Dr. Tom G Schwan Oral History
November 22, 2021

[Note there is no period after "G"]

This is an oral history with Dr. Tom G Schwan about his career at the National Institute of Allergy and Infectious Diseases (NIAID) on November 11, 2021. The interviewer is Dr. Victoria Harden, Founding Director, Emerita, of the Office of NIH History and Stetten Museum, National Institutes of Health (NIH). We are recording this interview via Zoom.

Harden: Dr. Schwan, would you please state your full name, that you know that this is being recorded, and that you give your permission for it to be recorded.

Schwan: My full name is Tom G Schwan. I realize that this interview is being recorded, and you have my permission to record it.

Harden: Thank you. Let's begin with your birth. You were born April 11, 1947, in Oakland, California, as the identical twin brother of Mark W Schwan to parents William Creighton Schwan and Beverly Lois Schwan. Your father was an electrician, electrical safety inspector, and author. Your mother was an elementary school secretary. Tell me about your family and about your education through high school, especially anyone or any experience that helped set you on a path towards science.

Schwan: I was fortunate to have a loving, caring home with wonderful parents. They supported my brother and me—and, by the way, I may actually at times answer "we" rather than "I," because as you mentioned, I have an identical twin brother. As I was being raised, my brother and I were very close and continue to be close to this day. Again, my parents were very supportive. They were not religious, but we were brought up by practicing the golden rule. That may sound corny, but that was what my mother wanted for us: to treat people as we would want to be treated. I still live by that rule.

We also had a close relationship with my mom's parents, my maternal grandparents, George and Freda Bauslaugh. They lived just a few blocks away while we were growing up. My grandfather loved to fish, and he introduced my brother and me to fishing. We spent a lot of time fishing with him. My dad grew up in a broken household and was not able to go to college. At the age of 17 or 18, he became an electrician. He was also a professional musician. I think he would have loved to go to college, but he just couldn't do it with a single mom working very hard to keep a roof over their heads and food on the table.

My parents were terrific. There was no pressure as we grew up to go to college. My brother and I did pretty much what we wanted. We both did well in school, and my mom pushed us to join almost everything we could in elementary school and junior high.

Gone fishing with Grandpa George. Left to right, Tom, Grandpa, brother Mark; 1958.

My brother and I learned to play the saxophone at school and from our dad. At school, we were in every performing arts program that you can imagine—choir, chorus, orchestra, band. We did that through junior high school, but when we got to high school, my brother and I said, "We've had enough. We've had enough performing." We did more things by ourselves in high school but continued to fish and play golf. Again, we were just so fortunate to have parents who supported us and let us pursue whatever we wanted.

My dad worked extremely hard, and he was always trying to improve himself. After working all day, often at night, he'd be in his little office studying for a correspondence course. He took many, many correspondence courses to learn more and become more “certified” in his mind. I think he really regretted not having the opportunity to go to college, but having said that, they didn't push us at all. When it came time to go to college, neither my brother nor I felt that we should go off to some expensive school and put our folks in debt—and it was highly unlikely that at least I could have gotten

into those schools. We chose a local California state college, California State College at Hayward, which cost 52 bucks a quarter tuition. That's where I started college.

Harden: Before we get to your undergraduate work, I have one more question. You've talked about fishing. Was your interest in insects, arthropods, small mammals, and other ecological subjects something that seemed innate to you, like the way you enjoyed fishing, or was there some person who tweaked your interest in that and helped you focus?

Schwan: My interest in nature and the outdoors really came from my Grandpa George, my mom's father. I didn't have any special interest in a particular group of animals. I just loved to get out and fish with my brother, and then later on with friends in school. A bunch of us would go fishing, but I never had an insect collection when I was a kid. I didn't look at birds per se. And nobody in my family had those interests. Those interests were not there until I got through a couple years of college.

Harden: You went to Cal State Hayward and graduated with a B.S. degree in 1969. Now tell me about your undergraduate career and how you decided to focus on biology and ecology.

Schwan: I started my freshman year in the fall of 1965. I chose biology mainly because I liked the outdoors and liked fishing, and I was terrible in math. I ruled out even thinking about physics or chemistry because of my inability to handle complex mathematical equations. Actually, I did have to take both physics and chemistry, and I did have to take math, but I struggled through all of them.

In my first year, we were on a quarter system—there were three academic quarters per year. After of the first two academic quarters, I had done poorly, so I was on probation. I realized that I could not go to college and earn good grades like I had in high school. In high school, I worked only as hard as I had to and still got As and Bs. It was not a problem. In college, I started off thinking I could do the same thing, but it didn't work. Even in the biology classes, in general biology, I was struggling. So after two academic terms, I had a D minus grade point average and was on probation. If I didn't get my GPA up in the next quarter, I was going to be out of school. What saved me was that during the spring quarter—this would've been 1966, my third quarter as a freshman—I took an invertebrate zoology class from a professor named Dr. Edward Lyke [Dr. Edward B. Lyke], and I loved the class. He was a great teacher. He stimulated the class, including me. I worked hard, got an A, and got off probation. And I realized that I had learned what I needed to do to perform well in a class. If I worked hard, I could do it.

After that, I never had a problem. That class and Dr. Lyke turned everything around for me. From then on, I did well in all the biology classes. In the occasional boring history class, I might struggle with a C, but I loved almost every biology and botany class I took. Cal State Hayward, although a small teaching college, had a master's program, and we had some wonderful faculty. For example, we had Howard Cogswell [Dr. Howard L. Cogswell], an ornithologist and animal behaviorist. I took many classes from him. Sam McGinnis [Dr. Samuel M. McGinnis] was the ecologist, herpetologist, and ichthyologist. I took

many classes from him, too. These were dedicated, smart teachers and researchers, who knew how to excite students about projects. I was extremely fortunate to spend four years doing my undergraduate work at Cal State Hayward. We had a great program there and I never regretted going there or not going anywhere else. And I got through not owing a cent thanks to my parents' help. It was inexpensive, but it provided an excellent education.

Harden: So often it does come down to a single professor's ability to communicate love for the subject to inspire students. In 1968, you married Virginia Haney, who was a fellow science student, and you embarked on a series of research assistant jobs in Alaska and in the San Francisco Bay area. Will you tell me about those?

Schwan: Ginger and I got married in September 1968, and then started our senior years of college. Of course, I also had to start working to support us. I was able to get some technical positions—assistantships on various projects. One of them was an odd one. It was to first separate and then identify benthic organisms, bottom-dwelling organisms, in San Francisco Bay that were collected near warm water outflows. The project was run by a professor at San Francisco State. The idea was to look at the ecological impacts of warm water outflows on the organisms that were living in the bay. That required a long commute for me from Hayward to Richmond. I did it as long as I could.

I was also working as a student assistant for Howard Cogswell on a project related to the hazards that gulls post to aircraft around San Francisco Bay. Some people call them seagulls, but that's not the right name. There are many species of gulls that overwinter in San Francisco Bay. The issue was that all the major airports in San Francisco Bay are on the edge of the bay near dumps, where the gulls feed and move daily between their roosting sites and where the food is. The project involved following the movements of all of these gulls, many species that over winter near the bay, to see when they move and how they pose hazards to aircraft. However, most of my time was devoted to updating topographic maps from recent aerial photographs.

When I graduated in '69, and was working on that project, Dr. Cogswell got a call from Dr. Frank Pitelka [Dr. Frank A. Pitelka] at Berkeley, who said, "We suddenly have money for a research assistant to help us up in Alaska, Northern Alaska near Point Barrow. Do you have anybody that we can hire?" Dr. Cogswell came to me and said, "Are you interested in going to Alaska? You have to leave in about five days." I said, "Well, let me talk to my wife about this." We discussed it, and both of us were fine with it. I viewed it as a great opportunity to go to the Arctic and work on the north slope and learn what it was like to work with a group of ecologists from Berkeley. So I did that, and it was a wonderful experience. It was tough work. The sun never set the whole time I was up there, as we were well above the Arctic Circle. That took a bit of adjustment. It is a fond memory; it was a great experience.

Tom Schwan, on the shore of Arctic Ocean, near Barrow (now Utqiagvik), Alaska, while working as field assistant on bird project; July 1969.

I worked under a graduate student from Berkeley. That situation was informative and helpful for me because I was this fellow's very first technical assistant in the field. He enjoyed the power. He enjoyed the authority he had over me. And there were times when I resented it even if overall it was a great experience. It was the beginning of many technical positions I held in which I was working for somebody else, and at some point down the road, I decided it might be better to call the shots myself than to do what somebody else was telling me to do. That helped me head towards becoming an independent investigator.

Harden: That is a story others of your colleagues have told me. One key question I must ask you at this point is how did you avoid going to Vietnam? When you graduated in 1969, this was the height of the draft for that war.

Schwan: I was wondering if you were going to ask me about this. You'll notice that no military experience is listed on my CV. I'll tell you this story, but I still hesitate because even today I could earn the wrath of some individuals who fought in the Vietnam War, and understandably so. Here's and abbreviated version of what happened:

I was against the war, and although I looked into some ways to support the US military, nothing worked out. Many folks went to Canada, but I could not do this. I felt I needed to stay and face the consequences of my actions. When I was called to report for induction, I went to the Army Induction Center of Oakland. When they said step forward and be sworn in, I did not step forward and take the oath. They said the maximum sentence for this offense was five years in prison and $50,000 fine. Even after multiple attempts to get me to step forward and swear in to the Army, I refused. Next, I was interviewed by an FBI agent and then allowed to go home. Some number of months later, I received a summons to report to the Federal District Courthouse in San Francisco, because I was being charged with refusing to submit to induction. I had inquired a year or two before about how to qualify to become a Conscientious Objector. I got the forms from the draft board and read through them. It appeared to me that I had to have had a long established relationship with an organized church to apply to be a Conscientious Objector, and I did not. So, I returned those forms blank to my draft board office. At any rate, I went to court in San Francisco, pleaded guilty, and was convicted. Luckily, I had a sympathetic probation officer. He wrote a supportive pre-sentencing report, and many people wrote letters for me to support what kind of person I was. I also had a good judge, or least a judge who was reasonable, I thought. I was fortunate. He sentenced me to three years' probation and required me to do alternate service approved by the court, which I did.

Harden: That's perhaps the most interesting Vietnam story I have heard among the many I have heard while doing many interviews! I am of that generation, so I have heard many stories about how young men got out of going to Vietnam, but yours may be the most straightforward and admirable for standing on principled opposition to the war and accepting the consequences.

Schwan: Here's a follow up to the story. After serving my sentence, I went into the Peace Corps and was sent to Kenya. While I was there, my folks sent me a package that allowed me to apply for a Presidential pardon from President Gerald Ford. And I really didn't want a pardon. I had done everything they asked. I didn't go to Canada. I didn't refuse to report. I reported, I did my alternate service, went through my probation. I had completed everything that the government wanted for me to do as someone who opposed the war. I wrote back to my parents and I said, "I'm not going to do this. Ford just gave Nixon [President Richard Nixon] a pardon. I don't want a pardon from him."

My parents didn't handle that very well. I got a letter back about two weeks later. When I lived in Kenya during the mid-1970s, remember, there was no internet, no email. Basically, communication was by aerogram that took two weeks to get to the States and then two more weeks for a response. It was a month between. By then, I had rethought the pardon offer, and my mom wrote, "You'll never be able to get a job. You're a convicted felon. Please, for your mother, accept this pardon." So, I agreed, and I have a Presidential pardon from President Ford.

There's one more, small footnote to the story here. When I moved to Hamilton, Montana, I decided to get a few things framed. To this day I have never put them up, but at the time I went to the local frame shop here with my Ph.D. diploma, a variety of other diplomas and other things including my pardon. When I got the call to come and get them, lo and behold, the only one they put on the wall for

everybody to see was my pardon for crimes committed against the United States of America. That's what they put up on the wall. Not that the fact that I got a Ph.D. at Berkeley or did a postdoc at Yale. The pardon is what they put up on the wall. I found that most interesting.

Harden: What was the alternative service you were required to do?

Schwan: There were two choices, two options. One was to do a halftime position for no pay, and since that allowed you to do something with the other 50% of your time, I chose that one. I was able to continue to do a little teaching at Cal State Hayward while I did my required alternate service working with the California Department of Health. That was acceptable to the probation office. I did two years of alternate service working for the Health Department. I was able to work with a group and an organization that really helped me develop my interests in vector-borne diseases.

Harden: Let's talk about your master's program at Cal State. Tell me about your work and who influenced you there.

Schwan: When I graduated with my bachelor's at Cal State Hayward, I initially applied for a master's program at the University of Alaska because I really, at that point, had decided I wanted to be a mammologist. I wanted to work with wild mammals. And so I applied to the University of Alaska in Fairbanks to do a master's program and work on the metabolic physiology of Arctic foxes. I wanted to study how Arctic foxes adapted to the Arctic environment and seasonal changes in temperature. But what happened was that the University of Alaska lost my application, and I never heard a thing from them. Eventually they admitted they had lost my application. But at that point I decided to do my master's degree at Cal State Hayward.

I already had good relationships with members of the Cal State Hayward faculty, and as I said before, I had been working on a project looking at the potential hazard that gulls posed to aircraft around San Francisco Bay. My professor on that project—Dr. Howard Cogswell—also wanted a graduate student to look at the movement of starlings, because there were massive numbers of starlings that followed similar patterns that the gulls did between roosting and going to dumps. And again, they could pose a hazard to aircraft. At that point, since I wasn't going to a program in Alaska, I felt like I needed to get moving. So I accepted the project and I worked for about two weeks following starlings around the dumps San Francisco Bay, but my heart just wasn't in it. I just couldn't see myself doing this for a master's degree. It was hard to go back and tell Dr. Cogswell that it just wasn't right for me.

Next, I went to an entomologist and said, "I'd be interested in studying fleas on mammals. Would you take me on as a student?" His name was David Horn [Dr. David Horn] and he subsequently moved to Ohio State, but at this time, he became my master's thesis advisor. He was an entomologist from Cornell. I began working on the fleas of small mammals in the grasslands of coastal California, and that became my master's thesis. I have to say, looking back and reading the thesis, it's not very impressive.

But I got it done. What really turned things on for me was collecting these fleas and seeing them scurrying through the hair on these animals. I was live-trapping mice, removing the fleas, and then releasing the animals. Then I took the fleas back to the lab and processed them, so I could identify them with the microscope.

I was hooked. Fleas are fascinating organisms to view with a microscope. I also learned that some fleas play an important role in the transmission of the plague bacillus, Yersinia pestis, that causes bubonic plague. I thought, "Here's a way for me to be a mammologist. I can work with mammals. I can be an entomologist and work with the fleas that are on them, and maybe I can do something of public health significance in relation to plague." So that's how it all came together. I did a master's thesis on the fleas of grassland rodents. It's a Master of Art rather than a Master of Science. I didn't have to do a foreign language at the time. I'm not sure if that was the difference, but any rate, I have a M.A. rather than a M.S.

At the same time, I was also earning money as a teaching assistant. I was helping with botany classes and anatomy and physiology classes. And then for one year, I taught some of my own classes. It was a great experience along with doing the master's work.

Harden: Your first publication was in 1975 in the Journal of Medical Entomology, "Flea reinfestation on the California meadow vole (Microtus californicus)." I presume it came from your master's thesis work?

Schwan: That's right. I struggled with getting my work published, and this paper was the first I had submitted to a scientific journal. The paper started out much longer and, I thought, more interesting. But it got hammered by two reviewers because of some of the things I had included, but the data published were solid. Clearly, I was trying to make more of it than probably was there. But that experience taught me right off the bat, how nasty anonymous reviewers can be. I mean, both of the reviewers for that paper were awful, just so unkind. One called me lazy, just insulted me no end. It was really hard to persist, but I finally got a part of it published as part of only one page in the journal. That's all it is, and it is of little scientific significance. But the entire process taught me a lot. It also taught me how not to treat authors when I reviewed their manuscripts for journals.

Early on, I published a variety of things that, looking back, were not very significant—and that's being generous—but they were important efforts for me to learn how to deal with the whole process, from the writing, to the illustrating, to dealing with the journals and the reviewers.

Harden: Did you have much support from Dr. Horn?

Schwan: No, at that point, he had gone to Ohio State, and I was through at Cal State Hayward. But as my advisor for my master's thesis, he was terrific.

Harden: You were your own?

Schwan: Yes, I was on my own. I learned subsequently who both of those reviewers were for my first paper, and they both came to respect me. They both have passed away now, but eventually, as I did more work, they realized I wasn't so lazy and as dumb as they accused me of being.

U.S. Peace Corps teacher training, Kapsabet, Kenya. Far left, Tom; August 1974.

Harden: Let's go to Kenya from 1974 to '76, when you were a Peace Corps volunteer. Tell me about your assignments there and what you did in those years.

Schwan: When my wife, Ginger, and I were at Cal State Hayward, we were contacted by the Peace Corps office in San Francisco, asking us if we'd be interested in teaching at the Pahlavi University in Shiraz, Iran. We wanted to go into the Peace Corps after I had completed my alternative service and probation, and we said that posting would be fine. They said they needed a biologist and a chemist to teach at the university, and we had those credentials. We put our applications together and waited. In the meantime, our house was broken into and gone through extensively, but nothing was taken. Everyone that I told this story to back then—and even today—say, "It must have been the Iranian Secret Service that did this."

Our application was rejected, even though we had been asked to apply. We thought, okay, so much for the Peace Corps. But a few months later while living with my brother Mark in Fairbanks, Alaska, we were contacted again about a program to teach secondary school in Kenya. We went to Chicago and met with all the prospective volunteers and Peace Corps recruiters. I'll cut the story short. It was a struggle to make the decision to go to Africa for two years, but we did it. And we went over with a group to teach secondary school but teaching didn't work out because of a placement issue. Instead, we were posted in a National Park where they needed a biologist and a chemist to continue ecological monitoring.
For two years we worked in Lake Nakuru National Park. At that time, I was also able to do some work on small mammals and fleas in relation to plague. Our primary posting was to continue an ecological monitoring program at the lake because there had been a crash of the blue-green algae population in

the lake prior to our arrival, which is the primary food of the Lesser Flamingo. With the crash of the algae, approximately 1 million plus birds left. That meant that tourists lost interest in going to Lake Nakuru, which was famous for the Lesser Flamingos. Our assignment was to continue the work that German scientists had begun three years previously. For two years we maintained the ecological monitoring of the lake's fauna and the lake water in relation to flamingo ecology. While I was in Kenya, I applied to graduate schools because after this posting, I wanted to come back and go right into graduate school.

Harden: What attracted you to this kind of work?

Schwan: Just like I loved to fish, I loved working in the field. Every time I went into the field, I learned something. I saw something new, almost every time. And I enjoyed having animals in my hand, looking at them closely, and then releasing them.

Early on, I knew I wasn't a lab person. I was a field person. I had to take organic chemistry at Cal State Hayward. I remember one day—I don't know if I can say this—I was in the men's room at the urinal and here came Dr. John Shelton [Dr. John Clarence "Pete" Shelton], the professor of organic chemistry. He takes the urinal right next to me. And I'm thinking, "What's going on here?" Then he looks at me and says, "Schwan, I don't know how you did it." And I said, "What's that Dr. Shelton?" He said, "You got the highest grade in lecture and the lowest grade in lab."

You see, in my lab work, milk didn't have any lactose in it; I couldn't find it. Coffee didn't have any caffeine; I couldn't extract it. I was terrible in lab, but I was okay in the field. I felt much more rewarded when I could be a biologist in the field than a chemist in the lab. Now, of course, that all changed when I went to Yale and then came to RML [Rocky Mountain Laboratories, NIAID, NIH]. Early on though, the lab wasn't my strength by any means. I was best in the field.

Harden: Let's come back to your Ph.D. program at the University of California, Berkeley. You obviously were considered an outstanding student, as you had all sorts of fellowships, grants and awards that supported your work along with your continued teaching responsibilities. So tell me about this part of your education. Who mentored you, who else was important and nudging you forward.

Schwan: When I was in Kenya, I had to decide where to apply for graduate studies. I went to Nairobi, where there was a library that had just about every college catalog from around the world that you could imagine. I looked through as many college catalogs from North America that had the programs I was interested in, those which would allow me to combine my interest in mammals and ectoparasites and maybe diseases but also did not require the Graduate Record Exam [GRE] because I never did well on standardized tests. I applied to the two programs that I found that had what I wanted and did not require the GRE. They were the UC Berkeley [University of California, Berkeley] Interdisciplinary

Graduate Group in Parasitology, and the Johns Hopkins School of Pathobiology. I applied to those schools and was accepted to both with faculty agreeing to take me on as their student.

As it turned out, I couldn't afford to go to Hopkins but as a California resident, I was able to attend University of California, Berkeley, which was not a financial hardship. This was all arranged while I was in Kenya. I left Kenya in August of 1976, and a week and a half later, I was sitting in a class at UC Berkeley, taking a class in entomology.

I was very fortunate at UC Berkeley. My thesis advisor was Dr. Deane Furman [Dr. Deane P. Furman], who was internationally known for his taxonomic work on mites. I turned out to be his last Ph.D. student. He was "hands off" with his students, so his students had to be very independent. In all the years he taught at Berkeley and had graduate students who produced doctoral theses, he never once co-authored a paper with a student regarding his or her thesis work. He did his taxonomy, and his students did their projects. That just wouldn't happen today. But because he was so hands off, I was not very happy during my first year there. I had hoped that I would have a mentor and a supervisor who would take me under his or her wing and help me develop as a scientist. Although Dr. Furman was always there in his office when I had a question, and he was honest and straightforward and a gentleman, I really wanted more. Luckily just down the hall was the Professor named John R. Anderson [Dr. John R. Anderson]. He goes by "Andy."

Mt. Palomar State Park, California, setting mouse trap for flea study; November 1981.

Andy took me under his wing. He hired me on a mosquito project, and I helped on his project on dung beetles. If it weren't for Andy, I probably would have left Berkeley. Just like Dr. Edward Lyke, who turned things around for me at Cal State Hayward, Andy really made me want to stay at Berkeley and work with him. And so I helped him early on, studying tree hole mosquitoes. He was not my thesis advisor, but he was the chairman of my graduate committee and oversaw my written and oral exams. He and his wife Shereen would have me over to dinner. We worked in the field together and published several papers together. He made all the difference. Andy is 90 years old now and we still keep in contact with each other.

Once I decided I was okay at Berkeley because of Andy, I began to learn about the other people there and make relationships with other graduate students. Then it turned out to be a terrific experience. I was fortunate to receive several small grants and fellowships that helped me attend meetings and

workshops, although I never had a scholarship that provided a monthly stipend to support my research. Thus, I worked first as a Research Assistant and Teaching Assistant for Andy, and later I also worked as a Student Assistant with the California Department of Public Health, helping on the plague surveillance program and identification of fleas for testing.

I also went through a divorce early during my time at Berkeley, which was difficult. My work and focus got derailed for a little bit, but it was for the best and I grew a lot personally out of that.

Harden: As you finished your Ph.D. in 1983, the historic Rocky Mountain Laboratory Tick Collection was sent to the Smithsonian National Museum of Natural History, along with James Keirans [Dr. James L. Keirans] to be its curator. Were you aware of this at this time? Did this even impinge upon what your knowledge about the field of arbovirus research and how the tick collection was used in it? Or was there any comment about the move among the faculty at Berkeley?

Schwan: My thesis advisor, Dr. Dean Furman, worked on a large monograph on the ticks of California while I was his student. That was his last big project. He actually went to the Rocky Mountain Laboratory on sabbatical, spent nine months here in Hamilton. And this was before the collection of ticks was moved to Silver Spring, Maryland. Because of this, I was well aware of the tick collection here at RML. I was also following the CDC's [Centers for Disease Control and Prevention] Morbidity and Mortality Weekly Reports [MMWR] on Lyme disease and what could or could not be causing it. I helped Dr. Furman, preparing maps for his monograph on the ticks. We were very well aware of what was happening in Montana and concerned for sure.

Berkeley, California, ready to drive to New Haven, Connecticut to begin postdoctoral fellowship at Yale University. Behind me (left to right) are my father, Creighton, mother Beverly, and grandmother Freda; August 1983.

Harden: When you finished at Berkeley, you went to the Yale Arbovirus Research Unit [YARU], which in 1964 had taken over from the Rockefeller Institute as the predominant national arbovirus research institution. Tell me about your work at Yale, what you studied and what you learned.

Schwan: I went to Yale to learn how to work on arboviruses and gain experience working in the laboratory because, as I said before, I had never been successful in the lab. To me, the prospect of working at the arbovirus lab at Yale was very exciting. I had heard about people there, like Jordi Casals [Dr. Jordi Casals i Ariet] who got infected with Lassa virus when he and his colleagues were working to isolate and identify the virus back in

1969. I knew a lot of the folks through the literature, but I didn't know them personally, such as Wilbur Downs [Dr. Wilbur G. Downs] and Tommy Aitken [Dr. Thomas H. G. Aitken] from Rockefeller, who studied yellow fever in various places. The faculty there was just amazing, but my projects started off one by one, learning techniques, because at the Yale Arbovirus Research Unit, each of the faculty were experts on numerous virological techniques to support their research.

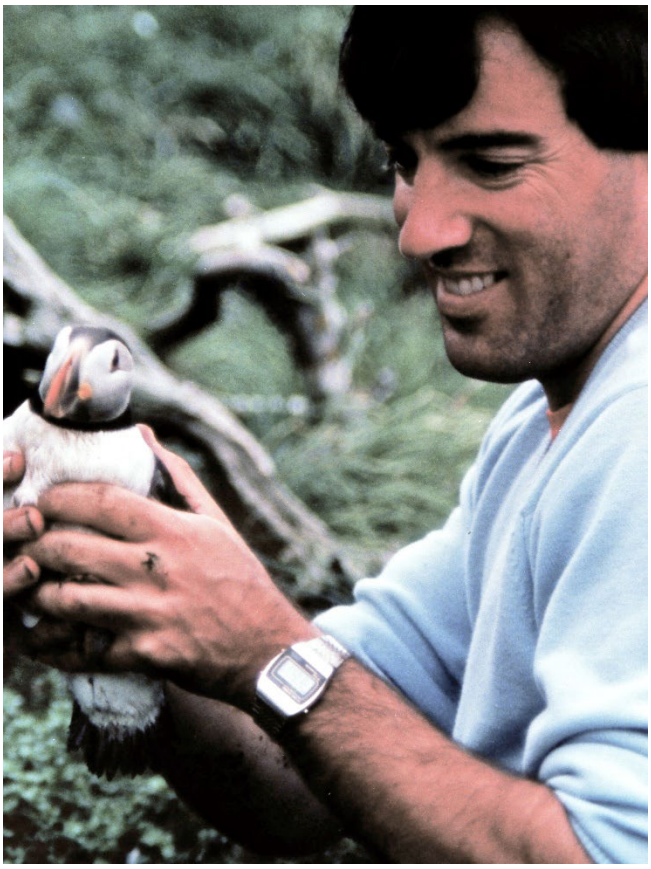
I worked primarily with Dr. Andy Main [Dr. Andrew J. Main, Jr.], but also at times with Dr. James Olson [Dr. James G. Olsen] , Dr. Robert Shope [Dr. Robert E. Shope] and Dr. Robert Tesh [Dr. Robert Tesh]. These individuals taught me numerous techniques to work on arboviruses and perform serological tests.

I arrived at Yale just before the cause of AIDS [acquired immune deficiency syndrome] had been discovered. Dr. Shope had me testing serum samples from a large population of gay men in Florida, to see if they had antibodies to certain mosquito-borne viruses that might link these mosquito-borne viruses to AIDS.

Mono Lake, California; ready to raft to an island to collect ticks for virus isolation at Yale University; January 1984.

Although I helped on that project, my own work focused on ticks and viruses from Mono Lake, California. When I was at Berkeley, I had begun a project on the ticks on islands in Mono Lake, a highly saline lake in eastern California. I started that work in 1981 with the help of David Winkler [Dr. David W. Winkler], a zoology graduate student at the time. I went to Yale in 1983, and then I was able to focus on the biology of these ticks and the viruses that these ticks transmitted to the birds and possibly to humans. A lot of my work was based on working with the ticks from Mono lake in the laboratory—

working out the life cycle, doing transmission studies of Mono Lake virus, which is the virus that these ticks transmit. And that really got me interested in tick-borne viruses.



Holding an Atlantic Puffin, Great Island, Newfoundland, while collecting ticks from burrows for virus isolation at Yale University; July 1985.

At Yale, as I said, I learned a lot of techniques and laboratory skills, and it was exciting to be there. I enjoyed living in New Haven for three years, and all of the faculty were tremendous and so willing to help me. I was supported by an NIH Training Grant awarded to Dr. Francis Black [Dr. Francis L. Black], an international measles expert. He would funnel out training grants to different units, and I was able to be hired for the first two years on his NIH Training Grant given to the Yale Arbovirus Research Unit. Again, I worked primarily on tick-borne viruses from California. We also did field work in Newfoundland and isolated viruses from ticks we collected from seabird colonies there.

Harden: When you finished your postdoc in 1986, you came to RML. Were you aware of the upheaval that had been occurring as NIAID decided to phase out its historic focus on medical entomology and instead emphasize molecular biology?

Schwan: I was aware of it but did not get involved. When I was still a student at Berkeley, I developed a friendship with Dr. Harry Hoogstraal in Cairo, who you may know was the world's leading tick expert. Harry was very concerned because he had a long relationship with the scientists at RML: Jim Keirans, Carleton Clifford [Dr. Carleton M. Clifford], Conrad Yunker [Dr. Conrad E. Yunker], and Glen Kohls [Dr. Glen M. Kohls]. Working together, these taxonomists and Harry described nearly a quarter of the world's tick fauna at that time, which was amazing. I totally understand that the NIH in general and NIAID in particular no longer needed to be in the tick museum business. I totally understand the move of the tick collection, although it was difficult for those folks whose work focused on tick identification, taxonomy and species relationships. There was much concern about it and many people I believe wrote letters to their senators and congressmen trying to stop it. I don't know details. When I got to RML in '86, it was a done deal by several years.

As I said earlier, I had first become aware of this shift during the latter years of my graduate school training because my thesis advisor, Dr. Furman, was working on a tick monograph. I started at Berkeley in '76. In 1978, I met Dr. Hoogstraal at a summer acarology course in Columbus, Ohio. That started a

wonderful relationship with Dr. Hoogstraal that lasted until he passed away in 1986. He was a tremendous man, generous, helpful to young scientists, and such a force in the field. We still miss him.

Harden: Let's go back to your recruitment at RML. I want you to tell me how it took place. With whom did you talk? Since John Gallin [Dr. John I. Gallin] was NIAID Scientific Director at that time, he must have made you the final offer. Was the recruitment a long process? Did you already know people? Tell me about being recruited.

Schwan: Sometime in 1985, I had a phone call while I was at Yale from a friend of mine, Glen Needham [Dr. Glen Needham]. He was on the faculty at Ohio State. He was a tick physiologist, and he was involved every summer with the acarology program that was hosted at Ohio state. Willy Burgdorfer [Dr. Willy Burgdorfer], who was a senior tick scientist at RML, was also involved with quite a few of these summer programs. Glen would invite Willy to come to Columbus and spend three or four days there talking about tick-borne diseases. In 1985, Willy mentioned to Glen that he was going to retire at the end of '85 and that there would be a slot opening up for someone at the Rocky Mountain Laboratories. Glen thought I might be interested.

I was interested, so I wrote a letter to Dr. Burgdorfer and told him that I had heard from Glen that he was going to retire and that a position at RML might then be open. I said, "If there is a chance to apply for a job at the Rocky Mountain Laboratories, I'd be very interested." And Willy wrote back and said, "I am going to retire and there should be a slot open, but there's a hiring freeze, so I don't think at this time there's anything that can be done." I said, "Okay, thanks," and that ended that for a few months. Then I got another letter, from Claude Garon [Dr. Claude F. Garon], saying that the freeze was going to be lifted, and if I wanted to apply, I should. So I sent letters to Willy and Dr. Garon saying that I'd be interested. Dr. Garon was Willy's chief at the time. I applied with a letter of intent and my CV. I was one of three people interviewed for the position and my interview was in early January of 1986.

Harden: At RML?

Schwan: At RML. I flew from New Haven, Connecticut (actually from Newark, New Jersey). I spent two days at RML, a Thursday and Friday, where I met with various people. On the first day, I gave a seminar on the work I'd been doing on viruses associated with Argas ticks and California gulls at Mono Lake. That Friday was also the day that Willy Burgdorfer retired. I remember sitting in the back of the old seminar room where they had a celebration for Willy.

It was not too long after that, when I was back in New Haven, that I got a letter from Claude Garon saying that I was being offered the position. And you were right. Eventually, I got a letter from Dr. Gallin offering me the position as a Staff Fellow. My interview was January 1986, and I came on duty at the lab July 6, 1986.

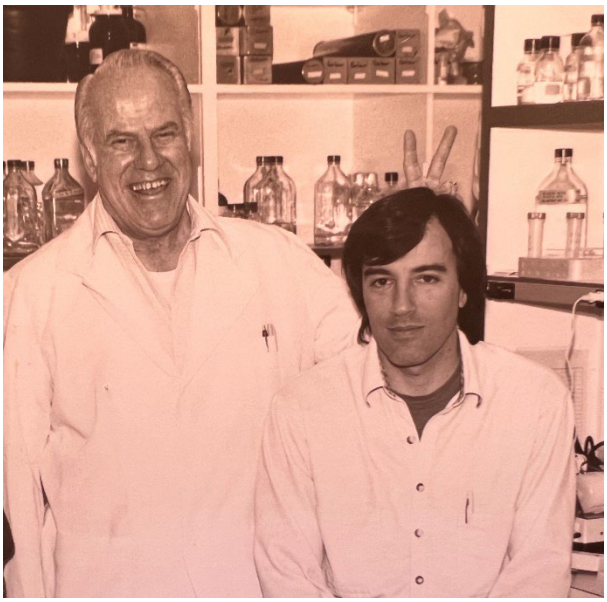
Harden: Now I want you to think back to when you arrived and tell me about your impressions of RML in general, at that time. Who was there? Was there anything especially interesting? Who were the people in the labs that you remember? I want you to create a picture of RML when you got there.

Schwan: There were four major laboratories. John Swanson [Dr. John L. Swanson] was Chief of the Laboratory of Microbial Structure and Function. That laboratory was a big group. Bruce Chesebro [Dr. Bruce W. Chesebro] was Chief of the Laboratory of Persistent Viral Diseases. At some point, not long after I got there, Harlan Caldwell [Dr. Harlan D. Caldwell] separated from John Swanson's lab and became Chief of the Laboratory of Intracellular Parasites. The lab I joined was, at the time, called the Laboratory of Pathobiology. Claude Garon was the Acting Chief. So there were four basic laboratory units. After a couple of years, Claude was made Chief rather than Acting Chief of the Laboratory of Pathobiology. At that time, the lab changed its name to the Laboratory of Vectors and Pathogens, and I was a Senior Staff Fellow in that laboratory.

I was provided two wonderful technicians, Merry Schrumpf [Merry E. Schrumpf] and Robert Karstens [Robert H. Karstens]. And I was able to hire summer interns and then eventually my first postdoc. I had a lot of help from some of the other folks because suddenly, I had to learn how to be a microbiologist. My job—the reason I was hired—was to be a medical entomologist who could apply molecular techniques to understand how these pathogens interacted with their arthropod vectors. I had much to learn, like how to purify nucleic acids, run protein gels, and perform immunoblots; how to do PCR [polymerase chain reaction], microscopy, and how to work with animals experimentally. I learned how to do almost everything one would do in the lab, except that I never did my own DNA sequencing. I had very good people in my lab who knew how to do this, so I didn't see the point in my doing sequencing, but I tried to learn everything else that I could.

In my first office at RML; Spring 1987.

When I first came on board, Alan Barbour [Dr. Alan G. Barbour] was still here, but he was basically packing up to move to San Antonio, Texas. Alan left in September/October of ’86, and I was given his laboratory space and his technicians, Merry and Bob. I even rented Alan’s house in Hamilton, and by the end of the year I bought his house. He and his wife Ann and two boys lived just a few blocks from the lab and this is where I lived for the next 22 years.



Dr. Willy Burgdorfer "playing" with me in the lab, RML; 1987.

When I started at RML, I may have made a mistake by wanting to work on too many things. I wanted to work on plague. I wanted to work on spotted fever. I wanted to work on Colorado tick fever virus. I wanted to do too much. But one of the things that I realized was that I was coming into a lab that knew how to work on Borrelia. The resources there to work on Lyme disease spirochetes were incredible. Both technicians who were going to work with me were highly skilled and had helped Alan Barbour with much of the early work on the Lyme disease spirochetes. And so when Alan left and Willy was there as Emeritus—although Willy had retired in January, he was still working at the lab as a Scientist Emeritus--Willy became my new mentor. He took me under his wing and helped me learn how to work with Borrelia. We collaborated on several projects for the first few years. It was wonderful working with him. He was great, so generous and helpful, and he cared about my career development. I was very fortunate.

Dr. Gallin was the Scientific Director at the time. Back then, and, in fact, the whole time I've been at RML, I experienced many administrative changes to the layout of RML, and the administrative units to which I belonged. It was bit of a rollercoaster over the years, and we can get into that if you want.

Being presented with the Federal Laboratory Consortium Award for Technology Transfer for Lyme disease serological test. Left to right, Dr. Philip Chen, Dr. John Gallin, Tom, and MaryAnn Guerra; April 1993.

Harden: I do, but first, tell me about your research to detect the agent of Lyme disease using monoclonal antibodies and nucleic acid hybridization probes. This is the research that permits scientists to distinguish Lyme disease Borrelia from relapsing fever Borrelia. And if I'm reading your CV correctly, this work led to patents in the 1990s and to your 1993 Award of Merit for Excellence in Technology Transfer. Would you tell me about this work?

Schwan: One of the things I thought about early on was the need for rapid diagnostics in relation to Lyme disease. I had believed from the time I joined the lab until the time I retired, that since RML is a part of the National Institutes of Health, a part of our obligation is to try to do something to improve the public's health. Since I was not an M.D., I believed that one of the things I could provide help on was rapid detection of pathogenic organisms. The faster an organism could be detected and identified directly in a patient, the faster physicians could apply appropriate therapeutic techniques. And so a lot of the work that I started soon after I got to the lab was trying to come up with improvements to detect and identify various vector-borne pathogens, including those that cause relapsing fever. You could see Borrelia spirochetes in a blood smear, but you just called them what you thought they were. There were no techniques to identify them accurately.

There were a lot of people working on Lyme disease in 1986. I believed that there was a need for accurate rapid identification and improved serology. And so—this is what I thought personally, not anything I was instructed to do by NIH—I believed that with Lyme disease, if one could improve the serological diagnostics and identify cases accurately, they could be treated appropriately and quickly, and this would be a tremendous benefit to the public's health.

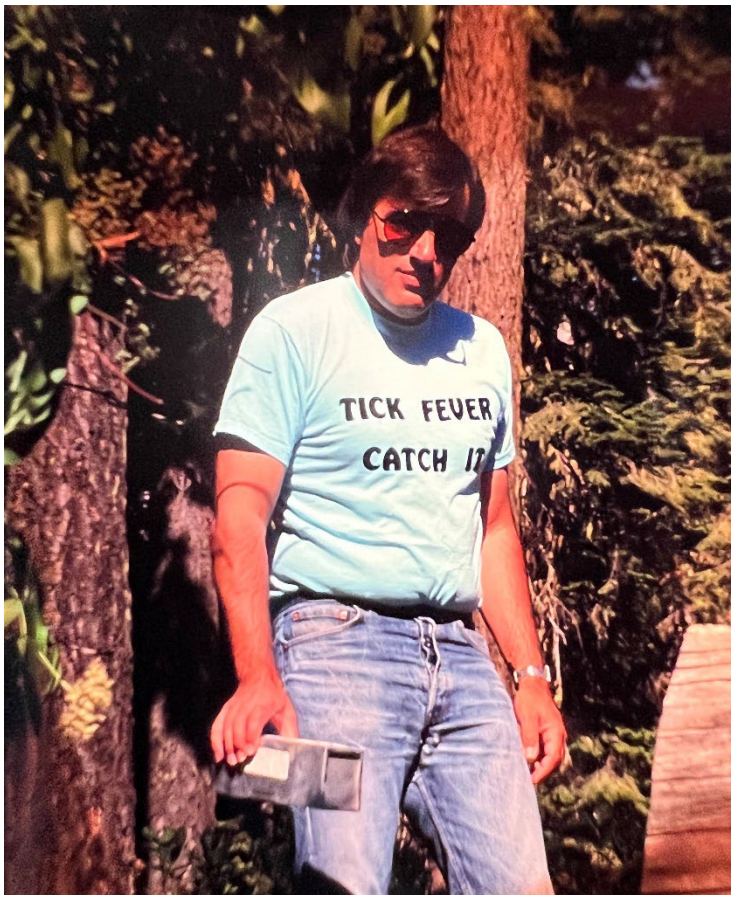
Luckily, in this work, I was able to recruit a postdoctoral fellow who was a New Zealander, a Kiwi. I met him in Minneapolis when I went to give a seminar at the University of Minnesota. When I met Warren Simpson [Dr. Warren Simpson], he was about ready to look for another postdoctoral position. I asked him if he might be interested in working on Borrelia at RML. His first question was, "Do they have a McDonald's in Hamilton?" I said, “Well, no, not yet. They do in Missoula, but not in Hamilton.”

He came despite the lack of a McDonald’s in Hamilton, and he was a tremendous molecular biologist. I learned a lot from him. He just loved molecular biology. It wasn't too long before he was able to create an expression library for Borrelia burgdorferi DNA in E. coli. He would enzymatically cut up Borrelia burgdorferi DNA, clone it into E. coli and then examine these bacterial clones for reactivity with human Lyme disease serum samples to see what proteins might be reacting in these clones. One of the Borrelia proteins that we found from this expression library was very reactive. We called it P-39 because it had a molecular mass of about 39 klilodaltons. It was quite immunogenic. And from our work, we could see that people who had other diseases caused by spirochetes did not react to it. People who had relapsing fever didn't react to it; people who had syphilis didn't react to it; people who had leptospirosis didn’t react to it. It was highly specific for Borrelia burgdorferi, the spirochete that causes Lyme disease.

This work led to the patent application for a diagnostic antigen. Warren and I were the co-inventors. And of course the Institute held the patent. It took a while for that to happen, but it felt good to actually do something in the lab that got developed into a product that was being used to help in the serological diagnosis of Lyme disease.

Harden: In the New York Times today there was an article about chronic pain and infection related to what some people call “chronic Lyme disease.” Other people say that there is no such thing as “chronic Lyme disease,” because no Borrelia burgdorferi organisms can be detected. Are you willing to comment on this controversy?

Schwan: I'll just say it's a really complex issue and because I am not trained in medicine and have never treated or cared for patients in this category, I don't feel qualified to respond.



Ready to set a rodent trap for relapsing fever project, northern California; September 1990.

Harden: In 1993 you were named the R. R. Parker Memorial Lecturer by INCDNCM, the International Conference on Diseases of Nature, Communicable to Humans. What surprised me was the topic on which you chose to speak. Instead of your work on Borrelia serology, you spoke on, "A Personal View of Fleas and Plague During the Last Two Decades." Why did you choose that topic?

Schwan: It was a topic still near and dear to my heart. Even though I had pretty much stopped working on plague, I decided it was something I wanted to talk about because I had spent a lot of years working on fleas in relation to the ecology of plague. In my early work at RML, one of the first things I did was to set up flea colonies. There were no flea colonies when I came to RML in 1986. So I read about flea colony protocols, and their diets as larvae – the adults feed on blood of course. In the insectary, I colonized three species of fleas. That permitted us to develop rapid diagnostic techniques to detect and identify the plague bacterium in fleas.

This work was possible because I got another wonderful postdoc, Joe Hinnebusch [Dr. B. Joseph Hinnebusch]. When Joe joined our lab, I presented a variety of projects that he could work on within the focus of the laboratory. These included Lyme disease spirochetes, relapsing fever spirochetes, and plague. And, although we did some things on Borrelia together, he was interested in working on the plague bacterium, Yersinia pestis, because of the ability to genetically manipulate this bacterium, to knock out genes to determine if there were phenotypic or biological changes in the mutants. At that time, you couldn't do that with spirochetes. Joe was very interested in looking at the interaction of plague bacteria with fleas to understand what possible genetic components were important for flea colonization. When Joe became a tenured investigator and throughout his career, he and his laboratory coworkers have investigated how Yersinia pestis has evolved and adapted to fleas for efficient transmission.

I chose fleas and plague as the topic for my INCDNCM Parker lecture. I didn't want people to think that I never did anything on plague or fleas. I had had a keen interest for a long time. I did my Master's thesis on fleas. I did my Ph.D. dissertation on fleas. I started the flea colonies at RML, and I had worked on plague in fleas at RML. I loved the work and I wanted to talk about it.

Harden: Did you happen to get to know Bill Jellison [Dr. William L. Jellison], who was the flea expert at RML for many years?

Schwan: Yes, I did. He was a dear fellow. He lived only a few blocks from the lab and I met him soon after I moved to Hamilton in 1986. He was quite elderly at the time. The problem with Bill was that he could talk your head off and at times it was difficult to squeeze in a question or two.

Harden: Yes, how well I remember. He had seen to it that Hamilton preserved so many of the artifacts about Rocky Mountain spotted fever research, and he absolutely loved to talk about it all.

Schwan: Yes, he was a wealth of knowledge, but once he got going, it was hard to stop him. l loved getting to know him, and I was the one that received in his name the 1991 Harry Hoogstraal Medal for Outstanding Achievement in Medical Entomology from the American Society for Tropical Medicine and Hygiene. I went to the meeting and brought the medal back to Hamilton for him since he was unable to attend. We did a ceremony at his home at which I gave him the medal, and there was a story about it published in the Ravalli Republic, our local newspaper.

Presenting Dr. William Jellison (right) with the Harry Hoogstraal Medical awarded by the American Society of Tropical Medicine and Hygiene; December 1991.

Harden: That's nice. One more sideways question here about the tick collection. After Dr. Hoogstraal donated his collection to augment the RML collection, it was all moved to Georgia Southern University. Was there any commentary at RML about why it was moved and whether it mattered? I don't know that there was, I'm just curious.

Schwan: I don't remember anybody here at the lab concerned about that. When I came in '86, there was nobody at the lab that had any interest in the tick collection. This collection is in good hands at Georgia Southern University with Dr. Lorenza Beati as Curator and Dr. Dmitry Apanaskevich as Assistant Curator. The specimens in this collection are being used by many researchers to improve the taxonomy on ticks from around the world.

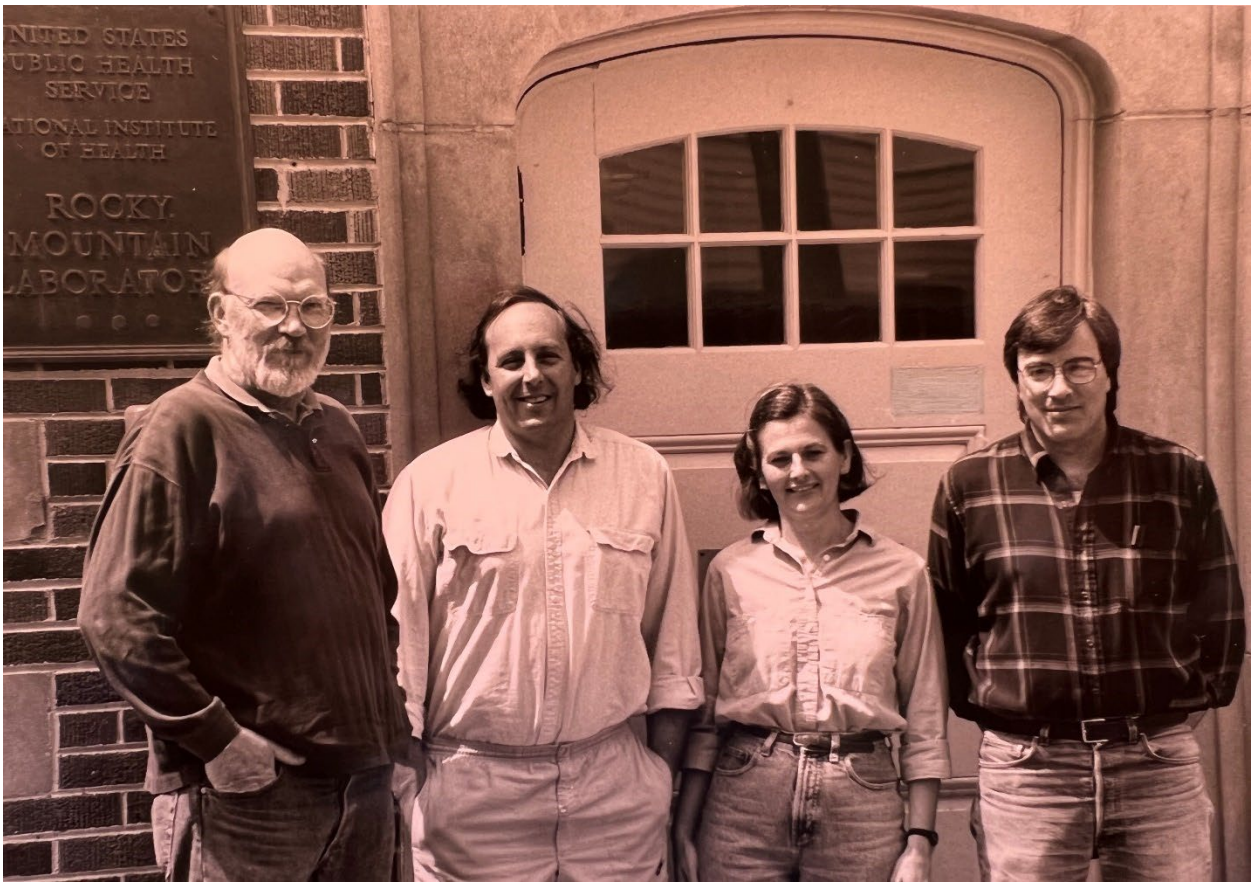
Harden: Thanks. It has been curious to me why it kept moving around.

Schwan: Jim Oliver [Dr. James H. Oliver, Jr.] wanted it there, and he was a major force in having that happen.

Harden: In July 1994, you married Carol Schwan [Carol Lynn Schwan] and adopted her younger son, Shawn [Shawn Fagan Schwan]. In your work life, you moved in late 1994 or early 1995 to the Laboratory of Microbial Structure and Function. Tell me about why you moved from the Vectors and Pathogens lab to the Microbial Structure and Function lab.

Laboratory of Vectors and Pathogens, RML. Tom (standing, fifth from left) between Dr. Willy Burgdorfer and Dr. Claude Garon (Laboratory Chief), fourth and sixth from left, respectively; October 1991.

Schwan: That happened when Dr. Gallin reorganized the laboratories. What had been the Laboratory of Vectors and Pathogens, with Claude Garon as Chief and in which I was a Senior Staff Fellow, was disbanded. Claude became Chief of the Microscopy Branch, which was separate from the other laboratories. My group was reassigned to the Laboratory of Microbial Structure and Function, where John Swanson was Laboratory Chief.



After reassignment to the Laboratory of Microbial Structure and Function. Left to right, Dr. John Swanson (Laboratory Chief), Dr. Seth Pincus, Dr. Patricia Rosa, and Tom; 1994.

Another group that had been in the Laboratory of Vectors and Pathogens was moved to Harlan Caldwell's Laboratory of Intracellular Parasites. That group worked primarily on Campylobacter and another group stayed with Claude's Branch. Claude also had postdocs who continued to work on Borrelia, and Claude and his team of microscopists provided electron microscopy support for all the researchers at RML.

Harden: What was your title when you got to John Swanson's lab?

Schwan: John had a different way of handling his folks. When I was in Claude's lab, I was Acting Chief of the Arthropod-borne Diseases Section. When I went to John's lab, my title was Microbiologist with no official section. I wasn't tenured yet.

Harden: But in 1997, you were named Acting Chief of the Laboratory of Microbial Structure and Function. Did Swanson retire? You were in that position for only two years and then moved to the position of Senior Investigator, Laboratory of Human Bacterial Pathogenesis. All these different

organizational names confused me as I was trying to figure out if the changes were important in terms of your research or whether they were just administrative shuffling.

Schwan: I can't say that they affected my research very much, although I did become embroiled in a variety of administrative things that I had not anticipated. What happened was this. I had joined John's lab in '94 and went up for tenure in '95 with John's strong support, and I was granted tenure in the fall of 1995. That period, 1994-95, was when Dr. Gallin stepped down as Director of Intramural Research for NIAID to become Director of the NIH Clinical Center, and Dr. Kindt [Dr. Thomas Kindt] became the NIAID's Director of Intramural Research. When I was reviewed for tenure in the fall of '95, Dr. Kindt was the DIR [Director of Intramural Research, NIAID], and John Swanson and I worked with Dr. Kindt in to put my tenure package together.

Then, in 1997, one day when Dr. Kindt was visiting RML, I got called down from my lab to see him in a conference room. He wanted to see me in 10 minutes. So, of course I went down to see him, and he said to me, "Tom, we're asking you to step up and be Acting Chief of the Laboratory of Microbial Structure and Function." This shocked the heck out of me and I asked, "Does Dr. Swanson know this?" And Dr. Kindt said, "Yes, we discussed this earlier before you came down." Thus, there was a very abrupt change in the chiefdom for the lab. And suddenly in one day, John Swanson was now a PI [Principal Investigator] in Harlan Caldwell's lab and I became Acting Chief of the Laboratory Microbial Structure and Function. This generated a series of nightmares I had for about the next six months. However, I believe this change was part of the plan to recruit a new permanent lab chief, and John had discussed with Dr. Kindt his interest in stepping down as chief in the not too distant future. I also want to say that during this time, John never came to me and told me what to do. And yet, whenever I needed his help and advice, John always had time for me and was most gracious. He was a tremendous help for me during this transition.

Harden: Ah, to be a fly on the wall when all that happened. At that point, how many people did you have to oversee, and what were they doing?

Schwan: This is where it made things difficult because up until that day, I had been all these people's colleague. And suddenly I was their Acting Chief. I'm not sure how well that went over with everyone. They were fine when I was just the same as they were. But I sensed a little bit of—I won't say hostility. I never felt any hostility, just a little discomfort and some resentment from a few folks.

Patti Rosa [Dr. Patricia A. Rosa] and her group were working on the Lyme disease spirochete. Bob Belland [Dr. Robert J. Belland] worked on Neisseria gonorrhoeae. Seth Pincus [Dr. Seth Pincus] worked on HIV and had some interests in bacterial pathogens. Jos van Putten [Dr. Jos P. M. van Putten], also worked on Neisseria. And my group worked on several vector-borne bacterial pathogens. Then the administration began a recruitment to identify a permanent lab chief to replace John (and me!) that resulted in the hiring of Dr. James Musser [Dr. James M. Musser]. But during the transition, it was stressful for me and it certainly did distract me at times from my own research projects.

In 1999, Dr. Musser came onboard as Chief of the newly created Laboratory of Human Bacterial Pathogenesis, which replaced the Laboratory of Microbial Structure and Function. I became a PI in that new lab. So, at this point, I had had Claude, then John, and now Jim as my lab chief. Jim came on board, was strongly supported, and built a big program. He and his large group were very productive in the number of publications. Then one day in 2003, he came to my office and told me that he was leaving RML to return to Houston.

Harden: Whoa.

After reorganization and reassignment to the Laboratory of Human Bacterial Pathogenesis, RML. Left to right, Dr. Joe Hinnebusch, Dr. Frank DeLeo, Dr. Michael Otto, Dr. James Musser (Laboratory Chief), Dr. Patricia Rosa, Dr. Robert Belland, and Tom; 2000.

Schwan: Suddenly, I once again became an Acting Chief. There I was again, trying to keep things going, keep the boat from sinking, and keep my own work going forward as best I could. But it was a bit more complicated this time as Jim, his new institution, and the DIR worked out an interagency agreement for Jim to continue to supervise all his folks at RML from Texas, with occasional visits to Hamilton.

Harden: Was he headed to the University of Texas Medical Branch [UTMB} in Galveston?

Schwan: No, he went to a large teaching and research hospital in Houston.

Harden: Let me step sideways once more and ask you about the shift of the Yale Arbovirus Research Unit to UTMB in Galveston. Having worked in this unit at Yale, can you tell me why it moved to UTMB? And as an additional question, I would ask whether you happen to know David Walker [Dr. David Walker] at UTMB?

Schwan: I don't know what happened to cause this move, but I think that both Bob Shope and Bob Tesh, who were the head guys at YARU, were no longer getting the support they needed and thought they deserved from the Dean. It apparently became a more difficult place for them to do what they wanted to do. And I think David Walker, who was then Chair of the Department of Pathology at UTMB and actively building a program in vector-borne and zoonotic diseases, provided a very attractive alternative to Yale, so they both left. The WHO collection [World Health Organization Arbovirus Reference Collection], went with them. They were both very involved with maintaining that arbovirus reference collection. David Walker, whom I've met only on a few occasions, I believe was very enthusiastic about getting them and the reference collection to Galveston. Dr. Shope passed away several years, but Bob Tesh continues to be a very productive scientist.

Harden: Thank you. I mentioned David Walker because he kindly wrote the foreword to my Rocky Mountain spotted fever book, and I met him when I gave a talk at UTMB. Back to your work at RML. In 2005, after you had served twice as the Acting Chief of a laboratory, you were named Chief, in your own right, of the Laboratory of Zoonotic Pathogens, where you stayed until you retired in 2014. Would you tell me about the work of the Laboratory of Zoonotic Pathogens?

Schwan: What happened to get my lab created was that one day, Tom Kindt was at RML, and he said to me, "We have a core here that would make a good laboratory on its own with the vector-borne people." That core was Patti Rosa, Frank Gherardini [Dr. Frank C. Gherardini], Joe Hinnebusch and me—three tick people and one flea person. Tom asked me, "How would you feel about creating a new lab with you as lab chief?" And I said to him, "Well, let me think about that." I agreed that we had a good cohesive unit and thought if I said, “No,” I might continue to be an Acting Chief for some years to come.

I accepted his offer, but it was only an idea at that time. Dr. Kindt still had to talk with Dr. Fauci [Dr. Anthony S. Fauci], the Director of NIAID. There was no separate recruitment per se. I don't remember all the details of how it happened, but Dr. Fauci approved it, and I became the chief of the new lab, and we named it the Laboratory of Zoonotic Pathogens. And Kathy Zoon [Dr. Kathryn C. Zoon], who in 2006 took over from Dr. Kindt as Director of Intramural Research, liked it. She liked the LZP acronym, as I did.

Coming out of the attic to collect ticks in a cabin associated with an outbreak of tick-borne relapsing fever, Wild Horse Island, Flathead Lake, Montana; August 2002.

Until I retired, I worked primarily on tick-borne relapsing fever. That became my passion and my focus. And it was during this time that Dr. Zoon and Dr. Barron [Dr. Karyl Barron], Deputy Director of NIAID, supported me in developing a relapsing fever project in Mali, in West Africa, at the research and training center outside Bamako. Dr. Zoon also was very supportive of my having a field project on relapsing fever in Montana. I really appreciated her support enabling my group and me to do some field work. It really saved me in a sense because I finally felt I was doing what I was hired to do back in the 1980s. I was finally able to bring together my interests and skills both in the field and what I had learned in the laboratory. And we accomplished some work I am very proud of.

We had great projects in Mali and in Montana, and I continued to work on spirochete-tick interactions with a primary focus on relapsing fever. Dr. Rosa's group focused almost entirely on the Lyme disease spirochete, looking at how these bacteria could adapt when infecting ticks versus mammalian hosts. Frank Gherardini's projects involved several bacterial pathogens. He worked on Lyme disease projects and recruited a series of young scientists to work on Burkholderia. We had a fairly large Burkholderia program, but it was all done by his postdoctoral fellows, and when they left, work on these pathogens ended.

Joe Hinnebusch and his group worked primarily on plague, on how the plague bacterium has evolved and how the bacterium interacts with its flea vector for efficient transmission. We had four PIs in LZP.

I hoped initially that when the new lab was established, I might be able to recruit someone to work on hantaviruses, but that didn't happen. Instead, the institute began to design and plan for a new group of virologists that would work on viruses that required high containment in a biosafety level four lab [BSL-4]. The idea of our working on hantavirus was no longer valid. That work was started by other folks once the Laboratory of Virology was established and the BSL-4 lab was built. Again, I focused almost entirely on relapsing fever, both in North America and West Africa.

Harden: During this period you were accumulating awards. I was especially interested in the fellowship from the Japan Health Sciences Foundation. I went to their website hoping to learn a little about it, but

it was only in Japanese, which I don't read. So tell me about this organization and what you did under the auspices of that fellowship. Did you go to Japan?

Schwan: Yes, I did go to Japan but basically there isn't much more to tell than that. I went over for a week and visited several laboratories. I gave seminars in Fukuyama and Sapporo. The fellowship was funded by the Japan Health Sciences Foundation, but I didn't live in Japan for any period or collaborate in a serious way. I had helped the fellow who put me up for the fellowship and hosted me. His name is Masahito Fukunaga [Dr. Masahito Fukunaga], and he's a wonderful fellow and now retired. He's the person who described Borrelia miyamotoi, which is the relapsing fever spirochete associated with hard ticks. And when he put his paper together in '94, he asked me if I would work on the paper and improve the English. I said, "Yes," not knowing what I was getting myself in for. He sent me a draft that needed a lot of work. I basically rewrote the whole thing--well, it felt that way--and he acknowledged me for that at the end of the manuscript. I'm not a co-author and did nothing to warrant co-authorship. But the effort took me a couple months with everything else I was doing. It's a landmark paper now, given that Borrelia miyamotoi is known to be a human pathogen in Europe, Asia, and North America. There's a lot of interest in that disease and the ticks that transmit the spirochete.

Harden: Now let's turn to your research in Mali. Tell me when you went, why you went, and what you did while you were there.

Dr. Ben Mans (left) and Tom identifying ticks at Malaria Research and Training Center, Bamako, Mali; December 2007.

Schwan: Because of my two years in Kenya, I had developed a fascination and love of Africa. I knew that there had been papers describing tick-borne relapsing fever in Senegal, which is contiguous with Mali, so

when I realized that our institute had an ICER, an International Center for Excellence in Research, at Bamako in Mali, I asked Dr. Zoon whether we might be able to start a project there. One primary goal was to determine whether or not some people may have been misdiagnosed with malaria and could actually have relapsing fever. Malaria is hyperendemic in Mali and quite seasonal. And yet some of the clinical manifestations of the two infections are quite similar. I approached Dr. Zoon and Dr. Barron about possibly doing a project there and they were both very supportive. The project would start with field work, to see if, by trapping animals and collecting ticks, we could find evidence of the spirochetes that cause relapsing fever.

My first trip, with much help from Dr. Jennifer Anderson, Dr. Robert Gwadz and Dr. Richard Sakai, was in December 2007, and my last trip was in November 2014. I had nine trips over there. I worked with our Malian colleagues as well as some postdocs in my lab (Robert Fischer [Dr. Robert J. Fischer] for one) and we did field work, traveling to numerous villages, trapping, collecting animals, collecting blood samples, trying to find ticks in the rodent burrows, and then bringing the material back to Hamilton to work it up to find evidence of spirochete infections.

And yes, we succeeded in finding many soft (Argasid) ticks infected with Borrelia crocidurae, which causes relapsing fever in other parts of West Africa. Then I began working on a human subjects protocol to allow us to collect blood samples from people in the villages where we had found the infected ticks and infected rodents. The goal was to see if there was evidence of human exposure by finding antibodies to these spirochetes in their blood or, if they were clinically ill, to see if we could detect spirochetes in their blood smear.

All this came to a halt with the political unrest beginning in 2012. Then there was a ban on going to those areas where we had found the ticks and spirochetes, which was in the Mopti region and villages near Bandiagara. We were not allowed to travel there, so that brought an end to the project. In 2012, we published the paper on Borrelia crocidurae and the potential for human infection in Mali. We also published two other papers on the fleas and trypanosomes and our finding of another species of Borrelia in the hard tick Rhipicephalus geigyi. My last trip to Mali was associated with the Ebola outbreak in West Africa.

Harden: Tell me about what you were doing during that Ebola outbreak. You ended up with a major award for your work there.

Donned with personal protective equipment (PPE) for handling human blood sample infected with Ebola virus, Bamako, Mali; November 2014.

Schwan: My trip to Mali was to help prepare the Bamako lab in case Ebola spread to Mali. I went with Dave Safronetz [Dr. David Safronetz] from the RML Laboratory of Virology. Heinz Feldmann [Dr. Heinz Feldmann], Chief of that laboratory, wanted me to go to Mali with Dave and help him if needed. Also, there had been one case of Ebola, and there was a blood sample there waiting to be shipped back to RML. Dave and I were there for a week or so to help plan for a possible outbreak, but nothing happened. There were no new cases. Things were looking pretty good. We suited up as best we could for BSL-4 work with Ebola to handle the one infected human blood sample from the patient, who I think was a child, if I recall correctly. I'm a little foggy now. And then we shipped the sample Stateside. I have an old low resolution picture that Dave took with a cell phone of me all gowned up. We were not in a BSL-4 lab. We just did all the personal protection we could to move a sealed frozen vial containing the sample from the freezer into a shipping container.

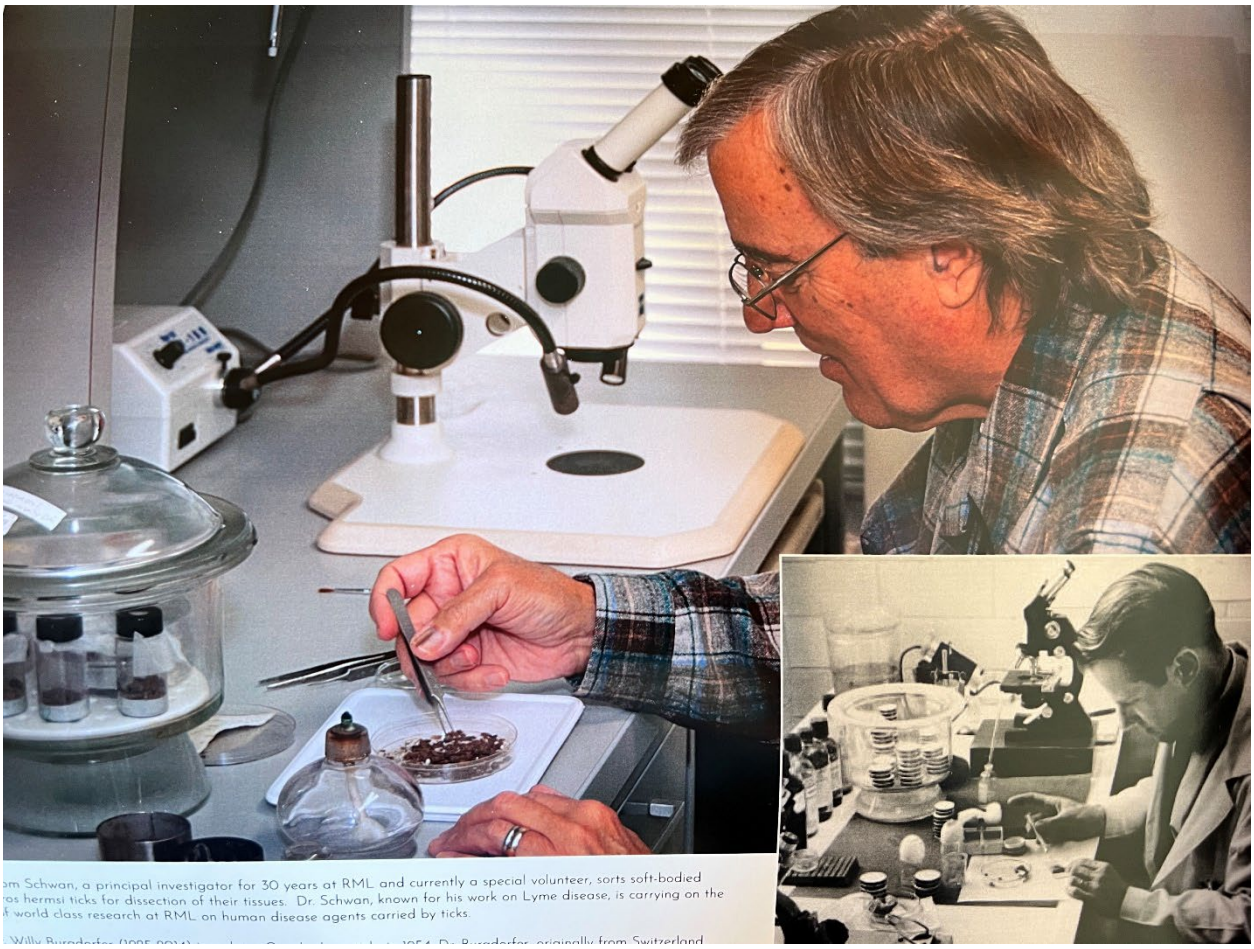
The night we left—all the flights that one takes when one leaves Mali are late in the evening—we were at the airport in Bamako waiting to depart, and Dave got a text that said, "Another sample's coming in. We're going to test it here in the Bamako lab tomorrow." We got on the plane and left. When we got back to the States, we found out that the second sample was positive, and it turned out that there was another case, but Ebola never erupted in Mali like it did in other places. So, I didn't do much at all. I was over there on one trip to help if needed and I got an award. As I told several people, "I don't deserve this." But there it is; I was on the list of awardees.

Harden: Between 2014 and 2017, you were retired from RML but continued to work as a halftime, re-employed annuitant. You also developed a class as an adjunct instructor at the University of Montana in Missoula. Tell me about this transition. Why did you decide to retire? What research did you want to continue to pursue? What you did for your course at the university of Montana.

Schwan: I always struggled with being a lab chief, and I never thought I was particularly good at it. I did the best I could do to make sure that the investigators in my lab got what they needed, and if there were problems, I dealt with them. But it wore me out. I love the science. I love being a biologist. But I have to say that the administrative aspects of being lab chief started to wear on me. And so I decided to retire. Dr. Zoon suggested I consider an appointment as a reemployed annuitant, which I'd never heard of. When I asked her about it, I learned that if someone has knowledge and skills that the institute

needs, the appointment is justified. The person can then be hired half-time for a maximum of three years. The maximum is actually based on a total number of hours, which is equal to someone who's working half-time for three years. I asked Kathy a few more questions, and she was all for it. Then I went home and talked to Carol about it, and she was also fine with it. I took the position, and for three years I had reemployed annuitant status, which was great. I almost felt like I was a postdoc again because I had no supervisory responsibility, but I could interact with other scientists and help when needed. For example, I helped on the trip to Mali during the Ebola outbreak, as I mentioned already. I still had my office, some laboratory space, microscopes, and the tick colony that I maintained. I didn't have a budget, but I could still do a lot of science with relatively few distractions. It was great and allowed me to finish projects that I had a heck of a time completing when I was a lab chief. I'm still trying to finish a few papers at home, where I write, rather than my lab office because of the pandemic.

The year before I "semi-retired," I had been asked by a fellow at the University of Montana in Missoula if I might help develop a course with the parasitologist Bill Granath [Dr. Willard Granath, Jr.]. I asked Dr. Zoon about this and she was very supportive. It basically meant driving to Missoula twice a week and giving a 90-minute lecture for half a term. The classes I taught dealt with the epidemiology of vector-borne zoonotic diseases. I did that for five years. My last class was in the spring 2017.



Photograph replicating an historical picture of Dr. Willy Burgdorfer (insert lower right) working with ticks; January 2018.

Harden: Since 2017, you've been a Special Volunteer in the Laboratory of Bacteriology. What have you been working on since then?

Schwan: What I've been doing for the last few years is writing. Each year I'm getting a couple papers published that should have been finished long ago, but I'm proud of getting them done. Frank DeLeo [Dr. Frank R. DeLeo], Chief of the Laboratory of Bacteriology, has been supportive, because I don't have a budget as a Special Volunteer. He's supported the publication costs on the papers I've been publishing. I really appreciate that. And Steve Holland [Dr. Steven M. Holland], our current Director of the NIAID Intramural Research program, has also been very supportive of my appointment as a Special Volunteer, which I appreciate very much.

Last year I was on the search committee through which RML hired three new Principal Investigators. I was happy to help. Because of the pandemic, I'm not going in during the week, so I miss interacting with the new recruits. I hope that things will change before too long, as I would like to spend more time in the lab. I still have an office, microscopes, and a computer there assigned to me, but I only go in on the weekends to check my government email, because I can't get it at home.

At home, I'm set up and working on a big project on tick salivary glands. I've got a microscope two feet away from me right now as we speak that actually belonged to the late Bill Hadlow [Dr. William Hadlow]. It was his personal microscope at home, and after Bill passed away, his daughter sold it to me. So, I'm still working, plugging away, trying to finish a few things, and learning something new each day.

With brother Mark (left) at the Minnesota River valley memorial honoring the Schwandt (later Schwan) family members killed on August 18, 1862, during the Sioux uprising. Tom's great-grandfather, August Schwandt, was scalped and left for dead; only he and a sister survived.

Harden: These are all the questions I have. Is there anything else that you would like to get on the record before we stop?

Schwan: I just want to close by saying I have been very fortunate to have been part of our institute's intramural research program at the RML. I am proud of the work our group has done on improving the serological tests for both Lyme disease, as mentioned earlier, and for relapsing fever. Our identification of new geographic areas of risk where people may acquire relapsing fever has been rewarding, as have been our findings of how tick-borne spirochetes specifically adapt to their tick versus mammalian hosts as they persist in their natural maintenance cycles. The folks that helped me along the way have been wonderful, and I am truly thankful. I appreciate everything you asked, and I appreciate your thoughtful interview.

Harden: Thank you so much, Dr. Schwan, for an excellent oral history.