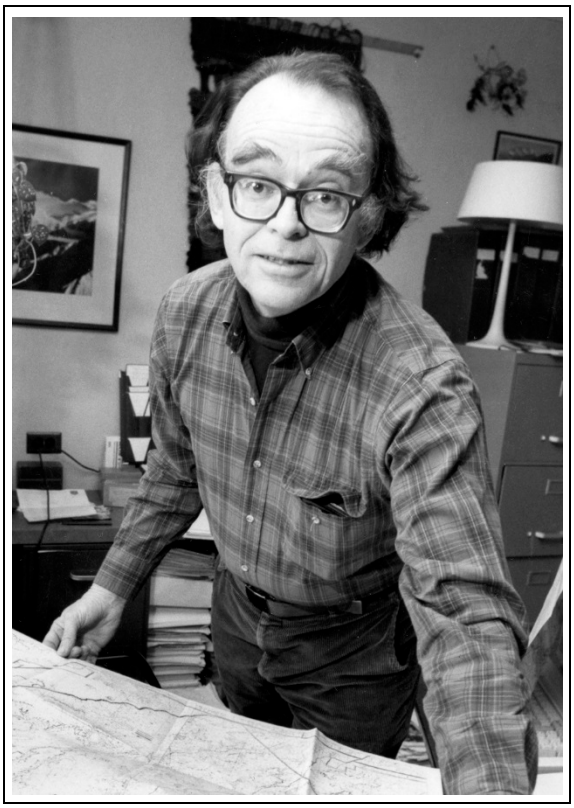

Thayer Scudder, 1987

THAYER SCUDDER
(b. 1930)

INTERVIEWED BY
SHIRLEY K. COHEN

December 19 & 27, 2000,
January 3 & February 20, 2001

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Anthropology, Africa

Abstract

An interview in four sessions, in December 2000, and January and February 2001, with Thayer (Ted) Scudder, professor of anthropology, emeritus, in the Division of the Humanities and Social Sciences. Dr. Scudder received his BA at Harvard College (1952) and his PhD at Harvard University (1960). He joined the Caltech faculty as an assistant professor in 1964, received tenure in 1966, and became a full professor in 1969.

In this wide-ranging interview, he recalls his upbringing as a "college brat" in Swarthmore, where his father was a professor of English; his education at The Fenn School and Phillips Exeter; his early interest in bird watching and mountaineering, and his eventual turn, in graduate school, to anthropology. He

discusses the fieldwork in the Middle Zambezi Valley with Elizabeth Colson among the Gwembe Tonga that led to his dissertation, and his subsequent studies on hydro-politics and the impact of resettlement of indigenous people to make way for huge dam projects, such as the Kariba Dam and the Aswan High Dam.

He recalls Caltech in the mid-sixties, his first friendships there, and the advent of the social sciences program. He describes the Scudder theory of successful resettlement and its four stages: pre-dam planning; adaptation to new habitat accompanied by physiological, psychological, and sociocultural stress; community formation and economic development; and handing over to the succeeding generation. He discusses the impact of dams on people living downstream; his work on transboundary flood regimes; his appointment to the World Commission on Dams in 1998; and the findings in their November 2000 report and the various reactions to it.

Administrative information

Access

The interview is unrestricted.

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ORAL HISTORY PROJECT

INTERVIEW WITH THAYER SCUDDER

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Thayer Scudder **by Shirley K. Cohen**

Pasadena, California

Session 1	**December 19, 2000**
Session 2	**December 27, 2000**
Session 3	**January 3, 2001**
Session 4	**February 20, 2001**

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COHEN: Tell me something about your family—your mother, your father, where they were from, what they did.

SCUDDER: Yes. My father was born in 1900. His father was a very interesting but very autocratic man; he came from a very old Long Island family that was Republican in those days. My paternal grandfather received a lot of his education in Europe—in Switzerland, in particular. When he came back to the United States, he switched to being a Democrat, and the family has been Democratic or liberal—sometimes radical liberal—ever since then. He subsequently became a member of Congress.

COHEN: This is your grandfather?

SCUDDER: Yes. My grandfather was voted into Congress by both Republicans and Democrats from Long Island, which was a fairly conservative area. He then became a Supreme Court justice in the state of New York. He had an amazing ability to marry very wealthy millionaire women. The first was a Welsh woman. She came over to the United States with her parents. Her father became a brewer in the Midwest and made a lot of money in brewing. So my grandfather married her, and they had a *huge* estate in Long Island, which of course I don't

remember, because it was before I was born. There's a place called Scudder Lane on Long Island, while Scudder, Stevens & Clark is a large investment firm. That's the Republican side of the family. So then, when she died early of breast cancer—this is my grandmother, my father's mother—he married an heiress of one of the large New York retail shops. She, again, was a millionairess. They bought a huge estate in Greenwich, Connecticut. You can see where they had their own chapel. Although my wife, Molly, says that he only had something like fifteen to twenty cocker spaniels, my recollection as a child is that he bred over a hundred cocker spaniels. My memories of my grandfather would be of him walking around this *huge* estate with a stream of cocker spaniels—a hundred or so cocker spaniels. [Laughter] They would be barking and yapping at his heels. Of course that brought him into difficulty with the neighbors, because they would serenade at night. A hundred cocker spaniels—you can imagine what they sound like when they go off. So he was sued by his neighbors, but, of course, being both a lawyer and a justice of the state of New York, he was always able to win his various cases.

Anyhow, that's the environment in which my father [Townsend Scudder] was brought up. My grandfather wanted him, of course, to go into business or law or politics, or something of that nature. My father was a very gentle person; what he really wanted to do was to be a naturalist or even a forester. He was enamored, for example, of James Audubon. This is important from the point of view of my own history, because my love of camping—birds in particular, nature, and so on—definitely came from my father. He did not resist his father but in his own sort of very gentle way was his own boss. He got his PhD in English. When I was born, in 1930, he was either an instructor or an assistant professor of English at Yale University.

COHEN: So you were born in New Haven?

SCUDDER: Yes. He did not get tenure at Yale but did get tenure at Swarthmore. He moved up the academic ladder there, became perhaps the most respected teacher. He got all the various teaching awards at Swarthmore. But then, in his mid-forties, he got tired of teaching, left Swarthmore on leave, and set up a foundation called the Center for Information on America. He was an optimistic optimist. He felt that if you educated the American public—educated civic society—they would always make the right decisions. That's the kind of optimism he had. His Center for Information on America started off as an organization to prepare reading-rack

materials for labor unions and laborers working in factories, and it took off. It was quite successful. Then its publications went into secondary schools and universities. He called it *Vital Issues*.

COHEN: Now, I'm deducing that he didn't worry about earning a living because of all the money that the—

SCUDDER: No. Grandpa went through the money of both of these two millionairesses that he had married, and he left virtually nothing to any family members that I'm aware of. So, no. Actually, Pa lived almost entirely on his salary as a professor. Then, when he was working for the Center for Information on America he got the family into a lot of debt, which I had to take care of later on after his death. The center was very successful, but it never became financially self-sufficient. The tragedy was that when he was in his eighties—still trying to get that endowment from the Ford Foundation or the Rockefeller Foundation, and still full of optimism that everything would work out—his board finally contacted me and said, "Look, he's senile. We have to close the center down." Shortly after that, he tried to commit suicide. He wasn't successful in that. Then he had very serious dementia for about the next three years and died.

COHEN: So this was relatively recent.

SCUDDER: Oh, yes. He lived until age eighty-eight. He died in 1988; I was in Ethiopia at the time. But there's no question that although I respected my grandpa, my father is the one who had the greatest influence on me. As I said, he was an incredibly gentle man. The biggest tragedy in his life was, of course, the closing down of the Center for Information on America, which had become an obsession with him. There's no question about that. It definitely colored his judgment, so that everything else was neglected.

As far as my mother is concerned, not too many women, as you know, went to a university at that time. She came from a genteel, relatively wealthy family in New York. She met my father when she was in a girls' boarding school in the New York area. They got married when she was something like eighteen, nineteen, or twenty. I think Pa had either just graduated from Yale or was close to graduating. She did not go to the university; she did not have much education. Her father, whom I think perhaps I take after in some respects, may be the only

American ever to be exiled from the United States to Australia. That was, I think, when my Ma was only about four years old, and she never saw her father again after that. She had a sister. The story is kind of interesting, and I have to mention this simply because in-search-of-grandpa will show up in books that I will finally write.

COHEN: OK. Go ahead.

SCUDDER: His father's name was Boody, of French origin, and he [my great grandfather] was the mayor of Brooklyn—obviously a very upright sort of Brooklyn family. My grandfather clearly was the black sheep of the family. I've never been able to find out exactly what he did, except that when he married my grandmother—that is, my mother's mother—they did a lot of entertaining. He was a charming person who had lots and lots of friends who, as they would say, did him wrong—probably got into gambling, horse racing, things of this nature. He was continually in debt and had a very hard time paying off his debts. In fact, the family had to come and pay off the debts. So they sent him out to run a gold mine in Colorado, and apparently that was the happiest time in my grandmother's life.

COHEN: [Laughter] When he was gone?

SCUDDER: No, she was out there with him. She was the only woman. The gold mine was back in the foothills of the Rockies. But apparently, again, that didn't work too well. So then he was sent out to Oregon to run some kind of family business out there, and, again, that didn't work. My grandmother was with him. The family finally decided that they just had to ship him off—get rid of him, in effect. So they put him on a boat. His brothers had all gone into finance and things of that nature. They put him on a boat in San Francisco with a one-way ticket to Sydney.

COHEN: [Laughter] It was a family-imposed—

SCUDDER: It was a family-imposed exile. He arrived in Sydney with no money, dependent on the Salvation Army. Apparently he went on to have a very interesting career in Australia. My mother has a huge box of letters that he wrote. He was trying, of course, to get his wife and two children to come and join him in Australia. What happened to my grandmother is that her father-

in-law and mother-in-law told her, “Look. We’ll support you and your children, since we’ve sent our son away. But if you ever go over to him and it doesn’t work out, don’t think about getting further assistance from us.” So my grandmother—I think she ran a teashop and things of that nature—it must have been a pretty sad life for her, because she remained unmarried. They were never divorced. I think she loved him very much. I think she wanted to join him. But again, being born and brought up in New York in that kind of Victorian environment, she was very worried about what would happen to her and her two little girls—my mother and her older sister—if they moved out to Australia.

Of course, the usual myths arose in the family. He was an engineer and took his degree at Stevens Institute of Technology, so these ventures in gold mining were all related to his profession. We were told that he was the engineer who built the Sydney Bridge—you know, the famous Sydney Harbor Bridge. When Molly and I went over to Australia, we immediately went to the Sydney Bridge and we went up into the Sydney Bridge’s archives, which is way up top. And no, he wasn’t the chief engineer. No, he wasn’t one of the engineers. We kept going down, down, down the list, and finally we got to the list of riveters. No, he wasn’t on the list of riveters. So what he actually did, after his dependency on the Salvation Army on arrival, we don’t really know. Hopefully it’s going to be in these letters, but they are virtually unreadable; his handwriting is such a terrible scrawl. We do know that he joined the Australian Army during World War I, was going to be traveling to Europe by way of New York, and was planning to meet his wife and children at that time. Then the war ended, so he never left Australia. Molly and I did find the house that he lived in, but unfortunately the people there were people who had purchased it from somebody he had sold it to, so they knew nothing about him. I finally got hold of his will. It was really quite tragic, because it said, “If my housekeeper stays with me until I die, she’ll get £100; my 1926 set of *Encyclopedia Britannica* will go to my friend so-and-so, and my desk to so-and-so, etc. If anything’s left, half goes to the Salvation Army, which took care of me, and half goes to Stevens Institute of Technology.” I haven’t yet contacted the Stevens Institute of Technology to see whether or not they got anything. I doubt it. I’d just as soon not find out, but I will one of these days.

So I think that takes care of the family. As I say, my mother’s childhood obviously was affected by not having her father. Having her father desert the family [when she was] four perhaps was the reason why she got married at the age of eighteen or so and lived a very

conventional life. Her career was my father and the garden club and things of that nature. She was a very sociable woman, very much loved. She's still alive now. She's ninety-eight and still has a good long-term memory. I have to talk to her today, for example, on the telephone. I'll be asking her a couple of questions that this interview brings to my mind. She's in a life-care center, a very nice one.

COHEN: She's in New Haven?

SCUDDER: Well, actually just outside of New Haven in a little place called Southbury. She had her ninety-eighth birthday just a little while ago. So that takes care of—

COHEN: Good stock. [Laughter] So then you grew up in—

SCUDDER: I grew up primarily in Swarthmore as a college brat, because I think my father must have gone to Swarthmore by the time I was four or five years old. All my early childhood was as a college brat. The only interesting thing, perhaps, was that during the Depression, when he wanted to write a book, we did go to Bermuda, in 1936 or '37, when I was six and seven years old. I don't have too many memories of that. In fact, I have very few memories of childhood at all.

COHEN: But I see they did send you away to school.

SCUDDER: Yes. My guess is that—my father was Townsend Scudder, my grandfather was Townsend Scudder, and my older brother was Townsend Scudder. Primogeniture was very much a factor there. I was Thayer Scudder. Thayer is the Welsh name of my father's mother, so that's how that ties in.

We lived in a very fascinating summer commune in Connecticut, a place called Yelping Hill. It was a cultural center. The families that belonged were primarily artists or academics. For example, the president of Vassar College was there. Henry Seidel Canby, the founder of the *Saturday Review of Literature*, was there. He was also a professor at Yale at that time. In fact, he was the person who brought my family in. We would have communal meals down at The Barn. It was a lovely place. But I think the problem was that I was by far the youngest child

there, and I was about three years younger than the next youngest. That would make me about six or seven or eight or nine, and the next youngest would be about eleven or twelve or thirteen, so I was ostracized by all of the other children. They didn't want to bother with this baby, see? Unfortunately, the one who was the next youngest, his name for me was "Useless," so I don't think that helped my self-esteem too much for the first thirteen years of my—

COHEN: [Laughter] Maybe that's why you don't have too many memories of your childhood.

SCUDDER: I think that may be true, because I spent much of my time with my brother's dog. You see, again, the primogeniture: My brother had a dog, but I didn't have a dog. I spent much of the time in the woods birding. Henry Seidel Canby, for example—the founder of the *Saturday Review of Literature*—gave me my first binoculars. I remember I used to spend literally days and days wandering in the woods. Some of my earliest experiences were—and these go back to about thirteen, fourteen, or fifteen—of finding a new species that I hadn't seen before, and that kind of thing. So the first thirteen years I don't think I had particularly high self-esteem; I just saw myself as a little kid. But then in 1943 my parents moved to Concord, Massachusetts, for a year. Pa was writing another book, this time called *Concord, American Town*, so he wanted to be in Concord while writing it. He had taken a leave of absence from Swarthmore. This is before he tried to start the Center for Information on America. There I went to a private day school called The Fenn School and for the first time realized that I had certain skills which other people didn't have, because they had a cross-country race which I won by running away from everybody else. I think that was the first time when I realized, "Hey! I can do something." This was at thirteen, while my brother was at Exeter [Phillips Exeter Academy] as a scholarship student. The next year, in 1944, I was sent to Exeter as a scholarship student. We'd wait on tables and that kind of thing. That, I think, is when I came into my own. Not as a scholar; I can't remember a single thing that I learned at Exeter. I mean I read all the appropriate books. I got lists of them.

COHEN: But no discovery from great literature.

SCUDDER: No, no, no. But I got very interested in extracurricular activities. I became the captain of the cross-country team. I was the president of the scientific society, the president of

the biology group, the founder of the Exeter mountaineering club, the president of the outing club. And of course bird watching—on weekends frequently two or three of us, all considered a little bit eccentric at Exeter, you see. But since I was captain of the cross-country team, I was never teased or anything. Frequently, oddballs at places like Exeter who go on to become physicists and astronomers have sort of a hard time. Now, I didn't have a hard time. I think those four years at Exeter were paradise, absolute paradise. Then, at the end, I was told by some of the faculty members that if I wanted to go to Harvard I really ought to bring my C+ average up to a B-. So I think for the first time academically I worked a bit—in the spring of my senior year—and brought it up to a B- and was accepted at Harvard in the class of '52. I think primarily it was because of my extracurricular activities.

COHEN: Of course, the family background doesn't hurt either.

SCUDDER: No, no. Well, except that my father, you see, had gone to Yale. The only reason why I picked— You know, our children, and our children's children, they revolt from their parental background by maybe trying drugs or doing some kind of insane kind of behavior. But in the 1950s, when you revolted against your parents and your father was a Yalie, you didn't go to Yale; you picked Harvard. In my case, my father was a Yalie, so I picked Harvard.

COHEN: OK. Revolts. [Laughter]

SCUDDER: But that was only a partial reason. The main reason was that my various good friends were going to Harvard, and by that time I had gotten very interested in mountain climbing. By the time I graduated from Exeter, there were only two things I wanted to spend my life doing: bird watching—and becoming a professional ornithologist—and mountaineering. Harvard had the Harvard Mountaineering Club, which was the leading mountaineering club in the United States except for the American Alpine Club, so that was the reason for going to Harvard.

COHEN: That's probably a better reason than most people have. [Laughter]

SCUDDER: Well, I suppose. Then I met Molly. Of course, living in this commune—in effect, it was a commune—in the summers there were no girls my age. The closest girls were miles and

miles away, so I didn't know what girls were. I knew what birds were, but not girls. I think I didn't have my first date probably until I was about seventeen years old. When I was at Exeter, the brother of one of my roommates had married a daughter of a Harvard professor, and he suggested to me that I look up her sister. Her sister was my present wife, Molly. We've just had our fiftieth anniversary, so you can see it's been a good marriage. When I came to Harvard as a freshman in 1948, I looked up Molly fairly quickly. Her father was an interesting scientist; he was the inventor of the iron lung. His name was Philip Drinker, a very close friend of [Arie] Haagen-Smit [professor of bio-organic chemistry, d. 1977] here at Caltech. He was also the initiator of the Department of Industrial Hygiene in Harvard's School of Public Health. He was a very interesting man because he didn't have a PhD. In those days, if you were an inventor, you could become a professor at a place like Harvard without a PhD. His field was industrial hygiene; he did a lot of work with the Atomic Energy Commission after World War II, and he played a very prominent role in dealing with a whole range of industrial problems, like silicosis and things of this nature. Anyhow, he was Molly's father. They lived in Hubbard Park, just off Brattle Street, in Cambridge. So, starting in my freshman year, I spent a lot of time there. Molly and I would go to the Harvard-Yale football games and things of that nature.

Going back, though, to my family. I wasn't intentionally revolting against the primogeniture and this kind of thing, but it is interesting that many of the things that I did were individual things: cross-country; mountain climbing—OK, a small group—and then I started hitchhiking. I started hitchhiking when I was seventeen. By the time I married Molly—we were married when we were both twenty, in 1950—I had hitchhiked over 30,000 miles. Not 3,000, but 30,000.

COHEN: Did you hitchhike to a destination?

SCUDDER: Yes. Well, for example, the summer of my senior year at Exeter, we all hitchhiked out to the West Coast to take jobs in Yosemite in the blister rust control. We'd work along the way, and then we'd work for maybe only three or four weeks at our destination. There were a couple of friends I would do this with. We'd quit when we had enough money and hitchhike up and down the coast between Canada and Mexico. Then the next summer again the same thing, but hitchhiking out to Oregon to work for the Bureau of Public Roads and taking off to go

mountain climbing, for example. I did become the president of the Harvard Mountaineering Club, and so mountaineering played a very major role in my activities during the summers. For example, as an Exeter senior I led a group of friends up the Grand Teton. The next year, as a freshman at Harvard, I joined some Harvard mountaineers on a trip in the Selkirks, where we were climbing peaks that had been climbed only once.

COHEN: But really, Ted, didn't you have to go to class at Harvard? [Laughter]

SCUDDER: Harvard was like Exeter. I cannot remember virtually anything I learned at Harvard. Molly and I got married at the end of my sophomore year. What she remembers is that about a week before finals I would vanish into Lamont Library, study madly to get B's, and then a week later would have forgotten everything that I had studied. [Laughter] During the Harvard years, up until the time of marriage, the emphasis was on— I ran cross-country, I was on the Harvard varsity as a sophomore. But then, when I got married, I figured marriage was better than running cross-country, so I stopped that.

COHEN: You gave it some time. [Laughter]

SCUDDER: And I put emphasis on the Harvard Mountaineering Club. I led an expedition to the Alaska Range in 1952, where we made the first ascents of several of the remaining peaks over 10,000 feet. These are big peaks right next to Mount McKinley: Mount Brooks, which was just under 12,000 feet; Mount Mather, which was over 12,000 feet. This was wonderful—the kinds of friendships that you develop on mountaineering expeditions which last three or four weeks, where your life is in effect dependent on the ability of your friends, and vice versa. Occasionally circumstances would happen where somebody would fall, and by golly, you'd stop their fall just before they would go over a 2,000-foot cliff. That's exaggerating slightly, but that kind of thing. It's a wonderful environment. So, at that particular time, in '51 or '52, even though married, really all I wanted to do was to chase birds, become an ornithologist, and climb.

COHEN: How did you support yourself?

SCUDDER: I was a scholarship student at Harvard the first year. My father was able to pay for my tuition for my second, third, and fourth years some way—and I never found out how. When we were married, we lived in a Harvard faculty member's home: Harvey Brooks, who became the chairman of Harvard's department of, I guess, applied engineering [Harvey Brooks was dean of the Division of Engineering and Applied Sciences—ed.]. We lived in his house, babysitting and things of that nature. Later, Peter Elias, who was a professor of [electrical] engineering at MIT—we lived in his house. While in graduate school, we took in a boarder in an extra room. You know, all of these little ways to squeak by, the way students can.

I suppose the person who had the biggest influence on me at Harvard was James Lee Peters, who was an ornithologist and was writing a *huge* series of books called [*Check-List of*] *Birds of the World*. James Peters was probably the leading academic ornithologist of his time. He lived on the fourth floor, or the top floor, of the Museum of Comparative Zoology—the Agassiz Museum at Harvard. My intentions were to become a professional ornithologist. Oh! That's another way we survived—I took jobs at Harvard. For example, I was his research assistant. Just like we have all of these books on file in the Archives, he had cases and cases and cases of bird skins from around the world. Now, of course they all got moldy, and so my job was to dust the mold off them. Then, in connection with his work on birds of the world, he wanted to get some rosy finches and some Louisiana water thrushes, which are birds that are in the West. The rosy finches are found only up at 10,000 feet, in mountains like the Rockies and Selkirks. So he said, "OK, I'm going to send you out to the Selkirks to get me rosy finches, since you are a mountain climber." I remember some lovely days when, with a little BB gun, we would shoot pigeons and English sparrows and starlings outside the window of the Museum of Comparative Zoology. He would train me how to stuff them properly, because pigeons have a lot—

COHEN: Where are the Selkirks Mountains? I don't know where they are.

SCUDDER: The Selkirks Mountains? If you're in Canada and you see the Rockies—Banff and Lake Louise and so forth—then you drop down into the big bend of the Columbia River, which goes around like this [gesture]. Now, if you look at a map, you see that the Columbia River goes like a huge inverted horseshoe, and what it's going around is the Selkirks, which is the range just

before the Coast Range of British Columbia. It's a heavily glaciated range, unlike the Rockies. It's just a beautiful area.

So, with considerable difficulty—because it was Canada—James Peters was able to get a permit for me from the [Royal] British Columbia Museum to shoot these little birds, rosy finches and Louisiana water thrushes. He sent me off to the Selkirks, and I went with Molly and some climbing companions. We got across the Columbia River and worked our way up into a high-altitude camp, because we were going to do mountain climbing afterward. There were a few small first ascents in the area that we wanted to do. I shot one bird, skinned it, and said, "To hell with this." That was the end of my career as an ornithologist. In those days, it was still largely classical biology at places like Harvard. Genetics had not come in, really—maybe one course was taught on genetics. I had taken just about every course. Again, I didn't graduate in any particular field at Harvard. I graduated *cum laude* in general studies, just sort of playing around with a whole range of things, but mainly biology. I decided, "OK, obviously I can't do ornithology," because what ornithologists did at that time, as naturalists, was go to Brazil and kill thousands of birds, which they would then skin and study—study their morphology and this, that, and the other thing. I decided, no, I wanted to look at birds rather than to shoot and analyze them.

The next thing, then, was to move from zoology into botany. A very famous professor at Harvard at that time was [Paul] Mangelsdorf, who was the principal biologist who had worked out the phylogenetic background of corn, using materials like *Tripsacum* and teosinte from the highlands of Mexico. He also got early archaeological remains from caves in the Southwest which ancestors of the Pueblo Indians had used. I became his research assistant. Again, this was another way to get a little bit of money. He had me dusting these tiny little cobs. Actually, I was more than a duster for him; I had graduated to also being research assistant. I had to dust these little pieces of corn. They were only about so big [gesture]. They had come out of Bat Cave in New Mexico. They were over 1,000 years old.

COHEN: So you're talking about one or two inches.

SCUDDER: Yes, one or two inches. My job was to clean them and then, under the microscope, measure them and this kind of thing. I got fascinated by the dust that was coming out of them.

So I eventually looked at it under the microscope. I found out that what the pre-Pueblo Indians had used these little maize cobs for was toilet paper, because after eating them they were very soft, tiny, little cobs. If you think about it, you'll see. If you have corn on the cob, after you're done with it the cob is soft.

COHEN: You don't think that's where Mad Cow disease comes from, do you? [Laughter]

SCUDDER: That was their toilet paper. So that ended my career as an ethnobotanist. I just didn't particularly want to continue fiddling around with that. I think I stayed on working for Mangelsdorf perhaps a little bit after that, but pretty much stopped.

So, having graduated from Harvard in general studies and not having a clue what to do academically, all I wanted to do was to climb mountains. The president of the Harvard Mountaineering Club always is given job offers dealing with mountains, because, as I mentioned, it's the leading mountaineering club in the country, except for the American Alpine Club. I was elected into the American Alpine Club at, I think, age twenty. Molly was aware of the fact that we were going to have a career where I would be climbing mountains but also making money doing it somehow or another. The head of the Arctic Institute of North America, Colonel Walter A. Wood, said, "We'd like to hire you, but we want you to have a little bit of experience in other things before we do that." So I went to work as a civilian for the [U. S. Army] Quartermaster Corps, testing equipment in different habitats under extreme environmental conditions. For example, in February—this was 1953—they sent us up to Hudson Bay, Fort Churchill, where the temperature was forty degrees below zero, and even colder if you took into consideration the wind chill factor. We had these poor GIs whom we would connect up with thermocouples, and then we'd dress them in these different kinds of environmental clothing, and then we'd head off out into the desert wastelands of the Arctic. Magnificent trips! I can remember going out in over-snow vehicles where the northern lights would be just covering the whole sky, and the wind would be blowing, the snow would be pouring across, and we'd be standing in the turrets of this thing, looking up at these northern lights. Then we'd have our GIs, who'd be sleeping in tents on caribou skins. The hoarfrost from our breath would wake us up in the morning, because it would dangle down like icicles and tickle our noses, and we'd wake up. Then we'd connect these poor characters up with their thermocouples.

COHEN: And march them off. [Laughter]

SCUDDER: And march them off. Then we would take expired air samples from them and analyze the CO_2 content in the air, using Haldane equipment and things of that nature. So that was the first winter—that was dry cold. Then in the spring, for wet cold, Mount Washington, in New Hampshire, which has some of the highest winds in the United States. It was a mountain environment, so it was ideal. Well, you can imagine where they sent us in the summer: It was Yuma, Arizona. We would be testing desert clothing and walking these poor characters across the desert. Molly did join me in Yuma. She taught our daughter, Lydee, how to swim at nine months, both under and above water, and took a lifesaving course with the GIs at the army's test station [US Army Yuma Proving Ground], which is just outside the city of Yuma. That was actually a fine summer, because we traveled to the West Coast and we came back by way of the Tetons.

The hot wet was going to be Puerto Rico. But by then the Arctic Institute of North America had said, "Yep, we're ready to hire you." I was going to be in charge of the safety of the scientists who were working in glaciology on the border of Alaska and Yukon. One of those scientists, believe it or not, was Robert [P.] Sharp [Sharp Professor of Geology, d. 2004]. Of course, I hadn't met Robert Sharp, but I found this out later on when I came to Caltech. They had a base camp in the snowfields, and they would fly into that. I was going to be in charge of the base camp, dealing with the logistics of supplying the scientists and, of course, keeping them from falling into crevasses and things of that nature. I can't remember exactly how long it was, but before I was to begin work with the Arctic Institute, Colonel Wood's daughter was going to be coming out as a debutante in New York. So they [Colonel Wood's wife and daughter] were flying out in July [1951]. They took off in one of these Norseman aircraft with ski wheels, and it was never found. They vanished. Subsequently, people think that probably somehow or other, instead of going east, it must have gone west and crashed into the Pacific Ocean and vanished. Or perhaps it crashed right after taking off into a crevasse and was covered with snow. But it wasn't found. As you can imagine, Colonel Wood was devastated. In effect, he stopped that whole program. It was called Project Snow Cornice. He stopped it for a number of years. I think he started it up maybe five or ten years later on, but of course by that time I had moved on

to something else. So there Molly and I were in September 1953. I had worked for a year for the Quartermaster Corps, so we had some money. What to do?

I had no idea of whether I wanted to go into academics or another kind of career, but having a father who was a professor, a father-in-law who was a professor—and of course my grandfather was still alive at that particular time, pressing for me to be something other than a mountain climber. [Laughter] I had been offered a position—I may be exaggerating, but I don't think so—with the Tenth Mountain Division as a ski mountaineering instructor at Camp Hale in Colorado, but shortly after that the mountain troops were de-emphasized. I hadn't gotten interested in that. I think it was mooted, "Would you like to do this kind of work?" And Molly and I pretty much decided no. So—and I suppose this will surprise you—I ended up at the Yale Divinity School.

COHEN: Yes, it does surprise me.

SCUDDER: Not to become a divine, but for the scientific study of religion. My grandfather—the retired Supreme Court justice of New York—was fit to be tied, because he actually thought that I was going to be religious or something. [Laughter] But at Harvard I had become fascinated by the study of primitive religion. I mean, I couldn't quite understand why people all around the world have all of these different religions to which they are absolutely fundamentally attached. So I thought that divinity school would be kind of fun. I was only there for one year. I told them at the very beginning, "Look, I'm interested in coming as a special student." They gave me a full scholarship, and the tuition was only half the tuition of the Graduate School of Arts and Sciences at Yale, but you could take half your courses in the Graduate School of Arts and Sciences. It was my first year of study where studying became the end. It was fascinating. I took courses in Old Testament religion, church history, Islam, Buddhism, and then, down at the graduate school, a full-year course on the geography of Africa and a full-year course on the history of Africa. Somehow or other, Molly and I had decided that the Arctic perhaps would not be a good career for the family but that Africa might be more interesting. So I was taking these courses on Africa, and it was a wonderful year! But my skepticism about religion continued throughout it. Occasionally I would go into the Yale chapel and sit there, while some of my classmates would be having religious experiences up in the front pews. I still can't understand

why some people have to believe something on faith and other people are not atheists, they're agnostics. That's when I realized that I was an agnostic. I just didn't know, one way or the other. At the end of that year, I had been— [Tape ends]

Begin Tape 1, Side 2

SCUDDER: I should tell you a little bit about where we lived in New Haven, because it gives you an idea of how we lived while I was a student. Just outside of New Haven on the coast is a place called West Haven, which is a whole bunch of summer houses which have virtually no wind proofing whatsoever, because they are used for summer people. Therefore you can rent them for virtually nothing in the winter. We lived in this house that had no heating, with these kinds of stoves that they call "death stoves." The hot water was a geyser that you had to turn off after about twenty minutes or it would explode. Poor Molly was living in this place while I went off to have these fascinating intellectual experiences at Yale. [Laughter]

COHEN: [Laughter] Had Lydee been born already?

SCUDDER: Yes, Lydee was born in 1952. Lydee was born a couple of weeks after the job with the Arctic Institute of North America had collapsed, and at the time we decided, "Well, we ought to get back to graduate school and eventually get a PhD." Molly told me later that the wonderful tunafish soufflés that she would feed the family two or three times a week while I was a graduate student were cat food. Our big treat—because in those days people hadn't discovered how wonderful they were—was jumbo lobsters, which would be five, six, seven pounds each, and very cheap. Now, of course, they are the most expensive, but in those days they were very cheap, so periodically we'd have jumbo lobsters.

Well, the year at Yale—as I say, it was just for one year—made me realize that, yes, I did want to go on and get a PhD. I was very interested in somehow combining biology, geography, and anthropology.

COHEN: This is the first time you've said the word "anthropology." I was waiting for it to come up.

SCUDDER: I had taken a lot of archaeology courses at Harvard as an undergraduate, but not too much social anthropology. I must have been becoming more and more fascinated with Africa as a continent that would be a paradise for somebody interested in biology—birds and things.

COHEN: But you had never been to Africa?

SCUDDER: No. In fact, I had never been overseas at all, except for the one year in Bermuda, which I can't remember too much about.

COHEN: Yes, but in those years not many people went overseas.

SCUDDER: Precisely. So, we decided to come back to Harvard. Serendipity does play a major role in people's careers, and certainly in my career in particular. The crash of that plane got us out of an Arctic career. If we had been hired by the Arctic Institute, I think we were going to be based in their office in Montreal, and heaven knows what would have happened.

There was a new foundation, called the Danforth Foundation, that was interested in giving full graduate-school scholarships to students who were very religious and were interested either in becoming ministers in various denominations or were going to want to teach religion—for example, in theological schools, and so on. The year that I finished up at the divinity school, just that one year, was the first time I had done well academically; all my courses were A courses, except for one on systematic theology. I realized for the first time that I didn't have to get C+ marks; if I was interested, I could do OK. The Danforth Foundation decided, "Well, this year we're going to take a couple of people who are just interested in the scientific study of religion"—no religious commitment. That year they took three of us, all of whom became professors of anthropology. In addition to myself, Robert Fernea was selected, now a professor of anthropology at the University of Texas, Austin, and certainly one of the best scholars dealing with the Arab world. He did his PhD dissertation on Iraq and subsequently was in charge of a very ambitious complicated study of the Nubian population that was going to be relocated because of the Aswan High Dam. In fact, I joined him on that; we'll be talking about that later on. Then Irv [Irven] Devore. Irv is now a professor of anthropology at Harvard. He did a pioneering study of baboons in East Africa and subsequently was involved in pioneering studies of Pygmies in what is now the [Democratic Republic of the] Congo. So the three of us, who all

ended up with PhDs in anthropology, were invited to join the group of very religious people in the Danforth Foundation. This was before NSF [National Science Foundation] scholarships were available. If it wasn't for the Danforth Foundation, we wouldn't have been able to go right through graduate school. We still might well have done it, but they paid all expenses for the next six years, including when I was doing my PhD research in Africa. I had to leave Molly and the two children in Cambridge—she was living with her parents at that time—for about eleven months while I was doing my fieldwork for my dissertation. The Danforth Foundation took care of all of that. I had no interest whatsoever in institutional religion as a career or as a belief system. I am indebted to them. Kenneth Irving Brown was the head of the Danforth Foundation at that time and obviously played a major role in helping us get the scholarship.

OK. So then we came back to Cambridge in 1954, and then I was just a regular graduate student at Harvard.

COHEN: You could use the Danforth money anywhere you wanted?

SCUDDER: Yep. You were told, "OK, go out and get your PhD or your bachelor's in divinity and then your PhD anywhere you want. Do anything you want." The only requirement was that once a year we would join all of the other student scholars at that time at the Danforth Foundation's summer retreat in upstate Michigan—I think Minnewanca was the name of the place. Those were very interesting, actually, because we'd meet all of these characters. I think at that time, too, they did bring in a few people from other religions—there were one or two Muslims—but I think they didn't go much further in that experiment.

So then back at Cambridge—not too much to talk about, actually. I mean, just routine, going ahead, taking coursework, except that the big thing there is that I found that there was nobody at Harvard in the Anthropology Department who had really done fieldwork in Africa.

COHEN: It was the unknown continent.

SCUDDER: It was the unknown continent. But just at that time Boston University had started one of the first African studies programs. A gentleman by the name of W. [William] O. Brown had started that, as opposed to Kenneth Irving Brown of the Danforth Foundation. I heard that there were some very good anthropologists over there, one of whom was Elizabeth Colson.

COHEN: She already was on the faculty there?

SCUDDER: She was on the faculty at Boston University. I went over to BU just to take courses. I sought out two anthropologists there. One was Elizabeth Colson and the other was an anthropologist who had done research in East Africa, Phillip Gulliver. I'll mention him a little bit later on. So I took Elizabeth's—I guess it was a full-year course on the social anthropology of Central and Southern Africa, which was her specialty. I have to mention a few things about her, because she's been by far the most important professional person in my career. She had taken her PhD at—at that time [1945] it was called Radcliffe, but essentially it was Harvard-Radcliffe. She had taken her PhD there and had worked for and with Clyde Kluckhohn, who was the famous scholar of the Navajo. I always have a difficult time spelling his name, because I didn't like him very well.

COHEN: [Laughter] We can look that up.

SCUDDER: He was the chairman of my thesis committee, and I have only forgotten one appointment in my life, and that was an appointment with Clyde Kluckhohn. I'm not fond of Harvard. I have not been back to Harvard hardly at all since getting my PhD in 1960, because, although I have a watch on today, very rarely do I wear a watch. The reason is that when you walked into your Harvard professor's office—Kluckhohn's or any of the other ones—within about five or ten minutes they started looking at their watch. They did not see teaching as their thing. It wasn't just me; all of us graduate students were complaining about this.

Anyhow, there were two people who were different in this regard. One was a lecturer, because his PhD dissertation had been torpedoed during World War II. He had done archaeology in Iran, and when the material was coming back from Iran, the boat was torpedoed and he never got his—

COHEN: Oh, you mean literally, not figuratively.

SCUDDER: Literally. He never got his PhD. He wasn't on the boat, just all of his data was. Derwood Lockard. He taught courses in the Center of Middle Eastern Studies, and I was his research assistant. He was a good person, a fine person.

The one who really impressed me at Harvard—and the only one who really impressed me both as a scholar and as a person—was, again, another woman, Cora Du Bois. She had done her research in India and took a direct interest, if not in students, at least in me. When I was preparing for my oral examinations, for example, she would meet with me one-on-one for I don't know how many weeks.

COHEN: She gave you some attention.

SCUDDER: She gave me some attention. She and Elizabeth Colson. I took this course of Elizabeth's my second year. Because I was a research assistant, I still hadn't finished my coursework at the end of that year. I hadn't taken any of my language exams. At the end of the course—this was in May-June of '56—Elizabeth said, "You know, I've been asked to come back" to what was then Northern Rhodesia. After she had gotten her PhD in 1945, she had gone to Northern Rhodesia in 1946 to do more field research; she had done her PhD research on a Native American group on the West Coast, the Makah Indians. She went out to study the Tonga-speaking people of the Northern Rhodesian plateau. She spent over five years out there, becoming the director in 1947 of what was called the Rhodes-Livingstone Institute—"Rhodes" after Cecil Rhodes, and "Livingstone" after David Livingstone—which was one of the three leading British-funded social science research institutes in Africa. There was the Rhodes-Livingstone Institute for Central Africa; there was another institute in Uganda at Makerere University; and there was the Eastern Africa Institute. These were all looked at somewhat askance by the British Colonial Service, but occasionally they would call on the anthropologists who were employed at these institutes to help them out.

Elizabeth Colson was the director of the Rhodes-Livingstone Institute until 1951, when she went to the University of Manchester for two years. She had come back to the United States after that and was slowly working her way up the academic ladder, first at Goucher College—because even at that time it was difficult for women who were first-rate to immediately start as assistant professors at first-rate places. Then she moved on to Boston University in 1955, where she was an associate professor at the time I took her course. She told me at the end of the course that she had been invited to come back to Northern Rhodesia, because they were about to build a big dam on the Zambezi River, the first mainstream dam on the Zambezi, and it was going to

flood out an unknown number of Tonga speakers. Since she already spoke Tonga and had studied the Tonga on the plateau, the then-director of the Rhodes-Livingstone Institute—a person by the name of Henry Fosbrooke— A very interesting chap, actually; Henry is still alive; he must be in his nineties. [Henry Fosbrooke died in 1996, at age eighty-seven—ed.] He is married to an African wife in Tanzania, living on the edge of a volcanic lake up near Mount Kilimanjaro, and apparently still going strong. He was a geographer. At that particular time, he was very interested that there would be a study of these— There were the Plateau Tonga, whom Elizabeth had studied, and then there were the Valley Tonga, who were way down in this rift valley, about 1,000 meters below the plateau, who had never been studied and were going to be disrupted and involuntarily relocated because of this dam.

So Elizabeth had been asked to come out, and since Henry Fosbrooke was a geographer, he wanted her to work with a geographer. Elizabeth said to me, "Look, see if you can find a graduate student of geography." She knew that I was in contact with Harvard's last geographer. I had been interested in doing a PhD in geography at Yale, which had a fairly good department at that time. When I came to Harvard, Harvard had by then eliminated its Department of Geography, but of course it couldn't eliminate its tenured professor. This was Derwent Whittlesey, who had done research in what was then Southern Rhodesia. He was a wonderful man, pretty much isolated on the top floor of the Museum of Comparative Zoology, sort of in a little hole right up against Peters' bird-dusting place. I spent quite a bit of time with him. Elizabeth knew this, and said, "Check with Whittlesey to see if you can find somebody who will meet Fosbrooke's requirements." I didn't look very hard. I went back to her a couple of weeks later and said, "Well, Elizabeth, I can't find anyone. What about me?" She looked at me in her classic fashion and said, "Humph. I didn't know you were interested." That was her response.

Now, at that time I was only a second-year graduate student, so I really hadn't thought too much about what I was going to do my PhD dissertation on, except that it was going to be in the mountains. I was going to find a tribe—and I had found a tribe; they were called the Bakonjo, and the Bakonjo yodeled. They were the only tribe that lived in the Ruwenzori Mountains of Uganda. These are the Mountains of the Moon—the famous Mountains of the Moon which go up to 16,000 feet, shrouded in mist, with gorilla neighbors on the volcanoes nearby, and the Bakonjo yodeling back and forth to each other on the ridges of the Ruwenzori.

That was what I wanted to do, and I was already beginning to think about drawing up a proposal for the Ford Foundation. Well, of course, the Middle Zambezi Valley is a rift valley.

COHEN: Hardly in the mountains.

SCUDDER: Hardly in the mountains; disease-ridden, hot, dry, a godforsaken place. Elizabeth said, "Well, OK. I didn't think you were interested, but if you are, fine." And so three months later, I was in the Middle Zambezi Valley.

[Tape ends]

THAYER SCUDDER

SESSION 2

December 27, 2000

Begin Tape 2, Side 1

COHEN: We were to start this time with your first field experience in what is going to be the path for your life.

SCUDDER: All right. Good. Just to review very quickly, by the age of twenty-six I had gone through two aborted careers and was about to start the third.

COHEN: Now, one was your mountain climbing?

SCUDDER: No. The first was to be a naturalist. That was a serious career. We talked a little bit about that; we went at it indirectly. For example, at Exeter I took four years of Spanish. My heroes at that time were all the various naturalists, like Thomas Barbour and W. H. Hudson, who had written about the Amazon and Latin America. I assumed that I would be working in the Amazon as a classical biologist, you see—as a naturalist. Well, that's one of the few parts of the world that I haven't been to yet.

COHEN: You've never been there?

SCUDDER: No. I had a brief trip to the Amazon just a year ago, but I've never worked there. Nonetheless, that was a very serious career. I had prepared myself in biology in particular, starting in primary school, and then at Exeter I favored biology. At Harvard, for example, I was allowed as a freshman to take courses that juniors and seniors were taking. I was really concentrating on biology, and by the end of my sophomore year I had taken practically all the courses that were available in biology.

COHEN: This, of course, was very classical.

SCUDDER: Yes. By my junior year I had pretty much done what was available, so I began to get interested in other fields. Then the mountain climbing came along, and that became a career, up until the time of the failure of the Arctic Institute of North America job because of the tragic deaths of Colonel Walter A. Wood's wife and daughter. So then, back to Harvard as a graduate student. At the end of my second year, that's when Elizabeth Colson invited me to join her in the Middle Zambezi Valley. My thesis advisor, chairman Clyde Kluckhohn, in effect said, "No, no, no. You can't do that. That's just not the way it's done at Harvard. You have to finish your coursework," which I hadn't done by the end of that year because I was on a teaching fellowship, and so I still had some courses to do. You had to take your general examinations and your language examinations, and then you'd go to the field. Well, we now know that's just the wrong way to do it. You should go to the field as soon as you can, because then you can think through all of the material that you're learning in books in terms of your own experience in the field.

COHEN: I see. Is that commonly done now?

SCUDDER: No, simply because it's pretty expensive now to go to Africa and Indonesia, and also because it's getting increasingly difficult. So the old pattern pretty much—

COHEN: Still exists.

SCUDDER: Although there are opportunities now for undergraduates. Well, like the Summer Undergraduate Research Fellowships [SURF] program at Caltech. Sam [Samuel J.] Clark, who was one of my students and a student of James [Z.] Lee [professor of history 1983-2003]—he went to the Middle Zambezi Valley on a SURF. That got his interest, and now he's getting a PhD that he's written on the demography of the Middle Zambezi Valley at the University of Pennsylvania. So there are exceptions, but the general pattern—

COHEN: But it has to be a very responsible student.

SCUDDER: It does have to be a very responsible student. So anyhow, Elizabeth and I took off in September of '56.

COHEN: He said no, but you went anyway?

SCUDDER: Well, he had no choice in the matter. He said, "No. You can't do that. It doesn't make any sense. You should stay at Harvard and go through the regular way." I can't remember that there was ever a major issue over it. I just said, "No. I'm heading off to Africa." Remember, I had this Danforth fellowship.

COHEN: That's right. You were independent financially.

SCUDDER: It was a fellowship that would take me until I graduated with my PhD. We went by way of England, and that was very interesting, because—

COHEN: Now, you had Molly with you or not?

SCUDDER: No, no. Elizabeth Colson is an amazing person. She said, "No children! If you take your children, they'll die," because the Middle Zambezi Valley is very disease-ridden, and the two children—one was born in '52 and one was born in '54, and now this is '56, you see. So it was strictly on my own, which would mean being away from family for about an eleven-month period, which was a very difficult time for them, as you can imagine.

COHEN: That's right. You said Molly went and stayed with her parents.

SCUDDER: Yes. Anyhow, in England we went to the meetings of the Association of Social Anthropologists. That was my first experience with the infighting among anthropologists, which I think tends to be worse than in most other academic areas and, as you know, in academia the infighting is pretty bad. That influenced me, I think, tremendously. In fact, that's one reason why I came to Caltech—just to get away from the infighting. When I came to Caltech—and Hallett Smith was the chairman [of the Division of the Humanities] who had recruited me—I told Hallett, not joking entirely, that if he ever hired another anthropologist, I'd resign. Anyhow, these British social anthropologists were just vicious. There was one poor chap by the name of Reo Fortune who had done a lot of good work in Melanesia, but for some reason or other they had it in for him, and they were just ripping him to pieces. This would be in the meetings. There

would be a talk, and then afterward they'd attack. When I say, "they'd attack," I mean no matter who it was, they would attack. Then they would go to the pub, and the attack would continue in the pub. So that was an interesting but not very pleasant introduction to the way social anthropologists do things, because in England you don't have general anthropology, as in the United States. For example, at Harvard you have to be conversant with five fields of anthropology: archaeology, linguistics, human biology, ethnography, and social anthropology. But in England you're either going to be an archaeologist, or you're going to be a social anthropologist, or you're going to be a physical anthropologist, and so on.

COHEN: They don't care about knowing the other fields?

SCUDDER: Well, even in the United States now, some departments like, for example, San Diego—you were at San Diego.

COHEN: For a short time.

SCUDDER: There it's social anthropology, from what I understand. The knowledge that you have to— I mean, really, it's almost requiring you to be a renaissance scholar.

COHEN: To know all those things.

SCUDDER: To know all of these different areas. It's just very difficult, and of course it gets increasingly difficult as time goes on. Anthropology, for example, is the only field that I know of where I can apply to the National Endowment for the Humanities, I can apply to the Social Science Research Council, and I can apply to the National Science Foundation, because all of my expertise sort of falls in those particular areas. So it makes it a very, very difficult problem to stay on top of that kind of thing. Of course, that's one reason why anthropologists are like historians: If they can live until ninety, their knowledge continues. Their brilliance, unlike with physicists, is able to come out in their latter years.

COHEN: [Laughter] I see. They mature more slowly.

SCUDDER: The other interesting thing in London was that there was a Tonga. The people we were going out to study in the Middle Zambezi Valley were the Tonga living in the Gwembe Valley. There was a student from a nearby area by the name of Mainza Chona, who was taking a law degree at Gray's Inn, and he taught me my first words in Tonga. I remember very much meeting him. He subsequently became the vice-president of Zambia. He's still alive, has a legal business in Lusaka, the capital of Zambia, had been ambassador to China, and things of that nature.

We arrived in the Middle Zambezi Valley in either the end of September or early October 1956, and it was hotter than Hell. That is the most extreme environment in Central Africa outside of the Kalahari Desert. It's low. It's in a rift valley, an extension of the East African Rift Valley, which goes all the way down, starting in the Red Sea, down through East Africa, down through Malawi, then up the Middle Zambezi Valley and over to the Okavango in Botswana. So you're down in this big cleft, with plateaus over 1,000 meters on either side. It was also the most isolated area at the time.

COHEN: How did you get there?

SCUDDER: You get there by four-wheel-drive vehicle. So we arrived in Lusaka. Of course, Elizabeth Colson knew the area. Although her research had been on the plateau among Tonga speakers, she had gone down to one area in the valley with carriers—walked down, it took a couple of days—in 1949. She spent some time down there in the forties. By the time we arrived, the first Land Rover track had been built down into the area, sort of halfway down what was called the Gwembe Valley and down to the edge of the Zambezi River. Now, just to briefly give you an idea of the geography, they were going to build a big dam. It created what was the world's largest artificial reservoir at the time it was completed. This is the reservoir behind the Kariba Dam, which was the first mainstream dam on the Zambezi. To give you an idea, the dam itself was about 400 feet high.

COHEN: Who was building this?

SCUDDER: Oh, we'll get to that in a moment, because that's the financing of the research and that kind of thing.

The reservoir backed up behind the dam was 130 million acre-feet. Now, to put that into perspective, that's four times the storage capacity of Lake Mead. It's a *huge* reservoir. It's over 150 miles long, and in places 20 or 30 miles wide, so it's a huge, huge reservoir. The construction of that dam and the reservoir was therefore going to cause the relocation of what we now know were 57,000 people who lived along the Zambezi, along that 150-mile stretch, cultivating the very fertile alluvial soils which had been deposited through thousands and thousands of years. In fact, the density of the population was pretty much determined by the availability of alluvial soils. This is important, for reasons you'll see in a few minutes.

The then-director of the Rhodes-Livingstone Institute, Henry Fosbrooke, whom we mentioned before, was aware that these people had never been studied and that they were soon going to be relocated. He was interested in two things: One, he felt that there should be a history of how they lived in this very difficult environment; secondly, he was very interested in what is now called development anthropology—that is, what could we learn from studying these people before they were moved and after they were moved? What would be the implications of the compulsory movement of *x* thousands of people? He had gotten money from the industries in what was called the Copperbelt in then Northern Rhodesia. They were the ones who wanted the dam. They were running out of electricity. They were importing electricity at that time from the Congo; they had been cutting down trees in the vicinity of the mines. These were large firms—for example, Anglo American, Rhodesia Selection Trust, and American Metal Climax. Rhodesia Selection Trust was the big one at that particular time, and they were the main financiers of the dam, along with the World Bank. The World Bank gave what was the largest loan that it had given in Africa until that time. Again, it gives you an idea of the magnitude of this thing.

It's very important to point out how big the dam is, how big the reservoir is going to be, how many people are going to be relocated, and the major financing. It was the first major dam in Africa, and it certainly was the first to be funded by the World Bank in Africa, along with the copper companies. The power was going to be divided pretty much between the power companies in Northern Rhodesia and the urban industrial centers in what was then Southern Rhodesia—Salisbury, Bulawayo, and places like that.

COHEN: So in some sense there was no thought of these people at all.

SCUDDER: That's right. The people were viewed as people in the way. Originally they were underestimated, in terms of the number. This is a very common problem with the big dams around the world—they do underestimate the people. This is part of the tendency, I'm afraid, to— It's what we call hydro-politics. These big dams often are as much political as they are economic, and a cost-benefit analysis is used. There is a tendency, almost without exception, to overestimate the benefits and underestimate the costs. Now, the costs are underestimated in part because one doesn't have the knowledge of what the environmental impacts and the human impacts will be, but increasingly we do know what those impacts will be. They still have a tendency to underestimate the costs, because these big dams tend to be pushed, if you think about it, by people right up to the level of president. We're talking about projects the size of TVA [Tennessee Valley Authority]. Who pushed TVA? That was [Franklin D.] Roosevelt's concept, in effect, for the industrialization and the bringing of cheap energy to homeowners in a poverty-stricken area. Then, when you think of the Aswan High Dam, who pushed that? That was Gamal Abdel Nasser. If you think about the Kainji Dam in Nigeria, that was pushed by Prime Minister Balewa, and so on.

COHEN: These are monuments. They are leaving a monument.

SCUDDER: Well, Nehru, in India, called them the temples of modern India. They are the pyramids, in effect, of modern times, seen by political leaders. The then prime minister in charge of the Central African Federation was one of the ones who was pushing Kariba. So the political aspects are there. There's a very powerful consortium of political leaders who want this as a monument to their regime and engineering firms who want to build it. The Three Gorges Dam in China, the estimates run between $20 billion and $25 billion. Now, that's exceptional. But most of the big projects that I'm working on right now are billion-dollar projects. These are the largest single infrastructure projects in a country's development portfolio. These are huge, huge projects. In the case of the Kariba Dam, there was an alternate dam which the Northern Rhodesians had been pushing called the Kafue Dam, which was entirely within Northern Rhodesia. They wanted that, but at that point in time you had what was called the Central African Federation, which was a federation of Northern Rhodesia, Southern Rhodesia, and Nyasaland—now Malawi—dominated by the British white colonists in Southern Rhodesia.

COHEN: So it was really an English colony.

SCUDDER: Yes, and it was a self-governing colony, Southern Rhodesia. They wanted to have the dam on the Zambezi, which was on the international border between Northern Rhodesia and Southern Rhodesia, so that the dam would give them greater control over Northern Rhodesia. At the same time, it would give them direct access to the power, because they put the transmission stations and the initial turbines on the Southern Rhodesian side. So you have this huge concrete arched dam with the power station on the Southern Rhodesian side and the transmission installation on the Southern Rhodesian side, then the power going back across into Northern Rhodesia into the Copperbelt, as well as south to Salisbury and the major cities of Southern Rhodesia.

COHEN: So they had a stranglehold on it.

SCUDDER: Yes. It was a single-purpose dam as far as the Northern Rhodesians were concerned—that is, for hydropower for the Copperbelt. But it was a dual-purpose dam as far as the Southern Rhodesians were concerned, because (a) they wanted the power, but (b) they saw this as a concrete linkage between the two Rhodesias, which would give them increased political control. That was very important.

Nobody cared at all about the people who were going to be relocated. They didn't even know how many there were. The original estimate was around 20,000-plus, and double that was eventually moved. Then, as they began to construct the dam, halfway through they decided that they were going to heighten it. Therefore a number of the resettlement areas on the Northern Rhodesian side which had already been selected by people—people had already begun to build houses and clear new fields—were subsequently inundated because the dam was raised. That created a tremendous problem, because 6,000 people who were just going to move inland from their current habitations had the areas to which they were going to be moved flooded, and there was an inadequate land base for the relocation. People around the world prefer to be relocated the shortest distance, geographically and sociologically, as possible so they'll remain in a familiar habitat. But since the land was not available, 6,000 of these people had to be moved into a different area below the dam. We'll be talking about that a little bit later on.

Now, on to the research. Colson and I arrived in the hottest part of the year, were outfitted with a Land Rover, drove down this relatively new track to the edge of the Zambezi, and established camp initially in the same village—right in the village. Colson, remember, was already familiar with this. Her tent was here [gesture] and my tent was right beside it. Now, she already speaks the language to a certain extent.

COHEN: She's an amazing woman.

SCUDDER: She is an amazing woman. But here I am, as a complete neophyte in this *very* different environment from the mountains that I had been in previously.

COHEN: Now, was it just the two of you? The rest of your team was made up of the local people?

SCUDDER: That's right. You hire local people for cooks and things of this nature. With anthropologists, I think culture shock sets in either in the first couple of weeks, when you realize, "My god, what have I gotten myself into? What am I doing here? How am I going to cope with this?" or it happens at the end of, say, the third or fourth month when the novelty has worn off, and you say, "My god! I've got to spend another nine months here just gathering data." In my case, it happened the second week that I was there. I remember wandering off, with the sun pouring down. I mean, like here, it was the worst kind of day, when the temperature is well above 100°. I remember wandering off and saying, "No, no. This was just a big mistake." I remember coming back to talk to Elizabeth Colson and saying, "You know, I'm not too sure whether or not this was a good decision." As I remember, she was just completely honest. She said, "Well, it's up to you. It's your decision," period.

COHEN: She didn't try to say it will get better or it will get worse or something else?

SCUDDER: No, no. Nothing of that nature. She just said, "Well, you have to make up your mind." Another thing that she had done for me which I can remember: You'll recall when we were talking before, that Fosbrooke, the head of the Rhodes-Livingstone Institute, wanted a geographer, so he pretty much wanted me to be his person. His concept of "his person"—

Henry was a wonderful person, actually, but he was a typical British colonial type. He had been an ex-district commissioner in Maasailand, in what was then Tanganyika, before he became the head of the Rhodes-Livingstone Institute. I have a wonderful picture of Henry standing on the lawn, almost with a whip in his hand—I mean, it was that kind of attitude but not actually with a whip in his hand—with three African staff crawling on their knees in front of him pulling out the crabgrass, with Henry wandering along behind them to make sure: "You get that piece. You get that piece." [Laughter] Well, he wanted me pretty much to be his research assistant. Here I was, just a second-year graduate student, and he began to put a little bit of pressure on me.

COHEN: How did you communicate? I mean, he wasn't down there.

SCUDDER: No, no. But you see, this is expeditionary kind of work. You've got to have tents, you've got to have Land Rovers, and you've got to have equipment. It takes about two weeks or so to get organized and get into the field. So during those times Henry is giving his concept of how the study should be and trying to—

COHEN: I see. So you had a couple weeks with him before you left.

SCUDDER: I guess. I can't remember the details—whether it was ten days or two weeks—but I'm sure we had to wait for the car, and in the colonies nothing goes very fast. I remember Colson sitting him down and telling him, "No. Ted is my *colleague*." That was the emphasis. I'm a graduate student, a second-year graduate student, but she's not viewing me as a graduate student, she's viewing me as a colleague and a co-worker. "We will work together," in effect she was saying. "The team will be the two of us. We'll be delighted to talk to you, Henry, and you can tell us things that you feel are important, but you leave Scudder alone." Subsequently I developed a very close friendship with Henry and with Henry's wife, Jane. I visited him in the 1970s after Jane died. Well, no, actually Molly and I visited him first in the 1960s in what was then Tanzania, when Henry had retired to Lake Duluti, a beautiful lake inside a volcano, very close to Mount Kilimanjaro. I learned how to water-ski there. Subsequently, with the two children, Alice and Lydee, I visited Henry in Botswana when he was in charge of a UN project there.

OK. Well, we were meant to have two Land Rovers, but the second one wasn't ready. That's one reason I was living cheek by jowl with Elizabeth, and of course comparing myself to her, who was doing a lot of work and I wasn't getting much done. In October I got my own Land Rover, a little short-wheelbase one, which I immediately turned into a convertible. I took off the roof and put down the windscreen. I got hold of a shotgun and moved out of the village that Elizabeth was in, which was in from the Zambezi a little bit. You couldn't see the river. It was perhaps more interesting anthropologically, but remember my interest in biology and ecology? I found a little village right on the edge of the Zambezi where I could actually see the Zambezi. It was a small village of 126 people. Right in the middle of the village—it was a village that was sort of extended along a ridge—was a beautiful Indian tamarind tree, and so I put my tent up underneath that. I had half the village on one side of me and half the village on the other. What I hadn't realized, of course, is that under the Indian tamarind tree was the dancing ground of the local villagers and the dancing ground of the local scorpions and snakes. I used to enjoy myself at night in my bed. I would turn on the flashlight and I would watch these scorpions copulating on the ground and jumping around. Then the locals would come in and they would be dancing outside and making a big hoopla. When you're in the village like that, obviously anything that goes on—any noises arise that sound interesting, like somebody dies and you hear the wailing—immediately you're up and over to it. Very quickly an elderly man by the name of Lice died, and I was able to observe and participate in the three-day funeral.

COHEN: And you were accepted? I mean, what did you do for language? Or by then did you know some language?

SCUDDER: Well, the attitude of local people— We expected—and this is rather interesting—we expected the people would be hostile to us because the government had told them that they were going to be moved. Their concept of a European—all whites from the north are considered Europeans; an American is not called an American, he's called a European. The Tonga name is *Mzungu*, which means a European. They had three categories that they stuck you in. One would be a missionary. Well, clearly I wasn't a missionary. Another would be a white trader, a shopkeeper. Indians from Gujarat were also in that category, and obviously I wasn't that. The third category was government official, and that is what they concluded Colson and I were,

because on our Land Rovers was written, "Rhodes-Livingstone Institute." Well, the Rhodes-Livingstone Institute—in effect, we were colonial civil servants. We were working for the British Colonial Service. We were being paid— In effect, our boss was the governor of Northern Rhodesia; our direct colleague was Henry Fosbrooke at the institute, but Henry reported to the governor of Northern Rhodesia.

We were a kind of European that they really had never had much contact with, because we were actually living in the village. Colson already spoke the language. I was struggling to learn it. One of my deficiencies, Molly says, is because of auditory perception difficulties. You probably have noticed, as a friend, that I have my own patois of English. There are certain words in English that I don't quite hear. The language Tonga is a tonal language, and I could never master the tones; I couldn't even *hear* the goddamn tones.

We were both meant to collect information on the totality of the sociocultural system—the people—because this was a benchmark pre-project study of how they lived in the Middle Zambezi Valley. Colson tended to put more emphasis on the social organization, the religious organization, and I was pretty much responsible for the interrelationship between people and environment—the human ecology—and the economics. I was in effect studying the production system. The way you start off in a village—the first thing you do—is you map the village. Then you find out who is living with whom. Then you begin to map the gardens. You find out who works with whom. Then this begins to get into the kinship organization.

Of course, you have to hire staff, and I hired a staff of three young men, all about my age. All of them had been to a little bit of primary school, but I had to train them. For example, I taught my cook how to cook. First of all, he had to boil the drinking water, "Bubbly, bubbly, five minutes." Then I learned very quickly, never to go into the kitchen, because, being a very hot area, the water would be put into these desert bags that you hang on your car window for evaporative cooling. I said, "Be sure you clean the bag out before you pour the new water in. Pour the old water out, shake it around and clean it out." One day I went into the kitchen, and just outside the kitchen there was a big hole in the ground where a former termite mound had been. The people use the mud that the termites have processed to build adobe-type structures. Gradually you dig down, and when it rains, it fills up with water, and the goats get into it and they pee and this, that, and the other thing. There he was, standing down in this hole—on the edge of it—taking my bag and dipping it into this brown muddy water, swilling it around, then

pouring it out, and then pouring the cool boiled water into the bag. So these are the kinds of things—

COHEN: [Laughter] Better you don't know them. Let me just interrupt for a moment. You hired three men. Would it ever occur to you to hire a woman? Or was that really not part of that society?

SCUDDER: No. Unfortunately, as in so many patriarchal African societies— Actually, although it was patriarchal, it's a matrilineal society. That is, for social purposes descent is reckoned through women, but that doesn't mean that women have the authority. Instead of your father having the authority over you, it's your mother's brother. For example, if you need to go to the hospital—to the local missionary clinic—you have to get the permission not of your father but of your mother's brother. You belong to the lineage of your mother. Nonetheless, the status of women was higher than it is in many African—and certainly many Middle Eastern—societies, in the sense that women did own property and [left it] to their daughters.

COHEN: Decision making was—

SCUDDER: Decision making, except in the domestic sphere, was pretty much an entirely male domain. Just to give you an idea of how serious and how bad it was: When wills came in, wives were frequently left out of the will. Even today in Zambia in these matrilineal societies, when the husband dies, the relatives of the husband, the matrilineal relatives of the husband—his brothers and sisters and sisters' sons—will come to his house and take all the furniture and the vehicle, if he's a wealthy, educated person living in the city. The wife will be left with nothing, unless she happens to have exceptionally good relationships with the in-laws. So it's still a difficult situation in that regard.

COHEN: So you had these three men working for you.

SCUDDER: I had these three men working for me. We spent a lot of time together. The topics that they talked about were women—as you can imagine—and hunting. I had a shotgun, and so I did a lot of hunting for the pot—mainly birds. In fact, entirely birds. The bird lover in me

made me not want to shoot mammals, but I did shoot the odd goose, ducks, and lots of guinea fowl and partridges. Then, of course, I would share them with my staff. We developed a good group. After I had spent about six months in this particular village and gotten to know it fairly well, it was my job also to tour the whole area—both sides of the Middle Zambezi Valley. We spent a lot of time loading up our Land Rover, taking off, and occasionally getting stuck. For example, one day it took something like eighteen hours to dig ourselves out. The Land Rover would just crunch down into a mud hole so that the axles would be lying on the mud. Then you would have to crawl under it, dig out, put in rocks, gradually elevate the Land Rover, build a temporary road under it, and move forward until you got out of the mud hole. Of course, this was all very exciting; there was still big game in the area. Sometimes we would have to light a fire around the edge of our camp when we were out on safari to keep the elephants from coming into it.

It's important to point out that the Zambezi River does not flow in the axis of the Middle Zambezi Valley syncline. It's a little bit off to one side. What that means is that the tributaries on the Northern Rhodesian side are coming in down-dip, and so you have very large deltas. That's why 35,000 of the 57,000 people were on the Northern Rhodesian side. You only had one-third of the surface area of the Middle Zambezi Valley on the Northern Rhodesian side, but you had two-thirds of the people. Fosbrooke didn't know the reason, and that was one of my jobs—to figure out why all the people were there. At that time, a university was just being formed in Southern Rhodesia, eventually called the University College of Rhodesia and Nyasaland. The geologist, and eventually the pro-vice chancellor, was a man by the name of Geoffrey Bond, who was a Pleistocene geologist. I got aerial photographs from him, and he was the person who explained to me that the Zambezi was not flowing in the Middle Zambezi Valley syncline. Then I was able to realize from the aerial photographs that over two-thirds of the alluvial soils were on the Northern Rhodesian side, and you had these beautiful deltas, where a tributary would be coming into the Zambezi. These tributaries would usually—as in Eaton Canyon—dry up in the dry season, but there would be flooding in the rainy season. These tributaries would come in; the Zambezi would be coming down in flood in March, so they would back up the water coming in from the tributaries, which would then deposit alluvium over these big deltas. So the villagers were scattered around the edges of the deltas. In a big delta, you might have seven villages of maybe 2,000 people living around the edges.

COHEN: So you proceeded with this study of yours on your own?

SCUDDER: I proceeded pretty much on my own, except that once a fortnight—occasionally more often and occasionally less often—Colson and I would meet. She had a set way of taking field notes. Every day she would sit down at her typewriter—we used typewriters in those days—for a couple of hours. I'd go out in the morning, mapping fields and studying agriculture and hunting, gathering, fishing, that kind of thing. Then I'd come back, have lunch, and then I'd sit in my little—you'd call them gazebos here—we called them chitungus. These are little grass shelters in which you'd sit away from the sun. We'd have grass around it to keep the sun out. I would spend a couple of hours typing up my notes. Then in the cool of the afternoon, around four o'clock, I'd go back out and work again. Then I'd have my supper and sit out watching the sunset, having my gin and tonic, overlooking the Zambezi.

COHEN: Very civilized. [Laughter]

SCUDDER: Oh, yes. That was great. Then I'd have my bath. Now, the bath would be one of these canvas baths that you find in safari camps. It's just a frame that folds out and you stick this canvas bath in it. I'd yell for my troops to bring a bucket of hot water, which they'd pour in, and then I'd sit in my bath, often with my first beer. I'd have my beer then, gin and tonic later on, and I'd sit there. Sometimes I'd have very interesting experiences. One time I'm sitting there in my bath and I hear a little rustling in the thatch on the side. I peek over there as a cobra comes out, so I yell to my troops to bring my shotgun, and sitting in my bathtub I shoot the cobra. [Laughter] I'm not going to go into details, but you have quite a few experiences.

COHEN: That's a good representative story.

SCUDDER: Another example of that is you learn lessons. During the first couple of weeks and first couple of months, I was told how a very serious disease—bilharzia, or schistosomiasis—is in the water and does very nasty things to you. The best way to get rid of it from the water is to put copper sulfate crystals in it, so I used to pour copper sulfate crystals into my water to the extent that it would get blue. Then quickly I learned that if the water sits for twenty-four hours, that kills the little cercaria and that's all you need to do. We had 44-gallon drums, so maybe

once a week we'd go off in the Land Rover with a 44-gallon drum, get it filled up with water, and then just let it sit. Then you didn't worry about it.

The other fear I had was snakes. I didn't want snakes to come into my tent at night, so every night I would seal the tent up. I would fold down the tent awning and I would put metal boxes all around the edge and seal it up. One night I sealed a snake in with me, because a snake had already been in there. He had come in to chase mice. So I woke up in the middle of the night. I had a map case, which was this big [gesture], and the snake had obviously chased a mouse into the map case, caught the mouse, and ingested it. But then, in the swollen part of its tummy, it had got stuck in the back of the map case. So the head of the snake was sticking out one end, the tail of the snake was sticking out the other, and the whole thing was sort of thrashing around with this big map case. It was a big snake. So what does the brave anthropologist do? I'm sleeping in my cot inside my mosquito netting which comes down like usual mosquito netting. Of course I call my staff, and it takes them about half an hour to get into the tent because it's all sealed up. But they finally arrive, and one of them just beautifully flicks his spear right through the head of the snake, and that's the end of the snake. Well, thereafter—and this is the only other snake story I'll tell—thereafter I would leave my tent wide open every night, not only with the awnings down, but two feet off the ground. Snakes would come in and snakes would go out, and other things would come in and other things would go out. So these kinds of lessons you learn.

Once I adapted, I didn't have the culture shock after three months. I was being a naturalist. The first career came back, except instead of studying birds in the Amazon, I was studying people in relationship to their environment, which required me to learn about their fishing, their hunting, their gathering. I collected, for example, well over fifty to seventy wild food plants and then took them all up to have them identified by the foresters in both Salisbury and Northern Rhodesia. So I was, to a large extent, a human ecologist. That year, especially through traveling, I got to know the whole area.

I should explain the valley a little bit. You have Victoria Falls at the top of it. Right below Victoria Falls, you have a beautiful gorge, which is one of the best white-water rafting sites in the world. Sobek Tours, for example, is there with the rafting. You have a Grand Canyon-type area. Then the Middle Zambezi Valley opens up. The top of this huge reservoir, called Lake Kariba, actually juts right up into the very beginning, the start, of this gorge. Then

the valley spreads out and extends down to where it then comes together again at Kariba Gorge, which is where the dam is built. Then it opens out again below Kariba Dam and continues right on down, pretty much to the Mozambique border. To go over to the Southern Rhodesian side, I had to drive around to Victoria Falls, cross the Zambezi, then drive back, and then back down into the valley.

COHEN: I was going to ask you how you got across the river.

SCUDDER: There was another bridge below Kariba Gorge, which was the main route between Salisbury and Lusaka. But the Middle Zambezi Valley itself had just this one access route through it, and then there was a Land Rover track that went all along the river.

COHEN: How many miles are we talking about?

SCUDDER: Oh, we're talking about 150 miles. It's a long area. You don't go along at much more than ten to fifteen miles an hour, except in the rocky spots, where you're going much slower than that.

COHEN: So let's progress along this now. You continued this study for the eleven months.

SCUDDER: We continued the study for the eleven months. The fascinating part was on the other side. The Zambezi was not a barrier to the people. They used dugout canoes to go back and forth. One of the problems in contemporary Africa is that countries become independent with international boundaries, but the ethnic groups are on both sides. There was intermarriage. I would frequently go across to the Southern Rhodesian side, which was much more isolated. There were no roads, except for this little track along the river. For example, I took this hunter across.

Begin Tape 2, Side 2

SCUDDER: OK. The hunting techniques on the south bank—I don't want to give an idea of a traditional society, because all these societies are dynamic systems. That's one thing we're

learning, is that all societies are dynamic open-ended systems. Nonetheless, on the south bank they still hunted, for example, hippopotami by catching them in pits. You'd build a pit that was shaped like an hourglass, and then you'd put a big spear sticking up in it. You'd put it on the path where the hippos would come out. They'd come out and they'd fall into the pit and they'd be skewered. You'd come in the morning and there would be a hippopotamus. You'd do the same thing with elephants. The hunters would climb into trees above elephant paths with big spears, poisoned with stuff they called *bunete*, they would jab that into the back of the elephant and then they'd track the elephant and eventually it would die. That I didn't see. But I did find hippo pits.

All right. The fieldwork was for an eleven-month period. We did what we were supposed to do, which was a detailed ethnology of the Gwembe Tonga people prior to Kariba—an overall study of their sociocultural system. Elizabeth Colson then wrote a book, published in 1960, *The Social Organization of the Gwembe Tonga* [Manchester, U.K.: Manchester University Press]. I published my PhD dissertation, which was subsequently published in 1962 by Manchester University Press, *The Ecology of the Gwembe Tonga*. It dealt pretty much with the agricultural system, the production system—

COHEN: So you in a sense were doing research for your thesis all that year.

SCUDDER: That's right. That was in effect my PhD dissertation. So when the research was over, then it was back to Harvard to do all the things that—

COHEN: Take your courses.

SCUDDER: To take my courses, my orals, my generals, and my French and my German. But of course, because I had done fieldwork before my colleagues, I was the first in my class to graduate. To get a PhD in anthropology usually takes—in those days—six, seven, eight, nine, ten, or more years. I had started in '54 and received my PhD in June of 1960.

OK. What then? We had applied for traveling fellowships and postdoctoral fellowships. I got a traveling fellowship and a postdoctoral fellowship with the Social Science Research Council to go to the London School of Economics. On the same day—a wonderful day I can remember in great detail—Molly and I were told that if we could get our old vehicle to the

garage, they'd give us $10. If we couldn't get it to the garage, they would charge us $10 to tow it to the garage. Well, it died just as we got into the garage. Then the moving vans came over, and we took what furniture we had and stored it all in the basement of my godmother, who lived on Coolidge Hill. Then that night we got on the plane and we left for London.

COHEN: With two little girls.

SCUDDER: With two little girls. We had arranged in England to buy—because this was June, you see, so we'd have to start at the London School of Economics the following September—to buy a Bedford Dormobile. It has a roof like an accordion bellows, which goes up, and a stove, a sink, and four bunks. We picked that up and spent the entire summer traveling throughout England and Scotland and then over to North Ireland and Ireland. We took up residence in London in September, and that was kind of interesting, because we didn't have much money. I can't remember what an SSRC grant was in those days. I think it was something like $3,500.

A friend of ours who had continued in mountain climbing was a member of the Makalu expedition, which I think was one of the ten largest peaks in the Himalayas. He was on his way to Makalu, and he dropped his wife and young son off with us. We rented together the old royal gamekeeper's cottage at Hampton Court Palace. Hampton Court Palace is here [gesture], then there's the main road, there's Bushy Park over here [gesture], and looking onto Bushy Park is the royal gamekeeper's cottage, which is a semi-subterranean house. The only warm part of it would be in the basement, where you'd have just your coal fire—sort of Thackerayan circumstances. We would climb out our back window into the park, where the fallow deer, the roan deer, and the red deer were, and the children would be out with the deer. It was a lot of fun. It was very nice. I think our rent was something like £5 a month, or something quite incredible. That was a very interesting year, because the person who was to be my advisor, Isaac Schapera, was a very well-known anthropologist who had done his work mainly in Botswana. As soon as I arrived, I went to his office to pay my respects to him, and he looked at me and said, "I'm too busy to have anything to do with you."

COHEN: Nice welcome. [Laughter]

SCUDDER: So immediately I lose my advisor. What am I going to do? Well, probably one of the most distinguished anthropologists in the world—I think he's still alive—is an anthropologist by the name of Raymond Firth, who did a classic study in Tikopia, a small island in the Pacific. Firth found out that I was at loose ends. He invited me to a graduate seminar for students who had done their PhD research, had come back, and were in the process of writing it up. We had all these British social anthropologists who had been doing research around the world, and a couple of postdocs. There must have been about twelve or fifteen of us. That was the year that was, for me, very, very worthwhile.

COHEN: So you were with these people then.

SCUDDER: Yes. On weekends we would hop in our Bedford Dormobile and travel all around local places. The following summer, we asked ourselves, "OK, what do we do the next year?" because Fosbrooke had arranged for money for us to do the benchmark study in '56-'57 and then a re-study in '62-'63 with the people who were resettled. I should have mentioned that our study was just before they were resettled. The resettlement began at the end of 1957, after we had left, and it continued throughout 1958, so the people were all in their new areas pretty much by the end of 1958. The dam was sealed in December of 1958, so people pretty much had to be out of the area. It was arranged for us to go back in 1962-'63, again with money from Rhodesian Selection Trust.

The postdoc was over by June of '61, so there was the question of what we were going to do during that interim year. We had two job offers. One was to be a lecturer at the University of Khartoum, in Sudan. That's one that Elizabeth Colson favored, because a colleague of hers was at the university there and she thought that could be a very good experience. But some old friends of mine—I had mentioned Robert Fernea who, along with me, had gotten a Danforth Fellowship the year that they were taking people who were just interested in the scientific study of religion. Bob Fernea was now the director of what was called the Nubian Study, which was to be a study of 50,000 Egyptian Nubians before their relocation in connection with the Aswan High Dam. Well, of course I was much more interested in that, because that was like the Kariba study. It would give me a comparative basis. That job offer was an assistant professorship at the

American University in Cairo, where I would be teaching for the first six months and then would be researching the Nubian project the remainder of the year.

That was a fascinating year. We lived in Maadi, which is a suburb just outside of Cairo to the south. The children were with us, just as they had been in England. Molly very quickly got work establishing a day-care center, the children went to Cairo American College, and I started teaching. Then, right after New Year's, we began our work in Nubia.

I was working with an Egyptian colleague by the name of Abdul Hamid el-Zein, a wonderful chap who subsequently [1970] got his PhD from the University of Chicago and was rapidly becoming one of the leading figures in symbolic anthropology in the United States. He was a rival of Clifford Geertz, who was at that time considered sort of the main person in symbolic anthropology in the United States—along the lines of Claude Lévi-Strauss in France but from a strictly American perspective. Geertz had been at the University of Chicago. I think perhaps he had been a teacher of Zein there. I hadn't thought of that before, but that may have been the case. I can't remember whether Geertz had already left. [Geertz left the University of Chicago in 1970—ed.] He's now at the think tank at Princeton [the Institute for Advanced Study]. Zein was very rapidly rivaling Geertz—I think he would have probably surpassed him, actually—but, like so many Egyptians, he loved to eat, and he died of a heart attack in his forties. Anyhow, he and I were working together.

We had a motorboat that we picked up in Aswan. We used this motorboat to work between Aswan and Wadi Halfa, on the border with the Sudan, moving up and down the river, because our job was to do an economic-ecological survey of the Egyptian Nubian population prior to their resettlement in connection with the Aswan High Dam. That meant working with a sociologist by the name of Peter Geiser, and of course Fernea himself. It was a fairly big team, actually. There were probably as many as six people working together, a number of them living in different villages, because the Nubian population is fairly complicated, in three different linguistic communities. Those three communities were being studied anthropologically.

Geiser was studying Nubians in Cairo, because the Nubian population was really probably the first population in Africa where the majority was urban. With labor migration over the years, they had moved into the urban areas of Khartoum and Cairo. Geiser was studying them. He had picked his sample. He and I worked together picking the sample. We had traveled up the Nile in a felucca, a sailing boat, stopping at about every other what they call

omedia, chieftaincy, along the area, taking a sample. From that sample I picked four villages that had a very high labor-migration rate of 100 percent.

The interesting thing about the Nubian area is, of course, the Aswan Dam. The original dam was built in 1902, and Nubians were resettled. Then it was heightened in 1913 and again more Nubians were resettled, some of them for the second time. Then it was heightened again in 1933, and some of the same Nubians who had been resettled twice were resettled a third time. So each time the Aswan Dam was heightened, the reservoir expanded more into the desert, so that by the time we were there in the 1960s—1963—there were virtually no resources for the people. The local habitat was such that they were relocating on the edge of this huge reservoir but surrounded by desert. I had four villages where there was not a single male over the age of thirteen.

COHEN: They had all gone to Cairo?

SCUDDER: They had all gone to Cairo and Khartoum to make enough to survive. They were separated from their wives for almost a year. Maybe they might go back at Ramadan—it was a Muslim society. Then I had four villages which had the lowest labor-migration rate, because my job was to figure out why this high labor-migration rate. Even in those villages, approximately fifty percent of the men were absent. So it was a matter of studying the economies of these eight villages.

COHEN: In some sense, there was really no benchmark study of these people. All this migration had started some generations ago.

SCUDDER: Well, this shows the dynamics. I had said earlier that even in the case of— Notice I say "benchmark studies," not "baseline studies." Even in our Tonga case, it was not a baseline study, because that would imply that nothing happened before then.

COHEN: Benchmark means "at that moment"?

SCUDDER: At that point in time. Take the case of the Tonga people. You might say, "That's a traditional society." Well, several of their major crops—maize, tobacco—those are all new-world crops. What was their economy like before the Portuguese introduced those crops?

COHEN: So there are the two words: "baseline" and "benchmark."

SCUDDER: I don't use the word "baseline," because a baseline doesn't exist. We were doing a benchmark study, as the Nubians were at that particular point in time prior to the Aswan High Dam.

COHEN: So was the Aswan High Dam built where all these other dams had been?

SCUDDER: It was built immediately below.

COHEN: So the original set of dams is under water.

SCUDDER: I always get mixed up on that. Is it below or above? I'll have to check that and find out. I can't remember.

COHEN: If it was used for hydroelectric, it must have been submerged—

SCUDDER: The original Aswan Dam was below. We were there in '61-'62, and I don't think the High Dam was completed until about '66, '67, '68, something like that. [The Aswan High Dam was completed in 1970—ed.] We had nothing to do with the *dam*. We were studying the *people*!

COHEN: Right. [Laughter]

SCUDDER: OK. We finished up in September. We had our Dormorbile. We had driven to Egypt as much as we could—that is, from England we had gone to Scandinavia, traveled through Scandinavia, then come across through Germany, through Switzerland, down to Genoa, got on a boat in Genoa across to Alexandria, and had the Dormobile with us in Egypt. Then, at the end of

the year in Egypt, we put the Dormobile on a boat and sent it down the Red Sea to Mombasa in Kenya. Alas, we put the two girls on TWA and sent them back, because the Middle Zambezi Valley is not a very good place for small children. So we sent them back to go to a boarding school in Connecticut, where they were only about ten miles from where my parents lived, and my parents would pick them up on the weekends. Well, boarding school in England may be a fairly good environment, but boarding school in the United States for small children— You can imagine these Hollywood actresses and others who send their kids there—people who, in effect, want to get rid of their kids rather than have no choice. So that was not too pleasant an experience for either Lydee or Alice—I think for Alice in particular, being the youngest, because she was only nine at that time.

Then Molly and I flew down to Nairobi and then went on down to Mombasa. We picked up the Dormobile in Mombasa and drove overland through Tanzania and then on down into Northern Rhodesia for the re-study of the Tonga, which was another full year.

COHEN: So Molly was with you on that re-study.

SCUDDER: Yes. She was with me until about May.

COHEN: I see her name showing up on your publications at one point.

SCUDDER: Yes. She did some evaluations of various projects. She was doing her own research, too, especially among children—learning processes and things of that nature—and she was teaching English as a second language in the local primary school. She came back in May, when the children were getting out of school, and I came back the following September.

I don't think we have to spend too much time on the re-study. We found that involuntary relocation is about the worst thing you can do to people, short of killing them. Involuntarily you're taking them from a preferred habitat, and I think a cultural universal is probably love of natal environment. You're taking them from a known area, you're moving them into an area that was previously unoccupied for good reasons—not good soils, inadequate water supply, wild animals, a whole range of reasons. The 6,000 people who had been moved below the dam had been moved into an area which was already inhabited by a totally different ethnic group, speaking a totally different language—as different, for example, as between English and

German—related but not mutually intelligible. The Tonga spoke a central Bantu language, while the people in this area, the Goba, spoke Shona, a southern Bantu language.

COHEN: Now, let me just interject something here. This is being done by an Egyptian government, not a colonial government?

SCUDDER: That's right—the Aswan High Dam. But in Northern Rhodesia it's still colonial, you see. So we're going from independent Egypt; when we were there in '61-'62, Gamal Abdel Nasser was the president of Egypt.

COHEN: Would you say there was a difference in the attitude of forcing this on local people, whether it was colonial or native?

SCUDDER: No. In fact, we're finding around the world that, irrespective of culture, geography, or political system, people respond to forced relocation in the same way.

COHEN: It doesn't matter who makes them go.

SCUDDER: Right. I mean, love of environment, not wanting to be told that you have to move, especially for rural societies with a strong religious orientation toward the land, like the Navajo Indians, for example, or Native Americans in general. These rural societies all have a tremendous amount of indigenous knowledge—scientific knowledge, in effect—which is very well adapted to making a living for hundreds and, in some cases, thousands of years. We thought that the relocation had led to increased mortality rates—we didn't have the data to prove it statistically—in part because there's inadequate land. I mean, you're moving people from preferred lands—alluvial soils along the river—back into, in this case, Karoo sedimentary deposits, which have much less fertility and cannot support permanent agriculture, unlike the alluvial ones. People who used to live on a river with a fantastic amount of water are now moved back in the bush, and they're dependent on wells and boreholes that may break down. Governments tend to aggregate these people, for administrative reasons, into denser settlements. This means, granted the inadequate water supply, that diseases like dysentery and epidemic diseases like measles are much more serious. So we noticed increased mortality rates

immediately following resettlement. We knew that of 6,000 people who had been moved below the dam into an area called Lusitu, something like fifty-three women and children died of a mystery disease. Now, the local people thought it was sorcery. I have to explain a little bit. The area that these Goba lived in—the Shona speakers, the different ethnic group that lived there—had been hit probably by influenza in the 1920s, because it was very sparsely populated.

COHEN: It was a worldwide epidemic then.

SCUDDER: Yes. It was very sparsely populated. That's why there was land for moving the 6,000 people. The Tonga bury their dead right outside the house, so you had individual graves scattered all around the village. The Goba, who are the ethnic group below the dam, bury their dead in cemeteries. When the Tonga came in and found these cemeteries, they were terrified, because they felt all these people had died because of a type of witchcraft called *ngozi*, where somebody dies who is bewitched. This type of witchcraft is such that everybody who goes to that person's funeral dies and everybody who goes to the funerals of those who die, they die, and so on. So in effect it wipes out a population. They figured that this area was riddled with *ngozi* witchcraft, and so they were terrified, and they refused to move to the area. The British came down with mobile police. To make a long story short, misunderstandings arose, the Tonga spearmen attacked the police, and at least nine people were killed and perhaps thirty injured, and that, of course, broke the resistance. The Tonga were crushed. They were loaded into vehicles and moved and dumped down in this area.

This area was different geographically. We think that probably during the dry season the women were out gathering just prior to the rains and that the plants that they picked were like plants in the old habitat but happened to be poisonous, and they died of poisoning. The only people who were killed among these fifty-three who died of this mystery disease were women and children, no men older than the age of thirteen. The Tonga interpreted it in terms of witchcraft.

Now, I should mention that as far as this study is concerned, we had eventually picked four villages that we were going to follow. By coincidence, two of these villages were moved down below the dam to this area called Lusitu. So we were able to study what happened to these 6,000 people, which was the largest, most dense population that had been moved. A third village

had been resettled just inland from halfway up the reservoir, which is where everybody had been in the first place—all along the reservoir. The fourth village was at the far end. So in fact we did have a pretty wide distribution. Now, we had hoped that one of those villages was going to be our control—that is, a village that was not relocated, so that we could compare them. But unfortunately that village, when they decided to raise the dam, was flooded out, so we lost our control. This is one of the problems you have with anthropology. So now we have these four villages, and our methodology is to study in every household every marriage, birth, death, divorce, et cetera. Well, actually, one was a very big one and we took half of that, but for the other three we have a hundred percent of the households, so that we now have continuity from 1956 to the present.

We found out that the resettlement had very adverse effects on the people—not just increased mortality rates but there had been psychological shock, especially for the elderly and women, who were especially associated with the burial grounds of their lineages in the home villages and in their inherited fields. There was sociocultural stress—that is, a wide range of cultural institutions and cultural patterns that were relevant in the old habitat could not be transferred to the new habitat. One of the problems below the dam was that the local people—the indigenous people, the Goba—weren't particularly happy to have the Tonga coming into this area, because they would be using land that their children and grandchildren would want to use. They also had their own customs. For example, they didn't do drumming at funerals. They told the Tonga, "Look. You're coming into an area where ritually you have no control, and you've got to follow our rituals." This was part of what I call the truncation or the narrowing of the culture. A whole bunch of cultural content is no longer possible, and this is tied up with the sociocultural shock.

So we found these three things: physiological, psychological, and sociocultural stress. How do people respond to that? Well, we've found that they respond to that—and we've found this around the world—by behaving as if a sociocultural system is an equilibrium system. Not that it *is* an equilibrium system. Under these circumstances, they are changing no more than they have to. They obviously don't want to accept new innovations that the government is trying to superimpose on them in addition to all of the stress of moving into a new habitat with new neighbors, with new government, et cetera. They cling to the familiar as much as possible, which means trying to reestablish old habits, reestablishing homesteads with kin, for example.

We've found that homesteads that have broken up in the old place—just the natural fission, as brothers separate and move away or as witchcraft suspicions come in and families break up—those families rejoined in the new area, and then they only subsequently broke up maybe five or ten years later on. So we found this cultural involution, in effect, as people tried to reestablish in the new habitat old behavior patterns. Certain interesting rituals that had died out—for example, hunting rituals that had died out over the years—were reestablished. This comes a little bit later, though, so I will move on.

COHEN: Let's get you into Caltech.

SCUDDER: OK, we'll get me into Caltech. That will be good.

Colson wrote a book on the social consequences of resettlement, based on the 1962-'63 work, and that came out in 1971.[1] We have coauthored a number of books that have come out more recently, which I'll get to later on. Molly and I had been overseas for three years. That meant, as far as the hiring establishment is concerned back in the States, "Out of sight, out of mind."

COHEN: Right. I was going to say: "Who is that guy?"

SCUDDER: Yes, who is this character we're seeing? Of course I remembered what Kluckhohn had told me. I had also been advised that when I got my PhD I shouldn't spend three years overseas. Good heavens, I'd be left behind! All my colleagues would be associate professors by then. It was the old story that I was told: "Don't go to the field and do your fieldwork before you finish everything. And, of course, don't go back and get more knowledge before you jump in." One of the big weaknesses of anthropology, I might point out, is that anthropologists don't spend enough time in their laboratories; their laboratories are the field. Up until recently—say, twenty years ago—the pattern was pretty much that you would do your biggest piece of fieldwork for your PhD dissertation. Then, on the basis of that, you would start your academic career. You'd write it up as a book. Fine, that would promote you to associate professor. You might go back

[1] Elizabeth Colson, *Social Consequences of Resettlement: The Impact of the Kariba Resettlement upon the Gwembo Tonga* (Manchester, U.K.: Manchester University Press, 1971).

to revisit the people for a month or so, but to a large extent your whole career would be based on that original period of research.

In part through coincidence, I had three years of fieldwork before my first academic appointment in the United States. I had had the appointment at the American University in Cairo. When we went back to Northern Rhodesia in '62-'63, it was still colonial—pre-independence—so I was then a senior research officer still working for the Rhodes-Livingstone Institute. We found out that forced relocation is really a grim experience. I then ended up at Harvard as a research postdoc in the Center for Middle Eastern Studies to write up the work that I had done in Egypt. That was a routine year. But then along in, I guess, around April, a former colleague of mine at Boston University, Phil Gulliver—I mentioned his name; he was the other person, along with Elizabeth Colson, whom I had gotten to know there—was asked by the World Bank to join a team to make a study of African smallholder agriculture, the first pan-African study of what small African farmers do. He couldn't do it, so he recommended me. The World Bank interviewed me, and I was recruited to work with four economists and an agronomist to do this two-year study. It was a fascinating thing. We went over to Africa almost immediately. I spent the whole summer there studying the agricultural systems of three populations in Kenya—the Kikuyu, the Kamba, and some Kalenjin speakers over in Elgeyo-Marakwet District. Then we went and studied the Sikuma in Tanzania, in an area where an Israeli group was trying to establish a *moshav* [a cooperative community of farmers—ed.].

Meanwhile, of course, I was applying for jobs. My colleagues at the World Bank were going to continue on to West Africa to continue studying specific farming systems there, but I wanted to begin to settle down. I had been in a different place every year for four years. I had applied for about four different jobs. The two most interesting ones were at Caltech and MIT. I was offered an assistant professorship at MIT. They didn't have an anthropology department at that time, either, but they did have an existing macro political economy group with Max Millikan, Daniel Lerner, Lucian Pye, people of that nature. They wanted to bring me in, as a microsocial scientist, into this large political economy group. It was an interesting offer, but I knew very well I'd be—

COHEN: The low man on the totem pole.

SCUDDER: At the bottom of the totem pole, with a heavy teaching load expected. I was expected to teach Marxism, Marxist socialism, a whole range of courses like that, which would have taken a lot of time. Caltech offered $1,500 more, which was a— Anyhow, I came out to visit Caltech in April 1964. This was all set up before. Ah, yes—we're getting ahead of ourselves. The Caltech offer and the MIT offer had both been made while I was a postdoc fellow at Harvard, because while I was there, Cora Du Bois—remember, she had helped me with my oral examinations, my one-on-one person—was a main recommender of me. The fact that I had three years of fieldwork actually enabled me to leapfrog over my colleagues. I remember that when Caltech, or maybe it was MIT, asked if there were any people at Harvard suitable, J. Otis Brew, the head of the Peabody Museum, told them, "Well, we've got two people. We have so-and-so, who was just out and has done his fieldwork, but then we have Scudder. There is no comparison. He has already completed three years and in two different countries." So I got the job offer and the other person didn't. That was in April, and then I went to work for the World Bank in May, June, July, and August. Then Molly and I drove out with the children and our dog Maggie. Of course, we still had the Bedford Dormobile. At the end of '63—Molly had come back in May, I came back, I guess, in September—I drove the Dormobile down to Beira, in what was then Portuguese East Africa, and put it on a boat full of coffee beans. When we finally picked it up in Boston or New York—I can't remember where we picked it up—coffee beans came pouring out. The window hadn't been completely sealed. [Laughter]

We had the Dormobile in Cambridge during that year as a postdoc, and later that summer we bought a little VW Bug. Then, with the family, our Airedale, and two hamsters, we drove the two vehicles across the country, arriving at Caltech in September of 1964.

COHEN: OK. I think we'll stop there, now that we got you to Caltech.

[Tape is turned off]

THAYER SCUDDER

SESSION 3

January 3, 2001

Begin Tape 3, Side 1

COHEN: We have you now at Caltech, in 1964, and you arrived without roots.

SCUDDER: Right. Let me say a little about putting down our roots, because we didn't cover that before. I had come out the previous April, while the job hunt process was on—April of '64. At that time, I visited the schools, and I concluded that the children would probably be best off in private school. They were accepted at Poly [Polytechnic School], so when we arrived in September they were able to move right into school. So they settled in quickly, and that was good. Molly was able, before we came, to get a full-time job as a head teacher at Pacific Oaks College & Children's School. However—and I'm not quite sure why—we couldn't come out as early as Pacific Oaks needed her to start. So they had another person in as head teacher when she arrived, and she started working there as a teaching fellow. But then that head teacher was fired, I think about the following January—January of '65—so Molly stepped in and took the job. So she was able to settle right into her profession of early childhood education. Pacific Oaks at that time—it's unfortunately run downhill considerably as far as the early childhood side is concerned—was considered the leading West Coast educational experience and academic universe for early childhood education. It was the West Coast equivalent of the Nursery Training School in Boston, which subsequently joined with Tufts University as [part of] the Eliot-Pearson Department of Child Study. So she was very happy, of course, and she stayed with Pacific Oaks right up until 1989.

All right. I had negotiated what was and apparently remained the lightest teaching load in the [humanities] division, because I had looked very carefully at what the teaching load was in the other divisions. It was mainly a one-one-one kind of situation, whereas in the Division of the Humanities it was a two-one-one kind of thing. Throughout my time at Caltech I've always been very sensitive—we can talk about this later on—about, in effect, sometimes intended, sometimes not intended discrimination by Caltech as an institution about our division. At the time I came,

the division was considered pretty much a place for gentleman scholars who would teach—who would humanize and socialize—Caltech students. Caltech students at that time were pretty uniform. They were pretty much all of Anglo-Saxon background. Unfortunately, women hadn't been accepted yet and Asian students hadn't come in yet. So relatively speaking we had a pretty homogeneous group of students, and of course, as I'm sure other people have told you, they had been valedictorians of their class and that kind of thing. Frequently the myth was that they were not particularly socially astute and not particularly knowledgeable about the humanities. So that was the function, so to speak, of the division. The division chairman at that time was Hallett Smith, who was a distinguished Shakespearean scholar. I think we owe Hallett a lot, because it's Hallett who, I think, started the process of recruiting people in the division primarily because of their research expertise. I was certainly one of the first to come in under those circumstances, perhaps *the* first. I'm not quite sure of that historically. Lance Davis [Harkness Professor of Social Science, emeritus] was another one.

COHEN: But he didn't come until '68, so that was later.

SCUDDER: Yes, that was quite a bit later. Hallett had taken a course in anthropology when he was at the University of Colorado, under Robert Redfield, who was one of the more distinguished anthropologists of that generation, so he was interested in that. He had asked Ned [Edwin S.] Munger [professor of geography, d. 2010] to do the recruiting, and Ned Munger had then gotten in contact with Harvard University and that led to my eventual recruitment. So Ned had actually played the key recruiting role, but Hallett was the one in the background who was pulling the strings to try to bring in anthropology.

COHEN: He was in charge.

SCUDDER: He was in charge. The reason why I picked Caltech over MIT had nothing to do with teaching. I didn't come here to teach. I came to Caltech as a monastery in which I could do research and not be part of a large department, as would have been the case at MIT with a heavy teaching load. Therefore I was not weighed down with administration, and I was just pretty much able to sit in my office with nobody having a clue who I was or what I was doing. Nobody was going to bother me, because nobody else knew anything about anthropology. Not too

jokingly, I told Hallett at the very beginning, "Hallett, if you ever hire another anthropologist, I'll resign." I probably make that story more and more positive as time goes on, but I think—

COHEN: But you meant it.

SCUDDER: But I meant it. I came here because I saw Caltech as a university of science. My father-in-law, Philip Drinker, who, as I mentioned, was chairman of the Department of Industrial Hygiene at Harvard, was all for us moving out to Caltech rather than staying at MIT. He had a very high opinion of Caltech. He was a colleague of Arie Haagen-Smit, and the two of them had done quite a bit of work together. So we looked up Haagen-Smit actually fairly early in our coming. That brings up another interesting thing. I think during the first couple of years that Molly and I were here, we were viewed as mascots, trotted out during half-time, a chance for Caltech to show it had a broader intellectual appeal, so to speak, than would have otherwise been the case. We were adopted, interestingly enough, by the physicists. During the first couple of years, they invited us to go out into the desert with them. I can remember some very nice trips—not too many of them.

COHEN: Which physicists were these?

SCUDDER: The physicists were mainly people like [Fredrik] Zachariasen, [Murray] Gell-Mann, [Jerome] Pine, Ricardo Gomez, Robert Walker—those were the main ones. They invited us to their houses and brought us in contact with a wider range of physicists. I think they were interested in what made an anthropologist tick, what made the wife of an anthropologist tick, and that kind of thing. It was a very nice introduction. During the first couple of years that I was here, until I began to hibernate in my office and concentrate more and more on research, which we'll be talking about at a later time, I would frequently go over to the Athenaeum. The first ten years that I was at Caltech, I was very active, actually, in general affairs. There was a committee then that had been set up [in 1967] by Neal [Cornelius J.] Pings [then professor of chemical engineering & chemical physics] that was dealing with Caltech's future, called the Aims and Goals Committee.

COHEN: Was this the committee that met at the Honkers, the old restaurant there?

SCUDDER: No, no. That was before that. The Honkers business I considered completely ridiculous as an anthropologist. I mean, any kind of ideology like that just is not my cup of tea. I do have to mention something about that, though, because although I came here to do research, for some reason or other—maybe because I was a Danforth fellow—there was the assumption that all Danforth fellows miraculously are good teachers. The first year I was at Caltech, I got very strong—not offers, because I didn't let it get that far—but queries of interest, both from [University of California,] Santa Cruz and from Hampshire College, both of which were experimenting with new modes of teaching, as to whether I was interested in moving to Santa Cruz or to Hampshire. These were both during my first year at Caltech. Of course I wasn't interested in either. I came to Caltech because I had been trained primarily in biology as an undergraduate. I enjoyed the company of scientists more than the company of people in the humanities. I never really understood the humanities too much, and the social scientists, of course, were not particularly scientific in those days. So during the first few years we were here, our contact was primarily with physicists.

Very early I met Dick [Richard P.] Schuster, who became my best friend. Dick, at that time, was with the Industrial Associates. He subsequently became the Director of Development. Again, to show the fact that I was mainly in relationships with the scientists here, we had a group of hikers that consisted primarily of Dick and Jim [James F.] Bonner [professor of biology, d. 1996] as the two regulars, along with myself. At least once a month we would go hiking up in the San Gabriels. Robert Christy [Institute Professor of Theoretical Physics, emeritus] also was a member of that group. Occasionally, for example, we'd climb Mount Baldy in early spring when there was deep snow. So again, I was pretty active.

COHEN: I've actually gone on some of the memorial hikes that Cathy Schuster Border organized after he died.

SCUDDER: For example, the day he died [May 25, 1979], we were going to go hiking the next day. Catherine called me that evening to tell me about the crash. Also, Dick brought me into the Caltech tennis community, and we used to play tennis at least once a week with people like Jim [James J.] Morgan [Goldberger Professor of Environmental Engineering Science, emeritus] and Norm Davidson [Chandler Professor of Chemical Biology, d. 2002]. We were all sort of club

tennis players—not of the caliber that Harold Brown [Caltech president 1969-1977] was, for example—but we were good tennis players.

I was on the Faculty Board—as an assistant professor, or maybe as an associate professor—fairly early in my career. I think I got to know the institute quite well during those days.

COHEN: Now, [Lee A.] DuBridge was president?

SCUDDER: Yes. So that was sort of the settling-in process. Regarding teaching, I very quickly decided that I would only teach once a week, in the evenings. I would have a two-and-one-half-hour class, initially on Mondays, which would then leave the whole week open for research.

COHEN: This was in anthropology?

SCUDDER: Yes. I'll mention the courses in a moment. Then, because of conflicts with the glee club, the dean's office requested us not to have courses on Monday nights. So then I picked Tuesdays. During the first few years I was here, I taught a routine introductory course in anthropology, Anthropology 22, and I very quickly got bored with that. I decided that the logical thing to do would be to teach my research. We'll be talking more about that at another time. So Anthropology 22 became a course that looked at long-term studies of the world's low-income majority. When I say "anthropology" henceforward, I'm always talking about social anthropology. One reason why anthropology was not considered a science is that anthropologists doing their fieldwork as graduate students would spend anywhere from one to two years doing a good slug of fieldwork, but then frequently the rest of their career would be based on writing that up as they moved up through the academic ladder. There were a few long-term studies, and one of those, of course, was Elizabeth Colson's and mine, where we had been in the field in '56, '57, '62, and '63. We'll talk about this later on, but it's important to mention it for the course. Around '64, when Zambia got its independence, we decided it would be very interesting to convert our study, which had been just a re-study of how people responded to resettlement, to become a systematic long-term study to assess not just the ongoing impacts of the forced removal on the people but also the impacts of independence. The country became independent in 1964. I had gone back, for example, a couple of times later on in the sixties. I

thought, "OK, I'll emphasize this particular society." Then there were two long-term studies that had been carried out in Mexico, another that had been done in India, another that had been done in France, and another that had been done in China. So the students had to read 150 pages a week out of these particular books. I tried to visit as many of the study sites as possible. I think I visited about half of them, including both of the Mexican ones and the China one.

Anthropology 22 in effect was social anthropology trying to become a science through long-term studies. At the same time, it gave the students a good feel for cultural diversity, because in Mexico these were indigenous populations who had been incorporated within a Catholic ideology. Then our study was people whose belief system was based primarily on ancestor worship. The Chinese study involved Taoism and Confucianism. The Indian study was Hindus. Another long-term study that I taught, because I had been working there myself, was the long-term study of the Egyptian Nubians, which was a Muslim society. One of the goals of anthropology is to give students an idea of the range of human experience—not just variations but also cultural universals. I would pull out in this course: "This is a variation, but isn't it interesting that certain things, like the nuclear family, religion, and so on, are cultural universals? Why is that?" And so on. So that was the initial course of Anthropology 22, and you had to take 22 to take 123.

Anthropology 123 then was, "OK, what's happening to these non-Western societies as a result of globalization?" See, 22 sort of dealt with them as they had been studied in the twenties, thirties, forties, fifties. Then, since they were long-term studies, 123 asked, "What's the situation today?" I think the students found that very interesting, because they were dealing with the contemporary world.

I have to say at this point that I probably was one of— I don't think there are more than three or four Caltech faculty members—and I'm including lecturers here, because they should be included—who have received the ASCIT [Associated Students of the California Institute of Technology] teaching award three times. Now, I'm not sure whether the students gave me the teaching award because I was frequently absent.

COHEN: [Laughter] It gave them more free time.

SCUDDER: I'd head off to the field, you see, and of course there was nobody to teach my courses when I was gone. Then I would come back and I would have two classes in a week. But I think one reason why they liked the course—and bear in mind that for Caltech students to stay awake for two hours and a half in the evening— The courses were all oversubscribed—well, not the course in the spring; we'll talk about that later on. I put a limit in the registrar's office of thirty-five students. As I say, I wasn't that interested in teaching, unlike Alan Sweezy [professor of economics, 1950-1977]. Alan Sweezy, in his courses in demography, would have seventy to a hundred students. I wanted my course to be a good discussion course, and I found that thirty-five was about the maximum where we could have the students actively involved. Of course there would always be some seniors who would come in weeping because they needed some more social science credits. They might not be particularly interested in anthropology, but they had to have the social science credits. I tried to discourage them. The students, fortunately, in all the evaluations of the course—although they said the course was good; they liked it—they always complained about too much reading, that it was a hard course from that point of view.

COHEN: Did they have to write also, Ted?

SCUDDER: No. I didn't require it. I know that the people in humanities want the students to write term papers. I didn't see that my job was to teach Caltech students how to write. My job was to show them what an anthropologist does, what anthropology is, and that it is a research science. All of these other functions that some of my colleagues feel so strongly about on the humanities side—not on the social sciences side; social sciences are very different in this regard—people like Peter Fay [professor of history, d. 2004] feel that it's a very important part of their function to make sure the students can write properly. I think that's excellent, but that wasn't my job.

So, Anthropology 123 required Anthropology 22 [and it] dealt with the impact of the increasing globalization of the world on these previously non-literate societies. I think one reason why the students liked the course, other than me being absent fairly often [laughter], was that during the two-and-a-half-hour period I would show about a hundred slides. In dealing with most of these societies, five of them at least—the long-term studies—I had actually been there. With two of them, I had actually been involved as a principal researcher. They were getting the

equivalent that they would be getting in biology working in a laboratory, or that they would be getting in physics working with people in their laboratory. They were seeing science as it was being done; and I think this was important. So that was Anthropology 123 in the winter.

Then in the spring, I tried an experiment. I was a consultant, I think in the early seventies, for the Navajo tribe that was involved in a very nasty U.S. government-induced relocation situation with the Hopi Indians, the Navajo-Hopi land dispute. Unfortunately, it was highly politicized, with politicians in Arizona making it look like it was one Indian tribe against another, whereas in fact the whole thing had been induced pretty much by the federal government. A result of this was a very tragic thing: the largest forced relocation of rural American citizens since the Japanese war relocation. The Navajo had brought me in as a consultant to put together a research team to research the impacts of forced relocation because of my experience with forced relocation. I thought it would be interesting to teach a course on the Navajo, but not a routine course on the Navajo. There was a limit of twenty students, but it was such an odd course that frequently there'd only be, say, fifteen or so. We divided the class into two sections. Each section had the responsibility, competing with the other section, to come up with a ten-year development program for the Navajo nation that had to make sense culturally. That is, it couldn't be non-culturally relevant for the Navajo, but, on the other hand, it had to bear in mind that Navajo were getting PhDs, there were Navajo lawyers, and that the Navajo were no longer a homogeneous population. It had to focus both on the Navajo living on the reservation—160,000 people—and therefore the limited resource base of the reservation, and what were they going to do to create jobs, enterprise development, and employment, and so on. It also had to deal with the Navajo diaspora, primarily in places like Denver and Los Angeles. This enabled them to do some fieldwork.

COHEN: Did they actually go out there?

SCUDDER: Each year one or two students actually did go, during the spring holidays. I would announce the course during the winter when I was teaching Anthropology 123. I'd say, "Well, this is what anthropology is going to be in the spring." And usually one or two students would get very interested. Some of the more recent students—like, for example, Michael Clemens, who is just getting his PhD now in economics at Harvard—were frequently students who decided

that they wanted to go on in social sciences. So, yes, a few would go out there, usually with a video camera, and they would be a resource person for their group. I think the students liked this very much, because, first of all, they had to decide whom they represent. Are they a bunch of Caltech students, are they a bunch of Navajo students, or are they an outside consultancy panel hired by the Navajo to come up with a development plan? Then they would have to assign jobs so that one person would be looking at economics, another at politics, another at the resource base, and what have you. They would in effect be operating like a little, small society. That was one of the purposes of the thing. It wasn't just to deal with the Navajo. I wanted them to see whether or not they could organize themselves as a small group to carry out a collective task. And by golly, there were very few slackers. Why? Because they really bugged the students who weren't contributing, because they knew that that would make them look a little foolish. Then at the end of the course they would have to give oral reports on what they had found, integrated into a development program. So at the end we would always have a fairly lengthy couple of sessions that would usually go on for about four hours, while they'd do this. Then they would be rewarded by coming back up to our house for a turkey dinner and so on.

So that's how my initial routine courses in anthropology very quickly evolved into courses which reflected my own interests and my own research, but which I thought would be relevant to the interests of Caltech students. I would say that the majority of the students taking anthropology were taking the course because they were interested. But there was always a significant minority who were taking it because they didn't want to take any social science but they had to have so many social science units, and so they figured, "Well, anthropology probably is the lesser of the various other evils." I had a few students like that. I learned very quickly that the only way to deal with them was to require the course to be graded. Otherwise, if it was pass/fail, they would try to squeak by with the minimum amount of work. I don't like failing people any more than I would want to fire people, so I tried very quickly to figure out ways that I could make sure that they would all pass, but pass legitimately. The way to do that was to require grades. That also, of course, cut down on a few of the students who would be taking it for reasons that didn't relate to the course. I suspect that maybe anywhere from two to five students a year got sufficiently interested that it influenced their intellectual development. For maybe one student every couple of years, it actually influenced their career, like Mike Clemens, for example.

That, of course, is one of the main liabilities at Caltech for a person like myself, being primarily interested in research—no graduate students, and knowing that not just the large majority but, say, over ninety percent of the students are taking the course because they have to take it. So, what is the best way to not get too isolated at Caltech? One way was the continuing research with Elizabeth Colson.

COHEN: She must have had plenty of students from Berkeley.

SCUDDER: She had plenty of students at Berkeley, and some of those students we actually brought in to do the research. Jonathan Habarad joined our research team for a while in the eighties.

In the middle seventies, in 1976, I got together with two colleagues. David Brokensha was a professor of anthropology at UC Santa Barbara, and Michael Horowitz was a professor of anthropology at State University of New York, Binghamton. The three of us set up a nonprofit research and educational organization called the Institute for Development Anthropology. Neither Brokensha nor I were interested in administration, whereas Michael Horowitz was very happy to be the executive director of the institute, which, although separate from, had an adjunct relationship with SUNY, Binghamton. It was based in Binghamton. IDA, as we called it—Institute for Development Anthropology—became very successful to a large extent, more than any other similar research institution in the United States. I think we pioneered social soundness research in the UN and USAID [United States Agency for International Development], because at that particular time there were the new directions at the World Bank. USAID was interested in what was called "development from below." Trickle-down theory was being pretty much rejected as a development approach. There was more and more interest in—not so much local participation but at least finding out about indigenous knowledge, working with local people, and things of that nature. Who are the experts in that area? Anthropologists.

COHEN: Now, where were you getting your money?

SCUDDER: We were getting up to $1 million a year from our main source of funds, which was USAID. Our biggest grant from them was something like $800,000 for carrying out a four-year study of the impact of a major dam on the Senegal River. In other words, the Institute for Development Anthropology actually gave me the chance to apply much of my research results

by working with development organizations. Our next major source of funding was the World Bank. We also got money from the United Nations Development Program, UNDP. I would say that those were probably our three—

COHEN: Now, how did this work with Caltech? I mean, you were a full-time employee here.

SCUDDER: Yes. As you know, quite a few Caltech scientists have their own companies, and profit-making companies, I might say.

COHEN: Yes. [Laughter]

SCUDDER: Our company was nonprofit. In fact, it wasn't a company. As far as the IRS is concerned, it was a research organization. Caltech allows all faculty members to consult one day a week. A gray area is: Is that a five-day week or is it a seven-day week? We never asked the question, "Can you consult three days a week or can you consult one day a week?" We won't discuss that anymore.

COHEN: I see. OK.

SCUDDER: One's weekend is one's own, so to speak. One is not teaching on the weekend. That consulting policy was worked out primarily for Caltech faculty who, like earthquake engineers, consult in California. You're not allowed, for example, to consult for extended periods of time. Well, you can't go over to Africa one day a week and come back, or go over to Asia for one day a week. So there again, the institute [Caltech] was really amazing. During the time I've been here, never once did they refuse any request for anything, whether it was a request for conferences, whether it was a request for leaves of absence, either with or without salary, depending on the circumstances. In this particular case, it was a request to be put on a nine-month salary basis, from September through the end of the academic year. Being put on a nine-month salary basis but being paid over twelve months meant that I had the summers to do as I wished, which was primarily research, but also—

COHEN: For this other company—I mean for your institute.

SCUDDER: It also could be part of my fifty-two days, and I tried very hard to keep it to fifty-two days. But, as I say, there was always this nice little gray area of whether it's fifty-two days—but what about the weekends, you see? Remember, coming from a Connecticut Yankee background, the Puritan ethic unfortunately has stuck to me.

COHEN: OK. [Laughter]

SCUDDER: [Laughter] So I do take regulations and things like income tax seriously—for example, all of my consulting money pretty much now comes from foreign governments. I get paid by the government of Lesotho right now—where I was at the end of November. I'm going to Laos two days from now. I'll be paid by the government of Laos. The IRS wouldn't know anything about that, but I always declare all of that kind of thing. Certainly I tried, to the extent possible, to follow the fifty-two-day requirement.

COHEN: So how did this give you students to work with?

SCUDDER: Oh. Well, we would get these contracts, and then we would hire people to do the work, and we've been very, very successful. I can't remember offhand how many countries—because I was thinking of preparing more carefully for this for the next time—but we've certainly worked in over forty countries. We had a society of fellows, a group of fellows. This brought me in contact with well over a hundred anthropologists working around the world on our contracts. Then, of course, a number of them were graduate students at Binghamton, and I became external examiner on their dissertations. Maybe every two years I would have one or two students who would have me either officially as external examiner or unofficially. They would find out, "OK, this is Scudder, the leading expert in this particular field. Why don't I contact him and ask him if he'd be willing to read my dissertation?" At the moment, for example, I have one student at UC Irvine who is working closely with me and is doing her research in Lesotho on a dam development project. I suspect that I'll be on her dissertation board. Another student in Australia has just approached me to be perhaps on her board, because she's studying the Three Gorges project in China. So through the Institute for Development Anthropology, our long-term study, and being an external examiner, I developed an ideal system, because in most anthropology departments, students will come to you and you'll have to accept

so many, but many of their topics will not be directly in your area. I suppose I was fairly selfish in this regard, because all the work through the Institute for Development Anthropology, all the work with Elizabeth Colson, and all the work with these various students—one from the Sudan, one from Sri Lanka—all were in my own area of expertise.

COHEN: So what you've done, Ted, in some sense, is eliminated all of the headache part, but what you've been willing to give up is recognition on your own home territory.

SCUDDER: That's correct. But now, that's the interesting thing. I suppose we'll talk about that later on. I assumed that giving up recognition in my home territory would mean that I would also be giving up recognition nationally as well as internationally in my field. But that hasn't happened. I think the reason why it hasn't happened is that the arrangement I've had at Caltech has allowed me to operate internationally and concentrate on particular fields of activity—which we'll be talking about later on—on which my peers consider me to be the leading expert in the world. So, much to my surprise, when the American Anthropological Association, which is our professional association— I was overseas, and I got a cable saying that I had been the first person to receive their Kimball Award [Solon T. Kimball Award for Public and Applied Anthropology]. That was a new award that they had just started.

COHEN: Now, that's recent, isn't it?

SCUDDER: No. That was about ten years ago—fifteen years ago, maybe [1984—ed.]. I was the first person to receive it. I assumed that it was for Elizabeth Colson and me, the two of us, because it was the first award that the association had come up with in the area of anthropology and public policy. I subsequently found out, no, it was just for me. I was very surprised; how did they know that I existed? There are only really four awards in the broader field, and I received all of them—the only anthropologist who's received all of them, including one from the Royal Anthropological Institute of Great Britain and Ireland. So, you know—

COHEN: It's been OK.

SCUDDER: It's been OK. Nobody at Caltech knows what I do, and I don't think anybody gives a damn, and that's fine. I think you have to be a lone wolf to not be bothered by that kind of situation. One of the tragedies, of course, was that I had developed these very close relationships with Dick Schuster and with James Bonner. Dick died; James was getting older and so no longer was hiking. Bob Christy, when he became provost [1970], pretty much stopped his hiking activities and got remarried. My main tennis-playing companion was Robert Huttenback [professor of history 1966-1977]. He went to [UC] Santa Barbara. Alan Sweezy was a close colleague. That means in recent years I have become more and more isolated. Peter Fay tries to pull me out of my office occasionally. But I would say that I'm in my office every day and probably only once a month do I eat away from my desk, because it's so difficult to keep up with your field. From that point of view, I think we've become more and more isolated. But the Institute for Development Anthropology still exists; I'm still on the boards of these various advisory committees for graduate students; and of course my relationship with Elizabeth Colson and my long-term research continue. Now, getting back to Caltech—

COHEN: I was going to ask you about the development of the whole social sciences program that you must have been very involved in.

SCUDDER: Yes. I may have to give you some more information on that later on. I think the biggest tragedy was the need to shift from Hallett Smith to the next division chair. Hallett stayed on too long. I always have felt tremendously grateful to him, because I think he started the process, the results of which we now see. But there was a strong feeling—there were a bunch of young Turks of which I, unfortunately, was one—who felt it was time for Hallett to go and for a new chairman to come in. David Smith [professor of literature, d. 1990] was very active in that. I won't say we were plotters, but I think we did hasten the day when Robert Huttenback took over. So that was a fairly active period. You'll know the exact dates of when Bob took over and Hallett stepped aside. [In 1971, Huttenback became acting chairman of the Division of the Humanities and Social Sciences; he was chairman 1972-1977.—ed.]

As far as the building of the social sciences program was concerned, there were a number of activities going on. One, of course, was Harrison Brown, coming from geochemistry into the division [professor of geochemistry, joint appointment in the Division of the Humanities and

Social Sciences as professor of science and government 1967-1977]. Harrison, as you know, had been the foreign secretary of the National Academy of Sciences [1962-1974]. He was interested in my work. He was instrumental, for example, in my becoming a consultant to the National Research Council. He was also responsible for me going on a trip with him and various other NAS members over to Ghana and Nigeria in the late 1960s. But Harrison Brown was not in the mainstream of the thinking of the division at that particular time. There were, rather, two forces. One was Dan [Daniel J.] Kevles's [Koepfli Professor of the Humanities, emeritus] interest in science and public policy, which is now taking fruit. But the main force at that particular time was a small group—Lance Davis, Burt [Burton H.] Klein [professor of economics, d. 2010], Roger Noll [professor of economics 1965-1984], Fred [Frederick B.] Thompson [professor of applied philosophy & computer science, emeritus] and myself—the five of us pretty much thinking about what would make sense at Caltech. Lance was without question the primary leader of that.

Begin Tape 3, Side 2

SCUDDER: He may deny it, but Lance was the builder of the existing social sciences program.

COHEN: That existed at that time.

SCUDDER: Well, that exists today. Without Lance's activities, I don't think we would have the program that we have at this particular moment. But Roger also was very, very active. There was somewhat of a difference between the two of them. Roger was much more interested in the public-policy aspects, as opposed to the more theoretical emphasis, although the microeconomic focus was there as far as both of them were concerned. Working with his wife, Bozena Henisz-Dostert, Fred was primarily developing a new very format-free language system called REL, Rapidly Extensible Language system. Fred was primarily interested in the information-science aspects. Burt Klein was a loose cannon—a very intelligent, very invigorating, very infuriating colleague. Burt was primarily, at that particular time, interested in what was happening in big businesses in switching from a hierarchical organization to a matrix organization and creativity within big businesses. I wanted the program to relate to the real world, but I very quickly realized that that was not going to be possible, that the program was going to end up as a highly

theoretical program, highly mathematical, highly analytical. That was going to be fine, because that made sense at Caltech.

COHEN: Now, was it before Harold Brown that this was happening?

SCUDDER: If you're going to ask me about the presidents of Caltech, I probably can tell you less about them than anybody.

COHEN: OK. Then I won't ask you.

SCUDDER: I can't tell you anything about them, because I had no contact with them.

COHEN: That says something also.

SCUDDER: I think that's more my responsibility, perhaps, than their responsibility, because as the years have gone by I've become more and more absorbed in my own research activities.

COHEN: What had your relationship been with the provost? If you weren't going to them for help, then you wouldn't be talking to them.

SCUDDER: Well, you see, that's one of the blessings of a long-term study. Elizabeth Colson and I have never had any difficulty in raising funds.

COHEN: So you would not have gone to these people.

SCUDDER: I've not had to go to them, other than requesting a leave of absence, with or without pay, depending on the circumstances. That's the only time I've had to go, and that's primarily the responsibility of my own division chairman.

COHEN: OK. So let's go back to what you're talking about.

SCUDDER: All right. So then Roger left to go to the Brookings Institution for three years [1970-73], and I think that pretty much tipped the balance toward the kind of program we have now. Lance was the main stimulus, as far as—

COHEN: Now this was a very theoretical program.

SCUDDER: A *very* theoretical program. We had brought in a number of—[economist] Ken [Kenneth D.] Frederick, for example. This is when we were in Spalding [Laboratory of Engineering]. I remember we were on the top floor of Spalding—Ken Frederick and I were up there prior to the building of Baxter [Donald Baxter Hall of the Humanities and Social Sciences]. Ken now is a very prominent person working with natural-resource issues for Resources for the Future. It became very clear to me that it was a waste of my time to try to push this kind of thing at Caltech. Rather, what Lance was interested in made sense, and so I became a very strong supporter. I think David Elliot [professor of history, d. 2007] did, too. I'll have to look at the notes and maybe make some corrective stuff later on, because I'm a bit of a pack rat and I've kept all of these committee reports. But I believe that David Elliot and I played a fairly major role in getting the money from the Carnegie Foundation, which played an important role in the recruitment of some of the first people: [Economists Charles] Plott, [David] Grether, James Quirk—those three. I think that it is important to tease out, if one does the history of the rise of the social sciences group, the role that David Elliot played, because I think he was the one person on the humanities side who played an important role in fund-raising.

Anyhow, funds were raised. The program began to move. I think once Charlie Plott came in [1971] , and Dave Grether [1970]—and of course Dave subsequently became division chairman [1982-1992]—then there was no more role for me to play. I don't have the training in mathematics that they do. Intellectually, symbolically, I understand what they're doing, and I think it's very important; I mean, they have an incredibly powerful set of methodologies for dealing with a relatively narrow range of problems. It's of no use whatsoever for my problems in the Middle Zambezi Valley or my global problems. Nonetheless, they are trying to expand the range that these very powerful methodologies can be applied to, and somebody's got to do it, and Caltech is the logical place to do it in. Right up to the end, even though people like Harrison [Brown] and Bob [Robert W.] Oliver [professor of economics, d. 1990] were unhappy that the

program had developed this way— Because we're in a teaching program, you see. Very few undergraduates at Caltech take a BS in economics; there may be a couple a year. But this didn't bother me, because, well— We were talking about the teaching role early on; this was a program for graduate students. Your daughter [Linda R. Cohen, professor of economics and law at UC Irvine] is an example of how successful the program has been. I think there are probably very few programs in the United States in any field, sciences included, that have been able to place, if not all of their students, most of them, or the large majority of them, in the kind of jobs that that large majority wanted: primarily tenure-track positions in universities. I mean, it's an incredibly successful program, and I think that's what we wanted.

Now, why did we want that? This gets back to my only complaint about Caltech. I can remember in the sixties, when I first came here, there were people who will remain unnamed who said, "Oh, you're an anthropologist. Oh, that's interesting. Why don't you study who eats with whom in the Athenaeum and who plays tennis with whom," and things of that kind. That was sort of the impression—the Mickey Mouse kind of research. On another occasion—this was fairly recently—somebody felt that I didn't really have any research commitments and they would like to teach a course in the division which would use chemistry and geology in relation to archaeology. Of course, not being in the division, they couldn't offer the course, but wouldn't I be happy to put in *x* number of hours a week to sponsor the course and to bone up on it and co-teach it? This is the kind of thing that they would never ask of a fellow geologist or a fellow physicist or a fellow chemist, but they're quite happy to ask it of somebody in the Division of the Humanities and Social Sciences.

COHEN: It's patronizing.

SCUDDER: Patronizing, yes. Initially they were very patronizing back in the sixties and seventies. Now, I don't think they're intentionally patronizing. But nonetheless, at the gut level, it's still patronizing without being aware of it.

COHEN: That's what they don't understand—what anthropologists do.

SCUDDER: Well, I'm not just talking about anthropologists. No, I'm not talking about my research primarily, although these examples are related to it. No, I'm talking about the

relationship of the institute to the Division of the Humanities and Social Sciences. That's what I'm saying is patronizing. In the case of my own research, they don't know what I do, and I've been pretty much isolated for the last twenty years anyhow. But I am seeing this in terms of the— Well, take the Feynman teaching award [Feynman Prize for Excellence in Teaching]. The reputation of our division was its teachers. That was meant to be primarily what we did. We haven't gotten worse over the years. Yet if you look at the people who receive the Feynman teaching award, not one person in the Division of the Humanities and Social Sciences has received that award, even though there are people in that division who have received the students' teaching award just as much as anybody else within the institute. So that's the kind of thing which I think is still continuing, although it gets less and less as time goes on and as the social sciences program gets more and more successful. Hopefully Dan Kevles's program in science and public policy will be able to develop in time the kind of reputation that is slowly developing—hopefully, although without graduate students it probably will not be able to do that.

COHEN: When you say you've become much more isolated these past twenty years, in what sense? You have continued to teach, because you have to teach.

SCUDDER: Yes. When I say much more isolated, I think mainly because the colleagues with whom I was closest outside my own area of interest are either dead or have left the institute, and I haven't replaced them. This has been really of my own making. Also, as my reputation has grown—you always have a pack of bright young scientists nipping at your heels who want to pass you. I'm not willing for that to happen.

COHEN: OK. [Laughter]

SCUDDER: Remember, I came to Caltech to do research. We have three retirement options. That's another topic that I feel very strongly about. Now that you can no longer fire people based on age, I think that puts universities in a difficult position, because you've got to maintain the balance between non-tenured faculty and tenured faculty. I really feel very strongly that senior faculty have an obligation to retire by seventy, and I think that those who are staying on well beyond seventy—

COHEN: Are selfish?

SCUDDER: Well, no. I won't give motivations to them, because in some cases they're needed, and badly needed. It has nothing to do with their effectiveness. If we take a place like Boalt [Hall School of Law, UC Berkeley], for example, where in effect the entire faculty is pretty much retired professors of law from other universities, or at least they're older people. So, no. I just think that in aggregate one has to keep in mind the balance. If the balance is beginning to get too skewed toward elderly faculty, the issue has to be dealt with. In my own case, I wanted to spend more and more time on research and less and less time on teaching, even though I was only teaching one day a week. It's interesting; it was the same thing with my father. He was a teacher of English at Swarthmore, but he finally got fed up with teaching and left Swarthmore entirely. He set up his Center for Information on America. I was ready to leave teaching, probably by the time I was in my mid-sixties. When these various options—scholarly leave option was one of them, which meant that you could stop teaching at sixty-eight, remain a full-time faculty member for the next two years, in effect on a scholarly leave, although I was here at Caltech most of the time, and working on research a hundred percent of the time. That came just at the right time for me, because I was appointed a commissioner on the World Commission on Dams in 1998. So during those two years I had been traveling all around the world, working almost full time on the activities of the World Commission on Dams, which I wouldn't have been able to do if I had been teaching these last two years. So it was very nice. It came along at just the right time.

COHEN: Yes. We should talk about this in your research segment. So we've done the teaching, we've done the establishment of the social sciences department and the bent that it had. We can spend this session on your research.

SCUDDER: I think that the division has been very lucky with its chairs. As I say, I think it would have been good if Hallett Smith had left earlier, but I think Hallett Smith deserves the credit for starting the ball rolling to make our division the equivalent of the other divisions at the institute, where all people hired are first-rate scholars. Hallett got that ball rolling. Robert Huttenback, I think, was a good chairman. Rod [Rodman W.] Paul [Harkness Professor of History, d. 1987]

was a good chairman for the division. I think Roger Noll, Dave Grether, and now John Ledyard [Davis Professor of Economics and Social Sciences] have been very favorable.

I think one thing which was fairly insurmountable—I had mentioned, for example, that my first year here I had gotten nibbles from Hampshire College and UC Santa Cruz. The second year I was here, I got an actual tenured job offer at the University of Illinois to take over their Big Ten program in comparative cultures. There was an anthropologist by the name of Joe [Joseph B.] Casagrande who had been the head of this program [Center for International Comparative Studies] at the University of Illinois, which brought together the Big Ten universities in a study of comparative cultures. They thought that I would be a good person to replace him, and so my second year at Caltech I was brought out to Illinois, Urbana-Champaign, twice—once in the fall football season, in the glory of the time. It was an interesting possibility. But I also asked them, "Well, look. Bring me out in the winter, too." So Molly and I went out there, I can't remember whether it was December or November, but it was colder than hell. [Laughter] But that helped my career at Caltech, because they offered me a tenured appointment [1966]. So I was only an assistant professor for less than two years here. That relates to historical accidents, being in the right place at the right time, so to speak, because you'll recall that the chairman of my [thesis] committee at Harvard, Clyde Kluckhohn, had felt that I was doing everything wrong. He thought that going overseas for three years would be a mistake, but in actual fact that accelerated my career. Then at Caltech, another thing that I think I pioneered—after I had been an associate professor for three years, I concluded I had to be the activist here, because nobody knows what I do. I think around the third year I decided, "Well, I should be appointed professor." I remember going into Hallett Smith's office and saying, "Hallett, it's time you promoted me." I remember that took him by surprise. He said, "What?" Prior to that time, I think most promotions in the division were internally approved. We didn't do what other divisions did. We didn't go out and seek letters from the leading scholars in that person's field. That was done in my case.

COHEN: They sought letters for you?

SCUDDER: Yes. I mean, I believe I told them to.

COHEN: I think they do now automatically.

SCUDDER: Oh, they do automatically, but, as I say, I was the first person, I think, for whom that was done. I can't remember whether it was two years or three years I had been an associate professor. [Dr. Scudder became a full professor in 1969—ed.] Hallett said, "Be patient, young man." I said, "No, Hallett. The time has come." Whether he said, "Well, OK, we'll have to go out and get letters," or whether I said, "Go out and get letters"—because I remember I gave him a list of various people. As I had expected, with a little bit of conceit, they came back and said, "What? You haven't appointed him already?" A distinguished professor of anthropology at—two people at Berkeley wrote letters; another person at the University of Colorado. I think there were something like four or five letters. Again, you'll have to check that, but I think that was the first time the division went outside.

COHEN: Really, the idea was to get people who were well known in their field.

SCUDDER: Again, this is the program that Hallett started, and so I would like to think that it was Hallett's idea: "Well, we'll go out and get letters." I think that was probably part of Hallett's moving the division from being gentleman scholars doing a good job teaching, socializing, and humanizing a bunch of asocial students.

COHEN: Now, that would be an interesting question to ask you here. You say that when you first started, everything was homogeneous.

SCUDDER: Yes.

COHEN: When did the change happen? Of course, girls came in the early seventies.

SCUDDER: Well, I'm guessing now, but I would say that one of the first changes was an increase of students from Eastern Europe. Because of political problems in the Middle East, students were coming in from Iran, students were coming in from Lebanon. I noticed this in my classes, that some of my more interesting students were coming from Eastern Europe and the Middle East. Then, I guess the next big change was the acceptance of women [1970]. I usually had in my classes more women proportionate to their percentage in the institute—I think in part for the

same reason why more women in proportion to their percentage in the institute are in biology. Now, whether that attracted certain males to come take the course, I don't know. [Laughter]

COHEN: [Laughter] You're referring, then, to the less mathematical aspects.

SCUDDER: Yes. That certainly was a very positive development. Then, even more positive in terms of the diversity in the class was the Asian influx, because that brought in both women and men. Almost always, of my ten brightest students, several of them would be Asians, and I keep in fairly close contact with a number of them. For example, Francis Chong just visited Molly and me this last summer with his new wife. He was a Rhodes scholar at Oxford and was a Chinese speaker from Singapore. He's now in the ministry of home affairs in Singapore. So I think Eastern European and Mediterranean—Levant—then women, and then Asian students have created a very different student body at Caltech today than there was when I first came. There's no question about that—all for the positive.

COHEN: Well, maybe we should stop here, and we'll have another session on your research over these past years.

SCUDDER: Yes. And I'll do a little bit of preparation on that, so that we can flow—not get too much detail to flow.

COHEN: Yes. OK.

[Tape is turned off]

THAYER SCUDDER

SESSION 4

February 20, 2001

Begin Tape 4, Side 1

COHEN: We were going to continue with the story of your life and your research.

SCUDDER: Thank you, Shirley. Well, on the research, for the last forty-five years, my research was pretty much on the impacts of very large—indeed, the world's largest—river-basin development projects on the majority of people who live in those river basins and are therefore affected, impacted upon, by these projects. So I'm including big dams, and, believe it or not, there are over 45,000 large dams in the world. Large dams are defined as any dam over 15 meters in height. Large-scale irrigation projects, large-scale canal projects, as in California, for example, for inter-basin transfers of water and so on. The majority of people who are impacted upon by these projects tend to be low-income people. I haven't been researching the effects, for example, of water transfers to cities. That could be very hard to study economically. Who benefits exactly and how, for example, from the water from the Colorado River, from the water from the Central Valley, from the water from Owens Valley? Who benefits exactly from that economically in Los Angeles? That would be a very difficult situation. On the other hand, if you build a dam like Hoover Dam, and you have to resettle a couple thousand people in the future reservoir basin—that you can study in a quasi-laboratory situation, because you can do a benchmark study before those people move, then you can do follow-up studies, and do a longitudinal study over the next thirty years. So you're dealing with several generations, and you can see how those people are impacted upon and how they respond to those impacts. Then, of course, you can do a similar study with another dam in a totally different context geographically, culturally, politically—like in China, for example, and then another one in Africa—and you can come up with a comparative, what my colleague David Brokensha called, back in the 1960s, the sociology of resettlement. The sociology of resettlement actually has become a major field in social science, because it's one of the few cases where you can do experiments—other than experimental economics at Caltech—with large living populations.

You can bring about a huge permutation in their life. That is, you're forcing them to move. All of these relocations around the world are involuntary, and people don't want to leave. So you're finding out how they lived in a context before a dam, then you're studying what I call the resettlement process, and you see how they respond to that process. It's sort of a quasi-laboratory situation.

COHEN: Now, how about the government's decision to do these things? Is that a different field?

SCUDDER: No. We'll get to that. That's where the policy aspects come in. This research has been going on for forty-five years around the world, mainly in the tropics and subtropics, on the impacts of dams on people being resettled in China, in Sri Lanka, in India, in Africa, in the Middle East, in Canada, and so on. My main output in regard to this research, and this is my main intellectual contribution, too, is to have formulated a theory which actually predicts how people, a majority in what I call involuntary community relocation—how you can expect the majority to respond in a successful relocation process. Now, notice the emphasis on a *successful* resettlement process; this theory only applies to a successful resettlement process. That brings in governments and policy, which we'll talk about later. Success has to include the definition of success by the people themselves. It's very easy for politicians to say, "This project's been a success." It's fairly easy for researchers to say, "Oh, well, this has been a fairly successful resettlement," because even I, having studied this for forty-five years, really cannot comprehend the impact of being forced to move from a preferred habitat. When you think about the impact psychologically, physiologically, culturally, and sociologically, it is hard to imagine that, especially if you're dealing with populations, as I have, which tend to be Native American or indigenous populations in Asia or ethnic populations in Africa—people who are minorities and, to a certain extent, have been outside the globalization which is currently going on in the world. It's not a small field, as we will see. Believe it or not, with those 45,000 large dams, over 40 million—not thousand—40 million people have been forcibly relocated, and the figure is probably even over 80 million. Unfortunately, governments have not taken resettlement very seriously, and therefore we don't even have accurate figures on the numbers. Often if you look at a particular dam project, they'll tell you all the engineering statistics on the dam, the volume of the reservoir, and so on. Ironically, it's very inadequate and very rare that they'll tell you

exactly how many people were resettled, and yet these are supposedly development projects. So let me talk then a little bit about the theory.

COHEN: Development for whom, you might say.

SCUDDER: Yes. Development for whom? Exactly.

When Colson and I first thought of our research—and I am going to go back a little bit before coming to Caltech—in the fifties, which was the first time that I had come in contact with a resettlement project—and it was a benchmark, pre-resettlement—we didn't really understand what people's responses would be. One reason why they were favorably inclined toward us, even though we represented the government that was going to resettle them, was denial. That is, the majority deals with uncertainty by, in effect, saying, "It can't happen to me." We have study after study, whether it's urban redevelopment in Brazil or what have you, where people hear a rumor: "Oh, you may be moved." Their response will be, "But of course the government's not going to do that, because the government's inefficient," or "Well, they can't do that, that's too big a thing." Since frequently these rumors begin to circulate ten, fifteen, twenty years—maybe forty years in China—before people are actually moved, if they started worrying about it forty years before the move, you can imagine the stress that that would create. So denial is a stress-handling aspect. We then went back in the sixties. I came to Caltech in 1964. By the end of the sixties, I had had a chance to look at resettlement in connection with probably the three largest reservoirs—what we call man-made reservoirs; we could call them "people-made" reservoirs—in the world. This was the Kariba project, which had, to give you an idea, four times the storage capacity of Lake Mead, or 130-million acre-feet of storage.

COHEN: Now, what country is this?

SCUDDER: This is in Zambia and Zimbabwe. Then there was the Aswan High Dam in Egypt, which had something like 120-million acre-feet of storage—again, about four times the storage capacity of Lake Mead. The Volta Dam in Ghana, which had roughly a similar size.

COHEN: Now, that's interesting. Of the three dams, I somehow know all about the Aswan, but these other two—

SCUDDER: Yes. I think one reason why you know about the Aswan is that it got very quickly involved in the Arab-Israeli confrontations. People like Claire Sterling, for example, wrote articles about it in the sixties. In Israel in the last couple of weeks, [Avigdor] Lieberman has actually said, "Oh, we should bomb the Aswan High Dam," or "Perhaps we will bomb the Aswan."

COHEN: Who's Lieberman?

SCUDDER: Lieberman is a member of the Yisrael Beiteinu Party.

COHEN: I see. He's a politician.

SCUDDER: He's a politician and a person considerably to the right of [Ariel] Sharon.

COHEN: OK. [Laughter]

SCUDDER: So that tells you quite a bit about him. But the Aswan High Dam also was in the news at the time, because it was going to be built with a World Bank loan. The U.S. government and [Secretary of State John Foster] Dulles were involved—a very high-level thing, because, as you know, Egypt and Israel are our two largest foreign aid recipients. The building of this dam was going to be a big U.S. involvement. Then came the nationalization of the Suez Canal. [Nationalization of the Suez Canal came after, not before, U.S. pulled out funds for the dam.—ed.] Dulles got very angry about that and subsequently said it was probably the biggest mistake he ever made in foreign policy. The United States pulled out, the Russians immediately jumped in, and it was Russian aid that was involved in building this huge and very successful dam. It's probably the most successful resettlement project of a large dam anywhere in the tropics. We'll get to that later on. Although in the long run, it may do more harm than good. But that's another issue.

As a result of having studied these three dams and the relocation in connection with them, in the late sixties I became aware of what I call the multidimensional stress of involuntary relocation for the majority. I hypothesized that this stress increased mortality rates, especially among the elderly. There's not too much research on that, simply because the necessary public

health surveys were not carried out before removal, so we just don't have the statistics. We found a number of epidemics, new diseases, seemingly high mortality among children from dysentery—ironically, because though these people live on rivers and then are moved because of huge reservoirs, the governments provide inadequate water supplies for the areas to which they're moved. At the same time, to increase administration, they have a tendency to aggregate the population in a denser area. So you have a denser population in an area with poor water quality and quantity. The result is water-borne diseases, dysenteries, and increased problems with sanitation, parasite loads, and what have you. Often an irrigation project comes in, so you have increased malaria and schistosomiasis. As a result of these epidemics of measles among children and what appeared to be a higher death rate than what was there before, I hypothesized increased mortality and morbidity. That was what I called physiological stress.

Then there was psychological stress. This has been very well studied with urban redevelopment in the United States. Grieving for a lost home—for example, a classic study in Boston in an Italian-American community, where this poor community moved out of what the developers called a slum but the people saw as a community, reaching out into the streets, where they would all meet in the evenings.

COHEN: There's an article this week on this block of Harlem they're trying to redo. That's exactly—

SCUDDER: Exactly the same kind of thing. Morton Fried wrote very good books on this, which he calls the grieving-for-a-lost-home syndrome. I always found that characteristic in all these resettlement projects. We also found an anxiety about the future, especially among women—because, remember, we're dealing with ethnic populations where frequently the women have never traveled beyond, let's say, a twenty- or thirty-mile radius. If they have traveled farther, it was just maybe once or twice, so there is very little knowledge of the outside world. Many of these societies, as with Native Americans in the United States, have a very strong tie to the land. You're forcing these primarily rural populations to leave an area where land has been inherited, where their religious sites have a very strong symbolism in the culture, which they have to leave, and can't be transferred. So, psychological stress. These had all been mentioned in previous research.

What had not been mentioned—and what was, I think, my contribution—is what I called sociocultural stress. I found, for example, as you would expect, that quite a few behavioral patterns, especially as they're related to agricultural production, are tied to the land, are tied to the area in which you live. If you then have to move, they're just not suitable elsewhere. Very important symbolic aspects of any people's culture may be lost. Again, we're dealing with ethnic minorities, often, in many cases. Why is that? Because these dams tend to be built in an area where you're going to have good storage capacity. It means you're often in the mountains. If it's going to be a hydroelectric dam, you're going to have to have the drop for the generation of electricity. So often these dams are built in what have been refuge areas, where minorities have gone and have lived all along or been pushed by the majority population.

COHEN: That's an interesting thing. So usually people affected are a minority among the bigger culture they're in?

SCUDDER: More often than not, they are. Because if you think about dams in the United States—the Garrison Dam in the Dakotas, that was Native Americans. The Kinzua Dam in New York and Pennsylvania—that was Seneca Iroquois. All the Hydro-Québec dams in Quebec, dams that have been serving not just Quebec but the northeast of the United States—that's Cree country. Ontario Dams—by golly, that's Cree on the other side of Hudson Bay. Dams in British Columbia for Alcoa, a big aluminum processor—those are also in Native American areas and of course along the Columbia River. The Native Americans who lost their fishing because of the construction of these dams were a whole bunch of Northwest coast tribes. It's the same thing in Africa. It's the same thing in much of Asia. For example, in India the estimate is that approximately sixty percent of the over 20- to 30-million Indians who have been relocated are tribal people who make up only a very small proportion of the total Indian population. So it gives you a feel. In Africa, of course, almost every population is an ethnic minority. So of course you're dealing there not necessarily with indigenous populations but you're nonetheless dealing with tribal populations at that time. So, often customs that relate to birth, puberty, marriage, funerals, are very distinctive.

When people are relocated, they are usually relocated into problem environments. That is, where in the world is good land unoccupied? These people were living in, in effect, what are

heartlands, and rivers tend to be heartlands of countries. Usually they were cultivating the fertile alluvial soils with high productivity. That all gets inundated, and they're either moved back into the bush, on less fertile lands, or they're just sort of sandwiched somewhere else, and there's inevitably a host population. The host population often belongs to a different ethnic group or has a different culture, and they tend to look down— I mean, obviously, they're losing resources. Resources that they were going to, in effect, give to their children and grandchildren are now being taken up by these people who are coming in to be resettled. We're dealing with large numbers. Aswan High Dam, 100,000 were relocated; the Volta Dam, 83,000; the Kariba Dam, 57,000. OK, you move these into host areas and you can see that that increases the competition for scarce resources and jobs. So, again, we can forecast—at least in part because of the theory—increased stress because of conflict with the host population, which, again, contributes to this stress psychologically, physiologically, and socio-culturally. They lose a fair number of behavioral patterns of symbolic importance, certain religious rituals that are tied to the land—tied to particular shrines that can't be transferred. They're lost. All of this contributes to what I call sociocultural stress.

Also, at the very time that they need strong leadership, in many cases their leaders lose their authority and their influence. Why? If they're against the relocation and fight it and then they're removed, obviously they show that they're not very effective leaders. If, on the other hand, they're pro-resettlement, since the majority consider resettlement undesirable and move involuntarily, they lose their credibility. So it's sort of a *Catch-22* situation. The main exceptions are where leaders before the resettlement do fight to improve the planning, the opportunities, the deal they're going to get. Resettlement occurs, and then they play a major role in making sure they get that opportunity. Now, this is something that we find is happening more often when the population is more educated—for example, the Iroquois in New York and Pennsylvania. They hired law firms to sue the government in the courts. They got Arthur Morgan, who had been one of the commissioners with the Tennessee Valley Authority, to come in and represent their interest. Some of their leaders did not lose their authority, because they were fairly effective in improving the benefits that they got afterward.

Every population, almost without exception, does things that are illegal or quasi-legal. It may be poaching. It may be smuggling. It may be the illicit making of alcoholic beverages. It may be the growing of narcotics. Well, resettlement, of course, brings them into a closer, more

heavily government-administrated situation, and so these things have dropped out. This is what I mean by sociocultural stress. In effect, there's a reduction in cultural inventory, and since it includes a number of things that are very important to people, that increases the stress. So I came up with the concept of multidimensional stress. Of course, all these things are synergistically interrelated.

Then, in the late sixties and in the seventies, I returned with Colson to the Middle Zambezi Valley, and I found a very interesting thing. This is the most interesting part of the theory. I found that in the period immediately after resettlement, the majority was risk-averse. Now, that makes sense, because, with all of the stress, you're not going to change in major ways at that time, because that will increase risk-taking, that will increase uncertainty, that will increase the stress load. So I found that after resettlement the majority— And remember, we're talking about the majority; with human people, you're always dealing with exceptional individuals. Part of the theory is that, notwithstanding what I'll be mentioning in the next few minutes, there are always individuals who can take advantage of any situation, no matter how bad, and improve their standard of living. But we're finding that the majority presumably has a reaction to the stress. I'm talking about the period that is never shorter than one year and usually anywhere from two to ten years. They behave as if the sociocultural system were a closed system. They behave as if it were an equilibrium system, and so they change no more than is necessary, to try to re-create that system in the new environment. They, in effect, change no more than they have to. We find very interesting situations where brothers, for example, who had split apart before resettlement come together and rebuild together. People resettle with kin. They want to move the shortest distance geographically as well as the shortest distance sociologically to deal with this stress. OK. Remember, we're only talking about successful cases now. I found that all of a sudden—it happens very rapidly; within about a one-year period—the majority shifts from being risk-averse to becoming risk-takers.

Now at this point I suddenly realized—and this is in the early seventies—that we're dealing with what I call now the resettlement process. This is when I realized that success has to be defined in terms of at least two generations. So we're talking about a twenty- to thirty-year period. Success has to be defined by the people themselves as well as by the researcher. It has to be sustainable environmentally, economically, socio-culturally, and institutionally. All right. So the first stage, obviously, is the pre-dam stage, the planning and recruitment stage. The

second stage is the adaptation and coping in the new habitat, characterized by multidimensional stress. The third stage then is a stage of risk-taking, which I call community and economic development. The fourth stage—I'll mention a little bit more about the third and fourth in a minute—is the handing over of this community building and economic development to the next generation that is able then to perpetuate it.

OK. We're only talking about successful cases: When the people come to feel at home in the new environment, they begin to realize—and this is a psychological aspect, hard to measure—"Gee, all right. Most of us did survive. We've adapted to a pretty rough situation." They have also regained their former self-sufficiency in food. That's absolutely essential. They've got to be back on their feet.

COHEN: They have to be comfortable economically.

SCUDDER: Yes, they've got to be comfortable economically. Then, when that happens, a potential for the third stage of rapid community formation and economic development happens. I've got a whole bunch of indicators of this. This is quite fascinating. During this stage of coping—what I call stage two, the risk-averse stage—you find that people are concentrating on reestablishing their own household. They're not worried too much about community aspects. But when you begin to come to the end of this stage, then, they begin to develop community organizations. The first, almost inevitably, is a funeral society.

COHEN: Death should be more important than education.

SCUDDER: Well, we'll get to education in a moment. They develop a funeral society, and then a whole range of new organizations. A key indicator is the reestablishment of religious edifices and the recruitment of people to be in them. I've got examples of this happening in Buddhist societies, where a Buddhist temple is built. I have examples in Hindu, Muslim, and Christian cases. When they begin to build a church, or when they begin to build a mosque, or when they begin to build a temple, then you know that the period of community formation has started and is getting under way.

So there are a whole bunch of indicators. Another indicator, too, is they begin to refer to the new habitat in song, dance, poetry, and things of this nature. Again, these are indicators of

coming to feel at home. Now, how do you measure that? Well, I measure it in terms of institutional and cultural responses—building new temples, poetry, the renaming of animals, birds, and plants in the new habitat—putting a cultural stamp on that. In some cases we found even a cultural renaissance, where rituals that had been dropped before relocation, before the people even knew there was going to be a dam, are reintroduced. People who were not taking their religion very seriously before resettlement begin to have household shrines. I found this in Sulawesi in Indonesia among Buddhists and Hindus. I found it in Sri Lanka with little Buddhist shrines on the wall in the houses of people who hadn't had that kind of thing before. So there are all of these indicators that begin to show, "OK, culturally we're beginning to feel at home, and we're ready to begin community building."

Now, what about the economics? As soon as people have regained their self-sufficiency, then we find a series of strategies. The first is education of the children. Now, that's very interesting, because it means that they're willing to forgo the labor of those children in agriculture, or in taking care of the goats and the sheep. In other words, they're feeling secure enough that they're thinking of the future, and so they're educating the children. Then we had some very interesting cases where those educated children then diversify the production system of the family, usually by moving into non-farm operations. Then we find, at the same time this is going on, they're diversifying their household production system. They're moving from concentration on food crops—growing rice or sorghum or wheat or maize—to higher-risk cash crops or livestock management. Then the next step is moving into a range of non-farm occupations, but actually on the homestead. The wife, for example, gets a sewing machine and begins to do tailoring—school uniforms for children, selling dresses to her neighbors. The husband takes time to develop skills or reestablishes his old skills—carpentry, masonry, and things of this nature. They begin to improve their house and rent out rooms to government officials or laborers. Of course, now the children are going to school, so they often begin to hire laborers. The hired labor then rents the house or rents a room in the house. So we have a series of strategies, moving from agriculture to a mixed-development situation, from farm to non-farm. Then the more successful minority begins using their capital: They may buy a tractor, which they then rent out, or a taxi, or a small vehicle like a minibus, which is used as a transporter. Then we find that even more successful ones begin to open businesses in the nearby commercial centers. Eventually the most successful either leave the area entirely—which can have negative

impacts, although it often allows new leaders to arrive—and move to the capital and establish businesses there. But usually the children or certain dependents will do that. They've diversified their production system. So, you see, in this third stage of community formation and economic development, we have a rather amazing and very similar set of strategies—

COHEN: Wherever you are—in Africa or Asia or China.

SCUDDER: It doesn't matter. Now, that's the next thing. What do they do with the increasing income, other than sending children to school and buying livestock? If you look at the furnishings that they buy and put in their houses, they're identical, whether it's Africa or Asia or the Middle East or the United States. I can walk into a house and within five minutes I can tell whether or not these people are suffering in stage two or whether they've moved into stage three. By stage three, what they'll have is, for example, a dining room table and chairs and a big stuffed sofa with a set of stuffed chairs around it. There will be a glassed-in cabinet, like this [gesture]—

COHEN: Here in the Archives Library.

SCUDDER: Inevitably that glassed-in cabinet comes very quickly. Why? Because it shows off the things that the family is proud of. So in it will be, inevitably—it may be a tea set or a coffee set. It will also include certain children's toys, and it will include bric-a-brac that they've picked up on pilgrimages, or things of this nature. If you look at the wall, inevitably there'll be a wall clock on the wall. There will be calendars all over the place, because they can't afford paintings and pictures, but there will be photographs of family members around the edge. There'll be a radio-cassette player. If they're some of the more wealthy ones, they'll have a battery-operated TV, and so on. Now, of course, these things that they buy—and this becomes very important from the national development point of view—the things that they're buying are not things that are imported from Japan or the United States and what have you; they are things that tend to be made domestically. In other words, what we're saying here is that the rising disposable income of the majority of people in a settlement like this, if you can have that throughout the whole nation, becomes the engine which drives development in the early stages of industrialization.

Then the final stage, of course, is the handing-over and incorporation stage. What "handing over" means is that frequently a special project authority is responsible for implementing the resettlement—a government agency, usually. That agency has to hand over the authority to, on the one hand, settler organizations—municipal councils, district councils, road councils, and what have you—and also to the area's government ministries. When you build a big dam, always the dam builders are responsible for putting in a big clinic and hospital. If you're going to have 6,000 people building a dam—or 40,000 in China—you've got to provide medical services. OK. Who takes over those medical services once the construction is done? If the government doesn't have the capacity, if the ministry of health does not have the capacity to take over, then the health in the area is going to deteriorate. So handing over is not just handing over political authority to the settlers, it's also being able to hand over to the public works department the maintenance of roads, to the agricultural department research and extension, to the educational department the running of the schools, and things of that nature. So that's the handing-over aspect—to the second generation.

COHEN: Now, these people then haven't moved very far. Do these people work on building the dam?

SCUDDER: No. They very rarely do, because, again, in most cases they don't have the skills. For example, in China the people who were building Three Gorges [Dam] were the 40,000 who previously were building the Gezhouba Dam. There's a tendency for there to be—just like people working on the railroad—gangs of workers. There were teams of Iroquois Indians and others who just moved into high-rise steel work, and they are going to be specialists. The other reason, of course, is that during the relocation process, at the time of the construction these people have enough on their hands. They have to prepare to move. Actually their income goes down at the time. Why? Governments frequently tell the people, "Well, we're going to build a dam here. Therefore, put your life on hold. Don't clear new fields. Don't start new businesses." People who have jobs in the city may come home to help their relatives and things of this nature. So we find that the income level actually drops immediately after moving, in most cases, which, again, contributes to the stress.

Now, getting back to the handing over. It's a two-phase kind of thing, both to the people—the settlers—and to the normal government agencies. The incorporation means that the area has to successfully incorporate within the economy of the encompassing region. These people have to be able to compete at least on a level with the non-relocated areas for scarce government resources for development, and things of that nature. So that's the fourth stage.

Now, just as we don't have an accurate figure on how many people are alive who have been forcibly relocated, the large majority of resettlement projects are failures.

COHEN: So just a small minority is a success.

SCUDDER: A very small minority. But this is where the policy implications come in, because we find out that the problem doesn't lie with the people. We find that because of stage three, yes, you can't expect them to change overnight. You can't expect to move them, change them, and make them beneficiaries. But if three things happen, you can plan for a successful project. The three things are the following: The people have to come to feel at home and they've got to feel secure in their new environment—point one. Point two: Interestingly enough, the reduction in cultural patterns that I mentioned in stage two, which are stressful—often their patterns tend to inhibit behavior. For example, you may have a situation where the land is controlled by an elite, or natural resources are controlled by an elite, or it's a theocracy of some sort. Relocation shakes up the system. So we find that actually this reduction of cultural inventory, once the people come to feel at home, can in fact enable a larger number of people who previously did not have access to resources and opportunities to move forward. Then the third, most important, factor is that the government, or the relocation authority, has to provide opportunities. You have to plan the resettlement not just in terms of new housing, which tends to be fairly easy, as is improving schools and clinics. The most important aspect is the economic planning. You've got to provide new opportunities that are economically sustainable. If these things are done, I'm convinced you can have a much larger number of successful resettlement projects.

This is when I get so angry—when I'm dealing with the policy. This is inexcusable. It's unnecessary, it's inexcusable, and it's not in the economic interest of the governments, because if these people become dependents, moving into slums, increasing the population there—and this is what's happening in India; they end up moving into Calcutta or Bombay—it can increase the

problems there. If, on the other hand, you provide these opportunities, yes, it costs a little bit more in the beginning, but it will be beneficial in the end, because these people are then becoming beneficiaries, are raising their standard of living. We know that this can happen, and we know that the people are not the problem. Then they contribute to the stream of project benefits, rather than contributing to the cost. So it's a win-win situation. But the large majority of cases continue to be failures, because resettlement is complicated. These people tend to be minorities. They tend to be poor. They don't have much political clout. The planners are primarily interested in building the dam, the benefits of which tend to go to a different set of people. Electricity goes to the cities. Irrigation tends to, as in the California Central Valley, go to agribusinesses. The local people may be the laborers, but they don't get the benefits.

This theory has not been sufficiently tested, because there are not that many success stories. But, although it hasn't been tested in dam-induced relocation, interestingly enough it has been used by a number of scholars dealing with other kinds of community settlement. A historian by the name of [Karen Ordahl] Kupperman has published a very interesting book using it to explain the stages of development among British colonists in the seventeenth century.

COHEN: Now, this historian calls it your theory?

SCUDDER: Oh, yes. She says, "Scudder's theory" blah, blah, blah. She has used it to explain Puritan settlement in Massachusetts and settlement in Virginia, in Providence Island, which is off the coast of Nicaragua, and so on. Another scholar, Della McMillan, is studying a successful voluntary government-sponsored settlement scheme in West Africa, where they had a disease-control program in river basins and, after the program was completed, people moved in. She finds that the four stages accurately predict the responses of people there. Another scholar, Anthony Oliver-Smith, who's the leading scholar of how people respond to disasters—earthquakes and what have you—has found that it's quite useful in analyzing responses of communities to earthquakes. So it now has been used in a number of PhD dissertations. I get e-mail quite frequently from students who are saying—well, for example, it's being used in a PhD dissertation dealing with government-induced refugee settlement in Mozambique, and so on.

There are two theories now relating to involuntary resettlement. There's this one. Then at the World Bank, the senior sociologist Michael Cernea, coming from Romania, has come up

with a complementary theory dealing with the unsuccessful case, where people are impoverished and their living standard has dropped. What he's done, being a World Bank person, is he's identified eight different types of impoverishment—loss of home, loss of job, loss of culture, mortality increases, and so on. Then, as a planner, he has looked at ways to avoid—again, as my theory has—the impoverishment and make these people beneficiaries. He is focusing on the failures. I am focusing on how you can make a success, while he is focusing on dealing with these impoverishment risks. Those are two theories that have dominated the field and complement each other.

COHEN: Do you talk to Cernea?

SCUDDER: Oh, yes. He's a very close friend. We've had lots of great fights, wonderful fights. I can remember one time we were standing in the rain in Wuhan, which is a multimillion-person city in China, on the banks of the Yangtze River. We were screaming at each other. Why? Because we had given a joint seminar, and Michael, who has a tendency to talk even more than I do, had taken up something like four-fifths of the allotted time. I told him that never again, when we gave a joint seminar, could he go first. [Laughter] Then, on another occasion, when we were in Chongqing together, we had a wonderful evening in the market, where we bought a just-cooked duck and pulled it apart. One half to Michael and one half to me, and we sat there eating this thing, juice streaming down our jackets. He's an infuriating person. He's one of the most difficult people I've ever dealt with. But he's a very close friend, and right now we're working together. So that's been the main outline of the research.

The research has also had three other outcomes. By the late sixties, I began to realize that this research was evolving toward how people respond, how the majority responds, and how the minority takes advantage and improves itself. But it had policy implications, and so then I did get very interested in how to apply what we had learned to actual policy. That is when the third person who has had the biggest impact on my career came into the picture. This is Gilbert White. He's a geographer and he's done a lot of work and is very active in river-basin activities. At the age of thirty-four he became the president of Haverford College. Then he was a professor of geography at the University of Chicago. He then moved to the University of Colorado, where he founded an institute for behavioral sciences [Institute of Behavioral Science at the University

of Colorado, Boulder—ed.]. He's still going strong. He's now in his nineties and living in Boulder. Very early on, he was a member of the National Academy of Sciences, based, to a large extent, on his research as a geographer on floodplains and why there is so much damage to floodplains because of people moving onto them, building dams and things of this nature. Anyhow, at that time Gilbert White was the senior advisor to the United Nations Development Programme on river basins. He was instrumental in bringing me in as a consultant to the UNDP. That led to consultancies with the UNDP and other UN agencies on the policy aspects of resettlement—how to plan a process of resettlement which these UN agencies were involved in. It was all pretty much on an individual-project basis, because none of these UN agencies actually had their own resettlement theory or resettlement policies. So I was working with them implementing my own theory.

Aside from individual dam projects, this initial policy work did not have any broad-based impact on how major donors like the World Bank related to resettlement. That has been, I would say, Cernea's major contribution. When he came to the World Bank in the 1970s, he was responsible for drafting the bank's pioneering guidelines on how to deal with involuntary resettlement issues. He came as the bank's first full-time sociologist/anthropologist. He was a specialist on co-ops. The World Bank was interested in co-ops. Very quickly he had a seminar series to try to educate people in the bank—the economists and the engineers—about the role of sociology. I was invited to give a seminar on my relocation theory. My talk was the first comparative analysis of the extent to which World Bank–financed dams were causing impoverishment among the majority being relocated. [Tape is turned off]

Begin Tape 4, Side 2

SCUDDER: So I gave this talk in the latter part of the 1970s. Cernea got very interested in it, asked me to provide him more case material, and asked me to recommend various people. I put him in touch with David Butcher, who was working for FAO [Food and Agriculture Organization of the United Nations]. A good scholar, a PhD in anthropology who worked in West Africa, David Butcher had written a resettlement manual for FAO in 1971. Cernea recruited Butcher through the bank and then began to recruit various other sociologists. In 1980 he finalized what the bank calls an Operational Directive for Involuntary Resettlement in

connection with bank-financed projects. Cernea is the first to admit that the policy, to a large extent, is based on the research by Colson and myself, and a few other scholars like Hussein Fahim, who worked on the Aswan High Dam, Robert Chambers, who did good work on the Volta project, and also, of course, Butcher himself. So the World Bank became the first organization that had *internal* resettlement policies. Then the bank—Cernea, again—influenced all the OECD countries to begin—

COHEN: OECD?

SCUDDER: Good heavens! The OECD—I always forget what it stands for. The Organisation for Economic Co-operation and Development has thirty member countries, including the United States, England, France, Japan. Cernea got OECD to come up with their policy. Then there was another scholar, Martin ter Woort, an economist working for Acres International, a big Canadian engineering firm. Martin has played a major role around the world in being instrumental in developing national resettlement policies. He's done it in Uganda, Sri Lanka, Vietnam, and Nepal. So now countries are beginning to have policies. So all of this research which the World Bank has done, also using my theory, is now having a major policy impact. I can say, "Hey! This research has actually benefited, and is benefiting, millions of people around the world."

Then there is a second aspect of my research. Two of the villages Colson and I have been studying over the last forty-five years were relocated below the dam because of the lack of land above the dam. It was at that point that I began to realize that more people living below the dam are being adversely affected because of dams than actually are being relocated. In other words, we have over 40 million people who have been relocated, but well over 100 million people live downstream from these dams, and they are being adversely affected. Why? Because usually when a dam is built, they're not what they call run-of-the-river installations. They're impoundment installations. They're creating a reservoir. The normal regime of a river in most cases is a flood regime. You find that in the tropics and subtropics especially you've got millions of people whose economy is dependent on that flood.

COHEN: That's right. I remember reading the Egypt thing with the onions.

SCUDDER: Yes. When the flood goes down, people plant the fertile alluvial soils. I mean the whole development—early Egyptian pharaonic society was to a large extent based on the natural flood of the Nile. That's where they developed their astronomy, to predict when it would come. It would flood into the Faiyum, then the water would recede and you would get that new silt—100 million tons per annum used to come into Egypt from the Sudan—100 million tons. Fifty million of those would be deposited along the Nile, in the Faiyum, and in the delta. Fifty million would go out into the Mediterranean. Now those 100 million tons are being caught in the Aswan High Dam reservoir. The delta is receding, which can be expected to be the biggest cost of the high dam in the long run. Gradually, salinity from the Mediterranean is coming into the delta, which is the breadbasket of Egypt. There is no fresh-water buffer to keep that salinity out, as there is, for example, in the Netherlands from the Rhine, because the water that is coming back into the lower Nile is saline from all of the irrigation. So there's probably no long-term solution to this.

COHEN: Can't they let more water out of the dam?

SCUDDER: Yes. But then, of course, that has an adverse effect on the hydropower. You get less electricity, less irrigation, and so on. So, yes, that may be what they'll have to do in the long term.

The Egyptian case is a good case. You build the Aswan High Dam. It has a very positive impact on flood control. I mean, there used to be terrible floods in middle Egypt. I can remember in August the floodwaters would back up into Cairo and come pouring out of the drains. There would be sewage lying all over the place between the railroad station in Cairo from places like Helwan and Maadi. Now, the Nile is an exception in the tropics and subtropics. There are cities all along the edge of the Nile. In most tropical river valleys, you are dealing mainly with rural populations, and these rural populations are dependent on the natural flood for fisheries, crop agriculture, and grazing, for example. We found that dams generally reduce the fisheries downstream by something like thirty to seventy percent. For example, the Aswan High Dam eliminated the 18,000 tons of sardines that were coming out from the delta fisheries. Now, in that case, that's more than compensated for by the increased fisheries productivity of the reservoir. But in most cases the tradeoff is negative, especially if a dam is built way upstream.

But then if the dam is fairly close downstream, it interferes with the migration of fish upstream, and so you don't necessarily get that high productivity upstream. Most of these people plant their crops behind the receding flood, as in the case of basin irrigation in Egypt. These rivers tend to go down towards the end of the dry season, when grazing is not available. Frequently herds of animals can only be supported— It's dried up inland, but they're able to graze the grasslands along the edge of the receding flood line.

I was the first scholar to point out the importance of how downstream rural communities in developing countries were being adversely affected by dams, and I came up with a suggested solution to the problem—not a total solution—and that is controlled releases from reservoirs. I wrote a chapter in 1980 on the topic. Controlled releases has had a long history in the United States, South Africa, and Australia for fisheries, not so much for indigenous people or people living along the river but for commercial fisheries and sport fisheries—salmon fisheries, for example. This is a big issue right now in the Columbia River basin—to get rid of the four dams on the Snake River. But I was pointing out that in the tropics and subtropics, unlike in the United States—although interestingly enough, the Hoover Dam ruined the economy of the Cocopah Indians living downstream, because they used to plant maize on the floodplain of the Colorado River as it receded. There were Native Americans living in the Colorado River delta. As you know, the Colorado delta is now a wasteland, because very little of the water now actually flows there. In fact, in some years some of these big rivers no longer reach the ocean. The Yellow River in China, for example, because of upstream dams and bad land management, doesn't reach the ocean for a number of months. You can imagine the impact of that on the people who used to be dependent on that river.

In this chapter in 1980, I talked about the importance of what I called transboundary flood regimes—these are cases where rivers are international—and the importance of controlled releases to at least release enough water to enable at least a proportion of the land to be cultivated and, for environmental reasons, for habitat restoration. Now we're beginning to realize how important wetlands are, not just for biodiversity but also for pollution control and a whole range of things. The productivity of a unit of water in a wetland may be higher than the productivity of that unit of water in irrigation. But when you build a dam, you often reduce more wetlands than you create with irrigation. So you're getting not only a reduction per unit of water but you're

also getting a reduction in the area that can be utilized. So I'm very pleased with that publication.

Controlled releases are no longer called instream flow releases, which were, as I say, being done in Australia, South Africa, and in the United States. The emphasis was pretty much on the fisheries and the aquatic resources. I've expanded that concept to deal with people who are living along those rivers and are dependent on those resources. Now a researcher, Jackie [Jacqueline] King, who is at the University of Cape Town in South Africa, has coined a term for these releases. They're called environmental flow releases. The environment here includes people. The World Commission on Dams is now using this term, so that now this whole concept of environmental flow releases for the benefit of the people and habitat has become a general concept.

COHEN: So these days you're really looking more at people below the dam instead of people above the dam?

SCUDDER: Both. And that brings up the final relationship with the research. Because of these forty-five years of research—well, at that time forty years of research—I did become a commissioner on the World Commission on Dams. The World Commission was set up on the initiative of the World Bank, representing donors and the biggest single donor of funding for dams, and IUCN, the International Union for Conservation of Nature, and Natural Resources, also called the World Conservation Union, which is a huge union of nongovernmental environmental organizations and environmentally concerned government ministries and other agencies. It has I don't know how many members, but a large number of members, including governments as well as NGOs [nongovernmental organizations]. Environmental NGOs and human rights NGOs have begun to realize how biased development policies are against the poor, against indigenous people. So they're playing a very active and, I think, very positive role in trying to embarrass governments to get them to plan better and to implement the plans. The World Bank and IUCN brought together about forty people in 1997 in Gland, Switzerland, which is IUCN's headquarters, representing all different viewpoints—pro, con, and scholars, like myself, although there were very few scholars. Much to our surprise, we unanimously

recommended that a world commission on dams be set up to establish new standards, guidelines, and policies for dams. The final report came out just this November.[2]

COHEN: Now, you all spent two years doing this?

SCUDDER: Yes. We spent two years. The commissioners came from all different viewpoints, not representing organizations but representing the expertise that different categories of people had. For example, Jan Veltrop, a former vice president of Harza Engineering, which is one of the big international dam-building engineering firms, who was the chief engineer at Harza responsible for building and designing many of these dams, became a commissioner. Donald Blackmore, chief executive officer of the Murray-Darling Basin Commission in Australia, which is today probably the most successful environmentally conscious and participatory river authority in the world, became a member of the commission. The chief executive officer of ABB—this huge Swedish-Swiss firm for creating electricity-generation equipment and selling it all around the world, including small-scale generation but also huge turbines—became a member. Our chair was the minister of water affairs and forestry in South Africa, Kader Asmal. Our vice-chair was an Indian, a former member of the Indian [National] Planning Commission, Lakshmi Jain. We then had a senior scientist from the Environmental Defense, an NGO. Judy Henderson, the former president of Oxfam [International]—this huge and, I think, probably the best nongovernmental organization dealing with development work—was a member.

Medha Patkar is probably the leading opponent of unfair resettlement policies in India who literally—until the police dragged her out—was standing in water up to her neck behind the rising Sardar Sarovar Dam, accusing the Indian government of murdering her and her colleagues with this dam. They were willing to die. It was not going to be suicide, it was going to be murder by the government. The government had to send the police in and pull her out. She's an amazing person—she originally got involved just to improve the resettlement. When she realized—and I was a consultant on the Sardar Sarovar project then—she realized about the same time I did that the Indian government had no interest whatsoever in these people, that it had no interest in meeting World Bank guidelines or even their own guidelines. So she became an activist—anti-project, obviously. Now, of course, people are screaming that she's irrational and

[2] *Dams and Development: A New Framework for Decision-Making* (London, U.K.: Earthscan Publications Ltd, 2000).

this, that, and the other thing. On the contrary, her whole stance is because of the inadequacy of government policies and plans.

The United States actually has one of the worst resettlement records in the world. Our resettlement policy is based on what's called "decent, safe, and sanitary housing"—see, the American fetish of having a house. Well, what good is that to black sharecroppers resettled in connection with the Tennessee Valley Authority dams? Over twenty dams and we don't even know what proportion—we estimate that probably half of the people we resettled were black sharecroppers. Who was responsible for their relocation? TVA left it to the farmers for whom they were working. Many of the farmers took their compensation and moved into the cities and got out of agriculture. What happened to the black sharecroppers? There's now been a master's thesis written on that. I can tell you that what happened to the black sharecroppers was pretty grim—not much better than what happened, for example, when South Africa resettled people. So this is an international problem.

These commissioners were first-rate people. We spent the first six months seeing who represented all these viewpoints, some being pro-dam but willing to look at the anti-dam viewpoint.

COHEN: How many people were you?

SCUDDER: There were twelve of us. Secretary General [of the World Commission on Dams] Achim Steiner eventually became a commissioner because he was so important. We had a secretariat in Cape Town of about ten very good professionals who were recruited from all around the world and who did most of the work, because we tried to have as transparent a policy as possible. We announced a whole range of contracts, including case studies of eight large dams and two country studies—China and India. For example, those who won the contract for the Columbia River and the Grand Coulee Dam were people from Stanford and Berkeley, and so on. Kariba Dam and other dams around the world were picked. Then we had seventeen thematic reviews, reviewing all the different aspects of the dam—social, environmental, economic, fiscal, engineering, flood control, irrigation, and hydropower generation. In effect, we were looking at it from the point of view of water resource and energy development. Then we had our 900 submissions. We had meetings in all parts of the world, and we solicited

submissions. All in all, we had a documentation base of over 1,000 items. There's no question that this was the most detailed analysis ever done, and it probably will be the only one, simply because $10 million was used over a two-and-a-half-year period. It is probably the most detailed analysis that will ever be done of the effectiveness—the developmental effectiveness—of large dams.

In our final report, we point out the record, which, from an environmental and a socioeconomic point of view as far as local people are concerned, is pretty bad—very serious environmental impacts. From a cost-benefit-analysis point of view, there were tremendous overestimated benefits and a tremendous underestimation of costs. Many of the projects, especially in irrigation, were nowhere near reaching the potential that was used to justify the building of the dam in the first place. A pretty grim report. But we did not conclude that dams, therefore, were bad. On the contrary. We concluded that dams had a very important role to play in twenty-first-century development but that one had to start off with a wider consideration of alternatives. This is something that Gilbert White did back in the 1950s. He was emphasizing this in a National Academy of Sciences paper, as I recall. It's not a new idea, but it was ignored. Why? Because in Africa a ministry of energy decides, "Gee, we need energy," and so builds a dam. In India, it was the ministry of irrigation: "Well, we need storage," and so they built a dam with no consideration of a wider range of options. This has got to stop. You can't have a ministry, no matter how good their agenda is, going ahead and doing something in the heartland of a country without considering whether there are better ways for doing that. Planners deal with the dam-building process, but, no, it's a water resources and energy process.

You need water and you need energy. What are the options? One of the options is a dam. Under what circumstances is a dam an option? If you decide you're going to build the dam, what should be the process, planning, implementation, monitoring, evaluation, management, and decommissioning? We dealt with the whole process. Then we came up with a whole series of guidelines and criteria and standards. One of the big problems internationally has been—and even the World Bank has been amiss here—lack of compliance with guidelines, with a country's own guidelines and with the guidelines of the donor, whether it's the World Bank or the Asian Development Bank. We came up with, first, an analysis of the lack of compliance and then an analysis of the carrot-and-stick approach to trying to ensure compliance.

The report came out in November 2000. So this part of my career is over, because we had a twilight clause. As soon as our report comes out, the commission ceases.

COHEN: Decommissioned.

SCUDDER: We were decommissioned, right. The secretariat continues into March of this year to get rid of the office and the computers and—the most important thing—to decide, "OK, who's going to run forward with the report?"

As part of our methodology, we had a forum of members coming from sixty-eight institutions in thirty-six countries. They were from the ICOLD, the International Commission on Large Dams; IHA, the International Hydropower Association; and so on. They're all coming together in Cape Town this weekend. I was supposed to be there, but because of Molly's broken ankle I had to cancel. They're coming together, along with a few former commissioners and a few people from our secretariat, to talk about the evaluation of the report. The World Bank, for example, sent two people all around the world talking to different governments to get their reactions. The British Dam Association has already come out with very strong support, and the British government—their equivalent of our Agency for International Development—said, "Not only are we going to follow the criteria of the commission but we'll give money—loans and grants—to countries to help them develop the capacity to follow the criteria." On the other hand, some of the representatives of the dam-building fraternity, both at the international level and in the associations, have come out very strongly against the report. They're saying, "Oh, it's an anti-dam report. It's going to make it much more difficult to build dams." Well, it *is* going to make it more difficult to build bad dams, there's no question about that. On the other hand, other individuals in those associations are for the report. But the two biggest dam-building countries, India and China, are coming out very negative. India has done a bad job. In recent years, China has done a good job, generally speaking, except with the Three Gorges project—they're doing a terrible job with that. So of course they see this as an infringement on their sovereignty. It's going to be a big issue.

Looking to the future, I suspect that I'll be putting quite a bit of time— In fact I've been asked by one of the critics—an engineer by the name of Asit Biswas, who is editor of the *International Journal of Water Resources Development*—to write an article in the next issue on

the World Commission on Dams process and my perceptions of it. I'm going to be very critical of anti-dam critics, because I think they're shooting themselves in the foot.

One of the most important aspects of our report is pointing out: "Look, when you build a dam, the financiers look to see what the risks are to them, and the governments who want the dam look at what the risks are to them. Who looks at the risks to the people who are affected? Who looks at the risks to the environment, and therefore to the country as a whole?" Up until now, nobody. So a big section of our report is called "Rights and Risks"—the rights of project-affected people and the necessity to do what economists would call risk analysis as it relates to the environment and to the people. Now, it's very interesting that these critics are practicing denial [laughter], the kind of denial that I was talking about with forced relocation. They're denying that the situation is as bad as the report says it is. They're denying that 40 million to 80 million people have been relocated. "Oh, no. That's impossible. We don't believe that." These are engineers saying, "We don't believe it." It's denial, and this is why I think they are shooting themselves in the foot, because it's going to be very easy to point out that they're really doing a disservice to themselves. This is why, I think, some members of these same organizations are saying, "Hey, hey, hey. Look. You may not agree with the report, but it is putting dam building into the twenty-first century."

COHEN: Well, now, the group that should listen to this report is people who are supplying the money, like the World Bank.

SCUDDER: Precisely. And we are worried at the moment that the World Bank— Who runs the World Bank? The United States plays a very important role, because we choose the president. But the *real* runners of the bank are the finance ministers of all the different countries who are members.

COHEN: I would say the real power is where the money comes from.

SCUDDER: Well, of course. Remember that the United States is the main donor, but just as we're trying to reduce our contribution to the UN, we're also—probably; I don't know for a fact—playing down what our contribution to the World Bank is. But India, China, and Brazil are beginning to have a bigger impact on the policies of the World Bank, as they should. I think the

bank, like the IUCN, tends to be a conservative organization, because it doesn't want to alienate its government members. The World Bank obviously does not want to alienate its largest borrowers. The three biggest borrowers are China, India, and Brazil, so you obviously don't want to alienate them. It's going to be very interesting to see what the bank's reaction is. But already the British government has said, "We're going to ensure that our export-import agency follows these guidelines." The German government is doing the same thing. So I think this kind of thing is probably going to put a little bit of pressure on the bank. The Asian Development Bank, I think, is fairly well disposed, as well as the African Development Bank, and the Inter-American Development Bank. But it's going to be very interesting to see whether or not people come together and say, "Well, look. OK, some of these guidelines are unreasonable, but we're willing to follow other ones," or whether it will increase the dispute.

COHEN: So you think you're going to have a big role in this, or some role?

SCUDDER: Some role, I think, along with some of the other commissioners. I'm already going to have to write this article for a journal that is published by the opponents to this kind of thing. But, no, I would say that this will be my biggest policy role. My biggest effort in the next two years will be to write two books. One will be a follow-on to the World Commission on Dams, which will be a very detailed analysis of the impact of dams on project-affected people—what we've been talking about. In fact, I've already started. I'll be finishing chapter one this week.

COHEN: Ah. Very good.

SCUDDER: It will take me a full year. When I finish that, it probably will take me at least two years after that to write up the long-term study that Elizabeth Colson and I have been doing, which will be a socioeconomic history of the impacts of the Kariba Dam on the 57,000 people—now 100,000 people—from 1900, so it will start before the dam, to the year 2000. So that's how I anticipate the future—writing up the results of research but keeping up with what's going on. I also anticipate staying involved in this policy area. One way I do that is I continue to be on three advisory panels for big projects in different parts of the world—big dam projects—advising them on environmental and socioeconomic aspects. One is in Laos, one is in China—although that's

been stopped because, I believe, of the Three Gorges project which is, as I say, a bad project—one is in Swaziland— Actually, four—and one in Lusitu.

COHEN: So you're still on the airplane.

SCUDDER: Twice a year now. This last year, I think I made nine international trips. This year I anticipate no more than two.

COHEN: Now, I'd like to talk about some of these awards you've gotten, but maybe we can do that in another session.

SCUDDER: Really? What?

COHEN: Well, you just got something last year.

SCUDDER: Oh, the awards. Oh, I can do that in about three minutes. You know, I came to Caltech in '64 to do research; we talked about that before. I was aware of the costs: One, I would have no graduate students; two, I would have no colleagues. Therefore I assumed that my reputation would not extend beyond Caltech. [Laughter] And I assumed, as we talked about before, that my reputation would not even be within Caltech. One of the ideal things about Caltech is, nobody knew what I was doing, nobody gave a damn, nobody bothered me. But I seemed to be doing reasonable things, so I had full support. It was ideal. Remember, I talked about the mountain and the monastery analogy?

COHEN: Right.

SCUDDER: Who gives you a festschrift? It's your students. No festschrift. [Laughter] Well, much to my surprise, I'm the only person in the field who has received, if not all the awards, the major awards for the public-policy implications of anthropological research. I was the first recipient of the Kimball Award. I remember I was overseas when I got the cable. I thought it was for joint work of Elizabeth Colson and myself. The Kimball Award was the first award that the American Anthropological Association had given for public-policy research, and I was the

first person to get that. Then the next one was the Lehman Award. Lehman had been the administrator of the American Anthropological Association and apparently had done a very good job. When he retired, the association set up an award called the Lehman Award. I was the first recipient of that [1991]. Well, for reasons that I don't know, they didn't sponsor that award again. [Laughter] So nobody is ever going to be able to get that award again, you see.

COHEN: You preempted it. OK.

SCUDDER: I preempted it. I'm the only person who's ever gotten it and that's why I can say I've gotten more awards in my field than anybody else. [Laughter] Then the next one was from the Society for Applied Anthropology, which is quite international. The American Anthropological Association is a North American-United States organization; meetings are always in the U.S. The Society for Applied Anthropology has meetings in Mexico and in different places in the world, so although it's based in the United States, it has an international membership. They have an award called the Malinowski Award, which is named after [Bronislaw] Malinowski, who is one of our great anthropologists of the past, before World War II. I received that award, I guess just two years ago. The fourth one is called the Lucy Mair award [the Lucy Mair Medal for Applied Anthropology]. That's named after a leading British anthropologist who did research that had policy implications. The Royal Anthropological Institute of Great Britain and Ireland decided that they would give a public policy award in her honor, and I was the first recipient of that. So of these four awards, I was the first recipient of three of them. My colleague Michael Cernea is probably the runner-up. He's received the Malinowski Award and the Kimball Award. He'll never be able to get the Lehman Award. [Laughter] Sooner or later he should definitely get the Lucy Mair award.

COHEN: Now, have these come just with honor? Has there been some award?

SCUDDER: Oh, anthropology has no money. For example, the honor that is associated with the American Anthropological Association award is a free subscription for life to the journal. [Laughter] For the Malinowski award, you had to give the distinguished lecture at the annual meeting and you got a nice medal. The Lucy Mair award—they didn't even have the money to bring me there. I was supposed to come by and give a lecture on my way back from Africa, but

then the African trip was canceled. I got that medal when they sent a member over to the Malinowski Award meeting, who then presented me with the medal there. [Laughter] If it were physics or astronomy or something of this nature, why, I'd be several hundred thousand dollars richer, but it doesn't happen in anthropology.

[Tape is turned off]