Events Leading to One Person's Career in Forest Entomology

by

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A. D. Hopkins Address
Southern Forest Insect Work Conference
Memphis, TN
July 31, 2000

Today, I'm going to discuss the subject that I know most about --the important **EVENTS** and the many MENTORS leading to my career in Forest Entomology at the Southern Research Station, as well as my past and present **COOPERATORS.** This includes my **VIEWS** on the present state of SPB Research, as well as my **FUTURE PLANS** for the next 40 years.

1947. My record in high school was not spectacular, and I had no desire to attend college. But all the GI's came back in 1947, sucking up all the jobs. So I entered the Ohio State University, which was just 3 miles up the road from my home.

1948. My first course in entomology. I liked the subject, but just as important, I saw job potential. Here, too, I had the opportunity to interact with several OSU professors that began shaping my career -- Donald Borror, Ralph Davidson (my undergraduate advisor), Dwight Delong. I also began my life-long friendship with Charles Triplehom, another undergraduate student, who was one year ahead of me.

At the same time, I discovered that there was a hackberry tree in the front yard of my home, the leaves of which were covered with lots of different kinds of interesting galls. I discussed this with Prof. Davidson, who encouraged me to observe and take notes regarding the different species. I was particularly intrigued by a tiny, metallic green wasp was that was flitting around and inserting its ovipositor into certain of the psyllid galls. I collected some of these wasps, and sent them to Mr. Gahan at the USDA Systematic Entomology Laboratory at Beltsville, MD, who identified the wasp as a new species of *Torymus. This* was really exciting to me because it meant that I, a lowly sophomore student, had observed something that no one else in the world had ever seen. This pivotal event launched my career as a research entomologist.

My summers of 1949 and 1950 were devoted to driving and assisting Prof. C. H. Kennedy on his collecting trips to Upper Michigan and the north coast of Lake Superior in Canada, where we "chased ants" (Kennedy's term). I was far from the first choice for this job, but I was selected because Prof. Kennedy was quite neurotic, and all the graduate students were afraid to work for him. Whereas he was difficult to work for, he was a great teacher, and here I learned the basics of ant behavior and systematics that were to play a major role in my future job selection in the Forest Service. Kennedy also said, "If you ever get a chance to work on *Atta, take* it; because this is where the real opportunities lie in myrmecology research ."

In 195 1, much to the surprise of myself and everyone else, I received my BS in Entomology from OSU. 1951 was the apex of the Korean War, and I was facing the draft. However, I was informed that my BS in entomology was worth a commission, the drawback being that I must enlist for a minimum of 4 years, and that I would be sent to Korea to participate in a pest control unit. The alternative was to be drafted, serve only 2 years, undergo 16 weeks of basic, but then be sent to work as a tech at a DOD research facility, and forever remain as a private. I chose the latter.

After basic, I was assigned to the Army Chemical Center near Baltimore MD, where I served as a tech at the Army Medical **Corps,** Insect Physiology Laboratory. Here I was

associated with several other of my early mentors, namely Leigh Chadwick, Detrich Bodenstein and Bertram Sacktor, three of the more important insect biochemists at that time. I was assigned to Dr. Sacktor, who was kind enough to include me as a junior author on one of his papers published in the 1953 Biological Bulletin entitled "Dephosphorylation of ATP by tissues of the American cockroach" – my first publication!

While at the Army Chemical Center, Dr. Chadwick permitted me to attend my first ESA meeting held in Philadelphia. But more important was the 3-day pass that I received each month. Normally I would visit New York City and Washington DC on alternate months. In DC, I spent a number of days with Barney Burks, the curator of chalcidoid wasps at the Systematic Entomology Laboratory. B amey helped me to diagnose and describe my new species of *Torymus*, in addition to several other new species of parasitoids that I had found associated with hackberry galls in Columbus, Ohio. In addition, he strongly encouraged me to apply to Cornell University as a PhD candidate.

In September 1953, I was released from the Army, and received the G.I. Bill, which financed the remainder of my graduate education. I then returned to my home in Columbus, and returned to OSU, where in 1954 I completed my Master's, majoring in biological control under Alvah Peterson. My MS thesis on hackberry galls and description of the parasitoid, *Torymus vesiculus was* summarized in the 1956 J. Kans. Ent. Soc. 29:57-62.

In September 1954, I entered Cornell University, majoring in Insect Ecology under Howard Evans, who taught courses in the taxonomy of Hymenoptera and the minor orders. In addition, I shared an apartment with Jack Franclemont, who taught Lepidoptera systematics. These two mentors shaped my present views on the meaning and values of systematic studies to the study of insect interrelationships. At Cornell, I continued my thesis on the natural enemies of hackberry gallmakers, which was published in 1965 as Bulletin 402 of the N.Y. State Museum and Science Service. Of my 100 publications, this is my longest, at 95 pages.

Several months before receiving my PhD in 1958, the Southern Forest Experiment Station sent Jack Coyne to Cornell to earn his MS. Today, Jack is known by his pioneering work of saving and planting scions of loblolly pines that had survived SPB attack. Jack informed me that a GS-4 position in Bill Mann's Timber Management Project at Alexandria LA was opening for a person to solve pine regeneration problems caused by the town ant, *Atta texana*. But I also had an offer of a GS-9 (then the going rate for a PhD) from the Northeastern Station to study predators and parasitoids of the Gypsy Moth. Prof. Kennedy's words rang in my ears, and I took the GS-4 position. I arrived in Alexandria on July 1, 1958, and was the first PhD entomologist for the Southern Station.

My instructions were to "eradicate" the ant, a project to last only two years at the most. This was my first experience with the Forest Service, "short-term," "practical research" syndrome. The project leader expected me to perfect methods of killing colonies with methyl bromide, but I somehow convinced him that real control could not be accomplished without knowing something about the biology of the ant.

In September 1958, I gave a seminar on the town ant at the Louisiana Entomological Society, held at LSU. Here I met Murray Blum, and in the next several years we coauthored a paper in Science on the chemical ecology of town ant trail pheromones, as well as others on the mating flight. Science also accepted another paper by me dealing with the tiny roach inquiline, *Attaphila fungicola*, which followed the ant trail pheromone. Since this compound was favorable to the receiver and not the emitter, I missed a golden opportunity to coin the word "allomone", a mistake that I vowed not to repeat again.

Ed Wilson was interested in the chemical ecology and other studies that Blum and I were doing, and he visited Alexandria twice during the 4 years that I spent with the Timber Management Project. Murray and Ed always entered the back door of the building

so that they would not have to pay verbal tribute to the project leader.

In 1962, the insect project was established at Alexandria under Bill Bennett, giving me the opportunity to transfer to a project much more to my liking. The AD, Les On, knew that I had a penchant for working with "small stuff', and asked me if I wanted to work on the mites associated with bark beetles – an opportunity that I jumped at. Les had a reason for asking me to do this. Les' interest in mites dated back to 1934, when Leach, On, and Christensen published a paper establishing that mites riding on *Ips pini* fed on and disseminated the blue stain fungus in Norway pine. Les wanted to know the identities of these mites and if the same thing was going on for SPB. He figured that I could find out everything that was needed to know in six months. Again, an example of the short-term syndrome of the Forest Service Research that still persists today in many circles. It seems that many Forest Service Research power brokers seldom realize that total reliance on short-term gains (usually for political purposes) leads only to disaster, and that long-term studies are the best rationale for the existence of Forest Service Research; this is because this agency is in the position to accomplish long-term research better than the universities, state forest entomologists, industry, or any other institutions, private or public.

In 1966, Dave Wood visited Pineville to explain the ongoing Western Pine Beetle advances in chemical ecology. He was fascinated with my chemical ecology findings regarding the town ant, and suggested that I contact Milt Silverstein to possibly identify the first ant trail pheromone. I immediately called Milt, who was excited about the chance to work on this interesting species. But first we had to finance the project. The Forest Service was less than enthusiastic about the ant, because Mirex had been found to adequately control this and other leafcutting ants. For this reason they were insisting that I terminate all work on the ant. So Milt applied to the National Institute of Health, which supplied us with $60,000. But first Milt had to make a personal visit to New Orleans to convince the SO power structure that this research should continue, which they reluctantly agreed to do. My job was to collect eight pounds of ants, which Milt and Jim Tumlinson (his postdoc) ground up and extracted 150 μg of pure trail substance. This and about 100 other allied compounds extracted from the ant poison sac were sent to me. My job was then to prepare artificial trails from these compounds and let the ant workers tell me which was the active substance, which turned out to be a pyrrole that we called "Attalure." Unlike most pheromones, which are highly volatile, this compound was a solid of low volatility. But this makes sense, because the ant has a persistent food source, thus requiring persistent trails, which in turn would require a persistent pheromone of low volatility. This substance is extremely powerful. Only 330 μg is required to draw a detectable trail around the world! We published these findings in two papers in Nature in 1967 and 1971.

I then urged Milt to direct our attention to the ant's mandibular gland, containing the alarm pheromone, 4 methyl-3-heptanone. Because this ketone was chiral, we had the opportunity to split the compound into its two enantiomers, and to bioassay which of these alarmed the ant. Although this procedure is now routine, it had never yet been accomplished for any bioactive substance, such as a pheromone. Milt's PhD student, Bob Riley, isolated the two enantiomers, and sent them to me. My bioassay established that only the (+) enantiomere that active. In 1974, these findings were published in Science.

Based on about 20,000 slides that we had processed since 1963, Lary Roton and I published in 197 1 the first paper listing the mites associated with SPB. This paper answered many of Les Orr's questions about the array of mite species associated with blue stain, and documented that the tarsonemid mites were probably the primary players. But the main problem was that the vast majority of the mites we found were new species in need of names.

Lary and I prepared the slides, and sent them to a number of mite taxonomists, then active

in the USA and Europe, to describe the many new species we were finding. In particular, Ed Baker, Bruce Boudreaux, Bob Smiley, Jay Woodring, Evert Lindquist, Henry Hurlbutt, Werner Hirschmann, Preston Hunter, Jerry Krantz, Earle Cross, Sandor Mahunka and Jersy Wisniewski. This resulted in a flood of descriptive papers, many of which I coauthored with comments on the biology of the various new species. Many of these persons have since died or retired, leaving no one to replace their taxonomic specialties.

Also in the early 1970's, Jack Coyne, Bill Rose, Ed Clark, and Robert Wilkinson sent me mite material collected in Mexico, Honduras, and Guatemala with pine bark beetles. In 1974, these results were published in Turrialba as the first list of the mites associated with pine bark beetles in Central America. This list would come in handy as a rationale, when in 1997, Jorge Macias and I would successfully apply for a Forest Service grant to survey the mite and insect associates of SPB in Chiapas Mexico.

Earle Cross and I published a number of papers on the systematics and biology of the pyemotid mites associated with bark beetles. This group of mites is important because some of the species are parasitoids, whereas other related species consume and may transmit the mycangial fungi of bark beetles. Pyemotids are unique because the females of many species have discrete forms that do not intergrade – sister forms that look so different, that they have been placed in different families. In 1975 we coined the word "phoretomorph" for one of these forms containing a large claw enabling the mite to hang on to the beetle during the mite's phoretic transfer from one tree to another. Two other sister forms are also recognized; a "normal" form with a small claw, and a "giant" form that can only be described as an "incredible hulk."

By the mid 1970's there was still no efficient way to trap live SPB for population studies, so Lloyd Brown and I devised a trap constructed from a paint bucket baited with frontalure. This was the first practical pheromone trap for SPB, replacing inefficient nets, sleeve funnels, and sticky traps then in use. This bucket trap only trapped male SPB , but did so in large numbers, enabling me and Dell to model SPB and its clerid predator's flight and seasonal activities over an entire year.

In 1986, data from these traps allowed Thompson and me to determine the temperature thresholds regulating the flight of SPB . Not surprising were the optimum and maximum flight temperatures of 27" and 38"; but the real surprise was that SPB flew as low as 6.7°C, the lowest temperature ever recorded for any flying bark beetle. Unlike its northern cousins, SPB is active all winter, and often needs to fly at low temperatures.

In 1974, I secured a PL-480 grant, together with Bohdan Kielczewski and Jerzy Wisniewski to study the mites associated with Polish Scolytidae. In 1975, I was able to visit to Poland, where we finished two publications listing the bark beetle mites of Poland, and the biology of *Pyemotes dryas*, as a parasitoid candidate for possible importation to the U .S .

In 1975, the expanded SPB program began. I was included for one year in this program, but then shut out, because my long term mite and population studies were deemed of no practical importance to the control of SPB , and because of my proposal to use the money to explore the insect and mite natural enemies of SPB in Mexico. The conventional wisdom of the persons running this program was that only short-term studies were needed, because we basically had all the knowledge needed to control the beetle. The fallacy of this reasoning was accentuated in 1985 – the year the program ended – by the largest SPB outbreak in history.

In 1984, 1989, and 1997, I teamed with Herman Bogenschutz, Hubertus Eidmann, and Kimito Furuta to produce keys and analysis of the mites and their associated fungi of *Ips typographus* in Germany, Sweden, and Japan.

In 1984, Yang Zhongqi arrived in Pineville to spend a year with me researching the biological control of the black turpentine beetle (BTB). With the aid of Jean-Claude Gregoire, we imported *Rhizophagus grandus*,

a predator of the European turpentine beetle, *Dendroctonus micans*, to rear, and possibly introduce for the control of BTB. But again, this long-term study was not regarded as something the Forest Service Research wanted to do, and after one year I had to scrap the study. But the experience gained here is now being put to good use by Dr. Yang, who is currently the Deputy Director of the Institute of Forest Protection, Chinese Academy of Forestry, Bejing. The red turpentine *beetle, Dendroctonus valens*, is currently causing large losses of pines in China, and Yang has imported *R. grandis* to use as a crash program for the biological control of this scolytid.

In 1985, I documented that phoretic *Tarsonemus* mites of SPB and several other coniferous bark beetles transported ascospores of bluestain fungi by means of a special morphological structure on tergite 1, termed the sporotheca.

In 1986, I teamed with Meredith Blackwell, Bob Bridges, and Thelma Perry to document in Science, the first case of a fungus with attachment mechanisms for phoresy on arthropods.

In 1990, Yang Zhongqi arranged for me to visit China for one month to see bark beetle problems in Central, West, and Northeast China. One of the more interesting visits was to see an outbreak of *Dendroctonus armandi,* which attacks only *Pinus armandii* in the Qinling Mountains of Central China south of Xian. This interesting bark beetle is still an object of study by Yang and his students, and he is currently collecting flying adults so that I can scan the mites.

RETIREMENT!

By mid 1989 the AD and PL were pressuring me to terminate my town ant and mite studies. Since I had no interest in their short-term replacement projects, I began seeking other solutions to this dilemma. One alternative was to consider retirement. With my military service, my total time served came to 32 years, which meant that my annuity would be an amount approximating one-half my salary. In addition, if I retired before Dec 3 1, I could withdraw all the money that I had previously paid into my retirement annuity, as a lump sum of $58,000, which I could invest. In addition, my investments were now delivering yearly amounts totaling twice my salary. Hence I was now in a position where I did not need my paycheck. So I decided to retire on Dec. 31, 1989, and immediately signed on as a volunteer. This meant that not only could I retain my ant and mite studies, but also that I could keep almost complete control over them. This arrangement remained a bit rocky for the first 7 years, but with the ascension of Kier Klepzig as project leader, the environment vastly improved.

In the past 10 years since I retired, my primary focus has been on the relationship of the tarsonemid rnites to the SPB mycangial fungi, as well as still keeping an interest in the leafcutting ants.

In 1995, Thelma Perry and I published an article in Mycologia documenting that two of the species of *Tarsonemus* phoretic on SPB not only carried the crescent-shaped ascospores of *Ophiostoma minus* bluestain fungus in their sporothecae, but these mites also carried the tadpole-shaped ascospores of one of the two SPB mycangial fungi, *Ceratocystiopsis ranaculosus,* a species described by Thelma in 1987. Efforts to model this association and others are now being pursued by Kier Klepzig , Matt Ayres , Maria Lombardaro, Rich Hofstetter, and others.

Kier Klepzig and I are also investigating the other SPB mycangial fungus, *Entomocorticium* n. sp., and how it may be dispersed by females of *Elattoma* sp., a mite phoretic on SPB. We already know that a closely related species, *Elattoma bennetti* (named after Bill Bennett), feeds on the nutrient fungus associated with *Ips avulsus,* and is phoretic on this beetle.

In the mid 1990's, I met Jorge Macias at the Western Forest Insect Work Conference. Jorge was pursuing a Ph.D. under John Borden, after which he planned to return to Chiapas to study the population ecology of SPB . I mentioned to him that my draft proposal to study the arthropod natural

enemies of SPB had been laying around since the early days of ESPRAP, and that this proposal had constantly been rejected as "pie in the sky" by the Forest Service. But I mentioned to Jorge that sooner or later the conditions would be right to enable funding. This happened in 1996 when President Clinton visited the President of Mexico. One of their "accomplishments" was to release some research money for cooperative forest research in Mexico. I then brushed off this old proposal, added Jorge's name, and submitted it to the WO Forest Service Staff.

The proposal was judged as second for all of the proposals received, and as a result, RWU 4501 received $15,000 in "seed money". The timing of this funding was perfect. It arrived late in 1997, at which time Jorge received his Ph. D. from Simon Fraser University. In November 1997 Jorge arrived at Tapachula, Chiapas, Mexico, and joined the Colegio de la Frontera Sur (ECOSUR), and immediately began his studies on the SPB problem in Southern Mexico and neighboring countries. Since his initial budget for this was nearly zero for research, at least, the grant money and the subsequent visit by the RWU 4501 staff in April 1998 provided a real boost for him. Since, there have been almost daily communications, as well as trips each April by the Pineville staff to Chiapas, and at least one trip to Pineville by Jorge to coordinate our joint research on mite and insect associates, mycangial fungi, and the impact of clerid predators.

At the 2000 ESA meeting in Montreal, Jorge and I presented our initial findings of this cooperative venture, and published the results in a special issue of the Canadian Entomologist dedicated to John Borden. In this paper, Jorge (a student of Borden) and I stated our reasons why we believe the SPB invaded the US from Mexico only about 5000 years ago, and is thus a recent immigrant -- a fact that has important implications for SPB control, and future research directions. For example, John Reeve and others have shown in the U .S . that SPB is controlled about 7 out of 8 years by the clerid, *Thanasimus dubius*. But in Mexico, ***T.*** *dubius* is absent, and another clerid, *Enoclerus ablusus*, fills this ecological niche, and seems to be controlling the SPB in Mexico and Central America. It is obvious to us that this fact has classic biological control implications for both Mexico and the US.

Although ***the*** *Tarsonemus* mites associated with the SPB mycangial fungi in Chiapas seem to be the same as those in the southern U.S ., the relative numbers of the ascospore species that they carry are not. Preliminary indications are that in Chiapas, these mites ***carry*** lots of the ***Ceratocystiopsis*** ascospores, but few of the ***Ophiostoma*** ascospores -- a situation that is reversed in the southern US. One possible explanation for this is that the species of these two mycangial fungi may differ in the southern U.S. and Chiapas .

We hear a lot of talk these days about biological diversity. But what few persons realize is that little progress can be made in biodiversity studies unless one knows which species that he is working with. For arthropods, at least, many that we find associated with the SPB are new species. But getting them identified and/or described is often a major operation, because today, taxonomists are a rare item. I propose to do something about this.

This relates to my legacy, and I'm going to share with you the major provisions of the wills of my wife and myself. As you may know, I have been and still am an aggressive stock investor. My current net worth is in excess of 4 million, a figure that has been appreciating at the rate of 20% per year. Last year my wife and I revised our wills to establish two chairs in entomological systematics -- one at Cornell, and another at the Ohio State University. Currently, the going rate for such chairs is two million each. But if our estate keeps appreciating, as I think it will, we may soon have another two million to fund several graduate students operating under each of the chairs.

And another idea circulating in my head is to perhaps propose to the Southern Research Station Director my ideas on funding a similar chair for RWU 450 1 – independent and free of administrative control-- to continue the 4501 bark beetle mite studies.

And if funds are still available, I want to pour another million into a fund to provide abortions for poor women in Louisiana ---- my parting shot to the religious right.

But this old soldier has no intention of fading away. I'm currently 7 1 and intend to be active for at least another 40 years. I've got a lot of things that I still want to do.

My files are stuffed with unpublished material. For example, I have a 1959 manuscript describing the complete excavation of a town ant nest. So far, all of the nest excavations of leafcutting ant nests, both in North-and South America, have been mapped only partially.

I also have cooperative agreements with a number of persons dealing with leafcutting ants and bark beetle mites. Peter Masan (Brataslava, Slovakia) and I are sculpting a manuscript describing a number of new species of uropodids associated with North- and Central American bark beetles, with keys to these species. Wojtec Magowski (Poznan, Poland) and I are revising the Triarcus group of *Tarsonemus,* in which we describe a number of new species of *Tarsonemus* mites associated with bark beetles in North America and Europe. One of these species of particular interest to us is the new species associated with the black turpentine beetle.

With Matt Ayres and Kier Klepzig and others, I want to assist in working out how these interesting mites interact with their bark beetle hosts and with each other.

Ronald Ochoa and I are describing *Bakerdania* n.sp., another phoretic pyemotid mite found in association with the SPB mycangial fungus, *Ceratocystiopsis* sp. in SPB galleries in Guatemala. We suspect that this mite, a relative of *Elattoma*, may feed on this fungus.

Lourdes Peralta and I are documenting an interesting case of biological control in Chile, where the tip moth, *Rhycionia bouliana,* is being attacked by a parasitoid pyemotid mite.

With Scott Cameron, John Reeve, Terezinha Della Lucia, and others, I want to determine differences in ocelli and eye size between the day- and night-flying species of leafcutting ants.

These are just some of my many projects currently in progress. For better or worse, I'm going to stay around for a while.

PROCEEDINGS

44th Annual

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