

**ARMED FORCES INSTITUTE OF PATHOLOGY
ORAL HISTORY PROGRAM**

SUBJECT: Mr. Ronald Carl Neafie
INTERVIEWER: Charles Stuart Kennedy
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Q: Mr. Neafie, could you give me a bit about your background, when and where you were born and something about where you grew up and your early education.

MR. NEAFIE: Okay. I was born in Youngstown, Ohio, December 15, 1933, and grew up in that area of northeast Ohio. My Dad operated sometimes two, but primarily one, High's-type store that we had there--small grocery, ice cream, that kind of thing. We moved from Youngstown, when I was about five or six, to a little town 30 miles away called Sebring, Ohio, which is where I essentially spent probably about the next 20 years. I went to elementary school there, and graduated from high school in Sebring, Ohio, a small town of about 4,000 people. Primarily pottery was the main industry there. I have three sisters (one is a step-sister) and one brother. So I grew up in Sebring, Ohio, and was not any kind of a student at all; in fact, kind of the black sheep of the family, you might say. I felt very fortunate that I ever got out of high school, and the concept of going to college or furthering my education was just not even in my brain. But I graduated from high school in 1951, and I worked in a box factory for the next three years, making the cardboard boxes that they packed this pottery in. I enjoyed that, but I guess I felt I didn't want to spend my life doing that, so I decided to go into the military.

So I went into the Army, and I was in the Army about a year and a half. And it was discovered while I was in the Army that I had a broken wrist, which, when I thought about it, probably occurred back when I was in high school. I did a lot of wrestling in high school. As I remember, I thought it was probably a sprain, but in fact it was a break. But at any rate, I got out of the Army in about a year and a half.

So when I got out of the Army, I knew I had a bad wrist that had been like that for some time, and I did decide to have it operated on. The operation was pretty successful, but not completely.

But at any rate, at that point in time, I felt I probably would be smart maybe to get paid for what I knew, since I did have a physical handicap there, so I decided to go to college. I'd been out of high school, of course, quite a while, and I never learned anything in high school, but I did apply and got accepted at Mount Union College in Alliance, Ohio. That's about seven miles from Sebring. The first six months were just very hard; I could have quit many times. I didn't like to study and all that. But I did like biology--one subject I did like. I pursued it, and I guess, after about the first six months, it started to sink in that I was going to probably make it and do all right. And since I had worked in a shop and been in the Army, I was a little older now. This was 1956, and I was 23, and I was really maybe ready to go to school and to learn. When I went to Mount Union, I don't think I ever cut a class, and I went through four years of college in three. I went the

two summers, so it was very intensive. Well, I graduated from Mount Union with better than a B average, which I felt was very good for me.

Q: Oh, absolutely.

MR. NEAFIE: In high school, I had no chemistry, no physics, no algebra; really very poorly prepared to ever go to college. So I felt I did very well.

When I got out of Mount Union College, I thought it probably would be a good idea to go on and get an advanced degree. I got accepted at the University of Maryland on a stipend, so I did some teaching of pre-med students in zoology and anatomy and physiology. I taught the labs as I went through and took my own course work. So, in 1961, I got a Master's degree from the University of Maryland in zoology.

Q: Excuse me, to backup a little, why did biology particularly strike you? Do you know? What was there when you were in college? Here you were, sort of unprepared for anything, then all of a sudden biology came up.

MR. NEAFIE: Well, maybe it was because I disliked so much of the other; I didn't really like literature and that kind of thing. And I guess I'd always liked and had animals from when I was a kid. And it might have been some about the teacher; as I remember, she was very enthusiastic. So I think there were several factors. But it wasn't that I was just dying to know more about animals. When I went to college, I had to choose something, and I felt that maybe that was the lesser of all the evils. But I really got to where I liked it and decided that that was the thing to do. So that was kind of the background on that.

But coming back now to what I was saying, I did get a Master's degree from Maryland in 1961. I decided not to get a Ph.D. at that point in time. I won't use the word burnout, but I'd just gone through five intensive years of education, with just a horrible background. Those five years were very stressful for me, because I had to really study hard and apply. But I ended up doing better than I ever thought I would, and in my last year as a graduate student, I got all A's. So I really felt I'd turned it around, but at the same time, I just didn't feel at that point that I wanted to really go on and do three or four more years for a Ph.D.

So when I got a degree at that time, believe it or not, I thought I might go into teaching. I took education courses in undergraduate school, because I felt I could always fall back on that if I had to. But when I got out with a Master's degree, I liked the teaching part and I always liked kids, so I taught locally here at a high school out in Beltsville, Maryland, High Point Senior High School. It was a school of about 2,500 students, and they had six periods every day. I got a job there teaching one of the courses, 10th grade biology. I think I had five or six classes of biology a day, and I taught there for two years, from '61 to '63. And I enjoyed the teaching.

In fact, I started the bowling club there (I like to bowl), and I also started the bowling team at High Point High School. And I enjoyed that; I enjoyed the kids.

I always had an interest in genetics, and I had kids doing science-fair projects with *Drosophila melanogaster*, the fruit fly.

Q: Could you explain a little what the science-fair project was?

MR. NEAFIE: Well, the science fair is something they have statewide, and all the schools participate, and they have awards and prizes that they give the kids. These are kids that are exceptional, maybe gifted, but really have an interest in science. You can have any number of kids work under you that you want. Of course, you have to give them some supervision, but at the same time, most of the project is theirs to do as they would. I had maybe a half a dozen kids that would come in after school and help make the media for the flies. We would mate various long-winged fat ones with short-winged skinny ones, and this sort of thing. The students did very well; one of the girls, I think, was second place in the county. It was set up by kind of a county system, and the winners in the county then go on, I believe, to the state. But I had several kids that did very well at that.

I enjoyed the teaching, but at the same time, there were some things about the teaching that I didn't like: being in school after hours, in charge of detention, with 30 kids that are already booked up for the next two years, and you've got to babysit them; going out to make sure that they don't shove each other under the wheels of the bus. Those kind of things I really didn't like.

I also felt, back at that time, that maybe the teachers were losing a little bit of control of the teaching. And I could see that the parents, I thought, were becoming more prominent, and the standards for the kids were becoming much lower. The teachers were, I thought at that time, losing control. And there's been no question in my mind, they have, as I've seen teaching over the years. Compared today to what it was in the very early Sixties, I don't think the teachers really... I feel sorry for them, but we need, obviously, good teachers.

I had done some research at the University of Maryland to get my Master's degree, and I kind of enjoyed that, so I thought, well, maybe I would try my hand at that again. So I kind of went around to various places in the area and just put out some feelers as to anybody that wanted someone to be involved maybe in doing some research.

There was one man, Donald Price, a colonel in the Army, who had graduated, I guess, from the University of Maryland, a little bit older than I, ten years or so maybe, who was working at the AFIP as a parasitologist, and he was looking for someone to be a lab technician and do various kinds of things for him. And at the same time, there was another man at the AFIP, Dr. Donald Winslow, an MD pathologist and chief of the Infectious Diseases Branch, who also wanted to do some research. So the idea was that they would have me work half-time between the two of them. Now the man who hired me was Dr. Binford, who died a couple of years ago, and he was chairman of what was then called the Geographic Pathology Division. But Dr. Winslow actually interviewed me for the job, and as soon as we talked, I was hired almost immediately.

So, in the summer of 1963, I quit teaching, turned in my resignation. I got

married that summer and took on a new job at the Armed Forces Institute of Pathology as a lab technician.

I was going to work half the time with Donald Price, and his interest was in filariasis.

Q: Which is what?

MR. NEAFIE: These are nematodes, and they are a particular group of nematodes: the adult worms produce microfilariae. It's an entity that predominates in tropical countries, primarily in Africa.

And on the other hand, Dr. Winslow had a great interest in Chagas' Disease, also called South American Trypanosomiasis, the disease caused by a trypanosome, the protozoan Trypanosoma cruzi. And he wanted to do some research involving animals.

So I was to spend half the time in filariasis, and half the time with Dr. Winslow in Chagas' Disease. And I did that for about three years.

Q: How did that work out? Having two masters is not always the easiest thing to do.

MR. NEAFIE: It actually worked out pretty well, because for one thing, I was not here at the AFIP in this building. Back at that time, space was very tight. In fact, this was a time when the AFIP still had space at the Old Red Brick building. So the main complex was here, but they had a certain number of people down there. In fact, that's where Donald Price and Winslow were; they were down at the Old Red Brick building. I was actually out at the Beltsville Agricultural Research Center in Beltsville, Maryland, and for three years we just used space out there, and I would come over to the AFIP. We had animals, and I was in charge of the animals, so I would come over here and take supplies out there to feed them and whatever. We had an animal caretaker that took care of them.

Q: Was there a connection with the Department of Agriculture?

MR. NEAFIE: Only in the sense that we had a room that we used. I reared reduviid bugs, Rhodnius prolixus, for Dr. Winslow. The disease is transmitted by bugs from man to man, down in the natural setting in Brazil and some other South American countries. So I was rearing the bugs, and we were doing experiments with them, with raccoons and with mice. And then, with Colonel Price, we were trapping animals for him. He was interested in the filariids that some of the wild animals had in the area, and we were trapping raccoons and groundhogs and rabbits and various other animals. So this actually worked out very well.

It was quite different being out at Beltsville. They didn't have too many buildings out there; they had a lot of space, and it was almost like being in a national park. Parking was no problem, and a great setting. There were other people out there as tenants, you might say, out at Beltsville. Johns Hopkins had some people out there that were working on bats, and they needed some space. Clarence Gibbs was also out there, and he was

working on kuru, on the slow virus, with monkeys. So there were several tenants out there, and this worked out very well for about three years, from '63 till about '66.

And then one day, Donald Winslow said, "Look, how would you like to come down to the Old Red Brick building and work down here full time?" And primarily work with him full time. Now we had experimentally infected a lot of animals, and we had autopsied the animals. And Dr. Winslow, being a pathologist, put all these animals in jars, all of their parts and whatever, in formalin, and he had them all downtown. He looked at all of the cases that came into the Infectious Diseases Branch on the consultation service, so his time was split-up between consultation and his research. And I came down there to help him only really with his research and to look at these animals that we had autopsied over this three-year period.

He showed me what some of these organisms looked like that we were dealing with in these raccoons, and he decided that I had a pretty good eye for finding these organisms. So he said, "Well, now here's a case I think is TB in a human." And he showed me an example of what an acid-fast organism is, *Mycobacterium tuberculosis*, which causes TB, and he said, "This is what an AFB looks like. See if you can find one in this lesion. It looks like TB to me." And sure enough, I found an AFB for him. I didn't always, but I found one. These were pretty hard cases. Usually only the hard cases come to the AFIP. If there are a lot of organisms and they're easy, they usually never even get here.

He was impressed that I could find the AFB, so then he showed me some fungi, "These are what fungi look like, and I think this is," and, well, yeah, I found fungi.

And then he started to show me what spirochetes look like, and I found spirochetes.

So he felt I really had an eye for finding organisms that cause disease in man.

Q: Is there such a thing as having an eye? You've been in the trade for a long time, would you say people either have an eye or don't have an eye?

MR. NEAFIE: That's an interesting question. I'm not sure just how to answer that. I guess, when I look back over the 30 years I've been here (I'm coming up on 30 years, by the way, the 6th of August I will be right here at AFIP 30 years), I've known people that have come into our department and been there for years and still never found things that to me were quite obvious. This happens to us all on occasion; we'll have a blind spot or whatever. But nevertheless, there are still some people that seem like they just never can catch on, and there are other people that can look and it's almost like they've been there all their lives. So I think there is a certain amount of that. I think there's a combination of: Do you have good vision? Do you make good observations? Are you inquisitive? Do you really look and study hard? Are you thinking all the time? So I think there's a lot involved here. In essence, I think there is something to "having a good eye," but I don't know that I'd want to try to define that in words.

Q: No, but I think you've given a very good explanation.

MR. NEAFIE: So he thought I was able to find these things, and before I knew it, he was giving me more and more cases. And pretty soon, I was just seeing more and more of the human material. Of course, I had only had a course in cytology at the University of Maryland, and that was just a very basic course. So now he's given me a lung. And, you know, it's one thing to say this is normal lung; it's something else to say this is lung of TB, where all the lung tissue is virtually destroyed and replaced by necrosis and granulomas and abscesses and so forth. So it took me a while to catch on as to really what the histopathology was, and to learn the organs and the significant features.

I would never in the world ever try to get by as a pathologist, and I've never gone to medical school. And I always make that clear to everybody--I'm not an MD; I'm not a pathologist.

But nevertheless, I guess, over 30 years of studying tissue sections and pathology, and sitting at the table with world-renowned experts, I have learned the pathology of most of the infectious and tropical diseases, just because I've sat there at the table and looked with world experts. You're bound to learn it; you can't avoid it.

So it's a combination of kind of being at the right place at the right time, and, of course, in our department, the material we see, I just can't think that it would be matched by any institution in the world, in the sense that it comes from Africa, the medical missionaries, the armed forces all over the world, Peace Corps workers. It's just unbelievable what we see here.

Q: Could you talk a little about what your department did? What was its mandate?

MR. NEAFIE: Well, of course, the whole Institute is based on the three-fold mission of consultation, education, and research.

The consultation, I guess today we still get about 50,000 cases a year into the Institute. Back in the Sixties, there was no involvement of any fee charged, and any pathologist in the world could send a specimen to the AFIP for a free opinion. Of course, this changed a couple of years ago, and now they charge. But at that time, we were a federal service to really the whole world, and anybody could send us material.

Now that's the consultation, and as I said, these are usually difficult cases, either where organisms are hard to find or they find an organism and they don't know what it is. They're kind of unique.

And, of course, this country really is not tropical-disease oriented. Things like schistosomiasis, malaria, leprosy, they're not endemic in this country. So if you, for example, had come back from Africa, and a physician, say, out in Missoula, Montana, thought you had malaria, it's probably been a long time since that man ever saw a case of malaria. And if he has a blood smear, he might well want to send that to us, because we look at malarial parasites all the time. That's our bread and butter, where a guy out in Dubuque, Iowa, might not have seen a case for 15 years. Odds are, he'll probably never see a case.

Q: So your department was set up to deal with more worldwide-type things, based on geographic areas.

MR. NEAFIE: Yes, the Institute was set up by systems--cardiovascular, hepatic, endocrine, whatever--until Dr. Benford came along, about 1953, and established the Geographic Pathology Division as a separate entity. And this was to handle specimens that came in from abroad, primarily. He was an expert in leprosy and also in fungal diseases, in mycology. He really liked infectious disease, so he set it up really for infectious, parasitic, tropical, but he knew that a lot of that would be coming from abroad, and that's why they called it the Geographic Pathology Division. So when he set that up, then material started to come into the AFIP involving exactly what I said: tropical diseases, or diseases that were infectious in this country, but were still rather rare.

Now the other thing is, pathology, of course, is tumor oriented. The pathologist is trained, and the bottom line is: Here is a biopsy specimen, is it benign or malignant? That is their expertise. If someone sends a case in of a lung that's got a worm in it, that's a whole new ball game. That's really not their area of expertise, and there really aren't many places in the country where the pathologist is an expert in that particular area.

Even the parasitologist is not an expert in that area, because most parasitologists have the whole worm out in front of them. They're interested in a worm passed in someone's feces, and they can measure it, and here's the head and the tail. But if you take that worm and it gets into the lung and it coils up, and a knife goes through there and takes a six-micrometer-thick section, of a cross section, of the middle of the worm, then the parasitologist has a real problem with that--he can't measure the length; he doesn't know where the end is.

So, primarily helminths, for example, are not a strong suit for the pathologist, and they're not a strong suit for the parasitologist. But I had a parasitology background, and I was studying the pathology back in the Sixties, so here was an area where I thought that I could really be of value to the pathologist.

Now Dr. Winslow was a great pathologist, and he knew a lot of organisms in tissue, including bacteria and fungi and other things. And he knew a fair amount about worms, because he was here and had an opportunity to study them.

But when I first came here, I did something a little bit different than anybody, to my knowledge, had ever done, and that is, I collected all the worms that I could, male and female if there were nematodes where the sexes are separate, and I gave them to the lab and had them take the whole worm and just cut the whole thing up. And then I mapped out all the organs and whatever. This had already been done in books, and you could see pictures, but no one really cut them up like this. So I had them all cut up, and then I studied them. So I got to know, here's a male worm, hey, this is Ascaris lumbricoides, this is what the gut looks like; here's what an ovary looks like; here's what the testi look like. So I did this with males and females; in the cestodes and the trematodes, where they're hermaphroditic, both the sexes in the same house, they are monoecious. So I got all these worms that I could, from all over; there were some here at the Institute, and I called people. So I cut all these worms up and studied them in great detail, and my

thought was that if you had a worm in a viaduct, in a liver, with one slice through it, I wanted to be able to tell them what it was. To my knowledge, that really had never been done previously on this kind of a scale. Maybe someone remotely published on one entity in some journal years and years ago.

Of course, by doing all this, this helped, because in 1976, we published our two-volume set on the *Pathology of Tropical and Extraordinary Diseases*, and all this work that I did, we put into that book. And I think that's kind of a classic, because I just don't think that anyone ever had done that previously.

For example, you can go to some of the older books and it would have a cross section of a worm, say, a Wuchereria bancrofti, in a lymph node, and they would say this is W. bancrofti in a lymph node, and you look at the picture, but it wouldn't tell you why.

So what I did was, I have the same picture, but I've got arrows on there: here's the cuticle; here's the somatic muscle; here's a section of the gut; here's a section of what the testes look like; here's the lateral body in the lateral cord region; here are the striations in the cuticle. Hopefully, so that anybody who has a cross section of Wuchereria bancrofti, in testes or skin or wherever they might find one in tissue, could identify it. So that was the purpose. As I say, that, I think, is a classic, and it was just a tremendous amount of work to do that.

But getting back to when I first was downtown at the Old Red Brick building with Winslow, tough cases would come in to the Infectious Diseases Branch, and Dr. Winslow was in charge of them, and he reviewed them, and then he would answer them. Either a contributor would send that case in, or it could be, let's say, the people in Cardiovascular, here in this building, happened to find a worm that was in the heart, they would send it to us as what they call an intramural consult to Dr. Winslow for his opinion. So the cases would come in in the same format we have now. We have two primary sources of material: those that come from outside this building, anywhere in this country or the world; and those that come up from another department. We get things from other departments all the time. Maybe down in Genito/Urinary there's a fungus in the kidney, and, like I said, their interest is in tumors, so they'll send it to us and want to know what kind of a fungus this is; how far can we go in identifying it.

The other thing is education. And, of course, the Department of Infectious and Parasitic Diseases, as it's called now, is very involved in education. We produce study sets and give lectures. In fact, we're giving our course this July in San Juan, Puerto Rico, on AIDS and the pathology of infectious and tropical diseases. Many of us in the department, as, in fact, during the whole history of our department, give lectures at the national and international meetings. Over here at Walter Reed, there's a visiting lecture series--we participate. I've participated in a Trop Med Course at WRAIR (Walter Reed Army Institute of Research), and I've given some lectures to the various histological societies. So education is a big part of it.

People are continually writing in. They're publishing a book and they want to know if we have a good picture of whatever it is, a clinical photo of W. bancrofti. Pictures in our book have been very popular, and they would like to use those in their

book.

So education is an ongoing thing--syllabi, study sets, whatever.

And then the last one is research. Now when I first came, that's what I was doing--research. We looked at these animals that Winslow had, and I helped him with some of that, but then I got more involved in the consultation. I was down at the Old Red Brick building for two or three years, working primarily with Dr. Winslow full time, and then he left and went down to Bay Pines, Florida. At that time, all his research in Chagas' Disease came to a halt, because the person who replaced him was not interested in Chagas' Disease. The person who replaced him was Dr. Daniel Connor, and his primary research interest was in filariasis, in filarial nematodes, and primarily onchocerciasis, a disease that's very common throughout much of Central Africa. So all the work that I had been doing in that time in research kind of came to a halt.

Well, when Dr. Connor took over the department, I had gotten to the point where I was pretty much familiar with most of the infectious diseases in man. So when cases would come in, I would pretty much review them with pathologists that were coming here for training, and we would work them up together. So it was kind of a switch: before, Winslow handled them and gave them to me to work up; now, I was kind of reviewing them and giving them to other people to work up. And this occurred for several years.

So I was involved in the consultation, and at the same time, I was cutting up all these worms and learning about pathology and learning about parasitology. Of course, that's been ongoing ever since. But at that time, I was really kind of getting settled in the department. And we did that book, which came out in '76. That was a lot of work; that took years to do that book, to get all those pictures together and have them labeled and whatever. So that was a very big project.

Now the other thing is, over the years, I've also been involved in research, but it's been a different kind of research. I've not really ever gone back to where I've had a project where I was inoculating any animals and that sort of thing. But I've done research in the sense that one fellow came here, for example, and he and I examined all the cases of ascariasis at the AFIP. And we published a research article on that, and gave their ages and where the lesions were and whatever. So that's a little different type of research. But that's what most of my research has been in the last 20 years, publishing for the first time something that has been found.

For example, a contributor sent us a case that I diagnosed as *Angiostrongylus costaricensis*, a worm that occurs in Central America. And this was from Africa. So this was the first reported case of this worm occurring in Africa. So I published this with a couple of other people.

So that's the kind of research. It's research in the sense that you take the case and you study it thoroughly. You get a lot of slides cut. You've got to review the literature and know what's already been done. So it's research from that area.

And I've had other people send cases in that were unique or the first time, and we published those. And I've been very fortunate. I had to do something not long ago for a curriculum vitae, and I guess I got about 125 publications on various exotic, rare, unusual things, some of them we actually describe for the first time.

For example, there's a worm that occurs in man, which has gone through an evolution of many names, but back at that time, it was called *Acanthocheilonema*, and then *Dipetalonema*. Today it's called *Mansonella*, and the species is streptocerca. And although the microfilaria had been found in the skin of man years and years ago, the adult worms had never been found in man. And we published an article describing, for the first time, these adult filarial worms in man, both the male and the female worms.

So, most of the research has been this kind of thing. But I've had an opportunity to work with great people and really publish a lot of good things.

Getting back to the evolution of my career, I got then to where I guess I knew most of the cases pretty well. We always had people that came here for training, usually two or three or maybe even four, primarily military, and there were other people who would come, from foreign countries.

For example, we had a man come here about 1971, Carlos Serapion, from Brazil. He was a pathologist, and he was interested in learning tissue pathology of tropical and exotic diseases. He spent a year with us in our department. So when he came, by that time, I wasn't handling the cases directly. I let him handle them. He worked them up and presented them at our conference.

Now we've always had a conference where the people in our department would get together and go over these difficult cases, either to try to find an organism that's the cause of it, or if it's something that's unique, it would broaden our experience. At the present time, our conference lasts from 10:30 to 12:30, four days a week. We have it Monday, Tuesday, Thursday, and Friday, where we all get together, the people in Infectious Disease, and go over these cases, either for education or because we're trying to solve a case. So we always run these cases through conference, and we've done this for the last 20 years. So that's what our conference is.

But we got to the point where these other people would bring the cases up for conference. They would work the case up. In other words, let's say a case comes up from the Pulmonary Department where there's a fungus in the lung and they want to know what it is. They might just have one H&E stain slide, and they might have a paraffin block. So whoever works that case up would take that paraffin block and submit it to the laboratory and order all the appropriate stains, and then study the case and make up his mind as to what he thought it was. And then he would bring that case to conference after he's done his homework on it, whatever that requires, if it required reading about it in a book or whatever.

So he comes to conference and he presents this case, as an unknown, to the rest of us. The number in the conference varies; it could be anywhere from three to ten people. But when we have conference, we kind of have an unusual format--we always like to look at these cases as completely unknown. I don't want to know anything; all I want you to do is to give me the slide.

For example, say you have a slide with skin that's got a worm in it. I don't want you to tell me where that's from on the body; I don't want you to tell me where that patient's from. I want you to give me that slide, and I want to be able to tell you what that worm is. Because, you see, if you tell me this is from an African, well, then right away

I'm thinking of the worms in the skin in an African, and that's going to prejudice me. And I don't want to be prejudiced, you see, I want you to give me that slide and I want to tell you this is Wuchereria bancrofti, and this patient has been in an endemic area. This patient did not get this in St. Louis, because there's no Wuchereria bancrofti infections in St. Louis. So this is the way we like to do it. Coccidioides immitis predominates out in the San Joaquin Valley in California, in southwest Texas, and in Arizona, so if you tell me, here's a patient from Arizona that's got a necrotizing lesion in the lung, right away I'm going to be thinking of coccidioidomycosis, you see. So for my training, I just want to look at the organism and see the reaction and see the morphology, and I want to go as far as I can. And sometimes I might not be able to go too far.

Then, of course, before we give our final opinion, before we write anything or call the contributor, we talk among ourselves about the clinical history and various tests that have been done. But we always run our conferences as an unknown.

A lot of conferences in the building are not run that way. They'll start out that here's a 27-year-old man from Chillicothe, Ohio, who has this plaque on his skin. If I look at skin and I think it's leprosy, it probably isn't going to be someone from Chillicothe, Ohio. And if they tell me they think this person has leprosy, then I'm prejudiced already. I want to look at it and say, hey, this is skin, I think the nerves are involved (leprosy's a disease of nerves), I think this could be leprosy. So that's the way we handle our conference.

Q: Were you all driven by events? You started there at the time when we were beginning to get involved in Vietnam. Later, we had a commitment in the Persian Gulf. In other words, you're a military institution, and military troops have to go places. Were they saying, Come on, let's concentrate now on problems in Southeast Asia? Or, later, problems in the Middle East?

MR. NEAFIE: I don't think we were really involved in the problems, but certainly I remember the Vietnam War very well. We were, in fact, down at the Old Red Brick building back at that time.

For example, we used to see a lot of malaria from the Vietnam War; malaria is very common there. And these were autopsies. It was unfortunate, but what would happen in a lot of them in that they would have a fever of unknown origin in Vietnam. Then they would evacuate them to Clark Air Force Base in the Philippines. And a lot of times these men would die because they just couldn't find the parasites. Either they were so low in number or they missed them or whatever. But I remember, in the Vietnam War, we looked at a lot of autopsies of malaria. The patient would go on and die, and they would do the autopsy at Clark Air Force Base, and then they would send the case to the AFIP--nothing more than just routine. And we have a lot of autopsies of people that were in Vietnam. Now I would say that since the Vietnam War ended, we have seen very few cases, autopsy materials, of fatal falciparum malaria, simply because we don't have troops in an endemic area.

I don't know that we really added anything to keeping the troops from getting the

disease. They have prophylaxi and drugs they should have been on. The drug companies are involved in that, and CDC (the Centers for Disease Control) recommends prophylaxes and so forth for the troops going there, just like in Somalia or in the Persian Gulf or anyplace else. I'm sure that they have malaria in Saudi Arabia, and leishmaniasis and other diseases. And there are other people that usually tell them what they should do, you know, Don't drink the water, and don't be bitten by any more insects than you have to. And they have insect repellents and so forth. So we're really not that much involved in epidemiology and prevention, although we are in consultation.

Now some of the people that went to the Persian Gulf came back and maybe got out of the military and were out in Denver and had cutaneous leishmaniasis, and they did a biopsy out there and they would send it to us, thinking that maybe he has cutaneous leishmaniasis and could we confirm that or whatever. And we had some cases like that, not really very many. I think the troops in the Persian Gulf got good information and did well in keeping from getting infectious and tropical diseases.

The wars have always been significant, but I do remember Vietnam. That was probably the primary thing that came out of there. Of course, they had cholera and some other diseases, but from infectious disease, I think malaria just overwhelmed anything else they might have gotten there. And, of course, *p. falciparum malaria* will kill you. There are three kinds that won't kill you: *p. vivax*, *p. ovale*, and *p. malariae*. They can make you sick and kind of miserable, but only *p. falciparum* kills you. And they had a fair amount of that there.

We still see an occasional autopsy from a Peace Corps worker. In fact, we had one not long ago, a military man who had gone from Japan to Africa, and they thought he had a viral infection, as I remember, and never suspected malaria, and, as far as I knew, never did any peripheral blood smears. This was a fairly young man, who died of malaria.

Q: Did you find that because of your geographic orientation, you were involved with assisting both the Peace Corps and the Foreign Service?

MR. NEAFIE: Oh, yes.

Q: I speak as a retired Foreign Service officer. They must have been a prime ground for supplying you with specimens.

MR. NEAFIE: Yes, they were. For example, I can mention one name, and that is Dr. Martin Wolf, who was with the State Department. He traveled all over when Kissinger was there; his personal physician. Marty is a good friend of mine. He has his own lab now down in Washington. He's retired from federal service, although I think he still goes down there part time. He sent us material probably for a 20-year period. He would get Peace Corps workers that he thought had onchocerciasis. And we published a couple of articles with Marty. But he saw a lot of Peace Corps types and other people that went overseas and got problems, and he took biopsies on many of them. And others that were

referred to him, he told them, yes, well, he knew people at the AFIP and would recommend that maybe some physician out in St. Louis would send us the material. And that's exactly right, we did see a fair amount of material from Peace Corps workers when they came back to this country and they needed a diagnosis. But that's right, we did, and still do, on occasion.

Q: What about external research labs out in the tropics or in cold countries?

MR. NEAFIE: We had a couple of projects quite a few years ago. One was in Onderstepoort, South Africa, where we sent a person there, I think it was on a one- or two-year basis, and they had kind of a rotation through there for about 20 years back in the Sixties and Seventies. I don't think they have that program anymore. They had another one with SEATO, I think in Bangkok, Thailand, where we had people go out there for training and, I think, spend a couple of years. We've had several like that. I'm not sure where else. I'm thinking whether we had anything in Cairo or not; I don't think so. But those are two that come to mind.

Now we have had some other things like this. From my own personal experience, this was kind of a unique thing. In 1972 and 1973, I was able to spend two rotations of three months each in Brazil with Project Hope. We collaborated with them.

Q: Would you explain what Project Hope was?

MR. NEAFIE: Well, Project Hope was established by William B. Walsh. He talked, I think it was Eisenhower, into giving him a ship that was kind of worn out and dilapidated. It was the old Naval hospital ship called *Consolation*. There were three of them built back in the early Forties: the *Repose*, *Sanctuary*, and *Consolation*. And Eisenhower turned the *Consolation* over to William B. Walsh, who was an MD physician who had a great interest in underdeveloped countries and providing some kinds of medical care for them. So he was the founder and director of what he called Project Hope. He got that ship and, through donations, built his program and staff up and made that into a good operating ship. I think it had about 12 or 15 voyages. Every year it would sail out and go to a port and would be gone about ten months. The doctors would fly in and out from the ship; they'd be down there for a two-month rotation. The nurses and so forth would go down with the ship, and they would be there for the full ten months. So that's the way this was run.

Some people get the wrong idea of Project Hope. They think that the ship's just going up and down all these rivers, curing everybody. And that's not it at all. The ship pulls into a port where they have a medical school, and the big thing is to teach. And, of course, they would bring some patients onto the ship. They had a hundred or so beds on the ship, and they had operating rooms, and some of the more difficult cases they would bring in on the ship. But primarily it was teaching. The radiologist on the ship would go out to the medical school and teach the residents radiology, and cardiovascular and whatever group. So the primary aim of Project Hope was teaching. That's the way it was

run. And they supplied medicine, vaccinations, and just many things that these people never had.

I was the first and only rotating parasitologist that was ever with Project Hope. And I went down there for two months in the summers of '72 and '73. Now this was a tremendous experience for me, because up to that time I never had seen a patient. I was working with slides in my office. And, of course, I'd never gone to medical school, so I never "saw" patients. So it was really enlightening to me to be able to go down there and work with the parasitologists, and go out in the field and see patients who had distended abdomens due to a variety of parasitic worms, or to work with a dermatologist and see patients that actually had cutaneous sores, and to go to the leprosarium and see some of the clinical manifestations of leprosy. It was just unbelievable for me. It was kind of like a kid in a candy store, you know. I'm sure they remembered me, because I photographed just about everything that didn't move, and many things that did move. So it was really enlightening for me to go down there and not only give lectures later on and talk about these things, but having some firsthand knowledge of really here is a patient and here is a lesion, and being down in an endemic area, and going out in really the boondocks, where they don't have good sanitation, they don't have running water, and the flies and the mosquitoes they have got. It was just a great experience for me in '72 and '73.

And with Project Hope, we did collect some material down there, clinical photos that I took, that we used in this book. That was '72, '73, and the book came out in publication in '76. So our aim was twofold, to go down there and help Project Hope diagnose parasitic diseases, and also collect some material for our book. It worked very well for both of us.

I had one other opportunity, and that was in 1977, to go abroad. Dr. Connor and Fred Duncan and Dr. Wayne Myers and I all went to Zaire for one month, to study filariasis. I was in Zaire, and this was 800 miles up country (just like you see in the old movies of Humphrey Bogart), where the Congo is and the tributaries and the lush vegetation and the savannah and the villages and all the other things that go with it. And that was also a tremendous experience. I saw a thousand patients up there, and I brought back a thousand smears of blood. We were studying filariasis, and we were looking for Wuchereria bancrofti microfilariae peripheral blood, but we also saw other filariids. And a lot of these patients also, in fact, had malaria. But that was another great experience, to be in Zaire for one month, and to eat their food.

When I was with Project Hope, I just knew ham and eggs; they just had such good food on the ship that I did most of my eating at the ship. I didn't worry about the local water and getting amoebiasis or some of these other things, because I did everything on the ship.

But when I got out to Zaire, of course, I didn't have any ship, so I had to eat the local food. That was very good; we had python, elephant meat, water buffalo, and a couple of other interesting things.

But I think it's valuable to, say, give a lecture and talk about these things actually having had some experience, rather than just out of a book or even out of a slide, and never having seen a patient. It just kind of puts it all together, you know what I mean?

So it was very valuable for me.

These projects that I was saying we had with Project Hope out in Thailand and in South Africa, we have nothing like that now. Those were closed down due to lack of money or different emphasis or various reasons. But we don't have anything going on like that right now.

Several years ago, they did have one thing going on in that area. Dr. Ann Nelson had a project going called SIDA, which was a joint venture with the Zaire government, the Centers for Disease Control, the National Institutes of Health (NIH) the Institute of Tropical Medicine in Antwerp, and the AFIP, primarily studying AIDS, especially trying to do AIDS autopsies. I'm not sure whether that project still exists or not. But as you know, autopsies are very difficult to get permission to do in an African, almost impossible. They're controlled primarily by spiritism, and that body will not get to heaven if you take any part out of it. So, trying to get autopsies done is very precarious.

Q: Did AIDS begin to appear in your work early on?

MR. NEAFIE: Yes. When AIDS first came out, and I guess it was first described in about '81, as I remember, but over the last ten years [tape end] ...see things that we have never seen before. In fact, multiple infections were not very common prior to AIDS. In other words, if you have the lung, you might have a patient who has cryptococcosis, and you have the fungus, and you identify it, and that ends it. Or he might have TB, an acid-fast organism, and that ends it. When AIDS came along, then you started to see a lot of multiple infections--a patient who has TB, cryptococcosis, histoplasmosis, CMV (cytomegalovirus). So now, in that lung, he's got four, maybe five, different infectious processes, because he is immunosuppressed. And organisms that normally he would not have, he's now going to have. So, multiple infections became very common; they became, many times, the standard rather than the eccentric.

The other thing is, of course, that in an immunocompetent person, most organisms that infect man cause a significant reaction, primarily a granuloma or an abscess. But in the AIDS patient, you can have many of these infectious agents and have no reaction at all. So you can't just look at that tissue or slide and make these diagnoses. A lot of times, you can get the feeling for the infectious process, or the organism, based on the histopathologic changes. In the AIDS patient, there might not be any histopathologic changes. So these AIDS cases became a lot of work, because you had to get a lot of these stains to look for organisms, just because you didn't want to miss them and they might be there. So they were very difficult cases--multiple infections.

And AIDS just mushroomed. When it first started, of course, they were incorporated and set up in our department. We have Microbiology; we have Infectious Disease; and now we have a group called AIDS. They had their own secretaries, and they ran their own shop. Most of these organisms, even in the AIDS patients, were still organisms that we were familiar with over the years, so we still had a very good handle on the organisms. So the pathologists in the AIDS group would bring these cases to our conference and show them to us, just to confirm they were what they thought they were,

to get our input on them, because most of them we'd seen, and also to enlighten us, so we could know what's going on in AIDS, too. So, over the last ten years, I don't know how many thousands of cases of AIDS we have seen.

I personally don't have any great interest in AIDS, and I've seen so many of them, for so many years, that I'm almost tired of looking at them. They get to be very monotonous; the clinical histories are the same, you know, gays and IV-drug abusers. It's sad and pathetic, but there's nothing, of course, that we can really do from our perspective. But Dr. Lo, upstairs, is doing some research on AIDS. There are, of course, lots of places where they are trying to come up with vaccines and treatment and whatever. So I don't want to sound like I have no compassion for these people; I do, but I'm just not in any position where I really deal with that.

Q: Are there any other strains that you're seeing? As we get better with various types of drugs, do you find mutations? I'm speaking as very much the layman in this, but are you finding that diseases are changing?

MR. NEAFIE: Some diseases we thought we had pretty much whipped are tending to come back.

For example, TB has been on the increase in the last several years. At one time, there were sanitariums all over the country, and not good treatment. Then I think some experts thought, by the year 2000, it would be eliminated altogether. Well, it's not, and it's on the increase. So that might be one example. And we see, believe it or not, a fair amount of tuberculosis up in our department, quite a bit from Africa, but a lot even still from this country. So it's a long way from being wiped out.

Now malaria, over the years they have gotten a lot of resistant strains to malaria. In fact, I had a case not long ago that was *P. vivax*. Usually the resistant strains are in *P. falciparum*, the one that kills, but now it's been pretty much accepted that there are strains of *P. vivax* that are also immune to the therapy, probably chloroquine; I think that's still what they use. So, in malaria, that is the case.

In some of the other diseases, of course, that is not the case. Syphilis, for example. I don't know of any in that area. And a big, whopping dose of penicillin probably is still curative for primary syphilis. Now lifestyles change a lot, and we may be seeing more syphilis and more venereal disease than we did at one time. We went through kind of an epidemic in this country of the herpes, back when the herpes, for a sexually transmitted disease, was such a big problem. And it still is, although that's one they usually don't biopsy. The chlamydial infections are probably more common now than they used to be, and we see more of that entity.

There are some organisms that they're doing EM (electron microscopy) on that they weren't able to do studies on years ago. So there's a much bigger area in this field for EM studies.

Also, there are a lot of immunohistochemistry studies coming in, and it's really not an area that I'm familiar with at all; I just don't even get into that. But there are various techniques other than just straight anatomy and microscopy. Some of these are of

significant value, and, of course, a lot of them have to be worked out yet. PCR (polymerase chain reaction) is another one that they do. There are various other things that they are doing currently in research and so forth, but I've not gotten into any of that area personally. I still like just to look at slides and make the diagnoses. And I think that's still of great value today, although, I don't know, down the years, there's automation and so forth, and who knows what it'll be like 10 or 15 or 20 years from now.

Q: In 1990, Dr. Connor left?

MR. NEAFIE: I guess he's been gone probably about five years.

Q: You moved up to become chief of the Parasitic Division, didn't you?

MR. NEAFIE: When Dr. Connor retired, he was chairman of the department, and I was simply a member in the department. I was the senior parasitologist in the department. When Dr. Connor left, Colonel Douglas Wear, an MD pathologist, replaced him as chairman of the department. Now, within our department, Dr. Wear thought there should be a smaller unit, a breakdown, a subdivision, so he came up with the Parasitic Disease Pathology Branch. That was only established a couple of years ago. I'm the chief of that branch.

Q: Turning to the operational side, rather than the medical side, you came in in '63, what were some of the most significant changes in how the AFIP operated, from your perspective?

MR. NEAFIE: I've already mentioned one of the things, the fact that now they charge for consults. Back then, they didn't. That's a tremendous change.

Q: That happened when?

MR. NEAFIE: Oh, I guess, a couple of years ago. They experimented, I think, with GYN/Breast first, and I guess that was favorable, so then they expanded it. I think there are only a couple of departments now that do not charge. We are one, in fact, that still does not charge. And even if we did, we would not charge these African contributors, because these are medical-missionary-hospital types, where they just don't have the money; they just couldn't afford to pay for it. So that is one.

Another one, I would say, is money. Back in the Sixties, I had the impression that if you wanted an electron microscope, all you had to do was virtually fill out a work order and you got it. Whatever you wanted, money was no problem whatever. For example, microscope bulbs. In 1963, if your microscope bulb burned out and you wanted another one, you probably could have ordered 50 bulbs--just ordered them and got them. Now, in 1993, you turn that old one in and they'll give you a new one--a one for one. So this is an example of how the money has changed.

About a year ago, up in our department, there were three people whose jobs had to be terminated because of lack of funds. That was the first time since I've been in this Institute that I heard of anyone at the AFIP who ever had his job terminated because of lack of funds. And at that time, I think there were 20 or 30 others in the building who also lost their jobs. The administration, to "tighten its belt," eliminated the whole Biometric unit.

So, money has made just a significant difference. I don't know where that'll end up, especially with a new administration, or how hard that is really going to affect us.

But back in the Sixties, money was no problem whatever, hiring people and getting any supply that you wanted. And today, it's nothing like that.

One other big area, as I see it, is that there have been a lot of changes in the way the Institute is now run, over the way it was run then. The administration has come up with more committees and has more directives and more "AFIP policy" and that this has got to be done in this fashion. At one time, I think that most of the chairpersons in the departments pretty much ran their departments pretty much the way they wanted. That has long gone, and now the administration up here is telling everybody more or less how they're going to do everything. And I think a lot of the things have pros and cons to them. From my perspective, a lot of the things I don't think help my mission. They've made a lot of changes, some significant, that I don't think help my job at all, or help me do what I am doing, even though there may be some value to it. To me, the minuses outweigh the positives. I think, in essence, the chairpersons have lost a lot of the power that they had at one time.

Q: You're talking about...

MR. NEAFIE: Institute policy.

Q: Basically overadministration, which is a creeping thing.

MR. NEAFIE: Yes, it's a creeping thing.

Let me just take research, for example. At one time, you could almost do anything in research that you wanted. You didn't have to get anyone's permission. If I was chairman of the department and I wanted to do a research project with NIH, we talked about it and we did it. Today, every research proposal I think, has probably got to go through some committee, and you've got to submit all this written agenda, and they evaluate it and decide on whether the money is appropriated. It is much, much more complex. That's one example.

Take the library. When I first came here, you could say to the woman who ran the library, "Hey, I saw this book and I would like it, could you get me a copy?" Just put down the name of the publisher and the author and whatever, sure, she'd go order it for you. They don't do that anymore. Now, there's a library committee; they've got to decide whether or not they think that book is pertinent. And you've got to sign out for it if you want to keep it in your room, or it's the library's and they'll keep it in the library.

Someone came along with the idea that they would like all the slides sent down to the central file, that you shouldn't have any of them up in your office, more or less.

Well, I'm kind of the archivist, you might say, for our department. All the cases that come through our department are to be funnelled through me, and I will make a decision as to whether or not these cases ought to be filed in our department, or whether they should go to the central file.

Now the central file is partially downstairs in this building, but a lot of it is over at Forest Glen, in a building over there where they store things, the repository.

Let's say you're a visiting pathologist from England, and you're visiting Mostofi down in Genito/Urinary, and you're leaving tomorrow, but you come up and you say, while you're here you would like to see an example of a case of pentastomiasis (these are very unusual parasites). And I'd tell you, "Well, they're in Central File. I'll put in a form and get it okayed by someone. They'll okay it here, and they'll send a truck out to Forest Glen, and, I don't know, maybe in two days I'll have it for you."

My point is, this should be available right in our department. We have a very good system. And I can say to you, "Sit down, and I'll go over to my file and I'll get you a case of pentastomiasis. And here is what they are." Or if you want to see a case of rabies, I'll go over, "Right here is a case of rabies." Or "Have you ever seen histoplasmosis in a two-year-old?" And I say, "Well, yeah, I think I have one."

A lot of cases I will send to Central File. You know, a case of TB in the lung is a case of TB in the lung. But if it's a case of TB in the lung of someone who, for example, is PPD negative, and there is no clinical evidence that they had TB, or some unusual case, then I file that in our department. So we have a tremendous collection up in our department of just about any infectious, tropical, exotic disease that you can imagine. You can come up to my office and I can show you. And I can also pull the folder out and give you the clinical history.

Now this was another thing, years ago they converted over, because of space, and they said we don't want to deal with these folders anymore, where you can read them. And they want all this on a little card, that you've got to go somewhere and put on a machine and turn it, and, yeah, you can look at it there. And if you want to take notes from it, then you're going to have to write them all out. Whereas, if you've got the chart in front of you on an 8 x 11 paper, you can just put that in a Xerox and xerox that history, and you've got all the history right in front of you. So, I can see the value of putting these on a microfiche, in some big machine, and it takes up no space, but they don't help me any, and I never use them. I don't use them. So what we primarily do is, when we turn folders in, a lot of times we will xerox that information and make a dummy folder. That's how you get around it. Because I want that history. If I want to show that to you, a microfiche sometimes doesn't work, you can't get bulbs for them, or someone else is using it.

Q: You're talking about the age-old battle of the specialist versus the administrator.

MR. NEAFIE: Well, yeah, it's a constant thing that's going on all the time. They rarely

ever come around and say, "Hey, how would this program affect you? or "What do think of this program?" The only thing I know is, here comes the DF telling me this is the way it's going to be done.

Q: A DF is a...

MR. NEAFIE: A disposition form, where someone's signed it, and this is the way it's going to be now, whether it's of any advantage to me or not. But over the years, I see where a lot of this has gone on.

You send these folders down to a central file, and they code them in a machine. So let's say you're in Dermatology and you would like to study all the cases of onchocerciasis, you can go to Accessions and they can tell you there are 800 cases in the Institute, and this is one of them, and they can give you a number and tell you, yeah, that's up in Infectious Disease.

But our files are available to anybody. If you're here visiting from the Gorgas Hospital and you want to see some unusual cases, even though you're down in Derm. Path. for a year, come on up and I'll show you what we have. They're available to anybody. To me, they're no good in Central File.

Q: As I say, you're describing a part of the clogging of the arteries of government.

MR. NEAFIE: Now I can see their viewpoint in getting all the slides in Central File, because when they're not in Central File and they're in your department, people come through and they steal them and they break them and they lose them. And if they're in Central File, at least you know where they are. So I understand that.

But we have a good system in our department; I don't know, maybe the best of any in this building, because in talking to some of these other departments, they have no system like this at all. So we may be unique. But you can come up to our department, and, say, you want to study some cases of syphilis, I've got a great teaching collection of syphilis up in our department. I can go and get you the folder with the clinical histories, and I can get you the slides on those cases, almost instantly. And you go to another department, let's say you go down to GYN, I think this is true, you go down there and you say, "Well, I'd like to study a couple of unique cases of breast cancer," they don't have any file down there, as far as I know. They might send you to Coding, and they'll put in adenocarcinoma of the breast or whatever, and they can punch out 400 cases or 10,000 cases, and you can order them from Central File. But I don't think they would have them in that department. We may be unique in that respect.

Now I feel that all the departments should have that. But a lot of the departments have a lot of turnover and a lot of military, and, you know, they come and go like flies. And it's, I think, been a blessing to our department, in a sense, that I've been there for 30 years and I've been the archivist and I've tried to get all these cases and maintain and keep a good collection. But it's taken a 30-year period to do this; I've built them up over all these years.

It's the same way with 2 x 2's.

Q: You're talking about file cards.

MR. NEAFIE: No, I'm talking about 2 x 2 slides that you project. People write in and call all the time. A guy over at the hospital called me two days ago, got a lecture on atypical mycobacteriosis, could I help him, over here at the hospital? So I said, "Yeah, I can help you." So he's going to come over next week and I'm going to loan him some 2 x 2's. But we have a central file in our department, exactly for that reason. Or if someone in our own department wants to give a lecture on atypical mycobacteriosis, we have 2 x 2's in our department. We've got a collection not only of the glass slides you look under the scope with, but 2 x 2's for giving lectures.

Q: What do you feel about the future of the AFIP? Do you feel that universities and research hospitals are supplanting the AFIP, or do you still see it, at least from your perspective, playing as much of a role as ever?

MR. NEAFIE: I will only speak from my perspective, being as I'm not a pathologist and I'm not in tumors. But as far as infectious and parasitic diseases go, as I said earlier, I don't think there's anyplace in the world that can compete with the AFIP as far as resources go. No place, worldwide, has the variety and the system that we have. That's number one. Just the material, there's no place comparable.

Now let's talk about expertise. Am I under oath?

Q: Oh, no, but the cameras are rolling.

MR. NEAFIE: Dr. Connor always used to tell the joke about the surgeon who was on the witness stand. They asked him who the best surgeon in the world was. And he said, "Well, I am."

Then the guy said, "Well, isn't that pretty boastful?"

And the surgeon said, "I'm under oath."

As I said, the area that I'm in is really a unique area. It's really not pathology, in a sense, and it's really not parasitology, nor really is it epidemiology, nor is it really tropical diseases. It's kind of a combination of all those. It's a unique blending of all those together. And I don't know of any other institution that can really match us in that scope. So, personally, I see no end to the contributions of our department.

Now I've been a great supporter of the AFIP ever since I've been here. It's the only medical institution I've ever been at, for one thing. But I am very pro-AFIP. I'm a guy who likes to come in on Monday, and who can work late every day, because I love my job. I've told this many times to many people: I wouldn't leave the AFIP for any amount of money, anywhere. I think I've got the best job anyone could possibly have, because I've got a great combination: a good balance of teaching education, consultation, and the ability to publish and do research. So I just think that our department is very

viable, I think it's very strong, and I don't think there's any institution that can really match us on our expertise in these areas.

Now there might be one place that knows more about fungi than we do. Or there might be one that knows more about bacterial infections, or about a specific bacterium, than we do. But I think it would be hard to beat us collectively, and especially, I guess, on the helminths in tissue. I think that's probably where we're most unique among any of them--the helminths.

Maybe this is primarily because of my own self. And when I retire here (I'm still under oath), that probably basically will be gone, because no one else has had the interest in the worms that I have had, nor have they done what I have done. Even though we've got 2 x 2's and we've got slides and we've got labels and whatever, it's a personal, individual thing for me, that I've really built all that up over those years. But maybe the good news is, I'm not interested in retiring right away. So maybe someone will come in over the next ten years, who will be a civilian, a young individual, who will be interested in pursuing this, and maybe he could be here another 30 years or whatever.

But we make diagnoses that I don't think anybody else could make, on some diseases. Everything is perseverance.

I'll give you one example. We had, quite a few years ago, a case from a medical missionary over in Africa, and I thought it was leprosy. And I disagreed with a couple of world-renown experts. I'm not even sure why, but they didn't think she had leprosy. Leprosy is not one of my strong suits, and I don't mean to imply that. But for some reason, I pursued that. I think I looked at three or four Fite-Faraco stained sections and couldn't find anything. And I think I went back two more times and ordered more stains, to demonstrate that organism. I know I spent maybe ten hours looking at slides from that woman, and did finally find an organism. And I made a definitive diagnosis of that woman having leprosy.

Now very few individuals in this world would ever spend ten hours to diagnose one case of leprosy. They don't have time. If you're in routine hospital setting, they just don't have time for that. They don't even have the patience for that. They would look for five or ten minutes, and if they didn't find it, they'd just write a morphologic description of it. They wouldn't pursue that.

So the ability to pursue it here, and to have that time, to that degree... And I don't do that with all cases, obviously, I couldn't do that. But I do have cases that took hours, many hours, to solve. Most places just couldn't do that, and they don't want to do it.

Q: Well, Mr. Neafie, on that very positive note, why don't we stop at this point. How's that?

MR. NEAFIE: Very good.

Q: I've really enjoyed this.

MR. NEAFIE: It's been my privilege to be here.

Q: Thank you very much. This was excellent.

MR. NEAFIE: Well, thank you.