

Maureen and Mike

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**Oral History Number: 120-012, 013**

**Interviewee: Cornelius B. Philip**

**Interviewer: Robert Philip**

**Date of Interview: circa 1984**

**Project: Bitterroot Historical Society Oral History Project**

Robert Philip: —Cornelius B. Philip [unintelligible] at the Rocky Mountain Laboratory, particularly as regards spotted fever—more or less reminiscences over the years. The sort of thing that you don't find in scientific publications.

Now it's going. Now to stop it, you just press [unintelligible]. Now it's going. This one right on the end.

Dad, you have more or less organized your thoughts.

[Break in audio]

This is an interview with Dr. Cornelius B. Philip in regards to...[unintelligible] at the Rocky Mountain Laboratory particularly as—

[Break in audio]

Cornelius Philip: Back in the period when you went to Bozeman in your graduate student days at the University of Minnesota, when we had one son, Robert, with us, we did not have the kinds of scholastic grants to keep us, as a family, floating financially and getting a little strapped. We had an opportunity to go to Bozeman, Montana, the Montana State College as it was then known [Montana State University] to fill a sabbatical leave of Dr. Cook who had a project going on cutworms. It was my assignment to carry on his cutworm project for a year while we recouped our finances. And Gladys and I, and often Rob, rode in a Model T coupe to Three Forks to do the [unintelligible] studies that Dr. Cook was engaged in at that time. Also did some teaching on the Department of Entomology staff and had several, since, eminent entomologists like Dr. Bill Jellison and Dr. Gunholz (?)—others in the class.

After the year was up, I still had a summer available so the Montana State Board of Entomology—composed of the state health officer and the state entomologist, Professor Cooley, and two or three other health officers—invited me to work for a summer up in the North River Valley where mosquitos had become a rural pest through the overflow of irrigation waters and very annoying. They sent me up there to do a very menial type of control just pouring crankcase oil on the overflow waters along the roads of both the Malta and Chinook on alternate weeks.

RP: Can I interrupt there a moment, Dad?

CP: Yes.

RP: I'm sorry. I recall as a child when you were reaching a decision to actually come to the Rocky Mountain Laboratory that there was also a choice at that time as far as mosquito control in the Milk River Valley. Is that correct? You had a choice there of going back to Malta on mosquito control business or actually then coming to the Rocky Mountain Laboratory.

CP: No, that's ahead of the story. Actually, when I returned to...Well, before I returned to Minnesota. During the Montana State Board mosquito control work in the summer, Dr. Bob Parker (?) and I paid a visit to the then Rocky Mountain Laboratory in the old schoolhouse on the west side of Hamilton. Before it had become a part of the Public Health Service operations. And I spent several days there getting acquainted with the problems they had, and also met Dr. Parker, who was then an employee of the Department of Entomology. Then returning to Minnesota, having recouped our finances, we continued graduate studies until I had finished my master's degree in 1926. That was before we went to Bozeman.

On returning to Minnesota, I was to resume graduate work to complete another quarter of my doctorate, and Gladys had gone to [unintelligible] to York, Nebraska, with a very young daughter and I was offered—on the basis of my mosquito experience in the Milk River Valley—a job as entomologist to the West African commission of the Rockefeller Foundation in Lagos, Nigeria, by the Rockefeller Foundation. This was pretty tough because it meant moving Gladys and a young daughter for perhaps a year and a half. The consensus among my professors was that it was too good an opportunity to pass up. So nobody was on Gladys' side about life was short, and could we do it. Could we do the separation that long? In the end, I accepted. And after the period where the West African yellow fever commission, when I really reoriented my own entomological experience toward transmission of disease by insects, having made the appointment so Dr. Parker—who by then was now one of the two specialists on Rocky Mountain spotted fever in the Bitterroot Valley in Montana—I was offered by cable a position as medical entomologist in the then newly organized spotted fever laboratory. I promptly accepted by cable also. So that another period of waiting had to occur in completing my doctorate at Minnesota. We drove to Montana in a very antiquated coupe, and I was very fortunate to arrive at a period which was really climactic in the spotted fever story in the United States.

We arrived in on January, 1, 1930. In that same year, scientists at the National Institutes of Health had documented for the first time unquestioned cases of Rocky Mountain spotted fever in the eastern United States. Very rapidly cases were diagnosed in Gulf Coast states and up the Mississippi as far as Minnesota. So that by 1932, Rocky Mountain spotted fever had become not the localized problem envisioned by the Montana State Board of Entomology in establishing the laboratories in Hamilton in the Bitterroot Valley that early, but had become nationwide. In 1933, with Dr. Parker and Dr. Jellison, we published a report on the experimentation we had done with ticks in transmission of spotted fever and the distribution of these ticks over the country, pointing out that no section of the country was without some species of tick that

experimentally and in two or three cases naturally carried a Rocky Mountain spotted fever organisms. Publications of that time [unintelligible] Rocky Mountain spotted fever virus, but the virus term has been opted by specialists since that time.

How much have—

RP: Okay, it's all set.

CP: All right.

RP: All you have to do is talk.

CP: Oh, talk. Those early '30s, were really a golden period in the history of the Rocky Mountain Laboratory objectives. At that time, Dr. Cooley had great hopes that tick parasites he had imported from South Africa might help to control the spotted fever ticks—wood ticks. But it turned out, they were just getting going in the summertime when the ticks were going into estivation, and the two did not overlap sufficiently to be useful. In 193s, I gave a resumé of the work of the laboratory to the National Library Association, and emphasized the work at that time was [unintelligible] for the tick-type vaccine which had been developed in the early '20s by Drs. Parker and Spencer. By the time of my talk, the demand for the vaccine had outgrown the supply, and for some of these, raising ticks for this vaccine had become really [unintelligible]. So that a new laboratory was mandated in 1933, and with the physical plant of the laboratory continued to increase, and a part of that was because of the pressure to provide more tick vaccine—tick-type vaccine—in lieu of the later developed yolk-type vaccine that was directly developed by Dr. Cox.

No commercial laboratory or producer was willing to undertake the hazardous type of tick rearing involved in the production of the tick-type vaccine. Also at that time, we had a growing interest in tularemia as a tick-borne organism, which also is carried by biting flies. And in 1932, it was my privilege to go to the vicinity of Lewistown where an epizootic was occurring in jack rabbits and sage hens. I shot a sage hen on the road, not finding carcasses at first, but later...and we isolated tularemia organisms from the carcasses as well as the shot bird, from jack rabbits, and also from the grouse tick—*Haemaphysalis cinnabarina*. Investigations at the laboratory continued, however, to emphasize spotted fever and its relation to the indigenous populations in various parts of the state, of the states. It was found that each species of ticks could carry the disease, including the rabbit tick which does not bite man but can infect—in mature hosts—on which the wood ticks in mature stages were feeding, so that the rabbit tick could involve the cycle in man.

About this time also, we were called on by various physicians and health departments in Western states to assist in problems where they were encountered such as in Western Nebraska where I was the first one to visit the first case of spotted fever in a retired railroad engineer who had been bitten for the first time by a wood tick. Rather peculiar coincidence. But

in traveling with Dr. Parker through various areas, it became evident that the local physicians and various health groups were keen to maintain liaison with the laboratory and the obtaining or more tick-type vaccine, which by that time had been shown statistically to reduce—for example, in the area of high virulence in the Bitterroot Valley—reduce the mortality rate from 70 percent in unvaccinated people to 21 percent in vaccinated. Only three vaccinated persons contracted the disease, generally with mitigating circumstances such as too early an infection in relation to the vaccination.

Actually, my original mission in coming to [unintelligible] was an assignment by Dr. Parker to study the mysterious differences in virulence of the spotted fever organism between the west side and the [unintelligible] east side of the Bitterroot. For five years with employees stationed in the field throughout the tick season, we tested immature ticks taken from rodents, we checked the ecological, perhaps [unintelligible] variation in the local flora, and found that there was as much as five days difference, average, in say the flowering time of buttercups or other flowering plants. But we never came up with any explanation of why the virulence in tick on the west side was so much greater than it was on the east side. And it was puzzling that just over the mountains in Southern Idaho, the virulence dropped to a low degree of mortality even though many cases were reported. This still remains a mystery.

An interesting development in the epidemiology of spotted fever was our finding that infected male ticks could infect females during fertilization. In other words, sexual transmission. This might be a way in which an infected male, in staying on an animal, could infect more than one female which he fertilized. Another interesting development quite early was the finding that there were strains of low virulence in the tularemia organism, both in ticks and in rabbits, and in water, which at first, Dr. Francis [Edward Francis]—the father of the tularemia picture—didn't believe. But we continued to isolate such strains over the years.

It will be convenient perhaps to carry further discussion on a chronological basis. In 1935, and again in '36, in the neighborhood of Ringling, Montana, an epizootic occurred in sheep and local jack rabbits on the Ringling Ranch, in which we found that ticks had become so abundant that they were not only disabling and killing sheep, but the local jack rabbits as well. It has been shown in the laboratory that as few as 75 engorging female dermacentors can [unintelligible] a laboratory rabbit. We found rabbits on the Ringling Ranch unable to stand with a massive infestation of adult ticks about the shoulders and head that were dying, apparently, just of tick-host anemia as it was then called. In 1937, a published report of six-years study of a natural tick population on a measured plot near Como Lake, painting the ticks a different color each year, and releasing them finding that some ticks lasted over two years on the same trail in the same place where we had encountered them first. Putting a dot of paint of that year on the ticks.

In 1938, an outbreak of equine encephalomyelitis occurred in horses in the Bitterroot Valley, which gave an opportunity for study with laboratory facilities immediately at hand. An interesting sidelight of this was that before a vaccine had been developed, it had become the practice to inoculate horses with initial symptoms in the neck with convalescent serum. Horse

so treated suddenly, in a percentage, developed almost rabid actions of walking through fences and trying to bump through doors. It was such a violent reaction in taking moving pictures of it and no label for this particular syndrome, I had a subtitle on the film of "X disease of horses". Strangely enough, this name got into the veterinary literature after a report of Dr. Hadlow (?) to a veterinary group in Chicago, to whom he showed pictures of the very violent horses. Fortunately, the development of a vaccine eliminated this problem. Horse, or equine, encephalitis was a matter for calling on [unintelligible] personnel for help where outbreaks occurred. One such, I visited in the Weld County area of Northern Colorado, and for the first time we demonstrated that that cause was due to St. Louis encephalitis virus. Later, it was shown at the laboratory that the horses were susceptible to this virus. Historically, this virus is of interest to me because in 1933, when an outbreak of sleeping sickness developed in St. Louis, I was called back home to bring mosquito cages and assist in an investigation of the disease in the local environs.

In 1940 and '41, I was given sabbatical leave to accept a Guggenheim Fellowship at Harvard School of Public Health, and just finished that work when the Surgeon General of the Army requested my loan to the medical department—the Sanitary Corps—for World War Two problems, both training medical officers at the Walter Reed—

[Break in audio]

Typhus in the civilian refugees and local residents was threatening the staging of American troops confirmed in German lines in central Italy. I had the interesting experience of being called to Foggia, Italy, to advise the first Canadians about septic arms that occurred in some of the personnel after vaccination with the Cox-type vaccine. It turned out in checking the routine vaccination that in one instance, every seventh man had been vaccinated with the same needle dipped only in alcohol and apparently through a septic sore that was transferred to the others. It was not the vaccine. Before the typhus commission personnel had completed their sanitary operations in Naples, in which the first demonstration had occurred of aborting a potential classic typhus interference in a military operation, I was called to the Southwest Pacific to join a group working in New Guinea on scrub typhus—a disease that was becoming alarmingly problematic in staging U.S. and Allied troops. An interesting sideline was that in being ordered by cable to the Southwest Pacific to report in Australia, the paper was signed by order of CIC [Counter Intelligence Corps]. I was treated with such arm's length reception, it was only later that I realized...or that I found out that the local people thought this was Counter Intelligence Corps rather than Commander in Chief as it actually was.

From New Guinea, we moved along to various areas, working with the military campaign entering new areas potentially infest with scrub typhus mites, and our group demonstrated for the first time the actual presence of mite-borne scrub typhus, or tsutsugamushi disease, and various new areas such as other parts of New Guinea, [unintelligible], and Philippines. And later, we even were transferred to the classic areas in Tokyo at the end of the war. We repeated the demonstration of endemic typhus as a military problem in Jamaica when we got

to the Philippines and found that cases of endemic typhus were also occurring in the civilian population in Manila.

In getting up to Japan, it was my privilege to make contact with the president of the Japanese Medical Society, Takeo Tamiya, also an authority on their tsutsugamushi disease. We showed for the first time that not only the species of field rat *Microtus* that they had suspected harbored and acted as a reservoir for the disease, but that a more common and widespread mouse—field mouse—*Apodemus speciosus* was also probably reservoir and carrier of the mites, following which about 19...in the '60s, Dr. Tamiya mentioned in a text on the disease an acknowledgement that I had been instrumental in refocusing her attention on this important epidemiological factor.

Immediately following the war, after return to the Hamilton laboratory, I was invited by an army scrub typhus team to check the efficacy of the new antibiotic chloramphenicol, both in the treatment and the prevention of scrub typhus in a field exposed personnel, which has proven to be very efficacious. In 1951, a peculiar case of Rocky Mountain spotted fever was serologically confirmed in a patient on the Olympic Peninsula in the early winter when the wood ticks had long since disappeared. On investigating this case and showing the patient specimens of different kinds of ticks, he immediately identified a winter tick, *Dermacentor albipictus*, as the one he took off himself, and it turned out he had acquired this tick in the process of skinning an elk on his back porch.

In 1952 and '53, two pertinent observations were made at the laboratory. One was confirming that Indian tick typhus can be transmitted by ticks, and the other was discovery that there have...there had been discovered an infection in dogs, which was later confirmed as related to the rickettsiosis in dogs in Northern Africa. This was the first time this disease had been reported in the New World except for an earlier report of its occurrence in Aruba, Dutch West Indies. In 1955, two pertinent accomplishments we made—one was a presidential address to the American Society of Parasitologists that I entitled "There's Always Something New under the Parasitological Sun." This involved new information on the very unique occurrence of rickettsia like organisms in parasitic trematodes [unintelligible], which was very much more complicated adaptation of rickettsial organism to the vector cycle in which snails, intestinal flukes, salmon, and [unintelligible] of hosts in dogs were involved. The other was the report of investigation of a die-off of a peak population of jackrabbits in Northern Nevada, which had built up maximum parasites, both ticks and botflies. While tularemia was isolated, the number of dead carcasses of rabbits that had no evident organism that could have caused the death left the die-off cause as much a mystery as in the past.

In 1957, because of the hodgepodge of names assigned to rickettsial organisms, I proposed a simplification along the lines that followed a zoological and entomological nomenclature. Following this, experts were impressed with simplification to the point that I was invited to provide a chapter in the *Authentic Bergey's Manual*, which in its successive editions has become the classification standard. As a result, I had the unique experience of being invited to

join the International Commission on Bacteriological Nomenclature as well as even their inner judicial council.

Oh, has that been running?

In 1963, an unusual episode occurred of rickettsial disease in Montana where human body lice are known only in the heads of school children, which therefore are head lice, but not body lice. In 1963, we investigated a case in an Indian squaw whose only travel off the Flathead Reservation was to the Calgary Roundup in weeks previous to her coming down with a confirmed case of classical epidemic typhus. Members of her family did not contract the disease even though one daughter had head lice. The only explanation we had for this was that possibly Mexican laborers had brought the disease north with them during the summer beet harvest. In 1964, I was complimented being invited to write the chapter on scrub typhus and scrub itch for the Historical Division of the Army Surgeon General's Office to cover these infections in American troops during World War Two. My assistant Jean Hughes (?) was always very ingenious and active laboratory worker among whose accomplishments he isolated a new virus from seabird ticks from an island in the Caribbean. This became the prototype of a number of such viruses taken from seabird rookeries as far apart as the South Sea Islands and an island near the mouth of the Red Sea. I named this virus Hughes virus to the consternation of some of my virological friends who have the practice of naming a virus only from geographic sources. But in this case, we had already isolated the same virus from a bird rookery in lower California, and it would be silly to put the name of the original island in the Caribbean on a virus already known only to span two oceans.

RP: Dad, thank you for the narrative of your scientific experiences both with the Rocky Mountain Laboratory and also the U.S. Army. Now, I would like to ask you some questions. You've been discussing, of course, the scientific aspects of your long years of association with the Rocky Mountain Laboratory. Actually, eight years before your arrival there in 1932 when Public Health Service was involved with Rocky Mountain spotted fever and the laboratory, and there were...have been some 13 years since you retired. But during the 40 years, you have been hand in hand with the Public Health Service and in its efforts in Western Montana both as a scientist and also under the second half as administrator. Now, I'm sure that there are many personal impressions and experiences that occurred during that 40-year period that might be of interest.

First back to the beginning in 1930, I believe that Dr. Parker recruited you, or approached you, as early as about 1928 or '29 about coming to the laboratory. And it isn't clear from the records that I've encountered thus far as to what the thinking was in terms of administration as far as expanding the program of the laboratory. I gather that they wanted to emphasize research aspects. Is this correct assessment of that time?

CP: Well, the primary function that Dr. Parker had put in a table to me was that research was needed on the mysterious differences in virulence in the Bitterroot Valley, and this was

supposed to be my primary operation out there since I already had an ecological background also. Unfortunately, I never was able to shed any light on that particular aspect, and very quickly I got involved in arthropod-borne disease, especially by ticks.

RP: In going back through the tick host records of the collections, there was a large effort as you indicated collecting small mammals and examining them for ticks between 1929 and 1932. The two areas in particular that were involved were Tin Cup and Harlan Gulch. Been puzzled as to the reason for including Harlan Gulch as the east side area. Was there actually a spotted fever case that originated in that in Harlan Gulch? Or what was the reason? Can you recall this?

CP: Well, Harlan Gulch was a more unaltered environment by man. Almost any other environment on the east side that gave reasonable access had had the tick population too much eroded. But in Harlan Gulch, it was practically a virgin territory to not only study the ticks but the rodents as well. For as ranchers in the open valley had long since been poisoning and trapping ground squirrels and reducing their populations back into the mountains, in Harlan Gulch, this had not been altered and that seemed to be a representative spot. But it was not because there were human inhabitants up there.

RP: In going through the old tick records and comparing those two areas, I got the impression that the numbers of immature ticks were about equivalent both in Tin Cup and Harlan Gulch. Is that the impression that you had at that time?

CP: Yes, we noticed very little difference. The top [unintelligible], of course, discouraged ticks more on the Harlan Gulch area, and they were more quickly dispersed than they were in the Tin Cup area. But in general, the infestations were about the same.

RP: In reading some of the correspondence between Cooley and Parker, I get the impression that Parker looked upon your arrival as a right hand man [unintelligible] spelling, in terms of being able to rely on someone to carry part of the load. Although, at that time, I don't believe that they had assistant director as such, I think he looked upon you in that sense as much as he could. But I gather that he was very autocratic person, one that liked to run a one-man show. Is that a correct assessment?

CP: Well, it's a correct assessment in as much as he had the idea that he wanted to know everything that went on, and as such even our personal correspondence with other professional people, he wanted to counter sign the letter—respectfully forwarded. In that respect...And then he also had an idea somehow that he was the ultimate referee on papers that were written, to a point where it was sometimes annoying that he would sit down with a draft of a manuscript prepared by a subordinate and immediately start tearing it up before he had read the very same thing he'd written in a few paragraphs on beyond. But as far as another study was concerned, later, Dr. Gordon Davis (?) came from the same yellow fever laboratory where I had been as bacteriologist, and Dr. Parker alternated between us when he would go away as acting director.

[Break in audio]

RP: I've lost the train of thought here. Now you were right in the middle of it...of spirochete, lapsing fever.

CP: Yes. Is it on?

RP: Yes.

CP: Oh. The bug samples sent to a lab in Spokane turned out to have spirochetes, and immediately Dr. Parker sent me over to see what I could find in the way of potential spirochete tick host. We located chipmunk nests under the cabin and found [unintelligible] coming up in the cabin and had apparently a youngster in a crib up on the main floor since the cabin was only occupied sporadically, not constantly. But this an example his fortuity in going after...if of encephalitis was reported, he didn't wait for local people to send for us, but sent us to see how we could help in anyway. But in certain instances like the state of Nebraska, state health officer would have none of us coming down without his first inviting us.

RP: Recently we did a survey of ticks northeast and west side, in 1977, and found that about ten percent were infected with Colorado tick fever virus. Yet curiously in going through these old clinical records, you don't run onto many mild cases of tick-associated illness which might be Colorado tick fever, and indeed even as late as 1933, Dr. Parker in one paper indicated—talking about Colorado tick fever—indicated it had not been found in Western Montana although it was present elsewhere. I'm rather surprised that the disease was not that important back in those days or was so mild that it simply was missed—a relationship to spotted fever. Did you encounter anything any illnesses at that time that were rather curious along those lines that might have been—

CP: No, we didn't actually encounter [unintelligible] illnesses that we could think were different than maybe a mild case of spotted fever of some other illness. We just weren't acquainted with Colorado tick fever, which is non-fatal, and it was studied by even our personnel in Colorado before they back to our home.

RP: After Dr. Parker's death, of course, there were changes in administration and orientation as far as the scientific program is concerned. Dr. Larson arrived, I believe Dick [unintelligible] was still his director of the institute at that time. But spotted fever in and some of the tick-borne illnesses were de-emphasized, were they not?

CP: Yes—

RP: Until '49—

CP: Yes, there were. This was the start I think of a re-evaluation of bench work compared to field work. We were...Our reputation internationally was developed on an interlocking field and laboratory work. But many of the hierarchy in NIH were bench-oriented and felt, I think, that we were using this as sort of a lark to get away from the lab. [laughs] But there was this distinct deviation from the old line, and there was, I think, the feeling too that supposedly we had milked the pump pretty dry of any new information and that we needed to get re-oriented or the less common microscopy.

RP: Of course, I guess that was about the time also that the Center for Disease Control came into being. And also I think...wasn't it this concept of a scientific director developed about that time with Hugh Smadell (?) taking over? I've forgotten when that was, and he had very definite ideas as far as what should be worked on out there.

CP: Yes he did. But he didn't demote the director out there. He just tried to re-orient people including myself with [unintelligible] to what our functions should be in public health [unintelligible]. But we never lacked for popularity among the public health officials in the West, and we always seemed to be very welcome anywhere that we wanted to work.

RP: One episode that you didn't mention in your resume is this business of Nine Mile fever. I believe that Parker had CCC [Civilian Conservation Corps] people and every one that he could get collecting ticks in order to prepare a vaccine, and I presume that this is how that originally was isolated from ticks that were collected by the Nine Mile CCC personnel. Am I right on that, Dad?

CP: Yes, you're right. I think that some lab personnel like Gene Hughes (?) went up there to supplement to the ticks that were reared and to increase their viability to get new strains into the laboratory, but it's curious that simultaneously in Australia they'd discovered the same agent so that Nine Mile was supplanted by Q fever, which didn't [unintelligible] as some people thought [unintelligible], but query fever.

RP: It's interesting also the fact that the tick vaccines actually contained a few fever rickettsia, and that many of the people were being immunized against spotted fever were very possibly also being immunized against tick fever.

CP: Well, certainly the records show later that during the period after more was known about Q fever that a tick vaccine was vaccinated in particularly the lab [unintelligible] against Q fever also.

RP: Well, I've about run out of questions. Do you have any further thoughts, both as regards to administration and politics, I don't know if you want to get into that, but... [laughs]

CP: Well, that part of it, I have some personal feelings about what has happened to the laboratory that are better just left off the record. But I do have one observation that occurs to me that would be worth winding up with.

The personnel in getting out, particularly on weekends collecting and so forth were visiting various areas in the valley, and we found that a particularly good spot was up at Blue Nose Mountain where there was a Forest Service fire lookout. Even while the lookout was manned sometimes by families, we were always welcome to take a sleeping bag and go up there if they wanted to and set up traps. Then I'm not sure of the dating, but sometime in the late '50s, early '60s I think they decided to abandon certain of these lookouts, no longer man them in lieu of their aerial survey work. So we continued to go up there and found that by sliding one of the window panels near the door, we could reach in and unlock the night latch that was on the door so we could put our bedrolls on the folding cots that were already there and use the cabin as well as preparing small meals on our cook stoves.

About the middle of the second summer that we'd been doing this, we had just closed the camp—taken our gear out of the cabin and closed it—and were packing into a nearby panel truck when two rangers arrived. Without saying much to us, they obviously were concerned about the cabin and came back and got in a discussion with us, and we immediately told them what we had been using the cabin as a base for collecting and showed them pinned insects in boxes that we had along with us and our gear that we'd used—the traps. They were impressed, and then admitted that they were there with the purpose of assessing the prospective destruction of the cabin in order to eliminate a possible public accidents while it was unoccupied. The suggestion was then made that maybe a use lease could be assigned to me as a representative of the Public Health Service to continue using the cabin for collecting. It was a great source of continued visitation by various personnel from Rocky Mountain Laboratories and other from the University of Montana, and it still is visited every summer by Professor [unintelligible] and his family from the University of Montana.

We did some maintenance for the lookout in between times. It is now getting into a state of disrepair and the Forest Service admits it but has no money to keep it in good condition. But it's still there, I'm told by Professor [unintelligible], and they still use it every summer. Who knows, it's quite a fond place in the memories of many of us at Rocky Mountain Laboratory.

So with that, I suppose...

RP: Well, thank you for this. It has turned out to be a very lengthy interview and very complete. I feel that...well, you have been one of the principals involved, almost half a century in this organization and that it's essential that we try to tap some of the information experiences that have [unintelligible] through the years from those who have been involved before these experiences are lost. So with that, thanks again.

CP: Necessarily, I didn't refer to the many personnel who were involved in the various studies that I mentioned—others on the staff that I collaborate with more often than by doing the work only by myself.

[End of Interview]