GOING AFIELD

CARLETON J. PHILLIPS AND CLYDE JONES
Editors

Museum of Texas Tech University

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Lifetime Experiences in Exploration, Science, and the Biology of Mammals

Carleton J. Phillips and Clyde Jones

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About the cover: Dirk Gringhuis drew this delightful (but bizarre) cartoon of tables turned. Gringhuis served as museum artist at Michigan State University during the time that Rollin H. Baker was Director. The original art was presented to Baker on the occasion of a birthday, and is used here with his permission.

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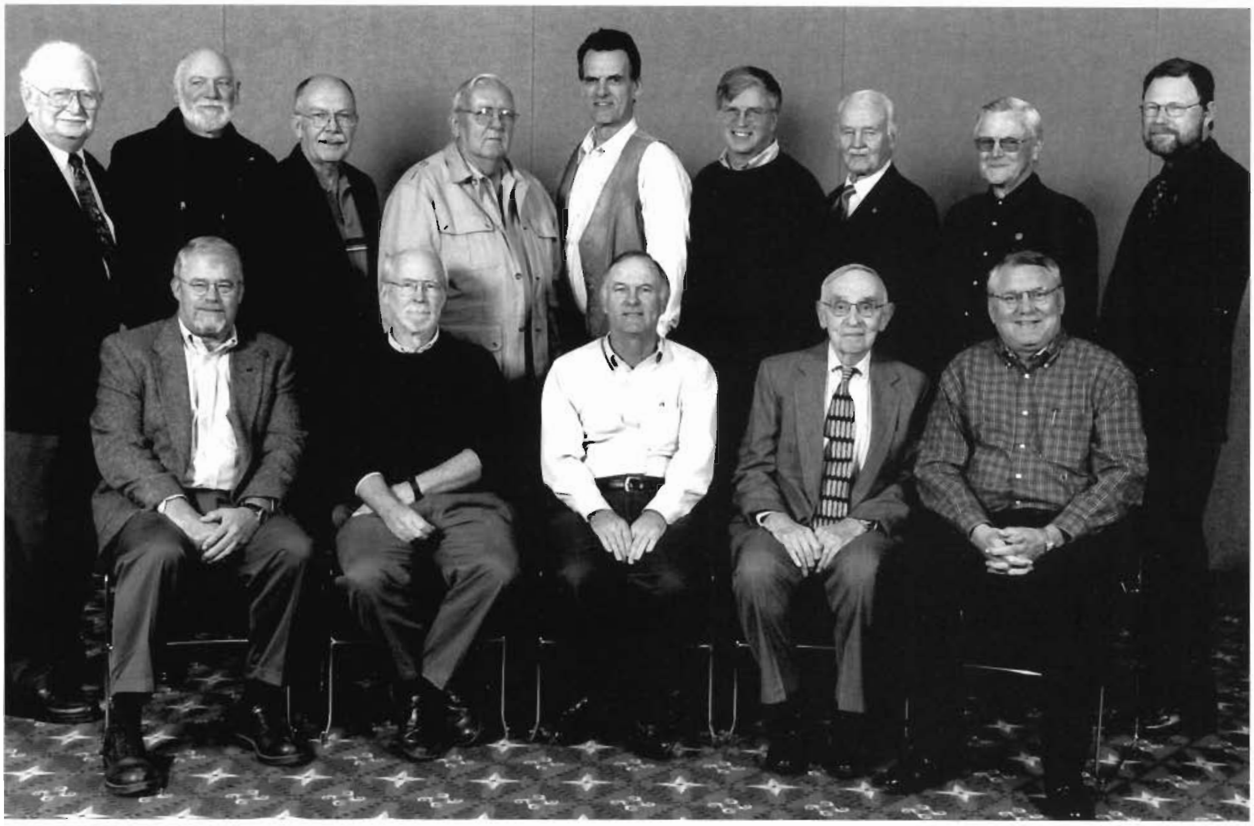

Frontispiece

Participants at the Texas Tech University Symposium November, 2001.

Front row (left to right): Carleton J. Phillips, James L. Patton, Don E. Wilson, Donald F. Hoffmeister, and David J. Schmidly.
Back row (left to right): William E. Evans, Michael A. Bogan, Jerry R. Choate, Clyde Jones, Alfred L. Gardner, Thomas H. Kunz, Rollin H. Baker, James S. Findley, and Robert J. Baker.

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Preface and Acknowledgments

On November 29, 2001 we hosted a "Mammalogy" symposium at Texas Tech University. The weather was inhospitable; traces of snow and ice dotted the West Texas landscape, the wind howled from the north at a steady 20 knots, and the nighttime temperatures dropped well below 0° C. Some of our invited guests encountered travel problems and airplane delays, some arrived shivering, and one was unable to come at all. We were particularly worried about the health and welfare of our "oldest" participants, although our very youngest were nearly sixty.

The Mammalogy Symposium at Texas Tech University was unique. Symposia come and go but ours was designed to capture the career experiences of a group of senior scientists who shared the common bond of having conducted scientific field studies on mammals. Our symposium focused on the personal experiences of growing up, becoming a scientist, and having a successful career.

How we came up with the idea is, frankly, unclear but we generally explain it in the following ways. From time-to-time the two of us compared notes on what we took to be "changes" in our scientific field. On other occasions we puzzled over the difficulty of explaining to students the "history" of major ideas and concepts. More than once we wondered aloud whether anything had been accomplished, scientifically, in our field during the course of our careers. This latter notion might seem surprising because there is a public consensus about what might be called "scientific progress," with the emphasis on *progress*. Although it is true that thousands of scientific articles and books had been published in our field just during our careers (presumably reflecting an accumulation of new knowledge), we nevertheless wondered whether any real progress had been made on the road to truth. It occurred to us during this type of conversation that there are two kinds of scientific progress: one kind is the accumulation of more and more information (sometimes literally new and sometimes more of the same) and the other kind is the development of new concepts and the rejection or replacement of older concepts. We wondered how one might go about surveying scientific progress in our field over a specific period of time.

Finally, and most importantly, we both agreed that it was essential for future students of mammalogy to have access to the "old-timers." Life is an uncertain business, and both of us have seen the unexpected passing of friends and colleagues who were influential in the science. Indeed, what mechanism is there to capture and store the experience of a professional lifetime before retirement or death eliminates it forever? Aside from the sense that the history of a scientific discipline is the sum of the professional experiences of individual scientists, we also believe that future pathways of the science will be reflections of the past.

After we decided to host a gathering of senior field mammalogists, we were confronted with a series of obvious questions. Who should we invite? What should we ask them to say? Should we publish their comments? We also encountered some criticism: at least one person demanded to know the value of self-laudation. Naturally, autobiographies *can be* self-laudation, but that depends upon the writer, or, more likely, the reader. In any case, this was not our concern. We simply wanted to create an opportunity for senior scientists to share their lifetime experiences in their own, personal, way. We assumed that their respective products would tell future readers a lot about them, like it or not.

We knew from the beginning of this project that our actual invitation list could never match our list of desirable speakers, potential speakers, good friends, and close professional colleagues. For one thing, the science of field mammalogy is global and there are a substantial number of senior field people residing in Europe, South America, Africa, Australia, and Asia! Closer to home, in Canada and México, there were many potential speakers within easy travel distance of Texas Tech University. Our respective "wish lists" of speakers grew to about 55, or nearly enough to require a weeklong symposium and a multi-volume set of published papers. In the end, however, we decided to restrict our invitees to a representative subset of American field mammalogists with at least thirty years of professional experience. This eliminated a substantial number of excellent scientists, colleagues, and friends, from our wish list and our plea is that they organize the next symposium a few years from now.

In addition to our thirty-year limit, we also admit to a bit of bias. As we prepared our final list, we tended toward selecting a subset of individuals who were interrelated in terms of their professional educational experience (both generational and institutional). With these criteria, we thought that the individual articles would form a coherent whole, with overlapping perspectives on the same events, people, and places. Among the eventual invitees, all but two agreed to attend and write articles for the project. One of the two had a foreign commitment that conflicted with the project, and the other declined the opportunity.

Our final group of speakers consisted of both academic and non-academic scientists and both terrestrial and marine mammalogists. Although we decided to not include their formal resumes in this volume, biographical information about many of them can be found in *Seventy-Five Years of Mammalogy (1919-1994)*, edited by Elmer C. Birney and Jerry R. Choate and published by the American Society of Mammalogists (Allen Press, Lawrence, Kansas), in 1994. Having said this, a few resume type data points are still in order. Six (Hoffmeister, R. H. Baker, Evans, Findley, Cockrum, and Patton) of the speakers are formally retired—although most are still active professionally, as illustrated by their participation. Among the others, one (Schmidly) is a University President, one (Phillips) is an Assistant Vice President for Research, two (R. J. Baker and Jones) hold distinguished professorships (called Paul K. Horn Professorships at Texas Tech), seven (Choate, Kunz, Baker, Jones, Phillips, Patton, Wilson) have served as chairs of academic departments, directors of academic programs, museum curators, or directors of Federal Units. In the entire group, most of the authors have spent their careers at academic institutions, but the Federal government has been the primary employer of four (Evans, Bogan, Wilson, Gardner). One of us (Jones) has worked and held administrative appointments in both environments. Our marine mammalogist, William Evans, was selected by the President of the United States to Chair the Marine Mammal Commission. He also served as Under Secretary of Commerce for Oceans and Atmosphere. Five of the participants (Hoffmeister, Findley, Patton, R. J. Baker, and Kunz) have served as elected President of the American Society of Mammalogists and five (Patton, R. J. Baker, Choate, Kunz, and Phillips) have received the American Society of Mammalogists' C. Hart Merriam Award for Research. Collectively, the participants are the authors or co-authors of more than 1500 research articles and books in their scientific field.

Our ground rules were simple, but intentionally vague and open-ended. We asked our speakers to share—insofar as they wished and in their own ways—their childhood memories, personal history, and educational experiences. Specifically, we wanted to know what influenced them (in their opinion) to become field mammalogists. We wanted to hear about their education and especially the people who affected them academically and personally. We asked them to tell us about their research and research interests. We wanted to know what changes they had observed during their careers—what concepts had been modified, what attitudes had changed, and what had we learned collectively? Is field mammalogy today the same as it was thirty or more years ago? Finally, we asked our participants to comment on advice for young mammalogists (scientifically or otherwise) and, if they wished, we said they could prognosticate future events in field mammalogy. To provide a different perspective, we asked a "younger" mammalogist, Robert D. Bradley, to attend the symposium, listen to the old fellows, and share his contemporary view. He chose to entitle his paper, *What the Old Dogs Said: Perspectives from a Pup*.

It is our shared sense that the symposium was a success. After listening to our colleagues' oral presentations and reading their written contributions we decided that projects such as this one have historical value. Other than the two of us (Phillips and Jones), none of the other mammalogists read the manuscripts submitted by other participants. Thus, each manuscript is a "sight-unseen" individualized contribution. In this way, each manuscript represents the personalized way in which each participant chose to respond to our charge. The writers chose their titles, their themes, and the length of their products. We did not send their manuscripts to peer-reviewers (some of our authors have no peers) and our "editing" was as light-handed as we could make it. Basically, we just looked for continuity and obvious errors (spellings of personal names or places) and attempted to create consistency with regard to institutional names or acronyms. We left grammar and style alone, figuring that these elements should reflect personality and intent. Occasionally, we added editorial comment or explanation enclosed in brackets. But, essentially we published these personal stories as they were presented to us. We believe that the diversity in responses, and styles, is noteworthy and by knowing our writers as colleagues, we also were able to "see" each of them in these autobiographical articles. Above all else, this is what we hoped for when we hatched the plan.

One writer (Wilson) expressed doubt that anyone would care a whit about his lifetime experience, but it is our hope and expectation that these highly personalized, professional autobiographies will be illuminating for students of mammalogy, historians of science, and people who enjoy the out-of-doors, mammal watching, and hunting. To a man, our invitees by any measure are hugely successful and influential people in their discipline (recall the 1500 articles and books) and in higher education (hundreds of undergraduate and graduate students trained and academic positions from Chair to University President).

Finally, we wish to acknowledge the support that made this project a success. Financial support for publication, and a Pre-Symposium Reception, came from the President's Office at Texas Tech University. Other financial assistance came from Clyde Jones' Horn Professor Account, staff assistance came from the Department of Biological Sciences, and, finally, some assistance came from the Natural Science Research Laboratory at the Texas Tech University Museum. Several people made the symposium a huge and enjoyable success. In particular we are grateful to Libby Moreland. Libby at the time was Phillips' Executive Assistant and Business Manager in Biological Sciences and she was the one who handled all of the complex organizational details of the Symposium. Several of our Mammalogy graduate students—Deidre Parish, Joel Brant, Rob DeBaca, Rex McAliley, and Jana Higginbotham in particular—and several of our Texas Tech colleagues—Robert J. Baker, Robert D. Bradley, Jon Whitmore, and David Schmidly—also were very helpful and supportive and we are grateful to them. When it came time to put together a book based on the symposium, Jackie Chavez at the Texas Tech NSRL took on the task with adroit good humor.

Carleton J. Phillips and Clyde Jones
Lubbock, Texas

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GOING AFIELD: THE MAKING OF NORTH AMERICAN MAMMALOGISTS AND THEIR SCIENCE

Carleton J. Phillips, Clyde Jones, and Rollin H. Baker

This book is about a breed (and we use the term 'breed' metaphorically with pride, affection, but due caution) of biological scientists that we call 'Field Mammalogists.' Because we are members of that breed, it is impossible for us to write dispassionately about it, but nevertheless in this first chapter we will try to explain it and a bit of its North American history, as though we are mere observers rather than practitioners.

A look at the Contents of *Going Afield* is instructive. It clearly signals the challenge before us. True, some of the titles, all of which were chosen by the authors rather than the editors, are benign. For instance, Donald Hoffmeister, Professor Emeritus from the University of Illinois, simply and accurately described his article as, "*A Personal Sixty-Year Retrospective*." But other titles are eye-catching, maybe startling. Jerry R. Choate, for example, entitled his autobiographical piece, "*When People Ask What I Do, I Say I Study Bats and Rats*." If this is so, people must be pretty surprised because officially Choate is a Professor and Director of the impressive Sternberg Museum of Natural History at Fort Hays State University.

The University of California at Berkeley is home base to radical-*chic* in America. That explains a lot about this important University, but does it explain why James L. Patton, a Professor emeritus at Berkeley, entitled his reminiscence, "*Species and Speciation: Changes in a Paradigm through the Career of a Rat Trapper?*" Don E. Wilson, a Federal scientist employed by the august and staid Smithsonian Institution, decided to call his contribution, "*Bats to Biodiversity: Spyder Had a Pretty Good Ride*." Finally, David J. Schmidly, the President of a major university, cried out, "*Where Have You Gone Vernon Bailey? The Laments of a Mammalogist*." Studying bats? Trapping rats? Lamenting? What is this mammalogy business all about? As one of Carleton Phillips' friends—a responsible and successful laboratory biologist—recently put it, "you field mammalogists are just a bunch of mouse trappers and it's about time you explained what in the hell you've been up to. You owe it to the rest of us." Well put.

Although we could argue that anyone who reads the autobiographies presented in *Going Afield* will gain their own insight into what field mammalogists are like, and what the science is all about, we nevertheless will try to explain it. Our explanation begins with two photographs (Figures 1 and 2). One shows the dramatic cover of the Smithsonian Institution's massive book on North American mammals. Don E. Wilson, the symposium participant who wrote *Bats to Biodiversity: Spyder Had a Pretty Good Ride*, is coauthor (with Sue Ruff) of this impressively produced collection of information about mammals. The second photograph shows another symposium participant, Robert J. Baker (*If You Don't Clean Up Your Act, You'll End Up at Texas Tech)*. In the photograph, Robert Baker is perched on a pickup truck tailgate with his son, Bobby. The two Bakers have just returned from

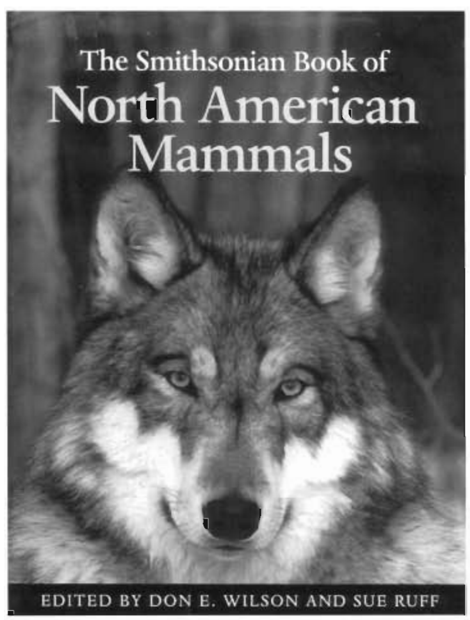

Figure 1. Dust jacket of Smithsonian Mammal book.

Figure 2. Robert J. and Bobby Baker with Bobby's West Texas buck (2002).

an autumn hunt and are displaying the antler rack on a deer killed on their West Texas ranch.

Don Wilson's book on North American mammals adorns tables in homes across our country, can be found in a thousand libraries, or can be taken from a shelf and scanned over a cup of Starbuck's aromatic coffee at major bookstores in cities large and not so large. This 750-page book has more than a hundred illustrations, many in color, for a large selection of the mammal species found across North America, with a few of the accounts authored by symposium participants. We suspect it is easy to take for granted that such books exist and can be purchased or borrowed. In reality, however, books such as this one—characterized by an enormous amount of reliable information—did not always exist.

Consider for a moment that the information in North American Mammals is reliable because much of it was culled from thousands of individual scientific studies and peer-reviewed articles. Consider also that nearly a thousand highly trained scientists have individually focused their attention—their intellectual capital and physical energy—upon the study of North American mammals, dating back to the late 19th Century. Consider that institutions of higher learning and state and federal agencies have made policy decisions to invest time, facilities, and money in the study of mammalian biology. Consider that a federally employed scientist, Don Wilson, was available to participate in the writing and editing of this volume.

Knowledge is an asset, and knowledge about our fellow mammals is relevant to many aspects of our daily lives. Most of what we know about human health and biology relates in some way to research conducted—or, better put, research that is conductible—upon our fellow mammals. Our lives are immeasurably enriched by the companionship of dogs, cats, horses, and other domesticated breeds and the realization that wild versions live in the world around us. Acquisition of knowledge about those wild species is where field mammalogists come into the story, and is where Don Wilson and others make their contributions.

Our post-deer-hunting photograph could tell several stories. One story could be about fathers and their sons, another about sharing generational traditions, another about competition (Boone and Crocket scores), and another about the taste of smoked venison. Naturally, there are complex reasons why people hunt other mammals, and this subject by itself goes far beyond the scope of the present book. We selected this photograph because it illustrates several basic characteristics of field mammalogists. One of these is that field mammalogists are scientists who look (and are) comfortable afield. Another aspect of this is the direct physical connection between field mammalogists and their objects of study. Baker's left hand at the antler base is aptly metaphorical. As a branch of biological science, field mammalogy must include this connectivity, and scientists unwilling to go afield and learn first-hand about mammals and their habits or unwilling to personally collect specimens as needed for their research need not apply. There is no armchair version of this branch of science.

Going afield can be pleasant or very unpleasant, depending upon a variety of circumstances. It clearly is not for everyone. At the very least, nearly every field trip has the potential of blistering heat, frigid cold, seasickness, rain, sleet, or snow, not enough food, not enough time, poor maps, inaccessible target species, health risks, mortal danger from unstable and unpredictable regional or local politics or deranged individuals. And this list is just for starters. We like the way a field mammalogist of a younger generation than ours, Michael Mares of the University of Oklahoma, explained it in reference to the challenges of doing a career's worth of fieldwork in Latin America (Mares, 1991).

> "[field mammalogists]... form a small coterie of frequently stubborn, single-minded individuals who have often overcome personal and professional adversity to pursue a passion for research on mammals...We comprise a group of individuals accustomed to doing things the hard way, for the simple path was rejected when foreign research was begun. Such biologists are generally inured to criticism and professional barbs from colleagues at home and abroad, and are accustomed to ignorance from...bureaucrats. They are also accustomed to the hardships involved with field biology, including disrupted home lives, unsympathetic administrators, and frequent health problems. (A quick perusal of a half-dozen of my colleagues, for example, shows that field research has resulted in blackwater fever, leshmaniasis, typhus, histoplasmosis, malaria, dengue fever, plague, ascariasis, snakebite, and other serious maladies.) Field [mammalogists]...do not suffer fools easily."

This image of field mammalogists, as painted by Mares, offers the outside world an explanation for why such scientists are the way they are and why they conduct business in a sometimes brusque and unyielding style. In effect it says, "I'm sorry for stepping on your toes and kicking you in the shins, but, you see, I've been afield." It quickly explains why gentlemanly Thomas Kunz (*Becoming a Mammalogist: On the Wings of Heroes*), a professor at Boston University, is known as 'Crude Tom' amongst his many colleagues and friends.

In academic settings, the so-called ivory towers of higher education, field mammalogists—and a couple other brands of field biologists also—have a reputation of being disruptive, tough to corral. Field mammalogists sometimes forget to wear their ties. Field mammalogists have been known to accidentally or intentionally horrify sedate English professors or historians or deans with blunt descriptions of field experiences, illnesses, personal encounters with guerrilla warfare, parasitic infections, and exaggerated sexual exploits (all done in poor taste, naturally). Field mammalogists probably comprise the only scientific breed willing to publicly dismember and devour their own.

When we wrote the previous paragraphs, we assumed that current readers from outside of the profession would surely scoff. Future readers—ones who delve into this book long after the authors have departed for the happy mouse and bat hunting grounds and thus are unable to answer questions directly—will be skeptical. Our solution to this problem is to call attention to the set of Encomia printed in a volume honoring the life and successful career of a field mammalogist named J. Knox Jones, Jr. (Genoways and Baker, 1996). The substantial number of scientists who contributed invited research articles to this 315-page book were also invited to write brief personalized encomia about their relationship to the deceased honoree. This they did, sometimes with neutral—sanitized—professionalism (Schmidly, 1996), sometimes with clever inside joke humor (Birney, 1996), sometimes with frankly realistic recollection (Chesser, 1996), and sometimes with unmasked hostility (Carter, 1996).

Anyone who reads these encomia in full will agree: there is something distinctive and perhaps a little different and frightening about the scientific practitioners of field mammalogy. All of this, of course, cries out for an explanation. Who are these people? What have they contributed to society or scientific knowledge? How did they get to be this way? These are some of the questions that we attempt to address in this opening chapter.

Having deliberately called attention to the 'colorful' characteristics of at least some field mammalogists, and our habitual candor about ourselves, there is an important point that we want to make before proceeding. Put simply, we assert that an exceptional work ethic, high standards of productivity and responsibility, leadership skills, and the raw courage to conduct fieldwork and explore new scientific ideas are the other characteristics of field mammalogists. These characteristics, which will emerge as we trace the history of the discipline, together with individual and collective documented professional successes, are the counterbalances to the colorful side. Being colorful—rough and ready, over-bearing, sometimes socially unacceptable, and intensely competitive—is a large part of the persona of field mammalogy, but that by itself hardly accounts for the scientific impact, broad influence, or academic credibility enjoyed by North American mammalogists. The autobiographical sketches in *Going Afield* very clearly provide the raw material for readers to draw their own conclusions about the interplay of personal characteristics that account for the overall successes of North American mammalogy.

Professional mammalogists are people who have transformed their enjoyment of their fellow mammals into a way of making a living. One of their principal markets is obvious. Most people in North America and Europe are interested in wild mammals. Captive live specimens attract huge numbers of visitors to national and regional zoological parks. Mounted specimens attract attention as the centerpieces of museum exhibits—take for instance the giant bull elephants that occupy the rotundas at several of our major institutions. Much (but not all) of the most credible information about the natural history, biology, and evolution of mammals has been obtained and disseminated by field mammalogists. The Smithsonian Institution book by Wilson and Ruff is one great example. Other examples involving contributors to our present book include Donald Hoffmeister's *Mammals of Illinois* (Hoffmeister, 1989), Jerry R. Choate's *Guide to Mammals of the Plains States* (Jones et al., 1985), Rollin Baker's *Michigan Mammals* (Baker, 1983), Dave Schmidly's *The Mammals of Texas* (Davis and Schmidly, 1994), and Jim Findley's *The Natural History of New Mexican Mammals* (Findley, 1987) and (with Clyde Jones) *Mammals of New Mexico* (Findley et al., 1975). Clyde Jones also is a coauthor of *Mammals of the Northern Great Plains* (Jones et al., 1983).

Michael Mares, the mammalogist quoted earlier in regard to the nature and attitudes of field biologists, believes that understandable books about mammals are one key to promoting their protection and conservation. His book *Guide to the Mammals of Salta Province, Argentina Guía de los Mamiferos de la Provincia de Salta, Argentina* (Mares et al., 1989) is bilingual from the duplicated title onwards. In addition to his contribution to the knowledge base of Argentine mammals, he also is a coauthor of a popular book about *Mammals of Oklahoma* (Caire et al., 1989). So, field mammalogy and the practitioners of field mammalogy can be described and explained in terms of the willingness to do fieldwork, the connectivity to other (nonhuman) mammal species, and the collection and distribution of credible information about the biology and biogeographic history of mammals. The credibility part comes from the fact that field mammalogy is a scientific approach, i.e., the modern practitioners have been formally trained in biological science and in the conduct of scientific research.

The relationship between science and the establishment and dissemination of credible information is crucial and complex. The complexity part is obvious: how can the scientific method of experimentation be applied to field observations, data points, and, essentially, mammalian natural history? In one sense this problem is exactly the challenge of field mammalogy. Most of the two generations of field mammalogists who contributed to the present book are representative of biological scientists whose entire careers have been devoted to converting the traditions of natural history into a science. An alternative and perhaps more accurate way to think about these contributors is to say that they are people who from boyhood were interested in mammals and secondarily sought ways to study them scientifically (converting hobby to career). However, two of the contributors, William Evans (*45 Years of Marine Mammal Science: One Scientist's Migration*) and James Patton (*Species and Speciation: Changes in a Paradigm through the Career of a Rat Trapper*), represent other pathways to field mammalogy. Evans was a scientific expert on bioacoustics who secondarily became interested in whales and porpoises while employed in the so-called military-industrial complex. If we were permitted to label him, we would say that Patton first was a cytologist and erstwhile anthropologist and that he later became interested in wild mammals and, ultimately, field-based mammalogy. Although Patton and Evans are exceptions to the rule defined by our book, the careers of these men illustrate alternative pathways in the process of converting traditional natural history into a scientific study of mammals, terrestrial or marine.

The conversion of 19th Century natural history into field mammalogy and field mammalogy into a science was, and remains, a crucial activity. Without this conversion, the enterprise would lack its credibility and, ultimately, its fundamental value to society. Natural history without the science part is more fun, full of delightful folklore, less conservative, and more easily enjoyed, but not particularly reliable in terms of really enabling us to understand mammalian evolution, distribution, ecology, behavior, and biodiversity or even larger issues such as the history of life on our planet. Without the science part, there is no accumulation of credible baseline data upon which to build meaningful, testable, hypotheses.

The foregoing brings us to a question: how did North American field mammalogy originate and prosper as a fledgling science? One obvious answer is that the establishment of a science based upon field mammalogy occurred (or is still occurring) through education. The development and offering of scientifically rooted college courses in "Mammalogy" was a key element in this process. One of our contributors, E. Lendell Cockrum (*E. Lendell Cockrum His Twenty-twenty Hindsight*), produced the first college textbook on Mammalogy (Cockrum, 1962). It is noteworthy, however, that this first book was not published until 1962 and before that time courses in mammalogy were taught from literature—selected published articles—and a professor's lecture notes. Students did not have the benefit of being able to use a text for background or additional reading. The absence of a basic textbook for any introductory course probably is an impediment to attracting students to the subject. At the basic level, textbooks symbolize the worth of a subject. Thus, the absence of a textbook seems to imply that a particular body of knowledge is somehow unworthy of being collected, organized, and codified.

Our most senior contributor, Donald Hoffmeister (*A Personal Sixty-Year Retrospective*), wrote about taking university positions in the 1940's, before Mammalogy courses existed in the zoology curricula at the University of Kansas or University of Illinois. In Donald Hoffmeister's career, he witnessed the development of original mammalogy courses and their inclusion in zoology curricula (a process that validates both their acceptance and their importance to baccalaureate degree plans). But by the time that Hoffmeister retired from the University of Illinois, he also witnessed a gradual extinction of courses designed to teach students about specific groups of organisms (the 'ology' courses) and their replacement by courses in cellular- or molecular-level biology. The predominance of the latter and absence or rarity of the former combines to insure that molecular biology seems like reductionism. If one knows little or nothing about the geographical distribution, physical characteristics, species diversity, systematics, and evolutionary history of mammals—or any other group of organisms—then there is no context or broader meaning for microscopic or molecular data. A principal role of courses in mammalogy (and the other 'ologies' too), is to create scientific context for students of biology.

It is noteworthy that all of the university-based authors in our book have developed, offered, or team-taught various types of mammalogy courses in their respective academic careers. Although such courses at various times and institutions might have been called 'Mammalogy,' or 'Marine Mammals,' or 'Mammalian Conservation,' or 'Biology of Mammals,' all versions shared a focus on wild species rather than laboratory or domesticated mammals.

The personalized training of graduate and undergraduate research students is another aspect of the role of education in the creation of a distinctive scientific sub-discipline. The significance of this process will be obvious after reading the articles that comprise our book. Virtually all of the authors described themselves as beneficiaries of professorial interest and guidance and nearly all took the opportunity to detail their educational experiences with their mentors. Secondarily, it also is obvious that interactions with fellow students are a substantial component of education at the graduate level. Once again, nearly all of our authors devoted pages to their student companions and nearly all of them credit some of their personal growth and success to one or more of their fellow students.

ACADEMIC ORIGINS OF NORTH AMERICAN MAMMALOGY

The academic side of field mammalogy and its scientific paradigm in North America can be traced directly to a man named Joseph Grinnell and the University of California at Berkeley. Grinnell literally was the first Professor of Mammalogy at an American University. It should be noted, however, that although this professorship was real in the sense that Grinnell became the academic father of the field, it also was *de facto* in the sense that Grinnell was not actually appointed to a faculty position with such a title. Much of what happens in American universities, and in American scientific scholarship, is the result of people taking it upon themselves to pursue what they find valuable or intellectually fascinating. Thus, much of the science practiced in the United States today is not the direct consequence of strategic planning. Instead, it is the direct consequence of influential individualism in earlier generations, Professor Grinnell being a case-in-point.

Readers of the autobiographies in *Going Afield* will find that there is something familiar about Joseph Grinnell. For instance, Grinnell was neither a city boy nor a child of an economically or educationally privileged class. Instead, he was born at Fort Sill, in the 'Indian Territory' now called Oklahoma. The year was 1877, and things were still pretty rough in that neck of the woods. Childbirth and early years did not unfold with any degree of certainty for any youngsters in those days at that place. On the other hand, tough as it was, the landscape and inhabitants of Oklahoma were sources of wonder and delight, virtual magnets for personal exploration and discovery. Quite naturally, young Grinnell grew up hunting and exploring near Fort Sill and then expanded his range along the western frontier.

Hunting, *per se*, is not an intellectual activity (although both bright and not-so-bright people indulge in it) but in doing it young Grinnell must have seen something that lay beyond the mere acquisition of game. Essentially the same thing could be said for nearly every author whose autobiography is printed in *Going Afield*. Somehow, Grinnell, the original academic mammalogist in the United States, was captivated by what he saw, and what he tracked down. Although he retained an interest in large animals, it was the meek and lowly non-game species—mammals that people lump together as mice and rats—that eventually grabbed much of Grinnell's intellectual attention. By 1895 Joseph Grinnell was a professional 'collector,' a person focused on obtaining specimens of mammals, mostly of small size and unlikely to qualify as game. This interest and activity on Grinnell's part obviously brings to mind Jerry Choate's autobiography entitled, *When People Ask What I Do, I Say I Study Bats and Rats*. Like Grinnell, Choate was born in Oklahoma (only 66 years behind Grinnell), and although Choate grew up to enjoy hunting, like Grinnell he actually preferred to collect and study the lives of small, non-game mammals.

Joseph Grinnell followed his instincts and intellectual curiosity about animal life, and his collecting activities took him all the way to Alaska. It was while he was working in Alaska that he became more widely known as a professional collector and hands-on expert of both mammals and birds (Dunlap, 1988). Eventually, the Museum of Vertebrate Zoology at the University of California at Berkeley offered him employment and it was here that Grinnell transformed from raw collector and outdoorsman to professor. And thus it was that Grinnell became the embodiment of North American field mammalogy as an academic enterprise.

At Berkeley, Grinnell had the opportunity to share his interest in taxonomic and geographic mammalogy with students. By happenstance or some special intervention, Grinnell's position at Berkeley, and the development of the Museum of Vertebrate Zoology, coincided with the presence of some unusually talented and enthusiastic students. Later, as Grinnell's reputation (and that of the University) spread, additional talented students were recruited into the fledgling program. An example of this is provided by one of the authors featured in *Going Afield*. Donald Hoffmeister describes how he gave up a career opportunity in medicine to study mammalogy in Grinnell's vaunted program at the Museum of Vertebrate Zoology.

As a result of Grinnell's professorial impact, and the presence of talented hard-working young people, individuals identified as Grinnell's students (or, more accurately, students who studied in the museum environment that he created) had hugely successful academic careers after leaving Berkeley. In fact, because most of Grinnell's students were highly influential at their respective universities, their ideas and opinions affected the development of biology and zoology curricula, institutional organizations, graduate programs, and research themes. Beyond their institutions, many of Grinnell's students also affected state and federal policies in regard to biological science and application(s) of scientific knowledge to political and social issues (Dunlap, 1988).

Many of Joseph Grinnell's students became heavily involved in the development of 20th Century professional scientific societies and their journals and other publications. The most obvious example is the American Society of Mammalogists and their scientific outlet, the *Journal of Mammalogy*, which began in 1919 (Hoffmeister and Sterling, 1995). Ironically, it also is true that at least some of Grinnell's students (E. Raymond Hall for one) helped split the mammalogy business into two distinctive paradigms and scientific societies—one devoted to general mammalogy (the American Society of Mammalogists) and one devoted to the study of mammals in the context of wildlife biology and management (as exemplified by the *Journal of Wildlife Management*; Dunlap, 1988; Phillips, 1995). The irony, of course, is that field mammalogy in both the 19th and 20th centuries emerged from youngsters who grew up hunting game animals, and fishing and camping, too. At the same time, it is noteworthy that although the original and current field mammalogists often started out as hunters, their focus at some point shifted to small, non-game mammals. Joseph Grinnell is a good example of this change in emphasis and his particular focus on small mammal taxonomy and zoogeography obviously affected his students.

The split of early American mammalogy into two versions—the museum-based one that we call Grinnellian field mammalogy and, ultimately, the wildlife department version that emphasizes management of game mammals—was not trivial. It affected Federal and State policies and regulations on land use, conservation, reintroductions, and predator control. The original philosophical split played out in acrimony and bitter dispute, often conducted in public at Capital Hill hearings (Hall, 1930, 1934). In essence, Grinnell's students (especially E. Raymond Hall and Jean M. Linsdale) fought against the predator control policies and indiscriminate poisoning advocated and conducted in the western United States by the U.S. Biological Survey on behalf of the Federal Government (Dunlap, 1988). The Biological Survey mammalogists favored control, and generally regarded mammalian predators such as the wolf, coyote, and mountain lion as threats to economic well being of the ranchers. The Grinnellian mammalogists displayed an emotional bond to their fellow mammals, large and small. In the end, Grinnell had instilled in his students a sense of respect for all species. Mammals were simply mammals, not ranked according to game, vermin, and no-count obscure beasts. The autobiographies in *Going Afield* might also hint that field mammalogists in general have an innate, unexplainable affinity for their fellow mammals. Olaus J. Murie of the Wilderness Society noticed this non-scientific, emotional connection to mammals, and in regard to E. Raymond Hall's dogged defense of coyotes, Murie (as quoted by Dunlap, 1988) wrote the following in 1952.

> "[Mammalogists] who became so concerned at the time did not, I believe, understand their own motivation...the big issue put forth was that 'innocent animals' were being killed incidental to poisoning operations. Deep

> in their hearts, if they had thought it out fully in those formative years of the opposition [to predator control], was concern for the coyote itself..."

There were several long-term consequences of the bitter split between Grinnell's students and the Federal scientists in the U. S. Biological Survey. One consequence was that the museum-based field mammalogists have tended to opt out of modern debates about conservation policies and focus instead on other, purely scientific, aspects of mammal biology. In reading all of the autobiographical accounts in *Going Afield*, it is difficult to find good examples of research that might be construed as primarily intended to test hypotheses relevant to management of wild mammals or their habitats.

In any case, the development of two types of academic endeavors—field mammalogy and wildlife mammalogy—was a consequence of the early battles over predator control and land use policies (particularly the conversion of natural landscape into pasture). At many universities, the field mammalogists have been associated with the academic museums or with Zoology or Biological Sciences departments, or some combination of these. There are exceptions. At Texas A&M University a Grinnellian field mammalogist, David Schmidly, was named Head of the Department of Wildlife Sciences. According to him (personal communication), he spent the next several years converting the Department into something more like a Department of Biological Sciences. At the University of Kansas, E. Raymond Hall was Director of the Museum of Natural History and Head of the Department of Zoology. Not only that, but he also prodded the Governor into creating a Kansas State Biological Survey with him as its Director! Under Hall, the Kansas State Biological Survey operated as an independent counter-balance to the State Fish and Game Commission.

Another example of the interaction between the Grinnellian mammalogists and the wildlife management mammalogists—and the fallout—can be shared through the eyes of one of us (Clyde Jones).

> "In 1970, I was employed by the U. S. Fish and Wildlife Service, but I was housed at the National Museum as Chief of the Mammal Section (the old bird and mammal laboratories). Due to passage of the Endangered Species Act and the Marine Mammal Act, my unit was given responsibilities to determine the status of those species proposed for listing. As a result, my unit received an influx of funds to hire people to carry out these tasks. Don Wilson, Mike Bogan, Al Gardner, and Bob Fisher (who now is the Collection Manager at the National Museum) were four of my hires at this time [see autobiographical articles for additional information: eds.] In pursuit of our duties, we earned the reputation of being world travelers.
>
> In 1979, our unit was combined with the old Denver Wildlife Research Lab of the Fish and Wildlife Service. I moved to Denver and the Fish and Wildlife Service named me as Director of the Lab. The orientation of the Denver Lab when I arrived was mostly toward wildlife control. They controlled predators, birds, and mostly whatever they wanted including migrating birds that built up large roosts. They even were involved in trying to control vampire bats in México—and they were very successful at it.
>
> Although we had two very different kinds of mammalogists working in the Denver Lab, we got along well as people even though there obviously were mission conflicts—control and management on the one hand and concern over the status of threatened and endangered mammals on the other. It probably helped that our unit was well enough funded to carry out both kinds of projects. Things changed when President Reagan took office. Reagan appointed a man named James Watt as Secretary of Interior. Watt and some of his underlings and I did not see eye-to-eye on issues such as the use of compound 1080 for killing coyotes. As a result, I left the Fish and Wildlife Service and moved to Texas Tech University.
>
> Upon reflection on my tour of duty as Director of the Denver Wildlife Research Center (1979-1982), it seems that it was admin-

istratively impossible to have the two units combined. There was a philosophical difference between the Grinnellian-type mammalogists, like me, and the people who controlled wildlife."

At many academic institutions, the field mammalogists virtually never interact with the wildlife mammalogists and the latter virtually never participate in museum-based scientific work. One of us (Rollin Baker) attempted to change this pattern by associating with Fisheries and Wildlife academic biologists and with State Game Biologists employed to study mammals. Baker recalls his experience at Michigan State University in the following way.

"At Michigan State University I purposely sought a joint appointment in the College of Agriculture's Department of Fisheries & Wildlife before I accepted the job in 1955 as Museum Director and a professorship in the department of Zoology, which was in the College of Natural Science. Why the triple appointment? Partly because mammalogy was taught in Zoology, but most all of the potential students in mammalogy were eager-beaver, outdoor-type undergrads. They had been initially attracted to the glamorous subject matter offered by Fisheries & Wildlife, and I wanted a piece of that action. And it really paid off since every fall term my class in mammalogy always had 50 or more undergrads out of wildlife in them – they came to me already half-way dedicated to field-related biology. Also, some selected ones like Gary Dawson, Lynn Rogers, Tom Struhsaker, Bill Gasoway, Phil Robinson, Mike Petersen, Pete Dalby, and Don Christian came over to the Museum to either work directly with me, attend my every-term grad seminar in mammalogy or join me on my annual 4-6-week summer Mexican field trips. Also while I was at Michigan State, I made a special effort to gain close association with the staff of the Wildlife Division of the Michigan Department of Natural Resources (DNR).

In fact, I had known some of the senior DNR staff members like Harry Ruhl, Fritz Stuewer, and Durward Allen from before World War II. So, when I was at Michigan State University we had some serious and jovial get-togethers. I invited them to attend and speak at my seminars and to join grad student bull sessions including my term-ending "beer busts" held in my ever-faithful, housekeeping, but near patience-exhausted wife Mary's basement recreation room. It was in those informal sessions that the DNR professionals had a chance to join me in acquainting students with what field science was all about out in the "cold and cruel" world and how to endure the trials and tribulations involved. These experienced guys stressed the necessity of getting along compatibly with the public – landowners, local sportsmen, overseas big game hunters, nature clubbers, travelers, politicians, service club members, etc. The emphasis was partly on how to pay close attention to these folks and gain insights on their attitudes about outdoor science and, importantly, how it relates to everyday life. I joined the DNRers in pointing out the value of chinning with rural folks [we used to call them 'fork-in-the-creekers' in pre-WWII East Texas]. Although these nesters may be unlettered and may often misinterpret wildlife antics, they surely know their animal life in ways that are not expressed in textbooks. So paying attention to what these people say, instead of looking down your noses at their seemingly sparse textbook knowledge of your science, brings valued returns. In short, ask for help and suggestions from all kinds of field-savvy sources! My close association with Game Division biologists also brought a bounty of specimens to the MSU Museum – also the annual take of possibly rabid bats from friends in Michigan's public health facility. I also served as the Chairman of the DNR's Endangered Mammal Committee. Although several of my students conducted their studies at such DNR research stations as Rose Lake, I didn't ask the DNR for much grant money. This was because almost all of my doctoral students took off, mostly after taking the Organization for Tropical Studies [OTS, a consortium of universities] summer field course in Costa Rica and/or being a member of one of my México expeditions, to out-of-country areas – México, Panamá, Namibia, Argentina, Chile, Brazil, Venezuela. And I had to dig up money for them from other sources. Incidentally, by the time that I left Michigan State University in 1982, well over half of the DNR wildlife biologists were Michigan State grads and had taken my mammal course."

The split and ongoing competition between Grinnellian field mammalogists and wildlife mammalogists is only one aspect of the Grinnellian impact on academics. Another impact might be termed "fecundity." Joseph Grinnell and his original students were so successful at recruiting and training successful students that nearly all of the present-day North American field mammalogists are academic descendents of Joseph Grinnell's (Jones, 1991). Indeed, with exception of William Evans and James Patton, all of the other authors in *Going Afield* have direct academic ties to Joseph Grinnell. These ties are illustrated as an academic genealogy—a pathway of training, ideas, and scientific concepts—in Figure 3. In the case of James Patton, his training at the University of Arizona so closely associated him with E. Lendell Cockrum, and Cockrum's students, that J. Knox Jones, Jr. (the self-appointed genealogical record-keeper) labeled him as an "honorary [academic] great-grandson of Grinnell (Jones, 1991)."

The real exception to the Grinnellian academic heritage, among the authors in *Going Afield*, is Bill Evans. Evans' independent academic lineage calls attention to the fact that marine mammalogy—the scientific study of a loosely organized collection of species including whales, porpoises, seals, walruses, sea otters, manatees, dugongs, and even polar bears—mostly has its own academic history and traditions. At the same time, marine mammalogy is not totally separate from the Grinnellian academic heritage for field mammalogy and Grinnell's academic descendents were not exclusively interested in terrestrial mammals. One of our authors, David Schmidly (*Where have you gone Vernon Bailey? The Laments of a Mammalogist*) developed an interest in marine mammals, and has made his own contributions to the field (Würsig et al., 2000). Moreover, one of Grinnell's own graduate students was a man named Remington Kellogg (an eventual Secretary of the Smithsonian Institution) who studied fossil whales, among other topics.

As the leader and academic starting point for a distinctive scientific enterprise, Joseph Grinnell's personality, his talents, proclivities, idiosyncrasies, and habits—both good and not so good—influenced the founding characteristics of mammalogy as a branch of science. Grinnell's dogmatism, vocabulary, daily work habits, and decision-making dominated the educational environment in the museum at Berkeley (Dunlap, 1988; Jones, 1991; Phillips, 1995). Grinnell's work ethic—workaholic in modern terms—was notable because it characterizes many of the modern field mammalogists. To quote J. Knox Jones, Jr. (1991), "[Grinnell was] said to have worked six and one half days per week, although my readings have failed to reveal what he did with the other half day." The same comment could easily be made about Jones, although in his case the published encomia about him (Genoways and Baker, 1996) also offer some insight into what he might have been doing with the half day. According to Robert J. Baker, J. Knox Jones, Jr. was editing his final scientific manuscript at the moment of his death, in 1992. Although desperately ill, he was home and in his own bed. Obviously, Jones was determined to use his last half-day to see to it that he completed yet another manuscript about mammalogy. But at that time he already was author or editor of 15 books and 376 articles!

A photograph of Joseph Grinnell seated at his hefty roll top desk (Figure 4), in front of a library of leather-bound volumes, gives the impression of a traditional, serious-minded "scholar." He projects no hint in such photographs of having spent serious time afield. Even so, Grinnell grew up out-of-doors on the frontier and had explored and collected vertebrates in the Alaskan wilderness. There also is no hint that Joseph Grinnell was likely to forget his tie or that he was capable of kicking shins. In other words, the photographic image of Joseph Grinnell as the academic father of field mammalogy seems inconsistent with the modern nature of the breed and clearly at odds with the photographs of mammalogists scattered throughout our book. At the same time, most photographs of Grinnell's own students are similar to this one of Grinnell. For example, E. Raymond Hall, who was one of Grinnell's illustrious students, always wore a blue serge suit, used

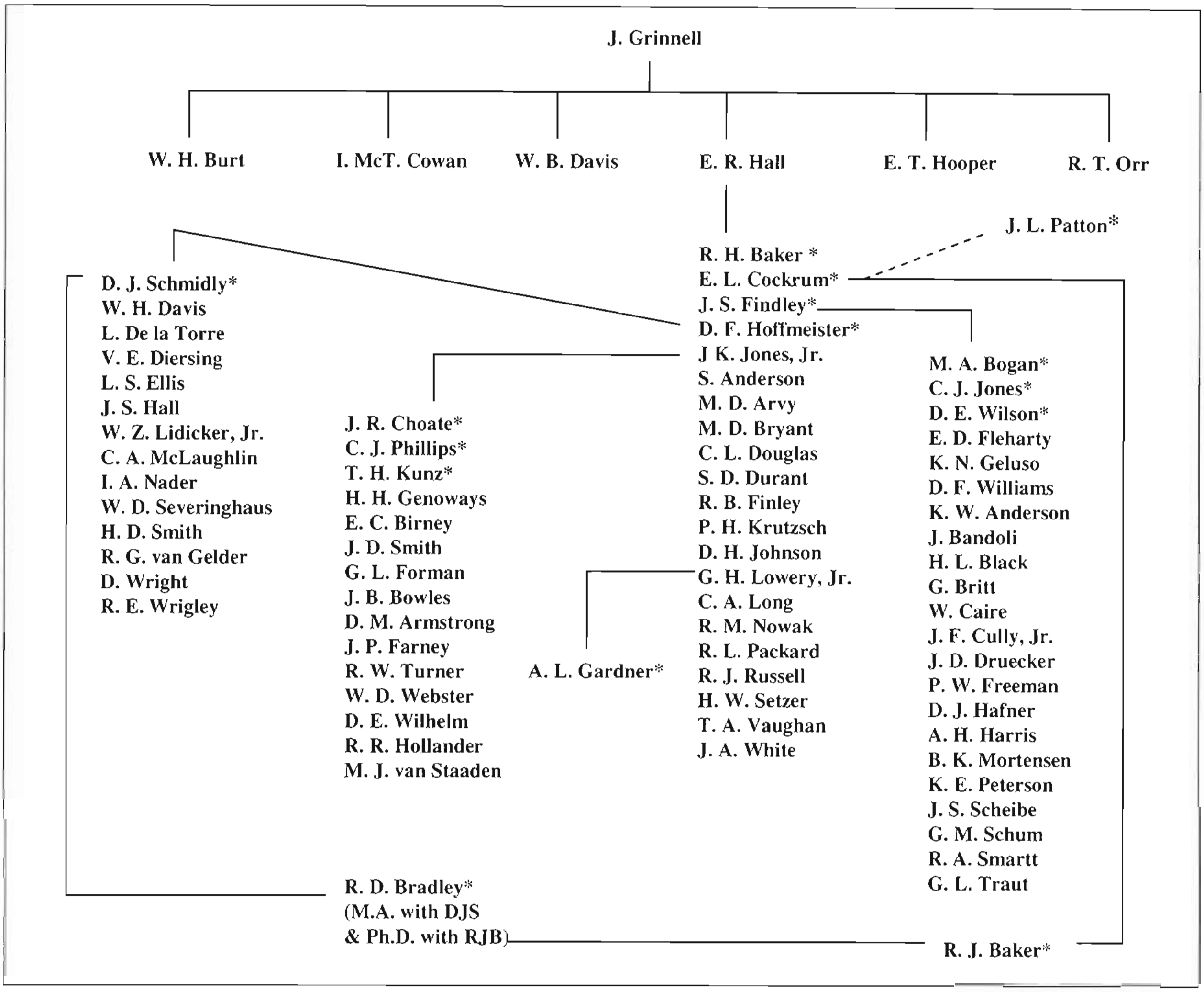

Figure 3. An academic "genealogy" illustrates the educational interrelationships among 15 of the 16 authors (asterisks). Ultimately, the paradigm of North American terrestrial field mammalogy traces to Joseph Grinnell at the University of California – Berkeley.

book. At the same time, most photographs of Grinnell's own students are similar to this one of Grinnell. For example, E. Raymond Hall, who was one of Grinnell's illustrious students, always wore a blue serge suit, used a swaggering gait, and often sniffed with disdain when viewing what he regarded as inferior findings when at work at the University of Kansas and while attending scientific meetings. Only careful observation—noticing for instance the gravy stain on his red tie or tobacco burns on the edges of his collar—suggested that he might be something other than a stiffly timid professor in a conservative blue suit.

Although Grinnell is described as small of stature, and shy, his personality somehow over-shadowed that of all of his subordinates. Thus, by the time that Grinnell's students graduated and took academic positions of their own, at least some of them were clone-like mimics of their former mentor. For example, E. Raymond Hall wrote an eerie obituary for his academic mentor (Hall, 1939); people who knew Hall reasonably well would agree that virtually every statement that Hall made about Grinnell could just as well have been applied to Hall himself! The significance of this observation is obvious: people who train graduate students pass on more than their knowledge. Graduate students exposed for several years to daily interactions with dominating willful mentors tend to acquire many of the personality traits of those mentors. Certainly they learn to do as their mentor did, but presum

So, in summary, Joseph Grinnell's personality and work habits determined the original academic culture of field mammalogy and this has persisted (perhaps with refinement) through at least three generations of scientists. Viewed from a distance, the conduct and flavor of any science is a summation of the personalities and behaviors of its participants (Kuhn, 1962) and Joseph Grinnell can be held responsible for some of this in field mammalogy.

Joseph Grinnell's academic assignment and his association with David Starr Jordan are two additional relevant things about his career at Berkeley (Phillips, 1995). As background, it is necessary to recall that the modern concept of academic departments originated late in the 19th century and major institutions typically had "Natural Science" departments in Grinnell's time. Currently, academic departments are primarily administrative conveniences and secondarily bastions of specific scholarly paradigms. In Grinnell's time, the roles were reversed; academic departments were built around, and represented, particular paradigms (Phillips, 1995). For instance, at the turn of the century natural science departments housed courses and research in such subjects as microscopic cytology, embryology, and comparative vertebrate anatomy. The still fresh theory of evolution was the unifying concept of natural science. Joseph Grinnell was not part of the natural science movement and his job at Berkeley was not in any of the natural science specialties. Instead, Joseph Grinnell worked in the museum and, consequently, modern North American field mammalogy originated in a museum environment rather than in that of an academic department. This is more than a subtle distinction. As Don Hoffmeister points out in his autobiography (*A Personal Sixty-year Retrospective*), the museum and the academic biology department at Berkeley were competitors for workspace and resources. This competition forced a schism, which in turn caused research and student training to be conducted separately, in intellectual isolation. And it still is to a degree!

The Academic Museum

In his museum role, Joseph Grinnell was a driving force behind (a) the acquisition of new zoological specimens, (b) the identification, organization, cataloging, and storage of specimens (the process called curation), and (c) the use of specimens as tools in teaching and objects of research. Grinnell's basic goal was to create a 'research collection' of specimens representing a broad array of geographic regions and habitats. The specimen's research values would primarily be for taxonomy and as physical reference points for the description of new species and for making comparisons among and between species. Thus, when students entered the graduate training program at Berkeley in the early years of the 20th Century, it was assumed that they would (a) learn how to collect and suitably preserve animal specimens, (b) learn how to organize and properly store the specimens so that they could be used in future research, and (c) learn how to conduct basic taxonomic research.

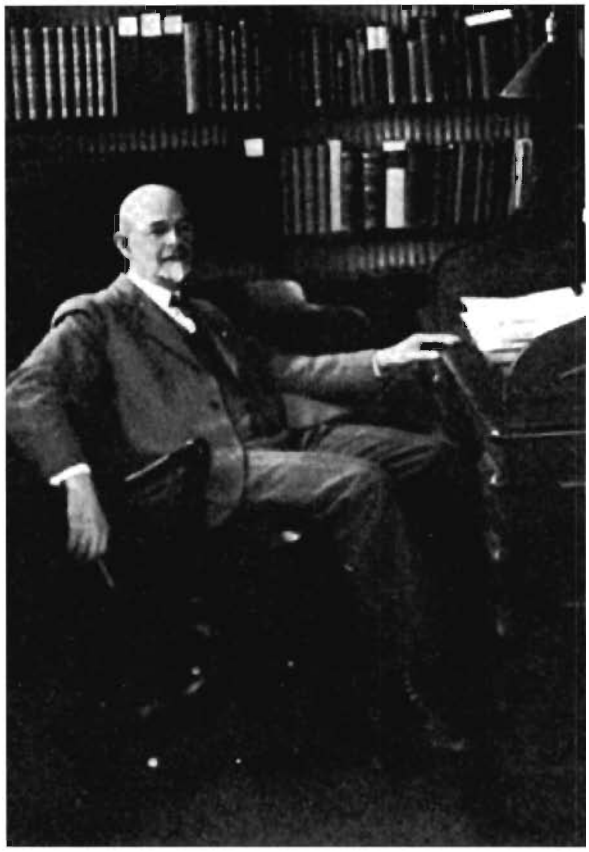

Figure 4. Joseph Grinnell in his office at Berkeley.

Grinnell was dogmatic about this specific focus. For example, he forbade a student zoologist, Tracy Storer, from conducting research on ecological principles or from studying living mammals instead of museum specimens (Dunlap, 1988). Grinnell's dogmatism and focus on what he viewed as acceptable academic museum activities had their intended impacts. Most of the early students of mammalogy who graduated from Berkeley went forth to create (or expand) their own academic museums with collections suitable for taxonomic research. E. Raymond Hall did this at the University of Kansas, William H. Burt and Emmett T. Hooper did it at the University of Michigan, Donald Hoffmeister did it at the University of Illinois, William B. Davis did it at Texas A&M University, J. Kenneth Doutt did it at the Carnegie Museum, John Eric Hill did it at the American Museum of Natural History, Robert T. Orr did it at the California Academy of Sciences, Remington Kellogg and David Johnson did it at the U. S. National Museum [the Smithsonian National Museum of Natural History], and Stephen D. Durant did it at the University of Utah. Likewise, many of the graduate students trained by Grinnell's own students—the second academic generation—also took academic positions and pursued similar goals: Hall's students Rollin Baker and James Findley developed the zoological collections in the Museum at Michigan State University and at the Museum of Southwestern Biology, the University of New Mexico, respectively. Another of Hall's students, J. Knox Jones, Jr., remained at Kansas for the first part of his career and while there he continued to build the mammal collection in the Museum of Natural History. In 1970, Jones left the University of Kansas. He took positions as Graduate School Dean and Vice President for Research at Texas Tech University and was instrumental in creating the Natural Science Research Laboratory (NSRL) at the Museum. He directly contributed to the dramatic growth in the NSRL mammal collection (which catalogued specimen number 100,000—a specimen of a previously unknown species of shrew—in June 2003).

Jerry R. Choate, Robert J. Baker, Clyde Jones, Hugh H. Genoways, and the late Elmer C. Birney and Robert L. Packard—six men who are three academic generations removed from Joseph Grinnell—carried on the Grinnellian museum tradition by substantially improving the mammal research collections at their institutions. Mammal collections at the Sternberg Museum at Fort Hays State University (J. R. Choate), the NSRL at the Texas Tech University Museum (R. J. Baker, C. Jones, R. L. Packard, and H. H. Genoways), the Carnegie Museum of Natural History and Nebraska State Museum at the University of Nebraska (H. H. Genoways), and the James Ford Bell Museum at the University of Minnesota (E. C. Birney) all were increased and modernized by faculty members functioning in the Grinnellian tradition.

The creation, development, or expansions of academic museum research collections were major challenges for each of Grinnell's scholarly descendents who chose that path. Mammal research collections are expensive: they require physical space with adjustable and predictable environmental control; they require airtight storage containers; they require upkeep, which means that there must be museum staff or collection managers; and they require an accurate, safely stored, specimen database (all done by hand prior to the advent of convenient computers and software). In other words, mammal research collections are not just a matter of tossing field specimens into cardboard storage boxes and forgetting them. Academic field mammalogists who built or expanded museum collections at their universities usually struggled to obtain institutional funding. Naturally, they typically were in competition with other academic units and, unfortunately, in some instances they were forced to compete directly with biology or zoology departments. It was not until the late 1970's that the National Science Foundation recognized the scientific value of collections and offered programs for competitive funding in support of collections. In the meantime, many field mammalogists turned to the private sector to support their work, just as Joseph Grinnell had done earlier in the 20th Century. In Grinnell's case, a woman named Annie Alexander was the critical benefactor and her endowment was significant to the program at the University of California-Berkeley.

In addition to the costs associated with collection maintenance, there also is the cost of conducting fieldwork and obtaining the specimens in the first place. Once again, private sector funding has been the most significant source of financial support both in academic museums and in major private museums such as the American Museum of Natural History in New York and the Field Museum of Natural History in Chicago. One of us (Rollin Baker) describes how wealthy individuals in Michigan made it possible to conduct fieldwork in México and presented the Museum with taxidermy examples of big game mammals. All of this in turn helped develop the mammal research collection at the Michigan State University Museum.

"I became friends with a great many wealthy big game hunters and travelers. Many times, I gained such friends by showing my amateurish films made on Mexican trips to the public, often to service clubs. Travelers and big game hunters would come up afterwards and swap field stories with me and describe their overseas and personal experiences with exotic wildlife. In the mid 1960s, I organized the Michigan Polar-Equator Club, with almost 100 of these well-to-do members attending three or four meetings annually. People like archery expert Fred Bear, for example, were members. I also published a quarterly Club Newsletter containing news about which member was going where and for what purpose. And it paid off since it was not a one-way street only in my Museum's direction. I tried hard to help these friends out mostly as a trip advisor and supplying them with details about the glamorous wildlife that they would see on their trips to remote areas. In the days before the endangered species slowed over-seas hunting opportunities, these people helped fill my research cases with skulls of ursids, canids, felids, etc. They also helped cover my range walls [the museum collection space is usually referred to as a 'range'] with skulls of Asiatic, African, and Neotropical bovids and cervids. This was done when one of these friends called to say that he was going to hunt in some out-of-way part of the globe. So, I would furnish this cooperating individual with a to-whom-it-may-concern letter that expressed my Museum's interest in obtaining examples of the unique wildlife in their area for educational purposes. Further I would state that the bearer would be trophy-hunting there with proper permits and guides, and at the same time, were official permission to be granted, they could collect a few additional kinds of wildlife as scientific and educational museum specimens. With this letter in hand, the outfitter would usually arrange for the take of additional specimens. It worked like a charm making both the hunters happy by gaining more hunting permits and our Museum pleasantly enhanced by wild-taken specimens of cervids, bovids, felids, ursids, etc. Of special interest was a complete skeleton of an African elephant with the hunter also paying to have it assembled for exhibit (a 1964 construction project in which Carleton Phillips participated) and an immense set of horns attached to the skull of an Asiatic "Marco Polo" sheep. On one occasion two hunters hired a museum-trained collector in East Africa to join their safari and do nothing but collect and prepare several dozens of study skins of birds for the MSU Museum. Sometimes, Michigan sportsmen actually asked me to suggest countries in which they might hunt to obtain trophies that they didn't have. As a result, I persuaded several to hunt in Neotropica and even one to go north for a muskox. This latter trophy finally came to the Museum as a full mount.

There are dividends obtained if you get out of your ivy-covered building, mingle with everyday folks, and demonstrate how your scientific knowledge has "practical" aspects, useful to the general citizenry. In fact, to illustrate citizen support, once when I was at the University of Kansas and when I spent a year as acting museum director while Director E. Raymond Hall was away, I decided, after getting Hall's approval, that the Kansas Museum needed someone in fisheries. At that time fish-farming in farm-type ponds was becoming all the rage. So what I did was to entice a number of friendly sportsmen to write letters to the university chancellor pointing out that this important discipline was lacking in Kansas institutions and that University of Kansas could give it a needed boost. And guess what? The administration, realizing that it was important, bought the idea and allowed us to hire a fisheries expert named Frank Cross. However, the administration never had the feeling that the persuasion came from the staff in Zoology or that the Museum of Natural History was behind the idea to get Kansas alums and other important citizens to point out the need for their help. On another occasion, the powerful chairman of the Ways and Means Committee of the Michigan House spent an hour or two chatting about his latest big game hunt. When I reported this event to my boss, he was astonished that this powerful politician was informally visiting on campus without the knowledge of the administration. Always remember, that such donors, often non-college types, are frequently highly flattered that their hobby of big game hunting has educational value, but they usually do it because of the friendly enthusiasm of their friend the museum person rather than for the institution itself. And it's the same way if they provide tax-write-off money for your field trips or those of your students. And when you hand them publications containing mention of them by name as supporters of the work, they are most pleased and receptive to providing more help. I hasten, however, to warn you never to ask favors from potential donors unless clearing such contacts first with your bosses."

Modern fieldwork in support of a mammal research collection is even more expensive than the earlier, traditional, types of specimen collecting. This partly is because of travel costs, which can include airfare and shipping, and partly because of the technologies required to insure that data return is maximized. The 1981 Alcoa Foundation-Carnegie Museum-Hofstra University Expedition to Tafelberg, an isolated flat-topped mountain in Suriname, required the use of a helicopter owned by the Paramaribo Gold Mining Company, which apparently was a front for CIA operations in the region. The 'company' provided its copter to Carleton Phillips and Hugh H. Genoways at the cost of $1,000/ hour for a total of $10,000 in cash. This cash, in turn, had been provided by Hofstra University President, Jim Shuart, and had originated as a private donation (see Phillips' *Ken Ward in the Jungle: Making Scientific Sense of Fieldwork*). The mammal specimens collected on top of Tafelberg can be studied in the collections at the Carnegie Museum of Natural History in Pittsburgh. It might be obvious, but perhaps we should also note that the $10,000 helicopter cost was in addition to all of the other expedition expenditures (Figure 5). The Texas Tech University Sowell Expedition to Ecuador, conducted over three and a half weeks in August 2001, is another example. The details of this complex operation are also provided in Phillips' autobiographical chapter. In this instance, the fieldwork cost $75,000. This experience involved educational opportunities for Ecuadorian students, and produced 763 mammal specimens from three general localities. In simple terms, the specimens cost nearly $100 apiece, although as pointed out in Phillips' chapter their real scientific value is incalculable. It is noteworthy that among these 763 mammal specimens, only 50% were deposited in the mammal collections of the NSRL at the Texas Tech Museum. The remaining specimens were returned to Ecuador in order to support the development of academic-based research-quality mammal collections in that country.

It was a consequence of Grinnell's educational foundation that acquisition of new specimens, development of research collections, and a focus on zoogeography and taxonomy (two types of research that were practical with museum specimens) were the major aspects of field mammalogy as it developed from 1900 through about 1960. At the same time, the present book also illustrates the abrupt departure from this mode as the central and only feature of field mammalogist's careers. Robert Baker, for instance, can be credited with creating the largest (in terms of species diversity) current collection of frozen tissues that can be used for molecular research and associated with a physical voucher specimen of the actual individual from whom the tissues were obtained. This particular collection is housed in the NSRL at Texas Tech University. Other authors from this generation (examples include Thomas Kunz and Carleton Phillips) pursued projects that often produced specimens, but otherwise were some distance from traditional museum-based work.

The museum origins of field mammalogy are critical to understanding this scientific sub-discipline of biology and, especially, its breed of practitioners. The emphasis on building research collections of preserved specimens translates into a need to collect the specimens in the first place. The collection process in turn requires going afield and obtaining specimens by whatever means are most suitable. With mammals, the means include trapping and hunting. Thus, the museum origins of the sub-discipline dictated that future practitioners would be persons who (a) were interested and willing to travel and live in outdoor environments, (b) enjoyed and were able to perform various forms of hunting or trapping, (c) could be trained to preserve their specimens in ways appropriate for museum research collection, and (d) had personalities amenable to keeping accurate, reliable records. The autobiographies collected for our book can easily be read according to this simple set of criteria.

FIELDWORK IN MAMMALOGY

The field part of field mammalogy—essentially the business of collecting and preserving specimens—requires additional elaboration because this activity probably is even more dominating than is the academic heritage of museums and Joseph Grinnell himself. Camping, hunting, trapping, and collecting and preserving mammal specimens are easily the most common themes in all of the autobiographies printed in our book. Various authors have individualized explanations for their interest in these activities, but the fact is that such things were a major part of their boyhood adventures regardless of the reasons. Obviously, then, entry into the profession of field mammalogy is facilitated by an interest in the fieldwork required for conducting scientific studies of wild mammals

So far as fieldwork is concerned, there are four elements to consider. First, what determined where and how field collecting would occur? Second, what influenced the 'culture' of fieldwork in mammalogy? Third, what determined the scientific—or other—goals of doing fieldwork? Fourth, what are the societal, economic, educational, and scientific values of such an activity?

Here in North America, specimen collection and preservation are intertwined with the great geographic explorations of the 19th Century and especially with state geological and natural history surveys (Hoffmeister and Sterling, 1995). Field mammalogists like to mention a former United States President, Thomas Jefferson, as the original American mammalogist. Although this at first seems like an extreme example of crassly appropriating the name, reputation, and persona of a famous person, it actually has substantial merit. The dramatic Lewis and Clark expedition to the Pacific Northwest was intended, in part, to obtain examples of the wildlife and plants native to a vast and largely unknown landscape. Some historians (e.g., Brodie, 1974) have argued that specimen collecting and preservation were secondary to other political motivations on Jefferson's part, but biologists have tended to believe that Jefferson was genuinely intrigued by exploration and natural history. In a sense, the way biologists see it Jefferson's importance was that he legitimatised intellectual curiosity about the relationship between geography and the distribution and variety of living things. If a person of Thomas Jefferson's stature thought that there was value in obtaining this type of information for North America, then it follows that activities directed toward this end are acceptable and important.

Figure 5. Field mammalogists embark for Tafelberg. The Alcoa Foundation-Carnegie Museum-Hofstra University Expedition, Suriname 1981.

Whatever it was that motivated Thomas Jefferson, the high-profile Lewis and Clark expedition resulted in significant linkages. The expedition coupled geographic exploration with searches for animal and plant life; it coupled information about locality and landscape with physical specimens preserved for later study; and it coupled the act of exploration with the creation of museum-type collections, thus creating a scientific enterprise. In the case of the Lewis and Clark expedition, the specimens were placed in the hands of the Philadelphia Academy of Natural Sciences. Although they eventually were lost, for several decades these particular specimens of native mammals and birds were exemplars of the outcome of the Lewis and Clark expedition. Furthermore, Jefferson's actions established the precedent for Federal funding for exploration and documentation of the national biota.

Spencer Fullerton Baird was another naturalist whose leadership was critically important to exploration and museum-based studies of mammal specimens.

Baird was born in Pennsylvania in 1823. He attended Dickinson College and by the tender age of 27 years was appointed Assistant Secretary of the newly established Smithsonian Institution (Hoffmeister and Sterling, 1995). The Smithsonian was founded in 1846 and quickly established its importance to Federal initiatives related to natural history. As for Baird, his reputation as a field naturalist, collector, organizer of field parties (often in conjunction with railroad surveys) and describer of new species was unprecedented in the first half of the 19th Century. Baird authored the first version of the Mammals of North America, in which he listed 232 species, 52 of which were new to the fledging science of taxonomic mammalogy (Hoffmeister and Sterling, 1995).

Exploration and specimen collecting became commonplace after the Civil War, and not surprisingly were intertwined with settlement of the American west. Earlier in the century, the Lewis and Clark expedition had been a huge human challenge because it ventured into true, uncharted wilderness. From our present perspective, the problems of long distance travel, logistics, and lack of reliable communication, seem truly daunting. Oddly, however, the post-Civil War expeditions might have been more difficult to accomplish because the westward movement of European peoples created a socially unstable, dangerously unpredictable environment in which to work. One consequence was that many of the post-Civil War collecting expeditions were closely tied to the activities of U. S. Army cavalry units. This led to assorted difficulties with Native Americans and the soldiers themselves. During the Long Expedition into the Colorado Rockies, disgruntled soldiers cast off specimens preserved in alcohol and, of course, consumed the preservative. The flamboyant boy general, George Armstrong Custer, was an avid hunter who regularly included a naturalist and bird collector, George Bird Grinnell (not a relative of Joseph), on his hunting and military expeditions. The naturalist accompanied Custer on his last, and most infamous, military expedition, but fortunately for him, he remained at the steamboat and thus did not join Custer on his final ride up to the bluffs of the Little Big Horn.

By the later part of the 19th Century the Federal Government recognized the need for a formal system of exploration, surveying, and natural history collection. Thus, the Biological Survey—a forerunner of the Department of Interior—was created and placed under the directorship of a man named C. Hart Merriam (Figures 6 and 7). While Joseph Grinnell served as academic grandfather to field mammalogy, Merriam served as the guiding light for blending exploration, geography, and field collection into a foundation for Federal policies on land use and conservation. More important than this, Merriam also demonstrated how field studies could be the stimulus for conceptualization about the natural world. Merriam's idea about Life Zones is a historically significant example of how time afield can be translated into scientifically testable concepts.

The United States Bureau of Biological Survey of the U. S. Department of Agriculture developed into the main vehicle for acquisition of museum specimens of North American mammals. Eventually, it also became the vehicle for establishing a credible baseline about the characteristics of native and disturbed habitats, especially in the southwestern United States (Schmidly, 2002). Moreover, historians such as Keir Sterling (Sterling, 1977, 1991) and early mammalogists such as W. H. Osgood (Osgood, 1947) also attribute the fundamentals of field mammalogy to the Biological Survey's first director, C. Hart Merriam. Sterling (1991) summarized it in the following way.

> "...Merriam's technique called for a collector to move into an area, lay out trap lines so as to secure a good sampling of the small and medium-sized mammals resident in the region, make note of the plants and other key life zone indicators, and perhaps collect larger species with a shotgun or rifle. All specimens would be carefully preserved and labeled, with notes taken concerning the circumstances of their capture, sex, measurements, and other important data."

Merriam must have been an authoritarian boss for the U. S. Biological Survey. According to Rollin Baker's geologist father, at the University of California-Berkeley C. Hart Merriam was nicknamed 'Christ Himself Merriam.' This was in contrast to the nickname given to C. Hart's paleontologist brother—John C. Merriam—who was known as 'Jesus Christ Merriam.' In C. Hart's case, the implication of his nickname matches the extent to which Biological Survey personnel carried out his directives.

Under Merriam's direction, the U. S. Biological Survey produced invaluable information about natural history and established the foundation for obtaining, collecting, and preserving specimens of mammals.

Four of the authors with autobiographies in *Going Afield* (Don Wilson, Al Gardner, Mike Bogan, and Clyde Jones) represent 'modern' versions of C. H. Merriam's original U. S. Biological Survey. Both Wilson (*Bats to Biodiversity: Spyder Had A Pretty Good Ride*) and Gardner (*Been There, Done That: After 44 Years of Preparation, What's Next?*) are employed by the Federal Government: Wilson is a Senior Scientist with the Smithsonian (but formally worked for the Fish and Wildlife Service, U. S. Department of Interior) and Gardner is in the Fish and Wildlife Service. Physically, both men work out of the Smithsonian's Museum of Natural History where they have access to one of the largest mammal collections in the world, including the specimens collected by C. Hart Merriam's boys, beginning in 1890. Both of these Federal scientists have enjoyed numerous opportunities to collect specimens of mammals, survey-fashion, in Latin America and elsewhere. Mike Bogan's (*Pass Those Cloverdale Spuds—Again*) current Federal employment also is descended from Merriam's old U. S. Biological Survey operations. In Bogan's case, his position is in a 'Coop' Federal Unit based at the University of New Mexico. In some ways this might be an ideal location because it hybridized a Federally directed mammalogy job with the academic environment of a university with a research museum (Museum of Southwest Biology) and tradition of Grinnellian academic field mammalogy through the presence of James Findley (*Mammalogical Reminiscences*). Finally, one of us (Clyde Jones) served with the Fish and Wildlife Service (U. S. Department of Interior) in Washington, D. C. and Denver, Colorado for twelve years. During this tenure, Jones increased the budget tenfold for fieldwork (including travel) and for collecting specimens of small mammals, especially those listed or proposed for listing on the Endangered Species List.

All four of these modern biological survey-type Federal mammalogists were trained in the Grinnellian

Figure 6. C. Hart Merriam (1902).

tradition (Fig. 3). In a sense, they represented an insertion of the Grinnellian tradition of small mammal museum-based research into a Federal Bureaucracy that had become heavily influenced by policy issues related to land, forest, and animal management, game species, and, of course, predator control in support of cattle and sheep ranchers.

Merriam's original field men, or collectors, were a critical part of the success of Merriam's enterprise. One example is a man named Vernon Orlando Bailey (Fig. 7). Bailey was appointed to the survey in 1887 as a Field Naturalist. Bailey's assignment was the Great Plains and Rocky Mountain region, a vast, difficult to reach, and significant piece of North America! Bailey's positions with the Biological Survey ultimately became entangled with related collecting and research for the U. S. National Museum (currently the Smithsonian Institution's Museum of Natural History). By the end of his formal career with the Biological Survey, in 1933, Bailey had become one of the most successful and observant of the survey collectors. Because of his connections with the U. S. National Museum, Bailey continued to conduct collecting trips (his last one being in 1937) to the west and studied museum specimens until his death in 1942 (Layne and Hoffmann, 1995).

Figure 7. C. Hart Merriam (left) and Vernon Bailey together on a field collecting project. Photograph courtesy of Biological Survey Archives.

Vernon Bailey was the fourth child of Hiram Bailey, a Michigan woodsman, hunter, and sometimes mason, who lived in Manchester, a small town southwest of Detroit. Bailey was born in June 1864, at a time when the nation was in turmoil—the Civil War seemed to be going poorly, at least insofar as citizens in the North were concerned. When the war ended, the next year, the social and economic fabric of the country was tattered. Thousands of veterans headed for home, and reconstruction began in the South. Against this backdrop, Bailey's family pulled up stakes and headed west by wagon in 1873. After a journey of several months, they settled anew on the Elk River in Minnesota. It was here that young Vernon ventured afield, exploring, camping, hunting, and collecting specimens. He was so successful at collecting and doing self-taught taxidermy that by the age of 19 he was selling museum-quality specimens to natural history brokers in Canada and Germany (Layne and Hoffmann, 1995). These specimens were what caught the attention of C. Hart Merriam, who immediately contacted Bailey and who later helped secure a position for him with the Biological Survey.

Enough time (~100 years) has passed so that the results from some of Vernon Bailey's survey projects can be meaningfully compared to contemporary conditions. One of our authors, David Schmidly, used vintage photographs and Vernon Bailey's meticulous field notes as benchmarks for analyzing one hundred years of change in the Texas landscape and distribution of mammal species (Schmidly, 2002).

One of us (Rollin H. Baker) had the good fortune to have met Vernon Bailey while conducting research at the U. S. National Museum in 1940 and prior to assignment as a Naval officer. Baker's recollections of the 76-year-old field man, collector, and taxonomic mammalogist are as follows.

> "Vernon Bailey was a talkative gentleman when we visited in March 1940. This was in the Biological Survey quarters, which then were adjacent to Curator Remington Kellogg's office in Division of Mammals. At that time these offices all were located in the south wing on the first-floor of what was then known as the United States National Museum. Bailey was a slight built, wiry fellow probably weighing 140 pounds at most. He had a unruly mop of hair and almost owl-like eyes enhanced by a rather small face and 'weak' chin.

At least at the time that I saw him, he appeared to be not what one might call a classy dresser, and if you will please pardon my saying so, he looked a mite slouchy.

Bailey's eyes twinkled when he perked up as I had told him that I visited the Texas type locality for his subspecies of beaver, which he had named *Castor canadensis texensis*. While doing fieldwork at the type locality, I had met a farmer named Brune. The fellow recalled to me that as a youngster he had been the one who led Bailey down to the bank of Cummins Creek in Colorado County and showed him where beaver had cut willow. I also asked Bailey about his adventures in the Big Bend along with ornithologist Harry Oberholser. Bailey described the place in the Chisos called 'the Meadow,' where he caught specimens of the yellow-nosed cotton rat, *Sigmodon ochrognathus*. The Meadow was the very same location where I personally had seen this rodent's runways when I worked there in the summers of 1936 and 1937.

Bailey confessed to me that he had ended his trapping days and was more interested in living mammals. He showed me one of his famous Verbail Live Beaver Traps. I had already seen these traps in operation in the Texas Llano drainage. He also developed a bow-like, spring-operated Verbail leg-hold trap that caught its victim by one undamaged leg with a tight chain. In fact, Walter P. Taylor had one that in 1941 my wife Mary set on the bank of Piney Creek in Polk County. Her successful catch was a very irate wood stork. As I recall, as part of my interview with Bailey, he generously took time to show me his personal Texas mammal collections and then introduced me to an assortment of famous but elderly mammalogists including Stanley Young, Edward A. Goldman, Arthur H. Howell, Norman Preble, Hartley H. T. Jackson, Viola Schantz, Gerrit Miller, and Remington Kellogg. What an opportunity for a lowly hero-worshiper like me!"

Not surprisingly, some of the early U. S. Biological Survey projects spilled over into México. As a consequence, two other hardworking but largely self-trained men, Edward W. Nelson and his assistant, E. A. Goldman, also became legends in the annuals of field mammalogy. They did this by doggedly collecting mammals in poorly known regions of México. From 1892 to 1906 they traversed rugged terrain—humid tropical forest, parched deserts, chilly volcanic mountains and grassy plains—dodged nervous Indians and bandits, and slept on the ground, in huts, under the stars, and in the rain. In the end, Nelson and Goldman collected and preserved 17,400 mammal specimens and described and named an astounding 354 species and subspecies of vertebrate animals (Sterling, 1991).

Establishment of an interest in Latin American countries was a noteworthy byproduct of the extensive Méxican expeditions and later museum-based research by Nelson and Goldman. Instead of focusing entirely on United States territory, 20th Century field mammalogists invested heavily in Méxican fieldwork. In the 1960's, for example, two of us (Baker and Phillips) revisited many of the type localities (geographic spots where the type specimens of mammal species had been obtained) established by Nelson and Goldman during their original expeditions south of the border. At least a hundred, and perhaps more, of the modern American field mammalogists also have conducted fieldwork in México. Hundreds of students have learned the basics of field biology on formal trips to México. At Texas Tech University, Robert D. Bradley regularly leads groups of undergraduate and graduate students to México to learn and put into practice the modern and traditional field techniques used to study mammals. Aside from the basic interest established by Nelson and Goldman's early explorations, it also is true that the landscapes and topography of México are almost ideal for testing hypotheses about speciation patterns in mammals. Snow-capped peaks, the twin east and west North-South mountain ranges, the Central Plateau, the rumbling trans-volcanic belt, the Isthmus of Tehuantepec, rain shadows, tropical forests, coastal scrub and thorn forest, arid plains, pine forests, and alpine meadows combine to make México a place of fascination for all sorts of field biologists. The residents, and their warm culture, are icing on the cake insofar as many American field mammalogists are concerned.

The training of the original México field mammalogists was another significant byproduct of American interest in the mammals that occur south of the border. Not surprisingly, one of Joseph Grinnell's prominent students, E. Raymond Hall, trained three key Méxican field mammalogists: Bernardo Villa-R; Ticul Alverez; and Arturo Jimenez-G. All three attended graduate school at the University of Kansas in the 1940's, 1950's, and early 1960's and all three returned home to successful scientific and professorial careers. One of the three, Bernardo Villa-R, became the father of Méxican field mammalogy and his students have made significant scientific contributions of their own.

By any measure, the fieldwork accomplished by Nelson and Goldman under the umbrella of the U. S. Biological Survey played a large role in setting the standard for American field mammalogy of the 20th Century. Given this, it is interesting to ask about their respective personal histories. As it turns out, there are striking similarities between them and Vernon Bailey. Edward W. Nelson (Figure 8) was a New Englander, birthed in New Hampshire in 1855. When Edward was just a lad, his father volunteered for service in the Federal army and died in the Civil War. Subsequently, Nelson's mother also left home to serve as a nurse in the wartime Sanitary Commission. As a result of this upheaval, young Edward Nelson acquired his interest in natural history while growing up on his grandparents' farm in upstate New York.

Nelson's eventual partner in field mammalogy, Edward A. Goldman, was a mid-western boy. He was born in Illinois in 1873 and grew up on family farms in Illinois and Nebraska and then on his father's ranch near Fresno, California. Goldman hunted in Nebraska and California and attributed his interest in the natural history of mammals to his father, Jacob Goldman (Goldman, 1935). By simple coincidence, the two men met in 1891 when Nelson stopped at the Goldman ranch seeking repairs to his buckboard (Sterling, 1991). They departed the ranch together on 10 October 1891. Going afield as a collecting team, the two Edwards rode Nelson's wagon and listened to the metallic clanking of two 42-pound grizzly bear traps hanging from the sideboards.

E. A. Goldman was commissioned during the First World War and assigned to the Army Medical Corps. After the conclusion of that European conflict, Goldman mostly worked in Washington, D. C. and because of his rank—Major—was often referred to as "the Major" when he was around the Division of Mammals at the U. S. National Museum. As with Vernon Bailey, Goldman was another early field mammalogist whose professional life intersected with that of a young mammalogist named Rollin Baker. Baker's experience visiting with the Major and his associates follows.

> "I had an opportunity to visit with E. A. Goldman in 1946, when I had just returned from a year of Pacific naval duty and received orders to report to the U. S. National Museum at the Smithsonian Institution. This was in order for me to write reports about my findings after spending most of 1944-1945 studying Micronesian wildlife. This unusual wartime opportunity began about mid-1944. This was after I had been transferred, thanks to some political string-pulling by my friend Remington Kellogg, from bridge watch duty as a destroyer officer attached to the Atlantic fleet to serve as mammalogist/ornithologist in preventive medical research on islands mostly in Micronesia. Along with David H. Johnson, who at that time was on active naval duty from his position as Assistant Curator of Mammals under Kellogg, we were attached as vertebrate experts to U. S. Naval Medical Research Unit No. 2 (NAMRU-2) with overseas headquarters on Guam. Our chief job was to cooperate with bacteriologists, virologists, and parasitologists to survey the local vertebrate fauna for pathogens that might influence the health of insular troop concentrations. By a gentleman's agreement, Dave prepared the report on mammals obtained while I would prepare the one on birds. The exception to this arrangement was that I wrote up my personal population study of Guam rodents, that I published in *Ecological Monographs* (Baker, 1946).
>
> For the study on the Pacific bird collection [eventually published as *The Avifauna of Micronesia, its origin, evolution, and distribution*; Baker, 1951], I occupied a desk in Curator Herb Friedman's personal office in

the Division of Birds where I also associated gained the expertise and assistance of Alex Wetmore, Burt Deignan, Harry Oberholser, and Dillon Ripley. I also spent time in the Division of Mammals where big, jolly, and kindly-gruff Remington Kellogg held sway.

Kellogg described his WWI duty as a sergeant under Major E. A. Goldman who as head of rodent control for the AEF [American Expeditionary Force] tried to rid the trenches of rats. Kellogg was indeed a genial fellow. My young daughter Betsy called him, to his face on several occasions, the whale man who sits in his chair. She rarely saw him get up but mostly watched when he put new filters in his rather unsanitary-looking pipe. From Kellogg, I received lengthy lectures about his alma mater, the University of Kansas, with some choice and earthy comments about E. Raymond Hall, R. A. Stirton, Jean Linsdale, William H. Burt, and other "KUer's" who in the 1920s departed Lawrence, Kansas for the University of California at Berkeley. In fact, Kellogg made the University of Kansas—also known as the institution on the banks of the Kaw River—so enticing and rosy, especially because E. Raymond Hall had only recently left Berkeley to go there, that I opted to do likewise.

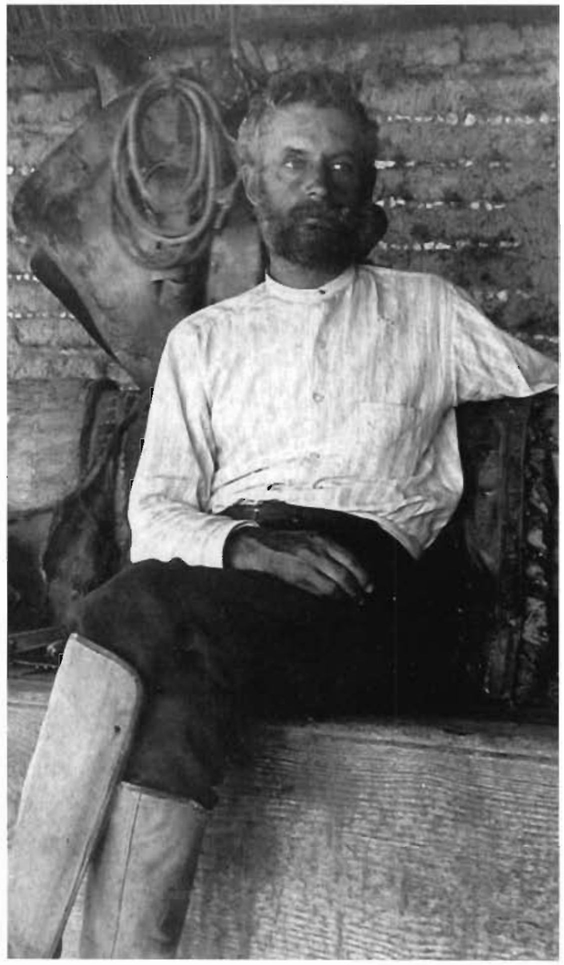

Figure 8. Edward W. Nelson in Mexico.

But to get back to my wonderful association with my hero, E. A. Goldman. When I arrived at the National Museum in 1946 all of the old stalwarts from the Biological Survey days and with whom I had hobnobbed during my first visit there in March 1940 had died except for Stan Young and Major Goldman. Luck was on my side when these two dignitaries did me the distinct and unforgettable honor of asking me to join them for lunch all those months, from January to June. Maybe I was singled out because I was an ex-wildlife biologist from Texas and/or because I wore a naval officer's uniform. Nevertheless, each weekday the three of us at noon dodged traffic to walk across Constitution Avenue to the Justice Building to eat lunch in the cafeteria that, as I recall, was on an upper level, maybe on the top floor.

Goldman was gentle, soft-spoken, and slightly-built (Figure 9). His hair was graying and sparse. He had regular, somewhat rugged and sunburned features. Like Vernon Bailey, he probably was about 5 feet 9 inches and weighed perhaps 140 pounds. However, unlike "country-boy" Bailey, Goldman was a "spit-and polished" individual, neatly attired in tailored suits, coats, shined shoes, white shirts, and handsome ties. He stood erect like an old soldier and had a commanding voice that I, for one, listened to closely whenever he spoke. For much of the conversation at the lunch table, I merely listened since the subject matter centered on the books that the Major and affable Stan Young were co-authoring about the wolf and about the cougar. And I learned plenty about both of these carnivores just by paying close attention to these two savvy field mammalogists.

But once in a while I would gently bring up some of Goldman's turn-of-the-20th century field exploits that he and E. W. Nelson experienced in México. Naturally, Goldman was interested that I had collected mammals in the summer of 1938 in the Méxican State of Coahuila. This gave Goldman reason to recall some of his adventures in that state. As in other parts of México, he and Nelson traveled whenever possible by train, but these opportunities were few and far between. Otherwise, they rented buckboards and teams of horses or preferably mules. For mountain work, they used pack animals. He didn't say much about his dealings with the local people. No doubt he used the ploy that most field people do when visiting with locals – and that part of the field investigations has to do with determining the economic worth of the animal life [both good and bad] to the farmer and rancher. Anyhow, listening to him talk about the diverse mammalian life of México made me more certain that I would get back to Coahuila, and finish looking at the mammals. And sure enough, beginning in 1950, I did (Baker, 1956)!"

The foregoing synopsis of the origins, triumphs, and interests of Vernon Bailey, E. W. Nelson, and E. A. Goldman is most interesting as a comparative benchmark for all of the autobiographical sketches in the present book. The similarities are strikingly obvious among the authors in *Going Afield* and Bailey, Nelson, and Goldman. With few exceptions, there are at least four common themes among our 19th and 20th Century field mammalogists: (1) growing up on farms or with access to farms or ranches; (2) hunting and camping; (3) interest in natural history passed on directly or indirectly by parents or grandparents; and (4) strong-willed determination to learn about mammals first-hand. To this we can add a comment about E. Raymond Hall, one of Grinnell's students whose name appears frequently on the pages of our book. After Hall's death, James Findley and J. Knox Jones, Jr. wrote the following (Findley and Jones, 1989).

"E. Raymond Hall was a farmer, trapper, and a naturalist at heart, and a prodigiously successful scientist by profession."

To a large extent, the breed of scientists represented in this book—people we describe as modern field mammalogists—are a blend of Joseph Grinnell's personal academic influence, the museum collection mind set, and the style and purpose of C. Hart Merriam's fieldwork as exemplified by Vernon Bailey, E. W. Nelson, and E. A. Goldman. Readers are at liberty to decide the extent to which individual contributors to our book match up with these three categories of influence, but matching in any category is remarkably easy to do.

FIELDWORK, MUSEOLOGY AND SCIENCE

Having explained the origins of field mammalogy, we now can address a basic question: what are the purposes and values of this sub-discipline of bio-

Figure 9. Edward A. Goldman. Photograph courtesy of Biological Survey Archives.

logical sciences? There are at least a couple of quick (but not simple) answers. One is that field mammalogy has produced the basic data upon which our knowledge of species and geographic distribution of North American mammals is based. Realistically, we can add that the same statement can be made for all other continents, albeit with the caveat that the contributions of American mammalogists blend in with those of mammalogists from other parts of the world. The global perspective, and international collaborations among field mammalogists, is easily illustrated by the careers of our symposium participants and authors. A casual survey of their resumes reveals foreign field projects conducted in: Cuba, México, Nicaragua, Costa Rica, Panama, Venezuela, Columbia, Ecuador, Brazil, Suriname, Guiana, Argentina, Namibia, Kenya, Gabon, India, Pakistan, Thailand, Finland, Ukraine, New Guinea, and Trinidad, just to name a few off-handed examples! It should be noted, however, that although this list of countries validates the statement that field mammalogists have had a global impact, it also lends credibility to the accusation that field mammalogists are basically scientists looking for an excuse to get away from home. Rollin Baker put it this way in describing his experience as a professor at Michigan State University.

> "A colleague once called me a non-quantitative, globetrotting mammalogist who used mouse catching in remote places as an excuse to leave campus and get out of attending faculty committee meetings—required duty for every warm or semi-warm academician."

Aside from a globe-spanning approach to research, another answer to the question about contributions is that field mammalogists have created and developed numerous museum-based scientific collections of mammal specimens that can serve as both documentation and as a resource for future research.

The importance of acquiring knowledge about mammal species and their geographic distributions is obvious—even intuitive—to field mammalogists. Field mammalogists seem to have an innate curiosity about the world, which drives them to wonder about animals in the context of landscape, space, and time. Such is not the case for all biologists, or people in general. Consequently, the intrinsic value of such information is not apparent to many laboratory-based or medical biologists who could profit from it. As a result, nearly everything known about mammalian cell biology, cell function, and the enzymes, proteins, and peptides produced by mammals is based on laboratory research involving perhaps 10 species (we are being liberal here, by including laboratory rats, mice, hamsters, gerbils, guinea pigs, rabbits, pigs, cows, and two species of primates.) In reality, most of these data come from three species—mice, rats, and human beings. Are these 10 species representative of all mammals? Is this too narrow a sample? Given the fact that field mammalogists have discovered, described, and named more than 4,000 mammal species representing an incredible diversity in adaptive features, it is obvious that something might be missing! The 10 species upon which a substantial body of biological data is based are not representative of mammals in general. The significant scientific question, of course, is: to what extent has this set of 10 species skewed and limited our knowledge base? It might be true (and we suspect that it is true) that mammalogists share the blame. Collectively, we probably have not done a very good job of communicating the purposes of our chief activity to the broader scientific community. Ironically, however, the fact that many people are unaware of information about species and distributions does not have any effect on the calculable value of such information to science.

So, what are the values of museum mammal specimens and their geographic data? Is it merely a matter of satisfying an inexplicable curiosity about the natural world? Scientifically, knowledge of species and their distributions is fundamental to understanding the history of life on our planet. Explanations of our world and all scientific concepts of biological relationships, major ecological features, and environment require this type of information. Although curiosity and a desire to make discoveries first-handed are a part of this, it nevertheless is true that the information has a deep and abiding value independent of the motives and personal drives of field mammalogists.

Field mammalogists in the first fifty years of the 20th Century, plus a few decades in the latter part of the 19th Century, were primarily devoted to the field collecting and the preserving of identifiable voucher specimens – mammalian study skins and skulls - for museum studies. Their uses were chiefly (1) to help inventory the array of mammals that occur naturally in the United States; (2) to give them scientific names and determine their relationship to other mammals; (3) to pinpoint their geographical distributions and, when fossil material was available, their geological distributions (thus creating a 3-dimensionsl picture of the evolutionary histories of species); (4) to publish scientific articles about their natural history, especially their food habits, and economic importance to human land-use practices; (5) to occasionally place on record details about their anatomical features; and (6) to study intraspecific geographic variation.

In order to do thorough appraisals of geographic variation in mammals, taxonomists needed more and more specimens from more and more localities, offering steady work to collectors. A taxonomist, for example, might purposely collect and then compare the skin-and-skull characteristics of specimens taken at localities in between the known ranges of two other alleged but similar species. If these specimens demonstrated intergradation of the morphological features that had been used to distinguish—define if you will—these two alleged species, as a result the two species could be judged as only subspecifically distinct. In addition, the intergrading individuals in the intermediate locality might also be named as new subspecies. Thus, by 1950—give or take a few years—the species status and geographical distributions of most North American mammals had been defined as a massive working hypothesis, and museums ended up with huge stocks of preserved skins-and-skulls. Persisting and unresolved, however, was the validity of many of the so-called 'man-made' subspecific categories, notably in such rodent groups as pocket gophers. Nevertheless, the publication of species revisions and the naming of "obvious" geographic variants as new subspecies were popular with museum curators, and also were the subjects of many doctoral dissertations.

In a sense, the first half-century of field mammalogy was a self-generating enterprise. No matter how many collecting trips were taken, and how many specimens were obtained, the need for additional specimens from additional localities was always generated by any museum-based taxonomic study of the preserved specimens. Such research required specimens in the first place, and the results of the research directed the field mammalogist to the next collecting locality. One might think that after more than 100 years of field collecting and preserving specimens, the job would be close to finished. On second thought, however, the world is large and the terrain complex. Vast regions are poorly known: much of South America and Asia are nearly unknown, even though American and local mammalogists have invested considerable time and effort in fieldwork. Not surprisingly, the most poorly known regions are the most tender, most likely to be destroyed through human exploitation of the native landscape. So, the places where there is a critical need for data about mammal species (and other animals and plants) are also the places where there are (a) the fewest data, and (b) the least opportunity to contribute to a solution.

In retrospect, it is obvious that the scientific contributions of Grinnellian mammalogy are linked - defined and limited by the activities of the collector and the needs of the museum curator. The scientific value of any mammal specimen is determined by however a quality museum specimen is defined. The original field mammalogists, and their modern counterparts, were unusually hardworking, energetic people (the proof being seen in their academic productivity). Nevertheless, what could be accomplished scientifically still would be determined more by the types of data obtainable from a museum specimen rather than by hard work alone.

Figure 10 illustrates the basic traditional components of museum-based mammal collections. Here, the museum specimen is the obvious centerpiece. In the late 19th Century and well into the 1960's of the following century the typical specimen consisted of a "museum skin and skull." This descriptor refers to the fact that field mammalogists were trained to skin, stuff, and dry specimens of small mammals (squirrel size and smaller) and to save their skull and lower jaw for later cleaning. Larger mammals were skinned, but not stuffed with cotton. Instead, the dried skins were transported to the museum and then sent out for tanning. The emphasis on dried skins and skulls is significant because it meant that specimens could be collected by means that killed them on the spot. Thus, the fieldwork conducted between 1850 through 1960 nearly always involved the use of guns or kill-traps as the primary collecting tools.

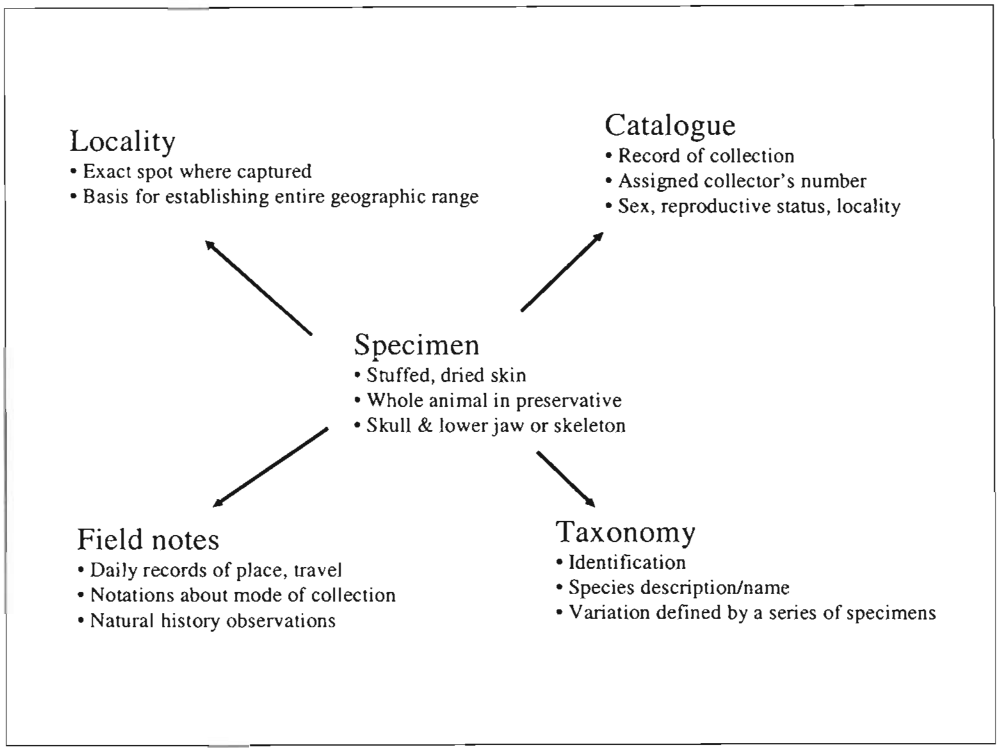

Figure 10. Traditional components of museum-based mammal collections.

The usual procedure for collecting small mammals was to set "lines" of snap traps (usually a specialized type called a 'Museum Special') in suitable habitat in afternoon. These trap lines were then 'run' the next morning, and the catch returned to camp for processing. Although this was an efficient, effective, and time-honored way of doing business, in reality it meant that all of the soft body tissues, and the vast storehouse of biological data that they represent, were discarded. The focus was totally on (a) geographic locality and everything it encompasses (habitat, microclimate, elevation, soil, land use) and (b) the taxonomically valuable elements of the specimen—its skin and skull. In retrospect, this type of collecting, and its limited focus, seems unfortunate. In fact, of course, to some extent it reflected the technologies available and the sorts of topics that were current in biology in general. At the same time, it also reflects the schism that separated museum-based biology from the biology conducted in university academic departments way back in the late 19th Century. To large extent, the autobiographies in the present book explain how all of this changed, beginning in the 1960's.

The original protocols, and attitudes, for doing fieldwork, collecting, and preparing museum specimens were codified so that museum mammal specimens have been collected, prepared, and labeled in a standardized way for more than 100 years. The current protocols trace back to Joseph Grinnell. The following quotation is taken from Dunlap (1988).

> "[Grinnell] supervised the museum [at Berkeley], which he directed from its founding in 1908 until his death in 1939, in minute detail, choosing even the tags and ink for specimen labels. He insisted on complete, cross-referenced correspondence files....and he ran the museum with an iron hand. Deviations from his standards earned the culprit a written note, which was to be returned when the condition had been corrected; Grinnell confirmed the change, initialed the note—and then put it in the person's file. Once, when Grinnell telegrammed from the field that some specimens he was sending in were to be prepared immediately, Raymond Hall pleaded that the diligent museum staff should be allowed to

take Christmas Day off. Besides, Hall added, the freight office was closed."

E. Raymond Hall wrote down his own guidelines—essentially Grinnell's guidelines—for collecting, preparing, and cataloguing museum mammal specimens. As an example of how to prepare a small, mouse-sized mammal, Figure 11 shows an assortment of original line drawings taken from E. Raymond Hall's *Handbook of Mammals of Kansas* (Hall, 1955). In addition to providing descriptions and range maps for all of the species known to live in Kansas, Hall also provided a detailed written narrative on how to collect mammals by trapping or hunting, how to prepare museum quality specimens, and how to record data. The drawings illustrate the critical field measurements in millimeters: total length; tail length; length of the hind foot (usually the right one); and length of the ear pinna, notch to tip (Fig. 11).

For a given specimen to be valuable for taxonomic studies or for establishment of geographic range, it was critical that the collector maintain careful records—usually called field notes and catalogue—and accurately label the specimen so that even if the catalogue or notes were lost, the specimen itself could still be useful. Once again, Hall's book on Kansas mammals (Hall, 1955) provides an example of the preferred method of cataloging and labeling (Fig. 12). The standard catalogue entry included exact location of capture (usually in straight line distance from some permanent mapped spot) and date of capture. The entry for each specimen consisted of a sequence of data: the collector's personal unique number for the particular specimen; a symbol for the specimen's gender; a tentative field identification; and a series of physical linear measurements and body weight. Similarly, labels were prepared for both the skin (always a rectangular tag on rag paper with cotton string that could be used to attach it to the right hind leg) and skull (either a small round or rectangular label with string that could used to secure it to the lower jaw). The data on the skin label duplicated those in the catalogue, whereas those on the skull label consisted only of catalogue number, gender, and the collector's initials (Fig. 12). Hall's directions for labels and field notes were explicit (Hall, 1955).

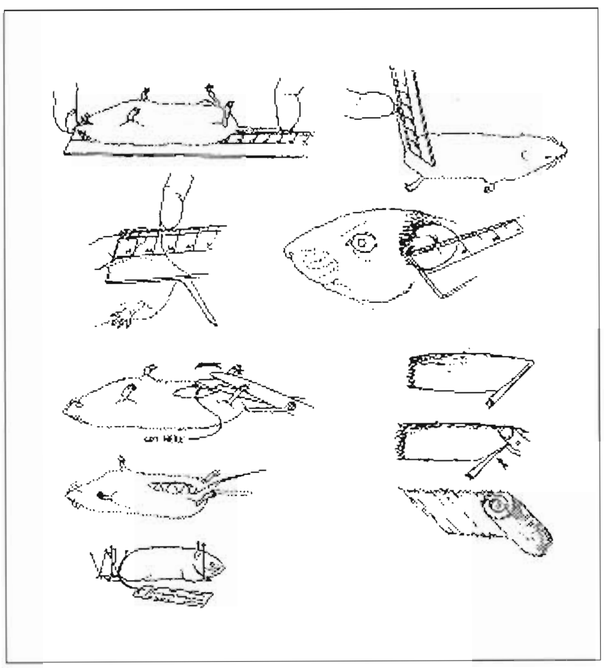

Figure 11. E. Raymond Hall's illustrations of how to prepare a standard museum mammal specimen.

> "Immediately after the collection is completed, prepare field notes...these should be written in Higgins Eternal Ink or Higgins Engrossing Ink, the latter having the advantage that it need not be washed... write full notes, even at the risk of entering much information of seemingly little value...describe vegetation...describe exact location of trap lines...keep record of closeness of settings of traps...keep full record of breeding data...dig out burrows if practical...record food plants...keep frequent censuses of diurnal birds and mammals...when feasible, interview old residents, trappers, National Forest and National Park rangers...give also your opinion as to his reliability..."

In Hall's guidelines to collecting and preparing museum specimens of mammals, the emphasis is totally directed toward dried or tanned skins, and cleaned skulls and lower jaws or entire skeletons. This emphasis was not unique to E. Raymond Hall. The thinking about mammal specimens, from the 19th Century well into the 20th, was that these types of preservations maximized the current and future scientific value of each specimen. A dried or tanned skin, for example,

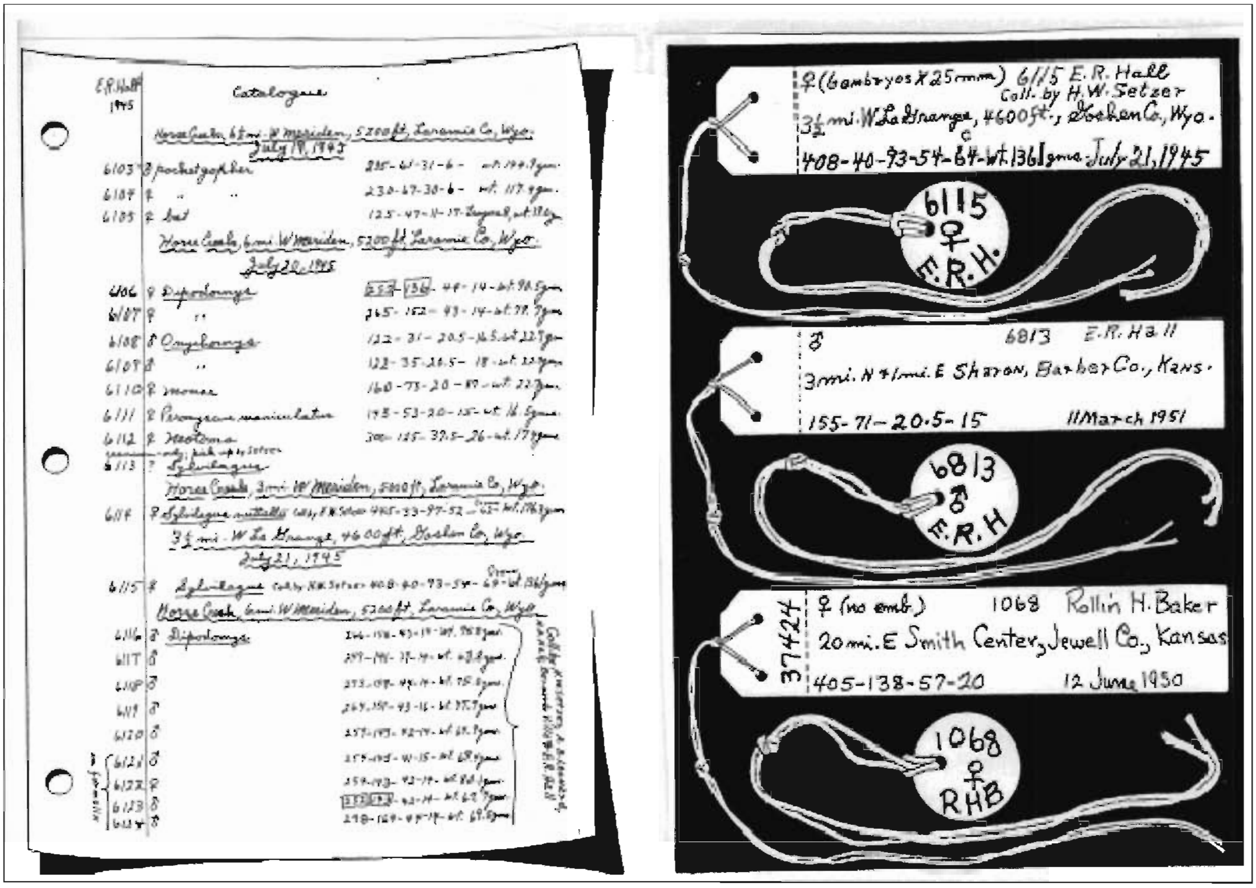

Figure 12. Examples of field cataloging and specimen labeling typical of field mammalogy.

allowed an observer to see the intricate and complex coat color patterns and to feel the texture and type(s) of hair or fur characteristic of a given species. Such details were essential for making comparisons among species; descriptions of hair colors were even standardized by using a rare book of color chips (Ridgeway, 1912) intended originally for use by ornithologists.

Hall's collection and preservation guidelines in his *Handbook of Mammals of Kansas* make no mention of preserving mammal specimens in fixatives. This was not an oversight. Until the advent of molecular techniques, and attempts to take them into the field environment, it was unpopular to even consider preservation of specimens by any means other than skinning, stuffing, and drying. Indeed, although fixation of animal specimens in formalin was acceptable for reptile, amphibian, and fish collections, it was regarded as a poor alternative for mammals. As often is the case, the dried skin versus fixation of the body in a fluid preservative was seen as a trade-off among options. Although this was understandable, it also meant that examples of tissues from all of the organs of collected mammals were discarded in the field. The exceptions were few, and formalin was employed in the field only when there was not enough time to skin and stuff the specimens. Ironically, then, although most museums have collections of mammal specimens fixed in formalin, these specimens typically were too rotten or damaged to prepare as skins. So, most formalin-fixed specimens are of little or no value to basic microscopic studies. Oddly, however, their poor fixation sometimes makes them suitable for immunohistochemical research! Some (but surely not all) of these specimens also are usable for gross anatomical dissections.

Relatively few changes to the basic methodology of field collecting have occurred in the past 100 years. One of the most obvious was addition of latitude and longitude information obtained through satellite-based global positioning systems (GPS). Such systems became a standard field tool in the early 1990's, shortly after their introduction to the world of boat and airplane navigation and before the introduction of small, lightweight and low-cost units to hikers and others. The addition of reproductive information is another common modification. In females this typically includes whether or not the specimen was pregnant or lactating and if pregnant the label would include data on the size, number, and location in uterine horns of embryos or fetuses. For males, it became commonplace to include a note about the testes—enlarged and scrotal as they are in the breeding season, or not descended as they often are in non-breeding males.

In the modern era (post-1960, to select an approximate date), field mammalogists greatly diversified the types of information obtained and recorded from each specimen (Fig. 13). This diversification followed the steady addition of technologies, beginning with the collection of blood for basic serology. The earliest example of serology conjoined with more traditional field mammalogy, at least so far as the authors are aware, took place in 1949. James Findley (*Mammalogical Reminisces*) describes participation in that fieldwork, which was conducted near Jackson Hole, Wyoming. This basic serology was replaced by protein electrophoresis, which in turn has given way to molecular genetics (DNA sequencing and isolation of mRNAs from tissues). C. J. Phillips, R. J. Baker, J. L. Patton, and A. L. Gardner provide detailed histories for the introduction and use of other technologies, such as karyotyping, tissue culture, histology and histochemistry, and ultrastructure in their respective autobiographical chapters.

Figure 13. Stephen L. Williams (left) prepares traditional museum mammal specimens, while Mike Arnold (right) does tissue culture in a remote camp in Suriname (1981 Tafelberg Expedition).

Protocols had to be developed for labeling and numbering samples of organs fixed for microscopy or field frozen for molecular biological research. It is not unusual for such samples to require as many as 20 small sample vials, and these need to be accurately linked to the mammal specimen from which they were acquired. At Texas Tech University, Robert Baker developed a revised cataloging book with its own number system (called the TK system) that could be linked to both the collector's personal number and the specimen itself. For efficiency, Baker and his colleagues also pioneered using a bar-code system so that data could quickly and accurately be computerized at the museum. Field karyotyping (preparation of microscopic slides with preserved chromosomes) and tissue culturing were two additional complications that appeared in the mid-1960's. Once again, systems had to be invented for recording the data in ways that it could be linked to individual specimens.

Overall, it was the expansion of data collection that really made the typical museum specimen into a true voucher specimen. In Figure 14 we present a complex-looking diagram that summarizes the huge variety of roles museum mammal specimens play in modern research. The individual preserved specimen is the centerpiece because once it has been accessioned into the museum, catalogued into a permanent database, and safely stored in an appropriate cabinet, it literally becomes a voucher specimen. They are, in short, the physical evidence that gives future research its potential for repeatability (see Baker et al., 1999 for an explanation of voucher specimens in the context of modern biological informatics).

Our goals in writing this introductory chapter were to explain the academic and field origins and the scientific paradigm of North American mammalogy and, in doing so, to explain the 'breed' we call field mammalogists. Hopefully we have accomplished our task, and we invite readers to enjoy the following collection of autobiographies.

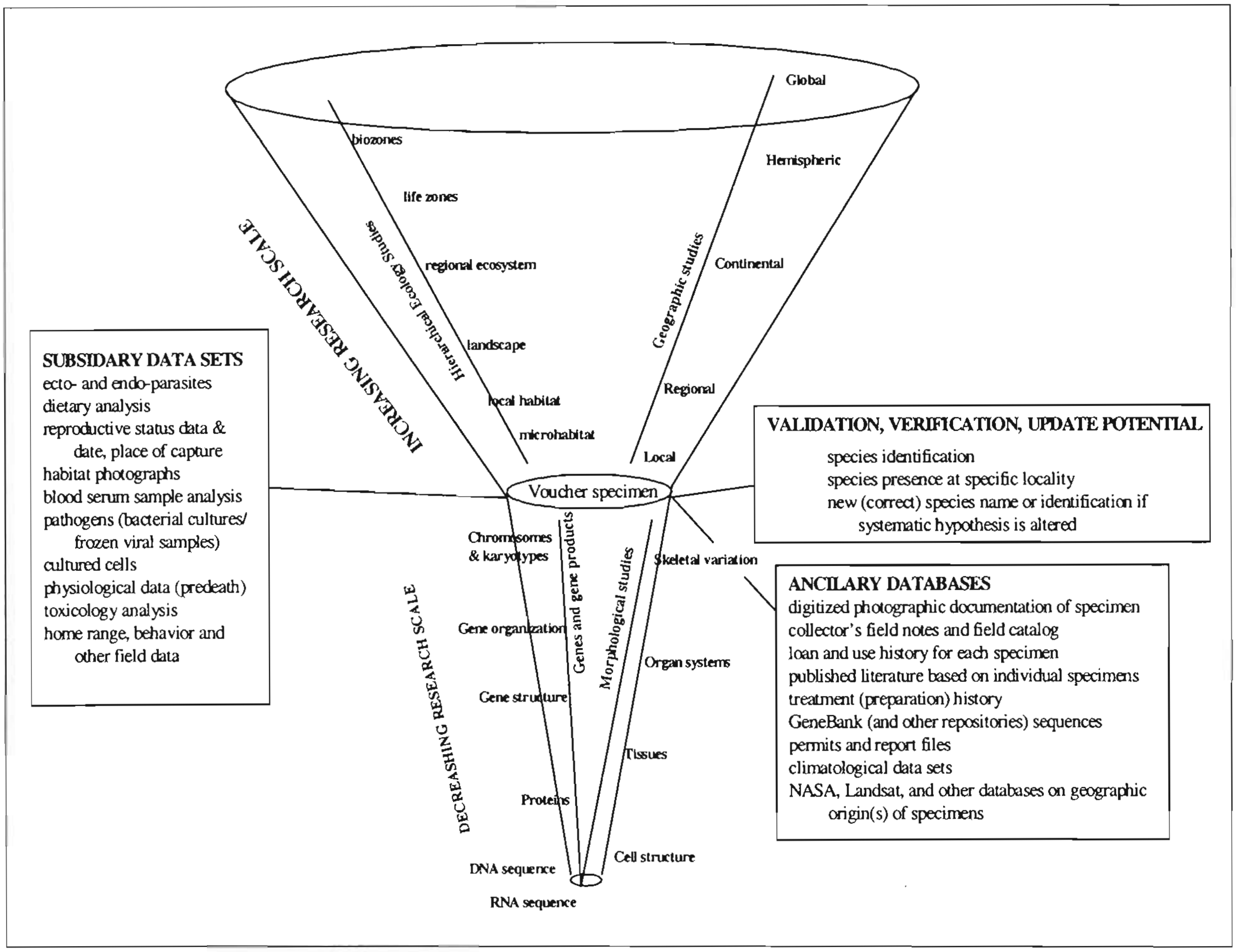

Figure 14. The modern museum mammal voucher specimen. This diagram illustrates the relationship of the specimen as a permanently stored physical representation of a once-living animal and the multitude of microscale and macroscale types of science that now can be based upon it (figure by C.J. Phillips and R. J. Baker).

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A PERSONAL SIXTY-YEAR RETROSPECTIVE OF MAMMALOGY

DONALD F. HOFFMEISTER

Donald F. Hoffmeister was born in San Bernardino, California on 21 March 1916. He attended San Bernardino Junior College for two years and then received his B. A. from the University of California-Berkeley in 1938. He also earned his M. A. and Ph.D. in Zoology in 1940 and 1944, respectively, from the University of California-Berkeley. He is Director Emeritus of the Museum of Natural History and Professor Emeritus of Biological Sciences at the University of Illinois.

I am perhaps the only one here who can reflect on the past 85 years. Being in the 80's has many drawbacks, but it certainly has some advantages. For example, if you forget your name or somebody else's name, or you recognize the face but not the name, or forget an appointment, or a telephone number, or promise to be in three places at the same time, just say you are in the wonderful 80's. If I have some facts mixed up or incorrect in this account, remember I am even in my 85's! Nevertheless, I will venture to reflect about mammalogy as I have known it for these past years. It is even more difficult to predict the next 30 years.

In my early years in southern California, my parents had me slated for a medical college education, following in the footsteps of my paternal grandfather. However, my father, an astute observer of nature, demonstrated to me how the talons of a hawk's legs open and release, how the lid of a trap-door spider's burrow works, how to make a figure-four box trap for catching squirrels. When in junior college I had a course from Dr. Elton R. Edge, who had just finished a summer session in Nevada with E. Raymond Hall's group collecting mammals. Edge introduced me to the art of making study skins and cleaning skulls. Soon I had a bag of Museum Specials [the standard mouse-sized snap-kill-trap designed for collecting specimen with minimal skeletal damage] and my dad eagerly accompanied me in setting them. He was intrigued with my preparation of study skins. Unfortunately, my father died when I was still a teenager.

To get a bachelor's degree I went off to the University of California at Berkeley. At the end of my senior year in 1938, I was one of 58 in the State of California accepted to the UC Medical School. At the same time I was accepted to graduate school in zoology. After much soul-searching the latter was chosen. My course in Natural History of the Vertebrates was the major reason for choosing graduate school as well as being especially impressed with E. Raymond Hall. This same summer of 1938, I attended my first annual meeting of the American Society of Mammalogists in Berkeley, as well as a chance to see C. Hart Merriam. Graduate years at Berkeley were pleasant but busy, in part because I was newly married and because my fellow graduate students were falling to the wayside as they sought advanced degrees. Dr. Hall helped fill the void of a father.

It is interesting to note that in the many years I was in zoology at Berkeley no course in mammalogy was given. There may have been a course listed as Economic Mammalogy, taught by Seth Benson, but no one to my knowledge took the course. During my stay at Berkeley, many talented students were there, including Harvey Fisher, Starker Leopold, David Johnson, Floyd Durham, Fred Dale, and Frank Pitelka. During this time, I helped Dr. Hall with his preparation of the *Mammals of Nevada*, his manuscript on weasels, the preparation of Bill Davis' *Mammals of Idaho* for publication, and other projects.

As an aside, let me reflect about MVZ [Museum of Vertebrate Zoology, UC Berkeley]. In the early 1900's, students working on or with mammals or birds got their advanced degrees under Charles Kofoid, head of the Department of Zoology. Joseph Grinnell may have helped the students on their research, but otherwise did not enter into the project. For example, William Burt, Harold Bryant, Tracy Storer, and others got their directions and degree from Kofoid. When E. R. Hall graduated from the University of Kansas he was attracted by Grinnell and went to Berkeley. He discovered that his work was to be with Kofoid. Because of some epidemic, perhaps plague, that invaded the ground squirrels in the Berkeley Hills, resulting in some human deaths, hundreds of squirrels were killed. Their skulls provided material for Hall's master's degree under Kofoid. Shortly thereafter, probably because of the increased financial support for the Museum by Annie M. Alexander, there resulted a greater separation of Museum and Department.

Because of this situation, museumites were concerned about people from the Department moving into quarters in the Museum. This was especially true when Alden Miller from the Department made such a move about the time of Joseph Grinnell's death. When the University was about to seek a new director for the Museum, E. R. Hall went to the University President to plead his concern. When it was obvious in 1944 that the President was about to appoint Alden Miller, Hall, with contract in hand from The University of Kansas to become Director of the Museum and Head of the Department of Zoology, publicly resigned from the University of California and took the positions at Kansas.

With the completion of a Ph.D. in 1944, I accepted a position at the University of Kansas as Assistant Professor and Assistant Curator of Modern Vertebrates on a twelve-month appointment of $2,000 per year. No course in mammalogy was being offered, so I taught comparative anatomy and assisted in a course in higher vertebrates. Among our students during my two-year tenure at Kansas were Walter Dalquest, Bernardo Villa, E. L. Cockrum, Stephen Durrant, George Lowery, William Jameson, J. Moojen, and George Rinker. In 1945, I attended the American Society of Mammalogists (ASM) meeting in Pittsburgh and then never missed an ASM meeting for the next 40 years. Incidentally, my wife Helen accompanied me on all of these.

In 1946, I accepted a position in the Department of Zoology at the University of Illinois as an Assistant Professor and Assistant Curator (=Assistant Director) of the Museum of Natural History through the approval of Carl Hartman. This was to fill the line-item in the budget created by the retirement of Dr. Victor Shelford and the forthcoming retirement of Dr. L. A. Adams in the Museum. In 1946 there were courses in Ecology and in Wildlife Management, but none in mammalogy at the University of Illinois. In 1947, my outline for a course in Mammalogy was accepted and the course started. In 1948, I became Director of the Museum.

It is interesting to note that during my stay at Berkeley and at Kansas, no course in mammalogy was given and none was in place when I came to the University of Illinois.

During my 36 years at Illinois, field trips were taken during the year, and especially in the summer, either in Illinois or the Southwest. The students had the opportunity to be associated with an outstanding naturalist, Woodrow Goodpaster. He accompanied us on nearly all of our field trips. His skills, energy, and enthusiasm impressed and stimulated the students. During the school year, graduate students worked with the museum collections and learned first-hand curatorial methods and procedures. In many cases, summer field work provided the background and materials for their research projects. In all, 14 students received their Ph.D. and 18 their Master's degree [under Hoffmesiter's direction].

It might be interesting to view the "ologies" at the University of Illinois. Mammalogy was not restarted after my retirement in 1983 until 1992. Herpetology, which was given by Hobart Smith from 1949 to 1970, was not restarted until about 1995. Ichthyology was taught for a short time around 1950 and restarted for a short time in the 1980's. Ornithology was taught more or less continuously from 1950 by a variety of people, including S. C. Kendeigh, Mary Wilson, James Carr, and Scott Robinson. It is obvious that the "ologies" have had varied experiences for the past 54 years at the University of Illinois. Even the Department of Zoology has had a varied existence from Zoology to Ecology, Ethology, and Evolution, in the School of Life Sciences, to Animal Biology, in the School of Integrative Biology. In spite of these changes the "ologies" are currently in popular demand, with students waiting to get enrolled therein.

As an aside, I attempted to popularize mammals when I agreed with Dr. Herbert Zim, a colleague of mine in the College of Education, to prepare a Golden Book on Mammals. It obviously reached a good many young people as well as others since over 2.8 million copies were sold.

Now, let us look at mammalogy for the next many years. Mammalogy at the University of Illinois will never be taught like I taught it. Remember, this is said by an 85 year old, with tongue in cheek. This is not meant to belittle the way it is currently taught because students are eagerly waiting to take the course.

No longer can students take a bag of Museum Specials to determine for themselves what mammals may be present in a certain area. Instead, they use live traps, but not with the intention of killing the animals therein. They cannot mark mammals by toe or ear clipping, or shoot bats or squirrels. How will they learn about the ecological restrictions of closely related species unless they can examine their skulls? How will they know for certainty if the subadult vole in their live trap is a *Microtus ochrogaster* or a *Microtus pennsylvanicus* unless they examine some cranial features? If they discover a small shrew in eastern Illinois, how will they know whether it is *Sorex fumeus*, which is only known as close as Western Indiana, unless they examine the skull? And, there is no need for them to develop the skill of preparing study skins or the technique of cleaning skulls. However, here in Illinois such restrictions are being imposed on such methodology by the animal care associates and the animal behaviorists. I trust that it is not like this elsewhere in the United States.

One cannot help but appreciate the advances that have been made in the understanding of the relationships of mammals by molecular systematists and cytogeneticists. However, it seems to me that they need to preserve some of the mammals on which these studies are based. Can they do this because of restrictions imposed?

In closing, let me say that mammals and mammalogy are here to stay. May the many journals for publishing about mammals, as well as the American Society of Mammalogists, flourish. And last, but foremost, my sincere thanks and gratitude to my many students who assisted me in many ways.

WHERE HAVE YOU GONE VERNON BAILEY: THE LAMENTS OF A MAMMALOGIST

DAVID J. SCHMIDLY

David J. Schmidly was born in Lubbock, Texas, on 20 December 1943. He received his B.A. (Biology) and M.S. (Zoology) degrees from Texas Technological College (now Texas Tech University) in 1966 and 1968, respectively, and his Ph.D. in Zoology from the University of Illinois, Champaign-Urbana, in 1971. His major professor for his doctorial degree was Donald F. Hoffmeister. Dr. Schmidly previously served as President of Texas Tech University and currently serves as System CEO and President of Oklahoma State University.

In preparing this manuscript, it dawned on me that my career as a mammalogist is coming to an end. It seems incredible a career that started in 1962 and lasted over 40 years, is now in its twilight stages. It is human nature to lament as you end a career and become nostalgic. Participation in this symposium has stimulated these feelings in me.

While reflecting, lamenting, and thinking about my career as a mammalogist, I was reminded of a song from my favorite movie in the 1960s, about the time I began to think of myself as a mammalogist. The movie "The Graduate" starring Dustin Hoffman included a song ("Mrs. Robinson"), sung by Simon and Garfunkel, with these famous lyrics about the great baseball player Joe DiMaggio:

> "Where have you gone Joe DiMaggio
> A nation turns its lonely eyes to you (woo, woo, woo)
> What's that you say, Mrs. Robinson
> Joltin' Joe has left and gone away
> (Hey, hey, hey ..... hey, hey)"

The lyrics refer to the passing of an era when things would never be the same again. In this context, for old mammalogists like me, the lyrics could easily be re-phrased as follows:

> "Where have you gone [pick your favorite old mammalogist]
> A profession turns its lonely eyes to you
> (woo, woo, woo)
> What's that you say,
> Joltin' [whoever] has left and gone away
> (Hey, hey, hey ..... hey, hey)"

My favorite mammalogist was always Vernon Bailey. Bailey had been recruited and trained by C. Hart Merriam to work as a field agent for the United States Biological Survey. One of his first assignments was to work in Texas, and in 1905 he published the first definitive account of the mammals of the State (Bailey, 1905). Subsequently, I had the opportunity to write a history of Bailey's work in Texas, and I had the pleasure to read all of his field notes (Schmidly, 2002). I was taken by the meticulous quality and vivid descriptions of Texas landscapes and mammals that his work provided, and I developed a fondness for the man much like I did for Joe DiMaggio in the twilight of his baseball career.

This essay describes my life and career, including many of my best memories, my interpretation and explanation about what happened, some of my laments about the past, and some thoughts and insights about the future. I was fortunate to work in a time when it was relatively easy to do field work, but my career pales in comparison to the legacy of Bailey and the early mammalian naturalists.

I was born in Lubbock, Texas, on December 20, 1943, the first son of Henry J. (Chick) Schmidly and Faye Norvell Akin. My father was in the European theater of WW II so I lived with my mother and paternal grandparents on a cotton farm 9 miles north of Levelland, Hockley County, Texas. I was especially fond of my grandfather who was an immigrant from Switzerland. When my father returned from the war, we built a small frame house on the farm and took up residence there. My father became a farmer. Six years later my brother, Steve, was born.

I was raised in a very practical, hands-on environment. We didn't have television or a telephone so I spent a tremendous amount of time outdoors playing sports (baseball and basketball were my favorites), and as soon as I was old enough, working on the farm. Very early in life I learned the value of hard work and responsibility. There was no other way to be successful on a farm. I also learned to love and appreciate the land and landscapes. Our farms had been converted from short-grass prairie and we had a large pasture that was never cultivated. It supported populations of small birds, rabbits, ground squirrels, horned toads, and various lizards. As a young boy I loved to hunt with my BB gun and later a 22 rifle. This was my early introduction to wildlife.

As a young boy I loved sports and competition. I participated in both the marching and concert bands and learned to play the piano, coronet, baritone, and French horn. I loved classical music. I also learned to enjoy poetry. My English teacher insisted that I participate in the University Scholastic League (UIL) poetry-reading contest and I won first place. So competing and winning, either in sports or the humanities, were high priorities in my youth.

There was nothing particularly notable about my early years in school. I had very good teachers and had to work hard to succeed. I was a good, not a great student, but my parents absolutely insisted that I do well in school. Neither one of them was educated so it was a high priority for them for my brother and me to get a good education. My mother also insisted that I learn how to type. So, I was one of the few young men who took typing in high school. This would prove to be a valuable experience for my college and professional development.

In 1962, I graduated from Levelland High School and entered Texas Technological College, living in a dormitory and successfully trying out for the freshman basketball and baseball teams. I didn't have a clue about my academic interests and my transcript proved it with one notable exception – freshman zoology. The very first college class I attended was an introductory class taught by Dr. Robert L. Packard, a new member of the faculty who had trained as a mammalogist under E. Raymond Hall at the University of Kansas. Dr. Packard had recently joined the Texas Tech faculty from Stephen F. Austin University in Nacogdoches, Texas, and freshman zoology was his first teaching assignment. I enjoyed Packard's methodically prepared and well-structured lectures, and I loved the subject matter. It seemed as if I had found something I enjoyed and could do well.

Initially I thought I wanted to be a pre-med major and go to medical school, but I did not enjoy or do well in chemistry. Packard counseled me and suggested that I major in biology. I also had to select a minor and choose geology because I really enjoyed the land and land formations. So, my academic interests were grounded in biology and field geology.

As a sophomore in college, two important events occurred that would shape my life and career. I met my future wife, Janet Elaine Knox, on a blind date and I enrolled in a two-semester course in Comparative

Figure 1. David Schimdly (right) in 1966 as a young master's student collecting kangaroo rats (*Dipodomys ordii*) near Kermit, Winkler County, Texas, with his thesis advisor Robert L. Packard of Texas Tech University.

Anatomy taught by Packard. Janet and I began to spend lots of time in the library and my grades improved dramatically. And I couldn't get enough of comparative anatomy. I enjoyed the subject, the detail, the framework of comparative phylogeny and the concept that you could see how things were related in time and function. For the first time in my life I began to think about evolution. I also developed a close relationship with Dr. Packard and began to trust him as an advisor.

Packard had received a small NSF grant to study rodent ecology, and he invited me to work for him at Kermit, Winkler County, Texas, in the summer of 1964. I had to get my father's permission because he expected me to help out with farm work in the summers. Needless to say, he found it weird that I wanted to spend the summer trapping rats, but true to his good nature, he laughed and said go ahead.

So, I moved to Kermit and lived in a shack for which Packard paid $1 a year in rent. I lived there with several graduate students of Dr. Don Tinkle, a well known population biologist and herpetologist, who was studying the side-blotched lizard, *Uta stansberiana*. I spent my days noosing lizards, my afternoons setting rat traps for kangaroo rats (*Dipodomys ordii*) and silky pocket mice (*Perognathus flavescens*), and my mornings toe-clipping and releasing my catch. Packard also taught me how to prepare study skins and keep a field catalog.

I spent my evenings drinking beer with Tinkle's graduate students, Gary Hodenbach, Norman Williams, and Hector Cuellar, at a local watering hole known as the 'Elbow Room.' We drank beer, played shuffleboard, and discussed natural history. Professors Tinkle and Packard would join us every month and the conversations were even more stimulating. Packard began to encourage me to read journal articles and to think critically while reading them. He also allowed me to examine his copies of the *Journal of Mammalogy*. My interest in mammals and natural history was now in full bloom.

The following year, my junior year, was different. I took courses in plant ecology and taxonomy, cell and animal physiology, invertebrate biology, and two courses in invertebrate paleontology. While I enjoyed these, especially the paleontology, they were nothing like Packard's comparative anatomy course. In the Spring semester of 1965, Packard invited me to join him and his students on a field trip to the Big Bend region of Texas, camping at the Black Gap Wildlife Management Area, Brewster County, just east of Big Bend National Park.

I was amazed at the diversity of mammals and loved every minute of the field trip, especially preparing study skins. We experienced a flash flood that almost caught us by surprise, and I really got excited about the adventure of field trips. When I returned from this trip I was more convinced than ever that I would pursue a career in zoology and specifically mammalogy.

The highlights of my senior year were getting engaged to Janet and participating in the recruitment of a new young faculty member to the biology department, Robert J. Baker. Janet was getting a degree in elementary education and offered to teach and support us while I attended graduate school. She encouraged me to think seriously about graduate school and to get a Ph.D. and pursue teaching and research.

In my last semester, I enrolled in a course in natural history taught by Dr. Packard. This was the academic highlight of my undergraduate career. In this course, we were obligated to maintain a field journal, species accounts, and a specimen catalog according to a system established by Joseph Grinnell, the founder of the Museum of Vertebrate Zoology at the University of California, Berkeley, and we took many field trips on the Llano Estacado around Lubbock. I graduated in May, 1966, and on June 2 Janet and I were married. Our first job was to run a summer recreation park in Lubbock where I learned to play croquet, a game I still enjoy today.

Dr. Packard extended an offer for me to be part of his graduate program and work on my master's degree. I gladly accepted, including the offer of a teaching assistantship, and began my work in September, 1966. After a semester of much discussion, it was decided that my thesis would focus on a study of population variation in local populations of Ord's kangaroo rat (*Dipodomys ordii*). I set up collecting sites in four counties on the High Plains (Canadian, Bailey, Borden, and Winkler counties), and Janet and I spent many weekends collecting k-rats for my work. [K-rats is a mammalogist's slang term for kangaroo rats of the genus *Dipodomys*]. The goal was to have 100 specimens from each site, representing all aspects of the population structure. Packard had me prepare the specimens and introduced me to the concept of curating and cataloging specimens.

In my first year of graduate school, I took an animal systematics class from Dr. Jack Mechum, a herpetologist who had replaced Dr. Tinkle on the faculty. The textbook for the course was Earnst Mayr's *Animal Species and Evolution* (Mayr, 1963). This was the highlight of my graduate course work. I read every chapter in that book and outlined the theories and concepts. I began to understand the broader application of my own research on population variation, and I became very interested in the topic of geographic variation.

I began to experience the close relationship and friendship of my fellow graduate students. Packard had another graduate student, Frank Judd, who was working on the systematics of *Peromyscus maniculatus* [deer mice] in West Texas, and we became good friends, so close we endured the trauma of a student murdering a janitor in one of the labs where we taught. I spent more time with two other professors, Bob Baker and Francis Rose. Both were young faculty members and had an abiding interest in science and fieldwork. I tried to spend as much time around them as I could.

In the spring of 1967, Bob Baker invited me to join him and his students on a field trip to México. This was to be the first such experience in my life and ignited a passion for that country that continues to this day. We left Lubbock, drove west to Tucson, Arizona, and crossed the border at Nogales, Sonora. Our first stop was a place in Sonora called Alamos, and here for the first time I netted bats and learned about preparing karyotypes in the field. We spent the entire trip collecting and karyotyping bats, rodents, and pocket gophers. I met Al Gardner and Jim Patton, two young men who had been graduate student colleagues of Baker at the University of Arizona.

Upon returning to Lubbock, it was time to complete my thesis. Packard had taught me how to use calipers and take skull measurements as a way of assessing population variation in my samples of k-rats. While many of my friends thought this was going to be boring, I loved every minute of the work and developed a very large database. Then, I began to think what to do with it. Most of the students in the department analyzed their data using a Monroe Calculator, meticulously punching in the numbers character by character and then calculating means, standard deviations, t-tests, and other such calculations. This I did find boring, and it seemed to me like it would take forever to analyze the large set of data I had.

Figure 2. Typical behavior of an exuberant mammalogist enjoying field work in Tamaulipas, Mexico, in 1977.

About this time, I met another graduate student, Don Willard, who was studying the taxonomy of mites with Dr. Russell Strandtman in the Biology Department. Willard had learned how to use the new mainframe IBM computer that Texas Tech had purchased, and he offered to teach me to use it for my data analysis. I punched my data onto computer cards and submitted boxes of cards with computer instructions on the front and ran them through programs in the Statistical Analysis System (SAS) package. I will never forget receiving my first data printout. I thought I had "died and gone to heaven" as we used to say in West Texas. In one night, I had all of the data and statistics to interpret variation in my samples. I began to study journal articles about using computers and statistics to study geographic variation. I became fascinated with the world of biometry.

As I began to complete my thesis, Packard urged me to think about three things – presenting a paper at the 1968 mammal meetings in Ft. Collins, Colorado; publishing my thesis work in the *Journal of Mammalogy*; and finding a place to do my Ph.D. work. I listened and began to work on all three areas.

In June 1968, I drove with Packard to Ft. Collins feeling very insecure. I was going to give a presentation of my work in front of all of these mammalogists I had read about but had never seen or met. Upon arrival, I was introduced to a who's-who of mammalogy; E. Raymond Hall, Steve Durrant, William B. Davis, Emmett Hooper, Donald Hoffmeister, J. Knox Jones, Bill Lidicker, Jr., Karl Koopman, Syd Anderson, Jim Findley, Dilford Carter, and Raymond Lee just to mention a few. I also met some of the other young and up-coming mammalogists, the so-called "young turks", Jerry Choate, Hugh Genoways, Carl Phillips, Elmer Birney, Earl Zimmerman, and Don Wilson to name just a few. I gave my paper, which wasn't very good, although many people encouraged me to continue my work. I left those meetings on a real high, knowing just what I wanted to do.

By this time, I had become fascinated by the challenging problems of deer mouse taxonomy. I had read some of the work of Hooper and Hoffmeister and decided this would be an interesting group to study. I applied to the University of Illinois in 1968 and was accepted to the Ph.D. program. I was to be supported as a teaching assistant and to have an office in the Museum of Natural History, with Dr. Donald F. Hoffmeister as my advisor. I learned before leaving for Illinois that Janet was pregnant with our first child.

So, Janet and I set off for Champaign/Urbana, Illinois, and our first experience of living outside of Texas. I will never forget seeing the mammal collection at the Museum of Natural History. It included almost 70,000 specimens and many of them prepared by one man, Woodrow Goodpasture, one of the last of a dying breed – the professional collector.

I occupied an office in the Museum, met my fellow office mates (Robert Wrigley and David Wright), and went to work on my Ph.D. degree, all the while wondering if I would lose my student deferment and be drafted into the armed forces for the Vietnam conflict. But that never happened and I was allowed to finish my work. Dr. Hoffmeister and I decided I would work on the white-ankled mouse, *Peromyscus pectoralis*. My work would involve the systematics and natural history of the species. But in order to work that out, I realized that I also would have to study a related species, *P. boylii*, and its various forms. The good news was that *pectoralis* and *boylii* occurred over much of Texas and México, the two places where I loved to do fieldwork.

Dr. Hoffmeister was an excellent mentor and teacher. He was extremely disciplined and meticulous. I officed at a small desk in a Museum room that housed the mammal collection. My desk was only a few feet from his office. Doc, as we called him, required that all of his students learn the proper methods of collection management. The mammal collection was extremely well curated and I had to learn the proper procedures for collection care (e.g., nothing, and I mean nothing, could be left on a museum counter over night!). Doc was also an excellent administrator. Little did I know at the time, but his careful and organized approach to administration would pay off later in my career. I could not have had a better overall mentor than Dr. Hoffmeister, and I am forever grateful to his interest and commitment to develop my talents.

There was another mammalogist at Illinois who was to become my very good friend and who had a profound impact on my career. His name was M. Raymond Lee, or Ray as I called him. Ray had worked earlier in the University of Illinois Museum of Natural History with Hoffmeister and prior to that had been a field collector for Hall at the University of Kansas. He had been a student of Steve Durrant at Utah where he received his Ph.D. A few years before my arrival at Illinois, Ray had left the natural history museum to establish a karyotype lab in the Zoology Department.

He and his Ph.D. student, Earl Zimmerman [now a professor of biological sciences at The University of North Texas], were using karyotypes to study mammalian taxonomy and systematics. Ray and Earl urged me to work karyology into my dissertation work with *Peromyscus*.

There weren't many specimens of *Peromyscus pectoralis* in collections so Hoffmeister hired Goodpasture to collect in Mexico and Texas. Upon returning to Cinncinatti after two months in the field, he called and told me to drive over and get the specimens. Janet and I complied and we were amazed to see the results. Woody had collected over 1,000 specimens of *Peromyscus* in just over 60 days and the specimen preparations were the best I had ever seen. Also, the field notes read like a book. All of this had been done by a man with no formal education in natural history and mammalogy. Boy, was I impressed!!

Hoffmeister insisted I begin to learn more about the history of mammalogy. He had written a history for the ASM and was the historian for the society. His writings introduced me to such people as C. Hart Merriam, Vernon Bailey, Wilford Osgood, and the many people associated with the U.S. Biological Survey. For the first time I gained a real sense for the rich history of mammalogy. I also saved enough money to acquire copies of *The Biological Survey of Texas* (Bailey, 1905) and the *Revision of the Genus Peromyscus* (Osgood, 1909).

While conducting my doctoral studies, I spent my summers collecting deer mice in Texas and Mexico. At the urging of Ray Lee, I switched from using Museum Special traps to Sherman live traps so that I could get karyotype information from each specimen. It was exciting when I discovered significant chromosomal differences in species and populations, and it became clear to me that karyology would be a relevant tool for further exploration of *Peromyscus* taxonomy and systematics.

After passing my comprehensive exams, I set about working full time on my dissertation. I took a couple of courses in biometrics and became very interested in multivariate statistics as a way of structuring and understanding patterns of morphological variation. This was in the hey-day of numerical taxonomy, and while I didn't buy into that philosophy, I was more than happy to take advantage of the computer algorithms. I also became very good at drawing "ball and stick" three dimensional models of variation.

On a personal note, Janet gave birth to our first child, a little girl we named Katherine Elaine. Also, Hoffmeister and Lee urged me to start preparing for the job market. Things didn't look particularly good in that regard. Already biology departments around the country were starting to abandon organismic approaches in favor of cellular and molecular studies. There were only a handful of academic programs in wildlife biology and most of them focused on wildlife management and game species as opposed to natural history. Most of the young mammalogists in my day were seeking museum jobs and there were very few of those around.

After completing and defending my dissertation, I had interviews at the Museum of Natural History at the University of Kansas and at the Field Museum of Natural History in Chicago. I was offered a job at the Field Museum in Chicago and given a few days to think it over. In the meantime, I had heard about an opening at Texas A&M University to teach mammalogy and curate the mammal collection in the Department of Wildlife and Fisheries Sciences. I called to inquire and was told the interview process was already underway and there was no opportunity to be considered. I called the Department Head, Jim Teer, and told him that I was so interested I would pay my own way to College Station if they would just agree to talk with me. He said okay, and I borrowed the money for a plane ticket from Urbana to Dallas and used Janet's parents' car to drive to College Station. I really hit it off with Teer and most of the wildlife faculty, and a few days later they offered me the job at what I thought at the time was the staggering amount of $13,100 for a 12 month appointment to teach and set up a research project with the Texas Agricultural Experiment Station. I accepted the position and called the Field Museum and declined their offer.

Now my graduate experience was completed and I was ready to begin my career. In retrospect, I had learned a lot – the museum and field biology approach to natural history (what most people would call the Merriam/Grinnell approach), laboratory work and

Figure 3. David Schmidly (right) with graduate student Tim Houseal (left) talking with an indigenous tribal leader while conducting field studies near Ocota, Nayarit, Mexico, circa 1984.

modern genetics (what most people today would call the Baker/Patton approach), the theory of evolution and systematics (heavily oriented toward the Ernst Mayr philosophy), the use of numerical and statistical approaches in analyzing complex patterns of geographic variation (modeled after the work of Robert Sokal), the value of partnerships and collaboration, and very importantly the value of initiative, bold action, and hard work. I felt I was ready for the real world of academia.

My academic career started in August 1971 as a young assistant professor in the Department of Wildlife and Fisheries Sciences (WFSC) at Texas A&M University. About a year after moving to College Station, Janet gave birth to our second child, Brian James. Janet resumed her teaching career in College Station, which lasted until her retirement in 1996.

Holding a faculty appointment in the WFSC Department turned out to be the biggest break in my career. A&M was a land-grant school, with research, teaching, and extension programs. I was hired to teach mammalogy (undergraduate and graduate), to start a research program on Texas mammals, and to curate the Texas Cooperative Wildlife Collection of Mammals. For me it was the perfect position. The entire focus of the curriculum was on organisms. The departmental curriculum included all of the "ology" courses, and the mammal collection was the largest in Texas and one of the 15 largest in the country.

While my interest heretofore had been strictly in systematics and natural history, now I began to think in terms of wildlife management and conservation. I started to learn about land use and began to realize the critical importance of landscapes and habitat in natural history. Even more important, I began to see the application of my work in natural history on conservation. The Endangered Species Act (ESA) had been passed by Congress in 1973 and concern was developing about human impacts threatening wildlife populations. I started a project, "The Mammals of Texas," that was sponsored by the WFSC Department, the College of Agriculture, and the Texas Agricultural Experiment Station. I received funding from agencies such as the Texas Parks and Wildlife Department, the National Park Service, and the U.S. Fish and Wildlife Service. My research involved both applied and basic science, management and biology, and was mostly field based, although I continued to incorporate karyology in my systematics work. Most of my undergraduate and graduate students were like me – they came from farm and ranch backgrounds. They were outdoors oriented and they loved fieldwork. We took field trips all over Texas and Mexico, collecting and documenting natural history data about mammals.

Three individuals really contributed to my early success at A&M. William B. "Doc" Davis, who started the WFSC Department in 1937, had recently retired from the faculty, but he came to the collection to work every day. He was authoring a book about the bats of Middle America. Doc was a legendary mammalogist, shrewd business man, and past President of ASM. He was an expert on Texas mammals and new world bats. He also was a very good writer and editor. He taught me much over the years and was a wonderful mentor. I always said that working around Doc Davis was like getting two Ph.D. degrees.

Another person of great influence in my career was Jim Teer, the WFSC Department Head. Jim took a chance and hired me. He also mentored me and help me avoid some serious mistakes early in my career. He kept me headed in the right direction. Jim left the Department a few years after I joined the faculty to take over as Director of the Welder Wildlife Refuge near Sinton, Texas, but he has remained a friend and supporter throughout my career.

Jim Dixon, the herpetologist at A&M, became both a close friend and a mentor. He had worked for years all over Mexico, and got me interested in expanding my work there. In particular, Jim and I began to collect and work in the state of Queretaro. I loved fieldwork in México and became even more fascinated by that country and its mammal fauna.

As Texas A&M began to emerge as a major research university, the opportunity to hire additional faculty was presented. In WFSC, we were able to hire Dr. John Bickham, who had worked on his Ph.D. degree with Bob Baker at Texas Tech University. John began to do karyology and genetics work and apply his concepts to wildlife population management. John and I would go on to develop a close personal and professional relationship.

Similarly, the Biology Department at A&M hired Ira Greenbaum, another one of Baker's Ph.D. students [for more on Greenbaum, also see Phillips, Chapter 2: eds.]. Ira combined karyology with biochemical systematics. With the hiring of Greenbaum and Bickham, a major team of mammalogists was now assembled at A&M with the ability to address mammalian systematics and natural history from diverse perspectives and with different tools and technology.

While at Texas A&M, I affiliated with the Southwestern Association of Naturalists, or SWAN, which was dedicated to the study of southwestern natural history. I began to attend their meetings and was fortunate to ascend into leadership positions. In 1980 I was elected President of SWAN. I also had an opportunity to assume a leadership role in the American Society of Mammalogists (ASM), having been elected to the Board of Directors for 6 different terms. I was fortunate to be involved with the founding of the state mammal society, the Texas Society of Mammalogists (TSM), which was organized in 1981, and I was elected President of TSM in 1985. I would come to look forward every year to the SWAN, ASM, and TSM meetings.

In 1972, I had my first encounter with marine mammals. I was sitting in my office one day when the sheriff of Freeport, Texas, called and said there was a whale on the beach and he wanted to know if someone from A&M would like to come and get it. So, I asked my graduate student, Chester Martin, if he would like to accompany me to Freeport to secure this specimen. We drove to the beach and much to our amazement there was a huge crowd around the whale with TV cameras and reporters. We had arrived in a half-ton pickup. After the laughter subsided, I announced that we had really come to bury the whale, but when I returned to College Station later in the day, the TV news introduced me as the Aggie Professor who went to get a 10 ton whale in a half ton pickup!! Right then and there I vowed to learn more about marine mammals and so I applied for and received a grant from the recently appointed Marine Mammal Commission to establish the first Texas Marine Mammal Stranding Network. Using students and volunteers we combed Texas' beaches in search of stranded whales and dolphins. By doing this, we substantially increased the number of species known from the western Gulf of Mexico, and I acquired a real interest in marine mammals.

In 1974, I met a faculty member in the Department of Recreation and Parks at A&M by the name of Robert Ditton. We began to talk about the issue of human impact and carrying capacity in national parks. Our ideas caught the attention of the National Park Service, and they provided funding for us to work as an interdisciplinary team to assess human impacts and carrying capacity along the Rio Grande in Big Bend National Park. This was my first experience in interdisciplinary research and started a lifelong friendship with Bob Ditton. Together we published 5 papers on our collaborative work, and in the process I became an advocate for interdisciplinary research.

I published my first book in 1974, *The Mammals of the Trans-Pecos*. It was published by the Texas A&M University Press. I would go on to publish 7 other books but nothing will ever compare to the feeling I had when I saw the first one in hard cover. Mr. Frank Wardlaw, who founded the A&M Press and was widely recognized as one of the greatest university press leaders to ever live, encouraged me to publish in book format. I will forever be indebted to Frank and to Noel Parsons (Editor of the Texas A&M Press and current Director of the Texas Tech University Press). To this day, I have remained active in publishing at university presses.

In 1984, Ira Greenbaum and I submitted a joint proposal to The National Science Foundation to study the biosystematics of the *Peromyscus boylii* group in México. The proposal was funded and provided us with the opportunity to conduct fieldwork for 4 consecutive years in México. Our project combined classical skin and skull mammalogy with karyotypic and biochemical markers to ascertain population relationships of *P. boylii* in the highlands of central México. What we discovered was an interesting complex of cryptic species that could only be detected with the newer genetic techniques. An exceptional group of students, including Robert Bradley, Timmy Houseal, Ian Ensink, Livia Leon Paniagua, Gerardo Ceballas, Juan Carlos Morales, and Daniel Navarro, participated in this project. This was the most enjoyable field experience of my life with an outstanding group of students from both the United States and México. We collected multiple data sets on about 5,000 mice and are still publishing material from those expeditions even today.

The young Méxican mammalogists I had met were hell bent on organizing a mammal society in that country. They had been discouraged by the old guard, but they persisted and established the Méxican mammal society (Associacion Mexicana de Mastozoologia, A.C. –AMMAC). Shortly afterwards, they invited the American Society of Mammalogists (ASM) to a joint meeting in Cancun, México, in June 1987. They invited Michael Mares and me to assist in organizing the meeting that brought together almost 300 people who shared an interest in Latin American mammals. Michael and I edited a book from the proceedings of the meeting (Mares and Schmidly, 1991). AMMAC has been immensely successful and today mammalogy is alive and thriving in Mexico, and I am pleased to say that I had some small role in its success.

Throughout my tenure at Texas A&M, I was blessed with great students – in fact almost too good to be true. Altogether, I trained a total of 43 masters and Ph.D. students. I particularly liked to work with undergraduate and master's students and place them in the finest Ph.D. programs with people such as Robert Baker and Jim Patton. Baker and I developed a great personal relationship and have remained the best of friends.

In 1986 I was asked by the Dean of the College of Agriculture, Dr. Harry Kunkel, to serve as interim head of the Department of Wildlife and Fisheries Sciences. Our Department Head had stepped down and Harry asked me to do the job until we could conduct a national search for a replacement. Little did I know that this would take my career in an entirely different direction.

Following the national search, the faculty and administration asked me to serve as permanent department head and I accepted. Now it was official – I was an administrator and would be limited in the amount of work I could do in teaching and research. I thought it would only be for a few years, but it turned out to be the dominant trend of my career. And the truth is that I loved it. I fell in love with leadership and the idea of building programs and developing strategies to accomplish big goals. It wasn't the management or administration that turned me on, but the leadership opportunities.

It seems to me that many faculty look down on administrators, which I can understand. I didn't want to be that kind of administrator so I worked hard to make things better for the faculty. I also began to see the power of being in administration and how that could shape departments and professions.

While serving as department head of WFSC, I met Dr. William (Bill) Merrill, the President of Texas A&M University at Galveston (TAMUG). I told Bill that we could build a strong marine mammal program in Texas that would be the envy of the world, if we combined the resources of TAMUG and WFSC in College Station. He and I agreed that the Galveston Campus would be the perfect place to locate the program. Bill provided the resources for the faculty positions, and I agreed to provide them faculty appointments in WFSC so that they could recruit and train graduate students. This was the beginning of one of the largest and most successful graduate marine mammalogy programs in the world.

What I really enjoyed about administration was the opportunity to think strategically and to do strategic planning. I began to see this as my most important work, and I believe that my work in mammalogy and systematics really prepared me well for my administrative career. I learned to think holistically and long-range, learned the value of hard work and how to get along with people (just what I had been exposed to in field biology), and how to explain research and programs so that politicians and influential people could understand and appreciate them (some would call this the art of BS!!).

As much as I enjoyed teaching and research, I could see an opportunity to really excel in leadership and administration. There was no doubt in my mind that this was my calling in academic life.

My stint as WFSC Department Head was successful. The department grew in numbers and stature, the faculty increased in size, and the overall program progressed to the point where it was one of the finest wildlife and fisheries programs in the country. It represented a diverse array of scientific expertise and a focus on all aspects of wildlife diversity. WFSC faculty worked in wildlife management, systematics and evolutionary biology, natural history, teacher education, as well as policy and the human dimensions of conservation and natural resource management.

In 1991 I was appointed to the Board of Trustees of the Nature Conservancy of Texas. Eventually, I would be asked to serve as co-chair of the Conservation Committee and as a member of the Executive Committee. I have served on the Board for over a decade. I am convinced the approach of the Conservancy offers the best opportunity to protect and wisely use natural resources.

I served as department head for a little over five years. By this time, both of my children had graduated from high school and were in college. In 1992, I was asked to take the position of CEO and Campus Dean of Texas A&M's campus in Galveston (TAMUG). Formerly a stand-alone university, A&M had made a decision to merge the campus with the flagship university in College Station and to create the Texas Institute of Oceanography. I was asked to serve as the leader of the Galveston campus.

In January 1992, I moved from College Station to Galveston to assume the leadership position of TAMUG. This would prove to be one of the best administrative learning experiences of my career. While the Galveston Campus was small, it included the same complement of parts as any large university (dormitories, a library, buildings, auxiliary services, and it even had a 450 foot ship that served as a floating marine classroom). It was the perfect setting for higher education executive leadership. I had to learn the "ins and outs" of every aspect of the business operations.

While serving as CEO/Campus Dean of TAMUG, I had the opportunity to work with some of the best marine mammalogists in the world – Bill Evans [William, E. Evans; see following autobiography: eds.], Bernd Würsig, Randy Davis, and Graham Worthy. And we recruited some exceptional students – people such as the late Steve Leatherwood who entered graduate school having published more than 100 papers and a dozen books. He set a new standard for graduate student excellence. The list could go on and on. Some of the finest students in the world entered this graduate program. We also received a very large grant to survey and document the natural history of marine mammals in the Gulf of Mexico. A book about the

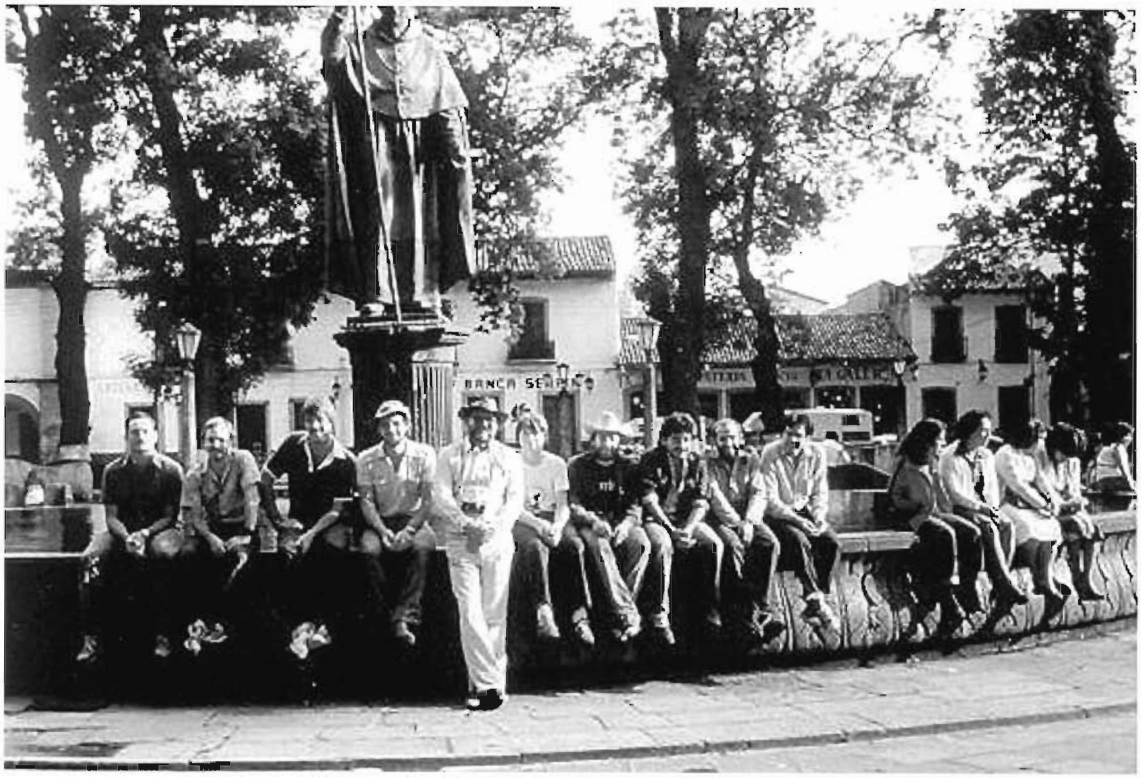

Figure 4. David Schmidly (center, standing) with a group of U.S. and Mexican students in the town of Patzcuaro, Michoacán, while on a field trip in 1985.

marine mammals of the Gulf of Mexico was published in 2000 (Würsig et al., 2000).

I have always felt that about 5 years was long enough in any administrative position. By then, it was my opinion that you had exhausted your good ideas, and worn out everyone, including yourself. So, in 1996 when Texas Tech University, my old alma mater, called about the position of Vice President of Research and Dean of the Graduate School I was ready to listen. I accepted the position, and Janet and I moved back to our old roots in West Texas.

I saw tremendous potential to build research and graduate education at Texas Tech and to be part of one of the largest and best-known mammalogy programs in the country. With Robert Baker, Clyde Jones, Robert Bradley, Mike Willig, and Robert Owen, just about every aspect of mammalian natural history was covered. Also, Texas Tech was home to the Texas Cooperative Fish and Wildlife Research Unit that was of considerable interest to me.

As I strategically evaluated the potential at Texas Tech University, as part of the higher education climate in Texas, it was apparent to me there was a significant opportunity for Texas Tech to develop a major research and graduate program in environmental toxicology. Texas had recently earned the reputation as the most polluted state in the country and none of the leading academic institutions was strongly focused in this area. A local military base [Reese AFB: eds.] had closed, so we used that opportunity to secure a building and funds to establish the Texas Institute of Environmental and Human Health (TIEHH). To staff TIEHH, I recruited a wildlife toxicology research program from Clemson University.

Ronald Kendall was in charge of the Clemson program, and we developed an instant friendship. I was able to convince him that we could build a world class program that looked both at the impact of chemicals on the natural history of wildlife populations as well as human health and bioterrorism.

While serving as Vice President of Research at Texas Tech University, I had tremendous support from the Provost, John Burns, and the President of the institution, Don Harragan. Both were not only excellent people, but very good administrators. We had a great team and I really enjoyed my work with them. The Chancellor of the Texas Tech System was a former politician by the name of John T. Montford. John was ambitious and had charted a plan to grow and strengthen Texas Tech.

Don Harragan decided to retire as President in 1999 and a national search for his successor followed. I was invited to apply and gladly agreed. In August 2000, I was selected to serve as the 13th President of my alma mater, one of the finest days of my life.

I began my Presidency at Texas Tech with the ambition to lead the university to a new level. I put together a strong leadership team and orchestrated a strategic planning process designed to transform the institution into a stronger research institution. I wanted natural history to be one of the primary areas of institutional strength.

By this time, another mammalogist, Carl Phillips, had become Chair of the Department of Biological Sciences. Carl was clever and visionary. Bob Baker and I took a trip to Boston to visit with E.O. Wilson about our goal. In the meantime, I had met the noted writer and natural history critic Barry Lopez. One of the Texas Tech regents, Mr. Jim Sowell, had a great interest in natural history and he agreed to provide the funds to purchase the papers of Lopez and a number of other literary thinkers about natural history and the natural world. As we became better acquainted with Lopez and his ideas, we hatched the idea of a new degree program in natural history and the humanities. It was to be located in the Honors College and would be an interdisciplinary program linking the sciences, humanities, and the arts. Lopez and Wilson both came and gave lectures, and the students and faculty showed a great deal of enthusiasm for the concept.

Figure 5. David Schmidly as a scientist and museum curator while serving on the faculty and as President of Texas Tech University in 2000.

Because of my background in natural history and fieldwork, I had always felt that introducing students, especially business majors and engineers to the importance and realities of the natural world had great merit. After all, what better way to improve development than to influence the developers!!

I served as Texas Tech President for nearly three years. While I loved the university and my position, I was not enamored with the administrative structure of the system. The President reported to a Chancellor and the lines of authority were seriously blurred. Earlier in my career I had learned that leadership was only possible under circumstances whereby titles, responsibility, and authority were properly aligned. So in November of 2002, when Oklahoma State University inquired about my availability to serve as President, after considerable thought and consternation, I agreed to accept the position. At long last I had a leadership position where authority and responsibility were linked so that real executive leadership was possible.

So my administrative career, which started with an interim appointment, then a department head role, then a Campus Dean and CEO, followed by a Vice Presidency and Graduate Dean, and finally two Presidencies was complete. Wow, what a ride!!

All the while I worked in administration I never gave up my academic work. Although I could not work at the pace of a faculty member, I nevertheless continued to publish, teach, submit grants, and train graduate students. During this time I had some of my greatest research successes. I revised the *Mammals of Texas* (Davis and Schmidly, 1994) listing Bill Davis as senior author in honor of his many contributions to Texas mammalogy. I co-authored *The Marine Mammals of the Gulf of Mexico* (Würsig et al., 2000), and in 1992 I launched a 10-year project that culminated in the publication of my most recent book, *Texas Natural History: A Century of Change* (Schmidly, 2002).

I feel that the Texas natural history book was the most important of my career. Vernon Bailey had directed the biological survey of Texas under the leadership of C. Hart Merriam. Bailey and a group of 14 federal agents spent 20 years traversing Texas and documenting the fauna and flora of the state as well as the landscape history of the time. In 1992 I discovered all of the archival material from their work in Washington, D.C. I secured grant money from several Texas foundations to copy the archives and locate them in Texas. While doing this, I discovered more than 1,000 black and white landscape photos that Bailey and the agents took while working in Texas.

Combined with my recent work on mammals, I was now able to ascertain how landuse had changed the state and influenced the distribution and natural history of mammals. I also wanted more people to have access to Bailey's original work that had been out of print for almost 75 years. So, in *Texas Natural History: A Century of Change*, I included a history of the biological survey in Texas, a reprinting of Bailey's original manuscript, and my documentation about how the Texas mammal fauna had changed during the 20th Century. So far as I was concerned, this was the culmination of my work in mammalogy in Texas.

It is only natural as you reach the end of a career to lament about what you did, what worked and didn't work, how things have changed, and what you think will happen in the future. This is like a right of passage to old age and the nostalgia associated with reflection as one approaches the end of a career.

With regard to my personal career, I don't have many regrets. I was fortunate to build a professional career in a state and a region that I understood and loved. Texas, with its geographic location adjacent to Mexico, has a diverse mammal fauna and a great legacy in mammalogy. I was fortunate to work in two excellent academic departments that had centers of mammalogy and strong collections. I had excellent mentors and colleagues throughout my educational and professional careers. I had great students, both at the undergraduate and graduate level, to teach and get to know.

Along the way, I made some strategic career decisions that worked out well for me.

On the one hand, I focused my work broadly in mammalogy and not on a specific group of mammals. On the other hand, I decided to limit the geographic scope of my work. Thus, I have worked with most orders of mammals, including marine mammals, but my work has mostly been focused in Texas and Mexico.

I expanded my horizons to include components of conservation and wildlife management. This allowed me to compete successfully for funding to do applied field work and to collect mammals for natural history and systematic studies. Combining applied natural history inventory research with my work in systematics and taxonomy proved successful in my career.

I was smart to get involved with interdisciplinary research. It broadened my horizons and opened new perspectives and applications to my work.

The many positive collaborations I have had with other mammalogists greatly aided my career. I have been fortunate to work and collaborate with many outstanding mammalogists, people like John Bickham, Ira Greenbaum, and Rodney Honeycutt at Texas A&M; Bernd Würsig, Bill Evans, and Randy Davis (all marine mammalogists) at Texas A&M at Galveston; Robert Baker, Clyde Jones, and Robert Bradley at Texas Tech; Terry Yates at the University of New Mexico; Michael Mares at the University of Oklahoma; and James Shaw at Oklahoma State University. These collaborations not only resulted in great friendships, but proved to be highly productive scientifically as well.

I decided to seek leadership roles in professional associations. In the scientific arena, I was active in the American Society of Mammalogists, the Southwestern Association of Naturalists, and the Texas Society of Mammalogists. In the area of management, I affiliated with the Wildlife Society and the Texas Chapter of the Wildlife Society. In the nongovernmental arena, my affiliation and leadership role in the Nature Conservancy of Texas has been my most rewarding experience.

The move into university administration was good for me. It allowed me to use my leadership skills to strengthen programs related to natural history, conservation and environmental sciences. Serving as President and CEO of two academic institutions taught me much about planning, leadership, and looking at the "big picture." Systematics and natural history proved

Figure 6. David and Janet Schmidly shortly after accepting the position as the 17th President of Oklahoma State University in January 2003.

to be great background and preparation for my administrative career in higher education. The corrolary is also true: my experience at the grass roots of academia has been one of my greatest assets as an administrator. I like to tell people that "I grew up the old fashioned way" and I am a better academic leader because of it.

I have seen mammalogy change substantially in the 40 years or so I have been part of the profession. When I started observation-based, descriptive field natural history research, and particularly "skin and skull" taxonomy dominated scientific study. That is no longer true as anyone who regularly attends the ASM meetings today would know. Now papers are more hypothesis than observation based, and there are more papers about theoretical ecology and macroecology and molecular phylogenetics than descriptive systematics or general natural history. Also, new techniques, especially those associated with molecular genetics and bioinformatics, dominate the paper sessions.

Vernon Bailey worked as a field biologist in the hey-day of natural history and the naturalist. Darwin's work was less than 50 years old, and there remained a great deal of interest in the natural world. Theodore Roosevelt, the 26th President of the United States, had been educated as a zoologist and naturalist and was regarded as a world-renowned hunter and conservationist. David Starr Jordan, an ichthyologist, was the President of Stanford University. C. Hart Merriam had organized the Bureau of the Biological Survey and was training federal agents, such as Bailey, to work in descriptive natural history.

Naturalists, such as Bailey, lacked an understanding about genetics and theoretical ecology. For the most part, they were generalists of deep expertise who appreciated the beauty of everything they hunted, collected or otherwise observed. They saw their job as collecting facts about nature, and describing and cataloging them meticulously. They had not been trained as academics. Morphological and systematic studies (based on "skin and skull" approaches) predominated and description of new observations and taxonomic studies predominated. Data were quantified but not analyzed. Naturalists did not use statistical methods of analysis, but they had a tremendous talent for assessing, describing, and identifying landscapes, habitats, and wildlife. Today, the modern naturalist, while maybe not as proficient in describing and understanding the landscape and all of its components, is greatly aided by conceptual advancements in fields such as genetics, macroecology, and mathematics. Evolution is more common in the interpretation of work. Mathematical modeling is more prevalent than laboratory experimentation and field studies are stressed less. The modern naturalist tends to be much more of a specialist than a generalist, and is less field-oriented in his/her approach.

A common denominator in the old and new mammalian natural history has been the continuance of specimen-based approaches. I believe this was the greatest legacy of the Vernon Bailey era. In many ways we are still doing the same things as Bailey did, but we have a much broader set of tools and more powerful scientific approaches to study population history. The rate and directions of progress are being strongly influenced by the development of new techniques.

Many professionals have written about the demise of natural history in academia. Indeed, throughout the country the attrition of academic naturalists has progressed over the past half-century. Wilcove and Eisner (2000) and Pyle (2001) have written about this problem and the multitude of reasons. Wilcove and Eisner argue that natural history has fallen out of favor in schools and colleges, in government agencies, and research foundations. No young biologist with the hope of securing a university faculty position would dare to call himself a naturalist, and there is little chance of advancement in academia for those already doing such work. As Wilcove and Eisner so cogently put the matter: "The deinstitutionalization of natural history looms as one of the greatest mistakes of our time, perpetrated by the very scientists and institutions that depend on natural history for their well-being. What's at stake is the continued vibrancy of ecology, of animal behavior and botany, of much of molelcular biology, and even of medicine and biotechnology." Pyle describes his frustrations in trying to put together a degree program in natural history while attending the University of Washington. He goes on to attribute the decline of natural history at academic institutions to three main societal developments – the so-called "hard sciences;" the depopulation of the countryside and the rise of the cities and suburbs; and World War II and the subsequent Cold War. Pyle does not believe that natural history is completely dead and I agree with his view. In his article, he lists at least 10 academic institutions with viable natural history academic programs. In perusing his list, it is interesting to note that most of them are housed at small liberal arts, private institutions. The only two large public institutions on his list are the University of Vermont and Texas Tech University.

From my experience, exploration and discovery, which played such a large role in the activities of 19th Century naturalists, still offer abundant scope to 21st Century mammalogists interested in natural history. This is particularly true in geographic regions where the mammalian fauna is still poorly known and described (for example, South America). Equally exciting are some recent large-scale, long-term, multidisciplinary studies whereby fundamental natural history is linked with modern molecular genetics and virology to support ecological and evolutionary forecasting about public health issues and disease outbreaks. The best example of this kind of work in mammalogy is the recent paper on hantavirus and rodent populations, which showed the interrelationships between El Nino episodes in the American Southwest and density-dependent rodent responses to predict heightened risk for human contraction of hantavirus pulmonary syndrome (Yates et al., 2002). This is a superb example of how understanding the biological complexity of natural and human-dominated ecosystems can assist modern medicine.

Of more concern to me about the immediate future as opposed to the days of Bailey is to witness the disconnection of people and the natural world. One hundred years ago, when Vernon Bailey was a field biologist working in Texas, there were only 3 million Texans and 80 percent of them lived in the rural areas of the state (Schmidly, 2002). Today, there are over 22 million Texans and only 20 percent live in rural areas. The vast majority of Texans live in urban environments and have no connection to nature. Is it any wonder they have little, if any interest, in the natural world?

Whereas natural history clubs were common place a century ago, in this era of television, video games, and personal computers, people have less interest in the outdoors and most are completely unknowledgeable about their local fauna and flora. As industrialization and urbanization reduce our direct interactions with nature, our interest in the variety of living things is becoming redirected toward human artifacts, with potentially grave consequences for biodiversity conversation. In a recent letter in *Science* magazine, a group of British zoologists (Balmford et al., 2002) demonstrated that young UK children are far more adept at identifying Pokeman characters than common British birds and mammals. It has been said that "ecological ignorance breeds indifference: what we know, we may choose to care for. What we fail to recognize, we certainly won't (Pyle, 2001)." Thus, the demise of public interest in natural history does not portend well for the future.

Barry Lopez has written extensively about the importance of place and the natural world to the psyche, morals, ethics, and values of human beings. In the 2001 issue of *Orion* Magazine, which is devoted entirely to natural history, Lopez asserts that a world having "a politics with no field biology, or a political platform in which human biological requirements form but one plank, is a vision of hell." In fact, many writers and thinkers believe that our growing indifference to the natural world is one of the root causes of the environmental crisis. More college graduates and citizens need to understand this.

Unfortunately, the term natural history is widely misunderstood and misapplied, and not everyone thinks of it as a badge of distinction. The one exception to this in modern biology is E.O. Wilson who is today perhaps our best know naturalist. This was not a problem in the era of Vernon Bailey and his mentor C. Hart Merriam. In their era, the term "naturalist" had both breadth and power. Unfortunately, in this era of specialization and popularization, natural history has come to be seen as nothing more than the description of nature. That is unfortunate because in reality the modern naturalist can be characterized as basically an explorer and tester of evolutionary and ecological ideas that are developed to reveal and explain regularities in nature: in genetic structure as much as phenotypic structure, and in ecological and behavioral function more than in physiological function (Grant, 2000). The search for answers to many questions is being taken increasingly to cellular and macromolecular levels, into the molecular realm of cytogenetic interactions, and into the realm of molecular evolution for the reconstruction of phylogeny.

Herman (2002) has provided a modern and broad definition of natural history that in my opinion has heuristic value. He defines natural history as ..."the scientific study of plants and animals in their natural environments. It is concerned with levels of organization from the individual organism to the ecosystem, and stresses identification, life history, distribution, abundance, and inter-relationships. It often and appropriately includes an esthetic component."

Anyone who regularly attends the ASM meetings appreciates that many mammalogists still work in natural history as defined by Herman. Certainly, we have not seen the end of the naturalist in mammalogy, but at the same time there can be no doubt that the field has changed and we are not likely to return to the days of Vernon Bailey. It is paradoxical, and in many ways sad, that this is happening during the period of the biodiversity crisis when we need naturalists the most.

Recently, I graduated my last graduate student, Chris Hice. She fits the definition of a naturalist and has the potential for a great career. Her work on the natural history of Neotropical lowland rainforest mammals definitely fits all of the criteria outlined by Herman (2002), and she has outstanding potential as evidenced by her recent selection as the recipient of the A. M. Shadle Award presented annually by the American Society of Mammalogists. She also is incorporating modern technology into her work, although her research is still fundamentally descriptive and specimen-based.

Typically, men dominated the study of the outdoors in the late 18th through the early 20th centuries. But there were women in the field, too – sometimes accompanying men and sometimes independent of them. Marcia Myers Bonita (1991) has written biographies of 25 of these women naturalists. One of the most prominent, and certainly the most prominent in ornithology, was Vernon Bailey's wife, Florence Merriam Bailey, the sister of C. Hart Merriam. One of the most encouraging trends in the last half of the 20th century has been the substantial increase in the number of women scholars in field biology and natural history. This is another positive about the future of the profession.

My biggest concern about the future is the availability and stability of jobs for young professionals entering the field. Museums still hire naturalists but there are precious few of those positions, and they are often targeted for elimination in tight budget times (witness the recent decisions at the University of Nebraska and Michigan State University to close their museum science programs and mothball their collections). Academic jobs seem destined to continue to dwindle, and the problem of orphaned collections at academic institutions is becoming increasingly common (Lane, 2001). Landrum (2001) has written eloquently about this problem in terms of descriptive systematics.

On an encouraging note, state and federal agencies have been hiring people with natural history backgrounds, and professionals are often hired by private conservation organizations (e.g., The Nature Conservancy and Bat Conservation International). Also, it is encouraging that many of the smaller regional academic institutions continue to focus on natural history research and collections. Two of the best examples in mammalogy are at Fort Hays State University, in Hays, Kansas, and Angelo State University in San Angelo, Texas. Natural history as a viable academic field is more likely to survive at these types of institutions than at the large, flagship, research-intensive institutions.

Vernon Bailey represents a by-gone era that is not likely to ever return in modern society. Our urbanized, fast-paced, gadget oriented society seems to have lost the interest and patience to appreciate the natural world. I regret this and will do whatever I can in my leadership roles to change it. This was one of the major driving forces behind establishing the natural history and humanities degree program in the Honors College at Texas Tech University. Maybe there we will find and educate future generations of naturalists.

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MIGRATIONS OF ONE MARINE MAMMALOGIST: FROM BIOACOUSTICIAN TO MAMMALOGIST

WILLIAM E. EVANS

William Evans was born in Elkhart, Indiana on 11 October 1930. He received a B. S. in Audiology from Bowling Green State University in 1953, an M. S. in Bioacoustics from Ohio State University in 1954, and a Ph.D. in Biology from UCLA in 1975. Evans served as the first Director of the Hubbs-Sea World Research Institute, Chairman of the United States Marine Mammal Commission, Assistant Administrator for Fisheries (Department of Commerce), Under Secretary of Commerce and Administrator of NOAA, and United States Commissioner to the International Whaling Commission. He currently is Professor Emeritus at Texas A&M University at Galveston, visiting Professor at Notre Dame University, and Editor of the American Midland Naturalist.

I have been asked to write about the development of marine mammalogy during the past 30 years. But to be accurate, I have to go back perhaps 45 years or more. Discussion will center on my personal history and experiences since my professional career, for the most part, is tied very closely to that history. Of course marine mammals have interested man since the days of ancient Greece. But it has only been about 50 years since marine mammalogy has become a separate science even though it is, in reality, a field of specialization within the science of mammalogy. I will try to explain this transition.

The years 1937 and 1938 changed my focus from the childhood imaginations of jungles and plains of Africa and, of course, Frank Buck and Tarzan, to sea monsters, Jules Vern and the oceans. During that period, a 100-foot preserved blue whale visited my hometown of Elkhart, Indiana. The following year a preserved 60-foot sperm whale came to town.

For 25 cents I could walk up the steps on the specially designed railroad cars and, standing on tiptoe, stare for hours at these enormous beasts. The smell of formalin, although overwhelming, was easily overpowered by the greatness of these special creatures. My interest in whales and oceans was locked in by this amazing boyhood experience. I began to read the tales of the great sea adventures like the *Cruise of the Cachalot*, *Darwin's Voyage of the HMS Beagle*, and, of course, all of Jules Vern.

The remainder of these reminiscences are divided into 6 periods: 1950-1959—the Beginning of my Professional years, 1960-1969—Navy Funded Dolphin Research, 1970-1979—The Environmental Decade, 1980-1989—My BioPolitical Phase, 1990-1999—The University Phase, and 2000 and Beyond.

Why has there been a roaring popularity of dolphins, whales, and other marine mammals during the past 40 plus years? I shall attempt an answer to that question. As a brief background, in 1938, Marine Studios in St. Augustine, Florida, was successfully maintaining a colony of dolphins in a large aquarium, one that allowed for close-up viewing. With the advent of the attack on Pearl Harbor, December 7, 1941, the Marine Studios was forced to close down before it had started to gain popularity. This necessitated the release of its dolphin colony back into the Indian River. In 1946, Marine Studios, later named Marineland of Florida by its new curator, F.G. Wood (Wood, 1973), re-opened with a collection of new animals and started what truly became a public romance with dolphins and whales. The opportunity to observe and study these exotic animals "up close and personal" not only attracted the public but the scientific community as well. Besides, who would not be impressed by a very large animal with a built-in smile, one that could jump through flaming hoops *and* play basketball?

In the meantime, the popularity of dolphins was accelerated by the popularization of a book published in 1961, *Man and Dolphin*, based the research during the 1950's of a psychiatrist, Dr. John C. Lilly (Figure 1). Just what was so interesting about John C. Lilly's research? The book provided a lengthy but important story within the complex web of marine mammal and dolphin research; equally important, it detailed the development of seals and dolphins as environmental icons.

The dolphin's new-found image spread gradually among a growing public whose curiosity about and interest in marine life had been quickly expanding primarily due to new oceanaria. The ability to view the charm and behavior of sea lions and dolphins up close held both great power and tremendous appeal. Then, in 1958, this new image of marine life was greatly enhanced by media reports quoting the views of John C. Lilly, M.D. Why do I use the term *views*? John Lilly had a tendency to go beyond his research results and speculate as to what it all meant. More often than not, the public seemed to be more interested in his speculation than the results (or lack thereof). In May of that year, Dr. Lilly delivered a paper at a meeting of the American Psychiatric Association in San Francisco entitled "Some Considerations Regarding the Basic Mechanisms of Positive and Negative Types of Motivations." Accounts of his talk appeared in newspapers under the following headlines:

A SCIENTIST HAS SHAGGY DOLPHIN TALE

A GOOD DOLPHIN IS KIND, LOYAL, BRAVE— PSYCHIATRIST WANTS TO MAKE DOLPHIN TALK

SHOCKED HAPPY DOLPHIN LAUGHS WITH SCIENTIST'S WIFE THEN DIES

No, these articles were not addressing a separate paper; they were alluding to the one noted above: the one on basic mechanisms of positive and negative motivation. Then, why did this paper trigger the remarkable publicity that followed?

In his San Francisco talk, Dr. Lilly told of his experiences in electrically stimulating "negative" and "positive" zones of the dolphin brain. Lilly told the group that on one occasion, the dolphin mimicked his voice so well that his wife laughed and then the dolphin copied her laughter. The vocalizations of this animal were

Figure 1. John C. Lilly, M.D., the psychiatrist who played a principle role in stimulating interest in ceteacean behavior.

the start of Dr. Lilly's belief that it should be possible to establish communications with dolphins and even teach them to speak English. He expressed his views this way:

> "Eventually, it may be possible for humans to speak with another species. I have come to this conclusion after careful consideration of evidence gained through my research experiments with dolphins. If new scientific developments are to be made in this direction, however, certain changes in our basic orientation and philosophy will be necessary. We must strip ourselves, as far as possible, of our preconceptions about the relative place of *Homo sapiens* in the scheme of nature. (Lilly, 1961)"

After finishing a Masters of Science degree at Ohio State University in psycho/bioacoustics, I entered the Army for two years of active duty as an Armored Field Artillery officer. At the end of that tour in December of 1956, I left with my family for California and my first professional job as an acoustic science specialist with the Douglas Aircraft Company.

Donald Douglas, the CEO of Douglas Aircraft, had a doctor friend, Earl Dudley White, a cardiologist who wanted to measure the EKG on a whale. Mr.

Douglas was aware that the California gray whales were abundant in the lagoons of Baja California in the winter and spring. Thus, I was given the task of providing all the background available on gray whales and the breeding lagoons of Baja. So I was off to the library followed by a visit to the new oceanarium on Palos Verde (Marineland of the Pacific) to first interview Dr. Ken Norris and then, later, Dr. Carl Hubbs at Scripps Institution of Oceanography and Dr. Ray Gilmore at the San Diego Museum of Natural History. (Can be found in Eerl Stanley Gardner's book, *Hunt for The Desert Whale* (1960) which included details on the White-Douglas Expedition). These events resulted in a change in my focus and a rekindling of my long-held interest in things marine and, of course, my introduction to bottlenose dolphins, whales, and the field of animal bioacoustics.

In 1959 I attended the Acoustical Society of America meeting in Los Angeles and met Dr. John Dreher, one the members of my thesis committee at Ohio State University. He had left Ohio State and accepted a job to start a psycho/bioacoustics program at Lockheed Aircraft Inc. in Burbank, California. Lockheed was in competition to develop a new Antisubmarine Warfare (ASW) Aircraft (P3V) for the U.S. Navy based on their new turboprop aircraft, the Lockheed Electra.

John asked if I knew anything about underwater acoustics. I hesitated —then quickly regurgitated all that I could remember from all that I had ever read and heard. He was duly impressed, and I was offered a job as part of his new psycho/bioacoustics group at Lockheed. This meant that I was finally able to concentrate on marine acoustic issues armed with support, including new equipment such as hydrophones, recorders, and other necessities on loan from the Navy (Figs. 2 and 3).

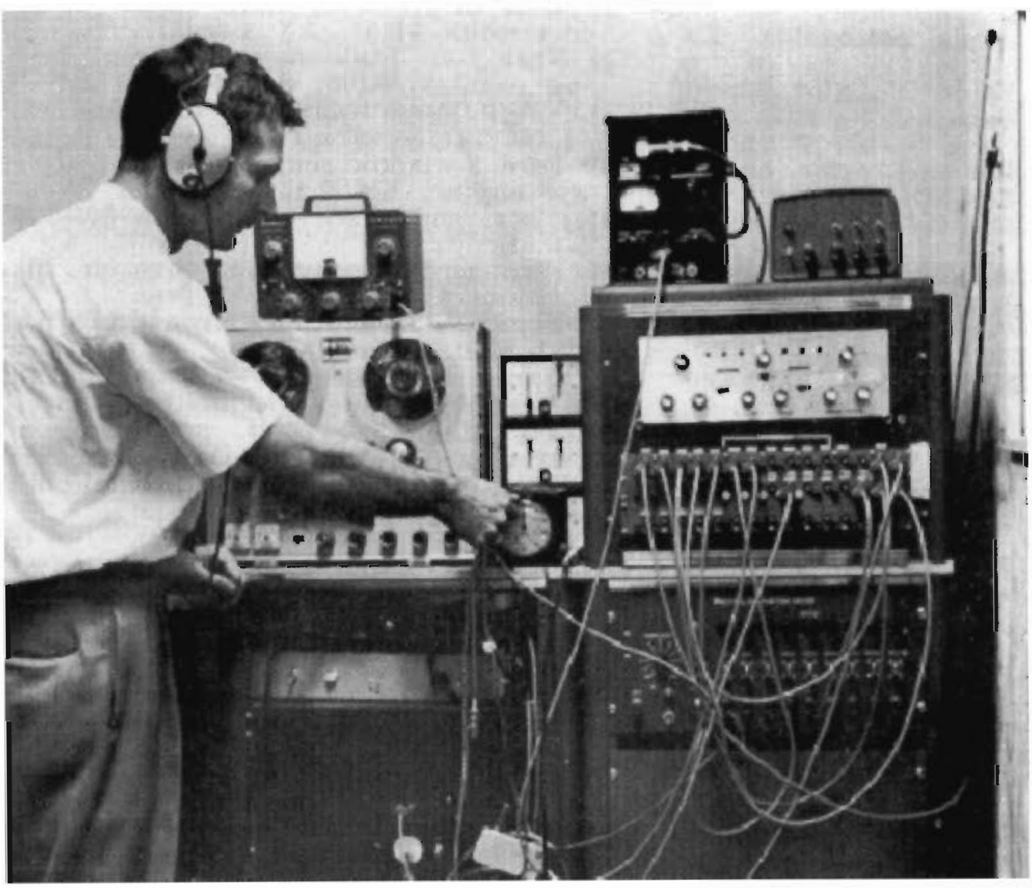

Figure 2. Bill Evans working at a panel console that could reproduce sounds for testing and analyzing reactions in humans and cetaceans. July, 1960.

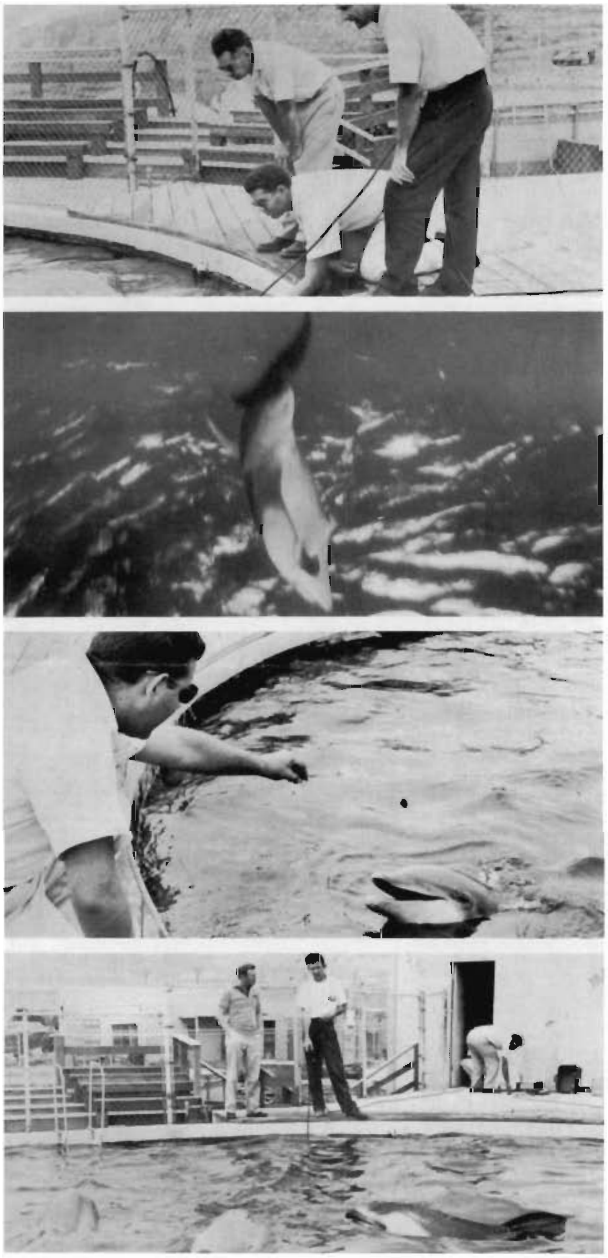

Figure 3. Bill Evans and other Lockheed scientists studying porpoise vocalizations at the Marineland of the Pacific. Top panel: Bill Evans (right) with Dan Painter (center) and William Southerland. Next panel: underwater photograph of porpoise heading toward the surface. Next panel: Bill Painter with porpoise test subject. Bottom panel: Bill Evans (right) talking with Marineland colleague. July, 1960.

We were allowed a certain amount of time for basic research, but we were also given the task of looking at psychoacoustics problems that might arise during the development of the soon-to-be P3V-ASW aircraft. Noise levels at the passive sonar stations were a critical issue. Since my Masters thesis research at Ohio State was on the effects of noise on speech intelligibility in a communications net, I had a good foundation. So I started looking at the effects of noise on operator performance in long- term monitoring tasks, *e.g.*, staring at a sonar screen for long periods as well as listening to nothing for long periods.

In the meantime I was making recordings at Marineland of the Pacific Oceanarium, including the pools with bottlenose dolphins and pilot whales. We were interested in how listeners would respond to false passive sonar targets. As they say in the ASW business, "one man's noise is another man's target (signal)."

While at Lockheed I also got my first chance to cortically observe dolphins and whales at sea. In January 1961, one of the oceanographers hired by Lockheed in their Antisubmarine Warfare group in San Diego invited me on a cruise to Scammon's Lagoon to make recordings of the noise levels in the lagoon. We headed south in Lockheed's research vessel, the *SeaQuest*. We decided to test whether or not gray whales could echolocate. We stretched a barrier of buoys across a narrow passageway used by the whales in order to determine whether or not they would avoid the buoys in turbid water.

To our surprise, a mother gray whale and calf approached the barrier and apparently did not detect it. They swam through, totally destroying all the buoys trying to untangle themselves. Unfortunately, we had not remembered all of Eerl Stanley Gardner's book on his adventures with gray whales in Scammon's Lagoon (Gardner, 1960) in which he described how aggressive gray whales were. During the 19th century, whalers called gray whales "Devil Fish." Feeling sorry for the whales, we jumped into the water, which, by the way, we found out later was shark infested, and swam toward the female with the intent to untangle her. She apparently misinterpreted our intent and tried to swat us like flies with her tail flukes (Evans and Eberhardt, 1962).

This was the first of two or three more cruises to the Baja gray whale breeding lagoons. The next season, I invited my mentor and soon-to-be major professor, Ken Norris, to cruise to Scammon's with the *Sea Quest*. In the time between the first visit and this one, the government of Mexico declared Scammon's Lagoon a Biological Reserve. In order to get permission for this trip, we had to pick up a Mexican naval officer in Ensenada.

Bill Kielhorn, head of the Lockheed oceanographic group, was to fly down and meet us at the Lagoon. He would use the landing strip that was developed to accommodate the Douglas expedition several years earlier. When he arrived, he told Ken he had seen a stranded gray whale along the beach. Ken was determined to investigate it further, as any true naturalist would. So Ken, Bill Kielhorn, and I prepared a lunch and the tools necessary to necropsy the whale and rowed ashore in the *Sea Quest's* dingy.

It turned out that the whale was quite distant from where we beached the dinghy. After a long hike, Ken spotted the whale and started to run towards it in great excitement. When we arrived, he unsheathed a machete and started to dissect the whale's ear bones. This whale had been lying in the hot sun for at least a week and perhaps longer. When cut deep enough, all the gas, entrails and bits of whale gushed out, soaking all three of us in rotten whale meat and fluid. What a disgusting mess we had on our hands and everywhere else! We washed off as much as possible and headed back to the dingy which was now high and dry since we were now at low tide. (There are 12-15 foot tides in the lagoon).

After pulling the dingy several hundred meters to the water, we headed back to *Sea Quest*. When we were about 100 meters from the boat, the captain waved us off, not allowing such an offensive crew aboard his vessel. We returned to the beach and waited until a change of clothes was delivered to us. The cruise had been productive of sorts, but we ran out of clothes and decided to return home with Bill Kielhorn by airplane (Kielhorn et al., 1963).

Shortly after this adventure, I decided to quit Lockheed and return to graduate school. Ken Norris had offered several times to take me on as a Ph.D. student and finally, with the encouragement of my wife, I decided to take the leap or the plunge, as it seemed at times.

During the 1960's the bulk of funding for marine mammal research was from the U.S. Office of Naval Research with an emphasis on the dolphin's biosonar capability. Therefore, some of my Navy research support followed me to UCLA and my graduate program. In 1964, I was brought aboard for a summer job at the Navy's Point Mugu Marine Bioscience facility. As a Ph.D. student with two sons and a wife, I badly needed the money as well as the opportunity to use their facilities to continue my dolphin and pinniped research.

Before leaving Lockheed I decided to test the hypothesis of Dr. Tom Poulter at Stanford Research Institute that California sea lions were echo locators. I contacted my friend, John Prescott, who was the curator of Marineland of the Pacific, and procured a beached California sea lion pup. For lack of a more imaginative moniker, I called her Roxanne, or Roxie for short. While at Lockheed, she was trained to wear a blindfold and was tested to see if she used sound while retrieving rings underwater. This activity demonstrated what good tactile sense she had but not any active echolocation ability (Evans and Haugen, 1963).

When I left Lockheed, they did not want the sea lion and suggested euthanasia. And, of course, I took her home with me. The Navy was unaware that they hired two for one, so when I showed up for work that summer, they were surprised by my companion, my own sea lion. Bringing Roxie along with me to the Navy's facilities served two purposes: first, my dogs were delighted to get rid of this strange kennel mate; and secondly, Roxie got a new home with real seawater, a better diet, and an excellent medical care plan. My task for the summer was to train Roxie to retrieve rings similar to those in the echolocation study but fixed with an acoustic pinger.

Since the goal was to start training her to retrieve at increasing depths, we worked from pools, to the lagoon, and then at sea. The sea trials were conducted near Anacapa Island, some 15 miles west of Point Mugu. Here the water was clear, and we were able to photograph Roxie's behavior. One of the underwater photographers from the Naval Missile Center followed her with his camera as she approached the ring at 120 feet, stuck her head through the ring and headed to the surface, not directly but in several oblique, spiraling excursions. Some thought this might be to avoid the bends, but at depth greater than 200 feet, she would return by a straight path. Roxie later dove to 240 feet and there were no indications that this was her depth limit (Harmon and Evans, 1968). This research later led to the development of the Navy's current Mk 5 sea lion mine recovery system.

When the Photo Department was assembling the movie footage for my final report, they called and asked: What do we title it? A We thought about it and responded: "Just call it 'Project Roxie.'" It wasn't until later, when I was presenting the film to VIP's from the Pentagon and a Navy Commander introduced the Project, that I discovered that Roxie was also the acronym for Retrieval of Xperimental Immersed Elements.

Shortly after my return to the University, I received a call from Dr. Bill McLean, Technical Director of the Naval Ordinance Test Station, China Lake, California. He complimented me on the success of the Roxie Project, which had now been accepted as a systems development program and was no longer viewed as a basic research program into sea lion diving behavior. He then offered me a full-time GS 13 Civil Service appointment to work on the Navy's growing Marine Mammal Program. This was again an opportunity that I felt I had to take. The question was: Could I handle this and my graduate program at the same time? I took as many required courses as I could and then got permission to start on my doctoral research project. The Navy job was going to be my "fellowship."

My first Navy project was working on dolphin discrimination experiments, some of which were classified but became my Ph.D. dissertation research. Ken Norris and Ron Turner, with some assistance from me, were working with a dolphin named Alice that they had on loan from the Navy. Alice was trained to detect while blindfolded the presence of a stainless steel ball of a certain size when compared with other stainless steel balls of slightly different diameters and thus, different target strengths. Alice turned out to be very difficult to train. She was very old and also not very cooperative. At about the same time, I started to work with a young bottlenose dolphin with an experiment that was looking at sonar discrimination but using a very different approach from that of Alice. Based on discussions with F.G. Wood and Scott Johnson at the Navy's Point Mugu Marine Mammal Facility, it was decided I would start a series of experiments to follow up on my earlier work at Lockheed in measuring the directionality of the dolphin echolocation signals but do this during sonar discrimination tests.

The animal selected for these experiments was a female bottlenose dolphin, 4 years old. For purpose of identification, she was named Doris. She turned out to be a very quick study. She was trained to wear blindfolds and then detect the presence and absence of specific targets, which were placed in a special area in a redwood pool with several hydrophones.

Doris was then trained to swim through a series of metal pipe guides designed to control her approach to the target area. In the morning after installing the pipe guides, I went to the pool only to find all of them flat on the floor of the pool. I was concerned because they were put together with nuts and bolt that were nowhere to be seen. Dolphins in captivity were notorious for swallowing foreign objects. Hours of searching the bottom of the pool and the drains resulted in finding no nuts or bolts. We were ready to have her x-rayed when she swam to the side of the pool, opened her mouth, and presented us with a mouthful of metal -- all the nuts and bolts. How a creature with no fingers or opposable thumbs can undo nuts on bolts assembled with a wrench remains a mystery to me even today.

On the floor of the pool, there was a white grid area divided into 20cm squares. A video camera was placed directly above to record Doris' movements as she approached the target. It was necessary to control her position if we were to get accurate measurements of her sonar beam while actively echolocating. We then turned the task into a discrimination task by adding additional targets that varied in target strength by 1-dB increments. By early 1966, we had finished the experiment and were going to present the results at the first NATO Biosonar Conference being held in Frascati, Italy, in the fall of that year. The conference covered bats as well as dolphin sonar.

Earlier that same year, Bill Powell and I were invited to present a paper at a Los Angeles Conference summarizing the Navy's Dolphin Research Program. Because of speculation by the press, all-of-a-sudden our research program became "secret" even though up to that time, it was not. Thus, the Italy conference was the motivation for the Navy to present the results of its research to date in a public forum to prove they were not hiding anything.

Re-reading a chapter "Kamikaze Porpoises" in F.G. Wood's book *Marine Mammals and Man: The Navy's Porpoises and Sea Lions* (Wood, 1974) brought back a bad but humorous memory, which illustrates what the press can do when sensationalizing science. Following are some excerpts:

> "Bill Evans, who was conducting the sonar study, and his coworker Bill Powell, had been invited to participate in a symposium on 'Modern Developments in Marine Sciences' in Los Angeles.... The two investigators had found that a dolphin named Doris could by sonar alone distinguish different materials."

> "Their presentation, entitled 'Current Cetacean Research,' consisted primarily of film footage and by spoken commentary illustrating some of the work that they had been doing. Also included was a reference to a dolphin's participation in SeaLab II working in the open sea."

It was the juxtaposition of the two subjects that got the Navy into trouble. Another reporter, who two years earlier had written about a secret Navy communications study, attended the meeting. Afterwards, he went back to his office and composed the following:

> "Navy scientists have taught porpoises to tell one metal from another - a valuable trick for creatures that someday may be used to detect submarines, mines and underwater missile installations. For example: A patch of metal not customarily found on submarines could help wide ranging bands of porpoises identify friendly craft. Any craft not identified would be subject to ramming by the porpoises, trained to carry explosives in body harnesses."

Although strictly a figment of the writer's imagination, it was considered to be true by the general reader, and since it made the Associated Press wires, it was widely distributed and resulted in Bill Powell and I being burned in effigy several times in several countries, primarily by Flipper Fan Clubs.

This story continued to be dug up out of the archives for the next 10 years. Wood has examples of the thousands of letters he received. Fortunately, there was a recording of the actual interview, which later proved that neither of us said anything about the research that could be interpreted as it had been written in the article. This later saved our hides from the potential castigation from the office of the Chief of Naval Operations. But from this time on, we were hounded by the media, right or wrong. This was the motivation for the Navy to tighten up on press releases on what was taking place at Point Mugu.

Whether or not all this controversy sparked the imagination of the Navy leadership in the Pentagon is an interesting question. It was shortly after that discussion into the potential of such ideas that research was funded. From this point in the development of research on dolphins and other marine mammals, funding came primarily from the U.S. Navy. This source of support was not to change until the early 1970's.

During this time, it was becoming very clear to me that I could not handle the job with the Navy and graduate school at the same time. So in early 1965, I took a leave of absence from UCLA in order to concentrate full time on my biosonar research program. This leave of absence would last more than 6 years.

This whole period of the late 60's and the 70's is a significant part of the History of Marine Mammal Science as a separate area of study. Because of the nature of the research, it became very multidisciplinary involving engineers, physicists, biologists, psychologists, medical doctors, veterinarians, physiologists and ecologists, to name a few. Later, we added a sociologist, artist, and lawyers. This mix of sciences, engineering, and other disciplines was later to become, in part, the motivation for forming the Society for Marine Mammalogy in the mid-70's.

At the BioSonar Conference in Italy noted above, Bill Powell and I presented the results of the Doris studies, and Ken Norris presented the Alice results. After these presentations, Don Griffin, author of *Listening in the Dark*, and the mentor of the bat sonar group, asked the following question: "It is all well and good that you can train a dolphin to use its obviously sophisticated sonar to discriminate metal plates and steel balls, but how do they use it in nature?" Of course, the main emphasis of the bat sonar studies was field based and the dolphin research at that time was exclusively laboratory based. This was an excellent question and one I thought about on the long trip home. The answer, of course, was that we didn't know since we focused all of our studies on captive animals. After the conference, my research shifted to working on ways that we could follow animals in the wild and learn more about their foraging behavior. This resulted in my spending a lot of time at sea, the development of radio transmitters for dolphins, and the design of a vehicle to view dolphins underwater. This also became the focus of the direction I used to guide my graduate student research.

In 1966-67, the Navy started plans to move from the Point Mugu Facility. Some of the staff went to San Diego and some relocated to Hawaii. Many of the marine mammal research program results were beginning to spark interest by the Pentagon as to the possibility for operational programs. This also, incidentally, was the time period that the U.S. active involvement in Vietnam began to increase.

To help me make up my mind as to whether I would relocate to Hawaii or San Diego, I was given the opportunity to ship my equipment and my family to Hawaii for the summer. One of my mentors and Committee Co-chair, Ken Norris, had moved to Hawaii and started his research program at the Oceanic Institute adjacent to Sea Life Park.

Perhaps in response to Griffin's question, Ken was developing a vehicle that could be towed and would allow for unobstructed underwater viewing. Since I was interested in also developing a vehicle, this gave me an excellent opportunity to observe and participate in the project.

This truly was the start of focusing on what dolphins and whales were doing in the wild. Were they using echolocation during foraging? This was also the progenitor of Ken's future studies of dolphin societies, especially spinner and spotted dolphins (Norris, 1974). It was also the start of a new direction in research for me, one that eventually resulted in my Ph.D. research on the natural history of the common dolphins, *Delphinus delphis,* and what later became known as *Delphinus capensis.*

The summer's experience in Hawaii was an incredible one for someone whose view of dolphins had been confined to seeing dorsal fins on the surface and behaviors in pools at various oceanaria. I was given the opportunity to observe a few herds of pantropical spotted dolphins (*Stenella attenuata*) underwater from what Ken called his "Sea Sick Machine," aptly named in response to its first open ocean trials. Due to the design, the viewing chamber had a pendulum motion that swayed rhythmically from side to side undertow. While testing during the first sea trials, Ken became very sea sick, so I got a chance to be the pilot. Unfortunately Ken had forgotten to tell me he lost his cookies while swaying to and fro. It was at this time I made the decision to consider a different design when I returned to California.

The ability to see definite social organization patterns in the wild was, to say the least, wild! The herds of dolphins observed appeared to move in predominant group patterns: The first composed of 5 to 9 adult females and juveniles; the second contained a single male, sometimes accompanied by one or two adult females; and the third consisted of only 4 to 8 subadult males. The most interesting thing to note was that they were arrayed in a vertical fashion with what appeared to be the most dominant animal on the top of the stack and closest to that vital resource for a mammal: air. It was amazing to see individuals on the bottom pull out of the formation to the side, go to the surface, and return to take their place at the bottom. On occasion, an animal would improve on his or her position in the stack. In the case of the male groups, they were distributed horizontally. These were new revelations, ones that were to be verified many times in future studies. But for now, it was as if we were eavesdropping on an entirely new world.

Upon my return to California, and armed with photos from Ken's vehicle, I convinced the powers-that-be in the Navy's program that we needed a mobile underwater observation vehicle, a stable one. With the help of Larry McKinley, an engineer at the Naval Ordinance Test Station at China Lake, we started the design of *SeeSea*. Fortunately, the Navy already had a mobile catamaran vehicle that had been designed as a tender for a Navy experimental deep submersible, a prototype of a rescue vehicle for downed submarines. This was fortunate since it decreased the cost of building *SeeSea* and allowed us to buy more equipment for the boat.

Most of the next two years was devoted to developing the *SeeSea* and dolphin and whale radio tagging equipment to assist field observations and recording of marine mammals off the coast of California and the offshore Channel Islands. Use of radio telemetry made it possible to stay in touch with a herd for extended periods. This resulted in the radio tracking of several dolphins, one pilot whale, and a gray whale. The *SeeSea* was finally commissioned in 1967.

Also during 1968 I was working on a chapter for Harald Andersen's book, *Biology of Marine Mammals* for Academic Press. Ken Norris was doing the chapter on echolocation, and I was asked to do the chapter on marine mammal communications. Since psychologists had done a great deal of work in this field, I asked Jarvis Bastian from the Psychology Department at the University of California at Davis to be a collaborator. The result was a chapter on "Marine Mammal Communication: Social and Ecological Factors." Our chapter contained much of the information on dolphin social behavior that came from our Sea Sick Machine and *SeeSea* underwater observations. The title reflected my changed focus from captive observations to the natural world. Academic Press published the book in 1969 (Evans and Bastian 1969).

In 1967 personnel from both the Naval Ordinance Test Center (NOTS) and the Naval Missile Center manning the Navy Marine Mammal Facility were placed under the newly formed Naval Undersea Warfare Center, later called the Naval Undersea Center (NUC), with headquarters in San Diego. The Center also included personnel from other Navy laboratories and was a wide-ranging organization with laboratories or facilities in Pasadena, Long Beach, San Clemente Island, Lake Pend Oreille in Idaho, and Cape Prince of Wales, Alaska. In 1968, a laboratory was established in Hawaii, and part of the marine mammal work moved there along with an ocean engineering group

Under the auspices of NUC, the Navy's growing Marine Mammal Program was developed and is still growing today (www.spawar.navy.mil/sandiego/technology/mammals). In terms of the staff of the program, the list of participants expanded and included several who are now well known in the Marine Mammal Science community. It was also this organization that was instrumental, along with others, in the formation of the Society for Marine Mammalogy.

Although we did not have animal-holding facilities until 1971, I visited Sea World, San Diego, and cut a deal with their Director of Animal Care. Shortly after that, a new bottlenose dolphin arrived, one which I had worked with at Pt. Mugu (Figure 4). Her name was Scylla. I then began training Scylla at Sea World to echolocate while wearing an array of attached hydrophones. While this entire Navy activity was going on, there were some significant changes in public attitudes towards wildlife and, particularly, marine mammals.

In response, in 1969 Congress passed a very significant piece of environmental legislation, the National Environmental Policy Act of 1969 (NEPA). Among other impacts, the Act established the need for the government to conduct environmental assessments of any federally sponsored program that may have a significant impact on the environment and produce an Environmental Impact Statement if necessary. Although not immediately impacting marine mammal research, NEPA set the stage for several other significant environmental laws that would.

In 1970 the United States Secretary of the Interior, Walter Hickel, announced that an international meeting of leading cetologists would be called to consider the plight of the great whales and their biology as related to their conservation. Under auspices of the Endangered Species Conservation Act of 1969, he placed 8 commercially sought-after whales on the Endangered Species List. This made it illegal to import any parts or products of these whales into the United States. The whales listed included the bowhead, right, blue, sperm, finback, sei, humpback, and gray.

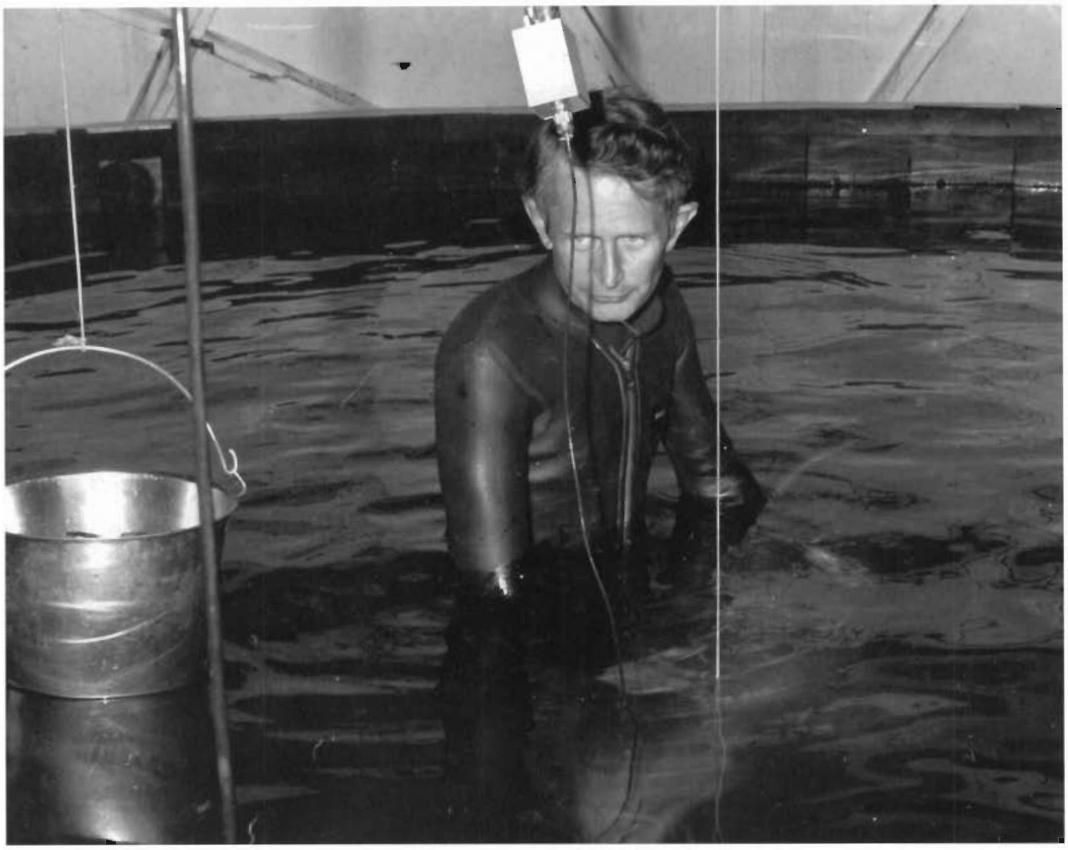

Figure 4. Bill Evans positioning a porpoise fitted with electronics gear to record vocalizations. Photograph taken in 1968 at the U.S. Naval Missile Center, Point Mugu, California.

The resulting meeting, The International Conference on the Biology of Whales, was held on June 10-12, 1971, in the Shenandoah National Park on the Blue Ridge of Virginia. The conference was sponsored by the Department of Interior, The Smithsonian Institution, the New York Zoological Society, and the International Biological Program as well as the Department of Commerce and the National Science Foundation. Significantly, several Non-Governmental Environmental Organizations (NGOs) were also co-sponsors, including the National Audubon Society, the Environmental Defense Fund, the Conservation Fund, the World Wildlife Fund, and the National Wildlife Federation.

The conference was organized and managed by a committee of the Marine Mammal Council of the United States International Biological Program consisting of G.C. Ray, W.E. Schevill, and Ken Norris. All three were recognized marine mammal specialists but, at that time, not connected with either whaling or sealing or their regulation.

Thirty-four participants and 20 observers represented 10 countries at the conference. The Russians were invited but did not attend. Many of the United States participants were also key players in providing advice for the drafting of the Marine Mammal Protection Act of 1972 (MMPA) and providing testimony before Congressional Committees. This is a good example of the fact that the funding for research is greatly influenced by public concern. Because of pressure put on legislators, we ended up with the MMPA and the Endangered Species Act of 1973 which was much more inclusive than the Endangered Species Conservation Act of 1969. This legislation provided a new source of funding for major field studies of marine mammals. It is unfortunate that the Shenandoah Conference and its results are not given the credit they deserve as a pre-cursors for the past and current international conservation of cetaceans. The proceedings were published in 1974 by Harvard Press (Schevill, 1974).

Because of the above legislation, the pressure was on to return to sea a 1-year-old California gray whale named GiGi. Sea World of San Diego had captured Gigi as a calf in March of 1971 and successfully held her in captivity for a year for research purposes. Since she was an endangered species, both the Fish and Wildlife Service (FWS) and the National Marine Fisheries Service (NMFS) had jurisdiction over her, and they decided she was to be returned to the wild sooner rather than later. In March of 1972, the U.S. Navy was given the job of assisting with the release since NMFS and FWS did not have the facilities to handle an animal of this size. They contacted the Naval Undersea Center Marine Mammal Laboratory and I, unfortunately, was put in charge of the release and the logistics.

Part of the agreement among all three agencies was that all of the research accomplished with this animal would be published at a conference following her hopefully successful release. The conference was held in April of 1972 at Scripps Institution of Oceanography, LaJolla, California (See *Marine Fisheries Review*, *The California Gray Whale*: *Special Issue*, (ed.) W.E. Evans, Vol. 36 Number 4, April 1974).

What was to unfold was a project of gigantic proportions. She was scheduled to be released in late March or early April, 1972, about the time of the peak of the northward migration of gray whales. That gave us only one month to prepare! And there were so many challenges to be faced. First of all, there was the design of the device to carry the two-ton-plus whale to the barge that would be used to carry her to sea. Next was the requirement to track her to assure the media that she was safe on her way north. So we pondered these two questions: What kind of truck was needed? What time of day should we schedule the release? We had started to have a radio transmitter designed by Ocean Applied Research that would measure location, depth of dive, and temperature as a function of depth. Based on observations of the northward migration, the date of 13 March was selected as the release date. For obvious reasons, I think we wanted to avoid the Ides of March.

As the date of release approached, the tension rose. The Navy was wonderful in providing support way above and beyond the call of duty. It was decided to move the 8.15 meter long, plus 5,000 kg whale before dawn and have her at sea by sunrise to avoid any heat stress. So at 4:00 A.M. a strange military convoy left Sea World headed for the docks at the Naval Undersea Center on Point Loma. Praise be to the gods that watch over mammals, the release was outstandingly successful and we were able to track the whale by air, by sea, and land late into April, with the last contact off of Monterey California heading north. The risky challenge had been executed successfully.

With the passage of the MMPA, NMFS was given the task of implementing the requirements of the Act. They had to develop several research programs to provide a better understanding of all marine mammals.

Based on observers in the Tropical Eastern Pacific yellowfin tuna fishery, the by-catch of dolphins and other cetaceans was in the tens of thousands. Because of this and concerns of the environmental community and eventually Congress, special emphasis was put on those species associated with that fishery. NMFS did not have the staff at that time, so they enlisted the assistance of some of the marine mammal researchers at the Navy Lab.

As it turned out, my experience with the U.S. Tuna Fleet and NMFS was the salvation of my Ph.D. project. I had a meeting with Professors Bartholomew and Norris about my dissertation project. After much discussion (over Betty Bartholomew's tuna, cheese and pasta shell casserole and a few glasses of wine), we decided I would work on expanding my radio telemetric studies with the common dolphin *Delphinus*.

While testing some new gear modifications for tuna purse seine nets offshore at San Diego, we (Bill Perrin, Steve Leatherwood, and I) made a set on about 1,000 common dolphins. Sadly, the gear modifications did not work, and we ended up with about 150 dead dolphins ranging from newborn to older adults.

That was about 10 percent of the herd. What should I do now? I put in a call to the commandant of the 11th Naval District, Admiral Jack Languille, and inquired about freezer space. He said he would radio back. About an hour later, after having collected samples from about 25 animals, the Admiral called. Since we were removing troops from Vietnam and thus not sending more perishable items to that sector, they had freezer space that was open. When the boat docked, the Navy was standing by with a truck. We loaded all 150 specimens and they were whisked away to the freezer. As a result of this unfortunate turn of events from the dolphins' perspective, I now had a significant sample size from one herd of *Delphinus*. I had my work cut out for me (please excuse the pun), with many samples of *Delphinus* from throughout the eastern Pacific Ocean and data from several cruises and radio tracks to analyze. My question to Drs. Bartholomew and Norris, who had no clue as to the research riches on which I was sitting, was: "Is there enough there to qualify for a dissertation?" They looked at each other and, after a little silence, broke into gales of laughter.

My efforts over the next three years were the analysis and writing of "The Natural History and Other Considerations of the Biology of the Common Dolphin, *Delphinus delphis*, in the Northeastern Pacific." This most certainly was a long way from bioacoustics. During the course of this project, with the help of Clifford Hui, a graduate student, I processed over 500 specimens to determine age, stomach contents, variation in color pattern, differences in skull morphology, and so on. I was actually working on taxonomy. Good Lord! I was metamorphosed from a bioacoustician to a mammalogist!

But I was unaware at the time that there was also a potentially serious down side to this kind of research. I was called by John Prescott of Marineland and told he had three common dolphin (*Delphinus*) from the Eastern Tropical Pacific, caught off of Costa Rica and brought in by a tuna clipper, in his freezer. "They're yours if you come and get 'em," John remarked. I drove up to Palos Verdes, picked up the specimens, and wrapped them in burlap, and headed back to San Diego. Unfortunately, in the midst of research fervor, I forgot about them. About a week later, I was driving in from my home near Rancho Santa Fe on the Pacific Highway when I heard a siren and spied the flashing lights of a state highway police car in rear view mirror.

I slowed down and finally pulled over, wondering what I had done. I was 5 mph under the limit and, as far as I knew, my brake lights were working. When the officer approached the driver's side of the car, he stopped near the rear and then pulled his gun while asking me to put my hands on my head and exit the car. In this position, he escorted me to the trunk of my car and asked me to open it.

When I did, the smell just about knocked both of us down. I immediately knew what the problem was: three dead dolphins, thawed, and rapidly decomposing. I uncovered them as the officer was backing away. I told him the history of these dolphins, he quickly glanced at the bodies, and, satisfied that I was only guilty of stupidity, shut the trunk and left quickly, bent over a bit, with a scarf over his nose and mouth. He looked back just once as if to redeem his action and shouted, "Your tail light on the right is out," and scurried back to his car. I quickly took my specimens to the lab where they were to further decompose, become food for a lot of beetles and enter my database. And, of course, after much cleaning and deodorizing, we sold the car!

In 1974 President Nixon signed the US-USSR Environmental Agreement. This opened the door for United States scientists to communicate and cooperate openly with Soviet scientists working on marine mammals.

This was of particular interest to me since I had been reviewing the Russian literature on marine mammals as a part of an assignment from the U.S. Navy to evaluate what they were doing and how it compared to our research, both classified and unclassified information.

My first trip to the Soviet Union was in October of 1974 to attend the International Theriological Congress. Sam Ridgway and C. Scott Johnson accompanied me. During the visit, we were ushered around to meet with several Soviet marine mammal researchers including Academicians Valdimir Sokalov and A.G. Tomilin. At the start of the meeting with Professor Tomilin, I was introduced by Professor Alexi Yablokov. In response, Professor Tomlin commented that I was much younger than he thought I would be and noted: "I am very pleased to meet a living classic." Sam Ridgway would never let me live that down and for several years would introduce me as a "Living Classic."

One of the main accomplishments of this first trip was establishing contacts with Soviet scientists working on related marine mammal problems. Several other trips followed and this cooperative marine mammal program, almost 30 years later, is still alive and well.

One of the more exciting parts of that program was the opportunity in 1976 to participate as one of scientists on board the Soviet Research Vessel *Vnuchetelnie*. Dale Rice of the National Marine Fisheries Service Laboratory in Seattle, Washington, was the other visiting U.S. scientist. The ship was actually a converted whale catcher from the Valdivastok whaling fleet.

Although the pre-history of the vessel did not seem important at the time, it became very important later. The cruise was to study the oceanographic conditions and distribution of cetaceans in the Eastern Tropical Pacific. This included visiting several oceanographic stations as well as tagging cetaceans. The Soviet cetacean expert on board was Alfred Berzin, who was in charge of tagging whales.

After forty-one days at sea and the end of my part of the cruise, we headed for Balboa, Panama, for re-supply. I was scheduled to fly out of Panama, back to California. When we sailed into the Port, we were radioed by the Port Authority and told to stand off and prepare for Panamanian Officials to inspect the vessel.

As they were going through all the passports, they thought it unusual that two of the crew had U.S. Government Passports. We were then escorted into the dock area and surrounded by Panamanian soldiers carrying automatic weapons. We were briskly informed that we were "impounded" for illegally entering Panamanian waters, and that we were not to leave the ship. Only the captain, his first officer, and the suspected spies with forged official U.S. Passports were allowed to leave with the Panamanian Officials. I requested to speak with the United States military authorities in Panama and was told they were all on vacation. I then requested permission to call my Navy sponsor at the Pentagon to clarify that this was a legitimate U.S.-USSR research project. I gave the ranking Panamanian Officer in charge the Washington D.C. phone number and he called. He put the call on a speakerphone. Personnel in the office of Bob Stone in the Navy Research and Development office answered, and the Panamanian Official requested to speak with Mr. Stone. The receptionist said, "Just a minute, I'll connect you."

Unfortunately, I did not know that Bob Stone was attending a meeting in McClean, Virginia, at the headquarters of the CIA. The next person to whom we were transferred answered, "Central Intelligence Agency." This was not helping the situation.

We finally got Mr. Stone on the phone, and he assured all involved that this was a U.S.-sanctioned research cruise. The underlying problem had been that when the Soviets turned the ship into a research vessel, they did not remove her from the International Registry as a fishing vessel. Research vessels frequently are given courtesy of the port; foreign fishing vessels are not.

But alas, this was not the end to one of most exciting research cruises in which I have participated. Once this problem was solved, I prepared to return home. My Russian shipmates had a little going-away party, and then I packed my sea bag and departed for the airport.

After checking in, the U.S. Customs inspectors had all the checked baggage lined up on the tarmac for identification before loading. Because of concern about drug smuggling, out came the canines. One big German shepherd worked his way down the line of bags and stopped at my sea bag and sniffed and sniffed. The customs inspectors pulled my bag out of the line and requested I unpack it. I responded casually, "It's mostly dirty clothes." They ordered me to unpack the bag, and as I unrolled some of my dirty underwear, out came an interesting assortment of cheeses, salami, and dried fish. It seems my Russian shipmates had given me several presents. Although not drugs, the inspectors immediately confiscated my contraband and sent me on my way. I think I either made one German Shepherd—or a bunch of customs inspectors—very happy.

In the year after I received my Ph.D. from UCLA, I was continuing to get more involved with the Navy's operational programs. However, this was not the direction I wanted to go in for the rest of my career. About this time, Sea World in San Diego was advertising for a director of their new research Institute to be named in honor of Carl and Laura Hubbs. I applied.

In December of 1976, I left the Naval Undersea Center and started as the first director of the new Hubbs-Sea World Research Institute. Although administration and fund raising took up a bigger share of my time than I wanted, I was still able to continue my research. Most of my research effort at Hubbs paralleled marine mammal interests stimulated by the requirements of the MMPA B, a better understanding of the movements and life history components of marine mammals.

There is a tendency for research, especially in marine mammals, to follow sources of potential funding. Fieldwork expanded from the Pacific coasts of North and South America to Alaska and the Antarctic. One of the more important research developments was the use of the U.S. Navy's Passive Sonar. Because of my years of involvement with the U.S. Navy, I was aware of the value of towed linear arrays of hydrophones to not only detect submarines, but also dolphins and whales.

In the late 1970's and early 1980's, there was an increased emphasis on more accurately determining the distribution and abundance of delphinid species in the Eastern Tropical Pacific. We convinced the U.S. Tuna Foundation that evaluation of this technology was a good idea. With support from the Foundation and Sea World, we designed and tested a towed linear array of hydrophones designed specifically to detect cetaceans.

These worked extremely well in tests, both from U.S tuna purse seine vessels and a NOAA Research Vessel, the *David Starr Jordon*. My administrative duties seriously limited my trips at sea. Fortunately, I had as my first ever post doc, Dr. Jeanette Thomas, who was and is a very accomplished biologist and acoustician. Jeanette became the lead investigator on these projects. Although there was a period in which these accomplishments lay dormant, they were re-invented in the 1990's by both NOAA and various university marine mammal laboratories.

About this same time, research on marine mammals had matured to the point where interest was expressed by some researchers in formalizing the conferences on the biology of marine mammals pioneered by Dr. Tom Poulter and the Stanford Research Institute. In June of 1977, George Harry, from the National Marine Fisheries Service Marine Mammal Laboratory in Seattle, sent letters to several of us working on marine mammal-related research regarding the desirability of forming a society devoted entirely to marine mammalogy. The replies George received varied from enthusiastic to very pessimistic. Ken Norris expressed some doubts, mentioning that the American Society of Mammalogists (ASM) included a section on marine mammals. As a result of this, there was discussion between Bob Hoffman and others, including Clyde Jones, about any potential conflict. Because only a few of us were or had been associated with ASM, there was pressure from non-mammalogists, including animal rights advocates, for a separate society. In the end, Ken was the one who worked the hardest to establish the society (and later became its first president). In December of that year, the second Conference on the Biology of Marine Mammals was held in San Diego. At this meeting, discussions took place in Ken's hotel room regarding the formation of the new society.

On another front, research on biosonar was making rapid increases in our understanding of this unique capability. The second NATO BioSonar Conference was held in April of 1979 on the Island of Jersey.

The period of the 1970s brought several advances in electronics, allowing much more sophisticated studies and experiments. Also, it was evident by the increased number of studies concentrating on habitat and the use of biosonar by dolphins, that Don Griffin's original question (how do dolphins in their natural habitat use their sophisticated sonar to discriminate?) indeed had an impact. The conference was dedicated to the work of Donald Griffin, accepted by most researchers as the "inventor of echolocation" through his pioneering work on echolocation in bats.

As was the case at the conference in Frascati, conference researchers from several countries presented the results of their most recent research. Unfortunately, our colleagues from the Soviet Union, who had been extremely active in research on echolocation in both bats and marine mammals, again were not able to attend.

The 1980's were very busy at Hubbs and even with the so-called 50% research time and 50% administrative time promised by the Board of Directors, my time management calendar turned out to include 10% research—if I could fit it in—and 90% administrative and fund raising. Also, at Hubbs I had my first opportunity to work with many talented graduate students who were to become pre-eminent scientists especially in the fields of marine biology and bioacoustics. In addition to Dr. Thomas, there was a host of notables who paraded through the "Halls of Hubbs," Dr. Ann Bowles, Dr. Steve Leatherwood, Dr. Randy Davis, Dr. Giuseppe Notarbartolo-Di-Sciara, and Dr. Ron Kastelein, just to mention a few. My teaching/mentoring instincts were satisfied.

Work on promoting and expanding the use of passive acoustics for marine mammal population studies was continuing. In 1982, as part of a National Science Polar Programs grant, we were allowed to put our acoustic array on board the U.S. Coast Guard Cutter, *Polar Star*. She was used as the vessel to carry an international inspection team to most of the Antarctic International research stations for inspections called for by the Antarctic treaty.

The tests were successful and in addition to excellent recordings of noise from an icebreaker and sea ice, our team recorded the Southern pilot whale. On the trip home, the vessel was scheduled to stop over in Rio de Janeiro.

My colleague, Dr. Joseph Jehl, a field ornithologist, thought this an excellent opportunity to test the array in warmer waters. Also, it provided an opportunity for me to whale-watch and for him to bird-watch in parts of the world's oceans new to us both. This diversion also facilitated my contact with several member of the Brazilian Navy also interested in array technology. And, of course, it was a chance for me to get back at sea for a little longer.

We made arrangement to tow the array through the Brazilian Navy's sonar test range for calibration. Dr. Jehl and I flew to Rio and spent a few days making arrangements with the Brazilian Navy and performing other activities before boarding the USCG icebreaker *Polar Star*. As we boarded the vessel and gave the officer of the watch our names, he informed me that I was to go immediately to the Captain's Office. Based on my extensive military experience (or fear of the military), I responded immediately. I introduced myself to the Captain whereupon he informed me that I had received a call from the White House and was to call a White House number as soon as I arrived on board.

He suggested that I make a collect call. His reasoning was excellent: If it were an important call, it would be accepted; if it were some White House staffer interested in getting some information on whales for a 5th grade child, they would not. The White House accepted the call.

I was requested to return home via Washington, D.C., for an important appointment with the Director of White House Personnel. I honestly did not have a clue as to what this was all about. It finally came out that I was being interviewed for a possible Presidential appointment to the Marine Mammal Commission. After much questioning, both written and oral, I was informed that I had been selected by the White House to fill the position of Chairman of the Marine Mammal Commission. I then spent the next several weeks preparing for my senate confirmation hearing. The United States Senate confirmed my appointment, and, on 30 March, 1984, I was sworn in, thus starting my biopolitical career.

For the next five years, most of my effort was in working on United States and international environmental policy and fighting with James Watt, the Secretary of Interior. Secretary of Interior Watt seemed intent on undoing everything that former Secretary Walter Hickel had accomplished with marine mammals. As the Chairman of the Marine Mammal Commission, I had the opportunity of experiencing the inner workings of the International Whaling Commission (IWC). Previously, I had attended several meeting of the Scientific Committee but usually avoided the very political Commission meetings.

I attended my first Commission meeting as a member of the U.S. delegation in the summer of 1983, one year after the passage of the controversial moratorium on whaling. I remained a delegation member until 1988, working as an advisor to the then United States Commissioner to the IWC, Dr. John Byrne, who was also the Administrator of National Oceanic and Atmospheric Administration (NOAA).

In 1985, Dr. Byrne resigned as the Administrator of NOAA and was replaced by Dr. Tony Calio. At the 1985 Commission meeting in Malmo, Sweden, Dr. Calio got his initiation into international whale politics. Everything but whale blubber was flying through the air at that meeting well, perhaps not that effusive. It was shortly after that experience that he called Hubbs Sea World Research Institute and asked that I come back to Washington as the Assistant Administrator for Fisheries.

After the tragic death of then Secretary of Commerce, Malcolm Baldridge, Dr. Calio resigned from the Federal Service and NOAA. In 1988, I was asked to replace Dr. Calio as the Under Secretary of Commerce for Oceans and Atmosphere. Thus, I was given an opportunity to see an entirely different and broader side of environmental management.

Unfortunately, the most significant U.S. environmental disaster happened on my watch. In March of 1989, the oil tanker, *Exxon Valdez*, went aground in Prince William Sound. The impact on marine mammals, especially sea otters and pinnipeds, is still evident 13 years later. Working on the case against EXXON, clean-up plans, and damage assessment took up most of the remainder of my term in office.

This tragic environmental disaster resulted in a major change in my focus, so that my desire to do research and teaching/mentoring became activated even more strongly. In July of 1989, I retired from Federal Service and accepted a position at Texas A&M University at the Galveston campus as a professor of marine biology and Dean of the Texas Maritime College. I was anxious to get back to my research and also work with graduate students. Thanks to Dave Schmidly's foresight [Schmidly was serving as Dean and CEO of the Galveston Campus: eds.] as to the value of a marine mammal research program, I became a member of the Wildlife and Fisheries graduate faculty.

Because of my very recent experience with the *Exxon Valdez*, it was very clear that the direction of the curriculum of the College needed more emphasis on marine environmental issues. Fortunately, I was able to hire one of the lawyers who had worked on the Exxon disaster, Dr. Wyndylyn von Zharen. This was one of the best administrative decisions of my career in academia and the birth of a whole new curriculum at Texas A&M Galveston.

During the 1990's, emphasis was being placed on conducting interdisciplinary research on large marine ecosystems such as the Gulf of Mexico. It had become obvious that to understand the distribution and abundance of marine mammals, we needed to understand their habitat and its dynamics. Again, environmental legislation was driving the funding and thus the research.

To continue to lease the deeper water offshore oil and gas leases in the Gulf of Mexico, the Minerals Management Service (MMS) needed a better understanding of the impact of exploration and productivities on endangered or threatened species. There were several sources of information on the distribution, abundance, and diversity of cetaceans in the Gulf of Mexico, including the Texas Marine Mammal Stranding network founded by Dave Schmidly. Cetacean stranding information has been systematically collected since the late 1970's. Other directed studies, historic whaling records, animal strandings, and opportunistic sightings have expanded the list of cetacean species known to occur in the Gulf (Jefferson and Shiro, 1997). However, until recently, relatively little was known about cetaceans inhabiting deeper waters of the Gulf of Mexico. The MMS and U.S. Fish and Wildlife Service supported aerial surveys of birds, marine turtles, and cetaceans in the Gulf from 1981-82 (Fritts et al., 1983).

The most extensive survey of cetaceans in the offshore waters (100 to 2,000 m deep) of the north-central and western Gulf of Mexico was conducted jointly by Texas A&M University, National Marine Fisheries Service (NMFS), and the Southeast Fisheries Science Center beginning in 1992 and was called the GulfCet I Program (Davis and Fargion, 1996). Fortunately for me, I was one of the principal investigators on this project (Figure 5). This study provided synoptic information on the distribution and abundance of cetaceans using both visual and acoustic survey techniques including:

Figure 5. Bill Evans (center) aboard *The Texas Clipper* during the 1996 whale survey in the Gulf of Mexico.

Shipboard Visual Surveys. A total of 21,350 km of transect was visually surveyed during the GulfCet I shipboard surveys. The number of on-effort sightings each season ranged from 14 during fall to 509 during spring. Nineteen cetacean species were identified during 683 sightings made on-effort. Most of the survey effort occurred during the spring, with the least effort during the fall.

Shipboard Acoustic Surveys. The acoustic surveys were conducted concurrently with the visual surveys. A total of 12,219 km and 1,055 hours of acoustic effort were completed. On-effort acoustic sampling occurred 95% of the time. A total of 487 acoustic contacts were recorded. Of that number, 124 contacts were from 12 identified species. Sperm whales were the most commonly recorded species, accounting for 56% of identified contacts. The most commonly recorded small cetacean was the pantropical spotted dolphin, with 22 contacts. A single recording of an unidentified baleen whale was made, probably a sei or Bryde's whale, based on its spectral characteristics. At the time, this was the first large scale survey done using the towed passive acoustic array technology.

Marine mammals are among the most difficult groups of mammals to study in their natural habitat. As we discovered in the *SeeSea* studies and other underwater studies by Norris (1974), what takes place in their underwater world is far more dynamic than what is observed at the surface. Fortunately, technology has advanced significantly since those first studies of the 1960's–70's and has provided at least a partial solution to these problems. Advancements in computer hardware/software and miniaturization of video and acoustic recording systems allow not only records of sounds, but also of associated behaviors (Davis et al., 1999). As satellite technology advances, our ability to not only track individuals, but to monitor their habitats, is becoming more sophisticated. These technologies will be key for use in the 21st century. But students of mammals, and marine mammals in particular, should not underestimate the value of good, hard fieldwork and visual observation. Also, in the study of marine mammals, the importance of museum collections should not be dismissed. In the heyday of the tuna purse seine fishery, unidentified delphinids were being found on a regular basis. Without the skeletal material available in some of the world's larger collections, we would have a host of "new" species that are not really new species at all. This is especially true in the case of several genera like, *Peponacephala*, *Stenella, Delphinus* and *Feresia*. Several genera known only from skull fragments are now known to be fairly common in the oceanic waters of the Atlantic, Pacific and Indian Oceans. The specimens of *Delphinus* that I collected have been used to determine that in the waters of the Eastern Pacific, there are really two species of *Delphinus, D. delphis* and *D. capensis*. I hope in the future that we can convince new graduate students of mammalogy that there are still many opportunities in the study of marine mammals that are almost as exciting as studying terrestrial mammals.

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COVETING OTHER MAMMALS

Rollin H. Baker

Rollin H. Baker was born in Cordova, Illinois, on 11 November 1916. He received his B.A. in Zoology from the University of Texas in 1937, his M.S. in Entomology from Texas A & M University in 1938, and his Ph.D. in Zoology from the University of Kansas in 1948. He is Director Emeritus of The Museum and Professor Emeritus of Zoology and of Fisheries & Wildlife at Michigan State University.

My interest in mammals dates from my earliest years, aided and abetted by a mother who was a determined birdwatcher and by a father, a vertebrate paleontologist turned stratigrapher, who provided me with abundant annotated experiences in natural history while on field excursions in varied environments mostly in California and Texas. But it was from my reading of books by such authors as Burgess, Seton, and Hornaday that I gained my first love for mammals, and this allure became thoroughly ingrained in 1927 in suburban Houston. There, I suddenly found myself literally surrounded by semilethargic hispid cotton rats (*Sigmodon hispidus*). As they huddled with nervous twitching in their grassy haunts, they allowed me to scoop them up into my red wagon and to transport them home for 'show and tell.' That hands-on experience was like a guiding light focusing my attention on vibrant and beady-eyed small mammals. And how very pleased I was to read years later an article by pioneering Texas mammalogist John K. Strecker who observed in the Waco area this very same cotton rat explosion (*Journal of Mammalogy*, 10:216-220, 1929).

After getting my necessary undergraduate exposure to basic but strictly laboratory science, I suddenly found myself immersed in graduate work in field mammalogy under not one but under two of the most savvy outdoor types of their generation, Walter P. Taylor and William B. Davis. And through them, I had also opportunities to meet many of my heroes of the old regime of field men like Bailey, Anthony, Osgood, Goldman, Howell, Preble, Miller, and Jackson. From these incomparable role models, I gained both inspiration and exceptional professional know how - to focus perhaps on a particular mammal; to ask quesions yet unanswered by others about its ancestry, distribution, and life style; to design investigational methods to determine answers; and finally to summarize and publish the findings. And with gracious and patient coaching from Walter P. and Bill, I published along with a fellow enthusiast my first studies in that most illustrious *Journal of Mammalogy* (9:418-423 and 9:505, 1938). Thus, I was suddenly off and running, and as luck would have it, in every salaried position that I ever held, I published papers about those most special creatures, mammals - even as a naval officer in a preventive medicine research unit I studied insular mammals (*Ecological Monographs*, 6:393-408, 1946).

After my several pre-war years of unadulterated fieldwork with game/fur mammals as major wildlife targets, I shifted to academia and again was fortunate to obtain positions where I could enjoy a three-way mammalogical commitment (in the field, in the museum, and in the classroom), first at The University of Kansas and later at Michigan State University. And I took every opportunity under this umbrella arrangement to combine these objectives as effectively as I could.

Now in 'active' retirement at age 87, I still tend to mammals - this time mostly those in the Texas county of Colorado where I am still asking questions and looking for answers. Why? Simply because some mammals that I studied there back in 1938-1939, like, for example, thirteen-lined ground squirrel (*Spermophilus tridecemlineatus*), long-tailed weasel (*Mustela frenata*), and eastern spotted skunk (*Spilogale putorius*), are no longer present. And I ask myself why are they gone? Perhaps I'll find out, since I'm going to keep on looking as long as possible!

So, ever since 1938 when I was 21 (Fig. 1), I've done my very best to be a productive mammalogist. And that means, at least from my own viewpoint, that I have contributed through publications in all of those many years since then to our knowledge of mammals. And it's been so very much fun because, as one might say I've done this, in part, to satisfy my inner self. And, of course, that's what one's career is supposed to do!

Now, at age 87 (Fig. 2), I can see it in your eyes. Your expressions indicate that you rather wish that old codger-type mammalogists would get lost in the foliage while running their trap lines and fade away. But oldsters like to nose around and look over the shoulders of the new players. In truth, however, their function may be most useful when they tell you what they did wrong so that you won't repeat their mistakes!

And I recall a certain one-on-one confessional to which I, at age 23, was a party. This was between the affable Vernon Bailey and myself, when we visited at a North American Wildlife Conference in Washington in March 1940. After he told me about some of his turn-of-the-20th-century fieldwork in Texas, he reminisced about what he would have done differently had he the chance to do it all over. Then, he mentioned what he regarded as his greatest field blunder.

It was when his brother-in-law sent him to collect mammals in North Dakota. While there, he often used train transportation to get from one collecting area to another. And at each train station was a huge pile of bison bones, range-gathered, and dumped at a siding. These were being readied for shipment to such rendering plants as Detroit's Michigan Carbon Company that produced bone-char and fertilizer (from the bones) and neat's-foot oil (from the hooves), etc. And all that wiry little guy did, so he told me, was to stare at the remnants of the slaughter, shed a few tears because of the herd's demise, and go on trapping bean mice.

And not too many years later, and well after the prairie had been cleansed of this hunting residue, did he and his Biological Survey buddies realize that no one had collected museum specimens representing skulls and other bones typical of either the so-called

Figure 1. Rollin H. Baker in the field in the vicinity of Ciudad Muquiz, Coahuila, Mexico, June 1938.

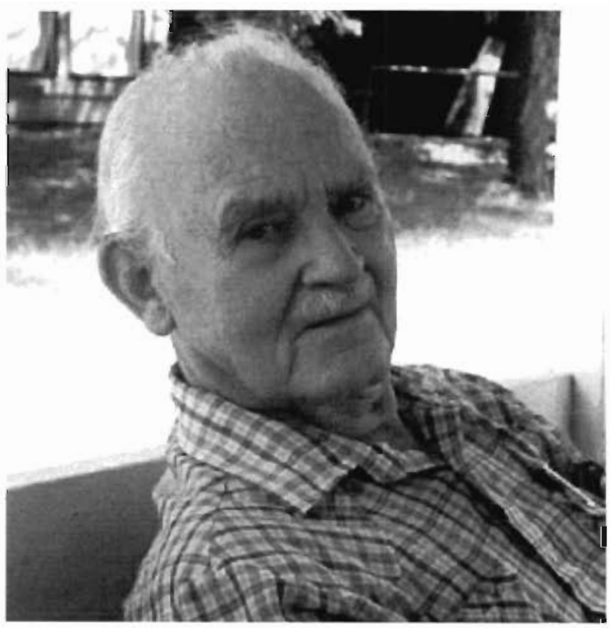


Figure 2. In active retirement at Muddy Creek Place, Colorado County, Texas (August 2001). (Photo by Betsy Baker Prescott).

northern herd or the so-called southern bison herd. I failed to ask him how boss Merriam [C. Hart Merriam, Director of the U.S. Biological Survey] took that gross dereliction of duty!

Despite that such human frailties might also prevail in my case, like Bailey and his dedicated associates I rushed headlong into the field of mammalogy and never wanted to escape. And so today I will try to pass along to you advice, based on my experience, on how to become addicted to mammalogy. I will frame my comments into several areas: nurturing my struggles; helping train young players; career focusing; gaining rewards; assignments for the future; and finally mentioning a few do's and don'ts for new adherents.

Luckily I had a father who majored in natural science at Oberlin, focused on vertebrate paleontology at Chicago, and opted for stratigraphy at UC-Berkeley. When he was home from field trips, he provided me with assorted travel books written by the likes of Sven Hedin, Richard Burton, and Marco Polo and natural history volumes by William Hornaday, William Berryman Scott, David Starr Jordan, and my favorite, Alfred Russell Wallace. On outings in the 1920s in such states as California and Texas, most family members ignored father's on-the-road lectures about the out-of-the-window features of the environment, its geology, flora, and fauna. But to me these instructive comments filled my young mind with insights about doings in the real world - and I found the biological component was the most challenging! Come to think of it, not one of my children took the bait when I presented them with similar travelogues.

Reduced family finances in the Depression of the early 1930s helped squash my notion of majoring in "field" Zoölogy at Cornell. Instead, my choice was a Texan institution with a famous but totally lab-oriented department with the classroom windows often too high for a look at the real world beyond. But my love affair with mammals was beginning to jell. I read the latest numbers of the *Journal of Mammalogy* and obtained my first in-depth examination of a mammal albeit pickled at the time. I was required in comparative anatomy that I "collect" my very own specimen for this lab dissection study. It proved to be one of my very first mammalian captures - done in sneaky fashion while the cat's owner wasn't looking. In summers, however, I gained field experience first with groups of geologists and later as a modestly-paid student wildlife technician for the National Park Service and stationed at a Civilian Conservation Camp (CCC) in the Basin of the Chisos Mountains.

So by the summer of 1937, in the midst of hard times, at age 20, and armed with a B.A. in Zoölogy from the University of Texas, I chanced to visit at Texas A&M. And bingo! I met Walter P. Taylor and William B. Davis who were just developing a curriculum in wildlife studies. And these two savvy mammalogists invited me to participate and almost surrounded me with small mammals, and I have been so ever since.

I was fortunate to be at the right place and at the right time not only to become a part of the Taylor/Davis mammalian combo but for positions as a field-oriented mammalogist ever since. These stimulating opportunities allowed me to serve as a mammal collector for a UT/UNAM-sponsored expedition in Coahuila (summer 1938); as a field biologist, Texas Cooperative Wildlife Research Unit (1938-1939); as a wildlife biologist, Texas Game, Fish & Oyster Commission = Texas Parks & Wildlife (1939-1943); as a Naval officer assigned as mammalogist for U.S. Naval Medical Research Unit NAMRU-2 in the western Pacific (1944-1945) and at the Smithsonian (1945-1946); and finally as an instructor in mammalogy at the University of Kansas (1946-1955) and Michigan State University (1956-1982).

I not only stood a few coeds on pedestals but; likewise, did the same with great naturalists. My hero-worshiping attitude began in childhood when I was exposed to the nature writings of Thornton W. Burgess and Ernest Thompson Seton. And by attending pre-WWII meetings of the American Society of Mammalogists and The Wildlife Society, I begot at least hand-shaking and brief conversational acquaintances with many early 20th Century field workers - E. T. Seton himself, W. H. Osgood, A. H. Howell, N. A. Preble, Aldo Leopold, K. P. Schmidt, Herbert Stoddard, H. C. Oherholser, H. E. Anthony, R. M. Anderson, A. Wetmore, M. W. Lyon, A. B. Howell, H. H. T. Jackson, G. S. Miller, Jr., and my two special idols, Vernon Bailey and E. A. Goldman. Just basking in the glow of their presence and listening to them comment on their early-day adventures were unforgettable stimuli!

I suppose that my luckiest break came in 1944 when Remington Kellogg, Curator of Mammals at the U. S. National Museum [now National Museum of Natural History], recommended David H. Johnson and myself, both naval reserve officers on active duty at the time, for billets as mammalogists in the western-Pacific with famed U.S. Naval Medical Research Unit Number 2 [NAMRU-2]. This biomedical outfit, loaded with famed microbe-hunting specialists, was dedicated to shielding combat-ready troops from diseases for which they had little or no built-in resistance. It was my job to collect Micronesian vertebrate specimens galore for virologists, bacteriologists, parasitologists, etc., to appraise for potential GI-infecting pathogens. And this experience reminded me that two of our prominent senior mammalogists at that time had also earned a few of their spurs in the same way as rodent control specialists in soggy French trenches with the AEF during WWI - Major E. A. Goldman and Sergeant Remington Kellogg. So Dave and I were in good company, and we tried hard to follow in their traditions. And later, I pulled a few strings in Washington to get Kansas grad student Knox Jones, a combat-ready reserve army officer scheduled for active duty, switched to a medical service billet and assigned as mammalogist with a reservoir/vector study outfit in connection with troop maladies in Korea.

Most old-timers had gone to the Happy Mouse Hunting Grounds by the time that the Navy ordered me to post-Pacific duty at the U. S. National Museum in late 1945. Fortunately, spry and alert Major Goldman was still coming to his office in the old Biological Survey quarters in the ground floor mammalian wing. And I had ample time to become well acquainted since he and Stanley Young generously invited me to join them for lunch across the street and in the cafeteria of the Justice Department Building. So each noon for several months, I would quietly listen as Stan and the Major discussed aspects, over lunch, of the two books that they were co-authoring, volumes on wolves and on the mountain lion.

In between, however, Goldman told me about some of his turn-of-the-century adventures as a field mammalogist in México. Since I was already acquainted with Trans-Pecos Mexican border biotic communities, looked longingly across at the high Sierra del Carmen from the Chisos' South Rim, and had collected mammals in Coahuila, I became totally fascinated on learning of the enormity of Goldman's adventures. And guess what? My Mexican urge paid off! Because by 1950, I was back in Coahuila completing unfinished business and seeking other study areas in Latin America that contained highly complicated topography, diverse plant cover, and little-known mammalian life (Figs. 3, 4).

And were I thirty or forty years younger today, I just might not be around to attend this impressive symposium. Instead, I might be in South America. Let's see, I could be, for instance, gathering data in order to compare rodent distributions and diversities in upper

Figure 3. Preparing lunch at a dry camp in the desert shrub of central Coahuila, Mexico, March, 1952.

Figure 4. University of Kansas Mexican field expedition. Party members are perched on a precipice overlooking the spectacular barranca country along the Durango-Sinaloa border. From left to right: South Van Hoose, Charles Fugler, Robert Packard, Robert Webb, Rollin H. Baker, John Keever Greer with Robert Dickerman behind him (24 June 1955).

humid belts from the eastern and western slopes of the Andes. At the moment, I'd probably be concentrating in the south, since it is currently politically hot at the north end. Sure, I know that the good Lord watches over children, drunks, Marsh's old-time fossil hunters in the Bighorn Basin, and field mammalogists, but like the man said about everyone being honest but always cut the cards, why take too great a chance!

When I finally became an academician with a specialty in mammalogy, I was forever looking for recruits. And I was just one of a team of professorial salesmen peddling career objectives to a throng of students. Most wandered by without a notion of what careers they wanted to follow. But sooner or later my watching and waiting paid off when a student appeared who had known all along that she/he wanted to study mammals. Other students, already interested in the subject, were attracted to my program when they spoke with me at meetings, received my answers to their letters of inquiry, or were recommended by former students or by such professorial colleagues as Nebraska's Otis Wade or Swarthmore's Robert Enders. And I never neglected to appraise the abilities of interested students who toiled immeasurably hard to gain a B grade in statistics. They could be "late bloomers" and understood the necessity for having a superior work ethic.

To cement career interests, I dangled jobs as museum assistants and laboratory instructors in front of their noses or tested their potential worth in the field by subjecting them to assorted deprivations on multiweek field trips. Sometimes such drastic action bore fruit - the love of dry camps in the desert and wet camps in the tropics. At least it did in my case by the time that Doc Davis [William B. Davis, Texas A & M University] got through with me.

Once students had survived a beginning course and wanted more, one way that I enticed them to delve deeper into the subject was by inviting them, both grads and advanced undergrads, to join me for weekly "noncredit" lunch-hour brownbag seminars. Sure I even made coffee for them. After everyone had hurriedly eaten, I would usually offer a brief Johnny Carson-type current-event dialogue about what was new that week in mammalogy - mostly garnered from articles that appeared in *Time* or *Science*. Then a student presented a more formal 15-minute discussion on some in-depth aspect of the subject. To enliven each post-presentation question-asking/answering session, the student handed to each participant one week in advance an outline of her/his subject matter. This gave each seminar member a chance to examine the text of the next week's discussion and be primed to ask intriguing questions.

I had one of these spontaneous programs in continual operation at Michigan State University for more than 15 years with all kinds of worthwhile results. And what a way for the beginners to gain breadth in the subject, to learn to make formal presentations, and to gain confidence by on-their-feet answering intentionally-dirty questions! And I delighted in watching their growing interest and in evaluating their abilities, not to mention becoming updated myself with the latest aspects of my own science. And I kept track of many of those participants who didn't go into mammalogy as such but opted for positions in the bulging field of wildlife biology. However, others chose to become physicians or CEOs. When these became rich, naturally they might donate field funds. Worth asking! And if they didn't take on mammalogy as a vocation, I always encouraged them to consider mammal study [also bird watching] as an avocation.

On the subject of dissertation selection, it was always my policy to have student research projects apart from mine. Then I was most gratified when these potential scholars came up with original lines of research (at both the master's and doctoral levels), although I might have prompted them a little. They had the feeling that their topics were their very own, and, in my view, that added to their inducement.

To be frank, I acquired this method from observing how E. Raymond Hall helped his students select their projects. In short, they weren't members of a research team but operated independently.

My job was, of course, to evaluate each proposed graduate research project as to its scientific worth and downsize it sufficiently so that it could be realistically accomplished in a reasonable time. And from my standpoint a reasonable length for a post-master's doctoral program was three years - first year: Pass qualifying inspections, acquire a guidance committee, complete formal course work and other requirements, pass doctoral written/oral prelims, get preliminary aspects – including literature search - of proposed research underway; second year: Gather dissertation data in field/museum/lab situations; third year: Write the dissertations, gain guidance committee approvals, pass final orals, and obtain the degrees. What's so hard about that? But to be on the safe side, get all of those requirements down on paper in advance and signed by committee members!

For students wishing to conduct research in Latin America, I often had them spend a field summer with me in México and/or attend the tropical ecology course offered in Costa Rica by OTS (Fig. 5). By such activities, a student could also get a paper or two in press. And as an added requirement, again following E. Raymond Hall's edict, I would never accept a dissertation unless it was in a form suitable for publication! In short, put all of the "busy" work in a big fat dissertation appendix.

And how did I expedite this activity? By personally digging up money to support data gathering for these dissertations. And I was fairly good at this task and acquired funds, very little from tax-generated sources, to send students for their research years off to such places as Chile, Argentina, Panamá, Namibia, Venezuela, Brazil, Colombia, México, Canada, Kansas, islands in Lake Erie, Michigan's Upper Peninsula, etc. And while they were off campus, I could sneak back to México each summer with another field party to catch mice for my own pet projects and to stimulate more incipient mammalogists.

Non-monetary rewards for me included surprises wrought by ingenious and resourceful students who came up, on very their own, with novel data-gathering ways. One illustration, if I may, concerns a study of housecat predation on meadow voles to which metal ear tags were attached. Conveniently, the housecats that foraged in the field that contained their potential ear-tagged lunches, defecated in a farmwife's flower garden, where the student could easily gather the feces periodically. To determine which of these numerous

Figure 5. My expedition to Mexico in the summer of 1964 featured the establishment of a supply base in Mexico City. Party members are standing on the flat summit of the Pyramid of the Sun at Teotihuacan. Left to right: Rollin H. Baker, Daniel Womochel, Mike Petersen, Carleton Phillips, and Photographer Charles Warner, Jr. (Photo by Mary Baker).

tight-fisted fecal pellets held undigested metal tags, the student, unbeknownst to me, persuaded a radiologist in the MSU Vet School to X-ray them. For a student to show such originality helps make a professorial day livable!

Now let us assume that as a student of mammalogy you have completed your doctoral degree, perhaps have had a post-doctoral assignment, have in press a concise summary of your Ph.D. research project, and possibly have published a few other papers. You are now looking for career employment, with visions of an exciting field-oriented career.

At this crossroads, you may opt, as I once did, for an attractive job as a state/federal scientist. Or you can accept a position, as I also once did, as an instructor in academia. In the former you are apt to be restricted in research project selection and ultimately your rank/stipend may depend on the number of underlings whom you supervise. In short, your personal hands-on field programs may stagnate when you find yourself doing nothing but directing others. And once you enter this system you can become entrenched.

Consequently, to be a career-long research mammalogist, your best direction is to accept a position in an institution of higher learning. Of course being associated with a major research universities has advantages, but sterling careers have also been forged in four-year liberal arts colleges. In either case, however, you get your foot in the door but must still prove your worth to make your way.

And you will need plenty of smarts, colleague/supervisor compatibility, favorable student interactions, energy, resourcefulness, public relations know-how, diplomacy, field trip leadership savvy, fund gathering acumen, ability to delegate, productive scholarship so to win "publish or perish" tournaments, and finally don't ever forget the ability to do more than your share in promoting a very necessary and often difficult career stimulus - marriage tranquility. Also keep in mind that there are very few Osa Johnsons around, and that most spouses prefer simple stay-at-home types - like wealthy 8 to 5 bookkeepers.

And don't ever forget that your major professor told you that your doctoral dissertation would not be the best piece of research that you would ever complete! And finally, take time while you are a graduate student to examine closely how your highly successful major professor conducted himself. Take notes describing his capabilities. Copy his positive operating features and try not to emulate ones that, shall we say, seem a bit on the negative side.

A colleague once called me a non-quantitative, globetrotting mammalogist who used mouse catching in remote places as an excuse to leave the campus and get out of attending faculty committee meetings - required duty for every warm or semi-warm academician. Worse than that, this critic also accused me of publishing scads of papers concerning range extensions of obscure small mammals not worthy of notice. Sure I have published a few notes in the *Journal of Mammalogy* (from vol.19 in 1938 to vol. 79 in 1998) and elsewhere. But adding to our knowledge is what being a scholarly mammalogist is all about - gathering previously unknown data, summarizing the findings, pontificating about their significance, and publishing!

Building a respectable and noteworthy publication portfolio in your first 20 or so years is the name of the game. So what do you do first to accomplish this objective? You study mammals that are poorly known, are preferably geographically handy, can be investigated inexpensively, may provide potentially publishable data, and will allow you to gain paper-numbers. Some might be progress reports leading eventually to major monographs. E. Raymond Hall, for example, demonstrated a respectable publication routine by writing a series of short papers as byproducts of his long-time mammalian investigations in Nevada and before his monograph on the subject appeared in book form. And, as a reminder, this prolific individual had no help from today's computers in order to organize his data!

As a researcher you might not need to go as far away as another state since the mammals behind your back fence or in a nearby nature center might become the subjects of an important 6-year inquiry! And so, you end up in your late fifties or early sixties with a substantial publication résumé and a certain degree of personal satisfaction. Sure you wish recognition from others but, like golf, this is a game that you are always playing against yourself. And if some one complains about the findings in one of your publications, place the blame directly on the sagging shoulders of the over-worked journal editor who accepted it.

But there is more, since at some stage in your late maturity you will have two choices. One is that you can retire gracefully while you are ahead, cease to go to your old office, resign from the *American Society of Mammalogy*, and instead collect stamps, gamble in Las Vegas frequently, dabble in politics, watch TV, play with grandchildren, or delve into organic gardening. The other choice is that you can keep in touch with mammalogy and even update yourself although the contents of some of the late-published papers will be difficult to comprehend.

And you mustn't forget that at or near retirement time you will have accumulated a substantial breadth of experience in mammalogy. In addition, you will have attained a degree of wisdom that allows you to look at the field broadly and impartially.

In short, in your youth, you mostly examined trees (the specifics) in your "formative" career phase. Once having attained all of those bits and pieces, as a senior you may now try to fit some of them together and, instead, look at the forest in this "coalescent" career phase. How do such mental gymnastics help answer major questions and/or apply somehow in the real world of mammals? You owe it to the profession to contribute in this fashion!

Such thoughtful broad-based papers may be the ones for which you will be best remembered. Allow me to mention a few examples: George Gaylord Simpson may be remembered more for this examination of the "evolutionary process" than for his work on Paleocene mammals; Joseph Grinnell more for his use of the term "niche" than for his contributions to our knowledge of warm-blooded vertebrates; W. D. Matthew more for his 1915 essay entitled *Climate and Evolution* than for his contributions in vertebrate paleontology; William H. Burt more for his short paper on "territoriality" than for assorted other works on mammals; Aldo Leopold more his "land ethic" than for his writings on wildlife management; Phil Hershkovitz more for his broad-based 1958 paper on the systematics of Neotropical mammals than for his work on Primates or Colombian mammals; C. Hart Merriam more for his floral/faunal distributions in "life zones" than for an unbelievable array of mammalian new species descriptions; and of course Charles Darwin more for his contributions to the "evolutionary theory" than for his work on barnacles.

All of us here today have or will look at the "big picture" when considering the systematics, distributions, life styles, etc., of mammals. I've even been guilty of the same in drafting short "essays" for the Newsletter of our Texas Society of Mammalogists. A brief example of one of these follows:

"Are Habits of Texas Mammals All Bad?"

Human parents often train - or try to train - their progeny to "indulge" in the vicissitudes of life in moderation! However, parents of our non-human mammals apparently do not instruct their offspring to follow such a rigid philosophy. Rather, their unenlightened progeny actually seem to "delight" in engaging in unrestrained conduct that exposes them excessively to their hungry enemies.

And these "devil-may-care" mammals endanger themselves when (a) they abandon all caution to pursue mates in order to satisfy their sexual drives and (b) when they consistently follow the same trails, use the same dens, defecate in the same sites, congregate in the same ways, feed in the same areas, etc. Sure they may "prefer" habit-prone life styles uncluttered with behavioral no-nos, even though these antics are "adored" by clever canids, mustelids, felids, strigids, tytonids, butionids, serpentids, etc. - making the game of catch-em-and-eat-em often effortless.

So, why do our assorted terrestrial mammals ranging in size from soricids to cervids/bovids persist in using life style features that apparently "needlessly" cause them to be exposed to rampant predation? Why aren't the natural selection processes working overtime in order to fix at least some of these "predator-conspicuous" habits?

Of course we need to inventory an area's mammals (as well as their habitats and associates) before we can begin to understand how and why they live there. In that part of our Western Hemisphere that is east of Panamá, this needed inventory has had a slow start and has a long way to go to be completed. In fact, we could use a generation or two of field-savvy Neotropical mammal collectors carrying on like super E. W. Nelsons and E. A. Goldmans egged on by dictatorial C. H. Merriams to ferret out the mammals from every nook and cranny. And then we will need O. Thomases, W. H. Osgoods, A. Cabreras, Phil Hershkovitzes, and Syd Andersons, equipped with both classical and modern systematics know-how, to classify them and define their distributions. We need these data before we can be truly picky about such details as the ecology of these Neotropical mammals.

However, in that part of our Hemisphere that is west of Colombia, reconnaissance mammalogists, by 1960, had nearly completed the species inventory and pinpointing mammalian distributions. And looking back, we can only be impressed with the zeal of the many dedicated workers, who spent parts of their careers in the field examining mammalian populations in assorted environments and the other parts of their careers in museums using collected voucher specimens (skins and skulls) to determine species and how they vary geographically (Fig. 6). And designating "recognizable" subspecies was a favored pursuit. And what fun it was - and also what a stimulus it was to undertake intensive fieldwork! Yet future mammalogists must, sooner or later, determine the creditability of this "bucket of fur-bearing worms" using both traditional and now modern techniques in systematics examinations. But it should be challenging as well as rewarding – especially for adherents who maliciously enjoy sinking subspecific names of pocket gophers.

A major part of our future in field-oriented mammalian investigations is to gather intimate details about each mammal's private life - its breeding biology, resource needs, intra- and interspecific interactions, density, behavior, limiting factors, economic importance, response to human intrusion, other environmental re-

Figure 6. Rollin H. Baker studying his collection of cotton rats in the Division of Mammals, Michigan State University Museum (19 April, 1982).

lationships. And don't be intimidated by some of the precise ecological work done by ornithologists. Mammalogists can do as well investigating their subjects – if not better!

Such intensive studies can be exciting and revealing but take considerable time, patience, ingenuity, hard work, and long exposure to the vagaries of the environments in, for example, a sweaty cloud forest or a skin-parching desert. And these assignments require much more money than do itinerant mouse-trappers. Besides needing pricey semi-permanent field headquarters, please ponder the expenses of buying all of those new-fangled remote sensing devices and other gadgets that might be applied to help one better understand mammalian ways in the wild.

Consequently, we need to train more inquisitive, energetic, observant, ingenious, persevering, and "hands-on" mammalogists to conduct these important missions. And our modern courses ought to be emphasizing more field methodology including a re-introduction of that basic "how-to-do-it" need - the preparation of mammals as museum-type voucher specimens for museum study and the preservation of innards for lab studies. Meanwhile, our less-field oriented colleagues need to augment existing details concerning both descriptive and comparative aspects of ontogeny, behavior, physiology, genetics, and especially long-neglected aspects of anatomy. And before the academic dean decides to convert your mammal range into more classrooms, use your ingenuity to demonstrate the scientific worth of a museum case filled with study skins and skulls of tree squirrels. There are full-time careers awaiting museum workers and their students to study an array of rarely-examined attributes of the preserved parts of deer mice and bobcats that may now be lodged in rarely-opened museum research cases these days.

Be wary of busy work that doesn't produce publications. Avoid "wasting" time gathering field data unless there is some assurance that your findings can be summarized and can appear in print. Design potentially publishable studies involving traditional class outings. Using the reliable, multi-annual, same-season/same-place scheme, student participants can help you to count squirrel leaf nests in the same wood lots, to analyze small mammal diversities captured at permanent trap stations, to identify mammalian skulls in barn owl pellets taken at specific intervals from the same roosts, etc. And always keep original data in fireproof buildings!

Favorable student interactions, as mentioned earlier, are major priorities. Sure you are paid to help them but do more – champion them. Be diplomatic, but think students first and your superiors second! And you'll know that you are doing a good job when both your spouse and your children accuse you for liking your students better than you like them! And please refrain from co-authoring published versions of your students' doctoral dissertations.

Sure, Fullbrights, visiting professorships, tours of duty with NSF, etc., are flattering and impressive. But be wary of these ego-enhancing "changes of pace!" Your productive years as a researcher may be threatened! Always ask yourself how an interim position like one of these can augment your thrust as a productive scholar?

And you may either feel confined and/or actually enjoy being employed in a small-college atmosphere. And there may be only a few other research-oriented associates with whom you can discuss scholarly work. Still you can be productive, and for proof you can think of celebrated college-based biologists who have done so. And please keep in mind that granting agencies are always highly receptive to funding worthy projects from small college scholars.

Attend as many local, state, and national meetings as you can. And do what I failed to do and persuade the august American Society of Mammalogists to change its meeting dates from late June to April or May. Why? At least in my case I missed attending 15 or more meetings because the ASM sessions conflicted with the start of my summer field programs.

At meetings, you command attention when you present papers and gain acceptance by becoming acquainted with your colleagues. And at such gatherings you may also interview worthy students looking for places at which to do advanced work. And keep in mind that some of the best candidates may come to meetings of within-state academies of sciences. So it pays to become friendly with biology instructors at other institutions in your state or region since they can direct potential students in mammalogy your way.

Since it is not always wise to encourage budding undergraduates to continue graduate training under your guidance, you may wish to send them to do their advanced work with colleagues at other institutions. So why not make informal arrangements to exchange promising youngsters?

Constantly talk about your projects with authorities in other subjects and listen carefully to what they say. An atomic physicist, a basket weaver, a theologian, or an unwashed forks-of-the-creeker might offer suggestions about what you are doing from directions that had never occurred to you. Believe me, picking the brains of your dentist and others is highly worthwhile. And, of course, filch data-gathering ideas from published studies about insects, crabs, salamanders, and even birds!

Granting agencies and foundations, as you know, are always looking for meaningful projects to sponsor. But donations may also be obtained from moneyed folks in your own locality! The names Annie Alexander, William H. "Bill" Harris, and in my case Floyd Amsden, Jens Touborg, and Karl Bailey come to mind. These potentially-generous souls have to be cultivated and outdoor types, especially big game hunters, nature enthusiasts, and world travelers, are receptive to supporting field projects, and the more glamorous they sound the better. Join "civilian" service, nature, and hunting-and-fishing clubs, measure big game trophies for recording by Rowland Ward and Boone & Crockett, give talks to off-campus organizations about your field activities, ask them to bring back specimens that have IRS write-off value for them and educational value for your teaching program, even invite them as "consumers" of the natural scene to present talks to your students about their overseas adventures. Speaking from personal experience, it is also essential that you keep your superiors well advised before you actually approach potential private donors for money. You are then off-the-hook if unbeknownst to you your institution's president is after the same persons to build new campus buildings.

Here's another way to recruit promising youngsters to replace us oldsters? Simply by encouraging more institutions to interest undergraduates by exposing them to basic field courses that include mammalogy. In 1949, for example, I handed my boss at the University of Kansas an impressive list of major Middle American universities that had no biology professors specializing in field mammalogy. At that post-WWII time, universities badly needed staff and looked with favor on professorial candidates in the environmental fields. By the time that I left for Michigan State in 1955, we had made a fair start in "gently" persuading some of these institutions to employ KU-trained field mammalogists. All of you can think of some of these successful arrangements – Bob Packard arriving at Texas Tech via Stephen F. Austin comes to mind! But now many universities/colleges have replaced, or are now replacing retiring field-type professors with lab-types. You young folks need to devise a plan to reverse this trend!

It goes without saying that mammalogists should always be respectful, charitable, and friendly on a person-to-person basis when dealing with members of your superior's administrative and custodial staffs. Why? Believe me, it can be a valued two-way street since these "weight-carrying" individuals dish out assorted perks and often have a say when the front office dispenses year-end budget surpluses. I gained goody points galore by always bringing the dean's staff gifts of foreign cigarettes, bottles of opal chips, etc.

Further, it is a good idea to request from your institution's alumni office a list of former students who live in that remote out-of-country area in which you propose to do field work. These former students can be highly effective friends and most helpful when "door openers" are required. And in rural areas, presenting double bubble gum to children, rather than giving money to their parents, is usually a good public relations gesture.

And always put in a good word for the conservation of non-human mammals. Populations of many, often-inconspicuous kinds, are in serious trouble through loss of natural habitat and incompatibility with human land use practices. As you know, humans have no real homes. They and their offspring, carrying their own creature comforts with them, are forever encroaching on and untidying the habitats of their "class" mates, that have stay-at-home-attitudes, have regular habits, try to mind their own affairs, and wish to "enjoy" their varying degrees of life expectancy. Living spaces of sufficient size in which mammalian populations can be sustained need preservation. Maintain a watch-dog attitude toward these fragile and precious creatures.

And finally once in a while get out of your ivory towers and talk about neighborhood mammals to the citizenry including K-12 school classes. Describe and publicize local mammalian diversity, ecological/economical values, and how the concepts behind that much maligned word "evolution" are employed as useful tools in your endeavors. On occasion, I thoroughly enjoyed having a cluster of highly enthusiastic middle-schoolers in a biology class surrounding me as I prepared a study skin of a freshly-caught small mammal. And perhaps most fascinating to them was when I opened the body cavity and showed them the different kinds of unpickled innards. Gee! I wonder if any of those youngsters grew up to become field biologists? Based on my own childhood experiences, kids at that level can gain lasting career interests with such simple exposures!

And at long last, among your greatest non-taxable rewards are: (1) your satisfaction in having your very first scientific paper published, (2) the extreme pleasure of getting a request from some notable colleague for a reprint, and (3) finally the biggest ego-trip of all is when for the very first time someone cites in her/his paper data from one of yours. I've been there and know the feeling (Fig. 7).

Figure 7. Preparing a museum study skin of a Sierra Madre mantled ground squirrel. This was Baker's last field trip prior to retirement from Michigan State University. His camp was at 9,900 feet just below the summit of Cerro Mohinora, the highest peak in northwestern Mexico (27 May 1982). (Photo by Bruce R. Baker).

KEN WARD IN THE JUNGLE: MAKING SCIENTIFIC SENSE OF FIELD WORK

CARLETON J. PHILLIPS

Carleton J. Phillips was born in Muskegon, Michigan on 17 November 1942. He received a B.S. in Zoology from Michigan State University in 1964 and an M.A. in Zoology and a Ph.D. in Systematics and Ecology in 1967 and 1969 respectively from The University of Kansas. He is Professor of Biological Sciences at Texas Tech University. In 2004 he was a William C. Foster Fellow in Proliferation Threat Reduction, U.S. Department of State and Special Advisor on Nonproliferation Coordination to the Coalition Provisional Authority in Baghdad, Iraq.

My interest in animals—mammals in particular—goes back to my earliest childhood. My baby photographs show a curly-haired fellow grasping a toy dog and in my parent's version they began toting me to zoos and airports when I was very young. According to them, I demanded access to furry animals and airplanes and showed an inordinate interest in each. The interest in mammals dominated my academic and professional career although as I will explain, the interest in airplanes reentered my professional life after graduate school.

In 1948-1951 my family lived on Roslyn Avenue in Worthington, Ohio. I went there recently and found the old house and then walked to where the street dead-ended at the bank of the Olentangy River. The pathways in the woods along the bank looked just as I remembered them from fifty years ago. When he had time, my father took me there to explore the woods, search the exposed riverbank for arrowheads, and puzzle over the sources of bubbles that arose from the green-brown depths of the river. My memory of the place is dominated by summertime; the trees are always leafed (the catalpas adorned with their distinctive beans), the air heavy with moisture, and the Olentangy smells of decay. Walking the riverbank was not the only activity that I remember from that time. In response to my obvious interest in the out-of-doors, one day my father handed me his copy of Zane Grey's book, *Ken Ward in the Jungle* (Grey, 1912) and suggested that I try to read it. Zane did not intend for his book to be reading material for Third Graders, so reading it was a monumental struggle and, in my memory, a noteworthy achievement.

I kept—never returned and still have—my father's copy of *Ken Ward.* The story engaged me to the extent that when I finished it, I knew that someday I would visit distant places (tropical México topped my list because that was the setting for *Ken Ward*) with the objective of exploring unknown terrain and collecting mysterious, exotic, and previously unseen animals, especially mammals. What could be a better adventure? Naturally, at that age I had no idea of how someone might make a living collecting animals and I am not certain that I even realized the basic economics of making a living.

I dusted off the old book recently, and re-read it with the critical eye of a fellow who (a) believed it was *the* trigger for a professional career, who (b) had actually done some exploration, collection, and discovery of new animals, and who (c) had become opinionated about field studies and their role(s) in the science of mammalogy. In Zane Grey's imaginary tale, Ken Ward and his brother, a roustabout black sheep American named George, and a Méxican Indian named Pepe, explore a fictitious river that empties into the Caribbean near Tamaulipas.

Growing up I practiced for my future profession as a scientist-explorer by camping, hunting, trapping, and fishing at every opportunity. I particularly enjoyed live-trapping small mammals with homemade devices and was astounded to discover that there were many distinctive kinds of "field" mice. Their apparent diversity was puzzling but delightful.

When I first announced my intention to "camp out" my father produced his copy of Horace Kephart's *Camping and Woodcraft* (Kephart, 1917) and instructed me to read it carefully. He claimed it had gotten him through many a scrape, camping up in the north woods of Michigan (near Oscoda, it turned out). I studied everything from fire building to camp cookery (Kephart's recipe for ground hogs is excellent) to first aid (using a surgeon's knot to tie a severed artery stuck in my mind). When I requested funding for a sleeping bag, my father said, "re-read Kephart," and I discovered that he meant for me to fashion my own sleeping bag from a wool blanket and safety pins! He confirmed that this was his intention and said, "don't be a sissy...just sleep on ground in your blanket." So, in all my early years of camping I slept on the ground wrapped in a blanket while my incredulous friends snoozed in comfortable bags atop air mattresses. So far as other technologies were concerned, my father regarded a waterproof ground cloth as permissible but hardly mandatory, a galvanized pail as good a cooking pot as any, and the ideal tent was a shelter-half without mosquito netting. In retrospect I realized that he taught me to "make do," which was the theme of American outdoor life and exploration in the 19th Century, and put me in direct touch with soils, rocks, and habitats—elements of natural landscapes. It produced a person who is somewhat indifferent to shelter, unless air-conditioning is available. It also produced a person who views camping as a business-like way of accessing places where creature comforts are unavailable, but not as a recreation at the edge of town.

My camping experience was pretty crude, but instructive. My friends and I drug our skiff upstream at Harrod's Creek in northern Kentucky, camped by a pool above the riffles, and set trot lines for catfish. I canoed the Kankakee River from the Illinois border into Indiana and on three occasions I canoed the AuSable from Grayling to Oscoda, Michigan (a distance of about 244 miles). Traveling by water, pitching camp, and pushing on in the morning was good practice and such a boyhood experience reinforced my interest in mammals. On the AuSable I was visited by a mink who came to my camp fire and took fried walleyed pike from my hand; when I slept on a backwater island, the beavers I displaced kept me awake by smacking their tails on the water; while sleeping on the ground a striped skunk came along, woke me, and sniffed my nose; at dawn white-tailed deer drank along side my beached canoe. I became accustomed to my crude camping gear although I upgraded to a surplus army "mummy" bag when I reached high school and it was a good thing too. By then I was pushing the envelope and went camping in dead of winter; the first trip was in a swampy state game area northwest of Pontiac, Michigan. It was so cold that my coffee froze—actually went to slush—before I could get it down and I know I did not sleep a minute.

By the time I enrolled as an undergraduate at Michigan State University (in fall, 1960), I was ready to be the next Ken Ward. I successfully moved things along by breaking rules. For example, as a sophomore I avoided my undergraduate advisor (the ornithologist, George Wallace) and on my own hook signed up for a graduate course in *Zoogeography* taught by a professor named Rollin Baker. It was a tremendous experience; I could scarcely contain my excitement to see the interactions among geography, old and recent earth history, and animal life. Here was an academic subject and an intellectual challenge that was absolutely stimulating. Part way through the semester I went to the Museum building to visit with Professor Baker and inquire if there were any jobs that might be had. Baker was Director of the Museum, so I went to his fancy first-floor office and waited to see him. Finally, his secretary ushered me in and I simply blurted out who I was and did he know of any summer research jobs that might be had in biology.

Rollin Baker apprised me and extracted his grade book from a desk drawer. I watched as he looked up my name and grade on his first exam (I had something in the 90's). Then, in an unforgettable interaction, he inquired about my graduate advisor—and dropped the grade book when I answered that I was only a sophomore. He exclaimed, 'you're not supposed to be in my course!' or something like that and then asked what I was interested in. I did not mention Ken Ward, but did say that I intended to do research on mammals and the sooner the better. Wordlessly, Professor Baker took me by the arm, led me to the elevator, and when we emerged on the research floor of the Museum, he pulled me into his large corner office, which faced toward the main University library. The room had the look of a study—dark, comfortable but old-fashioned furniture, and walls lined by over-flowing bookshelves. Baker pointed to an extra table set in one corner and said, 'that will be your desk. You'll work with me. I'll get you a key to this place and the Museum tomorrow. Meanwhile, spend the day here and start reading the *Journal of Mammalogy*.' After he left, I explored the office and discovered on one wall a photograph of Baker and a group of unknown people standing in a jungle camp. The label said it was Veracruz, México! By accident, I had discovered the real Ken Ward.

I read Bill Burt's classic *Mammals of Michigan* and went into the field for the first time within a week of landing in Baker's office and taking up residence as the sole undergraduate student working in the Museum mammal collection. It was a simple chore. Baker introduced me to Museum Special mousetraps—essentially familiar-looking snap traps designed to not crush and therefore destroy a mouse's cranium—and instructed me in their use. By Baker's formula, the Museum Specials had to be baited with rolled oats moistened into a gummy wad with human saliva. I never believed in eating cereals, and at first I hated the taste of dry oats. I trapped the dense grass along a barbed-wire fencerow between a recently cut alfalfa field and an old oak and maple woodlot east of the Michigan State campus. The next morning I ran the trap line and discovered that I had caught six mice. They were dark brown in color, had short tails, and smallish, beady, eyes. I did not know what they were, but Baker showed me how to identify them with a taxonomic key. My very first scientific specimens were meadow voles, *Microtus pennsylvanicus*. Eleven years later, as a young faculty member, I used optical microscopy to examine bone and dental tissues in these mice and wrote an article in which I presented a theory about the evolution of ever-growing molar teeth (Phillips and Oxberry, 1972). Thirteen years after that, I wrote an elaborate book chapter in which I used both optical microscopy and transmission electron microscopy to describe the histology and cell ultrastructure of the retina, salivary glands, and digestive tract in meadow voles (Phillips, 1985). My daughter, Kathrin, trapped the specimens that I used in the cell study. She was twelve at the time, and catching and identifying the mice living near our cabin in Maine was one of her favorite forms of woodland entertainment.

The next enterprise was to learn how to prepare the mice as "museum specimens." Professor Baker took me into the Museum basement, a place used as a preparation area by biologists, paleontologists, and archeologists. A piece of dried skin with red hairs was tacked to the wall near one of the preparation tables. Baker saw me staring at it, and casually remarked that it was mustache—taken from a human body discovered in Lansing. The Museum had assisted the police and FBI in their forensic analysis of what turned out to be some poor derelict. Part of the Museum contribution involved making the dermestid beetle colony available for cleaning the dried flesh off the fellow's bones. Baker proudly showed me his subterranean dermestid chamber, accessible by a cement staircase on one side of the museum building. Ordinarily the flesh-loving beetle larvae spent their time cleaning up mammal skulls—usually rodents—destined for the research collection.

The traditional mammal museum specimen consists of a stuffed skin catalogued with the skull and lower jaw. In Zane Grey's book, Ken Ward evidently did not know this and Grey had him salting and scraping the skin and apparently tossing off the skull and jaw. Learning to skin and stuff a mouse and having it come out looking the way it was supposed to look was not easy. Obviously, mice are small and their skin is delicate. Moreover, it was necessary to place small wires in their limbs and tail to keep them in the correct position and prevent breakage during handling. The tail wire was the most difficult thing, and Professor Baker made an elaborate show and tell. He cut a wire, talked about how important it was to keep it straight, and then checked its size by inserting it into the empty tail skin. He withdrew the bloody wire and wrapped it with thin layers of soft cotton. While doing this he licked the cotton to keep it in place on the wire. Mouth agape, tongue flickering, he remarked that in the old days everyone coated skins with arsenic but he was glad to drop that habit inasmuch as it made licking the tail wire "somewhat" dangerous. In this new century, universities have offices responsible for Environmental Health and Safety and scientists don latex gloves and never ever bring food into research laboratories. Forty years ago, Professor Baker and I drank coffee and ate sandwiches while skinning my first mammal specimens barehanded. Baker of course was sharing with me the outdoor tradition and culture of North American mammalogy, much like my father shared with me his understanding of camping and woodcraft. These twin cultures originated in a time when "risk" was understood and silently accepted. This was a matter of practicality—a realistic appraisal of life as it is. That being the case, there certainly was no such thing as fear of rare, or remotely possible, bad outcomes.

In retrospect, I came to understand that sleeping on the ground when necessary, eating whatever is available, slogging through swamps or jungles to set traps or hunt, and personally collecting, handling, and preparing specimens is what separates the field biologist and the white-coated laboratory jockey. A few years ago, the CDC in Atlanta proposed that field mammalogists wear respirators and Tivex coveralls when working with tissues from wild mice. The thinking was that there always is a chance that wild mammals might transmit some virus or other pathogen. As I write this, I am unable to name a single field mammalogist who has taken this seriously, although I can name one or two who have done a bit of public hand wringing over it.

The field man develops a bond with his fellow mammals. Mammals collected by the field biologist have nothing in common with the pink-eyed albino mice and rats that are shipped into the laboratory from breeding and supply companies such as Charles River in Boston. The wild species, as they usually are called, live out their lives—often very short lives indeed—in dangerous and complex natural circumstances. They cannot be bought. They do not arrive at the laboratory by Federal Express. In order to understand them, and know their story, it is necessary to find them personally, but the act of going into the field and discovering them and touching them is what makes them special and valuable. Installing them in a secure research collection makes them into "vouchers" representing the discoveries from field research projects. Museum voucher specimens will last for hundreds of years, at least, and the museum research collections around the world are treasure troves of scientific information, historical documentation, and intellectual stimulation. I recall working at the British Museum of Natural History in the 1980's, examining some of the Patagonia mouse specimens (leaf-eared mice, *Phyllotis*) collected and prepared by Charles Darwin. Handling the same specimens that he had handled, looking at their characteristics just as he had, and noting the geographic localities where he had obtained them, put me in touch with both Darwin and the mouse species. Eventually I went to Patagonia myself and collected my own specimens of *Phyllotis* and used molecular genetic techniques to unravel their biogeographic history (Kim et al., 1998).

While at Michigan State University I held two valuable undergraduate assistant positions. The first one, which I held as a freshman and sophomore, was in an NIH-funded dental caries research laboratory. In the 1950's, Charles Hoppert, Harrison R. Hunt, and Sam Rosen had bred two strains of laboratory rats, one resistant and one susceptible to tooth decay. This was truly seminal work because it demonstrated for the first time that there is a genetic basis to resistance to tooth decay and provided laboratory stocks that could be used to determine the factors that influence resistance. Hoppert and Hunt were near the ends of their respective careers, and Sam Rosen later left Michigan State and worked at the Dental School at Ohio State University. But, while I worked for them (doing menial laboratory chores) they encouraged me to ask questions, had me read reprints of their research, took time to explain their thinking and ideas about oral biology of mammals, and helped me develop my own scientific interest in the teeth and salivary glands of mammals.

My other job, which I held as a junior and senior, was in Professor John A. King's animal behavior laboratory. At the time, John King was nationally recognized as an expert on the behavior of small mammals, especially rodents. He had earned his reputation in part because of elaborate field observations of the black-tailed prairie dog, *Cynomys ludovicianus*. Working in King's laboratory was extremely valuable because I developed a basic appreciation for how animal behaviorists think and work, and what kinds of questions they ask. Thanks largely to John King, I spent time with graduate students such as E. O. Price. Ed Price was interested in how domestication affects behavior in mammals; he used wild-caught and laboratory-reared deer mice of the same species as a model system for deciphering the impacts of captivity and artificial breeding.

When I was a junior, John King took me (and his graduate students) to Chicago, to a meeting of the American Psychological Association. This was my first professional conference and I found it stimulating to spend time around professional scientists and eager graduate students from all over the United States and Canada. I nevertheless returned to campus thinking the organization should have been called 'neurotics united.' Professor Harry Harlow was perhaps the most interesting and insightful sideshow. Harlow at that time was world famous for his research on mother-offspring bonding (mother "love" as it was called) in the Rhesus monkey. At the scientific sessions I saw grainy black and white film footage of Harlow's most recent experiments and later on John King invited me to accompany him to a reception for the great psychologist. When we arrived, Harlow apparently was dead drunk, incoherent and scarcely able to stand. King introduced me, but that was as far as it went. Afterwards, Professor King mumbled something apologetic to me about Harlow being drunk. After I graduated from Michigan State, more than twenty years passed before I saw John King again, at a scientific conference. He was just as I remembered him—a sincere fellow and a true gentleman whose major interest was in educating young people to the wonders of animal behavior.

The advantage of the large university, on average, is that undergraduate students have regular access to nationally or internationally known scholars—the type of people who actually acquire, define, and shape the world of knowledge. Inevitably, this includes the good with the bad, the commonplace with the brilliant, the brilliant with the idiosyncratic. As an undergraduate at Michigan State I met the famous German rocket expert, Werner von Braun. Only seven years later, I presented a seminar on biological and psychological aspects of long-duration space flight to von Braun's group at the NASA facility in Huntsville, Alabama. I also met Sig Olsen, whose powerful books about hunting, living, and canoeing the boundary waters were (and still are) among my very favorite examples of outdoor writing. One night, my roommate, Stuart Marks, and I went to dinner and then spent a quaint evening with Joy Adamson, who had just written her popular book, *Born Free*. Yet another character, similar in nature to Adamson, was Lois Crisler (author of *Arctic Wild*). When I met her at a Wildlife Society meeting, she kept insisting that she hoped that 'wolves would enter' my life. I never deciphered what she meant by this, but she clearly possessed a powerful and perhaps odd kinship with wolves in the Northwest Territories of Canada.

As a final example, in 1963 Professor Baker invited me to join him for lunch with a fellow named John J. Christian. This proved enjoyable, and extremely valuable to my education. In the 1950's, Christian and his colleague David Davis had written scientific articles on the subject of stress syndrome and population size regulation in mammals. Their research built on the original work conducted by Hans Selye. Christian hypothesized that over-crowding (in the sense of density) affected the endocrine system through the adrenal gland-pituitary axis. Over-crowding thus caused a predictable breakdown in maternal care, reproductive success, and an increase in aggression on the part of males. The link to mammalian behavior and population size impressed me greatly and a few years later, when I took my Ph.D. comprehensive qualifying exam, I was fully prepared to answer a substantial question about stress syndrome and population regulation. It was an odd coincidence that it came up at all, and my advisor (J. Knox Jones, Jr.) was amazed that I could cite the literature off-handedly. I did not have the heart to tell him that I knew John Christian and had 'picked his brain.' Some people would use the word "luck" to describe what had happened, but in biological terms, I was *pre-adapted* for that graduate student exam question. Broad experience—a subtle and painless type of preparation—is yet another example of the advantage of a high quality undergraduate experience.

By using Baker's Museum office as a kind of personal retreat, and a place to study, I was thrust into contact with graduate students rather than my fellow undergraduate students. I hung out with this older, more experienced, crowd and looked over their shoulders as they conducted their research and wrote their thesis or dissertation. It was an exciting day when the late J. Keever Greer returned from his field studies in Chile, accompanied by his wife and thousands of mammal specimens. Robert L. Fleming, an ornithologist, was another interesting graduate student. Fleming had grown up in Katmandu, Nepal, where his mother (a medical doctor) and father (a social scientist and amateur ornithologist) served as medical missionaries. The

Fleming family opened Shanta Bhawan, Katmandu's first western-style hospital (Fletcher, 1964). The young Robert Fleming (he shared his father's given name) was a wild-eyed character. I credit him for introducing me to the fact that ornithologists of that time actually relished hunting small birds with shotguns. In the public mind, ornithologists are timid souls, little folk with field glasses and pith helmets. To the contrary, the field ornithologists of the time were a bloodthirsty lot. A third graduate student at the Museum was a fellow named Stuart A. Marks. Marks' middle name is Alexander, but he gleefully told strangers that the 'A' stood for Admondushay. Strangers invariably chortled or laughed outright at the sound of the fictitious name and then Marks would solemnly intone that Admondushay was his grandmother's name, God rest her recently departed soul. This of course preyed upon the social consciousness of anyone and everyone and Marks then would accept apologies, pats on the back, and sometimes hugs from tearful women.

Like Robert Fleming, Stuart Marks had grown up in a missionary family. In Marks' case his father was a dental missionary in the old Belgian Congo. Marks had earned a degree in Fisheries and Wildlife at North Carolina State University and had come to Michigan State to pursue graduate studies. When I met him, he had just returned from St. Lawrence Island, in Alaska. He showed me his film of hunting walruses with the Eskimos and told stories of growing up afield in the Congo. All in all he was an exceptional fellow clearly on a creative quest for some special, unique, brand of scholarship. He found it, too. His books about natural history, hunting, and the role of hunting in human culture and society, are excellent (e.g., Marks, 1976, 1991). We roomed together for a couple of years and I credit him with opening my mind to the study of anthropology. I hasten to add, however, that I never have been able to accept the "Nobel savage" concept or the tendency for cultural anthropologists to delude themselves with the notion that the human mind is a blank slate. Mammalogists quite naturally view human beings as typical primates rather than some magical species.

As a sidelight, it was Stuart Marks who taught me how to prepare elephant steaks. It happened that a small circus visited East Lansing and while there one of its Indian elephants went berserk. Why this occurred will never be known, although at the time it brought to mind the punch line of *Tobermorey*, a short story by Saki [H.H. Monroe]. In *Tobermorey* the *Times of London* reports that an elephant in the Berlin Zoo has trampled a professor known for trying to teach animals to speak. It is further speculated that he was engaged in trying to teach the elephant to use German irregular verbs. In East Lansing the police gunned down the berserk Indian elephant after it trampled one fellow on its rampage through a suburban neighborhood of white picket fences, imported shrubs, and trimmed beds of ornamental flowers. The carcass was removed to the School of Veterinary Medicine at Michigan State. The next morning Professor Baker sent me forth, knives and containers in hand, to collect some samples for the Museum research collection. I was shocked by what I discovered. The partly dissected carcass was the centerpiece for Vet students studying anatomy of large animals. However, I seized the moment and collected twenty pounds of wonderful steak, which fed Stuart and me for quite some time (Fig. 1).

One evening we had a special elephant roast, and invited a Kenyan student named Perez Olindo. Twenty-three years later I visited Perez in Nairobi, where he was Director of Wildlife. A few months later the President of Kenya sacked the poor fellow (for the second time, illustrating the capricious nature of Kenyan politics) and replaced him with Richard Leakey who set out to destroy elephant poaching. As for Olindo, the

Figure 1. Eating steaks from a berserk elephant; Carl Phillips (left), Stuart Marks (middle), and Adhemer Byl (right) at Michigan State University. (1963). Photograph by Julian P. Donahue.

only elephant meat he ever tasted was the steak fried in butter and garlic, by Marks, in East Lansing, Michigan.

My actual research experience began shortly after I became ensconced in Rollin Baker's upstairs Museum office. Baker helped me obtain an undergraduate research grant from The National Science Foundation, and set me to work on investigating the mammals of Beaver Island in northern Lake Michigan. This might have been the ideal first project because I spent the summer between my sophomore and junior years living on the island, collecting and preparing museum specimens, and conducting a live-trap mark and release study. In short, I was sent off alone, as an undergraduate, to work in the woods of northern Michigan doing exactly what professional mammalogists were doing all over North America. I was in the game.

Beaver Island is about 54 square miles of complex habitat: it has Lake Michigan beach communities, cedar swamps, upland mixed pine and hardwood, and fallow (old field) farm fields. The island is positioned about 18 miles from Michigan's Upper and Lower Peninsulas. I rode the car ferry from the mainland to the small harbor village of St. James and moved into a one-room log cabin, which would be my home and laboratory for the next three months. The basic idea was to survey the island's mammal fauna. Some of the species living on the island were known, but there was a chance that I would find species not previously known to occur there. I also was to ask whether the species living on the island exhibited any differences in habitat selection or ecology as compared to the same species on the adjacent mainland areas. I wondered if small mammals exhibited special features that I could attribute to island life—a condition characterized by less overall available living space but also by fewer species in competition for that space and food. I was thinking about all of this when I reached St. James, although it was not until years later that I could comprehend that this was a really complex question in evolutionary biology.

Beaver Island has a fascinating history and now is populated by an eclectic cast of characters. I was always interested in history so when I set off to lay trap lines, I took time to walk the barely discernible ruins of an old Indian village and peer at the massive but abandoned log homes of 19th Century settlers. Indeed, I was very attracted to the pattern of fallow fields, stonewalls, and remnants of rail fences that collectively told the story of 19th Century farming. What had happened to the settlers? Why had they abandoned their homes, I wondered. It turned out that the home-building settlers were a side-branch of the Mormon movement led by a man named James Strang. In the 1850's Strang wrote the first natural history account of Beaver Island but a few years later, carried away I suppose by the succession of the South, Strang declared his Mormon kingdom of Beaver Island independent from the United States. This was a bad idea. The Michigan mainland settlers, mostly Irish and French Catholic descendents of tough old time loggers and fur trappers, were pretty incensed by King Strang's Mormon habits, including his reputation as erstwhile husband to all of the teenaged girls in his kingdom. As a generalization from studies of mammalian behavior, it is typical for males to be enraged about social systems that seem to give one fellow access to all the best females. In any case, Strang was assassinated supposedly with the help of the United States Navy, and the Irish and French mainlanders arrived *en masse* and gave the Mormon band twenty four hours, or so, to abandon the place, lock, stock, and barrel. Leaderless, the Mormons did as they were told and the mainlanders moved in (and their descendents ran the place in 1962, when I worked there).

The Beaver Island project put me in touch with a Michigan State University graduate student named John J. Ozoga. John was an exceptional field man whose Master's research focused on the biology of the coyotes living on Beaver Island. Currently (2004) coyotes occur all over the mid-west and eastern coast of the country, but back in 1962 they were new and exotic arrivals in Michigan. The usual notion was that they were really timber wolves, so there was a certain amount of local concern among uninformed citizens. Michigan hunters, who generally believe that the State deer herd is raised and managed just for their benefit (an idea not far from truth), were edgy because they regarded the coyotes as competitors. In simplistic terms, if coyotes ate the fawns, or reproductive adults, there would be fewer deer for the hunters to kill in the fall. These were the kinds of public issues that attracted a game biologist like John, but were mostly a source of mirth for me (he was a student in Fisheries and Wildlife rather than Zoology, like me). John eventually did his Ph.D. research on deer, and spent his career at the Cusino Wildlife Experiment Station in the Upper Peninsula of Michigan. It would be totally accurate to describe him as a world's expert on deer biology and management (Ozoga, 1988).

John Ozoga spent countless hours afoot, tracking and trapping Beaver Island coyotes. In wintertime he traveled by snowshoe, following hundreds of miles of coyote tracks. He also collected their droppings, and sitting back home in the Museum preparation room he shifted through the bone fragments and hairs in coyote dung—or "scats" as they were termed. In the end, John probably knew everything there was to know about what the coyotes were really up to, and how they spent their time. John and I eventually teamed up, and in the year that I graduated with a B.S. in Zoology, the Michigan State University Museum published our small book on the *Mammals of Beaver Island, Michigan* (Ozoga and Phillips, 1964). This was a major moment for me because it tied together many of the things that had attracted me for years: mammals, outdoor fieldwork, collecting, analysis, and, at last, my first scientific publication. The down side, if there was one, was that the whole process convinced me that ways had to be found, or created, that would expand the amount of scientific data obtained from each specimen collected. Having the specimen, providing a species identification, and being able to measure it and compare it to specimens of the same species from the mainland was fine, but fell short of what I really wanted to do, or naively thought ought to be done. Years later, I realized that other young field-oriented students such as Robert J. Baker were thinking the same way and our generation ultimately developed and took to the field—to the most remote places on earth—the laboratory techniques for doing chromosomal studies, conducting cell tissue culture, analyzing comparative cell ultrastructure, obtaining molecular genetic data, and studying gene expression in wild mammals. We became, in effect, the transitional generation—the last of the mammalogists with one foot in the traditional realm of the late 19th and early 20th centuries and the other foot in the new, laboratory-based world of cell and molecular biology.

In 1963, when I was a junior, Rollin Baker surprised me by inviting me to accompany his annual field research expedition to México (Figure 2). At long last I was about to see for myself the tropical wilderness described in Ken Ward. Baker's field team included a herpetologist named Robert G. Webb (Webby as Baker called him), a Michigan State graduate student (in ornithology and entomology) named Julian P. Donahue, another undergraduate, Dan Womochel, and a student from Western Michigan University named Bernie Crippes (Figure 3). As a field vehicle, Baker used a modified Ford refrigerator truck; three people could squeeze into the cab, and the others sat on a bench seat bolted onto a ledge in the front section of the cargo hold. Windows had been installed in the cargo hold so that the passengers in the back could peer out at passing scenery, looking over the top of the truck cab. In those days, armchair adventurers thought of the Land-Rover as the only explorer's vehicle. Currently, that same type of person dreams that it is a gas-guzzling SUV, perhaps an exotically miss-named "Exterra" or "Explorer." In reality, of course, SUV's are intended for suburbanites and Baker's old refrigerator truck was a truly functional field vehicle (albeit an uncomfortable one at that). Driving through the wet summer heat of Houston, Texas, Baker advised the passengers in the truck cab to roll up their windows so snooty Texans would think the Michigan State boys had air-conditioning. Eventually, Dirk Gringhuis, an artist, painted an outlandish moose cartoon character on a side of the green field wagon, and labeled it

Figure 2. My first foreign field experience was with Rollin Baker. I had just completed my junior year at Michigan State University. Here I am, preparing specimens in our camp in Michoacan, Mexico (July 1963). Photograph courtesy of R. H. Baker.

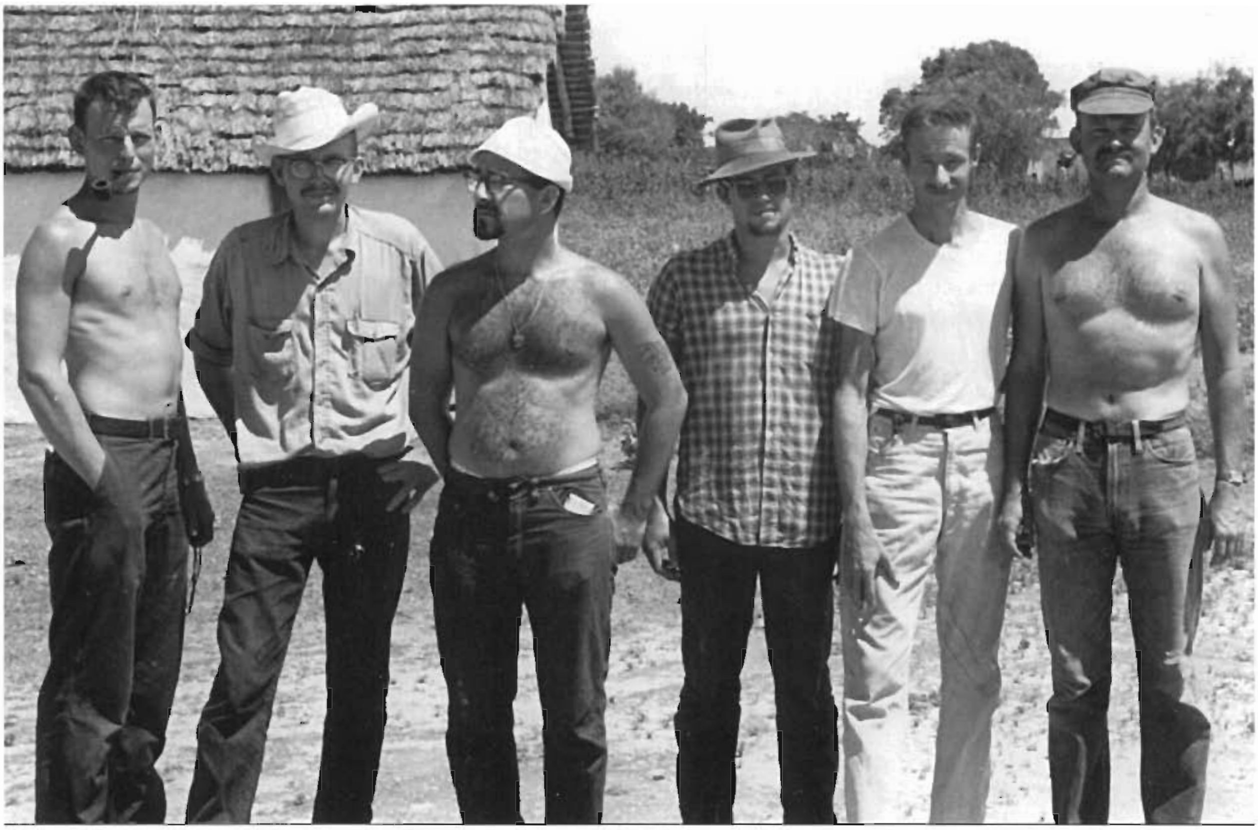


Figure 3. The 1963 expedition team: from left, Julian P. Donahue, Dan Womochel, Bernie Crippes, Carl Phillips, Robert Webb, Rollin H. Baker. At La Pesca, Tamaulipas, Mexico (August 1963).

'the FOOTLOSE MOOSE.' In December, 1964, *The Michigan State University Magazine* ran an article about the wagon, lavishing praise on its role in obtaining scientific data. In part, it said,

> "The Footlose Moose is a 10-year old road-hardened Ford truck that has done much international rambling in the interests of science. It has carried Museum teams on many expeditions into such remote areas as the steaming jungles and blistering deserts of Mexico and the frigid, snowy wilds of interior Canada. On its last five trips alone, it rolled up more than 40,000 rugged, trouble-free miles."

This first foreign field expedition met, and exceeded, all of my boyhood Ken Ward dreams and expectations of México and field research on mammals. To prepare, I had read and re-read A. Starker Leopold's *Mammals of México* and a host of individual scientific articles and monographs called to my attention by Baker. It is important to understand, of course, that in 1963 the mammalian fauna of México was still poorly known, especially in comparison to the United States or Western Europe. So, there was a sense of "exploration" in terms of not knowing what to expect from our collecting. Moreover, although rodents were Baker's main interest, he had purchased "mist" nets so that we could try our hand at catching specimens of bats. At that time, studies of the species and geographic distribution of bats was a booming scientific enterprise. This was because it was nearly impossible to survey bats before the availability of Japanese mist nets (which were designed for catching birds) and development of field techniques for using them to capture bats flying at night.

We crossed into México at Laredo and drove across the high deserts of Nuevo Leon and Coahuila, camping, sleeping under star-studded skies, and collecting specimens of mammals. We passed into the State of Durango and stopped briefly in Durango City. On this occasion I walked to the city park with J. P. Donahue. Julian was an excellent natural historian with a keen eye for the unusual. He had spent some of his boyhood in India, where his father had been employed as an expert on agronomy (funded by the Ford Foundation), and being around him always made me think of Kipling. One night, walking in the desert in Coahuila, Julian and I had come across a very large but immobile tarantula. Nearby we spotted a large female wasp (genus *Pepsis*) excavating a burrow for her tranquilized victim. These wasps dig a hole, place the spider in it, and then lay their eggs on the spider. In the Durango City park, the two of us stumbled upon a flock of about twenty black-crowned night herons (*Nycticorax nycticorax*) nested in tall trees. We had been reading Blakes' *Birds of México* while riding in the rear of Baker's field wagon and Donahue blurted that night herons were unknown from the Méxican plateau. Incidental new distributional information at that time was regarded as worthy of a stand-alone report, so after our return to Michigan State, Julian and I wrote a short "note" for the ornithology journal, *The Condor* (Donahue and Phillips, 1964). This paper became one of my two articles about birds, although colleagues who know me well still make jokes about the fact that Phillips hardly knows one bird from another. True enough, of course, but on this one occasion, with Donahue's help, I did know black-crowned night herons and also knew that they had not been reported from that region.

The other student in our team, Dan Womochel, eventually left biology to seek a graduate degree in geology. It was just as well, I suppose. Womochel was a pretty good outdoor person, having grown up camping and hunting in Michigan. Womochel regaled any and all potential listeners with stories illustrating his hunting skills, including his ability to hunt with black powder rifles. More important, it was the case that he was slightly deaf, a disability he quite naturally attributed to his shooting—both hunting and target practice. This deafness was a source of trouble because as a matter of vanity he refused to use any form of electronic boost—and usually was slightly out of touch with the rest of us.

Among our team, Julian Donahue in particular was annoyed by Dan's frequent use of the questions, "what? What did you say?" Donahue's anger peeked one afternoon as we drove up and over the Devil's Backbone, the ridge in the Serria Madre Occidental that separates Durango from Sinaloa. We had just descended onto the wet western slope and paused near the little tropical village of Concordía. Baker had spotted a culvert, and possible roosting spot for nectar-feeding bats, and ordered Donahue and Womochel to take a look. From where I stood, in the middle of the road, I saw trouble brewing. At one end of the culvert, Donahue had pulled out a revolver loaded with .22-calibre "dust" shot and was preparing to collect some specimens. Womochel was headed for the other end, which could only be accessed by cutting through thick tropical vegetation and jumping down into a steep-sided creek. "Stay back, Dan," several of us called, but Womochel, unawares as usual, leapt into the creek just in time for Donahue to shoot him. Donahue then emptied the cylinder, firing every time Womochel hollered, "wait!" Eventually, after being peppered with the fine shot six times, the slightly wounded but hysterical Womochel scrambled out from the creek screaming, "Goddamnit, couldn't ya hear me yelling wait?" Julian Donahue, with several specimens of rare nectar-feeding bats in hand, responded meekly, "what? What did you say?" The two students did not speak to one another for at least a week; Donahue feeling vindicated and Womochel feeling trespassed upon (and with raw red wounds on his chest to prove it). Rollin Baker admonished Donahue to be more careful but mumbled to me privately that every hunter should know what it feels like to be on the receiving end.

Fieldwork, I discovered, is a tough business and it takes its toll on the emotional well being of participants. Living and working alone, on Beaver Island, had not revealed this to me. But Zane Gray had known about it, probably from his personal experiences tarpon fishing in Veracruz. In *Ken Ward in the Jungle* he created in Ken Ward a fictional character who was steady and stable, regardless of circumstances, and contrasted him to his impetuous younger brother. Difficult interpersonal squabbles, including actions as outrageous as intentional shootings (or threats), can occur during real expeditions in direct correlation to either the inexperience of some team members or, in the case of more experienced people, the degree of human discomfort, perceived danger, and other tribulations associated with living and working under sometimes extreme conditions. When I spoke to von Braun's group at NASA-Huntsville in 1970, my conclusions about the psychological rigors of long-duration space flight were officially drawn from a NASA-funded project that we had conducted at Grumman Aerospace. Unofficially, and truthfully, my summation of what to expect, and my recommendation on what to think about in regard to long duration space flight, came directly from the seven previous years of personal experiences with the stresses and strains of field mammalogy conducted in wilderness in México, Nicaragua, the northwestern frontier of Pakistan, and New Guinea. Perhaps enough time has passed for me to also mention that I once listened to a confidential audiotape from Grumman's lunar excursion module (LM). The NASA Apollo astronauts had complained post-flight (Apollo 11) that the interior of the LM was exceptionally noisy. On this account, restful sleep was nearly impossible while on the lunar surface. In any case, our Aerospace Biology team listened in and the conversation between the astronauts was virtually identical in tone, wonder, and language to conversations among field mammalogists on expedition. The LM, by the way, *was* very noisy; pumps whirred and whined and sensors beeped. Besides that, one man had to sleep atop the engine cover, which, I suppose, was psychologically analogous to sleeping on the ground in a bug-infested jungle camp.

Along the western coast of México, Baker's expedition made camp a stone's throw from the coastal village of San Blas, in Nayarit. At that time, the only thing afoot in San Blas was a jaguar-hunting outfit run by the Lee brothers who had come down there from western Texas. The Lee's had dedicated their lives to being cowboys and hunters, and their collective ambition was to kill all the jaguars and mountain lions they could run down with their dogs. I returned to San Blas about eight years later (in 1972, accompanied by Stuart Marks) and it was a little livelier. A motley California coastal crowd was massed on the beach, awaiting the curl on the biggest wave. For me, camping in the wet, dense tropical forest was the best part of San Blas as it was in 1963. Although it was on the opposite side of México from Veracruz, it was still exactly what I had always imagined, from my first reading of Ken Ward. We used machetes to cut out a camp site, pitched some old and impossibly heavy canvas wall tents, set up army-style folding cots, placed palm fronds on the ground to make walkways, and felt right at home. It rained all day and night, continuously, or so it seemed, while we camped there. Our clothes, boots, and bodies were caked with mud. We hardly ever bathed—it wasn't worth it. The heat and humidity were oppressive. Ticks attached themselves by the dozens to our bellies, groins, and armpits and engorged before we discovered them. We slogged around, trapping spiny mice (*Liomys*) and mist netting exotic-looking bats. On the third day Baker, Webby, and I walked three or four kilometers together and were attacked by huge black swarms of small-sized mosquitoes that fed incessantly until we were nearly driven mad. Finally, Webby spotted a stream and the three of us ran and dove headlong into it and spent thirty minutes submerged save for our nostrils.

A few weeks after San Blas, in the Méxican State of Jalisco, we drove Baker's field wagon up a rough dirt switchback that ran from the village of Atenquique to an elevation of about 9,000 feet on a dormant volcano called *Nevado de Colima*. Here we pitched a camp in cool fir and pine forest adjacent to an open patch (we estimated it to be 15 acres) dominated by bunchgrass. We collected specimens of plants and mammals and later, after we returned to Michigan State University, Rollin Baker and I wrote a paper entitled, *Mammals from el Nevado de Colima, Mexico* (Baker and Phillips, 1965*b*). The Beaver Island book with John Ozoga and another paper with Rollin Baker (Baker and Phillips, 1965*a*) had fulfilled part of my boyhood dream but this one, reporting on the mammal fauna from high up on a massive volcano, a place only accessible by rough travel, perseverance, and camping in the forest, in México, enabled me to catch up, at last, with Ken Ward.

I planned to complete my undergraduate studies in 1964, and just assumed that I would continue on, in some fashion, until I could find employment as a mammalogist. One afternoon, in spring, Baker dropped by his upstairs office. By now, of course, I had more or less taken it over and kept all my papers and textbooks and small collection of reference books in various places. In a sense, Baker was coming to my office, although he surely never saw it that way. It turned out that he wanted to suggest that I apply for Graduate Studies at the University of Kansas. More specifically, he said that he had already recommended me to E. Raymond Hall, who was one of the most prominent of all the senior American mammalogists. This event led to the next phase in my formal education.

Before going to Kansas, I enjoyed a second field expedition to México with Rollin Baker. The 1964 team included Dan Womochel, who was planning to attend Graduate School at Texas Technological College (now Texas Tech University) and study mammalogy with Professor Robert L. Packard, a new Michigan State University graduate student named Michael K. Petersen, and a photographer named Charles Warner. Warner's job was to document the fieldwork. Having a non-scientist along to collect images of the expedition activity struck me as creative on Baker's part. Warner used an incredible movie camera, a Bolex, to obtain 16 mm footage. When I showed interest, he took time to instruct me in its use and a year later, when I saw the wonderful product—full of action, warm color, and photographic depth, I knew that I would try document some future expedition in the same way. Field projects are demanding and time-consuming and typically there is no opportunity for photographic documentation on the part of the scientists. As a consequence, the photographic record for most field projects is abysmal.

Eventually, in 1980, I took time out to film a field project in Suriname. I lugged a Bolex along on a helicopter ride to the top of a remote mountain called Tafelberg, and shot perhaps 900 feet of color film. I edited the footage and created a twenty-minute show about the expedition; it was shown on the NBC Today show, CNN, and even at the Explorer's Club in New York. I was proud of the outcome, and convinced myself that it successfully illustrated the incorporation of laboratory techniques into field projects conducted in the most difficult of circumstances. The film met its demise in Moscow, in 1984. I was working there as an inter-academy exchange scientist (United States National Academy of Sciences-USSR Academy of Sciences) and had agreed to show the film and present a talk on field research to an audience at the USSR Academy of Sciences. The projectionist installed my film on what looked like an antique machine. Within two feet of the opening segment—which showed us dramatically approaching the jungle in search of a landing site—the film stalled and I watched, horrified, as it melted on screen. It then must have recaptured a sprocket because it advanced a couple more feet, stalled again, and a new segment of film melted away. So, this is how my documentary unfolded: section by section, meltdown by meltdown. As if to drive home the point that the film was done for, when the lights came up I discovered that the projectionist had allowed it to fall on the floor and was, in fact, standing on it. His explanations were that he did not have a take-up reel and, besides that, what could one expect from a Polish-made projector?

I had better luck in 1992 when I filmed killer whales feeding on sea lions on the coast of Argentina. This film documented the "herding" behaviors of the whales as they interacted with their erstwhile prey. Wisely, I transferred the completed film to video, edited it that way, and then made multiple copies.

The 1964 Michigan State University fieldwork with Rollin Baker took us to regions and habitats in México that we missed the previous summer; most of our work focused on the States of Michoacán, Guerrero, Oaxaca, and Veracruz. Because these states surround the Federal District, Baker decided that it was practical to rent a house in Coyoacán, an old suburb of Mexico City, and use it as a rest stop every weekend or so. Baker's family, his wonderful wife Mary, his daughter Betsy (actually Elizabeth Alice), and his sons Bruce and Bryon, all stayed at the house and acted as a surrogate family to the weary travelers—whenever we managed to visit. After a week or two afield we generally were unwashed, unkempt, and dressed more or less in filthy field clothing. Our every-day language was probably despicable too. We showed ourselves late on Friday or sometimes at the crack of dawn on Saturday and, I think, disrupted things for the Baker family. One healthy by-product of these weekend sojourns was that Betsy and I became bullfight aficionados—actually bullfight *mavens* as New Yorkers would say. Betsy was a beautiful girl, a year or so younger than me; she had the most striking blue eyes and black hair I had ever seen. On about the fourth week, while we were camped on a cold rainy mountainside in Guerrero, Betsy's father and I squatted by a campfire late in the night trying to warm our selves. After a long silence he poked me in the ribs and said, "you know, Carl, I'd never let Betsy marry a field mammalogist." I didn't take it personally, but a couple years later, my pal Julian Donahue sent me a newspaper paper clipping announcing Besty's marriage, in upstate New York. The article said she was "... given in marriage by her father," and because of this statement, I always assumed the groom's major was not mammalogy.

For me, there were two important scientific highlights to the 1964 expedition. At my urging, Baker had agreed to let me try to collect and preserve blood samples from at least some of the rodent specimens. This was not an overnight decision on his part because it was a break in the tradition of snap trapping and preparing standard museum specimens and, more important, it added a layer of complexity to the program. How could we do all of the usual things, and also find time to undertake this new research activity, he wondered? This dilemma, and it truly is a dilemma, dogs field projects to this day. For my part, in 1964 I could not appreciate the seriousness of the decision. To me, still being naïve and without a real stake in the scientific outcome of the program, it was easy to argue that we should incorporate "modern" techniques into the work. I had been reading about the use of blood proteins to indirectly define and compare the genetic makeup and differences among species. The technology was being referred to as serological systematics. Frankly, I did not understand serology very well but intuitively I thought that it was something that would add scientific depth to the project and extend the ultimate value of each specimen. Nothing has happened in the last thirty-nine years to change my opinion on this subject.

To preserve serum proteins from rodents, it would be necessary to draw a blood sample from a live animal and use centrifugation to separate the various blood cells from the serum. No electricity would be available in camp, but one option was to carry a "hand" centrifuge, which relied on the human arm to spin small buckets containing test tubes with blood. Although the hand centrifuge solved one problem, it did not solve the problem of how to preserve the serum. Insofar as we knew, the serum needed to be frozen for storage. In 1964 the only practical solution was to design and construct a Styrofoam-insulated cold box that might enable us to carry dry ice for up to ten days at a time. In México it was possible to find dry ice in many rural villages, especially those that were remote enough to either not have electricity or have electricity only intermittently. A decade later it was routine for field mammalogists to carry a vacuum container of liquid nitrogen for freezing specimens.

Using dry ice did not prove to be very practical during our 1964 expedition and that forced us to develop a different strategy. We decided to collect live specimens in the field, maintain them in suitable mouse caging, return to Mexico City, and do our laboratory work at Universidad Nacional Autonoma de México (known as UNAM). This decision converted Baker's expedition into a collaborative effort with our Méxican scientific colleagues. We visited UNAM where Professor Bernardo Villa-R graciously allowed us to use his laboratory with its floor-model centrifuge and freezers. All of this new activity—live trapping, maintaining animals in cages, rapid travel back to UNAM, blood drawing, centrifugation, and central storage of serum samples—completely altered field work tradition. How would an individual frozen blood sample be linked to the exact museum specimen from which it was obtained? How far could we travel from UNAM and still be able to return with live animals in a timely fashion? How many animals could be maintained and sampled? If several people were caring for live animals, how would there be enough time to do all of the other fieldwork?

On each trip back to UNAM I did the cardiac punctures to obtain the samples and Dan Womochel or Mike Petersen usually took care of the centrifugation. At the conclusion of the summer work, I left for The University of Kansas and, eventually, Mike Petersen used my blood samples in his dissertation research.

I arrived in Lawrence, Kansas in late August 1964 and left there by mid-September. It was, I suppose, about as short a graduate school career that a person could possibly have. I had just settled into an office in the Museum of Natural History, had signed up for courses, and was thinking about doing a Master's project on the zoogeography of the mammals of Oaxaca, México, when Professor E. Raymond Hall summoned me to his office. He wasted no time in telling me that he had decided that I should sign on to work for one year as a mammalogist for the Bernice P. Bishop Museum in Honolulu. I was pretty stunned, but it was obvious that Hall had already made all of the arrangements for me through his contacts with an entomologist cousin of his named J. Lindsey Gressitt.

The Bishop Museum had wonderful exhibits of Hawaiian animals and marine life and also was regarded as an important contributor to the study of human ethnography in the Pacific. Moreover, the institution was famous for hosting the Pacific Science Information Center—which had grown up during World War II—and for having a modern research wing devoted to entomology. The entomology wing had affiliations to the University of Hawaii and was home to a collection of scientists who did research on such things as insect and spider taxonomy, malaria, and the ectoparasites of birds and mammals. The fellow working on mammal parasites, Nixon Wilson, needed a mammalogist who could help identify specimens of Asian mammals that were hosts to parasites of potential or known medical importance.

I pointed out to Professor Hall that I knew nothing at all about Asian mammals. He gruffly replied that virtually no one else did either (in his opinion) and besides, thanks to Rollin Baker, I was the most experienced new graduate student at Kansas. Moreover, he had personally made arrangements for me to start my new position by studying the Asian mammal collections at the United States National Museum (now the National Museum of Natural History) in Washington, D. C., and at the American Museum of Natural History (AMNH) in New York. Thus, within days I was en route to Washington and a totally new and completely unexpected part of my education.

In Washington, I lived with an old Michigan State friend, Bruce Bandursky, who was employed by the government. The U-S-N-M, as we called it then, sat along side the mall, not far from the White House and all of the other features that make the capital district so entertaining. In those days life was far less formal than it is today, and government buildings were open and easily accessible. It was thrilling to wander about, on my own, seeing the city for the first time. In the Museum, the professional mammalogists, especially the late David H. Johnson, received me very warmly. David treated me more like a newfound son than like a stranger. He invited me to lunch on a regular basis, took me around Washington and, most important, spent hours with me going over literature, explaining his own field experiences in the Philippines and Solomon Islands, showing me through the map and gazetteer collection, going over localities, habitats, and terrain features, and personally taking me through the vast collection of Asian mammals. Every day we examined hundreds of specimens of particular groups of mammals and David patiently explained their biology and showed me their diagnostic features. One afternoon a scraggly bearded and wild-eyed fellow burst in on us while we were looking at Asian giant squirrels. He was filled with ideas about mammals—everything from their evolution to their behavior. His name was John Eisenberg and I regarded him as slightly odd but charismatic. Eisenberg of course went on to a great career in mammalogy and the scientific literature abounds with his creative contributions.

Overall, my time at the national museum was an exceptionally stimulating experience and when I departed, after slightly more than a month of intense study, I felt filled to the brim with new knowledge of animals, geography, and history. Moreover, I had met a new bunch of talented mammalogists.

Two days before leaving for New York I received a telegram addressed to me in care of the Mammal Section at the USNM (as I write this, the idea of a telegram seems amazingly quaint). The message was cryptic, but complete enough to be alarming. It was from the Bishop Museum and said: PREPARE FOR IMMEDIATE DEPARTURE SIX MONTH ANTARCTIC EXPEDITION; HAVE PHYSICAL EXAM AT MARINE HOSPITAL IN BALTIMORE. What in the world had happened? I did not mind the idea of an expedition to the Antarctic—it soon would be the beginning of summer down there—but I could not think of how it would help develop me as a mammalogist. WHAT EXPEDITION? AM GOING TO NY, NOT THE ANTARCTIC FOR SIX MONTHS, was my response. At the Bishop Museum a decision was made to allow me to continue with my planned studies at the AMNH, and nothing more was ever said about the odd communications. It turned out, however, that this Bishop Museum expedition was a huge success. The scientific team returned with specimens of free-living mites that they found on the snow.

The AMNH is at the edge of Central Park; living for a month in an apartment across the street from both the Museum and the Park was dreamlike. I was twenty-one years old and an institution in Honolulu was paying my apartment costs, living experiences, and salary, and I could work in the Museum and spend autumn weekends drinking red wine and playing touch football in the park.

The collection of Asian mammals in the AMNH introduced me to the results of major expeditions into the wilderness of New Guinea and the mainland of China. I saw for the first time the vast array of marsupial mammals obtained during the Archbold Expeditions, and spent considerable time with the late Hobart van Deusen, who had actually been afield in New Guinea. Much to my surprise, one afternoon while studying mammal specimens in the Archbold Collections, I bumped into an elderly Australian gentleman named Ellis L. G. Troughton, or "Troughtie" as he liked to be called. We hit it off immediately. Troughton had written a book entitled, *Furred Animals of Australia*, which at that moment was the single best work on the subject. Among other things, Troughton had discovered the New Guinea "singing" dog, which he named, *Canis hollstromi*. In addition to spending delightful hours picking from Troughton's brain his varied historical and experiential recollections and knowledge of Australasian mammals, I had other privileged moments at the AMNH. The most memorable, perhaps, was an afternoon tea in the walnut-paneled Roosevelt Conference room. The late Karl F. Koopman, who already was quite famous for his research on bats, invited me to accompany him and when we entered the room I was shocked to see both Margaret Mead and George Gaylord Simpson seated at the table, engrossed in conversation with several of the curators. I took a chair across from Mead, sipped hot tea, and wondered how I could be so fortunate as to be in the presence of such intellectual luminaries. In retrospect, Mead's noble savage style of writing about the supposed sexual innocence of South Pacific islanders was too good to be true and I gather that her reputation has been tarnished posthumously.

My year in Honolulu further complemented my education, continued to expand my understanding of biology and, most significant, broadened my thinking into subjects that were previously unknown to me. For one thing, I worked with non-mammalogists who were primarily interested in acarine parasites. Nixon Wilson and William Voss spent entire days with their faces plastered against the oculars on their small dissecting scopes, examining in incredible detail the most subtle morphological features of ticks and mites taken from the fur of mammals sampled in Laos, Hong Kong, Taiwan, Viet Nam, New Guinea, and the Solomon Islands. In order to have scientific conversation I had to read their papers, which were mostly taxonomic descriptions of animals that I knew absolutely nothing about. Then, in order to learn about the biology of these odd tiny creatures, I had to dig out some textbooks and start reading. I claim no expertise on ticks or mites or chiggers, but while at the Bishop Museum I analyzed some field data and wrote a paper for the *Journal of Medical Entomology* on the subject of factors (body size, habitat) that affect ectoparasitic infections of small mammals (Phillips, 1966). A few years later, after I finally was really in graduate school at The University of Kansas, I discovered the first (and still the only) mites that live in the mouths of mammals. These tiny parasites dig their way down into the soft periodontal tissue that holds teeth in their sockets (Phillips et al., 1969; Radovsky et al., 1971). Eventually I enjoyed some minor-league celebrity on account of these mites. My friend Robert Baker, a devotee of the *New Yorker* magazine, sent me a copy of a poem about mites authored by John Updike. Talk about being cited! Updike had the wisdom to mention my oral mites (which by then had made their way into Parasitology textbooks). Updike wrote, "to dwell happy in the mouth of a long-nosed bat, like one species of Macronyssidae's protonymph, causing tissue destruction and loss of teeth..." This whole business played out over a period of twenty-three years because in 1992 my daughter had dinner with Updike (at Ursinus College) and persuaded him to sign my copy of his piece (Figure 4).

In fall, 1965, I returned to Lawrence, ready to focus on formal education. I brought with me a copy of my Master's thesis, which I wrote while working in Honolulu. I often have wondered if I am the only student to show up for graduate school with a draft thesis already in hand. Probably there are others, but I found it satisfying to finally begin my graduate studies with the thesis pretty well finished, expeditions already under my belt, studies at the USNM and AMNH already conducted, work experience completed at the Bishop Museum, and 14 articles published, in press, or about to be submitted. All of this, of course, is the consequence of the fact that I entered Michigan State University basically knowing what I wanted to do. The

to Karl—
for the discoverer of
Macronyssids
without which this
poem could not have
been written
John Updike

MITES

A house-dust mite (*Dermatophagoides farinae*)
is not a house-mouse mite (*Liponyssoides sanguineus*)
any more than speaking Portuguese is speaking Manx
or an elephant is a hyrax, though both are ungulates.

To be a mite at all! To be so small
you can rest as in a bunk bed beneath a flea's scales,
or expend a lifetime in a single chicken feather
or the mite-pouch (acarinarium) of a carpenter bee!

To dwell happy in the mouth of a long-nosed bat,
like one species of Macronyssidae's protonymph,
causing tissue destruction and loss of teeth,
or beneath the skin of a mammal, creating mange!

Think how Nature slaved over these arrangements.
Thirty thousand species of acarines,
fifty from the Antarctic alone, and some found
three miles up Everest, or a mile down in the sea!

Itch mites, cheese mites, monkey-lung mites, each
making its way through the several larval stages
to an awkward copulation (discounting
parthenogenetic ticks) and easy death—

what fresh perspectives! Specks of our shed skin
delicious boulders, our human pores
lubricious dish-shaped living rooms, and particles
our largeness elides palpable to mites, who loom

in the scanning electron microscope's patient gray
light as many-tentacled, with chelicerae,
as hobbled as stegosaurs by their quaint
equipment, as endearing as baffled bears,

these mini-spiders characterized by
lack of a waist, lateral eyes, and tininess.
We marvel; we pity; we loathe; we try to forget
perspectives from which *we* are smaller yet.

—JOHN UPDIKE

Figure 4. John Updike wrote a poem for the *New Yorker*, mentioning the oral mites that I discovered in the mouths of bats.

clincher was that Rollin Baker had sent me in the right direction and had applied serious course correction when needed.

Fortune continued to smile on me. When I returned to the Museum of Natural History at the University of Kansas, the new mammalogy graduate students included Hugh H. Genoways, Jerry R. Choate, James Dale Smith, G. Lawrence Forman, Ron Turner, and Duane A. Schlitter (Figures 5, 6, 7). Over the next few years, Elmer C. Birney, David Armstrong, John Bowles, Thomas H. Kunz, Larry Watkins, Merlin Tuttle, and Paul Robertson joined our group. Collectively, this group has published about a thousand scientific articles and books. Four (Phillips, Choate, Genoways, and Kunz) have received the top research award (the C. Hart Merriam Award) from the American Society of Mammalogists and three (Genoways, Birney, and Kunz) have served as their elected President. Tuttle founded Bat Conservation International (in Austin, Texas) and became a nature photographer. In fact, several of his bat portraits have been used on recent US postage stamps!

Figure 5. Armed, dangerous, and a little desperate-Jerry R. Choate on a hunting trip with Carl Phillips, Hugh H. Genoways, and James D. Smith. Isla Ometepe, Nicaragua (summer, 1967).

Figure 6. Hugh Genoways, nearly knee-deep in mud, retrieves a bat from a mist net. Nicaragua 1968. Hugh and I have done fieldwork together in Yucatan, Mexico, Central America, Suriname, and throughout the Antilles.

Most important, this talented group of graduate students formed tight bonds of personal and professional friendships that have lasted over the years. This is more than a 'nice' thing to say; I believe these bonds created a fabric that influenced and improved what any individuals could singularly bring to their careers, their home institutions, and their future students. Some of these friendships were more personal than others, some more competitive than others, and others waxed and waned according to various factors, but in the end nearly everyone felt that they could count on the others when chips were down. Sadly, two members of this group, Elmer Birney and Larry Forman, have already died.

It was in fall of 1965 that I really became acquainted with E. Raymond Hall. Hall was both a legendary figure and a real person and it sometimes was difficult to tell which role he was playing. The odd phrase "larger than life" comes to mind (Figure 8). Most of the incoming graduate students had heard "Hall

Figure 7. J. Knox Jones, Jr. (left) offers advice while James Dale Smith skins a deer taken in Nicaragua (summer, 1967).

Figure 8. E. Raymond Hall afield in Nevada in 1928. Note the overloaded field vehicle. Photograph courtesy of Biological Survey archives.

stories" from various sources, and the more senior students (such as Charles L. Douglas and Gary Packard) told us about their own experiences with him.

E. Raymond Hall was the rare American academician who expected students to address him as "Professor Hall," and nothing else. Indeed, it should be noted that most of his campus faculty colleagues also addressed him in this way; none called him Ray or Raymond or Eugene. Although he displayed a Teutonic formality on campus, Hall often referred to himself as "mister" when he dealt with non-university folks. One of my early observations was that he always was Mister Hall to local farmers or ranchers. At the same time, he could wield his title in ways intentionally designed to intimidate others. For instance, on several occasions while I was in his office he telephoned the Governor ("hello Bill, this is *Mister* Hall over at Lawrence") or even the Secretary of the Interior ("Stewart? This is *Professor* Hall") and displayed for me the ways he leveraged to his advantage by combining the friendly familiar with the formal. No matter what, E. Raymond Hall was always in charge, the final word on any subject.

Because I showed up with a draft thesis in hand, and various scientific articles published or ready for submission, my first interactions with E. Raymond Hall revolved around writing. In this regard, Professor Hall believed that I needed his assistance. He thought that my writing was dreadful, and said so in front of my fellow students. Naturally, they found this amusing, frightening, helpful, and instructive, depending upon circumstances. When I emerged from painful hour-long editing sessions with the great man, I was accosted immediately and barraged with questions. What did he say? What did I learn about writing? My fellow students gathered in my office and took notes as I reviewed my latest training in the basics of English.

Some of Hall's rules of writing were foolish and created awkward or even impossible problems with syntax. The most famous, of course, involved his prohibition against using nouns as adjectives and this led to a joke about "gopher mounds," versus "mounds of gophers." At the same time, Hall's actual point was that one ought to strictly follow the rules of English grammar so that one's writing could be understood locally and accurately translated elsewhere. This point was Hall's strength as a scholar: he taught his students that unambiguous communication of ideas was the essential ingredient to being a scientist.

E. Raymond Hall believed that one could judge intelligence and the quality of thought on the basis of a person's writing. As an extension of this, he insisted that words be used correctly. By this he meant that we were restricted to first meanings in unabridged dictionaries. He preferred dictionaries published before 1952 because this was the year that saw inclusion of slang and, therefore, a precipitous decline in the quality of dictionaries. Hall's dogma forced all of us to doubt our prior knowledge of word meanings (slang use having predominated in our earlier experience) and to consult an unabridged dictionary on a regular basis. It also gave rise to a famous Hall maxim to 'say what you mean and mean what you say.'

Having admitted that Hall regarded me as a poor writer, I also must brag that he eventually paid a few lukewarm compliments. Seven years after I completed my Ph.D., he wrote, "congratulations to you (and B. Steinberg) on the excellent and most interesting paper treating tooth structure in *Desmodus rotundus*." The paper he read (Phillips and Steinberg, 1976) had been published at Texas Tech. A year later, Hall congratulated me a second time, on the occasion of a paper entitled, *Salivary Glands: Comparative Ultrastructure of Secretory Cells* (Phillips, 1976). This time he wrote, "...the part I read was real interesting, clearly written, and a credit to the author." Incidentally, it also was typical of E. Raymond Hall to offset his compliments with something that might keep things in balance. So, in this instance he took time to make it clear that he had read "part" of the article, but certainly not the whole thing.

Later in my career—well after Graduate School—I frequently relied upon lessons learned at Hall's desk. The upside, in University politics, was that I could punch holes in social science diatribes. The downside was that the Hall style of writing was uninteresting and stilted and it has been difficult to break free and express ideas in a more creative or informal style.

Perhaps the strangest side affects of Hall's teaching were two events that nearly derailed my friendship with the late Elmer C. Birney. Elmer and I shared an office while we were at Kansas and became close confidants. Unlike me, Elmer had come into the program to work with J. Knox Jones and therefore had not had the pleasure of Hall's private tutorials.

Shortly after Elmer moved into the office, he made the mistake of asking me to read a draft of a paper that he was writing. I in turn made the mistake of rigidly applying Hall's editorial lessons to Elmer's draft. Elmer mumbled about my arrogant and wiseass comments for several days afterwards but I felt justified because it gave Elmer a taste of what it was like to work with Professor Hall. We both got over it, but did not write a paper together until nearly thirty years later! And that first joint paper was an editorial chore for both of us. Beginning in 1996 we enjoyed a burst of collaborative productivity based on our field program to investigate the biogeography of Patagonian mammals (Birney et al., 1996, Hillyard et al., 1997, Sikes et al., 1997, Kim et al., 1998, Monjeau et al., 1998).

The second event, which also had the effect of delaying opportunities to work together, revolved around a manuscript that neither Elmer nor I had written. In 1988 I served as Managing Editor for the Special Publication Series of the American Society of Mammalogists. At this time one of my tasks was to shepard a particular manuscript through to its publication as a small book. The manuscript was entitled, *Dispersal in Rodents: A Resident Fitness Hypothesis.* Paul K. Anderson of the University of Calgary was the author, and Elmer was Editor for the Special Publication Series.

Shortly after I became Managing Editor, Elmer forwarded Paul Anderson's manuscript to me at Hofstra University. In his covering letter, Elmer made the mistake of asking me to take time to read it over, and make any editorial suggestions that I deemed necessary. This was an unusual request because as Managing Editor my real task was to mark the manuscript for the printer, oversee the preparation of an Index, and select a dust jacket design (for which I did the artwork, and signed it too).

Although I already knew the manuscript had been accepted for publication and although I already knew that Elmer thought it was exceptional (and had been using it in his graduate level seminar class at the University of Minnesota), I nonetheless made the mistake of sitting down to read it with an editorial eye. Unfortunately, I did not like it. I stewed about my reaction for several weeks and then took pencil in hand and went about trying to improve it. In retrospect, I realize that E. Raymond Hall had seized my editorial soul (although Hall was now dead, he still operated through me, I believe, from some other-worldly hot spot). Elmer did not take my candid editorial critique very well, and it probably was a good thing that he was in St. Paul and I was in New York. It took several months, but we reached *détente*. Poor Paul Anderson was left with the task of making considerable revisions just when he was anticipating publication, at long last, of his masterpiece. I still believe that the final product was an improved version of the original, although I herewith apologize to Paul for my belated entry into the manuscript-bashing contest. Elmer and I did not speak another word to one another on this topic until 1995. The hatchet-burying occasion was an austral autumn day when the two of us were alone, setting Sherman mouse traps on a rocky bluff in the middle of nowhere—actually at Meseta el Pedrero, 46° 48.08' S, 69° 38.53' W in Santa Cruz Province of Argentine Patagonia.

Field expeditions offer a wonderfully unique, private, opportunity to clear the air. In all of our Patagonian fieldwork together, Elmer and I used the time to talk science, talk careers, and share personal insights. We always went out together when we hunted or set traps and our shared experiences are among my fondest memories. When Elmer died, unexpectedly, my contributions to his professional obituary focused on these occasions (Genoways et al., 2000). I truly miss him.

So, Professor Hall's ideas about writing loomed large and took their toll in various ways. When I returned to Kansas from Hawaii, in fall of 1965, I brought with me a partially written manuscript in which I intended to describe and name a new subspecies of bat from Malaita Island in the Solomons. I had examined the specimens and written a description while I was in Honolulu so when I returned to Kansas I only needed to touch up the manuscript (or so I thought) and add in a few details about the biology of this previously unknown animal. Accordingly, I wrote a letter to Philip Temple, a New Zealander who had collected the bat specimens for the Bishop Museum, and requested some information about habitat and circumstances of capture. In the conclusion of my letter I mentioned that I intended to name the new subspecies after him. Unfortunately, as it turned out, Hall was in a mood in which he adamantly refused to allow me to name this subspecies after Temple, so I named it *Hipposideros diadema malaitensis* (Phillips, 1967) and was obliged to write Philip an "oops, sorry about that," type of letter. Unbeknown to me, Temple at the time was doing some soul-searching, wondering if anything he had, or would, accomplish would earn him suitable recognition. Consequently, he did not take the experience every well. Eventually he received a Katherine Mansfield Fellowship, which is New Zealand's highest literary award. He used the occasion to present a speech entitled, *A Subtle Immortality*, which later was published by *Reader's Digest*. His version follows.

> "At this low tide in my fortunes, there came a glimmer of hope from an unexpected source. A letter arrived from Carleton Phillips, a graduate student working in Zoology at the University of Kansas. A bat I had collected in the Solomon Islands years before had arrived, pickled in alcohol, on his laboratory bench. Could I tell him where and when I found it? I remembered an island with dripping cave walls glinting in the light of my torch, bat guano beneath my boots, strange spidery insects that waved in the gloom, and the chittering of bats with faces like those of miniature Frankenstein monsters. I remembered it all, the stink and the heat.
>
> Phillips responded...that this Solomon Islands bat was new to science. Its Latin generic name, *Hipposideros*, meant "iron horse"—a clear reference to its grotesque features. To the Latin word would be added, in my honor, the word *templei*. I felt a small thrill of pleasure, but it was to be short-lived. Phillips' suggestion was overruled by his professor [E. Raymond Hall], who was in favor of giving the bat an *informative* Latin name [his italics]."

The good news in this story, for Temple, was that eventually someone named a mite after him; as he put it, "...whenever entomologists contemplate those tiny hairy bloodsuckers, they will remember me (Temple, 1979)." At least the fellow had a sense of humor.

As a sidelight to this story it is also worth remembering that graduate students never got anywhere by arguing with Professor Hall—especially on an issue ultimately so minor as whether or not to name a new subspecies after a person. If Hall said, "jump," a wise fellow just jumped without even asking, "how high?" Hall had the ability to intimidate students; intimidation was good in the sense that it drove all of us to do well, think clearly, and try hard. One down side to it was that we all harbored a certain amount of fear for the man. 'Dislike' is another way to express it, but I think this is too strong a word to describe the typical relationship that he engendered. A more important—and sad—down side was the realization that Professor Hall had little regard for most other mammalogists, particularly those more or less of his vintage. He seemed to go out of his way to disparage the ideas and scientific articles produced by them. For instance, he conducted long-term battles with William B. ("Doc") Davis at Texas A&M, Henry Setzer and David Johnson at the USNM, and even the venerable Steve Durrant at Utah. The most legendary of all of these battles was between Hall and Walter P. Taylor. Taylor had conducted extensive field collecting and study of mammals in the Philippines, served as an allied guerrilla fighter and spy during the Second World War, and had ended up at Kansas in the 1950's. In the legendary tales, Hall and Taylor did every thing but kill each other and in most versions that made the rounds in the 1960's, it seemed that Taylor usually got the best of Hall.

E. Raymond Hall's relationships with other people were common themes in the stories told about him. It was not unusual for Knox Jones and I to sit in Knox's office, espeically on weekends, and chat about everything and anything. On several occasions Knox became maudlin and sadly remarked that Hall was the only man he knew who had no real friends, "not one." As Knox put it, "no one was good enough for him."

Along these same lines, there also was an unspoken notion that Hall was a closet racist. Although I never heard him utter a sentence that suggested such was really the case, Hall's chilly—usually critical and hard-nosed—relationships with others and his opinions about our species in general did not help his reputation. Hall saw human beings as mammals, typical primates in fact, rather than as magical beings with a spiritual quality of some sort. In this regard his opinion was probably the same as that of many mammalogists. Where he got into trouble was his insistence upon applying the term 'subspecies' to races of human beings. His personal written comments about human beings were another source of trouble. When he wrote his monumental volumes entitled, *Mammals of North America* (Hall and Kelson, 1959), Hall quite naturally included *Homo sapiens*. In the species account he wrote, "Nor am I convinced that the gap in intelligence between some microgeographic races of man and some races of apes is very wide—no wider than that between genera of some other families....(Hall and Kelson, 1959:234)." There is only one way to interpret this statement, and it invariably leaves a bad taste. Hall's coauthor (Keith R. Kelson) and the many graduate students who worked on the book project were aghast. J. Knox Jones told me that they collectively insisted that Hall sign this statement so that it would be clear to readers that this was his personal view, and not that of his collaborators. Thus, the account carries the initials ERH.

It was widely believed that pretending to not know a person's name was another one of Professor Hall's unnerving idiosyncrasies. Indeed, this behavior was taken as indicative of and consistent with his attitude toward people. In retrospect, however, I decided that he actually did have trouble remembering names or matching names to faces. To compensate for this, he used a form of symbolic logic. For example, one afternoon Hall wanted to introduce me to a visiting scientist from a major museum. As was usual with him, he seemed unable to remember my actual name and when I did not speak up and introduce myself (a willful act on my part), he introduced me as 'rabbits.' At the time, I was working for the Kansas State Biological Survey, investigating illegal fur trapping of cottontails in eastern Kansas. This was a dicey task, and I admit to carrying a concealed weapon while doing it. On another occasion (in 1966) Hall told me of an interesting scientific paper by a faculty member at Hays [Fort Hays State University]. When I asked for the name of the senior author, Hall insisted it was a man named 'Strongbug.' Eventually I discovered that the man's actual name was Eugene Fleharty. A 'hearty' flea surely is a strong bug. I told my fellow graduate students about my experience and as a result Gene Fleharty's many friends still refer to him as Strongbug.

I quickly completed my Master's work with Hall (and published the thesis that I had written at the Bishop Museum; Phillips, 1968) and switched to J. Knox Jones, Jr., as my dissertation advisor (Figure 9). My reason was simple: Hall's view of science was far too restrictive for my creative impulses. For his part, Hall did not shed any tears and if he were alive today he probably would edit the previous sentence by replacing the word creative with the word foolish. In any case, had I wished to pursue traditional research in mammal taxonomy or zoogeography, Hall would have been an excellent advisor. Knox Jones, by way of contrast to Hall, made it clear that he would allow me to chase whatever ideas—no matter how off the wall they might be.

Figure 9. J. Knox Jones, Jr. afield in Nicaragua (1967) and enjoying baby four-eyed opossums (*Philander opossum*). We later wrote two papers on reproduction and postnatal development in Latin American opossums (Phillips and Jones, 1968, 1969).

In fall, 1966, I was offered an opportunity to do fieldwork with a scientific team from the University of Maryland School of Medicine. The project was funded by the United States Army Medical Research and Development Command (Commission on Rickettsial Diseases, Armed Forces Epidemiological Board) and headed by Charles Wisseman, Jr., who at that time was Head of the Microbiology Department at the University of Maryland. One of Wisseman's faculty members named Robert Traub—or Colonel Traub, or simply 'the Colonel'—was the fellow who actually organized and led the field team. Traub was a renowned expert on fleas, a raconteur, a vigorous debater, and, in the end, something of a delightful if slightly eccentric know-it-all. The opportunity to work with the Colonel, expand my field experience to Asia, and learn first-hand about the interrelationships among ecology and geography and mammal species and their important pathogens, was yet another lucky break for me. So, off I went with the result that between September and early November I was pretty much incommunicado, living in various stone or mud houses along the dirt roads connecting the villages of Dir, Gilgit, and Chitral, far up in the Northwest Frontier of Pakistan. On one clear morning I happened to look to the east and saw the snowy peak of Nanga Parbat—an enormous and intimidating piece of Himalayan real estate.

Life in the mountains was not easy on account of the wet (snow and rain) and cold autumn weather. Moreover, the research project was far from simple in scientific terms as well as in terms of logistics and politics. Our field team officially included Robert Traub's technical assistant, or "batman" and former Sergeant, James O'Keefe, a virologist from Johns Hopkins named Jerry Coleman, and two young Pakistani scientists named Abid Beg Mirza and Mohammad Iqbal. Various other assorted characters, mostly Pakistani spies, or military medical types and their assistants, hooked up with us here and there, sometimes collecting blood samples from villagers and sometimes illegally selling USAID-supplied medicines on the local black market.

We traveled much like a gypsy circus in several brightly painted and ornamented locally acquired trucks and a couple of jeeps. We carted along cages of laboratory mice so that Coleman could isolate viruses and rickettsia while we were on the move. Viewed from a perch on high rocks, it must have been some scene as we inched along the mountainous switchbacks leading uphill from the city of Malakand. Here and there we paused to allow human foot traffic access to the one lane road or to use our boots to push aside camels clogging the way and making life perilous. The first afternoon a truck filled with Pakistani soldiers, driving directly in front of my jeep, took a wrong turn, or perhaps collapsed the semi-existent "shoulder" on the road, and went tumbling over the side, into a virtual scrubland abyss. Those of us who were following braked to a halt and a substantial number of us (and assorted pedestrians and camels) crowded onto the roadway and looked downhill at the distant dust cloud that presumably marked the spot where the army truck had landed. The air was filled with shouts, curses, and prayers, all uttered in Urdu or Persian, and I still remember someone in our group muttering, "holy shit that's a long ways down there." We continued on, nothing much could be done for the dear departed Pakistani soldiers, whoever they were, and I began to think that we were entering some pretty dangerous territory.

Sure enough, in the vicinity of Dir, villagers were engaged in a tribal war. At nightfall, which was about five in the afternoon, gunfire echoed through the valley and even automatic weapons sometimes added their voices to the din. All in all it was noisy. Our task seemed simple and familiar: we were obligated to set out trap lines for rodents in late afternoon and check them during the night (more or less at about midnight) and again at sunup. Colonel Traub was adamant about the nighttime checks, and it was while doing these that I first had the experience of hearing bullets whistle past my head. Jim O'Keefe usually worked with me and being an Army veteran from both the Second World War and Korea he reassured me that anything I heard would not be a problem. "You won't hear that one that gets you," was how Jim put it and of course I felt much better. I also remember thinking that Rollin Baker had never mentioned this part of mouse trapping when I was a student at Michigan State University but then realized that I was mistaken because Baker had shared with me his experiences as a Naval officer in NAMRU-2. It was just that back then I did not appreciate the conflict of trying to collect wild mammals in the midst of human beings trying to collect their own species.

The Pakistan project caught my fancy because it opened my eyes to the interesting interrelationship between rodent species, their historical zoogeography and ecology, and their potential role as vectors. The scientific return from the collecting was enormous—museum specimens of the mammals ultimately would be deposited in the USNM where they would serve as vouchers for the disease-related research and as subjects of study for current and future systematists and others. After I returned home to Kansas, I set to work analyzing some of our Pakistan data and then wrote one of my favorite scientific articles (Phillips, 1969). My work focused on the relationship between species distribution of Himalayan voles (genus *Hyperacrius*) and their ecology and the occurrence of the chiggers associated with scrub typhus, or tsutsugamushi disease. My interest in this subject has not waned. I have studied viral infections in salivary glands (Tandler et al., 1998) and just recently I helped write an article (with Linda Allen) on mathematical modeling of viral co-infections in rodents (Allen et al., 2003).

When we completed our work in Pakistan, we bade farewell to our colleagues there and set out for the much warmer island of New Guinea, via Thailand and Malaysia. The New Guinea project was somewhat similar to the one in Pakistan in that the purpose was to document the distribution of small mammals and their associated ectoparasites, again with an emphasis on the ectoparasites species known to transmit scrub typhus.

Back at Kansas, J. Knox Jones, Jr., had pieced together a major field program in Nicaragua. The U. S. Army Medical Research and Development Command funded the program, which supported a series of graduate student expeditions designed primarily to obtain specimens of mammals and their ectoparasites (Jones and Phillips, 1969; Figure 10). Coincidental with this, I rediscovered an old interest of mine and also developed a new, totally unexpected, interest in microanatomy. The first interest was mammalian dentition, which traced back to my undergraduate experience of working for Hunt, Hoppert, and Rosen and their dental caries project. In addition to knowing something about dental pathology, I also was aware of their thoughts on the role of salivary glands, and saliva, in reducing the incidence of caries. The second and ultimately more important new interest occurred because I wished to look at the histology of teeth and needed to learn basic techniques for preparing and viewing calcified tissues.

My new interest in microanatomy was aided by happenstance. One of Knox Jones' students was G. Lawrence (Larry) Forman. Forman was hard at work in the Museum at Kansas, using optical microscopy and histological and histochemical techniques to compare sperm morphology and, later, gastric mucosae, in various species of bats. It had occurred to Larry that the gastric mucosae in bats that fed exclusively on fruit ought to be different from gastric musosae in insectivorous or netar-feeding bats. He tested this hypothesis in seminal work described in his published dissertation and went on to a highly successful faculty and administrative career at Rockford College in Illinois. For years after graduate school, Larry was affectionately known as 'nuts and guts' Forman.

When Larry and I first became acquainted, I had absolutely no knowledge of basic histology. In fact, while I was an undergraduate student at Michigan State University I had deliberately avoided taking the histology course and had been heard to say that I did not intend to do research on anything that could not be observed with the naked eye. How wrong I was. In retrospect, I believe that this was the only instance in which I effectively decided that I would rather not know a specific body of human knowledge.

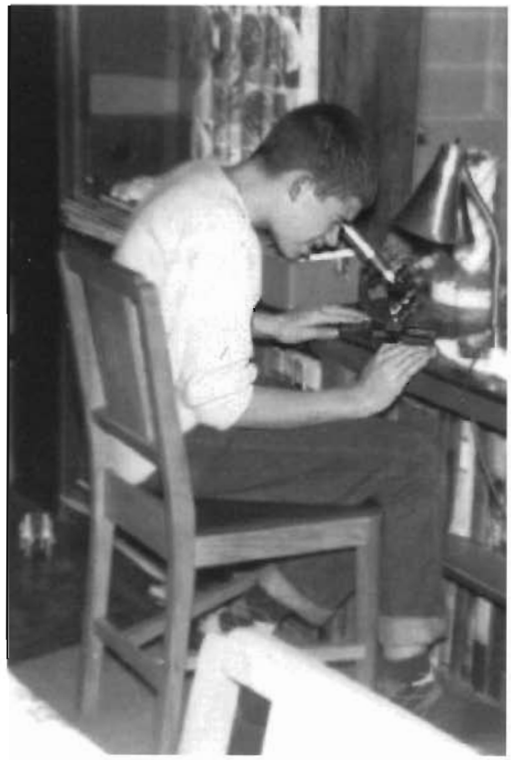
Larry Forman was patient in regard to my ignorance. He kindly taught me how to decalcify, embed, section and examine bat teeth and jaws. He also allowed me to use his makeshift histology laboratory to stain my slides. By makeshift I mean that his slide warmer consisted of a gooseneck lamp bent over a tobacco can lined with aluminum foil (nearly everyone smoked in those days and the majority of us preferred pipes and bought tobacco by the can) and his staining system consisted of a lineup of old Gerber's baby food jars. This histology "laboratory" was simply a bench in the small upstairs penthouse where three of our fellow graduate students—Jerry Choate, Jim Smith, and Hugh Genoways—were parked. Most important of all, however, this was a stealth laboratory. That is, it was kept secret from Professor Hall. The standing joke was that Larry and I referred to the setup as the "Hall Memorial Histology Laboratory," in recognition of the fact that Professor Hall did not believe that such activities should be conducted in a museum.

Figure 10. Carl Phillips (the guy on the right) with an anteater. Hato Grande, Nicaragua (summer, 1967).

In the end, it was largely because of Larry Forman that I eventually focused so much of my research career on investigating organ system evolution at the cellular level. The other person who affected me in this regard was the late Gary W. Grimes. Shortly after I accepted a faculty position in Biology at Hofstra University on Long Island (in fall, 1970), I was assigned the task of chairing a Faculty Search Committee. The Committee was to identify, recruit, and hire an 'electron microscopist,' and Grimes turned up among our candidates. Gary had earned his Ph.D. at Indiana University, working with a National Academy scientist named Tracy Sonneborn. Although he knew nothing about mammals, or field research, Gary did know a great deal about cell ultrastructure, transmission and scanning electron microscopy, and developmental biology. Developmental morphogenesis of ciliated protists was his area of interest, and through him I learned quite a bit about these interesting critters. Gary was also very interested in non-genic inheritance which might have important evolutionary implications. While Larry Forman had taught me the basics of histology and histological techniques when we were at Kansas, Gary took time to teach me about electron microscopy and cell structure when we were faculty colleagues at Hofstra University (Figure 11).

As I accumulated new knowledge about the basics of cell ultrastructure, I began to wonder if mammalian cellular diversity was as extensive as mammalian diversity at a gross—morphological—level. Giraffes, bats, mice, and whales are distinctive and it is easy to speculate about the significance of long necks, flippers, wings, but what was going on at the cellular or tissue levels? From the cytology and cell biology literature in the 1970's it appeared that there were morphological cell "types," but there was an unstated implication that cells of a particular type were essentially the same in all mammals. I noticed, however, that virtually everything known about cell structure in mammals at that time was based on two or three species, out of more than 4,000!

I wondered if particular cell types were essentially identical in all mammals. Although no one to my knowledge had formally asked that question, and sought an answer to it, it seemed possible that cells were highly conservative. On the other hand, from my experience with histology and my appreciation of the diversity among mammals, I hypothesized that at least some types of cells might evolve rapidly enough to be substantially different in various mammals. To me, cells were important because they provide the microenvironment in which gene expression takes place. I guessed that a combination of cell structure, location or presence or absence within particular tissues, and gene expression all could affect the nature and use of gene products. I struggled, and failed, to do an acceptable job at articulating this idea in the mid-1970s. At the time, of course, it was impossible to think about sequencing nuclear DNA or isolating mRNA from wild



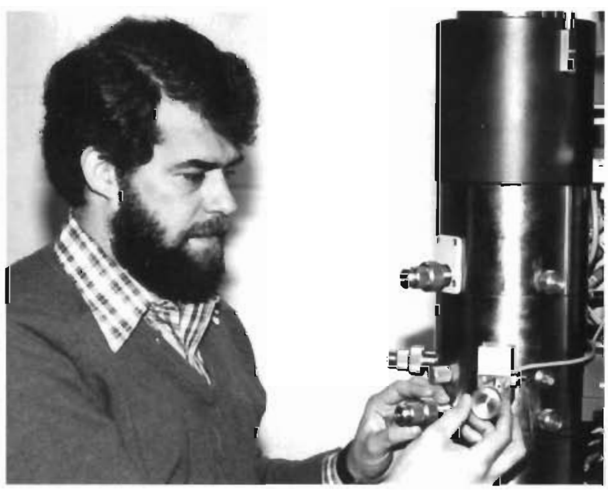


Figure 11. Top: Using my first microscope in 1958. Bottom: I learned about transmission electron microscopy when I was on the faculty at Hofstra University. Today, this Philips 101-TEM is nearly an antique. Fall, 1980.

mammals. Thus, my ideas about cell evolution were difficult to share, or understand. I did write quite a few papers on the subject, although it was not until 1988 that Larry Forman telephoned me from Rockford, Illinois, to say that he finally understood what I had been trying to say for nearly fifteen years. Larry had just read a comprehensive statement on the subject, which I had written with Bernard Tandler (Phillips and Tandler, 1987). Rather than being pleased by Larry's call, I spent the next few days in gloomy realization that if Larry had not been able to understand me, probably no one else did either.

In 1969 I completed my work at Kansas and was ready to seek some type of employment. Although nearly all of my colleagues dreamed of taking traditional curatorial positions in Museums, or faculty mammalogy positions at universities, I was convinced that I should do something different. Why this was the case is unclear even now, but I never regretted the fact that I saw graduation as an opportunity for new experiences rather than as a time to settle into an academic career. Knox Jones understood this about me, and confided that he had always anticipated that I would end up on a different pathway from Genoways, Choate, Birney, and Smith, all of whom were also about ready to complete their studies. To help me out, Jones had contacted his acquaintance from Russell, Kansas, who was a new United States Senator named Robert Dole. Knox suggested that I look into spending a "post-doc" year in Washington, perhaps working as a science staff member, and had asked Senator Dole to see if he could find something for me.

In March 1969, Bob Dole sent a recommendation letter on my behalf to the White House (the now old fashioned carbon copy, signed by Dole himself, is a prized keepsake of mine). Harry Flemming, a Special Assistant to the newly elected President Richard Nixon, wrote me, "...I want you to know that the President appreciates your interest." It did sound interesting, but the process was so slow that instead of going to the White House science staff, I accepted a post-doctoral position in Aerospace Biology at Grumman Aerospace Corporation on Long Island. Given the subspecies of characters that did go to the White House staff in 1969, I never regretted the way life unfolded for me.

My time at Grumman forced me to read a vast amount of new (to me) literature on human physiology so that I could write internal think tank type papers on such things as intermittent photic stimulation (for the E-2C Hawkeye), high altitude and high velocity ejection from aircraft (for the F-14 Tomcat fighter Fig. 12), bone calcification and decalcification under low or zero gravity conditions (future missions to Mars), psychological (yes, psychological) and physiological aspects of long-duration space flight, and the biological effects of microwave and ionizing radiations. As luck would have it, one night I received a telephone call from my boss, Robert Delvechio, who informed me that there had been an explosion in the command and service bay on the Apollo 13 mission to the moon. My first assignment was to read everything I could find on the effect(s) of chilly ambient temperature and dehydration on human performance (the sports science literature, which was still pretty weak back then) and develop part of the answer for the question: can the Grumman LM be used as a lifeboat for these three astronauts? The next day our group added another question, which was how to create an additional CO_2 scrubber out of available onboard materials. The recent book and movie about the Apollo 13 mission might give the impression of confusion, but in reality the Life Sciences and Systems Analysis folks at Grumman were pretty talented and calmly went about the business of helping solve the astronauts' dangerous dilemma. The Grumman LM was one star of the show and our lunar mission support team at Bethpage was another.

When I joined the faculty in Biology at Hofstra University, in 1970, I already had had an incredibly varied experience. The extent to which this proved helpful is difficult for me to assess, although I do know that it has made it relatively easy for me to appreciate the scientific interests of people well outside my own field. The shifting focus of my research interest, from traditional taxonomic and biogeographic mammalogy to histology and then to comparative cell structure and, eventually, molecular genetics to some extent must be a consequence of that broad early experience.

As a young faculty member at Hofstra I wanted to pursue my novel interest in comparative cell structure, but was confronted by a major obstacle. How

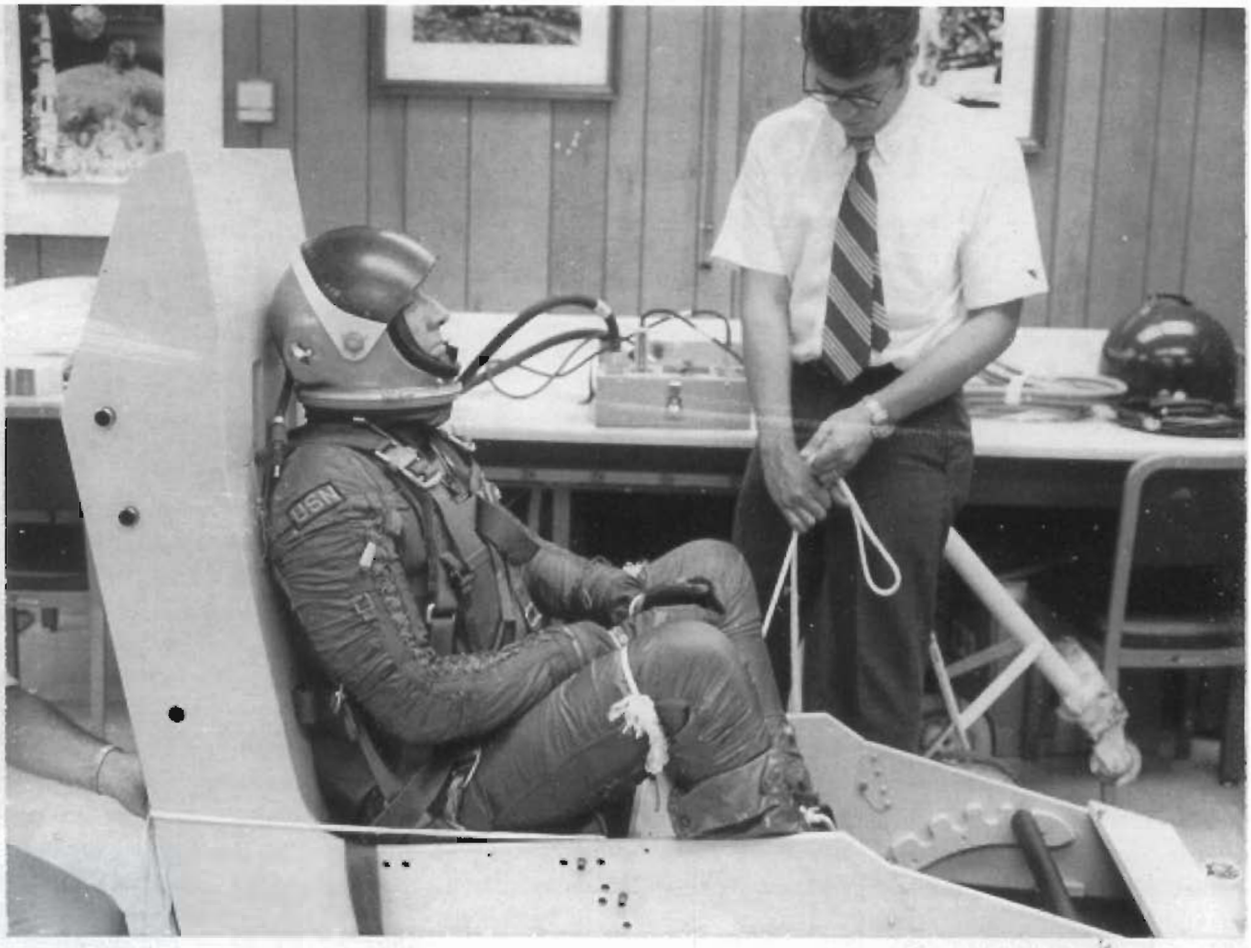


Figure 12. Post-doc days in aerospace biology: although I appear to be restraining an astronaut, this photograph actually is from a physiology and engineering study for a high altitude-high speed ejection system.

would I obtain the materials for study? Fieldwork alone was not the answer, although I could fairly easily do that. The problem was tissue fixation. Transmission electron microscopy (TEM) required special, ultra rapid fixations with freshly mixed chemicals. At that time it was believed that such fixation could only be accomplished in the laboratory and that acceptable quality could only be obtained through perfusion. Not surprisingly, at that moment (1971) nearly everything known about the ultrastructure of mammalian cells had come from examinations of cells from fewer than ten species with most of it from two or three species (less than one-fourth of one percent of all mammal species). If all cells in all species were identical—*e.g.*, if a rod photoreceptor cell, or a gastric parietal cell, or anything else, were ultrastructurally the same in every mammal—then there would be no reason to worry about examining additional species. But my hypothesis was that cells were not all the same and that evolution of cell structures and maybe even the evolution of new cell types was a significant part of the mammalian radiation. If I were correct, then our whole concept of mammalian cell structure would be highly skewed if it were based solely on a few laboratory species.

I boldly, or naively, decided to go into the field and collect tissues of wild mammals that would be suitable for transmission electron microscopy. The first step was to search for a solution to the fixation problem. Early on, I telephoned Professor Don Fawcett at Harvard Medical School. Fawcett was the acclaimed author of *the* book on cell ultrastructure, and was a hero of mine because he had shown an interest in the ultrastructure of gastric cells in insectivorous bats. I asked for his advice on field fixation and, basically, he

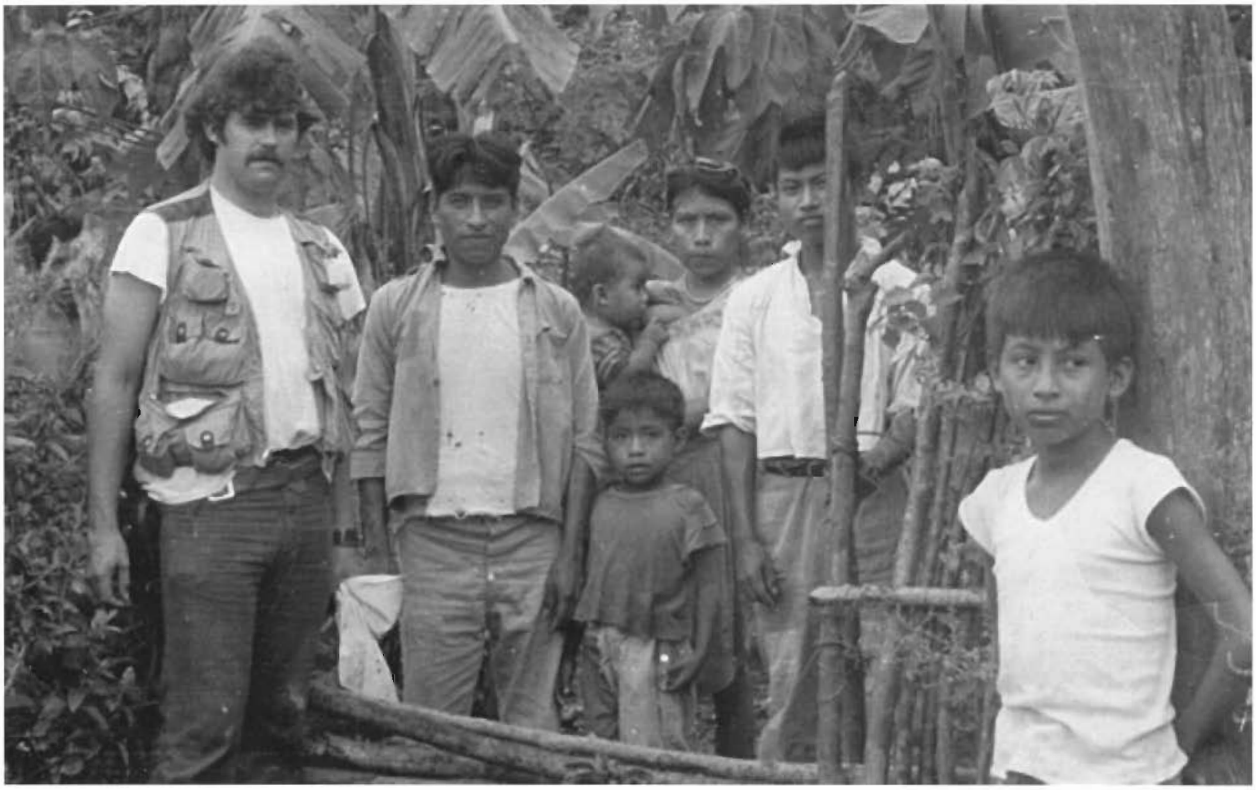


Figure 13. Where's Waldo? In 1972 I tested my new electron microscopy fixative while collecting bat specimens in Mexico. Here I am (with 70's hairdo) with an Indian family in Hidalgo. They kindly allowed me to spend the night and catch vampire bats that fed on their three cows and a mule. The nursing baby is obvious, but can you find Waldo? Hint: look in the woods behind me, to the far left.

responded, "don't bother, it can't be done." I was plenty discouraged. Fawcett went on to say that he had tried to do it, but had been dissatisfied with the results, but also added that he thought that scientifically it would be worth doing, if only it could be done with acceptable results. I stewed for a few days, but then went to México to collect specimens of bats and test out my own theories on fixation.

In summer of 1972 I tried various concoctions of osmium or glutaraldehyde in fairly weak phosphate buffers. At that time I believed that tissues fixed for TEM could not be stored for more than a few hours. I also thought that even such short storage required refrigeration at 4°C. Naturally, there was no refrigeration—no electricity to be more precise—in rural Méxican localities where I was conducting my project (Figure 13). So, on this early expedition the camp included an embedding setup and my graduate student Research Assistant (Brett Oxberry, now on the faculty at Temple University Medical School; Figure 14) and I actually did on-the-spot Epon embedding of fixed tissues! Back at Hofstra I sectioned these tiny slices of exotic tissues, examined the cells with transmission electron microscopy, and spent the academic year depressed over the poor state of their preservation. It seemed hopeless. Nevertheless, I tried again the next summer. This time, at Gary Grimes' suggestion, I just used 2% glutaraldehyde in 0.1 M PO_4 buffer and did not try to embed the tissues in the dripping wet jungle camp (water having been one of the problems the previous year). Instead, after initial fixation I stored the tissues in 3% glutaraldehyde. Some successes were achieved, and Gary and I used field-fixed tissue to write a description of the ultrastructure of bat salivary glands (Phillips et al., 1977). This literally might have been the first time that a combination of specimen collection and tissue processing in a crude field camp—

Figure 14. The 1972 expedition team: Stuart A. Marks (left), Carl Phillips (center), and the well-armed Brett Oxberry (right). Photograph taken on the road to Manzanillo, Mexico (August, 1972). A comparison to figure 8 shows cultural descent over a 44-year period.

rather than a laboratory—resulted in a description of mammal cell ultrastructure.

The success of being able to examine cell ultrastructure in the parotid and submandibular salivary glands of a fruit bat, *Artibeus (Dermanura) phaeotis*, convinced me of the importance of comparing homologous cells in mammals. For one thing, the bat cells were qualitatively different from the descriptions of cells in more commonly studied mammals such as laboratory mice and rats. Thus, homologous secretory cells were not the same in all mammals. Additionally, and more important, some of the other fixed tissues that I examined convinced me that homologous cells were not all the same among different bat species. This meant that cell structure probably evolved very rapidly and supported my thin-air hypothesis that cellular evolution might be an important aspect of adaptation. At the 1975 annual meeting of the American Society of Mammalogists, Gary Grimes and I reported on ultrastructural data from three species of bats and one of my students, Barry Steinberg, and I presented a "model for evolution of mammalian salivary glands." These two talks fell flat. Although both were well attended, no one in the room seemed to have a clue as to what I was trying to say! I had, quite obviously, gone off into a creative corner of my own making. I either needed a different audience or a better speechwriter.

Comparative salivary gland ultrastructure temporarily took a backseat to some other kinds of research. One enjoyable example of this developed quite by accident. As a recreational hobby, I had taken up the sport of sled dog racing (Figure 15). Most of these races were at venues in New England or upstate New York (in the Lake Placid or Saranac Lake region). I trained my assortment of mixed breed and Siberian husky sled dogs nearly every day on Long Island, beginning in early September when the afternoon temperatures fell below 50°F. One winter weekend, at a race in New Hampshire, I parked my truck adjacent to another racers' dog outfit. He was the current New England Champion and was racing in the unlimited class (15-20 mile races with teams of 12 or more dogs). With so many dogs to manage, he asked me to assist him. Thus began an acquaintance, collaboration, and friendship with perhaps the most knowledgeable dog man in the world—Raymond Coppinger of Montague, Massachusetts.

Coppinger, a product of Boston's Irish neighborhoods, was, and still is, a biology professor at Hampshire College in Amherst. Hampshire—a truly unique private experiment in the 'New College' tradition—undoubtedly has been the perfect residence for his academic expertise, scholarly development, teaching, and, most important, his particular personality. The first time I watched Ray race his dog team, assembled as it was from Border Collies and Alaskan village mixed breed dogs, he struck me as a slightly crazed Washington Irving sort of character. Nothing has happened since to alter my take on him. Any skeptical readers are hereby encouraged to read Ray's truly odd little book on *Fishing Dogs* (Coppinger, 1996). On his part, I suppose that Ray would counter by suggesting that readers check into my three magazine articles on the dingo, raccoon dog, and bush dog (in *Dogs* magazine, 1971). Touché. After reading *Fishing Dogs*, one might

Figure 15. Top: 1947 with my grandfather's dog Skippy. I regarded Skippy as my own "first dog." Bottom: Ray Coppinger and I raced dog teams and did research on sled dog physiology. Here is my racing team, led by the incomparable fleet-footed, honest working Border Collie *Emerson* (a.k.a. Ralph Waldo). New Hampshire 1979.

also wish to read another book by Ray (Coppinger and Coppinger, 2001).

Two biological aspects of sled dog racing made their way into winter conversations between Ray and myself. One was the issue of wet feet. As a generalization, a competitive sled dog racer would avoid purchasing (or raising) a dog that sweated excessively. Dogs have sweat glands in their toe pads and because of this there is a potential for snow and ice to accumulate between their toes. Under such a circumstance a sled dog can become temporarily crippled, and dogs prone to such a problem are worthless on competitive teams. Ray had observed that some dogs were more prone than others and when doing his culling, or selecting if you prefer, he avoided dogs that sweated when stressed physically or emotionally (as in high-strung dogs). All of this led us to wonder about wolves. How did they function in winter weather? And what about coyotes, which seemed to be a southwestern (warmer weather) canid? Ray already had hypotheses about this and we decided to test them by examining canid footpads histologically, in collaboration with Ray's student, Michael Sands. The most interesting thing that we learned was that wolves have almost no sweat glands in their toe pads, whereas coyote pads are rich with such glands (Sands et al., 1976).

Our second joint project was fascinating and fun and arose from our observation that some racing sled dogs seemed to over-heat more easily than others. We decided that an analysis of sled dog internal temperature would be an interesting physiological study. But how could we obtain our data? Measuring temperatures in working dogs was a type of field exercise that neither Ray nor I had ever anticipated. Our first attempt, in winter of 1978, involved having the dogs swallow tiny radio transmitters that monitored stomach temperature and broadcast it as telemetry. So far so good, but then we encountered our first problem. Some (but not all) dogs tended to scoop snow as they ran and when they scooped snow their stomach temperature dropped precipitously. Not surprisingly, the dogs that scooped snow tended to be the same ones that we thought were inclined to overheat.

In one of our early experiments, we decided to monitor temperatures in five dogs while they participated in an actual race, in Vermont. This experiment was enlightening, but also disastrous. Under the excitement of race conditions several of the dogs passed their transmitters while still on the race trail, and another voided hers early the next morning back at Ray's kennel. That was the end of the transmitters.

In 1979, one of Ray's students, David Schimel, designed and built an electronic rig that could record rectal temperatures as the dogs worked. This was the solution we needed, and as a result we were able to obtain accurate data. Our resulting paper, which showed the relationship of body size to heat production and cooling and which explained why the most competitive racing sled dogs weigh between 35 and 55 pounds (Phillips et al., 1981), attracted considerable attention from the sports physiology crowd. Our favorite reprint requests came from people at the East German Sports Ministry, the same outfit that was busily doping their human athletes.

In 1977 I was doing some library work when I happened to read an article on tissue fixation for electron microscopy written by Robert Kalt and Bernard Tandler. I had previously overlooked it because the title said that it was a study of fixation of amphibian embryos but then I noticed that Tandler had successfully used this same fixative in his laboratory-based study of the ultrastructure of hedgehog salivary glands (Tandler and McCallum, 1974). There was no exact reason for thinking that this very complex fixative would work in the field, although I was impressed by the overall quality of the results obtained by scientists at Case Western Reserve (Tandler and Kalt) and the University of Michigan (McCallum). At about the same time I had already decided that osmolarity was one of the major issues in storing fixed tissues under field conditions. So, working with one of my graduate students, Nadine Sabbatino, we created a version of the Kalt and Tandler fixative with a very dilute cacodylate buffer. Our laboratory tests suggested to us that our version of the Kalt and Tandler fixative would (a) work in the field, and (b) allow us to store tissues in buffer (without fixative) at tropical ambient temperatures.

The first real test of my new version of Tandler's fixative was conducted during the 1979 Alcoa Foundation expedition to Suriname in conjunction with my old Graduate School friend, Hugh H. Genoways (then at the Carnegie Museum of Natural History in Pittsburgh and Principal Investigator on the program). I had developed an elaborate protocol for preparing fresh fixative each day (using a small back-packing kerosene-fueled stove). In this multi-step process I measured out 3% glutaraldehyde, heated water with sodium hydroxide to dissolve paraformaldehyde powder and create fresh 2% formalin, cautiously added 1% acrolein (without a fume hood I stayed upwind), squeezed in 2.5% dimethyl sulfoxide (known more widely as DMSO) and diluted the mixture with calcium chloride, cacodylate buffer, and sucrose. Perhaps the scariest part of the operation involved carrying a container of acrolein (liquid tear gas) and I still remember flying in a light plane over trackless jungle thinking about what would happen if that container leaked, or broke open, while we were en route to our remote camping site. The pilot, oblivious of my concern, landed on a savannah about 300 km south of Parimaribo and truly in the middle of nowhere.

In camp, I obtained and fixed for electron microscopy tissue specimens from the retina, stomach, and salivary glands of 19 genera of Neotropical bats (a total of 103 individual animals), 2 genera of South American rodents (12 specimens), and 1 genus of marsupial (1 specimen). Intuitively I knew that if the field protocol finally worked, I would have a treasure trove of new data and, more importantly, I could continue to test my hypotheses about cell structure. Two years later, we returned to Suriname and I carried my fixative to the top of Tafelburg and successfully processed another substantial assortment of Neotropical species.

In hindsight, I now know that the 1979 and 1981 Suriname expeditions were historical landmarks in terms of the tissues and cell biology data that were obtained. Unquestionably, it was the most comprehensive field approach to comparative research on cell structure ever undertaken successfully. In some recent review articles, data from nearly 300 species of mammals have been presented and used to test hypotheses about evolution and dietary adaptation (Tandler et al., 1998a, 2001). As of 2002, data from the 1979 and 1981 expeditions have appeared in at least 40 scientific publications and probably another 70 invited lectures, seminars, and meeting presentations in the United States, Italy, and Japan. For historical purposes, and simple fairness, it is worth adding that this collection would not have occurred without the cooperation of Hugh Genoways, who emerged as one of my all-time favorite field companions.

The original, 1979, data set was far too extensive for me to study alone. Given the fact that my field fixative was closely derived from the one developed by Bernard Tandler and his student, and the fact that Tandler was undoubtedly a world-class expert on salivary gland structure, it was natural to forge a collaboration with him. This partnership has continued unabated, and resulted in numerous publications and presentations.

Berny Tandler was born and raised in New York City (Brooklyn to be more exact about it). He would be the first to say that he never considered doing fieldwork and, moreover, did not intend to add fieldwork to his resume after meeting me. At the same time, however, unlike many laboratory cell biologists who think that laboratory mice represent 'the rodent' and that any bat is 'the bat,' and that all specimens of interest can be purchased from Charles River in Boston, Tandler started out with a keen interest in zoology. He also understood diversity. This helped. Another thing was that he had grown up during the infancy of transmission electron microscopy and had a vast storehouse of knowledge about how to get the most out of the machines.

Finally—and I trust this will not unduly inflate his ego—Tandler is one of the brightest fellows in the business, and probably the best writer (E. Raymond Hall notwithstanding) too. Both of these traits are valuable, and I have borrowed his skills as often as possible.

Tandler's general interest in zoology and his appreciation of biodiversity came in part from his undergraduate education and in part from his innate keen interest in the world around him. His credentials in this regard are impeccable. I was present when he was awarded an Honorary Doctorate—'*Dootre Honoris Causa de Odontoiatria e Protesi Dentaria*' –by the University of Cagliari in Italy. Following the formal ceremony, Tandler gave a lecture on our joint research. The topic was the comparative ultrastructure of salivary glands in seven species of fruit bats of the genus *Artibeus*. Neotropical phyllostomid bats, pure systematic mammalogy as a framework for making interspecific comparisons, and description of basic field exploration, collection, and tissue fixation in jungle camps in Suriname, Trinidad, and French Guiana, all seemed right at home in the Italian baroque setting (Tandler et al., 1997; Figure 16). I had selected a seat in the front row, eager to be close to the action. The ambience brought to mind what it must have been like when the early explorers, such as Columbus and Cortez, returned to court and regaled royalty with amazing tales of the New World.

Tandler's boyhood and formal training were completely different from mine. His was urban (and urbane), whereas mine was rural (and rough-edged). His focused indoors, whereas mine was rooted out-of-doors. While he rode the subway, I slept on the ground. My professors, Rollin H. Baker, E. Raymond Hall, and J. Knox Jones, Jr., were explorers driven to understand the biology and history of mammals in their native habitats. Tandler's professors, by way of contrast, were totally focused on the tiny world within cells. More specifically, when he was an undergraduate student at Brooklyn College in the 1950's, a professor named Leonard Worley took him into his laboratory. Worley was a histologist, and an intracellular object called the 'Golgi Apparatus' held his fascination. Worley's objective was to determine whether or not the Golgi Apparatus was real or was an artifact of tissue preparation (Worley incorrectly chose the latter). This illustrates how after a hundred years of cytological research (1850-1950), the capabilities of optical microscopes still limited what was known about cell structure. Even when I took the introductory zoology course at Michigan State University, in fall of 1960, we were told that the Golgi Apparatus might or might not be 'real' and might or might not have some important 'organizing' function in cells. I still recall that with standard histological staining, the Golgi Apparatus looked like an empty (unstained) space observable by moving through the focal planes.

By sheer coincidence, when Tandler moved on to graduate school at Columbia University he undertook Master's research with Arthur Pollister, another expert on the Golgi. In contrast to Worley, Pollister correctly concluded that the Golgi was both real and functional. Pollister was a talented microscopist whose phase-contrast observations of living cells revealed the

Figure 16. Bernard Tandler standing before the University of Cagliari Senate (Sardinia, Italy). He has just received an Honorary Doctorate and is about to present a lecture on our research on the interspecific patterns of salivary gland cell ultrastructure in Neotropical fruit bats of the genus *Artibeus*!

3-dimensional structure of the Golgi. The final formal phase of Tandler's education was in the laboratory of A. J. Dalton, where the first transmission electron microscopic analyses of Golgi complexes were made.

The foregoing synopsis of Berny Tandler's educational path emphasizes how sharply it differed from my own. But there also were parallels. Indeed, whereas I drew intellectual inspiration from Charles Darwin's specimens of Patagonian mice (*Phyllotis*) in the British Museum, Tandler drew similar emotional energy from examining Professor Golgi's original silver-stained preparations (dating from 1898) through Golgi's own microscope in the museum at the University of Pavia in Italy. Tandler's sense of connectivity to Professor Golgi was further strengthened by a discovery regarding his collaborator, Alessandro Riva. Alessandro, or simply Sandro as we refer to him, is a medical professor at the University of Cagliari (Sardinia) and a prominent figure in European anatomy circles. Tandler had investigated cell structure with Sandro for several years before he discovered that Sandro's father had actually been one of Professor Gogli's students!

Bernard Tandler's scientific focus, from undergraduate days onward, was on an intracellular structure that turned out to be the organelle that sorts, modifies, and packages gene products destined for secretion by the cell. We can attribute this to fate, luck, coincidence, or predestination, but the fact is that our joint work hinges around Golgi complexes because Tandler and I ultimately address the role(s) of secretory products in the adaptation and evolution of mammals (Phillips and Tandler, 1987; Phillips, 1996). We have, by the way, also discovered and described a unique type of Golgi packaging in the salivary glands of bats of the genus *Miniopterus* (Tandler et al., 1994).

My collaborations with Berny Tandler demonstrate the fundamental rewards and the practical scientific value of interdisciplinary research. The establishment of workable interdisciplinary research teams is one of the biggest challenges in modern biology. Obviously, therefore, successful interdisciplinary collaborations are among the most satisfying experiences.

Biological academic 'disciplines' as we know them were first invented in the late 1800's. They developed through the first half of the 20th Century and each one of them seemed destined to have its own practitioners, its own code of professional conduct, its own set of important theories and hypotheses, its own priorities, its own university and college courses, and so forth. In short, each discipline developed its own paradigm. In one sense, this parceling of biology into multiple paradigms created colorful and productive outcomes—mammalogy being just one example. At the same time, however, it created the basis for rivalries and, ultimately, academic warfare that ranged from the silliest imaginable disputes and exclusionary curricula to serious power struggles over state and federal financial resources for science. For readers unfamiliar with the silly side of interdisciplinary warfare, I recommend David L. Hull's book, *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science*, which details the religious-like fanaticism that overtook my fellow University of Kansas graduate student, James Dale Smith (Fig. 7). Smith eventually took early retirement from the University of California at Fullerton, and as I recall became a big-game trophy hunter with mounted heads of African buffalo shipped home to prove it.

My association with the affable Berny Tandler—who incidentally can remember every off-color joke he ever heard and can make up his own too—created a painless and synergistic solution to the traditional barriers to interdisciplinary research. Together, we have been funded by the NIH and have managed to write more than fifty scientific articles. But more important than simple productivity, it is safe to say that we both have profited intellectually from the venture. Moreover, by collaborating we developed a network of associates. A Japanese scientist, Toshikazu Nagato (Fukuoka Dental College) has been our principal collaborator (Fig. 17). Our first collaborative paper with him described our discovery of a new organelle, which is a unique type of smooth endoplasmic reticulum (Nagato et al., 1984). Berny and I also have worked with Kuniaki Toyoshima (Kyushu Dental College), Carlin Pinkstaff (University of West Virginia Medical School), and Ed Gresik (City University of New York Medical School). All three of these scientists bring different perspectives and knowledge to our program.

Figure 17. The salivary gland team: Bernard Tandler (left), Carl Phillips, and Toshikazu Nagato (right) enjoying a restaurant in Okazaki, Japan, where all three of us were invited speakers at the Japanese National Institutes of Science (October, 2001). By last count, the three of us had published 22 scientific articles together, starting with the discovery of a new cellular organelle from bats collected in a remote area of Suriname (Nagato et al., 1984).

The establishment of the collaboration with Tandler was one of the highlights of my years at Hofstra University. But it was not the only one. Hofstra provided me with continuous opportunities to engage undergraduate and Master's students. The general quality of our students was exceptional, and I discovered real talent in nearly every course that I taught during my tenure there. For example, one day back in 1972, as I concluded my lecture in a Mammalogy course, a student rushed excitedly up to my lectern and blurted, "I've decided I want to become a mammalogist!" Suddenly, I was in Rollin Baker's shoes and I know that my memory of my own undergraduate experience—scarcely ten years earlier—immediately came to mind. At the same time, the fellow standing before me seemed like an improbable candidate: he was decidedly a product of New York City sidewalks. My rejoinder was something along the lines of, "have you *ever* been off cement?" To be honest, I was incredulous. In my narrow view, field mammalogy was a private club, with membership limited to hunting, trapping, sleeping-on-the-ground-in-the-mud types like me, for example. This student, whose academic record was impeccable, was an apartment dweller named Ira Greenbaum.

Ira Greenbaum turned out to be an exceptional student of mammalian biology and, much to my surprise, an absolute natural at doing fieldwork. I suggested an undergraduate research project and within a few weeks he was deeply immersed in a comparative histological study of tongues in long-nosed nectarivorous bats. Ira was self-motivated, and drafted his first scientific manuscript largely on his own (Greenbaum and Phillips, 1974). When Ira graduated from Hofstra University I sent him off to Texas Tech University to do graduate work with Robert Baker, who promised to keep the fellow supplied with New York bagels and send him into the field as quickly as practical. Greenbaum, of course, currently is a full professor at Texas A&M University, and a highly successful scientist too. In full regalia (boots and so forth) and with the outline of a tobacco tin visible through his jeans, this man from Brooklyn now passes as a cowboy.

Other talented Hofstra students showed similar abilities and like Ira have become great successes in their respective endeavors. A couple of examples will illustrate this point. An undergraduate student from Queens named Barry Steinberg—who supported himself by running a martial arts school in the nearby community—came by to (a) complain about the grade (a 'C') I had given him in Mammalogy and also to (b) express an interest in doing research on mammals. The sequence might have been the reverse of this, or perhaps it was all mingled into a single rambling conversation, but in the end it led to a scanning electron microscopic study of the teeth in vampire bats (Phillips and Steinberg, 1976). This was followed by Barry's participation in an analysis of age determination through dental histology (Phillips et al., 1982). Barry graduated and stayed on with me to earn his Master's degree, still occasionally complaining about his grade in my Mammalogy course. But what happened next made my hair stand on end. Barry departed for New York University Dental School, where he earned a Ph.D. in Anatomy with Ormond Mitchell (I served on his committee). A few weeks later, he telephoned to complain about his grade in my Mammalogy course and tell me he was pursuing a degree in Dentistry too. Good enough, I said, figuring that he could repay my kindness by filling any cavities for free. Two years passed and Barry phoned to complain about the grade and to also say that he was doing an exclusive residency in oral and maxillofacial surgery at the University of Virginia in Richmond. The next thing I knew, he was on the faculty at Ohio State University, and, of course, still complaining about the Mammalogy grade. When Barry earned his MD, and took a position at the University of Florida, he phoned from the operating room—a break in the action of rebuilding an accident victim's face—to once again complain about his grade. I nearly gave in. Barry probably is the most-degreed product of a mammalogy program: Barry Steinberg, BS, MS, Ph.D., DDS, MD. Well done!

John Morrissey, currently an Associate Professor of Biology at Hofstra University, is another example of successful undergraduates from there. John, who grew up on a chicken farm in Maine, came to my attention in my Comparative Anatomy course. He was an exceptional student both in terms of his academic skills and in terms of his maturity and open, friendly personality. After he completed the course, I hired him to work as an undergraduate laboratory Teaching Assistant and virtually let him teach dissection on his own. His fellow students, especially the females, obviously enjoyed his tutelage and gained demonstrable knowledge about comparative anatomy.

John was an example of a man with a mission. He knew exactly what he wanted to do. Before I even inquired, he announced to me that he intended to do research on sharks. Moreover—and this was the part that surprised me—he planned to start his research career with me as his advisor. I knew (and still know) little or nothing about sharks but because he was insistent, and bright, I gave in and accepted him as my student. As was the case with Ira Greenbaum, John Morrissey was a self-starter and completely capable of making steady progress with little more than pats on the back from me. That was a good thing, too. Eventually he appeared at my office door with his Master's thesis (on functional anatomy of shark jaws) tucked under his arm. Ultimately, John earned his Ph.D. at the University of Miami (where there truly are people who know shark biology) and it is a great source of personal pride that I was able to recruit him back to Hofstra University as a young faculty member.

By the time that he finished his doctoral work on lemon sharks, John had accumulated field stories similar to those told by mammalogists. In John's case, however, his stories involved salt water and close encounters with the serrated teeth of tiger sharks and similar species of predators. And that is how he earned the affectionate nickname "Shark-Bait." John's publications reflect his internationally recognized expertise in shark systematics and basic shark biology (*e.g.*, Sundström et al., 2001; Dunn and Morrissey, 1995), and include a book on the megamouth shark (Yano et al., 1997).

My fourth and final example is another student who took my Comparative Anatomy course and who showed up at my office doorstep several years later, after graduating from Hofstra. Unlike Greenbaum, Steinberg, and Morrissey, Allison Weiss did not know what she wanted to do and at the same time also wanted to do everything. She had nearly achieved a 4.0 average majoring in biology at Hofstra, but had really wanted to leave Long Island and attend George Washington University. She sincerely wanted to do research, and she sincerely wanted to pursue a business career in the biotech industry, and she was interested in mammals and evolution and virtually everything else, including, I believe, continuing to play on the University tennis team, studying law, making music, writing poetry, and undertaking rock and roll concert promotion. I might very well have left something out. Allison settled uncomfortably into a Master's program—inasmuch as it confined her to one path—did her own research on epithelial sensory innervation, and also collaborated with Tandler and myself (Phillips et al., 1998). The path that she eventually chose took her in the direction of molecular genetics, and a Ph.D. at Albert Einstein College of Medicine in New York. It was a good choice and as is the case with the other students mentioned, she also has been tremendously successful. A biotechnology company (SomaLogic) currently employs her, and has corralled her notable expertise in genomics, gene sleuthing, and the molecular genetics of myosin in cardiac muscle (Weiss et al., 1999a,b, 1993; Weiss and Leinwand, 1996). Obviously, research in this arena has significant health implications and her publications appear in prominent scientific journals.

These four examples of Hofstra students illustrate the potential diversity in science training, if one is willing to entertain broad rather than strictly disciplinary interests. Personally, I like the idea that the long tradition of field mammalogy—an outdoor organismal brand of science—can serve as the foundation for scientists destined for remarkably individualized futures. Some of my colleagues whose careers have focused exclusively on mammals or other groups of organisms have tended to agonize or perhaps even lament the changing emphases in biological science. I appreciate this, and worry about it too, but the real traditions and work ethic of field mammalogy can be kept alive and shared with any young biologists, regardless of their professional destinations.

I have often said that working at a medium-sized liberal arts university forced me to look outward, and consider biology far more broadly than I ever anticipated. It might also be true that this was in my nature anyway. Whatever the case, my entry into molecular genetics grew from this environment. In the late 1970's I chaired a faculty search committee that interviewed three or four candidates for a position in cell biology at Hofstra. One of these was a woman named Dorothy E. Pumo, who had graduated from the University of Michigan. Unfortunately for Dorothy, I hated the University of Michigan. This was not my fault. First off, I had gone to Michigan State University and the Ann Arbor crowd treated us like second-class citizens. Why, even the mammalogists at Ann Arbor—the hugely talented Bill Burt, the gentlemanly Emmett Hooper, the inestimable Lee R. Dice, and the incomparable mammalian paleontologist Claude Hibbard, all seemed to have a leg up on those of us in East Lansing. Moreover, my own father had trained me to believe that there might be something sinister about the way the Wolverines played football. I never would have hired Dorothy, on account of all this, expect for the fact that the University of Michigan actually is a great place, and she also had done post-docs at the University of Colorado and the University of Vermont.

At Hofstra, Dorothy Pumo developed her own research program with papiloma virus, but after she settled in on our faculty she also expressed interest in collaboration with me, and Hugh H. Genoways, on mammal genetics. Accordingly, in the early 1980's the three of us explored ways to investigate Antillean island bat populations. Both Hugh and I had done extensive collecting in the Caribbean, and I had even done some research there with J. Knox Jones dating back to the 1960's (Jones and Phillips, 1970, 1976). It seemed obvious to us that restriction site mapping of mitochondrial DNA would enable us to test hypotheses about the relationships among bats on different islands and, perhaps, even determine the most likely mainland source of the bats populating the Caribbean islands. There were two obvious challenges: first, we needed to find a way to collect specimens and preserve tissue samples so that they would be suitable for later laboratory analysis and second, we needed to learn the laboratory techniques that were just being developed and written about at that time. Simple enough.

At the time, it was thought that tissues would have to either be fresh or, at least, quickly frozen in liquid nitrogen. So, we traveled by air throughout the Caribbean carting along our canister of nitrogen and, periodically, one of us would catch a plane to Barbados or Jamaica to refill the canister. The folks at LIAT, the principal Caribbean airline, knew us well enough to stop wondering aloud about our peculiar travel habits. The field side of it sometimes worked, and sometimes failed. Back home, in the laboratory at Hofstra, we also encountered both success and technical failures. Rodney Honeycutt (now at Texas A&M University) claimed expertise on cesium chloride gradients, which in those days was the favored way to separate the mitochondrial from the nuclear DNA, so we invited him in for a seminar—and insisted that he train us too. Honeycutt got discouraged after a few days in our bondage, and claimed that it would work better if he could bring water from his own laboratory. Wes Brown, a gentlemanly scientist at the University of Michigan, was next on our list. Wes kindly spent several days with us, doing his best to illuminate the technology. We also dragged Bill Kilpatrick down to New York from the University of Vermont. Bill had made the mistake of claiming in our presence that his laboratory could do the job!

Although it sometimes was frustrating, and more than one of our students wearied, the whole process of joining new laboratory techniques with our field-based interest in Caribbean Island bats eventually paid off in a series of scientific papers on the zoogeography and genetics of the common fruit bat, *Artibeus jamaicensis* (Pumo et al., 1988, 1996; Phillips et al., 1989, 1991). The program successfully culminated in our publication of the entire mitochondrial genome in *Artibeus jamaicensis*, which was a first for any species of bat (Pumo et al., 1998).

My scientific career, which began with the most traditional type of field mammalogy, has focused on the nexus of fieldwork and technologies. I am scarcely unique in this regard. Robert J. Baker, James L. Patton, and Alfred L. Gardner are fellow American mammalogists who have made it practical to investigate chromosomes of field-collected mammals. Baker and Patton also have contributed to our knowledge of molecular genetics of wild species by taking tissues into the laboratory. Thomas Kunz, meanwhile, has found ways to combine basic physiological research with his field work on bats. But the question is: how do we keep this enterprise moving forward?

In 2001, Robert Baker and I attempted to push the technological envelope in another direction. We asked whether or not field mammalogists could contribute scientific information to the decision-making process in conservation biology. This opportunity arose because we had been offered private funding to explore mammalian biodiversity in geographic regions underrepresented in specimen and tissue collections. Our donor in this instance was a Dallas businessman named James E. Sowell. Jim has abiding interests in natural history and education and was eager to support projects that might contribute to Texas Tech University and to a host country as well.

In response to Jim Sowell's challenge, Robert Baker and I identified several geographic regions where the mammalian biodiversity was poorly known, or understudied. We settled on Ecuador as a high priority. Thirty years ago, at the beginnings of our respective careers, we would have organized a collecting expedition, sought formal permission from the Ecuador government, and then gone into the field to undertake our project and return home with the specimens. In this modern era, however, we employed a different strategy, which we thought might become a model for future international field mammalogy. Given our financial backing and the expertise of a team that we could assemble, we looked for ways that we could collaborate with colleagues in Ecuador, and perhaps share in either the development of our science in that country or in education of local students. To this end, we consulted with Xavier Viteri of Fundación Nature, an NGO located in Quito.

I had first met Xavier in 1994, when he came to the United States from Ecuador to pursue master's level study in the field of Conservation Biology at Illinois State University—where I served as Chairman of Biological Sciences. Because of my decades-long interest in Latin America, I had developed a Fellowship in Conservation Biology for students from that region, and Xavier was the first recipient. I cannot imagine a better candidate. This was Xavier's first experience in the United States, much less central Illinois, but he quickly adapted to the local customs and his command of American English improved remarkably in just a few months. As a graduate student, Xavier studied with Angelo Capparella, an Illinois State faculty member who had earned his own Master's degree at Texas Tech University under the guidance of Robert Baker. This is a minor example of the 'small world' phenomenon, but one worth our attention. In the course of my own career I have been repeatedly impressed by the extent to which science, and science education, is interdependent across national borders. Although there certainly are cultural differences in the conduct of science, and national differences in resources and priorities, the entire enterprise is interdependent because of the educational process. I often am reminded of the fact that the pioneering México an mammalogists (Bernardo Villa-R., Ticul Alverez, and Arturo Jimenez-G.) were trained by E. Raymond Hall at the University of Kansas back in the 1950's and early 1960's. Today, there is an impressive and highly capable cadre of homegrown Méxican mammalogists, and a thriving professional society.

With Xavier's assistance, we identified two ways that we might be of service to our colleagues in Ecuador. First, with his assistance we developed a collaborative arrangement with our fellow biologists at Pontificia Universidad Católica del Ecuador (known as PUCE), in Quito. Second, we offered the expertise of our field team to Xavier with the goal of providing him with mammal data for a Fundación Nature corridor project on the eastern slope of the Andes. Xavier, of course, was excited at the prospect of having a team of specialists provided to his project at no cost to Fundación Nature. The basic idea of connecting to an NGO such as Fundación Nature was not unique to the Ecuador project. Back in the late 1970's I had participated in field research in Suriname that involved a Dutch-based NGO known as STINASU. In that instance, Hugh Genoways (employed at the time by the Carnegie Museum of Natural History in Pittsburgh) had created a program that linked financing from the Alcoa Foundation to the Carnegie and the Carnegie to Suriname. The Carnegie-Alcoa-STINASU project was a huge success, and the fieldwork produced a mountain of new data about the mammalian fauna of northeastern South America. In terms of my own research, it resulted in numerous publications about the comparative ultrastructure of salivary glands and gastric mucosa.

Xavier's suggestion that we link our work to Pontificia Universidad Católica del Ecuador was based on his sense that their biology department was one of the best in Ecuador. Moreover, he was acquainted with several of their students, including two young men, Rene Fonseca and Juan Pablo Carrera Estupiñan, who were especially interested in mammals. With all this in mind, Robert Baker and I took a flight to Quito to meet our potential new partners.

The PUCE campus is an attractive oasis within the limits of Quito—a mountain town of narrow streets, ups and downs, and chilly nights that belie its equatorial location. At PUCE, an experienced scientist named Laura Arcos Terán leads the biology department. Her challenges are numerous, but typical of university science departments in Latin American countries. Given the limitations of local financial resources, how can her department deliver what one might call the "modern biology" of molecules and cells? Should students be encouraged to primarily focus on regional ecological issues (which are less expensive to study), or should they be prepared for future work in molecular biology, which is relevant to the medical needs of Ecuadorian society? Neither Robert Baker nor I had any simple answers (indeed, there are none), but with the help of Laura Arcos and one of her faculty members, Luis Coloma, we drafted an agreement of cooperation between our team, PUCE, and their Museo de Zoología. In my mind, the three major points were: 1) Texas

Tech University agreed to fully fund the graduate education (Ph.D. or M.S. as desired by PUCE) of two PUCE students; 2) Texas Tech University promised to help develop the mammal biology research library through donations of books, scientific articles, reprints, and a five-year subscription to the *Journal of Mammalogy*; and 3) Texas Tech University agreed to supply PUCE with electronic databases from the Ecuador project and new computer hardware and software commensurate with analytical needs in molecular systematics. Our colleagues at PUCE, in return, assisted us with our fieldwork; the Museo de Zoología shared specimens that represent the heritage of Ecuador and her natural resources and also provided practical logistical assistance and workspace as needed.

It is clear, at least in my mind, that the formal agreement between our team and the biologists at PUCE represents a form of international cooperation that will be essential to future fieldwork in mammalogy. Looking backwards—forty years to be precise—we have come a long ways from the Ken Ward in the Jungle approach that first attracted me to field mammalogy. American scientists have an obligation to share and help develop and sustain the educational and research efforts of our foreign colleagues, especially our neighbors whose economies and traditions do not always promote higher education.

Xavier Viteri's project involved the question of whether or not to promote creation of a "corridor" that would connect two Ecuadorian national parks, Sangay and Llangantes. Corridors are an interesting conservation concept: worldwide, native habitats are diminishing, either on account of humans with their fire setting, axes, and chainsaws or on account of natural factors. This habitat shrinkage or disruption can take several forms, but very often results in isolated individual "islands" of habitat. The question is: are small isolated habitat islands suitable for sustaining naturally occurring species of plants and animals? Presumably, most mammal species would be forced into inbreeding if trapped in small habitat islands, and really unable to disperse from one place to another. One proposed solution is to create or protect existing "corridors" of habitat that interconnect larger areas of habitat, and thus eliminate the isolating effect of island-like patches. In Xavier's project, Sangay and Llangantes parks are separated by about 40 kilometers of largely agricultural landscape. Moreover, a steep river valley, and the Río Pastaza bisect the mountainous zone between the parks. So, our question was: if a species of small mammal occurs in both parks, are the two populations essentially a single genetic group or are they already two different genetic groups separated by the Río Pastaza rather than by discontinuous habitat? If a mammal species occurring in both parks already was divided into two natural groups, then the establishment of a habitat corridor between the parks would be less important to their long-term survival. This project was intriguing because the typical corridor project does not involve genetic analyses, relying instead on intuition and a belief that habitat connectivity is always a good thing.

Our Ecuador team was large; a total of 11 people and all of their technical equipment and personal gear had to be organized and transported by air to Quito, and then moved on the ground in four-wheel drive vehicles leased locally. To assist us with experience and leadership, we invited Clyde Jones to be part of the project as it developed. Additionally, we decided to expand our personal horizons by hiring two outsiders—a journalist and a photographer—to be part of the program!

The idea of including a journalist and a photographer in our project was triggered by several events. Most notable among these, at least in my opinion, was the fact that a prize-winning outdoor writer, Barry Lopez had been brought into the embrace of Texas Tech University when David Schmidly served as President. Robert Baker and I had become acquainted with Barry, and this had led to conversations at various times and places that focused on linkages between natural history and the humanities. When we organized the project, Barry recommended that we include a freelance journalist named Sandy Tolan. Tolan, who lives in Gloucester, Massachusetts, is founder of Homelands Productions. His credits are numerous and include a series of radio programs broadcast by National Public Radio (NPR), feature articles in *The New York Times*, *Christian Science Monitor*, *The Village Voice,* and other leading newspapers and magazines, and a creative book entitled, *Me and Hank: A Boy and His Hero, Twenty-Five Years Later.* In his book, Sandy had used the experience of baseball legend Hank Aaron to explain and trace American race relations. Although Sandy had traveled extensively and had written about indigenous peoples and the social, political, and economic impact of the modern world on their cultures, he had not previously accompanied biological scientists.

As team photographer we selected Lynda Richardson of Richmond, Virginia, whose credits also are exceptional. Lynda's images—nearly all of which portray her sense of the natural world—are widely published and her name is among the artistic elite in her profession. Her portraits also are remarkable; after thinking it over for several days, she captured me, grizzled and literally half blind from a cataract (actually overdue for a lens replacement) while I fixed mammal tissues for transmission electron microscopy (Fig. 18). Fortunately for me, given my state of half-blindness while doing the Ecuador project, Lynda Richardson is one of the strongest—actually I would say most muscular—women in the world. She cheerfully attributes this to some combination of dragging camera equipment into the field, and to intense participation in whitewater kayaking. Whatever the explanation, she was able to drag me to my feet after a sightless nocturnal crash landing into the muddy floor of a patch of Ecuadorian jungle. I appreciated her strength, and my being rescued from the mud, but as I write this I can still hear her laughter.

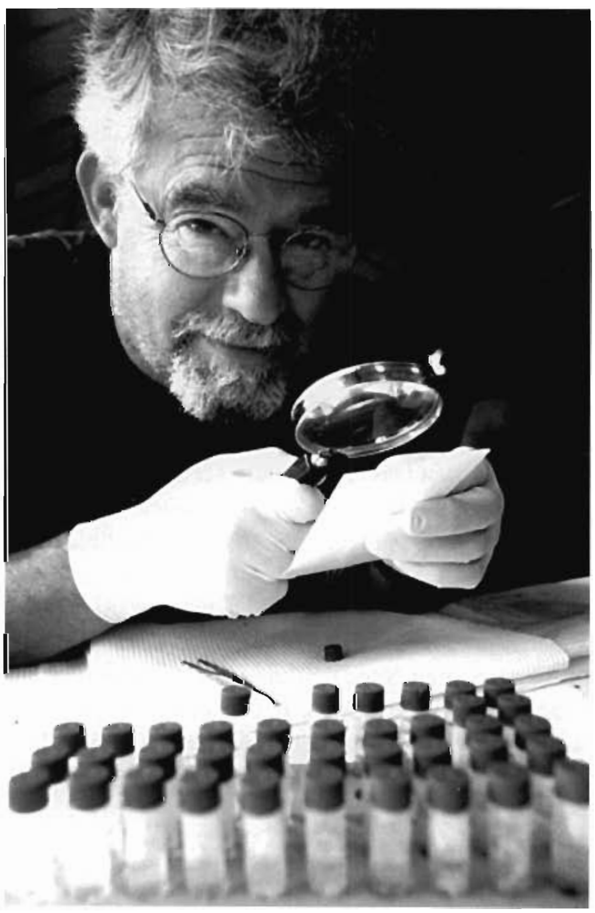


Figure 18. Here, Phillips is fixing tissue for electron microscopy while on the Texas Tech University-Sowell Expedition to Ecuador (August, 2001). Photograph by Lynda Richardson.

Overall, the Ecuador project represents the modern—and probably future—version of field mammalogy. Fieldwork is expensive, especially if it is conducted in a remote or rural area of a foreign country. The overall cost of a program—airfares, lodging, food, ground transportation, medical support (*e.g.*, inoculations, malaria prophylactics), and salaries—is very high. This alone dictates the importance of maximizing potential scientific return. Moreover, there is the matter of specimens. In our modern world we are obliged to explain why we must collect specimens of wild mammals in order to learn about them. There are several audiences that must be satisfied in the United States. One is pretty extreme, and unable to grasp the basic concept that more young are born than can survive. In the eyes of these people, animal life on the planet would prosper if all human beings were vegetarians. Educated people understand, of course, that agricultural practices required for growing domesticated plants are the most detrimental of all human activities when it comes to conserving animal life. But even amongst our fellow scientists—the ecologists and other believers in the observe, catch, and release version of science—there is the growing notion that animal species can be identified at a glance so there is no need to collect them. In reality, however, this cannot be done in most places, especially in understudied geographic regions. In order for our fieldwork to have a scientific basis, it is necessary to have museum voucher specimens that represent the physical source of data. In the end, the point is that the lives of our fellow mammals are valuable and our ethical duty is to maximize the scientific return from each specimen that we take. To ensure that this is the case, the modern, and future, field projects will consist of multidisciplinary teams whose goals sometimes conflict but nevertheless result in maximum data output.

In Ecuador we essentially had three overlapping projects. My own focus was on the collection and fixation of salivary gland tissues that could be used in my program. But while I fixed tissues from the right parotid and submandibular glands for the cell structure research, I also froze the contra-lateral glands for research on gene expression and secretory peptides and proteins. From many of these same specimens, Robert Baker and his graduate student Deidre Parish prepared karyotypes. These chromosome preparations will be used in basic systematics and also in highly sophisticated microscopic research involving *in-situ* hybridization techniques. Clyde Jones, our other co-principal investigator, focused his efforts on the basic collection of rodents, and with a bevy of graduate students ensured that organs were frozen in liquid nitrogen for molecular genetic research and that museum quality voucher specimens were prepared.

In my personal view, I believe that scientists in our discipline must continue to seek ways to unite basic fieldwork with both the emergent technological and educational thrusts (cell and molecular biology, genetics, and genomics, at least for now) and with practical needs and political decision-making (as exemplified by Xavier's corridor project). Our historical roots—which I experienced first-hand at Michigan State University back in 1961—are in geographic exploration and collecting and preparing mammal specimens suitable for museum-based taxonomic research. Our future lies in our ability to use our unique skills and interests in the discovery and integration of new knowledge into the core of biological science.

POSTSCRIPT

In October 2002, Robert Baker, Ron Chesser, and I walked the shoreline of Glyboki Lake, about a kilometer north of Chornobyl Reactor IV in Ukraine. It was the first reasonably warm day of that week, perhaps 50°, and it was a welcome change from the near-freezing temperatures, northerly wind, and low blanket of clouds that had shrouded the place for days. The Soviet-era central heating system in Slavutych, the small city where we were housed, was not in operation. The absence of a heated room in the only local hotel was the one consequence of importance to us. We had been sleeping in our clothes, living in what amounted to walk-in chillers.

On this day, a Thursday, sunshine finally peeked through steely clouds and a soft breeze stirred from the south, southeast. It was almost an Indian summer day. Every twenty paces or so we paused on the shoreline and Robert Baker placed a meter stick on the ground, held it vertical, and measured the radioactivity. Chesser recorded the reading and I placed a numbered orange flag in the ground to mark the spot. Trailing behind us, Alexi Ryabushkin paused at each flag and used a global positioning instrument (GPS) to determine the exact location. Ultimately, all of these data would be used by Chesser to mathematically recreate the radioactive plume that crossed the lake as Reactor IV melted down. By accurately modeling and geographically mapping the plume, we then could overlay that information with data on the distribution, molecular genetic, and physiological features of the small mammals living along the shoreline.

Today's work was part of nearly a decade of mammalogical research conducted in the shadow of Reactor IV, the world's first and hopefully only catastrophic accident at a nuclear power plant. Working together, Baker and Chesser, their students, and Ukrainian colleagues had produced a series of important scientific articles about the biological effects of exposure to residual radiation in the area around Reactor IV. I joined their team in 1999, and had made some small contributions to various projects (e.g., Rodgers et al., 2001; Chesser et al., 2001).

As we walked at the edge of Glyboki Lake, we observed a place where otters had deposited shells (probably rich in ^{90}strontium) from fresh water clams and mussels; we noted that the soft swampy earth was pockmarked with moose footprints and droppings; we paused where beavers were busily cutting oaks—some quite large—in four different places and wondered what the extinct giant Pleistocene beavers (which must have weighed hundreds of pounds) were like. The hike, the observations, the note taking, and the weather at Glyboki all reminded me of an October day in Michigan, forty years earlier. It was fall of 1962 and I was enrolled in Rollin Baker's Mammalogy course at Michigan State University. One of our class field assignments was to map the tiny runways left by meadow voles living in a grassy fallow field near the campus. I spent a gray blustery morning and sunny afternoon on my hands and knees, measuring and plotting the runaways, noting the locations of nests and piles of grass cuttings, and using a lab thermometer to obtain temperatures for microhabitat analyses.

When Robert and I completed our survey work along Glyboki Lake, we sat under an oak, its golden leaves shimmering in bright shafts of afternoon sunlight. Wind ruffled the steely blue surface of the Lake, creating cat's paws, and a small flock of ducks, roused no doubt by us, winged toward the northeast. We smoked Cuban cigars purchased the previous weekend, in Amsterdam. Being a diabetic, and always vigilant about his status, Baker used the break to prick a finger and measure his blood sugar. Afterwards, he kicked about, searching for acorns that he could plant on his ranch back in west Texas. I sat nearby, resting my sore vertebral column against a fallen limb, and pondered the privilege of being a field mammalogist. What better way is there to pass through the transient, fleeting, existence that is a single lifetime?

"This would be a beautiful place to camp, Robert," I said, imagining a shelter half tent, a blazing campfire, and some warm blankets. Just then, the electronic dosimeter in my pants pocket squawked to life. What would Ken Ward have done about that?

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WHEN PEOPLE ASK WHAT I DO, I SAY I STUDY BATS AND RATS

Jerry R. Choate

Jerry R. Choate was born in Bartlesville, Oklahoma, on 21 March 1943. He received a B.A. degree in Biology from Kansas State College of Pittsburg in 1965 and a Ph.D. degree in Zoology from the University of Kansas in 1969. He is Director and Curator of Mammals at the Sternberg Museum of Natural History and Professor of Biological Sciences at Fort Hays State University.

When I received the initial letter from Carl Phillips and Clyde Jones proposing this symposium, I was prompted to sit down with a glass of Makers Mark and think about the various persons and events that influenced my selection of a career and contributed to whatever successes I have had. The more I thought about it, the more it became apparent that no one person was exclusively influential and that many of the most important events in my life were, for lack of a better description, dumb luck. In short, I have stumbled through life making one mistake after another but learning from my mentors and colleagues and benefiting from chance. With these observations in mind, I offer the following encapsulation of how I became a mammalogist and how the decisions I made along the way influenced my career. Also, I voice brief opinions on how mammalogy has changed during my career and what the future might hold for our discipline.

I was born (on 21 March 1943) and raised in Bartlesville, Oklahoma. My dad had died in World War II, and my mother remarried when I was a child. Mother was a stay-at-home mom, and my step dad (who had never completed high school) worked hard as a draftsman for Phillips Petroleum Company. Dad's family had lost their farm during the Great Depression, and he therefore did not believe in buying anything on credit. It took him until I was nearly through grade school to save enough money to buy our first car, and we were among the last people in town (I was in junior high school) to buy an air conditioner and a television set.

I began reading before most kids my age, and I was an avid reader throughout my early years (I think I read all the Hardy Boys mysteries). As an only child in a family with a modest income, I became extremely independent and was earning money by mowing lawns and hawking newspapers by the time I was ten years old. I therefore was able to save enough money to buy a bicycle, a variety of baseball equipment, and some fishing gear. Even before I was 12 years old, I would ride my bicycle several miles to the Little League field and much farther to fishing holes. In high school, I began hanging out with two full-blood Cherokees who were in my class. Both were avid outdoorsmen, and they influenced me to develop a sincere love for hiking, camping, and nature in general.

High school was wasted on me. I had only one teacher who really got my attention—an English grammar teacher named Mr. Klewer. He was tough as nails (he worked as a construction laborer during summer), and he literally forced me to learn good sentence construction and the basic rules of grammar. At the time, I had no idea how important his class would be to me later in life. It was while I was in high school that the love of my life (Fi Walker) moved into the house next door. We became inseparable, but she went off to college while I was still a senior.

When I graduated from high school, I was totally clueless as to what I wanted to do with my life. All I knew was that I wanted to go to college and party. Bartlesville, Oklahoma, was the home of Phillips Petroleum Company, and there was a distinct social order. The executives at Phillips sent their kids to the University of Oklahoma, whereas the kids of rank and file Phillips employees went to Oklahoma State University. If your parents didn't work for Phillips, you probably didn't go to college. I don't know why it was that way, but it always had been and reportedly still is. Therefore, it was predetermined that I would attend Oklahoma State University if I could afford to go at all. Fortunately, I received a stipend from the War Orphan Aid Program (administered by the Veterans Administration) to help pay college expenses. Fi transferred to Oklahoma State University so that we could be together.

I arrived at Oklahoma State University with no idea what to expect. I met with the person who was assigned to me as my advisor, and he asked me what I wanted to major in. I told him I didn't know. At that time OSU evidently did not have an undeclared major, so he put me down as a psychology major. After a month or two, I switched my major to, of all things, philosophy (like I said earlier, I liked to read). Before I completed my first year of college, I was swayed by my love of nature and switched to a third major: forestry.

Young men from Bartlesville, Oklahoma, routinely pledged fraternities, so I did likewise. Looking back on it, this probably was a wise decision. I had never had to study during high school (or, at least, I never did even if I needed to). However, study was essential in college and my fraternity had required study sessions. As a result, I was able to blunder through my first semester with a low C average even though I studied no more than required by my fraternity.

Toward the end of my second semester, which I pretty well blew off, one of those strange events that influenced my life happened. One hot spring night, there was a massive panty raid on one of the girl's dorms. Thousands of young men took part, and I was in the crowd. I had a lot to drink that night and could barely walk, let alone run, so I was among the few who were arrested. To make a long story short, I was expelled from Oklahoma State University and did not receive any credits for courses in which I was enrolled that semester. Looking back on it, it was just as well—I was flunking two or three of my courses and would have gone on academic probation anyway.

Fi and I returned home to Bartlesville, and I took a battery of tests administered by the Veterans Administration to see whether they would continue to support my feeble effort at attending college. The tests revealed that I would do best as a truck farmer, and the VA evidently decided that forestry was close enough. Therefore, the VA agreed to continue my funding, and Fi and I began looking for other schools where we might go to college. We applied to about 20 colleges, and I was accepted to only one, then known as Kansas State College of Pittsburg. This was another of those instances of dumb luck, as Pittsburg State proved to be a major turning point in my life.

Only after I arrived at Pittsburg State did I learn that I could not major in forestry there. The closest thing to forestry that Pittsburg State had was biology, so I became a biology major. The advisor assigned to me was a southern gentleman named Professor Horace Hays (Fig. 1). Horace looked at my academic record and enrolled me in a relatively light schedule. I realized that I probably could not get into any other colleges if I flunked out of Pittsburg State, so with Fi's encouragement I worked harder than I ever had before and made respectable grades. Then, in my second semester at Pittsburg State (when Professor Hays enrolled me in me a more difficult schedule), I finally figured out what college was all about and made nothing lower than an A. I discovered that I enjoyed doing

Figure 1. Professor Horace A. Hays at Pittsburg State University.

well in classes and competing for good grades with my friends.

After another semester or two of good grades, the Dean called me to his office and told me he had authorized the Registrar to delete the transcript record that I had been expelled at Oklahoma State University. I had not requested this, and I will forever have a warm spot in my heart for Pittsburg State University as a result.

Fi and I were married in our second year at Pittsburg. At about that time, I took Professor Hays' course in mammalogy. Although Horace had done his doctorate in mammalogy under the tutelage of George Lowery at Louisiana State University, he was not a research mammalogist. Nevertheless, he was an inspiring teacher and a great advisor, and his gentle prodding led me to finally figure out what I wanted to do when I grew up. I didn't know what kind of job I might get with a degree in mammalogy, but the study of mammals was what I decided I wanted to do.

Two important things happened in my senior year at Pittsburg State. First, I received an academic scholarship. Given my earlier academic record, this proved to me that I was as capable as anyone and did much to enhance my self confidence. Second, as president of the local chapter of the biological honor society, I invited Dr. Bryan P. Glass to come to Pittsburg State from Oklahoma State University and give a banquet speech. Fi and I took Bryan out for lunch, and he told me all about the American Society of Mammalogists. He recommended that I get a membership and, moreover, he strongly suggested that I take the big plunge and get a Life Membership (it was only $100 at that time, in four payments of $25). This was a lot of money for Fi and me, but we took his advice.

During my senior year, I began to think about going to graduate school. I knew I could stay at Pittsburg State if I wanted to, but Professor Hays told me that, if I really wanted to be a mammalogist, I should apply to the University of Kansas and work with E. Raymond Hall. Horace's unselfish advice proved to be another of those historical events that greatly influenced my life.

Together with another student, I drove to the University of Kansas to meet with Professor Hall. However, he was gone that day and we ended up talking to a young mammalogist named Dr. J Knox Jones, Jr. After that experience, there was no doubt in my mind where I wanted to go to school. However, based on Professor Hays' advice, I still wanted to study under Professor Hall.

When I applied for graduate school, I literally did everything wrong. I only applied to one school, and I never once communicated directly with Professor Hall. No one told me about graduate assistantships, and I did not even apply for one. However, at about that time Professor Hall received a contract to conduct a biological assessment for a proposed watershed improvement project. Hall asked Jones what he thought about this Choate guy, and Jones evidently said I was worth the risk. Accordingly, I was admitted to KU with a graduate research assistantship to work on Hall's project.

Figure 2. My true love and I when I was just beginning graduate school.

Fi and I moved to Lawrence, Kansas (Fig. 2), and I began graduate studies. There, I met an amazing assemblage of graduate students: Dave Armstrong, Elmer Birney, John Bowles, Alberto Cadena, Larry Forman, Hugh Genoways, Tom Kunz, Carl Phillips, Jim Smith, and others. We all learned from one another, although I probably had more to learn than the others did.

Most of us were driven by the desire to see our names in print. At one point, Hugh Genoways, Carl Phillips, and I conducted a study together. When it came time to prepare a manuscript, I was hot to trot to get the manuscript done as well as confident in my ability to write (I had published two inconsequential short notes while still an undergraduate at Pittsburg State). Therefore, I quickly wrote the first rough draft and gave it to Carl so he could edit it. After reading it quietly, Carl said "Jerry, what say we revise this sucker so that it conforms to the standard format of Introduction, Methods and Materials, Results, and Discussion?" Carl hit the nail right on the head: I knew English grammar and sentence structure, but I did not have a clue how to put sentences and paragraphs together to make a manuscript. With Carl's input, the manuscript eventually was published (Choate et al., 1967). After that incident, I spent a lot of time looking over Carl's shoulder learning how to write better. Carl also taught me that, "If you want to be the best in the world at something, pick a specialty that no one else studies." As I look back on it, I can see that Carl followed that strategy throughout his career.

Hugh Genoways was the driving force that kept me headed in the right direction. He was totally committed to mammalogy, and his wry wit and sarcasm kept me highly motivated. He also taught me the fact that the squeaky wheel gets the grease (he would hang out around Knox Jones' office until Knox no longer could ignore him, and they would end up working on a research project together). From this I learned that, if you push hard enough, you eventually may get what you want (this proved helpful when I later tried to get university support for a new museum building). Hugh also had a rule about fieldwork: "The number of mice you catch is indirectly proportional to how hard you work when you set your traps."

Elmer Birney also had a memorable rule (previously published in his obituary by Genoways et al., 2000) that revealed him as a true master of taxonomic theory: "If you misidentify a mammal as to its genus, you surely will have a new species."

Professor E. Raymond Hall also believed in rules. For example, when you wrote something with him, you could never resort to the second or third definition of a word. I remember I once asked him to review a manuscript I was writing. He said he would call me when he was available, and he did—at 11 o'clock one evening. I drove to his home and sat down with him to read the manuscript. In the second or third sentence, I had written "This species was first encountered in Nebraska by Jones." Professor Hall leaned back, sucked noisily on his pipe (while drool dripped from the stem onto his old blue suit), and said: "So, just how startled was Jones?" I didn't know what to say, so he asked: "Was he surprised?" When I remained mute, Professor Hall pointed me in the direction of his well-worn, unabridged dictionary and told me to look up the word "encounter." When I did, I learned that to "encounter" means "to come upon something or someone unexpectedly." That was the day I learned one of the fundamental rules of scientific writing: "Say what you mean and mean what you say."

After about a year of graduate studies, I had a run-in with Professor Hall. He wanted me to enroll in traditional courses such as stratigraphy and had no interest in what I regarded as modern areas of mammalogy. I wanted to learn about numerical taxonomy, genetics, and other hot topics that were just beginning to develop in mammalogy. I asked Knox Jones if he would take me on as a student and, most importantly, if he would explain to Professor Hall that I could not work with him any longer. Knox did this for me, and I thereby began the next stage of my professional career.

I met another person during my first year as a graduate student who made a big impression on me. That person was Dr. Robert Packard. Bob had been a student under Hall at the same time as Knox Jones, and Knox and Bob were close friends. Bob had settled into a teaching position at a college down in Texas that had little going for it: no money, no reputation, no museum to speak of, and so on. The name of that little known institution was Texas Tech University. Knox frequently kidded all of us graduate students that, if we didn't shape up, he would send us to Texas Tech! Little did he know at the time that Texas Tech would become the Harvard of mammalogy and that he, Knox Jones, would spend the second half of his career working there.

Robert Packard had a way with students. He undoubtedly knew within five minutes of meeting me that I needed a lot of polish before I would amount to anything. Nevertheless, he spoke with me at length. I can't remember what he said, but I knew after that conversation that I wanted to become a good teacher and mentor to aspiring young mammalogists. It is noteworthy that, after Robert died, the Southwestern Association of Naturalists' top award for education was established as the Robert Packard Award. It is perhaps ironic that I was the first recipient of that award.

The likes of Professor Hays, Professor Hall, Hugh Genoways, Carl Phillips, Elmer Birney, and Robert Packard notwithstanding, the person who had the greatest influence on my life, and on the kind of mammalogy I eventually decided to study, was Professor J. Knox Jones, Jr. (Fig. 3). Knox may have influenced me even more than he did his other students because I was greener and more susceptible to his subtle and often sarcastic ways of teaching. I learned much of what I have used in my career from Knox, and I tried to adopt Knox's good qualities and not succumb to Knox's shortcomings. Learning to distinguish between them was, in itself, an educational experience.

Knox was one of a kind. For one thing, he was a curator who actually curated. His research may not have been at the cutting edge of science, but he probably discovered more new knowledge than any other mammalogist of his generation. He stressed accuracy in all things and could not abide mistakes. He had no respect for armchair biologists who seldom ventured into the field, and he was a firm believer that no re-

Figure 3. Hugh Genoways and I studying Mexican pocket gophers while Knox Jones looks on.

search project is ever finished until the results are published. He was bigger than life in almost every respect. How could you not learn from such a person?

Knox had been surrounded by some of the giants of mammalogy, and by young mammalogists who later would become giants, when Knox was just a student. These persons included the likes of E. Raymond Hall, Rollin Baker, Syd Anderson, Don Hoffmeister, Robert Packard, Jim Findley, Terry Vaughan, and numerous others. Those young mammalogists had a special relationship with each other, and it was only natural that Knox would have a special relationship with his students. He surrounded himself with a phenomenal group of graduate students, and he put his heart and soul into seeing that they succeeded in achieving their professional aspirations. One example of his success is the fact that several of his graduate students later served as presidents of the American Society of Mammalogists or became leaders in their field in other respects. Without a doubt, the most important thing I learned from Knox was how to work with graduate students.

Knox had little interest in modern mammalogical methods that developed when he was at the pinnacle of his career. Nevertheless, he did not object when his students wanted to learn about them, and he encouraged us to incorporate these new techniques into our research even though he personally did not understand them and could provide little guidance. Elmer Birney got it just right in his encomium to Knox (Genoways and Baker, 1996:xxxii): "Knox...encouraged each of us to pursue our own interests. He expected us to go beyond his own realm of expertise, to find the advice and guidance we needed, and to be the very best and professional at whatever we did."

The techniques that were just beginning to have an impact on mammalogy during my tenure as a graduate student were numerical taxonomy, electrophoresis, and karyology. At Kansas, we had no resources to speak of for karyological studies, and our resources for electrophoretic studies were crude at best. However, the University of Kansas was at the heart of the rising field of numerical taxonomy. Therefore, it was only natural that I would choose to become "expert" in numerical taxonomy rather than genetics. I would learn, after no more than a decade, that this was a poor choice, but by that time there was little I could do about it.

I admit that I was influenced in my selection of a research protocol by the fact that I still did not know what I wanted to do when I grew up. One of my career options was to work in a large museum where I would have lots of contact with mammals, but if I did I might have little contact with students. The career option that I began leaning toward was to find an academic position where I could study mammals while influencing the lives of students—in other words, to follow in Knox Jones' footsteps. I surmised that numerical taxonomy would be possible at most academic institutions, whereas modern genetic methods might not. As it turned out, I was correct. However, numerical taxonomy had a short life span, at least in mammalogy, and I additionally found that my expertise in biometric methods soon retreated from the cutting edge of the discipline.

As I now look back on it, numerical taxonomy was nothing more than a sophisticated method of mathematical estimation-*i.e.*, a numerical method of guessing. It was only natural that the precision of genetics would replace it as the systematic method of choice. Excuse me, did I say "the precision of genetics?" For a moment there I forgot that karyologic methods were quickly replaced by electrophoretic techniques, which were replaced by analysis of DNA sequences (both mitochondrial and nuclear), all of which seem to provide different answers to the same questions asked in slightly different ways. Likewise, phylogenetic analysis (I prefer the term cladistics) has proved to be an extremely useful technique even though, as noted by a plant taxonomist at Berkeley, "Cladistics is to systematics as paint-by-the-numbers is to art." In short, future generations of mammalogists may look back on us and laugh at how crude our scientific methods were.

When I was completing the requirements for the Ph.D., Fi was pregnant and we both wanted me to go out and find a real job. Therefore, I began looking for an academic position and did not even consider the possibility of a post-doctoral position. I eventually accepted a temporary position at the University of Connecticut, replacing Dr. Ralph Wetzel while he was on sabbatical leave, and Fi and I moved east.

The University of Connecticut was as close to an undirected post-doc as I could have found. I taught one course per semester (comparative anatomy and mammalogy) and spent the remainder of my time doing research. I even had an opportunity to teach a class in mammalogy at Yale University while supposedly on a full-time contract at Connecticut. Fi and I (and our son Judd, born three months after we arrived in Connecticut) remained in Connecticut for two years but were anxious to move back to the Great Plains.

In 1971, I accepted an offer of a position in mammalogy at Fort Hays State University. Elmer Birney was largely responsible for me even applying for it. He had done his undergraduate studies at Fort Hays State, and he raved to me about what a good university it was. A faculty member already at Fort Hays State, Dr. Eugene D. Fleharty, also encouraged me to join him there. I did, and even after 32 years I have never regretted that decision.

Fort Hays State University is a teaching institution that supports research and has a national reputation as a training ground for students who want to go on to research institutions and earn the Ph.D. degree. Therefore, the university was supportive of my research and my work with graduate students. However, I had some adjustments to make as I acclimatized to life in a small academic institution.

I had always envisioned myself working in a large institution. At Fort Hays State, I quickly became a large fish in a small pond rather than a small fish in a large pond. I had always envisioned myself traveling around the globe and conducting research in exotic places. At Fort Hays State, I discovered that it was more appropriate to become expert on the mammals of the Great Plains. I had always envisioned myself having access to a large museum. At Fort Hays State, I found a small but outstanding exhibit museum and numerous small but good departmental research collections. In no time at all, I had formulated a long-term plan to consolidate all the collections into a world-class museum. I had always envisioned myself serving as mentor to Ph.D. candidates. With no Ph.D. program at Fort Hays State, I took advantage of the opportunity to help aspiring Master degree candidates develop into someone else's Ph.D. students. I made these adjustments with little difficulty. On numerous occasions, Fi has asked me if I wasn't disappointed that I missed the opportunities I would have had at a larger university, and the answer has always been "No."

Without doubt, my two greatest accomplishments as a mammalogist have been my graduate students and my success in developing the Sternberg Museum of Natural History. Let me first discuss my students.

As of December of 2001, a total of 44 graduate students had earned the MS degree working under my supervision, and 24 of those students had then gone on to earn the Ph.D. degree in prestigious research institutions. A few of my former students who have made names for themselves as mammalogists are Mark Engstrom, Lynn Robbins, Sarah George, Nancy Moncrief, Cheri Jones, Brett Riddle, Phil Sudman, Jerry Dragoo, Jan Decher, Greg Wilson, Steve Hoofer, Dale Sparks, and Rob DeBaca. I like to think that Knox Jones would be proud of what I have done with my students.

It was largely because of my former students that I received the American Society of Mammalogists' top research award (the C. Hart Merriam Award) in 1988. Certain mammalogists at research institutions were incensed that someone at a teaching institution could win "their" award, and they saw to it that the rules by which a person could be considered for the award were changed to ensure that no one like me would ever win it again. It truly made me feel powerful to know that I threatened them so.

It took years to accomplish my goal with respect to the museum at Fort Hays State University, but I succeeded. First, I consolidated all the individual research collections in the Department of Biological Sciences as the Museum of the High Plains and managed to get the Museum of the High Plains recognized as a separate department of the university. Then, in order to qualify for grants from the Institute of Museum Services, I united the Museum of the High Plains with the other museum on campus, the Sternberg Memorial Museum, as the "Fort Hays State Museums." This was a reorganization on paper only, with both museums retaining separate names and directors. In fact, it was a delicate operation because the director of the Sternberg Memorial Museum assumed at first that I was after his job, which I was. However, I did work that he was not interested in doing—I obtained Federal grants for educational programming and the development of a temporary exhibition program. Finally, I completed the merger of the two museums as the Sternberg Museum of Natural History with me as the director (I saw to it that the director of the former Sternberg Memorial Museum was appointed Chief Curator).

However, the Sternberg Museum still occupied an inadequate exhibit facility with little room for its research collections (which therefore were scattered all over campus). Accordingly, my next task was to convince the University President that we needed a new building. Once I had him on board, he set about raising the money needed for the building (not an easy task when you live far from any metropolitan area, and no corporate headquarters are located nearby) while I oversaw the development of building plans. Although the President never really understood what museums were all about, he tried to micromanage the museum. As a result, I had several arguments with him but somehow managed not to be fired.

The Sternberg Museum of Natural History reopened in its new building (more than 101,000 square feet) on 13 March 1999. With its outstanding paleontological exhibits, room for large temporary exhibitions, and research collections totaling more than 3.5 million specimens, it truly is one of the best museums on the Great Plains and the finest museum at any small academic institution anywhere. I like to think that Knox Jones would be pleased with me for this accomplishment.

Of the books I have published, I am most proud of the volume that Elmer Birney and I edited as a Special Publication of the American Society of Mammalogists (Birney and Choate, 1994). Although few people probably have read it from cover to cover, it contains some outstanding literature on the history of mammalogy and our primary professional society. In terms of research, I am most proud of the research program dealing with systematics of the genus *Blarina* that Hugh Genoways and I developed (Choate, 1972; Genoways and Choate, 1972; Genoways et al., 1977; George et al., 1981; Moncrief et al., 1982; George et al., 1982; Jones et al., 1984; George et al., 1986; Genoways and Choate, 1998). However, I can honestly say that I found my true research niche when I carefully read E. Lendell Cockrum's *Mammals of Kansas* (Cockrum, 1952). Although I have never met Lendell, his book opened my eyes to historical biogeography (defined by me as changes in distribution during historical time, and not as described by Jim Brown and others in a paleontological sense). I subsequently wrote numerous papers (largely coauthored by my graduate students) on the historical biogeography of mammals on the Great Plains (e.g., Robbins et al., 1977; Choate and Williams, 1978; Engstrom and Choate, 1979; Sexson and Choate, 1981; Choate et al., 1982; Moulton et al., 1983; Burns et al., 1985; Zumbaugh and Choate, 1985; Svoboda et al., 1985; Choate and Reed, 1986; Riddle and Choate, 1986; Choate, 1987; Sudman et al., 1987; Dragoo et al., 1990; Wilson and Choate, 1997; Sparks and Choate, 2000). I realize that few of these studies were at the "cutting edge" of mammalogy, but they were at the "cutting edge" of my scientific interest and that is all that is important. Even today, I remain fascinated with the dynamism of mammalian distributions as I attempt to identify factors that influence those distributions or that brought about changes in the past.

If you ask 20 eminent mammalogists what the future holds for our discipline, most of those scientists doubtlessly will expound about how future generations of mammalogists will sequence the genetic code for all mammal species, and how this knowledge will provide answers to fundamental questions about mammals. However, as more and more mammalogists move into the lab and seldom venture into the field, and as they spend less and less time trying to truly understand the species that they sometimes regard merely as a good source of DNA, the need for old-fashioned field biologists actually will increase. I'm talking about traditionally trained mammalogists who don't feel the need to wear a spacesuit when they handle mammals. These will be the persons to whom geneticists will go for help in obtaining and identifying mammals, and these may be the persons who answer some of what I regard as the most interesting questions of our discipline. Let me identify a few of those questions.

The first few questions relate to dispersal. Why do species that have had relatively stable distributions for long periods of time suddenly begin expanding their distributions? I've spent much of my career trying to explain the dramatic geographic fluctuations of species such as least weasels (*e.g.*, Choate et al., 1979) gray foxes (Choate and Krause, 1976), spotted skunks (Choate et al., 1974), and numerous others, and I doubt that I yet fully understand what causes these phenomena. How do forest-edge species, such as white-footed mice and fox squirrels, disperse several kilometers from one patch of riparian woodland to another that is too distant to see across totally unsuitable habitats? Dogma says that they disperse in all directions and that some, by chance alone, find the next patch of woodland. However, they almost never are found in the unsuitable habitats that they must cross to reach those wooded habitats. Dogma responds that there once were trees all along the stream that were inhabited by the forest-edge species, and that vicariant events led to their isolation in solitary patches of suitable habitat. However, in many instances we know for a fact that this simply is not true. The white-footed mouse, for example, did not occur in western Kansas in the past, and today it can be found there living in isolated patches of trees in all but three or four counties (Choate and Fleharty, 1975). These are just a few examples of my contention that we don't yet understand even the basics of dispersal. To rely on vicariance to explain events that clearly resulted from dispersal is trendy but wrong.

The next question relates in a general sense to animal behavior. Every mammalogist who has spent much time in the field knows that mammals exhibit memory. If mammals other than primates can remember, can they also think? Dogma says No!, but anyone who has raised pets knows that pets express emotion, and I suspect that the emotion we see is the result of cognitive processes. I risk ridicule by even suggesting this possibility, but I truthfully don't think we know the answer to this question yet.

The next question relates to ecology. Ecologists are really good at formulating new terminology and ecological rules, but do they yet understand even basic ecological phenomena such as competition? In many respects, competition between species of insects is much easier to interpret than competition between species of mammals. The Competitive Exclusion Principle is an example of an ecological rule that may be correct, but the mammalian study on which the principle initially was based was seriously flawed. Someone should take a hard look at what really happens in competition within and among species of mammals.

Will we ever answer the most basic question of systematics: what is a species (Engstrom et al., 1994)? The biological species concept is a hypothesis that seldom can be tested, and the evolutionary species concept is a cop-out that is useful only if we wish to employ phylogenetic methods of analysis. Are there lots of different kinds of species, or are species merely a human construct? Again, I may be ridiculed for even suggesting that we don't know what species are, or for admitting that I still employ the biological species concept whenever possible, but I hope future mammalogists will test the dogma that serve as the very foundation of our systematic theory.

These are just a few questions that future mammalogists should ask. I sincerely hope there will continue to be academic institutions at which students can obtain the classical training in mammalogy that will be needed to find solutions to these and similar questions, and where mammalogists can still honestly identify themselves as persons "who study bats and rats" rather than just molecules (Fig. 4).

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I thank Al Gardner and Hugh Genoways (archivists for the American Society of Mammalogists) for providing the photograph of Hugh, Knox Jones, and me studying pocket gophers (Fig. 3). Professor Horace Hays sent me the photograph of him (Fig. 1). The photographs of Fi and me (Fig. 2) and of several of Knox Jones' former students (Fig. 4) are from my collection. Finally, my wife, Fi, read the manuscript, pointed out some major lapses, and offered suggestions about how to soften some of my more caustic remarks. As she has done for more than 40 years, she kept me pointed in the right direction.

Figure 4. Older and wiser, six of Knox Jones' former graduate students assembled at the annual meeting of the American Society of Mammalogists for the year 2001 in Missoula, Montana: Hugh H. Genoways, Thomas H. Kunz, G. Lawrence Foreman, me, Carleton J. Phillips, and Timothy E. Lawlor. Lawlor completed the M.A. degree and the others the Ph.D. degree at the University of Kansas under Knox's tutelage. The gathering in Missoula was my 34th consecutive ASM meeting.

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BECOMING A MAMMALOGIST: ON THE WINGS OF HEROES

Thomas H. Kunz

Thomas H. Kunz was born in Kansas City, Missouri on 11 June 1938. He received a B.S. degree in Biology and M.S. degree in Education from Central Missouri State College in 1961 and 1962, respectively, a M.A. degree in Biology from Drake University, and a Ph.D. in Systematics and Ecology from the University of Kansas in 1971. He is currently Professor of Biology and Director of the Center for Ecology and Conservation Biology at Boston University.

In recent months, we have witnessed innumerable acts of heroism in the aftermath of September 11, 2001. Like many children growing up in the 40's and 50's, my life has been punctuated by having several heroes—both imaginary and real. The Lone Ranger, Sky King, The Phantom, The Shadow, Superman, and Batman were among some of my early heroes. As a child, I vividly remember sitting on the floor in our living room, glued to the radio, trying to envision the acts of courage and heroism by these imaginary heroes, each of who seemed bigger than life. I also had my share of tangible heroes—favorite uncles, cousins, an older brother, teachers, coaches, and especially my parents. In this paper, I will highlight how these and other heroes have influenced my life, and ultimately why I decided to pursue a career in biology.

As noted in the title, this essay is about "mammalogists" and "heroes." For some readers of this exposé, these two terms may need additional explanation. For others, however, a simple definition may suffice or at least it will establish limits for what I have written. I thought that the best definition of mammalogy or mammalogist might be found by consulting textbooks that include mammalogy in their titles. To date, four major textbooks on mammalogy have been published: Frank Golley's *Mammalogy*, Harvey Gunderson's *Mammalogy*, Terry Vaughan's *Mammalogy* (now in it's 5th edition), and Feldhammer et al.'s *Mammalogy: Adaptations, Diversity, and Ecology*. Surprisingly, none of these books defined mammalogy or mammalogist. The most explicit statement that I could find in these texts was in Feldhammer et al. (1999), who stated that "Mammalogy..... can be approached from a variety of subdivisions, all of which ultimately are complementary and interrelated." For the sake of completeness, I consulted the unabridged *Random House Dictionary* and *Webster's Third New International Dictionary* for a definition of these two terms. A glance at the former gave a definition of mammalogy as "a branch of zoology dealing with mammals." The best definition of "mammalogy" that I could find was the following: "mammalogy—the science dealing Mammals are distinguished from all other vertebrate groups in having mammary glands (for suckling young).

"Hero" is a term that seems to have nearly universal meaning. Even young children can grasp and understand the basic concept of this word. Again, I consulted my trusty dictionaries and found the following definitions of hero: "A man of distinguished courage or abilities" *(Random House Dictionary of English Language)*, and "A man admired for his achievements and noble qualities and considered a model or ideal" *(Webster's Third New International Dictionary)*. I especially like the latter definition because it refers to a model or ideal individual who sets the highest standards to be emulated (equally applicable to heroines).

Without question, my first hero was my father—William H. Kunz—the firstborn son of five children born to German immigrants. His father Rudolph, a blacksmith, his mother Emily, a housewife, were of very modest means, but they provided a supportive, nurturing, and loving home life. A "city boy" born in 1901, my father had little formal education after the 8th grade, yet as a young boy he developed a passion for music and sports. World War I broke out in 1918, when my father was 17. He was considered too young to serve and was thus spared service in the military. In his early 20's, my father helped his own father renovate and add rooms to their modest home in St. Paul, Minnesota, space that would comfortably accommodate four siblings and a cousin, until they were all old enough to leave home.

In his mid-20's, my father met, courted, and eventually married a young country girl, Edna F. Dornfeld—who was raised on a dairy farm in Lake Elmo, Minnesota. My mother was one of eleven children born to first-generation German immigrants. During her early 20's, my mother moved to St. Paul to live with her oldest sister (Nora) and husband (William Flowers)—I suspect to be closer to my father who lived only a few hours away by foot, but less than 30 minutes by trolley.

Photographs of my father and mother, during the Roaring 20's and early 1930's, showed them as a happy, fun-loving couple. I recall seeing photographs in that period showing my father with his new model-T Ford, rumble seat and all, filled with his younger siblings and friends. I also remember seeing photographs of my father playing a violin, accompanied by his brothers Fred and Rudy (on the cornet and piano, respectively) and a friend (on drums). I would have loved to hear them play—it's regrettable that tape recorders were not available at the time to record their music. My father's band played for weddings and local dances—which no doubt provided valuable "pin money." An outgoing and charismatic man, my father spent his early years working as an office clerk and as a salesman. My parents were married on June 7, 1925. They deferred having children for almost 10 years, a decision that in part was influenced by The Great Depression of the 1930's. Jobs were far and few between during this period, and my parents sought their "riches" by moving first to Des Moines, Iowa, and later settling in Kansas City, Missouri. My father sold silk thread for a Chicago-based company, which later collapsed during the early days of the Depression. I remember my father would tell stories of walking the streets of Kansas City in search of almost any job, wearing shoes with holes in the soles that he often replaced with cardboard to keep his feet warm and dry.

My mother was the first to find a job as a stenographer, and later as a bookkeeper. Through a stroke of luck, my father found a job with the Kansas City Power and Light Company (KCPL), his place of continuous employment for over 35 years until his retirement at age 65. In his early years, my father was a meter reader, wore a uniform, and drove a yellow and black 1942 Chevrolet coupe, with a "Redi Kilowatt" logo painted on the doors.

I was the second of two children born to my parents. My brother, Jim, was born on May 27, 1935. I entered this world on June 11, 1938 (Figure 1). As a young boy, I remember being very proud of my dad. He looked dapper in his company uniform and leather-billed hat. My mother was a stay-at-home housewife, who cared for and nurtured my father, brother and me (Figure 2).

Figure 1. Tom Kunz at two months of age (August 1938).

Figure 2. Tom Kunz (age 2) and his brother Jim (age 5).

Before I entered elementary school, I remember that my father sometimes stopped by the house for lunch—or just to say "Hi" to us. After I began to attend school, I missed those noontime visits. However, for several years, I remember stationing myself about two blocks away from our house for my dad to return from work sharply at 5 PM. In those days, cars had running boards, and my brother and I often "hitched" a ride home, clinging to the door while standing on the running board. As I became a little older, my father let me sit on his lap to "steer" the car the last block on the way home. At these times, I was on top of the world. My father was my first hero.

MY SCHOOL YEARS AND SPORTS HEROES

For as long as I can remember, my father played catch with my brother and me, along with the rest of the neighborhood kids. When he returned home from work, I am certain that he would have rather put his feet up and just rested after a long day at work. We played so much ball in our backyard, the ground was usually worn bare of grass. I remember that my father played softball for the Kansas City Power and Light Company. He was a pitcher, and a very good one! His team wore a yellow shirt made of terry cloth with a Redi Kilowatt logo on the breast pocket. I thought his cleats were cool. I remember being very proud of my dad—because his team almost always won. His team won several championship trophies. He was my first sports hero!

In the winter months, when it was too cold to play ball outdoors, my father played "ping-pong" or table tennis, holding his paddle in the "Chinese style." My father learned to hold a paddle this way, as he would say, "like you hold a pencil." He learned this grip when he played some of the best Chinese table tennis players in Kansas City, as they toured the United States in international tournaments before World War II broke out in the Pacific.

He used to play competitively in a large gymnasium at the Kansas City Power and Light Building, the tallest building in Kansas City until the early 1960's. He usually played singles, but sometimes played doubles, including mixed doubles with a young woman who worked in his office. He was at his best as a singles player. In fact, he was awesome! I can remember that he would put so much "English" on the ball with his serves, he would absolutely baffle his opponents. It was rare that an opponent could return his serves, yet alone his "slams." I knew then that I wanted to be just like my father! During the 1940's, my father won several city championships—with trophies that I recall graced the mantle in our living room.

At the age of four or five, before my head could barely reach the top of the playing surface, I learned to play table tennis on a table that my father had fashioned from two pieces of plywood that he painted green, with white stripes and all. We used sawhorses for table legs. My father assembled platforms from wooden orange crates that my brother and I would stand on as we volleyed the ball back and forth with my father, who patiently waited at the other end of the table, often chasing errant balls, as we would often hit them onto the floor. My father was a very patient and generous man! I remember that he sometimes let us beat him at his own game. From these "wins" we developed confidence in our own skills as competitive players. It was only later that I understood what it meant to be a "real hero."

During my early teens I honed my own skills in table tennis, and became a pretty good player—at least in the eyes of my peers. I won just about every time I played against my friends at Bristol Teen Town (the local youth hangout on Saturday nights). I was competitive and uncompromising! As I grew older, I was certain that my father let me win many of those early games. As my skills improved, I even beat my father fairly sometimes. At least I thought that I could beat him fairly—only to find out later that he often switched the paddle to his left hand on many of those occasions. My father, brother, and I spent many hours playing, and competing for family and neighborhood "championships," much to the amusement and pride, I am sure, of my father. In those early years, he had accomplished something that remains a legacy in our family. As my father shared his love and passion for table tennis, I too taught my children, Pamela and David, how to hold a paddle Chinese style—as my father had taught me. My father continued to play table tennis, well into his 80's, often beating both my brother and me—usually in close matches. The first time my children played table tennis with their grandfather—just as he had played with and taught my brother and me—

I was deeply moved. He still had the patience that he had with me as a young boy. I admired his patience and ability to mentor—he was truly my first hero!

As it turned out, table tennis was the only sport that I enduringly shared with my father. Although I played on various softball and baseball teams as a young boy, and sometimes engaged in recreational bowling, as did my father, I never developed the skills needed to be the successful fast-pitch softball player that was his trademark. Perhaps I really didn't try to emulate my father in this sport, or else I discovered that I could not compete on the same playing field with him. I will really never know why I chose to pursue interests in other sports, continuing to play those in which I achieved moderate success. Nor did I become a championship bowler as my father had become later in his life. I also played basketball for teams in elementary school, YMCA, our church, and my Boy Scout troop, as well as pick-up games in our neighborhood.

When I was in the eighth grade, I met one of my early, non-filial heroes. Henry Tittle, the boyfriend (and later husband) of my next door baby sitter—Phyllis Ross. Phyllis had been my baby sitter when I was younger—she taught me how to use crayons to color in a very unique way that envied my friends and classmates. In 1952, Phyllis was a junior in high school and Henry was a senior and a star quarterback at William Chrisman High School in Independence, Missouri. The first time I saw Henry play, passing and running around and through opponents, I was awed! I was 14 and impressionable, and I knew from the first time that I saw Henry throw and run the football, I wanted to be just like him! He was my new idol and my hero!

Around the same time, my mother showed me a photograph of her youngest brother, Glen Dornfeld, when he played football in college. Glen was the only sibling of my mother who attended and graduated from college (Gustavus Adolphus, in Minnesota). I not only wanted to emulate Henry Tittle, but I also wanted to be like Glen--one of my favorite uncles.

As a young boy, I played sandlot football in the neighborhood—often in our backyard. Most of the neighbor boys were older than me—as was my brother Jim, by three years. We played touch and tackle football at the time, and wore no pads or helmets—and as a result sometimes ended up with scraped skin, bruises and occasional broken bones and chipped teeth. Today, I often wonder how we survived those days.

Compared to my brother and his peers, I was fast and scrappy—I had to be to survive. I loved contact—to hit and tackle bigger and older kids—and I loved to run with the ball, sometimes following the interference provided by the older neighbor boys. These games, which sometimes ended in pushing and wrestling "fights" when we accused the opposing team of "cheating," hardened me to play when I was hurt and being able to compete on the same playing field with boys three years my senior. My brother and a neighbor, J.D. Ross, taught me how to wrestle. They often urged me to beat up on one of the neighborhood bullies, Dickie Gross, who was three years my senior. I loved physical contact and to hold bigger opponents to the ground. I was trained as the neighbor "pit bull," especially when arguments broke out as a consequence of being a loser, or perhaps just because we had nothing else to do.

My high school (East High in Kansas City, Missouri), included grades 8 through 12. We could not play varsity athletics as eighth graders in 1951, but I attended every game that year. As a 9th grader, I tried out for the JV football team. At the time, I could not have weighed more that 125 pounds, dripping wet, and compared to some other boys in my class, I was not as physically developed. Our equipment and uniforms consisted of old, hand-me-downs, probably from teams 10 to 20 years before. The old helmets were made of leather, with dried out chinstraps, hardened from years of accumulated sweat. We thought the helmets were cool, as they looked like those that were worn by Knute Rockne, the legendary hero of all times. Our knee and thigh pads did not fit well because our pants were too loose from being stretched from years of use and abuse, and the cleats on our shoes were so badly worn, we just as well could have worn tennis shoes. We were a rag-tag team to be sure.

As a 9th grader, I made the "scrub" team; we were the whipping boys for the JV team during practice—I never played a real JV game that year. As a 10th grader, I advanced to the JV team, and although we had a 6-game schedule, we were the "red-shirt team" for the varsity team. As an 11th grader, I made the varsity team, and played right defensive end. One day in the locker room, I overheard our head coach, Roy

Brown, tell one of his assistant coaches, Buck Harris, that, "Kunz is going to make a very good end one of these days." I reveled for days from this complement, thinking that "I will prove coach Brown right." Another assistant coach, Bob Cross, started to work with me in practice. He had played at Missouri Valley College where he was a Little All-American end. I stood in awe of him! He too became one of my heroes. I remember that he showed me how to set myself into a defensive position to shed off would-be blockers and to keep my eyes on the hips of the quarterback and running backs. This advice paid off, for in my first game playing on the varsity team, as a junior, I sacked the quarterback and made several unassisted tackles of halfbacks trying to sweep around end. The one piece of advice that Bob gave me was to "always contain the end game." "Don't let anyone on the outside." This was advice that has stayed with me to this day.

Although I was a second string varsity player in the 11th grade, I did get to play during the second half of most games. We had an excellent team, and coach Brown always let his second team have playing time when we were comfortably ahead of our opponents. Unfortunately, mid-way through my junior year, during practice, a big lineman fell on my left hand, forcing it backward and breaking the fourth metacarpal. I missed playing the second half of the season during my junior year, but I attended all of the games, watching with envy from the sidelines.

I worked very hard during the spring of my junior year. I was on the track team as a quarter miler and high jumper. I was an average performer in these events. I also played softball for my church team in the spring. While at bat during a game with an opposing team, I twisted by right knee, locking it in a bent position, as I swung hard at a pitch—but missed. The pain was excruciating! I was unable to straighten out my leg. I had driven my Dad's car to the game that night, and after hobbling to the car, I drove home with an injured knee. To this day, I don't know why I didn't ask someone else to drive. To make a long story short, I had surgery a few days later to remove a torn cartilage.

During that summer, I worked very hard to strengthen my leg, lifting weights, and running as much as possible to get in shape for fall tryouts. My dream was to make the starting team—as the Kansas City Star (the local newspaper) projected our team to win the Kansas City school championship that year (1955). I made the varsity team and played mostly as a defensive end. That year we had a 10-0 record, and won the City Championship.

In the spring of my senior year, I again participated in track and field (Figure 3). I ran the quarter mile, was on the mile relay team, and competed in the high jump and broad jump (now called long jump). We had a good track team and frequently won our dual meets, although we did not win a city championship. Southwest and Southeast High Schools were the perennial champs in track and field. I was an average runner and jumper—my best ever time in the quarter mile was 58 sec; the highest I could jump was 5'10", and the longest I ever jumped was 18'0". In one of the dual meets, mid-way in the season, I reinjured my right knee while competing in the broad jump. Although my injury at the time did not require surgery, it did limit my flexibility.

Figure 3. Tom Kunz as a senior in high school standing next to the high jump pit (1956).

During the spring semester of my senior year in high school, I applied to and was admitted to the University of Missouri, to study entomology. I wanted to continue playing football in college, but I was considered too small (150 pounds, dripping wet) to play for a Division I school. I was disappointed, but I had resigned myself to the fact that I would watch college games from the sidelines, and perhaps play intramural football, while pursing my academic and social interests.

Upon high school graduation, I had a change of heart. I enlisted in a newly established six-month reserve program in the U.S. Army (Figure 4). I had tried to enlist in a similar program with the U.S. Marine Corps, along with seven of my best buddies, but I failed the physical exam because I could not do a full deep knee bend or stand up quickly from a kneeling position—all because of my gimpy knee. I was devastated! However, the following day, I went to the U.S. Army recruiting office and took their physical exam—and passed. I entered the military a few days after my 18th birthday (June 11). I was stationed at Fort Leonard Wood, Missouri, for my six-month tour of active duty.

Figure 4. Tom Kunz during basic training in the U.S. Army (1956).

Apart from my gimpy leg, I was in excellent physical condition. I won many of the physical training challenges in my company, and was appointed one of the squad leaders in my barracks. I loved the physical challenges, running, pull-ups, chin-ups, etc., often winning the highest marks and timed runs among my peers. Throughout basic training and advanced (engineering) training, I continued as a squad leader. I idolized some of my drill instructors, as they seemed tough and were in excellent physical condition. During breaks in training, some of my buddies and I sometimes played touch football and basketball—and I assumed key roles in many of these games. After six months of what was mostly physical training, I was in excellent condition. My weight had increased from 160 to 180; most of this increase was in muscle mass, although I am sure that some of it was from drinking a little too much beer (which was permitted on the post at the time).

I was released from my tour of active duty a few days before Christmas in 1956. It was great to be home. My original idea was to start college in January. However, I changed my mind and deferred entering college until the following fall. Instead, I took a job as a draftsman and rodman with the Missouri Highway Department (I always liked mechanical drawing in high school, and I was pretty good at this too). This job provided me with the flexibility of working indoors some of the time and outdoors at others. As a beginning draftsman, my responsibility was to draw the grade elevations and calculate the amount of soil that had to be moved for the new Interstate highway system that was being planned at the time. When I was not working in the office, I was a rodman on a survey team that worked an area north of Kansas City, near Platte City, where I-29 had been proposed. I really enjoyed the outdoor part of my job.

In the spring of 1957, some of my high school friends, and former football teammates, invited me to visit Central Missouri State College (CMSC), in Warrensburg, Missouri. This was a small school, about 3,500 students, located about one hour east of Kansas City, off of Missouri Highway 50. I was introduced to Ray Comer, the head football coach whose team had won the MIAA (Missouri Intercollegiate Athletic

Association) championship in 1956. However, he had just retired from coaching, and introduced me to the newly appointed football coach, Hal Yinger, one of his assistant coaches.

My interview with coach Yinger went well. I told him about my previous knee injury, and that I needed to wear a knee brace. Notwithstanding, he encouraged me to play at CMSC. He was trying to build a new team because many members of the 1956 championship team were graduating. Coach Yinger was interested in the fact that I really wanted to play football, I had played for a championship team in high school, and had already been in the Army (no risk of being drafted). For CMSC, a Division III institution, the size of players was not a major factor. Coach Yinger wrote to me a few weeks later and offered me a work-study scholarship and an opportunity to play football at CMSC. He told me that I had been highly recommended by Coach Brown, my high school coach, and that one of the freshman coaches was Bob Cross, the same coach that encouraged me when I played at East High School. After giving his offer some thought, I decided not to attend the University of Missouri, but instead accepted and ultimately matriculated at CMSU in the fall of 1957.

I knew many of the players on the team at CMSC—some had been my teammates in high school (Jim Dahman, Al Salmon, Otto Sales, among others). Several other players from East High School, whom had graduated before me, were also members of the CMSU football team that was returning (including Elton Valines, an excellent end). It was a good feeling to be joining a team with some of my former high school teammates. Bob Cross, one of my assistant coaches in high school, was one of two freshman coaches at CMSU that year—while he was working on his Masters degree. I was excited that I would have Bob again as a coach, because he was one of my early sports heroes. I had great admiration for Bob.

In 1957, NCAA and MIAA rules required colleges and universities to maintain separate freshman and varsity teams. I made the freshman team and started at defensive end—earning a provisional letter (the rule at the time was that if you had enough playing time on the freshman team, you would automatically get a varsity letter for that year if you made the varsity team and lettered the following year—and I did!). I played most of the games that year, but continued to be hampered by my gimpy knee.

As a sophomore, I made the varsity team and played in most, if not all of the games as a second-string defensive end, although sometimes I played offensive end (Figure 5). I lettered as a sophomore, but again injured my knee, requiring more surgery when school was out the following spring. I worked out hard during the summer, getting back into shape, especially focusing on my gimpy leg. I continued to wear a knee brace as a junior. I started most games as a junior—usually as a defensive end, but increasingly played offensive end opposite Jim Dahman, who played left end, as we had together our senior year in high school.

Figure 5. Tom Kunz during his sophomore year on the varsity football team at CMSC (1958).

Midway during my junior year, when playing a game in Springfield, Missouri, at Southwest Missouri State College (SWMSC), I was hit hard under the chin by an opposing lineman, breaking off my first upper left incisor. Today this tooth is probably buried somewhere beneath the turf at SWMSC. At the end of the season that year, along with my teammate Jerry Boyce, we were elected co-captains (Figure 6). In my last year of eligibility (1960), I started most of the games, but was plagued by injuries part of the time. I mostly played right offensive end in our straight-T formation.

Figure 6. Tom Kunz (second from right), as co-captain of his college football team (1960).

Throughout my college career, we never had a winning season. Our schedule usually included 9 games, and our average record during my years on the varsity team was 4 wins and 5 losses. As our coach would say, these were "character building" years.

Some of my teammates in college were also idols and heroes. I especially admired my friend Jerry Boyce, with whom I shared the position of co-captain, and whom I roomed with when we played away games (Figure 7). At a distance, I also had great admiration for Bob Haas, who was a close friend in elementary school, who had become a star quarterback and punter

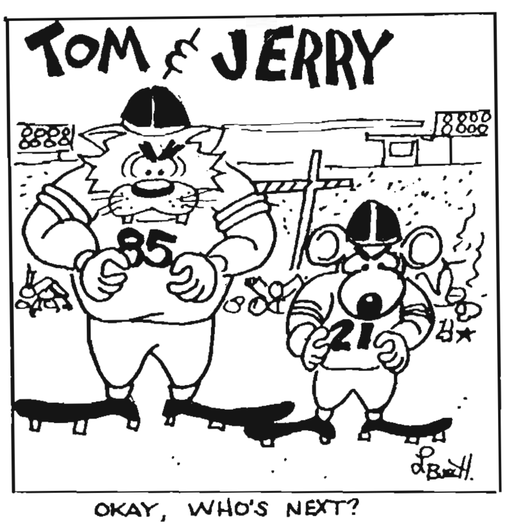

Figure 7. Cartoon of co-captains Tom Kunz (# 85) and Jerry Boyce (# 21) published in the CMSC newspaper, *The Student* (September 23, 1960).

at the University of Missouri during the "Dan Divine era."

Coaches and teammates have played an important part of my life—to me, they were often bigger that life. Many of them, unknowingly, have played an important role in my life, as they were models to emulate. They too were my heroes!

My First Experience Coaching

Having been a co-captain on my college team, I was invited to coach the freshman team in the year following my graduation. Hal Yinger had been forced to resign from the head coaching position, because of his loosing record. A new coach, Bob Hoff, an All-American guard from the University of Iowa, was appointed to begin coaching in the fall of 1961. I decided to accept the offer of assistant coach to be mentored by a former All American. It also allowed me to pursue a Masters degree in Education, and being paid to do so. It just seemed like the right thing to do, because I believed that coaching was one of the highest forms of teaching. Coach Hoff was a charismatic man—and was greatly admired for his past accomplishment and honors as a college player at Iowa, and for his aspirations of developing a winning tradition at CMSC.

I coached the freshman team in the fall of 1961, along with my friend Ben Gieringer, a former college teammate, and fraternity brother. When we were not coaching or attending classes, we were expected to scout for the varsity team. Our freshman games were played during the week, usually on Thursday afternoons, so that we could scout games of our competitors on Fridays and Saturdays. Thus, we missed many of the games that our varsity team played that year because of our scouting responsibilities. We had a pretty good freshman team, attaining a winning record with seven wins and three losses. The varsity team also had a winning record that year (5-4), but Coach Hoff was fired at the end of the season for insubordination—he bought new uniforms and equipment without authorization, among other indiscretions.

During the fall semester of 1961, while walking across campus one day, I saw the woman who would eventually become my wife—Margaret Louise Brown.

That fall, she had been appointed as an instructor in the Business Department at CMSC. She had just completed her Master's degree at the University of Missouri in Columbia. On several occasions, I remember seeing this absolutely gorgeous redhead, as she walked briskly across the campus to teach her classes.

I had an extremely busy schedule that fall semester, with course work, coaching, and scouting. It was not the kind of schedule that allowed me to pursue any serious romantic interests. Moreover, at the time I was carrying on a long-distance romance with another young woman, Kay Rutherford, whom I had met the previous summer. She had just graduated from Lexington High School and was taking summer classes before entering her freshman year that fall at the University of Missouri. Kay seemed a lot more mature than most freshman women I had known before—but she was after all just a freshman. Needless to say, my opportunities for establishing a new, on-site romance with the new redhead on campus were not ideal. I maintained my distance romance with Kay through the end of 1961—corresponding weekly by mail.

To make a long story short, Kay and I decided to part ways shortly after Christmas that year, and I soon began to court Margaret. From our first date (although I never told her this at the time), I knew that she was pretty special. We saw a lot of each other that spring and following summer, and became engaged in September of 1962. We were married on December 27, in that same year—on a cold blustery evening in Margaret's hometown, Faucett, Missouri. We lived in Independence, Missouri, while Margaret taught business courses at the Independence Campus of CMSC, and I taught at Shawnee Mission West High School.

WHY I BECAME A BIOLOGIST

My parents were not educated in the formal sense, as neither attended college, however, my mother had grown up on a farm, and as a young boy my father sometimes helped on farms and other odd jobs. Early experiences of both of my parents led them to appreciate and enjoy animals. During World War II, my parents had a Victory Garden and raised chickens—one of the chickens was my pet (Figure 8). We also had our share of dogs (Daisy May, Spot, and King, among others), but no cats (I just never liked house cats!). As a child, I also remember catching and playing with crayfish, frogs, lizards, snakes, and insects.

Figure 8. Tom Kunz with his pet chicken (1945).

During summer vacations, my father would take us fishing, teaching my brother and me how to put earthworms on a hook, and to handle fish without being spined (Figure 9). These were some of my first experiences with animals. While on a family vacation in Minnesota, at the age of 10, I had my first real encounter with bats. Before this experience, on summer evenings my brother and I often threw small pebbles into the air, and watched bats dart in circles trying to catch them—only to give up their pursuit when they discovered that the pebbles were not something to eat. The first night that we stayed in a small cabin on Lake Elmo (Minnesota), my father decided to light the small pot-bellied stove to "take off the chill." As he opened the stove to fill it with firewood, out flew literally dozens of bats. I really never knew how many were flying around in our cabin, but all of us grabbed what we could—brooms, pillows, dustpans, and the like—to swat at the bats. It was a rather frightening experience at the time! Before all of them escaped from the stove, my father had put on thick rubber gloves, that he used for his electrical work, and began to remove them from the stove, placing hand-

Figure 9. Tom Kunz with his father (left) and brother, Jim (center) after a successful fishing trip (1950).

fuls in a burlap bag. Many escaped his efforts, and continued to fly around the cabin. Only later did we have the sense to open the door to let the bats fly out. Overall, I don't remember this as a particularly pleasant experience, or one that would serve to enlighten me in years to come, however, I do remember being fascinated by these little winged "critters."

My fifth grade teacher, Miss Alma Reed, was by far the most instrumental in stimulating my early academic interest in biology. She seemed quite old at the time, but was probably not more than 40 years of age. She was an old maid, by standards of the time, who lived with her sister and brother about a block from my house in Independence, Missouri. Miss Reed raised silkworms, and each year at a certain time brought them to class to share with her students. At the appropriate time in their life cycle, she gave each student in her class a cardboard milk carton with some silkworm eggs and freshly picked mulberry leaves as food. We were expected to care for these eggs (and the larvae that hatched from them) and to write a report on what we observed. I remember that I was fascinated, watching the eggs hatch, and to see the larvae grow as they fed on the mulberry leaves, which I had to replace with fresh leaves every few days (from the small trees that grew along the edge of our backyard). I still remember when the larvae were large enough and they started to spin silk, first making supporting strands in the corner of the milk carton, and then later spinning a cocoon the size of a peanut shell. It was fascinating to watch as the larvae spun silk and turned around inside their temporary home. I don't remember much about my report, nor could I find it along with some of the other memorabilia that my mother had saved for me, but I do remember the thrill of observing silkworms feeding and making their intricate cocoon, and as they finally emerged as adult moths. This experience left an indelible impression on me, and highlights the power of observational learning!

I remained fascinated with insects, often looking for the eggs, larvae, pupae, and cocoons of moths and the chrysalis' of butterflies in my neighborhood. My brother and I used to capture all sorts of moth larvae and tried to feed them on assorted leaves—which many of them seemed to like. I also loved to watch the larvae of bagworms that fed on our evergreen shrubs in front of our house. I still remember seeing a large tomato worm (larva) feeding on the tomatoes in our vegetable garden, and pleading with my parents to watch it spin it's cocoon and emerge as adult moth. I also remember finding the aqua blue chrysalis of a monarch butterfly, with its golden crown, and bringing it inside for the winter, hoping to observe it emerge as an adult (which it did not, much to my disappointment).

Miss Reed only lived a five-minute walk from our house, and I remember visiting her after school hours to see her silkworms, birdhouses, flower garden, and other menagerie that she kept. She seemed to have a passion for her garden and silkworms, and she was always happy to share her enthusiasm for anyone who would dare visit. I remember sitting on the large swing on her front porch, paging through and reading books about insects that she happily shared with me. At the time, I did not know the meaning of passion, but that's what she must have had. To this day I think some of her passion for organisms must have rubbed off on me.

During my remaining years in elementary school (6th and 7th grade), my academic interest in insects was temporarily diverted—or perhaps distorted. Instead, I used insects to scare (or impress) girls, because I was not afraid to pick up all sorts of critters with my hands. I am sure that I was regarded as a brat by some of the girls.

In the sixth grade, I advanced from being a Cub Scout to a Boy Scout, and began to attend Camp Oceola (in the Missouri Ozarks) for 10 days each summer, at least until my 15th birthday (when girls and cars became of much greater interest). During the intervening years, I earned enough merit badges to earn an Eagle Scout. At camp we took nature study, Indian lore, crafts, swimming lessons, took long hikes, and participated in other activities. I especially remember our nature counselor, W.B. "Pappy" Grube, who was a business teacher at East High School, in Kansas City, during the school year. Pappy had a passion for nature, and was always willing to help us discover critters, whether in the forest or in streams. I remember being immensely impressed with his knowledge, although most of it was probably at the level of Golden Books and Peterson's Field Guides. Nonetheless, he instilled a sense of wonder in his charges, and encouraged each of us to explore the wonders of nature. During one summer, I remember taking great pride in earning my "Nature" merit badge, for learning the common names of many local frogs, birds, mammals, insects, and trees.

By the time I had become a 10th grader in high school, I knew that I wanted to be some kind of biologist. My high school biology teacher (Miss Eleanore Canny) also had an important influence on my thinking, as she too had a passion for biology. When we studied snakes, she offered us rattlesnake meat to eat. When we studied birds, we incubated chick eggs, and watched them hatch. We were required to keep a notebook from articles in the newspaper about nature (some of these articles were even found in the comic section of the Sunday newspaper—Mark Trail, the naturalist). We were required to make an insect collection—and ironically this brought me back in contact with my 5th grade teacher, Miss Reed. When I visited her several years later, she was most pleased to learn that I had pursued my early interest in biology.

During my junior year in high school, as I began to think about college, it became clear to me that I was more strongly motivated to pursue interests in biology than in the physical sciences (certainly my grades were better in Biology than in Chemistry). Having spent two summers working on my uncle's farm in Minnesota, in my early teens, I began to think what it might be like to be a farmer or rancher. At the time, I dreamed that some day I would own a ranch in Texas, with lots of cattle and horses (this dream is yet to be realized).

A TIME TO EXPLORE

When I entered college in the fall of 1957, I declared a double major in Biology and Physical Education. At the time, the Biology Department had only three professors—Dr. Sam Hewitt—affectionately known as "Smiling Sam the Botany Man," and who taught General Botany, Plant Systematics, Plant Morphology, and Microbiology; Dr. Laura Nahm—the Department Chair—who taught General Zoology, Comparative Vertebrate Anatomy, Human Physiology, and Genetics; and Dr. Oscar Hawksley, who taught Wildlife Conservation, Ecology, Ornithology, Invertebrate Biology, and co-taught Field Biology. During my Junior year, Richard Myers (who was hired to teach Embryology, Natural History, Teaching Modern Biology, and to co-teach Field Biology), was added to the faculty. He had completed his coursework and research for his Ph.D. at the University of Missouri, but still had to finish his dissertation.

In a small department, with class size seldom over 20, it was easy to get to know the faculty and my classmates. During my first two years of college, I took every course offered by Laura Nahm (whom I considered to be my "second mother"). She was an excellent teacher, although very strict. She could write on the blackboard without losing eye contact with her students—an amazing trait that I could never emulate. I had taken one course each with Sam Hewitt (Plant Biology) and Oscar Hawksley (Wildlife Conservation) during my freshman year, but because the curriculum was structured in such a way, I did not take another course from Dr. Hawksley until my junior and senior year—taking Ecology as a junior (we used the first edition of Eugene Odum as the textbook), and Field Biology as a senior. All of the courses that I took, with the exception of Wildlife Conservation, were lecture/lab courses with 3-6 hours of lab (or field trips) per week each. Field Biology also included a weekend field trip to the Ozarks.

When I was not playing football, exploring my interests in the opposite sex, attending classes, and studying (in that order), I developed an interest in exploring caves (Figure 10). The Ozarks of Missouri is pitted with caves, and some of my friends and I spent winter weekends exploring caves for recreation. During winter months, some of these caves housed enormous colonies of hibernating bats. I remember being fascinated by the large clusters that packed the ceiling of some of the caves that we explored. In addition to the spectacular limestone formations, we sometimes found fossilized skeletons of bats embedded in the flowstone, presumably having been unsuspectingly trapped in the caves, where they eventually died. At one cave in particular, I remember seeing hundreds of bat skeletons fossilized in the crystalline white limestone. I only later learned that these were known to spelunkers as "bat graveyards."

As we often crawled through mud on our hands and knees, through tight passages, guided only by our carbide lanterns, bats that had aroused from hibernation would sometimes buzz past our heads. I remember thinking "what an amazing life style." In the dim light of our lanterns, while standing in some of the enormous chambers, I recall seeing small shiny objects attached to some of the hibernating bats. We plucked some of these bats from the cave ceiling, only to learn that these shiny objects were actually aluminum bands, each with a number embossed on them. We carefully wrote down the numbers, and noted the inscription, "U.S. Fish and Wildlife Service." I remember being truly impressed—we had found bats that had been banded by someone.

Figure 10. Tom Kunz exploring a Missouri cave (1959).

When I returned to campus, I wrote a letter to the U.S. Fish and Wildlife Service in Washington, D.C. Several months passed, with no response to my letter. Sometime that following spring, after several more spelunking excursions to the Ozarks, I received a letter, with a green carbon copy of an original, stating the species name, where and when the bat had been banded, and by whom. To my surprise and delight, the bander's name was Richard Myers, the new professor in our Biology Department.

Most of my college courses were demanding, but especially in the sciences. The courses that undeniably had the most influence on my thinking were Comparative Vertebrate Anatomy, Invertebrate Zoology, Embryology, Ecology, and Field Biology. Not surprisingly, DNA was not included in our Genetics textbook in 1957, and was only mentioned in passing as a new (1953) discovery in lecture. Comparative Anatomy was the first course that made me think broadly. It had a strong evolutionary focus, and gave me an appreciation for anatomical structure and diversity. Ecology was a "new" discipline being offered in college curricula at the time, and was a course that helped me integrate ideas and subject matter from other courses. Embryology introduced me to the concept of germ-layer theory, and added additional evolutionary concepts to my thinking. By far, the most influential and demanding course (partly self-imposed) that I took in college was Field Biology. This was a team-taught course by Oscar Hawksley (whose Ph.D. research at Cornell focused on birds in the Bay of Fundy) and Richard Myers (whose Ph.D. research focused on migration in three species of bats). Field Biology was the only course that I took that required a field research project.

During my senior year, my brother, Jim, had transferred from Carson-Newman College, a small school in South Carolina, to complete a degree in Biology at CMSC. The only class we ever took together was Field Biology. We teamed up for our course project, entitled "A limnological study of a limestone quarry." In many ways, this project was pivotal in my development as a scientist. This project, among other things, taught me the importance of sample design, consistent sample collections, searching for relevant literature, about the trials and tribulation of fieldwork (we used SCUBA gear and wet suits to sample the 30-m deep, cold water quarry for physical and biotic variables), and the need to be flexible (being alert to new opportunities). In this project, we had the opportunity to use what we had learned in Chemistry (analyzing physical and chemical properties of water), Botany (identifying plant forms), Invertebrate Zoology (identifying aquatic invertebrates), and Ecology (understanding the complexity of a simple ecosystem). My brother and I each received an "A" for this project. Among the memorabilia that my mother accumulated from my college days, the project report for this course is one that I have kept and treasured.

One of the highlights of the Field Biology course was a three-day field trip to the Ozarks. It started with a trip by car, stopping at a local fish hatchery (operated by the Missouri Conservation Commission to stock local streams with rainbow trout), but most of the trip was taken by canoe on the Niangra and Current Rivers, two white-water rivers (both part of the National Wild River Waterways) that had been explored and mapped by Oscar Hawksley and published as a field guide by the Missouri Conservation Commission. Along the way, we camped and cooked on the banks of the rivers, seined streams for fish, mist-netted birds in the forest and bats over streams, visited some of the caves where Richard Myers had conducted some of his research, looked for fossils in limestone outcrops, rappelled from limestone ledges, and studied plant associations along the way. Toward the end of the trip, I remember running through a field of nettles in my shorts (if I had taken another botany course, especially plant morphology, I might have known better—but since then, I have never forgotten what nettles look like), afterward packing cold mud from the river bottom to ease the pain on my bare legs. It was an exhausting trip, but one of the most memorable field experiences in my undergraduate college career. Oscar Hawksley and Richard Myers had become my new heroes!

My focus on biology (especially ecology) was mostly short-circuited for a year, as I completed a Masters degree in Education, and coached the freshman football team at CMSC. Notwithstanding, I often relied on the knowledge I gained from my undergraduate physiology and anatomy classes, as I would assess my players for injuries, taped them for practice and games, and instructed them in weight training techniques, often explaining the importance of working on opposing muscle groups.

My dual interest in sports and biology were in many ways quite complementary. They both provided opportunities to be outdoors for much of the time, they provided outlets for rigorous physical activity, and both involved teaching. In thinking of ways to combine these interests, I decided that I wanted to teach biology and coach in high school. In the summer of 1961, while serving as a teaching assistant for a National Science Foundation sponsored Summer Institute in Biology at CMSC, directed by Dr. Laura Nahm, my college genetics professor, I met Dr. Leonard Molotsky, the coordinator of science education in the Shawnee Mission District Schools (Shawnee Mission is a suburb of Kansas City), who was taking a graduate genetics course at CMSC. I got to know Dr. Molotsky pretty well that summer, and he took a liking to me. He learned of my interests in teaching biology and coaching football, and told me that he would be looking for several biology teachers and coaches to staff a new high school in Shawnee Mission, slated to open in August 1962. This was perfect timing for me, as it promised to provide an excellent career opportunity. In February 1962, Dr. Molotsky called and asked me to submit an application to teach at Shawnee Mission. I did so and was offered a position to teach five 10th grade biology classes and serve as an assistant football and track coach at Shawnee Mission West High School. I happily accepted the offer, with a whopping salary of $6,200 for nine months—almost as much as my college professors were making in 1960 (we learned this by looking up their salaries in the Missouri Blue Book).

At Shawnee Mission West High School, my new air-conditioned classroom/laboratory was top of the line and well equipped for a modern high school (in 1962). There were four other classroom/labs just like mine—with adjacent prep and storage rooms. The labs were better equipped than my college labs—and everything was brand new. The Shawnee Mission School District was highly regarded nationally, and I was pleased to have been accepted to teach and coach in such a highly regarded suburban school system. That year, our school had been chosen as a Beta test site for the newly developed Green Version textbook, developed by the Biological Sciences Curriculum Committee (BSCS)—the first high school biology text with an emphasis on ecology.

In addition to teaching five biology classes each day of the week, and coaching or having games or practices 6 days a week, I found time on some weekends to take small groups of interested students on outing trips. On these trips, organized as part of a new outing club, we sometimes rappelled from rocky ledges near Kansas City, and at other times we drove to the Ozarks, where we spelunked and explored the local flora and fauna that I had learned about as an undergraduate. These trips turned out to be an epiphany for me—as I came to realize that I loved being in the field with students. I decided that I wanted to continue my formal education, and someday teach at the college level.

During my first year of teaching in Shawnee Mission, I met Stan Roth, a highly regarded teacher and naturalist at Lawrence (Kansas) High School, at an annual meeting of the Kansas Academy of Science. Ironically, I had met his wife, Jan, the previous summer, as she was one of the teachers selected to participate in the NSF Summer Institute in Biology for high school teachers at CMSC, a program in which I served as a graduate teaching assistant. In that summer, I also met Audrey Smith, and learned that she had been assigned to one of the other biology positions at Shawnee Mission West High School, and with whom I would eventually work for the next five years in Shawnee Mission.

Shortly following the lofting of Sputnik by the Soviet Union, the U.S. National Science Foundation announced increases in funding for programs to upgrade the quality of high school science teachers on a national scale. Many universities and colleges had begun to apply for and were funded to host Summer Institutes for high school teachers. I applied for one of the Institutes in Biology for the summer of 1963 (ironically at Boston University), but was rejected for reasons I will never know. In the following year, I applied to a similar program hosted by the University of Nebraska (Lincoln), and was accepted for the summer of 1964. As it turned out, this experience and another hero played a critical role in my future career.

IMMERSION BY FIRE

I took two courses during that summer of 1964 at the University of Nebraska--Advanced Ecology and Research in Biology. The former course was taught by a distinguished physiological ecologist, Dr. Thomas Thorson, who did research on osmoregulation of fresh-water sharks in Lake Nicaragua. Professor Thorson was an excellent teacher, who challenged me to think about the relationships between ecology and physiology. One of the field exercises in his course involved a detailed study of the autoecology of a local beaver pond. This field experience was immensely rewarding both practically and intellectually, as it forced me to think more about the ever-increasing complexity of ecological systems. It also introduced me to the value of making detailed observations and data analysis. I greatly admired Professor Thorson as another one of my heroes.

My research course was also supervised by Professor Thorson. For this course, I was expected to design and conduct a study of my own interest. From my experiences as an undergraduate at CMSC, and brief forays to the Ozarks on field trips with my own students at Shawnee Mission West High School, and a weekend field trip to south-central Kansas with Stan Roth, I began to develop a fascination for bats. I thought if I could conduct an independent research project on bats, this could help me decide if this was my future calling. I read all of the literature I could find on bats occurring in Nebraska and surrounding states. Hall and Kelson (1959), in "Mammals of North America," summarized and illustrated the distribution records of bats and other mammals, and I noted that many species in the area reached their marginal distribution in Nebraska or in adjacent states to the east and south (Iowa, Missouri, and Kansas)

In search of a study site, I observed what seemed like large numbers of bats flying high overhead at dusk at the edges of forests and along small streams just south of Lincoln. It turned out that there were not that many bats, but rather the same bats flying back and forth along the edge of the riparian forest. The bats were flying so high, I didn't think that I could capture them with a mist-net, as we had used on that memorable field trip in Field Biology that I had taken with Oz Hawksley and Richard Myers. However, few published studies on bats at the time had reported using mist nets. Most published reports on bats (before 1964) mentioned that bats were commonly shot as they foraged along forest edges and in open areas, although some reportedly were netted over stock tanks in desert regions. Most of this published research focused on distributional and faunal studies, with the primary goal of adding specimens to museum collections. I didn't own a mist net to capture bats at the time. Moreover, most of the streams were too deep, with soft muddy bottoms, so my only option was to use a shotgun.

I remembered that my father had an old .410 gauge shotgun that he taught me to use when I was in high school, and that he used on occasion to hunt rabbits and squirrels. On a weekend visit to Lincoln, Nebraska, my wife, Margaret, brought the shotgun with her, because "I needed it to conduct my research project." When some of my graduate student peers learned that my research project involved hunting bats, they enthusiastically volunteered to assist me. Because many of my summer classmates were from Kansas and Nebraska, they considered themselves "experienced quail and duck hunters," and were certain that they could bring down bats just as they did with game birds. They considered hunting bats to be a challenge.

In the first week of bat hunting, I was not very successful, and went through many shells each night. My "experienced" peers were even less successful, as they learned that shooting flying bats was not the same as shooting slow-flying game birds along a predictable course. Because I had been using so many shells sometimes for naught, I decided to purchase a shell-making kit, with brass casings that could be reused. For the next several weeks, each day before I headed out for an evening of bat hunting, I packed the brass casings with black powder, paper wadding, and bird shot. On the few occasions that my peers used shells with larger pellets, and actually shot a bat, the specimens were so badly damaged it was difficult to prepare them as study skins. The small size of birdshot produced a wider spread, and seldom damaged the bats beyond recognition, as had the larger buckshot.

As the summer progressed, I got better at shooting bats. With diminishing light at dusk, I soon learned that my most successful technique was to shoot the bats as they flew directly overhead. In this way, if I did make a hit, the designated target would fall at my feet in wheat fields where I often stood. I recall shooting a few bats in front of me, but was unsuccessful at recovering some of them because they landed in tall brush, or uncut corn and wheat fields, often some distance from where I had been standing. As dusk faded to near darkness, it was often difficult to judge the distance to a bat from my shooting position. Notwithstanding, I was able to shoot a fair number of bats. Upon returning to my dorm room on the successful nights, I prepared the specimens as study skins (saving the skull) as I had learned in my Field Biology class.

My "take" that summer included 23 individuals of three species, one of which I learned later (see below) were range extensions in the State of Nebraska. I had shot several others, but either could not find them in the vegetation, or they were too badly damaged to prepare them as skins and skulls. My research project was largely based on the bats that I shot, along with the data derived from these specimens, and what I had gleaned from the published literature about their distributions and natural history of each species that occurred in the area. Initially, I was only able to confidently identify two of the three species that I shot. I had read about the external characteristics of all local species, and those occurring in adjacent states, but I was unable to identify a few specimens that were about the size of *Myotis lucifugus*, but with black pelage, although they did not have silver-tipped hairs as did *Lasionycteris noctivagans*. They could not have been confused with the eastern pipistrelle (*Pipistrellus subflavus*), red bat (*Lasiurus borealis*), or hoary bat (*Lasiurus cinereus*), specimens of which I had examined in the Nebraska State Museum. These mystery specimens had a broader rostrum that *Myotis sp.*, but were smaller than a similar-shaped *Eptesicus fuscus*, which also has brown pelage. The wing bones of several individuals were not fully ossified, so I knew that they were probably young-of-the-year.

In the absence of comparative material, I remained perplexed. No other species that fit the description of these mystery bats had been listed in Hall and Kelson for Nebraska, Kansas, or Iowa. The only other possibility that entered my mind was that it might be evening bats, *Nycticeus humeralis*. However, the dark, almost black pelage of some of the bats that I had collected belied any published descriptions, which are typically based on adult characteristics. I consulted "Wild Mammals of Missouri," by Charles and Elizabeth Schwartz, but found no description of a "black bat," the approximate size of *Myotis*. With additional reading, I learned that the pelage of young bats was often darker than that of adults, which led me to think that these specimens could be young *N. humeralis*, but I was not totally confident in my assessment.

Much of my reading about contemporary mammals of North America, especially in the adjacent state of Kansas had been published by E. Raymond Hall, Curator of Mammals at the Museum of Natural History, University of Kansas (KU), and one of his recent Ph.D. students, J. Knox Jones, Jr., who had recently completed his Ph.D. dissertation on the *Mammals of Nebraska*—however this work had not yet been published by the summer of 1964. From my readings, it became clear that KU was a major center of research on mammals in North America, and that E. Raymond Hall was at the forefront of this research, and that he or Knox Jones would most likely be able to identify my mystery specimens.

I decided to contact Dr. Jones to arrange for a visit to KU, and to confirm the identity of the specimens that I had collected. [There were no active mammalogists at the University of Nebraska State Museum at the time, and the reference collection of bats was meager, to say the least.] I called the Museum of Natural History at KU to make an appointment with Dr. Jones, only to learn that he was on a field expedition in Nicaragua and would not be back until later that summer. However, I reached Dr. Hall and he invited me to bring my specimens to KU, so that we could compare them with others in the museum collection.

The weekend before my trip, scheduled for a Monday morning, my wife Margaret had visited me in Lincoln, and we had gone bat hunting on Sunday evening. That night, I had a reasonably successful shoot (by my standards), but was faced with the problem of preparing skins from these bats before leaving for Lawrence the following morning. I worked late into the night, skinning and sewing up the specimens as fast as I could. I was not very fast at preparing skins, and thus did not finish skinning all of them. That night, I turned down the thermostat to my room air-conditioner, as low as I could, to help prevent two specimens from badly decomposing—after all they had been shot, and some had sustained considerable damage from being shot.

The following morning, Margaret and I headed off for Lawrence, with freshly prepared specimens pinned neatly to pieces of cardboard, a cafeteria tray, needles, thread, cotton, pins, and with several unprepared specimens in plastic bags. As my wife drove our 1961 Chevrolet Impala (which was not air-conditioned), I skinned the remaining specimens as I sat in the shotgun seat, with skinning tray in hand. As we pulled into Lawrence, around noontime, I was just pinning out my last specimen. I had wanted my carefully prepared specimens to be appropriately dried before taking them to the museum. Naively, I had placed them on the shelf under the rear window of our car while we were in transit. Because our car was not air-conditioned and we had to drive with some of the windows open for fresh air, the bats dried too fast, including those that I was preparing as we drove—and most of them became shriveled, and looked pretty bad.

When we arrived in Lawrence, we immediately went to the Museum. Dr. Hall was waiting in his office. Carrying my recently pinned specimens, and others that I had collected and dried previously, we sat down in his office. I remember that he wore a dark blue suit and a white shirt with a tie. The shoulders of his suit had a collection of dust, his shirt was wrinkled, and his tie had what looked like spots of gravy on it. He held a pipe in his mouth that was held together with wire. I thought to myself, "he must be some character." His voice was gruff and he seemed very stern—not what you would consider the friendly type.

His first question was "what do you have there son?" I briefly told him what I had been doing, and that I was perplexed by the black specimens that I could not confidently identify. He asked me what I thought they were. I had read descriptions of all of the bats in his book, *Mammals of North America*, and hesitantly told him the closest I could come was *Myotis* *nigricans*, because the size and color description was as close as any I could find. He looked at me in a puzzled way, and told me that this was impossible, since *M. nigricans* was a tropical species. I told him that I realized this, but thought that they might have strayed northward, similar to the way some individuals of *Tadarida brasilienis* had been reported in Nebraska far to the north of their usual distribution. He dismissed this as being ludicrous, and began asking me questions about other species of bats that I had collected, including questions about their roosting habits and especially their geographic distributions. I remember feeling totally inept before this giant of a mammalogist—someone whom I had admired, based on his published work. However, from my answers, I must have convinced him that I had done some reading, and knew something about the bats that I had shot, or maybe he felt pity for me.

After what seemed like eternity, he told me that his former Ph.D. student, J. Knox Jones, Jr. was in Nicaragua, and would not be returning until later in the summer. However, he showed me a copy of the page proofs from Jones' dissertation, and then invited Margaret and me to the mammal collection on the 5th floor of the museum. By that time, I was beginning to feel better—at least I had made it past the first hurdle. He showed me the specimen cases where I could find the reference material to compare with my specimens, including uncatalogued specimens collected by Jones, as part of his research in Nebraska. I was overwhelmed by all of the specimens—rows and rows of the same species lined neatly in specimen drawers. He handed me the copy of Jones' page proofs for "Mammals of Nebraska," and then excused himself.

While I looked at specimens, Margaret took notes from the page proofs of Jones book—soon to be published. For the first time ever, I had the opportunity to look at specimens of *Myotis nigricans*. Immediately, I knew that my "mystery" specimens had to be something else. As I had thought earlier, they were *Nycticeus humeralis*, although I had not seen any previous specimens of this species, and all of the published descriptions were of adults. The museum had only a few specimens, and they too were adults. However, after looking at their skulls and dentition, I became convinced that my mystery specimens were indeed those of immature *Nycticeus humeralis*. This discovery was exciting because some of my specimens were of juveniles—the first ever to have been collected in Nebraska, and my specimens represented range extensions for Nebraska. The fact that I had collected immature bats also suggested that this species was most likely breeding in the state.

Margaret was still taking notes when I had finished examining specimens, and although we did not have much more time, we still wanted the information on bats that Jones had included in his dissertation. My only recourse was to go to the Museum office and ask the secretary if we could copy the remaining pages with their Thermofax machine (an early predecessor of Xerox machines). I asked the secretary if we could make copies, and she said it was OK. As we were copying the section on bats, Dr. Hall walked into the office and gruffly asked what we were doing. I told him that we had run out of time and were copying the relevant pages from Jones' page proofs. As I looked at him, I could see his face get red, and he blurted out loud, "You can't do that, this material has not yet been published." He grabbed the copies from the Thermofax that we had made, and admonished me for making copies. I offered a humble apology.

I explained to Dr. Hall that after examining the museum specimens, I was convinced that my mystery specimens were indeed *Nycticeus humeralis*. He seemed pleased to learn this, especially since they represented an important range extension for this species in Nebraska, and the fact that the young bats provided evidence of successful reproduction in the State. He stated that "Dr. Jones would be very interested to learn of my discovery." Dr. Hall also mentioned that he was working on a revision of *Mammals of North America*, and that these new records would make important contributions if I published these range extensions. He encouraged me to deposit the specimens in the Museum at KU. In the final analysis, we seemed to part on good terms, although I did not have the copy of Jones' page proofs in hand, that I so badly wanted. Dr. Hall invited me to return to KU when Dr. Jones returned. Margaret and I were both struck by the fact that Drs. Hall and Jones both used the initials of their first name. As Margaret and I drove back to Lincoln, I remember her asking me, "do you think if you became a mammalogist that you will be expected to change your name to T. Henry Kunz." We both laughed in amusement.

I finished my research report in late summer 1964 and sent it to Dr. Thorson for his critique and a grade. He complemented me on my efforts and gave me an "A" for the paper and research course. Interestingly, Dr. Jones completed his undergraduate work at the University of Nebraska, where Dr. Thorson taught. It turned out that Dr. Thorson and Dr. Jones crossed paths again years later when they both did field work in Nicaragra.

In the fall of 1964, back at Shawnee Mission West High School, I wrote to Dr. Jones, and sent him a copy of my research paper. He wrote back and encouraged me to write up this report for publication, noting that my work had "made important contributions to understanding the distribution of bats in Nebraska." He indicated that he would be willing to work with me to get my paper published. I arranged a meeting with Dr. Jones later that fall. By the time I arrived for our meeting, he had made numerous comments and marks on my paper—with a red pen. I thought that my written report was pretty good (after all I had received an A), but his comments and suggestions were numerous. As we met, he went over each line with me, pointing out how I could improve my writing, "written the way we do it at KU."

Over the next several months, I revised the manuscript several times, taking his suggestions verbatum—after all he had published before and I was a mere neophyte. He suggested that I submit my paper to the Transactions of the Kansas Academy of Science. This journal was not at the forefront of science, but it frequently published natural history notes and range extensions of other species. I took his suggestion and this paper was published in 1965, entitled "Some notes on Nebraskan bats." This was my first publication. I remember the pride I felt in having this paper published and seeing my name in print for the first time--I was enthralled and highly motivated by this experience.

My Growing Confidence

In the winter of 1965, I applied for another NSF-sponsored Summer Institute in Biology—this time for a three-year program leading to a Masters degree in Biology at Drake University, in Des Moines, Iowa. I applied to Drake, in part because I reasoned that Margaret could continue her graduate studies there at the same time, if I were to be accepted. I was accepted and began taking graduate courses in the summer of 1965. Dr. Rodney Rogers was director of the NSF-sponsored Summer Institute. I told him that I would like to do my thesis research on bats. He was a parasitologist, and indicated that he could not advise me on bats, although he could advise me on collecting and preserving endoparasites, if I could collect them from bats that I shot. That summer I did collect parasites from the bats that I shot, and this effort later led to two of my early publications—one as a single-authored paper (Kunz, 1968), and another co-authored with John Ubelaker (Ubelaker and Kunz, 1971).

I stayed in contact with Knox Jones, and met with him on several occasions during the spring semester of 1965. He expressed interest in me working in Iowa, because there had been little recent work on small mammals, including bats, in that state—except for game mammals. I told him that I was more interested in ecology than systematics, and he encouraged me to pursue my interests. I admired the work that Dr. Jones had done on mammalian systematics, and greatly appreciated his advice and counsel, even though my research interests differed from his.

At one of our meetings, Knox handed me a paper that had just been published by Clyde Jones (no relationship), a recent Ph.D. graduate from the University of New Mexico, whose research advisor had been Dr. James Findley. Clyde's paper focused on activity patterns of bats in the Mogollan Mountains in southeastern New Mexico—one of the first papers to be published on the ecology of an assemblage of bats in North America based on mist-net captures (Jones, 1965). Most previous studies, in which mist nets had been used, reported activity of bats collected over water holes in desert regions, with most studies terminating a few hours after sunset (following the first pulse of activity). Clyde's paper was important for several reasons. First, he was the first to report captures of bats netted well after the first few hours after sunset, well into the night, with results suggesting that different species may have different foraging times, although his sampling efforts too suffered from not being conducted all night long. Secondly, it was the first study of an assemblage of temperate bats that was designed to ask ecological questions. Most previous reports on bat activity were antecdodal, and were collected secondarily to a focus on collecting museum specimens. From my perspective at the time, Clyde's paper set a new standard for research on North American bats, one that largely focused on ecology. I admired Clyde's work, although I did not have an opportunity to meet him until a few years later.

Along with Knox Jones, who inspired me to pursue my interests in bat ecology, I consider Clyde Jones as one of my academic heroes. Clyde's paper in 1965 was pivotal in my decision to conduct studies that raised questions about how bats partitioned available resources, both spatially and temporally. My thinking at the time was also influenced by research published in the early 1960's by Robert MacArthur (1958), Gerrit Hardin (1960), and Joseph Connell (1961), whose writings on competition and competitive exclusion were hallmarks of modern ecology at the time.

During my first summer at Drake University, I identified a study area (Ledges State Park) near Des Moines, Iowa, where bats were relatively abundant. I continued to use my father's trustworthy shotgun to collect bats at other sites where mist netting was impractical. Although mist nets had become increasingly used for ecological studies of bats in the early 1960's, little had been reported on how to deploy these nets in field situations. Two individuals were instrumental in demonstrating to me how best to deploy mist nets in the field. Dr. William B. Davis, Curator of Mammals at Texas A&M University, frequently advertised the sale of Japanese mist nets in the *Journal of Mammalogy*. In fact, my first nets were purchased from Dr. Davis for about $8.00 each. I remember writing to him asking how to set mist nets over streams, where I mostly wanted to collect bats. He sent me a hand written note (on the back of the letter I had sent to him), with sketches of how to set nets, using rocks at the base of poles for support and rope cords tied to the poles and to surrounding vegetation and other objects for support. Dr. Charles O. Handley, Jr., Curator of Mammals at the National Museum, Washington, D.C., also responded to my request for information about mist netting, illustrating how to set mist nets so they would have an appropriate amount of bag. My first net poles were made of bamboo fishing poles, and my first bat holding device was a collapsible wire minnow basket that I often suspended from tree branches near the nets I had deployed. Only later in my research did I use small cloth bags and other devices to hold live bats (see Kunz and Kurta, 1988).

I started my project in Iowa with three 6-meter mist nets that lasted me for almost two summers. Whenever a bat chewed a hole in the net, which they sometimes did, I repaired it with black nylon thread. I treasured and cared for these nets because they were the primary tools needed for my research. Periodically, my graduate student peers and my wife, Margaret, accompanied me on trips to help collect bats with mist nets. My father accompanied me on two occasions (in his latter years, he often talked about what a memorable experience this was for him). During that first summer, I focused on netting bats at two locations, one on Pease Creek in Ledges State Park, and another over the same creek that traversed private land, a short distance outside the park. Ultimately, I decided to focus my research efforts on netting at Ledges State Park, because the abundance of bats was greatest there—as determined from my captures, and the park provided a convenient place to camp. I recall many nights sitting on a cold rock next to the bank of Pease Creek, waiting for bats to hit the nets that I had set in strategic places, adjacent to rocky ledges and beneath the over hanging tree canopy. I recall that on the nights that I was accompanied by Margaret, I would ask her rhetorically "tell me how much fun you are having?" especially on those nights when the air temperatures were so cool that no bats were being caught. My rhetorical question was frequently answered with dead silence (sometimes she had fallen asleep!).

On nights when I did not net, I hunted with my shotgun in other areas south of Des Moines, mostly where stream banks were too steep and muddy to set nets. At one site on a small tributary to the Des Moines River, I remember seeing many bats feeding and drinking over the water and beneath the riparian forest canopy. I was frustrated that I could not use my nets at these sites. The streams had steep, muddy banks with deep silt in the bottom, which made it treacherous to navigate, especially at night and especially when I worked alone. At these sites, in the summer of 1965, I sometimes shot bats as they foraged along the forest edge. To my delight, among other species, I shot several adult and young *Nycticeus humeralis*. These were the first records of this species for Iowa. I published this record in the *Journal of Mammalogy* (Kunz, 1966), reporting a range extension over one hundred miles to the north and east of previously published records.

My success at shooting bats was only exceeded by my increasing ability to net bats. My success was achieved largely because I often changed the positions of nets (having on several occasions observed bats avoiding nets when they were set at the same locations on successive nights). Not only did I regularly change net positions, but I also frequently deployed two or three nets together at the same site, sometimes setting them in a "T" configuration, at times in a "V" configuration, and on other occasions in a stacked, but offset, position. The two species that I captured most frequently were big brown bats (*Eptesicus fuscus*) and eastern red bats (*Lasiurus borealis*). I was intrigued by red bats, and read all that I could on what was known about these beautiful animals. From my reading, I learned that much of what was known about lasiurine bats at that time was based on observations made in Iowa, by Elliot McClure in the early 1940's (McClure, 1942), and by Denny Constantine (a research scientist and veterinarian associated with the Center for Disease Control) in the mid-1960's (Constantine, 1966). I was greatly impressed by the observations of Drs. McClure and Constantine, because their work focused on roosting ecology—a topic that had become of increasing interest to me.

I continued to teach at Shawnee Mission West High School through the spring of 1967, and participated in the summer NSF Institute in Biology at Drake through the summer of 1967. I began corresponding with Dr. Constantine, because I was fascinated by his work. He had since shifted his research interest to Mexican free-tailed bats (*Tadarida brasiliensis*), conducting research on population biology and assessing the incidence of rabies in large colonies of this species in Texas and New Mexico. In response to a letter that I sent to Dr. Constantine, asking him about how he was able to find so many roosting red bats, he explained his technique of looking for bats along hedgerows adjacent to plowed fields. However, in one of his letters, he pointed out that he had injured his neck when looking for red bats (and at the time wore a neck brace), because of the whip-lashing that he experienced as he stumbled over clods of soil in plowed fields when his head was tilted backward looking for roosting bats.

MAKING THE COMMITMENT

I stayed in contact with Knox Jones, informing him of my results and discoveries in Iowa. He was very supportive of my work and interests, and encouraged me to apply for a Ph.D. at the University of Kansas. I applied in the spring of 1967 and was accepted for fall admission of that year. My Master's degree at Drake was officially awarded in the spring of 1968. The results of my Master's research were published as two papers, one in the American Midland Naturalist (Kunz, 1971a) and the other in the *Journal of Mammalogy* (Kunz, 1973a). I soon learned that Knox was a master at finding financial support for his graduate students. For me, he secured a 3-year Kansas Biological Survey Fellowship. The Kansas Biological Survey was directed by Professor Frank B. Cross, a noted Ichthyologist at KU. This fellowship supported my tuition, paid a monthly stipend, and paid for most of my travel expenses to field sites for three years. I had full access to one of the Museum vehicles, for survey work and for my own research. In addition to collecting information on bats throughout the state, I also assisted other graduate students by collecting fish and herps for the Biological Survey of Kansas. During my final year as a graduate student, I served as a teaching assistant in the newly named Department of Systematics and Ecology, where I assisted in Population Biology and Biometry.

During my first year as a graduate student at the University of Kansas, I took courses in Mammalogy, Endocrinology, Mammalian Physiology and Biometry, and prepared for the department qualifying exams—which at the time were administered to all incoming students at the beginning of the second semester following their matriculation. It was administered as an essay exam, and students either passed at the Master's level or the Ph.D. level. Happily, I passed at the Ph.D. level. Fellow members of my entering class that year who worked under the direction of Knox Jones, included David M. Armstrong (currently, Professor and Director of Natural History Museum, University of Colorado), Merlin Tuttle (currently, Founder and Director of Bat Conservation International), Paul Robertson (currently, Mammalogist for the Texas Parks and Wildlife), John B. Bowles (now retired, but a former Professor of Biology at Central College, Pella, Iowa), and Larry Watkins (currently owner of Watkins Natural History Books). Other graduate students in the entering class of 1967 included Sievert Rohwer (currently, Professor of Zoology and Curator of Ornithology, University of Washington), a student of Dr. Richard F. Johnston, and Jan Caldwell (currently Professor of Ecology and Evolution at UCLA) and Marty Crump (now retired, but a former Professor of Zoology at the University of Florida), both students of Dr. William Duellman in herpetology. We all seemed green and naïve at the time, but in retrospect, this was truly a distinguished class of students, all of who have become successful in their own right.

Knox also had several continuing graduate students at that time, each of whom preceded me by at least two years. These included the late Elmer C. Birney (former Professor of Ecology and Evolution and Curator of Mammals at the Bell Museum, University of Minnesota), Jerry Choate (currently, Professor of Biology and Director of the Sternberg Museum of Natural History, Fort Hays State University), Hugh Genoways (currently Director of Museum Studies, Nebraska State Museum), Carleton J. Phillips (currently, Assistant Vice President for Research, Texas Tech University), James Dale Smith (retired, but former Professor of Biology at California State University, Fullerton), and the late G. Lawrence Forman (former Professor of Biology Rockford College). I was Knox's last Ph.D. student at Kansas, finishing shortly before he left for Texas Tech University in the fall of 1971. I always found it ironic that Knox ultimately accepted a position at Texas Tech, because he often told his graduate students that "if you screw up one more time, I'm going to send you to Texas Tech."

I initially, and perhaps naively, wanted to do my Ph.D. research on the ecology of red bats. I spent the summer of 1968 looking for roosting red bats in Lawrence. Margaret also helped me look for red bats roosting in local peach orchards (early reports in the literature suggested that these were favored roost sites for red bats). We had moderate success in finding roosting red bats, but our best success was achieved by advertising in the Lawrence newspaper. From these ads, several red bats were reported to me by children, to whom I paid $5.00 for each family cluster. Sometimes these bats were found by children as they played beneath the canopy of trees in residential areas, but at other times, especially after storms, whole families of red and hoary bats were discovered on the ground after the mothers were unable to transport their large litters back to their tree roosts.

In another paper published by Clyde Jones, this time in the *Journal of Mammalogy* in 1967, he described the postnatal growth rates and development of captive *Nycticeus humeralis*. Few previous studies had reported on postnatal growth and development of North American bats, and his paper seemed like another one to emulate—albeit with another species. As part of a pilot study during the summer of 1968, I captured several individuals of four species of bats that were pregnant at the time, including *Lasionycteris noctivagans*, *Lasiurus borealis*, *Myotis grisescens*, and *Myotis velifer*, in the hopes that I could observe parturition and study the growth and development of pups, similar to the way Jones (1967) had described for *N. humeralis*.

For this study, I was assigned a room in what was affectionately known as the "Animal House," a facility that housed an office and lab space for Ed Bryant, a graduate student in the Department of Entomology, who studied population genetics of houseflies, and a lab room that housed Elmer Birney's woodrat colony. Ed was an advanced student of Robert Sokal (Sokal had left the previous year for a faculty position at the State University of New York at Stony Brook), and I naively thought that Ed's house fly colony would provide a sustainable source of food for my bats—only later to discover that bats would not eat either the larvae or the adults. It is quite likely that this animal facility would not have met the IACUC standards of today.

During the summer 1968, while taking a French reading course, to satisfy one of two language requirement for a Ph.D. at the University of Kansas, I also assumed the responsibility of feeding, weighing, and measuring over 30 pregnant bats, with the expectation that they would give birth to healthy pups and I would be able to record their normal growth rates and describe their development. My wife Margaret spent many hours helping feed these bats, while I studied French or was away on field trips. It turned out that some of the females aborted (mostly the *Lasiurus borealis*). This species did not seem to adjust well to captivity, as the bats were maintained in an 8' wide x 10' long x 7' high wire flight cage. As the bats attempted to fly, their wings often became injured on the wire cage, and probably become dehydrated from the wounds—despite my efforts to treat them with antibiotics and analgesics, and hydrate them by offering them water from an eye dropper. A few of the red bats gave birth, but none of the young survived beyond a few days.

We had our best success in captivity with *Myotis velifer*. Most of the females gave birth to full-term pups, but the females did not eat well enough to sustain normal growth rates of their pups. Compared to free-ranging bats, the pups of these and other captive bats seemed stunted. Even some of the *M. velifer* appeared to be stunted. Notwithstanding, one of the most important lessons I learned from these experiences was that the room temperature and humidity needed to be regulated if I was to successfully manage and rear captive bats. We ultimately used a vaporizer to boost the humidity, but unfortunately, we could not control the room temperature. At times it was too cold (and both mothers and pups became torpid—and did not eat), and at other times it became too hot and dry, especially for the foliage roosting *Lasiurus borealis* and the obligate cave-roosting species *Myotis grisescens*. We fed bats on *Tenebrio* (mealworm) larvae and pupae, and occasionally adult beetles. Because mothers could not produce sufficient quantities of milk with what they had eaten, we supplemented the diet of their pups by hand-feeding them with the insides of mealworm larvae, which we stripped from the exoskeleton with a forceps. The teeth of immature bats were not sufficiently developed to penetrate the tough exoskeleton of mature larvae.

The most valuable aspects of my pilot study on captive bats was that it provided me the opportunity to develop ways to handle, weigh, and measure bats in an efficient manner. I used these skills to my advantage during the next two years of fieldwork. Among other field studies, I began research on the growth and development of free ranging *Myotis velifer* and *Eptesicus fusus*. These experiences reinforced my desire to concentrate my Ph.D. research on field research, largely to avoid the potential biases associated with studies on animals housed in captivity.

Given the moderate success that I had in working with *Myotis velifer* in captivity, the research that had earlier been conducted on this species by Jack Twente (Twente, 1955), and an early autumn trip to the Gypsum Hills with Stan Roth to explore several bat colonies, I decided that this species and south-central Kansas would be ideally suited for my Ph.D. research. I began my fieldwork in earnest in the fall of 1968, first focusing on winter ecology and seasonal movements. My research during the next two years focused mostly on reproduction, growth and development (Kunz, 1973b; 1974a), and feeding ecology (Kunz, 1974b). In addition, as part of my work for the Biological Survey of Kansas, I conducted research on *Corynorhinus townsendii* (Kunz, 1975; Humphrey and Kunz, 1976). Interestingly, in the year that I began my field research on *M. velifer* in the Gypsum Hills of Kansas, Steve Humphrey, a Ph.D. Student at Oklahoma State University, under the direction of Dr. Brian Glass, had begun research on the same species in the adjacent Red Hills of north-central Oklahoma. In January of 1969, we each learned that we were conducting research on the same species, in adjacent regions of Kansas and Oklahoma, as we began to capture banded bats that we had not banded. When we independently received letters from the U.S. Fish and Wildlife Service, indicating whom had banded the bats, we put two and two together. I remember calling Steve, arranging to meet him to discuss areas of common interest and to possibly resolve a potential conflict. We first met in Oklahoma in late winter of 1969.

Because we both had been collecting data on *Corynorhinus townsendi*, we decided that we should collaborate on a study of this species, with Steve focusing on the maternity period and me on winter ecology. We also decided that the only potential areas of overlap in our proposed Ph.D. research was that we shared an interest in studying seasonal movements and population dynamics of *Myotis velifer*, based on mark-recapture data from the bats we had banded. Steve indicated that he had not planned to focus on reproduction, growth, development, or feeding ecology. Ultimately, Steve dropped his research on *M. velifer*, and decided to concentrate his efforts on *M. lucifugus*. Much of the data for the latter study had been collected when he was an undergraduate at Earlham College, under the mentorship of Professor Jim Cope. Ultimately, Steve completed his Ph.D. research on *M. lucifugus* in 1971 and published it in collaboration with Professor Cope (Humphrey and Cope, 1976).

From October 1968, I made regular visits to south-central Kansas through the summer of 1970 to study various aspects of the ecology of *M. velifer*. Because this species forms maternity colonies in caves and buildings, one of my primary goals was to compare the roosting ecology, reproductive phenology, and postnatal growth rates of this species from these two contrasting environments. I predicted that individuals in buildings should grow faster than those in caves, because the microclimate of caves was cooler and thus demanded more energy for maintenance than those that roosted in warm barns. I chose Lost Colony Cave as my primary cave site, and a barn near Wilmore, Kansas, as my primary building site to conduct my research on growth and development. I chose these two sites after visiting several others, ultimately choosing them because they both were large enough (ca. 10,000 bats), and the sites were structured in such a way that I could easily capture bats from their roosting substrates with little difficulty. Unfortunately, the most accessible cluster of bats in Lost Colony Cave moved to an inaccessible place in the cave on the second night after I began capturing, banding, and measuring newborn pups. This experience ultimately led me to abandon the idea of comparing growth of *M. velifer* that roosted in a cave and a building. Other caves and cave-like structures in the area, including the National Gypsum Mine, in Sun City, housed large maternity colonies, but the roosts at these sites were not accessible because most of the bats roosted in areas over standing water or were out of reach, high on the ceilings.

There were other potential maternity colonies in the area that could be compared, but they were all in barns or other similar structures. I then decided to compare growth rates of bats occupying different barns, where colony size would be the independent variable, assuming that growth rates of bats living in small colonies would not have the same energetic benefits (from clustering) as those that roosted in larger colonies. Thus, I predicted that young bats from small colonies would have slower growth rates—largely due to increased energy demands on the mothers. However, I too encountered a problem with this approach; many of the smaller colonies roosted in only one area in the barns, and thus were more easily disturbed than those roosting in barns were there were multiple roosting sites. I was disappointed to learn that newborn pups that I had marked and measured at several barns with small colonies had also been moved by their mothers to inaccessible areas within the same structure.

Perhaps the reason the pups at the barn at Wilmore were not moved by their mothers as much as they were at other sites was because the bats in this large maternity colony were dispersed into several major wooden crevices. In effect, I could sample one crevice without causing notable disturbance to an adjacent crevice. In the final analysis, the Wilmore Barn is where I collected most of my data on growth and development. Data on reproductive status, sex ratios, and colony size were collected at several barns, based on trapping and hand sampling at less frequent intervals than was required for assessing growth and development.

My research on reproductive phenology and feeding ecology of *M. velifer* was greatly facilitated by the development of a double-frame harp trap (Figure 11). As peers in graduate school, Merlin Tuttle and I had each built two double-frame aluminum traps in the winter of 1968, before Merlin published the design of this trap (Tuttle, 1974). We each used these traps for our Ph.D. research, mine on *Myotis velifer* and Merlin's on *Myotis grisescens*. This harp trap facilitated major advances in how bats could be studied,

Figure 11. Tom Kunz standing next to a harp trap in south-central Kansas (1969).

as large numbers of bats could be captured near or adjacent to the entrances to structures that housed large colonies, a feat that would not have been possible using mist nets. This harp trap made it possible for one person to efficiently capture and handle large numbers of bats.

Much of the research for my Ph.D. on reproduction, growth and development and on feeding ecology was based on captures with two double-frame harp traps. My Ph.D. research would not have been possible without the moral support of my mentor, J. Knox Jones, Jr., the financial support from the Kansas Biological Survey, directed by Frank B. Cross, and the assistance and constructive criticism of other members of my dissertation committee (Professors Robert Hoffmann, Kenneth Armitage, and Richard Johnston). I am also grateful to Professor George Byers, entomologist extrodinaire, who patiently sorted, counted, and identified the insect remains in stomach contents that I collected from a select number of bats. My greatest disappointment as a graduate student was that Knox Jones never accompanied me to the field, except for a collecting trip with others to the Long Pine Hills of eastern Montana. Several years later, he apologized to me, expressing regret that he had not been more actively engaged in my fieldwork.

Many other individuals, too numerous to mention here, assisted me when I was a graduate student. However, the two most notable field assistants were Larry Watkins (fellow graduate student) and Eric Runquist (at the time, a high school student at Lawrence, Kansas, who had been mentored by Stan Roth). I am eternally grateful for their assistance. I also greatly benefited from discussions about bat ecology with Merlin Tuttle, exchanging trials and tribulations about our respective field studies that were more or less conducted concurrently in different regions of the country.

At one point during my graduate career, Knox was mentoring 13 students (10 Ph.D. students and 3 Master's students). It is little wonder that he had limited time to spend with his graduate students in the field. His own research had a strong systematic focus, and field trips for him were mostly for collecting specimens for the museum. Most of my graduate-student peers conducted research based on museum collections, focusing in part on questions relating to systematics and evolution. Because my research focused on ecology, in which live animals were essential, it took a different focus from what my mentor had experienced or previously advised. After all, I was his first student whose primary interest was in ecology.

Notwithstanding, the advantage of being part of a graduate program in which there were many students, with varied interests, is that we each intellectually fed upon one another. An environment that encouraged independence was one of the hallmarks of the mammal group at KU. Although each graduate student had a singular focus on their own research—as it should be—each of us shared ideas and our own expertise as needed, and offered opinions on topics unrelated to our own work.

Some of my most memorable discussions about ecology and evolution were held with Elmer Birney, Paul Robertson, Merlin Tuttle, and Larry Watkins—whose intellectual interests were closest to my own. Professor Robert Hoffmann, appointed Associate Curator of Mammals in 1969, brought a new perspective to the Museum. Although he had a strong interest in biogeography and evolution, he also had a strong interest in population ecology of mammals. His appointment helped change the way many of us viewed mammalian biology at KU. Paul Robertson and Merlin Tuttle both switched from Knox Jones to Bob Hoffmann for their Ph.D's. I remained as Knox's student, although I greatly benefited from discussions on ecology and population biology with Bob. I especially remember the moral support that Knox and Bob provided before, during, and after my oral qualifying exam, and their attention to my dissertation.

I attended my first Annual Meeting of the American Society of Mammalogists in 1968, held at Colorado State University, in Fort Collins. At this meeting, I first met Mike Bogan and Don Wilson, who were graduate students at the University of New Mexico, Scott Altenbach, who was a graduate student at the Colorado State University, and Al Gardner, who was a graduate student at Louisiana State University. My two travel companions from KU were Larry Watkins and Larry Foreman. We did not have enough money to stay in a dormitory room on campus, so we all slept in sleeping bags in a small park just outside the city limits of Fort Collins. At this meeting, I remember being awed by just seeing and being introduced to several distinguished mammalogists whom I had only known from the literature (Syd Anderson, Rollin Baker, Jim Findley, Don Hoffmeister, Karl Koopman, Jim Layne, Randolph Peterson, Tracy Storer, Richard Van Gelder, Terry Vaughan, among others).

In June of 1970, I presented my first oral paper at the 50th Annual Meeting of the American Society of Mammalogists, hosted by Texas A&M, but convened in a local hotel in College Station (a meeting those of us who attended will never forget!). That year, Bob Hoffmann was my roommate. He must have listened to me practice my talk at least three times that week, once only an hour before I gave it for real. He was very patient and helpful. His encouragement and moral support at that time meant a lot to me—as it helped me develop the confidence that I needed to give other talks and seminars in the years to come.

LEARNING TO BALANCE COMPETING DEMANDS

I began to analyze my field data in the fall of 1970, and started writing drafts of several chapters in the winter of 1971. Knox was a master at word-smithing, and I owe it to him for helping me improve my writing, and instilling in me the confidence needed to write effectively. Margaret worked in the Department of Geography at KU as an administrative assistant, and had access to one of the first word processors that was commercially available—archaic by today's standards, but with her help, my dissertation was the first at KU to be produced on a word processor. During late winter and spring of 1971, in addition to having to write my dissertation, I had other things on my mind—namely to get a job. I applied for several faculty positions, mostly in the Mid-West (where I thought at the time I would spend the rest of my life), and was successful in getting a few early interviews (including Avila College, University of Indiana at Fort Wayne, and Michigan Tech). I also, somewhat reluctantly, applied for a position in Mammalogy and Ecology at Boston University. I was reluctant because I was not sure that I wanted to live on the East Coast (although at the time I had never been there). That spring, I tried to balance my time between preparing for and giving job seminars, and trying to finish my dissertation. In early April of 1971, I got a call from Richard Estes (a vertebrate paleontololgist), chairman of the Department of Biology Search Committee at Boston University, inviting me for an interview. I was excited! I had one week to prepare for my interview. I wanted to make a good impression, so I made additional slides for my seminar, although I had already given three job interviews in the previous two months, and could have used most of these. Nonetheless, I prepared for and delivered a more complete seminar than I have given previously.

THE BEGINNING OF MY CAREER AS A MAMMALOGIST

In phone conversations with Richard Estes, I learned that Arthur (Sandy) Echternacht was also on the Biology faculty at Boston University, and that he too was on the search committee. Sandy had been a former graduate student of William Duellman in herpetology at the KU, and had completed his Ph.D. in 1969. Unknown to me at the time, he had accepted a position at Boston University after he graduated. I called Sandy and made arrangements to stay with him during my interview in Boston. The interview went very well. The chairman, Dr. George Fulton, expressed considerable interest in me joining their department. Two weeks after my interview, I got a call and was offered the position, to begin in September 1971, to teach Ecology in the fall semester and Mammalogy in the spring semester. I could not have asked for a better opportunity.

This offer of a faculty position at Boston University was the motivation I needed to complete my dissertation. I spent most of the rest of the spring and summer, first handwriting and later typing drafts of my dissertation. Knox had already officially resigned from KU in the spring of 1971 and accepted a position at Texas Tech, although he was not expected to move his family to Lubbock until September of that year. During that spring and summer I gave Knox, and other members of my graduate committee, drafts of various chapters of my dissertation. Knox would take them with him as he periodically flew back and forth from Lawrence to Lubbock that summer—as he made the transition to his new position. On one occasion, he left one of my chapters in the pocket behind the seat in the plane—never to be seen again. Fortunately, he was working from copy, but this was disconcerting to me nonetheless.

I had spent endless hours crafting my chapters into what I felt was the best I could write. However, my early drafts were returned with lots of red ink, not so much for substance, but for style. That summer, I learned a lot about science writing from Knox, and other members of my graduate committee. Apart from substance, logic and organization were paramount to Knox. I finished correcting the final draft, and defended my dissertation on August 31, 1971 (Kunz, 1971b). Margaret was instrumental in getting me through four years of graduate school at Kansas, especially in the final leg of writing, by providing both moral and tangible support.

I was scheduled to give my first lecture in Ecology at Boston University on September 7, 1971. I knew that the drive to Boston would take three days. Margaret and I began packing our meager belongings, and we left Lawrence for Boston by car, arriving on September 3, to an empty apartment (the moving van did not arrive for two more weeks). We rented half of a duplex in the town of Newton, Massachusetts (a western suburb of Boston) for one year, moved to a roomier duplex in Needham (another western suburb of Boston) for two years, and, in 1974, we purchased our first home in Wellesley, Massachusetts, where we raised our family (Figure 12). The rest, they say, is history. In the fall of 2003, I will begin my 33rd year on the faculty in the Department of Biology at Boston University.

Figure 12. David (age 7), Pamela (age 10), Margaret, and Tom Kunz (1982).

In the Company of Giants

One of the last questions asked by my mentor, Knox Jones, during my Ph.D. oral exam was "Who is your academic great grandfather?" I certainly was aware that Dr. Hall was my academic grandfather, and I knew that he had been awarded his Ph.D. from the University of California at Berkeley, but I had no idea at the time about the identity of his mentor (my academic great grandfather). After what seemed like an endless period of silence, waiting for an answer, Knox challenged me to go find the answer on my own. Only later did I learn that he asked this same question to all of his graduate students on their orals—although apparently there was some kind of secret pact among his students not to divulge the answer. Certainly, I had never heard this topic discussed when I was preparing for the face-to-face encounter with my committee.

In the week following my oral exam, after a bit of sleuthing in the museum library, I learned that my academic great grandfather was Joseph P. Grinnell (Figure 13A), a distinguished ornithologist and mammalogist, educator, and the first curator of mammals at the Museum of Vertebrate Zoology at Berkeley—a position that he held for 31 years (Layne and Hoffmann, 1994). Grinnell was a "shy but energetic worker in the field," and best known for having established the scientific basis for the "spatial niche concept" (Grinnell, 1917, 1924). G. Evelyn Hutchinson (1978) wrote that Grinnell "was perhaps the greatest student of North American birds and mammals whom the continent has yet produced." Grinnell also played a major role in developing the field of conservation. He served as president of the American Society of Mammalogists from 1937-1938 (Layne and Hoffmann, 1994).

One of Grinnell's most distinguished graduate students was E. Raymond Hall (Figure 13B), who continued Grinnell's "dynasty" at Berkeley for 15 years, where he advised several of Grinnell's students upon Grinnell's death (Whitaker, 1994). Dr. Hall produced a number of excellent graduate students, first at Berkeley and later at Kansas. Many of these individuals established major graduate programs and museums (Layne and Hoffmann, 1994).

One of Dr. Hall's most important early publications was *Mammals of Nevada*. Upon his resignation from Berkeley in 1944, Dr. Hall was appointed Director of the Museum of Natural History at Kansas. He served as president of the American Society of Mammalogists from 1944 to 1946, and quickly established KU as one of the leading centers for mammalogy in North America, if not the world. He co-authored *Mammals of North America*, with Keith Kelson (1959), which was later revised and published by Hall (1981), shortly before his death in 1984.

One of Hall's most prominent graduate students at KU was J. Knox Jones, Jr. (Figure 13C). A native of Nebraska, Knox was awarded a B.A. degree from the University of Nebraska in 1951 and M.A. from KU in 1953. His graduate career was interrupted by military duty in Korea and Japan from 1953 to 1955. Following his return from the war, Knox continued his graduate education at Kansas, and was awarded a Ph.D. in 1962. In that same year, he was appointed to Assistant Professor of Zoology and Assistant Curator of Mammals, later rising through the ranks to Professor and Curator of Mammals in 1968 (Layne and Hoffmann, 1964). He resigned from his position of Curator of Mammals at KU in 1971, assuming teaching, curatorial, and administrative positions at Texas Tech University, until his untimely death in 1992. Knox also served as president of the American Society of Mammalogists from 1972-1974. He was a prolific writer, superb editor, and supportive mentor. He was the author of over 300 publications and editor of 13 books. He was the recipient of the three highest awards given by the American Society of Mammalogists: the C. Hart Merriam Award, H.H.T. Jackson Award, and Honorary Membership (Layne and Hoffmann, 1994).

In his distinguished career at KU and Texas Tech, Knox produced 15 Ph.D. students and 16 Master's students in mammalogy (Hoffmann and Layne, 1994). I was fortunate enough to have been one of his Ph.D. students (Figure 13D). As a mammalogist, I have conducted fieldwork in North America (Iowa, Kansas, Nebraska, New Hampshire, Oklahoma, Massachusetts, and Texas), Puerto Rico, India, Malaysia, and Ecuador (Figure 14). In this period, I have authored or co-authored over 170 publications (most of which have been on the ecology and behavior bats), and have edited or co-edited four books, including *Ecology of Bats* (Plenum Press, 1982), *Ecological and Behavioral Methods for the Study of Bats* (Smithsonian Institution Press, 1988), *Bat Ecology and Conservation* (with Paul Racey), and *Bat Ecology* (with Brock Fenton) (University of Chicago Press, 2003). A complete list of my publcations can be found on my website (www.bu.edu/cecb).

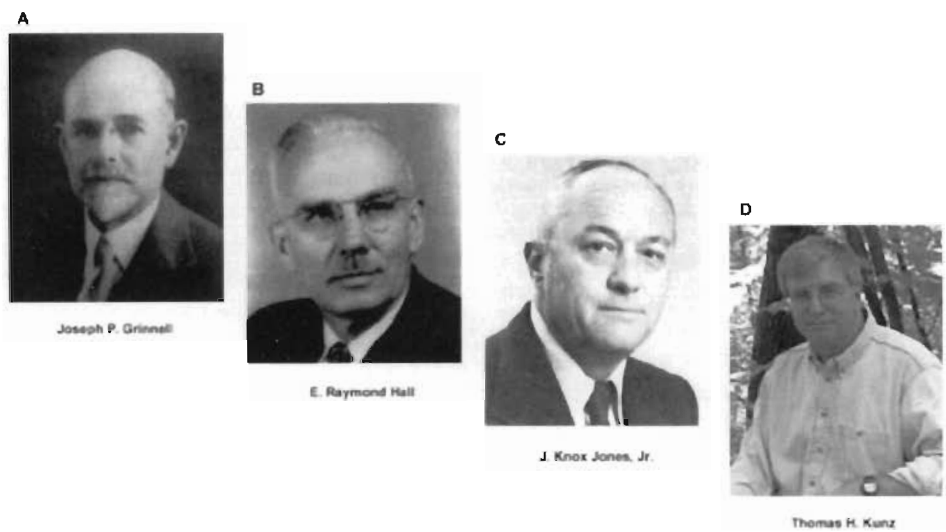

Figure 13. In the company of giants: (A) Joseph P. Grinnell; (B) E. Raymond Hall; (C) J. Knox Jones, Jr. (from Birney and Choate, 1994); (D) Thomas H. Kunz.

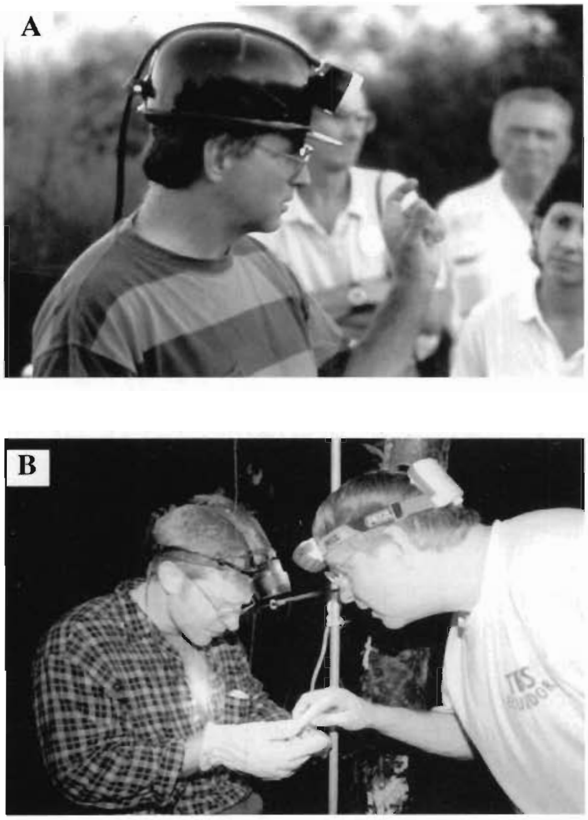

Figure 14. Tom Kunz (A) discussing his research with ecotourists in Texas (1987), and (B) assisting a graduate student at the Tiputini Biodiversity Station in Ecuador (2000).

On the Wings of Heroes

It is often stated that we must first learn to crawl before we can walk, and we must learn to walk before we can run. Figuratively speaking, we must also reach for the sky before we can fly. In the pursuit of a career in mammalogy, I have crawled, I have walked, I have run, and I have tried to reach for the sky. I have also fallen. My pathway to a career in mammalogy has been at times filled with bumps and turns, yet each time I have pulled myself up to try again. To achieve the highest level of success in any career, one must figuratively learn to fly. My career choice to become a mammalogist, has allowed me to fly—on the wings of heroes.

After arriving at Boston University in September 1971, I advanced from positions of assistant professor, associate professor, and full professor over a 14-year period—it was a journey that I have never regretted. In my 32 years on the faculty, I have been blessed with excellent undergraduate and graduate students, post-docs, and supportive colleagues. Along with funding from several sources, including the National Geographic Society and other non-government organizations, I have been fortunate in having been supported almost continuously by the National Science Foundation since I arrived at Boston University. I served as a department chairman (1985-1990), and I now serve as Director of Boston University's Center for Ecology and Conservation Biology (1996-present).

Over the years, I have been honored by my peers and colleagues, having been awarded the Gerrit S. Miller, Jr. Award in 1984, by the North American Symposium on Bat Research, elected as a Fellow in 1989 by the American Association for the Advancement of Science, and in 1999 awarded the C. Hart Merriam Award by the American Society of Mammalogists. I served as president of the American Society of Mammalogists from 2000 to 2002. In my research, I have been fortunate to have been associated with a terrific group of collaborators—to each I am eternally grateful. If there is anything that I have learned over my tenure in academia, the hallmark of success is teamwork (a lesson I learned as an athlete).

Before embarking on a professional career in mammalogy, I explored several other interests. As I reflect on these experiences, together they characterize my persona. Any success that I may have achieved over the years is a reflection of my relationships with teachers, coaches, friends, family, and colleagues, and a singular passion to try to do my very best. I chose a fruitful and enjoyable research path, focusing on the ecology and behavior of bats—a choice that I have never regretted. Who I am as a scientist, in large measure, can be attributed to fate, a bit of luck, hard work, and the profound influence and support of others. I am most grateful to my heroes, both real and imaginary, for providing me with the wings upon which to fly.

POSTSCRIPT

I have often been asked what traits I think are needed to be a successful biologist. My answer may not be unique, but they reflect my own experiences—guided and influenced by my heroes. I have listed these criteria in Table 1, not in any particular order, but as benchmarks to achieve along the way.

Table 1. Traits of a successful mammalogist.

Passion for organismal biology
Passion for reading
Passion for writing
Field and laboratory skills
Common sense
Patience
Perseverance
Enthusiasm
Commitment to do your best

ACKNOWLEDGMENTS

I am grateful to several individuals who helped me recall important events, dates, and names referred to in this paper. I am especially grateful to Dana Wendleberg Sutton, who was the source of important information about an early heroine (Alma Reed), and to Dick Butterfield who doggedly tracked me down to provide the name of one of my early heroes (Bob Cross). I am grateful to my wife Margaret, who has been a major influence on my life through her companionship, love, support, and encouragement. I too have enjoyed these relationships with our two children, Pamela and David, especially on field trips (when it was cool to do so). From these experiences, I hope to have conveyed the meaning of passion.

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MAMMALOGICAL REMINISCENCES

JIM FINDLEY

James S. Findley was born in Cleveland, Ohio on 28 December 28 1926. He received a B.A. in Biology with a minor in Romance languages from Western Reserve University in 1950, and a Ph.D. in Zoology in 1955 from the University of Kansas. He is Professor Emeritus of Biology at the University of New Mexico.

I was the only child of Dorothy Smith Findley and Howard Nevin Findley. I lived with my parents, then with just my mother after they divorced in 1931, in the household of my maternal grandparents, James Herbert Smith and Emma Schlosser Smith. My grandfather, known as Herbert, was born in Indianapolis, one of twelve children of an Irish immigrant couple. At an early age he came to Cleveland, and was unusual for his time in finishing high school. He studied the law on his own, and became a successful attorney and businessman. Emma, born in Cleveland, was one of two daughters of German immigrant parents who came as young people from the Darmstadt area. My father, a Clevelander, came from Scotch-Irish farming people in western Pennsylvania. Through his father, Frank Findley, dad traced our family to Connecticut in the 1630's.

The home was located in Cleveland Heights, a relatively affluent suburb on the east side of Cleveland. Our house was a large one, of three stories, located on a good-sized lot with sugar and red maples, beech, black cherry, elms, tulip trees, and several species of oak. Birds were common, and I remember with special pleasure wood thrushes, cardinals, robins, blue jays, chickadees, nuthatches, and many more. All the houses in our neighborhood were on large lots, and one had very much the feeling of living in a forest. Much of the suburban area of Cleveland was similarly forested, giving Cleveland its nickname of the Forest City. This tranquil, sylvan setting was the context in which my orientation toward, and love of, nature developed.

I was fascinated by animals, and always wanted to be outdoors, not unusual traits in a child, but I may have been somewhat obsessional about the animals. I've thought a lot about that, and after considering a variety of possibilities, including a certain amount of psycho-babble from well-meaning psychological friends, I've settled on an exceptionally concentrated genetic dose of biophilia as the most likely explanation for my strong orientation toward nature (Wilson, 1993). That orientation was indulged by my family, as I grew older, by honoring my request for "books about animals" when gift-giving times approached. Some of these works were truly excellent. Early on they were kid's books of course, some were stories, and others were actually texts as my reading ability allowed me to handle those things. I have retained many of those literary companions of my early years. An author emphasized in this library was Thornton W. Burgess, a children's writer on animals in the early part of the last century. A choice example is *Whitefoot the Wood Mouse* (Burgess, 1922). Burgess wrote a series of charming books about individual animals, *Reddy Fox, Johnny Chuck, Peter Cottontail, Danny Meadow Mouse*, and so on, in a sense a child's *Mammalian Species. The Burgess Animal Book for Children* actually covers North American mammals in a systematic way with factually solid text by Burgess and beautiful black-and-white and color illustrations by Louis Agassiz Fuertes (Burgess, 1935). At a more sophisticated level I find the 896 page *Natural History*, a multi-authored treatise on all the animal groups of the world, edited by Charles Tate Regan, a former Director of the British Museum (Natural History) (Tate Regan, 1937).

Then at Christmas, 1943, I was given a copy of the newly published first edition of Tracy Storer's classic text *General Zoology* (Storer, 1943) thus tying me firmly, though unknowingly at the time, into the scholarly tradition of the American Society of Mammalogists. I read most of this stuff, and retained a lot of it, and so had a tremendous store of information about animals when I started high school. No one in my family understood or shared my interest, but they were very tolerant and supportive, and of course they bought me books!

I met Norm Negus in the eighth grade and we quickly became fishing, and then collecting buddies. We collected butterflies and other insects with great enthusiasm, and took "bike hikes" eastward from Cleveland Heights into the countryside looking for choice specimens. The Negus family owned a small cabin in the forest on the Grand River in Ashtabula County, east of Cleveland. I was often their guest at this beautiful place, and Norm and I spent many happy days there enthusiastically pursuing our interest in fishing and nature.

I spent the ninth grade at a private boarding school, Western Reserve Academy at Hudson, Ohio. While I was there a scientist from the Cleveland Museum of Natural History, Arthur B. Williams, came to the school and gave an excellent presentation to our science class on mapping bird territories. Williams had been carrying out intensive studies of the beech-maple forests in the Cleveland Metropolitan Park system (Williams, 1936). That was perhaps the first time that I met an adult who spent his time in the woods studying nature.

From the tenth grade onward I attended Cleveland Heights High School where I received, or at least was exposed to, an excellent education. However my buddy Norm and I, together with several like-minded friends, spent as much time ditching school and trapping muskrats in a local park, as we did studying. Negus and I ended up in Auto Shop, the disciplinary homeroom. While there, we both took a biology course, and both of us got B's, the first time either of us had scored above a C in anything. The homeroom teacher, Mr. Weber, was so impressed that he devoted part of a homeroom period to praising us, and he lectured the class on how we had proven that even problem students could do well if they just tried. In counseling us, Mr. Weber asked what we intended to pursue as a profession. We told him we wanted to study animals. "Sounds like you want to do biological research" he responded. That sounded good to us, and from that time on "biological research" it was, if anyone asked us. At that point, though, we had little idea of what it was or how one actually made a living at it.

In 1942 or '43 Norm and I wandered together into the Cleveland Museum of Natural History, perused the exhibits, and decided that there were things they didn't have that we could probably supply. We volunteered our services, and were referred to the Preparation Department, then run by Arthur B. Fuller, a former student of taxidermy from the American Museum. We were given Saturday jobs at fifty cents an hour working on various exhibits that were in progress. We found ourselves surrounded by people whose chief interest in life seemed to be in plants and animals, and the conversations during work and during work breaks had to do with natural history. The experience had all the headiness of a dream fantasy, only this was a fantasy one could go back to at will, or at least every Saturday. Negus's mother balked at letting him work for fifty cents when he could earn seventy-five washing dishes at a local diner. My family was so overjoyed that I was doing something constructive and actually getting paid for it that my infatuation with the Museum had their total blessing. At some point I chanced to wander into the Mammalogy Department, and I remember the excitement I felt on seeing the trays of study skins of chipmunks, mice, and other small mammals, and in realizing that many of these things were found in the Cleveland region, and that there were adult people seriously engaged in their study. The adult people in this case were Benjamin P. "Pat" Bole, Jr., a member of a prominent Cleveland family, and Philip N. Moulthrop, a man then in his 30's, from a more modest background, who was a full-time employee of the Museum in the Mammalogy Department. I managed to get my Saturday job switched to Mammalogy, where I was occupied chiefly with cleaning skulls. In those days borax and arsenic were used in dusting both skins and skulls when mammals and birds were prepared, and the skulls would have been unpalatable for dermestids, but "bug" colonies were not in use in any case. Skulls were wrapped in cheesecloth and boiled, then picked free of flesh by hired minions such as myself. Tedious as this work was, it was an incredible introduction to cranial anatomy of mammals. Bole and Moulthrop, and indeed most of the people at the Cleveland Museum of Natural History, were amazingly tolerant of teen-age kids, and many of both sexes were employed in part time work. Although we worked diligently, lunch breaks were times when the leaders and us peasants sat around as equals and talked about all kinds of things, especially, in our case, vertebrate natural history. I have often felt that much of my basic knowledge in that area came from these bull sessions, along with the reading that I did on my own time. Certainly my basic knowledge of and enthusiasm for mammalogy came from that time. My first real activity in biology had been in ornithology. It was also well represented at the Cleveland Museum. The legendary systematic ornithologist Harry C. Oberholser was at that time Curator of Ornithology. I spent some time in the bird range, and learned to prepare skins, but Oberholser was a somewhat frightening personage for a teenager, although he was friendly enough to me. The Assistant Curator was W. Earl Godfrey, a younger man, (later Curator of Ornithology at the Canadian National Museum) and very accessible to a person of my status, and much of my initial feeling for professional ornithology came from interactions with him. I'm not certain, as I reflect upon it, why I stuck with mammals rather than veering toward birds.

Bole and Moulthrop had published a work on Ohio mammals (Bole and Moulthrop, 1942), and Bole, a former student at Harvard of Glover Allen's, had published a work on the quadrat method for censusing small mammals (Bole, 1939). I devoured these works, and, in the field with Moulthrop, learned the techniques used in those days in trapping forest small mammals in northeastern Ohio. I remember clearly, in 1944, walking into the small storage room in the mammal range where the *Journal of Mammalogy* was kept. I had never heard of this publication, and indeed, at that stage, didn't realize there were such things as scientific journals. When I leafed through several issues, and noted the large number of people, from all over, who were studying and publishing on mammals, it dawned on me that I might be able to do this same stuff, and possibly even earn a living at it somehow. I applied for membership in the ASM, and remember waiting anxiously to see if I would be accepted into the Society.

William J. Hamilton's *Mammals of the Eastern United States* served as our bible. We poured over the species accounts and tried to figure out where we might be able to get *Napaeozapus* and *Sorex*, magical words describing treasures hidden in the northern Ohio forests, that with diligence we might be able to find. Negus and I, together with high-school classmate George F. Ansley, began a mammal collection, soon designated the Findley-Ansley-Negus Collection, or FANC. After all, the Cleveland Museum was always abbreviated CMNH, so why not FANC? Ansley left to attend Goddard College in Plainfield, Vermont, and was soon sending home such exotics (for northern Ohio) as northern flying squirrels, snowshoe hares, and red-backed voles. Negus and I had our chance at northern mammals when we spent some weeks camping on Lake Eaton in the Adirondacks, and some of the mysterious animals from Hamilton's book began to fall into our traps. The high point came when we found a *Synaptomys* in a trap placed in an underground run in a pine grove. We jumped up and down yelling and cheering as though the war had ended or the Cleveland Indians had finally won a pennant. Through the generosity of my great-uncle, William P. Lewis, a traveling man, I was able to spend some time in Michigan's upper peninsula at a backwoods farm. There I had some simple chores involving milking, cranking a cream separator, and herding a couple of cows back and forth between the barn and a small pasture hewed out of the forest. This left plenty of time for setting mouse and rat traps. The maximum thrill of that trip came when I caught water shrews and star-nosed moles along a tannin-brown stream through the conifers.

While we were thus employed in learning the rudiments of a field biologist's craft, World War II was bearing inexorably upon us, and this happy period in our lives came to an end when Moulthrop died suddenly and unexpectedly of spinal meningitis contracted during field work in California, and Bole, Negus and I entered military service.

Returning from the Service in late 1946 I matriculated at Western Reserve University (now Case-Western Reserve), graduating in 1950.

In the summer of 1947 Negus and I attended Rocky Mountain Biological Lab located at Gothic, Colorado. There we took a course in ecology taught by a young professor at Brigham Young University, C. Lynn Hayward. Hayward led us over the montane communities of the region from the willow savannas of the East River valley to the high Alpine meadows. This was our first exposure to the Rocky Mountains and to the West in general, and I soon resolved that it was here that I had to end up. We took a mammalogy course taught by Frank Trembly, a prof from a small college in Pennsylvania. Trembly taught a good course, but his lack of familiarity with western mammals manifested itself on a field trip he led to the low country around Grand Junction. He had told us about pocket mice, and how they typified the xeric-adapted small mammals of desert regions. Our first morning in camp Trembly strode triumphantly in from checking his traps announcing that he had a pocket mouse to show us. We gathered around to view what Negus and I instantly recognized as a house mouse. We collected a lot of small mammals in the Gothic Region, and at Trembly's urging prepared a report that we submitted to the *Journal of Mammalogy*.

In the summers of 1948 and 1949 Negus and I had jobs at Jackson Hole Wildlife Park, located in Moran, Wyoming. We spent much of the time during both summers systematically running half-acre trapping quadrats in the various habitats in the region, again writing up our results in the form of a local faunal study and submitting them to the *Journal of Mammalogy*. During our stays in Jackson Hole we benefited from contacts with some outstanding scientists. Robert K. Enders, the famed mammalian reproductive biologist from Swarthmore College introduced us to his field in a fascinating way. Enders worked chiefly with smaller mammals, but on several occasions we had the opportunity to assist him in examining the reproductive systems of both black and grizzly bears. Robert Rausch, a parasitologist then at the University of Wisconsin, was a vigorous and convivial field companion from whom we learned methods of collecting endoparasites. Rausch was far more than a parasitologist, however, and has since made solid and significant contributions to our knowledge of Beringian mammals. While at Jackson Hole in 1949 we came upon a group of mammalogy students led by Rollin H. Baker of the University of Kansas. We visited the KU field party in camp at Two Ocean Lake. Baker later remarked that his favorable impression of the work we were doing was influential in my obtaining an assistantship at KU.

At Reserve I majored in biology, which involved traditional courses in zoology, genetics, vertebrate anatomy and embryology. The department was heavily under the influence of the Reserve med school that was across the street, most of the biology majors were pre-meds, and there was little encouragement to pursue an interest in biology for biology's sake. There was also an uninspiring ecology course, and one in the history of biology. A course that did affect my future career was cryptogamic botany taught by my old mentor, Pat Bole. A lot of the lab work involved microscopy, and, in some boredom, I found myself focusing the mirror of my scope on the lovely girl sitting across the table from me. Muriel Thomson (Tommie) and I were married in 1949. Bole gave us fifty dollars as a wedding present, about two months rent for a student couple at that time. But in summer 1949 Tommie and I lived rent-free at Jackson Hole, in an eight by ten wall tent, where one of our memories is of lying in bed watching the *Peromyscus* silhouetted by the full moon as they slid down the sloping roof of the tent.

In searching for a graduate school where I could pursue mammalogy I visited Ann Arbor and spent some time with Lee R. Dice. He was very courteous and showed me around his lab filled with caged *Peromyscus*, explaining some of his work in genetics. But I was still enthralled with collecting and systematics. I remember passing a worktable in the Museum where a preoccupied young man, Emmet Hooper, sat measuring *Reithrodontomys* skulls. I didn't get to meet Bill Burt on that trip. The University of Kansas offered me a teaching assistantship, and in August, 1950, Tommie and I packed our belongings, including a dog and a duck, into a large rickety baggage trailer and, with our infant son Stuart, headed for Lawrence, Kansas. There we moved into a small farmhouse seven miles from town on the dairy farm of the Wiggins family. For twenty-five dollars a month we got four rooms, a privy, a well with a bucket and pulley, electricity, and a kerosene space heater. The privy had the special feature that a slat was missing from the door allowing the dog and duck to stick their heads through and watch the person seated on the throne.

Zoology at KU was dominated by the personality of Professor E. Raymond Hall, who, in addition to being Curator of Mammals, was Director of the Natural History Museum and Chairman of the Zoology Department. Hall was a no-nonsense man who always wore a white shirt, necktie, and blue suit. He was quite strict in laying down rules for museum procedure, and the KU museum was the best organized and best run I've ever seen. The true Museum Conscience of Joseph Grinnell found a happy abode there. Indeed, Hall seemed to live with the constant concern that the standards established by Grinnell at Berkeley, and imported to Lawrence by Hall, might deteriorate were we not constantly vigilant. Hall was quite intimidating, did not encourage familiarity, and most of the students held him in awe. His wife, Mary, always referred to him as "Mr. Hall," and I never heard anyone call him by his first name. When Tommie and I were just settling into our farmhouse, however, Hall visited us, dressed casually in jeans, inquired solicitously after our welfare, and played with baby Stuart in a way that charmed us both. I never afterward was able to see him as a forbidding personage. We usually addressed him as "Doctor Hall," but he once explained to us that he preferred "Professor Hall," or "Mister" would be acceptable. His reasoning was that if we called him "Doctor" an outsider might think that he was (Horrors!) a physician. Of course "Professor" might give the impression that he took people on balloon rides at the state fair. "Mister" was nicely noncommittal, and still showed respect.

Fellow graduate students at KU included E. Lendell (Lendy) Cockrum , Phil Krutszch, John White, Bob Finley, Jim Bee, Dennis Rainey, Olin Webb, Bob Russell, Howard Stains, Syd Anderson, and Knox Jones. Rollin Baker was Assistant Curator, and served as some kind of intermediary between Hall and the grad students. Cockrum and White were completely awed by the program, but Krutszch had a great sense of humor and was less than fully respectful in the face of some of the pomposity and seriousness. Harrison B. (Bud) Tordoff, a student of Van Tyne's from University of Michigan, was just starting as Curator of Ornithology, and was a great friend and buffer against the system.

Probably the most important staff member for us mammalogy students during those years was Robert W. Wilson, the vertebrate paleontologist. Wilson taught a two-semester course in Vertebrate Paleontology, far and away the best classroom experience I had at KU, or most anywhere else for that matter. Wilson was a superb teacher and infected us all with great enthusiasm. Most importantly, he taught us basic mammalian phylogeny, classification and skeletal anatomy in a way that probably was not duplicated anywhere else, except possibly by Stirton at the University of California at Berkeley. Wilson was a thorough researcher, and resisted the pressure put on him by Hall to publish his results more frequently. Consequently he was never promoted to full professor. Long after I left he was put forward for an outstanding professorship, and the letters of support that came from many former students and colleagues awakened the KU administration to his worth, but by then it was too late. Wilson had accepted a position at South Dakota School of Mines in Rapid City.

My office mate during two years of my stay at Kansas was Bob Russell, from Gainesville, Texas, a former Texas Aggie who had come under the influence of William B. (Gopher Bill) Davis. Bob followed his mentor's enthusiasm and was pursuing doctoral studies on systematics of gophers of the genus *Pappogeomys* (then *Cratogeomys*). He was an excellent student, very respectful of the system, and one of those who stood in awe of Raymond Hall. One day Bob was measuring gophers in the mammal range, at a station close to Hall's office where the Old Man [Hall] was in full view, puffing his pipe and going through page proofs, when Ed Taylor, the famous herpetologist, emerged from the elevator. Hall and Taylor were bitter enemies, and Taylor was noted for his brusque, forthright, often profane manner. Spotting Russell, Taylor asked in a stentorian voice, "Mr. Russell, where is that son of a bitch?" Russell, shaken to the core at being thus caught between the two formidable persons, replied, "Er, which son of a bitch do you mean, Dr. Taylor?"

There was great interaction between the student group and most of the faculty, both in the academic setting and socially. It was a very egalitarian community, and a huge percentage of the educational transmission took place in this way rather than in the classroom.

Much important interaction also took place on field trips. One trip led by Baker featured an episode that marked Rollin for life. He had guided several of us, including myself and Knox Jones, to a site in western Nebraska where some surface water led to the establishment of a fish hatchery, and also some marshy land where we hoped to capture bog lemmings. But we were also doing general collecting, and one day as I wandered along a wooded arroyo looking for fox squirrels I flushed and shot a horned owl. Later that day we were sitting in the hatchery building preparing our specimens. I had just skinned the owl, and was sitting in my chair working with the skin, while the carcass lay on the skinning tray in front of me. At that point Rollin swept into the room, remarking that he was glad to see that we were all productively engaged. Spotting the owl carcass he remarked "Good! A pheasant! We'll have that for breakfast," whereupon he seized the bird and made off with it. We all looked at each other in puzzlement. Our professor was kidding, surely! But next morning as we stumbled out of our sacks we heard Baker's cheery voice "Come and get it you guys, pheasant for breakfast!" Unwilling to remonstrate with our leader, we sat down while Rollin plonked a chunk of fried owl in each of our plates. We were speechless, but each of us dutifully tried to eat his portion. I had a strip of leather-tough breast muscle. It still smelled faintly of methyl mercaptan, as the owl had evidently dined on skunk in the recent past. While the rest of us struggled with our breakfasts, Rollin consumed his owl with gusto, all the while remarking that nothing could compare with fresh pheasant. It was a mark of our awe and respect for established authority that we said nothing about the incident all day long, but every time we looked at each other we broke into hysterical laughter. Baker suspected something was up, and that evening in a small café while we were enroute home to Lawrence he finally said, "All right you birds, what's so damned funny?" After some hesitation Knox remarked "We thought it was damned funny that a renowned wildlife expert couldn't tell an owl from a pheasant!" Rollin refused to believe us, and remarked with finality that he has prepared and eaten dozens of pheasants. The next day someone told Hall, who spent parts of the rest of the day laughing uproariously in his office. A few days later the *Daily Kansan* featured as a headline **Professor Eats Owl!** It was nearing Thanksgiving, and someone brought an injured horned owl in to the Museum. Somehow the bird ended up in a cage on Baker's desk with a gift card stating "Thanksgiving dinner for the Bakers." Knox and I took to calling Rollin 'Hoot' after that, and the nickname stuck, one of our lesser contributions to the lore of mammalogy.

Knox Jones and I were close buddies during the two years that we overlapped at KU. We enjoyed collaboration on various small research projects, and after we had co-authored a few papers began calling each other "Co." Knox and I enjoyed the system and used it to our benefit. Our enthusiasm for research and publication earned Hall's approbation and respect. We respected him in return, at the same time having great fun imitating some of his behavioral and speech mannerisms, which were pronounced. One that was especially typical involved stroking his chin while exclaiming "Hmmm!" in a display intended to connote interest in what one was telling him. Knox imitated this to perfection, and soon everyone was doing it. Indeed it became almost a hallmark (no pun intended) of Hall's students of that era. In later years it came to our attention that Hall's major professor at Berkeley, the distinguished vertebrate biologist and ecologist Joseph Grinnell, had a pronounced goatee, and we could well imagine him constantly stroking it in a Hallian manner, perhaps to be imitated by his famous student. Here we may well be in the presence of an unusually clear example of cultural inheritance, extending over at least three academic generations.

The major action while I was Hall's student at Kansas was known as "The Navy Project." This involved the preparation of *The Mammals of North America*, which appeared in 1959 under the authorship of Hall and Keith Kelson. The enterprise was funded by the U.S. Office of Naval Research, hence the commonly used name. The funding was substantial, for the time, and it was not immediately evident why the Navy should be interested in paying for the preparation of maps and species accounts of North American mammals. The impetus and rationale came entirely from Hall. He had convinced the Navy that the threat of a biological warfare attack from the Russians through mammal-borne disease was imminent. We apprentices were treated to a demonstration of our professor's ability to awe not only students and staff members, but also U.S. Senators and high-ranking naval officers, when a group of these august individuals was assembled in the mammal range of the Museum to learn at first hand how Kansan mammalogists were laboring tirelessly to thwart the evil designs of the Soviets.

"Suppose, gentlemen," Hall began, "that a Soviet submarine surfaces off the arctic coast of Alaska. The Commander launches a small craft containing a couple of men carrying caged Siberian hares. These animals are infected with a particularly virulent and transmissible strain of shock disease. The hares are released and quickly come into contact with our native snowshoe hares, animals known to be very susceptible to this fatal malady. The infection spreads like wildfire. Alert biologists in Alaska and Canada detect the presence of the disease. But the question is, how to prevent it from spreading to the United States. Responsible military personnel seek our advice. Fortunately, in this case we are able to be helpful. Because we have been working tirelessly mapping the exact distribution of snowshoe hares in North America, we know that they enter the U.S. only along a narrow corridor in the Pacific Northwest.

(At this point Hall passes around copies of maps showing snowshoe hare range funneling down along the Cascade axis in Washington.) We are able to offer useful advice: Establish a hare-free zone across this narrow corridor! Stop the spread of infected animals into the United States! Unfortunately, gentlemen, I have to tell you that the snowshoe hare is one of the few American mammals for which we have the precise geographic information needed to deal with such a problem. There are hundreds of kinds of wild mammals in our country, and for only a few have we been able to prepare adequate maps. Our Soviet colleagues, however, are far ahead of us (at this point Hall brandishes a volume of S. I. Ognev's *The Mammals of the U.S.S.R. and Adjacent Countries* which began publication in 1948). If an attack came today, we could not tell you where to throw up blockades, but the Russians are ready for *us*!"

The apprehension among the group of VIPs was palpable. Knox muttered under his breath, "Save us, Professor Hall!"

None of the assembled dignitaries was able to see through the inaccuracies in the presentation, chief among which were the facts that shock disease is not caused by disease organisms, but probably by adreno-pituitary exhaustion brought on by high population density, and that there are many places where snowshoe hares enter the U.S. along broad fronts. But maybe the general idea had some merit. In any event, the funds kept flowing, and I, and many others who had research assistantships funded by the Navy Project, were helped in completing our graduate studies. And the result of the project was a work that is still the chief source of basic information on distribution and systematics of North American mammals (Hall and Kelson 1959).

My coursework at KU included 2 semesters of invertebrate zoology, a marine biology course at Pacific Grove, 2 semesters of vertebrate paleontology, systematic botany, speciation, bibliographic techniques, zoogeography, vertebrate natural history, ichthyology, herpetology, biology of the endocrines, ecology, entomology, stratigraphy, micropaleontology, invertebrate paleontology, speciation, statistics, and German. I was required to pass reading exams in German and French. From the standpoint of one hoping to become a professional mammalogist, the vertebrate paleo courses, taught by Wilson, were the most useful, hands down. From the conceptual standpoint the speciation course was a standout. We used Mayr's *Systematics and the Origin of Species* (Mayr 1942) and Dobzhansky's *Genetics and the Origin of Species* (Dobzhansky 1937) as texts, and those two outstanding works helped many of us imbibe the New Systematics paradigm that guided much systematic thinking at that time. From the standpoint of one required to spend time teaching in a biology department, all the rest was, of course, useful, though most of the material I used in teaching a variety of courses at the University of New Mexico I learned on my own before, during, or after graduate school. Hall's preoccupation was taxonomic research and the publication of research results. Everything else was oriented toward that goal. He taught us by example, not through formal pedagogical channels. It didn't take long to get the idea. As aspiring mammalogists we were out there to study mammals, and that study wasn't completed until the results were written up and published so others could make use of them. If you weren't following this protocol, perhaps you should go somewhere else, maybe the College of Education. Some students thrived on this, and others spent a fair amount of time in various passive resistance modes. Hall rarely entered a classroom, and on the few occasions that he did, in the team-taught vertebrate zoology course, he was less than inspiring.

I was hired at UNM as a mammalogist, and was expected to teach mammalogy and to establish a graduate program in the field (Figure 1). I think it fair to say that, except for the vert paleo courses and a beginning geology course I had at Reserve, no coursework that I ever had at KU or Western Reserve was very useful in preparing a course in mammalogy. The knowledge I had and used in that course came from association with mammalogists at the Cleveland Museum and at KU and from independent reading and research. Except for the Gothic mammalogy class with Trembly, I never had a formal mammalogy course, and had no preconceived ideas about how one should be organized. Because of my personal biases I organized my course along evolutionary and systematic lines, and put a lot of emphasis on field work, collecting, identification, understanding of the basis of mammalian classification, and recognition of museum material of major systematic categories. To me, that was basic mammalogy. Research that I pursued, and that I encouraged students to pursue, related at first chiefly to distributional and systematic questions.

The Government of the United States of America largely paid for my higher education, from enrollment in Western Reserve University through graduation from the University of Kansas. As a veteran of World War II I was entitled to the benefits of the "G.I. Bill of Rights." That program paid all my tuition, a book allowance, and, after Tommie and I married, a monthly stipend, which was increased with the arrival of each child. I might have figured out a way to get through the eight years of university work without that help, or I might well have been sidetracked by economic necessity. In any event, the timely completion of my studies resulted in large part from the generosity of our government toward war veterans.

As a professional scientist I've done two main things: pursued research and scholarship, and been a teacher. I never intended to be a teacher. That was not something I wanted to do. I've had no training in education, except in the U. S. Army where the dictum was: "1. Tell 'em you're going to tell 'em. 2. Tell 'em. 3. Tell 'em you told 'em." I must confess to a certain lingering disdain for teaching as a profession, and still believe that, at the university level, those who can do, and those who can't teach. A university is supposed to be a collection of professionals (admittedly a somewhat archaic definition in the twenty-first century). Part of the responsibility of the professional, whether an engineer, a writer, or a mammalogist, is to take on and mentor apprentices. I have been exceedingly fortunate in the quality of the people who chose to pursue their apprenticeships under my supervision. I'd like to think that the quality of my personality and scholarship attracted those good people, but to be realistic I have to admit that I played a relatively small part in such things as the seemingly spontaneous appearance of Don Wilson, Mike Mares, Art Dunham, and Donna Howell as students in my Comparative Vertebrate Anatomy class one semester. In trying to account for this kind of luck I take comfort in the language of self-organization theorists: "When information and control are distributed among many interact-

Figure 1. Two skunks and a young mammalogist. Jim Findley (center) lectures on coat patterns.

ing agents, organization can seem to arise spontaneously from disorder." (Pepper and Hoelzer 2001). The pleasure I took in working with my students, in enjoying our continuing friendships, and in watching the development of their careers has equaled or exceeded the satisfaction I derived from my personal scholarly activities. Without question those students constitute my important contribution to mammalogy.

As to scholarship, I have authored or coauthored a hundred works, most appearing in refereed outlets, many in the *Journal of Mammalogy*. Eighty-five of these dealt with mammals. The rest involved other organisms, or other subjects. I initiated and carried out the work that led to these pubs because that was what I wanted to do in life. There is a lot of joy in doing research, especially (for me) in the field, and tremendous intellectual satisfaction in pursuing and finding the answers to questions about nature. It's nice to look back on it and think about it, but the play's always been the thing.

A lot of the work that I have done can be described as reportage. Records of occurrence, descriptions of things, and faunal lists fall into this category. These things perhaps constitute the bread and butter of the field biologist's work, and are the building blocks of inductive science. By my reckoning 42 of the 100 papers fall into this category. Many were one page notes reporting an extension of the known range of something. Some were more extensive reports on the mammals, Recent or fossil, found in some area. I regard all of these things as worthwhile, though miniscule, contributions to the construction of a bigger picture of some aspect of mammalian biology. Gathering the information for all these things was enjoyable, but I remember several with special pleasure. Norm Negus and I collaborated on two papers reporting on fieldwork in the Gothic region of Colorado and in Jackson Hole, Wyoming, and I can't look at either without a vicarious re-run of the sheer joy of those three summers. Dick VanGelder helped me move some belongings from Lawrence, Kansas to Vermillion, South Dakota, where I had my first job out of graduate school. Enroute to Vermillion we assured each other with great bravado that when a couple of world-class mammalogists like us hit the state we would instantly come upon something new to science. Sure enough, we trapped eleven blarinas near Vermillion, the first known specimens from the state, and naturally we reported this find in one of those great one-page notes, one that has always reminded me of what fun it was to be Dick's colleague when we were both at KU.

In the course of the inductive process, isolated observations eventually fall together into patterns, which may be suggestive of a way in which nature is organized. Some works deal with synthesis, and recognition and characterization of these patterns. I judge thirty-two of my 100 papers to fall into this category. Most of these were done with other people, mostly with some of my graduate students at the University of New Mexico, Jim Sands, Clyde Jones, Art Harris, Jerry Traut, Don Wilson, Bill Caire, Mike Bogan, Dan Williams, and Neil Weber. Often these studies were based upon thesis research carried out by these investigators, or upon data gathered in the course of NSF-supported fieldwork aimed at producing *The Mammals of New Mexico* (Findley et al., 1975). That much-used work exemplifies the synthetic efforts in which I was involved, and I regard it as one of my more useful contributions to mainstream mammalogy. Knox Jones complained that it didn't have an index.

In the late '60s I became increasingly interested in bats, and especially in the genus *Myotis* that is well represented in the Southwest. That was a time when the potential of numerical methods and computers as tools in systematic research was being championed (Sokal and Sneath 1963), and I was intrigued with the possibility of applying those procedures to an entire worldwide genus. To that end I sought for and received NSF support for a several year study of *Myotis*. There were several worthwhile results from that project. First, the paper on phenetic relationships in the genus *Myotis* (Findley 1972) must have been acceptable, inasmuch as Karl Koopman, who generally poo-pooed doing systematics with a computer or even with the aid of statistics for that matter, told me that he thought it was "an important basic contribution," a substantial accolade, as those who knew Karl will appreciate. Secondly, the project enabled me to offer some support to Don Wilson who undertook the study of a tropical *Myotis*, *M. nigricans*, as a dissertation project (Wilson, 1971). Wilson in turn facilitated some visits to Central America for his revered major professor (me) that opened up for me new vistas in tropical biology.

My dissertation on *Sorex vagrans* (= *S. monticolus*) was a relatively straightforward systematic study (Findley 1955). But, as one steeped in the Modern Synthesis and the writings of Ernst Mayr, I attempted to apply the *Rassenkreis* hypothesis to the shrews, suggesting that in the Pacific Northwest the ends of a great circle of races overlapped in the form of the nominate species *S. vagrans* and *S. obscurus*. Hall thought the paper was hot stuff, characterizing it in a newspaper interview as a "bonanza," but the idea of conspecificity of *vagrans* and *obscurus* was resoundingly disproven by Darwin Hennings and Bob Hoffmann (Hennings and Hoffmann, 1977). As with many controversial presentations, though, the '55 study did stimulate a lot of useful study of western *Sorex*. One point from the dissertation, the proposal and characterization of the subgenus *Otisorex* DeKay, seems to have withstood the test of time. Ongoing study of this complex using molecular markers is forming a more durable picture of the phylogeography of these shrews (Demboski and Cook, 2001).

Ideally the end result of the inductive process is the emergence of hypotheses and theories that attempt to explain the perceived patterns, and then the testing of these ideas against new data and through experimentation. Fifteen of my published efforts fall somewhere in this ballpark.

The data that Negus and I gathered from running half-acre quadrats in Jackson Hole impressed upon me that the several species of *Microtus* that we captured were using different habitats. I summarized those findings in a short paper that appeared in 1951 (Findley, 1951). Then, while at KU, I got the notion that competition might be a factor in causing the habitat allocation that I observed. I wrote suggesting that possibility, noting that *M. pennsylvanicus* was found in a greater range of habitats in the absence of *M. montanus* and *M. ochrogaster* than it was when one or both of those species was present (Findley, 1954). I didn't pursue that line of thought at the time, but Peter Grant later singled out those two papers as marking "the starting point for the recent interest in competition among small mammals" (Grant, 1978).

I was mapping mammal distributions in New Mexico, while Robert MacArthur was mapping habitat use by wood warblers in New England (MacArthur, 1958) and Evelyn Hutchinson was contemplating beetle diversity in a pond in Italy (Hutchinson, 1959). I had been vaguely aware of these developments, but Hutchinson-MacArthur ecology came to UNM with a bang in the early '70s in the person of Mike Rosenzweig, whose advent was heralded by a paper in the *Journal of Mammalogy* (Rosenzweig, 1966). That brilliant, stimulating, and enthusiastic person opened my eyes, and those of many others at UNM and elsewhere, to the exciting world of community research, and to the role of theory and quantitative analyses therein. Energized by this excitement I suggested ways of applying multivariate techniques, learned from doing numerical taxonomy with *Myotis*, to the measurement of faunal diversity, and to niche dimensions and packing. The resulting paper (Findley, 1973) elicited more reprint requests than I had ever received before, or since, for that matter. I pursued this idea, applying the techniques to five bat assemblages from different parts of the world, noting similarities and differences between New and Old Worlds, and between temperate and tropical communities (Findley, 1976). My contention was that the multidimensional phenome of the animals was essentially a cast of the environmental mold, and that questions about community structure could be addressed through multivariate morphological studies (Findley and Wilson, 1982). An opportunity to test the correspondence between morphological and ecological structure of bat assemblages came when Hal Black was able to make a significant collection of bats from a cave in Zambia, East Africa. John Whittaker analyzed the stomachs of the fluid-preserved specimens (Whittaker and Black, 1976), and the bats themselves were deposited in the Museum of Southwestern Biology where I was able to undertake a morphological analysis. Hal and I essentially compared the dietary and morphological structures of that bat community and were able to show strong relationships between the two datasets (Findley and Black, 1983). On the basis of that study we devised a graphic model of community structure that I believe realistically depicts the ecological relationships of most communities of related organisms.

Related animals in community assemblages may be placed randomly in morphospace with respect to one another if interactive community processes are not dictating the configuration of the assemblage. Even if the community is strongly interactive, the arrangement in morphospace may appear random if there is little or no relationship between eco- and morphospace.

But on the heels of the work that Hal and I did on the Zambian bats, I was beginning to believe in the morphospace-ecospace correspondence. To test this hypothesis further I conducted morphospace analyses of a series of rodent communities in the Southwest (Findley, 1989). By comparing the morphospacing in these communities with spacing in randomly generated communities, I was able to show that in the real communities the animals are more distant from one another than they are in the simulated assemblages, a result to be expected if biotic interactions are affecting structure.

An opportunity to develop these ideas further came when I received an invitation from John Birks of Cambridge University Press to write a book on bat communities. That forced me to look critically at world bat faunas, and gave me an opportunity for still more adventures in morphospace. I concluded that, despite much evidence supporting the role of biotic interactions, when all bats in an assemblage are considered together without attention to trophic orientation, geographic and historic factors, not routinely considered by community ecologists, have exercised strong control over the evolution and structure of local communities (Findley, 1993).

I consider the suite of papers beginning with the numerical taxonomic study of *Myotis* and ending with the bat community book to be my most important scholarly contributions based upon work with mammals.

I was immersed in community questions by this time, and bats were not ideal subjects for that study. It is far from clear that the suite of bat species one may capture in a net at a given site are actually living in a potentially interactive community, where processes such as competition may work in influencing community structure. Gathering good data on the interactions of bat species seemed a labor intensive and inefficient activity, if the goal was to understand the workings of a whole community. I wished to pursue questions about communities, but became reluctant to continue using bats as model organisms. A new direction suggested itself through the confluence of several stochastic events. I was eligible for a sabbatical leave for academic year 1981-82. While I was deliberating the best use to make of that opportunity, Tommie read in *Mother Earth News* of a tour to French Polynesia guided by an old KU classmate, Paul Ehrlich. Field trips to look at reef fishes were to be part of the activity, and Tommie wondered why I couldn't study reef fishes and go along with Paul. It didn't sound like a bad idea. Paul, together with his wife and several others, had published an intriguing paper on community structure of coral reef fishes, chiefly butterflyfishes, based on work carried out at Lizard Island, Great Barrier Reef (Anderson et al., 1981). These colorful diurnal fishes are common on coral reefs in many parts of the world, and exhibit a cline in species richness from a few kinds coexisting on a single site in the Caribbean to thirty or more in parts of the western Pacific and southeast Asia. It seemed natural to ask what changes took place in community structure as richness increased by this order of magnitude. Does the whole community simply get larger? Do the numbers of each species decline as richness increases, as one might expect if resources were limiting the community and competition was in operation? We didn't go with the Ehrlichs, but we did design a butterflyfish study, and set forth to French Polynesia in November, 1981, to begin our work. Learning butterflyfish identification was easy, as easy or easier than learning spring wood warblers in the East. Between 1981 and 1997 we snorkeled about 204 kilometers on census routes at 36 islands or coastal stations around the tropical world, counting 46000 individual butterflyfishes belonging to 60 species, reporting our results in three papers (Findley and Findley, 1985, 1989, 2001). Butterflyfishes do become more abundant as richness rises, but the rate of increase in abundance declines, so that individual species are less abundant in the richest places and more abundant in the poorer regions. There is thus some evidence that a limit to richness might be reached, but the factor accounting for most of the variance in community richness is richness of the regional fauna. Thus, as in bats, history and geography exert overriding control of community structure.

To be sure, fishes are not mammals, and our fish work cannot be considered a contribution to mammalogy. The intellectual evolution from an interest in organisms to one in pattern and process is a common one, however, and it has affected many good mammalogists. After all, Joseph Grinnell, the academic grandparent or great-grandparent of many contemporary New World mammalogists, may be best known for his studies of a bird and of its niche relationships (Grinnell, 1917).

Mammalogy is an organism-centered discipline. The American Society of Mammalogists has always been populated by people whose first love is for the animals and their ways, and for whom concepts and the scientific method were things that came along a little later. In response to a question as to why he was a mammalogist, the great William J. Hamilton, Jr. responded, "I just like fuzzy little mice!"

Inevitably, many who start out as mainstream mammalogists become intrigued by patterns, processes, and concepts. But there will always be a need for an American Society of Mammalogists and its publication series. There will always, while biophilia genes survive, be people who get into science because they like fuzzy mice, or beautiful carnivores, or mysterious bats. That will not change. Concepts and tools and intellectual fashions change, but there will always be mammalogists. There has been no decline in submissions to the *Journal of Mammalogy*. The Society provides an intellectual, and indeed a social and spiritual base for those scientists who, though working also in other disciplines, regard the ASM as home. I think that this home-base role of the Society is a vital one. It's still possible for a good hard-working mammalogist to feel welcome in our Society and to get good papers published there. From our ranks have risen, and will continue to rise, outstanding scientists whose work fertilizes and enriches a diversity of conceptual fields of endeavor.

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YOU HAVE TO CATCH THEM FIRST

Clyde Jones

Clyde Jones was born in Scottsbluff, Nebraska, on 3 March 1935. He received a B.S. in Science and Education in 1957 from Hastings College, Nebraska, and an M.A. and Ph.D. from The University of New Mexico, Albuquerque, in 1961 and 1964, respectively. He is a Paul Whitfield Horn Professor Emeritus of Biological Sciences at Texas Tech University.

I was born on 3 March 1935 in Scottsbluff, Nebraska, the second son of Leona S. Hall and John W. Jones. The family had a small agribusiness a few miles away near Lyman, Nebraska. The lingering traumas of World War I, in which my father served in Europe, the Great Depression, and the Dustbowl were too much; my parents divorced. My father remarried, my mother did not; both parents died in 1973.

My mother, a former school teacher, decided not to reenter that profession. Instead, she entered the cattle business with her brother, Robert W. Hall, her sister, Mary F. Hall, and her mother, Grace H. Hall, who had been widowed in 1930. My mother, brother (born in 1926), and I moved to the family ranch in the edge of the Sandhills, ten miles north of Burwell, Nebraska. Three additional uncles and two other aunts lived nearby on farms and ranches; my home range was rather small. Although I was surrounded constantly by older people (my mother was 42 when I was born), I had a wonderful childhood. When I was three or four and one of my uncles was working nearby, within sight of my mother, I was allowed to walk out and hitch a ride back (Figure 1). Looking back, I have come to realize that I was cared for, trained, and protected by the aunts and uncles, as well as by my mother, especially after my brother (Figure 2) was killed in 1945 during the battle for Iwo Jima. Although they were discussed only infrequently in front of me, the family never got over World War I, the Great Depression, the Dustbowl, and World War II.

While growing up on the family ranch in the Sandhills of Nebraska, working with mammals was the livelihood and the life style. In addition to the attention directed toward the livestock (cattle, horses), some other mammals were of interest. For example, at an early age, I was given six Victor gopher traps and instructed to remove pocket gophers from the hay fields because the mounds of sands were damaging to the cutting bars of the mowing machines. Inasmuch as I was not strong enough to set the traps, uncle Bob provided a clamping device for my use in this function. For each pocket gopher removed, and the mounds flattened, I was paid ten cents. Of course, I did not realize it at the time, but I was deriving an income from a kind of mammalogical activity. Another of my uncles was an amateur taxidermist. I was somewhat enthralled with observing him prepare mammals, as well as accompanying him to either trap or shoot mammals on the family properties.

Prior to the development of the Rural Electric System in the area, and of course, long before television, the battery powered radios were devoted mostly to information about the weather and the livestock markets. Therefore, especially during the long winter evenings, we read magazines and catalogues, but the

FIGURE 1. Holding on to the hames, 1938.

Figure 2. The last reunion, 1944 (left to right, Leona, Carlyle, and Clyde).

I was enrolled in the first grade in a country, one-room schoolhouse about two miles from the ranch headquarters where we lived. My brother was in the eighth grade. We carried our lunches (Figure 3), which consisted usually of potato or bean sandwiches, occasionally beef, a homemade cookie or a sweetroll. The school grounds included the school, a barn, an outside toilet, a merry-go-round, and a slide. Most of the time we rode our horses across the north pasture, through a gate constructed for our purposes, and on to school. Sometimes we either rode or walked along the state highway and adjoining county road. And, occasionally we were either driven by our mother or we were picked up by the teacher, who commuted from Burwell. Inasmuch as we were usually the first to arrive at the school, we would load the heater with wood and start the fire. One day, someone had swiped my favorite pencil. When I tried to borrow one from my brother, he stuck his tongue out at me. The teacher saw him; he had to stand in front of the room with his tongue

Figure 3. Country school days, 1940 (left to right, Clyde and Carlyle).

emphasis on reading was directed mostly to materials housed in the small family library. My mother established the practice of reporting periodically on what we had read. Some of my favorite books from those early years are listed in Appendix 1. In addition, my mother would write stories about certain events at the ranch. Below are some quotations from one of my favorites, entitled "The Storm."

> "Tuesday afternoon it was rather showery off and on around the horizon, and about 6:30 several showers gathered in the west and northwest and kept getting closer and closer. Black wind clouds tumbled and rolled out north and we kept watching it. It appeared to be a heavy shower up north, with clouds moving to the west. We (Clyde and I) went into the house. Hail about 2 1/2 inches in diameter, some smooth and round, some jagged and rough, began falling, breaking all windows on the north...Every animal tried to get under cover."

out during the lunch time. He was kept after school, and I rode hard for home. He caught me at the gate to our property; I got mine on the way home. My mother rented a small house in Burwell so that we could attend school. I was enrolled in the second grade. About midway through the academic year, I was moved up to the third grade. Again about midway through the fourth grade, I was moved to the fifth grade, I was enrolled in classes in both grades for a time. Although these actions were somewhat traumatic for me, they were testimony for the times of reading and doing mathematical problems given to me by my private tutors, my mother and her two sisters. Living in town during the week and spending every weekend and holiday on the ranch was the norm for the people in that place and time.

On the ranch, everybody worked every day except on Sunday. Sundays were devoted mostly to visiting family and friends, attending a local rodeo, working in the shop on machinery, or just doing nothing. On many Sundays during the summer, I would ride my horse the one to two miles to the hay meadow, set out the gopher traps, and ride to a secluded valley, let the horse graze, and lay on my back listening to the breezes in the grass. Sometimes uncle Bob would join me, and we would talk about how the area must have been when the Sioux were here prior to the settlement by the likes of us. I was unaware at the time that years later I would become the owner of this beautiful, mostly unspoiled piece of sandhills property.

Other than football, basketball, and track, high school was not of great interest to me. For the guys that I associated with, you had to have a horse, reasonable tack (Figures 4 and 5), and subscribe to the Western Horseman, or you were just nothing. One of my problems was that, upon the insistence of my mother, I enrolled in a class of typing. As one of two males in the class, several interesting epithets were applied to us. However, upon reflections over the years, learning to type probably was one of the most important skills that I had obtained in high school. After all, who could ever forget "asdf and jkl;" on those old blank-keyed, upright Royal typewriters?

Following completion of high school in a large graduating class of 32 members, I worked for a year in a munitions plant as a civilian employee of the U.S. Navy, as well as some times still spent at the family ranch. Then, in accordance with the wishes of my mother and several other members of the family, I enrolled in Hastings College, Hastings, Nebraska. I had funds to pay for the first year of tuition and lodg-

Figure 4. Horse with tack, 1951.

Figure 5. Mounted up, 1951.

ing. Then, I went to work at part-time jobs in a bakery (cleaning ovens at night) and subsequently, as an employee of a funeral home. While at Hastings College, I became close friends with Eugene Fleharty; that relationship has persisted through graduate school to the present time. While at Hastings College, Gene and I took some classes together, studied together, and were participating members on the football team (Figure 6). Incidentally, in 1954, the football team went undefeated throughout the season, and won the Mineral Water Bowl in Excelsior Springs, Missouri (the members of the team were inducted into the Hastings College Athletic Hall of Fame on 22 February 1989). Of importance academically, I became well acquainted with my advisor, John Moulton, who was the professor of biology and geography at the college. During much of the time in college, my only career goals were to either return to the ranch or become a school teacher (or both). However, after a semester as a practice, part-time teacher at Hastings High School, I knew that being an elementary or secondary school teacher was not for me. Mr. Moulton guided me toward other serious pursuits for my life. As a result, for my senior year, I dropped out of the football program and enrolled in every biology class that was available.

Figure 6. Lineman at Hastings College, Nebraska, 1954.

Early in the senior year at Hastings College, I was influenced by Eugene Fleharty, who already was enrolled at The University of New Mexico in Albuquerque, and had entered the program in mammalogy directed by Jim Findley. Also, I was encouraged by John Moulton that I should apply to the graduate program at this university. He stated that New Mexico was a perfect fit for me, because "They never did anything practical down there." My mother wanted me to go to New Mexico to obtain experience with the "different cultures" of the area. My home range was expanding.

For some reason, my application to the graduate program at The University of New Mexico was accepted, and I was provided with a teaching assistantship in the Department of Biology. I enrolled there in the fall of 1957. Incidentally, the stipends for teaching assistants and research assistants were $100.00 per month. At an initial meeting with Jim Findley, he examined my academic record, which was somewhat less than spectacular, and stated that "if I had been there, I would hardly be a senior." Nevertheless, he agreed to take a chance with me; he enrolled me in several leveling courses (botany, chemistry), as well as in his course in mammalogy. Somehow, I survived the first year of graduate studies. Jim Findley provided me with a research assistantship for the summer to work in the field with him and Gene Fleharty. I must digress here to explain something of importance. Jim, Gene and I were camped north of Cloudcroft to collect mammals. One day I returned to the tent to find Gene sitting on his cot smoking, and Jim lying on his back on his cot smoking. As I entered the tent, I said "what is going on here, anyhow," whereupon Jim reached up and grabbed me. Being a bit nervous on this first real field trip, I said "watch out or I'll flop my doc out in dorks face." They both laughed at the moment; Jim Findley has been referred to as "dork" ever since. I hasten to add, dork is a term of affection and respect. Incidentally, I gave up smoking shortly after the aforementioned field trip; I could not afford to support the mooching. Gene and I linked up with Arthur Harris, and the three of us became close friends; we were the first three Ph.D. students directed by Jim Findley. We were allowed access to Jim's library and advised to read books to learn what mammalogy was all about. Several important works of the time became very influential to me. Some of these publications are provided in Appendix 2.

Jim and I used to talk a lot while driving down the road (a lot of biology can be learned in the front seat of a car). One of our topics was what other people thought of us. While on a summer visit back to the family ranch, I gained some insights into that topic. One morning I slipped into the kitchen hoping to find one of my favorite cinnamon rolls for a quick snack (it was on a dish where it had been when I was a boy). While eating it, I overheard something like the following conversation between my mother and two of her lady friends.

Other: "Whose car is that out front?"

Mother: "Oh, that's Clyde's car; he is here from New Mexico for a visit."

Other: "What does he do in New Mexico?"

Mother: "He is a graduate student at the University of New Mexico."

Other: "What does he study?"

Mother: "He is studying to be a doctor."

Other: "Good, we really need a young doctor around here."

Mother: "He is not studying to be that kind of doctor."

Other: "Well, what kind of doctor will he be, a veterinarian; we need one of those too."

Mother: "No, he will be a Ph.D., and teach in a university."

Other: "What does he do to prepare himself for such a degree?"

Mother: "He goes out into the mountains, sets these little traps, and catches little mice."

Other: "Oh my, what does he do with the mice?"

Mother: "He skins and stuffs them, and when they are dry, they are catalogued in the collection at the University of New Mexico. There are thousands of mice in the collection. They are studied later, but I am not certain of what kinds of studies they do with the mice."

Other: "How fascinating. Are all of the mice prepared by your son?"

Mother: "Oh no, there is a whole group of people doing this kind of work."

At this time, my mother slipped into the kitchen; she leaned over me and whispered "how did I do?" I gave her a double thumbs up. From a personal point of view, my association with Jim Findley was and is much more than a traditional mentor and student relationship. In many ways, I grew up with Jim and his family. I consider Jim and his family some of my closest friends. Jim and I have done many things together; we still contact each other occasionally. During a recent stint in the hospital, Jim came to see me; the visit was greatly appreciated. Also, I would be remiss if I did not mention here my relationship with Gene Fleharty. Suffice it to say that we have remained friends, kept in contact, and periodically visited each other for 50 years.

Overall, graduate school was a wonderous learning experience. In reflection, I consider those experiences as some of the best ones in my life. Under the leadership of Jim, we traveled (before New Mexico, I had ventured as far as Colorado, Wyoming, South Dakota, Iowa, and Kansas; now, I have been in all 50 states). We collected mammals in accordance with Jim's development of the Museum of Southwestern Biology, carried out research projects, and were taught to write and publish papers. The first paper with Gene Fleharty was published in the *Journal of Mammalogy* in 1960. Although there have been some "peaks and valleys," I have tried to maintain some consistency in publishing papers since. A self-evaluation of my more than 180 publications reveals that most are either reportive or descriptive; very few are conceptual. Some of my favorite publications are listed in Appendix 3. Some publications with which I wish that I had never been involved are not listed.

My first attendance at an annual meeting of the American Society of Mammalogists was hosted by the University of Arizona, a memorable meeting in numerous ways. Several other annual meetings of the Society have been memorable also, such as Albuquerque, Asilomar, Vancouver, Tampa, and, of course, College Station (the most memorable one of all). Through attendance at these meetings, in part, I have been quite fortunate to meet and interact with numerous mammalogists from some of the early generations, such as Stanley P. Young and Hartley Harold Thomas Jackson; unfortunately, I was too late to meet C. Hart Merriam, Vernon Bailey, and Edward Goldman. Also, I have had the good fortune to meet numerous others from subsequent generations. It seems that I may have been influenced in some way by the personal contacts and the writings of many of them. Especially in those early years of my career, annual meetings of the American Society of Mammalogists were especially important to me for the numerous kinds of interactions with other mammalogists.

Initial advice given to me by Jim Findley for expressing interest in field and museum oriented studies of the Mexican meadow vole was the simple statement that "You have to catch them first." This short piece of advice seems as relevant today as it was 46 years ago. A seemingly logical question was, "How many do I need to catch?" The reply was, "As many as possible, obtain at least 30 of each sex and each age group from every locality." By the way, I have never achieved that level of success in trapping mammals. A few years later, this was translated into a policy for collecting stated simply as, "If there are a lot of them, catch as many as you can; if there are a few of them, collect them all." For either better or worse, some of these attitudes and philosophies have changed over time.

Some reasons for such changes include general attitudes and philosophical thinking in accordance with changes in attitudes of society in general, and corresponding development of various laws and corresponding regulations related to some concerns about natural resources in general and wildlife in particular. Systems for obtaining permits to collect mammals were evolving at the state level. Early in my career, I received a state permit with the language that I could, "Collect mammals at any place at any time by any means possible." Later, I received another state permit with restrictive provisions for, "Collection of one male and one female (no pregnant or lactating females) from any one locality" (good as I am, I realized that I could not collect selectively in accordance with the requirements). Subsequently, some federal legislative actions enhanced attention to research and management of wildlife. For example, passages of the Endangered Species Preservation Act of 1966, the Endangered Species Act of 1969, the 1973 Convention on International Trade in Endangered Species of Wildlife Fauna and Flora, and the Endangered Species Act of 1973 served to increase the attention devoted to wildlife by the federal government and the states. Incidentally, the Marine Mammal Protection Act of 1972 was a milestone with regard to protection, research, and management (MSY-OSP) of marine mammals, and served as somewhat of a model for some subsequent legislative actions. In spite of the bureaucratic activities related to the aforementioned federal legislative actions, it seems that some stability and consistency (as well as some reality) has resulted with regard to permit systems.

It seems to me that some of the other changes and advancements that have taken place over the last 46 years warrant some discussion. I want to attempt to review some of the mechanisms for handling of data related to research on mammals. Remember that numerous technological tools that we now take for granted were either nonexistent or in the infancy of development. For example, means, standard errors, and standard deviations were calculated by hand. The slide rule was a very important instrument; use of this tool expedited the capability to add, subtract, and, most importantly, derive square roots. Cog-wheel calculators became available; we could key in the data, and then go across the street to have coffee while the machine was grinding out the results. The Leroy Lettering Set was an absolute necessity for the construction of graphs, charts, and maps. Relationships of mammals were depicted traditionally with Dice-Lleras Squares, all of which were constructed by hand. Other methods of expression of phylogenetic analyses were developed subsequently. The mechanics of construction of distribution maps was a very labor intensive effort. Plotting of geographic localities sometimes was a bit tenuous, at best. These tasks have been expedited by the development of Global Positioning Systems and Universal Transverse Mercator systems. Accurate construction of distribution maps now is relatively less complex with the use of computer-based GIS systems. Preparation of manuscripts involved the use of typewriters, with carbon paper for copies. The availability of word processors and desktop printing capabilities has replaced the earlier traditional systems. Incidentally, there was spell check in the early years; it was called the dictionary.

A major phenomenon in the history and advancement of science was the description of DNA. Incidentally, while trying to relax the evening prior to the oral portion of my final examination, I stopped in a store to purchase some refreshments. While waiting to check out, I noticed a Life Magazine with a color depiction of the double helix of DNA; I purchased the magazine with the thought that someone might ask me about this matter. At the examination the next day, Jim Findley asked me if I had ever heard of DNA. I gave a very brief reply that this was a recently described substance in the structure of a double helix; Jim said, "you found the magazine." The description of DNA has stimulated the development of a plethora of technological advances in mammalogy, ranging from karyology, to sequencing of DNA, and to the studies of individual genes, to list a few. Naturally, there has been a constant proliferation of development of technological mechanisms, techniques, and corresponding training programs. All of these activities have been important to the discipline of mammalogy, and the enhancement of knowledge about these animals.

Following completion of the requirements for the Ph.D., I was employed as the Assistant Curator in the Museum of Southwestern Biology. The most important aspect of this position was that it allowed me to work closely with Jim Findley in the museum, as well as in the field. However, in 1965, I accepted a temporary position at Tulane University while Norman Negus was on leave. I learned a great deal about life as an Assistant Professor; my teaching load was 18 contact hours. On the more positive side, a close and lasting friendship was developed with Royal Suttkus, even though he is an ichthyologist. Many miles, much new knowledge, and several publications have resulted from this fine friendship. Also, I became associated with several graduate students, with the result that John

Pagels was my first Ph.D. student, followed by Frances Miller Cashner.

One of the early National Primate Research Centers was associated with the University. Inasmuch as I had never seen primates, except a few in zoos, I visited the Delta Primate Research Center. I became acquainted with the Director, Arthur Riopelle, who invited me to attend a forthcoming seminar. At the seminar, I met Jane Goodall, who informed us that Louis Leaky was interested in someone to study primates in West Africa, especially the relationships between lowland gorillas, chimpanzees, and other species native to the area. Shortly thereafter, Arthur Riopelle invited me to the Center for a meeting. He informed me that he had contacts in Spain, and would I be interested in joining him in a proposal to study primates and other mammals in the then Spanish Colony of Rio Muni (Equatorial Guinea), to which I agreed, with the thought that it would never happen. Much to my surprise, the proposal was approved and funded by NIH and the National Geographic Society in the spring of 1966. A hectic schedule of events followed. I attended the Annual Meeting of the American Society of Mammalogists held at Long Beach University, where I met Robert Baker. Incidentally the flight to the meeting was my first experience on a commercial airline.

I returned to New Orleans from Long Beach, and four days later departed for Washington, D.C. to attend briefings about the project, where I met Louis Leaky. Then, I went to Barcelona and Madrid, Spain, to meet the Spanish collaborator, Jorge Sabater Pi, and to obtain the necessary permissions for travel to Rio Muni. Subsequently, I flew (a ten-hour flight) from Madrid to Bata, the seaport capital of Rio Muni. The two months in Rio Muni in the summer of 1966 were spent seeing the country, ordering a Land Rover, arranging to rent a house, and meeting with Spanish officials, as well as officials of the Autonomous Government of Rio Muni. I learned a lot. After returning to teach at Tulane for the fall semester of 1966, I returned to Rio Muni to spend all of 1967 and part of 1968. This small West African country (population 500,000) was ruled from 1968 to 1979 by Francisco Macias Nguema, an egotistical and violent dictator. For example, he changed the name of the island Fernando Po to Macias Nguema, and he murdered thousands of his people, and drove many into exile. Macias Nguema was overthrown and executed by his nephew, Teodoro Obian Nguema, who has shown no more respect for human rights than his uncle. Oil was discovered in 1995; since then, U.S. oil companies have poured $5 billion into the country. Most of the income goes to Obian Nguema; the average citizen takes home about $2 a day. Although there were some times of tension and fear, overall the experience was quite fantastic during the total of 20 months spent in Rio Muni (Figures 7-11). Upon returning to New Orleans, my temporary position at Tulane University had ended, so I was housed at the Primate Center and supported to organize and analyze data, and prepare manuscripts, as well as to readjust to our culture.

In 1970, I accepted a position with the U.S. Fish and Wildlife Service as Chief of the Mammal Section of the old Bird and Mammal Laboratories housed at the U.S. National Museum of Natural History. That was the beginning of the development of lasting associations with Don Wilson, Mike Bogan, Al Gardner, Bob Fisher, and many other outstanding mammalogists (including marine mammalogists), ornithologists, and herpetologists.

I must digress here and provide a brief account of my trip to Antarctica. In 1971, I was chosen for some reason to be the science representative for an inspection trip to Antarctica. I was loaned to the Arms Inspection and Control Agency, a branch of the Department of State. We were transported by military aircraft from Andrews Air Base to Christchurch, New Zealand, via San Francisco, Honolulu, and Papua. A weather expert named Charles Roberts, and I became friends; we toured the south island of New Zealand together. After being flown to McMurdo, we were flown around the continent to the old Byrd Station, the Dry Valleys, and the Byrd and Shackleton Camps. Also, we were flown to the South Pole, where we had the chance to run around the world at the geographic pole. We received a nice award at the end of the trip (Figure 12). Again, I must digress here and present my version of another event. We initiated a study of the mammals of Nayarit, Mexico, by having Don Wilson, Mike Bogan, and Bob Fisher go to Nayarit for a month. Al Gardner and I would replace Don and Mike for another month of fieldwork; Bob Fisher would remain throughout. After a month of fieldwork, we started home. As we were about to leave Nayarit, we pulled

Figure 7. West African Rain Forest, Rio Muni 1967 (left to right, Fang tracker and Clyde).

Figure 8. Foot bridge, West African Rain Forest, Rio Muni, 1967.

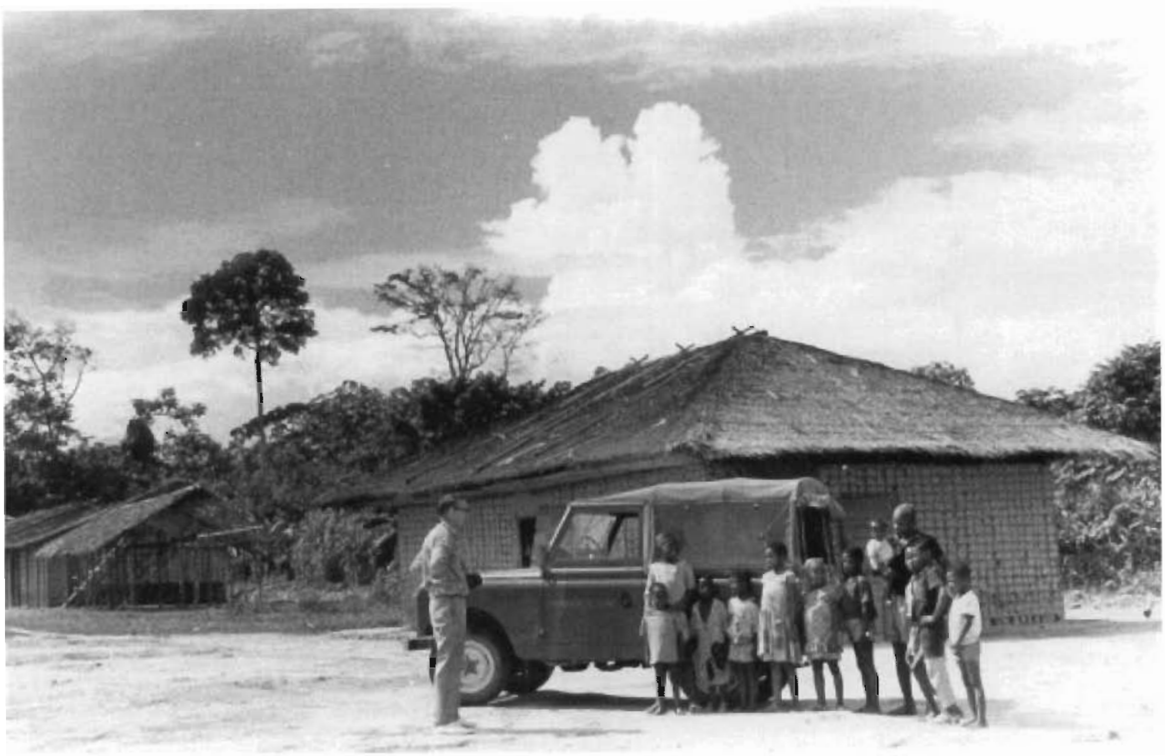

Figure 9. Making arrangements for lodging and a tracker, Evinayong, Rio Muni, 1968.

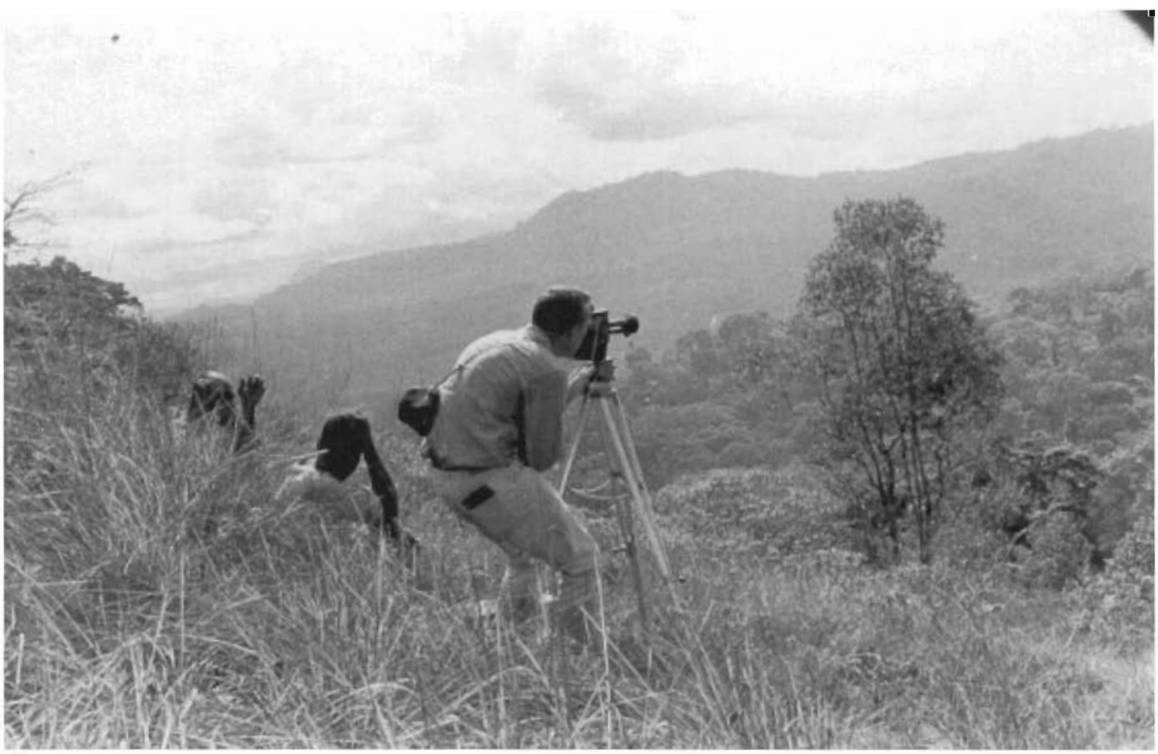

Figure 10. Habitat shot, Rio Muni, 1968.

Figure 11. Left to right, Craig and Cheri Jones with pet tree hyraxes (*Dendrohyrax dorsalis*), Bata, Rio Muni, 1968.

into a military checkpoint. The Commander recognized Bob Fisher, and proceeded to tell us the following story.

"He (pointing to Bob) and some other gringos were reported as camped by the river. We assembled some of the troops and sneaked up on them, just like

> in the movies (he pantomimed sneaking up). We got the drop on the two (Don and Mike) by the river first, and then we went to the camp, where he (pointing to Bob) was. We looked at the guns, ammunition, and the camp in general, and we left them. Man, were they scared."

This story has been reiterated many times, with numerous embellishments, I am sure. There are other stories, too numerous to relate herein. Anyway, various combinations of the four of us spent many happy times in the field studying mammals.

Through hard work and productivity by everyone, as well as strong administrative support, the Fish and Wildlife program flourished and expanded greatly for the next ten years. In 1979, the unit, now called the National Fish and Wildlife Laboratory, was combined with the old Denver Wildlife Research Center, and I was transferred to Denver as the Director of the Center. Although almost overwhelming in size, with a veritable world-wide scope of activities, the overall program continued to develop. However, with the emergence of the Reagan Administration, there were many changes in philosophy throughout the Department of Interior and the Fish and Wildlife Service. This was expressed vividly during a visit by the Secretary of Interior to the Denver Wildlife Research Center. It became apparent to me that Mr. James Watt and his

Figure 12. Receiving an Antarctic medal, 1971 (left to right, Admiral Welch, U.S. Coast Guard, and Clyde).

no sense underlings were in philosophical difference from me and some of the things that we had been doing at the Research Center. Shortly thereafter, I was informed of a transfer back to an administrative position with the Service in Washington, D.C. I resisted the transfer, and was given the option of accepting it or leaving the Service. I chose the latter.

Thanks to the efforts of J. Knox Jones, Jr., and Robert Baker, I moved to Texas Tech University in 1982 as the Director of the Museum, with an academic appointment in Museum Science and the Department of Biological Sciences. In 1985, I resigned as Director of the Museum, and held the position as Chairman, Department of Museum Science, until 1987. At that time, I moved full time to the Department of Biological Sciences. While at Texas Tech University, I have been involved directly (chair or co-chair of advisory committees) with more than 20 graduate students, and involved indirectly with numerous others. Suffice it to say, without my graduate students, I would have failed.

For several reasons, I made the decision to attempt to remain a field and museum oriented student of mammals. Although I have stepped over the line on a few occasions, I have continued to pursue the same interests in mammalogy throughout my career. I am comfortable with the decision and the pursuits over the years. These activities have led me to study, observe, and collect mammals in many places. For some of these reasons, I have had the good fortune to visit seven continents. After numerous abortive efforts to go to South America, it finally was achieved in the summer of 2001. Robert Baker and Carl Phillips invited me to accompany them and several students to Ecuador. I had a wonderful time, and am very grateful for my "last continent."

Through the aforementioned experiences, I have become quite impressed with what is known about mammals, as well as what is not known about this group of organisms. For example, at one time in my career, I naively considered myself as somewhat of an authority on the mammals of the Chihuahuan Desert. As a result of returning to work on the mammals of the area for the last 20 years, it seems that I know very little about the mammals of this interesting region. Students with whom I have been associated recently and I have determined that there are many dynamic changes taking place with regard to the mammalian fauna of the region. It seems obvious that the changes in geographic distributions of mammals are occurring at an accelerated pace over time. In other words, we have realized that distribution maps of mammals represent a single dimension of biogeographic reconstruction, and they are affected potentially by numerous extrinsic and intrinsic factors, which change through time and comprise the complex history of a species.

There has been a veritable myriad of changes in the discipline of mammalogy over time. These changes are the results of changes in attitudes and philosophies, technological advances, systems of analyses of data, approaches to the enhancement of knowledge, and other factors. In reflection, it seems to me that in the early years, the data overwhelmed the technology that was available. At the present time, the advancement of technology has overrun the data. Additional data are needed to support systems of analyses in order for present and future generations to understand the bio-

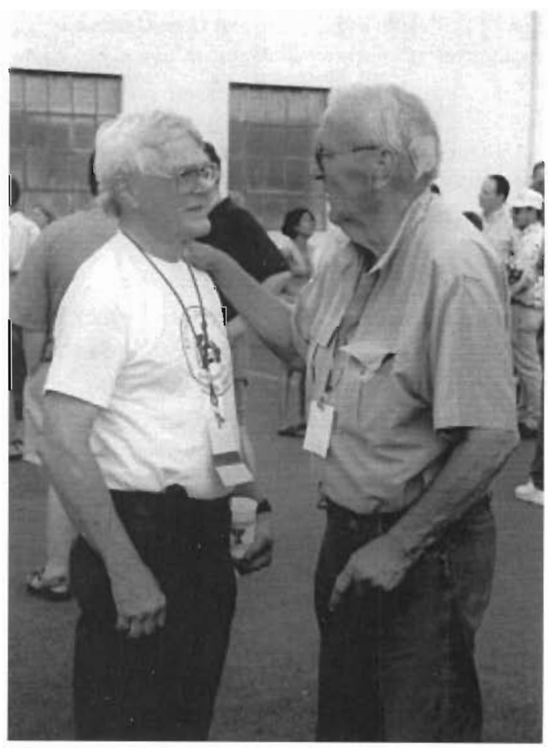

Figure 13. Jim Findley and I discussing...

logical systems of these important organisms. In other words, we need more data.

In summary, my personal life in general and my professional career in particular have been quite rewarding to me. As mentioned previously, I have been fortunate to travel to many interesting places. In addition, I have been extremely fortunate to have been associated with studies and writings about numerous kinds of mammals. In general, being at Texas Tech has been good for me, especially in the recent years of my tenure here. In the last few years, the Department of Biological Sciences has been a great place for me to work, and may be expressed as by Walter E. Brunauer.

"It gives a man confidence to know that
he has an organization behind him, one he
can draw on for backup and support"

Walter E. Brunauer

While at Texas Tech University, I have been honored by the Texas Society of Mammalogists, Honorary Membership, 1995, the American Society of Mammalogists, H. H. T Jackson Award, 1997 and Honorary Membership 2003, andTexas Tech University, Paul Whitfield Horn Professor, 1999. My home range has expanded.

APPENDIX 1

Some favorite books read in the early years that were (and still are) in the family library.

Foght, H. W. 1906. Trail of the Loup, being a History of the Loup River Region with Some Chapters on the State. Nebraska State Journal, Lincoln, 296 pp.

Holzworth, J. M. 1938. A guide to the Wild Animals of North America. Whitman Publishing Co., Racine, Wisconsin, 61 pp.

Lawson, J. G. 1935. Wild Animals: Photographs and Descriptions of 100 Important Wild Animals. Rand McNally & Co., Chicago, 64 pp.

Sandoz, M. 1935. Old Jules. Nebraska State Journal, Lincoln, x+424 pp.

Sandoz, M. 1942. Crazy Horse: The Strange Man of the Ogalalas. Nebraska State Journal, Lincoln, x+428 pp.

Schmidt, K. P. and W. A. Weber. 1934. Homes and Habitats of Wild Animals: North American Mammals. M. A. Donohue & Co., Chicago, 64 pp.

APPENDIX 2

Some works on mammals that were influencial on me in the early years of my graduate career.

Bailey, V. 1900. Revision of American voles of the genus *Microtus*. N. Amer. Fauna, 17:1-88.

Bailey, V. 1932. Mammals of New Mexico. N. Amer. Fauna, 53:1-412.

Baker, R. H. 1956. Mammals of Coahuila, Mexico. Univ. Kansas Publ., Mus. Nat. Hist., 9:125-335.

Durrant, S. D. 1952. Mammals of Utah. Univ. Kansas Publ., Mus. Nat. Hist., 6:1-549.

Finley, R. B., Jr. 1958. The wood rats of Colorado: distribution and ecology. Univ. Kansas Publ., Mus. Nat. Hist., 10:213-552.

Hall, E. R. 1946. Mammals of Nevada. Univ. California Press, Berkeley, xi+710 pp.

Hall, E. R. and K. R. Kelson. 1959. The mammals of North America. Ronald Press., New York, 1:xxx+546+79.

Hooper, E. T. 1952. A systematic review of the harvest mice (genus *Reithrodontomys*) of Latin America. Misc. Publ. Mus. Zool., Univ. Michigan, 77:1-225.

Vaughan, T. A. 1959. Functional morphology of three bats: *Eumops*, *Myotis*, and *Macrotus*. Univ. Kansas Publ., Mus. Nat. Hist., 12:1-153.

APPENDIX 3

List, in chronological order, of some favorite publications with which I have been involved.

Findley, J. S. and C. Jones. 1964. Seasonal distribution of the hoary bat. J. Mamm., 45:461-470.

Jones, C. 1965. Ecological distribution and activity periods of bats of the Mogollon Mountains area of New Mexico and adjacent Arizona. Tulane Stud. Zool., 12:93-100.

Jones, C. 1967. Growth, development, and wing loading in the evening bat, *Nycticeius humeralis* (Rafinesque). J. Mamm., 48:1-19.

Findley, J. S. and C. Jones. 1967. Taxonomic relationships of bats of the species *Myotis fortidens*, *M. lucifugus*, and *M. occultus*. J. Mamm., 48:429-444.

Jones, C. and Sabater Pi. 1969. Sticks used by chimpanzees in Rio Muni, West Africa. Nature, 223:100-101.

Jones, C. 1971. The bats of Rio Muni, West Africa. J. Mamm., 52:121-140.

Jones, C. 1972. Comparative ecology of three pteropid bats in Rio Muni, West Africa. J. Zool., London, 167:353-370.

Jones, C. and R. D. Suttkus. 1973. Colony structure and organization of *Pipistrellus subflavus* in southern Louisiana. J. Mamm., 54:962-968.

Findley, J. S. , A. H. Harris, D. E. Wilson, and C. Jones. 1975. Mammals of New Mexico. Univ. New Mexico Press, Albuquerque, xxii+360 pp.

Jones, J. K., Jr., D. Armstrong, R. Hoffmann, and C. Jones. 1983. Mammals of the northern Great Plains. Univ. Nebraska Press, Lincoln, xii+379 pp.

PASS THOSE CLOVERDALE SPUDS---AGAIN!

MICHAEL A. BOGAN

Michael A. Bogan was born in Kansas City, Kansas, on 29 September 1941. He received a B.S. in Biology from Baker University (1964), a M.S. in Zoology from Fort Hays State University (1966), and a Ph.D. in Biology from The University of New Mexico (1973). He is a Wildlife Research Biologist with U.S. Geological Survey, Fort Collins Science Center, Department of Biology, The University of New Mexico.

I grew up in Baxter Springs, Kansas, on the edge of the Ozark Plateau, a relatively atypical part of Kansas but one that was home to me. My father, Harvey Lewis Bogan, graduated with B.S. in Medicine and M.D. degrees (1940, 1942) from the University of Kansas and was the first in his immediate family to graduate from college. He was fairly "left brained" and medicine, science, and certain precise types of activities (e.g., carpentry) were his forte. My mother, Lucile Helen Davis Bogan, also the first college graduate in her family (KU 1937, B.A. in music education), was more right brained and had an artist's heart and wonderful musical abilities, especially on piano. My sister Patti was, and is, the smart child and around our table there was always some lesson or question that she seemed to "get" sooner than I ever did. Nonetheless, my father seemed convinced that if he could become a M.D., I surely could too, if I would only apply myself. And by the time I was old enough to pay attention to what adults in Baxter said, it seemed as though the whole town assumed that "little doc" would surely go to medical school. As a child this was flattering, but as I grew up it began to dawn on me that I didn't share my father's, or the town's, assumptions about my future.

When I was eight and nine I was bedridden with a rare disease (dermatomyositis), from which I recovered thanks to the unrelenting efforts of my father, who moved the whole family to Boston to ensure that I got the best treatment available at the time. While in bed and unable to get up and run and play, reading (always encouraged in our house) became very important to me and two of my favorite magazines were National Geographic and what I think was called Junior Natural History, a publication, I believe, of the American Museum of Natural History. I couldn't get enough of the articles that recounted expeditions to far-off places in search of either fossils or archeological treasures. The adventures in these magazines and others were my escape from bed while I was sick. Through these books I learned about collecting "specimens" and their subsequent study in museums. Although I'm sure that Junior Natural History had articles on biologists, it was the careers of paleontologists and archeologists that I think appealed to me most at the time. I had the usual assortment of "Golden Guides" (e.g., rocks, plants, and animals) that I carried with me on trips; another book that I perused continually was Hegner's (1935) Parade of the Animal Kingdom, which I still have. As a Boy Scout I earned several merit badges that dealt with animals and plants and I made small collections of "things" as well. My interest in wildlife, or at least "collecting", also may have come in part from my maternal grandfather, Allen F. Davis, who knew a good "series" when he had one (Figure 1).

I've tried to follow the thread of this early interest in science on into adolescence, but it seems to unravel among more immediate and hormone-driven needs like learning to drive, learning to drink, and learning about the opposite sex. If I had a cogent thought about my future at that time, I'm sure I can't remember what it was.

Figure 1. Allen F. Davis, my maternal grandfather, with a "small" series of waterfowl taken sometime in the late 1940s.

UNDERGRADUATE EDUCATION

Almost before I knew it I was being hustled off to good old KU (the family university it seemed) to do battle for a degree. At KU my French teacher insisted that I conjugate verbs, a concept that either was not taught at Baxter Springs High School or, more likely, did not register in my addled brain. And my chemistry teacher, surely one of Germany's rocket scientists and able to lecture only in heavily accented English, wanted 400 of my classmates and I to balance chemical equations. I was in over my head academically, had no idea how to study, and was bent on making sure I didn't miss any parties. KU was an unmitigated disaster for me and after two semesters and a summer session on probation the university cut its losses and told me to leave. Incidentally, my sister (B.S., 1965; M.S., 1968; Ph.D., 1971) just did excellent at KU.

To his credit, and my everlasting thanks, my father refused to let me quit and insisted that I travel down the road to Baldwin City, home of Baker University, and see if they would have me. Baker was so small (about 700 students) that Dean Benjamin Gessner, an intimidating man to one whose academic credentials were a shambles, personally interviewed me prior to my being granted conditional admission. Baker was supported by the Methodist Church (my Grandma Davis was one so I thought this was acceptable) and did have its redeeming features. Classes were small, professors attentive, and the academic pace was one I could handle. In addition, you could get extra credit (0.5 hr per semester) if you attended Wednesday "chapels." During my first semester or two I worried that every Wednesday someone was noting that I wasn't in attendance (I only went on those occasions when the assembled students chose the school cheerleaders. Otherwise I went to a diner and read and drank coffee.). Nonetheless, Baker and I hit it off and although it took me a little while to prosper, my record improved every semester. I began to think about where I wanted to go and how various courses might help get me there. I tried Psychology for awhile, took some art classes, but eventually gravitated towards biology. At that time Baker had a two-person Biology department that was strongly oriented towards providing grist for local medical (or worse, according to my father, osteopathic and chiropractic) schools and as a result there was essentially no field biology, although the chairman, Dr. Boyd, led birdwatching trips on the weekends. Nonetheless, I loved and excelled at zoology, histology, comparative anatomy (especially), embryology, and several others. On reflection, I think one of the things that appealed to me about biology was the hierarchical structure of it all and how everything seemed to fit together. I graduated in 1964 with a B.S. in Biology and, believe it or not, a minor in Chemistry.

At Baker, my mentor was John Nickel who taught many of the courses I liked best and who had a refreshing attitude about biology. John was, at heart, an educator and later went to Wichita State and obtained a doctorate. During my senior year at Baker, as I was starting to break the news to my father that I wasn't going to go to medical school, I talked with John about where I could go to obtain a M.S. degree that might start me on a path to making college teaching my career. As luck, or fate, would have it, John had obtained his M.S. working on *Peromyscus* at (what is now) Fort Hays State University in Hays, Kansas. He told me that the faculty was very student-oriented and that there was a new faculty member there interested in mammals. He suggested I go to Hays and talk to that Assistant Professor, Gene Fleharty, as well as the Chair of the Department, Dr. Jerry Tomanek. Furthermore, he said that the research emphasis at Hays was on field studies and that appealed to me.

FORT HAYS STATE

At my father's request I toured several other schools, including KU, Indiana, and Missouri where I visited biology departments that seemed to me to emphasize laboratory studies. All, frankly, had larger enrollments than I found comfortable. I felt most comfortable at Hays and as John promised, I found both Jerry and Gene to be very easy to talk to; they told me of the history of the school and department, and of the strong tradition of fieldwork there. I was assured that I wouldn't have any trouble being admitted and might even land a teaching assistantship. So, in late summer of 1964, Arnetta, Diane, Sean, and I loaded into a Volkswagen and drove across the prairie to Hays. We moved into half a new duplex on 32nd Street that was almost out of town then. Now it's at the south end of a large subdivision that stretches away to the north.

Dr. Tomanek told any of us that would listen that our graduate experiences at Hays would be something we would look back on with great fondness. (This comment was usually inserted between hints on how to identify some western grass; the plant itself having been removed from the soil with Jerry's marine knife, a souvenir from World War Two.) He was absolutely right. Some of my best days were at Hays and the group of students in Biology at the time was just exceptional. Elmer Birney tended to set the curve in every course I took with him. In later years Elmer and I always enjoyed seeing each other at the mammal meetings and I miss him now.

My colleagues included Elmer, Ken Andersen, Curtis Carley, John Farney, Dwight Ittner, Don Kaufmann, Tom Keyse, Ross Lock, Tony Mollhagen, Larry Olsen, Yar Petryszyn, Dennis Stadel, Jerry Walker, Eldon Whitmer, Bob Wiley (Ace as he was then known), Dallas Wilhelm, and others. Many of them were from western Kansas; I was the only one from eastern Kansas as I recall, although Larry and Ace were from Missouri. We took classes together, taught labs, went to the field, and in the most friendly way tried to best each other in all activities. Some of us learned how to play bridge at Gene Fleharty's house and continued our competition in that arena. Woe to the student who didn't make his "bid" if Gene was your partner. And there were a few local nightspots where other kinds of competition occurred.

I took mammalogy at Hays under Gene and used Cockrum's (1962) textbook and E.R. Hall's (1955) field guide to the mammals of Kansas. A year later, a bunch of us enrolled in "Advanced Mammalogy" with Gene and the required text was the two-volume "Mammals of North America" by Hall and Kelson (1959). I'm sure the small bookstore at Hays thought they had struck gold when all of Fleharty's students bought those volumes ($65 for the two volumes as I recall). I poured over this two-volume set and it was the distribution maps that really "caught" my eye. I don't think I took the maps as static in any real sense. Rather, I kept thinking of traveling to selected areas and "testing" those distributions. The two volumes later accompanied me wherever I went in New Mexico and Mexico (until Hall, 1981, was published) and their well-worn appearance brings back lots of memories.

Tony Mollhagen took primary responsibility for teaching me how to skin mammals at Hays and although I've improved over time, I still can't make the back end of a *Microtus* look the way I think only he and Syd Anderson can. I recently saw my number 1 at the Sternberg Museum and was chagrined to see how I had turned a nice *Peromyscus maniculatus* into a long, greasy, furry pencil! I acquired 258 numbers in my field catalog while at Hays. I still love preparing specimens with Tony; I skin, he stuffs, and we share the pinning. Over the years, we've put up some really nice specimens that any museum should be proud to accession.

I hunted in eastern Kansas, mostly ducks, which I hated because of the long wait in a cold, wet blind, and quail, which I loved because of the hiking and excitement of flushing the birds. I'd never been pheasant hunting and one fall morning Fleharty and a bunch of us went out to Cedar Bluff Reservoir to try our hand. We spread out with some of us in the draws, others up on the hills. My eyes were on the grass ahead when I heard a shot, looked towards Fleharty, and saw a mallard plummeting down ahead of him. He'd had the sense to keep his good eye busy and when he saw ducks flying under the overcast he was ready for them. Several of us "opened up" then and we shot several more mallards before they were out of range. It was only then that we realized we were a little short of "duck stamps" to legally take the animals. Fleharty, never at a loss, suggested a small party be dispatched into a nearby town to buy duck stamps to cover this problem. Later, I did get a pheasant. I think Jerry Walker and Tony Mollhagen were beside me as a pheasant, big as a B-24, got up in front of me and flew straight away. I brought my shotgun up, took aim, and got the pheasant. Just as I was wondering why no one else shot, Jerry congratulated me, "Nice hen shot Mike." My embarrassment was palpable as I walked past the pheasant and left it in the grass.

One of the best traditions at Hays was the "summer field trip" and in the summer of 1965 I accompanied Tom, Larry, Duane Houston, and Gene on a trip to the Southwest. It was an eye-opening trip for me that led to a real love affair with this part of the country, one that continues to this day. We were gone over two weeks and went from Hays to northern Texas to trap *Dipodomys elator* and then southwest to Carlsbad, where Tom Keyse gave the park ranger a hard time at the cave, and we trapped *Spermophilus mexicanus* on the Carlsbad golf course. We spent several days in the vicinity of Cloudcroft, New Mexico, trapping *Eutamias canipes* (Fleharty, 1960) and other montane species. I thought I'd died and gone to heaven. From there we went to Hidalgo County in the bootheel of New Mexico and trapped more rodents and I caught my first bat. Gene made sure we kept the stripes straight on the chipmunks we prepared, didn't get the mist nets wet, and taught us how to care for the cast iron skillet he used to cook our breakfast. Eventually, after Tucson and Willow Creek, we ended up at Jim Findley's house in Corrales and spent a couple of days visiting the University of New Mexico, netting bats over the irrigation ditch near Jim's house, and hearing stories about how Jim, Art Harris, Gene, and Clyde Jones had sort of made New Mexico safe for mammalogists. I was pretty certain I knew where I wanted to go after Hays.

While at Hays, I learned about the incredible tradition of mammalogy at KU, a tradition I had unknowingly bypassed with my academic problems. Gradually I began to acquire copies of several publications from the Museum of Natural History, including "Mammals of Coahuila" by Baker (1956), "Mammals of Veracruz" by Hall and Dalquest (1963), and several others. I read these books between classes and during labs at Hays. Hall and Dalquest in particular had a big impact on me. The heart of the book is a first-person narrative, that of Dalquest, that describes his adventures in Veracruz—a pretty exotic-sounding place to a boy from Cherokee County, Kansas. It reminded me of books I'd read as a youth that detailed expeditions to faraway places in search of little known treasures—mammals or otherwise. I now realized that I too could be a participant in such adventures.

My first publication, co-authored with Tony Mollhagen (Bogan and Mollhagen 1969), was the result of a project in Gary Hulett's Biometry class at Hays. We enjoyed the fieldwork, the analysis (an anova) on old rotary calculators, and writing the paper. Of all the great friends I had at Hays, I've stayed closer to Tony than anyone. We still do field work together and write the occasional paper (e.g., Mollhagen and Bogan 1997). Tony has incorporated a lot of our feelings about Gene and Hays into his website on the Henry Mountains (www.henrymountains.com). And of course it was a "no brainer" to help Jerry Choate in his fine tribute and festschrift for Gene (Choate 2000). My own contribution was co-authored with Paul Cryan (Bogan and Cryan 2000) and I liked the symmetry of working with a student in honoring my own advisor. Mike Mares got it right at the banquet for Gene when he noted that Gene made us all think we were better than we were. In terms of my career and professional life, I think it would be hard to overstate the importance of Gene's influence on me.

THE UNIVERSITY OF NEW MEXICO

In 1966 I took my shiny new M.S. degree and went to UNM to work with Dr. Findley. I thought Jim seemed to give me a lot of credit for my M.S., although in retrospect I suspect he just had faith in Gene to train students. Dr. Findley, in a spirit of equality and rebellion (I suspect) insisted we call him Jim. Over the years we were more inclined to call him Dork, a term of arguable endearment, or Jefe, in recognition of our esteem for him and the Southwest. Regardless, there wasn't a one of us that didn't have the greatest respect for Jim. I think it was Jim that first introduced me in detail to the Grinnelian lineage that, among other things, ties the contributors to this volume all together.

At UNM I was again associated with another really outstanding group of individuals. We tried to hold up our end in Jim's continuing fight against stupid rules and academic stuffiness. Although, when Jim found out that the small streaks on his mammal cases were the result of our nocturnal Frisbee games with coffee can lids he put a stop to it. At UNM I learned about *Myotis*, green and red chile, fieldwork in Mexico, Okie Joe's, calipers, and multivariate analysis. Jim was instrumental in imparting knowledge on all these things. We never went to the field without green chile, jalapenos, and beer. One of my first outings in New Mexico was with Don Wilson and L.M. "Mac" Hardy. We cruised old highway 66 in Mac's VW bus from Los Lunas to I-40, capturing snakes and kangaroo rats on the road and bats under bridges. Later, I went to Hidalgo County with Jay Druecker and netted bats in magic spots like Guadalupe Canyon.

In Spring 1967, Don Wilson, Mike Mares, John Darling, Keith Grisham, and I drove all the way to San Blas, Nayarit, and Puerto Vallarta, Jalisco, netting bats as we went. We had no permits, not much money, but we did have Hall and Kelson. We stopped on the Sinaloa-Nayarit line and set up some nets over the Rio Canas there. Soon we captured a *Noctilio leporinus* that appeared to be a range extension of some 400-500 miles, according to Hall and Kelson, and we couldn't believe our luck. Later we discovered how quickly knowledge was accumulating. Knox Jones and others had already taken this species even farther north in Sinaloa. We set a record on that trip for flat tires and had to pool our money to buy new tires to get home. As I recall, Mike was no help with Spanish and in fact got sick when we crossed the border. Don had been to most of these places and was our unofficial but essential tour guide. John tried to interest us in birds (the brown pelicans and magnificent frigate birds were a hit with me) and Keith was a gentleman with a moderating influence on the rest of us. We found a small hut on a beach just south of Puerto Vallarta and stayed several days, loving the tropics and the bat activity at night. I've always enjoyed netting bats with Don. He rarely got excited, was hard-working when that was required, and he was always great company.

Although I'd attended the Kansas Academy of Science annual meeting in Kansas, my first national meeting was the 1968 meeting of the American Society of Mammalogists in Fort Collins. As I recall, Dan Williams and I slept in Dan's camper and showered in Jim's room. Jim was rooming with Bob Hoffmann and it was a pleasure to meet Bob and talk with him. Dan and I also discovered we really liked Bob's Green Stripe Scotch, which we tapped whenever we took a shower. I remember skipping the banquet, probably due to a lack of money, and a group that included Hugh Genoways, Tom Kunz, and others stayed in the dorm and talked and drank beer.

Eventually, I visited KU as a respectable graduate student, examined specimens of *Myotis* in the museum, and even got Dr. Hall's opinions on them. I was in absolute awe of this man. I had a great visit with Carl Phillips, Jerry Choate, Elmer, Hugh, and others in one of their offices in Dyche Hall. A hot topic was field work in Mexico and everything that that entailed. I think I first heard the story about Knox and the condom in the beer bottle during that visit. Then and now, I feel especially close to this cohort of colleagues from KU.

In the summer of 1968, I went on a six-week trip to Mexico with Mac Hardy, again in his VW bus. He was after blind snakes (*Leptotyphlops*) and I after *Myotis*. We went as far south as the Isthmus of Tehuantepec but to my frustration we could not cross into Veracruz, as a bridge was out (Dalquest's adventures were on my mind). I netted my first vampires and collected a puzzling series of *Glossophaga* that was later determined to represent two species (Webster and Jones, 1980). We stayed with Ticul Alvarez in Ciudad Mexico and he was a great host. He took us to

UNAM and to the "polytechnic" where he worked and we talked to students and faculty and examined specimens. While we were at a bullfight, the bus was broken into but it was such a mess inside that as far as we could tell, nothing of value was taken.

I was the curatorial assistant in mammals in the Museum of Southwestern Biology for awhile and among other things got to process bats from Costa Rica and Panama that Don Wilson was sending in periodically. Jim went to Europe for a year to study *Myotis* (Findley 1970, 1972) and more or less left Dan Williams and I in charge. We taught General Vertebrate Zoology and made a couple of trips to northern Chihuahua to trap and net. We published (Bogan and Williams 1970) a little note in Southwestern Naturalist on new bat records for the area (my most requested publication by unofficial count). I also published (Bogan 1972) one on parturition in *Lasiurus cinereus* as a result of bat netting in the Sandia Mountains.

I realize now that working on a doctorate was a lot like "working" in real life and somewhat less like my time spent at Hays. At Hays, I knew there were other steps in front of me before I "grew up." At UNM, this was pretty much it—I was on a career track. Never one to learn the lessons of life easily, I made it tough on those around me by spending lots of time in the field. In 1969 Arnetta and I got a divorce. I subsequently filled in at Hays for Gene Fleharty during his sabbatical in 1970. I taught multiple classes, all firsts for me, and although I enjoyed being back at Hays and being around a new crop of graduate students, it was work. Nonetheless, a permanent job at a school like Hays was still high on my list of career plans. I returned to UNM with no visible means of support, uncertain of the future, and yet cognizant that I wanted to finish my degree. I discussed all this with Dr. Potter, then the Chair in Biology, who subsequently chose me to fill an Instructor position that included supervising the laboratories and teaching assistants in the beginning biology classes for majors as well as teaching Human Anatomy and Physiology. This was pretty much a full-time position and it made it difficult to make progress on my dissertation. On the other hand, had it not been for Loren's support at this time, I might have left school. I owe him for believing in me. Loren is still in Albuquerque and now tries to get me out on the ski slopes in his "senior" class.

Several of my close colleagues at UNM had moved on by then, Don to a postdoc with Dan Janzen and Dan to a job in California. Ken Andersen, Hal Black, Ken Geluso, and Tom Keyse were still there and Bill Caire, Rick Smartt, and Trish Freeman were new students of Jim Findley. In May of 1971 Tom, Hal, and I netted bats along the west coast of Mexico. I had gotten a small grant from UNM to assess the extent to which fruit bats might be vertically stratified in the forest near San Blas. We were gone about two or three weeks and managed to do significant damage to Tom's family-style car, but we didn't get much information on vertical stratification of bats. Going home, Tom threw all dietary caution to the wind and ended up with the revenge of Montezuma in a big way. Hal, in spite of being cautious, also came down with the same affliction and we had an interesting and frequently interrupted drive home. Someplace on the Mesa Central in Durango, Hal, in the midst of a violent peristaltic rush, dropped his drawers to the delight of throngs of passengers as a train passed by behind him. This trip was tinged with sadness, however, as my father died while I was in Mexico, a great loss to his family, medicine, and especially his many patients.

I kept working on my dissertation and teaching (don't do it if you don't have to) and went into full "job-search mode" but to no obvious avail. Jerry Choate had just accepted a new position at Fort Hays, a job I'd always wanted. In the spring of 1973, Scott Altenbach and I and our wives (Marilyn and Barbara, respectively) made a trip to San Blas, mostly for fun. We netted along the way and one night in southern Sinaloa, we stopped and put our nets up over some small pools in an otherwise dry arroyo. When a local farmer stopped by, we asked him the name of the river. He was, I'm sure, totally mystified as clearly there was no river and he said something like "no hay." We then asked if the arroyo had a name, clearly an unlikely idea in retrospect. He immediately responded, "El arroyo tampoco." We said, "El Arroyo Tampoco?" and he enthusiastically said, "Si." So the next morning as we took our notes and prepared specimens we diligently entered the locality "Arroyo Tampoco" into history.

I think I met Clyde Jones for the first time in the fall of 1971 at the second meeting of the North American Bat Research Group in Albuquerque. I gave my first presentation at this meeting—on distinguishing between *Myotis californicus* and *M. ciliolabrum* (Bogan 1974). Clyde was now with the Fish and Wildlife Service (FWS) at the National Museum, but was making periodic trips westward to update us on how new environmental legislation and subsequent regulations were starting to impact governmental research on mammals. Clyde had just hired Don and the two of them were re-invigorating the Mammal Section in the old Bird and Mammal Laboratory, a direct descendent of the original Biological Survey. I genuinely had never heard of the Fish and Wildlife Service when I first met Clyde but I certainly knew of early Survey workers like E. W. Nelson, E. A. Goldman, and Vernon Bailey. I saw Clyde again at the 1972 San Diego meetings of the bat group and he asked if I would be interested in applying for a job at the National Museum. He said he thought they might be adding a couple of new positions sometime soon. Sure enough, in the spring of 1973, FWS advertised two curatorial positions at the National Museum of Natural History. Al Gardner and I ended up being selected to fill these positions and I still can't imagine a better job! The fact that Al is still there is some recognition of that fact. I wouldn't have 30 years of service as a government research biologist had it not been for the support of Don and Clyde; and I'm grateful for that.

U.S. National Museum

In mid-June 1973, Barbara, Justin, and I took off for the East. I had 1,505 numbers in my field catalog when I left UNM. We settled in Vienna, Virginia, a really nice suburban community west of the beltway and Washington, DC. When I got there, Al had been hired but was in Alaska until fall, Don was in Costa Rica or some such place, and Clyde and Bob Fisher were in and out for mammal meetings and fieldwork. Thus, on almost my first day of duty I was Acting Chief of the Mammal Section. I had no idea what to do. Clyde, grasping that fact, took me to the Crown Bar and Grill and began to educate me on what the USNM was all about. I learned a lot from Clyde and he was an important mentor for me during my first 8-9 years in government service.

I spent the entire summer in DC, often spending my lunch breaks in the old Biological Survey files upstairs in the attic where I read the original notes ("Special Reports," "Plant Reports," and "Physiography") of Vernon Bailey, E.A. Goldman, E.W. Nelson, and others, all laboriously handwritten and later typed with multiple carbon copies. I met the Curators of Mammals at the Smithsonian Institution (SI), C.O. Handley, Jr., H.W. Setzer, R.W. Thorington, and J.G. Mead, and frequently joined Hank Setzer for lunch. Late in the summer, as the time for Don's return approached, Clyde said he thought it would be great fun if we would go into Don's office and move several bookcases up against the door on the inside. We did this, pulling the last bookcase against the door as we exited. On Don's first day back we had coffee (always in Al's office) and then Don walked to his office, anxious to get started on the work at hand. He opened the door, took a half step and stopped, unable to go further. Thoroughly disgusted, he turned and walked out of the Mammal Range. I'm not sure he's forgiven us yet.

In September 1973, I finally got out of the museum on my first official travel for FWS. I joined Jim Mead of the SI and we flew to Tampa to examine thousands of purported sperm whale teeth that the government had seized. Jim and I worked several long days going through box after box of teeth (yes, they were all sperm whale; Figure 2) and when we finished, Jim said he needed to coordinate with several of the marine mammal laboratories in Florida. That sounded good to me and we took off and ultimately visited several sites. Someplace down in the Keys, we

Figure 2. Portrait of me taken by Jim Mead of the Smithsonian Institution, following several hours of examining thousands of seized sperm whale teeth.

ran out of daylight and Jim suggested we just sleep in the car. Soon he was asleep and mosquitoes were devouring me. My tossing and turning finally awakened him and, ever the helpful fellow, he got into the trunk and pulled out a zippered bag for me to use. I spread it out on the ground, began to zip it up, and suddenly realized it was an army surplus body bag! Jim was an inveterate shopper at the government surplus sites around DC and I suspect the bag was destined to hold some marine mammal; I decided I'd put up with the mosquitoes!

In looking back at it now, time seems to have really accelerated during this period of my life. I was very busy (or thought I was) and saw my job as consisting of two intersecting spheres. One sphere was "academic" and consisted of practicing taxonomy, using museum specimens in research, doing fieldwork (often mammal surveys on public lands), and going to meetings where I frequently gave presentations. The other sphere, more "nonacademic," was associated with being employed by FWS, learning about the agency and its mandates, and what "role" scientists at a natural history museum could or should play in this arena. I always thought Clyde did a great job of tying these two roles, or spheres, together. He was an articulate and forceful advocate for improving the quality of science in the government and for having a trained cadre of scientists who could react to "the next problem" with dispassionate objectivity ("show me the data, Jerry"). He also enjoyed life and was great fun to be around. Clyde insisted that many of us spend some time at the wildlife divisional office over on "H" street. This turned out to be time well spent as in later years I interacted with many of these folks in seeking funding for scientists I supervised. Our laboratory was growing and now included several field stations (including Albuquerque) and more herpetologists and marine mammalogists than I could imagine. Our trips frequently took us to field stations where, among others, I met Norm Scott and Howard "Duke" Campbell. I think it was about this time (mid- to late-1970s) that I realized that in spite of all the earlier work on mammals, there was still a lot to be learned about mammals in the West. And, that I would be comfortable if I could contribute to that effort.

The USNM was an amazing place to me in those days. I couldn't believe my good luck in being able to examine the very specimens that Nelson, Goldman, and others had taken on their travels, to read their accounts in the SI archives, and to compare new material with older specimens. I took great pride in preparing specimens that would be deposited in the USNM and in introducing younger workers to this craft. I especially respected the curatorial "conscience" of Bob Fisher, who ensured that our specimens were properly cared for once they were accessioned. For several winters, Bob and I did field work in Florida on national forests and wildlife refuges and in the summers, Clyde and I made several field trips to Texas, New Mexico, Arizona, and California. Hall and Kelson (1959), copies of field notes of Bailey and others, and a strong sense of history accompanied us in the field.

A major early field activity that Don, Clyde, Al, and I undertook was work on the mammals of Nayarit. We all had more or less mutually chosen Nayarit as an appropriate place for a mammal survey and one where we would like to work. Between 1975 and 1978 or so we made multiple trips to Mexico, initially to Nayarit, but as our contacts in Mexico grew, we began to be asked to assist with mammal surveys elsewhere in Mexico. I subsequently traveled with various colleagues to islas Tiburon and Datil (Bogan, 1997) and elsewhere in Sonora, Sinaloa, and the Baja California peninsula. I frequently teamed up with Norm Scott on trips to Sonora and Baja California—I think we're both "desert rats" and really enjoyed that climate. With urging and assistance from Clyde, I started a project on the mammals of Baja California Sur. Between 1978 and 1982 I made 1-2 trips per year to the peninsula. We collected several thousand animals from poorly known areas of the southern half of the peninsula.

I really enjoyed this work and being in the field with Don, Clyde, Bob, Al, Brian Robbins, and others such as Cathy Blount and Donna Davis. It was exciting to collect species I hadn't seen before and to learn about their distribution and general ecology in poorly known areas. Unfortunately, and as some of you may have noticed, mammal inventories on Nayarit and Baja California Sur have not appeared in print. Several of us have published articles emanating from these studies (Bogan 1978, Carleton et al. 1982, Diersing and Wilson 1980, Fisher and Bogan 1977, Wilson 1991) but to date the overall syntheses remain unfinished. We all were busy doing other things, much of which was appearing in print, and although we were able to find the time to do the field work (the Siren's call) we didn't get the results written up for publication. Perhaps our focus was not sufficiently sharp to set priorities and get these projects finished up. We were blessed in those days with a budget sufficient for our needs, considerable budgetary discretion, and limited open travel authorizations! Our routine response to someone becoming grouchy, perhaps because of too much office work, was that they needed to (listen to the Siren and) go to the field!

It was an early field trip to Nayarit that led to a situation where I was reminded that one could die doing this kind of work. Don and I were again netting bats, mostly *Noctilio leporinus*, on the Rio Canas separating Nayarit and Sinaloa. We'd had a moderately productive night and were sitting on the back of the truck listening to a truck slowly grinding its way toward us. I assumed the truck was loaded with folks that had gone into town for the day and were now being dropped off at their houses. The truck stopped nearby and by the time we realized the meaning of the sound of multiple feet running towards us, it was (fortunately) too late to do anything but obey the commands to put our hands up. There was a .45 Colt automatic (or equivalent) pointed at each of our heads and another 10 or so frightfully young men in uniform surrounding us with automatic rifles. I remember thinking, "OK they're not bandits but what now?" As we lay on the ground I realized that the likely outcome was one of these young men would forget firearms rule number 3 ("Never put your finger on the trigger or inside the triggerguard until your sights are on the target") and dice us into little pieces. They "patted" us down and the cartridge belt I used to carry my lantern battery worried them considerably. The *Noctilio* in the collecting bag on Don's belt were more worrisome and he encouraged them to be careful. Eventually, they let us up and proceeded to search our truck, finding our shotgun and multiple shells in the process. As you would predict, we had the shotgun and ammunition but not the permit—it was in camp! Nonetheless, things were looking a little more relaxed as they took us to camp so they could examine the permit. What the Federales didn't know, but Don and I did, was that the individual they were hunting had stopped by our camp that morning and pestered us for awhile. In the process he volunteered to sell us some "*mota,*" showed us his revolver, and revealed the proverbial hog-choking role of dollar bills. We declined his offer, being on government time, and gave him a beer for breakfast instead. As he left, clearly not understanding these crazy gringos who would rather skin rats than smoke dope, he tossed a small plastic bag of "seeds and stems" into our tent. And so now I knew I was going to live, but that there was a good chance I'd spend the next few years in a Mexican jail.

Don hit the ground running when we arrived in camp. He told Bob Fisher to bring a tray of specimens and the lantern out of the tent (thus leaving it in darkness), he told me to tell the Mexicans about the bats in the tray, and he went to find the elusive permit. In ten minutes the Federales were gone, satisfied with confiscating our "excess" ammunition, and I was emptying the small bag of *Cannabis* into the stream. As I recall, Don and I didn't sleep until we'd emptied a full bottle of Jim Beam. This same group of Federales encountered our replacement crew (Al and Clyde, still with Fisher) about a month later and regaled them with the story and how it was "just like Hollywood." When I first heard this addendum, I still was having trouble finding anything humorous in the episode; I was really scared that night.

FORT COLLINS

In 1981 (with 3,589 numbers in my field catalog) I was offered an opportunity to take a position as Chief of the Ecology Section of the Denver Wildlife Research Center in Fort Collins. Clyde was now in Denver as Director of the center and Colorado struck me as a good place to stage trips to Baja California. I hated leaving the USNM and my friends there (Figure 3) but the thought of being in the West again was very appealing. I also had this strong feeling, likely misguided, that I could be of more value to FWS in this new capacity. I went out early and worked for a month, then returned to Vienna and picked up Barbara and Justin. They were not greatly enamored of this move and I didn't deal with their reservations very effectively.

I made only one more trip to Baja California, that in March-April of 1982 (Figure 4). Shortly thereafter, Clyde, in a disagreement with FWS, went to Texas Tech University as Director of their Museum. His departure, coupled with President Reagan's new fiscal policies, led to considerable change in the center, including the first round of government "rifs" (reduction in force) I had encountered. Budgets were smaller,

Figure 3. Photograph taken on my last day in the U.S. National Museum. I doubt if all those cups had coffee in them.

travel was more difficult, and I was now coping first hand with supervisors who were markedly less sympathetic to museum-based studies than Clyde had been.

Nonetheless, I enjoyed the next 10 years or so for the most part. I ended up supervising pretty much the same group that Clyde had supervised except we were now a "section" or "branch" rather than a standalone Center. I traveled a fair amount and oversaw research projects from Florida to Alaska and coast to coast. The one thing I did not find much time for was continuing to publish original work in mammalogy and my publication record for those years has lots of book reviews in it. However, I did initiate a project on surveys of mammals on national parks on the Colorado Plateau at that time. Actually the first survey we did was at Dinosaur National Monument (e.g., Bogan et al., 1988) where we surveyed the mammal fauna of the riparian areas of the monument. The park was involved in a reserved water-rights case and they were interested in bolstering their case with new information on the importance of the park to native wildlife. Dick Weisbrod, then of the National Park Service (NPS) was instrumental in getting this trip approved by NPS and Steve Petersburg, the Resource Management Specialist for NPS at Dinosaur was fundamental to trip logistics, often rowing a raft filled with biologists to the next stop. Steve had an impressive grasp of this big park and knew the areas that would likely be of interest to us. Every time he joined us in the field, we were treated to his Dutch oven "chicken cordon bleu." Great times. Bob Fisher frequently joined me on these trips to the park and we put many beautiful specimens into the FWS collection in Fort Collins, which I had overseen since the retirement of Robert B. Finley, Jr.

Figure 4. Somewhere in the deserts of Baja California, 1982.

These park surveys were desperately needed in my opinion. Newmark (1986, 1987) had discussed extinctions of mammals on national parks, including several on the Colorado Plateau, but I was troubled by what I thought were errors and inconsistencies in his database—which essentially consisted of lists of mammals from the various parks. In my opinion, he was naive about the validity of many records from national parks and seemed unaware of new distributional records for other species from parks (see Bogan et al., 1988). I discussed this with various folks in NPS, and hit a resonant note with Bob Schiller in the Denver regional office. His office subsequently provided a small stipend over several years that allowed us to begin mammal surveys on several national parks. From 1988 through 1995 we conducted baseline inventories for mammals at Capitol Reef, Natural Bridges, Glen Canyon, Mesa Verde, Zion, and Bryce Canyon national parks and continued work at Dinosaur and elsewhere as well. Each summer would find us in the field for a couple of months collecting specimens, followed by cleaning and examination of specimens and annual report writing. The field crew typically included Clyde, Bob Fisher, Cindy Ramotnik (who wrote most of the reports), Tony Mollhagen, Jacque Homan, Rick Manning, Ernie Valdez, Bob Finley, and others; the companionship we shared in the field was superb. In addition, we obtained significant new records of mammals from the parks, especially bats, and produced several thousand fine specimens that are in the collection in Albuquerque. I had modest success in convincing my own agency (FWS) they needed to conduct inventories on their refuges, in particular some refuges in Nebraska (e.g., Bogan, 1997).

In the late 1980's (Figure 5), I grew anxious to return to full-time research. By 1993, I was truly fortunate in having two good friends as sympathetic supervisors, Rey Stendell and Tom O'Shea, who were willing to accede to this idea. In addition, a couple of other things came together: Norm Scott of our Albuquerque office was relocating to California, thus leaving a vacancy, and the center had been advised to move the vertebrate collection out of the office building we occupied. Rey and Tom were supportive of the relocation of the vertebrate collection to the Museum of Southwestern Biology in Albuquerque. Cindy Ramotnik, the collection manager, and I (field catalog number now 6,370), were to accompany the collection to Albuquerque. Cindy had moved to Fort Collins in the mid-1980s after working for Bob Fisher in the USNM for several years. She eventually participated in fieldwork in Baja California and on the national parks, while obtaining her M.S. degree from Colorado State University on *Plethdon neomexicanus*. Cindy, now my wife, is an important member of our "team" and without her, our museum activities would come to a stop.

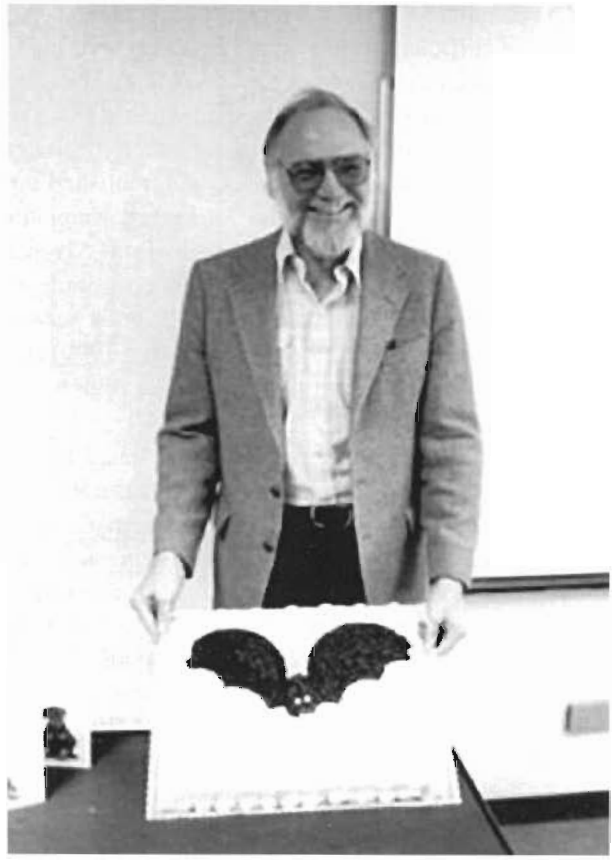

Figure 5. Photograph taken on my last day as Assistant Director, National Ecology Laboratory, Fish and Wildlife Service. The big smile is because I'm anticipating my return to research.

BACK TO NEW MEXICO

Cindy and I moved to New Mexico in November of 1993 and settled into a house in Corrales, a place I'd known since my first visit to the area in 1965. I think, with an exception or two, the last 10 years have been among the best in my career. It's been a treat to be back on a university campus and be able to initiate research projects that appealed to me and that might do others some good. Tom O'Shea and I have collaborated on several projects, including radiotracking bats in the Southwest, and I have continued with inventories of mammals on public lands, especially parks. Best of all have been the students that I've met at UNM: Paul Cryan, Ernie Valdez, Keith Geluso, Alice Chung, Christa Weise, and others. In terms of publications, often because of these same students, this has been a very productive part of my career. My current catalog number is 6,933 (last entry dated 16 October 2002).

SOME THOUGHTS ON SCIENCE

I think scientific intuition really happens, probably more (and more often) for some folks than others. For me, one of the really "sterling" times was when I first saw specimens of the small *Myotis* from the Islas Tres Marias off the coast of Nayarit, Mexico (Wilson 1991). These animals had been presumed to be *M. californicus mexicanus*, based on capture and examination by previous investigators. I had seen the skull(s?) in the British Museum and although they were not in the best of shape I was inclined to believe that they represented *M. californicus*, a taxon poorly known in Mexico. But, when Don came back from the islands with 20 or so specimens of skins and skulls it was just clearly, unequivocally certain that they weren't *M. californicus*. What species they really represented was less certain, although even then I thought they had a resemblance to *M. carteri* from the mainland. I had spent sufficient time examining all these animals and had collected pertinent material in Nayarit and elsewhere and it all just came together when I saw the island animals. I subsequently re-compared the material, performed statistical analyses, and published the description of *M. findleyi* in the *Journal of Mammalogy* (Bogan 1978). Years later, Sergio Ticul Alvarez asked me to write the chapter on vespertilionids of northwestern Mexico (Bogan 1999) for a book he was editing with Jim Patton (Alvarez and Patton 1999) and it was great fun to re-visit the bats of that region.

Another taxonomic problem that has plagued several generations of bat specialists has been the status of *M. occultus* and whether its systematic relationships were best portrayed by assigning it to *M. lucifugus* as a subspecies or whether it warranted specific status (e.g., Findley and Jones, 1967). Valdez et al. (1999) and Piaggio et al. (2002) provide the detailed background but when I first came to New Mexico, Jim and Clyde (Findley and Jones, 1967) had just published a paper strongly suggesting that *occultus* was a subspecies although it was morphologically distinct from its congeners. Barbour and Davis (1970) agreed with this view but later Hoffmeister (1986), in assessing bats in Arizona, disagreed. With this background, Ernie Valdez examined allozymic variation among the groups and decided the parsimonious course was to recommend subspecific status for *occultus* as there were no fixed allozymic differences among samples of *occultus* and *lucifugus* but there was still relatively marked morphological variation. Piaggio et al. then examined mtDNA in the same samples of bats that Valdez et al. studied. The results seem to clearly indicate that *M. occultus* represents an evolutionarily distinct monophyletic lineage and is best viewed as a species separate from *M. lucifugus*.

I've spent some time in the last ten years helping to put miniaturized radiotransmitters on bats to locate (primarily) maternity colonies. This has been great fun and a really hard-working cadre of students and assistants has helped me. When I was a student at UNM, I don't think we thought much about this subject. We were more focused on distibutional questions and systematic relationships. We looked for roosting bats but I don't think we gave much thought to looking at trees, other than perhaps for *Lasiurus* or *Lasionycteris*. Now, thirty years later we have a much better idea of where bats in the West live in the summer, including where critical maternity roosts are located and what the threats to those roosts might be. Furthermore we've been able to "download" this information to land managers (e.g., Bogan et al. 1998). This has been a result primarily of miniaturized radiotransmitters, especially those manufactured by Holohil.

And, speaking of "tree bats," one of the publications I admired as a graduate student was that of Findley and Jones (1964) on migration in hoary bats, *Lasiurus cinereus*. I think I particularly liked it for its imaginative use of museum records, all 300 or so of them, in addition to what it told us about the biology of the bats! And essentially nothing has been published on this phenomenon in tree bats since. Now, Paul Cryan, who just received his Ph.D. at UNM, has amassed a database of over 17,000 museum records of migratory tree bats, fed them into a GIS database, and is assessing various aspects of migration for several of the species. Paul has published two papers, one on seasonal distributions of tree bats that depicts monthly changes in distributions (Cryan 2003) and one on respirometry experiments on male and female hoary bats, demonstrating that females "defend" a high body temperature when challenged with low temperatures, whereas most males go into torpor (Cryan and Wolf 2003). His analysis of stable isotopes in hair of male and female hoary bats, which sheds yet more light on migration in this species, is now in press. It's been really rewarding to watch Paul put all this together and to be able to help a little at times—such as telling him where migrating hoary bats could be found in the spring in New Mexico!

I learned about *Myotis* at the knee of Jim Findley, so to speak, and by reading papers by some of his students, including Art Harris who does wonderful work on recent and fossil mammals. Much of this work has included species such as *lucifugus*, *occultus*, *yumanensis*, *velifer*, and others. For years, I've used the well-established subgenera of *Myotis*, including *Leuconoe* for the bats just mentioned. Jim's paper of 1972 tended, in my opinion, to support subgenera of *Myotis* so that it was reasonable to presume that historically, precursors of most of the known subgenera had invaded continental land masses and then diversified. Indeed, many of these land areas have species of *Myotis* that very much resemble many of our North American species. Now, Ruedi and Mayer (2001) contend, on the basis of mtDNA evidence, that their sample of New World species of *Myotis*, ostensibly representing three subgenera, clustered in a single, well-supported monophyletic clade and thus presumably evolved independently of their ecological equivalents from the Palearctic! This in turn suggests that all those characters and species that we thought represented subgeneric divisions are perhaps the result of convergent evolution.

It's been exciting to be close to some of the studies mentioned above and to be reminded of how the scientific process works—the building of new studies on old, sometimes with expected results and sometimes not. As an offspring of the Grinnell and Hall tree I haven't fallen far from my roots; certainly not as far as some of the more innovative of my elders like Jim or Syd Anderson. Nonetheless, I've really enjoyed what I've done and would surely do it all again, even if I couldn't change anything. I'd go right back down to Cloverdale with Clyde in an instant to chase hares (Bogan and Jones 1975), eat spuds, and drink beer!

With respect to those (Cloverdale) spuds, I have chosen to repeat, in full, the recipe for this unique dish as given in Jones (1987). I can't help but note, however, that the reader will have to be tolerant of Jones' unique writing style(s):

CLOVERDALE SPUDS

Abstract.—A quick-and-easy recipe is described that will provide caloric intake, as well as enhance the productivity and morale of field crews in many kinds of situation.

Introduction

The following field dish may be prepared on either a conventional two (2) burner Coleman stove or a small wood-burning fire with lots of coals. Cast-iron skillets with lids (especially for wood-burning fires) are preferred, although some of the more modern cookware will suffice if there is no other choice.

This recipe is for two (2) hard-working field people; add proportions, respectively, in order to accommodate the dietary needs of additional field personnel.

Materials

Hardware.—Coleman stove (preferred), cooking grate (for wood-burning fire), skillet(s) with lid(s), knife(ves), potato peeler (preferred), fork(s), spatula, plates, and pot holder (T-shirt, bat glove, mouse bag).

Software.—2-5 kg potatoes (4-10 good-sized tubers), 1-3 kg onions (approximately 4-6 roots; white and yellow cebollas are acceptable, purple onions are supreme), 0.5 l cooking oil (shortening, corn oil, lard, bacon grease, butter, margarine, 3-in-one, and 10W-40 have all been used with varying levels of success). For additional information, see the comments about additives in the section on results and discussion.

Methods

Light the fire and preheat the skillet(s) with the oil(s) added. Peel and either slice (preferred) or dice the potatoes directly into the skillet. Turn the spuds frequently and add oil(s) as appropriate. When the potatoes are about one-half cooked and beginning to turn brown, add the peeled and diced onions; continue to turn frequently and add grease as needed. Add salt and pepper in accordance with the taste(s) of the cook(s). When the potatoes and onions appear to be cooked, either turn off the flame or take the skillet off of the fire, put on the lid, and let the ingredients steep for awhile prior to serving.

Results and Discussion

This is absolutely a veritable, delectible[sic], delight of a field meal that may be utilized for breakfast, dinner (lunch), and supper (dinner). Depending on location and conditions in the field camp, this meal may be served either hot (preferred) or cold. Also, depending on the mood(s) of the field crew and the time(s) of day(s) and night(s), Cloverdale Spuds may be enhanced(?) by adding jalapenos (either diced into the skillet while the materials are cooking, split and placed on top(s) of the serving(s), or boiled and served on the side(s), adding eggs either mixed in with the other ingredients while cooking, fried and placed on the top(s) of the serving(s), or boiled and served on the side(s), and adding either Spam or any reasonable substitute (preferred). Each serving should be salted and peppered to taste(s). Depending on a wide array of other conditions in the field, as well as the time of day(s) and night(s), additional additives that may be considered, as appropriate, include Tabasco Sauce®, ketchup, mustard, tequila, bourbon, wine, and vinegar. No matter what the conditions and time(s) of day(s) or night(s), cold beer is a must.

Remarks

This recipe is named in recognition of the geographic locality [Cloverdale, New Mexico; not a place where one can buy gasoline] where it was first concocted and for the major ingredient(s) of the dish. Since its invention, this recipe has been further developed, enhanced, and tested in a wide array of places with the able and thoughtful assistance(s) of person(s) too numerous to mention.

This masterpiece of a dish can be supplemented by tender young kid (*Capra hircus*), appropriately shot and spit, and roasted over an open firepit, preferably on a beach in Baja California Sur. Dr. Jones also has that recipe, although several of us would agree that his temperature setting is too high.

Acknowledgments

This section seems unnecessary, as in many ways this paper has been an acknowledgment of the exceptional colleagues and friends I've known who have helped me along the way. To all of them I say "thanks." My sister Patricia Ann Self helped with family dates and details. My children, Diane, Sean, and Justin, have often borne the brunt of my trips to the field and I appreciate their tolerance of these activities. One person who won't get to read this paper, and who I think would have enjoyed it because of her interest in family history, is Angela Lu Self-Redcross (1971-2001), a dear niece of mine. Angie, this is for you.

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BATS TO BIODIVERSITY: SPYDER HAD A PRETTY GOOD RIDE

Don E. Wilson

Don E Wilson was born in Davis, Oklahoma on 30 April 1944. He received a B.S. in Wildlife Management from the University of Arizona in 1965 and an M.S. and a Ph.D. in Biology in 1967 and 1970, respectively, from the University of New Mexico. He is a Senior Scientist at the Smithsonian Institution's National Museum of Natural History in Washington, D.C.

Preparing a memoir is a daunting task. Faced with baring my soul to a largely unknown (and probably minuscule) readership, I am torn between the usual egotistical desire of sharing every exciting moment of my career and an inherent reticence based largely on the fear that precious few individuals outside of my immediate family really give a rat's behind about any of this. However, gleefully using invitation as justification, I throw myself on the mercy of you, dear reader, and ask only that you not judge too harshly. After all, you could find yourself in similar circumstances in the future.

Childhood experiences undoubtedly play a role in future careers. Interpreting these experiences with the keen vision of hindsight probably exaggerates their importance, but certainly things like a love of the outdoors and early exposure to wildlife are related. I was most fortunate to have been born within months of the end of the second World War, in the great state of Oklahoma (I've never been to heaven, but I've been to Oklahoma - Three Dog Night). I still occasionally awaken in a cold sweat, thinking how close I came to being a Texan. My father, Ellis Wilson, whose name provided me with the middle initial that somehow seems so necessary to scientists, was a construction worker with a healthy and hearty interest in hunting and fishing. His enthusiasm for outdoor life, combined with considerable tolerance on the part of my mother, Thelma Furrow, allowed me to explore my surroundings from an early age, and to develop an abiding curiosity about habitats and organisms.

We moved frequently during my childhood, from the hill country of southeastern Oklahoma to the Republican River plains of Nebraska, the subtropical Rio Grande Valley of Texas, the boreal forests of the Pacific Northwest, and finally to the stunning Sonoran Desert of Arizona. Although not without its downside, this peripatetic existence had a number of benefits. Like the boy named Sue, I grew up quickly and learned to get along with a variety of people. Actually, it was my younger sister who was named Sue; she was, in fact, born in Texas, and joined the family when I was eight years old. I was exposed to a wide variety of people and places in western North America. We lived in trailer houses (modern "Mobile Homes" bear little resemblance to the trailers of my youth) until I was in the sixth grade. Our trailer frequently was parked in close proximity to my father's construction site, in lovely, remote areas well away from population centers (best place to grow up by a dam site).

Undistracted by modern conveniences such as television, computers, telephones, and organized sports, my early childhood was a continuing exploration of the wonderful variety of habitats afforded by our wandering lifestyle. Although sitting on my father's lap and learning to steer his pickup before starting school is probably an experience I have in common with many others, perhaps learning to shoot prairie dogs from the same perch is a bit more unusual. Early exposure to hunting and fishing was invaluable in solidifying my interest in animals and their environments.

Life in the Rio Grande Valley, on the Texas-Mexico border, allowed me to learn playground Spanish at an early age. My best friend and companion in the great outdoors, Rodolfo Perez, gave me access to a completely new and different culture. Rodolfo and I somehow managed to influence our second-grade teacher so strongly that we were allowed to skip the third grade. Perhaps she was scheduled to teach third grade the next year and wanted to avoid problems, but the only thing that we seemed to have missed was Roman numerals, a deficiency that plagues me to this day. I have no doubt that those experiences carried over to my career-long fascination with Latin America, and smoothed the way for me to embrace people and places that might have been forbiddingly foreign otherwise. An early fascination with horned lizards (horny toads to us) led to a series of exciting encounters with rattlesnakes that left me exhilarated and my mother disquieted. Her deep and abiding fear of snakes somehow had the opposite effect on me.

A cross-country move from Nebraska to Oregon when I was in the sixth grade afforded me the first opportunity to systematically record my experiences with mammals. Our small caravan consisted of my father's pickup pulling the trailer house, and my mother's '54 Ford pulling a smaller trailer with two boats. Switching back and forth between the two, I religiously recorded every mammal we saw, dead or alive. Although the 345 jackrabbits remain firmly entrenched in my mind, it was my first look at Pronghorns that blew me away. These amazing animals, spooking in unison when approached too closely, would race off a short distance and then turn to watch us go by, giving me great views of an animal that still looks comfortably familiar to me on each arriving issue of the *Journal of Mammalogy*.

Life in the great Northwest was as close to paradise as it gets. We lived in an actual house, just like all of my peers, during the school year, in the small town of Canby, in Oregon's Willamette Valley. Hunting pheasants, quail, and deer, fishing for trout, crappie, and salmon, and exploring surrounding forests and rivers honed my interests and set the stage for me to continue such fun instead of working for a living. We spent our summers in the trailer, parked at whatever construction site my father was working at the time. Now an independent contractor, he and his partner built bridges for the states of Oregon and Washington, and for large logging companies such as Crown-Zellerbach and Weyerhaeuser. That meant summers spent beside some idyllic backwoods river, with access to a small motorboat and a .22 rifle. It doesn't get any better than that.

Actually, it did get better, when my father retired to southern Arizona. I finished high school in Bisbee, Arizona, smack in the middle of the Mule Mountains in the Southeastern corner of the state, snuggled up to the Mexican border. I rode a motorcycle on back roads and off-roads all over Cochise County. My hunting targets expanded to javelina, and exploring the nearby Huachuca and Chiricahua Mountains and the San Pedro River Valley occupied much of my time. I returned to the Southwest Research Station and the Chiricahuas this past spring, and found it like opening a door to my youth.

My formal education began in a one-room schoolhouse in the middle of nowhere, Nebraska. I was the only kindergartner, and the 15 or 20 other students ranged all the way up to 12th grade. The single teacher, a no-nonsense Cornhusker with a lazy left eye and a rapier-like stroke with a ruler, bundled me in with a handful of first-through third-graders, and I learned more in three months there than I did in the next two years of more normal schoolhouses. I still have a tendency to sit very still and be very quiet when large, raw-boned females are talking.

In addition to on-the-job training in the great outdoors, I was also fortunate to learn to read at a very young age, and to become easily carried away by the printed word. I inhaled books about all kinds of topics, including some that caused significantly raised eyebrows on the part of librarians. My father provided me with two books that had a lasting effect on me. The first was a folio-sized reprint of "Audubon's Quadrupeds". This was my first bestiary, and it survived many an hour of grubby hands riffling through the pages, reading species accounts and fantasizing over the paintings. I imagined myself stalking each of these animals, side-by-side with John James Audubon himself. A complete lack of artistic talent quickly laid to rest any plans of actually following in his illustrious footsteps.

The other book that had an enormous influence on me was "*Killers in Africa: The truth about animals lying in wait & hunters lying in print*", by Alexander Lake. Lake, an early professional safari hunter in Africa, provided some of the best natural history accounts of African mammals ever written.

Recently reprinted, it is a book that should be read far more widely. In addition to guiding big game hunters in search of all manner of trophies, he also led occasional scientists on collecting expeditions. His keen powers of observation and obvious fascination with his prey resulted in accounts that were absolutely riveting for an impressionable youngster, and that remain interesting and factual, even today. When I now lead tour groups of Smithsonian Associates to Eastern and Southern Africa, images from that book spring to mind when I see the animals he described, up close and personal.

My high school years, spread among three different schools in Oregon and Arizona, did little to further my knowledge or interest in natural history. I opted to attend the University of Arizona mainly to avoid joining the military or going to work for Phelps Dodge in the copper mines of Bisbee. Once there, however, the pursuit of knowledge began to seriously work its way into my blood. Having breezed through my public school years without ever cracking a book, I discovered quickly that trying to nonchalant one's way through college resulted in a barrage of "D's" at the end of the term. The beauty of a liberal arts education is that it allows you to find out early on that subjects such as economics and the history of western civilization are abjectly devoid of interest. Botany and zoology, on the other hand, are the real stuff.

My undergraduate years at Arizona served up my first real academic heroes and role models. My second semester of General Zoology was taught by Howard K. Gloyd, a distinguished and stately herpetologist who had retired from the Field Museum with a wealth of knowledge about vertebrate zoology and biogeography. Gloyd fanned the smoldering spark that remained from my childhood interests in all things creepy-crawly. I decided to major in Wildlife Management, a course that offered a practical way to learn about the organisms I was interested in, and also to provide gainful employment at the other end. This course of studies offered the additional benefit of requiring a host of courses that were great fun - mammalogy, ornithology, herpetology, ichthyology, and various other -ologies, while avoiding the bulk of the hard-core math, chemistry, and physics that were required of zoology majors. The course I most remember was zoogeography, also taught by Howard Gloyd. I devoured Philip Darlington's text, and still remember the passage about every aspiring biologist needing to spend time in the tropics.

I was encouraged early on by my advisor, Roger Hungerford, a quiet and patient soul who smoked a pipe and was very much at home in the field. However, the wildlife curriculum depended heavily on courses taught in the zoology department, and as the years passed, I fell under the influence of several of the graduate students there at the time. I was tremendously impressed with these worldly fellows, who let me tag along with them in the field and taught me how to eventually become a successful graduate student myself. I served as an undergraduate lab assistant to John Wright, among others, and it was John who encouraged me to go to the University of New Mexico to work with Jim Findley, the smartest move I could have made, and one I never would have figured out on my own. James Dale Lane taught mammalogy labs for E. Lendell Cockrum, and Robert Bezy taught the lab and field sections of Charles Lowe's ecology classes. Jim Lane showed me how to trap gophers, and how to laugh about everything that life threw your way. Bezy's knowledge of desert plants and animals amazed me, and spurred me to learn as much about them as I could. James L. Patton was among the brightest and best there, and his friendship and willingness to help a hangabout undergraduate were greatly appreciated.

However, the single individual who had the most influence on me in my Arizona years was undoubtedly Alfred L. Gardner. Al was the teaching assistant in my mammalogy class, and dubbed me with the nickname Spyder, for no apparent reason. I learned how to collect, prepare, identify, and appreciate mammals from Al. He taught me to identify bat skulls behind my back, and how to put up skins that were never quite good enough. Al was a mountain of a man with enough natural history experiences from his days as a professional collector in Mexico to satisfy even the hungriest of undergraduates. I followed him around in the field, spent long hours in the middle of the night helping him analyze data for his Master's thesis, and determined to become a mammalogist thanks to his mentoring.

I recently wrote the following account for a volume honoring Jim Patton. I repeat it here, as I think it typifies the unparalleled learning experiences I had access to at Arizona:

"I was an undergraduate at the University of Arizona when Jim was a graduate student there. I was trying to hang around with the big boys, and spent as much time as possible with Jim, Al Gardner, and others who were working on various vertebrate groups. Somehow I wrangled an invitation to accompany Jim and Al to Mexico, my first trip to the tropics. We worked our way down the West Coast, trapping the *Perognathus* that Jim was studying for his doctoral degree. We ended up in San Blas, Nayarit, where our goal was to trap vampire bats for the Arizona-Sonora Desert Museum. The museum was going to pay for the bats, and that was how Jim and Al were financing the trip. We knew we had to feed the bats a couple of times on the way home, even though we were planning to drive straight through. In anticipation of this, we had brought along a huge lab rabbit, which I believe my erstwhile colleagues had purloined from the physiology animal room. Jim assured us he knew how to do a cardiac puncture on this rascal, which would allow us to extract enough blood to feed our bats, but not harm the rabbit. That way we could bleed him a couple of times and get home with everybody alive. Having captured our bats, we headed for home, and made our first feeding stop in a small glade of tropical forest that reaches its northern limit near San Blas. Al held the rabbit, and Jim carefully inserted the needle straight into the heart. Bre'r rabbit gave one heavy sigh and promptly expired. While Jim looked puzzled and disappointed in the rabbit for giving up so easily, Al swung into action and, holding the quite-dead rabbit against a nearby tree trunk, relieved it of its head with a mighty swing of his machete. Exhorting me to hold the dish under it, he carefully captured what little blood would flow from the corpse. Although the amount was small, they efficiently defribrinated it with glass beads, and then fed each of the bats using an eyedropper. Heading on our way, we began to worry almost immediately how we were going to manage another feeding without our involuntary blood donor. Amidst talk of using the lone undergraduate on the trip, I began to come up with a series of alternatives that included buying one of the constant stream of chickens, goats, pigs, etc. that lined the highway. Finally, the next day, somewhere in Sinaloa, we stopped an elderly Mexican fellow who was tooling down the road in a cart drawn by an even more elderly, swaybacked horse. With considerable dramatic flair, my phlebotomist friends explained to the old fellow that we were medicos, and needed to extract a little blood from his horse. Although my Spanish was a bit too weak to understand everything, I could easily tell from his violent head shaking and protestations that he was not convinced of the soundness of that idea. However, when 10 pesos was offered as a small reward, he began to see the merit in it. The next problem was for our crack team to penetrate one of the bulging veins on the horse's legs to actually withdraw some blood. When the first attempts yielding nothing but bilingual cursing, the owner visualized 10 pesos flying away, and he began to point out other veins and suggest other possibilities. Finally, we succeeded in getting a syringe full, and managed another successful feeding. The bats all survived just fine, and provided the wherewithal to pay for the trip. Jim got the necessary chromosomal material from his pocket mice, and I got an education that was priceless."

I left Arizona in 1965 with a degree in Wildlife Management, a saint of a wife who stuck with me through thin and is still waiting for thick, a baby daughter, and a summer job in a fire tower on the south rim of the Grand Canyon. In addition to the excitement of over 200 lightning-caused fires that summer, I put my newly found skills of collecting mice, shrews, and bats to work and explored the fauna of the coniferous forests of northern Arizona.

When I arrived at the University of New Mexico in the fall of 1965, I was convinced that I wanted to work on bats in Mexico. Early and frequent discussions with the good Dr. James S. Findley, my major professor, quickly disabused me of that notion. Although he was favorably inclined to both boats and Mexico, he realized, quite correctly, that it would be expensive and logistically difficult for me to try to do a Master's thesis on them from Albuquerque. He suggested a variety of other possibilities, and one weekend he spent an entire day with me prowling all over the Sandia Mountains talking about the potential distribution of the five species of *Peromyscus* that lived there. His enthusiasm simply bowled me over, and I couldn't wait to start trapping mice. The rest is not exactly history, but it worked out well for me.

Jim Findley was easily the biggest influence on my early career and training. The biology department at UNM in the late 60s was not particularly outstanding, but Findley and his group of students were exceptional. Jim is an excellent scientist with a knack for asking interesting questions and pursuing the answers in a way that is both exciting and fun without being oppressive. Jim allowed his students to develop in the manner best suited to their own individual needs. Many of my fellow students were quite independent, and he gave them plenty of space to find their own way. I was young, impressionable, interested in everything, and oblivious to the proper relationship between student and professor. I wandered into Jim's office regularly and hounded him with questions and pleas for help with whatever I was doing at the moment. He was unfailingly available and helpful; he pushed when I needed a push, and pulled when I needed encouragement.

Jim Findley was a student of romance languages as well as biology, and he instilled in me a love of words and of using them properly. He provided me with careful editorial assistance, and explained why one needed to differentiate between "which" and "that", but that one need not avoid noun modifiers to the extent of "knobs of doors" and "mounds of gophers." He treated me as a colleague in talking with me about his own research, and made it crystal clear that accepting a Ph.D. degree meant dedicating one's life to research. He published frequently himself, and expected others to do the same. He led by example, and I learned to be a zoologist by watching him and listening to him. It was a great pleasure to help put together a festschrift for him on the occasion of his formal retirement (Yates, et al., 1997; Gannon and Wilson, 1997; Geluso and Wilson, 1997), and to dedicate my bat book to him (Wilson, 1997). My graduate training was ideal, thanks to Jim Findley, and I wouldn't change a thing if I could do it all over again. Except perhaps to take Latin so that I could decipher all those scientific names and come up with more useful common names than "Little Brown Bat" and "Orange-spined Dwarf Porcupine."

Findley attracted a cadre of good students, several of whom influenced me greatly during my tenure there. Chief among these was Michael A. Bogan, who started the year after I did, and whose sense of humor and work habits paralleled my own. I admired Daniel F. Williams, who I considered the sharpest student in the department during my stay there. John Darling, an ornithology student, shared an office and hours of delightful late night conversation with Mike, Dan, and me. Kenneth Geluso and Hal Black came towards the end of my time at UNM, but I enjoyed interactions with both.

I finished my Master's thesis in January of 1967 (Wilson, 1968), and prevailed upon Findley to let me work on bats for my Ph.D., after having served my obligatory time trapping mice. During my first year of graduate school, I learned about a course in tropical biology taught in Costa Rica by the Organization for Tropical Studies (OTS), from my good friend and fellow student, Lawrence (Mac) Hardy, a herpetologist who started at UNM the same time I did. Mac also introduced me to the work of Daniel H. Janzen, a young Kansas University professor who was working on the symbiotic relationship between ants and acacias in Mexico. I thought this was probably the most exciting bit of field biology I had ever heard of, and when I learned that Janzen was also associated with OTS and the course in Costa Rica, I resolved to take it. Findley was supportive, as always, and so in 1967 I applied for the course to be taught in February and March of 1968.

The OTS course was an epiphany for me. Acceptance into the course was quite competitive, and I found myself surrounded by extremely bright ecology students from Harvard, Berkeley, Michigan, Stanford, and other bastions of biological knowledge. As the lone skinny kid interested in bats from Desert Outpost U., I felt a certain amount of intimidation early on. Although the course taught me more about how to do science and about tropical habitats and organisms than I had learned in my entire academic career to that point, perhaps the most important thing it did was convince me that I could hold my own in that company. Although my fellow students could teach me volumes about ecological theory, not a one of them could identify bat skulls behind their backs. Although I didn't know the Lotka-Volterra equations from the Fabinacci Series, I had no fear of wandering through the forest in the middle of the night in search of anything that moved, and finding my way back to camp with a dead headlamp. Although I was pretty much a spectator in long-winded arguments about competition and population genetics, I was unquestionably the best shot in the course. The course coordinator, Norman J. Scott, was another influential mentor for me, and he has remained a good friend and colleague throughout my career. The same can be said for Daniel H. Janzen, who did indeed teach in my course, and I made arrangements to do a post-doc with him once I finished my Ph.D. I have continued to work with OTS over the years, serving as visiting faculty on numerous courses, and coordinating a couple of courses, as well as participating in various research activities. For the past 6 years, I have served as Vice-Chairman of the Board of Education and chaired the Education Committee.

The OTS course solidified my desire to work in the tropics, and the timing worked out well, as Findley had an NSF grant to study the genus Myotis worldwide. With his support, I left Albuquerque in June of 1968 with Kate and our expanding family, which now included two daughters, aged 1 and 5. We loaded both kids and all of our worldly possessions into a Volkswagen beetle, and drove to Costa Rica. After spending the summer serving as the Teaching Assistant in the next OTS course, and adding to my expanding dataset on various aspects of natural history of tropical bats (Gardner, et al., 1970; Mares and Wilson, 1970; Gardner and Wilson, 1971), we continued on to Panama, where we took up residence on Barro Colorado Island (BCI), the research station of the Smithsonian Tropical Research Institute (STRI).

My year on BCI was idyllic; non-stop research in an intellectually stimulating environment. I had a colony of 1,000 individuals of *Myotis nigricans* in the attic above my office. I literally lived with them for a year, and gathered a significant amount of data about their lifestyles and habits. The main focus was the reproductive pattern of these tropical representatives of the most widely distributed genus of mammals (excluding *Homo*). We knew a lot about the reproductive cycle of temperate zone *Myotis*, but precious little about tropical ones. I collected samples of 10 males and 10 females weekly for a year, and used that material to determine the annual cycle in some histological detail (Wilson, 1971a; Wilson and Findley, 1970).

Although this detailed documentation had to await a further six months of laboratory work back at UNM, I had a very clear idea of what was happening through my close observations in the colony. The reproductive cycle proved unique, based on our understanding at that time. Females essentially began the calendar year pregnant, and underwent post-partum estrous cycles after at least one, and sometimes two sequential parturitions. This meant that a single female could produce up to three young per year. More surprising, perhaps, was the finding that there was a reproductive diapause that occurred at the end of the calendar year. This meant that there had to be some environmental cue that was signaling both the onset and cessation of reproductive activity.

Clearly the annual cycle of these little insectivores was tracking the availability of food, and that in turn was determined by the annual cycle of wet and dry seasons. The conundrum was that, in order to wean their young at the time of year when insect populations were flourishing, these animals had to "anticipate" that time by about 3 months to accommodate gestation and lactation time. To make a long story short, the entire population began breeding around the time of the winter solstice, and ceased very near the autumnal equinox. Photoperiod had seemed an unlikely proximal cue at the time, because of the minimal differences in daylength at 10 degrees latitude. Verification of the effects of photoperiod on tropical bats remains a potentially rewarding topic for future students. In addition to the work on *Myotis nigricans*, I gathered specimens and data on many other species of tropical bats. I used much of this material in later publications that documented bimodal polyestry in tropical bats, and subsequent studies have shown it to be the norm for many species (Wilson and Findley, 1971).

That year on BCI afforded me a considerable amount of time to pursue tropical biology writ large. I was interested in everything, and managed to accumulate enough interesting natural history observations to keep me busy writing them up for a couple of years afterward (Leck and Wilson, 1970; Wilson, 1970a,b; Wilson and Tyson, 1971; Wilson and Findley, 1972). Another influential person from my Arizona days, Eugene Studier, had taken a job at New Mexico Highlands University, and we had kept in touch while I was in graduate school in Albuquerque. Gene visited me on BCI, and we began a series of physiological ecology studies with bats that was to extend over the next couple of decades (Studier and Wilson, 1970, 1979, 1983; Findley, et al., 1972; Studier et al., 1983a, b, 1994a, b, 1995).

We returned to the U.S. via the Panama Canal Company ship in September of 1969, and drove from New Orleans back to Albuquerque. I spent the Fall of 1969 completing the histological work on my samples, and writing my dissertation. I defended in February of 1970, and began planning my next tropical adventures. I had stayed in touch with Dan Janzen, and he had written me into his new NSF grant as a Post-doc. We left Albuquerque for good in June, 1970, after attending the College Station Mammal Meetings, a legendary affair in its own right.

Dan wanted me to collect seed samples of *Acacia farnesiana*, a shrubby legume, from Texas to Costa Rica, so we drove down through the Rio Grande Valley, and I began collecting samples about every 50 miles, all the way through Mexico, Guatemala, El Salvador, Honduras, Nicaragua, and northern Costa Rica. These dried bean pods have a fragrant odor all their own, and the smell permeated our Volkswagen Camper for the whole trip. This time we had 2- and 6-yr old daughters, and all of our worldly possessions, and we camped out all the way to Costa Rica.

We lived in San Jose that year, in the Apartamentos California in a sunny upstairs flat with a maid named Isabel. I used the camper to travel all over Guanacaste Province, collecting seed samples in support of Dan's research on Bruchid beetles, and other seed predators. In the evenings I pursued my studies of neotropical bats. Ted Fleming and his family lived in an apartment on the ground floor, and Ted and I worked together on a year-long bat community ecology project (Fleming, et al., 1972).

Working with Dan Janzen was another seminal experience for me. Dan came down 3 times during that year, and stayed for about 3 weeks each time. We traveled around the country day in and day out, working and living out of the camper. We collected enormous amounts of material, and Dan worked non-stop while I drove between field sites. He also lectured, questioned, probed, prodded, and spurred me to think non-stop during each of these field episodes. In between visits, while he was back in the states, he would write me long letters each week, suggesting lines of studies that I could set up and follow. Dan had more ideas in one week than I had in an entire year. I managed to do perhaps 10% of them, gathered some data on another 20% or so, and never even got started on the rest. Nevertheless, he was always encouraging, always grateful for whatever I had managed to do, and always moved things along enormously when he was present. I learned so much about tropical biology from Dan that it continued to influence my thinking and my way of doing science for years afterward, and probably continues to this day. We wrote up much of this material over the next few years, when I returned to Washington (Wilson and Janzen, 1972; Downhower and Wilson, 1973; Janzen and Wilson, 1974, 1977, 1983; Wilson, 1983a, b, c).

Part of my NSF fellowship required me to spend a quarter teaching at the University of Chicago, where Dan was now located. He kept putting off this quarter by quarter, and I never did make it to Chicago. The Post-doc was for two years, but Dan encouraged me to look for a permanent job right away, suggesting that I forego the second year if something good came along. I was enjoying myself enormously working with Dan, and so was fairly picky in applying for jobs. I did interview at the University of Utah, but lost out to Jim Brown. That was undoubtedly a blessing in disguise, because in the Fall of 1971, I was hired by the U.S. Fish and Wildlife Service as a curator of mammals in the Bird and Mammal Laboratories at the National Museum of Natural History.

We left Costa Rica in August of 1971, with our VW loaded to the gills and on top, and made our way back to the U.S. on the disaster-a-day plan. The poor old camper had been ridden hard and put away wet for 15 months, and it slowly disintegrated as we worked our way north. We kept finding good local mechanics to bailing-wire it back together and eventually limped our way into Tucson, where we had stored my library and most of our household goods with our parents. We loaded a U-haul trailer with all of our stuff and the heavy books and all pulled the bumper off by the time we drove from Bisbee to Douglas. Taking another tack, we shipped it all to Washington, and I mailed my books via cheap 4th class postage. That was a mistake, as many of the boxes broke open, and I make several trips to a postal service warehouse, picking my books off the shelves holding thousands of others that had suffered a similar fate.

We arrived in Washington in time for me to begin work on September 1, 1971. My new boss was Clyde Jones, the next person to have a positive and lasting effect on my career. Clyde was a kindred spirit in many ways - a fellow graduate of UNM and Findley student, a lover of the field, and a world traveler who had spent a year in West Africa and made a trip to Antarctica. He was the best supervisor I ever had, giving me free rein to tilt at whatever scientific windmills I wanted. He protected me from the bureaucracy, and encouraged me to push the envelope in establishing research programs in Mexico, Costa Rica, and South America (Jones, et al., 1996).

I spent 19 years working in that unit in the museum, and saw its name change many times, and my own position change almost as frequently, as I took on an increasing administrative load, while continuing my research. The National Museum is the closest thing to perfect as one can imagine for someone like me. The collections are unparalleled, the library facilities are among the best in the world, and support, both in-house and out-house, is readily available to anyone willing to go out and get it. My interests had expanded rapidly into ecological themes during my graduate work, and continued along those lines while post-docing with Janzen. Now those interests returned to systematics and biogeography, as I settled into learning more about mammals in an eclectic fashion.

I now had time to keep up with the literature, which I did assiduously. A paper on birds in Systematic Zoology encouraged me to attempt an analysis of trophic guilds of bats on a worldwide basis (Wilson, 1973a). Once I was established in the museum, and had written up most of my holdover material from the past few years, Clyde arranged for us to work with Jim Findley to finish up the *Mammals of New Mexico*. This was a long-term project of Findley's that Art Harris had contributed heavily to during his student years with Jim. Art had completed a first draft, Jim wrote up all the species accounts, and Clyde freed up my time to finish things up. My job was to examine the thousands of specimens in the USNM collections - collections that Vernon Bailey and his Biological Survey colleagues had gathered, as well as Mexican Boundary Survey material from E. A. Mearns and others. This I did, compiling lists of specimens examined, and making hundreds of dot maps using old-fashioned drop-bow compass and India ink. It was a labor of love, and seeing my name on there with Findley, Harris, and Jones was a thrill (Findley, et al., 1975). The project generated some spin-off systematic studies for me as well (Wilson, 1975).

I continued field work in Costa Rica (Timm et al., 1989) and Panama, working with colleagues in OTS (Wilson, 1990*b*, 1991*b*; Rodriguez and Wilson, 1999) and at the Smithsonian Tropical Research Institute. I gathered additional material for my studies on bat distribution and reproduction (Wilson, 1979, 1989, 1996), and on a variety of other topics as well, including toads, lizards, and beach vegetation (Wilson, 1973c; Wilson and LaVal, 1974; Wilson and Pine, 1974; Scott et al., 1975). Karyotyping *Ectophylla alba* afforded me the first opportunity to work with Robert Baker and his students (Greenbaum et al., 1975).

Jim Findley and I did a really interesting study on *Thyroptera tricolor* while teaching in an OTS course (Findley and Wilson, 1974; Wilson and Findley, 1977). That led me into a broader interest in the Family Thyropteridae, and I took advantage of subsequent trips to Brazil to ferret out some systematic problems (Wilson, 1976a; 1978). Jim and I continued to collaborate on subjects of mutual interest over the years, including trying to infer the ecological significance of bat morphology (Findley and Wilson, 1982, 1984).

In the early 1970s, I was approached by the National Park Service to study the declining population of *Tadarida brasiliensis* at Carlsbad Caverns, back home in New Mexico. I immediately made the smart decision to hire Kenneth N. Geluso as a Post-doc to oversee the project in the field. Ken was a recent graduate of UNM, and a fellow Findley student. We recruited Scott Altenbach, a UNM faculty member and long-time colleague, to work with us on the project. With Scott's ability to engineer a photographic solution to counting the bats in the cave, and Ken's hard work and conscientiousness with everything he approached, we were able to make rapid and significant progress on the problem. We homed in on the likely effects of pesticide poisoning early on, and through my Fish and Wildlife connections with the Denver Wildlife Research Center, we were able to get the necessary analyses done. Ken went to Denver and learned the techniques for doing these analyses, so that he could actually perform them himself. To make another long story short, we documented the negative effects of DDT and its metabolites on these bats during their semi-annual migrations (Altenbach et al., 1976). We learned a lot about *Tadarida brasiliensis* in the process (Wilson et al., 1978; Altenbach et al., 1979; Geluso et al., 1980).

Working for the Fish and Wildlife Service carried with it an obligation to work on species of concern to the service, especially where systematic expertise was needed. This afforded me the opportunity to work on Polar Bears, Wolverines, and Sea Otters (Wilson, 1976b, 1982; Wilson et al., 1991a). I also gained invaluable experience in curating a major collection of mammals (Wilson et al., 1987; Wilson, 1988, 1990a, 1994).

Al Gardner and Mike Bogan joined the FWS unit in the museum in the early 70s, and we formed a nucleus of like-minded individuals anxious to do field and museum studies on a variety of mammals. Clyde Jones, as our supervisor, helped make that possible through ample funding and support at all the right times. Mike and I worked with Henry Setzer, our fellow curator, on a knotty systematic problem with old world *Myotis*, and we recruited Findley to help as well (Bogan et al., 1978).

I had long harbored the ambition to work on the mammals of the Mexican state of Nayarit, stemming from the first early trip to Mexico with Al Gardner and Jim Patton. I called upon Al and Mike and Clyde to join me in this endeavor, and they all thought it a great idea. With Clyde's support, we began field work in the mid-70s, and over the years managed to amass about 5,000 specimens from all over the state. We drove down many times, took small planes to hidden airfields tucked high in the mountains, and even took a sailboat to the Tres Marias Islands and Isla Isabela off the coast. The field work was tremendous fun, and we learned a lot about the habitats and organisms of that beautiful little piece of Mexico (Figure 1). We also generated some legendary stories, such as the time Mike Bogan and I got overrun by the federales while running mist nets in the middle of a marijuana growing area.

The mammals of Nayarit became one of those career-long projects that has never quite seen the light of day. In spite of generating a considerable number of publications (Diersing and Wilson, 1979; Engstrom and Wilson, 1981; Carleton et al., 1982; Wilson, 1985, 1991a; Engstrom et al., 1987), the definitive work on Nayarit mammals remains a figment of our imagination. I suspect it will require the retirement of one of us, and a renewed determination to finish it up. Come to think of it, I think I need to make a couple more trips down there to tie up a few loose ends before I can write everything up.

From 1975 to 1980, Al, Mike, and I joined the late Charles Handley in establishing a long-term monitoring program of *Artibeus jamaicensis* on Barro Colorado Island, Panama. Handley was a consummate naturalist with extensive knowledge of the mammals of Panama. The four of us served as field crew leaders on many trips to the Island, working with a long series of volunteers and field assistants to mark 9,000 bats and document 16,000 recaptures during this time period, while maintaining year-round netting efforts. Summarizing the data generated from this project yielded the first intensive look at the demography and natural history of a neotropical bat (Handley et al., 1991a, b, c, d; Studier and Wilson, 1991; Wilson et al., 1991; Gardner et al., 1991).

Living and working at the National Museum brought me into contact with a wide variety of excel-

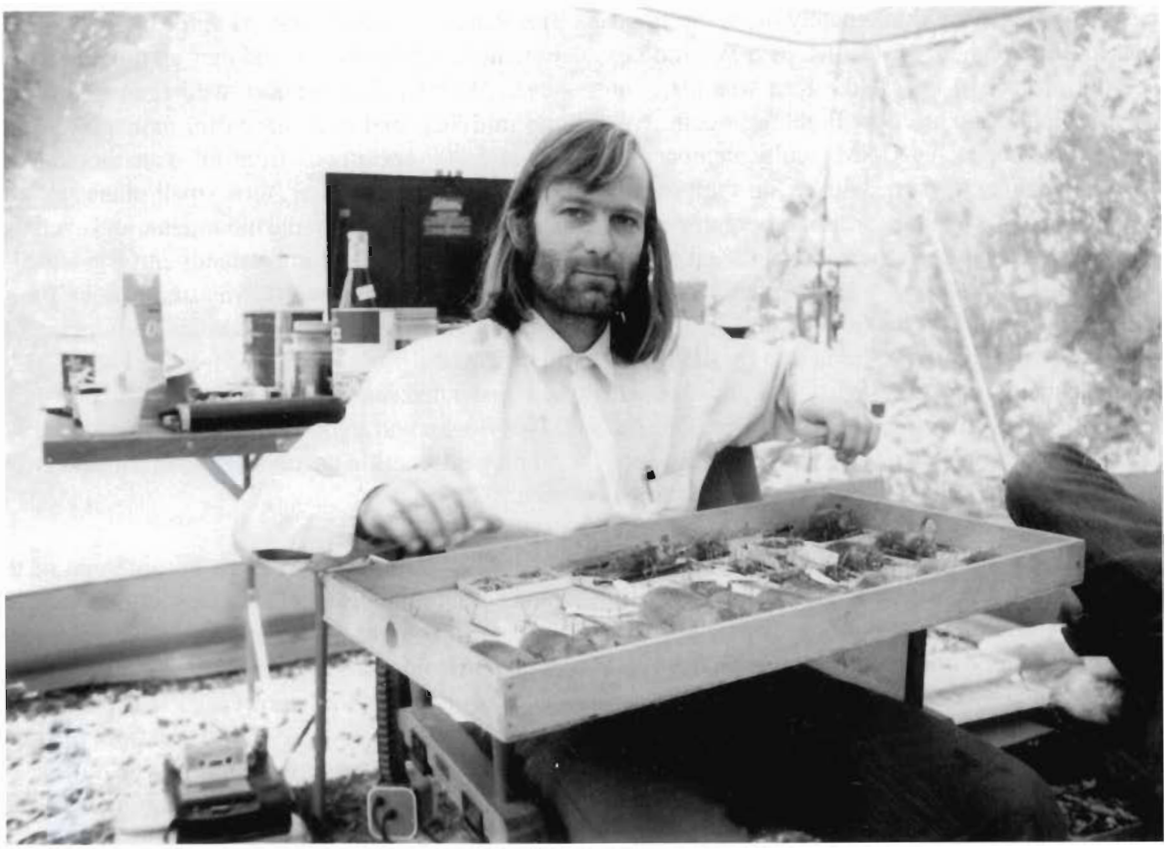

Figure 1. Always the traditionalist, Don E. Wilson pins mammal specimens while doing fieldwork at San Blas, Nayarit, Mexico in 1975.

lent biologists. I have always valued collaboration, and have gained enormously from being blessed with many (98 co-authors, to be exact) excellent collaborators over the years. Perhaps no better example exists than John Eisenberg. John headed up the research unit at the National Zoo when I arrived in Washington in 1971, and remained there until moving on to an endowed chair at the University of Florida some years later. John spent much of a sabbatical year in the museum, and that allowed us to begin a successful series of collaborations that included field work on his projects on howler monkeys in Venezuela. John took me along as a hired gun on several occasions, when I helped his field crews by shooting monkeys with a dart gun. We also worked on a variety of papers using the museum collections to look at brain size and correlate it with a variety of life history characteristics (Eisenberg and Wilson, 1979, 1981; Wilson and Eisenberg, 1990).

Eisenberg's student, Chris Wemmer, was finishing up at the University of Maryland when I arrived in Washington. We struck up an immediate friendship that has lasted 30 years, and resulted in some fine adventures in the field in Burma, and riding our bicycles from DC to Gainesville, Florida to raise money for the Future Mammalogists Fund of the American Society of Mammalogists. We even managed to do some useful science together from time to time (Wemmer and Wilson, 1983, 1987; Wemmer et al., 1993).

Another good example of this type of collaboration was with the paleobiology curator Clayton Ray. Clayton came by one day with an assortment of fossil bat bones that had been recovered from cave deposits near Terlingua, Texas. We were able to identify them as *Macrotus*, and Robert Baker was kind enough (and organized enough) to provide me with an original dataset that he had used to figure out the systematics of

Macrotus some years before. Using that comparative material, as well as considerable additional material from our collection and others, we were able to correctly allocate the bones, and make some useful inferences about the evolution of *Macrotus* in the process (Ray and Wilson, 1979).

Similarly, contact with Joe Chapman resulted in some systematic work on rabbits (Chapman et al., 1983; Dixon et al., 1983). I enjoyed working with James H. Brown on a review paper for the American Society of Mammalogists (Brown and Wilson, 1994), in conjunction with the 75th anniversary meetings of the society, which we hosted at the Smithsonian.

Early on, I began developing contacts with a variety of students throughout Latin America, and many of these resulted in joint publications covering myriad topics (Mok et al., 1982; Wilson and Gamarra, 1991). I had the great good fortune to meet Rodrigo Medellin, Hector Arita, Gerardo Ceballos, Daniel Navarro, and many other excellent students in Mexico. Our work together over the years yielded excellent times in the field, and many publications on a variety of topics (Navarro and Wilson, 1982; Ceballos and Wilson, 1985; Medellin et al., 1985; Wilson, 1991c). Rodrigo and Hector were invaluable to me when the Fish and Wildlife Service wanted surveys done on *Leptonycteris* to determine their conservation status (Wilson et al., 1985). They accompanied Dirk Lanning and me all over Mexico in search of these bats, and provided excellent company, intellectual stimulation, and an enduring friendship.

I have also benefited greatly from a long series of U.S. students who have worked with me over the years. These include participants in our summer research program for undergraduates, students from various colleges who have come to me through recommendations of colleagues, and walk-ins who have somehow found me on their own. They have provided excellent intellectual prodding in an environment that otherwise lacks this interchange, and we have also managed to publish the results of several joint studies (Hudson and Wilson, 1986; Lewis and Wilson, 1987; DeFrees and Wilson, 1988; Lassieur and Wilson, 1989; Ferrell and Wilson, 1991; Miller and Wilson, 1997; Owen-Ashley and Wilson, 1998; Colket and Wilson, 1998; Helgen and Wilson, 2001).

Beginning the in the late 80s, my research horizons were broadened by an opportunity to conduct surveys on Pacific Island flying foxes. Trips to Samoa and Fiji with my Fish and Wildlife Service colleague John Engbring introduced me to the world of these fascinating, king-size bats. We were able to pull together historical data and combine it with the results of our on-the-ground surveys to build a much better understanding of the status of the two common species on those islands (Wilson and Engbring, 1992; Wilson and Graham, 1992).

In 1989, the late Elmer Birney, who had succeeded me as President of the American Society of Mammalogists, asked me to take over production of the second edition of *Mammal Species of the World* (Wilson, 1993*a*, *b*, *c*, *d*, *e*, *f*). That began a long-term project that continues to this day, as we are now in the throes of producing a third edition of that work. I had the great good fortune of encountering DeeAnn Reeder early on in the process, at a time when she was interested in obtaining challenging work before entering graduate school. DeeAnn stayed with the project through graduate school and raising her family, and the resultant publication owes a tremendous amount to her dedication and hard work (Wilson and Reeder, 1993). This ongoing project has greatly expanded my own interest in the phylogeny of mammals of the world (O'Brien et al., 1999).

In 1990 I accepted a position as Director of Biodiversity Programs for the Smithsonian Institution. This was an easy move to make, as I was able to stay in the Museum, and even to maintain my research office in the Division of Mammals. I just added an administrative office to oversee eight programs involving inventory, systematics, biogeography, and monitoring in various parts of the world. This opened a new series of excellent collaborations, mainly with students from Latin American who were working on our projects (Ascorra and Wilson, 1991, 1992; Ascorra et al., 1991a, b; Wilson and Salazar, 1990; Lim and Wilson, 1993; Wilson et al, 1999; Wilson, 1999). Abelardo Sandoval and I summarized about 10 years work in Peru in a volume on Manu National Park (Wilson and Sandoval, 1996a, b; Ascorra et al., 1996; Wilson et al., 1996).

One important project that occupied a few years of my time was the organization of a consortium of institutions in and around DC to deal with mutual concerns for Systematic Biology. We put together a symposium to address what strides had been made in the field of biodiversity since the publication of E. O. Wilson's 1986 seminal volume. Ed then helped us edit the proceedings into a "Biodiversity II" volume (Reaka-Kudla et al., 1996a, b), thereby creating a modicum of Wilson and Wilson bibliographic confusion.

F. Russell Cole, an important collaborator and friend, joined me on a sabbatical from Colby College in the early 90s, and came back for another 7 years later. Russ and I have worked on a variety of projects over the years, and he has also sent several good students my way, who have joined me on other projects. Russ was instrumental in helping to finish the editorial work on a book on Standard Methods for the study of mammals (Cole and Wilson, 1996; Inger and Wilson, 1996; McDiarmid and Wilson, 1996; Wilson et al., 1996a, b), a book on common names of mammals (Wilson and Cole, 2000), and various other projects (Cole et al., 1994), some of which are still underway.

At the urging of Mary Taylor and Jim Patton, who was then President of the American Society of Mammalogists, I embarked on a project to produce a book on North American mammals. Again, by serendipity, I gained an exceptionally competent co-editor, who has continued to work with me on a variety of projects (Ruff and Wilson, 2000). Sue Ruff was between projects and agreed to join me on the North American mammal book early on. This was a particularly rewarding project, as I managed to involve over 200 of my colleagues in producing *The Smithsonian Book of North American Mammals* (Wilson and Ruff, 1999). Interestingly enough, Sue and I had pitched the book to the National Geographic Society early in the process, and they had rejected it when we insisted on covering every individual species in North America. About two years after turning us down, I got a call from an editor in their book department, explaining that they were doing a book on North American mammals, and wanting me to write some chapters for it. At first feeling stung that they had co-opted our project, upon reflections, I agreed to do it, just to keep tabs on what they were doing. Their book turned out to be quite a superficial summary, focused mainly at the family level (Wilson, 1998a, b, c).

After 10 years of running the Office of Biodiversity Programs, I gained a reprieve in 2000. A reorganization within the museum moved the various programs to other units in the Smithsonian, and I became a Senior Scientist, back in the Division of Mammals. This gave me the freedom to pursue my own research interests, and I immediately embarked on a variety of new book projects.

The last major book project I want to mention is called *Animal - the Definitive Visual Guide to the World's Wildlife* (Burnie and Wilson, 2001). I became involved in this effort through my continuing attempts to be a good citizen of my Institution. We have a relatively new office of Product Development and Licensing, which finds good matches for the Smithsonian in the private sector. This joint effort with Dorling-Kindersley, a major publisher and distributor of educational offerings, allowed me to involve 14 of my colleagues in our newly formed Department of Systematic Biology in helping to review all of the non-mammalian material in the book, which is considerable.

After an amazingly satisfying career, I am now at retirement age, but with no intentions of retiring at the moment. I am in the enviable position of having few responsibilities and a free hand to pursue my own research and publishing interests. Currently, I am working on a book on mammal conservation with Russ Cole, a Field Guide to North American Mammals with Roland Kays, A Field Guide to the Mammals of China with my fellow Senior Scientist Bob Hoffmann and several others, including Andrew Smith, Wang Sung, Xie Yan, Chris Wozencraft, and John MacKinnon, and a new series of Handbooks of Mammals of the World with Lynx Editions in Spain. That should keep me gainfully employed and out of trouble for the next decade or so. All in all, I would have to say that it has been a pretty good ride.

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E. Lendell Cockrum: His Twenty-Twenty Hindsight

E. Lendell Cockrum

E. Lendell Cockrum was born in Sesser, Illinois, on 29 May 1920. He received a B. Ed. in the dual majors of Education and Zoology from Southern Illinois Normal University (now Southern Illinois University) in 1942 and a Ph.D. in Zoology from the University of Kansas in 1951. He is Professor Emeritus of Zoology at the University of Arizona.

I think that my parent's backgrounds had a major influence on my life. However, 81 years, a long-standing history of myopia as well as relatively recent bilateral cataract surgery may have affected my hindsight. Is hindsight ever 20/20? But here goes:

My parents were the products of farmers in the Ozarks Foothills of Southern Illinois whose ancestors arrived in the region in the early 1820's from the Carolinas via Kentucky--attracted by land available for homesteading in the new State.

"Book learnin" was never a strong point, especially in the Cockrum clan. An "X" is the usual signature on homesteading records. Spelling varied from time to time, depending on the recording clerk. The 1840 US Census asked, "how many in this family can read and write?" "None" was the usual answer.

By 1887 my Grandfather Cockrum had inherited some of these homesteads. He had 14 children, 8 by his first wife and 6 by a second wife. My father, Ernest Elmer, was number six in the older lot. Two older brothers died at birth, two were girls--leaving my father as the "number two son." He attended school when farmwork permitted until he had finished grade four. By that time (1906) there were 9 children (4 boys, 5 girls). My father, age 10, and his one older brother, age 14, became full-time farm laborers for their father (no more formal schooling) helping support the ever-larger family.

My father continued to work on the farm until 28 May 1918 (age 22) when he "entrained" for U.S. Army service in World War I. By 28 July he was in Europe in time for the Meuse-Argonne trench-warfare activities. He was gassed, shell-shocked, and hit by shrapnel. Soon thereafter he was shipped back to the United States to recover. He was discharged at Camp Grant, Illinois on 26 July 1919 over 4 months before Armistice was signed 11 November 1919.

Seven days after his discharge, he and my mother were married.

My mother, Alta May Quillman, also the product of a farm family, had graduated from the eighth grade. World War I caused a teacher shortage - especially for "country schools". My mother obtained a temporary teaching certificate by repeating the eighth grade and attending a six-week "Institute" held in the County Seat by Southern Illinois State Teacher's College (now Southern Illinois University, Carbondale).

My father returned to being a "hired hand" on his father's farm. Just over 9 months later (29 May 1920) I (Elmer Lendell) was born. Soon afterward my father became a sharecropper, operating on someone else's farm for a share of the crops.

During the next 11 years, two sisters and a baby brother were added to our family. During the Great Depression years of the 1930's, we never had much money but we never starved. Farming and gardening produced most of our nutritional needs.

I attended 4 or 5 different one-room country schools (outdoor toilets, no running water), the last four years at Flatts School near Benton, Illinois (Figure 1). Mrs. Anna Johnson drove out from town to be our teacher. When I was in the eighth grade (1934), she hired me to be the janitor (keep the furnace going,

Figure 1. Young E. Lendell Cockrum (front row, fifth from left), in bib-overalls on the steps of his one-room schoolhouse.

haul out ashes, sweep the floor, clean the blackboards) and paid me $5.00 a month of her $45.00 pay! Since she was the only one that I knew who had a monthly cash income, I wanted to become a one-room country school teacher!

I attended 3 different high schools - in Benton, Greenville, and Sesser, Illinois. I took Biology class and I wanted to be a High School Biology Teacher! At Greenville, my biology teacher (William [Billy] Bills) was conducting research for an advanced degree (entomology) at the University of Illinois. I spent as much of my time as I could in the biology laboratory and helped maintain his cultures of the Dutch Elm Scale insects - the subject of his research.

That summer (1937), we moved to Sesser, Illinois. Within just a few weeks, Dad's failing health caused him to be admitted into a Veteran's Hospital for the next 3 years.

We had no money! I could find no work, not even part time. Others in my age group had gone to the Chicago or Detroit area (wherever they had a relative), unsuccessfully seeking work.

Our mother kept encouraging us to continue our education. I attended The Goode-Barren Townships high school in Sesser for two years. During my senior year, I applied for admission to the CCC (Civilian Conservation Corps, a federal relief program for young males in poverty-stricken families, operated by the US Army and working from camps on Forest Service and Soil Conservation projects. Pay: room, board, clothing, medical expenses and $30.00 a month, $25.00 of which was paid to the parents for their use).

I also applied for a NYA job (National Youth Administration, another federal relief program, this one for young males and females in poverty stricken families) being offered to students at Southern Illinois Normal University [SINU].

During the summer after graduation (1938) serious consideration was again given to seeking factory work in Chicago or Detroit but we had heard of no successes. I had no relatives in the area that I could live with until I had a job.

I did manage to get two local temporary jobs. One was plowing corn with a walking cultivator behind a team of mules on a farm near the edge of town. I walked out to the farm. Pay? $0.75 a day, bring your own lunch.

During the peach harvesting season, I picked peaches. Pay was based on the number of bushels picked. I worked hard.

The summer ended. I had heard nothing from either the CCC or the NYA. Registration for the fall quarter at SINU was about to begin. My mother encouraged me to pursue my dream of being a high school biology teacher.

I hitch-hiked the 36 miles to Carbondale, shared a bed in a rooming house (cooking privileges in the basement) with a classmate from Sesser, and with my $35.00, reported to the University (across the street!) for registration.

This was before the days of the SAT and other college entrance exams given in high school. We attended orientation lectures in the University auditorium, took an admission examination and were assigned to a professor's advising group.

By Friday I had heard nothing about my NYA job application and was scheduled to pay the registration fee ($18.75 per quarter for those agreeing to teach in Illinois, 1 year for each at SINU). With the $20.00 a month (25 cents an hour) from NYA I could not stay.

I gathered up all of the paper that I had accumulated and went to my assigned advisor, Dr. C. H. Cramer, a history professor. I laid my accumulation on his desk and told him that I was going home. "Why?" "No money. No NYA." "Wait a minute." He looked up my scores on the admissions examination. I was in the lowest 10% in knowledge about entertainment and current events! Fortunately, I was much-much higher in other measures and was in the top 10% in science and mathematics.

He called the woman in charge of the campus NYA program. "No, he was not one of those chosen for one of the few new openings. Yes, she could authorize one more. Where does he want to work?"

I had been impressed by the small "Museum of Natural and Social Sciences" on the campus. "The Museum." "Report to the Director, Fred Cagle, and tell him that you have been assigned there."

This was probably the major happening that resulted in my career as a mammalogist. In my senior yearbook (The Oblisk, Southern Illinois Normal University) Dr. Cramer wrote: "Best Wishes. I am glad that things turned out the way they did."

Three or four months after enrolling at SINU, I received orders to report to the County Seat (Benton) for induction into the CCC.

From Dr. Cramer's I went to the museum. The Director, Fred Cagle had, some time previously, arranged for a WPA (Works Progress Administration - another federal relief program, this one for poverty stricken families) "Museum Restoration Project." The staff included a project supervisor, a secretary and various craftsmen (carpenters, artists, taxidermists) devoted to preparing new museum displays.

Cagle was there but had not expected me. I was the only NYA student assigned to the museum. In the next week or so, I managed to pass Cagle's admission test: I "fleshed-out" a large, frozen, well-aged *Polydon spatula* (Spoon-billed Catfish from the nearby Mississippi River) for the "bug-box". I have no record of its' size but current literature report "usually 40 to 60 pounds" and "up to 7 feet in length and weight of 160 pounds." My 20-20 hindsight reports that Cagle's fish probably held the all-time world's record! Some time later the museum staff prepared the skeleton for a museum display.

Fred Ray Cagle was a native of southern Illinois, a SINU graduate, and a graduate student under the herpetologist Dr. Frank Nelson Blanchard, at the University of Michigan. In addition to fulfilling his academic appointment duties at Carbondale, he was gathering data for his dissertation: "The Life History of the Slider Turtle *Pseudomys picta troostii*..." that was published in the *Ecological Monographs* (20:31-54, 1950).

I soon became almost a full time weekend field assistant, especially during the spring and summer. Drainage ditches in the Mississippi flood-plains (about 25 miles from SINU) gradually dry up during the summer concentrating large numbers of Slider Turtles in the few remaining pools. We would fill the trunk of Cagle's club coup with turtles, take them back to campus where they were marked (by grinding a notch in a place of the plastron and/or carapace) in a numbering system. They were photographed, weighed, measured, and returned to the capture point and released. As the years went by, recoveries were more and more common, often at new pools and sometimes in other drainage ditches.

Cagle was also assembling a research collection of Reptiles and Amphibians from two counties (Jackson and Union). Specimen preparation, labeling and cataloging were soon added to my museum duties.

The Museum had been established in the 1870's at the time the state Legislature authorized a teacher's college at Carbondale. It was to accumulate natural history materials for broadening the experiences of the future teacher. One of the first directors was George Hazen French, an active collector of herbarium materials and an insect taxonomist. By the early 1900's he had accumulated over 5000 plant specimens and a large collection of insects that was stored in the glass-topped, cork-bottomed drawers of standard museum cases. French was a recognized authority in microdopteran taxonomy and described several new species.

Sometime in the early 1900's French was retired and the one-roomed museum was locked. For years, various faculty members had been assigned the chore of opening the museum for class visits.

In the early 1930's the museum was moved to new quarters and a director was appointed. The herbarium had survived but the insect collection was destroyed by dermestid beetles. It was a collection of insect pins, each with small neat labels giving collection data. Several had an additional label on red paper and labeled "type." The red-labeled pins were to be saved.

Needless to say, this permanently influenced my attitude concerning scientific collections, as distinct from display collections.

During this time, my father had been released from the veterans hospital and had a job working in a WPA road improvement project - the usual pick, shovel and wheelbarrow labor intensive procedure - near Sesser. While digging in a roadside marshy area, they encountered a few strange critters living in crayfish burrows. One was sent to me in Carbondale via a classmate who had been home for the weekend. This was a marginal record for *Siren intermedia*, and the basis of my first scientific publication (*Copea*, 4:265, 1941). Obviously Cagle was the major "behind the scenes" force in the production of this note!

During the summer, a colony of bats inhabited the attic of "Old Main," a three-story campus landmark. The attic had low ceiling at the eaves and high ceiling at the "ridge poles." Flooring had been installed over the third-floor ceiling beams. One Fall, guano odors raised the tempers of the faculty inhabiting third floor offices and classrooms. Physical plant employees were assigned to "eliminate the bats." Cagle learned of this and assigned me to go on the expedition and gather some dead bats for the museum taxidermist to prepare for displays. Needless to say, the mops and brooms could not reach the clusters hanging between rafters at the top of the attic. Some were killed and much of the accumulated guano was removed. Both adult females and young were killed.

I was fascinated. I had never seen so many bats and knew almost nothing about them. What kind were they? Where were the males? How many young did each female have (a lot more youngsters than adult females had been killed)? Since they were reported as being absent in the winter - where did they go?

As soon as I could, I headed to the font of all knowledge - Wheeler Library. Of course, answers to all my questions were to be found in some book or journal in the library. By this time the librarians had given up on me and let me go into the stacks to do my own searching! I found almost nothing. Probably the bats were a maternity colony of *Myotis lucifugus* - the Little Brown Bat. Dr. Mary Guthrie had begun reporting on their reproductive cycle, based on females taken from hibernals in the Missouri Ozarks.

I knew what I wanted to study - natural history of bats. Cagle secured some USFWS bird bands and I was in business. These activities resulted in junior authorship (with Cagle) of two *Journal of Mammalogy* articles.

During my senior year at SINU Cagle returned to Michigan to finish his PhD. I was appointed as "Student Director" of both the Museum and the WPA project, reporting to Dr. Joseph Van Riper, Chair of University Museum Committee. By this time I had been accepted for admission by the University of Michigan Graduate College.

In December, 1941, Pearl Harbor caused major changes in my life. My Draft Board ordered me to report for a physical. Knowing of my father's army experiences in WWI, I became a "Draft Dodger." I joined the US Naval Reserve V-7 officer training program that permitted me to finish my college degree at the end of the academic year. That summer, I was employed by the U.S. Public Health Service, working on a malaria mosquito control project at a nearby ammunition "Powder Plant." The Publich Health Officer in charge of the program thought that I should seek a transfer from the USNR to the PHS service and eventually seek an advanced degree in entomology.

November 10, 1942: I reported for active duty in the USNR. After being commissioned in March, 1943, I was sent to the Bureau of Ordinance Mine Disposal School. From there, I was assigned to their research and development station at Solomon Island, Maryland that was devoted to the development of "better" [= deadlier] naval mines. I had several duty assignments, ending up - finally, being transferred to the Hospital Corps. After a crash course in Epidemiology at the Bethesda, MD, Naval Hospital, I was assigned to an Epidemiology unit in "Lion Nine" (a group being trained for the invasion of mainland Japan). Included was a six-week course at Lido Beach, Long Island, NY, so I would know how to survive landing on an enemy shore from a LST (swimming in full uniform, complete with backpack...). Fortunately for me and untold hundreds (thousands?) of others, the atom bomb and VJ Day occurred before the invasion was launched. My Hospital Corp group did go to Japan where we were stationed at the Japanese Naval Base at Yokosuka on Tokyo Bay. My bed was in a room across the hall from that of Dr. Robert Eadie, a mammalogist from Cornell.

In November 1943, while on leave from the U.S. Navy Mine Warfare Test and Training Center at Solomon Island, MD, I went to Pleasant Hill, IL—across the Mississippi from Hannibal, MO, where my college sweetheart, Irma Schutte, was teaching high school. I talked her into marrying me. We were married in Hannibal (Missouri had no waiting period) on December 9. Little did I realize at the time the major role she was to have in my life.

We had two sons (David Lendell, born while I was still on active duty, USNR, Ward Andrew born while I was in graduate school) and a daughter (Sandra Sue, born in Arizona).

Irma gave up her career opportunities and became a "stay-at-home-with-the-children" mother. Our sons attained their doctorate degrees and are successful in their fields, primarily, I think, as the result of Irma being there when they needed it.

Dave is Academic Vice-President at Sul Ross State University, Alpine, TX; Ward is a reading specialist in the Northern Arizona University Center of Excellence in Education at Flagstaff. Tragically, our daughter died at age 9. More about this later.

Irma willingly took over the duties of homemaker, treasurer (with almost no money available in the graduate school days - the GI Bill and a graduate research assistantship), bookkeeper, typist (including all of the versions of my doctorate on The Mammals of Kansas, and classroom materials in the early days at the University of Arizona, (there being no departmental secretary!). We have just had our 60th wedding anniversary.

I was in Japan with the Epidemiology Unit, when I had "accumulated enough points" (= served enough time) to be released to inactive duty. However, some pressure was put on me to stay in for another year. The bait - promotion to Lieutenant Commander. However, I did not bite and was released to inactive duty as Lieutenant, USNR, in February, 1946!

By this time, I had decided to pursue an advanced degree in zoology, driven by continuing interest in the natural history of bats. From Japan I had contacted the University of Michigan concerning the availability of an apartment (wife and one child) and financial support from a teaching or research assistantship (the GI bill provided barely enough to support one student with no dependents). A more-or-less form letter stated that no out of state students were being admitted—even though I had been accepted before my "Cook's Tour" in the Navy!

Dr. Eadie had told me that Cornell (his school) was not accepting any out of state enrollees. Other mammalogy centers were the University of California at Berkeley, and the University of Kansas (Dr. E. Raymond Hall had recently transferred from Berkeley, back to his undergraduate almamater). Berkeley had no hopes of cheap housing for a married graduate student and no openings for assistantships. A reply from Kansas simply stated - no openings.

Back home in Little Egypt (southern Illinois), I borrowed my father's car and Irma and I drove up to the University of Illinois at Urbana. The Assistant Dean of Men had been a classmate of ours at SINU. He could find no leads for an apartment for us. If I enrolled, he would try to get us into the first available vacancy. Entomology was one field of interest to me (no mammalogy). Perhaps I would end up in Public Health Service after all. The Illinois State Museum (on campus) would hire me as an assistant - "pinning bugs!"

Imagine our discussions and thoughts on the way back to southern Illinois. Maybe I should forget the GI Bill opportunity and find a job teaching biology in a high school. After all, I still wanted to be a teacher.

Upon arrival back in Irma's home town (Pinckneyville) we found a telegram awaiting me. The University of Kansas had decided to give the applicant from Little Egypt a chance. They offered a Teaching Assistantship and had housing available for me and my family at nearby Sunflower (temporary housing for employees of a closed WWII powder plant operation)—complete with bus service available to and from campus each day. If I accepted, come on out the next week (beginning of the spring semester). Actually, the influx of Kansas veterans had forced the last minute establishment of additional laboratory sections - thus a dire need for more TA's.

I went west via the railroad. Irma and our young son (David) came out some days later.

I was accepted as a candidate for a Master's degree. I was assigned office space in the museum [the Museum of Natural History, commonly called Dyche Hall at this time] and duties as a Teaching Assistant for Dr. Byron Leonard in Embryology. I enjoyed teaching!

Very soon afterward, I was administered an admissions examination—a la Fred Cagle at Southern Illinois University. At the foot of the elevators in the basement level of Dyche Hall were some large heavy wooden containers filled with numerous elk hides preserved in a strong brine solution. They had been taken by Professor Dyche (who established the museum) from a herd in Minnesota. Even though the tanks had been tightly sealed, apparently over the years some slime mold or bacteria had evolved to the point that they were thriving by feeding on the elk pelts.

The Curator of Mammals, Dr. Donald Hoffmeister, rounded up some peon graduate students to help empty the stinking, rotting mess, transferring it to a truck just outside the door. At least once during the transport of a load, I tried to add to debris by emptying my stomach. "Dry heaves" soon took over.

Since his arrival at the University of Kansas with Dr. Hall, Dr. Hoffmeister had been accumulating information concerning the mammals of Kansas. I did not know that he had recently applied for the position as Director of the Museum at the University of Illinois.

During the "spring break" Dr. Leonard, classmate Henry Setzer and I visited some bat hibernals in northwestern Arkansas.

Natural history of bats was still my preferred research area. I know, I know—Natural History is not now considered to be Science, so I would now try to fake it by calling it Autecology!

At the end of the semester, I was called into the office and informed that I should skip the Masters degree and go directly for a Ph.D.

I would not have a degree in Science! My undergraduate degree was a Bed (Bachelor of Education). Thus I would not qualify for the elite group of real scientists: BS (found in barnyards), MS (More of the Same), PhD (Piled higher and Deeper).

I would be a teacher!

I later learned that all applicants were admitted into a Master's program—no matter how many MS degrees they already had. If you were told to skip the MS, you soon found that the MS was a terminal degree for you. You would not be accepted into a Ph.D program.

The Ph.D program would lead to a major in Zoology—not "mammalogy", "ornithology" or other limited parts of Zoology. All enrollees were required to spend a summer at some marine biology field station as well as taking such diverse courses as paleontology, invertebrate zoology, etc. We were to be Zoologists - not mammalogists who had little background in Zoology.

That summer, Professor Hall organized and led a summer field course to Wyoming, emphasizing the collection and preparation of specimens—especially the smaller mammals. I was in the course.

While we were camped at Fort Bridger, Wyoming, Dr. Hall was notified that Hoffmeister had accepted the position at the University of Illinois. While there, Dr. Hall offered me a position as Assistant Curator, Mammals, with the proviso that I undertake writing a book on The Mammals of Kansas as my doctoral dissertation. At the time I did not know that Dr. Hoffmeister had to leave his accumulated notes for his assigned Mammals of Kansas book.

I held the Assistant Curator position for two years—performing curatorial duties and accumulating data for THE BOOK. Evenings and weekends were devoted to trying to learn enough German to pass the required reading exam. During the summers, I lead the Wyoming field trip.

Back as a full time Graduate Student, I had a Graduate Research Assistantship assigned to duties associated with the project that eventually led to the two-volume Hall and Kelson's *The Mammals of North America* (Hall and Kelson, 1959).

Sometime during this period, two more major crossroads were met. Different decisions would have ended my teaching career.

Dr. Remington Kellogg, of the U.S. National Museum [now the National Museum of Natural History] contacted Professor Hall, seeking someone to fill a vacant position as Curator of Mammals. The position could be filled by a veteran without all of the usual Federal Civil Service exams, etc. Unknown to me, they discussed me as the victim. I could finish my doctorate at the nearby University of Maryland and the almost completed *Mammals of Kansas* could be my dissertation. Dr. Hall's proviso was that I must complete the degree in two more years or return all of my data and notes to him.

However, I wanted to Teach, not curate—especially in Washington (only a few miles from Solomons, Maryland, where I had spent much of my USNR career).

The other crossroad was more spectacular. I was sharing "office space" on a wooden deck in the Dyche Museum tower with (Dr. to be) John White. A couple of flights of wooden stairs on overhead opened out to the top of the decorative tower. At Christmas, physical plant employees had installed several strings of Christmas tree lights. Some malfunction (overloaded circuit?) triggered a fire. Several graduate students, including my "office" mate, John, were still in the Mammal Range, performing the "air of industry as well as industry fact at all times" edict—working on their dissertations.

At that time, Irma and I and our two sons lived in Sunnyside (WWII army barracks converted into apartments for married graduate and young faculty). Someone called me, reporting the fire.

All of my notes for THE BOOK, as well as an almost finished final draft were in my office. Irma had spent untold hours typing and retyping various versions of the manuscript.

I ran up the hill and up the steps to the base of the tower, only to discover that John White had carried my files to safety.

In hindsight, if John had not done this, I might have quit the PhD rat-race and might have become a CEO of some major corporation, complete with my own jet plane, etc.

In June, 1951, I was awarded my degree. Only limited job opportunities were available. The most attractive was to stay at the University of Kansas for another year as a Post-doctoral Research Fellow, assigned full time to the Hall and Kelson *Mammals of North America* project.

In the spring of 1952, a position opened up as an Assistant Professor in Zoology at the University of Arizona. Teaching duties were to include two laboratory sections (50 students each) in General Zoology and lectures and laboratory in Mammalogy in the fall. In the spring, the lectures and one laboratory in the second semester zoology course (mainly vertebrate anatomy) and a lecture course in Zoogeography.

In addition, the recently established US Fish and Wildlife Unit (assigned to the Zoology Department) wanted a book on *The Mammals of Arizona*. Part of my job was to produce such a report.

So much for my dreams of studying the Natural History (Autecology) of bats even though I was in a state that recorded more kinds of bats occurring at least part of the year than any other State in the Union.

The department had no departmental secretary. Any needed office supplies were doled out from a drawer in the Department Head's Desk. Irma continued to type various needed class materials as well as the Mammal book and the various books and papers that I published (see attached list).

The department had no graduate teaching assistants. Actually the total graduate enrollment was one Masters candidate. Apparently they had never had a Ph.D candidate. A faculty member and two upper division majors taught a laboratory section in the General Zoology courses.

At times I wondered why I ever wanted to TEACH.

I applied for and was awarded a small NSF grant to spend the next summer visiting museums with major holdings of mammals collected in Arizona (Biological Survey, USNM, University of Michigan, Chicago Field Museum of Natural History). We "farmed out" our two sons with Irma's parents and spent the summer examining specimens and measurements and examining field notes.

By this time I had learned that Dr. Hoffmeister had begun a long term, detailed study of the mammals of Arizona. I decided to put together an overview of the current records for the state (a key to species, literature records, specimens examined, taxonomy, and distribution maps). The specimens examined lists were based mainly on the notes that Irma and I amassed in the summer of 1953. Literature records and taxonomies were compiled during the limited available time in the next several months. The resulting 276 pages were published in the University of Arizona Press in 1960.

Dr. Hoffmeister's continuing studies of the mammals of the state were published as an outstanding book, *Mammals of Arizona*, in 1986 by the University of Arizona Press and the Arizona Game and Fish Department (602 oversized pages).

In the fall of 1953, I accepted the first PhD candidate in the department. Dr. Keith Justice had applied for and been awarded a National Science Foundation Predoctorate Fellowship.

Between 1953 and my retirement in 1985, 23 students completed the MS degree and 33 finished the PhD under my direction. I emphasized to each that they were getting a degree in Zoology, not Mammalogy. There are not many jobs available for full-time Mammalogists but there are several opportunities to TEACH zoology.

My field work (evenings, weekends) was mainly devoted to the study of bats.

Between 1952 and 1966 we (at first, Irma, and later, graduate students and any available volunteers and I) banded about 200,000 bats, mainly Mexican Freetails. Financial support had become available, mainly from grants from the National Science Foundation. Modifications of the Constantine Harp Trap (now sometimes called the Tuttle Trap) enabled the capture of large numbers of these Molossids as they left a day roost for the evening flight.

On April 25, 1957, a daughter, Sandra Sue, was added to our family. In 1963, she suffered the first of a series of syncope attacks (stopped breathing, no detectable heart beats). Mouth to mouth, rush trips to a hospital and spontaneous recovery before we got there. Examinations by local cardiologists and even examination by a Pediatric Cardiologist from a Boston Children's hospital failed to reveal the cause. On the morning of August 15, 1966 (when nine years old) she had not gotten up for breakfast, Irma found her dead in her bed. I had already gone down to the campus.

At an autopsy, the pacemaker of the heart was removed and, together with copies of some of her EKGs, all were sent to Doctor James at the Henry Ford Hospital in Detroit. He was trying to find the cause of the Sudden Infant Death Syndrome. Soon there was a response to our Cardiologist.

Sandra had died of the newly recognized Lange-Nielsen Syndrome, recognizable by an elongated Q-T interval in an electrocardiogram (EKG). The Q-T is the time lapsed between a pacemaker impulse to contract and the recovery of heart muscle membrane potential (necessary before the muscle can properly react to the next pacemaker impulse).

Stimuli to contract before membrane potential has recovered results in muscle fibrillation. No "heart beat" can be detected, blood circulation ceases and lack of oxygen to the brain causes syncope. If normal heart beats are not soon restored, death results.

Even worse was the report that the condition is inherited. Since we had two older sons, we thought that any of their children might have the same condition. However, we knew no similar deaths of children in either of our families. We ask near relatives to send EKG records. None showed a QT elongation.

Encouraged by our Cardiologist, I took Sabbatical Leave from the University and, with a portable EKG recorder, Irma and I went to Detroit to discuss the case with Dr. James. We then went to southern Illinois, where many of our relatives lived.

We made EKG recordings and asked about syncope attacks and/or sudden deaths in children. We compiled genealogies of our families based on oral histories; Church and County Court House records of birth, marriage, and death records; examination of tombstones—looking especially for early deaths of children; and U.S. Census Records.

Later, we visited with Dr. Lange-Nielsen in Norway (who had first reported the syndrome) and Dr. Peter Frogett, in Belfast, North Ireland, who had reported additional cases in Ireland.

We found no convincing evidence that the condition existed in our families. In the past few years, we learned of the SADS (Sudden Arythmia Deaths Syndrome) foundation. Their research has identified a suite of genes that variously interrupt the repolarization process of heart muscles, resulting in QT elongation, syncope, and even sudden death, not only in children but also in adults. We turned over copies of the EKGs, and our genealogy records, for their analysis. They agreed that they contained no evidence that the syndrome occurred in our family. They confirmed that Sandra's death was caused by the syndrome and concluded that it was a *de nova* mutation.

Further, they have found that the syndrome can be controlled by the "beta blockers"—heart medications that have been developed since Sandra's death!

After our sojourn into medical genetics, I did not continue the bat-banding studies. Rather, I became involved in some North African studies—both mammalogy through the Smithsonian; and developmental agriculture triggered by a year spent on USAID-sponsored project at MIT [Massachusetts Institute of Technology].

Then I sunk to even lower levels—I spent eight years as head of the recently formed Department of Ecology and Evolutionary Biology. At age 65 in 1985, I managed to get out of that position by retiring.

In summary, I never aspired to be a Mammalogist, I wanted to TEACH. Feedback over the years from a number of former enrollees lead me to think that I managed to do a decent job for a number of people. At times, I could not help to think that it might have been more fun in a one-room country school.

My most personally rewarding achievement was our medical genealogy study. Irma and I found that the inheritable QT syndrome that killed our daughter was probably a *de nova* mutation that would not affect the lives of our two sons.

I was more of a Rafinesquan Naturalist than modern Mammalogist. As a teacher, I am proud to admit that I did manage to guide several students into becoming major contributors to the field. I often told my graduate students that, for me to serve as their major professor, they had to sign an agreement with two provisions. First, they were to give me 10 percent of their first year's salary or 1 percent of each year's salary. I am the only major professor I know whose students never earned a dime!

The second proviso was that they were not to accept a job within 1000 miles of me. After all, I could not take their competition!

As to the future of mammalogy, the ever-increasing modifications of planet Earth by the growing human populations are going to provide many interesting studies of the effects on mammalian populations. Current laboratory findings will be even more applied to lives of mammals in their behavior and survival in nature. Naturalists will have their four-wheel drive, first class furnished campers, cell phones, lap-top computers, dry ice and many other modern gadgets not available to their tent-dwelling ancestors.

IF YOU DON'T CLEAN UP YOUR ACT, YOU'LL END UP AT TEXAS TECH

Robert J. Baker

Robert J. Baker was born in Warren, Arkansas on 8 April 1942. He received a B.S. degree in Biology from Arkansas A&M College (today the University of Arkansas at Monticello) in 1963 and an M.S. degree in Zoology under the direction of Dr. Bryan P. Glass from Oklahoma State University in 1965. He received a Ph.D. in Biology from the University of Arizona under the direction of Dr. E. Lendell Cockrum in 1967. Baker did a sabbatical at Harvard University with Dr. Rodney Honeycutt in 1986. He is Horn Professor of Biology and Museum Sciences at Texas Tech University. Other positions at Texas Tech are Director of the Natural Science Research Laboratory and Faculty Athletic Representative. This chapter is dedicated to Laura, Bobby and April.

My youth was spent in southern Arkansas, primarily in Bradley County, which lies between the confluence of the Saline and Ouachita Rivers. This region is under-populated and the economy is driven primarily by farming and lumber, mostly from cotton, tomatoes and Loblolly pines. My father was killed in World War II and I do not remember seeing him. I spent a large amount of time with my grandparents (Fig. 1) on their farm (100 acres with a small stock tank and crops: cotton, tomatoes, sweet and Irish potatoes, black eyed and purple hulled peas, and beans). My grandparents never owned a car or a TV, and days were spent plowing with a mule, chopping cotton, and tending the farm and associated cows, mules, chickens and pigs. This rural setting played a major role in my development of an interest in animals and the local biodiversity. Fishing for large mouth bass and blue gill in the stock tank and hunting squirrels were favorite pastimes (Fig. 2).

The most influential individual in my life was my mother's mom, Grandma Rosie. She was my best friend. She taught me many things and gave me a value system and a perspective of life. She made me feel special. Grandma Rosie read the Bible daily and practiced its teaching more than anyone else that I've ever known – but not with a hell fire and brimstone attitude. She was an honest, hardworking, pleasant and kind lady. I'm sure if there is a God, Grandma Rosie has an honored position there at God's right hand. Much of who I am is derived from Grandma Rosie's influence. I remember many conversations about values and what was right. Most of these were not lectures but simply pearls of wisdom. On one occasion

Figure 1. The author at Grandma Rosie's house in northern Bradley Co. Arkansas, circa 1944.

Figure 2. Squirrel hunting and fishing were a major focus of my teenage years. Here is a bobcat treed by my dog Sport on a squirrel hunt when I was 16 years old.

when I must have been eight or nine, I made some ugly comment about some black people who were helping us pick cotton. Grandma Rosie heard me and later that evening she took me aside and her comment was, "Bobby, you should remember that when their children hurt, they hurt too." I got the message.

Another individual that had a strong impact on my life was an English teacher, Mrs. Kelly. You may rest assured that my academic skills in English were never even minimally acceptable, but this lady spent lots of time talking to me. She assured me that I had all of the potential to become a successful person, not an opinion that many others had shared with me. Also, a football coach named Mickey O'Quinn was tremendously significant in my developing a strong self image. We lived eight miles outside of Warren, and my parents made it very clear that if I did any extracurricular activities at school, I had to find my own way home. Coach O'Quinn drove me home most evenings after practice. He spent time helping me visualize how things worked and how I could choose a rewarding life. He was an extremely demanding disciplinarian, and I flourished in that environment. Teachers like Mrs. Kelly and Coach O'Quinn have a great influence on the lives of their students and in so many ways, they are the heroes of our society. I shall forever be indebted to them.

I spent one year at Ouachita Baptist University in Arkadelphia, nearly ending my college education. My nine weeks grades were 15 hours of F and 2 hours of D. I finished the year with 35 hours of C and 2 hours of B (ROTC). I transferred to the University of Arkansas at Monticello (then Arkansas A&M) and took my first Biology course under Professor W. C. Hopgood. Mr. Hopgood had a grading scale where 79-100 was an A and no one made an A in that course that semester. I then took his Comparative Anatomy course and decided I was going to make an A. His tests typically had only five questions: two questions were easy, one was reasonably difficult, one was virtually impossible, and the fifth was impossible. In Comparative Anatomy, I needed to make an 85 on the final lab exam to make an A. He assigned us three different books with variations in the nomenclatural systems of the brain and the nervous system to learn for the final. For his lab test you typically brought in the animals you had dissected, he would look them over and ask questions. For probably the first time in my life I had literally committed to memory the chapters in those three books that he assigned and his first question was some structure in the brain of a sheep. I looked at him, and in a most assured tone, I told him that the structure that he had just asked about was not mentioned in any of the assigned books. He smiled and said, "Yeah, I know but I expect a little extra out of my A students." My 80 on the final got me a B. The third course I took from him was Parasitology and the textbook was Asa Chandler's classical text with over 400 pages of detailed taxonomy, structures and drawings. I was consumed with a need to make an A in a Hopgood course. I read and outlined the book in its entirety before the first class and went back over the entire book a second time before the first test. On the first test he asked his typical array of questions, again with two that were almost impossible. The ultimate question came from a figure legend on page 454. When he handed back our test and noted my 100, he smiled and said he was glad to see that I finally had taken an interest in my education. The benefit of that exercise in frustration was that I learned that I could remember a lot of information by reading and studying. From then on I made A's in all my classes, except for a damn French course. I certainly don't think that Mr. Hopgood's methods would have benefited many students and, indeed, he failed a huge number of my classmates that took his class, but that challenge to make an A in one of his courses changed my perspective and success in academia.

The final individual that I wish to acknowledge from my undergraduate days was a fish biologist, Dr. Claude M. Ward. Dr. Ward had just finished his Ph.D. at Oklahoma State under the direction of Dr. George Moore. Dr. Ward wanted to do a survey of the fish of southeastern Arkansas, so he and I seined every pothole, rapids, etc. that we could find within a hundred miles of the campus. He assigned me an undergraduate problem to collect the local mammals, which was the most fun I had ever had in college. As it turned out, it was fairly easy to find most of the shrews and rodents that should be there, but the bats weren't so easy. I spent nights with my car parked beside a water hole so the headlights illuminated the water surface shooting bats, or more accurately, shooting at bats as they came in to drink. I did not learn about mist nets until 1965 at the University of Arizona. Dr. Ward was like a father figure to me. He never gave me as much praise as I desired and I worked hard to obtain his approval. One night, while collecting bats, he assured me that one day I would be more pleased with catching a bat than I would be with killing a trophy deer. My response was what an incredibly stupid idea. Now, every time I catch a bat that pleases me, I remember Dr. Ward's vision. This was particularly true one night on Guadeloupe with Hugh Genoways when I took two undescribed species out of the net. That night, I knew that there was a high probability that neither of these taxa had been described and I remember thinking about Dr. Ward and wished he could have been there. These two species are *Eptesicus guadeloupensis* and *Chiroderma improvisum*. Dr. Ward died in the 1970's with Lou Gehrig's disease.

Dr. Ward encouraged me to apply to Oklahoma State and to work on a Master's degree with Dr. Bryan Glass. Dr. Ward convinced me that I had a future in academia and that I should do graduate work. Without him, I would probably be cutting trees or coaching sports in southern Arkansas. At Oklahoma State, I worked on the systematics of *Myotis subulatus*. My Master's work was no giant step for science and I never published the results, primarily because it was poorly written and I had not developed the organizational skills adequate to deal with things like specimens examined and data analysis. At Oklahoma State, I made all A's because I had learned how to extract information from textbooks and lectures as a result of my experience with Mr. Hopgood. While at OSU, Dr. Adolf Stebler was head of the Federal Coop Unit and he taught me six hours of special problems, during which we read Alle, Emerson, Park, Park, and Smith, *Principles of Animal Ecology* and Leroy Dice's book *Natural Communities*. I would read a chapter and Dr. Stebler would make me review and critique ideas in each chapter. This exercise was a very helpful experience in a time of substantial growth in thinking as a scientist. Another aspect of my education at Oklahoma State was that I taught labs in Mammalogy, Herpetology, Comparative Anatomy, Freshman Biology, Physiology and Invertebrate Zoology. Invertebrate Zoology was not something that I knew much about, but Dr. Troy Doris, who was the professor for the course, became ill during the semester and the decision was made to have me give the lectures. Obviously I was not prepared or qualified, and I virtually memorized the book as a form of self-defense because I was only a day or two ahead of the students in the course. Working with Dr. Stebler and teaching such a diverse number of courses prepared me for the incoming exams at Arizona. I did well enough on those exams that I was assigned only one course as leveling work for my Ph.D and that was a Paleontology course on Cenozoic Mammals.

After my first year at OSU, I called Dr. E. Lendell Cockrum at the University of Arizona. As I recall, the phone call was made on the 1st of September. I explained to Dr. Cockrum that I wished to work on a Ph.D. with him and he promptly dressed me down for not planning ahead and trying to get into the University of Arizona the week that classes started. Finally, I made the point that I was calling about enrolling a year from now and what I wanted to do was to write an NIH pre-doctoral fellowship proposal to be submitted through the University of Arizona. Dr. Cockrum became supportive and appeared a little shocked that I was planning a year ahead. I wrote the first draft of the pre-doctoral proposal and sent it to him. What I got back had no sentence even similar to what I had written in my original proposal. After several exchanges of drafts, the proposal was submitted and ultimately funded. If Dr. Cockrum had not been so helpful in developing the proposal, I'm absolutely sure that it would have never been funded. My family and I moved to Arizona in May of 1965. When I got to Arizona, things were a bit disarrayed as the U of A had never received an NIH pre-doctoral fellowship and they were kind enough to let me spend all the overhead money that accompanied the grant (I bet they don't do that today). Another aspect that would never happen today is that although I submitted the NIH pre-doctoral proposal through the U of A, I never applied for admission to graduate school, so when I arrived with my funding in hand, they simply let me enroll in classes and get on with my graduate education. In my second year at Arizona, the graduate school detected that I had not been formally admitted and they decided that I needed to apply for graduate school and to take the GRE exam, which was required for admission. I did both and my scores on the GRE exam were so low that I did not meet their admission standards. My quantitative scores were okay, but my verbal scores were abysmal. By the time they got my scores, I had already passed my comprehensive exams, written my dissertation and was within a month of defending it. The Graduate Dean called a meeting with Dr. Cockrum, the admission personnel and me to discuss the dilemma that had resulted from my low GRE scores. After some debate about compromising standards, the Dean said something like this: "Let me get this straight, this student isn't qualified to be admitted to graduate school but he's made A's in all his classes, he's done his research and written his dissertation in two years and has a couple of papers *in press*? I recommend we admit him to graduate school." The bottom line is that I was a very hard worker but my GRE scores accurately reflected my poor command of vocabulary and Basic English. It truly has been a struggle to master enough English, grammar and vocabulary to properly do my job as a scientist, professor and editor. I think it's safe to say that if I had applied to graduate school at the University of Arizona through normal channels and before I arrived on campus, I would never have had the privilege of working with Dr. Cockrum and attending the U. of A. My own GRE scores have always made me sympathetic toward students with marginal test results, and a number of my more successful students were admitted to Tech with substandard GRE scores.

My Ph.D. dissertation used karyotypic data to reconstruct a phylogeny for the nectar-feeding bats in the family Phyllostomidae. Jim Patton and I overlapped at the U of A and even then he was an exceptional karyologist and evolutionary biologist. We made several field trips together including one to the Padagonia Mountains to catch *Thomomys* at a hybrid zone. Dr. T. C. Hsu accompanied us on one of the trips and it was an incredible learning experience for me. Jim was clearly more advanced than I in his thinking of experimental design and scientific methods and I tried to learn by listening and watching.

The karyotypic resolution at this time was non-differentially stained karyotypes, which means we had diploid number and fundamental number as well as size and shape of the general karyotype. The major conclusions in my dissertation were that nectar-feeding was diphyletic in New World, leaf-nosed bats and that *Carollia* was more closely related to *Choeronycteris* and that *Phyllostomus* was more closely related to *Glossophaga* than *Choeronycteris* and *Glossophaga* were to each other. Of course today, we know that those relationships are not accurate, but alas, I still have my Ph.D. I published my dissertation, which was reasonably well accepted. Soon, however, I discovered that my conclusions were incorrect and it was difficult for me to convince the rest of the mammalogy community that I was incorrect. Resolution of the deep branching patterns in the phyllostomid bats has been one of my dreams and for several years, I wondered if I would ever know the answers. Our recent publication (Baker et al., 2003) of two gene trees that were reasonably congruent with each other gives me some confidence that we are near to the right answers. This publication (Baker et al., 2003) gives me a feeling of closure of my Ph.D. dissertation.

When I graduated from the U of A, there were three jobs that were advertised that I felt qualified to seek. One was at the University of Wyoming, the second was at Tennessee Tech and the third was at Texas Technological College. I did not make the short list at the University of Wyoming, but had interviews at the other two schools. I chose Texas Tech because they had a Ph.D. program, whereas Tennessee Tech had only a master's program in Biology. I joined the faculty in 1967 before Tech had granted its first Ph.D. in Biology. The first was awarded in 1969 to Herschel Garner whose dissertation was directed by Dr. Robert Packard. Dr. Packard was a strong proponent for the future of Texas Tech University and we spent lots of time talking about mammalogy. He had recruited me to apply to Tech at the American Society of Mammalogists meetings in Long Beach, California in 1966. He encouraged me to apply for a job to teach Histology and that is the position that I ultimately obtained. At Long Beach, Dr. Packard told me about his vision for a Texas Society of Mammalogists, where faculty and students from Texas universities could meet, learn about each other's graduate students and research interests, and have student presentations. Dr. Packard pushed for the development of the Texas Society of Mammalogists before his untimely death in 1979. The Texas Society of Mammalogists held its 21st annual meeting this last year (2004) in Junction, Texas.

Since my arrival at Texas Tech University in 1967, my research interests have embraced fieldwork to collect specimens to be archived in a museum and the employment of methods such as karyotyping, in situ hybridizations, G and C chromosomal banding, starch gel electrophoresis, restriction enzyme site mapping, DNA sequencing, construction and probing of cosmid and plasmid libraries, and differential expression of genes. All experimental procedures were designed around voucher specimens deposited in museums to better understand mammals at the organismal level. I believe that there are not enough voucher specimens in museums, especially with archived tissues to adequately understand systematics, genetics, zoonoses, phylogeography, evolutionary processes, etc. I appreciate that there are many mammalogists and zoologists who think that individual animals should rarely be sacrificed for science. But I strongly disagree with this conclusion because I believe that basic knowledge, such as how many species exist, where are the species boundaries, effected population sizes, documentation of breeding strategies, fitness values, etc., are necessary for conservation efforts, wise management of our biodiversity, as well as understanding the value of various forms of life for esthetic and economic uses. These and other critical questions can be answered best with voucher specimens that are documented by a complete database including locality, time, ecological data, etc. The molecular revolution of mammalogy has provided us with a chance to tease out the answers to many questions that were previously unanswerable. Although there are many publications on mammals, my opinion is that much remains to be discovered. I'm certainly not proposing indiscriminant collecting; rather I'm proposing continued growth of systematic collections that are archived in accredited collections that will be available presently as well as for future generations. Within each species, there needs to be reasonable sample sizes (probably about 20) from a variety of geographic locations. While there are exceptions, most populations of mammals are vibrant and more young are born than can survive and collecting a few specimens will not have a negative impact on the species. In cases where there are exceptions, collections need to be more carefully regulated or perhaps even prohibited.

As a major advisor who believes in building collections and that fieldwork is critical to the education process, I feel that all of my students should contribute to the building of museum collections. During the past few years, most individuals have spent a month to three months in the field. Field trips require a lot of time preparing for the trip as well as time to clean up and to get back into normal university productivity. Here is a dilemma that I'm not sure how to answer. If my students do fieldwork while students in other degree programs who are addressing some of the same issues do not, the students who do not do fieldwork have more time to sequence DNA, write papers, and build a publication record than those who do fieldwork. I am reluctant to loan tissues to individuals that have not been active in building tissue collections that are in accredited museums. Each loan request is decided on a case by case basis but my basic premise is that people who truly exploit tissue collections should contribute to the development of those collections. Museum collections (vouchers and tissues) have been built at a tremendous cost and labor. There is a need to develop an equitable system where individuals who have not and do not plan to contribute to the collections of tissues can have access to the large tissue collections but this access should not be to the disadvantage of the scientists and students who built their collections through fieldwork. This is an incredibly complex issue that needs to be addressed by coalition of curators who supervise such collections. Another museum issue is the observation that many molecular mammalogists are not associated with museums and if they build a collection of tissues, they are stored in their ultra cold freezers in their lab. Even if voucher specimens are deposited in an accredited museum, the specimens in their ultra colds are usually not available to the broader scientific community, searchable on line in a database, and perhaps worst of all there is no commitment to perpetual care for the tissues beyond the employment of the individual scientist.

With all that said, there is another dimension to the cost/benefit ratio of fieldwork. I love fieldwork and every hour I spend setting nets and traps, taking bats out of the nets, running trap lines, and being in the "natural world" is wonderful for me. I want to finish my career as a mammalogist by leading several extended field trips to areas of great mammalian biodiversity and to record the nature of that fauna by archiving in a museum as complete of a record as possible of specimens. It also pleases me that I've had the privilege of seeing live individuals of most of the species of Phyllostomid bats. I have had the privilege of collecting mammals in the following countries; Mexico, Guatemala, El Salvador, Honduras, Nicaragua, Costa Rica, Panama, Colombia, Venezuela, Suriname, Ecuador, Peru, Trinidad, Grenada, Dominica, Guadeloupe, Montserrat, Puerto Rica, Jamaica, Cuba, Tunisia, England, Ukraine, Russia, and the United States.

I was one of the first mammalogists to prepare karyotypic cell suspensions under field conditions and to take liquid nitrogen tanks to the field to freeze tissue samples from specimens collected. I first heard about liquid nitrogen tanks and their adaptability to field conditions from Carl Phillips while he was at Hofstra. The technology that is adapted to field conditions has expanded immensely. Mr. James E. Sowell was kind enough to fund expeditions to Ecuador and Honduras in 2001 and 2004. The 2001 trip to Ecuador was led by Carl Phillips, Clyde Jones and me. The field party for the 2001 Ecuadorian Sowell Expedition is shown in Figure 3. In this photograph, we attempted to capture some of the technology that was employed on that trip. In this photo, there is a liquid nitrogen tank, cryo preservation tubes with bar coded tags, a global positioning device, a video camera, a field microscope, a satellite phone, a laptop computer used to capture data electronically as it was recorded for specimens and localities, equipment for making fixatives for tissues for examination from an electron microscope for exploring cell structure, as well as the more typical field equipment including pinning trays, Sherman live traps, and mist nets.

As of 2004, there have been four trips made as Sowell Expeditions. The first two were in 2001, the Ecuadorian trip mentioned above (Fig. 3), and a trip led by Robert Bradley to Honduras the same year. A second set of trips funded by Jim Sowell and Alan Brown was made in 2004. Robert Bradley again visited Honduras, particularly addressing the northeastern regions. The trip to Ecuador was restricted to the western vercent of Ecuador. This expedition was a field party consisting of 9 individuals, Rene Fonseca, Sergio Solari, Peter Larsen, Adam Brown, Juan Pablo Carrera, Carl Dick and me, all from Texas Tech University, and Carlos Carion and Sebastian Tello from Pontificia Universidad Católica del Ecuador (Fig. 4). These field parties funded by Mr. Sowell have been tremendously important in providing a powerful database for theses and dissertations on neotropical mammals. Rene Fonseca organized and led the Ecuadorian trip. This was the best organized fieldtrip that I was ever associated with and resulted in 1500 mammal specimens. Rene was truly coming of age as a mammalogist. Unfortunately, Rene was killed in an automobile accident the week after the Sowell Expedition ended. Rene's death has been extremely difficult for me to place in a mental perspective that permits any understanding.

Figure 3. Modern field party. Sowell Expedition to Ecuador, August 2001. Front row seated left to right: Clyde Jones, Robert Baker (winner of the white legs contest), and Carleton Phillips. Standing: Trashanda Johnson, Jana Higginbotham, Federico Hoffmann, Michelle Haynie, Rene Fonseca, Juan Pablo Carrera, Rex McAliley, Joel Brant, Deidre Parish, Sandy Tolan, and Marcia Revelez.

Figure 4. Field party for the 2004 Ecuadorian portion of the Sowell Expedition. From left to right: Carlos Carion, Juan Pablo Cabrera, Peter Larsen, Sebastian Tello, Sergio Solari, Robert Baker, Adam Brown, Carl Dick, and Rene Fonseca. This site was a private preserve that was set up as a trust by La Cementos Nacional. Working conditions here were excellent, the biodiversity in bats was incredible, and we applaud their conservation efforts which are outstanding.

From my arrival at Tech until the mid 1980's, karyotyping mammals was the primary focus in the lab. We had a microscope room with a LEITZ eletrolux microscope, a billows camera for 4 x 5 film, a wet dark room, and all the equipment to print photos of chromosomes. We developed our film and each student printed out karyotypes for each specimen examined. The scope and dark room facilities were usually occupied for eighteen hours or more per day. During this time seven Ph.D. dissertations (Jerry Warner, John Bickham, Ira Greenbaum, Mike Haiduk, Fred Stangel, Mazin Qumsyeih, and David Kerridge) and ten masters thesis (Dale Berry, Brent Davis, Ed Pembleton, John Patton, Rebecca Bass, Annette Johnson, Mike Arnold, Cora Clark, Kim Nelson, and Hae Kyung Lee) were primarily focused on karyotypes. Karyotypes were prepared using double sticky sided tape mounted on poster board where chromosomes were arranged for comparisons with karyotypes from other individuals. Some of my most rewarding hours were spent looking through the scope searching for the perfect spread. During that time, microscope time was equal in value and pleasure to time spent netting bats and trapping rats. All aspects of karyotypic preparations were pleasing and rewarding. Today, I often think at the end of a day how wonderful it would be to experience more microscope time.

As a group, we had lots of fun in the search for information about chromosomes. We designed a T-shirt that Susan Smith made for us that contained a G-banded karyotype from *Peromyscus* that was entitled "Happiness is a Good Spread". Later, we had another T-shirt designed by Susan for our first *in situ* hybridization work. Holly Wichman had isolated a transposable element from *Peromyscus leucopus*. She had named this element *MYS*. The T-shirt was black with red representing chromosomal regions that were not hybridized and yellow representing areas where *MYS* hybridized to a chromosome. The legend on the shirt read "We can probe your jumping genes. Don't *MYS* it". There were several other "fun" activities for the lab. I'll mention one other, which were Halloween costumes. Over the years, my costumes included a pregnant lady, a newborn baby in a diaper and beef jerky as a dried umbilical cord sealed with hemostats, the comic book character Red Sonja, who was a Herkanian she-devil in light armor (Fig. 5), and Michael Jordan. I usually taught my freshman Zoology monster class for non-majors in a costume on Halloween.

Figure 5. Hyrconian she-devil in her light armor. Red Sonja at her worst.

During my first year at Tech I taught Histology and Cytology, and all the associated labs. Cytology, as taught at Tech, was actually Electron Microscopy of the Cell. I struggled to teach these courses. David Schmidly was a student of Robert Packard's at the time I came to Tech and I served as a member of his master's committee and took him on his first field trip to Mexico during Spring Break of 1968. David had been a member of the search committee that hired me. The Tech mammal collection had about 5,000 specimens, most of which were collected in eastern Texas, around Nacogdoches, when Packard was a faculty member at Stephen F. Austin University. Today the collection of mammals has over 100,000 cataloged specimens. I came to Tech for $9200 for nine months. The original offer was for $9000, but through my brilliant negotiating skills, I forced them to pay me another $200. Wow! To put that in perspective, people with a high school education hired as mail carriers in Lubbock in 1967 made $10,000 with substantially greater benefits. Of course, I could make a summer salary if I would either teach summer school or alternatively, get a grant that would pay my summer salary. Academic professors have to be pretty short sighted to agree to a nine month salary with the hopes that they can find three months of salary for the summer. It isn't like professors are paid in nine months the equivalent amount that would be typically paid over 12 months to a person with a doctor's level of education. The University expects us to do research, in the case of mammalogists like us, to make field trips to collect specimens, supervise graduate students, and to publish extensively. If we are to be successful in these expectations, then we must be productive during all 12 months per year. Over the past 37 years, my monthly load in the summer of doing what the University wants me to do has been every bit as intense and involved as my nine month load. On the other hand, I love what I do, and my situation can be described by the Mac Davis line from "Hooked on Music": "How can a man have such fun and still get paid?" I do wish that during my first few years at Tech I made a little more money to pay off loans for my field work for my Ph.D. and for hospital costs resulting from the onset of diabetes. It was always a struggle to make ends meet for my family and there was always a need to divert personal funds for collecting mammals, or whatever, with the hopes of generating enough preliminary data to compete for an NSF grant. For the last several years, I've had an adequate salary as well as plenty of money to pay myself a summer salary. So for all you young mammalogists: there is light at the end of the tunnel. One final thought for this particular discussion is that mammalogists are needed by society and just because we enjoy our work does not make it unimportant or less valuable to society. We should do excellent quality science based on well conceived experimental design. And of course, society should properly reward financially those mammalogists that do so.

When I arrived at Tech, I was sure that I would work hard and leave for a better position at another university that had a stronger program in mammalogy. J. Knox Jones, Jr. and the grad students at Kansas had always teased those of us at Tech by telling us that if you failed at Kansas you could always attend Texas Tech and get a degree in mammalogy, hence the title of my chapter. Now that I've been at Tech for 37 years I find that I have been reasonably successful by my own standards and living in West Texas is pleasant. There is plenty of hunting in West Texas, which is important to my quality of life. We have an abundance of dove, quail, pheasant, duck, geese, deer and turkey. West Texas land is cheap and over the past few years my family has purchased over 1,000 acres where I can play, raise cattle, hunt and fish. Life is good.

One significant contribution I made to Tech came in late '69 or early '70 when Tech had failed to hire a graduate Dean, and there was considerable debate on campus about how to attract the right person for the job. I had just given a seminar at Kansas and stayed at Knox Jones' home for late night bottles of Old Fitzgerald's bourbon and enhanced stories of field trips to the tropics. Knox mentioned that he might be interested in coming to Tech if a higher position in administration came open. I got a copy of Knox's CV and carried it to the chairman of the search committee and encouraged him to contact Knox. About three months later, the phone rang at my home and it was Grover Murray, the Texas Tech University President, who indicated that Knox would be coming in for an interview and that Knox wished to stay at my house. I was honored and, as they say, the rest is history. Knox joined Texas Tech University as Dean of the Graduate School in 1971-1984, Vice President for Research and Graduate Studies from 1974-1984 and as Curator at the Museum from 1984-1992. Knox's vision changed the nature of Texas Tech University and he played a major role in making mammalogy significant at Texas Tech. Anyone interested in who Knox Jones was should read the editor's preface, Knox's CV, his bibliography, eulogies and encomia in *Contributions in Mammalogy* edited by Genoways and Baker, 1996.

As a side note to my discussion of Knox, some comment is merited regarding the cactus garden that for many years was located immediately to the south of the NSRL. Knox Jones and his students at that time would dig up plants from their field trips, bring them back and plant them in an area that was very arid and unsightly. Although this garden eventually improved the landscape of the south side and the annual inflorescences of the century plants became a landmark of the NSRL, there is more to the story than meets the eye. When Knox began this activity, I suspected that he had gathered some protected species from federally regulated areas and that he did not have permits to remove them from his collecting localities. As Director of the NSRL, I asked him for the permits and the written permission to move the plants, and it seemed to antagonize him at great lengths that I had brought up the issue. It was difficult to find a relatively provocative way to antagonize Knox, but this seemed to be one such issue - at times he would smile and brag, and other times he would profanely tell me where I could go. One day, I asked him if he had permission from the university to plant the garden, pointing out that as the former Vice President for Research, he certainly knew of the operating procedures and rules for landscaping the campus. He pointed out that he did know the rules and he knew he was breaking them, but that there was sufficient inertia in the application of the rules that it was highly improbable that any action would ever be taken toward the cactus garden. His predictions have proven true. In fact, when construction of the new wing of the Natural Science Research Lab began in April 2004, a portion of the cactus garden was carefully removed so it could be replanted beside the new addition. This landscaping is being done with the blessings of the University administration.

Texas Tech has been very good for me. I probably could have been more professionally successful and raised my salary by switching jobs and environments but all in all the decision to stay at Tech was the right one. One benefit of being at Tech for so many years is that I learned how to work the system and built an effective working relationship with the faculty and administrators that potentially influence my life and my students' lives. Getting graduate students admitted to Tech, such as Jon Longmire, Mary Maltbie, Ron Van Den Bussche, Rodney Honeycutt, Kateryna Makova and Anton Nekrutenko, as well as others whose admission credentials contained problem areas, such as low test scores or being foreign students, could be accomplished by a few moments of visiting with the departmental admissions committee and the graduate dean. Often, it was just a matter of telling them I wanted to work with this student and they were admitted. I suspect at most other schools the system would have been more constraining. The success of the above students certainly justifies my faith that they deserved a chance to work on a Ph.D. degree.

Often in Ph.D. exams, we ask the student what they would do differently if they had it to do over. There are a number of things that I have done that were either blind alleyways or even worse, bad mistakes or flaws in my vision for success. This list is too long for this book, but I want to address some items. If I had it to do over again, I would first, write better field notes and keep better laboratory records. Second, it was common practice to loan all of the tissues from an individual for a species for starch gel electrophoresic studies. This was a horrible error. In retrospect, all loans should have been subsamples, keeping half of the material for the future. Hindsight may be 20/20, but I get sick at my stomach when I think of all the valuable tissues that I loaned or used in my own research that could have been subsampled. Another poor decision was going directly to Tech from Arizona rather than doing a post-doc. I could have been a much better scientist if I had experienced a post-doc within a program that was conceptual and theoretical. Today I like to think that I am doing a better job with experimental design; but certainly in my first few years, most of my work was descriptive. To quote one of the reviewers of a manuscript, my work was "pedestrian." I did spend two summers working for Dr. T.C. Hsu at M.D. Anderson Hospital and the decision to do so made me better educated and technically more advanced with G- and C-band methods. Dr. Hsu played a role in developing my thinking toward the bigger issues that could be addressed by molecular biology.

One decision that has profoundly affected my science was working closely with Ron Chesser and to study Chornobyl. I chaired the two search committees that hired Ron to the faculty at Tech (in 1981 and 1999), and during his first tenure at Tech, he and I worked closely together on several ideas that related to the structure of population size and chromosomal evolution. One publication in the journal *Evolution* (Chesser and Baker, 1986) clearly documents that population parameters and deme size are inadequate to explain major patterns of chromosomal change in highly rearranged karyotypes. In 1989 Ron moved to the Savannah River Ecology Lab and became more focused on Ecotoxicology. When he invited me to go with him to Chornobyl in 1994, little did I know how much time and energy I would ultimately put into trying to understand the biological consequences of the world's worst nuclear power plant disaster. In many ways, focusing on the biology as it relates to Chornobyl, gave me a chance to learn a new fauna and to think outside the areas in which I had been educated. Ron and I have very different strengths and, in my opinion, we have always made an excellent team. Ron's tenacity and vision were outstanding and effective in developing a program at Chornobyl even though working in the newly recognized country of Ukraine had great difficulties. The effort has been very rewarding. A personal dark side to the Chornobyl initiative, however, resulted from our publication of a cover story in *Nature* only to find out within six months of the publication date that the data we had published were inaccurate or perhaps, more accurately, described as "wrong." I made the decision that we had to retract the paper and for about a year and a half I felt like I was carrying a huge load on my chest and the sum was continual pain. I considered giving up science and finding a new direction for my life, but in the end, the retraction and my continuation in science was the right decision. There were several co-authors on the *Nature* paper and some of them were undergraduates. I owe a special debt to two of them, Lara Wiggins and Amanda Wright, for their continued support through the whole ordeal. Lara listened many hours to the various alternatives that I explored. Lara is an M.D. in Pediatrics who graduated from Baylor Medical School and Amanda received her Ph.D. last year from Harvard. I had worried that the stigma of a retraction might have a negative effect on my coauthors; especially the undergraduates. When Amanda thanked me in her acknowledgments in her Ph.D. dissertation at Harvard for "teaching her that science is a search for the truth," it not only made me pleased, but I also understood that she had used the experience of the retraction as a source of maturation.

I edited the *Journal of Mammalogy* through various jobs for ten years. The first two years (1972-1973) I edited the Notes section and the next two years (1974-1975) I edited the Feature Articles section. In those times the Notes editor handled all the notes, regardless of subject matter, and the Featured Articles editor handled all the featured articles. As a result of being raised in rural Arkansas and having a relatively undeveloped command of the English language, this experience had an incredible learning curve and at times was taxing. All in all, I think it did a lot for my professional development because I learned a lot about Mammalian Biology, as well as, it provided me a chance to interact with a large number of different mammalogists. One interaction involved a manuscript on the murid genus *Millardia* (Mistira and Dhanda, 1975). After the article was published, I got a letter from George Gaylord Simpson, who started the letter by stating "In a recent issue of the *Journal*, I encounter a term with which I am not familiar, 'planter pads'." He then went through several definitions of "planter" all of which did not fit and then he said, "Oh! Now I see! From the Greek!" and he wrote the Greek for 'plantar'. Then, one last line closed his letter: "There is something significantly wrong with the editorial system of the *Journal of Mammalogy* when these types of errors creep into its covers." My initial response was unhappiness that I had screwed up so badly, followed by a search for a better understanding of why this had happened. This manuscript was written by individuals whose native tongue was not English; two reputable mammalogists, who I will not name, had written extensive and very helpful reviews; and of course, I had edited the manuscript. All the way through all the copies, planter was the spelling. Eventually I just chalked it up to a major screw up. After a week or so of receiving the letter I decided that I should probably exploit this opportunity to interact with the great G.G. Simpson, so I sent him copies of two manuscripts to review under one cover and promised to send more within the next few days. I complimented him on his interest in the *Journal of Mammalogy* and in so many words said his letter made me sure that he would be willing to help improve the quality of the *Journal*. His immediate reply was "please forgive an old man for not knowing when to keep his mouth shut." He did say that he would be glad to review a few manuscripts, but requested that I be highly selective. We received a manuscript entitled something like "The Essence of a Cat." This manuscript was full of algorithms that mathematically defined why cats are cats. I certainly couldn't give an opinion on the scientific merit of the math, but I sent it to Dr. Simpson and a couple of other reviewers. Dr. Simpson's review can be summarized as follows: he doubted that the manuscript actually had contributed anything to our understanding of cats, and as such, should not be published in the *Journal of Mammalogy*. Alternatively, he said that it was possible that this guy mathematically had truly defined the essence of a cat, and if so, the paper was way too good to be appropriate for the *Journal of Mammalogy*. In either case, he assured me that I should be comfortable rejecting the paper. As a side note, I have repeatedly looked for Simpson's letter on the Greek origin of plantar, and it would appear that it doesn't exist in any of my files. What a loss! Perhaps there is a copy in the Simroe files that I believe are deposited at the University of Arizona.

Diabetes has been such a constant and obnoxious companion that it has often been a major statement of who I am. I developed diabetes in the summer of 1966. I lost weight to about 140 pounds and first recognized that I was truly sick on a field trip with Bill Davis and Roger Barbour to Alamos, Mexico. My vision, which had been 20/10, essentially became too poor to permit me to drive and I ended up in the hospital in Warren, Arkansas in July. I have now been an insulin dependent diabetic for 38 years. I made the decision that I would live my life to the fullest and I would not let diabetes keep me from being the mammalogist that I had trained to be. This meant field trips, long hours of work, and irregular meals. Not a lifestyle optimally designed for an insulin dependent diabetic. I still have reasonable eyesight (although both lenses have been replaced due to cataracts) and I still spend a reasonable amount of time in the field. However, it would be dishonest to say that diabetes doesn't control my life. If it's a good day, I think about diabetic control maybe 40 times per day and check my blood sugar 7-10 times. If I go a few hours without thinking of control of blood sugar, it will either go too high or too low. Both of these require a response and some level of pain or discomfort. I have been successful in spite of diabetes but many days diabetes wins the war. If I remain focused on control, it can usually be okay but there are days that no level of effort seems to make any difference. Diabetes just wins. None the less, I'm so lucky to have an understanding family, modern insulins, insulin pumps, glucometers to check blood sugars, improved medical procedures and wonderful doctors and nurses to help me survive and to do more mammalogy. I owe a tremendous amount to all those people who have either found me nearly unconscious and nonfunctional and supplied me with some form of glucose (yes you, Jim Reichman, for the bottle of syrup), or who have simply looked out after me and have been my friend in spite of diabetes.

A positive aspect has resulted from my studies of the prognosis and morbidity for diabetes. In 1966, I went to the library and read all the available articles in medical journals on diabetes. The general statement was that diabetes was a disease that you can live with. This very positive statement was followed with volumes of bad news including a one third reduction in the life expectancy from date of onset. Other afflictions such as loss of eyesight, amputation of limbs, kidney failure, heart and arterial disease, loss of nervous function, etc. occurred in a high percentage of diabetics. I concluded that my life expectancy and the quality of my health had taken a major hit. My solution was to view each day as a gift to be enjoyed and to take time daily to "smell the roses." Even though I have never learned to adequately control my volatile type A personality and anger is a significant problem, the perspective that each day I am lucky to be alive and healthy and being focused on the positive things in my life, does temper the beast inside..... a pretty nice gift from an ugly disease.

I need to address the issue of my students (graduate and undergraduate) and the role they have played in my life. I have found this section difficult to write because it would be impossible to comment on all of them individually and I don't know how to fairly choose some subset for focus. All of my students are special. As successful as my students are, I must be pretty lucky in who has chosen to work with me. In 1967, the first group of undergraduates that worked in my lab was Jim Bull, Brent Davis, Robert Jordan, Genaro Lopez, and Greg Mingden. Four of these individuals finished their Ph.D. elsewhere and Dr. Bull is a professor at UT Austin, Dr. Lopez, Ph.D. from Cornell, is at Southmost University at Brownsville, Dr. Jordan, now retired, was a professor at the Military Academy at Westpoint, and Dr. Mingden is a researcher at Southwestern Medical School in San Antonio. Brent Davis died prematurely with AIDS. Many of my students and I have maintained a rewarding relationship after graduation. For example, Jim Bull and I have been close friends ever since those undergraduate days and he named his son after me (Robert Bull matriculated as a freshman at Texas Tech University in September 2004). Another honor given to me was when John and Pat Bickham recommended that their daughter, Amy, go to Texas Tech University and work with me as an undergraduate. Amy published several papers as an undergraduate and is a Ph.D. student at the University of Texas at Austin. Terry and Nancy Yates gave me the privilege of taking their sons Brian and Michael hunting and fishing. My students have served as my extended family.

While my students and I published a lot of biology together, seeing them build a competent record, obtain a good job and move on in society has been the most rewarding aspect of this relationship. Not every student has been equally easy to work with and the strengths and weaknesses have varied greatly among individuals. The overall plan has been to amplify the strengths and cover the weaknesses of each individual. The idea that this process involves building a scaffold is a valid analogy. For any major improvements or changes on a large building, each scaffold has to be unique and fit the form of the existing and envisioned building. For each student, the plan, like the scaffold, had to be unique and built on the strengths and weaknesses of the student and what changes the student required in their education for their desired success. My students have been friends, antagonists, and the source of almost every other human emotion. I do not wish to be dramatic but they have been my professional life. Everyone has been a challenge and a reward. Thank you to all.

The students who have completed Ph.D.'s with me are: J. Hoyt Bowers, Jerry W. Warner, V. Rick McDaniel, William J. Bleier, John Bickham, Ira F. Greenbaum, Terry L. Yates, Rodney L. Honeycutt, Margaret A. O'Connell, Mike Haiduk, Fred B. Stangl, Jr., Mazin B. Qumsiyeh, Craig S. Hood, David C. Kerridge, Ronald A. Van Den Bussche, Meredith J. Hamilton, Alec Knight, Robert D. Bradley, Calvin A. Porter, Jonathan L. Longmire, Joaquin Arroyo-Cabrales, Cheryl A. Schmidt, James A. DeWoody, Mary Maltbie, R. Richard Monk, James Cathey, Burhan Ghariebeh, Kateryna D. Makova, Anton Nekrutenko, Kelly Allen, Brenda E. Rodgers, Jeffrey K. Wickliffe, Federico G. Hoffmann, Deidre A. Parish, and Adam Fuller.

The students who have completed Master's degrees with me are: Dale L. Berry, Omer J. Reichman, William J. Bleier, Brent L. Davis, Stephen L. Williams, Ira F. Greenbaum, John E. Cornely, Margaret O'Connell, Edward Pembleton, John C. Patton, Rebecca A. Bass, Laurie Erickson, Anette Johnson, Paul Young, Karen McBee, Mike Arnold, Ben Koop, Cora Clark, Kimberlyn Nelson, Hae Kyung Lee, Albert Kumarai, Kevin L. Bowers, Mary Maltbie, Shelly Witte, Susan Carron, Sergio Tiranti, Ted Jolley, April Bates, Ellen Roots, Britney Hager, Cole Matson, Oleksiy Knyazhnytaskyi, Nicole Lewis-Oritt, Raegan D. King, Emma M. P. Dawson, Amy S. Halter, Mark B. O'Neill, Mariko Kageyama, and Yelena Dunina.

During my 37 years on the faculty at Texas Tech University, I have taken only one sabbatical (the state of Texas does not believe in sabbaticals so they call them "developmental leaves"). In 1986, I went to Harvard and worked in Rodney Honeycutt's lab. While this may have been the ultimate nightmare for Rodney (having your former major advisor work in your lab), it certainly was a remarkably beneficial decision for me. My research, up to that point, had been almost exclusively chromosomal. G- and C- banding had reached a stage where students using these methods for a dissertation or theses could not compete for the best jobs. Therefore, I knew I needed to retool or get out of graduate education. So the design of the sabbatical was to build on that strength and to learn molecular methods to study chromosomal organization, especially as it related to *In Situ* Hybridization. Mary Lou Pardue at MIT was kind enough to let me work in her lab and do my initial *In Situ* Hybridizations there. I had hoped that ultimately chromosomal painting would be possible and that would permit me to trace out all the chromosomal changes in Phyllostomid bats. Seventeen years later, I still haven't accomplished my outlined goals, but we have published several *In Situ* Hybridization papers. Four dissertations, those of Meredith Hamilton, Robert Bradley, Mary Maltbie and Deidre Parish, were designed around *In Situ* Hybridization and genome organization.

The individuals in Rodney's lab, Kim Nelson, Scott Edwards and Ward Wheeler, were all very helpful in educating somebody who had never had a Biochemistry course. This was an exciting time with changes in molecular methodology occurring almost daily. I had a lot of time to read, think, learn and retool. I lived in Quincy House, a Harvard dorm, did not have a vehicle and could work as many hours a day as I wanted. I sat in on the Evolution course taught by Dick Lewenton and Stephen J. Gould. While I understood most of the subjects fairly well that they covered in class, I certainly profited from the level of organization and from the framework in which these ideas and theories were presented.

Rodney's office, which he shared with me, was just down the hall from the office of Ernst Mayr. I had submitted the paper by John Bickham and me on monobrachial speciation to Dr. Mayr for consideration for publication in PNAS. Dr. Mayr took a keen interest in this paper and very quickly got two reviews. One review was positive and wrote an excellent justification for publishing our work. The other review was less than complimentary and clearly written by a *Drosophila* person. Professor Mayr brought the reviews to me and said that he regretted that he would be unable to publish our paper. I read the reviews while he was still there and I quickly outlined all of the reasons that the reviewer's comments really were not germane to our paper, including that centric fusions, which are common in mammals, are fairly rare in *Drosophila* and with the lower diploid number there would be a very low possibility of this model applying to *Drosophila*. Professor Mayr left without comment. He came back in about 15 minutes, handed me a third review which he explained that he had just received that negated the criticisms of the second review and that he could now accept our paper. He was, however, quite concerned that in this manuscript we made the statement "in our model, chromosomes are the primary isolating mechanism in speciation and this mechanism is a postmating." At first, I had trouble understanding why he was concerned about this statement until it became apparent that he thought we were saying that speciation was sympatric = primary rather than what we meant, which was that speciation was accomplished between two populations when monobrachial centric fusions became fixed in the alternative populations. Clearly this could happen in allopatry, which Professor Mayr championed as the only type of speciation. We eliminated the word "primary" from our paper.

Professor Mayr extended an invitation to me to go to his summer home in Vermont and spend time with him. While there, I trapped mammals and he spent time pointing out aspects of the bird fauna in the woods where we were staying. I had been very careful not to address him in a casual way and always called him Professor Mayr. In his typical Germanic fashion, he asked (*i.e.* commanded) me to "call him Ernst." To do so, however, made me feel uncomfortable and even today it just doesn't seem like the right thing to do, although it has been the nature of exchange since that time.

One spring morning, we were walking outside and there were about 12 large pear trees that were overpoweringly white in full bloom. They certainly looked beautiful and healthy to me. In response to my commenting on how beautiful these trees were, Professor Mayr related that he had hired someone to remove those trees because they had been severely infected with some disease and that he thought they would never recover. He recalled that the worker never showed up and for that reason the trees were spared. At that point, he folded his arms, reflected a few moments and said to himself, "it is a horrible indictment of an evolutionary biologist to underestimate the resilience of life." I owe a great debt to Rodney for permitting me to work in his lab. I think the nature of my productivity changed as a result of those associations and I had a most enjoyable time at Harvard.

Ever since I first went bat collecting, I have always been fascinated by the idea of discovering new species. Of course, in 1963, when I was collecting in southern Arkansas, it was pretty improbable that I would stumble into something that I could recognize as new. During my Master's thesis work, my interest was enhanced by the intraspecific relationships of *Myotis subulatus*, as it was recognized at that time. Obviously specimens from Oklahoma were critical to answering the question as to whether the western and eastern forms were the same species or not. My unpublished Master's thesis did little to answer the question. When I began work at Arizona using karyotypes I was excited about using karyotypes to find previously unrecognized species. There was some success such as the description of *Rhogeesa genowaysi* (Baker, 1986). A second example was the description of *Uroderma bilobatum davesi* which was described based primarily on karyotypic variation (Baker and McDaniel, 1972). Today I believe that the molecular data suggests that *davesi* represents a different species rather than subspecies but this distinction is based primarily on karyotypic and molecular data (Hoffmann et al., 2003) and application of the genetic species concept (Bradley and Baker, 2001) not on morphological variation. A third example is *Geomys bursarius knoxjonesi* (Baker and Genoways, 1975). I am of the opinion that this chromosomally defined taxon is best recognized as a species. Again, the main support for this specific recognition is molecular data. Starch gel electrophoresis was originally touted as being a means to recognize biological species but this method failed to live up to its press releases and was not effective at either systematics or resolving species boundaries. Finally, DNA sequence data appear to be able to surpass even morphology at defining species' boundaries. My take is that species recognized by classical morphology are documented by DNA sequence data and further that DNA sequence data will provide resolution of unrecognized species where little was obvious from classical morphological studies. It is an observation of significance that even at the beginning of the 21st Century there are many species of mammals that remain to be recognized. Robert Bradley and I published a paper (2001) designed to establish standards for using a particular mitochondrial gene, cytochrome-b, to define species boundaries. I had preferred to have the title of the paper to be "The Cytochrome-b Species Concept" but Robert and I agreed to be more conservative and use the "Genetic Species Concept" in the application toward our conclusions. Four recent papers have introduced species descriptions that involved application of the genetic species concept as proposed by Robert and me. These taxa are: *Carollia sowelli* (Baker et al., 2003), *Notiosorex cockrumi* (Baker et al., 2003), *Reithrodontomys bakeri* (Bradley et al., 2004) and *Lophostoma aequatorialis* (Baker et al., 2004). In all four of these cases, the morphology reinforces the observations from the molecular data, but I believe it's safe to conclude that none of these four species would have been described and embraced by mammalogists as valid species without molecular data. I believe that as a result of molecular sequence data there will be an increase of 25-50% in the number of recognized species over the total in *Mammal Species of the World* (Wilson and Reeder, 1993). It will be fascinating to see how accurate molecular data are in defining species level taxa and their behavior.

Although the job of being a mammalogist has many positive aspects, one of the most negative features of the job is the difficulty in sustaining enough money to maintain a research program to collect, research, and study mammals. Clearly the average salary of a college professor is inadequate to fund these activities. The standard method of funding was to apply to the National Science Foundation for a grant. In my 39 years as a mammalogist, I have never had an NSF grant funded on its initial submission. Usually, the reviews of my grant proposals included criticisms that my work was not conceptual or that it was boring, pedestrian, unimaginative, etc. Over the years, preparation of NSF proposals became more labor intensive and more unlikely to be funded. It seemed to me that this was an awful lot of work just so your peers could describe your work as "unimaginative and pedestrian"; an activity that seemed at best a form of self-flagulation. An event increased the probability that the reviews would not be so negative was a grant proposal sent to me for review by NSF that was prepared by A.C. Wilson from Berkeley. Professor Wilson did a magnificent job of describing his contributions and results of previously funded NSF studies. Using his model for an introduction, I redesigned my introduction. In the next two NSF proposals I submitted, I began by describing what I viewed as contributions to conceptual biology. This included the canalization model of chromosomal evolution (Bickham and Baker, 1979), karyotypic megaevolution (Baker and Bickham, 1980) and the monobrachial model of speciation by chromosomal arrangement (Baker and Bickham, 1986) and how these theories (Chesser and Baker, 1986) challenged what we considered to be the dogma of the day concerning the relationship of deme size and demography to chromosomal evolution. Other conceptual papers included the testing of a molecular based hypothesis of chromosomal evolution (Wichman et al., 1992) and papers on how mobile DNA such as endogenous retroviruses is contained in a genome (Baker and Wichman, 1990; Wichman et al., 1992). While the probability of my NSF proposals being funded did not change, at least reviewers gave us credit for playing a major role in the conceptual development of the field of chromosomal evolution and mammalogy. Notification of rejection from NSF did not become a pleasure, but at least the abuse in the letter of rejection was greatly reduced.

About three years ago, I was appointed Faculty Athletic Representative for Texas Tech University. At first I really didn't want the responsibility as well as the commitment of time that I didn't feel I had to spare, but the experience has proven to be both challenging and rewarding. It opened whole new worlds and provided me with a new set of friends. The Faculty Reps for the Big 12 are a great group of people and we spend several days working together each year. The job has a steep learning curve for someone as uneducated in Athletics as I was, and my ability to represent Texas Tech has improved with experience.

Recently, as my health has been an issue (triple bypass surgery and greater difficulty in controlling diabetes), I have reflected on what is the best plan for finishing my career. I find it difficult to embrace retirement; maybe I do not wish to work as hard as I have in the past, but I cannot imagine giving up mammalogy in its entirety. Maybe the best plan is to die with a heart attack while netting bats somewhere in the tropics. It is really pleasing to continue to discover undescribed species of mammals and to see what the molecular data say about relationships and phylogeographic patterns. I cannot imagine having as much fun in retirement as I have in doing day to day mammalogy.

Some of Baker's favorite poems:

To a Mouse	Robert Burns
Crutches	Nikki Giovanni
Forced Retirement	Nikki Giovanni
A Certain Peace	Nikki Giovanni
Casey at the Bat	Ernest Thayer
Evolution	Langdon Smith
Professor Twist	Ogden Nash
The Way Things Work on the Ranch	Ted Genoways
The Clod and the Pebble	William Blake
The Garden of Love	William Blake
The Tiger	William Blake

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SPECIES AND SPECIATION: CHANGES IN A PARADIGM THROUGH THE CAREER OF A RAT TRAPPER

JAMES L. PATTON

James L. Patton was born in St. Louis, Missouri on 21 June 1941. He received a B.A. in Anthropology from The University of Arizona in 1963 and an M.S. and Ph.D. in Zoology, with a minor in Geochronology, in 1965 and 1969, respectively, from same institution. He became Professor Emeritus in the Department of Integrative Biology at the University of California, Berkeley, following his retirement in 2001 but retains his position as Curator of Mammals in the Museum of Vertebrate Zoology at UC-B.

I believe it was the second week of September 1963, when I first met Al Gardner, although I don't remember precisely. I was then a graduate student in the Anthropology Department at the University of Arizona, working on a masters degree in physical anthropology, and had enrolled in two courses in the Zoology Department that fall: a graduate course in human genetics, offered by William B. Heed, and Mammalogy, given by E. Lendell Cockrum. The graduate curriculum in anthropology was reasonably constrained; taking courses outside of the department was a bit unusual, and often required special arguments. A course in human genetics certainly made sense to my thesis advisor, Frederick S. Hulse, and to my department chair, Emil A. Haury, but a mammalogy course was something else again. My argument to both Hulse and Haury was that a physical anthropologist should know something about mammals other than primates—the argument worked. So, here I was in the Biological Sciences Building, probably coming out of the mammalogy teaching lab room and likely running into Al, who had just begun his masters program with Cockrum that fall, and who was working as a curatorial assistant in the mammal range across the hall from the lab room. I don't remember why we struck up a conversation, nor do I remember how it was that Al invited me to go with him to the Santa Rita Experimental Range south of Tucson that particular night to set traps for small mammals. But he did, and that experience was one of those "life-changing epiphanies" that happen. I remember distinctly opening the first closed trap, which held a kangaroo rat (which species, I don't remember; it could have been one of three), and, in the vernacular of the present, being "blown away". But, I am getting ahead of this story...

We were asked by the conveners of the symposium, of which this manuscript results, to address a set of interrelated topics covering the major events that prompted our entry into the field of mammalogy, to discuss the paradigms of the field and their changes through our careers, and to indicate where we believe the field is, or should be, heading into the future. This is a challenging task. Each of us has unique reasons why we became mammalogists, experiences that are unlikely to be duplicated by others, yet we also share commonalities of experience and we certainly have common visions of the future. I frankly don't relish writing about my own experiences, but I look forward to reading those of my colleagues just as I enjoyed immensely hearing their words at the symposium in late November 2001.

In concert with the charge of the conveners, I have divided this essay into two parts. The first is a personal history, the signal events in my own professional career that both led me to the field of mammalogy and then propelled me along a particular path. There are at least five such events that I describe in the pages that follow. As is often rather typical, many of these were serendipitous, and if the record were played again, it is likely that a different outcome would result—the evolutionary "contingency" of Stephen Jay Gould. The second is also a personal reflection, but one about the conceptual field of science where most of my career has been directed, namely that of species and speciation. I began in one disciplinary area and ended in another—while these areas of scientific enquiry are related, it has been my immediate colleagues at the Museum of Vertebrate Zoology, where I have spent my entire professional career, who largely dictated the actual directions that I have taken. I hope readers of the pages that follow will excuse the liberal citations to my own work, with little attempt to give proper credit, or perspective, to the broader array of published literature. Again, following the request of the conveners of this symposium, this essay is not meant to be a review of a conceptual field, but of our personal experiences within.

I loved being outdoors while I was growing up, but I wasn't particularly oriented to the natural world, either in St. Louis, where I was born, or in the environs of various military bases where my father, a U.S. Army surgeon, was stationed. I didn't spend time watching birds, investigating animals or plants, or reading about those who did. Rather, I was oriented to cultural history, not natural history, and I spent my childhood outdoors looking for Indian artifacts, or those left by early settlers of Missouri, Colorado, or other areas where we lived. I read every encyclopedic account that I could find about human history, in my family's collection of books or at the local public library, particularly the anthropological discoveries of fossil hominids and their artifacts—those from the Neander Valley in Germany, about the femur and skull cap discovered on the muddy banks of a stream in Java by the Dutch physician Eugene DeBois, of those who uncovered Peking Man in a cave in China or perpetrated the Piltdown hoax in southern England, or of the Paleolithic stone tools unearthed in France by a local customs officer, Jacques Boucher de Perthes. My time in my own backyard, wherever that was in any particular year, was thus spent looking down, searching the disturbed soil for obsidian flakes and the like. I knew from an early age that I wanted to study human history, and eventually learned that such was the discipline of anthropology. Consequently, when it came time to think about college, I considered only those schools with highly ranked anthropology programs, and located in areas of the country where I had not lived as a child. I choose two schools, the University of New Mexico in Albuquerque and the University of Arizona in Tucson, applied only to these, was accepted by both, and entered the latter as a declared major in anthropology in 1959. Fred Hulse taught the introductory anthropology course, and I came under his guidance as a first-semester freshman. That relationship lasted through my undergraduate years—through other formal lecture/lab courses and graduate seminars (taken while I was an undergraduate). Hulse served as my mentor on an honors thesis, which investigated quantitative genetic traits of Native American groups who lived in the vicinity of Tucson. Hulse was one of the pre-eminent physical anthropologists in the country at that time, elected to the National Academy of Sciences, in part for his seminal work on nutritionally mediated phenotypic plasticity in body-size of Italian-Swiss migrants to the vineyards of northern California. Certainly without any forethought, I would make my own contributions in this area 25 years later, not to humans but to populations of pocket gophers in alfalfa fields and adjacent desertscrub in the eastern California deserts (Patton and Brylski, 1987; Smith and Patton, 1988).

I knew even before I entered the university for my undergraduate degree that a graduate program would follow, and it was a natural extension of my undergraduate thesis to remain at Arizona for a masters degree. My undergraduate record had been strong, I had done particularly well in my major and had graduated with honors, and I had no difficulty being accepted to continue to work with Hulse. As a result, I enrolled in those courses in the Zoology Department in the fall of 1963 where I met the two individuals who would have the greatest influence on my early professional development—indeed, the two individuals who changed my career path irrevocably.

Al Gardner was an imposing individual—he still is—and I have no idea why he asked me to accompany him to trap on that particular night. Whatever the reason, I remain eternally grateful. Al and I became and remain close friends, we have published together, and it was solely because of him that I had my first taste of tropical mammalogy, with the Cashinahua Indians in the village of Balta in eastern Peru, in the summer of 1968 (Fig. 1). My own 30-years of work, off and on, in various tropical habitats in Latin America, and other sites around the globe, resulted from that "taste" with Al. He taught me how to set a trap, how to skin a mouse and how to cure the skin of a large mammal. Most importantly, he showed me a work

Figure 1. Al Gardner with a King vulture, near the Cashinahua village of Balta, Rio Curanja, Depto. Ucayali, Peru, summer 1966 (photograph by John P. O'Neill).

ethic that I have kept to this day—nothing is worth doing, unless you do it to your utmost capabilities. And, he instilled in me an interest in simply knowing what lives on the other side of the hill, and of making the effort to find out. If I have had any success as a professional over the past four decades, it is primarily because of Al Gardner's influence on my "early" life in mammalogy, and on his continued example of exemplary scholarship. I realized immediately from that initial trapping experience in the Santa Rita Experimental Range with Al that this was the kind of research I wanted to pursue, and that mammals other than humans were my real passion. Indeed, one of my greatest pleasures as a scholar was to name a new species of Amazonian climbing rat, *Rhipidomys gardneri*, in his honor (Patton et al., 2000).

The second turning point in my career that fall semester in 1963 was my interaction with Bill Heed in the human genetics course that he taught. Bill was then, and remains, an extremely personable if shy individual, much like myself, full of excitement for what he did (*Drosophila* ecological genetics) and for the natural world. He was a budding ornithologist as a youth, published on the birds of Rancho del Cielo in the cloud forest of Tamaulipas, Mexico, with Paul Martin and others early on, and went to study genetics at the University of Texas in the famous *Drosophila* labs of J. T. Patterson and Wilson Stone. He overlapped in the Patterson-Wilson lab with a young Chinese geneticist, T. C. Hsu, who was to play another significant role in my professional development, as well as those of other mammalogists, a few years later. Bill was, to me, the ideal combination of laboratory scholar and field naturalist, and he became widely known for both his seminal work on the ecology of the Hawaiian drosophilids, initiating some of the early studies of that spectacular adaptive radiation, and on the mechanisms of specificity of cactophilic species of fruit-flies with their obligate hosts of the North American deserts. He was also then a cytogeneticist who, in the tradition of *Drosophila* genetics at the time, was working to decipher paracentric inversion patterns in natural populations of several different species groups, both as a tool to establish phylogenetic relationships and as a means to understand processes of population divergence. These two areas were to become my methodological and conceptual orientation early in my biological training, just as mammals became the organismal focus.

There was a laboratory component to the human genetics course that Heed taught, and one of the tasks we were given was to prepare the karyotype of the laboratory mouse. The common methodology at that time was to squash cells from testes, or other tissue, which had been stained in aceto-orcein (acetic acid softened the tissue, making squashing possible, and orcein stained chromatin, making chromosomes visible). The method, however, was both difficult to perform and prone to counting errors because individual chromosomes could not be adequately separated (until 1956, for example, the human diploid number was thought to be 48, not 46). The method given to us in class was laborious and exceedingly cumbersome, required precise timing in various chemical treatment baths and delicate handling of cells, and it didn't work despite our efforts, collective and individual. I took this failure as a personal challenge, and spent nights in Heed's small lab filled with his stocks of living *Drosophila* developing a simple method that did work. Others before me had begun to combine a mitotic inhibitor, such as colchicine or colcimide, with cellular treatment in a hypotonic salt solution to solve the two basic problems of karyology: accumulating sufficient cells in mitosis, the stage where the morphology of individual chromosomes were most readily identifiable, and sufficient spreading to avoid the overlapping of chromosomes that could obscure both the correct count and morphology. Using colchicine and sodium citrate, I worked out a simple in vitro method whereby the chromosomes of a small mammal could be routinely obtained and observed in the short period of an hour or so. This method was published in the *Journal of Mammalogy* in 1967 (Patton, 1967), the only paper of the more than 150 I have authored that is considered a Science Citation Classic by the Science Citation Index Service (Institute of Scientific Information, Philadelphia, PA).

I became enthralled with small mammals, as a direct result of the incredible species diversity in southern Arizona and of the doors that the application of my karyological method could open. Heed was equally excited with the possibilities, as was, perhaps surprisingly, Fred Hulse. Indeed, as I began to spend more and more nights trapping and days karyotyping, Hulse recognized that my attention was rapidly being diverted from anthropology to other directions, which he nevertheless encouraged me to pursue. As it became apparent where my real interests lie, I talked to Bill Heed about switching my graduate program to zoology, and asked if he would consider to be my mentor. He agreed, the paper work went through the department and graduate school, and I formally transferred to the Zoology Department in the spring of 1964. The challenge was formidable, as I had to complete many of the undergraduate zoology degree requirements as well as those of its masters program, but I defended and filed a masters thesis in the spring of 1965. My thesis topic was the comparative karyology of pocket mice (including both *Perognathus* and *Chaetodipus*, as we know these genera today). Al Gardner and I finished in the same semester; indeed, we helped one another type our respective theses. This was well before the days of personal computers and even Xerox machines. Professional typists prepared theses on a per-page fee, but neither Al nor I had the money to hire a professional. Rather, we spent long nights in the mammal lab hunched over two electric typewriters with the same typeface so that we could work on both theses interchangeably.

Al left Tucson in the summer of 1965 for Louisiana State University to work under the guidance of George Lowery for his Ph.D degree. I stayed to continue in the Ph.D program with Heed, and Robert J. Baker began his Ph.D that summer with Cockrum. Bob (or Robert to most) came initially to work on the reproductive biology of *Leptonycteris* with Cockrum and Philip Krutzsch, who had been recruited to the faculty in the nascent medical school on campus. Bob and I spent a few days together that summer in the Chiricahua Mountains locating one of the summer roosts of this species. He, of course, moved on to do his dissertation on the comparative karyology of phyllostomid bats and to become one of the foremost mammalian cytogeneticists of our generation, a field in which he remains a leader today.

In the summer of 1965 I participated in a two-week methods course in cytogenetics at Brown University in Providence, Rhode Island. I was the only graduate student among the 20 or so enrollees, and was there primarily because one of the core instructors, Paul Morehead from the Wistar Institute in Philadelphia, had been in graduate school with Bill Heed. This was the third turning point of my career, for here I met T. C. Hsu, another instructor and classmate at Texas with Heed, and one of the world's leaders in the rapidly developing field of comparative mammalian cytogenetics. Hsu looked at my masters thesis on pocket mouse chromosomes, remarked forthrightly that my technique left much to be desired, and offered on the spot to bring me to his lab in Houston for training. I went there for two weeks that fall, he visited Tucson in February 1966 and accompanied a group of us, including Bob Baker, into the field in the Patagonia Mountains (Fig. 2), he hired me to trap *Peromyscus* for him and to biopsy skunks for tissue culture preparations, and I spent the summer of 1966 in his laboratory honing my skills and working on a variety of mammalian taxa. There were two other "happenings" that summer, but more about these later. My association with T. C. initiated a lineage of mammalogists who would work directly with him over the next decade or longer, a group that included myself, Bob Baker, Al Gardner, Jim Mascarello, Dean Stock, and Fred Elder, among others.

Also in the summer of 1965, Charles "Jay" Cole arrived in Tucson fresh from a masters degree with William Duellman at the University of Kansas to begin

Figure 2. Cytogeneticist T. C. Hsu (M. D. Anderson Hospital and Tumor Institute, Houston, TX) in the field with mammalogists, Patagonia Mts., southern Arizona. February 1966.

his PhD with Charles H. Lowe, the herpetologist on the faculty in the Zoology Department. Jay adapted my chromosome method to reptiles and amphibians, and began a long career in the cytogenetics of the lizard genera *Sceloporus* and *Cnemidophorus*, among others, that continues to the present in his position as curator at the American Museum of Natural History in New York. Bob Baker, Jay Cole, myself, and others in the graduate program in Tucson formed an active group of young cytogenetics investigators, helping one another both in the lab and field, co-authoring papers, and generally building a focal field that was picked up by others, independently or as a result of our success. Dan Williams and Mike Bogan, then graduate students with Jim Findley in Albuquerque, came over for hands-on instruction in our methodology, and Ray Lee at the University of Illinois shifted his research program primarily to comparative karyology of mammals. These were "heady" times—every species we trapped was karyotypically unknown such that even simple descriptive papers were of value. Collectively or individually we delineated the karyotypes of most local mammals and herps, worked on such exciting problems as the mechanism of origin of parthenogenetic whiptail lizards, identified cases of chromosomal race formation, and studied the dynamics of natural hybridization (as examples, see Baker and Patton, 1967; Lowe and Wright, 1966; Patton and Dingman, 1968).

The two additional events that took place in the summer of 1966 of substantive importance to my career were my marriage and my attendance at the annual meeting of the American Society of Mammalogists at California State University, Long Beach. These were linked, as Carol (Fig. 3, my companion in the field for the past 37 years) and I were married in New York in early June, then drove across country to "honeymoon" at the meetings, where I gave my first paper at a professional meeting. I talked on the karyological evidence for hybridization between the pocket gophers *Thomomys bottae* and *Thomomys umbrinus* in the Patagonia Mountains, based on material initially col-

Figure 3. Carol Porter Patton, with face painted and teeth blackened in the tradition of the Cashinahua, at Balta, Rio Curanja, Depto. Ucayali, Peru, summer 1968.

lected when we visited there with T. C. Hsu the previous February. This paper must have impressed at least one person in the audience, for later that summer Oliver Pearson (of the Museum of Vertebrate Zoology at the University of California, Berkeley) called to wonder both when I was likely to finish my degree and if I would be interested in coming to Berkeley when I did. This "moment" has to rank as the fourth major event in my career, without doubt. I traveled to Berkeley in the spring of 1967, gave a departmental seminar on my pocket mouse work, and then went back the following year for a formal interview. In the intervening time Seth Benson had elected to retire and a position as assistant curator in the Museum and assistant professor in the Department of Zoology was open. I was offered the position in the spring of 1968, and put off going until January of 1969 so I (and Carol) could accompany Al Gardner to eastern Peru that summer and needed sufficient time to complete my dissertation.

Carol and I arrived in Berkeley in early January 1969 driving our Volkswagen bus, my favorite field vehicle, loaded with everything we owned. I could write more than a book about my early experiences as a junior faculty member at Berkeley—of our introduction to the political force of student activism (the Third World Strike, People's Park, and other campus or community "activities"), or of the incredible intellectual caliber of my colleagues, which made it clear to me that this was not a place where I belonged. Indeed, I was convinced that the University would wake up, realize the mistake they made in bringing me here, and politely, but firmly, thank me before showing me the door. This never happened—I honestly don't know why. I was blessed with a superb group of close colleagues in the Museum, and others somewhat more distant in the Department. Pearson was then serving as Director of MVZ and had been working in Peru along with Carl Koford and Seth Benson for many years. The biggest mistake I ever made professionally was to say a polite "no" when he asked if I'd like to work with him there after my arrival at MVZ—I had too many unfinished interests with pocket mice and pocket gophers in the Southwest and Mexico. Yet, the altiplano and both Andean slopes of southern Peru, the focus of much of Pearson's and Koford's careers, were to form part of mine as well 25 years later. Benson was still a presence in the Museum and, while not active in research, was a treasure trove of mammalogical knowledge. Bill Lidicker was my immediate elder colleague with responsibility for the mammal collection, and we jointly established a course in mammalogy that we co-taught on occasion for more than two decades and that I have continued through the spring semester of 2003, despite my retirement in 2001. Many young scholars who have since gone on to establish their own credentials as professional mammalogists took this course. The most important of my immediate colleagues was David Wake, a herpetologist who had been recruited to Berkeley at the same time as I. A few years older than me, and with a singular focus on salamanders, David became Director of MVZ in 1972 when Pearson retired. He and I have been close colleagues since our joint arrival in 1969, and whether he realizes it or not, his research paradigm, intellect, ethics, intense focus, and passion for what he does have served as another role model for my own career.

Until I went to MVZ I would not have called myself a mammalogist—my training had been in genetics and mammals were merely the group to which I had applied most of my efforts. My earliest papers were on *Drosophila* paracentric inversion frequency clines, and I also published on vertebrates other than mammals as a graduate student. While I collected mammals and prepared museum specimens, the latter were primarily as vouchers for the karyological preparations I made. I was not a mammalogist, as that term was viewed in those days. I did take Cockrum's mammalogy course early in my graduate career, as noted above, but he was not a member of my dissertation committee. My mentors, beyond Bill Heed, were Chuck Lowe and Paul Martin, both superb natural historians but neither of whom were mammal-oriented, Wayne Ferris, a cytologist, and John Wright, a herpetologist who had obtained his PhD with Lowe at the same time I had completed my masters, and with whom I spent considerable field time in northern Mexico while still a student in Tucson and during my first 10 years at Berkeley. John and I also worked together in the Galapagos Islands in the early and mid 1970s. In point of fact, I "learned" mammalogy by curating MVZ's large collections and by teaching about mammals—for neither of which I had been really trained. In fact, I was brought to MVZ because of my genetics training, and the initial course I offered was cytogenetics, not mammalogy. Nevertheless, I became a mammalogist and was encouraged especially by Bill Lidicker to take an active role in the American Society of Mammalogists. The ASM became my primary professional affiliation, and I was first elected to the Board of Directors in 1972. The mammal collections at MVZ numbered about 136,000 specimens when I arrived in 1969; they now number over 200,000, and my personal field catalog is at 19,496 as of 8 January 2003. The collections of MVZ have always been built around the research activities of its staff and students, and the growth experienced during my tenure has been no different. Moreover, following Joseph Grinnell's dictum that the real value of a museum's collection won't be realized for perhaps a century after materials have been added (Grinnell, 1910), in 1972 we developed the first tissue collection associated with a vertebrate museum, a resource that is now heavily used by scholars of all types from throughout the world. Over the past 35 years I have engaged in fieldwork on all continents except Antarctica, but primarily in the New World. I have trained 31 PhD and 5 MA students and hosted 10 postdoctoral scholars. Many undergraduate students who have taken my courses have gone on to careers in mammalian or evolutionary biology, and several of my graduate students are now prominent mammalogists in this, and other, countries. Eleven of my graduate students have been from foreign countries, 9 from Latin America. I did not plan it at all, but I became a mammalogist and, as I look back over my career, I couldn't have followed a better path.

From my very earliest days in evolutionary biology, my research interests have been in microevolution, particularly on the nature of genetic variation contained within populations and the processes by which this variation becomes divided among them during the process of speciation. Thus, the nature of species and of species boundaries, as well as the mechanisms by which species are generated, have formed the underlying platform upon which most of my studies have been based. This is a large topic, clearly not one that I can treat adequately in a few pages. What I have chosen to do, therefore, is to identify three areas of research, all interrelated, where my studies and those of others working on mammals have elucidated changes in paradigms during our respective careers. These are the role of chromosome changes in speciation, hybridization and species boundaries, and development of explicit tests of models of speciation.

The paradigm in place at the time I began my karyological investigations was based on the presumption that chromosomal changes could drive speciation. Because nearly all structural rearrangements were believed to be negatively heterotic, causing severe fitness deficits when heterozygous, differences in karyotype provided potentially strong post-mating reproductive barriers. Few exceptions were known where malassortment of structural heterozygotes was circumvented (e.g., *Drosophila*, where crossing-over was restricted to females and where cross-over products were automatically shunted to the polar bodies during meiosis, or *Oenathera*, where alternate chromosomes in a polyvalent chain or ring automatically segregated to opposite poles). Rather, all variance in either chromosome number or morphology was expected to result in lowered fitness of heterozygotes due to the production of duplicate-deficient or acentric-dicentric chromosomes. Most standard genetics textbooks in the 1950s and 1960s were explicit about the expected meiotic difficulties in structural heterozygotes.

This underdominance paradigm presented three problems. First, meiotic imbalance in structural heterozygotes, while an expectation, needed to be established empirically in every single case. Second, the more severe the underdominance, and hence the greater the strength of the post-mating barrier, the more restricted were the population conditions where fixation was possible. Finally, even if fixed, the new structural homozygote needed to increase in frequency to prevent extinction by genetic drift. Proponents of this view of chromosomal speciation developed special models to accommodate these difficulties, such as Michael White's stasipatry (White, 1968). Others questioned the basic elements of the paradigm by studying cases where structural rearrangements did not generate meiotic imbalance, but instead where complex rearrangements were maintained as polymorphisms (as in the South American water rat, *Holochilus* [Nachman and Myers, 1989]). Still others observed heterozygote pairing in prophase and documented that suppression of crossing-over was common and thus the production of any unbalanced gametes could be easily circumvented (Greenbaum et al., 1986).

My own contributions to this on-going debate, and the changing nature of the paradigm of underdominance, were primarily in studies of the cytogenetics of pocket gophers of the *Thomomys bottae* complex (Fig. 4). On the one hand, these animals were poor subjects for detailed cytogenetic analyses because of their very high diploid numbers (76-80), yet pocket gophers were characterized by extensive interpopulation differences in kartotype, including the type of structural rearrangements thought tied to speciation. Moreover, the population structure of pocket gophers was generally believed to be conducive to the fixation of rearrangements that caused fitness deficits when heterozygous. I examined the meiotic consequences of structural rearrangements in contact zones between karyotypically differentiated populations, documenting reproductive impairment in cases of structural rearrangements (e.g., Patton, 1973) but free interbreeding in cases where the mechanism of difference was not structural (such as whole arm additions/ deletion of heterochromatin, e.g., Patton, 1971; Patton and Sherwood, 1982; Hafner et al., 1983). I also engaged in a multi-year longitudinal study of local pocket gopher populations at the Museum's Hastings Natural History Reservation in the central coast of California with Joanne Daly, then a Fulbright Fellow from Australia, to study population genetic structure at the local level. Here we were able to distinguish between the effects of short-term (=annual) small effective size (Patton and Feder, 1981) with a multi-annual view of expanded effective size due to dispersal (Daly and Patton, 1990). These types of studies convinced me that chromosomal speciation, as presented in the paradigm of the 1960s, was unlikely, even in animals, such

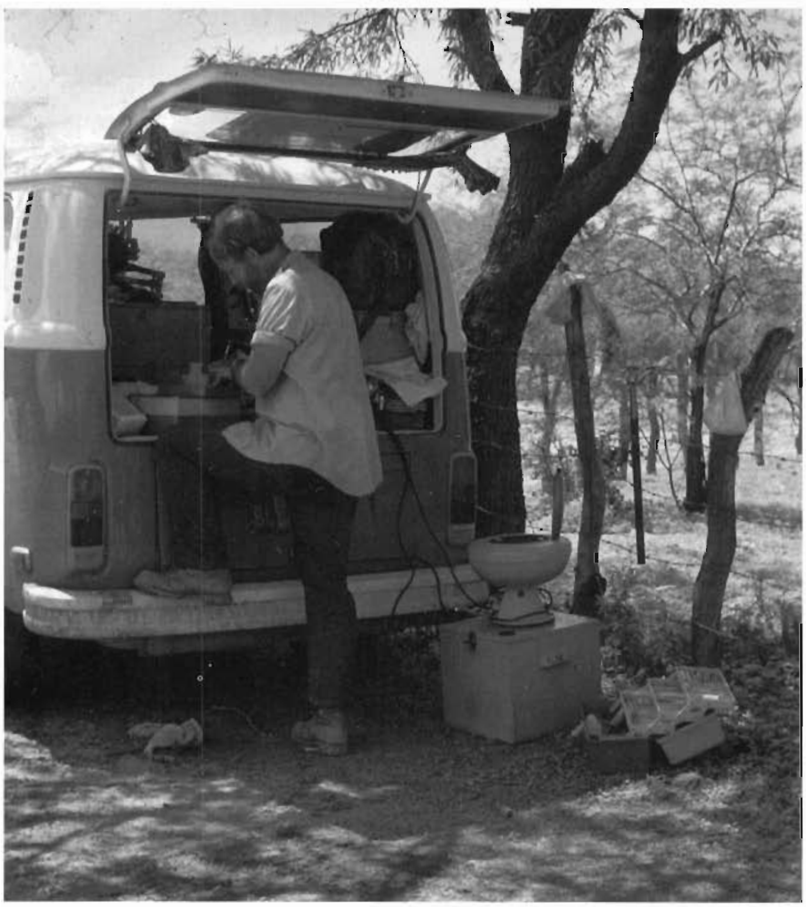

Figure 4. My "field laboratory"—the back of my Volkswagen bus—for preparing chromosome suspensions of pocket gophers, and other critters. Bacanora, Sonora, Mexico, July 1975.

as pocket gophers, where chromosomal changes of all kinds were common and where the local population structure was reasonably conducive to fixation (see Patton and Sherwood 1983). This is not to say that chromosome changes did not play a role in speciation, only that the specifics of that original paradigm of underdomiance of single structural heterozygotes lacked support. Other models, such as that of monobrachial homology, elegantly formulated by Bob Baker and John Bickham (1986), shifted the paradigm into new directions, as did the likelihood that meiotic drive drove underdominant rearrangements through the heterozygote bottleneck. It is curious that such a dominant and vibrant field for more than 20 years has almost disappeared from the list of active research programs in today's evolutionary biology. Why this is so would, itself, make an interesting essay.

Each of us can likely point to singular events that have shaped our discipline. One such event I would argue that it was the publication of E. R. Hall's and K. R Kelson's monumental two volume treatise *The Mammals of North America* in 1959. This publication, perhaps more than anything else, established a generation of research in the discipline of mammalogy within the United States. As the first detailed systematic and distributional compendium, with its often idiosyncratic taxonomic decisions, these volumes served as a lightening rod for many who took it as a personal challenge to extend the ranges of mapped species, to disagree with those species that were recognized, or to disagree with the authors over their nomenclatural decisions. "Hall and Kelson" was published a few years before I entered the field, but the new karyological data that were burgeoning forth during the 1960s provided a novel means to "evaluate" many of the species delineated within. Pocket gophers of all genera, but especially the highly polytypic *Geomys* and *Thomomys*, were one such set of "problem" taxa. Hall and Kelson included in their concept of *Thomomys umbrinus*, for example, not only the some 200+ described subspecies but forms that others had regarded as separate species. One major point of contention was whether the Valley, or Botta's pocket gopher, *Thomomys bottae*, was specifically distinct from a more restricted *T. umbrinus* (see Anderson, 1966, for a history of this debate). Southern Arizona was one area where presumptive populations of these two entities resided, and it was natural for me to examine the karyotypes of both to "solve" the problem. This I did, and the result was a series of published papers (Patton and Dingman, 1968; Patton et al., 1972, Patton, 1973) as well as the oral presentation that I gave at the ASM meetings in Long Beach in 1966 that apparently paved my road to Berkeley and MVZ.

A key tenant of the biological species concept, which was the only really accepted concept during the days of comparative karyology, is that gene flow will counter the force of divergent selection, and either prevent populations from differentiating in the first place, or cause them to merge genetically should populations re-contact after a period of physical isolation. If populations of differentiated taxa came into contact and did not hybridize, then their separate species status was uncontested. However, two other alternatives faced systematists, then as now, in the delineation of species boundaries. First, how did one deal with populations that were clearly differentiated but did not contact so that the "test of sympatry" could not be observed—were these merely subspecies or had "species status" been achieved? Second, if differentiated forms did contact and did hybridize, was there a qualitative or quantitative difference in the production of hybrids that could be used as a measuring stick of species status?

The dean of American mammalogy, E. R. Hall, once wrote "...hybrids are crosses between species and intergrades are crosses between subspecies" (Hall, 1943: 375), a statement that isn't very useful but was nevertheless the model by which species boundaries were often drawn. Hall went on to state that the differences between these two situations was in the proportion of "crosses" in the total sample—hybridization was when few individuals (1-5%) were intermediates; intergradation was when many (50-85%) were intermediates. The genetic nature or reproductive fate of intermediate individuals (hybrids or integrades) was not of importance, only their numbers. Of course, expectations were that few numbers of intermediates signaled some sort of reproductive barrier, pre- or post-mating, while a larger number of intermediates indicated a relative freedom of gene exchange because of the reproductive success of the "intergrades." However, as seemingly precise as this definitional distinction was, in reality it was exceedingly difficult to apply in the real world, as Hall made clear in his paper. Documentation of hybrid status at that time was limited to morphological indications of intermediacy, typically based on characters for which a genetic basis was totally lacking and which, moreover, limited one's ability to discriminate individuals of different hybrid classes or appreciate the full range of genetic hybrids present.

In my own studies, I felt the need for an operational definition of species that fit within my particular conceptual framework, one that was easily articulated and one that would make my decisions of where to draw species boundaries as unambiguous as possible (Patton and Smith, 1989; Patton and Smith, 1994). It made no difference if my conclusion was accepted by others or not, only that the basis for that conclusion was clearly understandable. My focus, because of my training and use of genetic markers (such as chromosomes), was on the identification of the hybrid status of individuals in contact zones and the distinction between reproductive isolation and genetic isolation (Patton, 1973; 1993). This view harked back to Hall's distinction between "hybrids" and "intergrades", but it emphasized the genetic constitution of individuals and thus the delineation of particular classes of hybrids and their parents rather than simple percentages of "crosses". To do so required appropriate genetic markers, and chromosomes were the first really good set whereby an F1 individual could be distinguished from various other hybrid classes that might result from various filial and back crossing.

This operational definition worked particularly well for pocket gophers, where geographic populations were often quite distinct but where contact zones could be found in nature and the hybrid nature of individuals determined (Fig. 5). Over the course of much field and laboratory work, I and others identified two general categories of contact zones: that in which hybrids are few in number and essentially limited to the F1 generation (such as between *T. bottae* and *T. umbrinus* in southern Arizona; Patton, 1973), and that in which hybrids are in high frequency and were largely the products of multiple generations of backcrossing (such as that between subspecies of *T. bottae* in eastern New Mexico; Patton et al., 1979). In the first of these two cases, F1 males are sterile and F1 females exhibit a 50% reduction in fertility. Here, genetic isolation was complete even if reproductive isolation was not. Since it is the former that is the key if evolutionary divergence is to continue, it was this distinction upon which I drew boundaries between species.

Ernst Mayr's seminal book, *Animal Species and Evolution*, was published in 1963, just as I began my graduate career in zoology. I well remember a graduate seminar on speciation taught by Chuck Lowe for which this volume served as the cornerstone. One of the architects of the neo-Darwinian synthesis of the early 1940s, where paleontology, genetics, and systematics were merged into a single conceptual field, Mayr has perhaps held greater sway on our thinking about species and speciation over the past 50 years than any other single individual. One of his predominant stances, of course, has been his unwavering adherence to, and defense of, allopatric speciation in all organisms. One can't help but wonder where Mayr's geographical frame of reference would have been had he worked on immobile parasites rather than insular birds with great dispersal potential.

My personal belief is that allopatry is the most probable geographic context of species divergence in mammals; certainly to me the likelihood of sympatric speciation by trophic disruptive selection is unlikely given the general catholic food habits of most mammals. However, the passive acceptance of allopatric divergence for all organisms is poor science, and contrary to a wealth of data and theory that supported other modes (Bush, 1975). More broadly, one could even chastise a dogmatic adherence to any particular geographic mode of speciation when focus might be more profitably placed on the explicit mechanisms, both genetic and populational, of divergence. For me, the most important paper about speciation since the publication of Mayr's book was that of Alan Templeton in 1980. I was an Associate Editor for *Evolution* at that time and remember serving as one of the reviewers. Templeton's clear exposition reformatted my own muddled uncomfortableness with "all things geographic", and caused me to begin to think more mechanistically about the processes. Another turning point in my views about speciation was John Endler's emphasis on selection differentials across sharp ecological gradients (Endler, 1977, 1982), and I began to consider ways in which opposing modes of speciation might be tested.

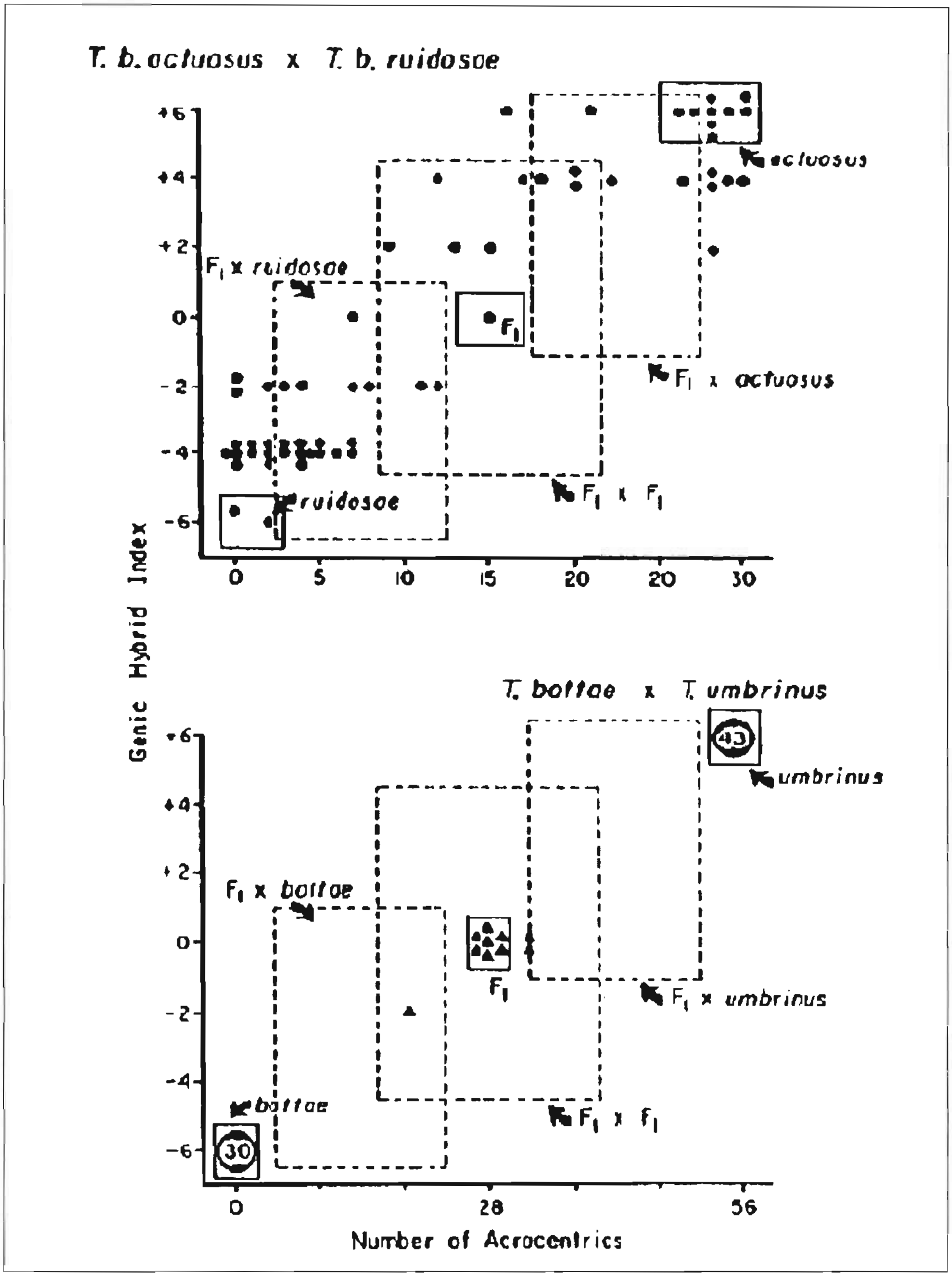

Figure 5. Two types of genetic interactions in pocket gopher hybrid zones, as indexed by the combination of allelic hybrid index (Y-axis) and chromosome morphology (X-axis) in hybrid zones of equivalent aerial extent and habitat difference. Solid boxes represent expected range of parental and F1 individuals, broken boxes represent the expected range of 75% of first filial generation and first generation backcross hybrids. Upper: few clearly identified parental or F1 hybrids but many back cross or multiple filial hyrid individuals; I regard the interacting forms to be intergrading subspecies (from Patton et al. 1979); Lower: extensive numbers of pure parental forms with few F1 individuals and even fewer backcross hybrids, which signal limited hybridization between species (from Patton 1973).

It is not a simple task to uncover the actual mechanism underlying speciation, especially in mammals with their relatively long generation times and general difficulty of husbandry in controlled laboratory settings. It is, however, relatively easy to measure species differences, and to use these differences to construct phylogenetic hypotheses. The different geographic settings of speciation, as enumerated by Mayr and others, are then amenable to explicit tests using such phylogenies. This has been my approach, applied primarily to tropical rodents where I examined the likelihood of gradient diversification in mice on the eastern slope of the Andes (Patton 1987; Patton and Smith, 1992) and southern Patagonia (Smith et al., 2001) and the importance of rivers as barriers promoting differentiation in Amazonian tree rats and other vertebrates (Patton et al., 1994; Gascon et al., 2000). This type of approach, particularly based on molecular characters such as DNA sequences, provides a powerful means to assess alternatives of diversification through falsification of a priori hypotheses (see Moritz et al., 2000).

My career in mammalogy has been rich and rewarding, in part because I had the dual opportunity of being active professionally when there were still large, biologically unexplored tracks remaining in the world while technology (physical and theoretical) expanded to allow me, and others, to dissect problems and address questions previously unapproachable. These technologies permit us to unravel the mysteries of the natural world almost faster than we can keep pace with the expansion in their development. I see this as a simultaneous blessing and problem. I put aside here any discussion of the rapid disappearance of natural habitats as human population expands, the role of technology in that expansion, and thus the increasing limits on our abilities to even find animals in their natural settings. These are problems that do exist, and will only become exacerbated for future generations of mammalogists.

Sometimes technology permits us to address questions that we might not be ready to answer, because our theory has not kept pace. However, technological advances too often make answers to easy, and studies are carried out without adequate thought to an experimental design, including even appropriate sample effort for natural populations relative to the question asked. Or, too often data seem to be gathered before the question itself is even identified. Our abilities in the laboratory, and especially with increasingly sophisticated analytical methods, are almost boundless now, but we all need to take the time, and care, to think about the questions we ask and to use methods that are appropriate and for which there is sound theoretical understanding. There are too many pressing problems associated with habitat fragmentation and loss; we need critical answers quickly, but these must be achieved within a proper framework of design, data, and analysis.

Today, as habitats and populations of organisms diminish, a time when we face the most immediate need for information from that which remains, it is paradoxical that society is also making it increasingly difficult to gain the necessary insights. In the field of mammalogy, animal activists, with their biologically inappropriate focus on the welfare of individuals, pose a risk to the welfare of populations in the natural world nearly equal to that of expanding urbanization. Individuals of all species have a finite lifespan, and for most mammals one that is short. But, for populations to persist, critical information that can only be obtained from individuals may go lacking because state and federal authorities, in this country and abroad, succumb to political pressure rather than sound biological thinking. In doing so, we are often thwarted from gathering the data that are really needed for adequate assessment and management.

Fewer and fewer regions remain to be explored, and more and more species need specific measures to be put in place for their survival. Inventory work and the documentation and description of new taxa, the collection of scientific specimens upon which both are based, are the underpinnings of biodiversity. But, we also run the risk of losing the cultural heritage of our own species, as aboriginal groups become fully acculturated and traditional knowledge is lost. For me, perhaps as a left-over from my early interests in anthropology, certainly the experience of working with non-literate peoples in Amazonia in decades past has been among my most rewarding experiences (Fig. 6)—a chance to study and learn a complex, diverse, and poorly known fauna collectively with a people who have lived with that fauna for their entire time on earth! I can only hope that those to come have the same

Figure 6. A mouse in a basket—studies of the enthnobiology the Aguaruna Jívaro, Rio Cenepa, Depto. Amazonas, northern Peru, summer 1977.

rewarding experiences as I, intellectually and culturally.

ACKNOWLEDGMENTS

I thank especially Carl Phillips and Clyde Jones, who conceived, organized, and presided over the symposium from which this manuscript results. It was a treasured experience for us all. I also thank Libby Moreland for arranging transportation and housing and for her patience in waiting for this manuscript. Carol Porter Patton deserves the special credit owed to a "significant other" who has spent her own life knee-deep in mud or swatting mosquitoes while setting traps and skinning mice. Al Gardner got me to this point, but is not to blame for what I may have made of the opportunity. Oliver Pearson, Dave Wake, Bill Lidicker, Harry Greene, Ned Johnson, Eileen Lacey, Frank Pitelka, Carl Koford, Bob Jones and others at MVZ have both tolerated and taught me over the years—my thanks to all.

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BEEN THERE, DONE THAT; AFTER 44 YEARS OF PREPARATION, WHAT'S NEXT?

ALFRED L. GARDNER

Alfred L. Gardner was born in Salem, Massachusetts, on 10 November 1937. He received a B.S. in Wildlife Management and a Masters in Zoology from the University of Arizona in 1962 and 1965, respectively, and a Ph.D in Zoology from Louisiana State University in 1970. He is presently employed as a mammalogist with the U.S. Geological Survey and has been stationed in the Division of Mammals of the National Museum of Natural History since 1973.

While in graduate school, I said to myself--as soon as I graduate I will be ready to get started; after graduating, I said to myself--once I get a job I will be ready to get started; after I got a job, I said to myself--once I get lectures planned I will be ready to start; after I went to the Bird and Mammal Laboratories in Washington, I said to myself--as soon as I get a few details out of the way and clean up some loose ends, I will be ready to start; here, many years later I am still getting ready.

I was born November 10, 1937 in Salem, Massachusetts, which is where I spent my early childhood through the third grade living in a neighborhood known as Gallows Hill. My third grade teacher was an amateur ornithologist who had a large glass-fronted case filled with mounted birds in her classroom. These specimens and class outings to marshes and fields near the school sparked my interest in natural history. In 1947 my family moved to a farm in North Andover, Massachusetts where I practically lived in the woods fishing, hunting, and trapping. I spent the 1948-49 school year along the upper Gila River in eastern Arizona--rode horses and hunted lizards, snakes, jackrabbits, and gold. The summer of 1949 was spent in the hills of southern California learning first hand about California Quail and poison oak. I returned to eastern Massachusetts in time to start the seventh grade and more serious hunting and trapping. As a freshman in high school, I sold my first fur and learned to recognize the game warden by sight at a distance sufficient to keep from being caught.

When my family moved to Tucson, Arizona in 1953, I quit school and went to work in a supermarket. But, because my father demanded my paycheck each week, I reconsidered the advantages of an education. 1955 was a big year. Got my drivers licence, joined the Army Reserves (24th Tank Battalion, 96th Infantry Division), graduated highschool and went through basic training (Ft. Ord, California), attended armor summer camp in the Mohave Desert, bought my first car (41 Olds coup), enrolled in wildlife management at the University of Arizona, and shot my first white-tailed deer.

My interest in mammals began with a mammalogy course in the fall of 1957. The next spring I dropped out of school and worked as a welder and sheet metal man. On weekends I explored the countryside and began banding bats found in caves and mine tunnels. The following summer I worked for E. Lendell Cockrum, traveling around Arizona banding bats. I also went on my first scientific collecting trip to Mexico. The next year I was back in school, but supported myself by working at night as a welder. When the opportunity came to return to Mexico in June, I jumped at the chance. I spent much of my time during the next two years collecting mammals and birds in Mexico. That is when I realized that if I wanted to earn enough to live on doing this sort of thing, I needed the "union card," a Ph.D. I finished my senior year at the University of Arizona and spent the next year collecting in Mexico. I began graduate school in 1963 and received my Masters in 1965. I then went to Louisiana State University, Baton Rouge to work on my doctorate under George Lowery. Along with two other grad students from LSU, I spent the summer of 1966 at Balta, a Cashinahua Indian village in eastern Peru. There I made one of several mistakes in my career: we prepared approximately 1500 specimens of birds and mammals, with several herps thrown in, and also caught some fish. Previous summer field trips by grad students normally yielded 300 to 400 specimens, and Lowery loved numbers. The year 1966-67 found me in Costa Rica working on a Leishmaniasis project and trying to do dissertation research. I was back in Peru in the summer of 1968 (this time with Jim Patton) and did more work in Costa Rica and Mexico in 1969. Highlights in 1970 were writing my dissertation on the taxonomy of the genus *Didelphis* in North and Middle America; giving my first paper at the mammal meetings in College Station after which we all met in the pool, most of us fully clothed; graduating in August; and going to Houston where I spent a year as a postdoc in T. C. Hsu's lab in the M.D. Anderson Hospital and Tumor Institute (now the M.D. Anderson Cancer Center). Add another trip to Peru in 1971. That was followed by a semester as an assistant professor at LSU, Baton Rouge and a year as an assistant professor at Tulane University, in New Orleans. On May 27, 1973, I joined the Fish and Wildlife Service, Bird and Mammal Laboratory at the National Museum in Washington, DC, where I have been ever since. The summer of 1973 was spent in the Noatak River watershed in northern Alaska. Since then I have participated in fieldwork in Mexico, Costa Rica, Panama, Colombia, Ecuador, Peru, Venezuela, and Brazil as well as in several states in the U.S. I have been reasonably successful collecting and preparing museum specimens--the last entry in my field catalog is 15,293. I have 92 publications, a respectable, but not outstanding record. Earlier this year I submitted a draft of the first volume on the *Mammals of South America* to the University of Chicago Press. This is a project Syd Anderson, Jim Patton, and I are both editing and contributing to. This first volume covers the marsupials, xenarthrans, insectivores, and bats. The submitted draft contained 1778 pages plus 342 figures. Of the 62 major sections included, I am the author or coauthor of 29.

I enjoy teaching, although during the past 29 years, teaching has been limited primarily to a few short field-oriented courses. I believe my success in the field has resulted from experience gained as a child roaming the woods hunting and trapping, and from the use of a variety of hunting and trapping techniques and methods. How many mammalogists today use deadfalls, snares, and steel traps along with their snap traps, live traps, pitfalls, and nets? How many mammalogists today know how to make traps out of materials at hand?

During the preparation of this short essay, I became aware that the most memorable projects I have been involved with are those during which I learned something. Easily the most important was the experience gained on my three visits with the Cashinahua Indians in eastern Peru. Jim Patton is to blame for teaching me how to karyotype mammals during my year in Costa Rica. During my year in Houston, I learned that molecular biologists put on their pants one leg at a time like the rest of us, although I found their discussion of genera and species of DNA unnerving. My work in Central and South America and visits to European museums led to the rediscovery and appreciation of the sphenofrontal foramen and groove in sorting out the species in the *Oryzomys capito* complex. This character also works with other sigmodontine rodents. My discovery with Maximo Drets of G bands (they weren't called that then) and shortening the C-banding technique from an overnight affair to a time span of about two hours while at M.D. Anderson was exciting. Realization of why microtines cycle and the fact that there are a number of conservation-savvy people working for the government and a lot of conservation-ignorant people working for conservation-oriented NGOs was learned in association with the Noatak survey in Alaska. While providing training in the early 1980s to Peruvian biologists in conjunction with a sylvatic plague research program in northern Peru, I discovered that I had contracted bubonic plague while in Brazil a few years earlier. I also spent several years working with the Ecuadoran government to establish the Yasuni National Park and tried to establish a research station in that park. My last trip to Ecuador while working on that project coincided with Reagan's inauguration. I also learned that many of the personnel associated with some of the wildlife conservation NGOs are more interested in the exclusive country club aspects of their organizations then they are in the purpose for which the organization was formed. I also came to realize that no matter how dedicated or well financed, conservation efforts toward protecting wildlife and wildland resources depend on the power and capability of the governmental agencies that control those resources. As it stands today in Latin America, these agencies can not protect those resources from squatters, poachers, the military, and the aristocracy who operate as though they have a special right. Equally important, these agencies cannot protect those resources from sister governmental agencies that have overlapping land-use jurisdiction.

While working on the Mammals of South America I learned a tremendous amount about nomenclatural issues and gained great respect for Angel Cabrera, Oldfield Thomas, Wilfred Osgood, and Remington Kellogg, among others. I also learned that one cannot always rely on colleagues to finish obligations. While teaching courses on field methods, I have learned that most biologists working for the government today are eager to learn everything possible about the fauna of the areas they are responsible for--with one caveat, you can't be intrusive--you can't harm the animals. They don't understand that the question drives the method, not the other way around. I suspect that urban backgrounds are part of the problem.

What's next? I have been working off and on for the past several years on a revision of the Catalog of Type Specimens of mammals in the National Museum. The 1942 published catalog covers about 2800 specimens; we now have about 3200. I also have a number of new species that need names. In addition there are numerous questions that interest me. Do hindgut fermenters grow their own protein? What happens to incisor growth in hibernating rodents? What is the optimal peanut butter-based bait considering the fact that shrews and white-footed mice dislike a popular major brand of peanut butter? Why do sloths descend to the ground to urinate and defecate? Is there any dietary significance in grooming by small mammals, other than by pollen and nectar-feeding bats? Is being vocal the reason shrews and some mice are active during the daytime? Is there a correlation between dynamic landscapes and solenaceous-fruit specialists among bats? Why is *Lasiurus cinereus* on the Galapagos but is unknown from southcentral Colombia to central Peru? What is the full story behind the banded *Lasiurus cinereus* Villa reported from Argentina? Is the black color phase in *Didelphis* under Mendelian control? Is the giant black bear I have from Alaska a hybrid between a black and a brown bear or does it represent a Pleistocene relict.

I have seen a number of changes over the past four decades. Among the more exciting are the molecular techniques that promise to help provide answers about relationships unobtainable in the past. Among negative changes is the increased difficulty in doing fieldwork in many countries. Working conditions also have changed dramatically in many countries. Can you imagine being grabbed by soldiers because you have an unregistered pistol tucked under your belt inside your shirt, being taken to a garrison and released after a few minutes with your pistol, all in Mexico and no bribe? This happened to me and a man working with me in a small town in Michoacan, Mexico, in 1959. Many wildlife agency people in Latin America are now convinced they know why you want to catch their bats and rats--you want to steal their genetic patrimony and become a millionaire at their expense. Also I cringe whenever I hear someone advocating "repatriating" collections.

What do I see as important issues facing mammalogy, field research, and science in general? It is foolish to ignore the need to build the capacity and power of resource management agencies in other countries so that they can protect their wildlife and wildland resources, yet at the same time push for more and more protected wildlands. Another important issue worldwide is the power and mindset of the animal-rightist movement. These people try to confuse legitimate animal welfare issues with their concepts of animal rights and don't understand that there is no morality in nature; that morality is a human construct. Another serious influence coming from our predominately urban population (a group I call the "Urban Subtribe") is that of post-modernism and new-age concepts affecting the public's understanding of science. The fact that many highschool science teachers prefer teaching creationism over evolution is disturbing. I am also disturbed by the commonly held faith in the magical power of crystals, in astrology, fortune tellers, the laying on of hands, guided communication, alien abductions and the fanaticism that accompanies some of these beliefs.

And finally, we are not training systematists in the classical sense. This is critical because we need practitioners who can correctly identify the vouchers--a necessity if we are to relate information on the population samples being studied to the specimens in collections on which our taxonomic literature is based.

A Field Mammalogist's Album

Figure 1. Paso de Soquilpa, Nayarit, Mexico, 1960. Person to left (second from right) of Gardner is Ciro Gonzalez Barragan.

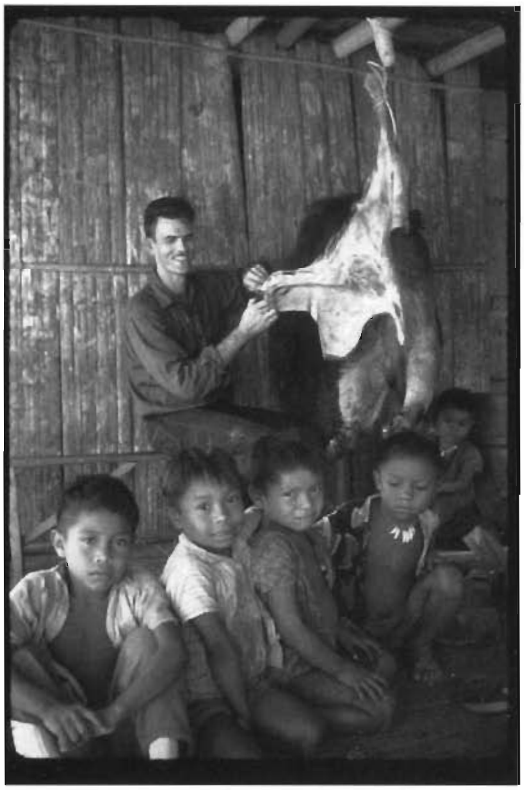

Figure 2. Balta, a Chasinahua Indian village on the Rio Curanja in eastern Peru. Skinning a giant anteater, *Myrmecophaga tridactyla*.

Figure 3. Balta, Ucayali, Peru. One of the large catfish in the Rio Curanja.

Figure 4. Balta, Ucayali, Peru. A crab-eating raccoon, *Procyon cancrivorus*, caught in a steel trap and brought back to the village alive. This raccoon is the first complete specimen from Peru of a relatively common animal.

Figure 5. Two species of *Didelphis*. Top: Virginia opossum, *Didelphis virginiana*, from Baton Rouge, Louisiana; bottom: common opossum, *Didelphis marsupialis*, from San Luis Potosí, Mexico. Note white versus dark color of cheek and the coiled versus straight tail. These are color pattern and behavioral differences exhibited by these marsupials.

Figure 6. San José, Rio Santa Rosa, Ayacusho, Peru, 1971. Processing cells from bone marrow to prepare slides of chromosomes.

Figure 7. Lake Feniak, Noatak River Basin, Alaska, 1973. This is one of nine camps established between 13 June and 28 August during a survey of the soils, flora, and fauna of the Noatak River Basin in northern Alaska.

WHAT THE OLD DOGS SAID: PERSPECTIVES FROM A PUP

ROBERT D. BRADLEY

Shortly after the Mammal Symposium held at Texas Tech University (November 2001), I was asked by Drs. Clyde Jones and Carleton J. Phillips to contribute a chapter pertaining to my impression of the "Old Farts Symposium" (their words, not mine). I believe my charge was to develop a perspective/interpretation that represented the sentiments of the "Middle-aged Cohort" (again, their words). Also, I was to relate my experiences as a beginning young faculty member (my words) to theirs, in terms of building a graduate program, conducting research, teaching, and directing graduate students. I was encouraged to objectively provide a synopsis of what I learned from their formal presentations and the not-so-formal social events that transpired during the Symposium. I remember thinking that "objectively" is an interesting choice of words as two of them (Drs. David J. Schmidly and Robert J. Baker) were my graduate mentors and that four of them (Schmidly, Phillips, Jones, and Baker) held administrative positions above me at Texas Tech University. At any rate, here is my recollection of what the Old Dogs said and whether it has anything to do with current mammalogy, research programs, and graduate education.

WHERE DID THEY COME FROM?

With a couple of exceptions, our illustrious predecessors came from rural backgrounds. Most grew up on family farms, and shared an interest in hunting, fishing, and other outdoor activities. Several mentioned running traplines to earn spending money or simply to help put extra food on the table. As most of them grew up during or at the end of the Depression, I suspect lack of funds and the absence of video games and the internet had a great deal to with their interest in outdoor activities. Also, I suspect that it was this background that provided the initial interest in nature that, over the years, would broaden into an attraction to zoology and natural history then eventually would be narrowed to a focus on mammalogy.

It seems that many of this distinguished group were from the Midwest. Most hailed from Missouri, Kansas, Nebraska, and Arkansas or neighboring areas. I remember thinking that six or seven of them might be members of the same deme. At any rate, it is fascinating that so many were born and raised in the Heartland of America. What was it about this geographic region that developed such an important pipeline into mammalogy?

Another common denominator uniting our speakers was that most were involved in athletics at the high school and collegiate level. Several cited basketball and football as areas where they developed their competitiveness and motivation for success. Perhaps their numerous accomplishments in mammalogy had its roots in the competitive desire to be the best. This trait certainly comes in handy in dealing with the submission of manuscripts and grant proposals, not to mention comparing catalogue numbers and skinning "bees".

Many of our elder colleagues attribute hard work, perseverance, and frugality as qualities, learned as a consequence of this rural lifestyle, which eventually would become part of their professional persona. Privately and publicly some admitted that graduate school and academia was much easier than their earlier experiences with farm life.

Personally, I really enjoyed and could identify with the recanting of their childhood and adolescent experiences. I, too, was raised on a family farm (in Missouri) and as a result developed my passion for the outdoors. I agree that our current profession, even on its worst days, is easier and more enjoyable than cleaning out the pig and cow barns. However, I concur that the lessons learned from the farm were invaluable and have served me well in this profession.

Today's cohort of younger mammalogists and graduate students probably more reflect the demography of our country than our predecessors did. Most are from suburban areas and probably developed their interest as a result of a particular high school course or early college class or experience. I do not believe this shift from a rural to suburban background has had a negative impact on the quality of student; however, today's student has a more specialized education rather than the broad training of their naturalist forefathers. Although specialization is needed to keep up with state-of-the-art research and job market, it troubles me that as a scientific discipline and society we are losing the "naturalist". Their holistic perspective will be missed.

You Have a Ph.D. and You Chase Rats?

Some of our preeminent colleagues were the first members of their families to garner a college degree. I suspect many were the first from their respective high schools or communities to receive a Doctorate degree. In current times, a college degree is viewed as a birthright instead of a privilege as it was 40 years ago. Certainly, obtainment of a Doctorate would have been a major accomplishment and a rarity for a Midwest farm boy during the 1950's and 1960's.

In the current academic market, many students are second generation college graduates and several have parents that hold a Doctorate. I received my Doctorate in 1991 and to my knowledge was the first graduate of my small, rural high school to do so. Many of my relatives were flabbergasted that an educated person would choose to study rats and bats. Many still think I am a medical doctor or conservation agent. Given my own experiences, I can only imagine the perception and reception given our elder colleagues as they arrived home with college diploma and skinning trunk in hand.

We Get Paid For This?

At least two or three of the speakers were surprised to learn that graduate positions came with a stipend. More than one commented about being paid to collect or to prepare specimens, activities that they gladly would have done for free. My impression was that they were so excited to either get an opportunity to work with their mentors, conduct fieldwork, or simply be part of graduate education that they had given little advanced thought to salaries. Alternatively, it may have been that graduate school was a concept that they knew little about, given their limited exposure to academia and the nuances of research and teaching assistantships.

Aside from farmers and ranchers, I suspect that field biologists are among a small group of people that place little emphasis on finances. If you make enough to survive on, you are happy. Keep a mammalogist in the three B's (bread, baloney, and bourbon) and you have a recipe for happiness. [However, knowing this group, the third B may require a substantial capital outlay.] That does not mean that mammalogists are not above asking for and accepting a raise; it simply means most are not going to leave the discipline for an unrelated job that pays a higher salary. After all, you are being paid to do a job that you would love to do as a hobby. Most school-teachers probably fall into this category, as well, which probably explains the poor compensation they receive for such a critical job. You would think that professional athletes would have a similar view and play for free.

I believe that most current students are more money conscious than preceding generations. I have seen several students turn down jobs or stipends because the salary was not sufficient to support their current lifestyle. Perhaps lifestyle is the key word in this case. One only has to look in the parking lot and compare the students and faculty vehicles for a perspective on how lifestyle expectations have changed. My guess is that this is a societal trend and not one that necessarily is associated with our discipline or the next generation of students.

Field work

It was very clear that every speaker possessed a passion for fieldwork. It is not clear whether fieldwork was something that relates back to their rural and outdoor childhood experiences or whether it was something that grew from their profession and the need for collecting specimens/data. I suspect it is the combination of the two scenarios.

To gain a perspective on the importance of fieldwork to our predecessors, one only has to mention a specific locality, collecting trip, or favorite taxon. An immediate and repeatable response is elicited: a smile forms and a "I remember when" story begins. Fieldwork and the related adventures provide a means of communication between mammalogists. Many fond memories and friendships are forged during field trips. Also, the art of one-upmanships are practiced and perfected when rehashing field experiences. Not all fieldwork results in happy tales; however, even these have value to the mammalogist from the standpoint of providing material for the "I can not believe we made it out of there alive" stories.

Fieldwork was what drew me and most of my contemporaries to mammalogy. To me, fieldwork encompasses both the professional and personal sides of our discipline. First, to understand or to be knowledgeable about an organism, it helps to know where it comes from. Knowledge about its habitat, elevation, feedings habits, etc. provide invaluable insight and allows us act on or to develop gestalt. Second, fieldwork is the fun part of the science. It makes up for administrators, committee meetings, hours at the laboratory bench, editors and reviewers, and the hundred other mundane obstacles that we get to deal with as faculty members. Not that fieldwork is a vacation by any stretch of the imagination, but it can reenergize you and bring some sanity back into focus.

I believe that the current crop of students is equally interested in fieldwork. Generally, I receive enthusiastic responses when inviting students to attend field trips. Most enjoy trapping, netting, and prepping with a passion. In my opinion, the tradition of fieldwork in mammalogy is safe with the next generation of students.

LOVED WHAT THEY WERE DOING

I get the impression that these guys would not change any of the variables if they had to do it over again. The title of Dr. Don Wilson's presentation (Spyder Had A Pretty Good Ride) probably illustrates the sentiment about as well as anything. I am sure that each, in their own way, suffered setbacks, tribulations, and regrets, but you seldom hear any negative connotations associated with their careers. Instead, you hear the positives, what fun they had or about how much they enjoyed their students, colleagues, or work. Obviously, they have done their share of griping, complaining, and grumbling along the way, but in the final summation, I am convinced that each would step back and do it all over again.

The fact they attended this symposium is a testament to their fondness for the discipline, colleagues, and students. I suspect they came not only to meet with old friends but out of a genuine desire to "talk about their experiences in mammalogy". Also, the fact that they never changed careers demonstrates their continued connection to mammalogy.

Does this same love and commitment to mammalogy exist with today's generation? I believe it does, at least, with most of my contemporaries. Most of us are still active in the field, although we have lost a few to the laboratory bench along the way. I think my generation had the attitude that mammalogy was our one and only career option. We would have considered ourselves failures if we had not been employed in a field directly related to mammalogy. We expected ourselves to be like our mentors - do research and teach at universities, be curators in mammal collections, or be field mammalogists for state/ federal agencies. However, most of our current students do not share this same swim or sink mentality ("I must be employed in mammalogy or else"). Perhaps it is because they have more opportunities to choose from or perhaps they are using mammalogy as a stepping stone. Perhaps the supply of mammalogists has exceeded the traditional demand. However, many students now use their skills to voluntarily secure employment in the biotech, computer, and biomedical sectors. Although there is nothing wrong with a career outside of mammalogy (the private sector does pay extremely well), I do see fewer students following the traditional pathway.

ICONS AND THE INFLUENCE ON MY GENERATION

No one would disagree against the icon status of this select group of highly successful mammalogists. Their generation had their icons in the likes of C. Hart Merriam, Hartley T. Jackson, Vernon Bailey, Emmet Hooper, E. Raymond Hall, and others. My generation had these guys and a few other of their contemporaries. Their personal achievements, honors, and awards would fill a separate volume and certainly is beyond the meager justice I could present in this article. However, I must comment on what I consider to be a subset of their icon status, that is, what these individuals meant to their own students and to other students in the discipline. I only can comment directly on my own two mentors (Drs. David J. Schmidly and Robert J. Baker) but I suspect that students of the others would agree with the following assessment.

First, this collective group of individuals probably has forgotten more about natural history, organisms, systematics, etc. than we will ever learn. As each generation becomes more specialized, more and more of the "big picture" is lost. Second, I learned more through informal interactions and conversations than I ever learned in any class. Two cases in point from my experiences. I learned a lot about small mammals from David J. Schmidly as we were traveling to and from field sites. While driving, we would discuss the various habitats we viewed through the truck window. Ultimately, Dave would ask "if you set 100 traps over there - what would you catch? How about over there?" We discussed means of identifying rodents - how do you tell this species from that one? These "games" helped me to develop an eye and "real time updates" as I travel from one habitat to another or peer into the bottom of a trap. Another example comes from work with Robert J. Baker during my Ph.D. years. Robert is spontaneous and science is always racing through his brain. I can not begin to tell you how many grant proposals, manuscripts, or experiments were outlined on McDonald bags or napkins during hunting trips, travel to scientific meetings, and other ventures. Through these spontaneous discussions, I garnered many tidbits of information, ideas, and scientific knowledge that are not presented in any textbook or classroom. I learned how to think quickly and to discuss current papers and ideas, while keeping an eye out for oncoming traffic.

I had an exceptional rapport with both of my mentors. In many ways we did not have the typical mentor/student relationship. We were much closer than that. As a student I worshiped the ground they walked on and as a colleague I continue to hold them in extremely high regard. Without this strong mentor/student bond, I would have fallen through the cracks. Perhaps mine was an unusual relationship but again, most of my contemporaries relate similar experiences and bonding with their mentors.

I believe the relationship between mentor and student has been transformed more into a business-like relationship than the traditional role model that my generation experienced and participated in. I do not believe that today's student identifies or interacts with his or her major advisor in the manner that my contemporaries did. I think this is the wrong path to go down, as I can not imagine not taking advantage of my mentor's wisdom and experience. I learned so much from ad hoc discussions and BS sessions. I am not sure how or when this change occurred, however, I genuinely believe students will not receive the full benefit of graduate education without a strong mentor/student interaction. It would be a mistake for the next generation if we allow graduate education to become a business.

ACADEMIC FOREFATHERS AND OUR RELATIVES

One thing all mammalogists enjoy doing is swapping tales and telling lies. We spend our time at meetings, around the campfire, or at the bar telling and retelling stories. [In fact, many excellent stories were transmitted during this symposium]. Through this tradition, the old timers have become immortalized and their adventures are passed from generation to generation. Personally, I think this is wonderful as it allows a connectivity to develop from the past to the present, a thread to the past if you will. It allows us to remember the pioneers in our field and the contributions they made. It has provided us with an oral history of the discipline and our academic brothers and sisters. Also it provides some insight into the personality, character, and demeanor of people whose name you know but perhaps you have never met.

As a group, mammalogists are proud of their geneologies and most make a point of knowing and reciting their ancestry whenever possible. We often introduce each other as academic brothers/sisters, aunts/uncles, cousins, etc. Many of us are double relatives as a result of obtaining degrees (M.S. and Ph.D.) from related mentors. Consequently, the inbreeding coefficient is relatively high. In fact, of the 13 speakers, I am kin to 10: two fathers, one grandfather, and seven second cousins.

It may seem ridiculous, but I think academic relatedness holds us together as a mammal society. Certainly, it helps open doors for collaboration, making acquaintances, and friendships.

PAST, PRESENT, AND FUTURE

Needless to say, times have changed since our speakers set their first Museum Special. In my generation alone, we started with dial calipers and numerical taxonomy, and moved to starch gel electrophoresis and allozymes, differentially-stained chromosomes, Southern blots, RFLPs, land marked based morphometrics, in situ hybridization of chromosomes, DNA sequencing (including a plethora of methods and variants), and microsatellites. With each new technique, the power of resolution has increased dramatically; we are now at the point where we can develop a genotype that is unique for each individual specimen. However, with each step, we move a little further away from the rats and bats.

We expect a lot from today's student. We expect them to be proficient in molecular genetics, population genetics, computers, statistics, GIS, etc. They must be able to use a whole cadre of electronic and computerized gidgets and gadgets. Consequently, they are better trained in state-of-the-art techniques than the preceding generations. [I certainly am glad that I do not have to answer some of the questions that get asked on today's qualifying exams.] However, it comes at a cost. What has been given up is the focus on organisms, natural history, and museum collections. How many of today's students collect their own samples and how many depend entirely on tissue loans? How many students pay the loan system back by depositing specimens and tissues in a natural history museum?

Certainly, our panel of speakers is familiar with current techniques; in fact, many helped develop or refine them for use in mammalogy. However, I believe much of their success came from the knowledge of the organisms and the associated primary literature. Their focus on organisms, literature, and intense fieldwork led not only to new discoveries but the ability to recognize new and important data points. They knew where to look for the next set of questions, or alternatively, which group of organisms should be used to investigate or to test a new hypothesis. Collectively, they carry with them a tremendous amount of knowledge of New World mammals. We had better listen closely; otherwise, this organismal perspective will be sorely missed.

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