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Understanding ourselves

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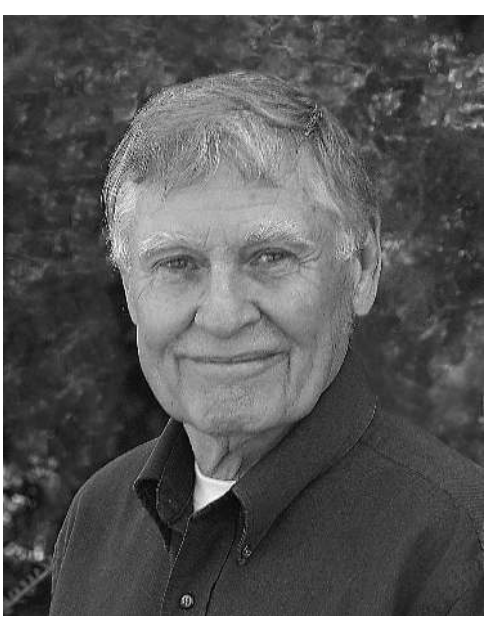

Biographies, as generally written, are not only misleading but false. The author makes a wonderful hero of his subjects; he magnifies his perfections, if he has any, and suppresses his imperfections. History is not history unless it is the truth.

Abraham Lincoln

Myth does not mean an absence of truth but a concentration of truths.

Doris Lessing

Would that all of our autobiographical myths could be concentrations of truths!

Autobiographies are trickier than biographies. Stanley Elkin (1993) has suggested that everyone has a worthwhile life story to tell about herself or himself, but no one is likely

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to tell it completely and accurately. Part of the reason for these failures, said Elkin, is that we never reveal everything in the darkest corners of the basements of our lives (his actual phrase was the "nasty hoard" in the "secret cellar"). Another part is that, sometimes, we simply cannot recall and interpret accurately, even if honestly, what really happened, when, and why.

When autobiographies are requested in scientific contexts, the accounts are probably expected to center around the scientific work of the author. This may or may not be the author's first choice in autobiographic materials. But such a focus surely eases the first reason for imperfection, though not eliminating it entirely. All scientists are likely to have at least a few too-dark secrets in the basements of their professional performances, especially in the sociality and ancillary responsibilities of their science. The focus on science also retains difficulties with regard to recalling and interpreting honestly, because all of us so-called scientists have gradually but certainly adjusted and re-adjusted our views of the tendrils of understanding and influence that were generated during the earliest stages of our budding careers, and that have contributed to our becoming what we are. We cling to our interpretations of the steps in our development and performance because they seem to make sense to us, and they are likely to exalt us more than the alternatives; and also because, as is surely healthy in moderation, we tend to like our own versions of ourselves. The saving grace is that, if the connecting aspects of our grown-up version of how we, as we might think, "came to be famous" were put together early enough in our careers, they may actually have influenced significant portions of our life's itinerary.

There is likely a parallel to all of this in the evident reluctance of people in general, including at least most biologists, to accept ourselves – meaning all humanity – as having evolved through a process of differential reproduction, an acceptance that necessarily calls for submitting to a thorough revealing of how we have evolved and what we are evolved to be, and to do. Even universal darkest corners of basements – and *a fortiori* those who publicize them carelessly – can be difficult to tolerate.

When I was invited to provide an autobiography for the first volume, I rejected the idea because, at age 52, I still held the fond belief that I had scarcely begun. Now, on the verge of age 80, thinking otherwise seems a little easier. As might be expected, I have sometimes favored information and activities not represented in my published work, or obscurely represented there. Because the different topics that held my attention across the past 70 years or so did not appear in a simple non-overlapping sequence, the reader will find me returning to earlier dates each time the subject changes. My professional attention to human behavior and evolution did not develop until the mid-1960s, but its origins, I realized belatedly, were older than any of my other academic interests.

Early life

I was born, and lived during the first 16 years of my life, in a modest farmhouse in Sangamon Township, Piatt County, Illinois. Our farm was not far from the north branch of the Sangamon River, and the extensive wooded areas along the river became my principal

boyhood haunt. My family made its living from a 151-acre general-purpose farm operated "on the shares" with the landlord, a high school classmate of my parents. We grew corn, oats, clover, alfalfa, and pastures, all as livestock feed – no cash crops. We bred, raised, and marketed hogs and beef calves, and sold cream from several milk cows and eggs from a large flock of hens. We relied heavily on chickens for our own meat because a chicken was the appropriate size for one meal so that there was little or no need for an icebox. Virtually all of the meat, eggs, and milk products that we consumed came from our own animals, and most of our vegetables and fruit came from our two large gardens. We separated cream and skim milk with a hand-cranked separator kept in the kitchen, and made butter with a small hand-cranked churn. Skimmed milk (today's "no fat" milk) was fed to the hogs. My mother preserved meat, vegetables, and fruit enough to last the winter, nearly all that we needed – at first in Mason jars, later with a hand-cranked home-canning machine. Her cook stove was fueled initially with wood, later with coal. She used a hand-operated washing machine and wringer at first, then acquired a used machine operated by a step-start gasoline engine. Our house was heated by two free-standing oil stoves, one in the living room and one in the dining room.

My home environment those first 16 years was a rich one, full of hard work and the incessant demands of a complicated livestock operation. All of our farm work was done with horses until I was 13, the same year that electric lines reached our farm. The independent play and exploration of farm kids in those less complicated days, when there were virtually no "No Trespassing" signs and a higher proportion of unsupervised and unrestricted activities, now seem to me to have been unusually conducive to development of a creative and imaginative approach to life (cf. Alexander 1991b, 2001a, 2004, 2005b, 2006b, mss. 1–3, 5)

I have two siblings: an older sister, Nell Beadles (dentist's wife and homemaker), and a younger brother, Noel (farmer). Both of my parents, Archie Dale Alexander and Katherine Elizabeth Heath Alexander, attended college briefly, and each taught in a one-room country grade school for a few years before changing to farming. I attended a one-room country school for seven years, starting at age five. The second year I was boosted by my teacher, Mrs. Edna Williams, to third grade, the same grade as my older sister (this same sequence occurred for my mother, my father, and my aunt Ruth; other than my father, Ruth was the only one of her eight siblings to complete high school). My school was elegantly spare, with no library other than an 8-volume set of *Compton's Pictured Encyclopedia*. When I was in seventh grade, an 8-volume set of *Book Trails* was added. In seventh and eighth grades, we were required at the end of the school year to spend a day at the county seat, taking written examinations constructed by Charles MacIntosh, the county superintendent of schools, to verify that we were qualified to proceed into high school.

My high school graduated 46 students in my class of 1946. I had spent six years in 4-H and four in the Future Farmers of America. I was chosen by the local Rotary Club as the "outstanding boy" in my high school class, but I graduated fifth or so, behind a slate of scholarly girls. I never won an athletic letter, a failure that has always bothered me. I did win "outstanding student" awards in art and agriculture, and a blue ribbon in a saxophone quartet at a state band contest; and I achieved State Farmer status in the Future Farmers of America,

only the second instance in my high school (the other my oldest male first cousin). I sang in the school chorus and in a male quartet, and played on "scrub" teams (nowadays called "junior varsity") in football and basketball. I also tried once to run the mile in track competition and came in last.

My favorite course in high school was English, with an emphasis on poetry. The use of language, in all of its aspects, never stopped being a passion, and I always regarded it as my second choice in teaching. It occurred to me long ago that even such a seemingly simple source as a dictionary can be used to extract broadly interesting and deeply significant information about ourselves as members of the human species.

I remember a serious discussion with our English teacher, Miss Katharine Turner, about Robert Frost's intended meaning for his poem *Mending Wall*. Miss Turner suggested to the class that the poem showed that farmers are generally too set in their ways to change in accord with newer times. Sensitive about the sometimes tense interactions of my father and his neighbor, who each held responsibility for half of the livestock fence between their two farms, I argued to her that the poem reflected Frost's belief that for neighbors to cooperate in repairing that no longer essential stone wall between their properties helped maintain their friendship and cordiality. I liked Frost's, "Good fences make good neighbors." But I haven't forgotten his question to someone who asked him to explain one of his poems. He is reported to have sat in silence for a moment, before he said, "What do you want me to do, say it in a worser way?"

In 2001, I was inducted into the Monticello Community High School Hall of Fame – a delightful experience that included riding with Lorrie, my best friend now of 61 years, and wife of 58 years, and with some of the grandchildren of my brother (our own four grandchildren were attending school in California). On the afternoon of Homecoming day we traveled slowly in a broad circle through the town of Monticello, in the lead car of the parade, with a huge sign on the door with my name on it. During the entire ride we called back and forth with friends, relatives, and classmates among the crowds that lined the parade route, some of whom I hadn't seen in more than 60 years.

In the fall of 1945, Helen Burgoyne, a teacher of Latin, whom I had not even known was my high school advisor, walked past my girlfriend and me in the hallway and paused to ask if I intended to go to college in the fall. I said I didn't know, and wondered aloud why she had asked. She looked at me for a moment and said, "Come with me." She took me to the school office and showed me my scores on college entrance examinations and other tests, all of previously unknown significance to me. I was dumbfounded, by her interest, by the existence of the tests, and by the scores she showed me. When I explained that my family had no funds to send two offspring to college at the same time, she told me about Blackburn College, a southern Illinois school with a student work plan and a total expense of US$250 for the entire year, including room and board. That was, of course, a time when a farm hand's pay was approximately a dollar a day, and room and board; and when a new Ford V-8 sedan cost a little over US$700.

After five semesters at Blackburn (described in detail in Alexander 2001b), I transferred to the Illinois State Normal College (now Illinois State University), where I majored in

education in biology, graduating in August 1950. I had decided I needed courses not given at Blackburn, and also realized I could work more hours outside classes there, earning my way completely by attending classes and retaining jobs 12 months of the year (Blackburn restricted students to 15 hours of work each week). Because of this schedule, and my indecision about professions, I accumulated over 150 semester hours of coursework. Without my being conscious of the reasons then, most of my optional courses were human-oriented, on such topics as introductory and educational psychology, abnormal sociology, philosophy, history, economic geography, and others.

During undergraduate years my interests progressed through a sequence of perhaps predictable changes in anticipated and desired professions: dairy farmer, veterinarian, county farm advisor, chemist, teacher, biologist, and finally entomologist. Two of these dreams failed for specific reasons. My necessarily out-of-state application to veterinary school, when I was a sophomore with a single course in biology, was rejected. I also discovered that the severe headaches that frustrated me in chemistry laboratories were a reaction to nitric acid fumes.

My "off-the-farm" work began at age 14 or 15, when I detassled seed corn. A year or so later, as a result of the World War II shortage of metal for repairing combines, I "ran a rack" (loaded and drove a wagon transporting bundles of oats to the thresher) with a team of horses, across 2–3 weeks and many miles, on the last old-time threshing run in Piatt County, Illinois (Alexander ms 2). At ages 15 and 16, I worked parts of two summers on a neighboring farmer's commercial hay baler (Alexander ms 2), and spent much of one summer mowing fencerows with a hand scythe on a neighboring farm – a half mile a day. During two summers, 17 and 18, I worked in a gravel quarry, first as clay-picker on a rock-crusher, and later as grease monkey on the gravel washing plant. For a short time I worked 60 feet above the ground helping construct overhead bins for sand and gravel, a stint that earned me the union wages of a high steel worker. I also went through the initial steps of learning to weld and to operate a dragline crane and a bulldozer (Alexander ms 2). At Blackburn I fired furnaces, tended the lawn, felled trees, hauled garbage, painted interiors and exteriors of buildings (dorms, offices, and a chapel), janitored classrooms, and helped tear out and remove an old railroad. At Normal I worked at several jobs simultaneously: handyman for multiple families, washing dishes and glassware in a restaurant and a chemistry department, and serving as biology department teaching assistant and stockroom clerk. As a graduate student at Ohio State University I was of course a teaching assistant. During the first summer break I bottled milk at the university dairy and later made illustrations for an entomology textbook being authored by two professors: my eventual doctoral advisor, Donald J. Borror, and Dwight M. Delong.

My roommate, across nearly all four years of undergraduate work, and ultimately my 60-year closest and most influential male friend, was the late Dr Carl Walter Campbell. He was, like me, an Illinois farm kid. As a University of Florida professor, he came to be regarded as the first-ranked tropical fruit horticulturist in the world and one of the three most important of all time. As students, Carl and I worked at the same jobs, took almost all the same courses, sang together in choirs, glee clubs, and quartets, and for nearly two years

cooked most of our meals in basement kitchens. Among other things we learned how to make a meal of bread and milk-and-flour gravy that included half a pound of pig liver, the last item costing seven cents. Carl and I thrived on thinking of ourselves as deep and profound thinkers, and serious critics of all that surrounded us. No topic was outside our limits. While undergraduates, we generated a strong interest in American folk music and occasionally performed together. As a result I learned to play, rudimentarily and left-handed, guitar, fiddle, harmonica, and five-string banjo, and I searched out and saved a few nineteenth century local songs, known only to one or two of the then oldest people in the farming community of my childhood (Alexander ms 2). Carl, who died in 2006, was an extraordinary intellectual influence across my entire adult life, but especially during our undergraduate years. In 2006, he and I received honorary doctorates in science from Blackburn College at the same podium on the same day without the then Blackburn officials knowing beforehand of our relationship. As well, only two years apart, we received the inaugural lifetime achievement awards of our most appropriate but quite different scientific societies. We each had a hand in starting one of those now international societies, dealing, respectively, with tropical fruit horticulture and human behavior and evolution.

Although I rejected a career in agriculture after only one year in college, I never lost my attachment to farming as a way of life. As a result, Lorrie and I have operated an 80-acre farm continuously for 35 years. We have raised mostly hay, and maintained several pastures; continuously bred and raised horses and cattle; and started and trained riding horses. Our major operation has been maintaining breeding herds of horses to support my effort to understand and write about horse social behavior and the horse–human interaction (Alexander 2001a, 2006b, ms 3). Included among our crops were our hard-working, hard-thinking daughters, Susan and Nancy, now school teachers in California, and, more recently, four diverse, exceptional, and dear grandchildren who continue to visit us regularly: Alex and Lydia Turner, Morgan Johnson, and Winona Johnson-Alexander.

I always liked school, but I sometimes had trouble staying with its specific projects. If I had been in elementary school more recently, I might have been diagnosed as a victim of one of the now widely discussed short-attention-span afflictions. As a freshman in college, having to study hard for the first time in my life, I realized that, compared with fellow students in my study hall, I had difficulty continuing an assignment until it was finished. I contemplated this problem seriously, until I generated the conscious strategy of continuing an intellectual activity only until my interest began to lag, then changing immediately to the most attractive alternative and repeating, returning to the temporarily abandoned projects one by one – over and over if necessary – until all were completed. I think I eventually learned to follow this procedure without conscious effort. It has made me an insufferably frustrating co-author and caused my working spaces to be cluttered to the point of being intolerable to others. But, even though I may have taken the strategy to extremes, it seems to have worked reasonably well. Several years ago a publisher's representative sat down with me at a meeting of the Human Behavior and Evolution Society, noted that her company had recently published the autobiography of a certain prominent evolutionary biologist, and suggested that I might also write an autobiography for them. I replied that I hoped to write an

autobiography someday, but it was not an immediate goal. I said I had a number of partly finished book manuscripts on my computer that I regarded as more pressing. After a moment she asked how many book manuscripts I had on my computer. I answered immediately and truthfully, "Fifty-five." After a significant period of silence (or a period of significant silence!), she rose and said, "Thank you," as she departed without looking back. I still have about 40 of those manuscripts to go, so I have little hope of reversing her opinion that I am stark raving mad.

In the fall of 1949, as a college senior, I applied for a high school science teaching job in southern Illinois, was interviewed, and accepted the position. While the contract was in the mail, two professors, Ernest M. R. Lamkey (bacteriologist) and Donald T. Ries (entomologist), heard about my prospective job and phoned me repeatedly, urging me instead to consider graduate school. I told them I knew nothing about graduate school, and, because of my father's serious (soon to be terminal) illness, neither my parents nor I had funds for such a purpose. As a result, each of these two thoughtful professors secured a teaching fellowship for me, by communicating with old friends, Ries with Howard Evans at Cornell and Lamkey with Alvah Peterson at Ohio State. I chose Ohio State because it was closer to my home in Illinois (thus, to my ill father), and entomology over botany and zoology, because of how well I had liked Ries's undergraduate entomology course. Otherwise, according to Howard Evans, I would have been his first doctoral student at Cornell. I likely would have studied wasp behavior because I was already attracted to it. Instead I became the first doctoral student of Donald J. Borror and studied, across the first phase of my professional life, speciation, acoustical behavior, sexual behavior, and aggression in the singing insects, and the evolution of life history patterns in crickets. My Master's advisor at OSU was Lamkey's old college roommate, Alvah Peterson.

A week after I obtained the B.Sc., Lorraine Kearnes, whom I met at Blackburn, and I were married and left immediately for Columbus, Ohio. On my first day at OSU, before classes had commenced, I talked with Dr Peterson about courses. One of them, we decided, would be an independent research course on dragonflies, an insect group that had fascinated me while I was amassing an insect collection for an undergraduate entomology course. I thought of dragonflies as the dinosaurs of the insect world. I had first paid close attention to them in an abandoned and partly flooded gravel pit in Normal, Illinois. When I left Peterson's office that afternoon I went directly to the Olentangy River, carrying an aquatic net and jars, and began to collect dragonfly and damselfly juveniles. By the end of the term I had distinguished the juveniles of all of the local species, and matched all but one of them to the known adults in the area. That one may have been a new species with cryptic adults. I never found out. But I had become permanently imprinted on the Odonata, and I have continued across many years an effort to explain their unique – perhaps uniquely bizarre – copulatory behavior, in conjunction with my interest in the origin of insect wings and the phylogeny of mating behavior in the Arthropoda (Alexander 1961, 1967a, ms. 4; Alexander *et al.* 1997; Carle 1982).

After the first term at OSU, I nearly dropped out of graduate school because of a B− grade in Donald J. Borror's course on insect systematics, which I regarded as my most important

course. I studied alone that first term, knew virtually nothing about graduate school or the department, and believed (erroneously) that Borror couldn't possibly expect us to learn "all that stuff." Lorrie earnestly talked me into continuing one more term, during which I managed to elevate my almost defunct self-respect by obtaining the highest grade in the second term of Borror's (to me, and to many others) all-important course.

In August 1951, I completed a master's degree with a thesis on the biology of arthropods living inside shelf fungi. I collected fungi everywhere I could within about 100 miles of the University, placing them in paper bags and watering the fungi at intervals, saving some immatures for illustration, and allowing the rest to become adults so that I could identify them. The entomology graduate students at Ohio State then were a wonderful group. Regardless of the nature of their thesis work, nearly every one had chosen a particular group of insects to study, biologically and systematically. Carloads of entomology graduate students traveled to unglaciated southeastern Ohio on weekends, often sleeping overnight in the woods. It was an exhilarating and rewarding time that I believe had a strong influence on my later scientific activities.

During the last part of that summer I completed 50 or 60 drawings for Borror's entomology text (including a drawing of a male ant used on the cover spine) before entering the Army in September. Most of my drawings are still in that remarkable textbook, which may have set a record by being continuously in print, and in classroom use, for 54 years; it is currently a text in the introductory entomology course at the University of Michigan. Imagine my pleasure when, for the most recent (7th) edition of what is now called *Borror's Biology of Insects*, I received a request from Charles A. Triplehorn, one of the current authors, and a close friend and graduate student colleague, 1950–51, for use of two of my more recent drawings.

In the fall of 1951, I was drafted into the Army – probably because my conscience, from having experienced the extreme patriotism of WWII, prevented me from completing the examination that determined whether or not a graduate student could be drafted. It was later explained to me in front of the Monticello, Illinois, courthouse and all the other departing inductees, that I was drafted out of graduate school because the fellow who otherwise would have gone had to stay home and help his father harvest corn. I accepted this decision with a faint humor but without resentment. In Chicago I was invited to apply for a direct commission. When I realized I would have to serve three years instead of two, plus the six months before the commission could be approved, I discarded the application, enabling me to return to Ohio State in the fall of 1953. Luckily for me, the war was formally ended less than two weeks before I was scheduled to be shipped to Korea as an infantry rifleman, one of the most dangerous roles in an extremely dangerous war. After basic training I became an entomologist in the Medical Service Corps at Fort Knox, Kentucky, in the winter inspecting mess halls and in the summer locating potential malaria-carrying mosquito breeding sites for the pest control unit. I was assigned to the latter job because of the concern that returning soldiers might bring Korean malaria back with them. During that time I also explored the caves of Fort Knox with a fellow soldier, became interested in speciation in the blind cave beetles there, and hoped for a while to do my doctoral research on that problem at the

University of Chicago, with Allee and Emerson, until I learned that both had retired. In 1960, Emerson served on the committee that awarded me the AAAS Prize.

Career stage 1: the singing insects

While in the Army I successfully sought to work with Donald J. Borror when I returned to OSU. When I walked into his office he handed me a new Magnemite battery-operated tape recorder with a hand-cranked spring-powered motor, and D cell batteries powering the recording apparatus. It was one of the first battery-operated tape recorders made following American acquisition of tape recording technology from the Germans after winning the war. Borror simply said to me, "Why don't you take this out in the field and see what you can learn about the singing insects." I did that, and by late summer I had managed to tape-record, collect, and identify all of the species of crickets, katydids, and cicadas in the vicinity of Columbus, Ohio. Included were many undescribed and misidentified species. Dr Edward S. Thomas, an astute and kind lawyer and natural historian curating insects in the Ohio State Museum, who knew most of the singing insects of Ohio well, took me under his wing. I also wrote for advice to Bentley B. Fulton, then a professor at North Carolina State. He sent me a wonderful letter welcoming me to the study of singing insects, and telling me exactly how he would proceed in my situation. Later I visited him, and we became good friends. When he died, I was invited to honor him by lecturing to the general session of the North Carolina Academy of Science. I titled my talk *Dr Bentley B. Fulton and the Singing Insects.*

Among other things, Fulton was the first to hybridize different species of crickets and show, by using ingenious devices he constructed himself, that the songs of the hybrids were distinctive, and never heard in nature despite micro-geographic intermixing of individuals of the species involved. Though he didn't mention it, his (1933) experiments also showed that species differences, as well as "complex organs" within species, are due to a history of what Darwin called "numerous, slight, successive modifications" (see also Alexander 1968b, 1978, 1979, ms 1). Fulton thus demonstrated that species differences can be explained by micro-evolution, and that, at least in this case, so-called macro-evolution is simply micro-evolution extended. The relevant one of Darwin's several remarkable challenges was: "If it could be demonstrated that any complex organ existed, which could not possibly have been formed by numerous, successive, slight modifications, my theory would absolutely break down." (Darwin 1859, p. 189; for other challenges, see Alexander 1979, ms. 1). Needless to say, not this challenge, nor perhaps any of Darwin's other challenges, has been met. This particular challenge of Darwin's, and Fulton's extension of it to species differences, nullifies a major argument of those who think "intelligent design" is necessary to explain species differences (see also Alexander 1978, 1979, 2008, ms 1).

My first contribution to science was a paper delivered in 1954 at the Ohio Academy of Science meetings in Athens, Ohio. The title was *Songs of the House Cricket*. It was an effort to analyze and explain functionally the array of stridulatory signals made by the introduced European House Cricket, *Acheta domesticus* Linnaeus, using the tape recorders acquired by Borror, and the only OSU audiospectrograph, made available by Joseph Hynek (later

director of the UFO project) in the astronomy observatory. The talk was something of a disaster. I had ambitiously – and recklessly – planned to synchronize slides of audiospectrographs of songs with tape recordings of the same songs. Perhaps this had never been attempted before, at least publicly. Unknown to me, the OSU professor, Carl Reese, who agreed to show the slides for me, had opened his car door into the street during the noon hour, just before my talk, and a passing motorist had slammed the car door around against the side of his car. The poor man came in late, still traumatized, and immediately got my slides irreversibly out of synchrony with the tapes. As a result the talk was hopelessly confusing and ran well over its time. In charge of the session was Alvah Peterson, a rigid taskmaster with respect to time-keeping in scientific meetings. He was known for responding to pleas for a little more time by calling for a vote from the audience on whether the speaker should be allowed to continue. No one wanted to run that gauntlet, and Peterson was not lenient just because I had been his master's student. Somehow I struggled through that first talk at a scientific meeting, but at the end there was considerably more laughter than clapping.

In 1955, I delivered two talks on my research at the Cincinnati meetings of the Entomological Society of America. One of the sessions, a symposium on systematics, was hosted by Professor Theodore H. Hubbell, then Director of the Museum of Zoology at the University of Michigan. Hubbell and I had a mild disagreement in the discussion following my talk, and I uncompromisingly expressed a strong view. He thought I was describing species without having located any morphological differences between them, although I hadn't actually done that – yet. He ducked his head, in what I would later learn was his politely humble manner, and asked, "What about the poor curator who cannot distinguish specimens of the different species?" I replied – I'm not sure how much defensively and how much arrogantly – "Well – if I were a curator I would rather label a specimen *Gryllus* sp., and know it was correct, than label it *Gryllus assimilis* and know it was wrong." At that time I "knew" I would never be a curator because I had declared more than once that I would not take such a job; nor did I know that Hubbell had labeled virtually all U.S. *Gryllus* specimens in the University of Michigan Museum of Zoology as *Gryllus assimilis*, and of course added on each label: "det. T. H. Hubbell."

Some time later, I learned that Hubbell and Delong had subsequently been on an ESA committee together, and had discussed me as a possible job candidate at the University of Michigan. As a result, even though the Museum of Zoology had hired a second entomologist in 1956, an unprecedented third position was established, and I was invited to give a job seminar. After my seminar, in a five-minute encounter in the darkness of the back porch of Hubbell's home, the chair of the Zoology Department, Dugald E. S. Brown, a molecular biologist, hired me half-time in each of the two units with a verbal offer of US$5200 for a 12-month position. There were no other candidates. This whole event is one of several I have described in this essay that probably can no longer occur "legitimately" in academia in America.

In August 1957, Lorrie and I moved our family to the University of Michigan, and I began as Instructor in Zoology and Curator of Insects. Almost 44 years later, following a five-year