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. Little Robert Baker at his grandparent’s house in Arkansas.
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 pot-hole, 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. 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 standard 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 diphylectic 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
two 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 Mammal-
gists meetings in Long Beach, California in 1966. He
couraged me to apply for a job to teach Histol-
ogy 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 in-
terests, 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 ade-
quately understand systematics, genetics, zoonoses,
phylogeography, evolutionary processes, etc. I ap-
preciate that there are many mammalogists and zoolo-
gists who think that individual animals should rarely
be sacrificed for science. But I strongly disagree with
this conclusion because I believe that basic knowl-
edge, such as how many species exist, where are the
species boundaries, effected population sizes, docu-
mentation of breeding strategies, fitness values, etc.,
are necessary for conservation efforts, wise manage-
ment of our biodiversity, as well as understanding the
value of various forms of life for esthetic and eco-
nomic uses. These and other critical questions can be
answered best with voucher specimens that are docu-
mented by a complete database including locality, time,
ecological data, etc. The molecular revolution of mam-
malogy has provided us with a chance to tease out the
answers to many questions that were previously un-
answerable. Although there are many publications on
mammals, my opinion is that much remains to be dis-
covered. 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 ex-
ceptions, most populations of mammals are vibrant
and more young are born than can survive and col-
lecting a few specimens will not have a negative im-
 pact on the species. In cases where there are excep-
tions, collections need to be more carefully regulated
or perhaps even prohibited.

As a major advisor who believes in building col-
clections and that fieldwork is critical to the education
process, I feel that all of my students should contrib-
ute 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 de-
gree 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 field-
work. 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 de-
cided 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 collec-
tions of tissues can have access to the large tissue
collections but this access should not be to the disad-
vantage 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 privi-
lege of collecting mammals in the following countries;
Mexico, Guatemala, El Salvador, Honduras, Nicara-
gua, 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
caryotypic 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 con-
ditions from Carl Phillips while he was at Hofstra. The
technology that is adapted to field conditions has ex-
panded 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 cap-
ture 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 tis-
sues 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 vis-
ited Honduras, particularly addressing the northeast-
er 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 Uni-
versity, 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 data-
base for theses and dissertations on neotropical mam-
mals. 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 mam-
malogist. Unfortunately, Rene was killed in an auto-
mobile 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.

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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 the La Cementos Nacional. Working conditions here were excellent, the biodiversity in bats was incredible, and we applaud their conservation efforts which were 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 elctrolux 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.

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 feder-
ally 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 try-
ing 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 'planter’. 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 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, Kimberly 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 Knyazhnyaskyi, 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 Lewonton 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).

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-flagellation. 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

**LITERATURE CITED**


