This is an interview with Dr. E. John Bell at the Rocky Mountain Laboratory August 5, 1985. The interviewer is Victoria Harden.

Harden: Dr. Bell, please begin by giving us a brief account of your background and how you came to be at the Rocky Mountain Laboratory.

Bell: I was born and raised in Helena, Montana, and went to high school there. I left in 1928 to go to college. During my high school days, I worked in the laboratory of the Sanitary Board in Helena, washing dishes, making media, etc., and I heard much about the Rocky Mountain spotted fever laboratory in Hamilton. I finished college at North Dakota State in 1935 and came back to Helena to work for the Sanitary Board. I was advised by some of the men there to go on to college and get a degree in microbiology. So, I went to the University of Minnesota in 1936, I believe, and got a Master's degree in microbiology in 1937. At that time I had no money to get back to Helena. I was lucky to obtain a chance to drive a truck to Spokane, Washington. It was wintertime--November--and on the way I decided to stop off at the Rocky Mountain Laboratory and see just what it looked like. When I finally saw the place, I loved it. That's where I wanted to work. So I talked to Dr. Herald R. Cox, who had been there just a short time, I believe. He was very interested in the fact that I had a degree in microbiology and that I was anxious to work. When I first started out, I believe I made about $80 a month. I was just very happy to get there, but I also wanted to get married. My wife was secretary to the Attorney General in Helena. She made about $175 a month, so she wasn't very anxious to get married. I went in "crying" to Dr. Parker about the situation. He said, "O.K., John, I'll raise you to $150." That was great--we did get married. After about two months, however, Dr. Parker came in to the laboratory where I was working on cultivating newer strains of viruses and Rickettsia in eggs. I was wearing my cap and gown, because everything had to be sterile--we didn't have penicillin to treat accidental infections or dry ice to freeze the cultures. Dr. Parker walked back and forth, coughing. Finally, I said, "What's the matter, Dr. Parker?" "Well, John," he said, "Civil Service says I can't raise you so many grades, so I have to cut you back to about $120." When I told my wife, she hit the ceiling. She said, "You tell him you're going to quit." I said, "No, he would just say, 'Goodbye,' and then what would we do? We'll just have to go to school." That summer, Dr. Cox arranged a scholarship for me at the Johns Hopkins School of Hygiene and Public Health. We left in early May, going first to summer school in Ann Arbor, Michigan, where I took anatomy and neuroanatomy. Then we went on to Hopkins. That was 1941. War broke out in December, so I finished that year and then joined the Army.

Harden: Before you go on, I'd like you to go back a bit and talk about working with Dr. Herald R. Cox when he was developing his egg yolk technique. Do you remember any of the things that happened when he was working that out? Were you here before he developed the technique?

Bell: Yes, I was working with Dr. Cox before he developed the chick-embryo yolk sac method of growing rickettsiae.

Harden: Is there anything in particular that you recall about the technique of egg yolk sac culture?

Bell: In early methods of using fertile hen's eggs to cultivate viruses and rickettsiae, the inoculum was placed on the allantoic membrane through the side of the egg. Spotted fever and typhus rickettsiae did not grow will in this tissue. Dr. Cox decided to try inoculating suspensions of the spotted fever agent through the air sac and directly into the yolk-sac tissue of the eggs. Guinea pigs injected with infected yolk sac suspensions developed spotted fever with high temperatures, typhical scrotal swelling and necrosis. However, we had some difficulty finding the rickettsia in stained smears of the infected yolk sac tissue. For some reason, which I don't recall, Dr. Cox let some infected eggs stand at room temperature three or four days after the embryos died. Microscopic examinations of smears of infected yolk sac from such eggs showed numerous spotted fever rickettsiae. Dr. Cox then turned to this method in his research on spotted fever vaccines. There was a big demand for typhus vaccine for the war, so most of the studies were directed toward it. My job was to maintain the rickettsiae cultures in eggs. I do remember Dr. Norman Topping coming to RML. We showed him slides of typhus organisms, but he believed they were just bacteria. Being young--twenty-eight--I was very upset about his opinion. I wondered if I had been doing a bad job of maintaining the cultures, even though I knew the guinea pigs had come down with typhus fever after injections with the rickettsiae cultures. We talked that over, looked at pictures, and discussed the pleomorphism of the organism. Fortunately, Dr. Cox had great faith in what I was doing, and, moreover, we didn't get any cultures in bacteriological media. So we went on with those studies. Dr. Cox was working on a method that would get rid of the yolk sac tissues and leave just the organisms, but he was having much difficulty. We were using phenol and other similar things. Then Dr. Craigie in Canada, in studies with typhus fever rickettsiae, came out with a method of using ether, which really separated out the materials very well. A number of people in addition to us were working on the typhus vaccine--people in

Canada, at Harvard, and at the NIH. Dr. Topping and Dr. Henderson were working at NIH.

Harden: Let's go back to where you left off chronologically. Would you tell me what you did during the war? You were with the U.S.A. Typhus Commission, I believe.

Bell: Well, at first I wasn't with the Typhus Commission, but Dr. Topping arranged to send me to New Guinea. Dr. Glen Kohls was there studying mites, and I was supposed to isolate strains of scrub typhus rickettsia so that we could bring them home and supposedly make a vaccine overnight. I was there for about four months. I isolated strains from human cases and from mites that Dr. Kohls would give me, and I brought them home. We had no way of bringing the strains back except in mice, so I carried all nine strains in mice, which I had to look after on the airplane. I had colored all the mice with different dyes for the different strains. When we were in California, we got stalled, and the people would come out to the field where I was to see the Easter-colored mice.

Harden: I suppose if people knew what you had in those mice, they avoided you!

Bell: Yes, especially Col. Plotz at the Army Medical School, Walter Reed Hospital, who put me in a room by myself. Well, it was rather dangerous. I think several researchers died after sticking their thumbs with a needle or some similar accident. But I was young and really didn't worry about it. Dr. Henderson at the National Institute of Health in Bethesda died of scrub typhus. He used the Waring blender to grind up some of the yolk sacs, and he must have breathed in some of the organisms.

Harden: After the war, broad spectrum antibiotics were developed that controlled rickettsial infections. Were you involved in any of the early clinical studies on them?

Bell: No, but Dr. C.B. Philip was. I went back to Hopkins to finish my doctorate and was there until 1947. Then I came back here. I worked for Dr. Parker for awhile, mainly on Q fever. The disease was being studied in California by Dr. Robert Huebner from NIH and Dr. William Jellison. Most of the work that we did was on trying to find out what tissue in infected cattle was harboring this rickettsia.
Dr. Herbert Stoenner came to RML about that time. We worked together on Q fever. I still show the little scar he gave me when he was autopsying a cow and mistook my knuckle for a node.

Harden: Did you catch the disease?
Bell: Oh, I probably had it. We worked with Q fever while I was in the Army, too. At one point I was in New York and felt very sick. I have a hunch that's what I had.
Harden: Did you ever take the Q fever vaccine?
Bell: I took it later.
Harden: Did you have any reaction to it?
Bell: Oh, you bet!

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Harden: Dr. Richard Ormsbee showed me the lumps he still has on his arms from that vaccine.
Bell: He still has them? I don't think I do. When we vaccinated cattle, they would get a lump about four or five inches square and maybe an inch and a half or two inches thick. It was really quite a reaction. When our formalinized Q fever vaccine was injected intraperitoneally into guinea pigs the spleens became much enlarged, and their sizes varied with the dose of vaccine injected. We attempted to use this observation as a method to aid in standardizing various vaccine preparations. That was the main work until Dr. Parker died and Dr. Larson took over. He changed things quite a bit. After Dr. Parker died, I went back to studying spotted fever organisms. I was studying different strains that we could get out of the valley. Some were severe disease-causing strains in guinea pigs, while others gave maybe a day's fever and nothing else--although they immunized the animals against spotted fever. Dr. Cox was studying a strain similar to what we could get in the valley that didn't cause any disease in guinea pigs. It was called the Iowa strain. There was much talk about making a live rickettsial vaccine against spotted fever. But we didn't have antibiotics, and I guess everyone would have been afraid to try it in case the thing would act differently in humans.
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Bell: There was a young man from California who later became a professor at Hopkins--I can't think of his name just now. After I found some strains, he came out here and repeated all the studies and found the same types of strains I had. I never got credit for this.
Harden: Could we talk about the viruses on which you worked? Were you working on polio during the 1955 polio scandal?
Bell: I did some work with Dr. Carl Ecklund. This was mainly on the recovery of live polio virus from commercially prepared poliomyelitis vaccines used for human vaccination. I did a lot of work on ECHO viruses.
Harden: What are ECHO viruses?
Bell: They are often called "viruses in search of a disease." They infect the human intestinal tract, are mainly found in sewage, and cause many different diseases--flu-like diseases, diarrhea, etc.
Harden: Did you work on these viruses during the period when RML was concentrating on "community studies?"
Bell: Yes, this was what Dr. Larson started. But these ECHO viruses had been found before, and I did some work with Dr. Ormsbee on them. This was something we had to do at that time. Dr. Larson wanted everybody to cooperate.

Harden: Yes, I understand that was the directive that came down from Bethesda. Dr. Joseph Smadel was interested in this, I believe.
Bell: Smadel was the name I was trying to think of earlier. He was first involved in the study of antibiotics. Dr. Philip was overseas with him. And there was a Dr. T. E. Woodward from the University of Maryland.
Back to the viruses--I worked with them when tissue culture was fairly new. When I did my thesis work, I studied with Dr. Howe and Dr. Bodine of Johns Hopkins Medical School. My work was on antibodies to viruses of poliomyelis in the nasal pharyngeal secretions. That's how I got into polio, and of course tissue culture came after that. My experiences with the other viruses began with isolations that we were supposed to make for Dr. Larson and with my interest in tissue culture. So there wasn't any particular outline program for me.
Harden: On what other things did you work?
Bell: I did considerable research on a strain of the spotted fever group rickettsiae found in eastern Montana. In the area studied there occurs together two tick species: D. andersoni and D. variabilis, which are vectors of the rickettsia causing spotted fever in humans. D. andersoni is found only in the western United States, while D. variabilis (also called the "dog tick") occurs in the central and eastern U.S. In earlier studies four different type-strains of spotted fever group rickettsiae were isolated from D. andersoni in the Hamilton area, while 90% of

spotted fever isolates from D. variabilis in the eastern U.S. were of a single virulent type. Our objective in the eastern Montana studies was to see if the two tick species occurring together and thus under similar environmental conditions harbored rickettsiae of the same types and in the same ratio as was found in the Hamilton area. Instead, however, we found only a single spotted fever type strain, which was apparently different from those found in the Hamilton area or in areas of the eastern U.S. Many meadow mice and deer mice were shown to be infected with this strain in eastern Montana as well as ticks which were feeding on them.
I got into studies of murine thyphus while I was studying ticks in the Crow Agency in eastern Montana, east of Billings. tested several humans there trying to see what I could get in the way of spotted fever antibodies. I also tested them against murine typhus for some reason or other. Numerous people had high titers for murine typhus, but there was no apparent disease prevalent at all. So I did studies on that with Dr. John Munoz and found that the murine typhus toxin neutralizing substance in people who had never had the disease was not in the antibody group of serum proteins but in the purified lipoproteins. Of additional interest was that the toxin of epidemic typhus rickettsiae was not neutralized by those lipoprotein fractions which neutralized murine typhus rickettsiae. This was certainly a fundamental difference in the two rickettsial strains and an observation which should have been researched strongly, but I retired soon after that study. After my study was published, I happened to see in the literature that Dr. John

Fox had had trouble when he was studying typhus vaccine in South America. He tested the serum for murine typhus, and he found that people who should not have antibodies were neutralizing this rickettsia. So this was not just an isolated observation of mine. However, he seemed to have ignored his results.

Harden: Thank you, Dr. Bell, for talking with me.