

William L. Jellison Interview
Interviewer: Unknown
No Date

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Jellison: These are diseases translated from animals to man. And the forward in it is this, which you can see. And he gives me here credit for continued interest and suggestions. This is the fifth edition and while he was Secretary to the American Medical Society, headquartered in Chicago, he put out four editions of this. Only diseases transmitted from the lower animals to man, either on the title or in the forward. And I called his attention to my thesis and so on and sent him copies and so this makes no mention of any place of diseases of lower animals. So this is a copy of that and he gives me credit for helping him, but he doesn't say just what in particular. But there's been two or three editions since then and in no case again does he mention lower animals. I was told that was out of style.

Charles told you about the background of this. In 1941 I'd worked at the laboratory here for about twelve years and got involved in these animal diseases and so on, and tried to make sense out of them. Then I wrote a lot for this production of the Smithsonian Institution, a set of volumes about so thick, on sciences related to the Smithsonian Institution. Here was the classification of the animals by prominent people, in high up circles, the methodology and so on. And they said man is no longer the top of the evolutionary theory, he belongs way down. We have five digits left, we have a simple alimentary tract, we have thirty-two teeth, and so on. A horse that's reduced to one toe, one digit, a cow to two digits, many animals were greatly reduced in digits, and so on. And when you summarized all these characteristics, man just falls intermediate in the scheme. He's higher than the carnivores, maybe, but even the rodent, rabbits, hoofed animals, and to the limit, the whales, they go way beyond him in modification. This is the wrong theory of evolution. Well, I said, let's plot the diseases we get from these animals on that basis. And at that time, there weren't many known, there wasn't the force of diseases we know now. Back in 1941. Well, the animals higher than man in the evolutionary scale, there were 48 or 49 diseases, and from the animals lower, one. Rabies, we get from dogs, regularly. We get it from other animals. We get it from bats at times, domestic animals, and so on, but dogs are the primary source and we relate rabies to carnivores so one against fifty was pretty strong evidence to me. If you accept the theory.

Now, most of these animals, their temperature goes up. Dogs, cats, and so on, have a temperature about like man or lower. Many of them have temperatures that drop down for hibernating season, but when you go to domestic rabbits in the laboratory, to cows and horses, their normal temperature is 102, 103, something like that. Another evidence in the steps of evolution. They came from cold blooded animals, from reptiles, beginning and through process of evolution. And that was another piece of material not mentioned at all by the zoologists. Well, I put this stuff together and made charts and I sent in a manuscript to the American Society of Pathologists. They accepted it with some enthusiasm. Their meeting that year was in Chicago, so I asked

permission (the laboratory usually gives professional staff one meeting a year). At least, I had permission to go to Chicago and sent my manuscript along with it and they denied permission in Washington, D.C. for the Government to pay my way to Chicago.

Question: DID THEY GIVE A REASON?

Jellison: No. They said they wouldn't pay for my expenses, anyway. And I told Dr. Parker I am going to Washington, my paper has been accepted by the Society. I am going and you can do what you please about it. And then they found that there was an attic back there some place that reported some ticks and that they might sponsor me to go and inspect an attic for ticks and come back by way of Chicago and give my paper. I said, I am going to Chicago to give this paper, told them so. And then, after I gave it, the Chairman, that year's Chairman, Dr. E. Raymond Hall, a famous mammologist and so on, wrote and said it was the best paper at the meeting. And when it came time to get up and go to the podium to talk, I couldn't find my manuscript, but I had lantern slides. I went about it and I just talked. He wrote there, I have a copy of the letter, it was the best presentation at the meeting. Okay, I sent in the manuscript to Washington, D.C. for clearance for publication and a chief back there, Dr. Eick, who was a prominent mammologist and zoologist and so on, Dr. Armstrong, a close friend of mine, had been from his visits here and contacts in Washington, they said there are probably other factors involved, not just evolution. And they would not accept the manuscript or allow me to publish it. They would not permit publication, at least under Public Health Service sponsorship. It was too much of a shock to the medics and so on. So I put the manuscript away. I gave a talk about this to the IMCD meeting in Canada one time, extemporaneously rather. We were talking about animal diseases and I said I would like to speak a few minutes, presented it, but that is not considered publication. We say in our bulletins that we put out, report of a meeting, this is not considered, these papers are not considered publications. At an informal meeting, we discuss a lot of things that are not ready for publication, and so on. So that clears the way. Then we talk very freely about some things we are ready to publish. So, that was the way of it and I just put the manuscript away. And then, about three years ago, they called a meeting here at the laboratory to discuss rickettsial diseases and they sent out invitations to prominent scientists. That included Traub and so on. And he went to work on a paper on these animal diseases and he studied the same things I did. He had studied parasites. We know that the parasites are the primitive animals. The marsupials, and so on, are primitive. The people that studied tape worms said the parasites are the primitive animals. And, as you advance up the scale, these things become more advanced, and so on. In the flea people, they had it reversed for a while. And I straightened them out on that with a publication that was the moles and choose and so on.

A very elaborate parasite and then as you get up to the pickings and other animals that have parasites that are reduced one tongue a paratram, they hardly have a larval stage. It goes from egg to adult, a tiny adult. The adult burrows in the skin and it lives there. There is no ornamentation on it. It is a very simple thing. And here is the advanced one, probably a true parasite of the pig. We don't know for certain. There's one found on mice now. And that is the way that elaborate fleas with lots of cones and bristles, and so on, are the primitive ones and the rule in parasitism, with advanced parasitism, there is reduction, reduction until some of them, there is nothing but the parasitic stage. And, it is some of the worms, some of the tapeworms and

so on and growth in the parasitic stage. Fleas have a larval stage which is not parasitic, and with most of them, that is where most of the growth comes. But, in some things, growth is concentrated back into the parasitic stage. That is the easy stage for them, and so on. So, all these things fit together and when I proposed this and worked on it, and so on, things began to fit together. You ask your husband about Mendel's table, periodic table, you study it. That put some order into chemistry and things began to fall in line, and so on. I don't know how much he helped. But it made order out of chaos. Logarithms in mathematics, who would conceive of an idea of adding through multiplication. Who would get that? Somebody finally did. Very simple and no concept. So this was a new concept and I said it applied to zoonotic diseases and had world wide application and things will fall in line if we follow this classification. And things that there is no reason for otherwise, I never found anything that was completely contradictory to it. I thought it was time for some consideration at high levels for some recognition and it never came.

Well, three years ago, they invited these scientists here for a conference and Charles got on to this thing from his own study of fleas and this evolution of parasites and hosts. He worked up ninety pages, I had three pages and half a dozen lantern slides. He worked up a ninety-page thesis on it and was about through with it, turned it in and someone suggested, you know, Jellison had something to do with this way back in 1941. Traub was supposed to have been at that time in Chicago, but he took off in the morning to go to Mexico for a field trip with Strong and some of them and I missed him by a few hours. We had correspondence and so on, but I missed him. And someone suggested to him that Jellison had this idea way back. So he took down the telephone and called me and said, what shall I do about it. I said I have worked up this thesis independently, it started on the same basis as you did with the study of fleas. He said, shall I scrap my manuscript, shall I withdraw from the meeting, or what can I do? And he said, I'll put you on as junior author, if you wish, if that will clear it with you. I said, I'd be very happy to be a junior author, but I don't have the energy to devote to your manuscript further than that. I'll supply you with charts and so on that I have. We'll go over your manuscript and those things and that was agreeable. The Government paid his way here and so on. He gave this lecture up at the Laboratory and I went up and listened to it. He had the longest paper that was presented. Had to fight with him pretty near to get it cut down and it still is the longest paper in the present Proceedings. We have copies of the Proceedings, and I am on there as a junior author. We have talked about it many times since. He had some reception up there about it. Some questions, I guess. I didn't raise any questions or volunteer information at all while I was on it. He had been giving a lot of lectures to medical students about it and tried to call attention to the mammologist's and get some discussion of it at one of their meetings. Has made some progress. He told me he talked to one group of medical students. They said why didn't somebody tell us about this sooner? That was their response. He said he didn't know how he was going to be received and they said, why didn't you tell us about this sooner. Well, that is the status of the Traub story, made no bones about it at all. He and I stayed on friendly terms. I sent him a lot of literature sets pertinent to it. At one point, in Australia, where they have marsupials of many kinds, carnivorous ones, grazing ones, they have tree climbing marsupials and people are in contact with those. They live on ranches, they mix with the domestic animals. They shoot them if they become a nuisance at the time. There was a big kangaroo drive. And there has never been a case of disease in human beings attributed to contact with the marsupials that I can find.

Maybe, some place remotely they will discover oil but, so far, nothing prominent comes from marsupials, too low on the scale.

So many of these things just fit in, just like a new element would fit into the periodic table. The Public Health Service never took any recognition of this afterwards, but yet they manipulated, they paid Traub to come out here to talk. Well, that's the story of that. I worked with Traub later. I said, you have brought this thing to light. If there is any award or recognition given to it, I will not monopolize it. I said I will not go anyplace except sharing this fifty-fifty with you. You revived the idea. If anything comes out of this, it is fifty-fifty because the subject was dead, otherwise. Maybe, ten or fifteen years, twenty years, somebody will get onto this manuscript and make a new discovery. He was faced with a proposition like Darwin was when he proposed the theory of evolution. Darwin had his theory worked out, his paper prepared, and he received a paper from down in the South Pacific, another zoologist had the same thesis and would have scooped him. He said, what shall I do? This man is after me to present his thesis at the highest standing zoological meeting in England and I have the same thing already prepared. He said, what shall I do? So he decided that he would read both manuscripts and give due credit to the man back there and the President of the Society was indignant about anyone proposing evolution and he was condemned by the majority of society for proposing such a thing as evolution in the development of man. It took years and years and still there is a controversy in schools and churches. There are laws being passed by some states that you can't teach evolution in public schools.

Interviewer: I HAD FIRST HAND EXPERIENCE WHEN I WAS TEACHING SCHOOL. NOT DISCUSSING IT, JUST SAYING THE WORD.

Jellison: No. I had a biology teacher in grade school. She told us about evolution. She had had biology courses in college and so on. She talked to us about evolution. I took the story home and my family was quite upset about it. They didn't go to bat about it. They were upset about sewing this seed about evolution. You can't be a good biologist and not believe in evolution. But how far would the program of teaching evolution go with the present administration. Or even for the Public Health Service to endorse a paper that is centered on evolution. Traub and I are good friends and I will remain so, sending him specimens, sending him literature. He has been here. We have been in Washington. I never saw him until I was overseas on duty and he wrote me that he had joined the medical corps of the Army and was looking for an assignment. I said, well I'll give you a call and I'll pick you up at the airport one of these days. Maybe three or four or five weeks later, I got a call. Traub. I had never seen him and he says I'm over here waiting for transportation. So he joined us there, worked with us for a while. He was offered malaria control for the Asiatic Theatre under the royal family of England and was really Commander in Chief for operations over there. Stationed down in what is Sri Lanka now. Traub could have had the title of "malariologist" for the Theatre before he left over there. And then he worked with us on scrub typhus. We were a whole group, Cole and three or four others from the laboratory. Traub and myself had a laboratory in North Borneo when peace was declared. Now you keep this. This is just a clipping, somebody run on to it in an ad down in the South and sent it to Brenner years ago. The laboratory being established, just take that, rather interesting reading. Here is a Xerox copy. Maybe you'd rather have that for bulk than a whole magazine. Use it for your references.

A visit to the Rocky Mountain Laboratory by some South Americans who were very interested in the place. They had some spotted fever down there. This was an article by Frenkel on toxoplasmosis. For years and years Frenkel worked on it in graduate studies and professional work. When he came here to the laboratory, he was working on toxoplasmosis in human beings. They didn't know the cycle of it. It affects infants in the womb. The mother may not be ill at all and the infant is born with a damaged brain. Many, many cases of it. And they didn't know the cycle. They didn't know where they picked it up. It didn't infect the mothers. Infant after infant born with severe brain damage. Toxoplasmosis. He tried to work out the life cycle of it. About this time, I made a field trip over in Idaho and we set out mouse traps at night. Mice had been raiding a grain bin from an old forestry station there. There were lots and lots of mice and I brought back some mice in refrigeration. Started autopsying the next from the station. Somehow or other, he asked for a write-up on these views. by accident, four or five years ago.

Gene helped in solving the identification of typhus over there. I had been over there for twenty-seven months at one station, under Dr. Brown [sp?] from Michigan came down from another station with another doctor and said what is the trouble up here? We got a lot of dead in the hospital and they have a lesion on them. You've got an eruption and so on. And a high fever. They are practically all Chinese coming out of one of the Chinese camps and so on. I said, well you're dealing with scrub typhus up there. He said, well will you go up and work with us. I said, no, I've got my orders to go home or am expecting them in a few days and I don't want to hear a bit of it. I've been over here for so many months, nineteen months, or something like that. I've had it. I am going home when the orders come. You are dealing with scrub typhus. My friends and others were working in the South Pacific on scrub typhus. I knew the symptoms of scrub typhus. You are dealing with scrub typhus.

I took off. A Public Health Service man, Gilliam [sp?], was sent over there just about that time. He arrived to work with these people. Within a few days, he had scrub typhus and they shipped him home by air. He got to Washington safely, was hospitalized and they isolated scrub typhus from him, and called it the [unintelligible] strain for years. He started off after I left my station, I came home by boat. It took a month's time. When I got to New York by boat, Gilliam had been over there, gotten sick, come back, they day. I ran onto a strange bug in the intestines of the mice, outside of the intestine, little white granules. I didn't know of anything like this. I quickly turned it over to Dr. Frenkel, our pathologist at the laboratory at the time. It was right along his line. He got the thing going in laboratory animals, mice and so on and he was able to send it away. And it proved to be, what he later called *leishnoid jellisonide* [?] A number of publications since then said the isolation of this in the laboratory animal having a close relation of toxoplasmosis available, it will grow in laboratory animals. We studied and worked with it experimentally and so on. This led partially to the discovery of the cycle of human toxoplasmosis. That has been acknowledged in several papers. It doesn't state in so many words here, but it says the finding of this organism gave us a clue. Here are several reports where the cycle of toxoplasmosis, Frenkel is one of the authors. He mentions this other organism several times.

Frenkel left here and after that went to a medical school as a pathologist, teacher and research man. And working with others, they find that cats are the reservoir and it is human contamination with cat feces, some way into the food. It affects the mother and into the fetus. They solved that since then and there have been a number of publications on it and they all seem to be in agreement.

We ran onto another toxoplasmosis-like organism in mice here. Glen Kohls was in charge of our laboratory party over in Burma when the war ended. The chief had gone back to Washington, flew over, paid visits to China and so on. And Glen was put in charge. He immediately assigned one of the men, to work with me and we isolated the strain from in Washington, D.C. They had a strain isolated. I think that confirmed that they were dealing with scrub typhus. It was the first isolation actually made.

So, after being home eighteen months, I went to a medical meeting in St. Louis and ran into one of the generals knew. He talked to me and asked when I would be ready to go back. I said I'll go back whenever Glen Kohls is ready to go back. You know we had been in the South Pacific all this time on the scrub typhus. If Kohls will go back, I will go back. In a week or two there were orders, please prepare to go to Burma. So I went to Burma. That was about three months before the war ended. Several of our people were already there, Dr. Davis was already over there and established a laboratory. So, I joined the group over there.

One of the head men of the party, an entomologist, in a conference one night, "said there is a lot of B.S. to this scrub typhus idea around here that chickens are carrying it." That was his statement. The rest of us were convinced that it really was scrub typhus we were dealing with. And Coles had been working on it, fresh on the job there. He had been dealing with scrub typhus in the South Pacific. Mecky [sp?] was gone so this outbreak occurred in Ledo which was across one of the mountain ranges where they had set up their main laboratory. I told Kohls to leave me over at Ledo [Road], close to this epidemic and leave us alone for a while. We went to Ledo and Gene Hughes was up in the morning at four o'clock, setting out rat traps and harvesting. I made a trip or two with him but couldn't swell enough every morning. And we were taking the chiggers off and shipping them by air. There was air transport back and forth practically every day or every hour between the two places. They were shooting them into white mice and gerbils. In twenty-one days, we had shipped them material that had resulted in twenty-one isolations from chiggers of scrub typhus, which kind of dampened the enthusiasm of some of them. They had said this has to be tick borne, this has to be tick borne. They repeated numerous times, if we could just isolate something from ticks we will have this thing pinned down. It was chiggers, it was chiggers. This was a Chinese camp where they had been in training. The Chinese soldiers had participated in driving the Japs out of North Burma. They left these camps with food supplies for their horses, they left garbage pits uncovered, and debris behind after the campaign in North Burma. The Japs were eliminated, they came back, moved right into their camp which was grown up with grass. They had rats streaming over the place. It was just an ideal situation. They moved right into there and then they started dropping out with scrub typhus.

We set out a line of traps and we would hear traps snapping behind us with rats in them and then we got those and lots of fresh specimens to work on. In twenty-one days we had given them

material for twenty-one isolations. Henry Coe [sp] had probably isolated the strain but it wasn't confirmed higher up, but he probably had isolated the strain before we got there. That's this thing on Gene Hughes, you covered that story through him. He was up at four o'clock in the morning. If he waited any longer to become daylight, ants would get on the rats and take off the chiggers and our animals would be spoiled, not good for autopsy and so on. He attended to that. That was the success of that operation. Then pretty quick the war ended, and it is surprising how fast everything collapsed and airplanes were flying out people and trucks were hauling away the equipment. They were disposing of equipment and so on. Pretty soon we were on our way home again. We flew home. That was part of that episode and that is why I give you this stuff on Gene Hughes. He only had a high school education, never went to college. I guess he got busy working with Dr. Phillips in the laboratory.

Interviewer: THIS WAS THE HUGHES VIRUS.

Jellison: Hughes group of viruses. They named it after him. He took the initiative to study these things that Clifford and some of the others were collecting from sea birds, things like that. That's enough on scrub typhus I think. Scrub typhus was a killer disease in WWI and WWII. In the South Pacific, there were places where there were ten, fifteen, twenty percent mortality. There were islands that were evacuated. Glen Kohls was posted out there.

Interviewer: OH YES. I HAVE READ SEVERAL OF HIS. WILL YOU LET ME MAKE A COPY OF THIS?

Jellison: I've got a Xerox copy right here. I had half a dozen of those at different times and gave them away. There's the folder. Here's a Xerox copy, including the cover.

Interviewer: TERRIFIC

Jellison: On the last page, they already isolated the agents in human blood I think. They had not gotten it from all the other things. This is devoted to Paul Merritt [?] and he deserved everything that he got. Huebner never tried to belittle or anything pertaining to his contribution. It is right in there, he helped me and he was a commercial pest control man. He had a grade school education and so on. He had been in contact with the laboratory here, bedeviled them with questions on ticks and things like that. Then Kohls and Cooley sent him literature on ticks. He studied it. He'd gone to summer conferences on pest control people and was alert and active, an active mind, and when this thing broke he had been in charge of this big apartment development as a sanitary man, control man, and knew the place and so on. When this broke in the newspapers, they were having trouble there, he got permission to go in there. He went down in the basements and he found on the incinerator walls, close by, crawling up the walls, these tiny little mites. He scooped up some of them and took the train to Washington, D.C. He went to the National Museum where he was acquainted. He asked for identification on them. They gave it to him in very quick time. Baker was the entomologist there in charge of that group. Told him what it was, but said it is not associated with diseases we know of. He went back to New York with his little vials of ticks, little vials of mites. The Health Department was very slow in doing anything about this. All the cases were concentrated in this one apartment development. It wasn't contagious from one kid to another in school. They were calling it chicken pox to start with. They called on the Public

Health Service for help. The Health Department came in the office one morning and found fourteen or fifteen ladies at the door. People were ostracizing them. People won't come and see us. People in these apartments are getting sick. Our children are getting sick and it's time something was done about it. Well, they called the State office and the State office called the National, NIH, and they sent young Huebner up there. It was his first assignment outside of just laboratory work. He had just joined the Service. He hit it hard and he was there, and they took him, when he first arrived, they took him on a conference. Going up in the elevator, this man got along side of him and started pulling out documents from the National Museum and vials and specimens and so on. The health people there greeted him, they apparently knew Paul Moran, and when they got to their floor, they separated Pomeranz [Charles] and sent him down the hall and took care of him and took Huebner into the conference. After the conference, he said, I'd like to see that man who's got the vials in his pocket. So, they called Pomeranz in and he showed them his correspondence with the National Museum and identification and so on, told him where he'd found the stuff. So, Huebner just jumped right onto it. He took Huebner over and showed him these mites crawling up the wall. The way Pomeranz collected them with a bit of paper was, he stuck it on his tongue to moisten it and then got a mite and put it in the vial and another one and so on. From then on, the two of them were close together. Several others made a pass at the thing and they gave up. So the Public Health Service wired out here for any one of three people, Kohls, Phillips or Jellison. Well, Kohls was busy with some project. Phillips was busy with something. Jellison was last in line and my desk was clear, so I said I would go. I stopped at NIH, I don't know if Huebner was in before I met up with him but they sent me on up there and I got a room to work in. I stuck with him, sent specimens of live mice and live mites and blood samples from the doctors and so on down to Washington. Huebner would come up once a week anyway and check on things. We moved into a room in the basement of one of these apartment buildings. A group of women came in one morning for their weekly sewing work, Red Cross work. Here were mouse jars and microscopes all over the tables. They retired and left us alone. After a month's time, six weeks' time, Huebner showed up one morning and said, "well we've got it. We've got it from mice. We've got it from mites. We've got it from human blood and tests back and forth, cross-immunity tests and so on. Everything works out. We've got the organism. We can see it. We can grow it in tissue culture." I said, I think I'm ill too. I woke up that morning with a headache, and I had a scar, an initial lesion, right here.

Interviewer: ON THE SIDE OF THE FACE.

Jellison: If I inoculated myself, I would have put it there, but I didn't. It just showed up right there. He looked at it. So he took me over to the Marine Hospital. We'd been complaining about the sanitation at the apartment house, the abundance of mice in the basement where incinerators were partly burned. There was garbage and mice used to go in and out of the incinerators as those pictures showed and the mites on them and the garbage was partially burned. It was a very deplorable situation all around. When they put me in a bed over at the Marine Hospital, quickly somebody showed up with mouse traps and set them around my bed.

So, I was only in a couple of days and then I went to Washington for another day or two and talked to people there and so on. Huebner was present when I came on home. But it was a little rocky for a couple days. But nobody died of it. There was no record in the literature they could

find similar to this. Within a few months, the Russians came up with the same thing. But we beat them to publication and we turned out a whole series of papers. I think you'll want a copy. Here, there are two of them. Take them.

Interviewer: OH, GOOD.

Jellison: This man, Shanker [sp], tried to get in on the story. He was one of the first to suspect. People were calling it chicken pox, but it was limited to people from one apartment and so on. He got in on it. He had treated many of the first cases. He deserved some publicity on it. But Pomeranz was the man who stuck with us all the way and got those pictures taken. We went to Pomeranz after I took him through Yellowstone Park one time. We've been corresponding. I don't think we've had a letter in the last year. He died some six or eight years ago and he's gone. I had a notebook downstairs, a thick notebook of all this rickettsial pox correspondence, correspondence with Pomeranz and so on. I don't find it. I may have shipped it with my stuff to Miami University. I don't think I gave it to you.

Interviewer: NO.

Jellison: Because it was a notebook that size.

Interviewer: NO. I DO NOT THINK SO.

Jellison: I had a notebook that size.

Interviewer: I DO NOT THINK THIS IS IT.

Jellison: That's that. I had a number of copies of this. Huebner's retirement.

Interviewer: OH, THAT IS GOOD.

Jellison: Then, after this, a little while later, they picked up this Q fever in California and Huebner wanted to get in on that. He sent Shepard out there. He got a few blood samples from cows and they came out positive for Q fever. He was satisfied. He had demonstrated for the first time an endemic center of Q fever. The Q epidemic spread in packing houses, but they didn't know which cows, from what areas had anything to do with it. We'd had an epidemic in the military in Italy by that time and two epidemics in packing houses, two deaths in a Texas packing house from Q fever. But they didn't know what the source of infection was. This doctor in Downey, California was having these summer flu cases and he somehow got on to Q fever and he called the city health department and they said we can't help you. We don't know anything about it. They called the state health department and they said we can't help you any. So they referred it to Washington, D.C. and that's why Shepard came out and took a few samples.

Well, Huebner wasn't satisfied with that. He said, here is an endemic center of this. Human beings are getting it. Let's hit it and hit it hard. So he asked me to take a truck load of equipment from here and meet him in Los Angeles and we'd go to work. We stayed down there eighteen months on that. And we turned up over two hundred human cases by serological tests. Every

drop of raw milk that we picked off the shelves. The raw milk was leading in California from certified dairies. A thousand cows. Medical people in charge of the sanitation and everything and still legal then to sell that certified, cow inspected, workman inspected, service and so on. Every cc, we could get Q fever out of it by shooting it into mice. We could never prove that it was the cause of a single case. It was the people living close by the dairy. In the spring of the year, their dairies are swamps and cows are dirty. They are wading in mud. Concentration of a thousand cows in a few acres can't help but stir up the mud. Then it dries in the summer time and the wind comes along. And it was the people living around these dairies that had the Q fever, and workmen in some of the packing plants were positive too. We could never prove a human case direct from milk. We asked if we could use some volunteers from the state prison, something like that. And, no, no, we couldn't do that. But that's one of the things they ever turned us down on. Otherwise, there was complete cooperation and we worked there for eighteen months. Mrs. Jellison came down in the summertime. We enjoyed some of the visit down there. She had relatives down there and so on. We worked like hell. We would send a group of samples, either milk or blood samples, half to Washington, DC to Huebner's lab and half to here at the Rocky Mountain Laboratory, where Dr. [David B.] Lackman did the testing and so on, inoculations, serologic tests and so on. Within forty-eight hours sometimes we would have a reply back by telegram or telephone conversation. Check the NIH against the laboratory here. Sometimes, one would be first, sometimes the other, but the tests all tallied out.

It was a good study. It was written up, there were a number of papers resulting from that study and that added a great deal to Huebner's stature. He pressed this thing. We had as many as twenty people working on the sera. We got help from the state public health people down there. They ran surveys. We got lists of consumers from these dairies and the residents of the packing house people and packing house employees. We had veterinarians with us, cattle up and down the area there. Tremendous dairies completely surrounded by these residents right in town. Some of them were out in the San Fernando Valley too. We were out there quite often. But there was never any interference. A telephone call came in one morning and a lady asked, what about this milk epidemic. I said, well... But we never got much publicity. We didn't want any. We went down there at the time, Dr. Parker came down to visit at least once and he talked to the veterinarians meeting down there and told them about Q fever. We already knew about it. It was partly discovered here in this valley, you know. And talked to them about Q fever and so on. The doctor who was advisor to the milk association, I think I'm getting this right, got up afterwards and said, Q fever, this is all a hoax. This is just propaganda the Public Health Service is putting out. I know nothing. I've never heard about Q fever. This is not right at all. And Parker got up and read him quotations, reference after reference in the medical journals. Q fever this, Q fever that, Q fever here. But Parker was well prepared for him. We had excellent cooperation down there. Well, what have we got over here? This is just a... You can have that copy. There's a folder labelled "Ricketts." Elizabeth Ricketts. She died 1910, Mexico City of Mexican typhus. He was due back here within a few weeks or a month to work with Cooley's group down at the old laboratory that we took you to. Then they got word that he was sick and few days later, dead of typhus fever. Left a little boy and a little girl. This is the little girl. She married a doctor and here are Xeroxed copies of correspondence with her. I wrote to her and told her I established a museum out there and called it the Ricketts Museum, like the old lab. And she sent contributions to support it. Her husband is a doctor, apparently a prominent one, and these are Xeroxed copies

of the correspondence. That will fit in with your data. Here is a good copy of it. You know, Dr. Ricketts was the first one to get a strain of spotted fever growing in domestic guinea pigs, things like that. So he did take that back to Chicago and shipped some back so others could work on it in the laboratories. According to his notes, his strain came from Etta Bradley.

Interviewer: YES, YOU SENT ME THE WRITE-UP ON HER. AND I HAVE HER PICTURE, I HAVE HAD IT MADE UP INTO A SLIDE.

Jellison: This was signed by Etta Bradley McKinna, so put that there. That's a Xeroxed copy. I knew her for ten years before she ever mentioned the fact that she was the one that gave [unintelligible]. This is an older map. I don't know if you ever got a copy of that. I'm sure you have some of these. The Tannen Quick ? laboratory has been placed on the National Register of Historic Places. I have the certificate out there. This is an old newspaper, 1952, Parker.

Interviewer: TERRIFIC. YOU HAVE THE LIBRARY RIGHT HERE. YOU HAVE ALL THESE THINGS THAT I AM GOING TO NEED.

Jellison: Here is a picture published of Dr. Edward Francis. I was personally interested. I ran onto this some place. Your NIH certainly has a copy of this Ricketts scrap book.

Interviewer: YES, I HAVE THE NATIONAL LIBRARY OF MEDICINE. NO I DO NOT HAVE THIS LIST BUT I AM GOING TO HAVE IT

Jellison: You have it. This is a picture of the laboratory staff. You may have a copy of this.

Interviewer: NO, I DO NOT

Jellison: Do you want to take that?

Interviewer: YES. IT IS AN ORIGINAL, MINE IS A COPY.

Jellison: This is correspondence with people, history of Dr. Parker and so on. We are out of touch with Parker's son, we don't know where he is. He was in the trucking business in Utah. We've covered rickettsial pox. Any questions.

Jellison: In 1941, I had been here at the Laboratory for about twelve years and was interested in these diseases and some background study of them and I had the idea that the diseases transmitted from animals to man should be put to some kind of organized position. I started working on that and going by the modern classifications of animals as put out by the U.S. National Museum writers, literature and so on. Man is no longer placed at the apex of the evolutionary tree, but is about half way up the scale. The marsupials, the egg laying mammals, the carnivores are placed below man in this scheme, and from then on up, it's the primates, the rodents, rabbits, hoofed animals, ending in the whale. This is based on the idea of morphological change, from the accepted primitive ancestors, reptiles before the mammals.

Interviewer: LET ME INTERRUPT YOU HERE. NOW, WE WENT INTO THIS A FAIR AMOUNT THE OTHER DAY. BUT I WANT TO KNOW HOW YOU CAME UP WITH THIS IDEA THAT IT WAS RABBITS THAT MAINTAINED SPOTTED FEVER IN NATURE OR IN OTHER THINGS.

Jellison: My manuscript was not approved for publication in Washington, D.C. So, after retirement, I said I am going to apply this theory to one disease, a strict classification of mammals to one zoonotic disease. This, of course, was tularemia, which has so many zoonotic implications. It is carried by so many animals. In doing this, I found that our definition of tularemia is incorrect, that much of the publicity of the epidemiology of tularemia was incorrect and so on. Tularemia was defined as a disease of rodent, transmissible to man. I showed that over 70% of the cases of tularemia are transmitted from rabbits to man and rabbits and rodents are no longer considered in the same order of animals. This made it necessary to change the definition of tularemia. The distribution of tularemia, most all writers insisted, cotton tails, jack rabbits, snowshoe rabbits, all in the same important host of tularemia. And going into the statistics, I found that over 70% of the cases of tularemia in North America occur only where cotton tails are bred. There were only two or three acceptable records of transmission of tularemia from snowshoe rabbits to human beings. There are maybe fifty cases of transfer from jack rabbits to human beings, which is a little more impressive. But the great majority of cases come from cotton tail rabbits. This held up in all studies. In Canada, tularemia is very scarce, or was at that time and the only rabbit of importance is the snowshoe hare. Lots of studies have been done on snowshoe hares and they were not found to be an important reservoir of tularemia. This paper was published in the journal and then I revised it and refined it for publication in my book on tularemia which was published in 1977. When I completed this manuscript, I discussed it with Dr. Parker. He was considered joint author on the earlier paper which was published in a journal. He said, "Why don't you apply the same kind of a study to spotted fever?". I got out my maps on spotted fever distribution and animal distribution and we were right back to the cotton tail rabbit, especially in the West, to one species of cotton tail rabbit which was a duplication of the tularemia story. We were right back to the cotton tail rabbit. I compared lots of maps of other animals, suspected at one time or another. The Columbian ground squirrel, chipmunks, the pine squirrel, and so on. There was no animal distribution map which in any way compared the distribution of spotted fever in the western United States. But the cotton tail rabbit, almost perfect. Out of all the cases we have had on record, there were sixty some cases in territories where mammalogists had not recognized the presence of the cotton tail rabbit. One of these was out near Harpster, Idaho, where there was a very highly virulent strain of spotted fever was present, where they had had over thirty cases of spotted fever, many of them fatal, in that one area. Published maps in journals and books did not indicate that the cotton tails extended that far north into Idaho. I told Dr. Parker don't worry about this. If I can make a field trip to Harpster Idaho, I will find cotton tail rabbits. I have that in writing that I will go to Harpster, and I will find cotton tail rabbits. The trip was arranged and Dr. Kramis went with me. We drove to Harpster and near Harpster we ran into some boys. And I said, where can we go and find some rabbits. They said, you go through town, and about a mile out there is a pine grove and you will do very good in finding cotton tails. Within fifteen minutes, we had gotten there, we scouted the area, we flushed a cotton tail rabbit. We bagged him with a shotgun. We put it in a sack and we brought it home with us, showing that cotton tail rabbits were in that area. And this wiped off fifty-two cases of spotted fever out of a total of

sixty-four, which were recorded as occurring beyond the limits of the cotton tail rabbit. I wrote this paper up and sent it in for publication. I had the official maps of each state, indicating the range of the cotton tail and indicating in each county, the number of cases of spotted fever that occurred there. Taking individually, map after map, it emerged as a very striking presentation. The manuscript went to Washington, D.C. for editorial approval and so on and the editors there consolidated all of my state maps, twelve of them I think, into one map of the western United States and at least made one significant error in which they put six cases of spotted fever in Glacier Park, which has never had a case of spotted fever.

The publication did not receive much attention. We've been looking for the reservoir of spotted fever for some forty years here in the Bitter Root Valley. Various animals have been designated and so on, but this was not accepted as the answer and very little fuss was made about it. Some scientific work was done here at the Laboratory, testing the cotton tails as a possible reservoir of spotted fever by inoculating animals and testing their blood for infectivity and so on. They came up with the conclusion that the cotton tail was not a big host to the spotted fever and that report was published. I knew about the work, had been familiar with the tests, knew the people who did it. They asked me to go on the authorship of it, and I consented, which I think now, was a mistake.

Since retirement within the last couple years, I got back to this manuscript and reviewed it, and checked it for information, supplementary information and so on to support my thesis and I made sixteen Xeroxed copies of my manuscript which totaled my first manuscript, which when published totaled three pages in a Public Health Service report, and I enlarged this to seventy-three pages with statistics, documentation, including a map of the western states where spotted fever is present. I did not submit it to high levels in the Public Health Service, but to my scientific associates who were familiar with my work. There was much correspondence about other scientific work and I still have copies of that manuscript in my files at home. I said it totals seventy-three pages of letters, documentation, maps, individual maps of these western states and so on.

Interviewer: ONE OF THE REASONS I ASKED YOU ABOUT THIS IS THAT IN A NUMBER OF ARTICLES THAT I HAVE SEEN, THESE REVIEW ARTICLES ON SPOTTED FEVER, THEY TEND TO SAY SPOTTED FEVER CORRELATES WITH THE RABBIT AS THE HOST. I THOUGHT YOU HAD DEVELOPED THIS WHOLE IDEA. IT SEEMS TO HAVE BECOME VERY ACCEPTED THAT AT LEAST THERE IS A CORRELATION WHETHER ANYONE WANTS TO CLAIM IT IS THE HOST OR NOT.

Jellison: There has been very little response to me directly. At the time of the 1941 publication, there was a paper in a popular natural science journal and showed the map for disease distribution and so on is not new, it was used in the study of sleeping sickness very early in Africa. Maps for the distribution of certain species of flies which carried the disease in the number of cases of human disease. It has been used by many other writers but not particularly in this instance. The distribution of the tick, the vector, does not match the distribution of spotted fever. There is a great area in western Canada where ticks are abundant, where they had many cases of human and animal tick paralysis. They collected ticks up there. They studied these epidemics in animals and

yet cases of spotted fever are extremely rare in Canada except right close to the American line. The maps cannot match the tick distribution beyond this line and spotted fever. It was not necessarily a new technique but had not been applied in this particular way to the study of spotted fever. We were looking for a possible reservoir for thirty, forty years. Did you see Dr. Ribí? You had a session with him?

Interviewer: YES, I DID.

Jellison: Here are his publications with Dr. Gordon.

Interviewer: OH, GOOD.

Jellison: This is a Xeroxed copy of an article on tularemia in Martha's Vineyard. It was also written up in the New Yorker. I don't think you have a copy of it.

Interviewer: I HAVE A COPY OF IT.

Jellison: Medical specialists in that country, in that area, were stunned because of the event they had, the epidemiology. They never realized that tularemia was often the cause of pneumonia. It isn't a common cause, but it is a cause of pneumonia. And some of those experts should have realized it and at least made some easy tests. By this time, some of the patients were recovering and a blood test would have shown it up in five minutes' time. They called in CDC and the CDC people worked diligently on all aspects of it. I think they came up with the right answer and saw got an amount of credit, publicity about it. Roueché wrote that up in the New Yorker. It is one of his last papers. His first medical writing was rickettsial pox.

When I first came here, I was often called in to Dr. Parker's office to talk with him, he had an office in the back corner, way back there on the second floor. Filled with jars and jars of specimens on his shelves, labelled fleas. He was not much interested in ticks and fleas since an early paper on fleas in the Bitter Root Valley. And this stuff had been stored away, worked over animals and saved the ticks. The fleas were put into vials and put into jars. There was a whole row of them. Somebody at the laboratory, Dr. Kohls and others were already working on ticks. One of our laboratory men, Dr. Philip, was working on biting flies, but no one seemed to have any interest in fleas. I said, this material is worth saving and it is worth studying. I was not a graduate in anthropology but I took considerable anthropology, knew some of the principles. I said, here is something to work on. So I started working over the jars of fleas and was able to identify them guided by Parker and Dunn's early work and literature that was available. And became quite proficient in the study of fleas. When I left for an overseas trip and spent overnight in California, a student came over to meet me and said, "I just wanted to look at somebody that could look through a microscope at a flea and tell what it was. "And yet, one of the head flea men in the West was at the University of California, close by at Davis, and was accepted as the expert on fleas in the Western United States.

We got a little proficient with fleas when there was bubonic plague in Montana and of course, with our field work and so on, we were delegated to go check on this lobar bubonic plague in Montana. Then Wally and I left here one afternoon to go to Billings and contacted the proper

people. The next morning went into the field and got the first animal we picked up was dead in the rump of the road. I assumed it had been hit by a car but I put him in a sack for autopsying. That evening, we autopsied a couple of other animals. I opened up this animal and here was the perfect picture of either tularemia or plague. They are not distinguishable by gross autopsy. I telephoned back to Hamilton and said, I want some guinea pigs here by daylight or else we will deliver this animal to you tonight.

Dan Wylie took the animal. We packed the animal, we refrigerated it and he started out over the rough hills and was here in Hamilton before daylight. He brought this animal over to Dr. Davis and Parker. They made inoculations and soon confirmed the animals over there were dying of plague. An epidemic swept through the Valley from the southern border, the neighborhood counties, clear up near Dillon, and thousands of ground squirrels died. We picked up in our survey, later that summer, we picked up eleven plague positive ground squirrels in one day. Plague was present in Montana. This was four hundred miles northwest of any known bubonic area of plague. So from then on, fleas were studied. Fleas are the carriers of plague. They killed a quarter to a third of the population of Europe and England at one time.

Interviewer: DID ANY PEOPLE HERE IN MONTANA GET PLAGUE FROM THIS?

Jellison: There was no proof of any human. I got sick when I was working over there. I had an accident in the field. I put in forty-nine days in the hospital at Dillon and came out of it and recuperated slowly, they made cultures of my blood and got an organism. They would not accept it at the plague laboratory in California as being found plague oriented. What I had, I don't know, peritonitis and was in hospital for forty-nine days.

Interviewer: IS PLAGUE STILL ENDEMIC IN MONTANA?

Jellison: Now and then, there is a reference, and now and then the newspapers say we don't know of any plague in Montana. We haven't plague in Montana. We have publications on this, it was well known and they have picked up the plague from fleas, collected from animals in Montana in recent years. The Public Health Service, a crew from California, said they found plague infected animals here in the Bitter Root Valley. It has been found clear across the line into Canada. About this same time, they recovered plague up there. They had a survey crew out and they recovered plague clear across the line into Canada from ground squirrels and wild animals. No confirmed case up there. We have had cases. I was called in on a case near Boise, Idaho of human plague that was fatal. There have been cases in Oregon from wild animals and cases in Utah, a few from wild animals, and so on, but no valid human case contracted in Montana. About three years ago, there was a young man who stopped through Hamilton, was taken ill, hospitalized and one of the new doctors in Hamilton who was familiar with plague, diagnosed him as plague. The diagnosis was confirmed, the young man was treated, survived and in a report to the city health department, he was also the city health officer, said nothing unusual in the way of infectious diseases in Hamilton in the last month. I took that and called it to the attention of the city council. This is the first documented case of plague to be identified in Montana, not contracted here, he came from Arizona, but that is no doubt where he got it. There are about eight cases of plague in the Western United States in a year and to say that a case of bubonic plague in Hamilton was nothing unusual in the way of infectious diseases was a little bit odd. I presented

that to the City Council and in the next monthly report, it was corrected by Health Officer that they had. Now, about the same time, there were five or six cases in the Western United States. One of them was a schoolteacher, teaching school, and she came down with plague at her. And bubonic plague can become epidemic. We have had two terrible experiences in California, with one twelve, the other fourteen visitors and families of a case of plague down there. Died of plague. Case in Los Angeles, thirty-two out of thirty-four, I think is the statistics, visitors, doctors, nurses, attending the plague case that turned into pneumonia which makes it contagious. So, that was pretty terrible. When you spot a case of plague, you try to isolate it. The doctors over there thought I had it and it was a little difficult to get nurses to come and take care of me. Two, one of them a very old gal, came and helped take care of me and my sister came. She was a registered nurse.

Interviewer: YOU HAVE HAD A LOT OF THINGS.

Jellison: From then on, I got interested in fleas and in cooperation with an anthropologist, Dr. Good, at the San Francisco plague laboratory of the Public Health Service, we published a review, an index of the literature on fleas in North America. I followed that up with two supplements with the help of my laboratory assistants to do a lot of this paperwork. Two supplements, indices of fleas of North America. And those were published and since then, I have worked consistently or quite a bit on fleas. At this time, I have sent about 20,000 slides of fleas to the U.S. National Museum. That was the first report, 12,000 slides. They were laboratory property. They moved at home with me when I left here, and then as time permitted I sorted this thing out and sent them to the National Museum in Washington, D.C.

And then a few years ago, my friend Charles, quit his job at the medical school at Maryland and was appointed Curator of Fleas at the U.S. National Museum [Smithsonian Institution in Washington] I have been in association with him since about 1940 by correspondence, field work and so on. Since he took over the fleas there, I sent another approximately 10,000 specimens to him that have been incorporated in the National collection. So, I continue to work on fleas and was accepted as one of the authorities on fleas.

In *Ripley's Believe It Or Not* a long time ago, it was published that Rothschild, the flea enthusiast at the *Tring Museum* in England, a very wealthy man, offered a reward for a flea from a grizzly bear. He finally got one from some trapper in North America and Ripley said he paid ten thousand pounds for that one flea. It was the most valuable specimen on record. His daughter, Miriam Rothschild, denied that they paid that much but she has not come forth with a statement as to how much they did pay. About four or five years ago, a student down near the University of Montana was examining grizzly bears and autopsying bears and probably examining bears' nest material and he sent me a vial of fleas with about twenty grizzly bear fleas. I would like to expose them.

Interviewer: ABSOLUTELY.

Jellison: Dr. Kearns mounted some of them. I saved the other specimens in alcohol. I sent more specimens to the *Tring Museum* to my friends over there. I sent specimens to Dr. Traub at the

National Museum [Smithsonian Institution in Washington] and I still may have one or two of these bear fleas.

Interviewer: I thank you.

End of Transcript