Dr. McWilson Warren Interview

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Dr. McWilson Warren Interview

Slater: This is Leo Slater, today is May 26th, 2005. I am here interviewing Dr. McWilson "Mac" Warren at his home in Grafton, New Hampshire. This is part of my Stetten Fellowship on the history of malaria research at NIAID. I just wanted to confirm to you, Dr. Warren, that we will be taping our conversation today.

Warren: I understand.

LS: I thought we could start chronologically -- maybe with a little bit of your family background and early education, your interest in science? I gather that you're not from New Hampshire?

MW: No. [laughs] I grew up in a small farming community in eastern North Carolina on a tobacco farm. My father was a manager of a tractor and equipment dealership in Clinton, North Carolina, and we had a farm outside the town. That farm was inherited by my father from some of the original land grants that came to the Scots who migrated to that area of North Carolina after the battle of Culloden. They established themselves in this area. It's a matriarchal society so that I don't know my father's family very well but my father's mother's family are extremely familiar to me. I grew up there, went away at 17 to the University of North Carolina. I was a little bit lost, I suppose, in terms of what kind of career could I envision for myself. Of course in the late '40s, people were very conservative. You were looking for something stable that brought you a check every month.

Having grown up in the south in the 1930s -- I was born in August 29th, 1929, and in October of '29, the world came to an end. Not so much with us because rural residents in the south didn't have any money in the stock market in the first place -- they had no money. But the general collapse was just devastating. My father earned scrip for almost a year and a half after I was born and we had no money. He got scrip that my mother could take to one dry-goods store and one grocery store. The only thing she bought at the grocery store would be coffee and flour. The corn meal came from the farm, and the meats were cured on the farm. We were desperately poor, so that I grew up thinking, "I want security." Career? Those issues were not part of what I thought were important. I wanted a stable life with a stable income, but that was part of my generation. If you talk to other people who are my age, you're going to find that this attitude is a dominant feature in our lives.

LS: Were there other things that led you to science? Did you have a teacher in high school, or when you went to UNC was there somebody particularly inspiring? Why biology?

MW: I had considered going to medical school but my grades were not good enough for that. I did have a course in parasitology in the Department of Zoology at the University of North Carolina. I don't remember the professor, but I remember the extraordinary aspects of the lifecycle of the schistosomes. Biologically I thought, "My God! That is the most incredible thing. These critters get this all put together with these different stages in the snails and so forth. And it all works!" I remember being so fascinated with parasitology, and that's when I met John Larsh. As I say, I was a very dilettantish type student. I did better in European History and so forth than I did in anything else, I guess. But I took the GRE, and I performed very well which canceled out a lot of the other things. When I went away to the University of North Carolina, it was

$40.00 a quarter tuition, that was all. But we had so little money that my parents couldn't afford it. I had to work. I worked as a salad chef in the dining hall. You didn't get money, but if you worked one meal you got to eat three, which was a big help. I also worked in the medical library three nights a week. That paid money -- 50 cents an hour. [laughs]

I was supposed to go to the Navy when I finished my Masters [1952]. I was deferred until I finished, and I was all prepared. I prepared to go to the Navy, but then they examined my ears. I had a bilateral mastoidectomy when I was five. I started having middle ear infections when I was about a year old, and then I had these episodes every winter and my eardrums had been ruptured many times. As a result, I was not eligible for the armed forces. So I was really caught without a job. That's when John Larsh found me employment.

I went to work for a short period of time for the Carolina Biological Supply Company in Burlington, North Carolina -- not stellar part of my background. Dr. Powell (the company owner) wanted a parasitologist to go out and track possums and take parasites and make slides to sell. I did that, and I was reasonably successful at it, but I thought, "Oh Christ, there must be a better way to earn a living." I hated the job, just hated being in Burlington, North Carolina.

So I went back down to Chapel Hill, which was not far away, to consult with John. John Larsh was one of the true gentlemen in this world. He was a very kind, sensitive, responsive man, just a super guy. He said, "Well, let's see what we can do." And that's when I got involved with Martin Young and the NIH malaria program. Suddenly I was doing something that I felt was worthwhile. You need a chance to think you're doing something that's more important than just being there.

I had received the master's under John Larsh at the University of North Carolina. And I became intrigued by malaria and the biological complexity of parasites, it just overwhelmed me in that first real exposure with John Larsh. My first real experience with malaria was in Martin Young's NIH laboratory in Columbia, South Carolina. Larsh and Martin Young were close friends, and I was looking for a job. Martin needed a technician to rear mosquitoes, dissect mosquitoes, and take blood smears on patients at the state mental institution in Columbia, South Carolina, where there was a program involving malaria therapy for neurosyphilitics. These were activities that were monitored extremely carefully by medical staff and boards before they could be carried out.

LS: When you were in Columbia did you get to see patients?

MW: Yes.

LS: What was the hospital like? What were the wards like?

MW: Oh Christ! Of course it was segregated, and the whites were in the big mental facility on Bull Street in Columbia, South Carolina, and State Park several miles away was the black facility. I remember one extraordinary experience working there. We infected mosquitoes with malaria, and I would take the mosquitoes to the patient areas. Mosquitoes were fed on infected patients in order to have infections for transmission to the next patient. So I took a bunch of *Anopheles quadrimaculatus* infected with *Plasmodium falciparum* to the State Park to infect this patient who had just been admitted.

LS: This was a neurosyphilis patient?

MW: Yes. And he was on some other planet. He had no idea what the hell was going on.

LS: He was conscious?

MW: Yeah, but just out in nowhere. So we fed the infected mosquitoes on him, and he tolerated this better than you might have expected -- I was surprised. Jimmie Skinner was there. We fed these mosquitoes and then you went back to the laboratory and counted them all up, how many had fed and had blood in them. Next, the blood-fed mosquitoes were dissected and you estimated how many sporozites he received. It was my job to do the blood smears every day on these patients. Once you infect them with malaria you have to do blood smears daily because you have to treat them; you can't let them get very sick.

LS: Especially if you're using *P. falciparum*.

MW: Yes. Well, we were doing *P. falciparum* on blacks because they are generally refractory to *P. vivax* and they are less susceptible than whites to *P.* falciparum. To my amazement -- after three months or four months -- he was talking. There was no rocket science about him, but he was back.

LS: He knew where he was and so forth?

MW: Oh yeah, he was back. We also did a lot of intestinal parasites studies because intestinal parasite transmission within mental institutions is very common. It's a very difficult process. For these intestinal parasite studies, you had to go collect stools from all of these patients. One of the things that is most embedded in my mind is how foul-mouthed the women mental patients became; they were just incredibly foul-mouthed. It was a striking phenomenon. Apparently this is not unusual. I was on the wards, and a few times in the prison, and I hated that --

LS: The prison in Atlanta?

MW: Yes. When those doors clang behind you and there is just something about it -- even though you know you're going to get out. When they clang that last door behind you, there you are, you can just feel the hair rising on the back of your head. Not a good experience.

I'm quite sure that in today's atmosphere, we'd never be able to take on that kind of activity that we had at Martin's laboratory. This was an NIH laboratory, and the chief of the division -- Parasite Chemotherapy -- was G. Robert Coatney.

Aimee Wilcox was the Senior Microscopist in the Columbia laboratory. She was very well known in the field of diagnostic malaria. Her *Manual for the Microscopical Diagnosis of Malaria* was used all over the world.[1] She became one of my closest friends and for several months, I spent two to three hours per day learning malaria microscopy under her tutelage. This experience was most valuable to me for the remainder of my career.

I worked for almost two years there. I had told Dr. Young early on that I wanted to go back to school, but I wasn't sure what I wanted to do. Martin was something of a martinet to put it

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[1] Wilcox's manual appeared in numerous editions: Aimee Wilcox, *Manual for the Microscopical Diagnosis of Malaria in Man, National Institute of Health Bulletin No. 180* (Washington, DC, Government Printing Office, 1943).

mildly, a very good friend and a tremendous influence on my career. He said, "Okay, if you're going to go back to school, do it. Don't just talk about it, do it." And so one day in April he called me in and asked me where I wanted to go to school. And I said, "I don't know, I haven't made up my mind." "Come back tomorrow with some definitive ideas of what you want to do." So I came back the next day and I said, "Well, if I could really do what I want to do, then more than anything else I would work with Asa Chandler at Rice University in Houston." And he said, "I think that's an excellent choice," and he contacted Rice, and he knew Asa very well, and within about four weeks I had an invitation to apply for acceptance to graduate school at Rice, and that's where I ended up getting my PhD.

LS: At Rice you worked with dwarf tapeworms?

MW: Yes. My thesis was on methods for detoxifying extremely toxic fatty acids in tapeworms that did not have an excretory system as such.[2] I worked on the idea that *Hymenolepis diminuta* managed to neutralize these fatty acids and store them in the proglottids, which were then sloughed off. It was a remarkably inefficient system for carbohydrate metabolism. The tapeworms left a lot of materials that could be broken down by other organisms into energy. But it was so simple for the tapeworm just to slough off this proglottid, and there was their excretory system. But I very soon learned that laboratory work was not my thing. I loved to teach, and I loved working in the field. Graduate study at Rice was a very special privilege. The institution was special. My fellow students were gifted and Professor Chandler was arguably the best! Teaching was a joy because the undergraduate students were superb. I felt extremely fortunate to be there. Then came a hiatus of no malaria for a while. I was in debt when I got out of graduate school, and I was fortunate to get a job as an Assistant Professor of Preventative Medicine at the University of Oklahoma School of Medicine.

LS: How did you choose Oklahoma?

MW: t was the job available. It was available and appeared to be something I could do and enjoy. Professor Chandler would get inquiries from many people looking for a parasitologist. They would tend to come to Rice and ask, "Do you have a candidate for a particular type of position. I had done, I think, a reasonably good job with my teaching responsibilities while in graduate school. Graduate students at Rice had to teach every semester and one could not teach the same course twice. You got a lot of experience in organizing and teaching materials for presentation to students. I got the job at Oklahoma. It was an enormously intriguing experience because I was the youngest faculty member there. This was a time -- the mid-1950s -- when the problem with infections transmitted in hospitals was beginning to surface. The staphylococcal infections were beginning to come up as a real problem. And the surgeons -- I became convinced at times that surgeons were not aware that Pasteur had ever existed because they were clever with their hands but simply did not comprehend the complexity of what went on with the transmission of infections within an operating room. I turned out to be the epidemiologist for the medical center and was so young and inexperienced that surgeons were just not interested in listening to anything I had to say, that sort of thing. But it was a wonderful experience, I thoroughly enjoyed it, but I was beginning to be antsy. I wanted to go overseas. That was something I had wanted all my life. My first year at the University of Oklahoma I got a China Medical Board Fellowship for study of tropical medicine in Central America.

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[2] McWilson Warren, "Studies on the Interrelationships Between Lipid and Carbohydrate Metabolism in *Hymenolepis Diminuta*," (Rice University, 1957).

LS: China Medical Board?

MW: It really was China Medical Board. It was a Rockefeller Fund, and China was no longer available as a site for teaching the kind of things that the Rockefeller Foundation, through Tulane University, had proposed. So Dr Henry Mellany down in New Orleans, began to organize a program in Central America that would send people on a two-month tour to go through a number of countries to gather teaching materials for your own classes and to see things that you had just never had an opportunity to see in the US I was in El Salvador, Guatemala, Honduras, Nicaragua, Costa Rica, and Panama. This experience whetted my appetite for being in the field. Previously when I was in graduate school, I had been trained as an invertebrate physiologist, strictly focused on laboratory research, using a Warburg apparatus for measuring respiration and other bench technologies.

As I noted, my time at Oklahoma was a wonderful learning opportunity, but was not what I wanted in a career. Two people come immediately to mind when I recall the time in Oklahoma City. Dr. Philip Smith was my immediate senior -- excellent parasitologist and gifted teacher. Dr. Kirk Mosely was chairman of the Department of Preventive Medicine -- a sensitive and effective administrator who grew up in China as the son of missionaries. One day, while I was conducting a seminar course in parasite physiology for graduate students in my office, my secretary came in and said I was wanted on the telephone. (I had requested that I not be disturbed during teaching activities. By that time I was a Vice chairman of Preventative Medicine). I asked the secretary to take a message and I would return the call. About three minutes later she came back and she said, “I’m sorry to interrupt you but this man, Dr. Coatney, will simply not take no for an answer.” And so Bob Coatney called and wanted to talk to me about the possibility about going to Malaysia.

I knew that Don Eyles had become infected with a malaria parasite that was from monkeys. I was aware of that but did not know they had setup this elaborate program for going to South East Asia to investigate what kind of problem this could pose as far as human malaria was concerned. Bob said, “When can you go?” And I said, “Well I have to finish the semester. I’ve got a class of medical students, and I’ve got graduate students. I can’t possibly go before the end of June.” But I said, “I don’t even know if I can go then because I have to talk to the dean of the medical school” and so forth. The dean of the medical school was very sympathetic, and he said, “Oh yes, this is too good a chance to miss,” and I had a leave of absence from the university and became a member of the US Public Health Service Reserve in order to take this assignment to Malaya.

LS: Do you know how Dr. Coatney got your name?

MW: Oh yes. I had worked with Martin Young, and I learned later that Martin was very disappointed that had not returned to NIH after graduate school. I did not realize that I could have contacted Dr. Young and he would have found a place for me after I finished my PhD at Rice. Under the circumstances, I took the job that was available. Dr. Young was disappointed that I’d never made the contact with him. Through these years he’d kept up with me, and I’d see him at meetings and that sort of thing. When they needed someone to go to work with Don Eyles, Martin said he was the one who pushed Bob Coatney to contact me. I did not know Coatney until then. The interesting thing was that he was so eager to get me to Malaysia -- to Malaya it was at that time -- that my passport was sent to me complete with necessary visas. I did have a chance to go back to North Carolina for a few days visit with my family -- my parents. I left for Kuala Lumpur, Malaya from Raleigh, North Carolina. It was just an extraordinary experience.

Overnight into Hong Kong, which for a kid from a farm in North Carolina was almost the ultimate in an exotic sort of activity. I stayed at the Peninsula Hotel -- at that time it had not been renovated and it was an old, ratty hotel but it was exciting. Don Eyles met me in Kuala Lumpur, and off we went.

LS: So you hadn't known him before?

MW: I had met him but I didn't know him well at all. Then came that extraordinary experience at the Institute for Medical Research in Kuala Lumpur. The IMR (Institute for Medical Research) was the epitome of a British establishment in the tropics. Mostly one story buildings with long porches scattered over an area of well-kept lawns and gardens. Hisbiscus and bougainvilla were everywhere. I felt as though I had arrived on a different planet!

Don Eyles, arguably, was the most accomplished, the most brilliant scientist/thinker that I ever had the opportunity to be around. We were committed to finding out about monkey malaria Nothing was known about the epidemiology or the variety of these parasites. So the world was our oyster. There was nothing that we could learn in the jungles of South East Asia that would not be pertinent to a better understanding of monkey malaria. Parasitology, entomology, mammology, and botany were all subject areas where expertise was needed. This was particularly exciting because we had there a large number of consultants representing many scientific fields to bring a new challenge and stimulus. Every day was new and special.

LS: Can you say a little bit about what motivated Dr. Coatney and the NIH to set up this project?

MW: If you were to ask people like Don Eyles, it was primarily being intrigued by the biological aspects of what had happened. Here is a malaria parasite that grows in non-human primates that is obviously infective to man and could present him with a disease problem.

LS: Don Eyles was working with…?

MW: He was working in Memphis, Tennessee, investigating the biology of the lifecycle of malaria. It had not been established that there was a cycle in liver at this time. Don was working on *Plasmodium cynomolgi.* His laboratory was infecting thousands of mosquitoes to produce large numbers of sporozoites for injection into monkeys. In that way there'd be enough sporozoites injected for him to find where they went initially in the primate host. He apparently got a little bit careless and there was no indication that he was working with a parasite that was infective for man. Don became ill with a febrile illness, and it was Mary, his wife, who finally pushed him, saying, "I think he's got malaria." Don had been nowhere to acquire the disease and malaria was not transmitted in Memphis in 1958.

LS: There was no human malaria used at the Memphis lab?

MW: No. They took a blood smear from Don, and sure enough, he had malaria. He had some malaria-free monkeys, and they re-inoculated his blood back into one of these animals and lo and behold it became infected. What difference did this make? On a worldwide basis, at this time, the commitment to malaria eradication had become a major issue. It was such a big issue with USAID [United States Agency for International Development] -- at that time the primary source of money for any research involving malaria control -- that if a country was not committed to malaria eradication then they couldn't have AID funds. They were simply not qualified. In all of sub-Saharan Africa, during this period, the only country that could receive any financial support for malaria control from the US government was Ethiopia. They said that they had an eradication program. Most knowledgeable people were aware that Ethiopia was no going to eradicate malaria. All of the rest of sub-Saharan Africa couldn't even talk about eradicating malaria. Politically, US policy was so committed to this idea of eradication that we couldn't send any funds the the rest of sub-Saharan Africa. We could not support any kind of activity going on in Nairobi or other places in Africa where there were institutes that would be doing excellent work in malaria.

About this time we were beginning to have the first inklings of possible resistance of anopheline mosquitoes to DDT. So they were pushing hard. Internationally, there was tremendous push on to get this done, to eradicate malaria before the resistance of the mosquitoes had overpowered DDT. There was a tremendous amount of emotional commitment to eradication, almost like a religious fervor that was extraordinary. Everything was dependent upon the eradication of human malaria. It was all dependent upon the fact that there was one source of human malaria parasites: other H*omo sapiens*. Now with Don Eyles' infection, we suddenly realized that someplace in South East Asia -- with huge populations of monkeys -- there was at least one monkey malaria parasite that would infect man. How many parasites were out there? What was going on? What was this going to do to the concept of malaria eradication? Geneva, London, and Washington were really, very spooked about the whole business because suddenly if there was a significant level of malaria in non-human primates infectious to man, then the concept of eradication had biologically gone down the tubes: Under the circumstances, there seemed to be no possibility that human malaria could be eradicated.

That was the primary focus. The NIH was prepared to invest funds into sending a team to Kuala Lumpur to investigate monkey malaria: What was it doing? How much malaria was present in the monkeys? What were the possibilities of having these parasites transmitted to man at any significant level? Because the threat to malaria eradication was perceived to exist, the laboratory was setup. There were at least two reasons for going to Malaya. First, was the presence of the Institute for Medical Research (IMR) which was founded by the British in 1900. It was a very famous institution, and second *Plasmodium cynomolgi bastianellii,* the parasite with which Don had been infected, was isolated from a Malaysian monkey. This was an extraordinarily fortuitous combination of a place to work with a competent staff located in the "right" place. Fifteen miles outside of Kuala Lumpur, at that time, was deep jungle. The IMR was pleased to serve as a base of operations for the study. Americans could be sent into this area with an opportunity to live comfortably and to work both in the laboratory and in the field Kuala Lumpur was selected on that basis to be the site of the NIH Far East Research Unit (FERU).

LS: And this was another Coatney run project?

MW: Yes. This was part of the Laboratory of Parasite Chemotherapy, which began really before my time. There was a Laboratory of Parasite Diseases that included two very strong people, Leon Jacobs and Bob Coatney. They did not work well together. So in some sort of Solomon-like decision the laboratory was broken in two: the Laboratory of Parasitic Diseases, which was Leon's and the Laboratory of Parasite Chemotherapy, which was primarily malaria, given to Bob Coatney. Coatney had facilities in Atlanta, Columbia, Memphis, and Milledgeville [GA] as part of that group of staff and programs that came to him as the Laboratory of Parasite Chemotherapy.

LS: In a few places I come across the "US Army Medical Research Unit." Were they an active player or was that a way to channel the funds to overseas US research units?

MW: The US Army laboratory -- USAMRU -- had been in Kuala Lumpur for several years when we first arrived there, and they were working on a variety of tropical diseases. What they would be working on depended on the interest and the activities of the individuals who were assigned to the unit, both civilian and regular Army. Sometimes they had other people from the armed services who would be assigned to USAMRU.

Malaria was not a major part of their activity at the time, but it was through the US Army Medical Research Unit that NIH was able to fund FERU. USAMRU had an arrangement with Malaysian government for bringing in funds from the outside to support activities in medical research at the Institute for Medical Research. This arrangement was the means by which funds for the special program in which Don Eyles was going to be working were brought into Kuala Lumpur.

At the same time the [George Williams] Hooper Foundation from the University of California had an active program at the IMR with extraordinary people, Fred Dunn, Lee Kian Jo, Paul Bausch, and others who were remarkable scientists and investigators. They were working on schistosomiasis and filariasis. The Hooper Foundation was a very important part of our working together. The Army, the Hooper Foundation, our unit, and the Institute for Medical Research became almost inseparable programs. We shared with the Institute for Medical Research the laboratory technicians, field entomology technicians, physical facilities, etc. Frequently we would be working in the field with professionals and technicians from all three programs.

The program for control of malaria in Malaya was under the Ministry of Health, but in reality, the Institute for Medical Research was so influential and intellectually such an important factor in Malaya that most malaria programs were moved forward with impetus from the Director of the Malaria and Filariasis Division at the Institute for Medical Research. Field research and understanding better control methods were under the control of the IMR. The reputation of the IMR extended beyond the national borders of Malaya. In other words, locating FERU at the IMR in Kuala Lumpur was a real coup!

At that time, [Arthur A.] Sandosham had two seats: he was the director of the Institute and he was also the head of Malaria and Filariasis Division of the Institute for Medical Research. Sandosham was a Malaysian Tamil of South Indian origin. He obtained his medical degree from the University of Singapore and was doing his residency there when the city fell to the Japanese in 1941. He remained in Singapore during the occupation. He was recruited by the Japanese to teach malariology to medical students he who became anti-malaria workers in the Japanese program to control the disease during World War II. He had to prepare a textbook and color renditions of the appearance of the parasites in the peripheral blood were prepared by his students. Therefore each copy of this remarkable book was unique. The forward to the book was written by Dr. Sato, the Japanese officer in charge of the program. Dr. Sandosham was kind enough to present me with a copy of this book when I left Malaysia after FERU was closed.

We also worked with [R. H.] Harry Wharton, who was an Australian entomologist and an extraordinary man. He was tragically killed three years after he retired. He went back to Australia, and he and his wife Helen were setting up a farm. He had a tractor, and he would go off every morning to work on his farm. Then, one day he didn't come home and they went out to look and the tractor had turned over somehow and killed him. It was just an extraordinary sort of death, very tragic, very early. His knowledge of the Malayan environment and the Malayan mosquitoes was virtually encyclopedic. He and his staff became an integral part of the FERU program. He was included as an author on many of our publications.

The Institute for Medical Research was so active and so vital after World War II that visitors from all over the world came for visits and consultations. Every time a visitor would come, there'd be a seminar and everybody would get together for a group picture or an extraordinary dinner. We would go to a restaurant, and in Kuala Lumpur you had Malays, Dravidian Tamils from South India, and Chinese. Every possible cuisine group in China was represented, so the food that was available was wonderful. We would have these wonderful banquets when people such as [Leonard Jan] Bruce-Chwatt came from Geneva, [P. C. C.] Garnham came from London, Bill Bray came from West Africa. There would be a seminar and a big dinner. It was a wonderful. If you look at the pictures, what you see is remarkable. Keep in mind this in the early 1960s -- all were at equal levels. There is simply no ethnic rivalry apparent. There were Chinese, Malays Indians, Americans, Australians, Brits, and Thais. The glue that kept us together was English, and a primary interest in tropical medicine, especially malaria. It was such a remarkable place with so many vital characters. [Robert S.] Desowitz from down in Singapore was a frequent visitor and participant in our program. It was really the most exciting place to be. I'll show the pictures later: Group photographs were basic part of the activity at the Institute for Medical Research. It went on all the time. Everybody would come in and have group photograph in front of the main building at the institute. The IMR was on a five-and-one-half-day-week schedule. After closing at 12:30 pm on Saturday, the group of people from the IMR who had an interest in malaria, and any visitors who happened to be in town, would meet for beer and finger food at the house of one of the permanent members. The talk at these remarkable sessions was extraordinary. The food available in multi-ethnic Kuala Lumpur was almost endless in its variety. These sessions became so well known that visitors would endeavor to be sure that their time in KL included a Saturday afternoon. Wives were very important participants in these social/professional get-togethers. It was during these Saturday afternoon sessions that I had an opportunity to extend my friendship with Mary Eyles. Mary was a gifted biologist and teacher. There was nothing in the field of biology that did not gain the interest of her very fertile mind. She and Don made a most remarkable pair and Mary became an integral part of the entire FERU operation.

Don Eyles, as I already have said, was probably one of the most vital and intelligent people that I have had the privilege of knowing. Working under his supervision was wonderful. He was a very special member of this vital coterie associated with the IMR during this period. Because we knew little about monkey malaria, there was virtually nothing we couldn't investigate. We were most anxious to know what the vectors were and what possible liaison vectors there would be: mosquitoes that would feed on monkeys and feed on man.

We spent months in the jungle with netted bait traps in a tree baited with monkeys, and netted bait traps on the ground underneath the tree baited with "people." The "people" got up every hour to climb the tree and collect the mosquitoes that came around the monkeys looking for a meal -- and at the same time, collected the mosquitoes in the human baited traps -- looking for species of *Anopheles* that could potentially transmit malaria from monkeys to man. At the same time we needed to know what kind of malarias were present in the monkeys. We described four or five new species of monkey malarias within the first two years FERU was in operation.

One remarkable story is associated with the swamp forest where we had a lot of malaria in *Macaca nemestrina*, the pig-tailed macaque. We would collect a mosquito identified as *Anopheles umbrosus* and at one time 15% of the wild caught mosquitoes of this species would be infected with sporozoites in the field. We'd inoculate these sporozoites in the monkeys and nothing would grow. We didn't inoculate them into people because we weren't prepared to do that in the field. Then Gordon Bennett, one of our colleagues out there, began to realize (he stained sporozoites from these mosquitoes in a particular way) that they were they did not physically, morphologically, look like sporozoites from primate malaria. We began then to say, "Well, what are they?" We had always assumed *A. umbrosus* was transmitting human malaria because anytime in the past that wild-caught mosquitoes had been dissected, sporozoites had been detected. It was assumed that they were the source of the problem for human malaria, because they would feed on man.

We found out that it wasn't human malaria or lower primate malaria. There is a remarkable little animal that lives in the swamp forest of Malaysia called the mouse-deer, *Tragulus javanicus*, an interesting little critter: a little thing that looks a little like a deer -- I don't think it's a *Cervidae* -- and is about maybe a foot long. We collected these. They are very hard to collect -- wow, trying to get a hold of these things is terrible. We collected some of them and did smears on them. And virtually 100% of the *Tragulus javanicus* in these swamp forests were infected with a malaria parasite. We finally got together to rear them -- a tremendous task. One technician didn't do anything except try to rear mouse-deer in the laboratory. We treated them, so we could clear out any malaria parasites they had, with primaquine and chloroquine. Then we had something we could use for these *A. umbrosus* sporozoites. And sure enough, it turned out the *Anopheles umbrosus* was infected not with human malaria, not with lower primate malaria, but infected with a new parasite that was from the mouse-deer, and so we then described this parasite and it was named *Plasmodium traguli.*[3]

There are all sorts of these little nuances: For example, spelunking because bats have malaria. The epidemiology of bat malaria was a whole new challenge. Don Eyles, Mary, and myself used to mist net for bats and take blood films. There are a lot of hemosporidia in bats but none of them were such that we could believe they were biologically involved in primate malaria.

To give an example of just what sort of biological interest Don Eyles had, there are limestone outcrops all over Malaya. They're separated from each other by maybe a mile, maybe a few hundred yards. There is debris that comes down the sides of these limestone outcrops that are partially forest-covered. This debris is filled with shells of tiny microscopic snails. We actually published a paper on the fact that the whorl of the snails on one outcrop was not the same as the whorl of snails from the next outcrop. They were definitive, distinct populations.

LS: Were these fossil snails?

MW: No, not fossils. These were the shells that would wash down with the rain and you could collect them at the base of the outcrop. That's not terribly important except it does illustrate that the Eyles' had biological interest in anything that lived. You could get yourself involved in anything. It was extraordinary experience.

LS: You mentioned a couple times collecting animals, I wonder if you could just say a little bit more about how you gathered your various animals, whether they were people's pets or from animal dealers?

MW: One of the extraordinary stories about this issue was the search for rhesus monkeys. Rhesus monkeys were the best laboratory host we could find. In Malaya, there are *Macaca fascicularis*, the crab-eating macaque, and they're very, very common; *Macaca nemestrina*, the pig-tail macaque; and *Macaca speciosa*, the stump-tailed macaque. And there are gibbons and siamangs. There is also one the leaf eating monkeys, *Presbytis crestatus.* This complex population had to be looked at in terms of how many of them had parasites. We found parasites in all of these monkeys. We could not work very well with the macaques in Malaya because they were all infected and they could not be used to determine the species of sporozoites collected from wild mosquitoes. In order to work with monkeys in the laboratory, we had to get the experimental animals from somewhere outside the range of the *Anopheles leucosphyrus* group. (We had found out by this time that members of the *Anopheles leucosphyrus* group were the transmitters of malaria among monkeys.)

For monkeys that we could use in a laboratory that were not infected and our primary source, at that time, was East Pakistan. That was a place where we could get monkeys. There was an extraordinary man by the name Saheed Mujibullah who was a monkey dealer in Dhaka. He was an extraordinary man. Don said, "Okay, we've just got to have some rhesus monkeys," and he sent me off to Dhaka (one had to go to Calcutta and change planes to Dhaka) to meet this guy that we had heard of, but none of us had ever seen before. We'd corresponded with him, he'd sent us some monkeys, but we had never seen him. I arrived and was not feeling well at all. Saheed took me out for lunch that afternoon when I arrived in Dhaka, and at the lunch I became violently ill and had become febrile.

As a part of this story, I had been working up with the aborigines in central Malaya. This was part of a process to look especially in jungle dwelling populations for parasites that could be of monkey origin. I had been up in the central part of the Malayan highlands working with the Temiar for several weeks when Don called me on

[3] R. H Wharton, McWilson Warren, Don E. Eyles, D. E Moorhouse, and A. A. Sandosham, "*Anopheles umbrosus* as a Vector of Mouse-Deer Malaria," *Medical Journal of Malaya*, 17, 1962, 79; and A. A. Sandosham, Don E. Eyles, R. H Wharton, McWilson Warren, and C. C. Hoo, "Plasmodium sp. and. Hepatocystis sp. in the Mouse-Deer (*Tragulus javanicus*) in Malaya," *Medical Journal of Malaya*, 17, 1962, 79-80.

the radio and said, "You have to come out. We've just got to have monkeys." We were really desperate for them. I probably at that time was a little bit cavalier about taking my malaria prophylaxis. I admit that I was. So I came out of the jungle and was only in Kuala Lumpur for half a day before I was on a plane to Calcutta. I became violently ill in Dhaka, realized very soon that I had malaria, there was no question about it. Saheed Mujibullah in his efforts to become a scientist had a microscope, and he had some Giemsa stain. So I stuck my finger and made a blood film and looked at it and I had *P. falciparum* parasites in large numbers. I was really, really sick, and at this time I was afraid, because unless something could be done I was very likely to die. I can remember riding around Dhaka, East Pakistan in an old World War II jeep which was the only vehicle that Mujib had, trying to find a chemist where I could buy some chloroquine. I found and bought some chloroquine, and then I holed up in a hotel for a week. I was so sick I couldn't even notify Don of what was going on. I had no contacts in the town except Mujib. Mujib was the only person I had ever seen in Dhaka. One day a guy from the embassy -- at that time we had an embassy in Dhaka. He had had a contact from Don Eyles saying, "Where is he? What happened to him? He has disappeared." That day was when I returned to the real world. It had been a week, and chloroquine was very effective. You felt a hell of a lot better in a big hurry if you took chloroquine with *falciparum* malaria, if the parasite strain was responsive to the drug.

Then we took a train to a place called Selet which is down toward the Burma border. The train was two extraordinary days and we got the monkeys. We were riding in the first class carriage, which was a small private cubicle with two benches. Mujib was accompanied by two servants who brought bedding for us to use at night. Periodically, the train would stop and people would jump out, build fires beside the track, and make tea! Longer stops were arranged for dinner. Mujib chewed betle -- a palm nut that is mildly narcotic. Users of this stimulant, and there are many in Asia, produced bright red sputum. Hence, sidewalks, etc. in Dhaka were red. As a sign that Mujib and I had become friends, he would occasionally chew betle when we were together -- something he would not normally do in the presence of a Westerner.

The approach to monkey collection was interesting. Beaters would run the monkeys -- large troops of rhesus - up a tree. Then they would throw nets around the tree and catch them as they'd try to run. Men would catch them and put them in boxes as they came out.

During this period, we had found a malaria parasite in the gibbon, which is very interesting. We needed some uninfected gibbons. Don sent me to Bangkok to look for these animals. We thought that the animals in central Thailand would be malaria-free because no non-human primate malaria and no *A. leucosphyrus* group mosquitoes had been identified in this area. I'm a little bit uncomfortable because I know that this activity was probably illegal. However, they were available and somebody was going to buy them and I thought, "Well if they're are for sale let me buy them because of our need in the laboratory." The gibbons at Bangkok were north of the transmission area and they would be parasite-free. That's how we found *jefferyi*, by using gibbons from Thailand and blood from an infected Malayan gibbon. The illegal buying and selling of animals was then -- and I think probably still is -- very common in Bangkok with all sorts of exotic animals involved. I found a taxi driver, extraordinary man, who took me around to little back-street areas of the city where it was very crowded, little houses and paths that went down between them. They had gibbons! I would buy the gibbons, put them in a box and send them back to Kuala Lumpur by air. Getting exit papers for the animals was a real challenge. I learned that anything was possible if one was willing to pay. The real issue was to get the proper stamp on the shipping papers. The cost of this stamp depended upon the size of the office where the official was located. If there were four desks in a room, the cost was minimal. The price went up according to number of people in the office. A single desk with a name on the office door could be quite dear!

LS: The rhesus monkey is a macaque?

MW: Yes. The one that you see most in the Indian temples, that's *Macaca mulatta*: the rhesus. But *Macaca mulatta* is extremely susceptible to *Plasmodium knowlesi*. Virtually 100% of the animals will die if infected with this parasite unless you interfere with therapy. There are no *Macaca mulatta* in Malaya because *Plasmodium knowlesi* is extremely common in that area. One has to get out of peninsular Malaya in order to find rhesus monkeys. In northern Thailand you can begin to pick up macaques that are *M. mulatta.* The Thais say they are not *mulatta* but I think they probably are. This is outside of the range of *Anopheles leucosphyrus* group mosquitoes, and one can find uninfected animals. The same situation exists in Burma. You don't come back into malaria infections until you get all the way to the southern part of the Indian subcontinent, and then there is another group of monkeys. *Plasmodium fragile*, for example, is described from Sri Lanka and South India.

Getting experimental animals was difficult, because we were in an area where everything was infected, and in order for us to work with the malaria parasites, we had to have animals that we knew were not infected. We screened our monkey holding houses at the IMR. Everything we had doors with curtained entrances to keep the local mosquitoes out of the monkey houses.

LS: Did they build you new facilities, or were these all preexisting facilities?

MW: No, with our funds, we built the monkey houses, and we finally occupied an office of our own. When I first got there, we were working in the lab with Harry Wharton, the entomologist of Institute for Medical Research. It was a vast old building with huge fans that went round and round and kept the hot, humid air moving. I had a little corner in that lab. But Don was working on getting us a laboratory of our own. At the time they produced a smallpox vaccine at the Institute for Medical Research. So there was a building with a stable where they would put the water buffalo and carry out the scarification procedure and then collect the material for the smallpox vaccine. Then all the people would gather around when they slaughtered the animals, they gathered around because the meat was perfectly safe. This building was right in the middle of the campus at the Institute for Medical Research. Associated with this was a small building that was no longer used. They were in the process of moving the whole smallpox vaccine process. So we got this little building and converted it into our laboratory, and offices for Don and myself and our laboratory. By this time we had our own technicians that we were training. We used the room where they did the scarification of the buffalo as a surgery where we did splenectomies on monkeys.

LS: Did you work with a vet?

MW: No, I had to learn to do splenectomies on my own. I taught anatomy when I was at Rice. The first one was pretty bumbling, you know, because you're trying to be sure every vessel was tied off, but then you very quickly learn that you can loop them and tie several vessels at the same time. I never lost a patient.

LS: You did your own anesthesia and everything?

MW: Oh yes. We had a technician named Elizabeth Guinn, an American from NIH. Liz and I would do splenectomies. She served as anesthesiologist and I was the surgeon. But we never lost one. [laughs]

[End side one, tape one]

MW: We worked with an entomologist, a Malaysian-Chinese entomologist by the name of Cheong Weng Hooi. And Weng Hooi was a very bright guy. He had done his study of entomology at the University of London, School of Hygiene and Tropical Medicine. He was a Deputy under Harry Wharton in the entomology division at the Institute for Medical Research. He was very deeply involved in all of our programs. And we had extraordinary technicians. I remember one Dravidian Tamil, by the name of Mohhadavan who is such an extraordinary man that I'll love him always He was just a wonderful person; big, rotund, but superb technician in the field. He and I slept in baited net traps for many nights climbing the trees to get the mosquitoes out of the monkey-baited net traps.

On one trip into the jungle, Harry Wharton, Don Eyles, Maha and myself had gone up the Pahang River in central Malaya. There was a little house that had been built by some of the game wardens close to a Tamiar longhouse. I remember the floor was split bamboo and it was maybe five feet off the ground; that was typical of dwellings in that area. Mohhaduvan, who weighed 250 pounds or more, was going to be the cook for the three of us. There was a little shed room which served as a kitchen. We were looking at the mosquitoes that we had collected that day, and Maha was out in the shed room preparing our supper, whatever it was going to be, usually tinned salmon and rice, absolutely dreadful food; but we weren't out there on a gourmet trip. All of a sudden there was this tremendous crash and the little house shook and the three of us said, "My God, what's going on?" Maha had gone through the floor, And there was our supper, all spilled out on the ground. We didn't eat very much that night.

When you're in that kind of situation, your life is absolutely filled with these little episodes that come back: little incidents that you suddenly remember. What this illustrates is that we were very closed to the technical people who really made the project. They were the best I've ever seen. In all of my working in Africa, Central America and so forth, I've never seen people who were so dedicated to being good at their job. The technicians at the Institute for Medical Research were, by all odds, the best I've ever encountered. They were absolutely wonderful. We had an extremely productive program, resulting in many publications. We became internationally known among people who work with malaria. Everybody wanted to come and see the NIH laboratory in Kuala Lumpur. But it was just Don and myself and then these enormously effective, bright, clever, well-trained, dedicated people at the Institute for Medical Research. Don and I were just there. It was all those other people who did such an extraordinary thing. It was really wonderful.

Later, after we had closed FERU, one case came up in the central part of the Malay peninsula that clearly indicated a *knowlesi* infection in man, and this was considered to be very important. So Bob Coatney (I was stationed in the NIH lab in Atlanta at this time) got busy with what he did best and found the money necessary to send me back to Malaya to investigate this finding. I put together a team from the IMR and we did venipunctures on hundreds and hundreds of people in this area in the central part of the peninsula of Malaysia. We pooled the blood from five persons to make up the inoculum for one monkey. The monkeys were bled daily, starting two weeks after injection. I have forgotten, but I think it was somewhere in the neighborhood of almost 2,000 people were involved in this study, and we didn't get one single infection.

As a side issue, I've got on my desk right now a letter: I'm writing back to Dr. Singh.[4] He has apparently found a cluster of human malaria infections with *Plasmodium knowlesi* in Sarawak. He has written to me to ask how would he go about identifying vectors. First of all, he's got to have some monkeys that are not infected. There are ways he can do that if he can screen the holding area. We've come full circle, and I think he's got a real bill. He identified the *P. knowlesi* first by DNA. Traditional microscopy is of limited value in this situation, because the peripheral blood stages of *Plasmodium knowlesi* and *Plasmodium malariae* in man look very much alike.

LS: You mentioned baiting the traps with monkeys and baiting the traps with people. I was wondering how much did you have to learn and invent to set up these systems for collection, and how much could you use someone's off-the-shelf methods?

MW: All new. We had a Chinese seamstress down the street from the Institute of Medical Research who sewed up our bait traps. All these technologies were new with us. Many of our earlier publications were on the field technology that we used.

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[4] Dr. Balbir Singh, University of Malaysia at Sarawak, Kuching, Sarawak, Malaysia.

LS: The mosquito trapping experiments sound intriguing. It seems like there must be some stories around this process? For example in one of your papers, you write: "An important factor in planning long-term studies involving the trapping of mosquitoes is the reliability of the trapper."[5] Did you encounter unreliable trappers?

MW: Yeah, you had to watch very carefully. This is what I was emphasizing with the quality of the people.

Mohhaduvan, Abu Hassan, Cheong Weng Hooi, etc. were professionals. Once the protocol was established, they were superb. The only time I found a protocol that went bad on us was while were doing some trapping in Quantas on the east coast of Malaya. There was a tiger! When tigers get old, they find that people are the easiest prey. Sometimes there would be a tiger in an area that would kill people. When this occurred, hunters would have to go out and get rid of that particular animal. There are also bears in the area, honey bears that are very common in the Malayan jungle. The boys we had set out to do the trapping were terrorized by bears. This combined with the reported presence of a killer tiger in the area was too much. We had to drop trapping in that area. It was the only time I can remember there being a physical issue that mitigated changing your protocols or getting out of a work area.

We considered the technicians we had in Malaya to be the best. You could not, for instance, set a net trap in a village and say, "Would you collect the mosquitoes?" You really had to have reliable traps and reliable workers to man the traps.

LS: As you wrote, "crawling round the outside of a net on a small platform 25-30 feet above the ground at night is both unpleasant and hazardous."[6] There's also in one of the papers mention of "marauding pythons"[7] going after the monkey baits.

MW: Pythons are fairly common, and you wake up in the middle of night hearing the monkeys scream. I remember this down in the coastal area where we were working with *Anopheles sundiacus*. The monkeys were screaming, and we went up, and there was a python trying to get to the monkeys inside the cage in which they were sitting on the tree platform. Oh yes, it's what I was emphasizing before: from where we were living and working in Kuala Lumpur, you could travel 15 miles out of the city and it was pure jungle at that time. It's not the same anymore. I went back to KL later for another type of job with the World Health Organization. The city was completely different - residential areas covered areas that were previously heavily forested.

As with Thomas Wolfe's *You Can't Go Home Again*, I found that to be true. We had an effective program, but one could not again put together the team and the excitement that we had in the beginning.

LS: Just a few more questions about materials. You were collecting mosquitoes out in the field. You had to bring them back to the lab for identification? They're all in tubes, and the entomologists were back in the lab?

MW: Yeah, we became pretty good at identifying them in a tube, but then when you got ready to dissect them there was a specific process for identifying every mosquito that you dissected. The members of the *Anopheles leucosphyrus* group can be very difficult. However, these technicians, Abu Hassan and Mohhaduvan, were

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[5] R. H. Wharton, Don E. Eyles, and McWilson Warren, "The Development of Methods for Trapping the Vectors of Monkey Malaria," *Annals of Tropical Medicine and Parasitology*, 57, 1963, 32-46, p. 42.

[6] R. H. Wharton, Don E. Eyles, and McWilson Warren, "The Development of Methods for Trapping the Vectors of Monkey Malaria," *Annals of Tropical Medicine and Parasitology*, 57, 1963, 32-46, p. 37.

[7] R. H. Wharton, Don E. Eyles, and McWilson Warren, "The Development of Methods for Trapping the Vectors of Monkey Malaria," *Annals of Tropical Medicine and Parasitology*, 57, 1963, 32-46, p. 44.

superbly trained to identify mosquitoes. Mahad was much better than I ever was. It was a gift, I think.

LS: You mentioned that you discovered a number of new parasites. I was wondering how you decided on the names? *Plasmodium coatneyi*, *youngi*?

MW: With the malaria parasites of the monkeys and the gibbons, we began to look at the people that had put our team together. Malariologists are sort of a funny, closed group, always have been. Garnham's name had already been used so that was not available. Bruce-Chwatt did not seem like a handy name to hang on anything. The first new parasite we described was *Plasmodium coatneyi*, clearly named for the chief of our group. The second was *Plasmodium youngi*, named for Martin Young, the well known malariologist who was also at NIH . Then we were working with flying lemurs -- lemurs are extraordinary, interesting little critters that live in the jungles, and we found that they had a malaria parasite. We had not really honored Professor Sandosham, and so that one's called *Plasmodium sandoshami*. Then we described one that was named for Dr. John Field. Names of people in our immediate group could not be used. Therefore, *whartoni*, *eylesi*, or *warreni were not available*. No biologist would name a newly described species after him- or herself!

But Dr. Field was a remarkable individual. He had been in the Institute of Medical Research, as a British civil service officer before World War II. He was a physician, very bright and much respected. He had worked as Malaria Officer at the Institute for Medical Research and had acquired in the late '30s a new microscope that he was very proud of. Then the Japanese came and Jack had to leave Kuala Lumpur. He went south and took his microscope with him. When Singapore fell, he was interned at Changi. Then he tried to work with the control of malaria among his fellow internees while he was a prisoner of the Japanese. Sometime, late in the war -- I think it was about 1944/45, a Japanese officer that Jack had only seen a couple of times, came in and took his microscope. It was a only a matter of months after that before Singapore fell and the Japanese were out. Jack made his way back, he went back up the peninsula to Kuala Lumpur. His wife had been evacuated to Australia with their children, early before the fall of Malaya. On his return he started to re-establishing his program for malaria control and malaria research at the Institute for Medical Research in Kuala Lumpur. One afternoon, he was working in the laboratory, (no microscope at the time, he had not been able to acquire a new one) when a young British soldier came in and had this microscope. It had a little brass plate that had been appended to it, that said 'John Field, Institute for Medical Research.' And so the soldier brought his microscope back, and by God that microscope was still at the Institute for Medical Research when we got there!

Jack came back later on and became a physician working with one of the big rubber plantations. We became very good friends. Hence, the name *Plasmodium fieldi.* After Don died, the first parasite that I found in the gibbons was named one *Plasmodium eylesi*. We also named one for Geoff [Geoffrey M. Jeffery], *P. jefferyi*.

LS: Can you add something here about how you collected infected monkeys?

MW: Monkeys and gibbons were favored as pets by the rural villagers. We would endeavor to get a blood smear from such animals and if found to have a malaria parasite, we would purchase the animal and return it to the laboratory in KL for further study. Once it was learned that we would purchase such animals, we would have people appearing at our camp sites or at the IMR facility with animals for sale. Eventually, most naturally infected non-human primates were obtained from animal trappers working in specifically designated parts of Malaysia.

LS: You mentioned gibbons just now. After the rhesus monkeys you were saying the gibbons were very interesting.

MW: One of the first gibbons that Don Eyles found became their pet. Gibbons are called WA WA in Bashasa Kabangsian and so they named this gibbon WA-two. He was a pet of the Eyles and absolutely devoted to Mary Eyles. When Don and Mary left, they left the gibbon with me. I was going to send the gibbon to them as soon as they were settled in their new post. Don was going to be working for the ICMRT [The International Center for Medical Research and Training] in Pakistan. He tragically died aboard ship in Penang Harbor on their way back to the US for leave. So I was left with a gibbon. I would feed this animal everything one could think of, trying to keep it going, but it pined away, it died. I could not get it to eat anything. Once Mary was no longer there, if refused to eat and it starved to death. Part of the tragedy that Mary Eyles had to face was the loss of this much cherished animal.

By the way, putting a little bit of a different spin on this, if you have the time you might want to talk to Mary Eyles. She's an extraordinarily competent biologist. She came back to Memphis after Don died. She wouldn't let me take his body off the ship. She said, "It doesn't matter. He's dead and that will not change." Mary and children went by ship from Penang and stopped at various places until they got to Genoa. I had arranged to have the body removed and flown to Washington from Genoa. The body had already been embalmed before they left Penang. Mary stayed onboard because she said, "Suddenly I've got three children and I've got to figure out what am I going to do? How am I going to live?" So she went back to Memphis and started teaching school again and eventually moved to Atlanta and her children are all grown up -- one of her sons, her oldest, was on Jeopardy, as a matter of fact. Mary was a superb biologist and taught biology at Druid Hills high school in Atlanta for many years. She's got a little book [2002]. She titled it, "I've been writing a book all my life," and she published it herself. I've got this volume of hers in my study. It's an extraordinary little tome, not so much for the science but for the insights into what was going on with all of us in Malaya. She was a part of it. She worked as librarian in the Institute for Medical Research while we were there. (Mary Eyles died on August 29, 2005 and was buried in Arlington National Cemetery, next to Don.)

LS: Was there consciously an ecological approach to all this work for you?

MW: I think it's very important to keep in mind that during the period we're talking about -- this probably starts with maybe the insights that came out of the Panama canal -- that people contributing to tropical diseases and their epidemiology, were basically very gifted biologists. We saw these among the British. Have you ever read any of Stanford Rafell's writings from Malaya and Singapore? The British sent out civil servants who had broad, open minds, especially in the field of biology. There are extraordinary books on Africa with descriptions of mammals and birds, etc. These people did extraordinary work. This was the grandfather of the malaria business that went on through the first six decades of the twentieth century. That's when everybody had to have this open mind. They were all trained as biologists. There were people like Dr. Fred Dunn who is at the University of California in San Francisco. He is a physician but had trained himself and worked with other people to become a basic biologist, as did many physicians. Jack Field considered himself a biologist first and a physician second. Part of the field equipment in Malaya were plant presses. If one is a true field biologist, everything is part of what you must see and understand Yes, in answer to your question, it is all basically the ecology of the system that constitutes the transmission of malaria. This applies to other infectious diseases as well.

Nowadays, the area of specialization has become so strong that I'm not sure how many people we could find if we had the same problem confronting as was true in 1957/58. I don't know where we would find those people who were broadly enough trained and had a broad enough interest to say, "I'll look at anything. I'll listen to anything," because everybody is so specialized today. This young Dr. Singh, is gifted in terms of working with DNA and genomes but he doesn't really have that experience to go back and start looking at what makes the system tick, and that is an ecological approach. He has discovered an area where *P. knowlesi* is being transmitted not only to man but from man to man. To really understand this kind of situation, you've got to look at plants and animals and the environment in which they are found.

I will say that frankly that if we had to start today, and we had the same issue evolve in Malaya, it would be very difficult to find people who were not so specialized that they would be unable to function effectively in that kind of milieu. I am not saying that we were so special. It was a different era, with a fortuitous grouping of skills and interests.

LS: I originally broke down my questions about the project into materials, locations, collaborators. I want to talk a little bit about locations, which you've already hit upon. Malaya to begin with, what was it like in the 1960s there? You mentioned that Kuala Lumpur was close to the jungle.

MW: Very. It was a very provincial city. I was just looking the other night at a program -- on National Geographic, I think, on megastructures -- on these extraordinary Petronas Towers that they have built up in Kuala Lumpur. I looked at the pictures of downtown Kuala Lumpur and there's skyscrapers everywhere. This was not part of the 1960's Kuala Lumpur. It was a provincial city in a politically stable area, which in that period of time was somewhat unusual. Sukarno was in Jakarta, and the Thais were getting along pretty well, but Burma was already beginning to show its warlord problems. Bangladesh was trying to figure out how it could relate to Pakistan all the way across India; Sri Lanka was already having trouble with the Dravidian Tamils that were in the country. Vietnam was a problem. Laos and Cambodia were really bad news. So Malaya sort of stuck out.

It was a place where they elected the king. Malaya, the peninsula, is divided up into series of sultanates and the sultans of the various states (Sultanates) get together once every five years and elect one of themselves to become the Agong, or paramount ruler. It's a set up like the monarchy in the UK in that the legislature is elected by the population. The only trouble that was going on in Malaya at this time was the effort of the Chinese to find where they fit in. They were already a very large segment of the population, and were dominating the economy.

LS: That's the ethnic Chinese?

MW: Yes. These were people who'd been there for generations, but they were now dominating the economy. The Malays were getting increasingly paranoid about what was going on among the Chinese because of a fear that they were going to take over political power. The Malays lived in their small kampongs (villages). They are an attractive beautiful people and have a wonderful culture. However, such features were not prepared to function very well in the mid-twentieth century. The Chinese couldn't care less. They were really out for themselves. Malaya was stable, and its independence from the UK came while I was living in Kuala Lumpur. Living there was wonderful. If you were really were dependent upon a lot of things coming from the outside, forget it, you're not going to get very much. Supermarkets didn't exist. You lived on the local economy which was very rich and very productive. I had a house that was assigned to me by the Malaysian government as a matter of fact. I first lived in something called a chalet, which were great little houses -- a bedroom, a small living room, a bath -- scattered around a park-like area with a central mess, British army style mess. That's where I first lived when I was in KL.

Among the US State Department people, Kuala Lumpur was one of those little known jewels that people didn't talk very much. There was a fear that everybody would find out about it and the wonderful back water would be gone. It was hot. The northeast and southwest monsoons both catch Kuala Lumpur. We never had quite the flooding from the northeast monsoons that comes to Thailand, Cambodia, and Vietnam, but it would rain. We had potential rain anytime of the year. Therefore gardening, growing hibiscus and orchids and so forth, was just a wonderful. It was a marvelous place to be. We enjoyed it so thoroughly because there was no real separations of people at ethnic levels.

As I mentioned, every Saturday, we worked until 12:30. Don, Harry, and myself started a process among ourselves of going to one of our houses at 12:30 on Saturday for beer and finger food. The finger foods you can get in Malaya are just, and everybody vied with each other to find something that was different to eat at these sessions. We'd go one week at Harry's, one week at Don's, next week at my house, next week at Sandosham's, and then we'd start over again. People from the US Army unit and the Hooper Foundation eventually joined the process. Potential visitors knew that if they came to Kuala Lumpur -- visitors from the London School [of Tropical Medicine and Hygiene], from WHO [World Health Organization], from wherever knew -- that if they came to the IMR at 12:30 on Saturday they would be able to find this little get-together and there would be cold beer, food, and talk. They could talk about biology, endless arguments about ecology and malaria and so on. Sometimes it would be four o'clock in the afternoon before you broke up. We did the same thing when I was at NIH. People would gather on Friday in the restaurant at the National Naval Medical Center, Bethesda, MD. Everybody knew then that the group of parasitologists would be there. This would include Leon Jacobs, Bob Coatney, Bill Pacheco, Allan Cheever, Kendal Powers, Harley Sheffield and many others whose names I cannot remember at the moment.

Most people interested in tropical medicine and parasitology knew that the group gathered in the restaurant at the Navy on Friday and had lunch together. It was again that same idea that if visitors came, they would find this group at the Navy at 12:30 on a Friday. They may still do this, I don't know.

LS: There were a lot of interesting places you went. You mentioned East Pakistan.

MW: Visits to East Pakistan were for the collection of malaria free monkeys use in the laboratory in KL. We also worked in Cambodia near Siam Riap. Extraordinary! We were in a little place called Pailin, and again, working with deep jungle populations, looking at mosquitoes for potential malaria vectors in the jungle. We were trapping mosquitoes, catching monkeys, and looking at blood smears from people. We had taken over our vehicle to the area. Some weekends I could drive about a half an hour or 45 minutes to Angkor Wat which was absolutely incredible.

Among the sights that I've seen -- I suppose I was awestruck at the Pyramids of Giza and the Acropolis of Athens -- most awe inspiring of all was Angkor Wat. There's something about it that's so striking that you just have no way of describing it. It's an incredible experience. Phnom Penh was a lovely little provincial city with beautiful girls. The Cambodians were very gentle, lovely people. It just absolutely boggles my mind that the Khmer Rouge with their brutality were Cambodian. I could not visualize how that could come out of these wonderfully gentle people. We were looking at any kind of population that lived in the jungle where mosquito liaisons between monkeys and man could be expected. The Mung in Thailand and the Montenard in Vietnam were initially interesting, but the absence of monkey malaria in Thailand and the negative results of the study in Cambodia made the human populations unlikely to have monkey malaria. The Dyaks in Borneo were of great interest, but we never had the time to get into this area. More recently, there has been demonstrated to be a human population with endemic *P. knowlesi*. It is interesting that I had an opportunity to re-visit these areas of Indochina (Cambodia, Vietnam, and Laos) as a part of my assignment to the WHO.

LS: Going back to a comment you made about Cambodia, you said you brought your own monkeys with you.

MW: Yes.

LS: These were uninfected monkeys into which you would transfuse human blood to see if there were monkey parasites in the human population.

MW: Yes.

LS: Can you expand on this? Did people react differently to venipuncture than to finger pricks, as might be used for smears?

MW: Venipunctures were more complicated than surveys with blood from fingersticks. When venipunctures were a part of the protocol, we had to have physicians in the team. All blood surveys were made with arrangements with local authorities in the Ministry of Health -- usually the malaria control program. When working in rural areas, we always attracted a considerable amount of attention. Children would be around watching what we were doing from dawn to dusk.

[break in audio]

MW: There was a project established by the World Health Organization to look into the potential for eradicating malaria in Malaya. (By that time, some people were beginning to ask questions concerning the biologic feasibility of world-wide malaria eradication.) The WHO project had a transect of Malaya from the central highlands down to the coast which included a whole series of different vectors. *Anopheles maculatus* was a known vector in the hill country. *A. sundiacus* transmitted malaria in the swamp areas near the coast. In between were mosquito species such as *A. philippinensis* and *A. barbirostris* whose vector potential had not been established. With the established transect they set out to prove that they could eradicate malaria in all parts of Malaya.

Dr. Huni was the director of the program in Kuala Lumpur. He had an entomologist who was his deputy, Douglas Moorhouse who's now at the University of Brisbane. We worked with Douglas a fair amount. Part of the team that I was talking about, included some of the people from the WHO group. All sorts of lines were crossed in terms of organization because nobody really gave a damn whether you were WHO or NIH or Army or Hooper Foundation or whatever, it was all one big group. The WHO group demonstrated very quickly that the techniques they had available were not going to eradicate malaria in Malaya, there was no question about it. It simply was not going to happen. One major problem was recalcitrance on the part of the people in the treated villages. The disappearance of the thatch roofs in the houses was very serious because normally they had to replace a thatched roof about every five years and they were now finding they had to replace the roofs every year and a half or so.

These Malay people in the villages are very house proud. Having to replace the roofs is expensive and they just couldn't afford it. They blamed the problem on the malaria program. Well, nobody believed that - "Come on, don't be ridiculous, what's the malaria program got to do with thatched roofs. Actually they did some studies and found an atap-consuming moth -- the larvae consume the atap -- that lays its eggs in the atap roof. There's a wasp that preys upon these larvae. 'Big fleas have little fleas upon their backs that bite them.' The wasps were driven away by the pesticide. The effect of DDT to repel insects was a major issue; you recall we had a trouble with mosquitoes landing on the walls and not spending enough time on the wall, being repelled rather than killed by the DDT. This was a problem, especially in parts of Africa where the *A. gambiae* wouldn't stay on the wall long enough to get a lethal dose. So DDT had that capacity for repelling critters, and the wasps weren't coming to the roofs anymore and it left a field day for the moth larvae.

In addition to that the Malays in the demonstration area also found themselves plagued with bedbugs. Again they blamed it on the malaria program, and they would lock their doors. There was a schedule for the sprayman to come around, and when the villagers knew the sprayman was coming, the village would be deserted, the doors were all locked. There was nobody there. Therefore the house could not be sprayed. What did the bedbugs have to do with the Malaria program? Well, the bedbugs lay eggs which are preyed upon by ants, and the ants left because DDT was there and the *Cimex* gets resistant to DDT very quickly, probably as quickly as the cockroach. Therefore the bedbugs really were proliferating.

So members of the group got together and wrote a paper dealing with these issues. Douglas Moorhouse, the entomologist from WHO; myself; Harry Wharton; Don Eyles; and Cheong Weng Hooi. The publication pointed out that with the technology and the management available, malaria would not be eradicated in Malaya. The WHO representatives reacted quite negatively. Douglas got a reprimand for being involved and they wouldn't let him be on the paper. It really got quite unpleasant.

At that time then, WHO was philosophically in one place and many others were someplace else. This was when WHO was trying to get countries to commit themselves to malaria eradication, and Malaya had come to the conclusion that they were not going to be able to achieve this goal. Control programs are not the same as eradication programs. If your philosophy is eradication then there are certain things you have to do -- you have certain goals, your technology, training of personnel. Everything you do is different. When you say, "We're going to control malaria." The goal might be to eliminate fatalities, for example. That's a whole different can of worms in terms of how you manage it, how you plan your protocols. Malaya came to the conclusion that they had to come up with some other approach. They were going to work on malaria control with the goals of reducing incidence, morbidity, and eliminating mortality. They felt they could actually eliminate mortality due to malaria.

The most advanced country in South East Asia by far and away was Malaya. Thailand had said they were going to eradicate malaria. Indonesia was going to eradicate malaria. The ultimate absurdity, was with Indonesia. It was a terrible blow to the international malaria community and WHO, to have this country [Malaya], which clearly had a better chance of making some inroads into malaria than any other in the area, saying, "We're not going to eradicate malaria. We can't eradicate. We have to have our approach be control." We all got accused of saying we didn't want to eradicate malaria because we were afraid we'd lose our jobs and this sort of thing. You'd be surprised at the accusations you heard. It was really quite unfortunate. Don and Sandosham went to a special meeting in Saigon during this period and stated in a presentation to a plenary session that Malaya could not eradicate malaria. They were accused of saying they wanted to keep malaria going because otherwise they were going to lose their jobs. It got to be very unpleasant because the commitment, the emotional almost religious fervor with which people dealt with the eradication had become so manifest that you really couldn't naysay them. This is what I feel was wrong with WHO: they could not listen -- they were not hearing anything that wasn't going their way. It was most unfortunate.

[End of side two, tape one.]

MW: Jeff Sachs and his group are involved in a program looking at economics and malaria and this is a whole new sort of world in which the primary issue is not concern with the individual. We as malariologists had always been concerned with the person in a village who's dying, the kid who has cerebral malaria on Guadalcanal, etc. Jeff's approach is economic. I don't really understand much of what he says because this is a level of economics that really gets beyond me. Jeff very quickly will move off to someplace that I don't really fathom. As I understand it, the ultimate impact of malaria on economics is not how many people die in Nigeria. In Jeff's view, malaria drives away every fund that could come into an area to better its economic situation. Investors are simply not going to come there because of the malaria. This is such a different way of thinking. Because we as malariologists were always motivated by the goal of eliminating malaria mortality.

For Jeff Sachs -- and I'm not challenging him, God knows, I'm not prepared to challenge Jeff on this sort of thing -- the sick individual was not the important issue as far as he was concerned. There's a publication going to come out of this with Andy [Andrew] Spielman and Jeff and others. It was a very interesting sort of business, but a totally different way of looking at economics and malaria. For somebody like myself, who's a classic malariologist, very much imbued by my own experience, it was very difficult for me to deal with this. I still have difficulty, when talking about malaria, if I cannot talk about the disease and its impact on individuals and villages and communities. The new approach to economics and malaria really is one that I don't understand.

LS: It's a whole different worldview. Before the break, we were talking about transporting blood in South East Asia and you made a face.

MW: I can remember we tried to bring the blood into this country. As a public health service officer, if we had notified them far enough them advance then there would be no problem when you came you entered in Seattle, San Francisco, LA, or Honolulu.

We transported blood back to put into the prison volunteers. Yes, we did that. As I say, those protocols were carefully evaluated at the time. We were within the parameters for how such protocols were then examined. Now I don't think you'll find any of us trying to defend the fact that we put whole blood into prisoners. This raised all sorts of issues such as hepatitis.

We're not trying to defend that, it's just that, within the purview of the protocols that were available, we did exactly what we were supposed to do. We did bring back chilled blood. I brought back most of the monkey malaria strains that we worked up, and that Bill has worked on down in Atlanta. I brought these back myself either in blood or mosquitoes. I remember bringing back some trypanosomes from El Salvador and I carried them in a tube in my pocket to keep them warm. When we were later out in Malaya the second time and my wife was there, she would take back blood for me. She'd be met by a public health service officer and not have to go through customs, she would just cruise through. It was one of the times when she enjoyed direct participation in the program.

The mosquitoes were a threat. We brought back many of the monkey malaria strains and some of the mosquito species this way. Some of the monkey strains we brought back in monkeys, that is we actually infected the monkey, and as soon as he became positive, put him in a crate and shipped him to Atlanta. That was how we got some of the strains back. Others were frozen, and now all of these are on deposit with the American Type Culture Organization. They are now available for use by anyone who requests them. On occasion, we brought back mosquitoes, but never infected mosquitoes. We might dissect out sporozoites to bring back, but we never brought back live infected mosquitoes.

LS: Some of the blood brought back was your blood, when you got *falciparum* malaria?

MW: Oh yes.

LS: From Cambodia?

MW: That is a dreadful story. I was about to be court marshaled. Because the first time that I had malaria was in Bangladesh. G. Robert Coatney was not happy about this. I got a very serious going over as a junior officer from the Public Health Service from NIH. Dorland Davis whacked at me, and it was all very serious.

I had gone to Cambodia and was working. This was when I was in the field near Battambang. I came back from Cambodia, and I came down with malaria. This time I had been taking my prophylaxis because I knew that I had to do everything just so. Don believed me, and we took some blood from me and sent it to Peter Contacos at the [Atlanta Federal] prison. Meanwhile up in NIH, Coatney was going berserk over the fact that I had malaria a second time and he was absolutely furious. Coatney had threatened to court marshal me. Peter put the blood into a prison volunteer who came down with *falciparum* malaria. They treated him with chloroquine, and he relapsed and saved my career. When the infected prisoner relapsed, it was establish that I was not at fault with my prophylaxis. In the meantime, Don Eyles was very concerned that I had [chloroquine-resistant] *falciparum*. We'd seen what *falciparum* malaria could do if you can't treat it, it's a damned ugly business. Don started working with Major Diamond at the British Military Hospital in Kluang, which is just outside Kuala Lumpur. I was hospitalized there with mild malaria. They treated me with quinine. But it was a dicey period: I was not to be in Malaya anymore, about to be thrown out of the Public Health Service. It was a serious bit of business because G. Robert was absolutely furious with me for having come down with malaria a second time.

LS: You were forgiven: You got him the drug-resistant strain he could work with.

MW: Yeah, that worked out. Peter Contacos had some pretty good influence with G. Robert at this time, and he was pushing: "When you see how lucky we are to get this drug-resistant strain and get it established" and so forth. I laugh about it now but it wasn't funny at the time.

LS: There are still a few people I would like to ask you about. Charles G. Dobrovolny. Did you work with him?

MW: He went out before Don did to establish the first liaison with the US Army Medical Research Unit at the IMR. I knew Dobrovolny but we never worked together. By the time I got out to Malaya, he was back in Atlanta and the program had gone to Don Eyles.

LS: I'm going to throw out a few more names, some of whom you've talked about a little. Gordon Bennett, you mentioned him, and he also had a malaria incident with *cynomolgi*?

MW: He came down with malaria, yes. I'm almost reluctant to talk about this because we had some *Anopheles introlatus* and wild-caught sporozoites and Gordon was inoculating ourselves subcutaneously with these -- which we never said anything about -- because I was very anxious to know whether it was a human strain and would grow in man. Gordon did come down with malaria but his was another infection and it was by accident in the laboratory which was not that uncommon. I had another accident in a laboratory in London which was really rather hysterical, I'll tell you about it some time. That was very interesting. [We get to the London incident, below.]

Gordon was the son of missionaries in India. Bright, clever, and we were looking for another professional to come into the laboratory after Don was no longer there. I'm not sure how G. Robert found Gordon. All of a sudden his name turned up and I began to look and say, "He's got all the stuff." So Gordon came out, and we worked together and published a number of papers together. We were very busy. He was the one who was finding abnormal or unusual morphology of the sporozoites. He called them cricket bats. They looked like cricket bats instead of being doubly pointed. He was the one who did much of that very strong work to identify the sporozoites in *Anopheles umbrosus* as being from the mouse-deer malaria and not from a primate malaria.

Gordon went back to Newfoundland, to the institute in Newfoundland,[8] and Marshall Laird was the director at the time, and later Gordon became director. I lost contact with him and then two years ago I heard that he died. I know very little about him. He and his wife had divorced. Sorry, there's not much to tell you about Gordon.

LS: Cheong Weng Hooi?

MW: He was the entomologist. He did his studies at the London School. Interesting guy, he was not very happy at times because he went to the London School while Harry Wharton was the chief of the entomology group at the Institute for Medical Research. He wanted to get his PhD from London because in Malaya, at that time, having a doctorate degree was very important. Harry felt that he needed somebody to come back into the

[8] Today home to International Reference Centre of Avian Haematozoa, Memorial University of Newfoundland, St. John's, Newfoundland.

institute and did not support Hooi staying for an extra two years in London to finish a PhD. He felt if Harry had given him the support he needed he would have finished -- bright guy, very bright. But probably not that dedicated -- whether or not this issue of the PhD influenced the way he thought about things is an open question -- but Hooi was never as willing as the rest of us to undergo the hardships and difficulties of being in the field. It was not something that he enjoyed as much as Don and Harry and myself. He was a good friend and he had a remarkable wife. Sue Yong was Hokin and he was Cantonese. So they did not understand each other, they both went to University of Singapore, and so they had to use English. English was their language because their dialects were so different. Sue Yung was a real gourmet about Chinese food, she knew all the various dialect groups of foods. We'd go out to eat, and she'd say, "Try this, and if you like it, I'll tell you what it is in Chinese. If you don't like it, I'll go ahead and tell you what you're eating." I remember one night she fed me something that was hard and not tasty good -- and I said, "No, I think we can forget this one." It was sheep oviducts. That was typical of Sue Yung. I tried something, and if I liked it she told me what it was in Chinese and I wouldn't have to worry about what it was.

But Hooi was the entomologist there, and he continued to be there after I left and after Harry left. He was a senior entomologist at the Institute for Medical Research for quite some time. He's retired now, still living in Kuala Lumpur.

LS: The only other person that I had on my list that you had mentioned but we didn't talk much about was Elizabeth Guinn. She was an American?

MW: Liz was an NIH technician. She was primarily a blood technician, working with blood smears. We had all known her for years in the laboratory back in Atlanta. She kept pushing and pushing to get to be assigned to Malaya. The first technician that they sent out from Atlanta to work for me in Kuala Lumpur was Jimmie Skinner. Jimmie came out, and he's a gifted, superb individual. I think he was one of the reasons that Martin Young was so adamant about my going away to school because Jimmie was my immediate superior at Columbia, South Carolina, when I first went to work there. He was very bright, very clever, really -- and he had been talking about going away to go to school. Martin was very fond of Jimmie and wanted him to go away and get his further education. Martin kept pushing and pushing, and Jimmie would never pick up on it. He would put it off, put it off, put it off. I think that was part of the reason that Martin was so adamant with me -- "Decide now. If you're gonna do it, don't talk about it, do it."

Jimmie came and worked for me for a couple of years in Malaya, very, successful, great technician. It was wonderful to have somebody with that sort of experience to be in charge of the lab because we read thousands of blood smears, both ones field-collected and the blood smears on 200 captive monkeys every morning. All those blood smears had to be examined.

When Jimmie went back, Liz had been pushing for this assignment, she was an older lady and wanted one last chance to be outside the country. So she came, and she worked as a blood technician, not as successfully as Jimmie. She was not as dedicated to the task. Jimmie would work as many hours as were necessary to get all these things done. Liz was not prone to spend that much time. But she was a good technician. I enjoyed having her there. She went back to Atlanta, and she died about 10 years ago.

LS: Jimmie Skinner also worked with Bill Collins for a long time.

MW: Oh yes, for many, many years in Columbia and in Atlanta. He was one of the people who had as much success at avoiding administrative issues as Bill did because as a technician it didn't make any difference how competent or how capable, or how dedicated you were, you weren't going to go above a GS-9 unless you took some administrative responsibilities. Jimmie did not want administrative responsibilities, and he wouldn't take them. He would not accept the responsibilities that would get him a GS-11, 12, or 13. So he worked for years at the technical level. He's retired now. He came to Bill Collins' celebration for 50 years of government service.

But Jimmie was not going to take any administrative responsibilities. Bill wouldn't take any, and he got away with it, but Jimmie just never got a promotion.

LS: When you first went out to Kuala Lumpur, what did you think the duration of this program would be? How long did you think you were going to be out there?

MW: A year. I had a leave of absence from the University of Oklahoma for a year. I had begun to write back to my immediate boss at the medical school in Oklahoma City saying, "This really is my thing, there's no question about it." I think Phil Smith understood this very well. Within a few months, Phil had begun to look around to see if there was somebody else who might replace me because he didn't think I was going to come back. They extended me in the reserve from one year to two years, and then I was sworn in as an active duty officer at the Public Health Service. I went out to be gone a year, a year's leave of absence, to return to University of Oklahoma. That was extended to a second year, and then I realized that I could not come back and work in a laboratory.

LS: How did you know when it was time to come home?

MW: It was getting less productive. We were having some difficulties with the army unit over how to administer the funds. The CO of the army unit had decided that he wanted to have more control over the scientific aspects of the program, which brought some conflicts. At this juncture, I would have done anything to keep it open if it had been as productive as before. We were reaching the point at which even if there had been another new species of monkey malaria, that sort of thing, that basically we had done what we went out there to do: to define the epidemiology of simian malaria and to ascribe a basic concept of how much of a threat this was to human malaria. At the same time, the malaria eradication program's ideas, concepts, and philosophies had begun to erode for other reasons so that the immediacy of finding out about monkey malaria was simply not there anymore. NIH was no longer that interested in keeping the laboratory open because we would only have continued to be productive in terms of new species of malaria. Once it was no longer making a major contribution to the concept of human malaria and malaria eradication, it was not a primary issue as far as NIH was concerned. It was decided that we'd done what we went out there to do, and there was no point in pushing it.

I was disappointed because I would have wanted to stay, but, I think looking back on it now, it was a good decision because you take something that is good, and it's very good, but then you are only keeping it alive in retrospect. It's only what <u>was</u> that makes you go through the day, not what you're looking forward to. If had we kept it open any much longer, it would have been only because of what it had done, not what we had anticipated it doing.

LS: So you came back and you went to Atlanta?

MW: [laughs]. I went to Atlanta.

LS: Why do you laugh?

MW: Oh, because it had gotten to be a hassle and I wasn't very happy about it. I went to Atlanta and was setting up a research program working with Bill Collins, then G. Robert decided he was going to go on his world tour. In order for him to go on his world tour, Geoff [Geoffrey M. Jeffery], who was the deputy director of the laboratory in Bethesda at NIH, would have to take charge. But Geoff couldn't handle it all by himself, and if G. Robert was going to go on this world tour, then he had to have somebody else there. So I was only in Atlanta for a few months before I was transferred from Atlanta to Bethesda.

Geoff and I had known each other for many years, but that was the first time when we really had an opportunity to be really close. Geoff had always wanted to come out to Kuala Lumpur. But he always had to mind the store back in the States. G. Robert traveled but Geoff didn't. That's when we started *The Primate Malarias*, the book.[9]

Geoff and I decided that we really needed to get all of these malarias organized, the plates of all of the primate malarias, not human but monkey, gibbon, and so forth. We had the material to make illustrated plates, the peripheral blood stages of all these parasites. We then went to the medical illustrations department at NIH, and they had a young lady, Gertrude Nicholson who was fantastically gifted in terms of doing drawings for plates. She and I began to work any hour she had free from whatever project she had, she came, we sat at the microscope and I found the individual parasites for each and every one of those plates, that is, the maturing stages. And she would render them and draw them, and I would look at them and say, "Oh, this is not right," and changes would be made.

Geoff and I did this work. There is a certain level of bitterness about this, too, in that Geoff and I were the ones who started the project. We were the ones who wanted the work done. G. Robert decided it was a good idea, only he was going to make it a much bigger thing. Geoff and I were just going to do a manual, but G. Robert had a vision of something much bigger, much more involved. Geoff was not invited to continue to participate. That's as mild as I can put it. He was just frozen out of it, he wasn't involved anymore. We went on and finished illustrating the parasites and developed the program and then wrote the book, and I had certain chapters that I was supposed to write. G. Robert and I did not end up being very good friends toward the end, before he died, we were not good friends at all. G. Robert retired from NIH, and Geoff became director of the laboratory. Then Geoff retired, and I was left at NIH, and I wasn't very happy. I really wanted to go overseas again.

Then Geoff went to El Salvador. He retired from Public Health Service, went immediately to CDC and was sent down to be the Director of the Central American Research unit of CDC in El Salvador. In the meantime, I was wondering what was going to happen to me. This is when Franklin Neva came in to the NIH. I really wanted to go back overseas, I wanted to go back and work in the field. John Seal was scientific director of the NIAID at the time. He sent me down to Panama to their Middle America Research Unit to see if there was something there that would attract me. Would I be happy down there working with Carl Johnson at the MARU [Middle America Research Unit]? I didn't find a whole lot there that really excited me. In the meantime, Bob Kaiser and Geoff Jeffery had approached me about the possibility of my moving as a Public Health Service Officer from one PHS assignment to another. I was transferred from NIH to CDC and immediately transferred to CDC in El Salvador: I was back working with Geoff again, this time in El Salvador.

That's when I had conflicts with G. Robert over the book. I was busy setting up a new program. I was out in the field all the time. I could be sympathetic to G. Robert's approach to the book, but that was a peripheral issue as far as I was concerned because when I got the plates done, my primary interest in the project was over. Writing up the details on each one of the species -- which Bill Collins, for instance, loved -- was not something I was terribly excited about. I had a very busy time, and I had an extraordinary experience with G. Robert who had retired and was living in Atlanta. He went to the Director of CDC, David Sencer, to have him pressure me to finish some chapters that I was thinking of writing for the book. David and I are good friends, and so David knew that Geoff and I had not been in El Salvador very long. We were really working seven days a week. We were as busy as we could be. So David went to Bob Kaiser -- this may be a name that you've heard -- who was

[9] G. Robert Coatney, William E. Collins, McWilson Warren, and Peter G. Contacos, The Primate Malarias (Washington, DC, US Government Printing Office: 1971).

director then of the Tropical Disease Research Program at CDC, and that's where I was assigned. Bob [Kaiser] said, "Well I am not prepared to step in because Geoff Jeffery is his immediate superior." So he went to Geoff and broached the subject -- I didn't know this had happened for a while -- but Geoff hit the roof apparently. He wrote back and he said, "G. Robert Coatney has absolutely nothing to do with what this officer, assigned to the Middle America Research Unit, has got to do. If this officer decides that he's got time to do this, that'll be his decision. There will be no pressure." This really created problems.

I really never had a good relationship with G. Robert after that. Bill Collins tried to maintain some sort of liaison but I think Bill was very much aware of how difficult Bob Coatney was. Bob was very single-minded. I give the devil his due. He was a very bright guy, very focused, and when he set out to do a project, by God, he got it done. Malaya is a very good example. He got the job done. However, he could be rather ruthless in terms of his own personal involvement. He had an ego that was as big as all outdoors. The biggest thing that we did in Malaya was to enhance the reputation of G. Robert Coatney -- as far as he was concerned. Sorry, that's not nice, but that's the way it was. That was probably, in all the years I spent in malaria, the only personal conflict I ever had.

When I was first assigned back to Bethesda and after Coatney had retired, and Geoff and I were trying to figure out what we were going to do, we had a number of people assigned to the lab and it was a funny business. This is when I had a technician, Sugia Shiroishi, who was a remarkable woman. One of the finest people I ever had an association with. She was a Neisi Japanese. She was evacuated from southern California at the beginning of WWII. Her father was a farmer in southern California, and in 1941, they were packed onto a truck and hauled to Arkansas. Her father died at an interment camp in Arkansas. She managed to get out, and she worked with Bob Lewart in Chicago and got her technology training. Then she worked she with Clay Huff at the National Naval Medical Center in Bethesda. When I was coming back to Bethesda, Clay was getting ready to retire from the Navy, and he said, "This young lady is best technician I've ever had. I'd like to get her established with something before I retire." And so Sugia came and worked for me at NIH and was just a wonderful technician.

But it was not a productive time. It was difficult working in the laboratory after that extraordinary experience in Malaya. It got to be that very little else was that productive.

LS: What were you working on in Bethesda?

MW: Looking into use of some of the bird malarias, *Plasmodium gallinaceum* in chick embryos, for test patterns, for drug studies, looking primarily for exoerythrocytic stages. But it was at this time that P. C. C. Garnham in London contacted the people at NIH -- he had contacted NIH before Bob Coatney retired -- about the possibility of my coming to London. At first it was out. Bob Coatney and Garnham didn't get along. Garnham thought that Bob Coatney was a good friend, but Bob Coatney never felt the same about Garnham. Prof. Garnham lived on a different a plane from all the rest of the world. He couldn't imagine that there was such a thing as conflicts and competition in the field of science. It was just inconceivable that that kind of thing could influence how you felt about things. That just didn't happen. We were still working on exoerythrocytic stages of malaria, so Garnham petitioned NIH to see if I would like to go to London and work at the London School of Hygiene and Tropical Medicine. I had not yet done a postdoc at this time. Bob Coatney left around that time and Geoff said, "I'll handle this place." Bob was not very happy about my going to work with Garnham because he assumed that the work we had accomplished would be associated with Garnham rather than himself.

I went for a little over a year as a postdoc working with Professor Garnham at the London School, a wonderful experience. That was when one of my mosquitoes infected his secretary. We were doing x-ray irradiation of *Plasmodium cynomolgi* sporozoites. I'd get all these mosquitoes infected by feeding them on infected monkeys. There would be hundreds of infected mosquitoes from the insectary there at the London School. I had hundreds of mosquitoes with sporozoites. Then it would be the day to inject them into an uninfected monkey. I was irradiating them with x-ray. We would dissect the mosquitoes to tease out the salivary glands which contained the sporozoites. I'd get all these people together and put up nets to prepare an area for the dissecting activity. There were some well-known names involved in this activity. Leonard Bruce-Chwatt, P. G. Shute, Bill Bray and Garnham -- some of the foremost names in the whole field of malariology and myself -- all dissecting mosquitoes to dissect out the salivary glands with the so I could get zillions of sporozoites. Then I'd run downstairs to Gower Street to catch a taxi and go to Guy's Hospital with this tube of sporozoites in my hand [laughs], run to Guy's Hospital -- Guy's was were Professor Garnham got his MD, and so he had an in there. I'd go down into the bowels of the Guys Hospital and they'd irradiate these sporozoites and then I'd race back to the London School, run upstairs and inoculate the monkeys with the irradiated sporozoites at different levels of radiation.

We first found this extraordinary thing. We were looking at the livers. I did liver biopsies, and there's another story that's extraordinary about the liver biopsies. I found out that, if I irradiated the sporozoites as a certain level, nothing came through. However, there was a level of radiation at which I would get a very attenuated infection. Giving the sporozoites that had been irradiated at a certain level, you would attenuate the infection and you were obviously producing an immunity with the sporozoites. But nobody gave it any serious thought because who the hell can get enough sporozoites to produce a vaccine in this manner. It was a very interesting finding. I remember sitting at the microscope for hour after hour looking at Giemsa stained liver sections. A technician in Professor Garnham's laboratory named Nesbitt was absolutely an artist when he stained the sections. I never really learned to do them as well as he did because it was alchemy. I don't know what he did. Poor Nesbitt did everything he could think of to teach me. I could stain them well enough, but I never had that artistic touch that Nesbitt had. He was one of those technicians who was a past master at being a technician. That's what he did.

I remember they turned the heat off at the London School at a certain day in April. it didn't make a damn what was going on outside. It was so cold that spring that lubrication on my microscope stiffened. It was very hard to manage the fine adjustment. I remember Professor Garnham coming in one morning, "Hey Warren, what's going on?" I said, "Sir I'm really having a terrible time." "Well what's the matter?" And he had this very high squeaky voice. I said, "Well it's so cold in this bloody place that I can't run the fine adjustment on the microscope." "Oh really?" A few minutes later one of maintenance people came up and for the first time in the history of the London School they turned the heat back on. [laughs] I had a parade of students coming in to thank me for getting the heat back at the London School because I couldn't turn the fine adjustment on my microscope.

It was a wonderful experience though. Then one day -- maybe it was about two weeks after one of these dissection sessions -- Professor Garnham's secretary was not feeling well and her family was not happy about her working in this pest hole of the London School in the first place. And sure enough, by gosh, we did a blood smear, and she had malaria, clearly *cynomolgi*. She was hospitalized at the Hospital for Tropical Diseases in London, and it was touch and go as to what impact this accidental infection would have on the school, really dicey. I wasn't surprised. We were dissecting so many mosquitoes that the fact that one of them got free didn't surprise me, but the damn thing went out of the room where we were dissecting, down a corridor, through a double swinging door, down another corridor to get to this woman sitting in Professor Garnham's office to infect her. She had *cynomolgi*, no question about it. We put her blood back in a monkey to confirm it.

The liver biopsies on the monkeys were a very primitive sort of thing. I had been doing liver biopsies in Atlanta, and we were pretty skilled at it. We didn't have a room set aside for the biopsies in London. We used a space that was alcove on the main corridor. We were using ether anesthesia for monkeys, which didn't ring my chimes very much. We didn't have any cauterizing irons that were appropriate. You have to cauterize the cut surface of the liver or they'll bleed to death. So I would anesthesize the monkeys and Prof. Garnham would come out to "help." We had this damn flame from a Bunsen burner. The flame was quite high and we had a cauterizing iron that appeared to be something you would use for ship building -- a monstrous thing. They'd put that thing [iron] on the burner and then you'd cut out the liver - Prof. would always cut out more than I thought the animal could afford - with the flame going and the ether going I thought, "Oh my god I'm going to die in here." Then comes this great cauterizing iron. We did all those liver biopsies with this extraordinary technique and it worked. We didn't lose any monkeys, and I found the parasite, *cynomolgi*, found them all, and it was a very successful program.

I had a wonderful time working with the people at the London School. Leonard Bruce-Chwatt was there as the director of the Wellcome at that time. Bill Bray was back. Walter Omerod was there. Frank Hawking used to come to the School from the research facility at Mill Hill. Frank Hawking worked in filariasis, and he has a son, a rather remarkable son by the name of Stephen. It was again one of those extraordinary places -- thinking about the things that are important in my life somehow seem to be so tied up with the kind of people that were involved. As you've heard, the IMR was just such an extraordinary experience. In London it was the same thing. It was an extraordinary group of people. I still hear from some of them, Bob Killick and Malcolm Guy. Malcolm Guy is just retiring from the MRC institute in Gambia. He's been there for 15 years, I guess. A funny, very interesting group of people. It was a wonderful time in London.

I love the city of London. I became a real Londoner. I didn't have a car. Every evening I was at the lab until late, and you know where Gower Street is? Where the London School is? It's just up from Bloomsbury and one tube stop down to Haymarket where the theater district is located. If one waited until eight o'clock at night, there'd be always a seat or two at some show. I went to the theater as often as possible, sometimes twice a week! It was just an extraordinary experience. If nothing else was available, you could go to the Haymarket to the repertory theater for Shakespeare. There was something different every night there. A wonderful place to be. London wasn't that expensive. Now -- I took my son back. He wanted to visit as a reward for his graduating from high school, but he waited until he was a sophomore in th university to tap into the offer. I took him back, and he wanted to see where I'd been to school and where I had lived. Crikey, it's gotten to be so expensive now. I couldn't do anything now, like I was able to then. Wonderful time. Sorry.

LS: No, it's fine.

MW: All these remembrances -- it's interesting. It's like a chain of consciousness -- I feel like James Joyce somehow, you know?

LS: Well, I'm interested in what you remember of the times, what was the texture of being there? So that's all interesting.

MW: The texture is people. It's very interesting, the texture is almost always people.

LS: Around this time I see you have some publications with Bill Collins and some other people on fluorescent antibody assays. This ties in with what you were doing in Malaya and El Salvador.

MW: Yes. The first paper Bill and I published was on the ecology of malaria in areas of Malaya was done by means of fluorescent antibody. We did a lot of this kind of work in El Salvador.

These were assessment programs. It was a means by which you could look at the age and sex distribution of antibody titers and begin to see what kind of impacts your programs were having on the actual transmission of malaria. That's why we were interested, because it was so difficult to track mosquitoes. With *Anopheles albimanus* in Central America, you don't get enough natural infections to get any real idea of what's going on, that's not going to be your technique.

LS: So it's a geographical and epidemiological survey?

MW: Sure, you actually go through and bleed populations, and then you break them up by age group and sex and then you're going to begin to tell what's happening to the malaria in that population by looking at changes in fluorescent antibody. Not necessarily individuals, but groupings according to age and sex. We were interested particularly in finding means of evaluating the application of a control method.

I was only the source of the materials that were going to be analyzed but the skills and techniques for the florescent antibody were not mine.

LS: The blood would go onto filter papers and you would send those back?

MW: We had zillions of filter papers, and he's still got them. Just thousands and thousands of them that we collected. We developed this technique that was so much easier: just putting a drop of blood on a filter paper, letting it dry, and then freezing it. We could keep them for years and years. There must have been tens of thousands of these specimens that I sent back from Malaya, and then I did them wherever I went. Wherever I would go I would get some line up of people and do blood samples for fluorescent antibody studies. I did them in Kenya, Ethiopia, of course in El Salvador, Brazil, Venezuela -- did I go to all those places? I guess I must have.

LS: Tell me about El Salvador.

MW: El Salvador was wonderful first of all because I was back to doing something that I felt I was qualified to do and that I was capable of doing. And I was no longer entangled in the business with Coatney and the new director. Frank Neva is a good friend and a very fine man. He wanted me to stay on at NIH. I have great admiration for Frank Neva, but I just didn't want to be at NIH anymore. Part of my euphoria with being in El Salvador was just the fact I was there. And working with Geoff was almost like being back in Malaya. The whole thing -- you could do almost anything. We were particularly interested in the adaptation of mosquitoes to malaria parasites. I don't know if you saw any of Bill's publications in which the relationship between as local strain of *Anopheles albimanus* and a local strain of *Plasmodium falciparum* was very specific: If you moved the strain of *falciparum* into a strain of *albimanus* from a different part of Central America they weren't nearly so well adapted. It was this very precise relationship that evolved. We were looking for markers in mosquitoes that would give us a handle. I got entangled in one of the most extraordinary experiences: If you looked at the mosquito pupae, *A. albimanus* would turn out to be different colors. We began to breed out certain colors and certain color patterns to see if that was a genetic issues that might be associated with susceptibility. I had populations of mosquitoes that had stripes, didn't have a stripe, were green, were brown and then I'd feed them on monkeys. Bill and I worked on this to see if we could find genetic markers on the mosquitoes that could be associated with this -- we never really found anything, but it was a hell of a fun job.

LS: It's like classical genetics, looking at phenotyopes --

MW: Oh yeah, it was fun. We had a great time at it. But El Salvador, presented that kind of opportunity. I had wonderful technicians in El Salvador.

LS: Where were you physically housed in El Salvador?

MW: When I first moved down there it was a furnished post, a USAID furnished post, so you had furniture. I found a house in the old section of the city that was really quite remarkable. It was an old house with tile floors and all this sort of business. It was wonderful. But it was right on a main street and very noisy. So after almost a year I found another place up on the side of the mountain above the Intercontinental Hotel, which had a back garden and a view of the whole city. I moved up there. It was long and narrow, like a railroad car. I had two servants that were just superb. Lola was the cook and she was so good that everybody in town wanted to hire her away from me. Wonderful experience. Personal life was great. Jane and Geoff [Jeffery] were very close friends. The three of us were very close. And then I met her.

LS: Your wife, Mary?

MW: Yes. And so the four of us became almost inseparable. When Mary and I were married in 1975 the only other people in the church were the priest, Jane and Geoff. Geoff served as the alter boy for our wedding ceremony. Mary was program officer for USIA [United States Information Agency] in El Salvador and other parts of Central America. The Marine Guard had a residence where they held open house each Friday evening. They sold drinks and thereby made money for their Toys for Children program each Christmas. Mary and I met at one of these open house sessions. I say that we "met in a bar in El Salvador." Mary is not too pleased with this characterization.

Personally, the life in El Salvador was just wonderful, and I loved being there. And I had projects in Honduras and Nicaragua. I was in and out of Panama frequently. Martin Young was the director of the Gorgas Memorial Institute at that time. Martin and I were old friends and so working in Panama was always a pleasure. Geoff and I did some work along Lake Gatun on the basic ecology of malaria. It was wonderful to work in the same place as Gorgas and Walter Reed. There's something exciting about being in the place where these extraordinary people had worked during the construction of the canal.

Then we started a project in Brazil, in the Mato Grosso, that was primarily fluorescent antibody. We were looking at changes in populations, changes in the experience with malaria, and we were helping to support part of the control program out of Brazil. We were looking at these populations, looking at the dynamics of change in antibody titers as a means of evaluating the impact of immigrants. The Mato Grosso was primarily controlled by big business interests in Sao Paulo. We were particularly interested in the people who lived out there worked on the fincas and diamond mines. There was a place called Diamontimo where they were supposed to be trying to control malaria. Geoff and I went, and we did our initial serologic surveys and six months later when we returned for a the second survey, the village was gone, burned, destroyed. All the people were gone. They weren't killed, but they were gone. The diamond mine owners had found that they were secreting diamonds in their mouths and they just destroyed the whole place.

Geoff and I were in a place called Rosario de Este. It was on the banks of the Piranha River. It was a little dinky village, really hard living, not very pleasant at all. I had learned to take a bath with a sarong when I was living in Malaya. I had a sarong with me, and I hadn't had a shower; there wasn't water to take a shower. So I went down to take my bath early one morning. It was very early, 5:30 - 6:00, and I had my sarong on. I'm washing because you could wash with the sarong on and keep yourself covered without offending anybody, and there were some kids fishing just up the river -- a distance of 10 or 15 yards. They were pulling out little fish about 6" long. I finished my ablutions, and decided to go and see what in the world they were catching. I spoke some Spanish. I didn't understand the Portuguese when they came back to me but they could understand my Spanish. So I looked at these fish and all I saw were teeth. They were piranhas [laughs]. And I had been taking my bath amongst the piranhas. It was an extraordinary experience.

Geoff and I were in Rio just after we'd come back from the Mato Grosso. We were staying with a friend of ours at the US embassy in Rio, and he lived in Ipanema. And it was just before Carnival, and Geoff decided that we really needed to get back. We'd been away from the lab for a long enough period of time We left the day before Carnival in Rio, and I told him I'd never forgive him for taking us away.

Mary was working in the same area. She was the budget and fiscal officer for the United States Information Agency for Central America, and Columbia was from time to time on her list. So she traveled from El Salvador to Guatemala to Managua [Nicaragua] and San Jose [Costa Rica]. She arranged concert tours for Duke Ellington -- that sort of business. She was also the primary control officer when things went bad. After the big earthquake in Managua, she was sent down to try to see about reorganization -- getting the embassy back on its feet because everything had gone down the tube. Apparently she developed a reputation for that kind of effort, because later she was sent to Cypress the day after the US ambassador had been assassinated in Nicosia.

El Salvador was wonderful, but in a different way than Malaya. Scientifically there would never be another Malaya. I was working in the control program in El Salvador. I was really involved deeply in malaria control, the management of the control programs. This is where we got our first exposure to the entomology group from Gainesville [perhaps USDA], that were working on the idea of releasing sterile males, sterile *Anopheles albimanus* males. The female is only going to mate once, and if she mates with a sterile male then she will produce eggs that don't hatch. Interesting idea, but you could never seem to get males that were either in sufficient numbers or competitive, because you had to irradiate all the males in order to sterilize them. We were always convinced that this irradiation reduced their capacity to compete with the wild males in that population. It was something that really never worked out. It was sort of like other things that get started in malaria, such as the vaccines. The effort to try and re-try and try and re-try a vaccine that has proven to be ineffective, one gets committed to it. These people from Gainesville -- they were very bright and clever people -- were absolutely committed to this idea that it must work. If you do it right it'll have to work, but it didn't.

LS: Did they know where breeding took place and so forth?

MW: Oh yes. We knew exactly where *Anopheles albimanus* were breeding, but they would raise huge quantities in a laboratory. They set up a laboratory in a suburb of El Salvador called Santa Tecla, and they reared tens of thousands of mosquitoes. One could separate the pupae by sex. This part of the program was very effective. They would wait until the mosquitoes emerged, irradiate them, and release them. However, going back and finding naturally caught females who had mated with these sterile males was very difficult. They didn't find very many, no matter how many males were released. Something was wrong. They weren't competing with the local/wild male population. It just didn't work. But it was an interesting idea.

LS: Speaking of finding mosquitoes, I was looking at a paper of yours from Malaya: using bronzing powder and gold powder to mark the mosquitoes. You recovered 2% of these mosquitoes hundreds of yards away in the jungle.[10]

MW: When Mary and I went back to Malaya -- when I worked for WHO -- we used the same idea. We had a problem with toads, great toads that would come into the living room in our house. I kept saying, "Mary it's the same damn toad coming back coming back in, the very same one." She said, "Oh I can't believe that." So I took it and I marked him and threw him out in the backyard, and I was right, every night he'd come back and I'd put a little mark on him. We had some of them with five marks, and he'd come back five nights in a row. But this is an old technique, bronzing of mosquitoes, to see what you can collect and how far away. This is one of the primary technologies that were first involved in getting flight ranges for *Anopheles*, because they vary strikingly for each species. It was exciting. You'd bring in the mosquitoes and you'd get them under the

[10] R. H. Wharton, Don E. Eyles, and McWilson Warren, "The Development of Methods for Trapping the Vectors of Monkey Malaria," *Annals of Tropical Medicine and Parasitology*, 57, 1963, 32-46.

dissecting scope and look to see if you had got the marked ones -- that was very exciting.

[break in audio]

LS: You were saying something more about El Salvador?

MW: The operational research that we were doing in El Salvador. I didn't mention this, but I think it probably is going to be inherently part of something important about working in Central America, that is that the political instability was always a hazard, always a problem. Mary and I were evacuated with our not quite two-year-old son. We were evacuated -- when I was down the second time -- because the war had just become so impossible that they couldn't leave us there. Political instability was always a problem, when you were crossing the border with Honduras and El Salvador there were always potential conflicts. This was in the time when -- the mayhem that these people seemed to be prepared to impose on themselves was so manifest. It was just the horror of civil war in El Salvador and the civil war in Guatemala, the brutality, the inhumanity that they put on each other. Both sides were involved in this. When we were working in El Salvador it was the time of the so-called Catorse, 14 families that controlled about 98% of the wealth in the country.

Charming, delightful, beautifully educated, sophisticated, wonderful, pleasant people to be around, the Catorse. But they looked upon the peones as just clever animals. It was very difficult as an American to try and see how you were going to deal with this, because many of our staff at the laboratory, the secretaries and so forth, were all from these wealthy families who were educated in the United States, who were wonderful. They were just waiting until they got married. That was what you had, very attractive young ladies. Socially in San Salvador, the people we were in contact with tended to be more the Girolas, the Martinez, and so on. You lived in this peculiar world which you fundamentally knew was wrong. No question in your mind that this was wrong, but you had no way of doing anything about it. It was not something that you could see any way of changing. In the city, if we went out socially, I was exposed to these sophisticated, erudite, very charming people -- absolutely charming, delightful people to be around. If you were to be in this cadre of people in the District you would find them charming, people you would like to see again. But then when you went out into the field you saw the results of their social attitudes, which were just absolutely appalling. This was difficult.

It was difficult for all the Americans working down there because you just didn't know how to merge the two -- on one side you just couldn't believe that people were treated that badly and on the other side they were socially charming and delightful. Many of the people working in our unit were members of these families. That was a difficult aspect of working in Central America. I thought it was difficult. It was difficult in Brazil too, in that the always brutally patronizing attitude of the San Paulistas, who controlled most of the land in the Mato Grosso, was pretty severe. It was evident every mile you drove how much these people were so put down. They'd go out from the coastal cities thinking, "Oh we'll get some land," and there was nothing for them out there except malaria and death, and it was difficult. As one gets older, you begin to be less comfortable with some of these dichotomies in a society. The interesting thing is that I did a lot of back looking, if you will. I was in El Salvador thinking, "Did I see this when I was in Malaya? Did I see this?" And I didn't. That kind of thing may have been there, but it simply was not manifested so that we had to see it all the time. We just didn't see it.

There were times when you were very uncomfortable because you'd hear people say things that you just simply could not agree with. But it was going to be of no benefit for you to challenge some lady who made some terribly compromising remark about peons in her finca or to stalk out and say, "I'm not going deal with this." Nothing is accomplished by that. But there were times in Central America that -- as pleasant as it was and as much as we enjoyed it, the research, the laboratory and living there -- there were times when you were very uncomfortable knowing that's the kind of society you were dealing in.

LS: In the late 1970s and early 1980s you worked with USAID in Haiti and Pakistan. Tell me about that.

MW: Haiti was something. I had a couple of projects in Haiti. Oh my goodness, where do you begin to talk about Haiti? I remember poverty, the absolute misery in which most of these people lived. It was just extraordinary. I had spent many years in various and sundry difficult parts of the world. I'd seen Laos and Calcutta and so forth. And I'm not sure that Haiti wasn't the worst that I saw. I'm not sure it was the most difficult. Lovely people, wonderful to work with and be around, but it was a benighted place. I remember so vividly a French restaurant up on the hill above Port au Prince called La Lantern. The hostess who owned the restaurant was absolutely beautiful. She was mixed European/African, one of the most beautiful women I'd ever seen. This restaurant was very spiffy and sophisticated. The menu was French from the word go and the food was fabulous, the wine list was incredible, and yet you drive down into Port au Prince which is three centuries behind this. I'll never forget that contrast between the people I was working with that had malaria and this place called La Lantern which was just like another world. They simply didn't relate.

We were doing studies on chloroquine resistance, primarily because there wasn't any and that was why we were so intrigued by it. Because there'd been enough chloroquine distributed in Haiti to cover the whole damned island two inches deep in white powder, and yet the *P. falciparum* malaria continued to be susceptible to chloroquine. We never could understand why. We did urine analysis to determine drug levels, looking at anything that would help us understand why so much chloroquine was used in Haiti, yet the parasites remained susceptible. It was a very interesting place to work.

Pakistan? God, what an extraordinary experience that was. The Japanese Sumitomo chemical company had been merchandising their chemicals very hard, fenetrothion, malathion, and so forth. They'd get countries committed to it because they'd provide it free. Then once the country became committed to its use, they would have to pay for it.

They started out with a program in Pakistan for spraying houses with malathion. They had been using the same techniques and so forth as they'd used with DDT. The malathion smells. I don't know if you're familiar with it, but it has a sulfurous, fetid smell. It was not liked that much by the Pakistanis, so that they were having trouble getting the Pakistanis to open up their houses to spraying. They'd move the furniture out and then spray. When some overly-zealous sprayman was trying to convince the people in one house that it was all right, the stuff was not bad, it would do them good. So he sprayed part of a cut melon with the pesticide and then ate the melon and died. This created quite a stir. Then we began to look into the situation and found that the number of people suffering from intoxication from malathion was very, very high. They appealed to CDC saying, "What can we do about this?" So Ed Baker must have gone to Pakistan to look at the situation, and I came a week later and organized a series of training programs to teach workers how to store malathion, where to put it, how to keep it, and how to use it, and that there were certain absolutely unforgivable things that you could do with this peticide that would only result in you being ill or dying. Unless we could keep the morbidity rate of illness down they weren't going to be able to use the pesticide.

That's what we were doing, teaching. I was moving from one part of the country to the other talking with big crowds of people. I don't speak any Urdu and I remember one extraordinary situation in Gilget in Baluchistan, the back end of nowhere. I was just a few miles from the Afghan border in Baluchistan and high up in the mountains. It was very cold. I was not too enthusiastic about this in first place, and I got there and it was a rinky-dink little hotel that was cold. I had dinner that night and it was wonderful. It was roast lamb and naan and it was just wonderful and I thought, "Well God, I can live with this." Well, the next morning you had cold lamb and naan, and lunch you had cold lamb and naan, and so on. So that was not very good.

I had a Pakistani who spoke Urdu and English from USAID in Islamabad. I spoke English when I would teach. There was a group of spraymen from the malaria program. I was using a curriculum designed to teach techniques for storage and use of malathion. I would speak in English, the guy who spoke Urdu would then speak to the guy who spoke Baluchi, and then the Baluchi guy would speak to the spraymen. The lesson went through three languages before the students got exposed to the information. I had very little hope that anything would come out of that. That was really, really quite an experience.

LS: It sounds like an introduction to a bad a joke.

MW: No, the whole thing was a bad joke. But I did run into a guy by the name of Afridi, one of the great tribes in Northern Pakistan in the Northwest Territory. I was staying in Peshawar, and one day he took me up -- the tribal Afridis controlled the area and I mean quite literally that -- right up to the Khyber Pass. It was extraordinary. I was terrified, but then nobody shot an Afridi. He was a remarkable man, a very erudite physician, trained in the United States, and I think genuinely interested in trying to better the situation in Pakistan. But when you get up to the Northwest Territories in Pakistan, it's under control of the tribal chiefs and the same thing is true in Afghanistan. This is the reason there's so many problems with Afghanistan now. Hamed Karzai may be in Kabul, but the people in the rest of the place could care less. The trip to Pakistan was interesting -- always a learning experience. I don't believe that I accomplished very much. I don't think this project was crowned with success. We had no more reports of mortalities. And the morbidity -- if the situation was serious, it was not serious enough to interfere with the spray program. The management of their malaria program was so dreadfully poor that there were a lot of other problems that were capable of doing it in. I do hope that we kept people from dying, maybe that helped.

LS: You mentioned briefly that you went back to Malaysia with the WHO in the 1980s. That was a full-time posting?

MW: Yes. I was on secondment to the World Health Organization. It was an inter-regional proigram -- and I was the director of the Inter-regional Secretariat for Malaria Training. We were there to design programs for training malaria control workers. I was in Kuala Lumpur which is part of the Western Pacific region -- Manila being the home office of the region -- but we also had to work in the south Asia or Delhi region.

That was a very interesting program. We worked on the training of trainers. We had no illusions that we were going to come through from a little secretariat and directly teach other people working malaria control. We worked on teaching people to teach. We'd hold seminars, three-day programs in Thailand, Burma, Pakistan, Indonesia, and so forth. We held these three-day programs to which we bought the materials. We had designed teaching materials: how to do microscopy, the way to do analysis of data, this sort of thing. The secretariat designed and distributed materials to teachers so that they could go back and teach their own people. The biggest part of this was to recognize your problem, because it's not the same throughout the region. The problems in Timor are quite different from the problems in Java even though they're both in Indonesia. They're not the same thing. If you were going to be training people, you had to train people to deal with the problem locally. You had to be able to organize your curricula so that you helped them to solve their own problem. What we were trying to do most of all, the biggest push, was to involve the villages themselves in their own issues. We spent a great deal of time on designing teaching programs that would help them in Indonesia or Burma or Pakistan or wherever it was to go into a village and begin to organize the support of the villagers themselves so that they became part of the program. They had to be an essential part of the control effort. We had come to realize by this time that every village was different, every country not only is different, but every village within that country could be different. You couldn't make broad statements. "We'll go do this in this country." That's nonsense. You're just going to get nowhere with that. You've got to organize your programs so that you involve these people in solving their own problems and then they become an integral part of the solution.

That's what we were doing, it was a training secretariat that was working out of Kuala Lumpur, and in this I worked from Kabul to Beijing to Manila to Guadalcanal to Bougainville to New Guinea. And all through Indonesia and Indochina. This was when my wife decided that enough was enough. The first year in Kuala Lumpur -- Mary was living there with our son -- I was away from Kuala Lumpur for almost seven months. Then I cut it back and didn't travel quite so much, but then in the fourth year I came back from one of these long trips where'd I'd been a stop here, a stop there: to conserve funds. I came back and Mary was with the driver to meet me at the airport in Kuala Lumpur. She said, "Maybe the next time you come in you can wear a carnation in your button hole so your son can recognize who you are." And that's when I said, "Okay, I hear you. That's enough."

We were caught up, I think, in the conflict between two regional offices. The regional office in Manila was headed by Hiroshi Nakajima who eventually became director of WHO. I thought he was an absolutely bloody fool. The WHO people in Manila would not talk with their counterparts in New Delhi. The people in Manila were always threatening the Secretariat because it was in their area. It was a threat to funding, because the support came to the program from both regions. I reported to Geneva primarily, my boss was Nahara in Geneva. The conflict between the regional offices, the political conflicts, were pretty severe. The Manila office said they were going to close the Secretariat down. Then, Nakajima came to me, and said, "If you stay we'll make it a Western Pacific program and we'll fund it, but if you go I'm going to close it down." By that time I was tired of the bureaucracy of WHO and said, "That's it."

There is one important facet of the time spent in Kuala Lumpur with the WHO. My office was back in the IMR where I had spent so much wonderful time two decades earlier. Many of my old colleagues were still working there. We had established the Malaysian Society of Parasitology and Tropical Medicine during 1960's. I became active in this society again when I returned to Kuala Lumpur. I was elected President of the society in 1984. This recognition by a group of highly valued peers was a real highlight in my career.

LS: Over the years you've consulted with the US Navy, USAID, and WHO. Can you say something about those experiences and how the organizations differed in philosophy or approach? Is there some comparison to be made or is it too spread out in time and space?

MW: USAID. Working with USAID was always difficult at best because I really never had a lot of respect for their programs. In many places, they had the only programs so you had to work with them and that was it. But you really were not very sanguine about whether or not the program was going to be worth anything or not. They had funds. That was the biggest issue associated with AID -- they could support you. I remember one extraordinary experience in Pakistan, on an earlier trip to Pakistan, with the AID representative in the country. We came into a village and there were balloons everywhere, colored balloons, and the children were playing with them. It turned out they were condoms. I thought, "Whoa, I don't think they're doing very much to control sexually transmitted diseases or to lower the birth rate at all." The next day I had to attend a meeting of the USAID program directors at the US Embassy. I was there as a guest. Their method of evaluating how effective their family planning program had been was how many condoms had been distributed. I listened to this and finally had to get up and say, "I think this is an absolute bunch of nonsense because they're not being used for what you think they're being used for." But the evaluators were sitting at a desk and counting and that was it.

I had some different experiences with AID, some good; a lot less than good. I'll be honest, I never had a lot of respect for either the people who worked in the programs or their qualifications or what they were supporting. At times you did encounter people in deep areas in Africa and so forth who were very strongly dedicated, hard working people. But for the most part, I did not find AID to be a particularly productive way of spending our money to do anything about disease control. Not very positive.

LS: When you say WHO was terribly bureaucratic, were they more so than other organizations?

MW: They gave lessons on how to be bureaucratic.

LS: You worked in a number of government agencies and so forth, so you've seen bureaucracy.

MW: The US government were absolute amateurs when it came to bureaucracy. In my opinion, what happens with this bureaucracy in WHO is that you are not allowed to make a mistake. There's no room for error. Therefore, you don't take any chances. You come from whatever country. You're well trained, and you may have started off with a very benevolent philosophy about making a difference. But so many of the workers that I encountered in WHO were only interested in getting a certain number of years in -- saving as much money as possible by staying in absolute awful places when they traveled so that they'd get the money, but not spend it, so that they could accumulate as large a fund as possible for their retirement. Everything -- everything -- was geared toward retiring, toward going back to wherever you came from, toward retiring with a level of funding that would allow them to live comfortably for the rest of their lives. Many spent 20 years at the WHO in these various and sundry places. Now, take that bit of philosophy and then try to visualize it for a person who's out making a judgment about whether we should go this way or that way in terms of how to carry out a program for malaria control, or whatever else it happened to be. They were not prepared to make any decision that had the potential of going bad, so they stayed strictly down the middle of the road. They were not empowered to make a mistake, and I think that was a deadly philosophy from the beginning. It was cover your ass. I'm sorry. Be sure you don't get yourself into a mess that would make you not be able to stay in that program. You don't dare do anything that would keep you from continuing in that program. And if you do, you go back to Sri Lanka or Venezuela or wherever it happens to be, and you're nobody.

[break in audio]

LS: When you left WHO, you went back to CDC?

MW: That's right, as a Director of Scientific Resources at NCID, the National Center for Infectious Diseases.

LS: And you were there quite a while? What was that all about?

MW: It was fine. I got several decorations and all that sort of business. It was effective, it provided the resources, it got there. I kept people from killing each other in the animal rooms and that sort of business. Basically I did my job, and I got a Distinguished Service Medal for it. But it wasn't very rewarding to me as a scientist. It was appropriate and helped the organization of CID to have a scientist in this position, if you follow what I'm saying, but it was not particularly rewarding to the scientist who was there. Being able to have Dr. Warren be the director was important because that gave credibility; it gave substance to the whole idea. But it wasn't very rewarding to you as a scientist.

So when it came time -- Jim Hughes managed to get me two extensions, so that was then 32 years. One's career in the public health service is supposed to be a 30-year thing and then you're approved to get the hell out of the way because there are all these 05 officers waiting because there are only so many 06 slots and you're occupying one of these. Unless you get out of the way, those 05s are not going to be able to be promoted. He got me two extensions, but then Washington said, "No more, that's it, don't even apply."

LS: How big a group did you have there?

MW: I'm trying to remember. The whole Lawrenceville facility was under my control; there must have been eight people out there. I would suppose somewhere between 250-300 people, but I really don't know to be honest because it was a big program. All of the glassware, the sterilization of glassware, the animals, the provision of vendor services, pathology services, everything for CID, all came under the direction of scientific resources. It was a big and involved program.

It was an important program. I never have denigrated how important it was to NCID, I just didn't enjoy it. That's why I said earlier that when it came time for me to retire I did not feel uncomfortable retiring, I was pretty glad to get out. By this time I was involved with being editor of the *American Journal of Tropical Medicine*, and I loved this. I really thought that was a great task.

LS: You'd been involved with the professional organization for a long time?

MW: As the editor you're it. That's where the buck stops. You and a series of scientific reviewers help to decide what's going to go in the journal every month. It's a wonderful task, a wonderful job because you get to know everybody in a society. I really loved being editor of the *American Journal*, and really only got out of it eventually because Mary and I were finding ourselves looking for more and more time we could to spend on things together, and I have a habit of either total commitment or not being involved. I had a good staff in Atlanta and I could work on the computer up here at the farm when we'd come up for a few weeks in the summer -- when Jamie, our son, was at the university. Even after I retired I went to the journal office at 7:00 in the morning and didn't come home until late -- and that happened six-and-a-half days a week. Mary and I began to think, let's begin to look about, do we really need this?

It was during this same time that I became the executive director for the Council of State and Territorial Epidemiologists. They had never had an office before. The state epidemiologist, every state has got one, and you know what their basic responsibilities are. I organized and established an Office of the Director of CSTE, but maybe I was tired, too old, I don't know, but I began to have conflicts because there were certain very rigid rules that you had to use about how the funds were expended. The grants that came from CDC to the Council were very specifically designated as to how they could be expended, what you could do with them and this sort of business. Those funds were not to be used to support CDC programs that they couldn't get supported otherwise. I found myself in the middle of programs at various parts of CDC where there would be funding coming from the outside that would be given to the CSTE, but I knew they came from CDC, that were going back to CDC, but you had bypassed their regulations on how they were supposed to be expended. There were programs that I was supposed to be supporting as a part of the Council's program that I had no control over whatsoever: They reported to somebody else and so forth. It was obvious that the funds were being spent illegally, and I got hit for this several times by the administrators at CDC: "You can't do this." So we decided to part ways. CSTE and I decided that I was not who they were looking for. That lasted about two years, and I was out of that.

But in the meantime I was still editing the journal. There's an institutional memory that is absolutely essential if you're going run the American Society for Tropical Medicine. Secretary Treasurer is somebody who may stay in the office for many years. That's an institutional memory there, that's very important. The Council comes on for three years, each Council member is elected for three years. The President is in for a year and then he or she is past President and they sit in the Council meetings, but there's no continuity. The only continuity is with the editor of the journal, the editor of the news, (*Tropical Medicine & Hygiene News*), and the Secretary Treasurer. It was very gratifying to find after four or five years that Peter Weller, who was the Secretary of the Treasury at that time and myself were the institutional memory for the American Society for Tropical Medicine. We knew more about what was happening, what had to happen, how funds could be expended, that sort of thing, because we worked with it all the time.

Editing the journal: I had an extraordinary experience at Dartmouth the first time I went to the dermatologist over at Dartmouth-Hitchcock Medical Center. I have all of these actinokeratotic lesions that I have to be looked at. We were very concerned about developing cancers and so forth. Every six months or so, I went to the dermatologist. The first time I went to this particular dermatologist he came walking in, a little man about 5'6", 5'7", you know white coat, gray hair, and he held up this thing and he said, "There can only be one McWilson Warren in the whole world." I looked up and I said, "You're probably right, but why do you say that?" Well, he was Sydney Klaus. I'd never met Sydney but he was at the Hebrew National University in Jerusalem for a while working on dermal leishmaniasis. We had published at least one of his papers in the *American Journal of Tropical Medicine*, and when he saw this McWilson Warren business he said, "There can't be two of these in the world."

It was wonderful because I loved working with the younger people in the society. It was very gratifying to go to the meetings. To the typical young investigator who is a member of American Society of Tropical Medicine the editor of the journal is the most important person there. Nobody else has the same level of importance to this young investigator as does the editor of the journal because ones developing career is dependent upon getting research results published. You got to meet all of these people. You got to spend time with them. I would go around looking at the poster sessions -- and by the way, I was the first one to ever establish poster sessions for the American Society of Tropical Medicine at their annual meeting. We had so many papers we couldn't possibly represent them all orally. You'd go around and you'd look at the paper and then the young fellow or young lady would introduce themselves -- because your name tag says who you are -- and then you would talk about when the material would be published: "I'd like to see this when you get it put together. I'd love to have the chance to look at it as a possible article for the journal." This was the greatest pleasure I got at the annual meetings, was the chance to meet the young investigators at their level and talk about getting their stuff out there so people could read it and see it.

I loved working with the journal, I really did. That was just joy. But, as I say, Mary and I had come to the conclusion that maybe it was time to do something else. Maybe it's time to see what Mary and Mac want out of this world. That's when we made the decision to retire from the journal and come up here, move to Grafton [NH], and renovate this house.

LS: When you sent me your CV you wrote a little note, and you commented that once you retire this kind of information doesn't seem very important, but I also noticed that you have your name on a publication from 2003,[11] and I guess that you're still staying in touch with people in the field?

MW: To some extent. As I say, I was working with Andy Spielman and Jeff Sachs and others down at Harvard. I wrote a section for this book on urban malaria in Africa -- strange bit of business. The urban transmission of malaria in Africa is a horror because they're moving into these cities and the vast slums are breeding *Anopheles gambiae* and malaria transmission is very high in these heavily populated areas. It is just overwhelming, and there's no infrastructure. The city's infrastructure is four miles/five miles that way somewhere, and there's no infrastructure out here to do anything, and they're just caught. Urban malaria in Lagos, Kinshasa, these places, is just horrible, just a real bear. But, as I had said earlier, I go at things wholeheartedly. When I take on a task, it's pretty well decided that is what I am doing. And when I took on the task of retiring, I decided I'd do it.

I stay in contact with Bill and Geoff and Andy Spielman and Peter Weller, but I like this here with Mary. This is where it is. You might be interested to know that the idea about the Friday and Saturday sessions that I told you about has carried over. Bob Ells is a very close friend of mine up the ridge here. We both had gained a little weight, more than we should, and there was a tendency in both families for us to sit down and have a drink before dinner, a glass of wine before dinner and so forth, not drinking to excess or being alcoholics, but a lot of calories. So we decided, between Bob and myself, all right, we'll cut it out, and we'll break the fast on Friday.

[11] Robert Vincent, Kate Macintyre, Joseph Keating, John-Francois Trape, Jean-Bernard Duchemin, McWilson Warren, and John C. Beier, "Malaria Transmission in Urban Sub-Saharan Africa," *American Journal of Tropical Medicine & Hygiene*, 68, 2003, 169-176.

We'll have a drink on Friday night at one house or another. We rotate around from house to house on Friday nights to have drinks and finger food, and then unless there's some extraordinary meeting, you don't drink in between. That's why we get together and we have a drink once a week on Friday; we break the fast on Friday. Which goes back to the Navy, it goes back even further than that, to the Saturday afternoon sessions at the Institute for Medical Research at Kuala Lumpur. We still do the same thing. It's very interesting.

LS: Is there anything that I should have asked you about but I didn't? That you were expecting to hear or wanted to talk about?

MW: No, I had to be absolutely open and straightforward, so I think in this long narrative that I've given you, we've talked about a great deal. There are things you're not proud of. I don't think I was as good for CSTE or the person they needed; I'm sorry that I wasn't more successful with that effort. That was the one place where a relationship was broken -- we both agreed it was time. They weren't satisfied with my performance and I wasn't satisfied with the job, you know, that sort of thing. Others, the WHO one, I got unhappy with, but I think I accomplished something in the process. I think I did something that was worthwhile as part of the process.

Highlights -- still the most extraordinary experience was Malaya. In terms of stimulus, in terms of challenge, there was nothing in my career like it. The closest was probably the *Journal* because the *Journal* needed help when I became editor. It was not doing well. That was a challenge: to establish an office, to bring in computer systems where such technologies were of limited use, to teach people to be line editors, to coerce people to be peer reviewers who didn't want to peer review. Getting people to peer review papers is not easy because they're busy. They're all scientists who've got other things to do, for heaven's sake. You really do a lot of arm-twisting. A third of my time at the annual meetings was to twist arms of people to become peer reviewers at the Journal, to make them so ashamed that they're not living up to their obligations that they wouldn't have any choice but to do what you needed them to do.

But I'm not sure, to be honest with you, that now is not the best time of all. You didn't expect to hear that, did you?

I just think that you have to go at it and you have to figure out what you're going to do. I happen to be married to my best friend, and that's a pretty big part of the issue. And we have good friends up here, lots of them. Mary is now an elected officer of the town. She supervises the voter checklist. I'm an official poll counter every election night. I remember last February we had elections and it had snowed all day, and by the time we got the votes all counted at 10:30 that night it was an absolute blizzard. We could not see the end of the hood in front of the truck on our way home. They had plowed, but it was just coming down so fast. The snow in our driveway was almost 16 inches deep when we came home. But it was something Mary and I do together. Being part of this town is part of what we are doing.

I love being retired. I don't read that much anymore of science unless it's something that [Robert S.] Desowitz writes or Laurie Garrett would write, I really didn't need all that business. But right now I'm reading [Stephen J.] Gould's *Rocks of Ages*. This one is fun. I love a murder mystery, a crossword puzzle. I walk -- this morning I didn't walk because you were coming, -- but normally I walk four-and-a-half miles every morning.

I think that the most gratifying thing about my time, my career if you will, is that I always seemed to feel as though I was doing something that had the possibility of being better than myself, that I was making some sort of contribution, and that was important.

LS: How do you feel about malariology today?

MW: Probably I'm not really qualified to talk about it today because it's become so specialized. I guess maybe my respect for the biological survival potential of the malaria parasite and anopheline mosquito is such that I am not terrible sanguine about malaria not continuing to be a problem for the foreseeable future. I think it will be. I watch and listen and read the papers, I read the journals as much as I can, keep up with things, what's going on. And it doesn't seem to me that that there's any vaccine that's going to make an appreciable difference right around the corner. Artemisinin may be the drug that has tremendous potential, but we still have management and distribution problems that are going to mean that the drug isn't worth a damn unless you've got some way of getting it to the people that need it at the time they need it.

At the risk of sounding like somebody who's overly bitter, and I'm not bitter -- really, I don't think you've seen me bitter -- I don't think the potential is there to go back and look and say, "We've got to find out what makes malaria tick." I don't think we've found out yet. And I don't believe that we're training people and promulgating a philosophy that will generate people who will do this. I can't visualize anybody, let's say, in their 20's or 30's now, going to Malaya, or to whatever Malaya happens to exist in that particular time -- to Kenya or Uganda or Burma. I don't think we're training people that way anymore, that's the first problem. And I don't think people are excited about biology anymore. You find people up here a lot --that Warner Shedd who writes this remarkable book *Owls Aren't Wise* & *Bats Aren't Blind* [2000] is an example, that potential to observe, that potential to assess and look at something that's real and alive, they don't do that anymore. These people are not very common anymore, and when you find them, they're writing books about shrews and that sort of thing; Shedd's a naturalist. In reality, Don Eyles and myself and Harry Wharton were naturalists in a real sense of the word. I just wasn't writing about owls and possums, I was writing about malaria parasites in monkeys. I don't think we've got naturalists coming into this business any longer.

Andy Spielman is -- well, you've read his book on mosquitoes.[12] There's a biological perception there that you have to admire. Andy is great fun; he's a great person to be around. Andy has a serious, severe speech impediment; he stutters badly in certain circumstances, but he can sit here -- he and Judy and Mary and myself -- can sit here in an evening, after dinner, and have scotch and talk and you never hear a stutter. But he has profound things to say about a lot of things. He's a very interesting guy.

LS: Any people I missed?

MW: Let me think for a minute. Yes, a technician in El Salvador, Ana Julia Larin. She turned up one day when we were recruiting in El Salvador. I needed a technician, and here is this extraordinarily good-looking girl who comes in and she has got charm and laughter and intelligence -- a really extraordinary girl. She had studied at Marquette. She was back home because her mother and father were getting old, she came back to El Salvador, but there was nothing for her to do in El Salvador, or maybe very little. So she came and she was my senior technician for years and I still see her from time to time. She lives in Milwaukee now; her parents are dead, so she's back living in Milwaukee. Yes, bright, clever, fun, a great dancer. Mary and I are very fond her; she's a very important part of our lives.

I'm trying to think. We've talked about a lot of people. I'm going from country to country now. Mujibullah: keep him in mind. He's one of these extraordinary characters, Christ, you don't see them very often, you know? But he was such a character.

LS: Did you see him again after that episode or just correspond with him?

MW: No, we kept in contact for 20 years but I never got back to Dhaka.

[12] Andrew Spielman and Michael D'Antonio, *Mosquito: A Natural History of Our Most Persistent and Deadly Foe* (New York: Hyperion, 2001).

[break in audio]

LS: Can add any other recollections of your travels?

MW: As I say Bill Collins didn't go overseas very much; he did not go out of the country very much. He and I went out a couple of times. Geoff and I traveled outside the country some, a few times. But of the three of us, I spent more time outside of the country than any of the rest of them.

In Ethiopia, Bill and I were doing blood surveys on a group of Coptic monks on an island in Lake Tana. And all of a sudden -- it really wasn't funny, yet I still laugh when I think about it, there was this guy -- they would come and stick their hands out so their fingers could be stuck -- he was a leper and he had no fingers, and he just stuck out this stump and he was pointing it at Bill Collins. Bill said, "Mac, what do I do now?" The guy from the Navy unit on the other side said, "Give him two aspirin, you'll see him in the morning." [laughs] Which sounds crude, but it did break some of the horror. But these monks took us to their monastery and they had illustrated manuscripts that were hundreds of years old that were absolutely glorious, they were just so beautiful. No museum in the world would turn them down. Wonderful opportunity. The floor of the monastery was covered with straw, and it was cold. I had on corduroy trousers and white socks and all of a sudden I felt something beating around my feet. One of the monks was beating around my legs and my socks were no longer white, they were brown. Fleas. Human fleas by the tens of thousands, I've never seen anything like it. Having worked at a biological supply company where human fleas are very hard to come by, for teaching material, I had thousands of dollars worth of teaching material that I couldn't do anything with.

LS: When were you in Ethiopia?

MW: This was in 1967.

LS: Any final thoughts?

MW: The Navy: it may be again the quality of the people you deal with. Working with the Navy unit out of Cairo. I was working with Harry Hoogstraal. Harry was arguably one of the truly great characters in the field of infectious diseases and research for the last century. Harry was a good friend. So traveling with him and with the Navy people was a rewarding experience. They knew what they were asking when they brought you in, they had something that they wanted you to do, they had a goal in mind, and they were willing to make the resources available so that you could do what it was they wanted you to do. I found the Navy to be wonderful. I really enjoyed my time with the Navy.

I was also on the armed forces epidemiology board for a while with the Army at Walter Reed. That was a very good experience -- an exciting place to be.

I was also at the Institute of Medicine for a while working on a malaria book we put out.[13] Do you see the mask up there on the wall above the bar? That's a bomo (Malayan Aboriginee medicine man) mask. The big one in the middle, that came from Malaya. It's the mask a bomo wears, and I never could find out whether it was to cure malaria or to prevent malaria. I think it was to cure. I think you already were sick when he saw you. This mask was part of his paraphernalia. That mask is on the cover of the report from the Institute of Medicine. This was an interesting experience.

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[13] Stanley C. Oaks, Violaine S. Mitchell, Greg W. Pearson, and Charles C. J. Carpenter, eds., *Malaria: Obstacles and Opportunities* (National Academy Press, Washington, D.C.; 1991).

We went off to an island in the Chesapeake Bay. It was a weekend meeting for this group that was doing the study for the Institute of Medicine. The McCormick family, you know the McCormicks of spices, they own an island in Chesapeake Bay, and we spent the weekend out there. It was just the most fabulous weekend, it was wonderful. Working with the Institute of Medicine for a special project was very rewarding, very rewarding. There were such extraordinarily good people; they were so special.[14] It was a short, special project, but very rewarding, very gratifying type of activity.

It's very interesting to me. You come back now, when you're 76 years of age you're so glad you've done this. Because if in the back of your mind there was some perception that you wanted to do some of these things, now you're now retired and you're too old to take on that kind of activity. So it's very gratifying to think, "Well, I've done that. I did go." Another thing I didn't do, I suppose, that I would have liked to was to go to New Zealand. I've never been to New Zealand. I did Australia a few times. Of course they had terrible malaria up in the northern part of Queensland.

LS: New Zealand doesn't have malaria, do they?

MW: No. I just wanted to go to New Zealand. I did get to Suva in Fiji and to Tahiti. One trip back from Kuala Lumpur I came back by Suva and Tahiti.

LS: Thank you.

[end of transcript]

[14] Dr. Warren was a member of the Malaria Assessment Committee, National Academy of Sciences (Institute of Medicine), 1990-1992.