The background of the cover is a detailed, painterly illustration of a lush green landscape. In the foreground, there are rolling green hills. A prominent feature is a long, multi-arched stone viaduct or railway bridge that stretches across the middle ground. Below the viaduct, a small village with several houses and trees is visible. The background shows more rolling hills under a pale, overcast sky. The overall style is reminiscent of a classic landscape painting.

# Parasites and Infectious Disease

Key Discoveries in Parasitology

Gerald W. Esch

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## Parasites and Infectious Disease

Discovery by Serendipity, and Otherwise

This series of entertaining essays provides a unique insight into some of the key discoveries that have shaped the field of parasitology. Based on interviews with eighteen of the world's leading parasitologists and infectious disease epidemiologists, the stories of their contributions to discovery in contemporary parasitology and infectious disease are told. Taken together, the essays represent a beautifully written account of the development of the field and provide a real insight into the thought processes and approaches taken in generating breakthrough scientific discoveries, ranging from immunology to ecology, and from malaria and trypanosomiasis to schistosomiasis and Lyme disease. Some of these discoveries were made serendipitously and others only after relentless effort pointed to a specific solution. This engaging and lively introduction to discovery in parasitology will be of interest to all those currently working in the field and will also serve to set the scene for future generations of parasitologists.

GERALD W. ESCH is the Charles M. Allen Professor of Biology at Wake Forest University, Winston-Salem, North Carolina, U.S.A. He is Editor of the *Journal of Parasitology*, the author of *Parasites, People and Places: Essays on Field Parasitology* (2004) and coauthor of the textbook *Parasitism* (2001). He is a recipient of the Louis T. Benezet Distinguished Alumnus Award from his undergraduate alma mater, Colorado College, in 1992, and of the Clark P. Read Mentor Award from the American Society of Parasitologists in 1999.



# Parasites and Infectious Disease

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Serendipity, and  
Otherwise

GERALD W. ESCH

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Winston-Salem, North Carolina*



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## *Preface*

Any book of the present sort requires a large number of sources, and I sought as many as I could. First, there were those folks who took the time to sit and talk with me. Without exception, they all opened up and answered every question I asked. But far more than this, they helped lead me into areas that I did not know about or might not have otherwise probed. Second, I want to thank them for all of the reprints they generously provided so that I could ‘bone up’ on their areas of interest before the interview was consummated. Third, there are several books that describe the fascinating history of parasitology. I am confident I have given appropriate credit for all that I used as I went along. There are, however, several special authors and books that I want to emphasize and from which I drew invaluable information. These of course include Bob Desowitz and all of his popular tomes, but especially those that dealt with the history of malaria and its treatment, primarily *The Malaria Capers*. Mark Honigsbaum’s *The Fever Trail* was an extraordinary account of the history of quinine and the cinchona tree from which this herbal remedy comes. It should be read by anyone with an interest in malaria. I also read a large number of general historical accounts dealing with our discipline. The best was *A History of Human Helminthology*, by D. I. Grove. This is a really excellent encyclopedia of human helminthology and should be in any university library.

I want to thank several folks who read various sections of the book along the way. This included a ‘bunch’ of my general parasitology students who were involuntarily cajoled into reading several of the essays as I wrote them. My good and treasured friend, Herman Eure, read several for me. Dan Johnson, a nonparasitologist colleague here at Wake Forest consented to read several of the essays and I appreciate his interest. Ralph Amen, my personal ‘editor’, read the entire book and offered invaluable input, in his own inimical way. One of my graduate

students, Nick Negovetich, read several of the essays and parts of the Prologue. He had some very good suggestions. Another old friend, Ron Hathaway, at Colorado College, read the entire book as well. I sort of 'conned' him into doing it so that he could be my 'shill' when I gave a presentation to the Rocky Mountain Conference of Parasitologists meeting in September 2006.

My friend Ward Cooper was my original Commissioning Editor at Cambridge University Press. He helped get things started at the outset, but then moved to Blackwell, where he is now a Senior Commissioning Editor. Katrina Halliday stepped in to take Ward's place and actually got the contract through the 'dons' at Cambridge. I really owe her a huge thank you! There were four reviewers who took the time to provide some excellent comments on the proposal. I know two of them and have thanked them personally. I don't know the other two, but I thank them now. I also want to thank Mrs. Vickie Hennings for helping in the *Journal of Parasitology* office. It made writing a whole lot easier! I also had the great pleasure of working with Clare Georgy, Assistant Editor at Cambridge University Press; she was most helpful while I was trying to get the final version completed. Janice Robertson was my copy-editor and she was very supportive as well – I might add, she was as thorough as any copy-editor with whom I have worked during my career. I also want to thank Jeanette Alföldi for her help in guiding me through the new indexing process.

When I was thinking about a cover for *Parasites, People, and Places* I recalled a photograph of Slapton Ley I had taken back in 1987 during my second visit to the University of Exeter and Clive Kennedy. Of course, this is the site where Clive spent 35 years doing research. I persuaded my daughter Lisa to paint it and my good friend, the late Charlie Allen, to photograph the painting so I could use it as the cover for the book. That cover has received some really nice compliments from a wide range of readers. For the cover of the present book, I searched and searched for an idea. I had thought about a photo from the Chelsea Physic Garden, or the front of the Natural History Museum, both in London. Then, I recalled a couple of photographs I had taken from a moving train as we passed through the Midlands along the western side of England in May 2004. My wife, Ann, and I were returning to London from Glasgow where we had gone so I could interview Keith Vickerman for the present book. An old, nineteenth-century train trestle that stands in the middle of a really beautiful green valley grabbed my attention and, as the train passed by, I snapped off a couple of what turned out to be really good shots. But, you say, what is the connection between an old train trestle

and discovery in my discipline of parasitology? I know it is a 'stretch', but throughout the book, I have attempted to link the past and the present. This train trestle represents the bridge I am trying to capture in the new book. As I said, this is a 'stretch', but I thought the photographs were really quite striking. So, I asked Lisa if she would paint me a new cover and she agreed. She has a real talent for capturing things like this on canvas. I think she has done a very good job again, and I thank her for her contribution.

Finally, I thank Ann for sticking with me while I was doing my thing over all these years, 47 to be exact. By the time this book is in print, I trust it will be 48!



# Prologue

## INTRODUCTION

In the summer of 2003, I finished work on a book entitled *Parasites, People and Places: Essays on Field Parasitology*. My wife, Ann, and I were in our cabin in Green Mountain Falls, Colorado, and I was trying to tell her the story from the book that had to do with the discovery by William Walter Cort of the cause of swimmer's itch back in 1927. At the same time, she knew I was sort of lamenting the absence of a new project. She must have been impressed by my tale, because out of the blue, she said, "Why don't you write a book about discovery in parasitology?"

This started me thinking about the possibility of doing something along that line. Gradually, over the next several months, I put together an idea. Stories regarding the discovery of the transmission of malaria or sleeping sickness have been told many times over the years, so they are sort of 'old hat'. But, then I thought, are they really?

I recalled the way I teach my own general parasitology course to undergraduates. I know that I mention Ronald Ross and David Bruce, among others, but I really do not get into much detail about how Ross and Bruce did their work regarding malaria or African sleeping sickness, respectively. Then, I began thinking about some of the new discoveries regarding malaria (*Plasmodium* spp.) and sleeping sickness (*Trypanosoma* spp.) that have been made since their life cycles, and those of other parasites, first were resolved. For example, consider the variant surface glycoproteins of the trypanosomes. Who did this research, and what led them to do it? Since this is definitely not my area of expertise, I honestly did not know for certain who did what until after I began researching the background information necessary to write this series of essays. So, I thought, why not briefly retell some of the old stories

regarding discovery in the nineteenth and early twentieth centuries, and link these historical accounts, where relevant, to some of the newer work that has been done in the last 50 years. In a few essays, I have simply gone to a prominent parasitologist and asked them to tell me about their lifetime of research.

Another idea emerged when I began thinking about this approach. I became intrigued by the possible role of serendipity in all of these parasitological discoveries. My *Random House* dictionary defines serendipity as “the faculty for making desirable discovery by accident.” Then, I began to wonder, do some folks have a faculty for making “desirable discovery by accident”? Was Louis Pasteur correct when he said, “In the field of observation, chance only favors the mind which is prepared”? What about the life cycles of *Plasmodium* spp. and *Trypanosoma* spp.? A number of investigators were looking for the way in which the malarial parasite was transmitted, not just Ross. Was Ross endowed with a ‘special’ faculty for discovery? Why did he ‘hit the jackpot’ sooner than the others? In the case of sleeping sickness, Aldo Castellani first saw tryps in the cerebrospinal fluid of humans, but he thought initially the disease was caused by a streptococcus infection of the heart. It was David Bruce, however, who is generally given credit for identifying the etiological agent of sleeping sickness. Why not Castellani?

I also began thinking about something else. Ross, Bruce, and the other giants of their era have been dead for many years. But, what about those individuals who made important discoveries in the past fifty to sixty years? It dawned on me that many of these parasitologists were either retired, or were close to it, and some have even died. They have important stories to tell, but they are generally not being told. I think these stories need to be out there as well, in full view. The new parasitology students need to know who these people were/are and they need to know why these folks, or what I call the ‘middle generation’ of parasitologists, did what they did.

Who would I choose to talk about and why would I select certain ones to focus upon? I realized early that this proposition could become rather ‘dicey’. Whenever anyone makes a selection of this kind, some folks will be annoyed because one of their favorite people was left out. On the other hand, since I am the one doing the writing, it must be my choice. So, that’s what I did, I made some hard choices. I considered a long list of ideas and possibilities. I then proceeded to choose my favorite ‘discoveries’, my favorite parasites, and my favorite people (at least some of them). As it turned out, it is a very eclectic group, in

all three categories. In a few cases, I could not conduct an interview because the person is dead. So, I had to rely on someone else to tell the story for them, but I was lucky because I had some very good sources.

Another point, I am writing these 'stories' as essays because I think this format gives me freedom to roam. I am not bound by a particular style, and can pretty much go where I want and take a particular topic as far as I want. I also decided to place in a Prologue at least some of the biographical information regarding each person that I have interviewed, or that I talk about extensively in the specific essay. Each of the interviews begins the same way. Where did you go to school, first as an undergraduate, and then as a graduate student? How did you get into parasitology? When did you graduate? Then, I wanted to use fairly standard questions in an effort to get some sort of idea about how they think, where they might have traveled to do their research, why their work went the way it did, etc. In obtaining information like this, I was able to generate some useful comparisons of some very good parasitologists.

It was fun!

DICK SEED

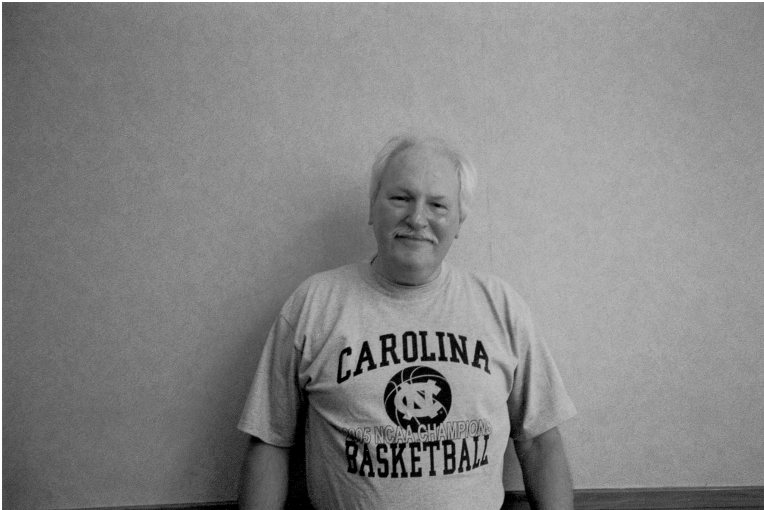


Figure 1. J. Richard Seed, retired Professor, Department of Epidemiology, School of Public Health, University of North Carolina-Chapel Hill, Chapel Hill, North Carolina

While thinking about discoveries in parasitology over the past fifty years, it occurred to me that the research on variant surface glycoproteins (VSG) would be of interest in connection with the story of how the life cycle of the African trypanosomes *Trypanosoma brucei brucei* was worked out by David Bruce. After doing some ‘snooping’ around in the appropriate literature, two names, J. Richard Seed and Keith Vickerman, F.R.S., recurred with some frequency. I decided to interview both of them and ask about their contributions to the VSG tale. Dick Seed was just down the road in Chapel Hill, North Carolina, an easy drive from Wake Forest and Winston-Salem, and Keith Vickerman was at the Glasgow University in Scotland, so I went there to see him as well, an easy trip by plane and train via London.

In May of 2004, I drove to Chapel Hill and spent the morning drinking tea and visiting with my friend Dick Seed. I have known Dick for nearly thirty years, having met him the first time at an American Society of Parasitologists meeting in San Antonio, Texas, in 1976. That was the year he received the H. B. Ward Medal for his contributions to the immunology and biology of the African trypanosomes.

Dick’s undergraduate days in the mid 1950s were spent at Lafayette College in Pennsylvania. He said that his interest in biology was already present when he entered Lafayette. While there, his enthusiasm for microbiology was ‘tweaked’ by Professor Willis (“Bugsy”) Hunt, a ‘Yaley’, who then encouraged him to follow his lead and also head for Yale and his Ph.D., which he completed in just three years, working in the lab of David Weinman. According to Dick, “The latter was an M.D., and had spent much of his life . . . doing parasitology all over the world.” Weinman was working on the African trypanosomes and this is where Dick focused his graduate research as well. While Dick’s experience at Yale was a good one, he lamented that it was too short, just three years, and that there were many techniques, etc., he felt he should have learned, but did not have the time. For any young person reading this book, this is very good advice. Along the way, someone may advise you to skip the Master’s degree and go straight for the Ph.D. I disagree. I strongly believe the Master’s degree is excellent preparation for pursuing the Ph.D. degree. It presents you with an element of practice for what is to come.

I was interested to learn that, as a youngster, Dick had read Paul de Kruif’s (1926) *The Microbe Hunters*, and that this is what stimulated his interest in infectious disease and, subsequently, in parasitology and trypanosomiasis. Another book, Geoffrey Beale’s (1954)

*The Genetics of Paramecium aurelia*, had also caught his attention. Among other things, the latter book detailed the phenomenon of antigenic variation, which ultimately led him to a postdoc with Irving Finger at Haverford College in Pennsylvania. His thinking was that “antigenic variation among the African trypanosomes was similar to what occurred in the mobilization antigen on *Paramecium*. If I learned about the latter, then I could use the tools of the *Paramecium* geneticist to understand the African tryps.” (When you read on, you will discover that the same two books had a huge influence on the career of Keith Vickerman too, another veteran of the antigenic variation effort, plus several others that I interviewed for the present book.)

As I will detail in my essay on African trypanosomiasis and VSGs, Dick did not really follow up on any of his Ph.D. research after he finished at Yale. In 1960, Jacob and Monod won a Nobel Prize for their discoveries regarding the regulatory processes associated with  $\beta$ -galactosidase in *Escherichia coli*. Dick’s mission for most of his post-Ph.D. professional life was to search for what turned out to be an elusive regulatory process in trypanosomes. In graduate school and throughout his career, he made several important discoveries, but not the one for which he was ultimately searching. As will be seen from the trypanosome essay, however, the absence of ‘ultimate’ success was not for a lack of trying.

Dick’s stay at Haverford was brief since ‘Uncle Sam’ decided to ‘hire’ him as a Medical Service Officer for the U.S. army. He was assigned to Fort Baker, just north of San Francisco, where he spent the next two years running a parasitology diagnostic laboratory. It was not a wasted time as he had two very good technicians from whom he learned a “lot about parasitology and diagnostic procedures.” Following Fort Baker, Dick traveled to New Orleans where he became an Assistant Professor at Tulane University, advancing to the rank of Professor over the next eight years. After that came five years at College Station, Texas, and Texas A&M University, where he served as Chairman of the Biology Department. It was then on to Chapel Hill, North Carolina, where he became Head of the Department of Parasitology in their School of Public Health. He serves there as Professor of Epidemiology, although this (2006) is his last year before retirement.

I really enjoyed that morning in Chapel Hill with Dick. My stay was greatly enhanced by the presence of Dick’s lovely wife, Judy, who provided tea and pleasant conversation during our breaks.

KEITH VICKERMAN



Figure 2. Keith Vickerman, FRS, retired Professor, University of Glasgow, Glasgow, Scotland

In late May 2004, my wife, Ann, and I drove to Charlotte, North Carolina, and boarded a flight for Gatwick Airport, near London. After a couple of days of getting our body clocks reset, we took a train to Glasgow where I was to interview Professor Emeritus Keith R. Vickerman, F.R.S. We arrived at Central Station around 2:30 p.m. and checked into our hotel. Ann suggested we take the underground out to the campus of Glasgow University and scout out Keith's office, so I wouldn't be late for my appointment the next morning. We not only found it, but also actually met and spoke with Keith for a few minutes that afternoon. Ann then wanted to visit the wonderful Hunterian Museum on the University's campus, which we did. We did not see much because they ran us out at 5:00. As we were leaving, we were caught in a classic thunderstorm. By the time we made it back to the tube station, we were drenched (no umbrellas – great planning!). When we emerged from the tube station at the other end, it was still pouring. So, she suggested we make a dash for a nearby 'Boots' (a chain drugstore in the U.K.) and buy a couple of cheap umbrellas. We came out with our new protection and walked back to the hotel, 'singing and dancing', in the rain. (By the way, as I could have predicted, we did not use the umbrellas over the next two weeks of our stay in the U.K.)

The interview the next morning went really well. Keith had even prepared a written autobiography and a full CV for me to take home.

He said it was part of an 'obituary' he was required to write when he was elected Fellow in the Royal Society. I don't know if he was 'pulling my leg' or not, but the written information did come in handy while preparing the essay on trypanosomiasis.

He told me that his entry into the biological realm was triggered by a serendipitous event, one that occurred early in his life. As a twelve-year-old in grammar school, he was enrolled in a second year science course devoted to the history of microbiology. With the sudden departure of his instructor, a mathematician with absolutely no training in biology was recruited as a replacement. Not knowing what to teach, the teacher, Keith said, "read to us from Paul de Kruif's *The Microbe Hunters*. I was spellbound," and a brilliant career began.

Keith went on to say that Elie Metchnikoff, the legendary Russian zoologist, was to become his idol. It was Metchnikoff who deduced that phagocytic cells in mammals may be involved as a defense against intruding pathogens while he watched similar-type cells attack thorns experimentally introduced into the bodies of larval starfish. Keith said he realized "later that it [Metchnikoff's idea] represents a perfect example of Popper's view of the scientific method – that a single observation inspires a flash of intuition that leads to a fashioned hypothesis that can be tested by further observation and experiment with a view to falsification or corroboration." What a great description of the scientific method!

Keith's undergraduate academic career began at University College London (UCL) in 1952, under the tutelage of Peter Medawar (who was to become a Nobel Laureate in 1960 for his work on skin graft rejection). While he enjoyed his relationship with Medawar, he considered him to be somewhat 'baronial'. When Keith announced his interest in parasitology for graduate work, Medawar was rather scornful, referring to parasitology, "as a somewhat philistine pursuit, far too long cut off from the advances of mainstream fundamental biology." In our interview, he said that Medawar considered the discipline as "impenetratively deaf to all the advances in biology for the past 50 years." (Keith told me he thought at the time, "Well, there must be an awful lot of catching up to do – all the more reason for getting into it.") However, for Medawar, protozoology was another matter. He was very impressed with the great American ciliatologist and geneticist, Tracy Sonneborne, and said to Keith, "The Research Councils are very concerned about the dwindling population of protozoologists, so why not become a protozoologist," and Keith responded, "I will." Keith actually became a protozoan parasitologist,

with an early, and then a long-term, research focus on the African trypanosomes.

In fact, because he wanted to work on trypanosomes, doing his Ph.D. presented some difficulty because he could not settle on someone with whom to study or who would take him on as a student. He finally ended up at the University College of the South West, in Exeter, with R. S. J. Hawes, working with protozoans of soil and soil-dwelling insects: “not exactly what I had envisaged, but I did manage to find a trypanosomatid in tipulid (crane fly) larvae.” He was not entirely pleased with his experience there, although he did manage to spend a “term” working at “Edinburgh University in Michael Swann’s thriving cell biology group.” It wasn’t totally bad at Exeter because he also met F. E. G. (Frank) Cox, a fortuitous event. Keith said that, “Frank was in a class I had demonstrated to and he had been a technician with P. C. C. Garnham” at the London School of Hygiene and Tropical Medicine. Keith and Frank were to become life-long friends and colleagues.

When Keith finished at Exeter, he went back to UCL and Peter Medawar’s immunology lab, where he was offered the opportunity to work on any protozoan he wanted. He naturally went to the African trypanosomes. He also chose to focus his efforts on antigenic variation because of a book by Geoffrey Beale, *The Genetics of Paramecium aurelia* (the same one that Dick Seed had read). When I returned from London/Glasgow, I phoned Seed to tell him about my interview with Keith and then sent him a copy of the information that Keith had prepared for me. A few days later, Dick returned my phone call and excitedly described for me how many parallels there were between his early biological experiences and those of Vickerman. Both were to have huge successes in their work on antigenic variation in the African trypanosomes.

Keith’s “fascination with life cycle changes and their relation to survival in changing environments” began as a student and stayed with him throughout his career. His research on the African trypanosomes actually focused on two areas, both of which were tied to life cycle changes and changing environments. Thus, for example, he spent a great deal of time attempting to understand the energy metabolism of the organisms, discovering in the process that in the tsetse fly gut, the amino acid proline is the main energy source. In the vertebrate, the trypanosomes switch to an aerobic glycerophosphate oxidase system. He was able to correlate these metabolic differences with significant morphological changes in the mitochondria as the parasite moves from the fly to the vertebrate host and back. It was while working on

these changes using electron microscopy that he made his second really important discovery. That was the physical presence of an antigen coat, the VSG, on the surface of the metacyclic form of the trypomastigote in the salivary glands of the fly. This was a significant event and one that was serendipitous – he was not looking for it. But, when he saw it, he knew about the significance of the coat, and he went after it. He made a huge discovery and a really momentous research contribution as a result.

Keith had a marvelous career at Glasgow University, retiring in 1993. He continues to write and do research in spite of a serious back injury recently suffered in a fall at his home.

#### BOB DESOWITZ

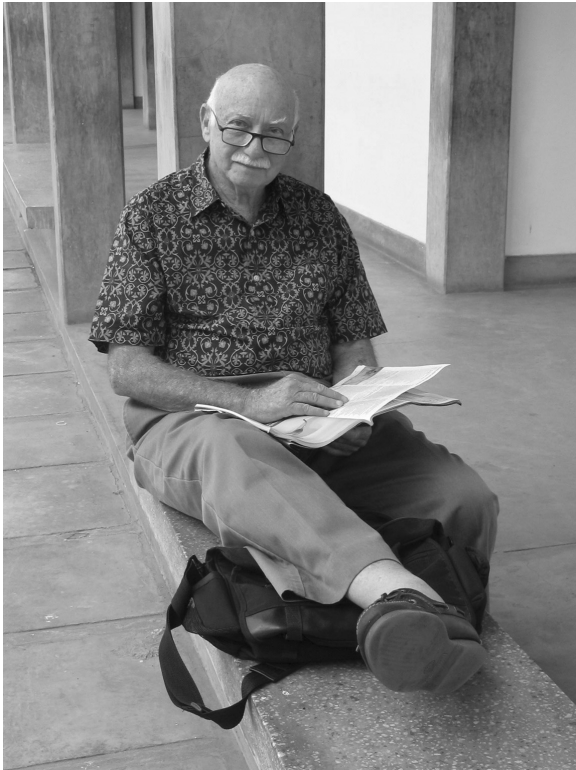


Figure 3. Bob Desowitz, retired Professor, University of Hawaii, Southern Pines, North Carolina

Bob Desowitz completed his undergraduate work at the University of Buffalo, in New York, with a two-year interruption by service in the U.S. Army right after WWII. He had become interested in microbiology and parasitology and decided to pursue the topics at the next level, graduate school. He asked his advisor at Buffalo where he should go and his mentor suggested the London School of Hygiene and Tropical Medicine. So, he applied and was accepted.

In the fall of 1948, Bob arrived at the London School of Hygiene and Tropical Medicine (LSHTM), ready to pursue his Ph.D. degree in protozoology with Henry Shortt, a former Colonel in the British Colonial Service. Fortuitously, Bob was in place to witness one of the great dramas of discovery in parasitology. Henry Shortt and P. C. C. Garnham were about to take the final step in pursuit of the *Plasmodium* spp. life cycle and solve a mystery that had been around for nearly 50 years. It seems that Fritz Schaudinn had reported in 1903 that the sporozoites of *Plasmodium* spp., on being inoculated into the blood of their vertebrate hosts by mosquito vectors, disappeared after about 30 minutes – correct! But, he also said that the sporozoites then penetrated red blood cells directly – incorrect! When Bob arrived, Shortt and Garnham had just finished the first effort to purge this assertion using *P. cynomolgi* and monkeys. I'll write more about this huge discovery later.

During my interview with Desowitz, he described Henry Shortt as a “truly wonderful man, and was marvelous to work for.” Bob also explained, “Shortt was big on lineages. He [Shortt] was taught by Sir Rickard Christophers, and Christophers was taught by Sir Ronald Ross, so I'm a direct descendant of Ronald Ross!” Bob's Ph.D. research was on *Histomonas meleagridis*, the causative agent of ‘turkey blackhead’. He told me that he kept all of his turkeys up on the roof of the building housing the LSHTM. It was soon after the war and there were still food shortages in the U.K. Each year, at Christmas time, Bob said, “Shortt and Garnham would show up at his lab space and inquire as to the availability of a control turkey.”

When he completed his dissertation research, he was searching for a place to publish his work. Shortt persuaded him to submit it to *Nature* where it appeared as his first publication (“not a bad place to start,” he proudly remarked during our interview). With his Ph.D. in hand, he was about to take a position in the local poultry industry when a monacled Englishman showed up in his office. He

introduced himself as Colonel (“another one!”) Hugh Mulligan of the British Colonial Service and said to Desowitz, “You come highly recommended by Henry Shortt. Would you like to go to work for me? I am setting up a new research institute in northern Nigeria.” Bob responded immediately, “Yes, but I’m an American.” “Don’t worry about that, I’ll fix it,” said Mulligan, “Let’s go to the pub and discuss it.” “So,” Bob continued, “we headed for the Duke of Wellington Arms to talk about it”, adding, “I think all recruiting for the British Colonial Service takes place in pubs!” He was off to Africa and nine years of research on trypanosomes as an American in the British Colonial Service.

Toward the end of his stay in Nigeria, the Provost at the University of Singapore, an old friend from his graduate school days at the London School of Hygiene and Tropical Medicine, contacted him. He was invited to take the position of Head of their Department of Parasitology. He accepted the challenge and spent the next five years there. He also switched his research into the diagnostic area of malaria. Then, during his last year in Singapore, he was persuaded by Elvio Sadun to come to the SEATO (Southeast Asia Treaty Organization) research lab in Bangkok, Thailand, as Chief of the Department of Medical Parasitology, and he accepted. The U.S. Army operated the lab as an activity of the Walter Reed Army Institute of Research and, although a Department of Defense civil service civilian, he was given the courtesy/substantive rank of “bird colonel”. He laughed about the service appointment because immediately after WWII he had been drafted into the army as a lowly private. He considered his rapid rise in rank rather amusing.

Several years later, he was approached by the University of Hawaii’s administration. After fourteen years of administrative responsibilities he told me it was a happy relief to accept a professorship with a more simple charge to run the parasitology component within the Department of Tropical Medicine and Medical Microbiology. He was tired of administration by this time, and accepted the offer with a quid pro quo that he would have an administrative assistant to handle some of the chores. After thirty years in Hawaii, he is now retired and living comfortably in Southern Pines, North Carolina. He has since emerged as a successful and widely read ‘popular’ author dealing with tropical disease and parasitology, mostly based on his wonderful experiences in Africa and southeastern Asia.

## K. DARWIN MURRELL

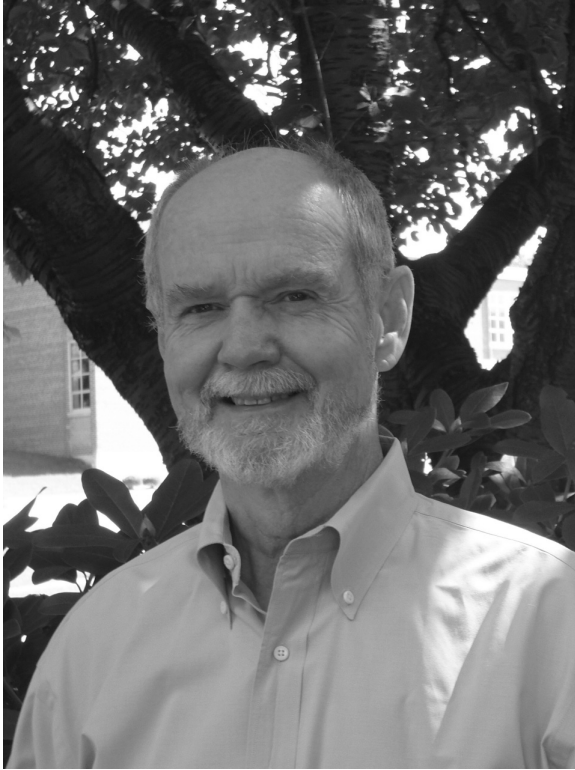


Figure 4. K. Darwin Murrell, retired scientist, Agricultural Research Service, U.S. Department of Agriculture, Beltsville, Maryland

As is the case with many of us who wind up in academia, industry, or in government labs to do research, we were drawn into our professions by a teacher, usually in a college or university when we take our first biology or zoology course. This was true in the case of Darwin Murrell. On graduating from high school in northern California, his intention was to join the Marine Corps, but one of his buddies ‘chickened out’. So, when the rest of his friends went to Chico State College, Darwin trailed along, not really knowing what he wanted to do. His first courses were in agriculture, but this direction was quickly aborted due to a colossal case of monotony. He recalled that in the spring of his first year, he signed up for a beginning course in zoology, taught by Don Wootton. Don always took his entire class for a week of marine biology at Bedoga Bay and, in Darwin’s words, “That blew my mind! Wow,”

he continued, "I never imagined such stuff. Then he taught us how to cook our own things from the sea." This reminds me of Mick Burt, the great Canadian parasitologist, who required his students to learn how to cook the hosts of whatever parasite project they might choose for his parasitology course – my favorite was garlic sea cucumbers (see p. 177 in Esch, 2004).

After being hooked by Wootton and his course in general parasitology, Uncle Sam sent him a letter indicating the Army needed him for a spell. With help from Don, however, he managed to secure a deferment. This allowed him to travel to the University of Michigan Biological Station (UMBS) at Douglas Lake where he took the helminth parasitology course from Jim Hendricks. Throughout his undergraduate career, each summer, he and usually a couple of other students would travel with Wootton to UMBS, camping along the way and visiting the great natural areas of the west on their way to and from Michigan. While he was at UMBS in the summers, he was talked into heading south to University of North Carolina-Chapel Hill and Hendricks' home base where he was to begin pursuit of his Ph.D. degree in the early 1960s. From his new Chapel Hill base, he returned to UMBS for one more summer. I asked him if Will Cort was still going up to the Station at that time, and he said no, at least Cort was not physically present. But, he told me that "Will Cort and Paul Beaver were still there in spirit, and their names and vials of materials were everywhere."

Before he could complete his degree at UNC, Uncle Sam decided it was time to end the deferment and he was reclassified 1-A. All of us who experienced those days are well aware that a 1-A classification meant a draft notice was on its way, and Darwin was informed in December that he would be called up in March. Rather than having his draft board make the decision on how he would serve, he headed for the offices of the local recruiters. Darwin was exceedingly lucky (serendipity) because it was from one of these recruiters he learned about the Naval Medical Research Institute (NAMRI) in Bethesda, Maryland. After filling out all the appropriate papers and interviewing with personnel at NAMRI, he received an ensign's commission in the Medical Service Corps as a parasitologist. He was initially based in Bethesda where he spent his initial four months in the service, before being transferred to Naval Medical Research Unit-2 in Taipei, Taiwan. It was there he had another stroke of luck. John Cross, a civilian, had just been named the new head of the parasitology section at NAMRU-2. John had lived in China for many years, making him a perfect fit for the position in Taiwan.

Their workload was heavy. Darwin told me about his “parasitological responsibilities in developing diagnostic tests for the Marine Hospital in DaNang, Vietnam, plague in Indonesia, capillariasis in the Philippines, parasitic diseases in Taiwan – we were on the road a lot.” Cross knew that Darwin needed to do some more research to complete his dissertation. He told Darwin that anytime he was not busy, he could work on his dissertation research, and that he would even buy the equipment for him. His Ph.D. research involved a study of the effects of antibody and complement on the tegument of larval cestodes, specifically cysticerci of *Taenia crassiceps*. One of the instruments purchased for Darwin was the old Warburg manometer. He reminded me, “I was taught how to use the Warburg from a young postdoc in Chapel Hill, by the name of Jerry Esch.” I had almost forgotten, probably because I hated the apparatus so much. Anyone who has ever used it would wholeheartedly agree.

A female missionary in Taiwan improved his German to the point he was eventually able to pass the language exam, then required for most of us seeking a Ph.D. in the old days. At night, he would write his dissertation and in the day, his wife, Joyce, would type what he had written the previous evening. It was then sent to Norman Weatherly, his dissertation advisor in Chapel Hill, who would work through it and then return it to Darwin for further revision. After his tour in Taiwan was completed, he returned to Bethesda. His Ph.D. defense was in 1969, four years after entering the Navy. He was tempted to stay in the Navy with NAMRI because of the opportunities afforded him by their research facilities in Africa and Asia. But, Darwin believed, the Navy had a policy of transferring personnel with some regularity and he felt he could not get a good research program started at any of these venues before he would be transferred again. Moreover, he felt all the moving and travel would be tough for a growing family.

So, after his tour of duty in the Navy, he headed for the University of Chicago and the legendary Bob Lewert, with whom he worked for the next two years. Darwin had, by then, begun thinking about a job and his long-term future. There was a particularly serious problem at that time though, since positions in academia were terribly scarce. Even with his wonderful résumé and overseas experience, his job search was not successful. By then, the University of Chicago had instituted a two-year M.D. program for folks with the kind of background possessed by Darwin and he was sorely tempted to pursue it. In fact, he applied and was admitted. However, just before entering the program, he received a phone call from Dick Beaudoin (head of malaria research at the NAMRI).

Dick informed him that Peg Stirewalt, who was leading the schistosome research program at NAMRI, was about to retire and encouraged him to apply for the position. So, he told Dick to pass the word to the appropriate people at NAMRI that he was interested. A few days later, he received a phone call with an invitation to come to Bethesda for an interview, which he did. The job was offered and he accepted on the spot. The folks in the medical school at Chicago made a considerable effort to keep him, but his mind was made up: he was going to Bethesda.

Life at NAMRI was good. He was there for seven years. Then, as he related, “The Navy’s Head of Research and Development showed up, asking for a report on their research. After the meeting, he said, ‘Well, you are doing nice research here, but I have to be honest with you, schisto research is of no interest. They don’t even have it in Brazil.’” Darwin was shocked, “I couldn’t believe he said that!” He consulted with a friend in the R&D Command of the Navy who told him he thought schisto research was going to be chopped. He believed malaria research would continue, but no more with the schistosomes.

Then came the next move. He knew the NAMRI job was coming to an end. He said that not long after learning his position with the Navy was going to be terminated, he ran into Ron Fayer. Ron said he had heard of Darwin’s predicament and that there was a lab chief’s opening in helminthology over at Beltsville (USDA). It would be in veterinary helminthology and a change in research emphasis, but he needed a job, so he interviewed and was hired. The interesting thing about the offer was that he was given *carte blanche*. He was told “to look around and see what you think is of importance,” another lucky break in his research career. He “decided that strongyloidiasis was a big problem in the swine industry. So I began working on *Strongyloides ransomi* and *S. ratti* as a complementing lab model.”

Things went well for about two and a half years when he received a phone call one day from an official in the Agriculture Research Services (ARS), the R&D agency of the Department of Agriculture. He asked, “Are you doing anything on *Trichinella*?” Darwin responded, “No, don’t you remember a couple of years ago when I came here that we decided to phase that out?” The ARS person said, “Well, the pork industry raised a hell of a lot of money for the ARS a few years back and they want to know what we did with it. Can’t you come up with something to do with regard to *Trichinella*?” Darwin was then told by his ARS colleague to “think about it.”

“Well,” as Darwin put it, “it was one of those coincidences in life that a scientist over in the Meat Institute called me up about

that time and said, 'you know I have been working on a grant from the fast food industry and the pork industry to look at ways to cook pork very fast for fast food outlets.' They want these methods evaluated for their effectiveness in killing trichina and I don't know anything about how to do it." He invited Darwin to collaborate, and he agreed. He immediately set up animal bioassays for the rapidly cooked pork.

Irony in their research was subsequently to come in several unusual ways. For example, when they completed their work and wrote up the paper, it was submitted to the ARS for clearance prior to being sent to a journal for publication. It had to go through this sort of process because *T. spiralis* was on the list of 'sensitive' parasites, even though no research sponsored by the government had been done on these worms for several years. The conclusion in their manuscript was that fast cooking with microwave technology could not guarantee that trichinae in pork would be killed, due to uneven cooking by microwave ovens. The next day, he received a call from his colleague in the ARS saying, "Jesus Christ, what are you guys trying to do, get me fired? If we let you publish this before we inform the pork and microwave industry, all hell will break loose." Darwin said that they went ahead and published the report anyway. As predicted, "All hell did break loose! Newspapers, radio, television, everyone, wanted to know about our *Trichinella* results! Why?", Darwin asked rhetorically, "Because microwave cooking won't protect you from *Trichinella*." On the other hand, he remarked, again rhetorically, "Have you ever known anyone infected with *Trichinella*?" Their results ultimately created such an in-house ruckus at ARS that further work on *T. spiralis* suddenly became an absolute necessity, and the next appropriation from Congress included a sizable amount of money for *T. spiralis* research. That is how he developed a "lifelong and intimate relationship with this exotic parasite". A further irony was that research in the Department of Parasitology at UNC-Chapel Hill, where he obtained his Ph.D. some thirty years earlier, was almost entirely focused on *T. spiralis* immunity. As Darwin said, "What goes around comes around."

In our conversation, Darwin remarked that after nine years he took a particular fork in his career path that, in retrospect, was not altogether wise because it led him away from the lab for the next thirteen years, and into administration. Although life as an administrator had its fine moments, the nagging regret of not being 'hands on' in research never left him. The only way he could be involved was through the occasional review or book chapter he was asked to contribute.

Then, a stunning loss to the worldwide veterinary parasitology community came with the untimely passing of Peter Nansen, who was Director of Denmark's Centre for Experimental Parasitology, where their focus was on basic research on parasites, mostly helminths, of veterinary importance. Darwin had spent six months in the Centre as a Fulbright Fellow in 1996 while on sabbatical from ARS in 1996. Knowing that he had retired from the USDA, the Foundation's Board called on Darwin to become the new Head, replacing Nansen. He signed a three-year contract, and he and Joyce were off to Copenhagen. I could tell by our conversation that he really enjoyed his tour of duty in Europe. Their research was focused on any number of parasitic diseases, ranging from *Ascaris* and *Trichinella* to neurocysticercosis, and he was an active participant in much of it. In fact, one of the projects focused on neurocysticercosis in collaboration with a group in India. Their target was to develop an immunological diagnosis for the single cyst granuloma form of the disease, which, apparently, was behaving differently than it was in many other parts of the world. In India, the preponderance of infections are apparently single cysts, which are very difficult to diagnose in contrast to other areas where the easier to diagnose multiple cyst infections predominate. In India, the epidemiology of neurocysticercosis is not well enough understood to be successful in the disease's control.

Another particularly rewarding project he undertook was to conduct epidemiological studies with colleagues in Serbia on the resurgence of trichinellosis following the outbreak of civil war there in 1989. In addition to identifying some key social and agricultural factors that greatly increased the risk of *Trichinella* transmission, he guided a project that produced the first experimental data on horse infections, which have been responsible for the majority of the recent outbreaks of trichinellosis in western Europe. Sadly, it was also an opportunity of seeing what devastation a senseless war could bring to a society and a country.

After the three years in Denmark, he decided to break free of administration, so he and Joyce returned to their home in Rockville, Maryland. However, this does not mean he has stopped working; he has just gone off in a new direction. One of his current projects involves fish-borne parasites in Vietnam, funded by the Danish International Development Agency (Danida). Apparently, there is still a huge problem with 'honey buckets', raw sewage, raw fish, and fish-borne flukes in many areas of southeastern Asia. Another includes a study with an Australian collaborator on canine zoonoses in Thailand. He is also advisor to an FAO research project on trichinellosis in Argentina, and

frequently serves as a consultant to WHO and FAO, particularly in editing new guidelines for cysticercosis and trichinellosis. For Darwin, as for so many colleagues, there is no retirement in the classic sense. He is now enjoying every minute of his newly discovered occupation, that of a freelance parasitologist.

BILL CAMPBELL



Figure 5. Bill Campbell, retired scientist, NAS, Merck, Rahway, New Jersey

We transition now to another *Trichinella* expert, although that is not the reason I wanted to interview Bill. Among other things, I consider William C. Campbell to be among a handful of true ‘Renaissance Men’ in the American Society of Parasitologists. Whether one agrees with my judgment on this notion or not, something one cannot disagree about was his justifiable election to the National Academy of

Sciences, U.S.A. The primary reason for this esteemed and high honor was his contribution at Merck to the discovery of ivermectin, a truly remarkable drug for the treatment of parasitic helminths and certain ectoparasitic arthropods in both humans and their domesticated animals. In addition to treating a broad spectrum of diseases, the drug is now being used to save the eyesight of thousands of people in developing countries where *Onchocerca volvulus* is still a dread organism.

Bill's journey from his native Donegal in Ireland to Rahway in New Jersey was a long, but fascinating, trip. After boarding school in Belfast in Northern Ireland, he headed for Dublin, the Republic of Ireland, and Trinity College, the charter of which was granted by Queen Elizabeth I in 1592. His headmaster had thought he should pursue medicine, but his biology teacher at boarding school had such a great influence on him that he ended up in natural (biological) science, a very wise choice as it turned out. I asked if he had made contact with the great parasitologist, J. Desmond Smyth, in his first year at Trinity and he responded in the affirmative. At the time, Smyth was not *the* professor in the department, although he did acquire a professorship subsequently. One should know that in those days it was typical in the British system to have but a single professor in an academic department. Faculty members without professorships had to wait for death, retirement, or transfer of their senior faculty member before a younger faculty member could rise to the highest rank in the British system. More often than not, the professor was also head of the department as well, making this person, in many cases, almost 'god-like'. Fortunately, the system in Britain has changed a great deal since then.

As Bill said, "Yes, I had contact with him. Desmond Smyth became a hero of mine." This was not surprising to me, because several years after Desmond had influenced Bill, I was on sabbatical at Imperial College of the University of London, and was able to work with Smyth for nine months. He became a hero of mine as well. I am absolutely confident that Bill and I represent just the 'tip of the iceberg' in this regard, because I know this wonderful fellow had a huge influence on a lot of young parasitologists of that era. He was not only respected for his science, he was cherished as a man and a friend by all fortunate enough to know him.

Bill continued, "You know, there was something strange about that. At the time, I was rather diffident, totally lacking in self-confidence, coming from Donegal into a situation like that at Trinity." He told me that there was fairly high attrition among the biology students who came in with him. There were 48 at the beginning and "something

like 14 who went ahead with their fourth year.” This was the honor’s year, when each student could pick a topic and pursue research on their own, but always under the tutorial of one of the faculty.

“As the third year came to an end, we were told we should pick a special subject for our final year. There were two guys in our small group . . . that had grown up in Kenya, part of the British colonial system in Africa. They were tremendously cosmopolitan and were ‘way up there’ in terms of sophistication. They had arranged to do their projects with Desmond Smyth in parasitology. It was part of the culture there that one does not try and ‘horn in’ on somebody who had already staked out a piece of the action.” Bill said that he was greatly, shall we say, chagrined that he was unable to pick Desmond as a person with whom to work. In retrospect, he said his feelings “were totally irrational, but,” he continued, “I didn’t have the nerve to say that I wanted to do that because Desmond had already committed to these two boys.” So, he thought he would work with someone else. “But, when I came back from the summer holiday to start that final year, there on the table holding the assignments, etc., were three folders, including a notebook and suggested projects by Desmond for parasitology.” I asked, “Then he must have picked you?” Bill responded, “Yes, he picked me. I don’t know whether he sensed I wanted to do it. But that changed my life!” he said excitedly. “The fact that he had made three folders and put me into the group was just amazing.” He went on to say that early in his third year, he had been asked by Desmond to stay on one afternoon to watch the master remove a large plerocercoid of *Schistocephalus* from a stickleback abdomen to use in an in vitro culture experiment, and that this was the moment of his ‘capture’ by Smyth and by parasitology.

“The year passed quickly, but in the spring, I recall that we three honors students were called together and told of a letter Desmond had received from Arlie Todd at the University of Wisconsin. Even though Smyth had never met Todd, the latter had written to Desmond asking him if he had any graduate students who would like to come to Wisconsin to work on their Ph.D.s.” Bill admitted to not having given much thought to beyond the fourth year, but that he became intrigued with the idea when Smyth encouraged all three to apply and they did – and all three were accepted. The only trouble was that the other two dropped out, leaving ‘diffident’ and shy Bill Campbell as the only one of the three to go across the ocean. Bill recalled one of his two friends writing him a letter just before he was to leave and adding a postscript that said, “For God’s sake, don’t panic when you get off the boat in New York City!”

Getting ready for the great adventure was rather humorous. First, he had to get a visa for entry, so he headed down to Dublin and the U.S. Consulate. On arrival, he had to answer a bunch of questions from an official. One of them was, "How much income will you have to live on?" Bill responded, "\$1280 per year as a research assistant." The official replied, "That's not enough." So, he was instructed to go back home and write a letter to Todd. The official wanted to see a statement from the new mentor in Wisconsin that he could make it okay on \$1280. Todd responded quickly that it was enough money. The Consulate official muttered on reading the letter, "Well, it wouldn't be enough in New Hampshire, where I come from." Bill thought to himself, "I don't even know where New Hampshire is!"

The next problem was how to pay for the trip. Bill said, "At that time, most European countries were strict on how much cash one could carry when traveling abroad. In the Republic of Ireland's case, it was only \$15," a real problem indeed. So, he applied for a Fulbright travel grant through Trinity College, the first student from there to ever apply for such a grant, but he received it, enough to pay his travel costs. He knew, however, that \$15 would fall far short of covering his living expenses (including rent) for the month, and he would have to wait before getting his first check as a research assistant.

By a very strange quirk of fate, his father was to receive a letter from a man named Cavanaugh in Chicago, Illinois. It seems that Mr. Cavanaugh was a native of Donegal and had left Ireland as a boy to travel to the U.S.A., where he was to grow up and become a successful lawyer. He offered to Bill, through his father, the princely sum of \$200, as a loan to be paid back as time would allow. The funny thing to Bill over the years was how this man found out that he was going to the U.S.A. and he still does not know. He said that Mr. Cavanaugh had also included newspaper clippings describing the University of Wisconsin's upcoming trip to the Rose Bowl – he hadn't the faintest idea of what the Rose Bowl was, but he was now ready to leave.

On arrival in New York City, he was met at the dock by an 'angel', Mrs. Minucci ("At least that's the way I would describe her," said Bill), from the Institute of International Education. She was a volunteer whose job it was to get him settled into a hostel in New York City where he would stay until he could pick up the \$200 from Mr. Cavanaugh's friend. Bill described the first day as one in which he was actually too afraid to speak. He even found a 'deli' where he could point to the dish he wanted rather than ordering it out loud in a restaurant. This made things rather difficult because after taking a very long tourist

walk in the city, he had to remember how to get back to the 'deli' where he could eat another meal without speaking. In a letter to me subsequently, Bill explained his reluctance to talk out loud was because of his lack of confidence, his Irish accent, and because "I didn't know the names of the food and the dishes that were on display or listed on menus, or how to pronounce them, or even what they were. Words like ravioli, hash browns, hot dog, and so on, were to be mastered gradually, over several months. Even after being settled in Madison, I had breakfast every morning at the same little place, not only because it was cheap, but also because I had discovered the words 'short stack' would magically produce a plate of pancakes!"

When he finally arrived in Madison, he was hit by a couple of serious culture shocks. First, he discovered he had to take more courses. This was unheard of in the European system, for in Europe the Ph.D. was strictly a research degree. Second, he also had to secure housing. Despite the \$200 advance from Mr. Cavanaugh, he found himself in the hole financially, almost immediately. He said that Todd got out his checkbook and wrote him a check for \$50 and he was covered. He remarked, "It wasn't that long before I had it all [his debt] taken care of."

His experience at Wisconsin was evidently a good one as he spoke very highly of Arlie Todd, who was a faculty member in the Department of Veterinary Science, and of Chester Herrick in the Department of Zoology, from which he also obtained a degree. When I first contacted Bill about submitting to an interview, I mentioned in passing something about Wendell Krull and *Dicrocoelium dendriticum*. In his response to my request, he told me an interesting story about a seminar he had given at Wisconsin that had to do with the discovery of this parasite's life cycle, Wendell Krull, and the venerable T. W. M. Cameron. During our interview in Philadelphia, I reminded Bill of our correspondence and he roared with laughter.

He told me that he was "required to give two seminars as a graduate student that I can remember. Maybe I gave more than that, but I can't remember. One was on the newly discovered life cycle of *Dicrocoelium dendriticum*. I was really intrigued by it and wanted to learn more." He went on, "You know, of course, that at that time at least, seminars and debating societies in the British Isles were a big thing, more so than here, in terms of being effective. So, you wanted to include some sort of surprise or humor as a way of advancing your presentation. There was always a high standard and I had never given a seminar before, so I wanted to do well. And for some reason, I was struck by

the fact that T. W. M. Cameron had published an erroneous account [in 1930] of this parasite's life cycle, so I wrote to him about it. What possessed me was that I was giving this seminar and I wanted to do a good job of making a presentation, and I felt this would be really neat to find out what his response would be" to the new accounts so recently published by Krull and Mapes and their new discovery. "So, I wrote to him pointing out that he was wrong, and wondered what had happened.

"Now then, one must understand the whole situation. T. W. M. Cameron was a huge persona in international parasitology during those days and had been for many years. For example, he was the first Director of the Institute of Parasitology at McGill University in Montreal, a cornerstone of Canadian parasitology. At that time, he was probably at the pinnacle of his great career. He also was of the 'old school' and, I am certain, was unquestionably unprepared for a letter that came 'out of the blue', from a lowly graduate student that I am sure he would not know from 'Adam'. Moreover, the implication of the letter, though written very carefully and with great respect, was clear – how in the world did you 'blow it' when you published that paper in 1930 where you said the life cycle of *D. dendriticum* was identical to that of *Fasciola hepatica*?" Bill said, "Even though he was absolutely wrong, he wrote me the nicest letter. It was on blue stationery, and handwritten. He could have torn up my letter, or slapped me down in some way, because he had a reputation for being a very tough sort of person." We agreed that he "had rank and was aware of his position," but he certainly didn't act that way. "So," Bill said, "I gave the seminar and, at just the right time, I pulled Cameron's letter out of my pocket and read it to my colleagues." He recalled, "that Cameron could not account for his findings, except it was obvious he was not working in an ant-proof barn!"

On completing his degree at Wisconsin, he said he was certain he would go into academics. However, chance (serendipity?) was to play a huge role in his actual career decision. In fact, he said, "I was just beginning to look at the names of colleges, and colleges that might be looking for new, young faculty." However, he recalled that Desmond Smyth received a letter from Arlie Todd asking if he had any students who might want to pursue a graduate degree, and the same thing happened again. This time it was a letter from Ashton Cuckler at Merck who wrote Todd asking him if he had anyone who was about to finish up the Ph.D. "Todd showed the letter to me," but he said his reaction was the same as when he was shown Todd's letter to Smyth at Trinity, "I didn't really want to go into industry." But, he said, "Todd was very

pro-industry and was very cagey about it. He said, ‘why don’t you at least go back and talk to them about it? What do you have to lose?’” So, he decided to go.

The first thing he did was to look at a map, and, he said, “That didn’t look very promising at all.” But, he thought, “I’ll go back; at least I will get to see New York again. So, I went at Merck’s expense, and one of the first things I did was to go see Charles Laughton (for \$8) in George Bernard Shaw’s *Major Barbara*.” Since I knew of Bill’s great interest and success in amateur theater over the years, I asked if he had had any previous interest in the theater. He said he hadn’t and I asked why he went. He responded, “Oh, well, I went for the same reason you would go see a play in London’s West End. But, there was a name I could recognize from the movies.”

During his interview at Merck, he spent time with Ash Cuckler and, “He was very nice, and the work was much more interesting than I thought it was going to be.” He sort of hesitated at this point in our conversation and said, “You know I really had an interest in chemotherapy,” like it was some sort of memory that he was conjuring up from his past. Then he remarked, “I think I had an interest in chemotherapy for a long time. I remember going to an agricultural show . . . when I was in Ireland and the only thing I recall about it was picking up a brochure at an ICI booth dealing with a drug used to treat *Fasciola hepatica* in sheep. It was hexachloroethane. I devoured that leaflet. And you know, that only came back to me recently when I was looking up something having to do with *Fasciola* or something like that.” It was then he told me that at Wisconsin he had also dabbled in chemotherapy in trying to treat cattle or deer that were infected with *Fascioloides magna*. He said, “There was something fascinating about the idea of curing a disease. There was something there early.” Even so, he said, “I really didn’t take the interview at Merck very seriously.”

As soon as he returned to Wisconsin, he received a letter from Ash Cuckler offering him a job at Merck. He said, “I was just at the point of writing colleges, and here was an offer in hand.” It was, needless to say, a real quandary, “I didn’t know what to do, a salary of something like \$9080.” So, he consulted with one of his zoology professors and asked, “If I take this job for a year or so, it won’t taint me, will it?” The professor laughed and said, “‘Oh no, it isn’t that bad.’ He said he wouldn’t hesitate. He went on to say, ‘You’re not doing it for life.’” Bill said that Todd was quietly implying the same thing. “He did not have an anti-industry bias, as I did, and I certainly had an anti-industry bias, at least at that time, but I accepted the offer.”

While at Merck, Bill's contribution to parasitology was to be enormous because, as will be seen in a later essay, he was to play a central role in the development of ivermectin, one of the most important drugs in modern veterinary medicine. The story of its discovery is an absolutely fascinating tale.

RICHARD TINSLEY



Figure 6. Richard Tinsley, Professor, University of Bristol, Bristol, England

The first time I met Richard was when he was teaching during the late 1980s at Westfield College, part of the University of London system. My wife, Ann, and I were on one of my U.K. junkets. I had made arrangements to see Richard for tea and we ended up spending the entire morning talking about his research in the southwestern part of the United States. After that, we would see each other at