

Dr. William Chin Interview

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William Chin Interview
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Leo Slater: This is Leo Slater, today is October 13th 2005. I'm here interviewing Dr. William Chin at his home in Martinez, California. This is part of my Stetten Fellowship on the history of malaria research at NIAID. I just want to confirm to you, Dr. Chin, we will be taping our conversation today.

Dr. William Chin: Yes, I understand.

Leo Slater: I thought we'd start chronologically with your family background and your early education.

Dr. William Chin: My father was a laundry man in Brooklyn, New York. Just prior to the onset of World War II, when he saw that the Japanese were getting closer and closer to our home village located approximately halfway between Hong Kong and Canton, he sent for the family: me, my brother, and mother, to come over and join him. We arrived early in 1939 -- I remember that was the year of the New York World's Fair, which I visited -- I was 10 years old when we arrived. My brother was 5. Since I didn't know a word of English, I was placed in the first grade with five-year-old students at P.S. 99 in Brooklyn. When you're immersed in a language, it's the best way to learn, so pretty soon, I acquired enough skill in English to be socially promoted up the ranks. By the time I graduated from high school, I was less than two years behind the rest of my friends. After school, elementary and high school in Brooklyn, my father, who was relatively uneducated, asked the pastor of his church, a Chinese minister, for advice on where his first son should attend college. The minister said, "I have just the school for you! All of my children went there. It's the University of Dubuque in Dubuque, Iowa. It's a Presbyterian college, small; it has a seminary." That, of course, delighted my father because he had wanted me to be a minister of his church. Failing that, he wanted my brother to be a minister of his church. But above all, he had wanted to be a minister of his church, but because of his lack of education, there was no way. So, he sent me to Dubuque, and it turned out, for me, a disappointing experience, and for my father, a disaster, because I was arguing constantly with the evangelical teachers over interpretation of the Bible, literal versus what I perceived as common sense. Every passage of the Bible was interpreted literally. I couldn't accept that. By the time I was of

college age, I knew at least enough about science and evolution to distinguish between dogma and reality. To save my parents the cost of room and board, I worked in the kitchen scrubbing pots and pans used by the cooks -- three times a day, six days a week.

The University of Dubuque was a small college, with very limited subjects to choose from other than literature, history, and religion. I decided after the first year that I was going to transfer. So in my second year, I started looking for a school. In those days, the University of Michigan was truly champion of the West. They were first in football; basketball, hockey and -- tennis maybe -- and they were grabbing headlines in local papers. So I said, "Ah! That's the school I should go to!" I'd been enjoying some of those sports, not as a participant but as a spectator. I applied to Michigan, was accepted, and went to Michigan for my third and fourth years. I received my bachelor's degree from Michigan.

At the time I graduated from Michigan, the country was in the midst of the draft for the Korean War. Most students, including myself, were trying to find a way of avoiding the draft by staying in school. My life goal was not firmly set, although vaguely, I had some idea that maybe I should go to medical school. But I knew that my grades were rather low -- at best a C+ average -- and I would have very little or no chance at all of being accepted. So I decided, in my senior year, to take a course in parasitology, and I think something in that course grabbed me. I became very interested in medical parasitology, particularly a disease called malaria, and that turned me on.

After graduation I went back to Brooklyn, and enrolled at New York University (NYU) thinking that I would do a graduate course in medical parasitology. But the transition from a campus environment in Ann Arbor to a New York City environment, where I had to take the subway to NYU on a daily basis, made it difficult for me to adjust. I was able to complete one course, histology, which I even got an A in, as a matter of fact. But all the other courses, I just gave up on. I never finished that semester at NYU other than that one course, which I had taken during the summertime.

I went back to Michigan thinking I'd pick up where I left off. By this time, I was being hounded by the draft board. I was getting notices asking me to update them on my plans and what I was doing. I consulted with a faculty advisor and explained to him what my future plan was: It was indefinite, but I felt that maybe I would enjoy learning more about medical parasitology. He said, "Well, why don't you start taking courses at the School of Public Health? Maybe earn a degree in Public Health and see where that might lead you." I enrolled at the Michigan graduate school, began taking, I believe, another parasitology course at the School of Public Health, and before the semester was over, I was drafted. I received this infamous card "Greetings...." So I went back to Brooklyn and went to the recruiting office. By the way, I didn't even tell my parents that I was drafted, because I knew they would be so disappointed. I simply got on the bus and was inducted and went to Fort Dix for basic training.

At Fort Dix, I underwent regular basic training, and at the end of the training, I was listed as an infantryman. I knew that unless I could get that changed the odds were virtually 100% I would wind up in Korea. Those were the days when there were rumblings that the Chinese were about to enter the conflict. And so toward the end of my training, I was discussing this problem with the sergeant in charge of our company, a fellow who scared you to death as an NCO, barking at you and intimidating you, but at night, by everything I saw, he was a really nice guy. I still remember his name, Sergeant McVicars. I explained my problem to him: "I'm really afraid of going to Korea and meeting Chinese." He told me that I made top grades in all these tests -- after arriving at Fort Dix, we had to take a series of tests, including this aptitude test and I suppose something resembling an IQ test. He said, "Why don't you apply to OCS, the Officer Candidate School? You look like you'd make a good soldier." I had won awards a couple of times as a Colonel's orderly on inspection, which allowed me a free day of sitting in the colonel's office doing nothing. The most important reward was I got a free weekend of leave.

I thought, "That's a good idea!" I put in an application for OCS and when my company completed the training, everybody received orders. I was left behind because my application to OCS was pending, and I'd say 95% of our company went to Korea and the

other 5% Germany and various other places. There were a few of us left over for one reason or another. This was an interesting time. I pulled various odd details. The first one was as a member of the burial detail: Those were the hardest few weeks I spent in my life, because -- as a Chinese -- I was part of an escort to attend these funerals of soldiers returning from Korea in a casket. You go through the whole drill, folding the flag, firing the rifles, and so on. And I felt self-conscious because I knew that the people sitting there looked at me, might perceive me as an enemy. I wasn't sure if they knew I was Chinese, but I felt very uncomfortable and was totally relieved after about three weeks when I was told to do something else -- to help train recruits, of all things.

Just about a week later, an event took place in Fort Dix that changed my life completely; I think that was the first turning point in my life. There was a devastating epidemic of influenza at Fort Dix. And nearby, at the University of Pittsburgh, was a young researcher by the name of Jonas Salk. He was attempting to develop an influenza vaccine at the time. When he heard of this epidemic at Fort Dix, he hurriedly came and arranged to have a study undertaken at the base using the new recruits to evaluate his vaccine. He would inoculate some with vaccine, not others, but regardless, there was a lot of venipuncture that had to be done when recruits came in for their basic training. Dr. Salk wanted Fort Dix to provide technical assistance for him. So they started screening for people who were available and found two of us who had whatever they considered as medically qualified backgrounds. I had taken courses in physiology and public health and parasitology, and the other fellow -- I still remember his name, Joe Grillo -- was an Ivy League student who had finished his Bachelor's Degree as a pre-med but never got into medical school. The two of us were called in to a newly created office headed by a Captain who was part of the Army Medical Service Corps. I wasn't sure what he wanted of me. The first question he asked was, "Chin, do you know how to draw blood?" I didn't know what he wanted so I played dumb and said, "Yes, sir, I know how to draw blood." I hadn't ever drawn blood in my life!

When you enter and are processed through the Army, you're bled several times. Any person with half a grain of intelligence could see what the process involved. So the Captain gave me a test tube rack filled with 10 test tubes with a needle attached to the

vacuum tube and ten names on the list. He said, "Go up to the hospital and draw blood from this list of recruits." I said, "Okay," and he explained to me how to draw the blood. In those days we used a device where you didn't need a separate syringe, at least for this procedure. The needle was attached to a rubber tube which was attached to a vacuum test tube and in between the needle and the tube there was a blockage by a glass tube. Once you get into the vein you just snap that glass tube through the rubber tubing and break the vacuum seal, and then the vacuum draws the blood.

I went up to the hospital [laughs], and to my surprise, one of the patients was a recruit whom I had been helping "to train" just a few days before. He was somewhat dubious watching me come in with this needle. And I was dubious, because I was shaking like a leaf, but at the end of the day, I came back with 10 tubes of blood, and the captain said, "Chin, how would you like to help us with this study?" I said, "Fine." So he made arrangements to transfer me from being an infantryman to a member of the Medical Service Corps. I learned a great deal about venipuncture in those days. We had to do hundreds and perhaps thousands of them. Whenever a recruit came in, we had to bleed him, and every time someone was sick, we bled him twice -- once when he was admitted to the hospital and once while he was convalescing.

After the epidemic was over, we all had the pleasure of shaking Dr. Salk's hand, which was, in those days, not a big deal, because nobody knew Dr. Salk. But remembering back now, I get sort of choked up with emotion about that. He thanked us, and we went on our way. Then I received orders to go to Walter Reed Army Medical Research Center as a medical technical assistant in the laboratory. There I was assigned to work with Dr. Donald McMullen, who was a famous parasitologist. He used to teach at the University of Oklahoma in parasitology and worked for a few years in Japan following World War II on a *Schistosoma japonica* project. Dr. McMullen assigned me to examine stool for intestinal parasites. In those days, the State Department had an agreement with Walter Reed that Walter Reed would examine the stools of all their returning foreign service officers after completion of overseas tours in underdeveloped countries. The test consisted of examining three stool samples on three consecutive days. I did the

examination and if any parasites were found, we referred them to the State Department for treatment.

It was a dull kind of job, routine, but I was fully capable of doing it. And Dr. McMullen was a real gentleman; I enjoyed working with him. The only thing that's memorable, I recall now, was examining specimens from this one secretary, spinster-type, in her early 50's, perhaps. The first two stool samples she submitted were just huge. I showed the first one to Dr. McMullen and his comment was, "She's got to have a mega-colon," because it was so big. Then we laughed. The second stool was also massive. She gave the third sample to me with a sheepish grin. I opened it and burst out laughing because it was a pea-sized piece of stool, and on a tissue paper she had written, "With apologies." I showed this to Dr. McMullen and we really had a great time! That was the most memorable incident I recall.

Leo Slater: Did you see any malaria while at Walter Reed?

Dr. William Chin: No. I was working more or less exclusively with Dr. McMullen on enteric parasites only. After about two or three months, they reassigned me. The Walter Reed people, in their infinite wisdom, sent me to the US Penitentiary in Atlanta, Georgia, where there was an NIH malaria study unit doing work on testing of various drugs, and I suspect in those days even some on monkey malaria. I'm not sure about that. So I drove down to Atlanta. I recall it was a Friday afternoon. I checked in at the unit, met the commanding officer, Dr. [Albert V.] Myatt. Dr. Myatt was surprised to see me, he said, "What are you doing here?" I said, "I have orders from Walter Reed to come down and report to you." He said, "But we're closing the project." So I said, "Well, I didn't know. I wasn't told anything about that." Dr. Myatt was very gracious: "This is not your problem. Why don't you find yourself someplace to live and stay in contact, but you don't have to come in here. I'll call if we need you." I said, "Fine." I found a room in a rooming house a block from the prison and essentially sat by the phone waiting. First day, nothing. Waited a week, nothing. Then I hopped in my car and went to the library and took out a whole stack of novels and other books, because in those days, believe it or not, there was no television. There was only a radio. So I came back and started reading and waiting

for that phone call. A month elapsed and still nothing. I just sat there and read I don't know how many books. Finally I went to Fort McPherson in Atlanta to get my paycheck after the first month. I collected my paycheck, went back to my boarding house and waited some more. Another month went by. I went back to Fort Mac and got my paycheck. This time I went to see somebody in the personnel office, some sergeant. I told him the problem: "I'm sitting in my boarding house, nothing to do. I'm reading, I'm not complaining, but I feel a little bit guilty not doing anything here and earning this money. So would you please contact Walter Reed and find out what's going on or get me back to Walter Reed?" He said, "I'll work on it." Still nothing happened, and the *third* month went by. I went to Fort Mac, collected my check, went to see the sergeant and I said, "What happened with contacting Walter Reed?," "Oh," he said, "your orders just came through a couple of days ago. I was going to get in touch with you, but I didn't know where you lived." I had received orders to go to San Juan, Puerto Rico.

I had to go back to Walter Reed and finish up doing what I was supposed to be doing, administratively. And I took a couple of days of leave, to say goodbye to my parents. Then I went by train, down to Mobile, Alabama where I was supposed to catch a plane from the local Air Force base to go to San Juan. I landed in Mobile, the temperature was way over 100 degrees; it was humid. In those days, segregation was a real problem, and I didn't know how I was going to be treated, but I was in my uniform sweating, soaked through. I decided to risk it, and I went to the movies, just bought a ticket and walked in very brazenly. It was delightful; it was cool. They were showing *Stalag 17*, a famous picture starring Bill Holden. I sat through the first run -- it was less than two hours, and I sat through it three times, because I didn't want to get out of that cool theater. By the time I finished the third round, I felt it would be cool enough to venture outside, and go to the airbase. I made it to the airbase and went to San Juan, Puerto Rico. There I was assigned to a barrack at Fort Brooke and told to report to the Army's Tropical Disease Research Laboratory, which was housed in a building right off the beach, down the road from Fort Brooke. It was a delightful place where you could take your lunch break inside a gazebo-type structure overlooking the ocean with the waves pounding beneath you. It was a completely idyllic and very picturesque setting.

The commanding officer of that installation was Lieutenant Colonel Bud Benenson. Colonel Benenson interviewed me, and I told him what I did: I worked with Dr. McMullen. So he assigned me to another Walter Reed project that was evaluating a new way of processing stool samples for microscopic examination. It's called the MIF technique and consisted of a solution inside a test tube. MIF: M stands for merthiolate, I for iodine, and F for formalin. It was a combination of three components, and the idea was very simple, very elegant. You take a peanut-sized stool sample and emulsify it in this solution. This accomplished several things: one, the formalin preserved whatever you were looking for -- the parasites and their eggs -- the iodine stained the nucleus of an organism like the *Amoeba histolytica*; and the merthiolate stained the cytoplasm of these animals. At the same time, because of the difference in density, the parasite, the eggs and so forth, tended to float to the upper layer of this solution, so all you had to do was pipette out a sample of the upper layer, put it on the slide, and you essentially had a concentrated, stained preparation of what you were looking for. It was a very neat procedure. I enjoyed participating in this field evaluation.

I did that for three or four months. When we had completed the study, I was assigned to help Dr. Jose Olivier Gonzalez who was a Puerto Rican parasitologist, noted for his research on schistosomiasis. In fact, he developed the serologic test to detect antibodies to schistosomes. It was a fairly simple test. *Schistosoma* eggs were harvested from the liver of infected mice and put on a slide, and then a drop or two of the serum from a patient was added, followed by overnight incubation. If the serum contained antibodies to this *Schistosoma*, there would be a reaction consisting of the formation of precipitates around the egg in the shape of globules: That was a positive test. If no globules, it was a negative test. Dr. Gonzalez assigned me to the tedious work of recovering eggs from his infected dead mice. It was foul-smelling work, but you got used to it. I did that for the duration of my tour in San Juan.

Dr. William Chin: I was in Puerto Rico for about a year. Dr. Gonzalez and I seldom communicated on a social basis, it was all business. But a couple of months before my enlistment was over, he took me aside and we had a talk. He asked me what I was going to do, what my aims were. I told him that it sounded like medical parasitology was a good field to go into. He

told me, “You’re going to go back to graduate school at Michigan...” I was thinking of that. He said, “If you have a chance, and you still want to do medical parasitology, why don’t you do it after an MD degree? The time it will take you to get either degree is about the same, and you will have a lot more freedom doing medical parasitology as a physician than as a PhD.” He felt personally bitter that here he was, the most prominent parasitologist in Puerto Rico, yet the chairman of his department had no special training in parasitology, but he did have an MD degree. He felt that he should pass that advice to a young fellow like me. I said, “Well, thanks very much, I appreciate it.”

A couple of weeks later, he was visited by a friend of his, a classmate as a matter of fact from the University of Chicago, Dr. Richard Porter. Dr. Porter -- I had taken a course in basic medical parasitology from him -- was teaching at the School of Public Health at Michigan. He was a parasitologist, who also had malaria research experience. I reminded him that I had been one of his students. He and Dr. Gonzalez both agreed that if I wanted to pursue medical parasitology, I should go back and apply for admission to medical school.

After discharge, I went back to Michigan, talked to my faculty advisor and basically he agreed with Drs. Porter and Gonzalez. He said, “If you’re not sure, why don’t you take a degree in Public Health, and see where you want to go from there.” So I enrolled in the School of Public Health. In those days, Michigan had passed the GI Bill of Rights, which granted Korean vets one year of college education. Ironically, even though I had not served in Korea, I was considered a “Korean vet.” Even more whimsical is the fact that service in Puerto Rico qualified me as Korean vet who had served “overseas.” I took advantage of the Michigan GI Bill and completed my MPH degree at Michigan. Most of my courses were concentrated on medical parasitology, and other laboratory courses such as bacteriology. I did very well, and I was elected to the Delta Omega Honorary Society in Public Health. With my MPH degree, and with the improvement in my grades, I decided that, maybe, medical school was not such a bad option -- at least I’d give it a try. I took the next year off to work as a technician at the microbiology lab at the University Hospital, and in the meantime, applied to the School of Medicine at Michigan. To my pleasure and surprise, I was accepted.

I was then an old 28-year-old freshman medical student, whereas my classmates were 21, 22 at the most. I started medical school and utilized the Federal Government's GI Bill, which provided four years of college education. The GI Bill was very substantial in terms of its payment, it paid for the tuition, and it paid for all the textbooks and all other fees. It also gave a very livable stipend for living -- \$600 a month, which for a young person, was more than adequate. But at the time I was married, so \$600 wasn't enough to support two people. I asked my wife -- my first wife [Susan] -- who had worked as a secretary and administrative assistant before we were married, whether or not she would help me out by going back to work so that we wouldn't have to struggle financially. She looked at me as though to say, "Are you crazy? If I had wanted to work, why would I have married you?" And she confirmed that attitude by telling me practically just that. So I had no choice but to go and see my former colleagues at the microbiology lab and ask, "Is there anything I can do to earn some money?" They said, "Well, we are looking for an on-call technician to work after-hours and weekends." After the lab closed at 5:00 pm and before they opened at 8:00 am, whenever a specimen would come in, somebody had to process it, so they needed some on-call person to do that. I said, "Okay, I'll give it a try." Which meant that oftentimes, my studies at night would be interrupted by a phone call to come in to process a sample, and worse for me, during football games on Saturday. You are watching a football game at the stadium and over the loudspeaker would come the call: "Mr. Chin, call the hospital operator." But it meant a half-time job. The salary wasn't bad for being on-call.

My boss was Dr. Burgess Vial. He was a surgeon but was also designated as Chief of that microbiology lab. He saw that I was having a tough time. Probably, I looked exhausted sometimes. After two years, he gave me a little bit of respite by hiring somebody to replace me on alternate weekends on call. So I had, from the third and fourth year, every other weekend relatively free, which was just great. But during that first year, it was very difficult. Within the first month, I realized that with keeping the job and going to school full time, I wasn't going to pass all of my courses. I stood a good chance of flunking out. I made a deliberate decision: I would pretty much downgrade the course I was having the most problem with -- which was biochemistry -- and give that

rather low priority in terms of studies, and concentrate on passing the others. It turned out just that way. I failed biochemistry the first year, but I passed all the other courses, and received an A in microbiology of all things, but given my training, any other grade would have been a major disappointment. It balanced out, and I was able to repeat the biochemistry in the summer. Completing that one course was no problem. I finished the rest of the four years with essentially little or no problem, other than being kept busy all the time. During the summer vacation after my junior year, I was working on three different jobs: continuing with the on-call at the lab; working as a field assistant for the School of Public Health as part of an epidemiologic evaluation in Milan, Michigan, to determine the source of a histoplasmosis outbreak; and I was awarded a grant to study the prevalence of staphylococcal contamination of operating rooms.

Toward the end of my senior year, I visited several hospitals -- one in Connecticut I recall very vividly -- to find a place to intern. (It was not the hospital in Connecticut itself, but the freebies given to our senior class from a drug company to visit New York City, which were memorable.) Everywhere I went or asked about, the pay was so dismal, I just couldn't afford any of those internships. I decided, "Okay, since I have a degree in public health, I'll join the Public Health Service as an intern." And I did. When I was accepted, I began collecting the salary of a second lieutenant, or "Ensign," in the Navy, and the question then became, where should I intern? At which Public Health Service hospital? I was given choices to list. I knew from my two years in the Army that the Federal Government works in mysterious ways: I wanted to intern in San Francisco. I liked the Bay Area very much. But I sensed that if I put San Francisco as my first choice, I would never get it. So I put New York as my first choice, because I was not fond of New York. I didn't want to go back to Brooklyn or New York. I put New York as my first choice, and San Francisco as my second. Lo and behold, San Francisco won out. I was asked to report to a Public Health Service hospital in San Francisco, where I did my year of internship.

Leo Slater:

Can we backtrack for a moment to Michigan? What kind of work did you do in the microbiology lab?

Dr. William Chin: When I worked there full time that year between public health school and medical school, I was involved with several activities. I was in charge of all the TB [tuberculosis] staining and culturing, and I did some blood culture work, some stool-parasite examination, but most of my work was tuberculosis. In those days, tuberculosis was a major problem. I also stained blood samples to demonstrate L.E. (Lupus erythematosus) bodies.

Leo Slater: You chose a Public Health Service internship. How do you think that was different from another type of internship?

Dr. William Chin: The public health service internship was generally a bit light on clinical training, but that wasn't my concern. I knew I didn't want to get into clinical work. I knew what I wanted to do was medical parasitology in some capacity. My primary determining factor was that you can live on a second lieutenant's pay, whereas you can't live on 50 dollars, or whatever the other hospitals are paying you. That was the primary concern.

Leo Slater: Drs. Gonzales and Porter thought an MD degree would be better than a PhD for your career, were they right about that?

Dr. William Chin: I can only reflect some of Dr. Gonzales' thinking. He felt that with an MD degree, you have more flexibility in doing what you want to do -- with a PhD degree, you're pretty much strapped to teaching or research on a particular parasite. But with a medical degree, you can have the whole, full range of public health -- research, disease control, epidemiology, and even administrative medicine -- open to you, and still do what you want to do in medical parasitology. I think the versatility of the MD approach is really what appealed to me.

Leo Slater: I get the impression that you weren't very focused early in your career about what you wanted to do, but you didn't become a minister. What led you to science?

Dr. William Chin: I think because I've always had some interest in science, and I always had the feeling that if I couldn't award my father with becoming a minister of his church, I needed some

other way of serving, I suppose, to make him proud of me. Some sort of service, such as public health, had always been part of my thinking. Then when I found out about the parasitic diseases, their high prevalence and incidence in the world, I suppose I felt it was one field where I could contribute something to the greater good. There was nothing definite about how I went into this field, it was just all these little things that sort of guided me toward the final decision.

Leo Slater: You did your internship in San Francisco, and then you signed up for the Peace Corps?

Dr. William Chin: I didn't sign up for it; I was dragged into it! [laughter] A little more than six months into my internship, I was wondering what I would like to do as my next assignment. By then, I think the idea of medical parasitology was foremost in my thinking. So I start asking around, and I was told by colleagues and friends, "Well, maybe you should contact the NIH. They are still doing research on malaria." I found out that there was a Dr. G. Robert Coatney in charge of this project in Bethesda. I think I contacted Dr. Coatney, I called him asking about working with his group in any capacity. I gave him a brief thumbnail sketch of my vital data: I was a Michigan graduate, I had a family by that time, I had a young son, and I was interested in parasitology, particularly malaria. And I had taken courses with Richard Porter at Michigan. He said, "Oh, Dick Porter, yes!" And he said, "Yes, there's a possibility." Then he called me back, I think, a few days or maybe a few weeks later, and said, "We have a field station in Kuala Lumpur doing research on monkey malaria. Unfortunately -- since you have a family -- we have no living quarters at that institution, but if something else comes up, I'll let you know." I said, "Okay."

Coincidentally, my brother, Jim Chin, was also looking for an assignment overseas. He was five years younger than I was, but whereas I graduated from medical school at age 32, he graduated from medical school at age 24, so there was a big difference. I didn't know this at the time, but apparently he, too, called Dr. Coatney, a few months after I had. And Dr. Coatney said, "Chin. You're from Michigan, right?" My brother said, "Yup." Because he got his bachelor's from Michigan. "And you have a young child, right?" "Yup." My brother's son is two weeks older than my son. Then Dr. Coatney

told my brother, “Didn’t one of my people tell you that there was no living space for a family in the lab in Kuala Lumpur.” He said, “No.” Dr. Coatney said, “How would you like to work for me at the penitentiary in Atlanta, Georgia?” My brother said, “No, thank you.” And he forgot about it. But subsequently, my brother did find his way to Kuala Lumpur; he was granted a two-year fellowship from the Hooper Foundation to support his field research in Kuala Lumpur. I believe he was working on various aspects of infectious disease under the auspices of the Institute for Medical Research in Kuala Lumpur.

There was an almost a comical confusion on Dr. Coatney’s part between my brother Jim and myself. When one of us called, he didn’t know who was calling. This confusion lasted for quite a while until even I was confused. (Yesterday, as a matter of fact, I called brother Jim to see what he recalled of his conversations with Dr. Coatney in those days.)

I finished my internship and decided to take two weeks annual leave. I took the family to visit Disneyland. When I came back and checked in at the hospital to see if there were any messages -- I was concerned about my assignment -- there was a message: “Call Dr. Leo Gehrig, Peace Corps, Washington.” Dr. Gehrig was the Chief of the Medical Division of Peace Corps. I called Dr. Gehrig and he said, “Chin, how would you like to go to Ghana? We need a medical officer to accompany Peace Corps volunteers.” I said, “Well, do you have any other places I can go besides Ghana?” He said, “No, because you’re the most qualified candidate for this first project,” because, I suppose, of my relative age and so-called maturity and my medical parasitology background. There was no way of talking him out of it, even though I really wanted to go to Kuala Lumpur, Malaysia. My brother, by then, was in Kuala Lumpur, and I knew that the Peace Corps was training people to go to Malaysia. But Dr. Gehrig said, “No, we’ve selected you for Ghana.” So I said, “Okay.” That was how I wound up as a Peace Corps physician in Ghana. It was really more by default than actively seeking the position.

Leo Slater:

What sort of job was that?

Dr. William Chin: It was a public health job. All the officers of the Peace Corps Medical Division were Public Health Service officers, including Dr. Gehrig. The job was very simple. What you did was accompany these young, healthy Americans overseas, and to visit them, at least in my Ghana project, every three or four months. When they come into Accra, the capital, I would see them, talk to them, and find out if there were any problems. My primary responsibility was to practice preventive medicine: make sure that their immunization was up to date; make sure that they knew where to go in case they've had illness; make sure that they located either a government hospital or a private, mostly missionary-type, hospital, way out in the bush; and make sure that they practiced preventive measures against snake bites, against malaria – particularly keeping up with their prophylaxis. It was an easy job. It entailed a lot of travel, which was great for me because I got to see a lot of Ghana and also put me in contact with a British pathologist, Robert Kay, who became a very good friend. We became friendly because of our shared interest in malaria.

Bob Kay was a British Army officer seconded to the Ghanaian Army Medical Corps and worked in the military Army hospital in Accra. He was seeing all these dependents of Ghanaian Army personnel, particularly young children, dying of fever. He knew that most were probably dying of malaria, but he wanted to confirm it by doing autopsies, so I had an interest, too. I proposed that I would help train his technicians in malaria microscopy and basic malariology, and in return would he let me watch some of his post-mortems. He said, "Yes." That was how we developed our friendship. I saw firsthand the pathology of fatal malaria -- mostly of the cerebral type -- something I had read about but never seen. That was very dramatic.

After Bob Kay left Ghana, he left the Army and took a civilian job, I believe, in a hospital in Brunei or some such place. We lost contact shortly after that. I did hear from Bob one more time: Following President Kennedy's assassination in 1963, I received a very touching letter from him. He sent his condolences on behalf of mutual friends -- British friends -- on Kennedy's death. I thought that was a really great gesture on his part, since we were no longer communicating.

Leo Slater: The Peace Corps was new at that time?

Dr. William Chin: It was brand new. It was established by President Kennedy in 1960, as a matter of fact. And coincidentally, I think he proposed that project while speaking at the University of Michigan. So there's a lot of Michigan connections here. Ghana was the first project, and the volunteers' job was to staff the new secondary schools that the Ghanaian government had decided to build in order to bring up the level of education of Ghanaian society. They created a dozen or more of these brand new secondary school campuses all over the country, but they didn't have the teachers. So when the Peace Corps came, hey, here is a great opportunity! Our first group, I think, had 51 volunteers assigned as teachers in these secondary schools.

Leo Slater: Can you add something about getting to meet President Kennedy?

Dr. William Chin: Prior to our departure for Ghana, President Kennedy was gracious enough to arrange a reception in his office and at the rose garden of the volunteers and staff. He gave a ten minute extemporaneous talk on the history of Ghana and the importance of the Peace Corps' role in Ghanaian education. We were then ushered into the oval office where he shook hands with each of -- with a photographer recording each hand shake. If nothing else, having a photo shaking hands with President Kennedy was worth the two-year tour in Ghana.

Leo Slater: How did you get from the Peace Corps to Atlanta? Did you get back in touch with Dr. Coatney?

Dr. William Chin: Not quite. I had not heard anything from Dr. Coatney, so I assumed he wasn't interested. A little more than a year into my Peace Corps assignment, I knew that I wanted to stay overseas and continue working and learning. I also knew that at that time there was a cholera research laboratory in Dhaka, Bangladesh. It was part of a SEATO¹-sponsored

¹ Southeast Asian Treaty Organization.

activity and headed by, of all people, Colonel Bud Benenson, my boss from my army days in Puerto Rico.

I wrote Colonel Benenson that I had finished my first year was going into the second year of my assignment as a Peace Corps physician in Ghana. In the second year, I was also Peace Corps physician to volunteers in Togo, which was a next door neighbor. I was very keenly interested in staying overseas and working on some of these tropical disease problems. And I wrote, "If you have a position, I would even consider working in cholera."

I sent the letter out, and there was never a response. (A few years later, I met Dr. Benenson at a medical conference, and I asked him why he didn't respond to my letter seeking a position. He simply said "I never received your letter." Isn't there an expression "for want of a nail, a kingdom was lost...?") As the second year was coming to a close, I was getting desperate because I had no assignment. I didn't want Public Health Service to assign me to the Indian reservations, or wherever they needed somebody. So out of desperation I wrote Dr. Coatney again. I asked him if there was any opportunity, as more than a year had elapsed. I received a very prompt response from him. In those days when we were in Ghana -- even though we were Peace Corps and were not supposed to be connected with the embassy -- all the mail went back and forth to the States by diplomatic pouch. It was rapid and it was very secure.

I think at this point that Dr. Coatney still didn't know whether I was Jim Chin or William Chin. But he said, "Yes, we have a need of a medical officer because we want to assign that person to Pakistan. We have a new drug -- very exciting -- that we're developing. If you would like to take this assignment, I can bring you back to Atlanta. You can work in the prison for a few months, and we can teach you as much as we can about clinical malaria and you can also learn about this new drug. Then you go to Pakistan and help with the field trial of this drug." I wrote back: "Great. That sounds exactly like what I wanted to do." I accepted the assignment and that's how I wound up in the penitentiary again, somewhat ironically some eight years after I was told by Dr. Myatt, "Oh, we don't need you to come, the project is closing."

Leo Slater: Did you learn anything there about the history of the program?

Dr. William Chin: Not really because I had minimal contact with Dr. Coatney because my sole purpose there was to study under Dr. Pete Contacos, and at that time there was another fellow, Joe Lunn, who was helping Pete Contacos with some of the studies. It was a foregone conclusion that as soon as they had the study in Pakistan established, I was going to go there.

I worked with Dr. Contacos for a few months. (I was living in the outskirts of Atlanta in Chamblee and the penitentiary was maybe 10 miles away.) Dr. Contacos then left for Pakistan to locate a trial site. He started in Lahore, Pakistan, and he worked with a group of people from the University of Maryland. They had a research group in Lahore. The ICMRT -- International Center for Medical Research and Training² -- he worked out of there. They were helping him locate a research site and helping him with technical assistance.

Pete went out and started searching for cases. The drug we were working on was the famous 'CI-501,' that was the code name. It was nothing more than an old, British drug, Paludrine or chlorguanide -- with cycloguanil pamoate as its active metabolite -- but formulated in a different way. Instead of an oral preparation, this was formulated with peanut oil, forming a viscous mixture. The idea was to inject this mixture into the gluteal muscle, where it would remain as a glob inside the muscle. Then being held by the peanut oil, the drug would slowly seep out and would protect a person against malaria for up to a year after just one shot. I mean it was great. This was during the height of the malaria eradication era, and they certainly could have used something like that. One shot for a year.

² From the University of Maryland, Baltimore, website (<http://medschool.umaryland.edu/Epidemiology/ihdiv.html>): "The International Health Division in the Department of Epidemiology and Preventive Medicine was established in the 1950s at UMB as the International Health Program, with overseas research facilities (International Centers for Medical Research and Training; ICMRT) in Pakistan and Brazil and seminal investigations on malaria chemotherapy and vaccine development in Baltimore. It became a division in the Department of Epidemiology and Preventive Medicine in 2000."

Pete started looking for cases to study. After he found the cases, he started treating them with Paludrine (chlorguanide). Chlorguanide was a 7-day treatment, very cumbersome. It doesn't work well in the field from an operational standpoint because of this 7-day treatment. But disappointingly the first thing Pete found was resistance to this drug on the part of the parasite. It was disappointing, particularly because they had focused on this area since this drug had not been used there for years and years, but somebody had forgotten to tell them that a related drug, pyrimethamine, was used more recently.

Pete was frustrated and disappointed. He made a few more treatment observations and everything was a total failure. To make certain, he sent the blood back to Atlanta to test the parasites for Paludrine resistance in prisoner volunteers. We treated the cases with Paludrine (chlorguanide), and watched under clinical control conditions, and lo and behold, they were indeed resistant. That spelled bust to our Pakistan project.

So there I was no place to go again. But then Pete, after he came back, decided that he had had enough of the prison life. He transferred to the other NIH field unit, which was located in Chamblee, GA. In Chamblee, they were working with monkey malaria and rearing various species of mosquitoes for transmission work. Pete became chief of that unit and left Joe Lunn as chief of the prison unit. Then Joe left a few months later: he had completed his two-year tour with public health service. So Dr. Coatney asked if I would take it over, and I said yes. I became chief of the prison unit. That was in 1965.

Leo Slater: Can you tell me any more about Joe Lunn or Pete Contacos? What they were like to work with? Their backgrounds and so on?

Dr. William Chin: Joe Lunn was an internist when he joined the USPHS to fulfill his 2 years military commitment during the beginning of the Vietnam War. He was pleasant and friendly and acted as a very patient mentor during the early months of my assignment at the Malaria Project. We did not work together for that long because he left the project a few months after I arrived and went back, I believe, to his alma mater, University of Syracuse School of Medicine.

Pete Contacos and I became close friends. One reason was that we lived one block apart from one another. Another reason may have been that Pete's roommate as a medical student was another Chinese fellow from Hawaii. Pete is a Harvard grad who then went to Tulane for his combined PhD (in parasitology) and MD degrees. When he retired as the officer in charge of the prison project, we commemorated his retirement with a ceremony and invited Dr. Coatney to come to Chamblee to join us. Months before, I had arranged with a couple of exceptional inmates who were working as nursing assistants in the malaria ward to create two items as farewell presents to be presented to Pete by Dr. Coatney. One was an oil painting on canvas of Pete's two dearly loved pets, Lhasa Apso dogs of Tibetan origin with the Himalayan Mountains looming in the background. The other was an impressive medal of about 3 inches in diameter and some half an inch thick depicting Pete's profile on one side and the other side the name of the medal boldly engraved, "SOCIETY OF BIOLOGISTS" on the top along with notation of Pete's tenure of service at the prison. The presents were a surprise to both Pete and Dr. Coatney and to this day, I am still mystified by Dr. Coatney's keen perception when he presented the medal by saying "so this is an SOB award" because to my knowledge, no one at the Chamblee facility had seen the medal prior to the presentation. Dr. Coatney was so impressed by the medal that he took it back to Bethesda to have it encased in a thick block of plastic. Unfortunately, a few years later, this SOB medal was stolen from Pete's house along with other valuable items during a burglary. Of interest, it should be noted that the engraver of this medal was a second generation of his family to be incarcerated at a U.S. penitentiary because they turned their exceptional skill to an illegal enterprise by the engraving of dollar bills. In 1967, it was Pete's recommendation on my behalf to Dr. Bob Kaiser, Chief of the Malaria Eradication Program, CDC, that resulted in my being named as Chief, TMORU/USAID, CDC (Thailand Malaria Operational Research Unit) in Bangkok. Pete's passion was orchids. He cultivated them and aspired to be a judge at regional orchid shows. After his retirement from the PHS in the late 1970s, he moved to Fort Lauderdale, and we have not communicated with one another since.

Leo Slater:

While you were in Atlanta, did you collaborate, beyond Chamblee, with the Southeast Asia group in Kuala Lumpur, or with any of the other NIH units?

Dr. William Chin: No, but we did make available the opportunity for Dr. Mae Melvin -- parasitologist in charge of training in malaria microscopy at CDC -- to come to the Malaria Unit to make hundreds of teaching blood smears [slides] whenever we had parasites of special interest, including *Plasmodium ovale* and *Plasmodium malariae*. All other collaboration or coordination was done from Bethesda through Dr. Coatney's office.

Leo Slater: How was it, working for Dr. Coatney?

Dr. William Chin: He was an interesting person. When I had first met him and was talking with the various colleagues, I was told that he was a very stern disciplinarian. There was one method of disciplining officers that received notoriety at the time. If he was really displeased with you, he would transfer you to work as a medical officer on board a Coast Guard cutter in the Arctic Ocean, accompanying the Geodetic Survey people, so the stories go. It was essentially a six-month cruise in the Arctic Ocean, so he would exile you to that assignment. But to my knowledge, after knowing him I don't think he ever carried out this dire reassignment of his personnel. The closest he came was when one of my officers made a breach of etiquette in government protocol. He was in one of these two-year draftee positions, and he felt that I was derelict in my duty as commanding officer and was not telling the volunteers the whole story about the risk they might face. Instead of writing a letter of complaint, showing it to me, and having me to send it to Dr. Coatney -- which is the usual way you handle these complaints -- he addressed it to the Surgeon General of the United States Public Health Service. I got a phone call from Dr. Coatney, "What's this about? I get this letter from the Surgeon General written by," well I won't give his name but his initials are H.K., "written by H.K. complaining about you." I told Dr. Coatney that it was not true because when we explained the procedure to the volunteers, we had a cardboard with the information printed on it in front of us. It was almost like the police pulling out their Miranda card and reading the same thing over again. The volunteers all knew that one of the risks was death. You can die of malaria.

Dr. Coatney was infuriated, he said, "I'm going send him to the Arctic Patrol...." I said, "Calm down, Dr. Coatney. This guy has a family with two children, and while this might please you to send him up there, his family's going to suffer...." So he relented and

reassigned H.K. to an assignment almost as bad, as medical officer to one of the Federal mental institutions. He was exiled to a mental institution for the rest of his service.

Leo Slater: How big a unit did you have at the penitentiary?

Dr. William Chin: We had a separate "Malaria Ward" housed in the basement of the hospital. It was a sizable ward. I suppose we could have put in 20 beds or more in it. There were the officers, and we were assisted by a group of retired naval personnel. These were retired Naval Medical Corpsmen, they knew a great deal about first aid and nursing care. Then we trained them on malaria techniques: taking blood smears, staining, and even microscopy. We depended on these ex-Navy people, I believe their title was MTAs (Medical Technical Assistants). In addition, we had a few nursing assistants from the inmate population. When you get into a prison like this penitentiary as an inmate, you have to do some sort of work -- you're assigned a job. If you have no preference or no skills, you're assigned to the factory where they produce first the canvas and then make the bags for the post office. All the mailbags for the US Postal Service were made in Atlanta at the time. Some of them didn't like that work, so if a job was offered as a nursing assistant they would take that.

Leo Slater: And if you were a volunteer in the research, you got out of your job while you were doing that?

Dr. William Chin: Yes, but only when they became infected and we had to bring them into ward as in-patients. They volunteered because there were certain benefits that we explained to them. The first benefit, and it was important to many of them, was that they get fifty dollars right away. Many of these people coming in had nothing -- were just flat broke. If you're an inmate, you need money to buy cigarettes, candy, other items. They had a commissary where you could make the purchases. But you can only do that if you have money. So that was the first thing, the fifty dollars.

The second thing we would tell them was that, if they volunteer, they would get, I believe, one month off of their sentence for every month they participated in the project,

something like that. I cannot recall the exact award. But another benefit, probably the most important, was that we would write letters on their behalf to other jurisdictions holding warrants against them. Once they get out of this federal prison, some of them would face arrests on state charges, and we would write letters saying they volunteered for this humanitarian service and had sacrificed their health to the benefit of medical science. Oftentimes the state would just ignore their charges or future sentence and give them a free ride when they got out. So that was an extremely valuable incentive for them. But we also explained to them that we would inoculate them with malaria, they would get sick, we would hospitalize them, and worst comes to worst, they could die of malaria -- it was a routine speech we'd set up. They were then given a physical examination and if found healthy, then they volunteered.

Leo Slater: You were largely screening drugs at this point?

Dr. William Chin: The work we were doing was two-fold. One, we were screening parasites for their response to various antimalarial drugs. When I first went there, most of the parasites were sensitive to chloroquine, so there was a minimum of drug research. The other part was to try to infect volunteers with various monkey malarias. This was a subject of Dr. Coatney's keenest interest -- and not just academic interest. He was the one -- probably more than anyone else -- who made monkey malaria a very prominent subject in terms of malariology. He and his colleagues probably found and identified more new species of monkey malaria than any other group.³ The next question was would these monkey malarias infect man? This was during the malaria eradication era. Dr. Coatney was concerned we (the World Health Organization's malaria eradication program) could not be successful in eradicating malaria, if monkey malaria could make the jump into man and take over? If eradication of human malaria could be done, this would present an entirely different problem. So that was his focus. We had these two areas: drug evaluation and monkey malaria.

³ See, for example, G. Robert Coatney, William E. Collins, McWilson Warren, and Peter G. Contacos, *The Primate Malarias* (Washington, DC: US Government Printing Office, 1971), also available online at http://www.dpd.cdc.gov/DPDx/HTML/Search_Choices.htm.

Later on as we received more and more of the resistant parasites, we studied the treatment of resistant parasites. At the same time, this was now in the mid 1960's; Vietnam was looming in the background. During previous conflicts, there was a dormant organization, a commission on malaria with members from the Public Health Service, Army and Navy principally. The Services would send representatives to the commission who would organize research subjects -- and even assign research topics -- and prioritize what was needed. As with previous conflicts when the Services organized a malaria commission, so it was with Vietnam, particularly because of the problem of drug resistant malaria. To my knowledge, the Malaria Commission was formalized during the Vietnam War. As to the details of its composition and who organized it, I am not aware but do know that Dr. Coatney was an active member. When drug resistance became a problem particularly during the Vietnam conflict, we were assigned to work on sulfonamide as an alternative treatment. The Navy was assigned something else and the Army something else. Our major concern became sulfonamides.

Leo Slater: The Vietnam War did influence the work you did?

Dr. William Chin: Definitely, definitely.

Leo Slater: Did you have at the penitentiary all the biological materials needed, insectaries, that sort of thing?

Dr. William Chin: No, the insectary was based in Chamblee. Whenever there was a need to infect mosquitoes by feeding on an infected volunteer or to feed infected mosquitoes on a volunteer, I or one of my officers would stop in Chamblee and bring the mosquitoes into the penitentiary. Once it was done, we shipped them back to Chamblee. In Chamblee, they also maintained a monkey colony.

Leo Slater: Were you also maintaining malaria strains in patients at the penitentiary?

Dr. William Chin: We inoculated patients with various malarias, and periodically we would take blood samples, freezing and storing them in our Revco freezers in the malarial ward.

Leo Slater: How many officers did you have there?

Dr. William Chin: Generally speaking there were two besides myself.

Leo Slater: What else can you tell me about the malaria ward?

Dr. William Chin: The officers and the MTAs assigned to work in the malaria ward wore uniforms identical to those worn by naval personnel except for different insignias. To reach the malaria ward, one had to go through two securely guarded steel gates. The atmosphere of working inside a maximal Federal penitentiary was fairly relaxed. The guards seldom bothered us except on rare occasions such as during a sweep of the entire prison searching for contrabands particularly “home made” weapons. One time, they thoroughly searched the ward because of an alcoholic aroma emanating from the lower level of the hospital. After several hours, they found a vat fermenting alcohol on the floor of the elevator shaft located just outside of the malaria ward.

One evening, I was called to return to the prison because one of the volunteer’s parasite count was high. A fundamental rule drummed into all of us by Dr. Coatney was that when dealing with *falciparum* infections, the volunteer’s malaria must be monitored every 8 hours by the examination of a blood smear and if the count is near or over 5,000 parasites/mm³ of blood, then treatment has to be started stat. When I was coming into the prison, there was a great deal of activity with many more guards than usual during those hours of the day. It turned out that all the commotion was over the arrival of a notorious bank robber with a reputation for breaking out of prison with ease, the one and only Jesse James Robert. Sure enough, some 2-3 months after he arrived, he escaped one foggy morning by climbing over a very high wall using a ladder he began fabricating probably the very next day after his arrival out of cut up water pipes.

Another absolute prohibition imposed by Dr. Coatney is that no officer should use himself as a study subject. The reason for this edict is that “if you are ill, who will take care of the volunteer inpatients?” I will admit now that I breached that rule on one

occasion as we were trying to infect volunteers by feeding mosquitoes infected with *Plasmodium knowlesi*. We were down to the last volunteer and there was one mosquito left. Tried as I knew how, I was not able to coax the mosquito to make even a probe of the volunteer's arm. I didn't want to give up so I transferred the caged mosquito onto my arm and within seconds, she jabbed her proboscis into my arm for just a few seconds but like the other volunteers, I came down with a *knowlesi* infection. To continue the conventional use of initials to identify the volunteers but more importantly to avoid any possible reprisal, I became simply "W.C." in the report of this study published in 1968.

During my tenure of some four years assigned to the prison, we were fortunate not to have encountered more serious medical complications in the in-mate volunteers while infected with malaria than the two cases of spontaneous rupture of the spleen experienced by two volunteers infected with *vivax* malaria. Due to the swift intervention of the hospital surgeons, both volunteers recovered uneventfully after undergoing splenectomy. Another problem we faced during the last 6 months of my stay at the prison was the siphoning of potential volunteers by another newly authorized volunteer project, the evaluation of treatment modalities against gonorrhea, sponsored by the CDC's STD (sexually transmitted diseases) unit. Because gonorrhea is a well-known infection in the prison population, volunteers perceived this disease with far less threat and apprehension than against malaria causing a dramatic decrease in the recruitment of volunteers for malaria studies.

After I left the prison to go to Thailand, I learned that several volunteers became infected with a viral hepatitis and filed suit against the NIH claiming their hepatitis was caused by the transfer of blood from one volunteer to another during attempts to induce malaria infection by blood inoculation. Also at about the same time, there was mounting political pressure, particularly from members of congress who argued that from a legal standpoint, an inmate of a prison, because his incarceration is against his will, is therefore not capable of volunteering of his own free will for whatever purpose including medical studies. The conjunction of these elements finally caused the demise of the NIH malaria program at the U.S. Penitentiary in Atlanta and also similar programs sponsored by the U.S. Army at state prisons as well.

Leo Slater: Before we move on to Thailand, I just want to ask about Bill Collins. Did you meet him while you were in Atlanta?

Dr. William Chin: Oh yes. I worked intimately with Bill because he was the one in Chamblee who was rearing the mosquitoes for us. He remains there to this day; he's still working. He and I were good friends from the get-go. We got along very well. Bill was the only one of my former colleagues who visited us and stayed here as a guest. I don't know if you know anything about Bill, but he's like John Madden, afraid to fly, so...

Leo Slater: He has to take the bus like Madden?

Dr. William Chin: He has to "take the bus." For his trip here, he drove his Cadillac [laughs] all the way from Atlanta and then drove back. He would fly to medical meetings very reluctantly and was very anxious about flying.

In some ways -- particularly his dietary habit -- Bill is rather provincial. One time we were at a medical meeting in San Juan, PR. At a restaurant while we were looking at the menu, Bill saw an item "sauté squid in its own ink" and promptly said, "Who would want to eat that?" Of course to increase his obvious distaste, I ordered that dish and enjoyed it immensely -- to Bill's utter disgust.

He taught me the basics in the handling and dissection of mosquitoes. In return, a few years later when I was using the *in vitro* method to cultivate malaria parasites, I was able to reciprocate by supplying him with parasite antigens for his work on immunization of monkeys.

Leo Slater: Why did you leave Atlanta for Thailand?

Dr. William Chin: In 1967, the primary responsibility for U.S. participation in the malaria eradication program was shifted officially from USAID to CDC. Previous to that, USAID was the

agency responsible for the collaboration with the WHO on malaria eradication.⁴ Bob Kaiser was the director of the Malaria Eradication program at the CDC. He realized that the old method, USAID's method, of operating was totally insufficient. What USAID did was to recruit people from various disciplines -- particularly sanitarians and entomologists -- give them a several month course on malaria and then send them out as malaria advisors. Bob Kaiser's approach was to recruit young physicians, train them as epidemiologists, first of all; then give them additional training as malariologists; and then send these people out with more training and more technical capability than the previous group. In those days, I believe, we had something like 18 foreign countries where the U.S. was supporting the malaria eradication effort through donations of foreign aid. Each one of these nations was assigned one of the newly trained epidemiologists.

In 1967, I started working with Bob Kaiser, first as a trainer for these new epidemiologists. I gave the course on clinical malaria, as well as treatment and epidemiology. He brought in others to lecture on mosquitoes and other aspects. We organized and trained as collaboration between NIH and CDC, and Bob wanted to conduct field research in the operational aspects. In other words, he wanted to take a very close look at some of these operations in countries receiving USAID support and see what the technical problems were how to resolve them, and whether or not they were impeding the possibility of eradication.

He organized a group to go out to Thailand for an initial assault. After discussing with Pete Contacos and G. Robert Coatney, he agreed that I was a suitable candidate to head this operation in Thailand, since the major effort was to evaluate the problem of drug resistant *falciparum* malaria. In the middle of 1967, I was transferred from the NIH to CDC and took the job in Thailand for the next five years.

Dr. William Chin: My official title was Chief of the Thailand Malaria Operational Research Unit, or TMORU. In some ways, it was a very exhilarating experience. In other ways it was totally frustrating, because I arrived in Bangkok and was confronted with a USAID

⁴ USAID = United States Agency for International Development, CDC = Center for Disease Control, WHO = World Health Organization.

director who didn't want me there. The reason he objected was because USAID had been supporting the malaria eradication program in Thailand for a number of years. He thought that -- since the economy of Thailand was picking up -- the Thais should also pick up the costs of the program, and USAID should phase out its support. He felt it was inconsistent to phase out the support of the eradication program and at the same time support the effort to do research. He didn't feel that the two objectives were compatible. He delayed full implementation of the project for something like three years.

For three years, I ran around Thailand using one vehicle donated to me by the malaria program, using personnel who worked for the Minister of Health, and with limited funds in terms of support for per diem. I spent half my time just arguing with USAID/Thailand over the need to implement the project agreement between the CDC-USAID and the malaria program of Thailand. In fact at one time, I think the whole issue went up to the White House because I had a call from Bob Kaiser. This was, I guess, after two years of this stalemate. Bob said: "One of the CDC people is passing through Thailand, and there's a two hour layover in Bangkok but he's going to be at the airport, so please go meet him at the airport, and he will update you on what's the latest." I met him, and he was telling me of CDC's efforts through the White House to force USAID to extend full cooperation, but it didn't work out.

Finally after three years, USAID finally signed an agreement with Thailand. I was able to get two entomologists and an epidemiologist to be part of my team. One of the entomologists was Dr. John Kliever and the other was an entomology technician, Carl Vickery. The epidemiologist was a newly recruited EIS [Epidemiologic Intelligence Service] officer, Mike Bear. None of the three new additions to the TMORU staff had prior experience in malaria research.

We worked two more years. And even though USAID no longer had the mandate to run the project, they still had control of the budget -- they were the ones who authorized the funding for this program through the CDC. Toward the end of the fifth year, USAID sent out an evaluation team. We were criticized mostly because of the lack of sufficient accomplishment. So the project was closed after five years. But during those five years,

most of what I was able to publish was from a few drug trials that we conducted with severe limitations in resources and in time.

Leo Slater: Your primary research was drug trials?

Dr. William Chin: Yes. We had hoped to go into the mosquito aspect as well because the major weapon of the eradication strategy was house spraying with insecticides. In fact, that was the sole method used in Thailand. I was convinced that it wasn't working in Thailand because many of the mosquitoes were outdoor feeders so what good was spraying the interiors of houses? We needed to document this. Without an adequate crew, we couldn't do that.

Leo Slater: When that shut down, you went to the Central America Research Station in El Salvador?

Dr. William Chin: Yes. In 1972, I was recalled back to CDC and Bob Kaiser didn't know what to do with me because I wanted to remain involved with malaria and he couldn't see any possibility of another similar assignment for me. He sent me to a CDC affiliate station in Phoenix, Arizona, to see if I might be interested in working on the problem of toxicology of insecticides. I went and I looked and I told Bob I wasn't interested. Then he sent me to San Juan, Puerto Rican again, see if I was interested in the CDC study of dengue fever. I told Bob, "No, dengue fever is not my thing." So he said, "Well, okay. We have a research station in El Salvador, but they're not really doing malaria research so much as insecticide and mosquito-related problems."

Dr. Kaiser had established dual research stations, one in El Salvador and one in Thailand. The primary objective of the Thai operation was to do research into the treatment of drug resistant *falciparum* malaria. The primary concern of the El Salvador station was to work on insecticide resistance and interaction with mosquitoes. He felt that I would be somewhat lost and without much to do in El Salvador, but in spite of that he finally agreed to send me there. I spent most of my time looking for resistant parasites, which after more than four years I was unable to find any. I traveled to Panama, in parts of Guatemala, all over El Salvador, in Honduras, and in Nicaragua. I used an *in vitro* testing method to detect drug resistance, and couldn't find any.

Other than that, I also dabbled in my interest in intestinal parasites and did some work -- not so much on the *histolytica* problem -- but on the worm burden of the children of the study site. Part of the study was to evaluate a periodic treatment with a very effective drug, mebendazole, to rid these children of worms, and determine whether this would have any effect on their growth and nutritional status. Between that and the *in vitro* testing, I kept myself gainfully occupied.

Leo Slater: What was the *in vitro* method you used in looking for drug resistance?

Dr. William Chin: This was a method developed by Karl Rieckmann, a colleague and friend. He developed a method where he would draw up a tube of blood; put 1 cc of the blood into a series of test tubes with a growing agent -- essentially glucose -- plus a known concentration of drug like chloroquine. After 24 hours of incubation he would sample the blood and see at which drug concentration there was complete inhibition of development of parasite. This was a somewhat crude test -- what's called a macro *in vitro* test -- requiring about 10 cc's of blood, which you can't always get from some of these patients, particularly young children. Later on the WHO came out with a micro test using a finger puncture, which facilitated the *in vitro* test.

Leo Slater: Did you cross paths in El Salvador with Geoff Jeffrey?

Dr. William Chin: Oh yes. I never worked with Geoff, but Geoff at one time was director of the El Salvador facility, so I worked under him. And before CDC took over the NIH malaria operation, Geoff was Dr. Coatney's deputy, so I interacted somewhat, minimally, with Geoff. After CDC took over the NIH operation, and after people like Bill Collins transferred to the CDC, Geoff also joined. Geoff was in charge of the CDC, formerly the NIH, Chamblee operation, where Bill Collins and the rest worked under Geoff.

Leo Slater: In either El Salvador or Thailand, did you work with any local public health people?

Dr. William Chin: In El Salvador we never did. We were a totally distinct entity. We had no relationship with the host country institutions. In Thailand, the original concept was for me to work very closely with the personnel of malaria eradication. That was why they were able to lend me a vehicle, technical assistance, and other support. But the theory was that we would interact with them very closely, find out first of all what they perceived as their problems, technical, or administrative. Then we would conduct the research, see if we could document and verify these problems, assign priority, and try to come up with solutions. In all of this, we were supposed to be working through our counterparts in the eradication program. But we never had the opportunity to do that because; I never had staff to do that.

Leo Slater: After El Salvador, you came back to Atlanta. Why?

Dr. William Chin: It was 1976, and there was a simple reason “why.” In 1976, Dr. William Trager of the Rockefeller Institute reported on his ability to cultivate *falciparum* parasite *in vitro*.⁵ And that was an exciting finding because up until that point, people who tried it were totally unsuccessful. Bob Kaiser recognized the potential of this new technology. He thought that -- with my background in the laboratory -- I was a suitable candidate to return to CDC’s Chamblee facility and head up the new lab for *in vitro* cultivation. He brought me back to CDC, just for that purpose. And I set up the *in vitro* cultivation: I spent two weeks with Dr. Trager learning his technique and went back to CDC and started the lab.

Leo Slater: What was Trager like? This was up at Rockefeller?

Dr. William Chin: Yes. He was friendly but somewhat aloof. We didn’t have any personal contact. We’d say hello, welcome, and that’s about it.

Leo Slater: You set up the program at Chamblee. Did you continue to work with that?

⁵ William Trager and James B. Jensen, “Human Malaria Parasites in Continuous Culture,” *Science*, 193, 1976, 673-675.

Dr. William Chin: Yes. I spent the rest of my career with CDC doing that. After we mastered the technique of cultivation, we worked on cultivating parasites isolated from patients who had been infected with *falciparum* malaria. We then used the cultivated parasites to determine the response to various drugs *in vitro*. I also worked on a process of cultivating these parasites on a relatively large scale compared to the usual method. Instead of using small Petri dishes, I devised a large flask kind of operation, and recovered a lot of parasites, which Bill Collins used in some of his fluorescence and immunology studies. I was able to recover large amounts of these parasites, which were later used for preparation of vaccines. This was the big thing back in the late 1970's. After Trager's discovery, the buzzword was vaccine development. With all these parasites you could develop vaccines, so that was what some of my co-workers, particularly Bill Collins, did. I was not involved in any of the vaccine investigations.

Leo Slater: Can you add any other recollections of this work?

Dr. William Chin: Producing the large amounts of *falciparum* parasites was done using a semi-automated large-volume method that was reported in 1979 in the *British Transactions of the Royal Society of Tropical Medicine and Hygiene*.⁶ This method was labor intensive, requiring the changing of culture medium on an approximate 8 hour basis. Since I was doing this by myself, much sacrifice had to be made by limiting time spent with family. This hectic pace of daily activity, 7 days a week, continued for some 2-3 months.

In addition, I was trying to adapt some of the monkey malarias to *in vitro* cultivation. Up until that time Dr. Trager was the only one who had accomplished the cultivation of malaria and that was only with *falciparum* parasites. He either had no interest in the other parasites, or he had no access. Access was no problem, he could have called us, but he wasn't interested in monkey malaria. We started deliberately to try to condition some of these monkey malarias into growing in the laboratory *in vitro*. After a couple years we were successful with two of them, which had never been done before: *Plasmodium fragile* and *Plasmodium gonderi*.

⁶ William Chin, "A Method for Large-Volume Cultivation of Malaria Parasites Based on the Principle of the Trager-Jensen Culture Method," *Transactions of the Royal Society of Tropical Medicine and Hygiene*, 73, 1979, 334-335.

Leo Slater: You had some interesting things to say about USAID. I see that you did some consultant work with them in Indonesia in the 1970's?

Dr. William Chin: USAID really had no professional malariologists on their staff, so if they wanted technical evaluation of any malaria program they had to call on people with some malariology experience. Usually they would call on the CDC for that. Some of these evaluations were joint efforts between USAID and the host country. In 1977, I was asked to help evaluate the malaria program in Pakistan. I was a member of an evaluation team; there were also WHO representatives. I evaluated the malaria program in Indonesia on one occasion, in 1977 or thereabouts. I was visiting Dr. Trager's office one day when Dr. Kaiser called me in Dr. Trager's lab and said, "Would you like to go to Indonesia for a couple weeks?" I said, "Yes."

That was a very interesting evaluation because the WHO, more than USAID, wanted to evaluate the microscopy capability of the microscopists trained by WHO personnel. The chief trainer of these microscopists, Bill Rooney, and I were basically the two members of this team. We went to the regional headquarters of the microscopy effort in some of the major island districts in Indonesia, just to see how they were doing in terms of the accuracy of their diagnosis and the competence of their microscopists. The only significant finding we had was on one island -- I don't recall the name. We focused on the laboratory chief's crosscheck efficiency in the microscopic diagnosis. We were suspicious because this lab had reported virtually 100% error rate of the slides they crosschecked. We asked the person in charge -- and this person was trained by Bill Rooney himself -- why she was finding virtually 100% error. We asked her to show us a slide where it was reported negative and she found positive. She took a slide and focused and said, "There." So Bill and I looked, couldn't see any parasite. I said, "Where's the parasite?" "Oh, don't you see those red dots?" And Bill Rooney's jaw just dropped. She explained that her understanding after taking Bill Rooney's course was that any red pigmentation was a parasite, any blue was not a parasite. She failed totally to understand the concept of the morphology as well as staining quality. In everything you stain, you

always find something with color pigment. So every negative smear with same red stain was positive. That was very revealing.

Leo Slater: You eventually went back to Pakistan for five years as a long-term USAID advisor?

Dr. William Chin: Back up a little. In 1979, my first wife [Susan] divorced me. Cicely and I met while we were both students at Michigan, and we started communicating after my divorce. We got married in 1980. Cicely is a Los Angeles girl; she spent nearly her entire adult life in LA. And all of a sudden to be living in Atlanta was just something she couldn't take [laughs]. It was just dismal for her. I had reached a point where I didn't know what else to do in CDC, so I retired in 1982.

I retired from the Commission Corps of the Public Health Service and CDC, and interestingly enough following my retirement, I had several offers of jobs. One of them was from USAID. It would be a personal service contract, to serve as malaria control advisor to the government of Pakistan. I had other offers including one from the WHO. There was an opening in the western Pacific region of WHO to be their chief malaria advisor. Jalil Karam of USAID/W [USAID/Washington] sent me to Pakistan to evaluate the situation and for me to decide whether or not I would take that job. I told the people in Pakistan that on my way back I was stopping in Manila to discuss with the WHO staff about the Manila job to cover the western Pacific.

The WHO job was very intriguing because the area covered extended from the tiniest of islands in the eastern Pacific all the way to the main continent of Asia, which meant that I would have opportunity to visit all these exotic countries -- Vietnam, Korea, Burma, on and on and on. That sounded exciting to me. The part that was not exciting was the salary; my income would not be that great. Plus, I would not get housing. I would have to pay rather high rental rates in the Manila section. But I submitted an application to them anyway. I told them that I need to have them consider my application seriously and expeditiously because my bills were accumulating, while my only income was from my pension. I returned to the States, and I was staying with my brother in Berkeley at the time. I called Jalil Karam and told him that I was probably going to accept the job in

Manila. I waited a week, and I got no word from Manila. Finally I weighed the factors and decided I would take the Pakistan job. I called Jalil again and said, "Is that job still open?" "Yeah." So I took it under a personal service contract. Originally I thought it was for three years, but I extended it to five, because it was extremely lucrative. My salary was limited to what the Ambassador was paid. In addition, we were given free housing with all utilities paid for, free transportation, and an annual R&R trip out of Pakistan. We could go around the world if I wanted to, every year. Plus I was still getting my full pension while working as a contractor. Those were the primary reasons that motivated me and not the technical challenge because I knew before I even accepted the job that I would be confronting a vast Pakistani bureaucracy and even possible corruption.

As a matter of fact, while I was there to assess what the job would entail, I met one of my former colleagues from the CDC who was also in Pakistan as a consultant to the World Bank, evaluating the Ministry of Health and their activities. We got together over breakfast one morning in a hotel. I told him what I was planning to do, he told me what he was doing. He said, "Bill, if I were you, I wouldn't take this job. Just walk away is my best advice to you." He explained, of course, there was the problem of gross inefficiency and corruption. But I felt that the incentives were sufficiently high that I could live with it.

Leo Slater: Tell me what it was like to be in Pakistan.

Dr. William Chin: My wife and I enjoyed it, because the job required that I travel periodically to each one of the major provinces and connect up with the provincial secretaries of health, district health officers. And, as I said, technically the job wasn't that challenging. I also acted as a coordinator between USAID, who was still funding this malaria control program, and the Pakistanis. I had to formulate a budget every year in terms of how much money was needed, mostly for insecticide, equipment, and training costs. As I recall, there was a budget of some \$4 million on an annual basis. One of my responsibilities was to decide how to divvy up this \$4 million pie.

I was also able to conduct various training courses and technical training courses. I introduced the micro *in vitro* drug assessment kit that the WHO had provided and gave training to some of their senior technical personnel on how to do this. But mostly, I was to help the Pakistan government develop their own research capability, training people, setting up laboratories. In this regard, I came into direct conflict with the University of Maryland. The University of Maryland had this research group -- I mentioned it earlier where Pete Contacos received some assistance in setting up the initial study back in 1963 -- based in Lahore, the ICMRT [International Center for Malaria Research and Training]. When the University of Maryland realized that USAID was funneling a great deal of money into the local malaria program, they decided to take part of this grant to support their center and to fund their own research programs.

When I was in Washington finalizing the agreement with USAID on taking the job, one of the people who wanted to meet me was Tom Strickland. Strickland was a retired naval medical officer who was professor of tropical medicine at the University of Maryland Medical School, and he was director of this center in Lahore, Pakistan. He was able, through the support of his chancellor and a Senator from Maryland, to take part of the funding for malaria control to support this research center. The bill that he sold USAID/W was that it was going to take too long for the Pakistanis to do their own research. Strickland's intention was that I would join his center and do the research for them as a contractor. In return USAID would pay to support the research, as well as the center. Strickland had no one experienced in malaria research, he was not a malariologist, so when I was in Washington finalizing details, and he had me over for lunch and proposed a deal with me. He said, "Bill, instead of taking this job, why don't you take the job I'm offering you. Why don't you come work for me as the Chief of the Malaria Division of this lab, and I'll pay you whatever salary you would make."

I was noncommittal, but after thinking it over, I decided that this was entirely inappropriate. Why would USAID give money to try to develop a research capability for the Pakistanis themselves, and then have a US agency come, take this money away, and say, "Well, we'll do the research, because the Pakistanis have no capability of doing it." I told the USAID person in charge of the Pakistan desk in Washington exactly that. I told

him I disagreed with what Strickland was proposing to do. But by the time I landed in Pakistan to undertake the assignment -- some two or three weeks later -- this was a settled issue. They had gotten their money even though I was not part of the package.

I argued with Strickland and his group often and acrimoniously. At first the disagreements might be heated but they were not bitter -- until I tried to do my first field study. At the time I didn't have any staff. I didn't know enough of the personnel at the Ministry of Health or Malaria Control to get assistance. But I did know that this research center of the University of Maryland had personnel, and they had experience in field work. So I approached them and said, "I'm doing this study; I'm trying to document the presence or absence of chloroquine resistance in these parasites. I understand you have a study site. If I could accompany you and take some blood samples to test for resistance I would appreciate it." Strickland was not in the office at the time I made this proposal. I was speaking to his deputy who was a Pakistani Brigadier General in the Army, retired. At the time, I also had a friend who had been at the center for years, a Japanese American entomologist. He was doing entomologic research including malaria vectors. He took me out to dinner after I made this proposal to the Pakistani Brigadier. At the dinner he told me, "I'm sorry Bill, but the Brigadier said it was inappropriate for you to accompany us to this site and do this."

The Brigadier was concerned that if I found resistance and reported it, this would be the first documented report of resistance and would beat his people to these results. I ended up going on my own into another district that I didn't know. On my own, I went and conducted this study. Living conditions were harsh; there was no hotel. The only place I found where I could stay was at a Pakistani Army Veterans' Hostel, where you were not charged any rent, but you were asked to give a contribution. There was no heating. There was no hot water. There was no indoor toilet. Everything was outdoors. It was wintertime. After a week of cold showers, fortifying myself with a glass of whiskey before running out to the outdoor shower room, after suffering all this deprivation, I was so angry with Strickland and his group for denying me the comfort of at least accompanying them into a civilized area that my opposition to his stealing this funding became much more personal than professional. When I returned to Islamabad, Strickland

called and apologized profusely. He said, “It wasn’t my intent, my Brigadier didn’t have any instruction. He acted on his own.” I told him that it didn’t matter. His main concern was that I would find something to report that would more or less show his people off. For me, the major concern was if there was resistance, somebody should finally report it, for the sake of the malaria control program of Pakistan.

We never got along after that. USAID then asked me to take part in the review of ICMRT’s research protocols. Again, I must admit that I let my personal bias override some of the technical issues. But clearly, when you read a protocol draft on malaria research by people with little or no experience in this type of work, there were glaring errors that could not be ignored. And because of my personal bias, I took delight in pointing out these errors. One example was their careless usage of simple words, “mosquitoes are brought to the field, kept cool in dry ice.” I said to myself, “Dry ice, that’s CO₂ frozen. If you put it in with mosquitoes it’s going to kill every mosquito.” What they meant was this canned ice that you freeze. I made a big point of that and other minor and major errors, and every time we had a meeting over the approval of these protocols, there was a battle between Tom Strickland and his staff and myself and some of my Pakistani colleagues.

After about a year of this fighting back and forth, the Ambassador finally got wind of it, not because he paid any attention but because the Chancellor of the University of Maryland came out to visit him. He heard that this guy, Chin, was giving his people a lot of problems, and he wanted the Ambassador to put a hammer to my head and just quiet me down. The Ambassador called a meeting, and I was invited. Strickland was there, the University Chancellor, the Embassy physician, and the Ambassador. We discussed the pros and cons of continuing support for this group, and finally after about an hour of going back and forth, the Ambassador, Ambassador Deane Roesch Hinton, who previously had been the Ambassador in El Salvador, got up and said: “Gentlemen, I think it’s time we Pakistan-ized the center,” meaning that we should turn it over to the Pakistanis. Basically that is what I wanted, but it didn’t turn out that way. Instead of it becoming Pakistani, which to me meant the Pakistani personnel in malaria control, he turned it over to this Brigadier who was Pakistani. But it remained a political hot potato.

That was how I was able to at least separate out the American component of this research center, which to me was not just a cynical attempt to pursue your own policy, but was stealing money from a very vitally needed activity in training local Pakistanis.

After this meeting with the Ambassador, the Embassy physician, who took care of the State Department personnel in Pakistan, wanted to speak to me. He said, "I have a patient I think has malaria, but her smears are negative. I don't know what she has, so I'm sending her to Frankfurt, Germany to the Army Hospital for further consultation, but I'd like you to see her and give me your opinion." I said, "Fine," and went down to the health unit, and here was this lady, obviously ill. I took a history; it was compatible with possible malaria. Her symptoms and signs were compatible with malaria. So I said, "Okay, I'll tell you what. I'll take a blood smear and I'll examine it, and in the meantime, you go home and pack for your trip." I took a blood smear, went to the lab, and quickly stained it and examined it, and it was just loaded with *falciparum* parasites. I told the doctor, and he finally called the patient: "Stop your packing, come on back here, we're going to treat you for malaria." I started asking; how can this technician, trained in Pakistan could have misdiagnosed this case -- report as negative when the parasites were just all over the place? It turned out that he didn't know how to properly stain the smear. Everything that he examined was negative, as far as he was concerned. I had to give this poor fellow lessons on staining. But that's an interesting aside, I think, that many of these so-called experts had little or no knowledge of the actual facts involved with a diagnosis or treatment of malaria. They just go on doing their work supervised by higher-ups who knew less than they.

This is a cause of concern for me. This is the same reason why every year in the States we had -- at least when I was active -- a dozen or so people dying of malaria, because malaria was a disease about which clinicians in this country have just totally forgotten. Also distressing is the likelihood that parasitologists have also neglected to keep current on the technical aspects of malaria. A notable example is the following:

Some time in 1981, I had a phone call from a friend and colleague, Karl Rieckmann who was teaching and conducting malaria research at the University of New Mexico. Karl

was excited and asked what we (CDC) were doing about a malaria epidemic in the San Francisco area. I told him I was unaware of such an epidemic. He then told me about a phone call one of his colleagues, a parasitologist teaching at the University of California San Francisco school of medicine had just called him to let him know that they were doing surveillance for malaria among the recently arrived Asian refugees, particularly Vietnamese, by examination of blood smears and that they found some 50% were infected with *falciparum* malaria. I told Karl that the information was not only interesting but also highly significant and that I would consult with Bob Kaiser to discuss a plan to assess this situation. Bob sent me to San Francisco to evaluate the problem. The first thing I did was to go to the hospital where most of the blood smears were taken and examined. I asked the laboratory supervisor to select 10 positive smears for me to examine. After some time, I was not able to find what I would consider a parasite in any of the smears. I then returned to the supervisor and asked her to find a parasite for me to check. The first thing I noticed was that she started to examine the thin smear. After more than 15 minutes or so, she found one and asked me to look. I examined the image under the scope and saw a shape that superficially resembles a crescent shaped *falciparum* gametocyte. There were no other diagnostic features that would qualify that image as a malaria gametocyte including the total lack of pigment. I was sure it was not a malaria parasite. I then asked her why she examined the thin smear and not the thick smear since the thick smears, as the name implies, would concentrate the blood being examined by some 10 fold. She told me that they would find the same organisms in thick smears also but there did not seem to have any degree of concentration and besides, she noted, it's much easier to identify the organism on thin smears. When she told me that I knew then that what ever they were finding was not of blood origin since common sense would indicate that it would be some 10 times easier to find these organisms in a thick blood smear than in a thin because of the concentration factor. I never had a chance to discuss this with the parasitologist but did learn that he was preparing a paper for submission on his "dramatic finding of an epidemic of a potentially fatal infection in the Bay Area of California." I took some of the blood smears back to the CDC and shared them with a few colleagues including Geoff Jeffrey and Mae Melvin and we all concluded that the organisms were not *falciparum* gametocytes but most likely a spore stage of a fungal contaminant well known to most "old time" malariologists, particularly

those of us who were familiar with the 1963 publication by the IMR (Institute for Medical Research) of Malaysia, "The Microscopic Diagnosis of Human Malaria." The source of the fungus was most likely from the buffer solution used in the staining procedure. Errors such as this occurring in microscopic laboratories in developing countries may be excusable because of the lack of sufficient education of the microscopists, but similar errors in the U.S. and other developed countries have no such excuse. While this trip proved to be one initiated because of false positive findings, it did provoke questions from some of us at the CDC, notably, "what is the likely positivity rate for malaria infection in the newly arrived Asian refugees?" To answer this important question, important because California is known to have mosquitoes fully capable of transmitting malaria with one in particular, *Anopheles freeborni*, being a most notorious and highly susceptible host to all species of human malarias and even some monkey species, (Bill Collins is without doubt, the world expert on this mosquito) we were authorized to spend some 4 or 5 separate paid "vacations" at the Oakland International Airport to screen the Asian refugees by taking a blood sample for various assessments including a blood smear to examine for malaria parasites as soon as they disembark from their chartered jumbo jets. Our findings, from the separate surveys, that the positivity rate for malaria was consistently less than 2% were rather reassuring.

The problem of relative ignorance with malaria in the U.S. in recent years is understandable because of the dearth of opportunity by most of the medical personnel to encounter a malaria case. To mitigate such a problem, the CDC, during my tenure at that Center, established a "hot line" for the medical profession to call and consult with one of its officers knowledgeable about clinical malaria. During the last three or four years prior to my retirement, I became the senior officer to respond to such questions on a 24-hour basis. One of the most memorable calls came one evening, I believe, in 1978. The caller, a physician from the medical center at the University hospital in Madison, Wisconsin, was asking for advice on how to treat a semi-comatose and critically ill patient with *falciparum* malaria. The patient was a young lady of Caucasian ethnicity who had been working in Ghana, West Africa on a summer project for the University of Wisconsin. Shortly after her return home, she felt ill but did not seek medical care until at least a week after she became ill. She also reported that while in Ghana, she had not taken any

prophylactic against malaria. When I asked the physician how high was her parasite density, his response was that he did not know. I then advised him to ask a technician to examine one of her thin blood smears and examine just one high power field by counting the total number of RBCs and then count the number of infected RBCs to derive an approximate percentage infection of the RBCs. I also told him to call back as soon as he has this information. He called back shortly and said that some 70% of her red cells were infected. I advised him that the patient most likely has only hours to live and the only possible way to save her life was by performing an exchange transfusion. (During my earlier days as a malariologist trainee at the prison project, I recall Pete Contacos telling me that a member of the black clergy with a similar story of having returned recently from Africa died of *falciparum* malaria when his parasite density was just below 50% infection of RBCs even though he was being treated aggressively with intravenous quinine. At the same time, I also have had experience in Thailand of advising Thai physicians on the treatment of patients with extremely high parasite density, the use of exchange transfusion. I vividly recall two such cases in children of five or six years of age and was amazed that both survived after the transfusion operation.) The next day, I called and as the saying goes “hoping for the best while preparing for the worse” and to my delight, I was told the patient’s parasite density had dropped to just under 10% infection of RBCs. The patient was treated with intravenous quinine first followed by a standard course of chloroquine and made a full recovery after suffering additional complications including partial renal failure. This dramatic case was reported in the *American Journal of the Medical Sciences* in 1979.⁷ Another method Bob Kaiser used to increase the awareness of the medical profession to the problem of malaria during the few years prior to my retirement was to send me to various medical teaching institutions to lecture on malaria, particularly clinical and treatment aspects.

Leo Slater: Is there anything else you want to add about the Pakistan experience?

⁷ Richard L. Nielsen, Richard B. Kohler, William Chin, Leo J. McCarthy, Friedrich C. Luft, “The Use of Exchange Transfusions: A Potentially Useful Adjunct in the Treatment of Fulminant *falciparum* Malaria,” *American Journal of the Medical Sciences*, 277, 1979, 325-329.

Dr. William Chin: After five years, there was still agreement to continue the support of the malaria program by USAID. But I was told I need not request an extension because they were convinced by my counterpart that my approach was wrong. My approach -- particularly in a country where malaria endemicity is not that high, as was the case in Pakistan -- was that they should not depend on insecticide coverage, because this can only be done if there is foreign assistance to pay for it. Once there is no foreign assistance, what are you going to do? I proposed strengthening their laboratory capability and emphasizing the treatment of detected cases as an alternative to continued reliance on house spraying. But the person who became Director of Malaria Control when I was finishing my five years was not a sympathetic listener: The Director's job was turned over to a Pakistani entomologist -- who, unlike my counterpart for most of the five years was not a physician -- and his concern was of course house spraying. One reason for this, which I heard through the rumor mill, was that by bringing in all this insecticide, many in the program received payoffs by diverting some of the material onto the black market. I was never able to substantiate that, but that's what I heard. That may be one of the reasons he concentrated on the insecticide approach. So I said, "Okay, fine. I've had five years, I've had enough, so we left more or less amicably."

Leo Slater: After that experience with USAID, you did more work with USAID in Malawi, is that right?

Dr. William Chin: I had nothing against USAID. The people I dealt with were not the same people who make policies. The people I dealt with were basically left over from the old malaria eradication era, trained technicians. It's the policymakers who made these determinations. In 1989, I was asked to go to Malawi to spend a month to evaluate their malaria program and also try to assess the possibility of drug resistance in that country. I went and enjoyed the experience, but again I was in conflict with some of the other evaluators of that same program -- one Britisher in particular. I made a drastic proposal, as some of these people deemed it. I suggested that since they didn't have a truly modern control program nor the resources for house spraying and attacking the mosquito, their emphasis should be solely on trying to identify the parasite by examination of blood smears and then treating the cases. Therefore, they should work to develop their

capability in malaria microscopy, training people in the small clinics and hospitals, even, to make a smear diagnosis, to confirm malaria before treating it. When I was there, the routine was -- in any of these African countries since my Peace Corps days -- that if you came in with a fever, you had malaria and therefore you were treated for malaria.

Maybe they were right 50% of the time at most, but they could also be wrong 50% of the time. They weren't practicing good medicine, as far as I was concerned. My approach was give some money to support the development of a microscopy corps so they could examine the parasite, give the results to a doctor, and the doctor could then apply appropriate treatment. I also said that since they did not have a laboratory service, this could be the nidus for the development of future clinical laboratories. They could then expand from malaria microscopy: with further training they could do blood differential counts and other simple tests, and then gradually expand the capabilities, and pretty soon they would have a full-blown clinical laboratory. Some of my fellow members, particularly my British colleague, who worked in Malawi for some time said, "You're never going to do it. These people are not educated. You can't train them to be microscopists." They could have made the same argument in Thailand and other countries, yet, in the 1960s, the same poorly educated individuals were doing a superb job. (Girls from the villages were trained as microscopists. They did a fantastic job.) I believe that the same process could have been applied to a situation like Malawi. I basically ended that evaluation by suggesting that they coordinate rather closely with the CDC malaria program for future advice.

Leo Slater: I have a few more questions. You acted as a reviewer for the *American Journal of Tropical Medicine Hygiene*. I was just wondering if you were active in the society during your career.

Dr. William Chin: I was just a member attended the meetings, but I was never active in the sense that I actively sought to be an officer. That was not my cup of tea, but I did volunteer to be a reviewer of manuscripts, particularly field-oriented studies, such as drug studies, a subject where I had the most experience. I would get three or four papers a year to review. Most of the time, the papers came from researchers in this country, but once in a

while you'd get papers from Africa, India, and other less developed countries. Those were badly written and just totally inappropriate for the journal, and my usual answer to those authors was, "It would have much more significance if this paper were published in a local journal." And that was it.

Leo Slater: I wanted to ask you about how the broader field evolved during your career. You mentioned Trager's *in vitro* cultivation leading to a lot of vaccine work. What big shifts have happened across your career in the relationship between chemotherapy and vaccine development, in research, or in control versus eradication? Is there something you can take away from that big picture?

Dr. William Chin: I think the basic weakness of the malaria eradication effort has been this tendency to seek the silver bullet all the time. When DDT was discovered, "Oh, this is it! All we have to do is spray houses, mosquitoes will rest on the DDT, get killed, and that's it; we've solved the whole problem." Same thing with more recent times. When this was not successful, the next silver bullet was, "Okay, a drug like CI-501. One shot, one year, finished. You cover the population." That didn't prove out. Then, with the *in vitro* cultivation we said, "Aha! A vaccine, that's it! We develop a vaccine; give it to the people, period." I had basically been an opponent of the vaccine development approach, not because of the academic or technical challenges of creating a vaccine -- which I can see. But I was realistic enough to know that all this propaganda about using the vaccine to eradicate malaria was just pure nonsense. If you look at the development of the vaccine, it's extremely technical. Beyond that, if you do have a vaccine, the cost of producing a vaccine would be extremely expensive. Which means every time you give a dose, you're talking about tens or more dollars per dose. Now you apply that to a situation in Africa. Who can afford \$10 - \$20 per shot to protect themselves against malaria? The only way would be if you give them the vaccine to do it, and then you have depend on their limited capability, with the insufficient health infrastructure, to deliver this product.

My perception, from the very beginning, was that vaccine development was aimed, not to solve the malaria problem for the whole world, but to solve the malaria problem of

Americans and other people of developed countries when they go overseas to malarious areas: military personnel, missionaries, travelers. They would get a vaccine and become protected against malaria, if it worked. But to rationalize this and say we're going to use it to eradicate malaria from Africa, from Haiti, from Cambodia, made no sense at all as far as I was concerned.

The silver bullet approach has been one that I have always been against. I've always been a proponent of slow development, using whatever is available and affordable by the respective nations. Take countries like Taiwan and Cuba as two primary examples and contrast them to an island state like Haiti. The experience in Cuba and Taiwan was that they participated in the eradication program by using the standard house spraying program.

Dr. William Chin: Cuba and Taiwan followed the protocol of the eradication campaign closely. You spray a couple of cycles of DDT, you kill off most of the mosquito vectors, and then you follow up with intensive case detection. You go out seeking cases, either actively or passively. By active, I mean you go out, send people from door-to-door checking for malaria cases. Passively by setting up these stations or at the health clinic and people come in to give a blood sample for the detection of malaria. They did this very well. Using this combination, they were able to achieve malaria eradication within three or four years.

A country like Haiti was supported heavily by USAID, but they also started spraying, which was carried out, effectively or not, we don't know. But they certainly did not follow up with the case detection. Even today, malaria is still a problem in Haiti after all these years of trying to eradicate it. I admit the governments of Taiwan and Cuba are more authoritarian. They required, in fact demanded, much more cooperation from the people and would probably penalize them if they didn't cooperate. This versus the free society of people in Haiti and other countries where you have to really convince them to cooperate. The concept of eradication was sound, but the execution depended on the discipline of the people and their willingness to cooperate. This was the major difference.

Leo Slater: Is there anything I should have asked you about? Or anyone that I should have mentioned that I haven't?

Dr. William Chin: The only thing that I'm still disappointed about is the current neglect of the malaria problem by major donor nations, and it's understandable given the rise of the AIDS epidemic. Whatever funding is available to assist public health endeavors and health initiatives, for the most part, has gone to the assistance of AIDS programs. Malaria, which in the long run may still cause as many deaths and certainly more morbidity than AIDS, is, for practical purposes, getting zero funding these days. That's a major concern for me.

How do you reverse this? Again, it's a policy matter. Malaria, after all these years, is no longer attractive. We are no longer training people in malaria. People like me, the so-called "old-timers," are dying off, and I don't see any real replacement on the horizon. It's far different now than in the era when I first entered the field. There were people like Don Eyles in Kuala Lumpur, who was responsible for finding new species of monkey malaria. Unfortunately, I never met the man, but I heard a great deal about him from Dr. Coatney and some of his other colleagues. People like him are no longer being trained, no longer being motivated to enter this field. I feel that there is still an urgent need for people with my interests, my background, to continue in the malaria struggle.

Any time I talk to a young person -- most recently I was in Singapore talking to some of Cicely's grandnieces and grandnephews -- if I see somebody with a keen intellect and motivation to achieve as a scholar I say, "Have you considered being a malariologist?" I try to make a sale, but chances are, they will look into it and see no future in it and forget about it. This is the plight; this is the sad part of it.

I think when I was doing the *in vitro* cultivation work, there were a few of my colleagues who came in to learn first the process and then to carry the process further on, but I don't know of anyone after these few people who is willing to do that. In the meantime, we are left with this forlorn hope of a vaccine, which, to me, may still be decades away.

This is another interesting sidelight. When I was finishing up in Bangkok, a USAID sponsored team came out and concluded that our accomplishments were minimal, and we essentially had not done the job we were sent out to do. The person who gave me that harsh verdict, I still remember his name. He was USAID, Director of Strategic or Technical Studies, Dr. Joel Bernstein. He came to Bangkok and set up a meeting to let me know that I was no longer needed in Bangkok. My type of work was no longer needed because, he said, "A vaccine is around the corner. We're using the money for this project and we're going to put it into vaccine development, and within a few years, we're going to have it." I told him, "Dr. Bernstein, you're lucky if you have the vaccine within 20 years." An interesting development in the vaccine story took place some three or four years after Dr. Bernstein's meeting in Bangkok, when a few of the major USAID consultants were involved in a scandal for misappropriation of funds. (You can ask Bill Collins for greater detail.)⁸

Some twenty years later I'm in Pakistan, and I'm listening to a broadcast from the BBC, and this broadcaster was interviewing malariologists. I've forgotten, maybe it was Wallace Peters, a respected malariologist, and he was talking about the vaccine. He said, "I think we'll have a vaccine in 20 years." So we're talking about 40 years now into the future, and still we're not closer to a vaccine than before. This hope of a vaccine I think is just a simple bureaucratic diversion to justify the neglect of funding of all these malaria programs worldwide. That's basically my major complaint at this time. Although, from my brother's perspective, I may be all wet since he's a USAID consultant on AIDS, at least he was. He wrote a book on his retrospective assessment of the USAID and the WHO's support of the AIDS control effort, and apparently it is so controversial that no one wants to be associated with it.⁹

Finally, a few last words: Malaria is a disease dating from prehistory. Its distribution was worldwide at least until the middle of the twentieth century. In the U.S., malaria transmission in the southern states began to recede by the 1930s and totally ceased by the

⁸ For more on the sad tale of USAID's malaria vaccine program, see Robert S. Desowitz, *The Malarial Capers: More Tales of Parasites and People, Research and Reality* (W.W. Norton, New York; 1991), pp. 257-276.

⁹ James Chin, *The Glorious Myth about a Generalized AIDS Pandemic: Collision of Epidemiology and Political Correctness*, to be published by Radcliffe Publishing in the UK.

1950s. Malaria transmission ceased in the northern states far earlier. Malaria disappeared in the U.S. and other developed countries not because of a well executed eradication program but because the socio-economic development in these countries reached a point where the environment was no longer conducive to support malaria transmission. Housing was better and built with screened windows and doors. Water was generally brought indoors via pipes thus eliminating outdoor pools and puddles needed for the breeding of anopheline mosquitoes and finally, modern medical care was becoming readily available to treat the last few residual cases. Viewed from this perspective, it takes little imagination to realize that malaria transmission will remain a major health problem for people of developing or underdeveloped countries for many generations or even centuries to come. Given this imperative, the decision to ignore the malaria problem of the underdeveloped world by the policy makers in those countries with the resources to assist in the effort to control malaria, particularly the U.S, is unconscionable.

Presently, our national leaders, more often than not, are making public health decisions affecting us as well as people in foreign countries based on political expediency or on the behest of small fringe groups including religious zealots and not on practical realities. Policy decisions are influenced by groups who are out to promote their own agenda by gaining access to those making the decisions. The question of how best to continue the control of malaria in developing countries over the long haul of many generations to come or longer is being aggressively addressed by those urging a “high tech” approach, the development of a vaccine. Even though such an initiative, when it was first launched, promised that a vaccine would be available in a matter of years, the reality is that more than 30 years have passed and millions and millions of dollars have been spent, and there is still no sign of a useful vaccine in the foreseeable future. In the mean time, other “low tech” cost-effective methods have been discarded and abandoned for lack of funding resulting in death or morbidity by countless millions of victims due to malaria infections over the same span of more than 30 years. Had even half of the funding already invested in vaccine development been used to seek more affordable methods to reduce malaria incidence in the less developed world, the outlook for controlling this disease would be far brighter today. The resources needed to develop local competence in malaria control

is at most, modest: Train national experts in the epidemiology of malaria to supervise the control program; develop a core of trained microscopists with fully equipped laboratories to diagnose malaria by examination of blood smears; aggressively pursue the concept of minimizing contact with infected mosquitoes by the liberal use of clothing impregnated with a repellent and use of bed nets impregnated with an insecticide and finally make available effective drugs to treat resistant parasites. Such a plan is affordable when compared to the annual expenditure for vaccine development. It is time the policy makers reassess the resources already squandered over the past three decades in their pursuit of a vaccine by comparing the total expenditure already spent to the progress made in controlling malaria in order to decide, pragmatically, whether the investment has been worthwhile. If not, then money should be shifted to control malaria the old fashion way, practice malaria prevention aggressively and then gradually reduce the infective reservoir by the elimination of malaria a few cases at a time through treatment.

Leo Slater: Thank you.

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