

This is an interview with Dr. Glen M. Kohls at his home in Hamilton, Montana, August 2, 1985. The interviewer is Victoria Harden.

Harden: Dr. Kohls, would you begin by giving a brief account of your early life and how you came to be at the Rocky Mountain Laboratory?

Kohls: I'm a 1929 graduate of Montana State College in Bozeman, which is now Montana State University. I went to high school at Kalispell--Flathead County--High School. In college I was persuaded to take entomology. The head of the Entomology Department and the State Entomologist at that time was Dr. R. A. Cooley. He was the experiment station entomologist, professor and head of the department, and also Secretary of the Montana State Board of Entomology. He wore three hats. I practically owe my whole career, to Dr. Cooley.

I had to earn my living while I was going to school, and after my freshman year, I was hired to do certain jobs around the department. By my junior year, Dr. Cooley had become interested in a tick parasite on which Dr. Emile Brumpt in Paris was working. Dr. Cooley organized an expedition--he and his wife went to South Africa in search of these things. To make a long story short, when he came home, he persuaded me to drop out of college at the end of the second quarter of my junior year and take local charge of the project at Hamilton. I was to rear these parasites in mass numbers and release them here in the West in an attempt to control spotted fever by biological control of the tick. I arrived in the Bitterroot Valley in April of 1927.

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I wanted to finish school, but I promised to work for a year. After that, he let me go back to complete my degree. I finished, and ever since that time, I have been connected with the Laboratory.

The tick-rearing work was done at the old lab. We had to rear hundreds of thousands of ticks. The basic knowledge about how to rear ticks had been developed when they started to produce spotted fever vaccine in large amounts about 1925 or 26. So the techniques for rearing ticks in numbers were fairly well developed. I had to learn that. I spent one year in the old building. When I came back the subsequent year, the new one was being built.

Our technique continued to improve. One year we reared twenty-seven gallons of wood ticks. At one time I figured out how many ticks that is.

Harden: An astronomical number!

Kohls: It was certainly a lot. We used I don't know how many thousands of rabbits and guinea pigs a year.

I started graduate school at the University of Minnesota in 1933, during the Depression. After I graduated, I came here full time, working for the State Board of Entomology under Dr. Cooley. I kept that status until July 1931, when all the work that was done by the state of Montana was taken over by the Public Health Service. That is, the research work, not the tick control work--rodent control, cattle dipping, and that sort of thing.

Harden: How long did the control work continue?

Kohls: I wish I could tell you. I think it may be recorded in Esther Gaskins Price's book. Part of my duties at that time were checking the arsenic content of the dips used in the valley.

Harden: Do you think the work continued for another ten years?

Kohls: I think it petered out in the late thirties, probably. After they found that the vaccine was successful, the less emphasis there was on dipping livestock. Then, about 1948, the first antibiotics were used to treat spotted fever. With the combination of vaccine and antibiotics, ticks were no longer feared quite as much as they were prior to that time. People often asked why spotted fever work was centered in the valley. That was simple--it had the highest death rate from spotted fever of any place in the country. There were several places in the West where spotted fever was definitely known to occur and the Bitterroot Valley was well known for the extremely high case fatality rate that occurred here.

Harden: I discussed with Dr. Ormsbee this morning why this situation occurred. He seems to think that there was probably a mutation in the rickettsia at some time.

Kohls: If you want to get the modern story on that, I think Dr. Burgdorfer is the person who would know. He has been working on some of those things, on why it was on the west side and not

on the east side--that sort of thing. That's a whole story in itself. Willy has been very instrumental in trying to unravel some of those puzzles.

Harden: Yes, I have talked with Dr. Burgdorfer. Now, I interrupted you.

Kohls: I started graduate school because everybody had to take forced vacations. Times were tough in 1933. Here was an opportunity. I could go to graduate school--I wouldn't get paid for it, but at least I would have a job when I came out. I finished a quarter and returned to work here at the lab. One time, I had an offer to go to South Africa. The South African government, through Dr. P. J. de Toit of Pretoria and Dr. Emile Brumpt of Paris, wanted me to come down there to attempt to control ticks by colonizing and releasing parasites in South Africa. My wife and I weighed all the considerations. We didn't have any children at that time--this was late 1933. Dr. Parker, however, didn't want me to go. He said, "I've got a place for you here. If you'll stay, I will put you in charge of all the tick rearing for vaccine making." That was getting to be an increasingly big job, and his offer was one of the things that convinced us to stay here. I have never regretted making that decision, because I visited South Africa several times since then. I didn't miss much by not going. But it would have been a very interesting experience.

The tick parasite project, however, didn't pan out. The life cycle of the parasite did not coincide with the life cycle

of a tick in such a way that it could be an effective control. We discovered that in the late thirties. Then the work was quite abruptly stopped, and we went into other things. Dr. Cooley retired from his job in Bozeman and became entomologist at the Laboratory. Later on, in 1946, when he retired from this job, I succeeded to his position. Very briefly, that is how I became involved here, and I have been with the Laboratory ever since, except for the war years.

Harden: Who else was at the Laboratory doing research during the thirties? Dr. C. B. Philip was, I believe.

Kohls: Yes, he came over from Bozeman in 1931. We had several University of Minnesota graduates--Jellison was here in the lab. I got a Master's there. We had quite a history of people going to the University of Minnesota.

Spotted fever research was going on. Q fever was also discovered about that time. Maybe Dick Ormsbee has told you something about that. That came in the late thirties. In the meantime, tularemia was coming to the forefront as a disease of considerable importance in this country, especially here in the West.

Harden: Yes--I believe Dr. Edward Francis came out here to work on that at some point.

Kohls: He did, but that was before I got here.

The work on ticks expanded during this time. One of my great interests was working on the taxonomy, the classification of ticks on a world basis. Our project was to build up the collection which Dr. Cooley had started and by the time he retired and I retired, it had become the outstanding world collection of ticks. The only one comparable to it was the one built by Dr. Harry Hoogstraal at the U.S. Naval Medical Research Institute in Cairo. We worked with Dr. Hoogstraal on many projects. With Dr. Cooley, Dr. Hoogstraal, and others, I wrote--some independently, some jointly--a whole series of papers on the classification of ticks on a world basis.

Harden: Was Dr. Gordon Davis working here then?

Kohls: Dr. Davis came just about the time that Dr. Philip came.

Harden: What did he do?

Kohls: He was a bacteriologist who got his Ph.D. from Johns Hopkins. He went to Lagos, Nigeria, to the Rockefeller Laboratory. Neil Philip was also there as an entomologist. They had a very small staff, so Davis and Philip got to know each other well. Davis was looking for a job after he left Rockefeller. Dr. Philip persuaded him that to come here. He came and he liked it.

Harden: Did he stay until he retired, also?

Kohls: Yes. He was here until he retired. He then went back east. He had a sister living in New Hampshire. He was an easterner and so he went back there. While he was here, he became interested in relapsing fever, which first got some attention in southern California around a resort district, Lake Arrowhead. They found that Ornithodoros ticks--which turned out to be a new species--was causing this thing. His main contributions to the work of the Laboratory here was his very thorough and detailed study of these ticks in relation to the spirochetes. He found that each species carried its own specific parasite that could be transmitted only by that species of tick. That spirochete couldn't be transmitted by any other species. They are very highly host specific but they all produce one disease.

Harden: During this period, what was the Laboratory's relationship to NIH in Bethesda? Was the connection tenuous--that is, was the RML fairly independent in its work?

Kohls: Yes. Dr. Parker enjoyed a great deal of latitude. We had complete support under Drs. Dyer and Topping. Topping was Dyer's number one man. Dyer was director of the National Institute of Health. We had good support from the people back there, and Parker was given very much of a free hand as director to choose programs and do just about as he wanted within budget limitations--which were set by Washington--personnel ceilings and that sort of thing.

Harden: And then the war came along, and you . . .

Kohls: The war came along, but before that, another field of work opened up. Herold Cox, another Johns Hopkins graduate--in fact a classmate of Gordon Davis in the Ph.D. program--was hired here. He was specializing in rickettsia, and Parker wanted him to work a way to produce the vaccine other than in ticks. Rearing ticks was costly. It was dangerous, and we sought a way to cultivate rickettsiae in large numbers in something besides ticks. So Cox got on that and discovered that he could grow vast numbers of them in the yolk sacs of chick embryos. He found they were loaded with rickettsiae, and they then became the basis for making a new vaccine. Tick vaccine then was phased out. In fact it was turned over to some drug company back east. They dropped it all together with the coming of antibiotics following the war.

Also, Cox's arrival coincided with the outbreak in the west of equine encephalomyelitis, which is transmissible by mosquitoes. Cox was hot on that. That was 1940. I was doing some field work at that time with Jellison. Our headquarters were in Rugby, North Dakota. They had formed a Burma-Yunnan Railway Malaria Commission, and Jellison was chosen for that. He went to the Orient, so naturally I took his place in North Dakota. So we opened up a whole new field at the laboratory working on encephalitis. Dr. Leo Thomas came. He has been working on this virus, on encephalitis, for a good many years. So we went through a whole period of that kind of research.

Antibiotics, however, changed the picture. It was felt that spotted fever was pretty well under control. Parker died in 1948 or 1949. He was followed by Carl Larson, one of Dr. Cooley's graduates who got his M.D. at the University of Minnesota. He

was a Commissioned Officer in the Public Health Service. He was interested in tularemia, but not in some of these other things. With Parker's death, people in Washington were kind of jockeying to see who would control RML. Each had his own pet project on which he thought RML should be working. Larson came out with his ideas on tularemia and some other things he had been working on, such as psittacosis. That, I think, was the beginning of the change away from the work of the historic mission of the Laboratory, which was at first tick-borne diseases, then expanded to include all kinds of diseases that could be transmitted to human beings by arthropods--mosquitoes, ticks, fleas, or whatever. That was the beginning of what you are seeing now in the last few years. With Burgdorfer's retiring and the rest of us retired, that old historic mission has gone down the drain.

Harden: Is that good or bad?

Kohls: Well, it's hard to say. There is a whole new generation of work coming up here. I've lived long enough and worked in this field long enough to know that there are such things as fashions in science at different times. There is a phenomenon called "getting on the band wagon--big findings are just around the corner." But often, if somebody else is two or three years ahead of you, unless you are extremely lucky and come in with a bright idea, you are going to play catch-up all the way along.

I can't criticize too much. I have a feeling about it. I think that skills are being lost because schools are not teaching the sort of things that we have done at this Laboratory. Our

skills were extremely valuable during the war. I don't know how many of us went into the service. We were very much in demand. Now, that's fading out, and there is a movement to go solely to bench work. With Willy's going, there is nobody in the Laboratory who could do anything at all like what was done here during the past fifty years.

It's all new--DNA and many other things--it's a change. I can't help but feel that something is lost that should not be lost. There are many pressing problems now--certainly AIDS is a very important problem now--times have changed. Maybe in five years, they will get control of AIDS. And something new will come up. I still, however, have a feeling that some of these skills, techniques, and knowledge that proved valuable some time ago will also prove valuable again. We are going to have a lot of catching up to do if we become involved, for example, in a tropical war. Many of us feel that there is still a place for the kind of work that has been done, but at the present time, it is not fashionable.

Willy Burgdorfer's work on Lyme disease is probably the last you'll see of that type here. I think it is a fortunate thing that he is retiring under such favorable circumstances. He's gotten a lot of attention from his work on Lyme disease--it was a fine piece of work. He has directed more attention to our lab--favorable, scientific attention--with his Lyme disease work than the lab has received from some of this new work.

Of course, I am not an expert in these fields. I don't read the journals, but I do know that RML is not the only place in the country doing this sort of bench work. I think we have lost a

reason for being. There was a reason for the Laboratory being here in Hamilton at one time. It was established here because this was the center--the hotbed of a serious disease infecting many people and causing economic hardship. There is no compelling reason for the Laboratory's being here now. No reason whatsoever, except that a nice set of buildings exist. The work that is going on now could be carried on in any university campus or medical school.

Harden: I want you to tell me a bit more about your work with the tick collection and about the cooperation with Dr. Harry Hoogstraal in Cairo.

Kohls: Harry Hoogstraal graduated along with Bob Traub. Ever hear of Robert Traub?

Harden: Yes, I visited him at the Smithsonian just before I came to Hamilton. He showed me his fleas. I learned more about fleas than I thought was possible.

Kohls: He'll show you more fleas than you would think existed! It so happened that Traub and Hoogstraal both got their Ph.D.s in entomology from the University of Illinois. I got acquainted with Traub during the war. He was in Burma working on malaria control until he was assigned to the Typhus Commission. Harry Hoogstraal was in New Guinea in the South Pacific. He also turned up in the Philippines. I never met him then, but Neil Philip did. Anyway, Hoogstraal was interested in ticks, but he

had never published on them. He was into something else. He became a field collaborator for the Field Museum in Chicago. When the war was over, he stayed on in the Philippines for about a year and collected ticks with a friend of his, who is now at the University of Arizona. He sent a great deal of material to me to be identified and studied. Because of this work, I turned out a first and only paper on ticks in the Philippines. That started Harry's interest in ticks. The Navy had a lab during the war in Cairo to make typhus vaccine. This was U.S. Naval Medical Research Unit 3--NAMRU-3. That was the beginning of Hoogstraal's interest in the Rocky Mountain Laboratory. After he got out of the Army, he went on a Navy expedition to Madagascar, I think, for six months, collecting animals, ectoparasites, etc. Over the years we had reams of correspondence, exchanged specimens, and the like. Ormsbee spent two years there on one project. A French lady believed that in certain circumstances typhus could be transmitted by ticks instead of lice. She stirred up quite a hornet's nest, but a lot of work done by Ormsbee and others finally showed it just didn't happen. There was something wrong with her experiments.

Harry Hoogstraal, however, continued to be the senior researcher at NAMRU-3. Then NAMRU-3 was expanded--it was always viewed very favorably by the Egyptian government. Even though there were periods when things got tight and tense--over the Aswan Dam, for example--NAMRU-3 was protected. It had no trouble whatsoever.

Harden: Is Dr. Hoogstraal still living in Cairo?

Kohls: Yes. On the verge of retirement, perhaps.

Harden: Your work, then centered on the taxonomy of ticks, while Dr. Jellison worked on fleas and mites?

Kohls: He worked on fleas. I worked on fleas, and I did some basic work on ticks and diseases they transmit--the classification, biology, etc. Bill and I together, he separately, and I separately did work on tularemia in water. Bill probably told you the story about tularemia in water samples here. I had a variety of things going on in research interests.

Harden: When did you retire from the Rocky Mountain Laboratory?

Kohls: The first of November, 1969. I became a Commissioned Officer in the Regular Corps in July 1949. When I came back from the war, my job had been kept open for me. I had been Civil Service then. They wanted personnel in the Commissioned Corps, however, and they made me an offer I couldn't turn down. So I joined in 1949. Half of my career was Civil Service and half Commissioned Corps. There is a mandatory retirement at the age of 64 in the Commissioned Corps, and that is why I retired in 1969.

Harden: Could you tell me more about your experiences during the war?

Kohls: The war years were especially interesting. Those of us who were able to follow our own occupation in the military were extremely fortunate. My entire three years was on research on scrub typhus--tsutsugamushi disease. I was a member of the first commission that went out. When I went into the Army in April, 1943, I was an instructor at the Army Medical School at Walter Reed. Neil Philip had gone in the year before. Several others from Hamilton had gone, because they wanted to get a field job as soon as they could. So I told Neil Philip, I'd take a commission. In fact, I had tried for one the year before, but I couldn't pass because of my eyes. I got a commission the in April of 1943. Neil Philip left and went to the Mediterranean and worked on sand fly fever. I succeeded to his job in Washington. I was there about six months at Walter Reed.

The Armed Forces Epidemiological Board formed a commission to study scrub typhus, a strange disease in the Southwest Pacific that was affecting troops. It had the military pretty badly worried. The commission was headed by Francis Blake, Dean of the Medical School at Yale. The four other members were Kenneth Maxey, professor of epidemiology at Johns Hopkins; Colonel Joe Sadusk, a medical officer who had been a student of Blake's; John Bell, and myself. We went to New Guinea in September 1943. The other four members came back to the States in December, but I stayed to continue research and to devise a method to prevent troops from being bitten by the very tiny mites which transmitted the disease. They said I could have anybody I wanted to help me. To make a long story short, they sent out young Lieutenant Bushland from the Department of Agriculture laboratory in

Gainesville, Florida, and he, under my direction, devised a method for treating the clothing with dimethyl phthalate. We simply dipped the clothing in that chemical and wrung it out. Even after five or six washings, the treated clothes they were still toxic to mites. The treatment was practical and effective in preventing the disease when properly used.

About that time we also started some other work. Neil Philip had written me that he would like to join me. He was over in the Mediterranean, so I did a turn about and got him out to the South Pacific. We worked together for the better part of a year.

Harden: Who was left at the Rocky Mountain Laboratory during the war? You were gone, Dr. Jellison was gone, and Dr. Philip was gone. Dr. Hargett was here, and I presume Dr. Parker was here. Was Dr. Cooley still here?

Kohls: Yes. He retired in 1946. I came back from the military about the middle of May 1946 and Dr. Cooley retired the first of July. I succeeded him at that time. He continued to live here in retirement--in fact, his wife is still here. She is ninety-nine. She could probably tell you some tales, too, but her hearing is not good. Her mind, however, is just as clear and bright as anything. She was Dr. Cooley's second wife; they married about 1924. I started college in 1924 and I remember meeting her in Bozeman at that time. Dr. Cooley had lost his first wife and his only son in the flu epidemic of 1918.

Harden: Is there anyone else about whom I should be aware during the war years?

Kohls: Gordon Davis went in. He got a commission. In fact, he took my place at Walter Reed when I went to New Guinea. That, too, is an interesting story. R. E. Dyer of the Public Health Service was the chief of the United States of America Typhus Commission, so he was vitally interested in the scrub typhus situation. There was a lot of rivalry at that time between the Department of Agriculture and the Public Health Service in matters dealing with insects affecting man and animals. The Public Health Service was interested primarily in arthropod-borne diseases, but the Department of Agriculture was interested in the animals and arthropods. So there was quite a bit of head knocking going on.

The Department of Agriculture wanted to send a man down to New Guinea to investigate this outbreak. Dyer, however, insisted that I was the one who was going to go down. I didn't know about all this fighting between the two agencies until afterward. John Bell had heard about it. He was in Washington with me, downstairs in another department. In fact, I think he was the one to say, "You had better start packing your bags." I said, "What's going on?" He said, "Dyer is favoring you for a job." Anyway, that's how it turned out.

When word came that they definitely wanted me, I went to my commanding officer at Walter Reed, Colonel Gallagher. I told him they wanted me for this commission to go to New Guinea. He just hit the ceiling. He started cussing. He said, "They are raiding

me all the time. I can't keep this department filled. I am not going to let you go. I've had enough of it!" He was storming around, and I said, "Sir, I would like to go." He said, "Can you find a replacement?" "Yes," I said, "I think I can." I mentioned Gordon Davis, who had got into the service but was stuck in a hospital in San Francisco. He hated it. So he was pleased to be asked to take my job. Later on, toward the end of the war, he, too, went overseas.

I came back from New Guinea in 1944 and was stationed right here in Hamilton on active duty. I lived at home and got back on my feet--I had lost twenty-five pounds overseas. This was not medical leave, however.

In the meantime, they had started up another unit in upper Burma for research on scrub typhus. Davis had gone out there, and I was also asked to go back overseas. Jellison had come back after three years in India. He often said, "If you ever go overseas again, I want to go along." I had no trouble getting things arranged. So we went back over together. Strange how--Bill has probably mentioned this, too--our careers have paralleled, in a way, ever since high school.

Harden: Yes, he did mention this.

Kohls: It was strange working in the same place, living close together, going to school in the same general area, and having the same fields of interest. It's just a bit unusual in a way.

When Davis joined the service during the war, that didn't leave very much here. The lab was smaller then. Dr. Parker set

a high priority for war work--he was very generous. He did not stand in anyone's way. He didn't raise a fuss about anyone's going or complain to Washington that his laboratory was being "raided." He cooperated fully.

Hilda Holley was Dr. Parker's secretary, and she kept a scrapbook on subjects of the field of the laboratory. and she had quite a collection that included personnel awards. I think that would be quite a source of history for you.

Harden: Yes, I am copying as many of those types of things as I can find. Thank you, Dr. Kohls, for talking with me.